FUZZINESS, STRUCTURE-DEPENDENCY, AND 'STRUCTURAL ANTHROPOLOGY': AN EXTENDED REPLY TO PARKIN

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WHEN I was initially asked to write a review for *JASO* of Robert Parkin's *The Munda of Central India* (Parkin 1992) I balked, on the grounds that my ethnographic specialities do not include South Asia. The editor of this journal then renewed his efforts to enlist my services, with the argument that the book contains a great deal of material on kinship, a field in which I have demonstrated some degree of expertise. I suspect he had some difficulty finding a South Asianist who was prepared to wade through Parkin's kinship data and its analysis;[1] and that this difficulty stems from the oft-heralded retreat of kinship studies from pre-eminence in anthropology (e.g. Collier and Yanagisako 1987; Howell and Melhuus 1993: 39; Shapiro 1982: 257)—a retreat which, I argue below, has not proceeded with equal pace on all fronts. Whatever the case, it is by no means unprecedented or unjustified to allot a book to a reviewer on the basis of his 'theoretical' proclivities rather than his ethnographic ones. And if that book demonstrates a truncated and botched sense of theory, as Parkin's does, it is the reviewer's job to point this out (*JASO*, Vol. XXIV, no. 2, pp. 218–20). I did just that, and now I shall try to do the same for his response (*JASO*, Vol. XXV, no. 3, pp. 269–72).

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1. *Editor's note:* For the record, Professor Shapiro was the first and only reviewer we approached to review Dr Parkin's book.

May I be permitted first to place on record my personal pain at Parkin's utterly mistaken surmise that I would restore the state of siege that existed between our respective countries two centuries ago. My own doctorate in anthropology is from a Commonwealth university built upon the 'Oxbridge' model, and from this base I was able to launch a stable and rewarding early career, vestiges of which (as this debate indicates) I am pleased to retain. My last sabbatical leave from my American post was spent mostly at the LSE, and I regard this period too as one of great productivity, and with considerable affection. Some of my best academic friends are British anthropologists.

Moreover, I endorse Parkin's sense of a 'transatlantic anthropology', both as a desirable state of affairs and as an already—if (as I hope to demonstrate) imperfectly achieved one. Indeed, it is far from clear—indeed, it may even in certain senses approach meaninglessness to ask—who is European and who American. My own sojourns have just been briefly noted. The far more illustrious ones of Lévi-Strauss are a matter of published record (Lévi-Strauss 1961). Radcliffe-Brown and Rodney Needham (among others) have dallied on the western fringes of the Atlantic, while David Schneider (among others) has frequently gone to its eastern shores. This is hardly an unusual situation in human social life, as Eric Wolf (1982) reminded us not long ago. Furthermore, in this instance its pace has quickened with time. Thus, more than forty years ago Murdock (1951) could come close to hitting the bull's-eye in his remarks on the parochialism of British anthropology, but two decades later Murphy's comparable indictment (Murphy 1971: 17–27) had to be more qualified. Another two decades have passed, with the result that a commentator upon the matter in 1995 would have to pay yet more attention to its 'statistical' character.

Which is to say that boundaries between categories are usually 'fuzzy'. Cognitive anthropology—what Needham (1971: xxix–xxxi), not I, dubbed 'American formalism'—has been making and substantiating this point for approximately thirty years, since Floyd Lounsbury's seminal articles (1964, 1969) on prototypicality and extension in kin-classification.[2] Lounsbury's argument, since pursued most arduously by Harold Scheffler (see references), is that the prototypical or 'focal' members of kin-classes are those who *by local ethno-genealogical reckoning* are close kin.[3] In his multitudinous writings on 'prescriptive alliance' over the years, Needham (see references) has not so much contested the argument as rejected it summarily, or ignored it; and his students and *their* students have blithely followed suit (e.g. Cunningham 1967, Rivière 1969, Korn 1973, R. H. Barnes 1974, Parkin 1992). Moreover, the Platonic type has been

2. For a recent review of this and other issues in cognitive anthropology, see D'Andrade 1995.

3. The expression 'ethnogenealogical' seems to have been put forward by Conklin (1964), another 'American formalist'.

stroked and maintained at nearly all empirical cost.[4] Since his two-part article on 'Terminology and Alliance' (Needham 1966, 1967), Needham has insisted that there is no correlation between the (alleged) structure of a scheme of 'social classification' and the network of connubial relations among the associated and (supposedly) isomorphic 'descent groups'—or even the very existence of such groups;[5] and again his scholastic lineage has simply followed him. And the contention made in his early work that 'prescriptive alliance' is linked with pervasive dualistic classification (e.g. Needham 1959: 129–34, 1960a: 108–16, 1962b: 87–96) has been discretely abandoned in the face of overwhelming contrary evidence, some of which appears in the essays in the collection he edited (Needham (ed.) 1973).[6] The philosophy-of-science requirement (see especially Hempel 1965: 146ff.) that typologies be 'natural' in positivist discourse—i.e. that they point to a real-world association *of some kind or other*—is not usually considered in the writings of Needham and his followers (see also Scheffler 1975: 232).

The heuristic value of Needham's work on 'prescriptive alliance' lay in the ethnographic caution that there may be kinship-like arrangements in the non-Western world that do *not* have ethnogenealogical referents as their *focal* members. But, in fact, neither he nor any of his followers, including Parkin, has genuinely considered this an open question. Exactly the same can be said of David Schneider's 'cultural constructionist' approach (see especially Schneider 1967). In my review of Parkin's book I gave Schneider deserved credit for unearthing—and encouraging his students to unearth—an impressive array of ethno-embryologies. But his repeated rendition of these as non-genealogical is quite beyond me (Shapiro 1982: 265–7). So too is the closure of his scheme. Thus in 1981 my monograph-length treatment of my Arnhem Land materials appeared, containing extensive argumentation that native kin-categories do indeed have as their primary referents close ethnogenealogical kin. I presented a copy to Schneider, who in the usual gentlemanly manner of his personal correspondence sent me a note of thanks, and proceeded to ignore my analysis.[7] I have no way of knowing whether or not he read the book: certainly it does not appear in any of his bibliographies. But he surely read my later article in *Man* (Shapiro 1988),

4. In a relatively early contribution, Schneider (1965) made precisely the same point. Though at one time frequently cited, however, his essay rambles clumsily from a celebration of the advantages of alliance theory over descent theory to a nasty assault on Needham. Schneider's own position, discussed below, is flawed in remarkably similar ways.

5. I justify below the parenthetical rendering here of 'alleged' and 'supposedly'.

6. I am unable to identify a point in Needham's scholarly career at which the alleged linkage is renounced.

7. It bears noting that I received exactly the same response from Needham. Scheffler (1991: 377) has recently commented upon Schneider's disinclination to address critics.

which contained a less extensive but otherwise comparable reanalysis of Greek-American materials originally treated by one of his students (Chock 1974). For he published a retort (Schneider 1989), which I found rich with 'postmodern' significance but otherwise insubstantial (Shapiro 1989), and patently devoid of commentary on my reanalysis. I do not take this personally: he has given Scheffler's devastating critique (1976) of his own work on American culture much the same short shrift. All this should disabuse Parkin of the idea that I am prone to giving jingoistic renditions of the history of kinship studies. It should instead call attention to one of the strangest 'transatlantic' bedfellowships in this history (see especially Needham 1962b).

Between the respective fates of Needham's and Schneider's work on kinship, though, there is an important difference. Needham's mode of analysis has passed into the quasi-oblivion under which kinship studies nowadays labour, sustained mostly by Parkin and other loyalists of Oxford derivation. In contrast, Schneider not only has a comparable array of students in my own age-grade,[8] he is also a venerated elder of a regnant anthropological 'cultural constructionism' in which kinship studies continue to flourish (e.g. Delaney 1991: 14–15; Yanagisako and Collier 1987: 29–34; Yanagisako and Delaney 1995), and which has traceable ties to 'postmodernism' and other exemplars of the literary left. These ties are only partly dependent upon his alliance with Clifford Geertz. Although the explicit call of 'cultural constructionism' is for closer links between anthropology and the humanities rather than the sciences (see especially Geertz 1983: 19–35), the practical effect is to sustain a *sui generis*, relativist, and particularist discipline with no concern (if not contempt) for larger theoretical issues (Spiro 1986). By this route Schneider takes us right readily back to the parochialism that Murdock saw in British anthropology in 1951. When I was last in the UK, in 1989, I noticed that Schneider and especially Geertz were the most oft-cited American anthropologists in seminars (whose atheoretic quality, I might add, struck me forcibly); I think this is not accidental.[9] For my part I would keep my distance from this mode of 'transatlantic anthropology'. So, I take it from his remarks, would Parkin.

Parkin's charge that I 'use the depths of history as a means of expressing distance and disapproval' is mistaken. I have great respect for Morgan and Lévi-Strauss and find their 'global' sense of the human situation far more engaging than the 'local knowledge' advocated by Geertz, Schneider, and all too much of British anthropology; and if I have been critical of them, as I have been especially of Lévi-Strauss (Shapiro 1982: 260–62, 1991, 1992), I sometimes feel rather like a gnat attacking an elephant. At the same time, it seems to me self-evident that, in the case of Morgan, a man now dead for a century is perhaps someone on

8. For some examples, see Shapiro 1982: 266–7.

9. A 'lateral' influence is the 'rationality versus relativism' debate, which is especially strong in British philosophy and which has counterparts in philosophical circles on my side of the Atlantic; see, especially, Hollis and Lukes (eds.) 1982.

whom to build, but he is unlikely to have provided the most penetrating analysis of all the materials available in his day, much less the fuller corpus we now possess. And I am as bothered by the closure of Lévi-Strauss's theories as I am by those of Needham and Schneider. Of particular relevance here is Lévi-Strauss's continued romance with 'the atom of kinship' he first formulated in 1945, despite what is by now an enormous amount of analytical critique and contrary empirical evidence (Lévi-Strauss 1976: 82–112, 1985: 63–72).[10]

My own view, which I began to systematize some time ago (Shapiro 1982), is that the 'cognitive extensionism' most closely associated, in recent years, with Scheffler's name is far more analytically telling and theoretically impressive.[11] I remain sceptical of the ethnographic reality of some of Scheffler's 'rewrite rules' (but see Scheffler 1972a), but it is simply not true, as Parkin (1994: 269) suggests or claims, that cognitive extensionism is not significantly different from componential analysis, and that it does 'not add anything that cannot be provided by a conventional analysis using genealogical denotation'. Such denotation, to which componential analysis has almost solely confined itself, proffers no appreciation of questions of focality; Scheffler has repeatedly made this very point (e.g. Scheffler 1972b: 324–5, 1972c: 129–30; Scheffler and Lounsbury 1971: 72–3) and it is now a central issue in semantic theory (MacLaury 1991). To ignore it, as Lévi-Strauss, Needham, and Schneider have all done, is to abandon anthropology's most fruitful cross-disciplinary ties for one or another scholasticism, and/or for the present 'postmodern' rage. I take this up again below. Here I would contest Parkin's reference (1994: 270) to 'Scheffler's blank refusal to consider the affinal terminology [in systems of 'prescriptive alliance'] as anything more than an epiphenomenon of terms for consanguines'. Scheffler has not 'blankly refused to consider' anything: here as elsewhere he has *argued* the issue—in this instance too on the basis of focality (e.g. Scheffler 1971: 235–7, 1978: 135–7; Scheffler and Lounsbury 1971: 198ff.). For my part, I think it possible in some cases to contest his argument (though this is not the place to do so), but the 'blank refusals' have come not from him but, in the opposite direction, from Lévi-Strauss, Needham, and their followers.

Let me try to illustrate the power and theoretical import of cognitive extensionism by appealing to a time-honoured problem in kinship studies, one which has engaged the attention of Morgan, Lévi-Strauss, Needham, and many others, including Parkin: the association between bifurcate merging kin-classification, with or without special affinal terms, and orderings of the sort that have been

10. The pertinent literature here is enormous. Some of it is summarized in Scheffler 1973 and in Shapiro 1982.

11. This was not always the case in my own thinking. Early on I was much attracted to that version of alliance theory that is, I think, best exemplified in the writings of Louis Dumont (see, for example, Shapiro 1968, 1971a, 1971b).

canonically rendered as 'unilineal descent groups'.[12] The correlation, though not without exception, is beyond question (i.e. beyond chance). Murdock demonstrated it in a classic article (1947) on the positivist method in anthropology, extended its empirical base in his seminal *Social Structure* (Murdock 1949: 164–6), and some years later suggested that many of the exceptions could be accounted for by an appeal to a logically connected structuring through some form or other of unilocal residence (Murdock 1960). But exceptions remain aplenty, especially in Amazonia (see, for example, Basso 1973, Gregor 1977, Kaplan 1975, Rivière 1969, Seeger 1981). What is more, the structure of most bifurcate merging systems, and the practical application of probably all, is by no means fully consistent with unilineal descent or unilocal residence. Scheffler and I have been arguing the point for some time now (Shapiro 1982: 267–8), but clearly with insufficient impact. Supplementary argument is therefore required.

Any system of kin-classification for which a claim of isomorphy with (exogamous) unilineal descent can be sustained must distinguish between the terms a man applies to his children and those a woman applies to hers. But in fact in the overwhelming majority of bifurcate merging schemes, as in English kin-classification, husband and wife apply the same term, or set of terms, to their mutual offspring. That is, if relationship terms are 'group'-specific, as alliance theorists and others have argued, and the 'groups' are exogamous, my wife and I, as members of different 'groups' must by deduction have separate ways of referring to our mutual offspring. The 'groups' argument can here be salvaged by positing that locality rather than lineality is the formative principle, but in that case it has to deal—and it cannot—with the fact that my brother and my wife's sister use the 'offspring' term or terms for *our* children in nearly all these systems, *whether or not these kin are co-resident with us.* Even more patently destructive to 'group' interpretation is the equation, again in the overwhelming majority of bifurcate merging systems, of paternal and maternal grandparents, which neither a 'unilineal' nor a 'unilocal' theory can for an instant sustain. And there are further difficulties with these theories at more distant collateral positions (Lounsbury 1968: 133–4; Scheffler 1971). It seems fair to suggest that 'group' renditions of these terminologies are naïve in the extreme, and that they disrespect ethnographic data that have been secured since Morgan's day (again see Lounsbury 1968: 133–4). In view of Parkin's admiration for Morgan, I would suggest he

12. In what follows I use the expression 'unilineal descent groups' in what I take to be this canonical sense. But I think I am entitled to observe, in 1995, that the rubric refers to an incredibly mixed bag of ethnographic materials, one which is not yet fully unpacked by any means. Thus, for example, Adam Kuper's assaults on 'lineage theory' (Kuper 1982a, 1982b: 43–50) strike me as important but, none the less, perhaps only the tip of the proverbial iceberg. Other pertinent literature is far too extensive to cite here. I would note, though, that my use below of the term 'groups' in quotes signals this same dissatisfaction: I think it can fairly readily be shown that most of what appear in the theoretical literature as 'groups' are in fact categories (Keesing 1975: 9–11).

attend more closely to the tables in *Systems of Consanguinity and Affinity of the Human Family.*[13]

Most Aboriginal Australian systems of kin-classification are remarkable in that they seem to satisfy all the requirements of a 'unilineal' interpretation, including separate 'man's offspring' and 'women's offspring' terms and a terminological separation of paternal and maternal grandparents. But this conclusion rests upon an analysis only slightly less superficial than the one made by alliance and other 'group' theorists for the non-Australian systems. Let me illustrate by appealing to my own extensive data from north-east Arnhem Land.

Here a man calls his MB by a term I shall gloss as 'uncle'. It is the 'uncle' class that is the appropriate WF category—so much so that, even when a man's father-in-law belongs initially to another class, he is sometimes *but by no means always* reassigned to the 'uncle' category. This is somewhat—only somewhat—helpful to alliance theory. But now consider that the designation of MB as 'uncle' rests solely on the ethnogenealogical reckoning of him as MB, and that the same designation of other men nearly always rests on comparable reckoning—of the sort 'I call him "uncle" because my mother called him "brother"'. Although he can be construed as a potential 'wife-giver', this construction is irrelevant to his designation. This is bad news for alliance theory.

Now if my MB has a son, and if I base my classification of his son solely on my classification of the boy's father, I shall call that boy by another term, which I here gloss as 'cousin'; and if that 'cousin' has a son and I base my classification of *that* boy solely on my classification of *his* father, I shall call this youngest boy 'uncle'. So the pattern is: 'uncle' – 'cousin' – 'uncle' – ('cousin'). Informants readily recited this cycle in the abstract, though for reasons that will soon become clear, it is not frequently realized in my concrete genealogies. Even so, these are the best data for alliance theory and other 'group' renditions of systems of kin-classification. The 'unity of the lineage'—to employ a notion that alliance theory has borrowed from Radcliffe-Brown (1941), though it assiduously avoids his expression—has been preserved; and the fact that other 'lineages' display the

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13. Alf Hornborg is one among many who tries to save the day for 'group' interpretations of those bifurcate merging systems lacking special affinal terms by appealing to the alleged 'two-line' structure of these systems *sui generis*. In this cause he employs an argument, glossed over by Scheffler (1971: 34), that 'each term has a different meaning for male as opposed to female speakers' (Hornborg 1987: 456). But the 'two-line' interpretation still runs foul of the absence of lineal distinction at the grandparental (and other) levels, which Hornborg (ibid.) simply dismisses as irrelevant in generating a 'symmetric alliance structure'. This juxtaposition of tactics thus raises the question as to whether the alleged 'structure' or 'structures' exist in Hornborg's head or in the data he analyses. As I have argued elsewhere (Shapiro 1985), they do not exist in the data. In general, his article is remarkable only because of its strategic employment of the most tenuous Lévi-Straussian mysticism about 'underlying structures' (see below), and because he is the only alliance theorist who even begins to confront the structural problems that his analysis of a system of kin-classification faces. But, thus begun, the confrontation is abandoned in an attempt to salvage the theory. See also Hornborg 1993.

same pattern is no problem, for alliance theory has long insisted that it is the *category* of 'wife-giving lineage' and not specific 'lineages' that counts (see especially Maybury-Lewis 1965). And especially in recent years it has become enamoured with the 'alternate-generation' pattern we see in evidence here, as Parkin's response indicates.

But now recall that this neat scheme is based upon native notions of patrifiliation. If, by contrast, comparable notions of *matrifiliation* are employed, the entire structure can and does collapse. Suppose, for example, my above-mentioned 'cousin' through patrifiliation sires a son through a woman I call 'sister' and I now choose matrifiliation to classify this son, we have—as we say on this side of the Atlantic—a whole new ball game, to wit: 'uncle' – 'cousin' – 'sister's child' (for further pertinent data see Shapiro 1981: 34–8).

Now for alliance theory this is sheer chaos: 'wife-giving lineages' (i.e. those with men ego calls 'uncle') become 'wife-taking lineages' (i.e. those with men ego calls 'sister's child'), and any semblance of 'the unity of the lineage' collapses. This is because in alliance theory 'lineage unity' is a gimmick based (as I have argued, and will argue further below) on a limited analysis of a limited genealogical space (see also Scheffler 1970: 262–4).

It bears repetition that resort to matrifiliation in this way is a very frequent procedure, not only in north-east Arnhem Land but in others parts of Aboriginal Australia as well. Indeed, the available evidence suggests that it is at least as common as patrifiliation in this sphere (see, for example, Falkenberg and Falkenberg 1981: 169ff.; Shapiro 1977: 30, 1981: 35; Turner 1974: 16–18). But I would insist that it is alliance theory that is chaotic. Aboriginal Australian systems of kin-classification, for their part, are quite orderly *when viewed as systems of kin-classification* and not as schemes of 'prescriptive alliance'. This is Scheffler's argument (see especially Scheffler 1978), and he is right as can be, or nearly so (but see Shapiro 1982).

There are, however, even more data pertinent to this argument, and they are even more important for anthropological theory. North-east Arnhem Land semantic structure divides all kin-classes into 'full' (*dangang*) and 'partial' (*marrkangga*) subclasses. In the case of the uncle class informants were likely to nominate for 'full' membership any 'uncle' of ego's mother's patriclan, and/or whose mother's patriclan is that of ego's MM. Which is to say that the 'uncle' kin-class focuses on certain persons who are *by native criteria* kin and not non-kin (see Shapiro 1981: 40–41). The fact that it also includes, in a secondary sense, others who are rendered by these same criteria as non-kin is of interest, as is the fact that this is the normative kin-class for male ego's father-in-law. Alliance theory's error—and here it commingles with more than a century of anthropological thought on Aboriginal Australia—is to elevate these secondary semantic considerations to primary status.

The foregoing analysis can be pushed still further. In the form I have so far presented it, it applies equally to women of a kin-class I would gloss as 'mother', who usually refer to men ego calls 'uncle' as (a term I would gloss as) 'brother'.

Now, since Dumont's well-known article (1953) on 'Dravidian Kinship Terminology as an Expression of Marriage', alliance theory has suggested that the designation of ego's genetrix is structurally secondary, an epiphenomenon of sorts of her being a sister or wife of a male in an affinal relationship with another male. But in point of fact, not only is the genetrix the focal member of the 'mother' class in north-east Arnhem Land—a point I have argued in greater detail elsewhere (Shapiro 1981: 87ff.)—but she is also the focal member of a superclass whose members include the denotata of both the 'mother' and 'uncle' classes. This is so because men of this latter class too are sometimes called 'mother'—as if membership in the superclass were extended, without regard to gender, by appeal to its quintessential member. And this is *precisely* how my informants put it. They often referred to men of the 'uncle' category as *ngama darramu* (literally 'male mother'), expanding with *nakuna ngama, yurru darramu* ('like mother, but male'). And they idiomized their relationship to their own sisters' children by referring to the latter as *ngarraku gulun* ('my abdomen', 'my womb') or by touching their abdomens—this despite their knowledge (or, in deference to the 'cultural constructionists', their 'construction') that men sire children but do not give birth to them or have wombs (ibid.: 16–20). Nephews and nieces, for their part, sometimes say that they 'come from the wombs' of their own mother's brother (*gurrukanawuy* or *gulunpuy*), which is to say they use the maternal idiom of generation (ibid.: 87–8). And they signal the mother's brother by touching the right nipple, just as the genetrix herself is indicated by the left nipple. These are, I submit, decisive points against alliance theory, as well as against the (enormous) relativist component of 'cultural constructionism', but there are parallels throughout Australia (Scheffler 1978) and, indeed, elsewhere.[14]

None of the foregoing should be taken to mean that 'groups' and other local institutions have no role in the shaping of ethnogenealogical space, or that native users of kin-terms do not employ such considerations in assigning people to kin-classes: there is abundant evidence for these operations, and Scheffler (e.g. 1972b: 324, 1973: 767–9; Scheffler and Lounsbury 1971: 198) has, I think, shown a consistent appreciation of them. Nor can Murdock's important findings, some of which were noted above, on the salience of 'institutions' in kin-classification be dismissed. It is not that these operations and findings are irrelevant: *it is that they are secondary*. And they are secondary not—or not only—because

14. Some of this argument may seem to be indebted to Radcliffe-Brown's classic formulation (1924) of the MB/ZS relationship in Bantu Africa and elsewhere, in which case I would retort that his extensionist analyses are often on the mark. As Scheffler (1978: 70–74) has observed, much later non-Australian ethnography supports him. And although, because of his most famous comparative treatment (Radcliffe-Brown 1931), he managed to lead subsequent generations of scholars to see Aboriginal Australian kin-classification only as 'allied patrilines', shortly before his death he proffered a far more insightful—if less well-known—analysis (Radcliffe-Brown 1951; see also his (1953) retort to Dumont). The left/right dichotomy as a representation of the female/male one is, of course, widespread, as Needham's important compendium (Needham (ed.) 1973) shows.

Lounsbury, Scheffler, and some other anthropologists, including myself, say so but *because the natives say so*. Parkin can cite Needham, just as Needham can cite Fison, Hocart, and the Dutch Masters until the cows come home (Shapiro 1975). And Schneider can score points by appealing to the self-hatred of the trendy left (Shapiro 1989) until the bimillenium, and some recent commentaries suggest he will do just this (e.g. Schneider 1989, 1992, 1995). But despite both Needham's and Schneider's professions of concern for careful ethnographic analysis, neither has rendered it, partly because both are awesomely ignorant of current issues in semantics, especially prototype theory. Bifurcate merging and other systems, I would suggest, appear so different from the 'lineal' ones of English and other Indo-European languages because for well over a hundred years we have, in analysing the former, mostly ignored questions of subclassification, discursive commentary, and ease of translation or 'codability' (Brown 1956: 307ff.). We have founded a neo-relativism on *precisely* the same kind of translation fallacy that forged the initial great period of relativism in anthropology *circa* 1930.[15]

I now come to a key point. I submit that, when we entertain these questions, we have substantial evidence for the genealogical unity of humankind, for which Schneider and Needham, among others, have expressed such contempt (see especially Needham 1962a; Schneider 1984: 119ff.). This conclusion is mostly implicit in Scheffler's work, but it is more fully blown in the theorizing of certain other anthropologists, for example, J. A. Barnes (1973), Derek Freeman (1973), and Roger Keesing (1990).[16] If this seems like 'biological reductionism', it is a 'reductionism' with considerable evidential support, and there is no reason whatsoever to ascribe to it the immutability that has been used historically to sustain polemics against such interpretations. Indeed, there is some evidence that the new reproductive technology already requires its modification (see Shore 1992, Strathern 1992). But I daresay it can be sustained for most of human history and ethnography and is thus an accurate rendering of an aspect of the human situation, not an ethnocentric distortion of the facts.[17] As for the debate at hand, I endorse

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15. On this, see Lounsbury's distinction (1969) between 'limited' and 'complete' relativism and Lukes's important contribution (1982). The latter contains important references.

16. Judging from his most recent critique (1991) of 'cultural constructionism', I suspect that Scheffler is about ready to join them.

17. In contrast, both alliance theory and 'cultural constructionism' have charged cognitive extensionism with overcommitment to Western notions, or even to notions more or less confined to certain anthropologists (e.g. Needham 1962a, Schneider 1984). Schneider in particular has shown consistent opposition to innatist argument (Schneider 1976, 1995). Needham occasionally uses essentialist language in reference to bipartite schemes of classification—'a fundamental feature of the human mind' (1960b: 106) or 'primary factors of human experience' (1973a: xxxi)—but such claims, he rightly notes, 'rapidly pass the limits of proof' (ibid.). It seems safe to speculate that neither is aware of the mutability allowed for in recent biological theory (e.g. Hinde 1982: 85ff.; Lehrman 1970; Ridley 1993: 313–20), or of the fact that—and I think this is especially important given the present hegemony of 'cultural constructionism'—'biological'

Parkin's admonition to attend to Morgan and Lévi-Strauss, but I should certainly add at least Freud and Darwin to my reading list.

Finally, and related, there is the question of how seriously 'structural anthropology' in the UK, France and elsewhere takes the notion of 'structure' in human affairs. Lévi-Strauss pays lip-service—nothing more—to neurology (see Rossi 1982: 267–74). Needham is at least as constricted. In one of his more mature statements on 'prescriptive alliance' he distinguishes among 'three main aspects of collective conduct and representations...(1) behaviour, (2) rules, (3) categories' (Needham 1973b: 171), giving the last a decided analytical priority on grounds that, I think, can be fairly described as Platonic. And a more recent article (Needham 1986) suggests a yet more profound retreat into the realm of Pure Forms. All this corresponds quite closely with Lévi-Strauss's decided preference for 'elementary' as opposed to 'complex' structures. The point that Needham's 'categories' and Lévi-Strauss's 'elementary structures' have been misanalysed has already been made and is not the issue here. What is remarkable at this juncture is the simplistic sense both scholars have of 'structure', particularly the way in which they regard it as separate from 'behaviour' and as having an intrinsically 'collective' character.

In contrast, consider what Howard Gardner (1985) calls 'the cognitive revolution' in psychology. In one of its pioneer articles, Karl Lashley (1951) called attention to a variety of behavioural operations that we now call, following Chomsky (1972: 61), 'structure-dependent', for example, transpositions in typewriting. One of the commonest, to which the editors of scholarly journals will surely attest, is the simple transposition of adjacent letters ('bald' rendered as 'blad'), but there are considerably more sophisticated ones. Yet even the simplest call attention to the fact that chunks of behaviour do not occur atomistically, as the early cognitivists' behaviourist opponents would have it, but depend instead on an overarching plan or structure; and two generations of ethological research have attempted to identify such structures not only in *Homo sapiens* but in the rest of nature as well (Gardner 1985: 31).

Now, it may seem a long way from typing transpositions to systems of kin-classification, but this is true only if one sees the latter as structurally simple, intrinsically collective, and devoid of context and history, as Lévi-Strauss, Needham, Schneider and their followers all tend to do. But in fact there is an enormous amount of evidence, in addition to that of my north-east Arnhem Land materials presented above, that this is not the case. Which is to say that such 'systems' are often less than systemic or, if I may borrow an expression the late Roger Keesing liked to use, 'messy'. This being so, there is no reason to follow the purity-quests of Lévi-Strauss and Needham and suppose that 'prescriptive alliance' schemes are any less messy than 'behaviour'. Both have elements of

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theories of human affairs have at least as often been associated with the political left as with the political right (Degler 1990).

'chaos' that it is the job of analysis both to appreciate and to order, attending in the process as closely as possible to comparable operations performed by native informants. Lévi-Strauss and Needham may take refuge in ancient Durkheimian fiat to escape this messiness, just as Schneider achieves the same result by embracing the literary conceits of his friend Geertz that 'culture' is intrinsically shared (Geertz 1973: 10) and that it exists apart from species constraints (ibid.: 33–54). But 'the cognitive revolution' and its implications are here to stay; and only an increasingly desperate polemics, often combined with scholarly ignorance of the most forced sort, can pretend otherwise.

So there is no ultimate analytical difference between 'terminology' and 'behaviour', as Parkin, clearly following Needham, says there is.[18] Those of us who see this do indeed have more sweating to do, but it is sweating over rich data, in an effort to find analytical schemes that respect them. By contrast, alliance theory and 'cultural constructionism' are defending houses of cards.

18. At this point in his response Parkin conflates several distinct issues in the history of anthropology—among them the alleged isomorphy between kin-class and behavioural class, the relationship between such classes and ongoing behaviour, and the structural analysis of behaviour in general, whether or not it is associated with kin-classification. For a partial untangling, see Shapiro in press. In contrast, in an earlier exchange, Maybury-Lewis (1974: v) observes that my critique (Shapiro 1971b) of the first edition (Maybury-Lewis 1967) of his monograph on the Shavante of Central Brazil 'misses the point of structuralism', which, he claims, is the unified analysis of behavioural and cognitive data. He thus assumes towards me much the same role Lévi-Strauss (1960) adopted towards him some years earlier, and which Hornborg has more recently endeavoured to effect (see note 13 above). All this suggests that some of the grand (and probably untestable) claims that Lévi-Strauss makes for 'underlying structures' have diffused not only to the UK (and the USA), but also to parts further removed from the transatlantic centre of the discipline. In any case, I am now sympathetic to Maybury-Lewis's programme. However, his analysis of Shavante kin-classification has many of the defects of alliance theory with which I have more recently charged Parkin. Neither Maybury-Lewis nor Parkin can be accused of 'missing the point of structuralism' as a scholastic niche. What they lack instead is a sense of the requirements of an earnest structural analysis of real-world data, whether cognitive or behavioural.

REFERENCES

BARNES, J. A. 1973. 'Genetrix : Genitor :: Nature : Culture?', in Jack Goody (ed.), *The Character of Kinship*, Cambridge: Cambridge University Press, pp. 61–73.

BARNES, R. H. 1974. *Kédang: A Study of the Collective Thought of an Eastern Indonesian People* (Oxford Monographs on Social Anthropology), Oxford: Clarendon Press.

BASSO, ELLEN B. 1973. *The Kalapalo Indians of Central Brazil* (Case Studies in Cultural Anthropology), New York: Holt, Rinehart and Winston.

BROWN, ROGER W. 1956. 'Language and Categories', in Jerome Bruner *et al.*, *A Study of Thinking*, New York: John Wiley & Sons, pp. 247–312.

CHOCK, PHYLLIS PEASE 1974. 'Time, Nature, and Spirit: A Symbolic Analysis of Greek-American Spiritual Kinship', *American Ethnologist*, Vol. I, no. 1, pp. 33–47.

CHOMSKY, NOAM 1972. *Language and Mind* (enlarged edn.), New York: Harcourt Brace Jovanovich.

COLLIER, JANE FISHBURNE, and SYLVIA JUNKO YANAGISAKO 1987. 'Introduction', in their (ed.), *Gender and Kinship: Essays toward a Unified Analysis*, Stanford: Stanford University Press, pp. 1–13.

CONKLIN, HAROLD C. 1964. 'Ethnogenealogical Method', in Ward H. Goodenough (ed.), *Explorations in Cultural Anthropology: Essays in Honor of George Peter Murdock*, New York: McGraw-Hill Book Company, pp. 25–55.

CUNNINGHAM, CLARK E. 1967. 'Atoni Kin Categories and Conventional Behavior', *Bijdragen tot de Taal-, Land- en Volkenkunde*, Vol. CXXIII, no. 1, pp. 53–70.

D'ANDRADE, ROY G. 1995. *The Development of Cognitive Anthropology*, Cambridge: Cambridge University Press.

DEGLER, CARL N. 1990. 'Darwinians Confront Gender: Or, There Is More to It than History', in Deborah L. Rhode (ed.), *Theoretical Perspectives on Sexual Difference*, New Haven: Yale University Press, pp. 33–46.

DELANEY, CAROL 1991. *The Seed and the Soil: Gender and Cosmology in Turkish Village Society* (Comparative Studies on Muslim Societies 11), Berkeley: University of California Press.

DUMONT, LOUIS 1953. 'The Dravidian Kinship Terminology as an Expression of Marriage', *Man*, Vol. LIII, pp. 34–9.

FALKENBERG, ASLAUG, and JOHANNES FALKENBERG 1981. *The Affinal Relationship System: A New Approach to Kinship and Marriage among the Australian Aborigines at Port Keats* (Oslo Studies in Social Anthropology No. 1), Oslo: Universitetsforlaget.

FREEMAN, DEREK 1973. 'Kinship, Attachment Behaviour and the Primary Bond', in Jack Goody (ed.), *The Character of Kinship*, Cambridge: Cambridge University Press, pp. 109–19.

GARDNER, HOWARD 1985. *The Mind's New Science: A History of the Cognitive Revolution*, New York: Basic Books.

GEERTZ, CLIFFORD 1973. *The Interpretation of Cultures: Selected Essays*, New York: Basic Books.

. . . 1983. *Local Knowledge: Further Essays in Interpretive Anthropology*, New York: Basic Books.

GREGOR, THOMAS 1977. *Mehinaku: The Drama of Daily Life in a Brazilian Indian Village*, Chicago: University of Chicago Press.

HEMPEL, CARL G. 1965. *Aspects of Scientific Explanation, and Other Essays in the Philosophy of Science*, New York: Free Press.

HINDE, ROBERT A. 1982. *Ethology: Its Nature and Relations with Other Sciences*, New York: Oxford University Press.

HOLLIS, MARTIN, and STEVEN LUKES (eds.) 1982. *Rationality and Relativism*, Oxford: Basil Blackwell.

HORNBORG, ALF 1987. 'Lineality in Two-Line Relationship Terminologies (Commentary)', *American Anthropologist*, Vol. LXXXIX, no. 2, pp. 454–56.

HORNBORG, ALF 1993. 'Panoan Marriage Sections: A Comparative Perspective', *Ethnology*, Vol. XXXII, no. 1, pp. 101–8.

HOWELL, SIGNE, and MARIT MELHUUS 1993. 'The Study of Kinship, The Study of Person, A Study of Gender?', in Teresa del Valle (ed.), *Gendered Anthropology* (EASA Series), London: Routledge, pp. 38–53.

KAPLAN, JOANNA OVERING 1975. *The Piaroa—A People of the Orinoco Basin: A Study in Kinship and Marriage*, Oxford: Clarendon Press.

KEESING, ROGER M. 1975. *Kin Groups and Social Structure*, New York: Holt, Rinehart and Winston.

. . .   1990. 'Kinship, Bonding, and Categorization', *TAJA: The Australian Journal of Anthropology*, Vol. I, nos. 2–3 (Special Double Issue 1, 'On the Generation and Maintenance of Person: Essays in Honor of John Barnes', guest ed. Warren Shapiro), pp. 159–67.

KORN, FRANCIS 1973. *Elementary Structures Reconsidered: Lévi-Strauss on Kinship*, London: Tavistock Publications.

KUPER, ADAM 1982a. 'Lineage Theory: A Critical Retrospect', *Annual Review of Anthropology*, Vol. XI, pp. 71–95.

. . .   1982b.   *Wives for Cattle: Bridewealth and Marriage in Southern Africa* (International Library of Anthropology), London: Routledge & Kegan Paul.

LASHLEY, K. S. 1951. 'The Problem of Serial Order in Behavior', in Lloyd A. Jeffress (ed.), *Cerebral Mechanisms in Behavior: The Hixon Symposium*, New York: John Wiley & Sons, pp. 112–36.

LEHRMAN, DANIEL S. 1970. 'Semantic and Conceptual Issues in the Nature–Nurture Problem', in Lester R. Aronson *et al.* (eds.), *Development and Evolution of Behavior: Essays in Memory of T. C. Schneirla*, San Francisco: W. H. Freeman, pp. 17–52.

LÉVI-STRAUSS, CLAUDE 1960. 'On Manipulated Sociological Models', *Bijdragen tot de Taal-, Land- en Volkenkunde*, Vol. CXVI, no. 1, pp. 45–54.

. . .   1961. *Tristes Tropiques*, New York: Atheneum.

. . .   1976. *Structural Anthropology, Volume II* (transls. Joachim Neugroschel and Phoebe Oss), Chicago: University of Chicago Press.

. . .   1985. *The View from Afar*, New York: Basic Books.

LOUNSBURY, FLOYD G. 1964. 'A Formal Account of the Crow- and Omaha-Type Kinship Terminologies', in Ward H. Goodenough (ed.), *Explorations in Cultural Anthropology: Essays in Honor of George Peter Murdock*, New York: McGraw-Hill Book Company, pp. 351–93.

. . .   1968 [1964]. 'The Structural Analysis of Kinship Semantics', in Paul Bohannan and John Middleton (eds.), *Kinship and Social Organization* (American Museum Sourcebooks in Anthropology), Garden City, NY: Natural History Press for the American Museum of Natural History, pp. 125–48.

. . .   1969. 'Language and Culture', in Sidney Hook (ed.), *Language and Philosophy: A Symposium*, New York: New York University Press, pp. 3–29.

LUKES, STEVEN 1982. 'Relativism in its Place', in Hollis and Lukes (eds.), pp. 261–305.

MACLAURY, ROBERT E. 1991. 'Prototypes Revisited', *Annual Review of Anthropology*, Vol. XX, pp. 55–74.

MAYBURY-LEWIS, DAVID H. P. 1965. 'Prescriptive Marriage Systems', *Southwestern Journal of Anthropology*, Vol. XXI, no. 3, pp. 207–30.

. . . 1967. *Akwe-Shavante Society*, Oxford: Clarendon Press.

. . . 1974. 'Preface', in his *Akwe-Shavante Society* (revd edn.), Oxford: Clarendon Press.

MURDOCK, GEORGE PETER 1947. 'Bifurcate Merging: A Test of Five Theories', *American Anthropologist*, Vol. XLIX, no. 1, pp. 56–68.

. . . 1949. *Social Structure*, New York: Free Press.

. . . 1951. 'British Social Anthropology', *American Anthropologist*, Vol. LIII, no. 4, pt. 1, pp. 465–73.

. . . 1960. 'Cognatic Forms of Social Organization', in his (ed.), *Social Structure in Southeast Asia* (Viking Fund Publications in Anthropology No. 29), New York: Wenner-Gren Foundation for Anthropological Research, pp. 1–14.

MURPHY, ROBERT F. 1971. *The Dialectics of Social Life: Alarms and Excursions in Anthropological Theory*, London: George Allen & Unwin.

NEEDHAM, RODNEY 1959. 'An Analytical Note on the Kom of Manipur', *Ethnos*, Vol. XXIV, pp. 121–35.

. . . 1960a. 'Alliance and Classification among the Lamet', *Sociologus*, n.s., Vol. X, no. 2, pp. 97–119.

. . . 1960b. 'A Structural Analysis of Aimol Society', *Bijdragen tot de Taal-, Land- en Volkenkunde*, Vol. CXVI, no. 1, pp. 81–108.

. . . 1962a. 'Genealogy and Category in Wikmunkan Society', *Ethnology*, Vol. I, no. 2, pp. 223–64.

. . . 1962b. *Structure and Sentiment: A Test Case in Social Anthropology*, Chicago: University of Chicago Press.

. . . 1966. 'Terminology and Alliance, I: Garo, Manggari', *Sociologus*, n.s., Vol. XVI, no. 2, pp. 141–57.

. . . 1967. 'Terminology and Alliance, II: Mapuche, Conclusions', *Sociologus*, n.s., Vol. XVII, no. 2, pp. 39–54.

. . . 1971. 'Introduction', in his (ed.), *Rethinking Kinship and Marriage* (ASA Monographs 11), London: Tavistock Publications, pp. xiii–cxvii.

. . . 1973a. 'Introduction', in his (ed.), *Right and Left: Essays on Dual Symbolic Classification*, Chicago: University of Chicago Press, pp. xi–xxxix.

. . . 1973b. 'Prescription', *Oceania*, Vol. XLII, no. 3, pp. 166–81.

. . . 1986. 'Alliance', *Oceania*, Vol. LVI, no. 3, pp. 165–80.

NEEDHAM, RODNEY (ed.) 1973. *Right and Left: Essays on Dual Symbolic Classification*, Chicago: University of Chicago Press.

PARKIN, ROBERT 1992. *The Munda of Central India: An Account of their Social Organization*, Delhi: Oxford University Press.

. . . 1994. 'Munda Kinship and Transatlantic Anthropology: A Response to Shapiro (Comment)', *JASO*, Vol. XXV, no. 3, pp. 269–72.

RADCLIFFE-BROWN, A. R. 1924. 'The Mother's Brother in South Africa', *South African Journal of Science*, Vol. XXI, pp. 542–55 (reprinted as Chapter I in Radcliffe-Brown 1952, pp. 15–31).

. . . 1931. *The Social Organization of Australian Tribes* (Oceania Monographs No. 1), Melbourne: Macmillan.

RADCLIFFE-BROWN, A. R. 1941. 'The Study of Kinship Systems', *Journal of the Royal Anthropological Institute*, Vol. LXXI, pp. 1–18 (reprinted as Chapter III in Radcliffe-Brown 1952, pp. 70–84).

. . . 1951. 'Murngin Social Organization', *American Anthropologist*, Vol. LIII, no. 1, pp. 37–55.

. . . 1952. *Structure and Function in Primitive Society: Essays and Addresses*, New York: Free Press.

. . . 1953. 'Dravidian Kinship Terminology (Correspondence)', *Man*, Vol. LIII, p. 112.

RIDLEY, MATT 1993. *The Red Queen: Sex and the Evolution of Human Nature*, New York: Penguin.

RIVIÈRE, PETER 1969. *Marriage among the Trio: A Principle of Social Organization*, Oxford: Clarendon Press.

ROSSI, INO 1982. 'On the "Scientific" Evidence for the Existence of Deep Structures and their "Objective" and Mathematical Nature (Training Session for Rodney Needham, Ronald Cohen, Peter Caws and Paul Chaney)', in Inno Rossi *et al.*, *The Logic of Culture: Advances in Structural Theory and Methods*, South Hadley, MA: J. F. Bergin, pp. 265–93.

SCHEFFLER, HAROLD W. 1970. '*The Elementary Structures of Kinship* by Claude Lévi-Strauss: A Review Article', *American Anthropologist*, Vol. LXXII, no. 2, pp. 251–68.

. . . 1971. 'Dravidian–Iroquois: The Melanesian Evidence', in L. R. Hiatt and C. Jayawardena (eds.), *Anthropology in Oceania: Essays Presented to Ian Hogbin*, San Francisco: Chandler, pp. 231–54.

. . . 1972a. 'Baniata Kin Classification: The Case for Extensions', *Southwestern Journal of Anthropology*, Vol. XXVIII, no. 4, pp. 350–81.

. . . 1972b. 'Kinship Semantics', *Annual Review of Anthropology*, Vol. I, pp. 309–28.

. . . 1972c. 'Systems of Kin Classification: A Structural Typology', in Priscilla Reining (ed.), *Kinship Studies in the Morgan Centennial Year*, Washington, DC: Anthropological Society of Washington, pp. 113–33.

. . . 1973. 'Kinship, Descent, and Alliance', in John J. Honigmann (ed.), *Handbook of Social and Cultural Anthropology*, Chicago: Rand McNally College Publishing Company, pp. 747–93.

. . . 1975. 'Appendix III: A Structural Semantic Analysis of the Karo System of Kin Classification', in Masri Singarimbun, *Kinship, Descent and Alliance among the Karo Batak*, Berkeley: University of California Press, pp. 212–34.

. . . 1976. 'The "Meaning" of Kinship in American Culture: Another View', in Keith H. Basso and Henry A. Selby (eds.), *Meaning in Anthropology* (School of American Research Advanced Seminar Series), Albuquerque: University of New Mexico Press, pp. 57–91.

. . . 1978. *Australian Kin Classification* (Cambridge Studies in Social Anthropology 23), Cambridge: Cambridge University Press.

. . . 1991. 'Sexism and Naturalism in the Study of Kinship', in Micaela di Leonardo (ed.), *Gender at the Crossroads of Knowledge: Feminist Anthropology in the Postmodern Era*, Berkeley: University of California, pp. 361–82.

SCHEFFLER, HAROLD W., and FLOYD G. LOUNSBURY 1971. *A Study in Structural Semantics: The Siriono Kinship System*, Englewood Cliffs: Prentice-Hall.

SCHNEIDER, DAVID M. 1965. 'Some Muddles in the Models, Or How the System Really Works', in Michael Banton (ed.), *The Relevance of Models for Social Anthropology* (ASA Monographs 1), London: Tavistock Publication, pp. 25–85.

. . .   1967. 'Kinship and Culture: Descent and Filiation as Cultural Constructs', *Southwestern Journal of Anthropology*, Vol. XXIII, no. 1, pp. 65–73.

. . .   1976. 'The Meaning of Incest', *Journal of the Polynesian Society*, Vol. LXXXV, no. 2 (Special Issue, 'Incest Prohibitions in Micronesia and Polynesia'), pp. 149–69.

. . .   1984. *A Critique of the Study of Kinship*, Ann Arbor: University of Michigan Press.

. . .   1989. 'Australian Aboriginal Kinship: Cultural Construction, Deconstruction and Misconstruction (Correspondence)', *Man*, n.s., Vol. XXIV, no. 1, pp. 165–6.

. . .   1992. 'Comment [on Shore 1992]', *Current Anthropology*, Vol. XXXIII, no. 3, pp. 307–10.

. . .   1995. 'A Symbolic Theory of Incest (Comment)', *Journal of the Royal Anthropological Institute*, n.s., Vol. I, no. 1, p. 167.

SEEGER, ANTHONY 1981. *Nature and Society in Central Brazil: The Suya Indians of Mato Grosso* (Harvard Studies in Cultural Anthropology 4), Cambridge, MA: Harvard University Press.

SHAPIRO, WARREN 1968. 'The Exchange of Sister's Daughter's Daughters in North-east Arnhem Land', *Southwestern Journal of Anthropology*, Vol. XXIV, no. 4, pp. 346–53.

. . .   1971a. 'Patri-Groups, Patri-Categories, and Sections in Australian Aboriginal Social Classification', *Man*, n.s., Vol. VI, no. 4, pp. 590–600.

. . .   1971b. 'Structuralism versus Sociology: A Review of Maybury-Lewis's *Akwe-Shavante Society*', *Mankind*, Vol. VIII, no. 1, pp. 64–6.

. . .   1975. Review of Korn 1973, *Mankind*, Vol. X, no. 2, pp. 123–4.

. . .   1977. 'Structure, Variation, and Change in "Balamumu" Social Classification', *Journal of Anthropological Research*, Vol. XXXIII, no. 1, pp. 16–49.

. . .   1981. *Miwuyt Marriage: The Cultural Anthropology of Affinity in Northeast Arnhem Land*, Philadelphia: Institute for the Study of Human Issues.

. . .   1982. 'The Place of Cognitive Extensionism in the History of Anthropological Thought (Review Article [re Scheffler 1978])', *Journal of the Polynesian Society*, Vol. XCI, no. 2, pp. 257–97.

. . .   1985. Review of *Makuna Social Organization: A Study in Descent, Alliance and the Formation of Corporate Groups in the North-Western Amazon* (Uppsala Studies in Cultural Anthropology 4) by Kaj Arhem (Stockholm, 1981), *American Anthropologist*, Vol. LXXXVII, no. 3, pp. 713–14.

. . .   1988. 'Ritual Kinship, Ritual Incorporation and the Denial of Death', *Man*, n.s., Vol. XXIII, no. 2, pp. 275–97.

. . .   1989. 'Australian Aboriginal Kinship: Cultural Construction, Deconstruction and Misconstruction (Correspondence)', *Man*, n.s., Vol. XXIV, no. 2, pp. 166–8.

SHAPIRO, WARREN 1991. 'Claude Lévi-Strauss Meets Alexander Goldenweiser: Boasian Anthropology and the Study of Totemism', *American Anthropologist*, Vol. XCIII, no. 3, pp. 599–610.

. . . 1992. 'Speculations upon the Origins of *Elementary Structures* (Commentary)', *Cultural Anthropology*, Vol. VII, no. 2, pp. 255–8.

. . . in press. 'Dr Hiatt and Mr Brown: Gidjingali Sociality and Culture Theory', in Francesca Merlan *et al.* (eds.), *Australian Aborigines and Contemporary Social Enquiry: Essays in Honour of Les Hiatt*, Canberra: Aboriginal Studies Press.

SHORE, CRIS 1992. 'Virgin Birth and Sterile Debates: Anthropology and the New Reproductive Technologies', *Current Anthropology*, Vol. XXXIII, no. 3, pp. 295–301.

SPIRO, MELFORD E. 1986. 'Cultural Relativism and the Future of Anthropology', *Cultural Anthropology*, Vol. I, no. 2, pp. 259–86.

STRATHERN, MARILYN 1992. *Reproducing the Future: Essays on Anthropology, Kinship, and the New Reproductive Technologies*, New York: Routledge.

TURNER, DAVID H. 1974. *Tradition and Transformation: A Study of Groote Eylandt Area Aborigines of Northern Australia* (Australian Aboriginal Studies No. 53 / Social Anthropology Series No. 8), Canberra: Australian Institute of Aboriginal Studies.

WOLF, ERIC R. 1982. *Europe and the People without History*, Berkeley: University of California Press.

YANAGISAKO, SYLVIA JUNKO, and JANE FISHBURNE COLLIER 1987. 'Toward a Unified Analysis of Gender and Kinship', in Jane Fishburne Collier and Sylvia Junko Yanagisako (eds.), *Gender and Kinship: Essays toward a Unified Analysis*, Stanford: Stanford University Press, pp. 14–50.

YANAGISAKO, SYLVIA J., and CAROL DELANEY (eds.) 1995. *Naturalizing Power: Essays in Feminist Cultural Analysis*, New York: Routledge.