

404

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Reply to D. Paul Lumsden Review

Translated by Patricia Klammerth

To sum them up at the outset, Professor Lumsden's conclusions concerning our Anyi volume are the following: „...the study's theoretical, methodological and ethnographic flaws are deep and real" (L p. 26), and "...the Anyi study must be judged a failure..."(L p.27). In effect, our reply to these two statements is already on record. Following the initial publication of the German original in 1971, we agreed to two further, unrevised editions in German; we went to considerable trouble and expense to have the English version published -unfortunately in very condensed form; and between 1971 and 1979 we published twelve additional articles (PMP Ref. 154,157 ,158, 160,161,162,163, 164,165,167 ,168, 169) which were based in part on the method employed in the Anyi study and on the study's findings. Thus we find it superfluous to make still another attempt to justify in different words our conviction that the study cannot be regarded as a failure. Perusal of the "highly critical remarks" (professor Lumsden's letter to Parin 14 September 1981) has done nothing to change this conviction.

Instead of defending our publications, we would like to try to show just how professor Lumsden may have arrived at some of his critical remarks. We cannot deal with all of them: if we did, our commentary would be

405

longer than his Essay. But by proceeding in this fashion we may be able to demonstrate more clearly the fundamental difference between his evaluation of the Anyi book and our own. Our task is facilitated by the precise references given in the Review Essay. Naturally, it must be understood that our comments refer exclusively to this Review Essay and the arguments advanced in it. They do not in any way imply an evaluation of Professor Lumsden's scholarship in general or of his person.

The reviewer has three idiosyncrasies which have gotten in his way repeatedly in his polemic: *the way he reads, his utilization of scientific literature, and his stubborn conviction* that our work fails to meet the logical and ethical criteria of anthropological research.

In many cases Professor Lumsden seems incapable of understanding the sense of what he reads; in some cases he even manages to distort what we meant into its exact opposite. The following two examples should suffice.

Summarizing his criticism that we had -improperly -neglected the aspect of somatic disease, he states that (Parin and his co-workers) "...have rendered their characterizations of the Anyi even more suspect or unlikely. Indeed, they libel the Anyi in ways they do not grasp (cf. 2-3)" (L p.10). In the initial approach to our topic (PMP p.1-3) we explain under the somewhat fanciful heading "Caligula Africanus" that, like all anthropological researchers, we are proceeding on the premise of an inevitably erroneous picture, "such as is normally handed down by tradition in our culture," but at the same time that "it is the purpose of scientific investigation and description to reduce the number of these inaccuracies," and that "we are convinced that we have found an appropriate tool in the psychoanalytic method..." Surely one cannot interpret this passage as an admission that, even after the conclusion of our investigations, we had , "libelled" the Anyi; and even less does it imply an admission that we had failed entirely to understand them. We do add the following caution, though: "Anyone who rejects Freudian psychoanalysis...is advised at this point to read no further. „For such readers will certainly not be able to acquire any deeper insights about the Anyi from our research. But Professor Lumsden cannot seriously assume that three experienced psychoanalysts will disavow their own method or that they have failed to read their own book in detail.

No less crass, but even more consequential is the following example of the way in which the reviewer reads. He quotes almost nine lines from the first part of Chapter 8 (PMP p. 315-316) and comments: "Couched in terms of Freudian psychoanalysis and of Ego Psychology, and perhaps reflecting the analysts' countertransference, their approach yielded the following characterization of Anyi 'modal psychology.'" In a footnote (L p.30), citing three recent works (Panken 1981, Roland 1981, Rycroft 1972), he refers to the well-known fact (stated exactly 70 years ago, in 1911, by Sigmund Freud) that "countertransference" can interfere with "correct interpretation",

406

and concludes with a mention of "the heavy influence of Margaret Mahler on the Parins' recent writings..." -as if that had anything to do with countertransference. The part of our book from which the nine-line quotation was taken bears the title "The Prerequisites of Metapsychological Description" (PMP p. 314). It would be odd indeed to find a conclusion or a "characterization" under the heading "Prerequisites." The quotation contains not a single psychoanalytical or ego-psychological term. Finally, it has escaped the reviewer's notice that the material quoted is from

the beginning of the chapter on "Metapsychology" (58 pages), in which the psychology of the Anyi is examined with the greatest possible care. This chapter as a whole (and not merely its terminology) is in fact based on psychoanalytical theory and could well be characterized as "modal psychology." This, of course, is the sense in which the phrase "modal psychology specific to their culture" (PMP p. 379) must be understood. (In the original German the phrase was "die kulturspezifische Psychologie" (PMP 1971), which could be translated as "Psychological outfit specific to their culture" or "culture-specific psychoanalytical model.") It is difficult to understand what prompts the reviewer to look for "shorthand" terms, not only in this instance, but in many others as well, in a volume whose purpose from beginning to end was to do full justice to the complexity of the phenomena described. He may have in mind Abram Kardiner's (1945) „basic personality," since he mentions this author in a footnote and suggests that a comparison of our thought with his might be rewarding. (Kardiner's reductionist approach has been criticized by us in earlier articles.)

The reviewer substantiates his statements with references to an imposing bibliography of some 45 books and articles. Among these 45 there is one 17-page article (L Ref. Lystad, 1959) we *did* fail to consult that might well have made a genuine contribution to our work. Otherwise, however, the reviewer's approach to the relevant literature is unsatisfactory, to say the least. Repeatedly, he criticizes us severely for not having read certain publications which appeared only *after* our book had been published.

One example: "It is a great pity that Parin and his co-workers have not consulted Haliburton's 1971 biography of *The Prophet Harris* (1973)" (L p.24). For the „number of historical and ethnographic errors" (L p.3) that can be found in the text primarily as a result of these sins of omission is so legion, says the reviewer, that „I do not regret the deletion of 'a fairly exhaustive discussion of the history of the Akan peoples' „ (L p.3).

And the above is by no means an isolated instance. Professor Lumsden 's bibliography (L p.38-42) contains no fewer than 18 anthropological works we should have read that were published *subsequent* to our own book and papers. The reviewer is fully aware of the fact that both our book and the article on the „Prophet" Edjro Josue appeared in 1971 and that both were abridged, but *not* revised (L p.3) for English translation. One might interpret

407

this grossly unfair criticism to mean that the reviewer is of the opinion that the nine-year-old work ought to have been revised prior to having it translated. Why does he not simply say so?

The reviewer restricts himself to citing or quoting only those anthropological works on the Anyi which have appeared in English; he mentions not a single French-language source. Inasmuch as the Anyi live in the Ivory Coast Republic, i.e. within the territory of a former French colony, this means that Professor Lumsden is simply disregarding a great part of the literature. This would not be quite so annoying if he did not persist in presenting all of his quotations as correct and all of ours as dubious or erroneous. Nor can his attitude be explained by the possible lack of a knowledge of French, for an article in English by the Dutch anthropologist A. J. Kobben (PMP Ref. 26), who is also the author of the classic work on the social, political, and economic structures of the Anyi (PMP Ref. 25), was also ignored completely. Kobben completed his research work in one of , „our“ villages (M'Basso) in 1956, ten years before we began. Had the reviewer consulted Kobben, he could hardly have set aside our characterization of economic conditions with the categorical statement: „These claims are grossly wrong“ (L p. 20).

Professor Lumsden's approach to our publications and his irrational treatment of the available literature cannot be accounted for entirely by his critical view of our methods and his doubts concerning our findings. The entire Review Essay is permeated by a conviction that we simply do not possess the moral or logical prerequisites for anthropological research. It is this that he is trying to prove.

We could, of course, simply permit Professor Lumsden's statement „I am not being patronizing toward the authors' labours“ (L p. 26) to stand unchallenged, accept his quasi-praise („It is laudable that Parin and his co-workers are not uncritical of their own ideas and data...“ (L p.24), and break off the discussion at this point. But this would not produce the wide-ranging discussion desired by him (letters of 14 September and 12 October 1981) and by Dr. Howard F. Stein. If we have consented to such a discussion, then, it is because the reviewer does, on occasion, advance arguments that seem deserving of wider attention.

Before we begin the discussion of substantive problems, perhaps it would be wise to point out some of the specific criticisms that have led to the reviewer's overall opinion of the logic and the ethics of our research. Professor Lumsden finds that „it should be seen as a breach of ethics“ that we have published „identifying photographs“ of some of our analysands (L p.24). He deplores the fact that, instead of observing the established Western practice (according to which the physician or researcher has the sole right to decide what to reveal about his subjects), we permitted the Africans themselves to decide what we should do about identifying them (PMP p. XIV). „The authors do claim to have read Rattray's works“ (L p.22)

is a clear reproach, obviously because we have not made uncritical use of all the statements of this classic anthropologist (classic as regards the Ashanti, that is). First, Rattray's books are fairly old (PMP Ref. 32,33 1923, 1927) and thus in some respects outdated; second, his findings are questionable in a number of respects because of his methodology; and third, we were not concerned primarily with the Ashanti, and a good many of Rattray's statements about the latter are applicable to the Anyi only to a limited extent. Not only are we unable to read, we also are incapable of expressing ourselves –“It also is a pity to find some colonialist terminology (re 'heathens') cropping up...” (L p.24). Since the term „heathens“ apparently has a pejorative connotation in English, this must have been an oversight on the part of the translator (the only one, we might add, that has been called to our attention so far). The German word „heidnisch“ (= heathen) means simply „belonging to a nonmonotheistic religion“; it has a pejorative association only in the context of certain fanatic missionary movements. To continue, „West African mothers...resort „to the use of an enema which in most places is given by means of the mouth.” They have now changed ends...” (L p.32). The sentence quoted is from the English translation of an earlier publication (PMP Ref. 156); Professor Lumsden's ironic apercu is the sort of remark that could be made only by someone bent on proving that we are unable to express ourselves correctly and someone who has never heard, or observed, that African mothers sometimes make use of a piece of bamboo or a papyrus reed, through which they blow the chili suspension from their mouths into the child's rectum. (Nowadays the Anyi generally use rubber syringes to administer their enemas.) Professor Lumsden also states that we do not know whom we ought to be investigating, „the king's sister,” for example, for we „do not seem to have even interviewed this (non-French-speaking) elderly woman...” (L p.35). Ahoussi de Bernard himself was born in 1880 (PMP p. 20) and thus was 78 years old at the time of our investigations; as we point out, his sister was even older (84, to be exact). Does Professor Lumsden assume that it was merely negligence that prompted us to refrain from subjecting her to interviews 'carried out with the assistance of an interpreter?

„The authors make the erroneous assumption that there is cross-cultural or academic agreement as to what 'masculinity' really *is* and how 'it' is displayed“ (L p.15-16), and Professor Lumsden wonders whether a number of our metapsychological comments do not „reflect the European authors' own 'patriarchal' view of women's proper place“ (L p. 35). „Even psychoanalysts must collect and provide adequate census data“ (L p.7), and „The analysts totally have failed to investigate for a somatic contribution...” (L p .9) . According to the reviewer, we repeatedly make historical and other errors, underestimate, fail to grasp, contradict ourselves, and „seem to downplay...socio-economic data because of (our) enematic focus“ (L p.21).

409

We are of the opinion that the resulting image-one of sloppy and superficial anthropologists who are adherents of culturism, pro-colonialism and thus bigoted, unable to read with understanding and incapable of expressing themselves with accuracy -applies to us just as little as that of naive armchair analysts who cannot think beyond their own ingrained prejudices concerning what is meant by „male“ and „female“ and have no grasp of reality.

Let us see just what Professor Lumsden would like to see corrected, just what annoys him so greatly in our book, and why.

He has detected three errors, quite possibly made by the typesetter rather than the translator: 1) „patrilineal structure“ (L p.22, PMP p.5, line 1) should, of course, read „patriarchal structure“; 2) The remark to the effect that „...Fanti' and 'N'Zima' (sic) are not 'cities' ...but rather are Akan ethnic groups“ (L p.23) is technically correct; its ironic tone seems out of place. The word order should be rearranged to read „...impressively designed cities of the Ashanti, Fanti, and N'Zima...“ (PMP p. 78-79). That Fanti and N'Zima refer to ethnic groups is clearly stated in the enumeration of the Akan peoples (PMP p. 17); 3) The third error, „Kwah“ (L p.22, PMP p.17), is incorrectly corrected by the reviewer. The proper spelling is „Kwahu“ (Morgan and Pugh, 1969, p. 274), and not „Kwawu.“

Professor Lumsden is perfectly right when he states that the index of our book is inadequate. Because of the heavy abridgement of the text required by the publishers, we had suggested omitting an index altogether. The complete index of a comparable volume (Kracke) covers 22 pages. The publishers insisted on at least an abbreviated index, however, and thus it seemed only logical to exclude those terms which were referred to with extreme frequency.

Professor Lumsden could have spared himself the remainder of his corrections if he had read our text more carefully. He points out, for example, that „...the Portuguese did not introduce all domesticated plants...“ (L p.22); the passage he objects to is the following: „...Portuguese traders had introduced domestic plants...“ (PMP p.19). Or, „'The use of money' was not a European innovation for all West Africa (cf. cowry currency)“ (L p. 23). In the passage concerned (PMP p. 92), the phrase „the use of money“ refers back to the previous sentence, which speaks of „the introduction of a money economy“ (quoted from PMP Ref.65). The reviewer is not convinced „that all or most such women (female shamans) are 'lesbians' „ (L p.8); at no point do we state that all female shamans are lesbians (noun), merely that those whom we observed were lesbian (adj) in their behavior. Professor Lumsden notes that „...a deceased Asantehene's blackened stool is quite a different matter from the one and only Golden Stool“ (L p.22). And the distinction is perfectly

clear in the passage cited (PMP p.30). We refer to the first „'golden' stool“ and continue in the following sentence: „Together with the original one, the

410

blackened stools of later kings...were preserved...“ Professor Lumsden informs his readers that „Asantehene Osei Tutu died 15 or 20 years before the 1731 date given on p. 19“ (L p.22). This is one of the numerous assertions which the reviewer substantiates by reference to the literature (in many cases of works that have appeared since the publication of our book). The year given by French-language authors for Osei Tutu's death (most recently in Ki-Zerbo's history, 1978) is different than the one the reviewer happens to quote.

A large number of Professor Lumsden's other *historical criticisms*, e.g. in regard to the messianic healer Edjro Josue (L p.24), can be accounted for by his total disregard of the literature available in French and his determination to accept only what he himself has seen in print. On the other hand, we certainly do not wish to exclude the possibility that the later historians whom he cites may on occasion have interpreted doubtful historical events more accurately. But the reason for a good deal of the confusion created by Professor Lumsden is very simple -he has unaccountably overlooked completely our statement „Since their exodus to their present territory at the beginning of the nineteenth century, the Anyi have developed a history of their own...“ (PMP p.18, 19). He constantly confuses the Anyi with the Ashanti and takes us to task for describing conditions among the Anyi which Rattray (PMP Ref. 32,33) or Busia (PMP Ref. 6), both of whom were writing about the Ashanti, had presented in an entirely different light. In similar fashion, Professor Lumsden claims that we have misrepresented economic or ethnological phenomena, for example „...*sunsum* does not absorb *ntoro* 'at puberty'...“ (L p.29). The situation happens to be different among the Anyi of Alangouan than among their cousins, from whom they have been separated for approximately 160 years. The reviewer only compounds the confusion further when he *equates* (instead of *comparing*) the Anyi with other neighboring matrilineal people and expresses his indignation at the fact that we describe phenomena among the Anyi which do not exist in the same form among the Ashanti or the Abron. We are at a loss to understand, incidentally, why he refers consistently to the Ashanti and the Abron, and never to the Baoule, a people that in many respects is just as closely related to the Anyi.

We realize that it may be somewhat difficult to see why, in our historical account of the Anyi and in our description of their „culture“, we sometimes refer to Anyi traditions held in common with other ethnic groups of the Akan peoples (the Ashanti, for example), and sometimes treat the Anyi as a separate entity. If it had not been necessary to compress into a mere four pages (PMP p. 17-

21) the 47-page historical survey (including the colonial period) that appeared in the German original, much might have been clearer. Nevertheless, in our opinion Professor Lumsden's practice of criticizing historical and anthropological statements concerning a specific ethnic group as erroneous merely because different conditions

411

happen to obtain within another group (e.g. L p.19-22) is totally arbitrary. Since the reviewer is in any case unfamiliar with the basic source works on the *economy* of the Anyi (L p. 21, PMP Ref. 14,25), and in addition persists in treating the Anyi of Sanwi, who live along the coast, and the Anyi of Alanguan, who live in the interior, as a homogeneous entity, there seems little point in demonstrating in detail just why his speculations on the economy of the Anyi are bound to be unfounded.

On the other hand, his assertion that the Anyi must be good businessmen because they can look back on a long past as traders and cocoa planters, „...an adequate time-span within which effective 'adaptation' (to trade) would proceed...“ (L p.21 and p.36) has to do with the fundamental task of ethnopsychanalysis. It is precisely the fact that-in spite of their long decades of experience-the Anyi have failed to develop a talent for business and to evolve a social class of businessmen that implies a need to look for additional, possibly psychological factors in the fabric of their social life to account for this failure.

Professor Lumsden finds that „it is hard to believe our authors' claims that 'class' differentiation is barely 'rudimentary' and class conflict is nonexistent...“ (L p. 21) , namely because „rich men“ exist and „money lending is 'very popular' ...and results in 'ruinous transactions and a great deal of strife“ (L p.21), and deplores the fact that „Our authors seem to downplay such socio-economic factors...“ (L p.21). Apparently he simply does not know that social classes are determined by the relations of production, and not by the ownership of property, and thus implicitly concedes that he is totally uninformed in this particular area. Obviously, then, there is nothing to be gained by any further discussion of his views on economic theory.

The application of the psychoanalytical method is justified, according to Professor Lumsden, only when a „random or stratified *sample*, „ based on „adequate census data“ (L p.7), has been investigated with the help of psychoanalytic techniques. Moreover, again according to Professor Lumsden, our sample should naturally have included older men and women, witches, chiefs, and „queenmothers,“ properly distributed among patrilocal, matrilocal households, etc. We would prefer to leave it up to the sociologists to decide how many individuals would have been required to establish a „stratified sample“ of the

approximately 1,000 inhabitants of Alangouan, or possibly for the far more than 100,000 Anyi making up the ethnic group as a whole. Considering the numbers and variety of sociological parameters, even a „random sample“ could not have been appreciably smaller if it were to have the degree of „representativeness“ (L p.8) which the reviewer considers desirable. Apparently, with this point of criticism, Professor Lumsden is giving us to understand that he considers the entire ethnopsychanalytical approach pointless, for no research team could even begin to meet these requirements in practice. Since the appeal to

412

statistical relevance is frequently justified by the number of necessary data, psychoanalysts have repeatedly pointed out that it is not the number of individual analysands that is important, but rather the number of individual bits of information obtained. And even a single psychoanalytically oriented interview reveals many hundreds of such bits. This statement is technically true, but it fails to take into account the fundamental difference between studies that are positivistically oriented and extensive and studies that are intensive. We see no need to explain our standpoint, which „...left (the reviewer) unpersuaded...“ (L p.8), in any greater detail (PMP Ref. 162, 165, 169). We would, however, remind the reviewer of certain specific aspects inherent in psychoanalysis. In „The Interpretation of Dreams“ (1900), Sigmund Freud was able to arrive at certain fundamental and still valid conclusions after studying the dreams of only a very few persons, including himself; his „Studies on Hysteria“ (1895) is confined to the case histories of four women belonging to the upper middle class of the Austrian capital. The most important attempts to establish a psychoanalytical characterology (Sigmund Freud, Karl Abraham, Wilhelm Reich) have contributed to laying the foundations of psychoanalytical anthropology, and in no case did the researchers waste a moment's thought on whether or not the persons they examined constituted a representative sample of the culture concerned or a statistically relevant selection from among its various social groupings. Conversely, what has so far been the most ambitiously conceived and most carefully planned project (Leighton et al., 1963) to provide definitive statistical coverage of at least psychiatric disturbances in a given African ethnic group has yielded almost no usable findings at all. The reviewer is demanding from psychoanalysis something which it is neither required nor able to provide, and in this he goes even further than LeVine (L Ref., p. 203-214), who is frank to admit his doubts about the practicability of ethnopsychanalytical research, but at least considers its possibility worthy of thorough discussion.

A fundamental objection of the reviewer is the following: „...ethnopschoanalysts *must* first investigate and rule out any role being played by *somatic disease* and nutritional deficiency in the symptomatology and 'modal psychology' of interest, before proclaiming *and testing* a psychodynamic aetiology“ (L p.10). We cannot accept this objection as valid.

The „condensed version“ of our book makes brief mention of our medical office hours, and the reviewer has taken note of our references to somatic disease (L p.30). Unfortunately, the short section dealing with „Somatic Disease and Hypochondria“ in the German original (PMP 1971, p. 388-390), which points out that we carried out physical examinations of 660 patients and conducted 2,200 medical consultations, had to be omitted in the translation. But presumably this discussion would not have satisfied the reviewer either, since it does not include any medical examinations of

413

our analysands. Professor Lumsden is apparently unable to distinguish clearly between somatic and psychic disturbances, possibly because he lacks the necessary medical and psychopathological knowledge. As soon as one ceases to describe psychic symptoms exclusively from without, as deviant or pathological behavior, but instead engages the sufferer in a conversation, it soon becomes a simple matter (except in very rare cases) to distinguish the psychic consequences of cerebral or other somatic ailments from psychogenic manifestations. Acquisition of the necessary knowledge in this respect is a basic prerequisite in any and all psychoanalytical training. The reviewer's suggestion that the explanation for hypochondriacal complaints must be sought in a medical examination can no doubt be attributed to the facts that 1) he is not aware of how hypochondria manifests itself psychopathologically, and 2) he is unwilling in principle to accept our analytical interpretations. As a result, his views are naively reminiscent of the anxiety-laden fantasies of the hypochondriacs themselves. (Our case reports, Suzanne for example (PMP p. 130), contain a number of good examples of the way in which psychic processes can trigger off psychosomatic ailments.)

A more exact pharmacological analysis of the suspension used by the Anyi in their enemas would have been appropriate only in a study of Anyi folk medicine, which in itself would indeed be of great interest. For the purposes of our investigations, however, it could not have provided any useful insights whatsoever. For the layman in psychiatric matters, this can perhaps be made clear with the help of an analogy. Let us assume that a patient who is addicted to codeine comes in for psychological examination and treatment. Under these circumstances it is totally irrelevant to know that codeine is an effective cough remedy, and it makes no difference whether the patient

began to take codeine to relieve his coughing, or whether he simply takes it without ever having suffered from a cough. For a rationalization such as the one that chili-pepper enemas are an effective remedy in the case of intestinal infections, it is irrelevant whether these enemas are good for the patient's health, have no effect whatsoever, or are physically harmful. Only the context in which they are administered and their immediate effects (cramps, pain, emotional reactions) are of importance in assessing their psychological significance. The Anyi themselves, incidentally, gave a variety of reasons for their enemas, by no means all of them having to do with hygiene. Presumably the suspensions made up of extracts from and/ or parts of plants do contain pharmacologically effective substances, but probably in inadequate dosage. This is true of a great many of the herbal remedies used by West African peoples. In the case of infants and toddlers, this usual dosage is apparently harmful. At the time of our study, according to the doctors in the District Hospital of Abengourou, approximately 40 small

414

patients died each year of volvulus (stoppage of the bowels), and in these cases no other pathological agents could be found apart from the excessive chilipepper enemas. This is a very high figure when one considers how reluctant the Anyi are to take their children to a hospital in the first place, and the fact that the extremely competent surgeon on the staff at that time was able to save numerous children by means of an ileus operation. Here, too, there is a lack of exact statistical data, a lack which does nothing to detract from the validity of our findings, but which certainly ought to be made a concern of the statisticians employed in the public health services. Professor Lumsden states categorically: "Clearly it is this somatic health situation, and not psychodynamics, which is explanatory for much if not all of the Anyi's 'widespread hypochondria,' of their 'general predisposition to depression,' their 'often seemingly dull-witted children,' their 'tendency to tire so quickly', and surely is explanatory too for much of Anyi 'paranoia,' 'despondency,' 'stupor' and potency problems' (L p.9). To the anthropologist with the unfortunate idea that he is called upon to discuss medical matters, with the aid of a quotation here and there from the literature, with a team of experienced physicians who have carried out extensive medical work in the land of the Anyi the only proper reply would seem to be: „Si tacuisses...“

We could, of course, be brief in the *discussion of our actual method*. Professor Lumsden is simply not prepared to accept the basic premises of ethnopschoanalysis. In our Preface we wrote: „Even at the outset we had not expected to find simple, easily defined recurrent phenomena that would 'explain' the life-style and culture of the Anyi...“ (PMP p.XIII). Professor Lumsden has

misconstrued an apercu intended to capture our initial phenomenological impression in a metaphorical nutshell and to serve as an introduction to the psychoanalytical discussion (see above) as being a conclusion drawn on the basis of our research. He calls it „modal psychology“ and then proceeds to prove that it is erroneous in every respect. Finally he reveals what he had expected to find, namely „...a Stress and Coping-focussed, social phenomenological Medical Anthropology...“ , and criticizes the absence of „the *study* of Suffering“, for „...societal reform-must be central“ (L p.27). (Incidentally, as regards this appeal for greater consideration of the psychohygienic aspect, we had already drawn up a plan which we presented to the government of the Ivory Coast Republic. This plan is outlined briefly in one of our articles (PMP Ref. 154, p. 65).)

What Professor Lumsden expected and what he demands is neither unfair nor unreasonable. It is merely something entirely different from what we are able to offer. We had asked ourselves at the outset whether it was wise to write a psychoanalytical treatise in which not only the semantics of the terminology, but also the entire method as such, including its observation techniques and its underlying theory , were bound to be unfamiliar to

415

many (though by no means all) scientists from other disciplines and for this reason alone likely to be misunderstood. We would have been delighted to be in a position to report on a psychoanalytical research project in terms that everyone could understand, even readers who knew nothing about psychoanalytical thought, and readers who held it in low esteem.

Unfortunately, this was impossible. Again and again (L p.4, for example), the reviewer compares statements on the overt external behavior of an individual with its psychoanalytical interpretation and finds the results senseless and contradictory. But any attempt to compare phenomenological or psychiatric diagnostic terms on the behavioristic level with unconscious processes, including emotional events, is tantamount to an attempt to compare chalk with cheese.

That even the *technique of psychoanalysis* is unfamiliar to the reviewer is demonstrated in countless instances. He finds that we have failed to assess properly the very real distrust of our analysands toward their analysts (based on their fears that we might steal their gold, on our friendship with the King as an authority figure, etc.) (L p.5,6), despite the fact that this point is discussed in detail (e.g. PMP p.21-27). Of course he is unable to explain why this distrust, which can intensify to the point of paranoiac anxiety, should also affect other Anyi with whom we had no contact whatsoever (as was shown repeatedly in all the interview sessions). He also suggests that we have confused an inadequate mastery of French with a deterioration of speech (L p.6). But

how could an inadequate knowledge of French explain the performance of an individual who speaks perfect French for several hours, then suddenly begins to produce „word salad”, and just as suddenly regains his previous command of the language, or our observation that the same thing happens to the Anyi when they are speaking together in their own language-so often in fact that they have evolved traditional remedies for such lapses.

Professor Lumsden reveals most clearly that he knows nothing about psychoanalytical procedure, or that he considers it worthless, when he takes individual interpretations out of their context and appeals to „...the place and importance of ordinary common sense“ (L p.23) to demonstrate how ridiculous the interpretation is. „When a baby happens to urinate, must this really and always *mean* that its mother is 'overflowing' with feeling?“ (L p.23). When psychoanalysis was first introduced some 80 years ago, Sigmund Freud was constantly confronted with deliberately erroneous interpretations of his analytical interpretations. Critics claimed that his interpretations of unconscious processes were a denial or a distortion of reality. Today, psychoanalysts have a right to expect critics who wish to be taken seriously to refrain from repeating such nonsense. In view of his inability to comprehend the purpose of the psychoanalytical investigative technique, it is not surprising that the reviewer also has distorted views of *psychoanalytical theory*, of the way in

416

which its concepts take shape, and of what it is intended to explain and can explain. Above all, he seems to think that what we were engaged in was „psychodynamics“ (L p.2, 33), notorious as the shorthand extract of psychoanalytical thought for the everyday use of the busy psychiatrist. (The term probably originated with Franz Alexander; Sigmund Freud never used it.) In order to obviate just such misinterpretations and analogous misunderstandings, our book includes a separate section (PMP , 'Culture-Specific Metapsychology', p. 333-334) discussing , „how metapsychological explanations come about and what purpose they serve.” In spite of this, the reviewer accuses us of having modified some of our theoretical formulations during the past few years (L p.33), disregarding completely the fact that one important aspect of metapsychology is the principle that a theory , i.e. a group of more or less satisfactorily proven working hypotheses, must be modified in the light of direct observation. („Psychodynamics,” on the other hand, refers to the practice of explaining the phenomena observed with the help of a ready-made repertoire of psychological shorthand terms.) That the reviewer reproaches us with having replaced „imitation and identification“ by „introjection“ „in West Africans' psychodynamics“ (L p.33) is just about as logical as if a critic reproached a geographer for writing on one occasion that West Africa has

railways that run on electricity, and on another that West Africa has railways that use diesel locomotives or steam engines. Apparently, authors are expected to make up their minds definitely one way or the other, so that the reader will have an easier time of it!

Long stretches of discussion in the Review Essay suggest that the author's objectivity has been clouded by his inability to understand that the social, political, and sexual role of the father, i.e. the male, with all the prestige, power, privileges, and obligations accruing to him, is not identical with the formation and the vicissitudes of the representative „father“ or „male“ (L p.15-20). When Alexander Mitscherlich, in his book „Society without the Father“ (1969), attributed certain changes in the behavior patterns and psychology of the Western industrial nations to the „absence“ of the father in the Western nuclear family, he certainly did not intend to imply that our young people are the result of parthenogenesis or have grown up as illegitimate children, or that the leadership role in society, government, or the economy has reverted to the female. Although we refer repeatedly, in detail and in summary form, to the roles, functions, and importance assigned to the men and fathers (as well as to the women and mothers) in the present-day (1966) Anyi world of Alangouan, Professor Lumsden refuses to believe us. Almost beseechingly, he offers us the „...five 'types' on a 'gradient' of domestic groups authority, 'from extreme brother control to extreme husband control' „ (according to Alice Schlegel, L p.33), and reminds us urgently that at least in patrilocal nuclear families the „father“ is bound to play a larger, more significant role in the

417

psychic development of his offspring-as if we ourselves were these unfortunate (and in the eyes of the reviewer misinterpreted) Anyi youths who seem so little inclined to develop into fathers and males in accordance with the pattern of their „culture.“ He refuses to see that one of the most significant findings of our research is precisely the explanation for this dichotomy within the Anyi community-namely that it *has* chiefs, fathers, and male figures, both in reality and in the form of fantasied ideals, but that in spite of this so few of its young men are able to assume these roles effectively. Perhaps Professor Lumsden would have found it easier to understand our findings if he had borne in mind the Anyi proverb „When the king' s breasts are full of milk, it is his people who drink“ (PMP p.29), and if he had been able to read the biography of „our“ old village chief and king (unfortunately not included in the abridged translation). The reports of our psychoanalytical interviews with another village chief, Brou Koffi of M'Basso (also omitted from the English volume), on the other hand, would

only have confused him; he would have been at a loss to comprehend how such a man was able to function as a respected and efficient village chief (that is, as long as he was identifying with his role and its functions), and at the same time to experience himself as a passive, „womanish „ man, abjectly dependent on the world of the mothers, plagued by anxieties that immobilized him completely. So far we have no socio-anthropological formula and no brief „modal psychology“ to explain this discrepancy-only the endlessly painstaking procedures of ethnopsychanalytical research, which the reviewer is incapable of understanding or appreciating.

It remains to inquire just how the reviewer arrives at what he terms our „*enematic-matricentric explanatory system*“ (L p.14). Professor Lumsden 's Review Essay (beginning with the title) has indeed transformed

our book into something which is „ ...both too culture-specific and extreme...“ (L p.14). He has managed to reduce a highly specific, complex, and necessarily incomplete account of dialectically interdependent factors to a monocausal explanation (cause x leads to result y), only to conclude with indignation that our project was a failure. He finds our „bald claim“ that „social factors have no causal relationship with psychological factors“ unacceptable (L p.23), and even believes that he has found proof in a recent publication (P 1978, PMP Ref. 165) (exemplifying a method in psychosocial dialectics) that we, too, are adherents of this same causality. We recommend Robert F. Murphy's „The Dialectics of Social Life“ as an introduction to dialectical thinking in the field of anthropology. Since it is not based on the psychoanalytical method, it is quite possible that the reviewer may find it more comprehensible than our publications. If it is true (as it is) that these ominous chili-pepper enemas, together with the emotional withdrawal of the mother during the baby's second year of life, exert an important influence on the psychic development of the Anyi, an influence that makes itself felt in a very special way in the specifically ordered

418

areas of social functioning in which Anyi children grow up and that affects and reinforces *their* matrilineal kinship structure in spite of the influence of a wide variety of historical, political, and economic factors, then why does the reviewer insist upon asking whether there are other matri- or patrilineal, Western or African peoples that also make a habit of enemas? The chilli-pepper enemas have a definite place in the deep-rooted interdependence between socialization, psychic development, and unconscious processes on the one hand, and ecological, historical-political, and a wide variety of economic and social processes on the other hand. For an Anyi infant who still enjoys the nutritional attention of the mother, they are merely a source of discomfort and occasionally result in serious impairment of his health; toddlers, who are emotionally and

sometimes even physically isolated from the mother, experience them as a violation; older children, and many adult Anyi, are addicted to them. Because they are psychically processed, enemas help to form unconscious fixations that subsequently make themselves felt in the fabric of social life, including the traditional matrilineal structure characteristic of the Anyi of Alangouan, and that help to explain some contradictions and tensions. It is evident that the enemas of the Anyi have prevented Professor Lumsden from producing a more balanced assessment of our work.

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