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Introduction

The paper that follows is the English original of a preface written for a monograph by Dr. María Matilde Suárez, *Terminología, Alianza matrimonial y Cambio en la Sociedad Warao* (Caracas, 1972), and published there (pp. 7-38) in a Spanish translation made by Isabel Bacalao. This introductory note provides an explanation of its appearance here, and outlines enough of the background to make its concerns intelligible.

After field research among the Warao of the Orinoco delta, Venezuela, Dr. Suárez, who had already published one book on them (1968), came to Oxford in order to make a deeper and more theoretical analysis of her materials. We worked together in the academic year 1968-69, and the results of the investigation were published, under the title "Terminology, Alliance and Change in Warao Society", in the *Nieuwe West-Indische Gids* for April 1971. A Spanish edition of this study was then undertaken, and Dr. Suárez kindly proposed that I might supply a preface. I readily consented, and her book, with my "Prólogo", appeared in Venezuela in 1972. Dr. Suárez's monograph was thus deservedly made available in English and in Spanish, but the quite lengthy preface was accessible only to those who could read Spanish. There was the possibility, too, that the place of publication might militate somewhat against a wide dissemination of the arguments presented in the monograph. So since the preface embodied an independent analysis, and in my view could also have theoretical consequences outside the field of Amerindian studies, I thought it might usefully be printed in English. The Koninklijk Instituut voor Taal-, Land- en Volkenkunde generously agreed to publish it. The English text is reproduced here virtually unchanged from that which was translated into Spanish: the only noticeable alterations are that footnote references to the literature

have been inserted within parentheses directly into the exposition, and that the sources have been collected into a consolidated bibliography at the end. Except where otherwise indicated, the tables referred to are those of Dr. Suárez: these are identically numbered in both the English and the Spanish versions.

Ideally, a reader of the present commentary would make the most of it by reading Dr. Suárez's English account (1971), but since that acquaintance cannot be presumed I need to state very shortly what her argument is about. The central problem is posed by the Warao relationship terminology (Suárez 1971: 84, table 16), reproduced here as Table 1. The first ascending and first descending genealogical levels exhibit lineal distinctions consonant with a two-line classification, yet there is none in the other levels, particularly in that of Ego. These

Table 1

WARAO RELATIONSHIP TERMINOLOGY

1. <i>nese-nobo</i>	FFF, FMF, MFF, MMF
2. <i>nese-natu</i>	FFM, FMM, MFM, MMM
3. <i>nobo</i>	FF, FFB, FMB, MF, MFB, MMB
4. <i>natu</i>	FM, FFZ, FMZ, MM, MFZ, MMZ
5. <i>dima</i>	F
6. <i>dehota</i>	FeB, FZH, MZH
7. <i>dimuka</i>	FyB, FZH, MZH
8. <i>dakatai</i>	FZ, FBW, MBW
9. <i>dāni</i>	M
10. <i>daku</i>	MB, FZH, MZH
11. <i>danihota</i>	MeZ, FBW, MBW
12. <i>danikatida</i>	MyZ, FBW, MBW
13. <i>dahe</i>	eB, FBS, MBS, FZS, MZS (older than Ego)
14. <i>daka</i>	yB, FBS, MBS, FZS, MZS (younger than Ego)
15. <i>dakoi</i>	eZ, FBD, MBD, FZD, MZD (older than Ego)
16. <i>dakoi-sanuka</i>	yZ, FBD, MBD, FZD, MZD (younger than Ego)
17. <i>damitu</i>	ZH
18. <i>tida</i>	W
19. <i>behetida</i>	BW, WZ
20. <i>harayaba</i>	WZH
21. <i>dami-sanuka</i>	WB
22. <i>dahi</i>	WF
23. <i>dabai</i>	WM
24. <i>auka</i>	S, BS
25. <i>aukatida</i>	D, BD
26. <i>hido</i>	ZS, ZD (older than Ego)
27. <i>hido-sanuka</i>	SW
28. <i>natohorani</i>	DH
29. <i>dawa</i>	SS, SD, DS, DD, BSS, BSD, ZSS, ZSD, BDS, BDD, ZDS,
30. <i>natoro</i>	ZS, ZD (younger than Ego)
	ZDD, SSS, SSD, SDS, SDD, DSS, DSD, DDS, DDD

discrepancies suggest to Dr. Suárez a hypothesis of social evolution, according to which an earlier two-section system is held to have been subject to a gradual and partial disintegration in the direction of a cognatic and non-prescriptive type of organisation. An analysis of genealogical data, interpreted in the framework of this hypothesis, reveals a significant preponderance of symmetric marriages such as would be characteristic of a two-section system of prescriptive alliance. "Thus the Warao may exemplify a test case of structural change from a two-section system to a cognatic system" (Suárez 1971: 113).

I

When Dr. Suárez worked with me at Oxford on the analysis of her Warao materials, in a collaboration which was at once pleasing and instructive, the result was the argument that has now been published in a Spanish edition. It would be redundant, therefore, if I were to write here that I approve the analysis or, more particularly, that I find it cogent in one respect or another.

A more useful course, it seems to me, is to attempt a more detached assessment and to subject the hypothesis of structural change to a somewhat skeptical scrutiny. Not that I object in principle to the type of hypothesis, let alone that I subscribe to the confused notion that structural analysis is incompatible with the study of social change, for on both counts my position is quite the reverse. My intention is guided, rather, by the simple methodological precept that so soon as one has conceived (or been presented with) a plausible or convincing hypothesis, one should as a matter of course do one's best to disprove it.

As former director of studies to Dr. Suárez, I am a little at a disadvantage in such an exercise, but perhaps the lapse of time since then has placed me at a suitable distance to form a fresh view of her argument. However that may be, there are a number of points which now appear to call for further attention.

II

In chapter IV, after a discussion of the genealogical data, the Warao marriages recorded are interpreted on the basis of a model satisfying the requirements of a two-section system.

Since it may not be immediately evident why this model should be chosen, let us begin by making this matter explicit. The discussion of the terminology, carried out in chapter III, shows that the Warao terms

do not compose a consistent scheme. In particular, lineal distinctions are made in the first ascending and the first descending genealogical levels (see table 17) but not elsewhere. This much granted, the question remains how many descent lines should be distinguished. Methodologically, the demand is for as few lines as will account for the equations and distinctions in the terminology, and on this result will depend in part the characterisation of the system.

In the first ascending genealogical level, where one can normally expect to find definite signs of the character of the classification, there are clear lineal indications but also some obstacles to an immediate interpretation. Let us start by isolating the singular denotations for F and M. These can be set aside, for they have no systematic implications: each is specified by only one elementary genealogical term; there are no convergent specifications defining the positions in question, nor are there any extended specifications indicating a classificatory scope. There is no formal reason, moreover, why the parents should not be distinguished from other statuses in these positions; and there are in fact prescriptive classifications in which this is done, e.g., in the two-line terminology of the Sinhalese of Pul Eliya and, nearer to the Warao, in that of the Akawaio of Guyana.¹ This leaves the lineal distinction $FB \neq MB$. There is no indication at this level of any descent line in addition to those of FB and MB.

But the distribution of the genealogical specification FZH reduces the clarity of this distinction. Under *dehota* and *dimuka* we find $FZH = FB$; whereas under *daku* we have by contrast the equation $FZH = MB$. The former equation does not accord formally with a two-line scheme; the latter does. The denotation of the father's sister's husband poses in fact a special problem. In Wilbert's list (table 12) there is no entry for FZH: *dehóta* is FeB, *dimúka* is FyB, and *dakú* is simply MB. In Vaquero's list (table 13) FZH is *daku*, a term which is also specified as MB, while *dijota* is rendered as FeZ and *dimuka* as FyZ. It is only when we come to the list recorded by Dr. Suárez in her analysis of 1971, based as it is on the study of extensive genealogies, that we find FZH denoted by the same term as FB.² Without further

¹ Leach 1960: 124; terms 3 and 6. Unpublished Akawaio terminology kindly communicated to me by Dr. Audrey Colson.

² In Dr. Suárez's earlier and less technical work, *Los Warao* (1968), the relationship terms listed are more clearly systematic: *daku* is translated simply as "mother's brother"; *dehota* and *dimuka* are rendered respectively as elder and younger "father's brother" (p. 112). The designation of FZH is not reported.

published details on this point it is not feasible to go far into this issue, but as the ethnography stands it looks as though the Warao whom Dr. Suárez studied have latterly come to apply the terms for FB to the father's sister's husband. If this is the case, it represents a further disintegration of the principle of linearity and is thus consistent with the hypothesis. In any event, the distribution of FZH indicates only that the father's sister's husband may be denoted by terms belonging to both of the hypothetical descent lines: there is no singular designation for FZH such as might indicate, at this level, a third line.

The other level at which there are lineal features is the first descending. Here, under the terms *auka* and *hido*, we find the distinction $S \neq ZS$. Structurally speaking, there are no other masculine positions denoted by further terms and there are thus only two lines to be distinguished. It is true that at this level there are also separate affinal terms making the distinctions $SW \neq ZD$ and $DH \neq ZS$, but these do not establish any additional descent lines.

There are indeed, therefore, two descent lines to be established on the basis of the limited lineal features in the terminology (see my fig. 1).

Fig. 1. WARAO TERMINOLOGY
(Consistent denotations)

In the face of the distinctions made in the first ascending and first descending genealogical levels, it would be incorrect to represent the Warao terminology as non-lineal; and given the number and distribution of the terms examined it would be incorrect to postulate more than two lines. To judge by the lineal features alone, the Warao terminology contains a symmetric element. But even this limited conclusion is subject to a further qualification. The two-line scheme derives from an examination of terms for masculine positions, but it cannot be established on the basis of feminine specifications. In the first ascending level $FBW = MBW$, so that if we represented the classification by matriline there would be no lineal contrast. Only in the first descending level is it immaterial whether masculine or feminine positions are taken into account. In a perfectly consistent terminology, of course, the feminine specifications would correspond structurally with the masculine, so that, e.g., $FB \neq MB$ would be complemented by $FBW \neq MBW$; but the general character of the Warao terminology is not consistent, and we can only try to isolate its distinctive features wherever they happen to occur. To the extent, then, that lineal features do sporadically exist, the Warao terminology distinguishes only two lines.

III

A striking character of the terminology, considered historically, is its apparent stability. The examination of the sources carried out by Dr. Suárez indicates that there may have been no changes in the period 1825-1968. This is a period of 143 years, or about six or seven generations, during which the cultural pressures on the Warao must have been gaining momentum, and yet so far as the evidence goes there have been no changes in the terminology. How can this stability be accommodated to the hypothesis of structural change?

The first response to be made is, of course, that we do not possess comparable accounts of the terminology throughout the period in question. The number of terms and specifications recorded has been constantly increasing, and even among modern accounts there are gaps, discrepancies, and uncertainties. It is not possible, therefore, to say for sure that the principles of Warao social classification have been stable since 1825. Nevertheless, so far as the evidence goes the terminology does seem to have been stable, and on this premise it is a question why we should need to introduce the idea of a prior two-line classification. That is, if the terminology can be employed by the Warao for so long,

in ordering their social lives, why need we think that it is not a good enough classification as it is?

In fact, there is no necessity to postulate that an inconsistent terminology is the result of changes in a previous terminology which was more consistent. The idea that inconsistency is a sign of instability is not a logical or theoretical premise; it is a prejudice to which anthropologists are likely to be tempted. Social anthropologists try to discern order in multifarious and fluctuating phenomena, and they tend therefore to favour consistent representations of the facts over inconsistent. In the case of prescriptive classifications in particular they are encouraged in this course by two factors. In the first place, there are numerous terminologies of the kind which really are beautifully consistent. In the second place, a prescriptive terminology is by definition one in which the classification is ordered by an invariant relation that articulates categories and lines throughout the classification (Needham 1971: 20), and in its paradigmatic form it is thus internally consistent. But neither of these circumstances is enough in itself to justify the inference, in any individual case, that an inconsistent terminology was once less inconsistent.

More generally, also, there is a common presupposition on the part of social anthropologists that there will usually be an overall congruence between terminological features and social forms, such that jural institutions ought largely to be inferable from the categories. If, then, the terminology is not entirely orderly, the notion proposes itself that the forms of social life are themselves disorderly; and the picture of contradiction, or even of conflict, among institutions creates the impression of a disturbed and perhaps unviable society. But the degree of harmonious integration implied in the presupposition is neither logically necessary nor justified by factual correspondences. There can be great disparities between terminologies and institutions,³ and there can also be great disparities between institutions, but these need not preclude or impair the maintenance of a stable form of social life.

In the Warao case, therefore, we should start simply from the premise that some features at least of their terminology may govern to some extent certain aspects of their social lives. Our task is not to say that the Warao might do better, in elaborating a consistent classification or in making a closer fit between categories and action, but to find out in

³ See Lowie (1917: 103) on "the frequent coexistence of distinct and even contradictory principles in the same 'system'"; cf. Needham 1971: 18-22.

what precise respects they actually manage to order their collective conduct. Within the limits where the terminology has real significance to them — in marrying, fishing, house-building, and so on — certain formal features may be found to correspond well enough to their rights, duties, and customary expectations. A total analysis might then show that the lineal distinctions had in themselves a present significance which was sufficient to account for their presence and continuity. Similarly, the "inconsistent" features might one by one have quite various meanings in relation to other concerns. In other words, the terminology can well be formally inconsistent because the stable conduct of social life need not be systematic.

On the other hand, there is certainly some connexion between categories and institutions, both are subject to change, and it is expectable that there will be some degree of concomitance between the forms of change. We know that the Warao have been subject to many pressures to change, and we may assume that changes did not begin with the first European records early in the nineteenth century. The terminology that we know cannot be assumed to be primeval and unaltered since it was first composed, but it must be the product of cumulative pressures and alterations. Given these premises, the question is what was its prior character, and the fact that it may have been stable for the past six generations has no bearing on this matter. We do not know how long ago the present terminology evolved from an earlier form, and since we can know nothing about the attendant circumstances either there is no theoretical interest in the question. The present terminology contains indications of a two-line classification, and the evolutionary question is whether it has moved towards or away from a consistently symmetric scheme of the kind.

My own response is that I have never encountered a society which appeared to have adopted prescriptive alliance. On the contrary, prescription seems to be an elementary property which tends to become weakened or disintegrated in favour of some other principle or type of order. I have suggested elsewhere a typological scale of structural evolution in which two-section systems are transformed ultimately into non-prescriptive systems.⁴ As long ago as 1917, too, Lowie pointed to

⁴ Needham 1967: 45-6. Layrisse and Wilbert report that the Arawak-speaking Wapishana of Guyana have adopted prescriptive features into their terminology from their Carib-speaking neighbours. "Into what appears to have been originally a Hawaiian or Eskimo system, they have introduced the terms and corresponding behavioral practices of a Carib bifurcate-merging system." viz.,

"the most powerful circumstantial evidence from distinct quarters of the globe to prove that Hawaiian features develop secondarily within the Dakota scheme" (Lowie 1917: 162; cf. Needham 1971: 22-3), in other words that a lineal terminology may change into non-lineal. This is the direction in which, according to Dr. Suárez's hypothesis, the Warao terminology has progressively changed; and, on the supposition that the lineal features are signs of evolution, I think this is right. However stable the terminology may latterly have proved to be, it can still plausibly be seen as the result of the supersession of a prescriptive classification by the cognatic recognition of relatives.

IV

A central part of the argument is that the pattern of marriages shows a proportion of symmetric instances which is too high to be merely fortuitous. It is only when the ambiguous cases are computed against the hypothesis of symmetric alliance (table 22) that the proportion favours non-symmetric marriages. This is an intriguing result, but it too is a conclusion that should not be accepted without question. Do the symmetric marriages really indicate that Warao society was formerly a two-section system?

The implication is that prescriptive alliance was abandoned, and that the effective terminological distinctions in Ego's genealogical level were given up, yet that for some generations the Warao continued to contract, in a high proportion of instances, a type of marriage for which there was no longer a categorical directive. This is a very hard matter to assess, but the marriage prohibitions and permissions listed by Dr. Suárez (table 18) agree significantly with the hypothesis. In spite of the fact that there is no distinction between parallel and cross-cousins, the regulation of marriage has two points in common with a two-section system: (1) "Marriages between genealogical levels are . . . rejected," with the exception that (2) a man may marry his sister's

FZ = MBW, FZS = MBS, FZD = MBD (Layrisse & Wilbert 1966: 155). Given the generally two-line character of Arawak terminologies, this is unexpected. The inference proposes itself that what has actually happened may rather be that a two-line Wapishana terminology, into which Carib terms may well have been adopted, exhibits in its "Hawaiian or Eskimo" features the kind of changes that are ascribed to the Warao; or else that certain Wapishana have simply adopted into their symmetric terminology certain terms from a Carib terminology of the same type. Dr. Rivière, in an analysis of scattered data published by W. C. Farabee (Rivière 1963), has concluded that the Wapishana terminology does indeed indicate a two-section system.

daughter. These are characteristic features of two-section systems, and they are especially noteworthy in the Warao case. On the one hand, there are the two-line indications in the terminology, and the marriage regulations accord with these. On the other hand, the marriage regulations do not accord systematically with the generally cognatic organisation of Warao social life. Although there is no prescribed category of spouse, therefore, there is nevertheless a categorical limitation on marriages such as is characteristic of a two-section system of prescriptive alliance. This limitation can be seen as a relic of such a system among the Warao, and it is concordant with the high proportion of "symmetric" marriages.

There remains, however, a difficulty about the significance of the marriages themselves. These have been reduced to structural types by plotting them against a two-section scheme, and there is no denying that the results are suggestive, but it can be argued that the proportions do not in themselves prove a derivation from a former system of the kind. Two further tests of this hypothesis propose themselves. One test is to disregard the symmetric features in the terminology, and to plot the marriages against another kind of prescriptive scheme, e.g., a three-line system of asymmetric alliance.⁵ This would show, in a prescriptive context to begin with, whether the Warao marriages could similarly be interpreted as relics of a former asymmetric system. It seems to me not out of the question, in principle, that some such result might be obtained. The marriages would be compared, not as symmetric, non-symmetric, and ambiguous, but instead as prescribed asymmetric, non-asymmetric, prohibited asymmetric, and ambiguous (by three criteria, this time, rather than just two). If it turned out that there was a high proportion of hypothetically prescribed asymmetric marriages — in spite of the fact that the Warao terminology contains no asymmetric features at all — this would demonstrate that the method is at fault and not to be relied upon for the present purpose. The other test is to take the marriages of a cognatic society, one with a non-lineal terminology and no connexion with any neighbouring or previous prescriptive systems, and to plot these marriages against a two-section scheme. If in this case it turned out that there was a high proportion of "symmetric"

⁵ Thus the fact that marriage with the ZD is actually permitted by the Warao, though prohibited in a three-line system, would in particular be overridden for the sake of the test. To be plausible, if this effect were called for, this type of marriage could be explained away as being itself a sign of change from a former asymmetric system.

marriages, this also would demonstrate that the method is incapable of proving a structural evolution.⁶

In the absence of such tests, I am afraid, the ultimate cogency of the present analysis must be considered uncertain in so far as it relies on this method alone. But this is to judge the case by very severe standards, and to demand a degree of proof such as anthropological arguments are not in general called upon to supply. The grounds for objection to the present analysis of the Warao case are hypothetical, and until they in their turn are demonstrated to apply in fact they do not negate the significance of the figures in question. As they stand, these exhibit an interesting preponderance of symmetric over non-symmetric marriages, and they are at least consistent with the hypothesis.⁷

V

The argument for structural change, from prescription towards cognation, does not depend solely upon considerations internal to Warao society. A comparison of the Warao with certain other societies in Venezuela offers a further means of assessing the hypothesis.

Dr. Suárez cites linguistic authority to the effect that "the Warao are grouped with the Piaroa, Maco, Guahibo, Chiricoa, Yaruro, Guaharibo, and Guaica (Yanomamö) as 'independents' or 'archaics',... thus differentiating them from the Carib and Arawak speaking tribes." This appears to be a residual category, indicating that the languages are not readily classifiable by positive features, but the grouping includes a number of societies that should in any case be taken into account, and it makes a convenient start to a comparison.

Even a rapid survey of these societies discloses certain structural features which are relevant to the Warao question. Let us take the

⁶ I should gladly make this test on the cognatic Penan of the interior of Borneo, but they pay so little attention to genealogical connexions that I could not establish a useful enough number of marriages to examine in this way. My materials on the cognatic Siwang of central Malaya, who could also be a suitable test case, are too exiguous for the purpose. I do not know of any sufficiently detailed published evidence on other such societies.

⁷ It remains a problem, of course, to discover by what considerations the Warao are governed in contracting the marriages which compose these proportions. If they are not guided by a symmetric terminology, or even by any lineal distinctions in the central genealogical level, how is it that they end up to a significant degree as though they were? Presumably the lineal distinctions in the first ascending genealogical level have some significance in this respect, but one would like to know the facts.

tribes simply in alphabetical order, without introducing any factor of location or cultural connexion.

Chiricóa. These are classified as a sub-tribe of the Guahibo (Layrisse & Wilbert 1966: 225), q.v.

Guaharibo. These are Yanoama (Layrisse & Wilbert 1966: 183), or Yanomamö, q.v.

Guahibo. The terminology is not wholly consistent, but in the main it can readily be identified as a prescriptive classification based on two lines (Wilbert 1957: 91-5).

Macó. These are "a small dialect group of the Piaroa and represent a variant of the general Piaroa culture type" (Layrisse & Wilbert 1966: 209), q.v.

Piaróa. This society has an almost classic two-line terminology, with the addition of separate terms for FZSS/MBSS and FZDD/MBDD; these terms also agree, however, in their bilateral specifications, with a symmetric prescriptive system (Wilbert 1958: 171-3).

Yanomamö (Guaica). A simple two-line terminology, with the qualification that there are no distinct terms for the second ascending and second descending genealogical levels (Lizot 1971: 28-9).

Yaruro. A simple two-line terminology (Petrullo 1939: 219).

The structural type of these societies is therefore that of symmetric prescriptive alliance, and to the extent that the Warao may be connected with them it is a reasonable inference that at some time in the past Warao society also was of this type.

Among these societies it is the Yanomamö who are the most significant. Recent authorities have classified both their language and that of the Warao as Chibchan (Layrisse & Wilbert 1966: 196; cf. 199-202), and there are in addition numerous cultural features which connect them. Layrisse and Wilbert have compared a number of prominent traits in both societies and have concluded that the similarities may best be explained as "aspects of a common cultural heritage" (1966: 202-6). In addition to these features there is genetic evidence in the absence among the Waica, and the only very slight presence among the Warao, of the Diego gene (Layrisse & Wilbert 1966: 207-8). A linguistic and cultural comparison of Yanomamö and Warao seems to indicate, in the opinion of these investigators, that "several thousand years ago the Chibchan tribes of Venezuela represented a more or less closely-related Paleo/Meso-Indian group of people . . . According to our preliminary

glottochronological data, there is an internal divergence between their languages corresponding to approximately 4,000 years." (Layrisse & Wilbert 1966: 208).

With this much encouragement, let us quickly remark the distinctive features of the Yanomamö terminology. In a slightly simplified form this is as in table 2. (The terms are numbered as they are by the ethnographer, Lizot).

Table 2

YANOMAMÖ TERMINOLOGY

1. <i>šoayə</i>	MB, FZH, WF
2. <i>yəsiyə</i>	FZ, MBW, WM
3. <i>hayə</i>	F, FB, MZH
4. <i>nayə</i>	M, MZ, FBW
5. <i>šoriwə</i>	WB, MBS, FZS, ZH
6. <i>suwəbiyə</i>	W, MBD, FZD, BW, WBZ
.....	
9. <i>ēiwə</i>	eB, FBS, MZS
10. <i>aiwə</i>	yB, FBS, MZS
11. <i>amiwe</i>	eZ, FBD, MZD
12. <i>hekamayə</i>	ZS, DH
13. <i>thatəyə</i>	ZD, SW
14. <i>ihiruyə</i>	S, BS
15. <i>təəyə</i>	D, BD

It is plain that there is no immediate lexical similarity between the Yanomamö and the Warao terms, but the comparative lists of suggested cognates in their languages, drawn up by Layrisse and Wilbert (1966: 199-200), hardly support the expectation that there might be any easily recognisable agreement. Given, however, that both languages are Chibchan, the interesting point for our purpose is the structure of the Yanomamö terminology, and (as can be seen in fig. 2), this is undeniably a two-line scheme of symmetric prescriptive alliance.

There is no great puzzle about the present disparities between the two systems, or about the considerable changes that the Warao appear to have undergone. The Yanomamö remained until relatively recently hidden deep in the forests of the upper Orinoco, Ventuari, and other rivers, and for the most part they successfully resisted contact and external influence; whereas the Warao have since prehistoric times inhabited the Orinoco delta and the adjacent swampy regions, where they were far more exposed to agents of change. Nor is there in principle any real puzzle, as we can now see, about the stability of the Warao terminology since 1825, for against an estimated time-span of approxi-

Fig. 2. YANOMAMÖ TERMINOLOGY

mately 4,000 years this period is proportionally trivial.

In order to test the generalised hypothesis that these societies originally shared a two-section type of social structure it would be useful to make a comparison with another Chibcha-speaking society. The Barí are the third member-tribe of this language family in Venezuela, and these are indeed a particularly promising people since their homeland has for centuries been "a small pocket of inaccessible terrain representing a typical marginal niche of retreat, with natural resources sufficient to maintain its population, until very recently in what amounted to a beleaguered but absolutely self-sufficient fortress"; but unfortunately "as yet we know almost nothing about the social organization . . . of the Barí Indians" (Layrisse & Wilbert 1966: 162, 164). Another possible check is by reference to the Sanemá, one of the distinguishable tribal units in the Yanoama grouping, for Wilbert has written that his impression "suggested that the Sanemá have a Hawaiian system" (Layrisse & Wilbert 1966: 185), i.e., a non-linear terminology. But in his fuller ethnographical account of this people Wilbert writes that he could not obtain a list of the kinship terms, for lack of an informant, and that he had merely the impression that it was "a Hawaiian system". The only rule of marriage that he reports is that marriage is prohibited

with a "cousin", whether cross or parallel (Wilbert 1963: 211), which is unexpected, and without a more detailed record of Sanemá institutions we cannot know to what extent they fit the historical derivation proposed.

As for the other tribes in the area, it is well known that the Carib-speaking peoples typically have two-section systems. Most prominently, the Makiritare have a straightforward two-line prescriptive terminology (Wilbert 1963: 167-74). There seems not to be a published record of the Pemón terminology, but a general report on their social organisation is largely consistent with a two-section system: "Cross-cousin marriage is preferred, and marriage with a niece through Ego's consanguineal or classificatory sister as well as through cross-cousins of either sex is allowed . . . Kinship is bifurcate-merging and of the Iroquois type . . ." ⁸ Of the Cariña, finally, it is reported that "the kinship system is of the Iroquois type, and cross-cousin marriages seem to be preferred among the Cachama sub-group" (Layrisse & Wilbert 1966: 93). Since these are the typological labels by which Wilbert usually characterises relationship terminologies which are certainly symmetric, with a bilateral prescription, the inference is that these Carib-speakers may also have a two-section system.

For the present, we may conclude that the distribution of two-section systems in Venezuela, and in particular the clear case of the related Yanomamö, indicates that Warao society also was probably of this type.

VI

Among many other interesting matters raised by the analysis of this society there is one that I should much like to learn more about. This is the relation, which has a special relevance to the hypothesis, between age and category.

In an essay on the significance of relative age I have argued that in lineal descent systems categories are preponderant over age, whereas in cognatic societies the factor of relative age is preponderant over categories, in the conduct of social life (Needham 1966a). In the course of the argument the incidental proposition is advanced that marriage between genealogical levels may tend to blur or preclude social distinctions between the categories affected; and I have suggested also that, where status is determined by category, marriage with a recognised relative will be confined to the subject's own genealogical level. The

⁸ Layrisse & Wilbert 1966: 83. The reference to cross-cousins of "either sex" is inconsistent. Perhaps cross-cousins of "either side" is intended.

main contrast established is that between the absolute nature of relationship categories in a lineal descent system and their more contingent character in cognatic societies.

To the extent, therefore, that Warao society has changed from prescriptive to non-prescriptive marriage, and from lineal to cognatic organisation, one may postulate that the relationship terms, as jural classifiers, have tended to decline in importance and to give way to the factor of relative age. The contrast between linearity and cognation in the constitution of the terminology makes this a particularly interesting question, and the limitation of marriage to the central and first descending genealogical levels adds to this interest. We cannot tell from Dr. Suárez's monograph how far the lineal and other distinctions have an absolute force in Warao life, but we do know from table 19 that the great majority of marriages are contracted within the subject's genealogical level. This suggests that the categories may retain a significance such as is correlated (according to my argument, at any rate) with a lineal system, and if this can be shown it will be a further support to Dr. Suárez's hypothesis.

A related issue in the study of Warao society is posed by an argument that I have made in another place. In an analysis of the contrasts between terminology and alliance, in certain selected cases, I have suggested that prescriptive systems tend to change in regular ways. "The postulated tendency is from the simple to the complex, from system to arrangement, from prescriptive to non-prescriptive, from categorical to preferential, in a continuing process of increasing latitude and individuation..." (Needham 1967: 46). To judge by the evidence presented by Dr. Suárez, this may be the course that Warao society has followed, and in this connexion also it would be most interesting to learn more about the significance of the relationship terms as effective categories of social classification. The general interest of the Warao case, in this regard, is that it may serve as an example of a common process in the evolution of social systems, namely from the absolute to the relative regulation of social conduct. One marked feature of systems of prescriptive alliance is that they are examples of absolutism, and a chief lesson of their changes into non-prescriptive types of organisation is that they exhibit the supersession of categorical constraints by more circumstantial factors (Needham 1973, sec. IV).

On these grounds, it seems to me, an analysis of the comparative importance of categories and of relative age in Warao society could prove well worthwhile. I have invented a simple technique for plotting

the vectors, and also for calculating the index of overlap among individuals as classified by genealogical level and by age (Needham 1966a: 14, 18). These procedures, I should like to suggest, might profitably be applied to the subjects whose marriages are treated in this monograph. The results could show more clearly the real criteria of Warao social classification, and they would thus indicate something of the degree to which the Warao may still regulate their affairs by the absolute determinations of a prescriptive system.

VII

After Dr. Suárez had completed her work at Oxford there appeared an article by Gertrude Dole in which the Warao type of terminology was isolated as a topic for comparative study. The argument was historical, and suggested a causal explanation for the non-linear features in the central genealogical level (Dole 1969).

Dole refers to the type as a significant variant of generation nomenclature. "This variant combines generation cousin terms with bifurcate merging avuncular and nepotic terms. It differs from the bifurcate merging pattern only in that the terms for cross cousins are the same as those for siblings and parallel cousins." She proposes that this pattern of terminology, which she names "bifurcate generation", is causally related to "kin group endogamy, which in turn is associated with demographic disturbances and disruption of the alignment of kin relations." The factors at work are to be observed, she states, among the Carib-speaking Kuikuru of central Brazil, who are in the process of adopting this type of nomenclature. The Kuikuru state some preference for unilocal residence, local exogamy, and bilateral cross-cousin marriage; but in practice their society is characterised by ambilocal marriage, variable cognatic groupings, and permissiveness in the choice of spouses. In particular, "the Kuikuru do not reckon descent but only filiation." These disparities between received ideas and actual behaviour are argued by Dole to be results of relatively recent changes.

The indigenous population of the upper Xingu has been reduced by disease to less than one-third of what it was sixty or seventy years ago. Several of the tribes have been so diminished that they could not survive independently, and the Kuikuru settlement contains individuals from at least four such tribes. Since the Kuikuru had married into these groups, the amalgamation meant that locally exogamous unions became endogamous. This has had the consequence that cross-cousins grow up as members of the same group. "In this way local endogamy tends to

blur distinctions between affinal and consanguineal kin that are made on the basis of residence and group affiliation." Lacking other "devices to differentiate affinal from consanguineal relatives . . . growing up in the same settlement will make cross cousins virtually identical with parallel cousins and siblings in their social relations." Concordantly, most Kuikuru now refer to cross-cousins by the same terms as are used by parallel cousins and siblings, while only a few older people still employ a separate term for cross-cousin. This "vestigial" cross-cousin term indicates, in Dole's opinion, a former "bifurcate merging nomenclature". The Kuikuru still use, however, "bifurcate avuncular and nepotic terms", and this is explained by their continued practice of some cross-cousin marriage.

The terms actually employed by the Kuikuru are not reported in the article under discussion, and the descriptive vocabulary on which the author relies is not precise or systematic, but this case is clearly of special relevance to the interpretation of Warao society. In fact, in her survey of terminologies elsewhere in the world Dole states that "non-exogamous kin groups and bifurcate generation nomenclatures are commonly found together in the tropical lowlands of South America", and as one of her examples from that area she refers specifically to the Warao. They have, as she says, a terminology of the type in question and they have no exogamous descent groups. The issue, then, is whether her argument about the Kuikuru applies to the Warao, namely that "it is precisely the marriage of cousins in an endogamous group that initiates a change to generation cousin terms."

Without denying a possible correlation between the type of terminology and the lack of exogamous descent groups, I must say that I am not really convinced by the argument. It is by no means clear why cross-cousins who live in the same local group should thereby become "virtually identical" with parallel cousins. It is true, by the very definition of the situation, that distinctions between kin and affines which were previously marked by residence and group membership will no longer obtain under local endogamy; but this does not explain why lineal distinctions among cousins should be given up while those in the adjacent genealogical levels are retained. There is in any case no inherent contradiction between co-residence and the efficacy of categories, whether consanguineal or affinal. Whatever the causes of the endogamy, therefore, and whatever the composition of the resultant group, there is no evident reason to accept that local endogamy is the cause of the terminological change. As for the Warao, furthermore,

endogamy among them means no more than that they "prefer to marry women from the area surrounding the river where they were born" (Suárez 1971: 95), and there is no reason to think that the criteria of relationship among such a dispersed population would tend to become blurred by this preference. On the other hand, the evidence recorded by Dr. Suárez demonstrates how difficult it is to maintain that the lineal ("bifurcate merging") features of such a terminology are explained by the continued practice of "some cross cousin marriage".⁹ In a case of this kind, which is defined precisely by the lack of categorical distinctions among cousins, the first problem, as is shown by tables 19-23, is to determine just what is a "cross-cousin".

The argument presented by Dole has also been found insufficient by Ellen Basso, who has herself conducted field research on the upper Xingu, among the Kalapalo.¹⁰ To put her carefully detailed case very briefly, she reports that the Kalapalo employ a classification which combines two sets of terms: when referring to individuals as "kinsmen" the Kalapalo will select terms which are differentiated primarily by genealogical level; whereas when referring to affinal status they select from another set of terms. To consider only the former gives the impression that lineal distinctions are not made in Ego's genealogical level; but to consider the classification under the aspect of alliance brings out a straightforward two-line prescriptive scheme. There are conventions governing the situations in which one set of terms or the other may be used; and if a distinct term for "cross-cousin" is only rarely heard the reason may be, not that the term is vestigial, but that the ethnographer is less often in the situation to which that term is appropriate. Among the Kalapalo, too, there have been drastic changes in the size of local groups, with considerable amalgamation of villages, such that marriages between members of different groups living in a single village take place with greater frequency. But Basso reports that "these marriages appear to be formed along the same principles as those which occur between members of a single village group or between members of different village groups living apart." In other words, although local endogamy may have become more prevalent, there is no evidence that the means by which marriages are contracted have changed. Not only this, but there are definite indications that a period of serious population

⁹ Dr. Dole does not provide any figures from the Kuikuru in support of this explanation.

¹⁰ Basso 1970. I am grateful to Dr. P. G. Rivière for directing me to this highly pertinent paper.

decline in this system leads eventually to increased exogamy.

In these respects Basso makes a very cogent case, and on such grounds also Dole's argument appears considerably weakened. However, Basso's own argument, telling and subtle though it is in its application to the Kalapalo, has only limited theoretical implications, mainly because it deals with certain cultural details among this people which may not be paralleled among other peoples whose terminologies possess the non-linear feature in question. Moreover, the Kalapalo do make consistent distinctions in the central genealogical level when they are concerned with affinal alliance, whereas in the Warao and other cases it is precisely the ascertained absence of such categorical distinctions that poses the problem. More generally, although the Kalapalo example controverts Dole's argument, it does not disprove it. Dole contends that "bifurcate generation" terminologies are transitional forms that follow disturbances in the size and composition of kin groups, and that the pattern may be associated especially with the breakdown of exogamous descent groups and a tendency to local endogamy. These propositions depend on a large comparative study, and Dole adduces cases from all over the world (North America and Oceania, as well as South America) in support of them. Clearly, one negative instance in the case of the Kalapalo (or two, if we count the Warao as negative) will not suffice to test a comparative argument of this kind.

For that matter, it is a methodological question whether the Kalapalo should even be admitted to the comparison. The contrasting principles of order that Basso so well reports are, after all, not peculiar to any type of classification or social structure. It is, on the contrary, a common fact that different principles of classification can be employed according to context, and in many different types of system.

Among the cognatic Penan of Borneo, for instance, alternative appellations are possible according to the means chosen, and it often happens that Penan "will state or claim one relationship in one situation and another in a different situation" (Needham 1966a: 7-8). On Sumba, the Mamboru, who practise asymmetric prescriptive alliance, can resort to two different principles in classifying non-agnates: either they trace genealogical connexions and end up with a denotation in a definite genealogical level, or they can make a connexion by reference to affinal alliance between descent groups and will end up with a denotation that overrides distinctions of genealogical level.¹¹ Among the Purum, who

¹¹ Field enquiries, 1955.

also have an asymmetric prescriptive system, certain persons can be addressed either by a specifically jural term according to categories of alliance, or by a general term of civil discourse which takes account only of age (Needham 1966b: 173-4). The Kodi, of western Sumba, who do not have a prescriptive alliance system but a bilineal descent system, can similarly classify individuals either by the strict determination of category or by relative age, according to the situation (Needham 1966a: 16).

Although it is intrinsically most interesting, therefore, that the Kalapalo classification rests on two contrasting principles, applied differentially according to the situation, it is neither singular nor surprising. The clarity of the contrast in this society sharpens the methodological question of how the Kalapalo are to be treated in a comparison, but it does not make a special theoretical case. On the point of method, the Kalapalo terminology can be counted either as belonging to Dole's class of test cases, by one principle, or as not belonging to it, by the other. Since we are dealing with a problem that has to do essentially with categories in connexion with marriages, I should be inclined for this present purpose to treat the Kalapalo terminology as an instance of a consistently symmetric prescription, which would remove it from the scope of Dole's comparison. But of course this is a debatable question of definition, not an issue decidable by further analysis. The analytical significance of Basso's paper lies, rather, in her demonstration of the extent to which a form of classification must be mastered, in its social setting and various employments, before a comparison can be attempted.

This much readily granted, there yet remains the general problem of accounting for terminologies of the type that we find among the Warao. The similarities with consistently two-line schemes are too marked to be adventitious, and the incidence of non-linear central levels indicates a regular process of change. An evolutionary explanation is at least plausible, and formally speaking it is an obvious line of conjecture. Moreover, it is not a modern suggestion by Dole, Suárez, or myself, but it has been proposed over half a century ago by one of the most brilliant students of social classification and institutions.

Rivers, in 1914, proposed that in Oceania there are two main varieties of social organisation: in one, marriage is regulated by clan exogamy; in the other variety, marriage is regulated by genealogical relationship. "Between the two are numerous intermediate cases, and the series so formed runs so closely parallel to that representing *the transitions between different forms of the classificatory system*, that it seems out

of the question but that there should be a relation between the two" (Rivers 1914: 68; *italics mine*). Later, in 1920, Rivers actually isolated the very type of "classificatory system" that we are dealing with, and suggested its place in a scale of social evolution. He takes as his basis what he regards as "the most characteristic form of the classificatory system", in which "the children of those classed with the mother's brother and the father's sister are distinguished in nomenclature from the children of those whom the father calls brother and the mother calls sister." These relatives thereby fall, he says, into two groups: that is, the classification in question is that of a two-line terminology.

Immediately after establishing this type, he turns to "a variation from this system . . . which is of not infrequent occurrence." This is a relationship terminology in which, "although the mother's brother and the father's sister are distinguished from the father's brother and the mother's sister, their children are all classed together." Here we thus have, clearly identified by formal criteria, the precise type of social classification that constitutes our problem.

In its turn, next, this variation "seems to be only an intermediate step towards a form of the classificatory system in which the father's brother is classed with the mother's brother and the father's sister with the mother's sister," in other words a non-lineal terminology found in a form of society "now usually known as the Hawaiian system" (Rivers 1926: 59-60). Decades ago, therefore, Rivers discriminated the type of classification now reported from the Warao by Dr. Suárez, and he located it within a series of transformations from a two-line terminology to a non-lineal terminology, a series functionally related to social changes from a system of exogamous descent groups to cognatic forms of organisation. These hypotheses appear to receive further support from the present analysis of terminology and alliance among the Warao.

Finally, however, there is the additional analytical factor, which is brought out by Dole's comparison, that this type of terminology is not distinctively correlated with symmetric systems of prescriptive alliance. The lack of lineal distinctions at Ego's genealogical level is found in many forms of otherwise lineal terminology, and not only in association with a two-line scheme of classification. It is common, notably, in the plains area of the western United States, where there are no prescriptive systems, and it is reported also from the northwest coast. What we are faced with, therefore, is not a process relating to the study of prescriptive alliance alone, but a much more general kind of change which cannot be elucidated simply in terms of prescriptive systems. The special interest

of such systems is, rather, that their elementary and absolute character makes them more readily comparable, and thus particularly instructive, in the approach to a more general explication of the erosion which is so frequently (and apparently inevitably) undergone by lineal classifications. It is under this aspect that the present study of Warao society has, in its detail and novelty of technique, a marked value.

VIII

I hope these comments will have brought out some of the many points of interest that are to be found in Dr. Suárez's examination of Warao society. It is a system that will repay much further study, and the present edition is a welcome means to that end.

It would overburden a mere foreword if I were to set out at any length certain other points that attract attention, but perhaps I may just mention a few. One is that the Warao case shows something of the feasibility of making historical reconstructions, of conjecturing the course of evolution in a social system over a long and unrecorded period. This is a kind of enterprise that has fallen very much out of favour in social anthropology, largely perhaps under the influence of Radcliffe-Brown's dictum that "pseudo-historical speculations . . . are not merely useless but are worse than useless" (1952: 3). The contention is that "the acceptability of a historical explanation depends on the fullness and reliability of the historical record," so that when such records are lacking any conjectures about the past are merely pseudo-historical. But, as Hocart cogently argued, this view is based on a defective and self-defeating conception of evidence, according to which only direct testimony can count, and of the means by which past events or states can reasonably be inferred even when we have no positive knowledge of what they were (1970, chap. 1; cf. lxx-lxxxi). It is to be hoped that Dr. Suárez's argument will at least induce some who are inclined to adopt that dogmatic view to consider again whether structural reconstructions are really useless. In particular, I trust that few will any longer accede without argument to Fortes's abrupt assertion about Rivers that "his basic hypotheses were absurd" (1953: 26). There is nothing in the least absurd about the hypotheses that societies change, that classifications and jural institutions tend to change concomitantly but at an interval, and that prior stages may thus sometimes be inferred from present formal inconsistencies. On the contrary, each one of them is internally valid, possibly true in fact, and capable of empirical test.

Indeed, a resumed attention to what were Rivers's concerns and hypotheses, on the basis of modern ethnography, gives them a demonstrable cogency which should only encourage further work of the kind.

The method by which evolutionary reconstructions may best be carried out is another matter. Dole has suggested that the causal factors in the emergence of this kind of terminology are depopulation and a consequent local endogamy. But, quite apart from their apparent irrelevance to the Warao case, it is a question whether a general elucidation of the form of classification is to be arrived at by resort to such imprecise contingencies or to so few of them. Relationship terminologies are remarkably simple and compendious constructs, but the social interests and pressures to which they respond are on the contrary exceedingly complex and disparate. It is therefore not probable *a priori* that the typical causes of change in a form of social classification are to be located in the correlation of a small number of factors which are common to all instances of such change. It is far more probable, I think, that many quite various causes may have led, in different societies, to classificatory changes which have assumed common forms.

We may better appreciate this theoretical alternative by considering the several setbacks that social anthropologists have met with in their repeated attempts to explain, by one institution or another or by a combination of institutions, the common origin of some particular type ("Iroquois", "Omaha", etc.) of terminology. I have argued elsewhere that comparison in terms of the conventional substantive typology are ill-founded in principle and misleading in practice, and that a more valid procedure is to isolate the institutional correspondences with logical possibilities. In doing so, I have maintained, we shall take as our bearings those very constraints which must have been operative in the fabrication of the forms of classification under study (1971: 11). In the same vein, I now add the rider that innumerable particular factors may have contributed to the formation of any distinct form of classification. Thus many quite disparate interests and influences, incomparable from one society to another, may have contributed to the formation in each instance of what we recognise as a two-line terminology. It is undemonstrated, and at first sight improbable, that common social factors or intentions should have led to the adoption of such terminologies in South America, the Himalayas, South India, Melanesia, and Australia. But it is wholly conceivable that men with greatly disparate concerns should in similar ways have taken advantage of elementary possibilities in the formation of the schemes of categories

by which they govern their lives. To a degree, indeed, they must have done so under certain constraints, and these are the obverse of the classificatory latitude which has resulted in the considerable variety which first strikes the student of relationship terminologies. Whether we phrase the issue as the exploitation of possibilities or as a subjection to constraints, the crucial point is that very unlike social factors can produce like forms of classification. Thus, in the case of the type of terminology exemplified by the Warao, it is quite conceivable that a great many various and possibly unique factors (not only depopulation, abandonment of descent group exogamy, or local intermarriage) may have been responsible for the wide incidence, which Rivers already called "not infrequent", of this form of classification in so many different societies around the world. The decisive factors, I suggest, have been, not particular empirical circumstances or legislative motives, but general possibilities and constraints of a purely formal nature.

If this is so, it makes the more feasible the reconstruction of evolutionary stages. In this connexion the Warao case has a special relevance to the scheme of evolution that I have outlined for systems of prescriptive alliance. In the series of changes that I have proposed the successive types of systems are: symmetric alliance — asymmetric alliance — bilineal descent or cognation.¹² The analysis of the Warao system, and the wider consideration of societies with which it is terminologically comparable, suggests that on the symmetric side we should allow also for the transformation of a two-line classification,¹³ via a Warao ("bifurcate generation") type, into a consistently non-linear terminology.

Analytically, too, there is a further interest to the Warao case in the means by which we are led to conceive the problem. When analysing the Garo system the problem was constituted by the disparity between a consistently symmetric prescriptive classification and a regularly asymmetric contraction of affinal alliance; and these conditions were largely replicated in the Manggarai and Endeh systems as well (Needham 1967; 1968; 1970). But in the Warao case the issue of possible evolution has been posed by the terminology alone, and only subsequently — in the

¹² Needham 1967: 45-6. Dr. D. B. Hicks has proposed, on the basis of his researches in Portuguese Timor, that one might also include a change from asymmetric alliance to simple (non-prescriptive) lineal descent. This is correct, and in a full treatment of the matter should certainly be done. In the place cited, however, I included provisionally only those forms for which I thought I could at that time provide empirical substantiation.

¹³ Not only this elementary form of symmetric classification, of course, but also a four-line symmetric scheme such as that of the Dieri (Korn 1971).

light of the hypothesis itself — do the marriages acquire a diagnostic significance.

Finally, underlying the many intriguing aspects of the study of Warao society, there is a fundamental problem that we have not the evidence or the theoretical resources to solve. This study is concerned with change, and specifically with a postulated concomitance between changes in social classification and changes in modes of conduct. Men conceive their social lives in terms of the categories imposed on them by their cultural tradition, and they regulate their conduct under these categories by explicit rules. In a prescriptive system especially there is an absolute categorical determination which is hard to evade or to change. The invariance of the constituent relation in the terminology, together with the "total" character of the classification in its application to matters far other than descent and alliance, tends towards conservatism. Yet prescriptive systems do change, and the problem is how precisely they do so. The crucial issue is the extent to which individuals make conscious alterations and adjustments; for the more deliberately they are supposed to act the more striking it is that their cumulative decisions should result in a common type of transformation.

In this regard, though, we have practically no detailed information, and certainly not on such a scale as might permit general propositions about the process. For that matter, there are exceedingly few intensive studies of any mode of terminological change, and it is a further merit of Dr. Suárez's monograph that it may conduce to a resumption of work on the topic (cf. Eggan 1937; Spoehr 1947; Faron 1960). In that event, it is to be hoped that detailed information may be supplied on the individual instances and occasions of change. It is often possible, in retrospective analyses, to discern such institutional factors of change as missions, schools, and government agencies; but, if we are to understand the process, we need particular reports on how individuals initiate, maintain, and propagate the numerous modifications which accumulate into a general transformation. To that end, it is necessary that social anthropologists should more widely take up the problems of structural change, and it is in this enterprise that Dr. Suárez's work claims a particular attention.

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