Review: **D'ARCY RYAN**
UNIVERSITY OF WESTERN AUSTRALIA (RETIRED)
PERTH

Reviewing another scholar's restudy of an area and topic that had been the subject of one's own major fieldwork thirty years earlier presents

peculiar difficulties. Apart from the normal changes to be anticipated in the people themselves (thirty years, in this case, spanned the transition from colonial government to independence; from an almost untouched traditional situation to membership of the international world), there is also a generation's accumulation of ethnographic knowledge and (by no means unimportant) the shifting fashions of its interpretation. One must therefore try to balance the differing viewpoints and personal predilections of two separate observers, of opposite gender and a generation apart. Do we attribute discrepancies in the accounts to Ryan, to Lederman, or to a new generation of Mendi?

Although, in general, our actual observations are in accord, there are certain differences, some quite important, which will be the subject of this commentary.

Variants in glossary are probably for the most part attributable to our use of different phonemic orthographies; but it must also be remembered that the Mendi language *(Mend piri angal)* not only exhibits noticeable dialectic changes over distances as short as a mile or two, but also is subject to constant and deliberate alterations for social reasons such as name taboos and "local identity" (see D'Arcy Ryan, "Names and Naming in Mendi," *Oceania* 29, no. 2 [1958]: 109-116). I found most of Lederman's Mendi terminology quite recognizable or at least deducible from context.

Other differences, however, are more significant, and fall into two classes, the factual and the interpretive or theoretical.

In the factual area: A point on which Lederman took me to task was my assertion that everyone married who could, and that marriage, preferably multiple, was an essential mechanism for the attainment of big-man status through the exchange system. Lederman contradicts this with examples of bachelor big-men. This does surprise me. I knew a few mature bachelors, all non-agnates, all of whom hoped to acquire a wife sometime and all of whom would remain nonentities, if not "rubbish men," until they did. The operations of a big-man required a breeding pool, or some other accessible source, of pigs, most of them reared by his wives or other female relatives. Today, it is apparently possible, as Lederman shows with her examples, for an ambitious bachelor to develop the necessary pig-tending support system; but in the 1950s it would have been extremely difficult. As I mentioned above, all the mature bachelors I knew or heard of were non-agnates (not true agnatic members of the lineage with which they were residing). Although it was generally maintained that such people enjoyed the same status as their agnatic coresidents, I demonstrated that their "marriage potential" had

significant limitations (D'Arcy Ryan, "Gift Exchange in the Mendi Valley" [Ph.D. diss., Sydney University, 1961], 76-79) : they married less often, had fewer contemporaneous wives, paid smaller bride-prices, and so forth). I would be inclined to suggest that, even in Lederman's time, twenty to twenty-five years later, a similar situation would apply and that her two or three bachelor big-men were indeed exceptions. I have dwelt on this apparently trivial point because the important agnate-non-agnate distinction has implications that Lederman seems to have ignored.

Another question is that of female *twem.* I did certainly have the impression that the Mendi exchange system, like the others in the New Guinea Highlands, was primarily a male operation, and that women, although essential in private supporting roles, like pig-rearing and soliciting contributions from their male kin, rarely performed in public displays or in the network of private exchanges *(twem)* that made the public ceremonies possible. When women did perform such roles, it was always as agents or surrogates for their menfolk.

Lederman's evidence, however, indicates that many women do, in fact, engage autonomously in the *twem* network with both male and female partners, and that their operations are regarded as their own business. I can only comment that my own informants, male and female, were definite and unanimous in asserting that women did not make *twem* on their own account; and my observations over more than two years gave me no reason to doubt this statement. Perhaps I should add that, although the term for "exchange partner" *(twem ol;* lit., "*twem* man") was in constant use, I never heard its female equivalent, *twem ten* ("*twem* woman"), nor is the latter term in Lederman's glossary; perhaps *twem* persons, like female anthropologists, were regarded as "honorary males"? On the other hand, it is quite possible, even probable, that twenty to thirty years have seen a complete shift of emphasis in the manifest functions (i.e., the perceived purpose) of the whole system. This brings us to what I consider the most interesting aspect of this book.

In my own work, I laid special emphasis on warfare, which I described as "endemic" and an important factor in the cultural and structural adaptation of the region and, more to the point, a situation into which the whole exchange system was inextricably locked. At the time of my first arrival in Mendi, warfare had been officially banned for only four years and, in the Mendi Valley itself, there were still sporadic outbursts of interclan fighting. Even in this new, and still uneasy,

state of "peace," there was yet a vast backlog of war-death compensations or *ol peya* (Lederman's *ol tenga)* to be made.

In addition to the actual death payments, there were ceremonies *(ma-shogensha)* associated with peace making, in which enemies (or even quarreling groups) offered and counteroffered prestations that were repeatedly refused by both sides because "only friends exchange." After several such performances, extending perhaps over a year or more, the parties were felt to have established their credentials and goodwill, the goods finally changed hands, and the exchanges were completed: an explicit ceremonial pantomime of that basic Mendi injunction, "Only friends can exchange, and all friends must exchange," a theme that is pointed up by every example of Mendi exchange. (This, incidentally, explains why the Mendi don't marry their enemies: they can't organize exchanges with them; or why men who never make *twem* are "rubbish men" [*ol timp*]: they have not bothered to make friends, thus failing to create any social identity.) Moreover, as I think I demonstrated (Ryan, "Gift Exchange," chap. 9), the big interclan pig kills *(mok ink)* could be analyzed almost entirely in terms of the establishment and maintenance of military alliances. I also made the point that the Mendi system of alternative residential options was an essential security strategy to provide refuge for survivors of military defeat (in those days, a constant threat); the exchange system was an important mechanism for keeping these options open, for the people with whom one exchanged were all, among other things, potential refuge sponsors.

As Lederman points out (pp. 66-67), I focused on the major corporate exchanges not (as she says) "in keeping with the then contemporary anthropological emphasis on corporate groups," but because they were the most salient features of the society and, in an "exploratory" study (I'm a bit coy about the word "pioneer") of what was then a virtually unknown type of society, not to have emphasized its most indicative features would have been downright perverse. It is also true that, although well aware of the all-pervading presence of *twem,* I did regard it as a system of amassing goods for the major exchanges. Certainly, it had other functions: *twem ol* were "brothers" and allies, sources of valuable mutual assistance; it was also an exciting game in which big operators got the same thrills from their coups as entrepreneurs do in our society --and similar rewards in status and prestige. *Twem* provided opportunities and excuses to circulate socially, visiting and meeting new people. But I do not believe these "side benefits" were its original function--although they could well remain attractive enough to maintain the sys-

tern today, even when the "warfare" imperative has lost all, or most, of its relevance. This could also lead reasonably to the emergence of females in the system. The above remarks apply as well to the *mok ink,* the preparations and public ceremonies for which were probably the most exciting and prestigious sources of entertainment in Mendi life.

I am also unconvinced by Lederman's arguments (if I have understood them) that *twem* networks give rise to a sort of "ego-centred body-corporate," or sodality, forming a structural principle independent of, and sometimes conflicting with, that of clan affiliation. Nor does her summary statement--"Exchange relationships . . . are part of the way in which the Mendi make themselves as autonomous social agents or persons, and as such they are very much a part of Mendi 'politics', broadly construed" (p. 216)--really clarify the matter for me.

Lederman has done a good, scholarly piece of work, particularly in her examination of the female side of Mendi life to which I, for various circumstantial reasons, did not, at that time, have access. Such data are essential, of course, to our understanding of any society. It is a pity, however, that her theoretical interpretation of her material did not pay more attention to the historical dimension. Not only, as I remarked in the beginning, would changes be expected during twenty years of transition from precolonial, small-scale autonomy to independant nationhood, but there is the additional fact that the culture of Mendi society seems to be as flexible and pragmatic as its structure. Like many other Highlands peoples, the Mendi welcomed novelties: new sorceries, religious cults, crops, agricultural techniques, cash economy; if they worked, the Mendi incorporated them, and if not, discarded them. Not only has their exchange system not been discarded, but Lederman's work shows that, if anything, it has been expanded. I have suggested some obvious reasons above, but they are by no means a full answer.

We come at last to that vexed question: the structure of Highlands societies. First, I must take issue with Lederman's interchangeable use of the terms "tribe" and "clan-cluster." There is no group in Mendi (nor, as far as I know, in the Highlands) that would fit any of the accepted definitions of a "tribe," and certainly not the occasional, and largely fortuitous, clan alliances that I have called, with prosaic literalness, "clan-clusters" (Ryan, "Gift Exchange," 44-51). It is theoretically possible that some Mendi clans might belong to no cluster at all: an inconceivable situation in a tribal structure.

It is true that I (like most of my colleagues of the 1950s) approached our respective societies with African models in mind. It was Barnes who

first seriously questioned their relevance, in his well-known "African Models in the New Guinea Highlands" paper of 1962 (*Man* 62:5-9). He pointed out, quite correctly, that the patrilineal paradigm that Evans-Pritchard abstracted from the Nuer did not fit the residence patterns in the New Guinea Highlands, neglecting to stress (as Evans-Pritchard himself frequently did) that neither did the Nuer conform to their structural paradigm but were, in their choice of residence, as flexible as the Mendi, or even more so, and for very similar reasons. But Evans-Pritchard's Nuer have become established as one of the stereotypic patrilineal models, so if the Mendi are not patrilineal, then neither are the Nuer! (A short digression: At professional discussions over the last few years, an intriguing question has forced itself onto my attention. There seems to be a growing reluctance among contemporary anthropologists to admit not only the existence but even the very concept of a "patrilineal" society. If this suspicion is correct, one wonders, Why? The answer, if pursued, might provide someone with a stimulating and contentious paper.)

Nevertheless, the debate grinds on: Are New Guinea Highlanders patrilineal, multilineal, cognatic, even "cumulatively patrifiliative" (a grotesquerie of Barnes's, cited approvingly by Lederman, p. 257, n. 3), or do they have no descent groups at all? Most of us seem to be agreed about the ethnographic facts; the dispute appears to be about how we are going to assemble them into a coherent model. Now, most members of almost any society must carry in their minds a consensual, abstract model of their society's social structure; this is necessary to enable them to organize and predict social behavior and to discuss it with others. While practical exigencies may permit or necessitate a wide range of deviation from the "ideal," this does not invalidate the model from their point of view, nor from ours. The test of an anthropologist's model is whether it makes sense to the society's members and can provide a basis for mutually comprehensible discussion. As far as I could determine, my model was very similar to that which the Mendi were using; in short, they seemed to be operating Nuer-type abstract models from which they deviate, when expedient, as much as the Nuer did. Considering the evidence for male domination in Highlands society, it would be quite amazing if their structure were not strongly patrioriented, as of course, all the evidence, including Lederman's own, clearly shows it to be.

Unilineal systems (as every undergraduate knows) are faced with the constant problem of striking a balance between populations and land-

resources and, by the vagaries of demographic reality, no such system can function rigidly according to the pure, abstract model. Through warfare, disease, and demographic accident, some unilineages decline in numbers while others increase. Thus, no unilineal system can function for long without devising some institutionalized mechanism for redistributing its residential groups in accordance with its subsistence resources. Moreover, in no society of any kind can residence and affiliation be entirely separated. It follows that redistribution of residence will always be accompanied by some flexibility of affiliation. The question then is: At what degree of flexible affiliation do we decide that the system is no longer "unilineal"? Whatever the necessary accommodation made to such exigencies, the system itself does not cease to be unilineal until the people themselves stop thinking about it in terms of a unilineal model.

May we see an end to this boringly tenacious non-issue?