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THE CULTURE OF KINSHIP STUDIES

Rethinking Kinship and Marriage, edited by Rodney Needham;
A.S.A. Monograph 11, Tavistock Publications, London 1971,
cxix, 276 pp. £ 4.00.

I

The Morgan centenary year (1971) saw a larger-than-usual outpouring of books on kinship and marriage, even though their appearance on this occasion was apparently no more than a coincidence, since in none of them is the centenary date noticed as such.¹ More importantly, the reader of these books, with a single exception, confronts merely a restatement or adaptation of older views or the original statements of those views, reprinted. The one exception is the last-published of all these books, *Rethinking Kinship and Marriage*, presently under review. These facts in themselves may say a great deal about the modern study of kinship fully one hundred years on,² besides making ASA 11 an essential text for all those anthropologists who are (as Needham once remarked of Hocart) "not much concerned to pander to received ideas".³

¹ In addition to *Rethinking Kinship and Marriage* (ASA 11), there had already appeared in France, the U.S.A. and Britain: L. Dumont *Introduction à Deux Théories d'Anthropologie Sociale: Groupes de Filiation et Alliance de Mariage* (Mouton, 1971); H. Scheffler and F. Lounsbury *A Study in Structural Semantics: The Siriono Kinship System* (Prentice-Hall, 1971); N. Graburn (ed.) *Readings in Kinship and Social Structure* (Harper & Row, 1971); J. Goody (ed.) *Kinship* (Penguin Books, 1971); L. Mair *Marriage* (Penguin Books, 1971); J. A. Barnes *Three Styles in the Study of Kinship* (Tavistock, 1971). More recently, there was published a memorial volume: P. Reining (ed.) *Kinship Studies in the Morgan Centennial Year* (Anthropological Society of Washington, 1972).

² Fred Eggan tactfully observes that Morgan, as founder of modern kinship studies, "would be puzzled by the length of time it has taken us to get this far beyond him" ("Lewis Henry Morgan in Kinship Perspective" in G. E. Dole, ed., *Essays in the Science of Culture*, Crowell, 1960, 179-201).

³ See "Editor's Introduction" *Kings and Councillors*, Univ. of Chicago P., 1970, xxiii.

The timeliness of ASA 11 goes much deeper than this. It is reflected in the relative positions of its two senior contributors and, even more strikingly, in the organization of the volume itself. Their joint participation in the work of ASA 11 may serve to remind us that Needham and Leach have in common: — (a) military service during World War II in the Burma Hills, where their anthropological interests prominently lie (lxvii⁴); (b) authorship of two of the outstanding critical works on kinship and marriage (*Structure and Sentiment*, 1962, and *Rethinking Anthropology*, 1961); (c) being the foremost exegetes and propagators of the work of Lévi-Strauss in the English-speaking world (xcii); and (d) reputations as probably the best-known, most prolific and accomplished British anthropologists at present working, yet still without a chair of anthropology at the universities they have served for almost the same period (since 1953 and 1956 respectively) (see 254-5; Leach has lately been awarded a *personal* chair).

The very organization of the volume seems like a hidden commemoration of the centenary decade just past. This may convey to some readers the appropriate calendrical allusions: there are ten chapters by as many social anthropologists and added to them is the editor's introduction, the text of which ends on pages cx [110]. Furthermore, the book is, as we have seen, the eleventh in the ASA monograph series; and finally, publication in Britain took place on 11/11/71. A true collective representation for kinship studies.

II

Needham, in his two capacities as editor and contributor, provides perhaps the severest — and most lengthy — challenge in ASA 11 to a number of current and lingering dogmas in the study of kinship. In his "Introduction" (xiii-cxvii), he follows Berkeley and boldly asserts: "it is time to do away with false principles" and "for someone to start complaining about an unnecessary obscurity and cloudy refraction in the study of kinship and marriage" (xx). For a start, he argues, philosophy can be a helpmate. Anthropologists do not often formulate their hypotheses and generalizations clearly; and they create terministic screens (as Kenneth Burke calls them) with their *Latinisms* and all this implies in biological-evolutionary bias in discourse.

From Needham's sections "Formalism" and "Analysis", anthropologists can gain a valuable lesson in intellectual history. The proponents of

⁴ Citations from *Rethinking Kinship and Marriage* by page number only.

componential analysis have claimed that this particular technique of formal analysis is both new and almost forbiddingly difficult to apply. As for difficulty, Needham points out that such techniques are "not only gratuitously complex but also rather pretentious" (xxx).⁵ As for novelty, specialized notations and techniques for kinship analysis go back at least to 1883, when at the behest of E. B. Tylor, an Edinburgh mathematician named McFarlane propounded a rigorous notational system and specifically applied it to the English laws of marriage and "descent of property" (*Journal of the Anthropological Institute* 12, 46-63). But such systems have never attained a footing in anthropology. The reason may be that these styles of analysis far from exhausting the relevant facts instead exhaust the analysts, so that, as Leach says (75, and quoted by Needham, xxi) they "habitually" sacrifice their regard for the facts. Because they explain so little, these techniques simply (as Nadel might have had it) *cancel out* the very understanding we want to achieve.⁶

What are the real anthropological questions, then, if not the uses and abuses of formal analysis? Two that are ably, and rightly, singled out by Needham are Affect and Prescriptive Alliance. These are questions that are genuinely factual concerns. Affect is a topic according to Needham "of ultimate importance . . . for students of human experience" (liii). The following possibilities for developing its study are suggested: Leach's approach to linguistic attitudes (Chapter 4); Lévi-Strauss's formula for the social balance of sentiments (used by McKnight, Chapter 7, and by Needham, liii for Iatmul); and the problem of incest (Chapter 8).⁷

Prescriptive alliance is, as Needham observes, more than adequately reflected in the contents of *Rethinking Kinship and Marriage*; of the societies reported on in the papers, most — Sinhalese, Kachin, Iatmul,

⁵ In his formal analysis of Gurage terminology, Needham notes, "it would easily have been possible to trick out the analysis with elaborate notation" and all the rest, but without commensurate returns (xxxvii).

⁶ Thus, in the opinion of Lévi-Strauss ("The Future of Kinship Studies" *Proceedings of the Royal Anthropological Institute for 1965*, R.A.I., 1966), Lounsbury's and Buchler's work constitutes nothing more than an elaborate check for internal consistency. The point becomes all the easier to see now that Lounsbury, Goodenough and Scheffler have applied their techniques to the classic cases (Trobriand, Kariera, Kachin), on which really synoptic analyses (by Leach and Dumont) are already available.

⁷ However, neither Leach nor Needham mentions the useful distinction between (i) spontaneous, highly individual sentiments, and (ii) prescriptive, collective sentiments, though this is prominently recognized both by Bateson (*Naven*, 1936, 2nd ed., Stanford Univ. Press, 1958, 29-30) and by Lévi-Strauss ("L'Analyse Structurale en Linguistique et en Anthropologie" *Anthropologie Structurale*, Plon, 1958, 46).

Wikmunkan, and Purum — exhibit classical prescriptive, or elementary, features.

Next, he cites The Purum Case which is, for Needham himself and for kinship studies, a kind of turning-point. It of course concerns prescriptive alliance, but in addition it has repeatedly raised during the past fifteen years the uncomfortable question of Competence and Authority in anthropology.⁸ The latter becomes the subject of the last and longest (27 pp.) substantive section of the Introduction. Needham here concerns himself not with issues but with the kinship theorists themselves.⁹

The case against Radcliffe-Brown is that he presented as largely or entirely his own work methods of kinship notation and distribution studies that were little more than imitations of work published many years before by MacFarlane (1883)¹⁰ and N. W. Thomas (1906). And "it really is about time . . . [Needham states] that someone pointed out that Radcliffe-Brown never published a single scholarly, technically exact, and comprehensive analysis of any 'kinship system' " (lxxxviii). Apparently, Radcliffe-Brown gained his immense reputation in anthropology only by means of "an assured and didactic" literary style (xc) and the fact that his students have assiduously propagated his views.¹¹

⁸ For example, making it necessary to ask: "... if only Schneider, Harris, Fortes, and so on would seriously apply themselves to the comprehension of the ethnographic evidence — work before talk" (lxxx). This suggestion is in perfect accord with Murphy's: "... we must *think* about the data more. There is a common refrain that Simmel, Freud, Lévi-Strauss, and Goffman were or are quite brilliant fellows, but that they give us no method by which we can follow them and replicate their results. The missing intervening link is *thought*" (*Dialectics of Social Life*, Basic Books, 1971, 209, emphasis in original).

⁹ The focus on theorists will also be found in Dumont's *Introduction à Deux Théories d'Anthropologie Sociale*, op. cit., and Barnes' *Three Styles in the Study of Kinship* (Murdock, Lévi-Strauss, Fortes).

¹⁰ Concerning a system of formal notation for kinship analysis published by him in 1930, Radcliffe-Brown stated quite plainly that he "invented" it ("Introduction" to *African Systems of Kinship and Marriage*, edited by A. R. Radcliffe-Brown and C. D. Forde, Oxford Univ. Press., 1950, 7). Needham therefore seems eminently justified in prolonging the agony a page or so as he does do, charging that Radcliffe-Brown must have lifted this system from MacFarlane.

¹¹ Ruth Benedict met Radcliffe-Brown in 1932, and wrote to Margaret Mead who had earlier studied with Radcliffe-Brown in Sydney: "He seemed to me impenetrably wrapped in his own conceit . . . As it stands, I don't think Brown is fighting for good work over against bad, but for work done by disciples over against work by non-disciples. And that's fatal." And further: "If only he held to a high standard of achievement and required language control, intimacy with total culture, fundamental understanding of kinship, I could understand his scorn of work so far done in America. But it's sad . . . I know

To assess Fortes' claim to a place as a kinship expert, Needham proposes this test: imagine the response of another kinship expert — Hocart, Kroeber or Rivers, for example, in the face of Fortes' pronouncements. The ingenuity in such a test leaves little doubt as to results.¹²

With Lévi-Strauss, as the last of his examples, Needham alters rather drastically the criteria of judgment. He does not deny that Lévi-Strauss, as distinct from the other theorists, has done real work in the analysis of kinship systems (xciii-iv) and has stimulated much subsequent work in the right direction (xcv). He is only able to point out that *The Elementary Structures of Kinship* is much overpraised by non-anthropologists and is, worse than that, incomprehensible within the profession (xciii). I do not myself find this a very convincing criticism of Lévi-Strauss's kinship work. A proper assessment, it seems to me, would have to include such mundane considerations as the generally unhappy effect on Anglo-Saxon readers of Lévi-Strauss's synthetic tone of utterance and the fatigue which seems to overtake most of them before the end of the first — and least informative — part of *The Elementary Structures* (Chapters 1-10).

In brief, says Needham, these experts may be imperious, but they simply haven't a clue. It is clear that some other observers of the kinship scene were deeply embarrassed at the publication of Fortes' *Kinship and the Social Order*¹³ but, for all that, they *said* nothing out loud (reviews

my [North American] material and Brown doesn't. It's nothing against him, but it's silly of him to take such a line with me" (*An Anthropologist at Work: Writings of Ruth Benedict*, by Margaret Mead, Houghton Mifflin, 1959, 326-7).

¹² My own choice would be C. N. Starcke, whose *The Primitive Family in Its Origin and Development* (1889) is cited by Fortes in *Kinship and the Social Order*. I imagine Starcke, acting as a "ghostly critic", would react as follows (see *The Primitive Family*, 2nd ed. 1896, 207-8): «With all respect for Fortes' diligence as a collector of facts, I am more disposed to agree with Needham that his work is altogether unscientific, and that his hypotheses are a wild dream, if not the delirium of fever. His statements throughout are based on such vague analysis and such irrational psychology, that they can only confuse the question, unless they are altogether ignored.»

¹³ Dumont's critique was written too early to take account of Fortes' book. The other reviewers had to contend with statements such as this:

"I do not . . . regard alliance [as the subject of marriage is now fashionably described] as the crux of kinship structure . . . My interest lies in the institutions that are indispensable for the continuity through time without which there can be no ongoing society. Hence I regard the succession of generations in the process of physical and social reproduction focussed in the relationships of filiation and descent as the keystone of kinship structure. For this process to go on, *institutionalized forms of alliance are not essential*. What is indispensable is parenthood, and for this *any form of*

by S. F. Moore *Man* 5, 1970, 711-12; H. W. Scheffler *American Anthropologist* 72, 1970, 1464-1466). Rodney Needham's "rather plain speaking" on the other hand will certainly ring true for a great many students of anthropology attempting to master this crucial, nay ineluctable, field of the subject and continually finding it rotten with European orthodoxy. Indeed, it is Needham's message that the teaching of staunchly critical attitudes should be a part of all anthropology courses, quite as much as ethnography and the history of the subject.

In his briefer "Remarks on the Analysis of Kinship and Marriage" (Chapter 1), Needham follows a new line of attack, with — in effect — a deliberate attempt to undermine the conventional rubrics of kinship studies. Thus, "kinship" is, in Wittgenstein's phrase, an "odd job word"; failing to appreciate this, anthropologists have mistaken context for content: "we assume that it [the word] must have some specific function" (5). In short, our expectations about the information conveyed by the term "kinship" are substantialist, reificatory and conventional. (Southwold's paper unfortunately is a good example of just this error.) The word marriage on the other hand seems to direct us "more precisely to an identifiable kind of relationship". Yet even here, there is no agreement on the boundaries of the field of marriage; in this sense it has never been adequately defined.

Concepts of "descent" are usually undermined by typological confusion. The rules of descent, like the rules of marriage, are jurally formulated and can be elicited from good informants. But to proceed no farther than this is to yield to the premature urge to classify a whole society according to "the" descent rule; and the practice is very common in social anthropology. Rather, Needham suggests, we should generate the rules *first*.¹⁴ This procedure would closely approximate to "the practical fabrication" of the rules by the people who actually use them.

The typological urge is especially strong with regard to terminologies. The major fallacy is the belief that there are broad correlations subsisting between "objective" terminological features, e.g. MB = MBS, and jurial rules, constituent groups, and other institutions in the society.

permitted procreative cohabitation is sufficient" (*Kinship and the Social Order*, Routledge, 1970, 308, emphasis supplied).

Notice that this view follows with some precision the most fantastic of the Victorian anthropologists' conjectures about primitive promiscuity.

¹⁴ In the manner of Radcliffe-Brown "Patrilineal and Matrilineal Succession" 1935, reprinted in *Structure and Function in Primitive Society*, Cohen & West, 1952, 32-48; and D. Maybury-Lewis "Parallel Descent and the Apinayé Anomaly" *Southwestern Journal of Anthropology* 16, 1960, 191-216.

Needham does not wish to deny that a terminology has a structure (see 18-21), but the illusion of exclusive type-cases needs replacing by some looser arrangement of combinatory possibilities, as proposed long ago by Lowie.¹⁵ So, the pernicious "craving for generality" can be checked, and "the quest for essential features . . . given up" (23).

Finally, on the notion of "incest", anthropologists have, just as mistakenly, tried to generalize about a homogeneous class of offenses. The most we can say about incest, says Needham, is that everywhere "incest" relates to cultural rules about access to women and the rules are prohibitions. (Yet it seems to me that even this much is capable of theoretical development, as we have seen in Lévi-Strauss's work; but Needham does not elaborate.)

In conclusion, Needham warns that the construction of substantial types or individuals is a serious conceptual delusion; it is a habit of thinking perpetuated chiefly by Radcliffe-Brown and Murdock and as such has misdirected anthropological research. (Needham here echoes Leach's attack on Fortes in *Rethinking Anthropology*.) A purely formal approach, starting from the *possibilities of phenomena* (Wittgenstein's phrase), makes for a less biased start to analysis. To understand this way of thinking we must free ourselves of conventional theory and go back to Lowie (*Culture and Ethnology*, 1917 reprinted Basic Books, 1966) and Wittgenstein of the Blue Book (written 1933-34, published Blackwell, 1958).

III

Leach's paper, "More about 'Mama' and 'Papa'", is to me the most exciting of all the papers. Here the two anthropological questions which were singled out by Needham — Affect, and Prescriptive Alliance — are bound up together, and this masterly formulation of problems is one reason why within a few short pages Leach is able to fulfill practically all the aims of the broadest anthropological analysis. The paper should be recommended to anyone who wants to find out what anthropologists are supposed to do and how successful they can be in doing it.¹⁶

¹⁵ The most succinct statement of the relevant formula — "A limitation of possibilities checks variety" — we owe to Goldenweiser ("The Principle of Limited Possibilities in the Development of Culture" 1913, revised in *History, Psychology, and Culture*, 1933, Peter Smith, 1968, 45).

¹⁶ Leach's kinship papers constitute, in effect, an extended series on kin terms as 'category' words: see "Jinghpaw Kinship Terminology: An Experiment in Ethnographic Algebra" (1945 reprinted in *Rethinking Anthropology*, Univ. of London Press, 1961, 28-53); "Concerning Trobriand Clans and the Kinship

Most textbooks of anthropology accord no importance to kin terms as words, despite the fact that, by definition, they are always part of a particular language. (This omission, Leach suggests, reflects the situation for ethnographic practice and kinship analysis.) How total this neglect is may be judged by looking at a recent textbook on the cultural context of language (R. Burling *Man's Many Voices: Language in Its Cultural Context*, Holt, Rinehart & Winston, 1970). In the chapter on Kinship Terminology, the author concerns himself solely with the "extralinguistic variables" and not with speech as such. For Burling, as for so many anthropologists to-day, there is a much more overriding preoccupation:

"The universal nature of the human family and of genealogical [sic] reckoning make it relatively easy to assign meanings according to *the genealogical ties* between the speaker and the people to whom the various terms refer" (Burling *Man's Many Voices*, 18, emphasis supplied).

This picture of what kinship is — seemingly nothing more than a crude and expendable disguise for *blood, family sentiments, pedigrees* and *pride of ancestry*, in short, as Dumont so clearly shows (*Introduction à Deux Théories d'Anthropologie Sociale*, op. cit.), substance — is, curiously, none other than the folk-image of kinship that is prevalent, as it was also in Morgan's day, within the English and American sub-culture in which these anthropologists were raised and live.

Even assuming the analysts are able to reject this genealogical distortion, there is a linguistic bias too. It is, roughly:

1. That the sound images we call words are mere tags, and as such are arbitrary in relation to their objects.
2. That each word is an isolated thing, semantically distinct (or distinguishable) from any other given word.
3. That each kinship word has only one type of referent — a human one; or that the human locus is all the anthropologist needs to be concerned with since kinship is nothing if not about people.

Category 'Tabu'," (in J. Goody, ed., *The Developmental Cycle in Domestic Groups*, Cambridge Univ. Press, 1958, 120-145); "The Language of Kachin Kinship: Reflections on a Tikopia Model" (in M. Freedman, ed., *Social Organization: Essays Presented to Raymond Firth*, Frank Cass & Co., Ltd., 1967, 125-152); and "The Sinhalese of the Dry Zone of Northern Ceylon" (in G. P. Murdock, ed., *Social Structure in Southeast Asia*, Quadrangle Books, 1960, 116-126). In particular, the 1967 paper on Kachin terms, appropriately cited by Needham (xxxviii-xl) as "one of the very finest ethnographical analyses of kinship ever published" (cf. civ-cviii), is a valuable — though not necessarily integral — overture to the present essay.

KACHIN

Social implications	Males			Females
Phonemic correspondences	Unvoiced stops hp t ts sh hk	Continuant w	Voiced stops j g	Nasals m n ɲ
Kin terms	<p><i>woi</i> FM</p> <p><i>tʃa</i> MB, WF, MBS <i>wa</i> F</p> <p><i>hpu</i> eB <i>hkau</i> male c.c. <i>rat</i> eBW (M.S.)</p> <p><i>hkri</i> ZS, ZD <i>sha</i> S, D <i>shu</i> GC</p>			<p><i>ji</i> FF</p> <p><i>mo</i> FZ <i>nu</i> M <i>ni</i> MBW</p> <p><i>na</i> eZ <i>nam</i> yMBD <i>niŋ</i> sister-in-law (W.S.) <i>nau</i> yB, yZ</p> <p><i>ma</i> C</p>
Pronouns	<i>shī</i> he, she, it			<p><i>ɲai</i> I, Ego <i>ǎn</i> we <i>nǎŋ</i> thou</p>

Table: Consonantal Patterns in Kinship Terminology.

Source: principally Leach in *Rethinking Kinship and Marriage* (Tavistock, 1971).

Does such a linguistic phenomenon require to be *explained* at all? If so, how do we go about it? What determines the selection of sounds in any particular phonological patterning? Leach follows the late M. M. Lewis *Infant Speech: A Study of the Beginnings of Language* (Routledge, 1936, 2nd ed. 1951; Cedric Chivers Ltd., 1968) and Roman Jakobson "Why 'Mama' and 'Papa'?" (1960, reprinted in *Selected Writings I: Phonological Studies*, Mouton, 1962, 538-545), and suggests we look at child-language.¹⁷ One aspect of kin terms which anthro-

¹⁷ Child-language is one of the classes of data specifically rejected by Scheffler and Lounsbury (*A Study in Structural Semantics*, op. cit., 61-62): "... the order in which a child learns the several distinct meanings of a word cannot reasonably be used as evidence for the etymological relations between those meanings. Similarly, it cannot reasonably be regarded as evidence for the structural relations between them." (Cf. Southwold's remark in *ASA* 11, 39: "We are agreed now that the order in which a child learns the application of such terms [for parents] is nothing to the point . . .") We understand better from such statements Leach's and Needham's point about how far away some methodologists are prepared to move from the facts in order to make their techniques "work".

pologists have long known to reflect child-language is parent-terms.¹⁸ In its original form, the hypothesis is that adult kin terms for parents as reported in ethnographers' listings are "strikingly similar right across the map" because they are always reformulations of baby-language terms (see 80); it has been extremely easy to verify this no matter how crude or limited the data (see 77-83 for the phonological background, from Jakobson). Any extension of it, however, was thought to be another question entirely.

Lévi-Strauss, for one, was thoroughly sceptical:

"Mais il serait vain de pousser plus loin le parallèle, par exemple en cherchant des corrélations entre la structure des attitudes et le système des phonèmes, ou la syntaxe de la langue du groupe considéré. L'entreprise n'aurait aucun sens" ("Linguistique et Anthropologie" 1952 reprinted in *Anthropologie Structurale*, Plon, 1958, 83).

Nadel was even more categorical. Language, according to Nadel, is one of the "actions autonomes": "Its phonology, grammar, and partly its semantic system, in one word, its structure, is in no way bound to group arrangements; it neither mobilizes determined social relationships nor bears within itself a reference to them" (*Foundations of Social Anthropology*, Cohen & West, 1951, 88).¹⁹

¹⁸ Leach gives an impression of novelty to Jakobson's paper in speaking of "this relatively novel field of enquiry", 77; cf. his 1967 paper: "In the whole vast literature of the subject I am only aware of one short paper which argues explicitly that these [sound-semantic associations] are aspects of kin term systems which call for investigation as problems of phonological linguistics" ("The Language of Kachin Kinship", op. cit., 126-127, emphasis supplied). Whereas, as long ago as 1852, J. C. E. Buschmann published a study virtually identical with the pair of papers to which Leach refers, the one by Jakobson and one by Murdock upon which Jakobson's was a commentary. In this paper ("Über den Naturlaut" in *Philologische und Historische Abhandlungen der Königl. Akademie der Wissenschaften zu Berlin Aus dem Jahre 1852*, Kön. Akad. Wiss., 1853; translated as "On Natural Sounds" in *Proceedings of the Philological Society*, Vol. VI, 188-206), Buschmann cites the still earlier observations by Charles de La Condamine dating from 1745, namely that in a large number of systems "parents took these words for 'father' and 'mother' from their children" (*Proceedings of the Philological Society*, 189; emphasis supplied). Buschmann's work was known to Westermarck and probably to his pupil, Malinowski, and Lubbock had, according to Westermarck, independently assembled similar evidence though in fact less impressively than Buschmann.

¹⁹ Nevertheless, Lévi-Strauss, in a somewhat later comment on the language question in anthropology, allowed that languages commonly exhibit two short-cuts or mnemonics which noticeably reduce the arbitrariness of language.

"Si nous admettons donc, conformément au principe saussurien, que rien ne prédestine *a priori* certains groupes de sons à désigner certains objets,

But see what Leach, by extending the hypothesis about parent-terms, has done. He asks: "What about the words for other close kin . . . What about personal and possessive pronouns?" (18) We have already looked at consonants and their patterning in the Sinhalese and Kachin kin term sets. Here now are some key categories in the Kachin system, arranged according to marriage possibilities (+) and impossibilities (—):

		(+)	(Ego)	(—)
<i>na eZ</i>	<i>nu M</i>		(Δ)	<i>hkri FZD</i>
	<i>num W</i>	<i>nam MBD</i>		
<i>hpu eB</i>	<i>wa F</i>		(O)	<i>tsa MBS</i>
	<i>la H</i>	<i>gu FZS</i>		

Looking at the right-hand side for the moment, Kachin *hkri*, as Leach points out, sounds like *hkyi* "faeces". *Tsa* means, connotatively, among other things, *mātsa* "curse". The phonemic patterns on the other side too are surely striking. I suggest that, for male Ego, the phonemes of all the terms on the left-hand side can be translated as follows: *n* = woman; *u* = lives with me, *a* = lives elsewhere; *m* = belongs to me, in marriage. I am quite sure Leach would *not* allow such literal equations, but there they are, and they seem to me to make sense for the overall picture of Kachin social life. The patterning for female Ego is less sharp, but the vowel-contrasts on the left-hand side surely derive

il n'en semble pas moins probable qu'une fois adoptés, ces groupes de sons affectent de nuances particulières le contenu sémantique qui leur est devenu lié" ("Postface aux Chapitres III et IV" 1956 in *Anthropologie Structurale*, op. cit., 106).

One of these devices operates at the vocabulary, another at the phonetic level. Thus, words considered in isolation (assumptions 2-3 above) rarely if ever have any intrinsic natural — or vocal — relation to their objects, but words in the context of a semantic field (for example, in response to the suggestion of colour-associations) often do. Leach calls this feature of language "semantic colour". Incidentally, these points suggest that the ethnocentrism of anthropologists with regard to the language of kinship mentioned above also stems in part from a vague resentment at the plenitude of meaning which would result if they considered word-play, pun, metaphor and so on in nomenclatures. I suspect this applies moreover to the wholesale rejection of McLuhan's work in certain quarters. No one, his critics insist, would be a poet and not know it!

meaning by being the exact *opposite* of the corresponding terms used by male Ego.²⁰

The question for Korn ("A Question of Preferences: The Iatmul Case", 99-132) is: how would Lévi-Strauss have treated the Iatmul system had he considered it more fully than he did in *The Elementary Structures of Kinship* (Chapter XIII)? Where does it fit in with the other types of elementary structure? In a very concise, perhaps over-redundant, manner, Korn shows that Iatmul kinship and marriage cannot be reduced to a simple formula; at best the system is atypical. The terminology presupposes a form of matrilineal cross-cousin marriage, but it is one involving both asymmetry and alternation, the latter feature more to be expected in association with *bilateral* c.c.m. The difficulties in classifying the Iatmul system do not stop here: there is not one marriage preference (the category *iai*²¹), but several, called by Bateson "cliches"; and it is reported that, in practice, the Iatmul marry very nearly "at random". In Korn's eyes these several additional marriage preferences and practices in no way reflect a single underlying order; rather, they "conflict" with (1) the stated *iai*-preference, (2) with each other and (3) with the category system. Such numerous and elaborate "variants" make it impossible to classify a system such as the Iatmul on Lévi-Strauss's scheme. At the end, Korn manages to quote Lévi-Strauss's own words to damn him.

Korn's argument and the effect of her final bite are not as penetrating as they might have been; she misunderstands too many points about the analysis of kinship and Lévi-Strauss's theory. For one, Korn obstinately supposes that the preferences — given in her paper as *separate* diagrams — indicate the breakdown of Iatmul culture and suggest "a prior state in which the actual institutions may have corresponded more closely with the terminology" (128). (For a critique of this kind of argument, see my note in *Man*, o.s., LXIV, 1964, 119-120.) Lévi-Strauss's point of course is that "real" (or actual) situations are always being construed

²⁰ I have rearranged slightly Leach's syllogisms (see 96). Compare the economy and limpidity of presentation in Leach's analysis with this:

"Finally, since *nam* is reciprocal to both *moi* and *gu*, and since *nau* is related to *nam* in the way in which *na* is related to *moi*, and since *khau* [sic] is related to *nam* in the way in which *khau* is related to *gu*, the class and subclass taxonomy involving *nam*, *nau* and *khau* is superficially more complex in appearance (see Figure 9c), but it derives entirely from the two reciprocal sets just described" (Scheffler and Lounsbury *A Study in Structural Semantics*, op. cit., 202).

²¹ Korn's Table 1 (104) contains an error: for FMSD opposite 3, *iai* read FMBS.

to fit practically any model,²² and that therefore we should first of all look carefully at the way the rules are constructed. Korn is oblivious to this, the keystone, of Lévi-Strauss's position. And then too, she interprets Lévi-Strauss as saying that kin terms and marriage rules are unrelated, which is quite wrong. Above all, Korn fails to carry the Iatmul analysis far enough to prove her case. For example, however numerous their preferences, the Iatmul have one highly significant category *prohibition*, that is, on MBD marriage (*nyame* = M, MBD), which suggests, I think in line with Lévi-Strauss's own argument, that the Iatmul have thought about the hole in the doughnut and found it useful.

In both Leach's and Korn's papers we find an understanding — perhaps more implicit than otherwise — of what Lévi-Strauss is trying to do. Their attacks on his position show all the more forcefully its inner strength.

IV

The contributors to ASA 11 were for the most part asked simply to write about what interested them in the study of kinship and marriage. The title of the published volume only really applies, as we should expect, to a nucleus of the papers — to Needham's and Leach's, and to Korn's, written while its author was a student of Needham. I have felt it worthwhile to devote most space to these papers. The authors of the remaining seven chapters deal with kinship issues in more conventional ways and with varying success. Where they use "test-cases" to make a general point (Southwold, Fox, Rivière, Beidelman), they run the risk of poor documentation; where they discuss the issues pertaining to one particular case (Forge, McKnight, Wilder), they will probably arouse the interest only of the specialists.

Martin Southwold, in his paper "Meanings of Kinship" (appears as Chapter 2, 35-56) aims to devise a method or set of concepts for giving, rather curiously, a "more adequate translation" of some of the "important role-terms (role names)", as used by the Ganda, than would be given by the "genealogical" style kinship method. This job of "translation" he takes (incorrectly) to be Leach's programme (in "Concerning Trobriand Clans and the Kinship Category 'Tabu'", *op. cit.*). We are

²² A thesis independently corroborated, as I take it, in Sahlins' striking "On the Ideology and Composition of Descent Groups" (*Man*, o.s., LXV, 1965, 104-107).

not told very much about the Ganda terms; the main point evidently is that the most frequent and salient usages have *group* (not "genealogical") reference. This observation is supposed to raise the question "whether, or how far, kinship in Buganda is really kinship". The argument is strenuous and in the end results in only three translations.

In fact, Southwold's approach to meaning and the Ganda material seems to have been stimulated not by Leach's 1958 paper but by Iona Mayer's on the nearby Gusii (*The Nature of Kinship Relations: The Use of Kinship Terms among the Gusii*, Manchester Univ. Press, 1965), which purports to qualify Leach 1958. It seems that for Southwold, what is sense for the Gusii is sense for the Ganda! A doubtful inspiration. In his rigorous development of the notion of "parentation" Southwold is fully anticipated by MacFarlane ("Analysis of Relationships of Consanguinity and Affinity", op. cit.), and in his "new-minted word" congeniacy at least partly by Geertz (*Person, Time, and Conduct in Bali: An Essay in Cultural Analysis*, Yale University Southeast Asia Studies, 1966), who borrows Schutz's "consociation". There is a certain amount of misunderstanding about "the whole grand edifice of kinship theory",²³ and there is not enough analysis, as was the case with Korn's paper. For instance, following Leach, we might ask how Southwold would account for the following linguistic peculiarities, or "intensional" aspects (52) of the terms *muganda: ganda; nnyina: mwannyina; kojja: jjajja?*

For his part, James J. Fox ("Sister's Child as Plant: Metaphors in an Idiom of Consanguinity", Chapter 10, 219-252) remonstrates with approaches such as Lounsbury's and, in a description of the avunculate among the Rotinese of Indonesia, presses the advantages of the "more open, multi-contextual attention to linguistic use". The acknowledged inspiration for Fox, as for Leach, is Jakobson (in a paper of 1959).

In Rotinese kinship, sister's child is called *selek*, a word formed from the verb *sele* «to plant». Its reciprocal is *toök*, a word designating all the «mother's brothers» — possibly more than two thousand individuals, as Fox is careful to point out — but when modified by the suffix *-huk* «trunk, stem, cause, origin», designates one and only one living in-

²³ From among citations given by Southwold, three examples: the genealogical method, as given in *Notes and Queries* (6th ed., Routledge, 1951) (39,45); Radcliffe-Brown (in "The Study of Kinship Systems" quoted, 43); and, as already noted, the point of Leach's "Concerning Trobriand Clans and the Kinship Category 'Tabu'" (35).

dividual. Ideally, the oldest brother in a sibling group is *toō-huk* to all his sisters' children; he can also be *baī-huk* «grandfather of origin» to the children of his sisters' daughters. *Toō-huk* (*baī-huk*) is a ritual role the tenure of which, in practice, depends upon the continuance of the series of "performances" on behalf of *selek* throughout their lives. Ritual performances make the *toō-huk* (*baī-huk*) into a grower of the sisters' children "from «seed» to «sprout/shoot» and from this to the mature plant".

Fox does not bring out the precise extent of this man-plant-cosmos complex on Roti, nor does he say enough about the kinship system; and to me, Roti strongly suggests the Tikopia system as a comparison (R. Firth "Marriage and the Classificatory System of Relationship" reprinted in *Essays on Social Organization and Values*, Univ. of London Press, 1964, 88-122). The scope of Fox's paper therefore seems unnecessarily restricted.

According to Peter Rivière ("Marriage: A Reassessment", Chapter 3), "the functionalist argument", as he calls it, which explains marriage "in terms of what marriage does" (primarily legitimizing the children) is not wrong, just inadequate. Anthropologists should "consider the parts from which it is constructed" and "in the first place concentrate on the categories of male and female" since marriage is "part of the total male/female relationship". Especially disappointing is Rivière's failure to consider the central importance of marriage rules, upon which a decisive piece of work has been published by Sturtevant in the form of a short appendix concerning the limiting case, institutionalized sibling marriage.²⁴ This superb ethnographic summary deserves to be better known and a "reassessment" of marriage would have provided a good occasion.

Beidelman ("Some Kaguru Notions about Incest and Other Sexual Prohibitions", Chapter 8) finds, interestingly enough, that in their folklore the Tanzanian Kaguru "belabour sibling incest" more than any other type. Incestuous or prohibited behaviour (*mahasa* "confusion") is defined not only as marriage but also as sexual intercourse and as eating or living too closely with certain persons, "especially... one's sisters", or one's spouse's sibling (opposite sex). Aggression between

²⁴ "Sibling Marriage Comparisons" in W. H. Goodenough (ed.) *Explorations in Cultural Anthropology*, McGraw-Hill, 1964, 202-207. Lucy Mair, in a book which appeared fully seven years after this article has no reference to it. Mair even assures her readers that the subject is still obscure (*Marriage*, Penguin Books, 1971, 27).

prohibited relatives (F-D, B-Z) is a common theme in the twenty folktales selected and summarily arranged by Beidelman (193-196); another theme is a son's desire to live alone with his mother, or a man with his children. However, Beidelman does not make very good use of his data. He actually obscures the question with his gratuitous stress on "matriliny" and the allegedly special problems of this form of descent system. Sibling incest, which preoccupies the Kaguru, is more than any other kind of incest *independent* of the descent rules, as S. F. Moore has shown ("Descent and Symbolic Filiation" *American Anthropologist* 66, 1964, 1308-1320).

Anthony Forge ("Marriage and Exchange in the Sepik") was asked to comment on Korn's Iatmul analysis in the first instance because, for some twelve years, he had been at work on studies of neighbouring peoples, notably the Abelam. Forge sets up his commentary against a background of what may be seen as a Highlands New Guinea "puzzle". Among the societies in this region there is a culturally uniform ideology of "balanced exchange", i.e. basic symmetry. The puzzle lies in the varying toleration and compensation in these societies of the marked group-instability and status-inequalities, or in contradictions arising chiefly from "the imbalance inherent in the relationships set up by marriage" (136). Because the Iatmul have a distinctly *asymmetric* component in their "exchange" system, Forge concludes that they are "somewhat unusual". He is "confident" that the tendencies generated by "an actually working system" of the Iatmul kind are so foreign to the culture type that "none of Lévi-Strauss's elementary asymmetric structures could exist in New Guinea as working systems and that they are unlikely even in ideology" (139). It is odd that Forge makes no mention of Bateson's analysis of stability, exchange and related questions among the Iatmul (*Naven*, op. cit., cf. Needham's comments, liv). Perhaps this is one of the reasons Forge has to admit that he has "no solution".

The papers by McKnight (Chapter 7) and Wilder (Chapter 9)²⁵ deal largely with the literature about two of the societies Needham, in his own estimation, has already most satisfactorily re-analyzed, viz. Wikmunkan (Wik-mungkan), a structure of restricted exchange, and

²⁵ This is my own paper. I can offer two factual points to alleviate what some may charge to be scholastic incest in reviewing ASA 11: firstly, as the editor notes (xiv), those papers given and discussed at the Bristol A.S.A. meeting in 1970 (Chapters 1-7) were little, if at all, revised by the authors for publication, moreover three papers had not been received, and the Introduction, almost a third of the book, was not yet prepared; secondly, after this

Purum, a structure of generalized exchange; both papers are more than adequately treated by Needham in the editor's Introduction.

Of the anthropologists specifically invited to prepare papers for the Bristol conference in 1970, only McKnight was a scholastically unknown quantity (xiv). There is an outstanding tradition of Wikmunkan field and theoretical studies (notably by McConnell, Thomson, Lévi-Strauss, and Needham), there are also McKnight's own fieldwork and documentary collections (genealogical records); yet McKnight signally fails to get to grips with any of this. Who, for example, would think it necessary in 1970 to "disprove" the thesis of *Marriage, Authority and Final Causes* or even of "Structural Analysis in Linguistics and Anthropology" or to reproduce yet again McConnell's diagrams, when these are already reproduced and, what is more, expertly discussed, in Needham's articles of 1962 and 1963? All this is churned out by McKnight.²⁶

Concerning my own paper, "Purum Descent Groups", I need only say that, like Korn and Needham, I felt that I "had to do something about an increasing accumulation of theoretical pronouncements [about Purum society] which had less and less to do with social reality or responsible analysis" (Needham, xxxii). My question is really double-barreled — as was Leach's when he asked why the Trobrianders had four clans (1958). I contend that you cannot understand marriage in Purum society without a grasp of the dynamics of descent-group formation and, more especially, without recognizing that (on account of a perpetually-active segmentation process) it is not possible to assign an *exact* number of groups for purposes of analysis. Yet some anthropologists — for example, Harris (1968) and Livingstone (1969) — have tried to do just this.

V

Let me now add a few scattered remarks on some crucial kinship matters *not* broached by the authors of ASA 11, although they easily might have been. What I think *Rethinking Kinship and Marriage* proves, willy nilly, is how astonishingly little has actually been done,

review was completed in draft I received a letter from Professor Adrian Mayer who, as new general editor of the ASA monograph series, informed me that Tavistock Publications are arranging to have a French translation of ASA 11 published by Éditions du Seuil of Paris, omitting the last three chapters (this includes my own paper, Chapter 9).

²⁶ There are two minor errors in the text: on page 152, the designations of *wunya* and *ponta* should be "oB" and "yB" respectively; and on page 160, line 4, for "patricians" read "patriclans".

as far as most anthropologists are concerned, on the fundamentals of kinship study; the reader of the book easily gains an impression of unrelenting gloom and despair. This is partly due in ASA 11, as I have occasionally hinted above, to a neglect (or an imperfect understanding) of the Lévi-Straussian work. Of course, it is also due, in part, to the very success of some of the authors in making their points. As Needham says in opening his "Remarks", the situation is highly paradoxical.

In some matters, however, no paper in ASA 11 argues far enough. Part and parcel of the dogma of conventional theory, as Southwold's paper shows, is the supposedly "genealogical" or "descriptive" resonance to kinship-relations. No explicit argument is deployed against this pernicious notion in the discussions in *Rethinking Kinship and Marriage*, and it seems one must be made.²⁷ A genealogy, or the *idea* of genealogy, does not constitute a whole; kinship does. A kinship system, in Lévi-Strauss's words,

"ne consiste pas dans les liens objectifs de filiation ou de consanguinité donnés entre les individus; il n'existe que dans la conscience des hommes, il est un système arbitraire de représentations, non le développement spontané d'une situation de fait" (*L'Analyse Structurale en Linguistique et en Anthropologie* *Anthropologie Structurale*, Plon, 1958, 61).

Leach, following, with Lévi-Strauss, Kroeber's argument of 1938 ("Basic and Secondary Patterns of Social Structure" *Journal of the Royal Anthropological Institute* 68), has all along insisted that the immediate determinants of the meaning of kin terms, or kinship systems, is nothing so sophisticated as "genealogical knowledge"; it is the residence pattern. The fact that definition and internal coherence can be shown in genealogical terms is sheer redundancy; the same things can be shown for residence/age grading, and on at least two linguistic levels, at least for some systems, as Leach has done.

Another crucial complex on which it seems to me clarification is still much needed is incest. Perhaps one of the few major failings of *The Elementary Structures of Kinship* is the total absence (in Part I) of any definition of the *types* of incest and this is also true of ASA 11. Of the three possible types of incest only sibling incest is "pure", or presupposes no previous union. I don't know that this point has ever been made in anthropological discussions of the incest question, but it

²⁷ Cf. Needham (4): "These jural [kinship] systems and their component statuses can be genealogically defined. *Why this should be so is a fundamental question that has never been properly resolved, and I cannot take it up here. Let us merely admit the fact*" (emphasis supplied).

appears to be all-important to our understanding. To make the atom of kinship possible, for example, and its function in showing the inter-linking of groups, only the ban on sibling marriage *must* be presumed; cross-cousin recognition and a genealogical network could be maintained in the absence of all other prohibitive regulations. Prohibition of sibling incest is above all an epistemological necessity.

Finally, what is kinship, after all? Kinship is, because it must be, a mode of person-definition (which is not to say it is solely "about" people); as such it is of necessity neither Ego- nor socio-centered, but time-centered. Kinship involves "the recognition . . . of biological aging, the appearance, maturation, decay, and disappearance of concrete individuals" (C. Geertz *Person, Time, and Conduct in Bali: An Essay in Cultural Analysis*, op. cit., 42). Kinship relates not to biology but to *biography*; it is a vast and vital metaphor for a human system. We should instead of talking about organisms by analogy talk about organs rather more directly. If the capacity for articulate speech is at a "deep" level built into humans in a way that it is not in other animals, then it may be that kinship is too, as Leach's paper in ASA 11 so brilliantly demonstrates, almost to confirm Lévi-Strauss's ideas along this line. The "socialization" of the body and the "organization" of society are inherently complementary processes.

I would say there is ample evidence in ASA 11 to substantiate these propositions. Morgan need no longer be puzzled: in this book anthropology students, having been bored to death by the competition, will find the greatest of encouragements towards further study of the subject.

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