

C O M M E N T A R Y :
(REJOINDER TO BARNES)

THE COMPARATIVE METHOD IN SOUTHEAST ASIA:
IDEAL AND PRACTICE

It is a pleasure to begin my rejoinder to 'The Leiden Version of the Comparative Method in Southeast Asia' (*JASO* Vol. XVI, no.2) with a few words of appreciation for the way in which its author, Dr R.H. Barnes, has written this review article on *Unity in Diversity* (P.E. de Josselin de Jong, ed., 1984). Instead of reviewing each of the twelve chapters (several of them double chapters, by nineteen authors altogether) separately, he has given a general survey of the 'Leiden version', i.e. the Field of Anthropological Study (FAS) approach and its history. He has done so with expertise and fairness. This means that, on the whole, I can confine my present reply to Barnes by giving some additional information and introducing some nuances in order to clear up possible misunderstandings.

Such misunderstandings can easily arise, since Barnes and I are discussing publications by Indonesianist anthropologists, mainly associated with Leiden University, who together do not constitute a monolithic 'FAS' school, let alone propound a dogma. Besides individual differences of interest and opinion between members of the FAS group, one has to take the time dimension into account. The FAS programme was launched in 1935, and in Leiden as elsewhere anthropological and philological research carried out during the past half century is marked not only by continuity, but also by change. In fact, discontinuity is a prominent factor in the FAS group's history. As Barnes remarks (p. 88), Indonesian studies in the Netherlands were in the 'post-colonial doldrums' during the war years, the 1950s and early 1960s, before starting afresh with largely new personnel. In addition we must bear in mind that, then and now, no research ever was or is exclusively, or even principally, directed towards the FAS programme. The FAS approach supplements the researcher's field and/or library research

on one or a few Indonesian societies. On the one hand, the anthropologist may consider whether data obtained elsewhere within the FAS can shed light on apparently inexplicable features of the society he or she is studying (what I have called the 'mutually interpretative' approach); on the other, data from his society can be put forward as local or regional variants of a phenomenon of common occurrence in the FAS. In other words, a FAS study is not autonomous and hardly ever an end in itself.

In the following pages, I shall first deal with some specific points (most of them factual, and some of them matters of detail); we shall then turn to two topics which involve more general problems. The first of these is the study of kinship and marriage in the FAS, which brings us to the relation between models and empirical data; the second, the validity of 'Indonesia' as an anthropological and linguistic concept in connection with the 'ideal' and 'practice' mentioned in the title of the present article.

Following the page sequence of Dr Barnes's article, we shall first offer a few *addenda et corrigenda*.

'Blust's claim', referred to on p. 93, is not quite fair to H. Kern, who did more, in the field of comparative linguistics, than placing his Old Javanese studies in the Malayo-Polynesian context. See, for example, the 500 pages devoted to comparative Austronesian linguistics in Volumes IV, V and VI of his collected writings (H. Kern 1916, 1917). We shall return to Blust's main point, i.e. that Dutch scholars tended to define the scope of comparative Austronesian linguistics 'in political terms', towards the end of this paper.

On the following page, Barnes turns to the FAS's two progenitors: we are given a brief critique of the work of F.A.E. van Wouden (following James Fox) and J.P.B. de Josselin de Jong. Both require a few comments. Van Wouden's *model* (my italics) of a two-phratry system in conjunction with marriage classes, matrilineal marriage and double unilineal descent has not been confirmed by any of the ethnographers who contributed to *The Flow of Life*. True enough, so we must enquire whether this undermines the very basis of the FAS effort as it is still being pursued today. In the first place, we must not forget the time gap already mentioned above: it is most unlikely that any anthropologist anywhere would build up an argument in the manner Van Wouden did fifty years ago. Nor would there be need to do so: Van Wouden's Ph.D. thesis was the result of library research, using 'defective data'. 'The gaps in our knowledge are nearly all the result of highly superficial descriptions or sheer lack of data' (Van Wouden 1968: 1, 85). It is of greater importance, however, to note that even the critics' objections to Van Wouden are not entirely relevant by present-day standards. The elements of social organisation towards which the criticism is directed are presented in a concentrated form in Van Wouden (*ibid.*: 90-4) - but what is our author's aim with this passage? Not to describe any or all of the societies of eastern Indonesia, but to form a model which explains the concepts he is

using (and which will be unfamiliar to most of his readers), and to demonstrate their relationship to each other. He does not claim that all the elements of his model are to be found, as a coherent whole, in any single culture of his area, let alone in all of them, for he stresses what is culture-specific:

Although we shall be dealing primarily with the social factors of these cultures,...it should be kept constantly in mind that these social phenomena are firmly rooted in the totality of the culture. We intend, therefore, not to detach the social component arbitrarily from the culture.... (Van Wouden *ibid.*: 1).

Or, as Locher (1968: ix, x) expresses it more clearly:

An important aspect of analysis by means of models, as exemplified in Van Wouden's argument, is that diverse possibilities can be implicit in the model, not all of which will necessarily be realised in empirical phenomena; that is, they may be present but remain in a latent state The extent to which such possibilities may be realised in the form of distinct social groupings in empirical societies is of course a question for closer research in the field.

Whether a model with the properties of Van Wouden's is an acceptable tool for research has been discussed in *Unity in Diversity* (pp. 241-5), so we need not deal with this question here.

Turning from Van Wouden to J.P.B. de Josselin de Jong, Barnes objects that his 'structural core' model is heterogeneous, as it includes the Indonesian resilient manner of responding to foreign influences, and this 'though important in Indonesia...must be given careful consideration everywhere' (p. 94). Here we touch on a misunderstanding of the purpose that the FAS approach is meant to serve. It was never designed to demarcate the field by drawing up a list of distinct features which typify it, in the manner of the North American 'culture area' studies, but to bring to light features which are so frequently recurrent in the field that they give a family likeness to the cultures within it. I grant, however, that the feature of 'Indonesian resilience' is much less specific than the others in J.P.B. de Josselin de Jong's model.

Barnes states on the same page (94) that the basic points of Van Wouden's social system model had 'already been discovered and demonstrated by Fortune (1933), though neither Van Wouden nor J.P.B. de Josselin de Jong refers to this paper, so directly related to their own model'. Here we should remark that J.Ph. Duyvendak (1926: 124-8), using material from Seran, had preceded Fortune in discerning a structure made up of 'unilateral marriage relations' and phratries. This early work was of direct relevance for Van Wouden, as he himself points out (1968: 8, 153).

In Dr Barnes's review article, I think pp. 96-8, which give a concise survey of studies on asymmetrical connubium systems, are particularly useful, as are pp. 100-2 on what we might call numer-

ical classification: 4-5 groups, 8-9 groups, etc. To the examples given on these pages, one could add (e.g.) the *pata*-5 and *pata*-9 groups of Seran, discussed by Duyvendak (1926), and the *waktu*-3 and *waktu*-5 of Lombok.

To conclude the present section, we must give attention to a few more fundamental matters, which will also lead us into the two following sections.

On p. 102, Barnes mentions that, in the volume under discussion, I expressed my doubts about the utility or status of J.P.B. de Josselin de Jong's notion of four 'structural core' elements in the FAS (viz. asymmetric connubium, double unilineal descent, socio-cosmic dualism and the resilient response to foreign influences referred to above). Barnes wonders whether my preference for a notion to be called 'basis for comparison' is 'anything more than a shift of metaphor'. It certainly is. A core is a marked element or set of elements: *this* is the core, the other elements are marginal with respect to the core. A basis, on the other hand, is not exclusive: it may be expanded, and the phrase refers to the beginning of an enterprise. The relevant glosses in the *Concise Oxford Dictionary* are: '...foundation; beginning; ...common ground for negotiation, etc.' Contributors to the *Unity in Diversity* volume propose or use, for instance, the following basic elements for comparison: the fabrication, use and design of textiles (Sandra Niessen in Chapter V), boat symbolism (Leontine E. Visser in Chapter X), social classification terms (Rodney Needham in Chapter XI) and, published elsewhere, certain themes in political myths (P.E. de Josselin de Jong 1980).

I therefore entirely agree with Barnes: 'In addition to the themes of descent and alliance, or their absence, there are further comparative factors requiring consideration' (p. 103). This is precisely what is being done by present-day members of the FAS group. We 'work with an expandable list' (p. 104) - and this is why I personally prefer not to use the notion of a 'core'.

As we are continuing 'to work with an open-ended set of comparative themes' (p. 103), that is to say, carrying out a programme, I found Barnes's rather disparaging remark on the same page unwarranted: 'There is something unusual about a fifty-year-old programme that has not yet constituted itself into a synopsis of proven results'. A historian of the FAS effort could compile such a synopsis, if he wished, but this is not the aim of its present practitioners. As is the case with other past and present 'isms' in anthropology, FAS research does not strive to reach the 'finish': it can be carried on indefinitely, using certain procedures and putting certain questions to the data.

On the FAS enterprise itself, Dr Barnes writes (p. 104): 'That it ought to be tenaciously defended and preserved at all cost now, I doubt'. I hope the foregoing pages have given some indication that those who adopt 'the Leiden version of the comparative method' do not have such a hidebound attitude towards their predecessors' or their own effort. We are modifying it when necessary. In fact, the very aim of the symposium which led to the *Unity in Diversity* volume was to elicit critical response from fellow anthropologists

whose outlook differed from our own. Of how many symposia can this be said?

In this section, which is concerned with descent and connubium, my principal aim will be to clarify what apparently are obscure passages in our book. However, I can hardly be blamed because I do not define (p. 94) or spell out (p. 95) what I mean by 'idea principles' as opposed to 'rule principles'. On p. 7 of *Unity in Diversity* I refer the reader for such a definition to Moyer (1981) and remind him of this reference on pp. 242, 248 and 249. In the present context, however, I think the term can best be elucidated by seeing how it is applied in a specific case, namely the social organization of Sumba.

The original 'structural core' model contained, as we have seen, double descent and asymmetric connubium, but when Van Wouden came to carry out fieldwork in Sumba, he encountered the 'empirical difficulty' that this descent type proved to be 'characteristic only of Kodi in the west, while asymmetric marriage alliance is practised in the east', i.e. in the region named Mangili (Barnes p.95; cp. *Unity in Diversity*: 6; Van Wouden 1977: 218; Onvlee 1977). This is where, at a later stage, I applied the 'idea principle'.

We then see that the principles of social organization occurring in west and east Sumba should not be placed in a simple opposition to each other, as diagrammed below:

<i>West</i>		<i>East</i>
+	double descent	-
-	asymmetric connubium	+

As for descent, we see the matrilineal principle playing an important role in apparently patrilineal east Sumba, as those claiming membership of the highest nobility validated their claim by means of pedigrees which, except for the most recent period, only reckoned with matrilineal descent. That is to say, it is correct to call east Sumbanese society unilineal (*in casu* patrilineal), but only if we treat patrilineality as a monothetic (Needham 1975) concept, defined by *our* criteria of having only named and organized patrilineal groups. As soon as we take the social participants' 'ideas' into account, however, we see that the matrilineal *principle* is recognized, although it is not given the form of organized matrilineal *groups*.¹ The other half of the island, and the other social

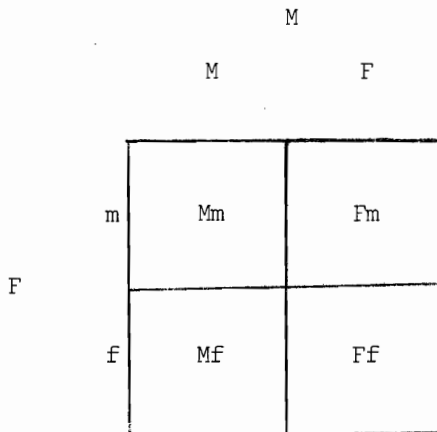
¹ In eastern Sumba, the negotiations leading to a wedding are accompanied by gift exchange of a type that is not unusual in the FAS: the (prospective) bride-takers give golden ornaments and horses, the bride-givers textiles. This looks like the familiar pattern: the male side gives masculine, the female side feminine goods. But 'the livestock must be both one stallion and one mare', while the counter-gift of four cloths is specified as a man's dress,

feature with which we are concerned, are no less enlightening. His fieldwork in Kodi, in the extreme west of the island, led Van Wouden to the conclusion that there are no connubial arrangements in that area. This is true for Kodi, but discussing western Sumba as a whole, Needham (1980: 34, 35) sums up the situation in these words: '...there is a scale of transformations from asymmetric prescriptive alliance, exemplified by Mamboru in the east [i.e. the east of western Sumba], to the bilineal non-prescriptive system, exemplified by Kodi in the west'. One point of interest is that in Needham's table of social features in sixteen 'domains' of west Sumba, kinship terminology is a prominent feature, as might be expected - and terminology, as a cognitive system, is of direct relevance for the 'idea principle'. Of no less importance is the conclusion that the west Sumbanese societies show a 'scale of transformations'. This is precisely in accordance with the concept which present-day FAS anthropologists have adopted, instead of the search for resemblances which occupied their predecessors (see, for example, *Unity in Diversity*, p.4) - and which, I may add, is still often attributed to the present-day group by their critics.

After this lengthy passage, I shall be brief on the other points concerning social organization raised by Dr Barnes. First, one final remark on idea principles. That 'rule principles should be entirely distinct rather than one kind of manifestation among many of idea principles seems doubtful' (Barnes, p. 95). This cannot be a criticism, as we never expressed that 'doubtful' opinion. On the contrary, we entirely agree, although we would prefer to say that the idea principle may, but need not, become manifest as a rule principle (e.g. participants' idea of two descent lines in their society may or may not be manifest in the occurrence of two organized types of descent groups).

On descent, and double descent in particular, I must ask the Editors of the *Journal* for still more space and the readers for still more patience, as Dr Barnes's review raises three more ob-

a woman's shirt, a masculine loincloth and a feminine headscarf (Onvlee 1977: 157, 158). The structure is in accordance with a 'double descent' model:



jections to the 'Leiden authors'' use of the term. They use it 'even when its features are relevant only for a segment of a community' (p. 95); we are not clearly distinguishing 'between double descent and cognatic kinship' (ibid.); and we seem to be rejecting the concepts of parallel and alternating descent (p. 96).

The answer to the first objection is: that is so. The east Sumbanese claimants to highest nobility status are members of their society as a whole, so when they keep pedigrees showing their noble ancestry in the matriline, this indicates that in this overtly patrilineal society, the matrilineal descent principle is recognized, and can be adduced as evidence to serve the purposes of a particular individual or group.

This same case can also be cited to show that we do distinguish between double descent and cognatic kinship: a Sumbanese wishes to demonstrate his membership of a discrete set of persons defined by *lines* of descent, and not that he is 'a member of several similar-purpose groups at the same time', as is the case when we are dealing with cognatic descent groups (R. Fox 1967: 147). The *linear* principle is basic in the Sumbanese case as well as in the Toba Batak case and in all others which have been used to prove the recognition of both descent principles.

That we do not discount parallel descent is proven by *Unity in Diversity* (p. 249), which refers to page 7 for 'a few Indonesian cases', and summarizes: 'the latent or alternative line...resembles a single-sex line of the kind which is basic in systems of so-called parallel descent'. It is true, though, that I failed to explicate my reasons for subsuming parallel descent under the more inclusive category of double descent. To remedy this, I now propose the following taxonomy:

- | | |
|--------------------------|-------------------------|
| 1. non-linear (cognatic) | 2. linear |
| | 2.1. unilinear |
| | 2.2. double-unilinear |
| | 2.1.1., 2.1.2. |
| | 2.2.1., 2.2.2., 2.2.3., |

where 2.1.1. is matrilinear, 2.1.2. patrilinear,

- | | |
|--------|------------------------------------------------|
| 2.2.1. | 'true', organized double descent, ² |
| 2.2.2. | parallel descent, |
| 2.2.3. | alternating descent. |

Finally, we pass from descent to connubium. Barnes (p. 98) notes correctly that I now prefer the term 'asymmetric' to 'circulating' connubium. My reasons, however, differ from his and resemble those for including systems 2.2.1., 2.2.2. and 2.2.3. under the

² For clarity in this diagram, I distinguish between double unilinear and ('true') double descent, but in the text itself I also call the more inclusive category 2.2. 'double descent'.

heading 2.2. in the above taxonomy. Asymmetry is a fundamental feature, standing in opposition to symmetry. On the other hand, it is a feature of some societies that the asymmetric connubium is also circulating: see, for example, Renes (1977) for such a case, and Salisbury (1956) for an asymmetric system which is not, and cannot be, circulating.

It is satisfying to note that Barnes (p. 89) 'agree[s], naturally, with de Josselin de Jong's rebuttal to Leach'. Coming from British social anthropology, even such an *en passant* statement on a controversy of some empirical and theoretical importance is welcome.

Looking back on the matters discussed in this section, I think the main source of the differences of opinion between the authors and the reviewer - apart from imprecision or vagueness in our formulations - lies in the fact that our *signifiés* are not the clearly defined, cut-and-dried concepts of the text books but their modalities, also appearing in the ethnography and in the theory as 'idea principles', which need not be associated with organized groups. A result of this is that in the FAS we discern (*not* 'construct') polythetic rather than monothetic classes. One example of this is our discerning societies with 'double descent', where we use 'descent' as meaning a society's recognition of a principle for the inter-generational classification of categories of persons.

In this final section, we must turn to the matter raised by Barnes on pp. 91-3 and 104: the Leiden group were and are wrong to take 'Indonesia' as their Field of Anthropological Study. As a subject for fruitful discussion, this is a non-starter. While one can understand that a reader of *Unity in Diversity* may wish a further explication of the way the FAS group distinguishes between double and parallel descent, or double descent and cognatic kinship, it is beyond my comprehension how anyone can attribute to them the notion that Indonesia, in the sense of the territory of the Indonesian Republic or the Netherlands East Indies, is a linguistically or anthropologically defined, 'self-contained' (p. 91), distinct unit. True, J.P.B. de Josselin de Jong's seminal lecture was called 'The Malay Archipelago as a Field of Ethnological Study', and the *Unity in Diversity* volume's sub-title is *Indonesia as a Field of Anthropological Study*; but one would have thought the passage on page 5 of that volume would have obviated any misunderstanding:

The preliminary assumption...was based partly on the study of available ethnographic reports, but also, and very strongly, on the known fact that the Indonesian languages are closely related to each other. This is where a problem arises, for it is - and was, in 1935 - equally well known that these languages are also related to Melanesian and Polynesian branches of the Austronesian family. What then is the FAS: the area of the Indonesian, of the Austronesian, or even of the 'Austriac' languages? [...etc.].

To conclude this paper, I shall summarize the linguistic aspects of the FAS approach. Starting from scratch, we must consider three matters: the genetic basis for the comparative effort; the validity of the term Indonesia(n); and actual practice, which cannot always conform to a recognized ideal.

First, given the established fact that all but a few of the languages spoken in the FAS area (*which* area that is comes under our second question) are genetically related to each other, it is a reasonable assignment for comparative anthropology to study whether, and if so how, the genetic relatedness of the cultures associated with these languages can be perceived. Whoever undertakes this task has, of course, to take account of several limiting factors - in the first place, that similarity or even identity of language need not indicate similarity of culture; but within our FAS this factor has hardly any disturbing effect. In addition, whenever one can associate a set of cultures, or cultural elements, with each other as a series of transformations, one is not always entitled to attribute this to the genetic relatedness of these cultures. The features in question may be of world-wide occurrence, or they may be due to convergence (independent invention) or diffusion - either from one culture to another in the FAS, or to both (or all) from an external source.

The above remarks are more likely to be regarded as truisms than as contentious. The second question, however, is of a different nature: do the FAS anthropologists define their language-and-culture area, which they call Indonesia, as coterminous with the Indonesian Republic, the *ci-devant* Netherlands East Indies? We do not, and as we have taken comparative linguistics as our guide, I need only refer (as was done in *Unity in Diversity*, p. 239) to a few earlier and later publications in this discipline. Dempwolff (1934: 23-36) gives an 'Analysis of three Indonesian languages: Tagalog, Toba-Batak and Javanese'; R.A. Kern's article (1942) on prefixes in Indonesian languages includes data on Bisaya and Tagalog; and Uhlenbeck (1971: 55), discussing 'the history of the study of Indonesian languages', refers to 'the Indonesian languages of Madagascar' and to Malaya. Turning to anthropology, I mention only the book *Minangkabau and Negri Sembilan* (i.e. an Indonesian and a Malaysian society), subtitled *Socio-Political Structure in Indonesia* (P.E. de Josselin de Jong 1951).

A more valid objection, however, is that more recently Dr Blust, in several articles published since 1977 and in his *Unity in Diversity* paper, has demonstrated that it is incorrect to consider 'Indonesian' languages (i.e. the Austronesian languages spoken in Indonesia, Malaysia, the Philippines, Taiwan and neighbouring islands, and Madagascar) as one of the distinct subgroupings of the Austronesian language family. Does this not affect the FAS enterprise in comparative anthropology? It certainly does. Assuming that Blust's new subgroupings will not be rejected or drastically altered by other linguistic research,³ I am convinced

³ As was the case with the admittedly weak association between Indonesian and Thai languages proposed by Benedict (1942).

it should and will be a most valuable guide for future mutually interpretative research of the Leiden type. As such research on, for example, the social organization, the mythology or whatever of the cultures of the entire FAS (i.e. the Austronesian language area) is never practicable as a single project, it is methodically sound to carry out with one of the new sub-groups (in particular the Western Malayo-Polynesian or the Central Eastern Malayo-Polynesian), rather than the formally defined Indonesian language area, as the researcher's universe of discourse.

This brings us, finally, to the 'first matter' for discussion (see previous page). The situation is as follows. As comparative linguistics has been and remains the exemplary model for the FAS enterprise, the 'field' in question remains, *in principle*, the Austronesian language area. In practice, though, FAS work was limited to the 'Indonesian' language area, and as this concept now appears to be untenable, linguistically speaking, we are in the process of setting our sights on new sub-areas within the Austronesian 'field'. So far, so good; but the objection can be raised (to which Barnes alludes on pp. 91-3) that, even during the period when 'Indonesian languages' was an acceptable term, the FAS group did not study 'Indonesia' in the sense of the Indonesian language area, but in the sense of the territory of the Netherlands East Indies and later the Indonesian Republic. This is true, but I should emphatically state that this practice was never based on the notion that this politically defined area was ever regarded as a linguistically or anthropologically bounded, distinct field. Why, then, was the FAS enterprise in practice almost entirely limited to this field?

The answer is obvious, but should nevertheless be formulated to avoid any possible misunderstanding. In the first place, it was purely realistic that we should not bite off more than we could chew by trying to cover the entire Austronesian area. Uhlenbeck (1971: 55) describes how this area came to be carved up between the colonial powers as separate research areas during the nineteenth century; this meant that the Netherlands concentrated its human and financial resources and its library facilities - in brief its expertise - on that part of 'Indonesia' to which it had direct access. After decolonization and the 'doldrums' of the first decade after 1945, the existing and partly rejuvenated resources were re-activated - hence the cultural convention with Indonesia, the Programme for Indonesian Studies in Indonesia and the Netherlands, and the inter-faculty curriculum for Indonesian studies (*Indonesiëkunde*) at Leiden University. Our concentration on the politically and historically defined unit 'Indonesia' was and is, therefore, one of practice and not due to the mistaken notion that, in spite of its growing internal unity, it is an entirely self-contained Field of Anthropological Study.

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