

## A SOCIOLOGIST'S REFLECTIONS ON AN ANTHROPOLOGICAL CASE STUDY<sup>1</sup>

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Although an individual can live only one life, he can gain a measure of "understanding" of aspects of the lives of others. There are limits to that understanding: an individual cannot "step into the shoes" of another, except in a relatively superficial sense; nor can he share the innermost thoughts even of those closest to him. Anthropologists have however evolved "scientific" procedures of field research, and insist on the investigator spending a certain (or rather, uncertain) period amongst the people he studies, to enable him to gain a modicum of insight into their behaviour. It is important that he adopt a learning situation which leaves him in a position to communicate the understanding he derives to others, for solipsistic involvement precludes the possibility of objective study of man and society. Malinowski's prolonged sojourn among the Trobrianders—two periods of one year each, of which total only six weeks were spent in European company—gave his reports an "actuality of relationships and a richness of content" which ethnographic accounts hitherto lacked.<sup>2</sup> The rich detail of his monographs, particularly in so far as he succeeded in communicating a sense of the "imponderabilia of actual life" impressed his students. A host of epigones came to regard a period of field work as the hallmark of a professional anthropologist, and the longer one's stint among "savages", the more illuminating and scientific the fruit of one's labours.

The intellectual background of the pioneer British anthropologists<sup>4</sup> was one of a self-satisfied middle class. Not even the heritage of an extensive colonial empire peopled by exotic tribes and races eroded the parochialism of Victorian intellectuals who took their society and culture to be, if not universal, at least the most "natural" and desirable. To jolt him out of this complacency, and to transform the dogmatism of the untravelled into a more receptive and objective state of mind, a therapeutic withdrawal from his own society was deemed as important to the anthropologist as self analysis was to the psychoanalyst. The doctrine that a given society can be studied objectively only by one alien to it seems unjustified nowadays,<sup>5</sup> anthropologists are not so smug, and a literate person can read enough about alien societies to approach his own with detachment. But compulsive withdrawal from one's own society in order to deepen one's understanding of Society, is not merely dictated by scientific considerations. It is a ritual having deep emotional significance. The suggestion has been made that he who offers himself as an anthropological acolyte prepared to undergo a period of field-work as a rite of passage to the charmed circle of "trained" anthropologists, is in psychological need of withdrawal from a society in which he is ill-adjusted. Be that as it may, anthropologists frequently betray signs of being tormented by a guilt of incomplete isolation, and to derive solace from the naive belief that a fixed period of time (say two years) gives the same quantum of "understanding" to all alike.<sup>7</sup>

But surely there are differences in the receptivity and sensitivity of individuals. What one man learns in a month may take a more gifted person a week, while yet another may never perceive the elusive imponderabilia. I therefore find it puzzling that Dr. E. R. Leach in his admirable case study of land tenure and kinship in a village in the Ceylon Dry Zone, refers with awed respect to the 15 months spent in another part of the Dry Zone by his protegee Dr. N. O. Yalman,<sup>8</sup> while his own study of Pul Eliya village, limited to some six months, is admitted to be open to the "obvious criticism" that it covered "too short a period of time to be of real scientific value."<sup>9</sup> It is not an echo of the Puritan

doctrine of the merit of making the best of available time, but rather a gnawing sense of guilt that withdrawal from one's own society may not be long, and complete, enough. One can well imagine the pangs of conscience experienced by the bored anthropologist each time he sneaks out of the village and motors a few miles to the rest house for a drink in urban surroundings. For the six months solitary confinement to which he has sentenced himself should not be interrupted—certainly not for more than the proportion of six weeks in two years which one of the Founding Fathers allowed himself.

The fact is that the logical result of too great a concern with obtaining insight into nebulous imponderables must result in solipsistic isolation. Awareness of this basic fact led anthropologists to map out objectively demonstrable areas of investigation. But the idea of collecting as much data as possible without any definite purpose, though apparently outdated, dies hard. It seems to be sanctioned in successive editions of *Notes and Queries on Anthropology*. Indeed, many celebrated scholars have brought out a number of books and papers based on data collected in the course of a single field trip, for they "know" no other society besides the one to which they withdrew for a few months, a decade or so previously. And this constituted their life experience, since, until recently, the mores of the profession precluded research in one's own society. (The case arose of a conscientious lady of the fraternity who, asked to deliver a course of lectures on social structure in a leading British university, insisted that she could talk only about the single society which she had studied.) Nowadays, even if a great deal of "interesting" information creeps into the anthropologist's notebooks, e. g., observations on the sexual significance of hair,<sup>9</sup> the tendency has been to limit oneself to the sanctioned foci of investigation, particularly the more "structured" aspects of the social system, such as kinship and land tenure. This structuring of the foci of investigation has reduced the length and intensity of participation required of the anthropologist.

The informant has played an important, though often unacknowledged role in anthropological investigation. He can on occasion irritate the fieldworker<sup>10</sup> but he can also minimize, and even relieve the anthropologist of the stresses and strains that direct participant-observation involves, besides shortening the period he need spend in the field, and rendering a working knowledge of the native language superfluous. Since Dr. Leach does not mention that he employed the services of an interpreter, some of the statements and opinions attributed to the villagers give the impression that the author was in perfect rapport with the subjects. But is he being as honest with his readers as he claims?<sup>11</sup> On the other hand, Ryan<sup>12</sup> acknowledges the contribution of two native collaborators who first assisted him when his study of a Sinhalese village began as a "directed student project" and continued and intensified after they had graduated, their "collaboration" being "of an order amounting to co-authorship". Besides, four persons of the village "served frequently as our mentors, informants and co-operators." Clearly, the involvement of so-called participant-observers can be vicarious, delegated.<sup>13</sup> The intermediary (variously designated—informant, interpreter, collaborator, research assistant) is the catspaw used by the anthropologist to derive sufficient communicable understanding of alien societies in terms of familiar categories. Thus Ryan<sup>14</sup> renders the Sinhalese term *gê* (literally "house") as "patrilineal family" and illustrates it by the analogy of "the Lodges or the McGintys of Boston, Massachusetts". This questionable interpretation may be gratifying to those who are aware of the supposed American parallel, but it does not elucidate the Sinhalese concept. Dr. Leach is hardly more successful in his lengthy analysis of the Dry Zone *variga*, an endogamous and partly fictive kingroup.<sup>15</sup> But his comparison of interests in village land to share-holding in a joint stock company is certainly appropriate.<sup>16</sup>

The inevitable outcome of limiting his range of observed fact, and of employing informants, is that the anthropologist tends to lose his penchant for the imponderables, and seeks what is quantitative and measurable. His denigration of the statistical social survey (which he may mistakenly assume to be the method of sociology) hardly helps to resolve his own methodological dilemmas. In an earlier paper Leach<sup>17</sup> blandly declared that the sociologist's methods were statistical in contrast to those of the anthropologist who was interested in relational configurations derived by participant-observation over prolonged periods of time. In his latest monograph he claims to "examine quantitative ('statistical') facts of a particular case and show that these possess a structural pattern which is independent of any ideal paradigm"<sup>18</sup> True, at several points he resorts to simple enumeration of all instances, e.g. kinsmen invited to and excluded from a puberty ceremony,<sup>19</sup> extant marriages,<sup>20</sup> extents of land holdings.<sup>21</sup> But the presentation of other facts is hardly quantitative. Thus the instance of the young lady who had "married" seven times, thrice with the same husband, is cited as an extreme case but not unique.<sup>22</sup> But if not unique, a statistical count of frequency of marriages for each individual in the village (or for a representative sample) is to be expected if, as the author claims, his data is quantitative. If these frequencies are expressed as an average (or if standard deviations are computed) we are in the realm of the abstract, in the sense that a concrete individual cannot be said to have married three and a half times.

A crucial dilemma results from Dr. Leach's unreal dichotomy between specificity and abstraction. Claiming that he has let his evidence "speak for itself" he proceeds to suggest that by doing so he has been more honest with his readers than some of his colleagues have managed to be with theirs.<sup>23</sup> The burden of his complaint seems to be that these others<sup>24</sup> have presented ideal accounts which may have done violence to the complexity of empirical reality. In a previous monograph, he argued: "if one attempts to interpret a social structure by means of analytic categories which are more precise than those which the people use themselves, one injects into the system a specious rigidity and symmetry which may be lacking in the real life situation."<sup>25</sup> The solution is to make ethnographic reporting as exhaustive as possible, examining the so-called "statistical" facts of a particular unit to show that they exhibit a structural pattern which is independent of any ideal paradigm. In fine, meticulous description reveals a single pattern per se.

Now in a limited universe comprising, a single village inhabited by 167 interacting persons, it is mathematically feasible to count the frequencies of certain significant relationships and actions. But not precisely: interaction between two persons (or rather two role relationships) is admitted to be only "rough approximations". If so, the table of "behaviours expected between pairs of males"<sup>26</sup> turn out to be ideal paradigms. In short, every verbal description of actual behaviour contains some element of an ideal paradigm, unless it refers to specific actions of an individual. But these behavioural constants which we posit on the basis of observation of an adequate number of instances, are not proved to be wrong on account of a few deviations. The crux of the problem has been stated thus by Emmet:

abstract types are formed by generalizations from the behaviour expected as appropriate in the various roles they define. The types are idealizations, in that no one may behave just like this as a son or a headman; but they define roughly how people think sons or headmen ought to behave, and this in a way not too far removed from how they usually do behave.<sup>27</sup>

I submit that the naive empiricist doctrine: that evidence recorded with meticulous detail reveals a pattern, is untenable. The patterns that emerge are idealizations. Dr. Leach is clear in what he himself calls his "insinuations" against his colleagues. Writers like Evans Pritchard and Fortes are accused of having adapted their facts to ideal concepts "by means of fictions".<sup>28</sup> I maintain that he himself is equally prone to resort to fictions, and that he has failed to conceal this from the reader by his ingenious verbal manipulations, e.g., that actual behaviours are "rough approximations" vis-a-vis idealized patterns. A clear contradiction is involved in his central argument, for he denies that the behaviours he postulates (for instance, role relationships between pairs of males) are rigid stereotypes, since they do not bear a perfect correspondence to the details of empirical reality. In that event they can only be abstractions, idealizations, fictions.

The root of the matter is that one can be more or less specific in detailing social facts. Evans Pritchard, describing a tribe whose main interest centred around cattle, wrote:

When a Nuer mentions an ox his habitual moroseness leaves him and he speaks with enthusiasm, throwing up his arms to show you how its horns are trained. 'I have a fine ox', he says, 'a brindled ox with a large white splash on its back and with one horn trained over its muzzle'—and up go his hands, one above his head and, the other bent at the elbow across his face.<sup>29</sup>

This is an abstract statement, a generalization, expressed with the "beautiful lucidity" characteristic of this author's writings. According to Leach that "beautiful lucidity" was possible only because Evans Pritchard limited himself to descriptions of certain ideal-type situations.<sup>30</sup> On the other hand, I attribute that stylistic elegance to the degree of abstraction rather than to anything inherent in the situation. Further, I submit that nothing is gained by reducing the abstract statement quoted above to a statistical enumeration of the precise mode of reaction of each individual Nuer, or a representative example of the tribe. Yet a conscientious plodder might well do precisely that, and report that "4.7 per cent of the tribe reacted to a question on cattle with an interested grin, without raising their hands or talking in the manner described by Evans Pritchard".

The question is where to draw the line. If we want to see the unit of investigation as a whole, too much detail might obscure the overall picture. Even a billiard ball, if observed at a certain nearness (through a microscope) is no longer smooth. My objection is to the dogmatism and intolerance of an approach which, having decided on an arbitrary degree of specificity, seems to decree, "this is where I draw the line, ergo this is where it should be drawn". No objective criteria are proposed, for the simple reason that there are none. The greatest of the Founding Fathers, Radcliffe Brown, once suggested that there were no rules to guide one's choice of units, that "the procedure contains an arbitrary element, and the only question is whether you are acting expediently or inexpediently, and probably you cannot tell whether your abstraction has been expedient until you have made your study and seen what results you get".<sup>31</sup>

If Dr. Leach is satisfied with what he claims to be the "excessive detail" of his case study, I am equally satisfied with the abstract treatment of the Dry Zone village in my *Sinhalese Social Organization* which was intended to serve as a "model" of what the available evidence indicated to be the social system of a whole kingdom, of which the idealized Dry Zone village was only a relatively isolated part.<sup>32</sup> It could have been made more abstract and, consequently, more readable. But the historical data available precluded the "excessive" detail of Dr. Leach's study of a contemporary village.

In a great deal of anthropological discourse it is not the commonsense recommended by Radcliffe Brown that seems to determine decisions as to the degree of abstraction, but rather an irrepressible desire to indulge in a bout of head-hunting, to appear "different" from one's colleagues. (I hasten to add that the ill-mannered denunciations of the Young Turks of anthropology which adorn the back pages of *Man* are here replaced by subtle and dignified "insinuations".) The danger of this intellectual climate is that any account can be challenged as being "ideal". The "excessive detail" for which Dr. Leach makes such exaggerated claims can easily be shown to be sadly deficient. Thus he has presented "idealized" behaviours expected between certain pairs of male kin roles, but there is no parallel information about the basic relationships between male and female roles. Not even is the husband-wife relationship considered in any detail. Yet information on the prevalence of wife beating, adultery, etc., might well have been crucial. "Marriage" is treated in an abstract manner, and the role of the wife in day to day affairs ignored. In fact, it is a male view of the social system that Dr. Leach presents, though that abstracted presentation is highly competent and full of interest. But I question whether there is anything unexpected, although I do not doubt that it is "an addition to the already substantial literature relating to Ceylonese land tenure". An addition, to be sure, which serves to broaden the existing knowledge without altering it.

Pul Eliya is even less successful where it attempts "to provide a critical test of certain features of the theory and method of contemporary British social anthropology".<sup>33</sup> All too often, what purport to be "critical tests" turn out to be little more than academic polemics, buried beneath an imposing mass of empirical data. The "evidence" can hardly be taken seriously if, as Dr. Leach argues, it is put in by way of illustration of "some rather preposterous hypothesis of a very general kind" which itself is the product of the anthropologist's "private intuition".<sup>34</sup> The careful reader soon finds that the hypotheses adduced are "preposterous" only in so far as they challenge the findings of certain British anthropologists. But taken out of the context of parochial disputation, they appear in different light. The reader who is innocent of these local controversies and tensions will not fail to be struck by the quibbling over nominal differences, the magnification of "errors", and the failure to refer to parallel findings of others. It appears that just as kinship principles are intelligible only in the context of locality—one of Dr. Leach's "findings" — anthropological hypotheses are meaningful only in the context of a local universe of discourse.

Lest my strictures appear to be preposterous, let me justify them by "evidence" from Dr. Leach's monograph. Let me begin with his charge that a number of anthropologists separate social structure and natural environment.<sup>35</sup> Thus Evans Pritchard "in writing *The Nuer* (1940), clearly tried to integrate the analysis of ecology with the analysis of 'social structure'. In practice each half of the book is autonomous and makes sense without reference to the rest". But what else could the author have done? For his exposition he divides the book into chapters, and each chapter deals with a specific topic. He can draw the threads together in a "conclusion". He can make the trite proviso in his introduction that "although the book is divided into chapters, the contents are interrelated".<sup>36</sup> But otherwise, each section must necessarily appear to be autonomous.

The point is that functionalistic demands have sometimes to be sacrificed for the sake of exposition. Furthermore, it is in fact expedient to treat certain segments of a society separately e.g. a factory or firm, a school or university, a clique or workgroup, a village or hamlet, and so on. It is idle to assert that "since everything is related to everything else, the whole must be treated simultaneously". Indeed, if Dr. Leach's contention is valid, his own case study can be challenged on the ground that it treats a

single village as an autonomous unit, although on occasion he betrays its connectedness to larger wholes. Thus he refers to "customary behaviour of Sinhalese villagers of the North Central Province in 1954"; to "the Sinhalese situation",<sup>38</sup> and to the villagers' contacts with the outside world.<sup>39</sup>

Let us act on Dr. Leach's exhortation<sup>40</sup> and go back to first principles. Now whether we are demarcating categories, or whether it is a question of deciding on the expedient degree of abstraction, some line must be drawn. Radcliffe Brown explained the first principles clearly, and made a plea for tolerance and good sense in their application.

Where do we, for the purposes of a study, draw our line around a system? Remember the system is separated from all the rest of the universe. In all science the scientist attempts in separate conceptual what he is studying from its environment. To some extent . . . . such abstractions are arbitrary, but some abstractions are expedient and some are not. The art of science is to know where to make the abstraction. The art of the scientist lies in his ability to distinguish between the expedient and the inexpedient, thus assuring that his investigation will have positive results. This distinguishes a good scientist from a bad. It involves a certain amount of intuition.

. . . . From certain points of view it would be convenient to take the whole of the United States as a unit—obviously so if you are dealing with certain political problems. On the other hand, in considering economic institutions, the whole world is society now. Again, for certain other kinds of social problems, the United States is far too big, and therefore, what we have to do is to take as a unit a smaller community which we can separate out, define, and study as a system.<sup>41</sup>

Having chosen the unit, it may be expedient to treat certain behavioural constants separately, e.g., political behaviour for land tenure and/or kinship. True, in some societies the various aspects are closely knit—for instance, Bailey<sup>42</sup> showed that analysis of the market for land in a village in Orissa (India) involved a study of almost the entire social, economic and political fabric. But in other societies there is much less interdependence. Again, we conceptualize and talk of "roles" that though aware that the same man takes several roles, we have entitled to analyse each role separately. Dr. Leach has made certain decisions regarding his unit of study, and the degree of abstraction with which he treats his data. My complaint is that he does not regard his decisions as matters of expediency, but rather as the only decisions which are legitimate, and therefore binding on all.

I now turn to his failure to give due recognition to the parallel findings of others. This is a venial sin, but it militates against the codification of concepts, hypotheses, and "intuitions". Thus the "effective" *pavula* (family) is distinguished<sup>43</sup> without reference to the distinction made by Firth and Djamour<sup>44</sup> between "effective" and "non-effective" kin. Finally, there is Dr. Leach's "awkward doctrine that, in this society, the kinship system is not 'a thing in itself', but rather a way of thinking about rights and usages with respect to land".<sup>45</sup> This is neither new nor awkward. It is sociological commonplace which figures prominently in Maciver's early text books, stated as a general proposition that locality is the essence of community. More specifically the present writer adduced detailed evidence to demonstrate the linkage between kinship, marriage, and property in land.<sup>46</sup> Generally, kinship as well as marriage, can have various "functions"; any of which may be dominant in a given society.<sup>47</sup> In Pul Elyia the dominant "functional" relationship was between kinship and rights in land.

One aspect of this monograph which made me uneasy was the fact that the people studied and their village were not left anonymous or pseudonymous. Is this cricket? Is it sufficient merely to express the author's "thanks and apologies" to a people whose private lives he has laid bare to the world? Of course this author is not alone in this respect. In a recent study of a Sinhalese village which is mentioned by name, Ryan<sup>48</sup> inserted a number of photographs of individuals. Would he have dared to do the same in the United States? It is not that there is no scope for anthropological studies in Europe and the United States, but that scholars can extract funds from opulent Foundations to probe the intricacies and intimacies of human relationships in underdeveloped countries without risk of libel actions instituted by their unsuspecting victims. One can only live in the hope of either a change of heart on the part of the scholars, or of protest by those investigated. Is it not time that the "savages" of the world united in revolt against this unconscionable probing into their private lives by anthropologists looking for "evidence" to support their private intuitions?<sup>49</sup>

If what I have said here appears to be a disenchanting view of anthropology, I must conclude by acknowledging that Dr. Leach's excellent book holds out fresh promise for the social sciences in that he has transcended the pollution-complex which keeps anthropology and sociology poles apart. He has confounded those (including himself) who insist that the difference between the two disciplines is the sociologist's exclusive concern with statistical methods and quantitative data.<sup>50</sup> In fact, the really unexpected aspect of Pul Eliya is the author's admission that anthropologists are entitled to undertake "quantitative" studies, although his conception of "statistics" is confused. Srinivas is more explicit in his evaluation of the role of statistical data and methods in anthropological studies:

..... Where as formerly it was enough to say that in two different societies there was a preference for marrying mother's brother's daughter, nowadays an anthropologist would try to find out how many of the total number of marriages in either society are mother's brother's daughters. He would also try to relate it to other institutions of the society. The sanctions which are brought to bear on those who fail to marry their mother's brother's daughter would also be mentioned, preferably from actual case histories. Nowadays an anthropologist carries out a census of people, livestock, houses and, less frequently, of occupations..... In short, with social anthropology becoming definitely sociological in its orientation, the need for quantification has been felt. And with the extension of the field of social anthropology to the study of large villages, towns, factories, and even regions, statistical techniques will have to be increasingly employed.... It goes without saying that quantification should be resorted to only when necessary and not as a fetish our best insights may come from work which has not involved the use of statistics.<sup>51</sup>

It is to be hoped that anthropologists will proceed to make use of the insights, concepts and methods of those on the other side of the academic fence without undue fear of pollution and mental disorganization.<sup>52</sup> Such a trend was already begun by Radcliffe Brown, who did not hesitate to lean heavily on Durkheim although the latter was an armchair sociologist who had not undergone the requisite rites of passage for qualification as an anthropologist. Indeed, Dr. Leach might have avoided much of the confusion and misrepresentation in his discussion of the "idealized" nature of existing studies of the Dry Zone, vis-a-vis the essential rightness of his own down-to-earth empiricism,<sup>53</sup> had he been familiar with Max Weber's analysis of "ideal types".<sup>54</sup> If he does not believe in typologies it is not even necessary to go to Weber to realize that any abstract treatment cannot be the same as a faithful account of a single village, if such

an account can ever be completed. Hence my account of the Sinhalese village<sup>55</sup> is abstract. As Baden Powell noted in the preface to his classic. *The Indian Village Community* (1896) it is impossible to avoid "the errors which an abstract and unified conception of 'the village' can hardly fail to produce". But such abstraction can be as expedient as Dr. Leach's "excessive detail" both from the theoretical and practical points of view.

#### NOTES

1. The study in question is *Pul Eliya. A Village in Ceylon*, by E. R. Leach Cambridge University Press, 1961.
2. Phyllis Karberry. Malinowski's Contribution to Fieldwork Methods and Writing of Ethnography in *Man and Culture*, ed. Raymond Firth. London, Routledge & Kegan Paul, 1957.
3. Evans Pritchard was guilty of rank heresy when he stated that Marcel Mauss, in his essay on *The Gift*, was able to gain the same kind of insight which anthropologists derive in the field, from his flat in Paris.
4. Throughout this paper I refer to *social anthropology*, although using the abbreviated 'anthropology'. The first university chair which bore the title social anthropology was an honorary professorship held by Sir James Frezer at Liverpool University in 1908, cf. E. E. Evans Pritchard. *Social Anthropology*. London, Cohen & West, 195, p. 3.
5. cf. Evans Pritchard, *op.cit.*, p. 76.
6. Although it persists. "I think that many were able to accept and talk to a stranger in a frank and honest way that would have been impossible among themselves" R. E. Cole, *Japanese Blue Collar*, University of California Press, 1971, p. 44.
7. In fact the venue of Dr. Yaiman's major anthropological exploits was a village in the Wet Zone (Teripeha, Walapane District) cf. Nur Yaiman. The Flexibility of Caste Principles in a Kandyan Community in *Aspects of Caste in South India, Ceylon and North west Pakistan*. Cambridge Papers in Anthropology. ed. E. R. Leach, 1950, pp. 78-112.
8. I find it disconcerting that much of what Dr. Leach claims to be obvious is far from being obvious to me, while what he considers unexpected is pretty obvious.
9. cf. Leach, *Magical Hair*. *J. Royal Anthropological Institute*, 88/1, 1958.
10. Evans Pritchard. *The Nuer*. Oxford University Press, 1940, preface.
11. Leach. *Pul Eliya*. p. 12.
12. Bryce Ryan in collaboration with L. D. Jayasena & D. C. R. Wickremasinghe. *Sinhalese Village*. University of Miami Press, 1958, pp. v-vii.
13. The late Professor D. N. Majumdar of Lucknow once complained that an American anthropologist who had produced a voluminous book on an Indian village, complete with pretty photographs, had not slept a single night in that village, and depended entirely on student interviewers for his information. And students are readily available in countries having high rates of graduate unemployment. At a time when Foundations are anxious to promote research in "applied anthropology" the whole business of "rapport" becomes a matter of intellectual chicanery. Srinivas has described a new type of academic structure headed by a Director who

is an academic entrepreneur who has succeeded in securing research funds, while at the base of the hierarchy are postgraduate students who are made to do the dirty work of collecting data through questionnaires drafted by the middle ranks of the hierarchy. Needless to say, the report is ascribed to the Director, who had little to do with the actual research.

14. Ryan, *op. cit.*, p. 48.
15. Leach. *Pul Eliya*. pp. 67-78/
16. *ibid.*, p. 157.
17. Leach. An Anthropologist's Reflections on a Social Survey. *Ceylon J. Historical and Social Studies*. 1/i. 1958. The survey in question was *The Disintegrating Village* by N. K. Sarkar & S. J. T'ambiah, Ceylon University Press Board, 1957.
18. Leach. *Pul Eliya*. p. 8.
19. *ibid.*, p. 121.
20. *ibid.*, pp., 82, 83.
21. *ibid.*, p. 152, Map E.
22. *ibid.*, p. 90.
23. *ibid.*, p. 12.
24. The chief offenders among writers on Ceylon are R. W. levers., H. W. Codrington and R. Pieris. Among anthropologists, the 'idealists' include Evans Pritchard, Meyer Fortes and J. C. Mitchell *ibid.*, 305.
25. Leach, 'An Anthropologist's Reflections.....', *cit. supra* p. 106.
26. Leach, *Pul Eliya*, p. 126.
27. Dorothy Emmet, how Far Can Structural Studies Take Account of Individuals? *J. Royal Anthropol. Institute*. 90, ii 1960.
28. Leach, *Pul Eliya*. p. 8.
29. Evans Pritchard. *The Nuer*. p. 38.
30. Leach. *Political Systems of Highland Burma*. London, G: Bell. 1954, p. 282.
31. A. R. Radcliffe Brown. *A Natural Science of Society*. Glencoe. The Free Press. 1957. pp. 61-62.
32. Ralph Pieris. *Sinhalese Social Organization. The Kandyan Period*. Ceylon University Press Board, 1956, p. 6.
33. *Pul Eliya*. p. 12.
34. *ibid.*, p. 12.
35. *ibid.*, p. 305.
36. Thus Pieris, *Sinhalese Social Organization*, p. 26, states "the artificial divisions of the book tend to obscure the fact that the various parts of the social system are functionally interrelated."
37. Leach. *Pul Eliya*. p. 96.
38. *ibid.*, p. 94.
39. *ibid.*, p. 30-33.
40. *ibid.*, p. 304.

41. Radcliffe Brown. *op. cit.* pp, 61, 62.
42. F. G. Bailey. *Caste and the Economic Frontier* Manchester University Press, 1957.
43. Leach. *Pul Eliya*. p. 113.
44. Raymond Firth & Judith Djamour, Kinship in South Borough, in *Two Studies of Kinship in London*. University of London: The Athlone Press, 1956, p. 45.
45. Leach. *Pul Eliya*. p. 146.
46. Pieris. *op. cit.*, pp. 209-210.
47. For a discussion of the various functions of marriage cf. Leach. 'Polyandry, Inheritance and the Definition of Marriage'. *Man*, No. 199, 1955.
48. Ryan. *op. cit.*
49. In situations where visible paper and pencil are inadvisable, an anthropologist says, "I take notes in a pocket of my pants or jacket" I W. J. McEwen. A technique for Ethnographic Note-taking *Amer, Anthropol.*, 61/iv. 1959.
50. Leach. "An Anthropologist's Reflections. . . ." *cit. supra*.
51. M. N. Srinivas Social Anthropology and the Study of Rural and Urban Societies. *Econ. Wkly.*, Bombay. XI 4-6, 1959.
52. cf. W. R. G. Horton. Social Science, Logical or Psychological Impossibility? *Man*. LXI/4.
53. Leach. *Pul Eliya*. p. 2.
54. Max Weber. *The Theory of Social and Economic Organization*, Glencoe: The Free Press, 1947.
55. Pieris, *op. cit.*, Part II.