L'auteur ne nous dit pas si la stabilité des institutions et des valeurs résulte avant tout de leur plasticité, de leur capacité d'absorption d'éléments importés, ou plutôt de la faible intensité de l'impact extérieur. Quelle est à cet égard l'évolution survenue en d'autres îles des Carolines, mises en contact plus dense et permanent avec l'étranger? Si par ailleurs la prééminence originelle de l'île Yap peut s'expliquer suffisamment par sa position privilégiée à l'égard des contraintes écologiques, celles-ci suffisent-elles à exliquer l'ensemble de la hiérarchie des îles? La thèse de l'auteur paraît être ici un peu trop restrictive. Il n'en reste pas moins vrai que l'ouvrage de Alkire est d'une tenue excellente.

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Closed Systems and Open Minds: The Limits of Naïvety in Social Anthropology. Max GLUCKMAN (editor), Chicago, Aldine Publishing Company, 1964, x, 274 pp.

This reviewer has found *Closed Systems and Open Minds* a provocative, stimulating yet extremely disturbing book — at least those parts written jointly by Professors Devons (an economist) and Gluckman. I have found it disturbing because its essential message, that social anthropologists should not trespass (too deeply?) into other disciplines, might well take us back to an approach which we thought had been left behind — not to mention the danger that the author's recipe is likely to cut us off from some of the main currents of intellectual development and perspective.

Of course, every social anthropologist, nay every scientist, puts his microscope on a tiny slice of human behaviour or natural phenomenon. To do otherwise merely means that we would not understand the intense complexity of regularity and order which we assume exists both in human action and in the physical and natural world. But invariably social and natural features are woven together so closely that any discipline can be the instrument in exposing the complexity of any act and any event. No man can comprehend all of human behaviour or the living world. Hence the heavy hand of arbitrary demarcation by disciplines drives us all into little boxes. But the intellectual revolution of our time is desperately trying to get us out of these boxes. Why? Because no fact, no reality, no event, no act, has meaning if it stands as the centre of an expanding yet constantly limited field. All of our efforts must be devoted to the dual mission of achieving far greater exactness in our own special fields while at the same time recognizing the limitations which disciplinary boundaries impose on our understanding of what we thought was our very own preserve. Of course we shall always make naïve assumptions about those things which we do not understand — but this holds just as true for the social anthropologist who sticks to his own last as it does for those who realize that their data and their interpretations only assume reality if they do

transgress into other disciplines. What destroys our competence in giving reality to even a small slice of human behaviour is not our naïvety about other relevant disciplines but rather our inability to recognize that every word of caution offered by Professors Devons and Gluckman applies to the field of social anthropology itself. Reality only assumes exactness when this is realized. After all there are some social anthropologists who don't know much about kinship and some who have become great in social anthropology yet also write in the field of law!

I think that all this is realized by Professors Devons and Gluckman, who state: "In a fundamental philosophical sense all... aspects are part of the complex reality of human life..." Yet they go on to insist that "if one is to succeed in studying society, one must split up reality by isolating a particular aspect which presents certain regularities and is *relatively* autonomous and independent of other aspects". (p. 161)

To give body to their argument that social anthropologists dangerously and naïvely transgress into other disciplines, which they are unable to handle, the editor reproduces five important studies all but one of which was written in 1957. Thus, Professor Turner continues his brilliant study of the Ndembu with a chapter on "Symbols of Ndembu Ritual". While he is concerned with what is normative (and hence sticks to his anthropological last), the charge against him is that he is naïve because he works with a "fairly crude, view of human personality, though he takes account of unconscious feelings and motivations", in order to explain the emotional meaning and content of ritual symbols for the Ndembu. Professor Bailey, in analysing inter-caste disputes in two Orissa villages, is forced into a documentation of external events, i.e. Hindu culture, influencing village life, and thus slides over into the realms of the political scientist, the historian and economist, with the result that this part of his analysis is in danger of dealing with a much larger social field than is said to be appropriate for the social anthropologist. This forces him to "circumscribe" his field in an arbitrary manner, i.e. what is relevant to his analysis and what is not. Epstein contributes a chapter on the development of African political activities and thought by showing significant differences in the context of a Mine Township and a Government Township. Thus the traditional social anthropologist slides naïvely over into urban sociology but to do so he too must circumscribe his social field of investigation with the result that he must (in Epstein's own words) "set up an extremely simple model of the urban social system". The results, the editor insists, are "naïve assumptions", "simplification" and the "compression of external factors". At this stage the reader of this closely argued critique begins to wonder whether the editor himself is not guilty of transgressing the very limits he has set for social anthropologists by presuming to be able to judge those who have transgressed into other disciplines. What is more, how can we be sure that this exposé of allegedly naïvely held assumptions about Freud's work (pp. 232-240) is the correct analysis of the contribution of this intellectual giant? Neither Professors Devons nor Gluckman are psychologists or psycho-therapists.

In the next chapter, Profesor Lupton and Dr. Sheila Cunnison present their data on the influence of social characteristics on the output of workers in three Manchester factories. In order to grasp and analyse the reality involved there, these two authors must depart even further from traditional social anthropology and enter the real of the economist and psychologist. They do so believing that "at least an elementary knowledge of the work of these other students of society, and their methods of analysis, is called for". But as they do not have any real sophisticated knowledge of these fields and their techniques, they are forced to compress and circumscribe in such a manner that any economist, let us say, would consider the work most inadequate. Finally, Professor Watson writes about "Social Mobility and Social Class in [Scottish] Industrial [Mining] Communities". To do so he refuses completely to stay within the boundaries of his discipline: thus "he ceased to be a social anthropologist and became himself a sociologist". Why — because he refused to compress and circumscribe. He entered the social field of national life and thus found himself in the area of "macroscopic theory".

Every discipline is caught in the same dilemma and makes use of the same devices to cope with its own predicament. (Conceptualized as "circumscribing" — the rather arbitrary closing off of a field; "incorporation" — a complex of facts taken for granted uncritically; "abridgement" — summarized and oversimplified conclusions taken over from various sciences; "naïvety" or "compression" — highly simplistic assumptions about events, thought to be relevant but involving interpretations from another discipline; "simplification" in social science due to impossibility of presenting all the complex data *in toto*.) In this respect *Closed Systems and Open Minds* is a penetrating critique but without a penetrating answer. One cannot escape the observation that what has been said here has all been said before. But the way the editor and the authors say it is brilliant. Like others, I suspect, once I had started reading this book I did not want to be disturbed. Nowhere else in the anthropological literature have I seen so closely argued an analysis of our failure to present reality — there are no independent systems. The editors know this so that their concern then is to show what happens when we lock ourselves away in little boxes. It sometimes appears that British social anthropologists have a completely different philosophical approach, a different *Problemstellung*, a way to fit the data into an ordained scheme of things.

But the book goes beyond what to this reviewer was a negative conclusion — *don't tresass without training and skills* — "a writer in prose should not read poetry, or a poet prose". The very studies the editor has selected indicate that modern social anthropology is approaching the age of maturity: we transgress because we need to. The age of comparative studies of customs and institutions is now being recast into the mould of total system analysis which requires that we see customs and institutions as being shaped equally by both internal and external agencies. And if we agree to this then it is a brave scholar who will stop when he reaches deep into the external.

Is it not better to be incompetently adventurous than to be competently restricted?

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Manual for Kinship Analysis. Ernest L. SCHUSKY. (Studies in Anthropological Method Series.) New York, Holt, Rinehart and Winston, Inc., 1965. vii-84 pp., 41 figures, 19 exercises, glossary, bibliography. $1.50 (paper).

Ernest Schusky's *Manual for Kinship Analysis* appears in a series presumably designed to give both students and outsiders a view of accepted anthropological methodology in particular sub-specialties of the discipline. This volume would have perhaps sufficed for this end had kinship studies ceased 25 years ago, but they did not, and the publication of this small book does advances since that time a serious injustice.

This may sound severe, but the comfort size, the attractive format, and the crying need for a concise introduction to 'kinship studies' all mean that this book likely will be used by a great many students — students who will accept the contents as a precise statement of the basics of kinship analysis, and who will assume that more advanced kinship studies are built upon this. Herein lies the trouble, for much recent methodology and theory suggest that a fundamental *rejection* of many of our previous tenets of 'kinship' (those which Prof. Schusky appears to accept without question) is required in order to get further. Our time is precious little at best, so to read and assimilate materials which must be 'unlearned' in order to advance seems to me extremely inefficient. Students today can ill-afford such sterile exercise.

The small book is divided into two major sections — the first dealing primarily with mechanics, principles of kin reckoning, and definition; the second portion chiefly with "patterned kin behavior" and marriage (of which Prof. Schusky says, "Marriage customs, as well as patterned kin behavior, are closely related to the study of kinship [p. 57]."). This is the tenor of the entire book. On the first page of the text, 'kinship' emerges as a phenomenon *sui generis.* Prof. Schusky is certainly not alone in this view, but the serious student should also know that neither is it accepted everywhere.

If this does not convince the reader of Prof. Schusky's orientation, he is left with no doubts at all when brief mention is made of recent kinship studies. Prof. Schusky refers to Murdock's paper (*American Anthropologist,* Vol. 66, No. 1, 1964, pp. 129-132) which he says (p. 65) provides a "most recent analysis" of bilateral groups, but without any reference whatsoever to Mitchell's excellent statements on the kindred, to which Murdock was in fact replying. From the same *American Anthropologist,* Prof. Schusky quotes Ackerman (Vol. 66, No. 1, 1964, pp. 53-66) as having "seriously questioned Needham's analysis (p. 62)", as if to dismiss it. The next issue of Vol. 66