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HISTORY AND SCIENCE IN ANTHROPOLOGY

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A RECENT presidential address by Mrs Hoernlé[1] deals largely and interestingly with the old question of laws and history in anthropology, but seems to rest on an incomplete conception of certain currents of recent anthropological thought. Particularly is this true of the attitudes imputed to Boas; and this misunderstanding, if real, is certainly of importance because of the outstanding position of Boas in contemporary anthropology. During the last forty years he has not only trained many of the American and some European ethnologists or social anthropologists active today, but certainly influenced all of them, at least in the United States. The question of his methodology is therefore much more than a personal one in its significance. On the other hand, individual elements inevitably do play a part; in fact it is probably ignorance of some of these that has led Mrs Hoernlé to fail to realize the Boasian position in its entirety. If therefore the following remarks seem at times to savor of the personal, it is because I believe it to be necessary for full understanding. And if I take on myself the presumption to act as spokesman or interpreter for another, it is for three reasons. The first is that as the leading public character of anthropology Boas is in a position where even his individual attitudes are of public concern. Second, I have been trained and influenced by him. On the other hand, and third, my methodological views do not wholly coincide with his, and I have been criticized by him for them, and have replied; so that I believe I can speak at least with a certain detachment.

1

To begin with, it is of indubitable significance that Boas' educational training was in the physical laboratory sciences—in physics, in fact. This led him into psychophysics and physical geography: his doctoral dissertation was on the color of sea water.[2] This in turn led to a one-man, two-year,

[1] New Aims and Methods in Social Anthropology (South African Journal of Science, Vol. 30: 74–92, 1933).

[2] Beiträge zur Erkenntniss der Farbe des Wassers (Kiel, 1887).

geographical expedition to Baffinland which brought with it intimate contacts with natives. The result was the "Central Eskimo" in 1888; and a career of anthropology since. From physics Boas brought into anthropology a sense of definiteness of problem, of exact rigor of method, and of highly critical objectivity. These qualities have remained with him unimpaired, and his imparting them to anthropology remains his fundamental and unshakable contribution to our discipline. Compared with them, his or others' views as to the degree of validity of sociologic laws or historical reconstructions are of only secondary moment.

This source from an exact and highly developed laboratory science is particularly significant by contrast. So far as I know it is unique for social anthropology, at least among its leaders; certainly it is exceptional. Inevitably it brings with it also certain limitations or colorings of objectives and method; and it is the non-recognition of these limitations that has led to misunderstandings like that of Mrs Hoernlé and many others.

Next, it is almost certainly significant that Boas stands alone also in having worked simultaneously in three fields as diverse as ethnology, linguistics, and physical anthropology; not merely with occasional sideline contributions, but with massive and basic ones. This fact of course presupposes a wide scope of method as well as interest, which must not be lost sight of in an attempt to understand his position in a single field such as that of social anthropology. The sole generally accepted department of anthropology in which he has shown little interest is that of archaeology. He has not even to any serious extent utilized the authenticated archaeological findings of others in his interpretations. In view of the fact that archaeology and ethnology both deal with cultural material, and somatology does not, this lack of preoccupation with archaeology while physical anthropology is actively pursued, may seem a strange inconsistency. It is entirely self-consistent, however, in that his basic approach is throughout "scientific," only rarely and hesitantly historical.

This may seem a strange dictum in the face of the fact that Boas has always emphasized the historicity of cultural phenomena, and that his "school" has sometimes been designated as that of "historical realism." But the epithet of "dynamic" has also been applied; and obviously neither is wholly accurate. In fact, there is no "Boas school," and never has been, in the sense of a definable group following a definable, selective program. For that matter, there have been no "schools" of any sort in American anthropology, as compared with British, French, and German. This national difference is in large measure due precisely to the Boas influence, which has consistently been exerted against the singling out of any one method—psychologic, sociologic, diffusionist, functional, or Kulturkreis—as constituting a king's highway superior to others. This is again a result of the exact or laboratory science point of view. These sciences recognize fields or departments, like organic chemistry or spectroscopic physics, and differences of technique, but they do not recognize schools differing in method; there is only one method in physical science. By contrast there is something immature, or partisan and incomplete, in the very fact of the anthropological schools advocating each its program. In reality they differ, and legitimately enough, in objective; and that means that they differ at bottom in what they are most interested in. But from this they have too often proceeded to make propaganda not only for their interests but for their results, until in extreme cases special methods have been advocated almost like panaceas.

In competition with these more one-sided movements, Boas has been at a loss to explain his position in terms intelligible to the members of schools. When he cautions against the one-sidedness of historic-reconstruction interpretations, he is construed as wanting to be a functionalist of his own private "dynamic" variety. When he is skeptical of sociological or psychological laws, and insists on the historical complexity of cultural phenomena, he is promptly labeled as "historical." Mrs Hoernlé has at least realized that matters are not so simple as that. But she fails thoroughly to understand Boas' real position when she pictures him as at first defending "historical method" and later "conceding" or "admitting" or retreating to a dynamic functional program and methodology. To the best of my understanding, he has always used both sets of labels with hesitation, in a kind of last-resort effort to make himself intelligible to people who insisted on seeing things more one-sidedly. In fact both terms, "historical realistic" and "dynamic," are not his own slogans, but were coined by his followers. Far truer than Mrs Hoernlé's picture would be this one: Boas has never really followed the historical method except in a rather narrow, special sense which I hope to make clear; but he was a functionalist, in that his prime interest lay in structural interrelations, change, and process, before Radcliffe-Brown or Malinowski had written a line.

Process, rigorously determined process as such, is the one constant objective of Boas' work; and it is the one common factor, though present to a varying degree, in the work of those definitely influenced by him. What is this but an objective, and therefore a methodology, taken over from the physical sciences? Of course its limitations and difficulties in the field of culture, as compared with the inorganic world laid on the laboratory table, are numerous and obvious; and Boas was intelligent enough never to delude himself on this point. When he came on the scene, he found anthropology taken up with schematic interpretations—Morgan's will serve as a typical example; and he unhesitatingly proceeded to show that these schemes seemed valid only as long as the fact was ignored that they were built up of subjectively selected pieces of evidence torn out of their historical context, that is, their actual context in the world of nature. In his insistence that this context may not be violated, Boas may have seemed, possibly even to himself, to be following historical method. But it was merely historical method applied as a critical safeguard; the problems with which he concerned himself were not historical except in minor cases, but concerned with process as such. Obviously, historical method as something positive becomes operative only when one is trying to do history. As regards specific schemes of the type of Morgan's, all trained and even halfway sound historians have always distrusted them profoundly; as much so as physicists in their field. In fact, all schematic explanations seem essentially a symptom of a discipline's immaturity.

2

The treatment of art may serve as an example. In his many studies of the subject, culminating in the 1927 book, Boas has considered the whole gamut of process factors: conventionalization, influence of technique, symbolism and secondary interpretation, virtuosity, cursive slovening, and the rest. The examples are from all over the world: superficially they look as diverse as those in a book following the old "comparative method" of schematizing. But they are never wholly out of their context; and they are never in a scheme. The ultimate conclusion is that the factors of involved process are many and variable; they differ in each succeeding case; and objectively critical analysis is needed to determine them in the complex variability of phenomena. As a historian might say, the uniqueness of all historic phenomena is both taken for granted and vindicated. No laws or near-laws are discovered. But neither are there any historical findings. Even the special chapter on North Pacific Coast art goes no farther in this direction than to record an "impression" that this art was formerly more geometric and less symbolic than now. The methodological requirements of history—such as continuity (with context as corollary) and uniqueness—are fully observed; but no history is done.

Allied evidently in origin and certainly in significance is the fact that style as such is never dealt with in the book. It is recognized as part of the context in each situation; but only that. There is no examination into what an art style is, of how or why styles develop; no characterization even of the essential quality of any style as such. When any style is dealt with at all it is as briefly as possible and merely as a point of departure for inquiry or proof in some problem of conventionalization, virtuosity, symbolism, or other "dynamic factor" or process. Surely a book on art which leaves the fundamental element of style out of consideration as much as it can must seem strange to orthodox writers on art who deal with the definition or history precisely of styles. These remarks are made not in depreciation or censure, but in analysis of a method whose importance is great enough to warrant its being clearly understood: as to its limitations of aim as well as for its positive accomplishments.

The one serious exception in Boas' work to the rule that he does not do history, is, significantly enough, "The Central Eskimo"—his first major ethnological production. It is also the only one in which the geographic setting is given other than perfunctory or minimal consideration. I may add that a distinguished British anthropologist has confessed, privately, that he found Boas' descriptive ethnographical works, valuable as they undoubtedly were, extremely difficult to use and even to understand—except for this same Central Eskimo. Evidently Boas' characteristic pattern of approach had not yet become settled in this work of self-apprenticeship. It must be admitted that some of us on this side have at times shared a little in our trans-Atlantic colleague's perplexities. The cause, however, is plain on a little reflection. It is not lack of lucidity: I doubt whether there is an argument or sentence by Boas in print whose meaning is not perfectly clear and exact, provided it is approached with reasonable intelligence. The cause is rather a lack of interest in factual description for its own sake, in other words, in phenomena. This is of course allied to lack of interest in historical depiction. In each case the exposition as such suffers.[3] To Boas the descriptive facts of a culture are always only the materials for the setting up of a problem, or series of problems. These problems deal with processes. Naturally the presentation does not yield the same integrated *picture* as a presentation made primarily on its own account with process left implicit or secondary. But from its own point of view, it is just as orderly, coherent, and clear. If the marshaling were all from the angle of one process singled out as the all-important one—as a more or less universal explanation—the scheme of presentation would probably seem lucid enough to everyone. But it would then be a scheme; and Boas' endeavor is normally to prove the multiplicity of factors.

[3] The lack of organization on the purely descriptive side is perhaps also due in part to an intense conviction of the urgency of rescuing at all cost as many perishing data as possible without wasting time over their arrangement for the convenience of the user.

Here again we have the science approach. A physicist or chemist does not give a descriptive picture of what he encounters in nature. He starts with a problem; then presents such data as bear on it, and no others. Of course this method cannot be transferred directly to cultural anthropology because this is not a laboratory discipline; and in general it is not feasible to deal in each case only with those data immediately pertinent to the problem; sooner or later the descriptive context of the whole culture or set of cultures in which the problem lies must be made available. With the quantity or quality of descriptive data secured and recorded by Boas no one would quarrel, especially in view of his duplicating in linguistics his achievements in ethnology. It is only the form of presentation on which there have been strictures. In fact, considering the primary impulse always to formulate problems dealing with process, the mass of new and accurate descriptive data secured by Boas is really stupendous. I doubt whether it has been surpassed by any worker.

In brief, one may define the Boas position as basically that of the physical scientist, but fully aware of the requirements of cultural or human material: the need for all possible context, the strong element of uniqueness in all the phenomena, an extreme caution of generalizations savoring of the universal. All these are criteria of sound historical method; and because he observes them, Boas is right in insisting over and over again that he uses historical method. Only, he does not *do* history. And that does make some difference. Every thou-shalt-not which a professional historian might exact is fulfilled; but next to no positive historical results are produced; instead a problem about the dynamic factors involved is answered or attacked.

This strange attitude is evident not only in reluctance to prosecute history, but in strictures upon those who do so within anthropology. Granted that historical reconstruction from ethnographic data is a different thing from the writing of history from documents extending over a range of datable time—a point to which I shall revert below—it will I think be admitted that assailing the historical reconstructions of Wissler, Elliot Smith, Schmidt, Spinden, and myself all together,[4] is treating the extreme and the moderate sinners as equally guilty. This can only argue that historical reconstruction is *per se* unsound or vicious, irrespective of the degree to which it is carried or the method by which it is arrived at. Since dated documents are not available in ethnography, it would mean that we are to follow historical method rigorously but perpetually refrain from

[4] Primitive Art (Oslo, 1927), p. 6.

historical interpretations. To be sure, archaeology is extolled as sound method for those who wish to know about the past of unlettered peoples. But as its data are admittedly always incomplete, that does not help very far. And, more significant still, Boas has practically never made use of archaeological findings in his own work!

It seems clear that there is involved here a resistance to historical interpretations of any sort, at least within the limits of anthropology—what the historians of profession do with their written documents of the past is perhaps over the fence and none of our concern. Such a resistance is most easily understood as the deep-seated distrust of a mind schooled in the approach of the inorganic exact sciences, toward a fundamentally and qualitatively different type of interpretation; although also a mind intelligent enough to realize that in dealing with historical material—as cultural material is, in the wider sense—the methodological safeguards of history must be observed.

In this connection an incident of the 1928 International Congress of Americanists may be of interest. On the last day of the session an informal group gathered to discuss historical method in anthropology. Present were Nordenskiöld, Bogoras, Koppers, Gusinde, Preuss, Boas, Sapir, Kidder, Wissler, and several others. At first the discussion revolved around Kulturkreis principles; but before long it shifted, until for the last two hours it became a debate between Boas on one side and all the rest, including the Kulturkreis representatives, on the other; Boas consistently maintaining that his work was genuinely historical! It is small wonder that Mrs Hoernlé in distant South Africa should have failed to get his position clearly. But she can rest assured first that Boas has not recanted his faith that his activity is historical, and second that the majority of his colleagues do not see it as essentially such.

3

It is evident that we are at a point where it is necessary to try to define somewhat more sharply historical activity or the historic approach, as distinct from merely historical technique or safeguarding procedure. I suggest as the distinctive feature of the historical approach, in any field, not the dealing with time sequences, though that almost inevitably crops out where historical impulses are genuine and strong; but an endeavor at descriptive integration. By descriptive I mean that the phenomena are preserved intact as phenomena, so far as that is possible; in distinction from the approach of the non-historical sciences, which set out to decompose phenomena in order to determine processes as such. History of course does not ignore process, but it does refuse to set it as its first objective. Process in history is a nexus among phenomena treated as phenomena, not a thing to be sought out and extracted from phenomena. Historical activity is essentially a procedure of integrating phenomena as such; scientific activity, whatever its ultimate resyntheses, is essentially a procedure of analyses, of dissolving phenomena in order to convert them into process formulations.

These two approaches are applicable to all fields of knowledge, but with varying degree of fruitfulness. It is in the nature of things—I do not pretend to explain why—that in the inorganic realm the processual approach of science has yielded most results, but as we pass successively into the realms of the organic, psychic, and social-cultural-"historical," this approach encounters more and more difficulties and its harvest diminishes. It is customary to say that the phenomena are more "complex" on the organic and super-organic levels. I incline to doubt this, and to believe rather that the difficulties lie in their being epiphenomena—from the point of view of the analytic, processual science approach. Hence the constant tendency to resolve organic phenomena into physico-chemical explanations, psychological phenomena into biological ones (the reflex arc), social-cultural phenomena into psychic ones. From the angle of science this procedure is perfectly correct; because so far as it can be applied, it yields coherent and verifiable results.

The historical approach, on the other hand, was first applied, and proved most readily productive, in the field of human societies; and it encounters increasing difficulties as the inorganic is approached. In the organic field it is still fairly successful; in geology and astronomy it leans so heavily on processual science that the nature of these disciplines, which by their objectives are clearly historical, is generally understood as being completely "scientific." As regards biology, I recently pointed out, in an essay on that subject,[5] that a whole series of phenomenally formulable "processes" familiar in anthropology—convergence, degeneration, areal grouping, etc.—were equally important in those biological activities covered by the old term "natural history;" and that the problems of natural history run closely parallel, at many points, to the problems of human or cultural history. I do not believe in the slightest degree that these resemblances are "mere" analogies and empty and misleading. That may be true from the point of view of processual, experimental science. From the point of view of historical science, however, or history, or the historical

[5] Historical Reconstruction of Culture Growths and Organic Evolution (American Anthropologist, Vol. 33: 149–56, 1931).

approach to the world, they are obviously of methodological significance, because corresponding objectives involve corresponding methods.

I am not trying to assert that these two approaches can never meet, still less than they are in any sense in conflict. Ultimately, and so far as possible at all times, they should supplement each other. The degree to which astronomy has profited by leaning on and borrowing from experimental science is a case in point. But, precisely if they are to cooperate, it seems that they should recognize and tolerate each other's individuality. It is hard to see good coming out of a mixture of approaches whose aims are different.

As to the element of time sequence: if I am correct that the essential quality of the historical approach is an integration of phenomena, and therefore ultimately an integration in terms of the totality of phenomena, it is obvious that the time relations of phenomena enter into the task. I am not belittling the time factor; I am only taking the stand that it is not the most essential criterion of the historic approach. Space relations can and sometimes must take its place.

If this is correct, the point often made, not only by Boas and his followers but by sociologists and functionalists, that history is legitimate and proper, but historical reconstruction unsound and sterile, loses much if not all validity. I would maintain on the contrary that history and historical reconstruction have the identical aims and approach and make use of the same mental faculties. (In technical language, they possess the same basic objective and method; but it seems best to avoid the latter word because it is likely to be ambiguous in the present connection.) It is true that history has the time relations largely given it in its data whereas historical reconstruction largely seeks to ascertain them. But this makes the latter only a special and somewhat more difficult case of the former, taken in its widest sense.

A little reflection will show that all historical procedure is in the nature of a reconstruction; and that no historical determination is sure in the sense that determinations in physical science are sure; that is, objectively verifiable. Historical determinations are in their essence subjective findings; and at best they only approximate truth or certainty. They differ from one another in seeming more or less probably true, the criterion being the degree of completeness with which a historical interpretation fits into the totality of phenomena; or if one like, into the totality of historical interpretations of phenomena.

History is supposed to tell "what really happened." But obviously this is impossible: the "real" retelling would take as long as the happenings, and be quite useless for any conceivable general human purpose. The famous principle is evidently to be understood obversely: history is not to tell what did not happen; that is, it is not to be fictive art. More useful is the definition of a historian as one who "knows how to fill the lacunae." But even this is too narrow. The professional historian is no doubt most conscious of the occasions when he encounters frank gaps in his data; but he is all the time, habitually and largely unconsciously, reading between the lines of his data on the one hand and omitting less significant data on the other. If he did not, he would never reach an interpretation. Whether this procedure is avowed or not, or if avowed whether or not scientists know it, does not much matter: it has been and is the procedure of all historians. If some of us ethnologists attempt to do time history for the poor dateless primitives, we have an additional unknown to deal with, and our results are undoubtedly more approximative only. But if we frankly admit that fact, there seems no valid reason why we should be condemned as inherently unsound for doing under greater difficulties the same type of thing which historians are respected for doing. That historians pay little attention to us, their poor relations, is expectable enough: who are we to enter the houses of the substantial when we do not possess even one document written before our day?

Many scientists do not know what history is, or merely assume that it is not science. But it is old and reputable, and is accepted as long as it sticks to documents. In counterpart, scientists make scarcely any effort to apply their methods to documentary materials. If the aim of anthropology is to ascertain the processes of change or dynamics in human societies and cultures, why this timorous sticking to the primitives whom we can observe only an instant, while rich data on change for centuries back are available on our own and other lettered civilizations? The usual answer is "complexity." But is this a serious obstacle as against the advantage of operating with timed data in studies of change?

Well, the result is that historical reconstruction on the basis of datable documents is not seen as reconstruction and is held up as laudable or permissible even though not scientific; but once the reconstruction in patent, because the dated pieces of paper are not there, it is considered wasted effort or unsound.

4

Of course not all reconstructions are good, either. In general, their value seems proportional to their being made with the qualities that characterize sound straight-historical work.

The Elliot Smith and Perry reconstruction suffers from the fatal defect of positing the cardinal event of culture as consisting of the originating of one complex at one time in one place. Any documentary historian who proposed half so simple an interpretation would get no hearing whatever from his colleagues. The scheme is really little more than a formula, and has been able to subsist only because it was posited in the obscurity beyond the boundary and attention of history. Some definite results of value have been attained by the "diffusionists;" a new weighting of the stagnancy, from one point of view, of many primitive societies relatively unexposed to higher culture contacts; also of the rôle of deterioration or possible extent of cultural losses; and certain resemblances and probable connections between particular clusters of elements far separated in space. These are worth-while positive findings. But compared with the scheme into whose frame they are set, they are specialties, and they do not in the least retrieve the scheme itself, which remains contrary to all historical precedent. Significant broad historical findings are not much more likely to emanate from laboratories than significant chemical ones from scholars' libraries.

The case is different, and rather puzzling, for the earlier form of the corresponding German reconstruction, the Kulturkreis theory, because Graebner, the leader of the group, is said to have begun as a professional historian. His "Methode der Ethnologie" is in fact based largely on Bernheim's "Lehrbuch der Historischen Methode," reduced and made over to some extent to allow room for his own scheme. It may be conjectured that Graebner, finding no suitable outlet in his earlier career, tried to force one by attempting in the unpoliced no-man's-land of ethnology what would have been promptly suppressed or ignored in history. That he operated with six or eight wholly disparate blocks instead of only one is no palliative to any historian, as long as the principles of continuity and uniqueness are fundamentally violated.

The reformulation of the Kulturkreis scheme into the Kulturgeschichtliche "Methode" of Schmidt and his collaborators is to be taken more seriously, because Schmidt undoubtedly possesses genuine historical insight, in regard to language as well as culture. The skill with which he has gradually remodeled the stark Graebner scheme out of all semblance to its original form, is evidence of this capacity. However, it does remain a scheme, and therefore all Father Schmidt's keenness, immense knowledge, and love of argument cannot make it a genuine, empirically derived, historical interpretation.

Spinden did begin empirically, restricted his field largely to part of

America, and seems to me to have genuine historical feeling. He has evidently yielded at times to an infatuation for the grandiose; but his chief defect appears to be an over-early and rigid crystallization of a formulation which started off on a sound enough basis, but ended by tending to blur the variety and unique features of many of his data.

Rivers, with his "History of Melanesian Society," is the classic case of a man of undoubtedly very high ability trying to apply a laboratory type of formula—he was trained in physiology and psychology—to a historical problem. His "Todas," though valuable for its new observations, shows the same lack of realization of there being such a thing as a pertinent historical approach. This strange little culture, obviously a specialized modification at innumerable points of the higher Indian culture, is treated with scant reference to this context; although its relation to this is the most significant problem which it presents. Boas, though also a laboratory graduate, has shown far more adaptability in similar cases; witness the keenness of his scent in trailing well disguised motives of Old World origin in American Indian tales.

Radin, who possesses the feel and insight of a historian, fails in his "Story of the American Indian" not because he lays these qualities aside when reconstructing, but because he reconstructs hastily without sufficient pains and detail. In securing and evaluating ethnological documents, he has shown extraordinary skill; which, if exercised in the field of orthodox history, where good documents are recognized and valued, would have brought him far more appreciation, and might have spared us certain sharp outbursts in an otherwise stimulating recent volume on ethnological theory.

Myself, who has been bracketed with several of the foregoing, I shall not attempt to judge or defend. I will express the purely personal opinion that those of my reconstructions which were published in professional organs for a professional public as an end-product or by-product of intensive preoccupation with a body of material,[6] continue on the whole to satisfy me as sound workmanship of their kind. It may gratify those who react differently to learn that these reconstructions have brought me some censure, no commendation whatever that I know of, and for the most part have been

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[6] The History of Native Culture in California (University of California Publications in American Archaeology and Ethnology, Vol. 20: 125–42, 1923); The Patwin and Their Neighbors (University of California Publications in American Archaeology and Ethnology, Vol. 29, No. 4, 1932), pp. 391–420; Yurok and Neighboring Kin Term Systems (University of California Publications in American Archaeology and Ethnology, Vol. 35: 15-22, 1934); Archaeological Explorations in Peru. Part I: Ancient Pottery from Trujillo (Field Museum of Natural History, Anthropology, Memoirs, Vol. 2: 108–14, 1930).

as completely ignored by my colleagues as I expected them to be by the larger world.

On the other hand, one of the genuinely significant reconstructions in ethnology was made by Boas himself. I am not now referring to scattered suggestions, nor to a brief paper on northern elements in Navaho mythology,[7] but to a formal, undisguised reconstruction: "The History of the American Race."[8] It is true that this is a presidential address, that it is brief and sketchy, and that since many years Boas seems to have avoided reference to the article. But it was an illumination and inspiration to many of his students and former students. When Wissler a few years later published "The American Indian," it contained many other things, but its main unifying synthesis after all was a more detailed development of Boas' reconstruction; and others, including myself, have followed with partial developments. Perhaps it was the very fact of the influence exerted by his own suggestive interpretation that helped drive Boas farther into his profound distrust of all reconstruction. But that so many other Americanists were ready to accept his outline as sound and valuable, and that so far as it went it has never been challenged, should at least indicate that there are better and worse reconstructions.

In many qualities Spier's general anthropological product perhaps stands nearest to that of Boas: high grade observation, definite restraint, conscious rigor of method—all the "scientific" qualities. Recently he has turned against reconstruction as misleading and unnecessary,[9] and out-Boased Boas in including in his condemnation his own sun-dance history which everyone else had always accepted as reasonable and worth-while. Driver and I, reusing his data with another technique, statistically, have since come to virtually the same conclusions as Spier originally formulated regarding tribal participations in the growth of the sun-dance and therefore one aspect of its indicated history. The case is perhaps of no great moment in the present connection except as an instance of how far and strongly the current against a historical interest in ethnology has run.

5

Recent developments in American linguistics illustrate the same point. More than forty years ago Powell had a list and map of linguistic families

[7] Northern Elements in the Mythology of the Navaho (American Anthropologist, Vol. 10: 371–76, 1897).

[8] Annals of the New York Academy of Sciences, Vol. 21: 177–83.

[9] Problems Arising from the Cultural Position of the Havasupai (American Anthropologist, Vol. 31, 1929), p. 222.

north of Mexico compiled. Most of the participants in the undertaking were wholly untrained in philology; the leader was a biologist; but the work was consistent, impartial, business-like, filled a practical need especially as regards ethnic relationships, and at once became standard, even though almost no evidence was presented. Many of our younger students probably know the famous major only as the author of "the Powell map."

About the same time Boas was beginning his collecting studies and analyses of American languages, a labor carried out with such qualitative and quantitative success that the product, even without his work in ethnology and physical anthropology, would have been a monument. Until a very few years ago it was literally true that every competent worker in American linguistics except one or two had been trained as well as inspired by Boas. With that, his own output—the fundamental monographs on the Chinook, Salish, Kwakiutl, Tsimshian, Kootenay, Keres languages, besides contributions on many others—was as great as that of any two of his juniors; in each case a basic body of texts with an analytic description of the structure of the language in terms not of an abstract pattern but of its own characteristics. The value of this body of work is probably unparalleled and certainly incalculable; the method, so far as it goes, thoroughly sound.

As more material accumulated, it became apparent to a number of workers—Swanton, Dixon, myself, Sapir, and others—that some of the languages classified as separate by Powell were indubitably related. If so, this meant ethnic relationship, hence conclusions of obvious ethnologic-historic significance. Some of our group were perhaps primarily interested in these non-linguistic significances, and did not push the search for linguistic evidence much beyond the point of establishing a more or less strong probability of connections. Sapir took part in this movement; but, being primarily a linguist, and having been trained in orthodox "philology" as well as by Boas, he went farther and proceeded to apply the reconstructive method of this philology in the American field.

Indo-European philology, which constitutes the overwhelming bulk of what is conventionally called or miscalled philology, is a discipline with a highly developed methodology and rigorous technique. It uses the comparative method for historical objectives under a strict set of principles. It reconstructs the hypothetical original Indo-European speech not as an ultimate end in itself but as part of a method of tracing the changes which have taken place in the several Indo-European languages. On the history of many of these we possess only intermittent and brief documentation. If philology had confined itself to studying actually documented changes, its history of this group of languages would be fragmentary; in fact, mostly lacunae. It has gone on the principle that by operating with a hypothetical Indo-European, built up not by random guesses but according to a consistent methodology as exhaustive as possible—the seeming exceptions as important as the seeming rules—it could make this history far more complete and significant. The nature of language happens to be such—its range is narrow and apart in comparison with culture but its forms are precise and readily definable—that convincing results were easily obtained by these reconstructions. At any rate they have been accepted as convincing, and philology has had, deservedly, the repute of enjoying probably the strictest methodology and most exact technique of any discipline among the social studies and humanities—in the Geisteswissenschaften. This discipline is called comparative, but its ends are historical, and its fundamental mechanism of operation is precisely reconstructive.[10]

Now when Sapir began to apply this well-established method to a somewhat widened Athabascan and Algonkin, and when those of us who were less ambitious drew more elementary conclusions as to speech relationships which if true must have been of definite ethnic and cultural influence, Boas reacted negatively and has continued to do so. The evidence was declared insufficient, our procedure dubious, the problems themselves unfortunate because they distracted attention from more important problems of process. So far as I know, Boas has never analyzed and refuted the positive evidence offered for specific relationships, but has tried to throw the whole case out of court on the ground that no satisfactory evidence was being offered in the premises.

His chief argument of rebuttal has been that the similarities, even in structure, might be due to contact-influencing of originally unrelated languages. It must be admitted that there is a real problem here, to which Boas began calling attention forty years ago. On the other hand, it is obvious that the problem cannot be attacked without recognition of the factor of relationship—for instance on a purely geographic-statistical basis—else similarities undoubtedly due to common origin, as between French and Spanish, or Navaho and Apache, would be undifferentiable from similarities really due to contact transfer, as between French and Basque. The argument in short can be run indefinitely in a circle unless certain facts as

[10] It is true that orthodox Indo-European philology has tended to become an isolated, highly-specialized, self-sufficient pursuit somewhat sterile in comparison with what it might become with broader objectives, or has thought at times that it could attain these broader objectives by injecting bits of metaphysics. But the fact remains that it enjoys universal respect for a sound technique while being historically reconstructive.

to relationship are first agreed on as established. What shall this basis of agreement be? The fifty-eight families lined up more than a generation ago by an ornithologist for an administrative head who had been a geologist-geographer? Or the much smaller number of families to which these fifty-eight have been reduced by a group of anthropologists trained for work in language by Boas, and headed by a linguist of the eminence of Sapir?

Granted that some of us, including Sapir, may have been at times over-enthusiastic and a bit speculative—most Europeans consider us, as a body, ultra-conservative—a reasonable basis might have been found for a temporary working agreement, and Boas's own substitute problem could have been genuinely attacked by now, instead of being merely advocated as a reason why linguistic effort in the American field should remain restricted to collecting and analyses.[11]

It cannot be believed for a moment that the Powell map has any fetishistic value for a man like Boas. For one thing he is too fundamentally impatient of all classifications. Nor may one believe that with the stealing on of the years he had begun to feel the need of a sure, unchanging world. The new problems he is developing, the old ones he is extending, his receptivity to certain new movements like the psychological approach in ethnology, all controvert such a suspicion. He is not looking for a secure retreat but for new enterprise. The only convincing explanation for his opposition to problems of speech relationship is that such problems are in

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[11] Boas' contact-modification problem is of genuine intrinsic interest. As usual, it bears on process. The overwhelming mass of precedent in the history of languages is to the effect that large absorptions of content can take place, also some modification of phonetic form, but that imports or assimilations of structure probably constitute normally only a minute fraction of the structural growths that develop internally. The opinion of strict philologists is not particularly conclusive on this point because they usually begin and end by concerning themselves only with changes internal to a family; but there are linguists as well as philologists. The real problem of course is when, how, and to what extent the process of imitative borrowing from outside takes place. This has not yet been investigated systematically, and is worth being investigated, even though most linguists may feel that their experience warrants them in estimating that the external factor will turn out to be a minor one. It is a tribute to Boas' insight that he formulated the problem, and did so before he used it as a weapon against the historians of speech.

Of a different sort is the opposition of Uhlenbeck and Michelson to some of Sapir's findings. This springs not from any anti-historical bias, but from an over-complete submergence in orthodox philology, in which both men were reared. They will not admit any relationship until it is proved with the same intensiveness as in Indo-European languages, which have had hundreds of students for one in the American languages. This means that the formal code of a highly organized discipline must be adhered to to the last letter even in pioneer situations; in short, the code is more important than results.

their nature historical, and that he genuinely distrusts the historical approach and historical interpretations, no matter how made or by whom, at any rate in his own discipline of anthropology and in regard to his particular American field.

This linguistic example is somewhat special for anthropology as a whole but illustrative on account of its clearcutness.

6

In physical anthropology Boas' important contributions on growth and type changes have been through statistical rather than anatomical procedures. He has in fact made original contributions to statistical theory. At first sight it may seem strange that he has never applied statistical method to cultural data. Efforts made in this direction have been ignored by him; and his few general utterances on the subject are to the effect that statistics cannot be used in ethnology, as Tylor's error of method shows. Since Tylor was attempting to solve a universal problem, one of inherences; and since statistics can be and have been applied to specific historical situations within a given time and space frame, Tylor's insufficiency, like that of Hobhouse-Wheeler-Ginsberg, obviously does not close the issue as completely as Boas seems to assert. I believe again that his opposition is due to a fear that statistical method will be used in ethnology for historical findings, and especially of a reconstructive kind; as indeed it inevitably will be.[12]

7

A seemingly strange product to come out of the Boas movement, and an attest of its strength and breadth, is the characterization of cultures in prevalently psychological terms by Fortune, Mead, and Benedict in recent years. Perhaps "in association with" would be more accurate than "out of" the Boas movement, for one of the three has been stimulated also by Malinowski. Malinowski's final interpretations, however, are psychological to a considerable extent, whereas the works of these three investigators remain essentially cultural analyses with a strong psychological coloring. That is to say, the findings are in part expressed in psychological terms, but they are findings about cultural phenomena, not resolutions of them

[12] Perhaps the difficulty of measuring and defining cultural material as precisely as anatomical material also plays a part. But in that case the definition of elements, whose use takes the place of direct measurements in statistical ethnology, deserves a destructive examination. In one of his early monographs, Boas counted elements—folk-loristic motifs or episodes—to establish routes of historical transmission; but since then he has used such elements chiefly to deal with processes.

into their psychic springs. There is also definite consideration of the place of the individual in his society, of his rôle in his culture. There is no sharp line of demarcation from the Malinowski attitude, but at least historically this approach stems mainly from Boas. At any rate it has his definite approval and encouragement. Two of the group deal with data of their own collecting, the third interprets chiefly materials already recorded. All three concern themselves with the functioning of cultures as wholes. Their analyses therefore do not primarily serve to extricate processes as such, but are preliminary to a coherent synthesis of the totality of the culture conceived much as a living organism, not pictured statically. Closely allied is the work of Bunzel.

In vividness of characterization, quickness (in both senses) of insight, ability to coördinate masses of detail into a unified and on the whole convincing picture, the work of this group is of a very high order. I say this explicitly, because in reviews I have once or twice felt compelled to dwell also on certain deficiencies of workmanship which did not seem enforced by the nature of the undertakings but to spring from an overpersonalization of approach. This perhaps is almost inevitable in first attempts at a type of presentation as intimate as this one; and in the present connection, where we are concerned rather with the nature of a kind of approach than with a precise appraisal of particular works, I do not wish to emphasize previous strictures. I mention them only because while I have not withdrawn them, I wish to be understood, as I meant to be in the reviews, as regarding the work of all members of the group as valuable.

What is of special relevance in the present connection however is that all this type of approach aims not so much to isolate process as to show it at work in a picture of the culture conceived in terms of its own totality. The method may therefore be called dynamic or functional or psychological; but ultimately it is a form of the historical approach. It does, as a means of heightening its own particular quality, deliberately leave out the time element and all its functions, and therefore passes as non-historical. But, as I have said before, time is only an incident in the historical attitude, although an important one. The essential types of apperceptions and evaluations that count in the Fortune-Mead-Benedict approach seem very closely allied to those requisite in a good historian, or for that matter for a reasonable culture-historical reconstruction. The elements needed to build up the picture are selected, and those not needed are omitted, or slurred with intentional subjectivity. On the other hand the painstaking analysis and non-selective objectivity of the "scientific" approach are lacking. Criticism of the group has indeed been based largely on the subjective quality of their work; which however is no longer a defect as soon as its essentially historical nature is accepted.

On the contrary, criticism perhaps should lodge rather for failure to be broadly and completely historical. It is easier to obtain a sharp, unified picture by cutting out antecedents and surroundings and focussing on the impressionistic, cinematographic image which is being unrolled. Such restriction of aim is not *per se* a fault of method; but it tends to result in a series of dazzling, disconnected effects. These pictures of course ought sooner or later integrate into a picture larger in geography as well as duration; and on reflection many problems of the how of development and interrelation arise; but these larger views and further problems have not, at least not yet, been followed out by the authors in question.

Mutatis mutandibus, the work produced by this group seems close in its essential character and spirit to, say, Burckhardt's "Renaissance." Boas realized this when in his preface to Benedict's book he speaks of her approach as being concerned with the "genius of a culture." Here appears to lie the real quality of these productions. They are analytic; but so is Burckhardt—intensely so; and like him, they analyze in order to build up an integrated picture. Like him, too, they succeed in so doing; and this is the one aspect of their work of which to date we can positively affirm the value. Benedict's psychiatrizing formulations are original, suggestive, and stimulating; they may open up new and fruitful approaches; but on the other hand they may remain mere analogies. Personally I am sympathetic and hopeful; but also realize the danger of over-enthusiasm; the real proof lies in results: and Benedict will have to work over more material, and think her results through farther, and others will have to test her approach before we can be sure what it really means. On the contrary, she has given us an integrated picture of Zuñi and Kwakiutl culture seen from a psychological angle which we know to be valuable. The same holds for Mead.[13] She may think, and she may be right in thinking, that the ultimate value of her work lies in the generalizing chapters, those which deal with process or with applications to our own lives. But I would not trade them for the picture of Manus, the high quality of whose workmanship is immediately convincing, whereas the value of the reasoned remainder remains subject to test. The authors themselves may put the emphasis the other way; but if so, this is presumably due to their springing out of an environment which rates science high and history low. The whole conditioning of nineteenth and twentieth century civilization is in this direction. The way to be suc-

[13] Fortune and Bunzel have hewn somewhat more closely to the line.

cessful is to be scientific. But I am trying to see the less transient values--without closing the door to newer ones.

This group, then, may or may not have made an important contribution to scientific anthropology; it has made one to historical anthropology.

8

It may seem that this discussion has revolved largely about a personality. It has of necessity, because this personality is not only the largest in anthropology, but has stood most distinctively and successfully for the application of scientific method in the subject. The Boas movement comprises probably the most numerous group of active, able, and sound workers in anthropology today. If those less directly but still traceably or partially under its influence are included, there is no doubt as to its being largest.

If now we try to sum up this influence, the following findings seem salient. First of all, the movement stands for the application of what is generally recognized as the method of science to a body of material previously treated either historically only or merely by naïve methods, broadly speaking. Next, the movement recognized that this body of material was sufficiently distinctive that it could not be treated by the direct transfer of methods evolved in the experimental sciences: hence the failure to seek "laws," or sociologic surrogates. Third, it did avail itself of existing sound historical method, has consistently practiced it, and to that extent may properly claim the title by which it is most often known. But, fourth, perhaps because it emanated from science, it never fully understood the underlying objectives of history, therefore in general failed to formulate its problems historically, and actually took from history essentially only its negative safeguards. The consequence is that the results are to an overwhelming degree unhistorical, and that the attitude of the movement has been anti-historical in tendency. This is perfectly consistent with its scientific origin; and the outcome may be all to the good, ultimately; but the situation should be recognized for what it is.

I will only repeat, to prevent possible misunderstanding, that by "historical" I refer not primarily to a preoccupation with time sequences, but to a basic and integrative intellectual attitude of which such preoccupation is normally an outflow.

9

It remains to consider another side of anthropology, that which does not claim to observe historical method and frankly disavows all attempts at historical results. These movements have usually been labelled sociologic or functional. In their nature, they must obviously be concerned if not with laws then with constants in the field of culture. At the outset it must be said that this is not an objective which, *per se*, anyone would quarrel with or has quarrelled with. The only question is, whether fruitful results are obtainable and how.

The most active and influential exponents of one wing of this movement at present are the Année Sociologique group and Radcliffe-Brown; of another, Malinowski.

Durkheim and Mauss are avowed sociologists who have specialized on primitive culture. Their method is the "comparative" one, their findings are general conceptualizations. They observe, in general, the safeguards required by history: they do not deal with small bits of culture torn out of their context. Nevertheless, their results are not integrations in terms of a larger culture whole, and therefore historical, but integrations in terms of conceptual constants, and thus unhistorical. What are these constants? With Durkheim it resolves ultimately, if I understand him aright, into a social group's sensing its culture as at once its raison d'être, its cohesive force, and its life blood, and trying to maintain or shape its culture in accord with this integrative principle. The emphasis seems to be more on this principle, or its dim apperception or symbolic expression as that which holds social forms together, than on the social forms as such. This seems to savor of mysticism; but the mysticism is perhaps mostly due to difficulty in formulating such ultimate concepts. The concept appears to be a perfectly valid one as a hypothetical explanation, but of course difficult to connect satisfactorily with specific evidence.

Dukheim has at least built some kind of a bridge across the gap which has always separated sociology and anthropology.[14] He does deal primarily with the social group, the social machinery; but this, according to him, succeeds in existing and functioning only because of another element, its culture, which thereby becomes, if my understanding is correct, a sort of *primum mobile* for society. This is not an idea to be discarded lightly as merely mystical. It certainly is not a historical concept. It verges on the philosophical; perhaps falls most nearly within Geschichtsphilosophie; and can become scientific in proportion as it is empirically verifiable. Obviously, however, such verification is difficult on account of the breadth of the

[14] The persistence with which these two theoretically allied disciplines, born nearly at the same time in western Europe, have in general kept separate from each other, is in itself an interesting problem in culture history. It suggests that they spring from different sets of impulses and aim at different ends.

concept and its remoteness from the surface of phenomena; and to date Durkheim remains mainly a prophet who has glimpsed a great vision.

Mauss comes nearer to earth again, and the "comparative" method is more in evidence. However, not only, in contradistinction from an earlier generation, are historical requirements as to context observed, but the constant found is not so much a specific one as the fact that elements function in relation to one another. That is, the older naïve type of interpretation that A normally produces B, and B, C, is replaced by the conclusion that A, B, and C normally function in relation to one another in a larger, integratively functioning whole. Few would be disposed to disagree with this, and the point is well worth being kept in mind, especially by the hasty in specific interpretation. But it is hard to see the attitude as of much utility in a concrete attack on concrete problems. Here the philosophic paternity—or perhaps more exactly, ancestry—is evidently still operative. An expression, too, of this strain, is visible in the reluctance of the group to embark actively in field studies, which the definitely scientific as well as historical minded students of primitives have since more than a generation pretty unanimously felt as a real need.

Mauss's categorizing also fits badly with the procedure of both the main currents of anthropology. We no longer feel the grouping of phenomena under such concepts as Gifts or Sacrifice to be profitable, because these concepts are derived from common, unscientific experience, and not specifically from the cultural data under investigation. No physicist or biologist would approach his data from the angle of the categories "long" and "flat" and "round," useful and real enough as these concepts are in daily life. The historical approach, it is true, does not shrink from currently using concepts of this order: it is one of the characteristics of history that it does not need, or at any rate has not generally employed, technical or symbolic terms. But historical treatment can follow this seemingly slovenly procedure because it organizes its material in terms of the time or space or phenomenal content relations, never primarily in terms of concepts derived from unhistorical experience. Similarly the descriptive ethnologist may group his new data under headings of this sort—warfare, religion utensils, etc.—but this is merely a convenience of external, conventional order, not of underlying or significant organization.

10

Radcliffe-Brown perhaps stands nearest the French group. He has not hesitated to admit that his aim is sociology. He does not repudiate history as illegitimate; but he realizes that it is a different thing from sociology and insists on their being kept separate. He does explicitly intend to work without unnecessary historical considerations; and he does believe that there are laws in the socio-cultural field and that they can be found. These laws are not merely similar patterns within which the phenomena of culture have recurrently happened, but they refer to factors which bring it about that culture phenomena do happen, must happen, in certain ways. For instance, the parts of a culture function with reference to each other so as to produce as integrated a whole as possible, and when they fail to do so readjustments in this direction are set in order. From the French sociologists Brown perhaps differs most conspicuously in his insistence on first-hand investigation, on the type of acquaintance with materials which permits them to be freshly dissected; in short, field work. He is therefore an empiricist, and can claim to stem from science rather than from reasoning or philosophy; as indeed he does, biographically: he was trained in psychology by Rivers.

The segregation of social anthropology from history is not necessarily to be condemned. While the whole tenor of my argument is that the definitely historical approach is justified and valuable in all disciplines dealing with cultural material, it is certainly legitimate to lay it aside in the hope that a rigorously non-historical attack may yield new results. The test after all should be by results. Now here the general verdict to date is that if Brown's generalizations are broad they are also tenuous, whereas in proportion as they are concretely applicable, they tend to lose their universality and are no longer laws or constants. This verdict it is difficult not to concur with. It appears to be part of the old dilemma of the sociologist: by the time he finds a formula that no one can cite exceptions to, it has become so essentially logical, so remote from phenomena, that no one knows precisely what to do with it. Its only value is as an end in itself. Brown's thesis that every society or culture tends to function integratively, is of this order. As a point of view to be kept in mind it is no doubt sound enough, and may prevent distorted apperceptions; but neither as a tool for further inquiry nor as a final synthesis will it satisfy either the scientifically or the historically minded. Its significance seems to be in itself, to those who find satisfaction in that type of formulation. Every physiologist would accept the fact, probably takes it for granted, that there are strong integrative tendencies in the functioning of all organisms. But would any physiologist consider such a principle to be either the end result of his science or a specific tool for prosecuting it further? He would view it as a background presupposition, to be invoked when one-sidedly dissociative interpretations threatened the balance of his discipline. It is in something of this light that we must see this law or basic hypothesis of Brown's: it represents a reaction or corrective against the extreme analytic tendencies of the Boas movement.

Mrs Hoernlé's cited examples of cultural laws in Bantu legal and marital systems of course are not laws at all, but only descriptive summaries of uniquely occurring phenomena. They are really fragments of good history which she does not recognize as such because they are presented without reference to the time element. They are also raw materials for potential scientific interpretation.

Apart from his program or propaganda for laws, Brown's specific attitudes are really very close to those of the majority of American anthropologists—I mention them because they include no diffusionist or Kulturkreis adherents. Particularly would his position be close to that of Boas, if only he would refrain from specifically ruling out historical control method. After all, even a heretic like myself is not dreaming of making a weekly exercise of historical reconstruction obligatory on all anthropologists, but merely pleading that those of us who wish to give cultural phenomena reasonable positive historical treatment be permitted to do so without having a yellow cap set on our heads for it.

11

Malinowski is also a functionalist, but with a more psychological trend in his final interpretations than Brown. He does not professedly look for laws. Both his field data exposition and his interpretations are stimulating, important, and sane. But his data are drawn almost wholly from one limited area, and within that overwhelmingly from one small culture. For the rest, his conclusions depend essentially on the exercise of a keen mind. His generalizations therefore may lack some of the validity which they appear to possess. After all, there is no more reason to infer cultural or psychological universals from Trobriand culture than from our own. That is the first and by now quite elementary lesson of anthropology. To be sure, Malinowski is very careful not to assert universally binding validity for his findings; but his points tend to be developed with an elaboration of manner which is likely to convey to any non-anthropologist who is not highly cautious, the impression that they are universal or near it. There is general agreement, to which I heartily subscribe, that Malinowski's conclusions, so far as they really go, are suggestive and generally sound. But it is clear that they are so because he possesses an unusually keen imagination and intellect, not because of his method, which as something transferable seems exceedingly limited. We know enough by now of the little culture area of which the Trobriands form part, in fact in certain respects enough about all Melanesia, to make it evident that many characteristic Trobriand institutions and attitudes are reworkings, specializations, or warpings of institutions and attitudes widespread in the area. Obviously such facts are of bearing even in a picture of the culture *per se*. They are still more important if generalizations beyond the culture are to be attempted. Whether it is a question of the Kula potlatch or of the relation of father and child, the data on institutionalized giving or trading and on the relations of near kin in Melanesia as a whole, or at least in the Massim area, are obviously pertinent in proportion as generalizations of breadth are undertaken. I am of course not asserting that the first prerequisite to any other work is a reconstruction of the past history of culture in Melanesia. Problems enough can successfully be approached with a complete omission of time factors, if one so prefers: on a merely one-moment basis which is comparative within a limited and patently interrelated area. Even this modest concession would be historical. In fact so definite a functionalist as Radcliffe-Brown has made it the basis of his approach in his "Social Organization of the Australian Tribes," which many of us, presumably for that reason, consider perhaps his most valuable single piece of work. But Malinowski so far has preferred to travel his dazzling orbit unhampered by even rudimentary historical considerations. It is the more pity because his insight is excellent and his mind fruitful.

12

I am afraid I have transgressed the twenty minutes—even of silent reading—in which souls are supposed to be saved; and therefore regretfully pass over a number of other important workers: Wissler, for instance, some of whose methods I have recently discussed in detail;[15] my colleague Lowie, whose soundness is so careful that his basic approaches would require intensive dissection to analyze out; Nordenskiöld, who has made historical reconstructions which for once no one has found fault with, and added to them empirical investigations of the conditions surrounding invention; Kidder and the other archaeologists, whose approach is of necessity primarily historical. It would however be inappropriate if in an essay devoted to emphasizing the importance of the historical attitude, I were to deal only with contemporaries. To save space, I shall confine myself to two pairs of figures customarily bracketed together in Germany and England: Bastian and Ratzel, Tylor and Frazer.

[15] S. A. Rice, ed., Methods in Social Science (Chicago, 1931), pp. 248–65.

Bastian need be mentioned only on account of his name. His real service was the fervor with which he preached the need of collecting data and objects while there was still time. As a thinker, he was if not a mystic at any rate highly obscure. He had a certain quasi-philosophical point of view, but no method; and he did not perceptibly influence anyone.

Ratzel the geographer is a strange personage to figure as one of the founders of our discipline, and it is only the amorphous condition of nineteenth century anthropology which allowed him to attain even that conventional repute. His influence on anthropology was not primarily environmentalistic—his minor sins in that regard are badly over-emphasized in his English version, and he started no environmental movement within anthropology—but definitely historical. He saw and emphasized historical problems where the documents customary among historians were lacking; and he recognized the phenomenon of peripherality. If his influence even among German anthropologists was not greater, it was perhaps primarily because his discussions of primitive peoples were incidental to geographic considerations and insufficiently clear-cut, referring somewhat ambiguously to peoples and their cultures.

Sir James Frazer is still with us from another generation, the generation of the unity of the human mind as an active spontaneous principle, and of the importance of survivals. In these days when we are so conscious of method—over-conscious, the reader may have concluded—the suave, urbane unconcern of our forbears sometimes seems like the golden age of untrammeled innocence. Frazer pursued the exotic story of forgotten nooks as an end in itself. If ever a sense of scientific problem or time perspective troubled him, it was but transiently. Reared in a classical tradition steeped in history, he became the supreme antiquarian. Yet his impress on the educated public was for a time as wide and deep as it has been light on more recent anthropologists. He must have undermined much formal religious dogma by implications almost inevitably drawn from his works. Professionally he seems to stand above all for an interest in cultural pathology. His preoccupation is with those customs and beliefs that deal with the ever-present problems of incest and its regulation, with sacrifice and cannibalism, with the will-to-power attempts of magic, the security devices of taboo—all the neurotic manifestations of helpless cultures. To a considerable extent he has been read from the same interest that makes readers of erotica, pathologica, mystica. We younger men, and women, are of sterner if less cultured stuff, and leave these palatable morsels in order to bite into tough problems or psychiatric formulations. Nevertheless Frazer, though lacking in any formal method, did feel in his phenomena—they are ever-recurrent, so far as cultural phenomena can be—some kind of an import, which Freud was quick to see even if we walked by it. The last few years have seen the beginning of an inclination—in Malinowski, Fortune, Mead, and others—to approach these phenomena once more with somewhat the interest of Frazer, though of course through the medium of a more modern psychological and cultural methodology.

Tylor, so often coupled with Frazer, seems related to him in the fact of sharing certain presuppositions and evaluations typical of their time, rather than by any inner kinship. They both belong to the period when anthropology was beginning to crystallize out as a subject. In Tylor the sense of problem is as strong as it is deficient in Frazer. Spiritually, if not formally, he was a man of science: he saw the need of proofs. His famous attempt to make a demonstration by treating the frequency of seemingly independent "adhesions" failed, as was recognized by some even at the time, because the cultural independence of his ethnic units remained unexamined. Also, his constants, like "avoidance," were only roughly constant. Nevertheless the attempt revealed a genuine sense of problem and method. That it was not repeated for long, shows Tylor to have been ahead of his age. But he did not only seek laws; he realized the importance of historical connections; and again he sought a method of establishing these where the continuity in space and time had become interrupted.

That Tylor accepted the essential unity of the human mind should not be held against him, for we do so too, though less explicitly. That he drew positive and specific inferences from this postulate which we no longer draw, was the fault of his being a pioneer. In the two generations since his prime we, in common with psychologists, have come to realize intensely the plasticity of this mind material, the enormous conditioning to which it is subject. Inevitably, therefore, we are much less ready to define the mind, or to use its unknown quantity for explaining phenomena which we are able to define better than we can define it. But this after all means only that we operate with a more critical methodology. The fact that this methodology insists on dealing first with the measurable or characterizable phenomenal factors A, B, and C, and relegating the difficult and protean X of the mind to the rear, does not abolish the X. The X, or its relation to the Y of culture, does remain our ultimate problem. This fact, in our enthusiasm, we tend to forget; and, probably more than we know, we are bringing up our students and successors in an ultra-behavioristic attitude of operating with a scientifically sound methodology and a minimum of orientation as to the end-purposes of the method. These lines are of course not a plea for the reintroduction of a metaphysical entity; nor are they strictures on the point of view underlying the modern methodology—only a caution against this being taken as the end-achievement. If there is a human mind, it has a structure and constitution, and these must enter into its phenomenal products. It is a sign of advancement of our studies that we realize the difficulty of defining this structure and constitution; but it remains a factor in our basic task none the less. We have learned by experience that we can reach more specific results by setting ourselves partial problems which are so rigged that they omit the mind, even where the approach is psychological. But it is well to remember that we are making a deliberate omission for practical purposes for the time being; and above all that we have not yet proved that X equals 0.

Tylor's fundamental position is therefore far from being liquidated, though many of his specific findings may be. He possessed genuine scientific curiosity of a high order, sanity and far-sightedness, and balance as between alternative approaches. He must be construed as easily the greatest of Boas' predecessors.

13

The point of view which underlies the foregoing discussion is that there is a historical attitude and approach as well as a scientific attitude and approach, and that, in a field like anthropology, each has its genuine problems and equally important and fruitful results. If I have leaned one way, it is because the current of the day runs the other. At least so it seems to me: there may be bias. My education included some contacts with experimental science which I found highly stimulating, but consisted primarily of generalized activity in the linguistic-literary-historical field, remaining rather undifferentiated until I settled upon anthropology as definitive profession. It seems only fair to make this statement after commenting on the influences that have borne on others.

History of course is in the present connection to be understood as an attitude of mind of which history *de métier* is only one and an imperfect expression. It is necessary to repeat that while the time factor can never be permanently left out of consideration in history, preoccupation with sequences is not the cardinal quality of history. It may have been so in the annalistic origins; but even Herodotus was already beyond that. And it is genuinely significant that he was not only the "first" historian but the first ethnographer. In modern times Burckhardt was a real and a great historian though time sequences scarcely enter into his "Renaissance." And there is nothing in his attitude, in the problem or task he set himself, or in the methods he used, which is not good anthropology. Obviously I am not trying to restrict anthropology to work of the Burckhardt type. But I am trying to prevent endeavors of this type, or of any soundly historical type, including reconstructions—Burckhardt's "Renaissance" is nothing if it is not an integrative reconstruction—from being ruled out of anthropology. That is why I have tried to show that some of our work which passes as meritorious because it seems scientific has its major values lie in being, though unrecognizedly, historical in character.

The two approaches need not conflict. We are fortunate in having both of them available. We need them to supplement each other. The scientific element has freed anthropology from some of the limitations of conventional history. We are ready to face process as such, which historians will scarcely do. But pulling any number of process demonstrations out of the mass of phenomena does not really prove very much that is positive, because the processes which anthropology has succeeded in isolating have so far failed to integrate into a larger system of processes to any considerable degree, as they do integrate in the experimental sciences. Unless we stand ready to content ourselves with demonstrating that cultural or historical material is very difficult to resolve wholly into processes, we must fall back into doing something with the phenomena themselves. What we generally do besides merely recording or enumerating them, is to define their patterns. But a pattern is not a process; it is a descriptive representation of a constellation having its basis, or believed to have it, in the reality of phenomena. It is fundamentally a historical and not a scientific formulation, even if its description be exact or quantitative. If we discern generalizable process at work in it, nevertheless the pattern always remains a unique historical phenomenon. It is simply larger and more relational than a single historical fact, element, or event. When patterns interact, we can again see familiar processes operating in fluctuating strength, but what is most definable to our understanding is the product, the new patterns resulting. Sound history, and at least to a considerable extent sound anthropology, concern themselves with finding patterns and putting them into their actual relations essentially on the phenomenal level. At any rate such has been the case until now. In proportion as a historian specifies "causes," he is to be distrusted, and generally is distrusted by other historians.

Basically a functional approach is rather close to the historical approach. It does not, if it is wisely critical, specify causes. It does not, for the most part, distinctively isolate processes. It really concerns itself largely with depicting patterns and their interrelations. It does try to view these as living: "dynamically" or "functionally" instead of "statically," or, speaking in analogy, physiologically as well as anatomically. This is just what historians do. Only, having their data given them in the flow of time, they take for granted that they deal with them functionally, whereas we with our momentarily known primitives have had to discover functionalism and are still somewhat elated about it. When a functional program goes farther and attempts to discover laws or calculable processes, it has, at any rate until now, mainly done one of three things: it has discovered patterns and mislabelled them laws; or it has formulated laws which are so predominantly logical or conceptual as to be of little service in investigating phenomena; or it has isolated processes whose strength however is so variable and incalculable that they remain inadequate instruments for helping us to understand the fullness of phenomena. I wish I could see the situation more optimistically. So does every historian. It will be a great and intensely stimulating day in the course of human understanding when we determine definable and measurable processes operating under precise laws in history and culture. But a realistic attitude compels us to admit that that millennium is not yet here.

As to historical reconstructions, they can be defined as a special form, under special circumstances, of the endeavor to see and understand phenomenal relations of culture patterns. If they are honestly that, they are as justified methodologically as anything else that a legitimate historian attempts. They are even necessary at times, because the whole aim of history is to understand in terms of successively larger integrations, not to cling timorously or mechanically to the thread of narration and re-narration of the known. That reconstructions are more tentative in result than interpretations based on continuous data is an obviousness to be taken for granted, not an argument for putting them under the ban. On the contrary, in the hands of those who do not sense what a culture pattern is, reconstructions become verbal bridges over the unknown, or fictive pretenses—fictive without the value of art.

Anthropology, as an accident of its materials, stands with one foot in the field of the undoubted sciences; with the other, squarely in history. The fact that its central theme is the unlettered and forgotten peoples, kept it from absorption in narration and from overemphasizing the particular event, the particular individual, and directed its attention more readily to culture as such. The most obtrusive data on a primitive tribe are its culture. Once culture-conscious, anthropology did not have far to go to become pattern-conscious. For much the same reason, it became process-conscious. The efforts of the pioneers like Tylor and Ratzel, however fumbling, were at least partly in this direction. It was Boas who first made us all able to see and deal better with process as such. This is his great contribution; this and the unswerving rigor of his critical standards. But process has not and cannot displace pattern, which retains its intrinsic significance in all historic material. The two simply are findings of different orders. And they are not in conflict. With knowledge of the processes at work, patterns as patterns are undoubtedly better understood. Without realization of the inherent patterns, the application of process concepts to material like culture leads to highly incomplete results. Not that the two approaches should be mixed; that would be fatal. They need intellectual differentiation, precisely because we shall presumably penetrate further in the end by two approaches than by one.

One can write a Q.E.D., or a virtual Q.E.D., under a scientific demonstration of process, or hope to do so. No sane historian writes a Q.E.D. under anything: neither a piece of history, nor archaeologic prehistory, nor a reconstruction, nor a pattern formulation. Those who like proofs above everything else are certainly entitled to make them. It is all to the good to have proofs made. It is also the privilege, in fact the wisdom, of those so minded to stop where their material no longer yields critically valid proofs. But this limit is not necessarily the limit of all intellectual endeavor because it is the limit of one approach; nor is what is beyond it necessarily the field merely of problemless antiquarians, biographers, and story-tellers. Differences in approach are probably at bottom largely dependent on differences of interest in individuals. It is perfectly legitimate to confine one's interest to the scientific approach, or to the historic, or to use alternately one or the other according to occasion. But sympathetic tolerance is intrinsically desirable; and certainly advantageous to deeper understanding: to "scientia."

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