Review Essay

Scholasticism versus Discovery in Kinship Studies: Review of Maurice Godelier's *The Metamorphoses of Kinship*

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Abstract: Maurice Godelier's supposedly comprehensive treatment of kinship studies is in fact decidedly truncated: it derives less from an earnest concern with kinship as a human phenomenon and more from established conventions of textbook writing. In particular, it overemphasizes typologies derived from alliance theory and descent theory and, at the same time, pays almost no attention to real discoveries, particularly concerning kinship semantics and the genesis of incest taboos.

Keywords: kinship, extensionism, typology vs. discovery, alliance theory, descent theory

Résumé : Le traitement prétendument compréhensif des études de parenté que propose Maurice Godelier est, en vérité, tronqué : en effet, celui-ci découle moins d'une véritable préoccupation pour la parenté comme phénomène humain, que des conventions établies dans l'écriture des manuels. De manière précise, la méthode de Godelier met trop l'accent sur les typologies dérivées des théories de l'alliance et de la descendance alors qu'en même temps, elle n'accorde presque aucune attention aux véritables découvertes, en particulier celles qui touchent aux champs sémantiques de la parenté et à la genèse du tabou de l'inceste.

Mots-clés : parenté, compréhensif, typologie ou découverte, théorie de l'alliance, théorie de la descendance

Introduction

Before reading this book, I read a laudatory review of it by Sir Jack Goody (2005). So, I thought, when an eminent anthropologist praises a work by another eminent anthropologist, I would be in for a major intellectual treat. I was, to say the least, disappointed. But before I give my reasons for this contrarian response, I need to say the obvious—that is, that any book must be evaluated on the basis of how well, in the reviewer's opinion, it meets its stated goals. This is not easy here, for Maurice Godelier nowhere expressly tells us what these marks are. Nor does Sir Jack, though he *does* imply that it is a major synthesis of what we know about human kinship. So, it is on this basis that I shall attempt to assess it. I want to add, to make my critical remarks more comprehensible, that, by my lights, the primary job of an anthropologist is to find out things about parts of this world and communicate his/her findings to colleagues, students and the general public. It is *not* many other things—chief among these, for present purposes, parading typologies, usually based on outworn ethnographic understandings, for their own sake, as if the cultural world consists of instantiations of types, the Words, so to say, Made Flesh.

Filiation and Descent

Godelier breaks down kinship into several "components," the first of which is "filiation and descent." The latter is mostly organized into a unilineal/cognatic dichotomy,[1] a vestige of kinship studies *circa* 1960, when Murdock (1960) tried to untangle the (even by then) hopelessly mixed bag of "cognatic societies." Murdock's attempt, though understandable in context, has achieved the oblivion it deserves, and I know of no one since who has attempted a comparable synthesis. Let me start by working with what I consider a more

productive dichotomy, if only slightly, namely, Goodenough's (1961) distinction between "ego-oriented" and "ancestor-oriented" kinship constructs. By the former, he means what anthropologists usually call "personal kindreds" and, by the latter, what are conventionally labelled "descent groups." I shall have more to say later about ego-oriented constructs. First, I want to focus on those kinship constructs with an "ancestor orientation."

The first thing to note is that the "ancestors" are a motley crew. They are most certainly *not* recalled human beings. Thus, Fortes (1987:76–77), in his summary of African ancestor cults, reminds us that supposedly ancestral beings throughout this area are divested of any personal or idiosyncratic qualities their flesh-and-blood counterparts possessed. Instead, they are depersonalized into categories. One need only read the Bible, written by heirs of such cults, to see that this is so. Abraham does mostly heroic things; he procreates but does not (e.g.) defecate. Jesus does neither. By contrast, in Aboriginal Australian creation myths, the heroes of the Dreaming do both, but it is only the *defecation* that counts, because it, along with other bodily emissions, becomes *externalized* in the form of features of the landscape (Munn 1970). Procreation, by contrast, is purely recreational and often involves beings in relationships, in what Aboriginal Australians call "this time now," wherein anything even hinting at sex is forbidden, for example, mother-in-law/son-in-law (Róheim 1971[1925]: 40). The suggestion, then, is that the beings dubbed "ancestors" in the ethnographic literature are more accurately rendered as "anti-ancestors": they are conceptually opposed to flesh-and-blood people or, more accurately, to the flesh-and-blood part of such people. This being so, they are uniquely qualified to engender what, following Gudeman (1972), I should call "the spiritual person." Often they do this not by anything like descent but rather by instantiation. Thus, Jesus left no fleshly heirs who could procreate and become (really) ancestral to any of his followers; rather, in the Mass, he merges with them. Similarly, Aboriginal Australians, in the course of ritual, imitate the Dreaming beings and become them. This is not "descent" but "anti-descent," a denial of the mortal implications of carnal creation. Even in the classic African "patrilineal" systems, the role of sacrifice and its antithetical relation to carnality have been underappreciated; thus, Jay (1992) has argued that in many such systems, group membership is conceptualized largely in terms of co-participation in ritual killing, rather than agnation per se.

But surely (it might be argued) "matrilineal" systems are based solely on real mother–child bonds. Not necessarily. Consider the classic example of the Trobriands. Malinowski's earliest statements on the alleged "igno-rance of physiological paternity" in the Trobriand Islands (Malinowski 1916, 1929) make it seem as if the mother creates the fetus, with the assistance of a clan ancestor; the father, for his part, supposedly has only the subsidiary role of giving form to the fetal mass once it has been generated. But these statements were challenged quite early on. Read (1918) made it plain that spirit entry occurs not at conception but at fetal quickening, and, some years later, Rentoul noted a decided concern among Trobriand women *"to expel the male seed"* after intercourse (1931:153, original emphasis). How can we account for Malinowski's error? We now know that Trobrianders, especially Trobriand men, dislike talking about sex for several reasons; among them, according to Jerry Leach, is that such talk offends "the ears of the spirits" (Glass 1986:47). What we have here, then, is another example of a body–spirit opposition, even to the point where spirit entry is held to occur non-vaginally, though the woman's forehead.[2] Godelier is apparently unaware of these modifications of the initial reports and thus renders Trobriand "conception" ideology in the mistaken terms in which Malinowski (1929:248–250) reported it, missing entirely the subtleties of that ideology.

The other classic case of "ignorance of physical paternity" in Aboriginal Australia[3] is strikingly similar. Scheffler (1973:750; 1978:5–13) has shown that, at least for some parts of the continent, spirit entry is held to occur, as in the Trobriands, at fetal quickening and not at conception, as had been thought. Moreover, at least for the Western Desert, it is known that such entry is supposed to occur non-vaginally, through the woman's stomach, foot or mouth (Shapiro 1996). Finally, there is evidence from several areas that *men*—especially older and ritually prominent men—are disinclined to discuss sexual generation and instead prefer spiritual discourse (Scheffler 1973:750–751). What we have here, too, is a body–spirit distinction, and again Godelier fails to appreciate it (p. 251).

He does note that conception ideologies frequently posit what Sahlins (2013:4) has more recently called a "third party," in addition to the mother and the father, in the generation of the person, and this is an important contribution. Here are his words:

> *Nowhere … do a man and woman alone suffice to make a child.* What they make together, in proportions that vary from one society to the next and with a diversity of substances (sperm, menstrual blood, fat, breath etc.), is a foetus but never a complete, viable human child. For this, other agents are needed, who are more powerful than humans, … normally invisible and who add what is lacking for the foetus to become a child. What is lacking is what we customarily call a soul. [p. 299]

But both Sahlins and Godelier tend to see these agents as additive to the parental contribution, whereas they are, in fact, antithetical to it. Further examples of such antithetical factors include naming in many populations (e.g., J. Crocker 1985:63–67; S. Hugh-Jones 2006; Nuttall 1994); male initiation, also widespread (e.g., Jackson 1996; Keesing 1982; Meggitt 1962:281–316); and, of course, godparenthood in many Christian churches.

Something like this formulation may also hold for the "totemic clan" systems of Native North and South America, whose matrilineal or patrilineal character has been canonically emphasized. But Tooker (1971:362), referring to the North American materials, concludes that "clans are ... a type of ritual relationship to a supernatural being" and that "there is often evidence that the members of the clan consider themselves not linked by reason of the relationship through their mother or father, as the case may be, but by reason of their relationship to a clan bundle or fetish and its associated rites" (360). Similarly, Murphy (1979:224) describes Amazonian clans as positing "a kind of descent that incorporates within itself qualities of timelessness." The best-documented cases are the patrilineal clan populations of the Northwest Amazon, wherein men enact rites, which women may not witness, in which they instantiate "ancestral" beings and generate, that is, *give rebirth to*, boys as men (Goldman 2004:194–215; S. Hugh-Jones 2006; Jackson 1996).

This of course recalls the Aboriginal Australian data, supposedly based on systems of patrilineal clans; indeed, Radcliffe-Brown's (1952:32–48) classic exposition of localized unilineal corporations takes the Kariera "horde" as its model case. I shall have more to say shortly about the alleged locality factor. For now, a review of Aboriginal Australian notions of "descent" is in order. Many years ago Elkin (1932a:130; 1932b:331) noted that Aboriginal Australian clan affiliation is primarily based on the locale in which a child's father "finds" his/her spirit in a dream and that, because of settlement on European-dominated locations and the consequent decline in the importance of local distinctions, the effective principle has become simple patrifiliation. This is an important point. But Elkin, following Radcliffe-Brown, erroneously ascribed an on-the-ground local significance to the clans. I return to this below. The thing to note now is that these two principles hardly exhaust the possibilities of clan affiliation actually encountered in Aboriginal Australia. The most remarkable examples are from the Western Desert. I quote nearly in full from Myers' account of the numerous principles he found among the Pintupi.

There are numerous reasons for referring to a place as one's 'own country.' If the place is called A, the following possibilities may constitute bases for such a claim:

1. conception at the place A;
2. conception at a place B made by and/or identified with the same Dreaming as A;
3. conception at a place B whose Dreaming is associated mythologically with the Dreaming at A;
4. initiation at A (for a male);
5. birth at A;
6. father conceived at A or conditions 2–5 true for father;
7. mother conceived at A or conditions 2, 3, or 5 true for mother;
8. grandparents ... (including all kin types so classified) conceived at A or conditions 2–5 true;
9. residence around A;
10. death of close relative at or near A.

[Myers 1986:129–130; see also Tonkinson 1978:50–54]

I doubt I would have much difficulty finding more principles of recruitment if I were to consider further ethnographic materials; indeed, Rivière (1993) has done just this for Amazonia. But my point is not to compile an exhaustive enumeration, much less to name the "types," as if naming constituted theory, a recurrent error in anthropology and one remarkably on display in the work under review. My point, instead, is that labels like patrilineal, matrilineal and bilateral, all of which have by now passed to the general public, are not particularly revealing.

Then there is the consideration that, whatever "descent" or "anti-descent" principles exist in people's heads, they may have minimal relevance for on-the-ground action. There is surely no better example than the allegedly classic "patrilineal" case provided by Evans-Pritchard's accounts of Nuer sociality (e.g., Evans-Pritchard 1940, 1951). Several scholars (e.g., Buchler 1963; Gough 1971; Southall 1986), working with Evans-Pritchard's materials, have shown that the predominant links in Nuer local groups are those among matrilateral and affinal kin, not agnates, whose relationship is often strained over conflicting claims to cattle. To be sure, such groups are dominated by a man or men of the patriclan said to be associated with the land on which the group subsists, and there are other agnatic symbols, like the spear names called out in ritual (Evans-Pritchard 1956:240–247). But there is no indication of any "tracing of descent," so that one commentator (Verdon 1982) has argued, correctly to my mind, that the Nuer have no lineages at all.

Godelier shows not the slightest awareness of any of these developments in conceptualizing descent and its importance in human affairs. Instead, as noted, he relies almost entirely on an antediluvian unilineal/cognatic contrast, the stuff of which introductory textbooks are made. This inability to extricate himself from established categories, as I hope to show, is a recurrent and fatal flaw in his book. For now, I would only note that all the textbook "wisdom" about the "tracing of descent" in unilineal systems is largely spurious, applicable mostly to those segmentary lineage systems that are ordered by what Lewis (1965:89) calls "national genealogies." I find no such "tracing" in many of the "classic" cases, including the Nuer and the Trobrianders. What we have in both instances is selective attention to one (rather than the other) of the parent–child links, not "lineages." This last expression has become applied indiscriminately to local groups based on unilateral filiation and marriage, to action groups similarly based and to lower-order segments in true segmentary lineage systems and, as such, is probably the most overused term in all of kinship studies (Murphy 1979:221–224).

Alliance and Residence

Godelier's second and third "components" of human sociality are alliance and residence. The former is based on Lévi-Strauss's (1969) typology, to which Godelier claims he supplies a refutation. In view of the enormous currency attached to this typology—this too passed to the general public—such an undertaking may seem nothing less than heroic. On the other hand, one might consider the plainer facts that Lévi-Strauss's initial formulation appeared in 1945 (reprinted as Lévi-Strauss 1963:31–54) and that his major tome on the subject followed four years later. But even if we discount this and consider only the English edition of the latter (Lévi-Strauss 1969; hereafter ESK), we are dealing with a volume that at the time of Godelier's writing—the original French edition appeared in 2004—was already three decades old. One of anthropology's problems is a certain scholasticism, an attachment to ideas which have long been shown to be false or, much worse, unfalsifiable and, as already noted, a marked tendency to see the world as an instantiation of typologies rather than something to learn about. The Marxists provide the best examples. Even in recent years, we have had instances of "finding," in the Third and Fourth Worlds, "cases" wherein, *pace* Engels' (1972[1884]) ancient musings, the nuclear family is lacking. But Lévi-Strauss on kinship and his followers, including Godelier, constitute a pretty close second. Here are several reasons why Lévi-Strauss's "alliance theory" is utter nonsense.

There is a very marked tendency in it to assume what one is, supposedly, trying to prove, which of course speaks to its scholastic character. More specifically, almost every marriage anywhere is treated as an "alliance." When, then, is a marriage not an alliance? It would appear that an alliance is said to exist whenever a relationship is established by a marriage, or antecedent to it, with anyone outside the conjugal pair, usually between one or the other of the pair and his/her partner's kin, less commonly between these kin themselves (Scheffler 1973:784–786). But calling such an arrangement an alliance adds absolutely nothing to the description and is most certainly not an explanation in any predictive sense (Hempel 1965:308–319). It is a rhetorical device identifying the author with "alliance theory," "structural-functionalism" or some other scholastic tradition—it has no empirical content whatsoever (Hempel 1965:319–325). Similar remarks apply to "exchange." We are left with the truism that marriage usually involves relationships beyond that between the married couple, hardly news to anyone who has ever been married.

Further, the nature of the "allied" or "exchanging" units is inconsistently specified. ESK introduces these ideas in connection with exogamous moiety systems (Lévi-Strauss 1969:69–83), but this is entirely inconsistent with other anthropological as well as popular notions of marital alliance, wherein the units are not conceptual categories (like those of an exogamous moiety system) but spatially separated groups of people—canonically, the historically documented and self-conscious marital alliances between European noble houses. In his initial formulation, Lévi-Strauss writes of affinal "relations" between "elementary families," stating that "it is not the families (isolated terms) which are elementary but the relations between those terms" (1963:51). This is the foundational statement of Lévi-Strauss' "structuralism." But I would add that it is also a violation of notions of logical priority and that, taken historically, it is no more than a hen-and-egg question. In practice, Lévi-Strauss and his followers tend to stress exogamous groupings of the "unilineal" sort. This is a highly questionable analytic category, as I have shown. He and those who follow him compound this sloppiness by calling any marriage outside the grouping an "alliance," without the slightest concern with providing any evidence external to this tautological scheme that a real alliance has occurred.

Actual local groups may intermarry, without the marriage per se having any implications for either of these groups as such. A case in point is provided by the Mae Enga of Highland New Guinea. Meggitt (1965:101)

proffers a native adage, which he translates as "we marry the people we fight," that makes it seem as if local patrilineal group membership is the sole criterion of combative behaviour, but it is clear from his comprehensive analysis that close egocentric kinship is a more salient factor (1965:215; see Shapiro 2013:176 for a more detailed reanalysis of this example). More generally, "alliance theory" renders human sociality primarily or even solely as a matter of intergroup relationship, without any attention to the complexity of the actual principles employed in human affairs (Keesing 1971). "Descent theory" does this too, as Schneider (1965:74–75) and Wagner (1974) have pointed out, and sometimes, as with the Mae Enga, native platitudes help it along. But such platitudes, though of interest, are no substitute for sound ethnographic analysis.

In keeping with its scholastic character, "alliance theory" renders in its own terms almost every marriage rule or preference known to ethnography. Thus, a man who marries the sister of his brother's wife is said to be "strengthening" "the alliance," no independent evidence for strengthening (or alliance in the first place) being thought necessary. Or when a man may not marry the sister of his brother's wife, the "alliances" of his "group" are said to be "scattered," with no independent evidence for either scattering or alliance. Leviritic marriage is held to be "strengthening" in this way, without any attention to the numerous cases in which its basis is egocentric kin claims and not group membership (Scheffler 1973:764–765). Polygyny likewise "strengthens" or (if non-sororal) "scatters" alliances, presumably having nothing to do with men's desire for sexual diversity and numerous heirs. Most prestigious of all, cross-cousin marriage, more particularly in its bilateral form, is held to be the "elementary structure" of all kinship, "creating" or "strengthening" an "alliance."

This last point leads to some highly pertinent ethnographic data, for which "alliance theory," to the extent to which it can be considered an empirical theory, is hopelessly inadequate. As long ago as 1955, Fred Eggan, drawing on the research of others who worked directly with Northern Algonquian hunters/trappers in the Eastern Canadian Subarctic, noted that "what we have here is a relatively new type of social structure for [Aboriginal] North America: *a bilateral band held together by cross-cousin marriage*" (1955:521). The English translation of ESK was a decade and a half away, and there is no indication that Eggan read ESK in the original French. Hence, it is significant that he makes no mention of "alliance." In fact, it was already clear from a considerable ethnographic literature that cross-cousin marriage in these populations has absolutely nothing to

do with intergroup relations but instead is a rule or preference *sui generis* (e.g., Hallowell 1937; Landes 1937; Speck 1918). In kin classification, the parallel/cross system said by Lévi-Strauss to be the simplest "structure" of "alliance" (1969:98–99) is there, but there is not a shred of data indicating that it is conceptualized by the natives as such or that intergroup relations are involved; on the contrary, Eggan says expressly that "there is no evidence that particular extended families systematically intermarried" (1955:524).

The same pattern occurs widely in the other half of Aboriginal America. Lévi-Strauss' own findings among the Nambikwara of Central Brazil, perceptively rendered as "the decisive moment" in the development of alliance theory (Coelho de Souza 2009:85), are well known; he witnessed a group of Nambikwara intermarrying with congeners who had formerly been strangers by extending the "cross" kinship categories to them (Lévi-Strauss 1943). But this tactic, though it may be employed elsewhere in Amazonia, is not especially frequent; more common are populations that, like the Northern Algonquians, are locally endogamous (e.g., Kensinger 1995:109; Kaplan 1975:73; Viveiros de Castro 1992:162). To be sure, in the northwest of Amazonia we find exogamous patriclan communities with much the same cross/parallel distinction in kin classification (e.g., Arhem 1981; Chernela 1993; C. Hugh-Jones 1979); here, possibly, an "alliance" interpretation may be in order. But Oberg (1955) has suggested that such local clanship in the region is a historically late phenomenon stemming from population expansion. Goldschmidt (1948) theorized as much for parts of Aboriginal North America, where the simple cross/parallel distinction is often supplemented by Crow/Omaha terminologies and the prohibition of all cousin marriage, and Callender (1962) provides an excellent analysis of the matter for farming congeners of the Northern Algonquians. So it may well be that *endogamous* communities with cross-cousin marriage are a very old characteristic of Aboriginal American sociality; that is, "elementary structures" were replaced by "complex structures," just as theorized by Lévi-Strauss (1969:459–477).

If so, this is no vindication of his scheme—and *not* just because the "elementary structures" were realized in endogamous communities. *It is a historical statement of a documentable process*. The gravamen of ESK is an exercise in what has been called "origin-and-essence" mythology (Shapiro 1990), comparable not to anything that actually happened, however long ago, but to the "origin" myths found in our ethnographies, in which origin imagery is employed to express construed "essences." So, for example, Essential Man in the Judaeo-

Christian tradition is Without Sin; in Marxism he is a Primitive Communist; in ESK he is an Advocate and Practitioner of Cross-Cousin Marriage. But this is metaphysics, not history, even if most of the anthropology we dub "evolutionary" partakes of it. There is no logical reason why Crow/Omaha systems could not precede cross/parallel ones.

These remarks apply as well to Aboriginal Australia, allegedly the *locus classicus* of "elementary structures." One can create a Guttman Scale suggesting that Kariera … Murngin … Gidgingali …Walbiri … Kimberleys … indicates a regular sequence in the historical development of sociality on the continent, which I did quite some time ago (Shapiro 1971). Be this as it may, and despite the fact that, in the wake of ESK, one could fill several fat volumes with published articles attempting to show that Aboriginal Australian "local groups" are related by various forms of "prescriptive alliance"—the term is of course Needham's (e.g., 1962)—alliance theory has virtually no place in our appreciation of Aboriginal sociality on this continent. There are two reasons for this: (1) the "local groups" are not really local groups but clans, often, as noted above, having something to do with patrifiliation; and (2), and this is worth stressing, *there is not a shred of evidence that these clans act corporately in the politics of marriage.*

I need to expand on these remarks. The idea that actual local groups among Aboriginal Australian people consist of male agnates and their in-married wives was put forward by Radcliffe-Brown (1931:4). For many years this statement, based solely on expressions of commitment to "their" countries by Aboriginal Australian men, was taken as authoritative. Then, in the 1960s, first Meggitt (1962:70–71) and then Hiatt (1965:18–19) reported multi-clan communities for the Walbiri and the Gidjingali, respectively. They did not, however, discover the principles by which actual groups organized locally. This was done by Peterson (1970) and myself (Shapiro 1973), working in different parts of northeastern Arnhem Land. Peterson presented quantitative data showing that the local group he studied was based not on the father–son tie, as Radcliffe-Brown's model presupposed, but on the wife's parents–daughter's husband tie; and he theorized, using the "developmental cycle" approach to local grouping pioneered by Fortes (1958), that when a man is relatively young he lives with his parents-in-law on his wife's father's estate and then, when older, with his parents-in-law gone, he moves with his wife to his own estate and the cycle is repeated. But he presented no evidence for this latter move. In my own study, based on four local groups living away from Europeans, as well as on native settlement at a mission station, I was able to confirm the first part of Peterson's model, that is, that the key tie between males in a locality is father-in-law–son-in-law, but I found no evidence for the cycle Peterson proposed. On the contrary, the locale of residence was thought irrelevant; instead, the key motives were said to be the right of a man's affines to bride-service and his obligation to provide it. Moreover, I attempted to discern whether this pattern antedated European contact by asking older men, who were born before missions were established in the area, where their fathers "found" their spirits (see above). Approximately half were "found" on estates to which neither they nor their fathers had claim. Since Peterson's and my analyses have appeared, a very considerable amount of research in the Western Desert, some of which I have already noted, indicates multiple bases for estate claims and thus, presumably, nothing even close to Radcliffe-Brown's model.

So much for residence. It is still possible, *pace* Lévi-Strauss, that somehow scattered agnates come together to negotiate the marriages of their daughters. This possibility was demolished by Hiatt's seminal monograph on north-central Arnhem Land, wherein he puts it starkly: "Patrilineal groups were not units in wife-exchange systems of the kind implied by Lévi-Strauss's theory on kinship and marriage" (1965:xiv).[4] Rather, a Gidjingali girl was bestowed primarily by egocentric kin outside her patriclan, especially by her mother and mother's brothers. The latter, moreover, were entitled to reciprocity, specifically, to the sister's daughter of their own sister's daughter's husband. In other words, there is an exchange of females here but no notion of interclan "alliance": in Hiatt's words, "the patrilineal group affiliations of the potential brides were irrelevant" (1965:38). Under certain conditions, a girl's father could bestow her, but when he did, he acted on the basis of his egocentric kin relationship to his daughter and not as a member of her patriclan (Hiatt 1967:474; see also Hamilton 1970). Hiatt continued his assault face-to-face with Lévi-Strauss at the first Man the Hunter Conference (Hiatt 1968). One might assume that the latter would have been impressed by the "niece-exchange" Hiatt reported, but he was not. Here is part of his retort: "If Hiatt's recent observation of what is left of a collapsing Australian tribe should carry more weight than the bulk of the older literature, then let us ban the books" (Lévi-Strauss 1968:210–211). In fact, the "older literature" (reviewed in Hiatt 1967), written when, presumably, there was less "collapse," contains numerous examples of "niece-exchange." Two things should be noted about Lévi-Strauss' pitiful response to Hiatt: (1) his primitivism, that is, his attempt to find a Timeless

Reality underneath the "corrupt" data; and (2) his marked tendency to play heads-I-win-tails-you-lose with his adversaries, to proffer what philosophers call "closed systems of thought," immune to negative evidence. Both, by my lights, disqualify him from being taken seriously as an empirical scholar, a conclusion I am hardly the first to reach.

My own research, farther east in Arnhem Land, supports Hiatt's conclusions in nearly every particular, down to "niece-exchange," though here not as wives but as mothers-in-law. Such exchanges, though recognized as a possibility, occur only rarely, for reasons spelled out in Shapiro (1981:99). Although a man here may marry his MBD but not his FZD, this has nothing to do with the "generalized exchange" systems of Southeast Asia; I made this point in a pair of articles published four decades ago (e.g., Shapiro 1968, 1971) and in *Miwuyt Marriage* (Shapiro 1981). But in the work under review, Godelier (p. 510), following Lévi-Strauss (1969:168–196) again, renders Murngin marriage as "asymmetric alliance."

So Aboriginal Australian marriage is not about "alliances" between "local groups." *It is an entirely egocentric affair*: the parties involved are involved because of their egocentric relationship to the girl and the egocentric relationship between the would-be husband and his would-be wife and her egocentrically close kin. The key difference among particular systems, as I have suggested elsewhere (Shapiro 1979:57–58), is between kindred endogamy, such as we find along the Arnhem Land littoral, and kindred exogamy, which is much more common. And I rather suspect, with Scheffler (1973:764–765), that this is the case very widely elsewhere.[5]

Which brings me back to Godelier. His "refutation" of Lévi-Strauss, so far as I can discern, consists of two points which seem eminently supportable. Thus, he cites examples in which it is men, not women, who are exchanged, concluding that "the formula: kinship is based on the exchange of women between and by men does not have the universal validity Lévi-Strauss … attributed to it" (p. 126). This is fine—although the small number of cases he adduced could have been supplemented by the findings, noted above, of Hiatt and others on Aboriginal Australian women bestowing other women, as well as a much larger corpus of findings in which women, in conjunction with their husbands, give their daughters in marriage. But Godelier remains "allied," if the pun be pardoned, to exchange as crucial to kinship, which, I shall show shortly, is utterly mistaken. He also notes that some forms of marriage "may not imply any form of exchange" (p. 128). This is consistent with part of my argument above, but Godelier regards it as

an aside; his sole example is "marriage with a captive" (p. 128). Otherwise, the "alliance" part of this chapter is Lévi-Strauss *redux*.

So, again, Godelier shows himself "captive" to established formulas, without the slightest regard for their empirical soundness. This is repeated in the much smaller "residence" part of the chapter, which presents the typology developed by Murdock (1949)—matrilocal, patrivirilocal and so on—for comparative purposes. But attention needs also to be paid to Goodenough's warning that

> it is a procedural fallacy to use these concepts as a basis for classifying the residence choices of individual members of a society. They do not choose on the basis of criteria which are outside their culture, *which exist only in the heads of anthropologists*. They choose on the basis of criteria … which may be quite different … from those used by the anthropologist in classifying their culture. [Goodenough 1956:29, emphasis added]

The significance of this quote for subsequent ethnographic theory is impossible to overstate: it, along with Goodenough's (1951) monograph on the Micronesian island of Truk, is the prototypical statement of a quest for replicable ethnography, for what we now call "the natives' point of view," pursued primarily by American scholars in subsequent decades and now morphed into a cognitive anthropology (see D'Andrade 1995 for a relatively recent statement). But Godelier seems to be utterly unaware of these developments.

Kinship Terminologies

I place no quotation marks around this designation for Godelier's fifth "component," because here, at least, he calls them what they are. They are egocentric classifications *based on locally posited genealogical connection*— on ethno-embryologies, as I would have them called. They are *not*—not basically, anyway—terminologies expressive of alliance relationships and, *pace* Schneider (1984) and Sahlins (2013), they are *not* terminologies whose non-procreative referents have the same semantic status as their procreative ones. The corpus of data supporting these conclusions is nothing less than monumental; indeed, *this is cultural anthropology's most secure finding*, and only the thickest of intellectual blinders can make it seem less than crystal clear. Here is the classic statement of the matter, now more than three-quarters of a century old:

> It is important to remember, as bearing upon the status of the family, that in many primitive tribes the terms used for the immediate members of the family

are either distinguished from the same terms in their extended uses by the addition of some particle, or terms corresponding to 'own' are used ... Family is family, whatever the system of relationship. [Goldenweiser 1937:301]

The Baruya of Highland New Guinea, studied firsthand by Godelier, are no exception. Although he provides no comprehensive analysis of native kin classification, there are pertinent tidbits throughout his corpus. Thus, he notes a special term for "a male friend" and one for "a female friend," telling us also that these terms can replace the usual "brother" and "sister" terms, respectively (Godelier 1999:144). And he adds that a "male friend is (like) a brother, a female friend (like) a sister." I presume from this that such likening is expressly made by the Baruya. If so, one's brother and sister are what semanticists call the foci of the classes signified by the labels "male friend" and "female friend" respectively, and these two kin types provide models for the extension of "sibling" significance, much as in English I might say of a close male friend, "He's like a brother to me!" Elsewhere Godelier (2011:329) tells us that "the mother's sisters are regarded as other mothers," though it is not entirely clear how this "otherness" is expressed in the Baruya language. My guess is that there is indeed a separate term translatable as "other" employed here; certainly this is very widely the case elsewhere (e.g., Bromley 1980; W. Crocker 2002; Handy and Pukui 1972[1958]:65). If I am correct, then the mother is the focus of her kin class for the Baruya. In any case, we know quite definitely that the mother's brother is singled out from others of his kin class by a lexeme which Godelier (1999) translates as "of the breast," suggestive of his being a "male mother," a notion that, as we know from Radcliffe-Brown (1952:15–31) and others, has wide ethnographic occurrence. Note that it has never been reported that the mother herself is a "female mother," which suggests that everywhere the focal referents of the "mother" class are taken to be female. Godelier (1999) seems also to indicate that there is a Baruya lexeme translatable as "classificatory," though this, too, is not entirely clear. In any case, comparable lexemes have been widely reported elsewhere (e.g., Bromley 1980; Feinberg 2004:81; Shapiro 1981:38–41). Godelier is more explicit when it comes to male initiation, where he points out that "every young initiate has two sponsors of different ages, chosen ... from his mother's lineage; one of these is said to be 'like his mother,' and the other, 'like his sister'" (2011:333; see also Godelier 1986:39, 154). He absorbs semen, likened to mother's milk, from both men (2011:333). This, then,

is a ritual complex of the sort that Hiatt (1971) calls "pseudo-procreative," which of course occurs widely—not only in New Guinea but in Amazonia, Aboriginal Australia and elsewhere as well (e.g., Gregor and Tuzin 2001; Shapiro and Linke 1996). Godelier, referring to the two sponsors, states it quite succinctly: "The maternal functions are transferred to the sphere of ... male initiation and transposed into a masculine mode" (2011:333). He also tells us that, through such initiation, the boy is "born again" (1986:52), though, once more, it is not quite clear whether the idiom is his or that of the Baruya.

All this may seem arcane and of little significance for anthropological theory. It may be the former, but it is most certainly not the latter. For what it—along with, I repeat, an enormous amount of comparable data in the ethnographic record—demonstrates beyond a shadow of a doubt is that what anthropologists have long called "primary" kin relationships are indeed primary semantically and that kinship is indeed founded on the nuclear family, just as Malinowski (1962[1930]:47), Murdock (1949:1–22), Lounsbury (1965) and many others have argued. Marxists will of course oppose this conclusion, as will "alliance theorists," but they will be able to do so only on scholastic grounds. Schneider's admirers and those of Sahlins will call attention to the numerous "performative" criteria for establishing kinship, utterly oblivious to the demonstrable fact that almost every one of these is modelled on ethno-embryological notions. To repeat: support for this conclusion is there in major as well as minor ethnographic treatises, and it has been there for decades.

Godelier follows Lévi-Strauss in insisting that "the family ... cannot be the ultimate basis of human society" (p. 217). But his reasoning is different: he says that this is so because "it is never simply kinship relations that are stamped on to bodies and consciousness. For, at the same time, bodies and consciousness are imprinted with the political and religious power relations that prevail in the society" (p. 217). Sahlins (2013) shares this Durkheimian position. But then who does not? But I would also maintain, if I had anyone to contest, that bodies and consciousness are also influenced by the air people breathe and the food they eat, neither of which has anything to do with semantic analysis.

So what does Godelier have to say about the extensionist position and the light it sheds on human kinship? Almost nothing. He mentions Lounsbury and Scheffler several times in passing, but his treatment of their ideas (p. 167) is far too truncated. He seems entirely unfamiliar with the sort of semantic analyses just demonstrated, even when it applies, as I have shown, to his own field

materials. And he pays no attention at all to more recent developments in kinship semantics (e.g., Keen 1985; Kronenfeld 1996:147-170; Wierzbicka 1992:329–370).

Can it get even worse? Yes, it can, and it does. But before I show this, I need to attend—briefly—to Godelier's sixth "component," which has to do with ideologies of procreation. As I have already noted, he introduces the important idea of a "third party" in such ideologies, but he fails to appreciate its antithetical relationship to the parental contribution. Hence, as also noted, he misinterprets the Trobriand and Aboriginal Australian materials.

"Incest"

Godelier compares incest prohibitions with other norms about proper and improper sex as locally construed, such as necrophilia, having sex in a sacred place and so on. This seems reasonable enough. But then he denies that sexual aversion can be unconscious:

> Can an anthropologist interpret these conscious representations and these explicit rules of conduct as a surface that ... hides different rules which determine ... individuals' conduct without their being aware? This is probably true in the case of ... language, but it does not apply to ... norms or the explicit rules that govern ... kinship relations ... Does an anthropologist have the right ... to introduce totally alien meanings into a culture and to claim that these meanings really exist in the culture but in forms of which those who experience and obey them are unaware? We do not think so. [p. 347]

This strikes me as incredible, coming as it does from a disciple of Lévi-Strauss, on whom a vast literature exists that is critical of his cavalier positing of "forms of which those who experience and obey them are unaware." Anyway, when it comes to incest, Godelier is quite wrong. Over a century ago Westermarck suggested that "there is an ... aversion to marriage between persons living very closely together from early youth" (1903:544). The suggestion was ignored for decades, during which time the prevailing "explanation" for incest rules was the "final cause" one adduced by Lévi-Strauss (1963[1945]:51), for whom they are part of a "structure" that also contains rules of alliance. Then, in his research on Israeli collective farming settlements (kibbutzim), Spiro (1975[1958]:347–349) noted that in the early days of the kibbutz movement, children who had been raised together in communal nurseries never married—despite there being no external prohibition on marriage other than close kinship. This finding would seem to be confirmed by Shepher (1971), who stressed native state-

ments of repugnance toward such marriage possibilities: "We sat on the same pottie together," one of his informants observed. Around the same time, Arthur Wolf (1966, 1968, 1970; Wolf and Huang 1980) noted something remarkably similar in his research in rural Taiwan. In traditional Chinese marriage, husband and wife meet for the first time at the wedding ceremony, provided the groom's people could afford the brideprice. If they could not, an alternative was available. The girl could be sent to the boy's home as early as one year of age, to be raised by his parents. Then, around age 16 to 18, the couple married. Wolf found (1) that such "second-class" marriages produced significantly fewer children than "first-class" ones; (2) that they were sometimes resisted altogether; (3) that divorce was significantly more frequent than in "first-class" marriages; (4) that men who married second-class, though usually poorer, nonetheless frequented prostitutes more often than their more affluent compatriots; and (5) that women married second-class were more likely to pursue adulterous affairs than more affluent women.

Wolf has continued his inquiries over the years, gradually refining them so that we now know more about such factors as the age range for which such sexual aversion is most effective, whether it is more severe for the younger or the older partner, and so on (Wolf 1995; Wolf and Durham 2004). Reading him is a lesson in scientific method as he tests hypothesis after hypothesis against his data, considers alternative explanations for his findings adduced by other scholars, and brings to bear on his Westermarck-derived theory the research of others (Wolf 1993). All this may be very favourably compared with the ponderous scholastic style of Lévi-Strauss, whose hostility to contrary findings and circularity of theory have already been noted, and, to a somewhat lesser degree, that of Godelier as well. Anyway, we now know from a substantial body of studies (see also Gardner 2009; Lieberman 2009; McCabe 1983) that Westermarck was right. Even some of the apparent exceptions, emphasized by Godelier (pp. 336-389) have turned out to be insufficiently examined (Scheidel 1996, 2004). This body of evidence points to a secured anthropological finding of great generality and popular and theoretical interest. What does Godelier have to say about it? Almost nothing: Westermarck rates a passing citation, but none of the recent research by Wolf or anyone else is even mentioned.

The entailed idea, I would add, that human behaviour is in accord with unconscious structures, which Godelier denies outside language, began in fact to be demonstrated by linguists and cognitive psychologists around the time the original French edition of ESK

appeared (e.g., Chomsky 1957; Lashley 1951; Miller et al. 1960). But this research is, so to say, off the radar of most anthropologists, including the so-called structuralists.

Conclusion

This is key. It is not that Godelier's book makes no good points: it does. I have already mentioned his idea that a "third party" is always posited in the creation of a child. Moreover, the chapter on kin classification, though based almost entirely on Murdock's (1949) typology, shows that the types so recognized have regular historical relationships with one another. There is a great deal of erudition here and a certain expertise. But it is an expertise on what certain anthropologists have said about human kinship, not an expertise on human kinship per se. Its treatment of adoption, for example, is inadequate, and it has nothing at all to say about attachment theory, kin recognition in animals (including humans), what evolutionary anthropologists have called alloparenting, the use of kinship metaphor in expressions of nationalism, the Western communitarian tradition and other topics. It is not clear whether Godelier intends here an introduction to human kinship, but like (other) people who proffer such introductions, he is mostly unable (as one says these days) to think outside the box: for him kinship consists mostly of descent groups, alliance theory and kinship terminologies—all rendered according to typologies produced by established "authorities," none of whom he seriously questions. There is very little left *to discover*.

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Notes

1 Godelier also recognizes the supposedly "parallel descent" of the Apinaye of Central Brazil first noted by Nimuendajú (1939) and the "ropes" of the Mundugumor of New Guinea reported by Mead (1963:176). But even if we accept these reports at face value, these forms of "descent" have only rare occurrence ethnographically. For updates see Da Matta (1982:84–85) and McDowell (1977).

2 See the extra-Biblical notion that the Virgin Mary was impregnated through the ear.

3 Rendering all of Aboriginal Australia as a single ethnographic "case" is, admittedly, incautious—I do it here only to highlight the stark parallels with the Trobriands. The available evidence does not permit my analysis to be generalized to the entire continent.

4 Whether Aboriginal clans, even when membership is based solely on patrifiliation, should be called patrilineal is questionable, for reasons indicated above. But this need not concern us here.

5 Mention should be made here of Yalman's (1962) conclusion that the so-called Dravidian systems of South India and Sri Lanka in fact regulate marriage within endogamous kindreds rather than between "descent groups," which may or may not exist.

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