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Why leave so much unsaid and unargued? (Review of:) A handbook of method in cultural anthropology, edited by Raoul Naroll and Ronald Cohen; Published by the Natural History Press, New York 1970; 1017 pp.

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WHY LEAVE SO MUCH UNSAID AND UNARGUED?

Review of *A Handbook of Method in Cultural Anthropology*
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Natural History Press, New York, 1970; 1017 pp.

The Handbook discusses the comparativist method at length and from a positive viewpoint, makes brief mention of structuralism (pp. VII and VIII) and of holistic approaches (p. VII), treats the material from a philosophic-idealistic standpoint (e.g. Ch. 3) and ignores Marxism completely.

Both in part I and parts V-VII of the book a number of scientific requirements are stated and worked out. On the one hand these boil down to, "... proving, disproving or creating generalizations that transcend any specific culture..." (p. VI), or, "... accumulation and expansion of explanation... rather than ethnographic information in and of itself..." (p. VII). On the other hand they concern such general methodological and theoretical principles as the prevention of contamination, avoidance of begging the question, and so on.

The present review is focused on the question of how much justice the editors of and contributors to the handbook have done to the latter principles. Assuming provisionally that these are scientifically acceptable — as the present reviewer does — it is easy to see that the value of a handbook of method will stand or fall according as to whether or not especially these principles are properly applied.

Below follows a summary of the topics discussed in the Handbook and the order in which the material is presented. The Chapter titles have been abridged here; the original numbering has been kept.

Preface.

Part I - General Introduction: 1. Method; 2. Logic of generalization; 3. Natural selection: epistemological model.

Part II - General Problems: 4. Cause and intention; 5. Linear causal analysis; 6. Theory of political development; 7. Ethnographic bibliographies; 8. Source criticism; 9. Cultures through time.

Part III - Field Work Processes: 10. Research design; 11. Experiments; 12. Experimental design in the study of culture change; 13. Entree into the field; 14. Art and Technology of field work; 15. Sampling in ethnographic field work; 16. Observing and recording behaviour; 17. Survey research; 18. Method in psychological anthropology; 19. Comparative method: single investigator and team approach; 20. Field-manual anthropology; 21. Group differences in personality; 22. Translating etc.

Part IV - Models of Ethnographic Analysis: 23. Role analysis; 24. Economic system; 25. Political system; 26. Religious system; 27. Personality as a system; 28. Study of literate civilizations; 29. Ethnoscience (Method and theory).

Part V - Comparative Approaches: 30. Comparativists and non-comparativists; 31. Concomitant variation studies; 32. Statistical studies of continuous geographical distributions; 33. The Human Relations Area Files; 34. Methodological findings of the Cross Cultural Summary; 35. Societal Research Archives System.

Part VI - Problems of Categorization: 36. Concept definition for comparative studies; 37. Taxonomy in comparative studies; 38. Coding ethnographic materials; 39. The culture-bearing unit etc.; 40. Empirical cross-cultural comparison; 41. Scale analysis; 42. Settlement pattern scale of cultural complexity.

Part VII - Special Problems of Comparative Method: 43. Cross-cultural sampling; 44. Data quality control; 45. Dimensions of error in cross-national data; 46. Data quality control and library research on political parties; 47. Galton's problem; 48. Ditto; 49. Museums.

Of these, Chapters 5, 7, 8, 10-17, 19-23, 28, 29, 34, 35 and 49 have so far not been read by the reviewer. It is evident from this that the reviewer has given first preference to chapters dealing with theoretical principles, as well as economics, politics, history and human personality. He has read most of the chapters treating these subjects, excepting the chapters on field work. Hence he has studied 600 of the 1000 pages of text (including bibliographies) so far. But he even so ventures the prediction that the contention of the opening paragraph of the present review will apply to the whole of the handbook, which it will be easy

enough for anyone taking the trouble to go through the remaining 21 chapters to check.

A contemporary handbook on methods in cultural anthropology ought in my opinion to have as one of its central themes the investigation into the question of whether or not a *combination* of certain empirically confirmed holistic methods *with* the comparativist procedure will yield the relatively best results. Instead this theme is only touched on incidentally here and there, as, for instance by Köbben (Ch. 30, esp. p. 593), the only form of non-comparativism that is mentioned at all being structuralism, moreover.

The total exclusion of Marxism (dialectical and historical materialism) is — if one proceeds on the basis of the conditions imposed on methodology by the editors of and contributors to the handbook themselves — one of the gravest shortcomings from which a modern handbook on methodology may suffer. In the first place, Marxism — irrespective of whether one is for or against and regardless of whether it will in the end prove empirically tenable — cannot be regarded as a negligible quantity anywhere in the world, not even in the so-called West. In the second place, the fact that a handbook of this kind passes Marxism over in total silence is to be regarded as no mere coincidence, but as a symptom of a *s y s t e m a t i c* attitude, even though this may not be a deliberate and conscious one. That it is systematic can be statistically demonstrated by checking the handbook itself on this, without necessarily going into elaborate calculations. And in my opinion it stems from what the editors and contributors of the handbook rightly do dread more than almost anything else — bias.

We look in vain in the handbook for any Marxist concepts — even in the context of the comparativist method —, if only as themes of discussion, as, for instance, culture in the Marxist sense, ditto society, classes, class interests, class viewpoints, class struggle, class consciousness, value (in the politico-economic sense), exploitation (ditto), mode of production, productive forces, production relations, and so on. This is inadmissible in a handbook published in 1970 A.D. in which the authors indulge in methodological discussions on such subjects as “Toward a Theory of Political Development” (Raymond Tanter, Ch. 6), “Cultures Through Time” (Jan Vansina, Ch. 9), “The Economic System” (George Dalton, Ch. 24), and “The Political System” (Ronald Cohen, Ch. 25).

Meanwhile, one comes across the valid thesis that, “Many concepts must be considered culturally biased because they are value-loaded or

have specific connotations in Western culture" in the Handbook (Tatje, p. 692). Strangely enough it seems not to have occurred to Tatje that at least two of the terms used by him in the sentence cited here probably cover such abortive concepts, namely the words "culturally" and "Western culture". As regards the former of these: where Naroll *et al.* use the word "culture", they seem to eliminate such possibilities, *judging from their own personal practice* (cf., for example, *Handbook* Ch. 39), as class interests, class viewpoint, and class consciousness, not to mention class conflict. Moreover, they do so *tacitly*. Thus it is all very well to introduce such a sweepingly general term as "Western Culture", but who is able to maintain that even the "shared, learned way of life" of class conscious workers is in the main socially akin to that of class conscious big entrepreneurs? The Marxists posit — and in my view they have practical reality on their side — that in this respect the way of life of each of these two groups is diametrically opposed the one to the other. Thus according to the present writer "Western culture" (also found, without any further definition, on pp. 21 and 491 of the Handbook) denotes in actual fact the culture of the dominant class and of those identifying themselves with the latter in a capitalist society. If the definition we have tentatively put forward just now is to be regarded as a hypothesis, such a hypothesis should at all events not be passed over in a modern handbook on method in cultural anthropology, while on no account should a different viewpoint be adopted tacitly and without any argumentation — as is regularly done in the Handbook.

The same applies to, among other concepts, "(Western) democracy" (*Handbook*, pp. 21, 459, 494), "Welfare state" (*ibid.*, p. 460), and "totalitarianism" (*ibid.*, pp. 958 ff.).

We shall give no more than these few examples.

Is there nothing in the Handbook that relates at all to Marxism? Of course there is — where does one find *nothing* that bears any relation to Marxism? But the concept "relating to" is a flexible one. Where this is a matter of more (bad) luck than management, dialectical and historical materialism are obviously not done justice — on the contrary.

We shall adduce one example here. In Chapters 40, 41 and 42 Tatje and Naroll (40), Carneiro (41), and McNett respectively present a number of lists which probably display some systematic connection here and there with a classification based on the *level of the productive forces* (Cf. De Leeuwe, *Bijdragen tot de Taal-, Land- en Volkenkunde*, *Anthropologica* 1970 and 1971). But the first four of these authors have *reduced* the concept of "level of the productive forces", which

is explanatory for sound reasons, to "degree of complexity" (pp. 767 ff.), a superficial, tentatively chosen characterization, which, though it may undoubtedly be provisionally of value in formulating, and subsequently testing, statistical comparativist hypotheses, is useless by its superficiality especially in an attempt to distinguish a contemporary socialist mode of production from a capitalist one, — these two perhaps hardly differing in "complexity"; furthermore, the socialist mode of production is losing certain features still preserved in the capitalist one. (And it is partly because of this that the characteristics in question are useless for certain scales discussed in the Handbook by the four aforesaid authors.) Meanwhile, the capitalist and present-day socialist mode of production differ enough ... for all big capitalists to oppose the latter mode.

2.

As far as the reviewer is able to judge, the Chapters on testing methods in the Handbook (and more particularly Ch. 30-49) are excellent on the whole. The discussion of Galton's problem requires further elaboration, notwithstanding the detailed attention given to this problem by the Handbook. Galton's problem; reduced to the simplest terms, involves that when using certain specific mathematical and statistical methods to determine whether there is any correlation between the presence of one particular trait and the presence of another particular trait within each of the societies ("culture bearing units") in which one might expect to find either one of these traits, one should observe the utmost caution in taking a sample from the universe of societies (according to Galton). For, as the argument continues, if the aim of the computation is to test a hypothesis concerning a functional correlation between the two traits in question, one should have sufficiently valid grounds for assuming that the correlation so established is in fact functional. That is to say, that the nature (or background) of the two traits in question implies that they have a non-incidental tendency to occur in combination (positive correlation), *even where they are not jointly spreading from a particular common centre.*

Of course in the latter case such a functional correlation is by no means to be ruled out — according to the investigators — but it is impossible to calculate this by means of an "ordinary" random sample. In other words, it is impossible to trace by means of the "ordinary" method in this particular case whether the two traits are directly or indirectly causatively connected with one another, either positively

or negatively, or whether there is a correlation only because the traits happened by coincidence to occur together initially — without any functional relation — and subsequently spread from one society to the next together.

As Galton *et. al.* say, one would therefore need a sampling method excluding to a large extent (preferably completely) the possibility of the societies of the sample displaying the two traits in question as a result of a kind of cultural oil stain effect, that is, without any functional character worth mentioning.

The solution to this problem might be to exclude from one's sample one way or another any societies (= culture-bearing units) which may be reasonably supposed to have pairs of cultural traits in common as a result of spreading. A random sample (where necessary a stratified one) should be taken from the known universe of culture-bearing units on this basis. The Handbook makes detailed mention of a number of solutions that have been found so far for Galton's problem (Ch. 47 and 48).

As contaminated societies (hence those that should be excluded from the sample in this particular case) they regard all societies which are located within too close geographical proximity of a society already included in the sample; and furthermore any societies which have developed relatively recently from a common ancestor with a culture that has already been included in the sample. In all these cases cultural traits might be correlated in a non-functional way as a result of systematic spread.

However, the Handbook *fails* to take into account, quite unjustifiably so, the possibility that the presence of contaminated societies in large, stratified world samples in its turn (a) is as a rule adventitious, and (b) does not as a rule affect the sample significantly. There are sound arguments in favour of this dual hypothesis, which the present writer has developed on the basis of the following fact: time and again it has turned out to make little difference to the statistical result of a prediction (the P-Value) whether or not one adopted the increasingly stringent safety measures of the solvers of Galton's problem when drawing a *stratified world sample*. One might adopt these precautions only where one wishes to make accurate calculations of functional correlations if one wishes to have the greatest possible certainty of avoiding oil stain correlations.

I hope to describe this hypothesis as well as its testing in *Behavior Science Notes*, also referring in so doing to the views of M. Ember ("An

Empirical Test of Galton's Problem", in *Ethnology* of January, 1971), which has come to my notice only recently. Ember also arrived at the conclusion (by a different route from that taken by the present reviewer, though also through practical experience) that Galton's problem need not be taken too seriously provided the other rules for drawing random samples are observed.

3.

Not infrequently are hypotheses (cf. the discussion in part 2 of the present article) developed, either partly or wholly, via exploration with the aid of a previously drawn sample. For testing such a hypothesis a *second*, comparable sample is essential. Naroll suggests this in passing, while Tatje and Naroll talk about "restudy" (*Handbook*, p. 770). However, the problem of replication of statistically valid empirical cultural anthropological research is not discussed explicitly or as a matter of fundamental importance — once more unjustifiably so.

The word "replication" is used several times in the *Handbook* (e.g. pp. 19, 64, 183, 617, 636, 718, 881, 882), but nowhere is it used with a statistical denotation in the definite sense of: calculating the results of one and the same prediction in constantly the same way with the aid of at least two equivalent independent and representative samples from the universe concerned.

In short, the *Handbook* contains on the one hand a number of methodological and theoretical data that are indispensable for the contemporary pursuit of empirical cultural anthropology. These it discusses and arranges in a series of cases in an efficient and lucid manner. The *Handbook's* greatest merit lies in the way it deals with the formal aspects of comparativist methods. On the other hand, the title is open to different interpretations, which is an undesirable state of affairs in the case of a handbook. "A Handbook of Comparativist Method in Cultural Anthropology" would have been less ambiguous.

But apart from this, the book has the essential shortcoming, as is apparent from the above (especially section 1 of the present article), of seriously violating (even within the framework of comparativism) certain general principles of empirical methodology — principles which the editors and contributors of the *Handbook* have in themselves formulated correctly and to which they allegedly subscribe.