

## Historical Approaches in American Ethnology

### A research survey

**T**HE IMPACT OF AMERICAN ANTHROPOLOGY on theories and methods in European ethnology has been overwhelming since the end of World War II<sup>1</sup>. This influence is primarily manifested in such fields as functionalistic and structuralistic ethnology. In its traditional main topic, culture-historical research, most of the continental European ethnology still follows the methods which have been developed by its devotees in the course of time<sup>2</sup>. The influence of American anthropology seems here negligible. Or perhaps it is felt in the way in which, among many scholars, historical methods and historical perspectives have been gradually abandoned? This question focusses our attention on the place of history in traditional and modern American anthropology. What has happened to the historical research programs of an older period? Has American anthropology, with its high-developed discussion of research methods, entirely discarded historical methods?

Of course this could not be the case. In America the study of archaeology and prehistory is part of anthropology, and even if typological classifications still play a considerable rôle in archaeology the main emphasis is increasingly on chronological sequences and historical reconstructions. Linguistics, another sub-discipline of anthropology, is largely structuralistic but has historical results in view—witness for example the recent discussions on glotto-chronology.

As ethnologists we are, however, first and foremost interested in the sub-discipline which is sometimes—although not so often nowadays—called ethnology<sup>3</sup> and is mostly referred to as cultural anthropology (a term which may include archaeology, prehistory and linguistics). It is in this great branch of anthropology that the leading functionalistic and structuralistic methods have been refined. More seldom we hear of the use of historical methods. Since the position of historical ethnology in the United States may have some importance for the future development of historical ethnology in Europe it seems desirable to present here a summary of its present situation and the course leading up to it.

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1. Cf. Å. Hultkrantz, *Anthropology as a Goal of Research: Some Reflections*. *Folk* (København), vol. 7 (1965), pp. 5-22. The same article published in Swedish, in: *Folk-liv* (Stockholm), vol. 25-26 (1962-63), pp. 5-19.

2. Å. Hultkrantz, *Some Remarks on Contemporary European Ethnological Thought*. *Ethnologia Europaea* (Paris), vol. 1 (1967), pp. 38-44.

3. Cf. Kroeber's pessimistic words at the Wenner-Gren Foundation's Symposium on Anthropology in 1952: "Now how about ethnology? I am about ready to abandon this baby to the wolves" (S. Tax et alii, *An Appraisal of Anthropology Today*. Chicago, 1953, p. 366). Cf. also Kroeber's characterization of the term ethnology as having "connotations of antiquarianism, outmodedness". [A.L. Kroeber, *The Range of the American Anthropologist*. *American Anthropologist* (New York), vol. 48 (1946), p. 298].

## 1.

IN HIS PREFACE TO DE ROHAN-CSERMAK'S MONOGRAPH on Eurasiatic sturgeon hooks<sup>4</sup> Professor Robert T. Heizer, well-known archaeologist and ethnologist at the University of California, Berkeley, observes that in central and northern Europe the study of folk cultures constitutes "a special kind of ethnography". In general, however, the idea of peasant cultures as introduced by Redfield, Lewis and Foster "does not seem to form the broad interpretive base for European studies, most of which are essentially straight reporting like the American Indian ethnographies of forty years ago". This sounds like a negative judgment on European ethnology, but is not to be understood that way, as emerges from Heizer's praising words of Rohan-Csermak's opus and from his subsequent pronouncements. He points out that European ethnologists dealing with European folk cultures have a rich archaeological and documentary evidence at their disposal. This evidence provides them with time depth with regard to aspects of contemporary folk culture. Heizer states: "This is a kind of ethnography that cannot be done in North America north of Mexico, and it is therefore largely unfamiliar to anthropologists in the United States"<sup>5</sup>.

In this connection Heizer also indicates another difference between European and American ethnology. Referring to the book at issue he says that it represents "the kind of useful and stimulating study of material culture that has unfortunately gone out of vogue in American ethnology"<sup>6</sup>. As a matter of fact, there is a direct connection between the low rate of investigations on material culture and the sparse frequency of the use of historical methods in American ethnology.

This is clearly demonstrated by a retrospect on the past development of American anthropology. Such a retrospect also gives us a clue to the present cautious attitude to historical research among American anthropologists. The new trends, appearing after World War II, in this kind of research must be understood against the background of past experiences<sup>7</sup>.

As will be remembered, the lofty speculations of unilinear evolutionism which dominated so much of the 19th century ethnological thought were in

4. G. de Rohan-Csermak, *Sturgeon Hooks of Eurasia*. Viking Fund Publications in Anthropology, N° 35. New York, 1962.

5. Heizer in: Rohan-Csermak, *op. cit.*, p. VII.

6. Heizer, *ibid.*, p. IX.

7. See the following general surveys: A.I. Hallowell, *The Beginnings of Anthropology in America* [in: F. de Laguna (ed.), *Selected Papers from the American Anthropologist 1888-1920*. New York, 1960]; R.H. Lowie, *Reminiscences of Anthropological Currents in America Half a Century Ago* [*Amer. Anthropol.*, vol. 58 (1956), pp. 995-1006]; idem, *The History of Ethnological Theory* (New York, 1937); A. Goldenweiser, *Recent Trends in American Anthropology* [*Amer. Anthropol.*, vol. 43 (1941)]; B.J. Meggers, *Recent Trends in American Ethnology* [*Amer. Anthropol.*, vol. 48 (1946)]; A.L. Kroeber, *The History and Present Orientation of Cultural Anthropology* (in: A.L. Kroeber, *The Nature of Culture*. Chicago, 1952); C. Kluckhohn, *Developments in the Field of Anthropology in the Twentieth Century* [*Cahiers d'histoire mondiale* (Neuchâtel), vol. 3 (1957), pp. 761 ff.].

the 1890's succeeded by the rigid historically oriented procedures applied by Boas and his followers. These procedures were not the same among all of these scholars, but they were sufficiently uniform to motivate the label "the American school of anthropology"<sup>8</sup>.

We may, of course, together with Kroeber and Radin, ask ourselves if Boas really was historical-minded<sup>9</sup>. He himself characterized his research principles as historical<sup>10</sup>, and an observer like Father Schmidt judged his reaction against evolutionism as a sign of conversion towards historicism<sup>11</sup>. It would, however, be an exaggeration to consider that Boas' procedures were provoked by a culture-historical interest. Two pronouncements made by Boas would help to enlighten this. First, he defined the divergencies between European and American (: his own) ethnology in the following way: "The difference between the two directions of study may perhaps best be summarized by the statement that American scholars are primarily interested in the dynamic phenomena of cultural change, and try to elucidate cultural history by the application of the results of their studies; and that they relegate the solution of the ultimate question of the relative importance of parallelism of cultural development in distant areas, as against worldwide diffusion, and stability of cultural traits over long periods to a future time when the actual conditions of cultural change are better known"<sup>12</sup>.

It is obvious from this quotation that Boas was not historical for history's own sake, and this is even more apparent from his next pronouncement, formulated already in 1896, according to which the outmost aims of the historical (diffusionistic) studies should be to find the cultural processes: "The immediate results of the historical method are, therefore, histories of the cultures of diverse tribes which have been the subject of study. I fully agree with those anthropologists who claim that this is not the ultimate aim of our science... When we have cleared up the history of a single culture and understand the effects of environment and the psychological conditions that are reflected in it we have made a step forward, as we can then investigate in how far the same causes or other causes were at work in the development of other cultures. Thus by

8. Goldenweiser, *op. cit.*, p. 157.

9. A.L. Kroeber, History and Science in Anthropology [*Amer. Anthropol.*, vol. 37 (1935), pp. 539 ff.]. See also P. Radin, *The Method and Theory of Ethnology* (New York and London, 1933), pp. 13 ff.

10. F. Boas, History and Science in Anthropology: A Reply (in: F. Boas, *Race, Language and Culture*. New York, 1940), p. 305.

11. P.W. Schmidt, Die Abwendung vom Evolutionismus und die Hinwendung zum Historizismus in der Amerikanistik [*Anthropos* (St. Gabriel-Mödling, bei Wien), vol. 16-17 (1921-22), pp. 487 ff.]. Cf. *idem*, Die kulturhistorische Methode und die amerikanische Ethnologie [*Anthropos*, vol. 14-15 (1919), pp. 546 ff.]. It should be added that Boas was no anti-evolutionist in principle: cf. R.H. Lowie, Evolution in Cultural Anthropology [*Amer. Anthropol.*, vol. 48 (1946), pp. 226 f.].

12. F. Boas. The Methods of Ethnology (Boas, *Race, Language and Culture*), p. 283.

comparing histories of growth general laws may be found"<sup>13</sup>. We may indeed discern a gradual change in Boas from historical to functionalistic problems. According to his own written statement it was around 1910, when he considered his historical methodology firmly anchored in cultural anthropology, that he began to deal with "the problems of cultural dynamics, of integration of culture and of the interaction between individual and society"<sup>14</sup>. This reorientation paved the way for all those non-historical trends in American anthropology which largely left their mark in the post-Boasian era.

It is possible to say that the historical perspective was better perceived among Boas' pupils, at least during certain periods; witness Kroeber's first edition of his *Anthropology*<sup>15</sup>, Sapir's theoretical manual for historical reconstruction<sup>16</sup>, Wissler's contributions to American Indian history<sup>17</sup>, Radin's formulations of an historical method<sup>18</sup> and the cautious historical procedures of Lowie, Dixon and Wallis. Still, with the exception of Radin, who unhesitatingly endorsed Maitland's dictum that "anthropology will have the choice between being history or nothing", none of these scholars was dedicated to the historical approach. The critical Radin remarks for instance, concerning Kroeber, that "his may be the face of history, but it certainly is the voice of the natural sciences"<sup>19</sup>. We all know how Sapir turned to studies of culture and personality<sup>20</sup>, Wissler to pseudo-ecological considerations<sup>21</sup>, Lowie to analyses of social

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13. F. Boas, *The Limitations of the Comparative Method of Anthropology* (Boas, *Race, Language and Culture*), pp. 278 f.

14. Boas, *History and Science in Anthropology: A Reply*, p. 311. Ruth Benedict, on the other hand, refers to a piece of verbal information from Boas according to which his conscious working on these problems did not take place until 1922 [R. Benedict, *Franz Boas as an Ethnologist. Amer. Anthropol. Ass., Memoirs*, vol. 61 (1943), p. 31]. It is doubtful whether he was, as White asserts, influenced by European functionalists [L.A. White, *Evolutionary Stages, Progress, and the Evaluation of Cultures. Southwestern Journal of Anthropology* (Albuquerque), vol. 3 (1947), p. 185 n. 79]. Lowie retorted that as a teacher in anthropology Boas had already discussed cultural dynamics during the first decade of this century [R.H. Lowie, *Some Facts about Boas, Southw. Journ. of Anthropol.*, vol. 4 (1948), p. 69]. See also Bunzel's note in: M. Mead and R.L. Bunzel (eds.), *The Golden Age of American Anthropology* (New York, 1960), p. 402.

15. A.L. Kroeber, *Anthropology*. New York, 1923.

16. E. Sapir, *Time Perspective in Aboriginal American Culture (Geol. Surv. of Canada, Anthropol. Ser., No. 13, 1916)*.

17. C. Wissler, *The American Indian*. New York, 1917.

18. Radin, *op. cit.*

19. Radin, *op. cit.*, p. 184. Cf. Kroeber's distinction between history and biology in anthropology in his article *Eighteen Professions* [*Amer. Anthropol.*, vol. 17 (1915), pp. 283 ff.].

20. Cf. D. Mandelbaum (ed.), *Selected Writings of Edward Sapir* (Berkeley and Los Angeles, 1949), pp. 507 ff.

21. C. Wissler, *The Relation of Nature to Man in Aboriginal America*. New York, 1926

organisation<sup>22</sup>, and so on. Kroeber became the expert *par préférence* in problems of cultural analysis<sup>23</sup>.

In what respect, then, did Boas and his followers perform historical investigations? Although there was considerable diversification in methods and aims we may summarize their achievements by stating that they mostly resorted to diffusionistic approaches. It was a conditional diffusionism, however, at least in the hands of Boas, Lowie and Dixon. Boas, for instance, who introduced diffusionistic methods of procedure in the 1890's<sup>24</sup>, was very careful not to prove too much. He used diffusionism in order to refute evolutionism, and he obtained the materials from the sphere of folk-tales and mythology<sup>25</sup>. As we all know, folk-tales lend themselves easily to such a treatment, and it is certainly no coincidence that from the turn of the century there appeared a series of articles on the dissemination of tales in the folkloristic papers. Thus there was a fertile soil in American folkloristic studies when Stith Thompson introduced the Finnish historic-geographic method in the 1920's.

There is, however, a very important difference between Boas and the later folklorists: the latter wanted to establish time sequences, Boas limited himself to indicate continuous traditions. As a matter of fact, he made polemics against those of his pupils who dared to reconstruct chronology on the evidence of diffusion<sup>26</sup>. Some of his pupils applauded him, for instance Goldenweiser, who declared that "at bottom diffusionism is anti-historical"<sup>27</sup>. Boas' opposition to speculative historical reconstructions parallels, in Europe, Radcliffe-Brown's and Malinowski's objections to "conjectural history"<sup>28</sup>. Perhaps his structures partly reflected his own disinterest in history. He was, however, first of all the sound scholar who considered historical development too complex to be

22. R.H. Lowie, *Social Organization*. New York, 1948.

23. Cf. A.L. Kroeber, *The Nature of Culture*. Chicago, 1952.

24. It has been said that American ethnology was dominated by Graebner during the Boas era (A. Brock-Utne, *Studiet av Primitive Folk*. Oslo, 1938, p. 49; cf. pp. 168, 210). Since the diffusional studies were used in the United States long before Graebner's *Methode der Ethnologie* appeared (1911), this judgment would appear to be without foundation. A close comparison between the two methods shows moreover few similarities.

25. See F. Boas, *Dissemination of Tales among the Natives of North America* [*Journal of American Folk-Lore* (Boston), vol. 4 (1891), pp. 13 ff.]; idem, *Indianische Sagen von der nord-pazifischen Küste Amerikas* (Berlin, 1895); idem, *Tsimshian Mythology* (*31st Annual Report of the Bureau of American Ethnology*, Washington, 1916).

26. F. Boas, *Primitive Art* (Institutet for sammenlignende kulturforskning, ser. B:8. Oslo, 1927), p. 6. Boas' standpoint is, however, unclear (see R.H. Lowie, Franz Boas 1858-1942 [*Journ. of Amer. Folklore* (New York), vol. 57 (1944), p. 64]. Thus, he thought it admissible to judge the antiquity of a cultural trait by its universality (F. Boas, *Race, Language and Culture*. New York, 1940, pp. 320, 569).

27. Goldenweiser, *op. cit.*, p. 161; cf. pp. 156 f.

28. Cf. below, footnote 55.

caught in broad syntheses. Maybe his analytical inclination did not suit such working procedures. His own efforts to reconstruct great historical sequences were, as Lowie has observed, less satisfactory<sup>29</sup>.

Whereas Boas mainly restricted his diffusion studies to the establishment of cultural connections, some of his pupils brought their diffusionistic analyses closer to the research methods of their European colleagues. Thus, Clark Wissler inaugurated the American so-called age-and-area theory, and Kroeber, Sapir, Spinden, Spier and others followed suit. This theory, which is founded on theoretical considerations developed by Ratzel and Tylor<sup>30</sup>, postulates that the wider the distribution of a cultural trait, the older is its age, and that the distribution runs in the form of concentric circles extending from the centre of the "cultural area" to its periphery, the marginal sub-area<sup>31</sup>. The concept of the culture area had been introduced by O.T. Mason in 1896 and was elaborated upon by Wissler — and later, Kroeber — as the denomination of an area with similar cultural traits<sup>32</sup>. The similarities were supposed to show a high degree of intensity in the centre of the area which therefore, according to the reasoning of these scholars, was the origin of the trait diffusions. Particularly old traits spread over the whole area, and outside it, whereas younger traits were thought to have a more limited distribution. These latter traits were according to the same scheme generated at the cultural centre, and as a consequence one always finds younger cultural elements in the centre of an area rather than at its periphery.

The most instructive expositions of the age-and-area method will be found in Wissler's, Kroeber's, Lowie's and Spier's comparative organisations of field data from the Plains, Southwestern and Californian culture areas between 1916 and 1928<sup>33</sup>. Investigations of this type went on much longer, but became scarce and more guarded after the 1930's. On the other hand, geographic studies of culture-element distributions continued, inspired by Kroeber, Clements and

29. Lowie, *The History of Ethnological Theory*, pp. 151 ff.

30. A. Hultkrantz, *General Ethnological Concepts* (International Dictionary of Regional European Ethnology and Folklore, vol. I. Copenhagen, 1960), pp. 24 f.

31. A.L. Kroeber, The Culture-Area and Age-Area Concept of Clark Wissler [S.A. Rice (ed.), *Methods of Social Science: A Case Book*. Chicago, 1931]. For a sharp criticism of the theory of concentric distribution, see R.B. Dixon, *The Building of Cultures* (New York, 1928), pp. 167 ff. (on the sun-dance complex).

32. Hultkrantz, *op. cit.*, pp. 76 ff.

33. Cf. for instance C. Wissler, *Distribution of Moccasin Decorations among the Plains Tribes* (Anthropological Papers of the American Museum of Natural History, vol. 29:1, New York, 1927); A.L. Kroeber, *The History of Native Culture in California* (University of California Publications in American Archaeology and Ethnology, vol. 20. Berkeley, 1923, pp. 125 ff.); idem, *Native Culture of the Southwest* (same series, vol. 23. Berkeley, 1928, pp. 375 ff.); R.H. Lowie, *Plains Indian Age-Societies* (Anthrop. Pap. of the Amer. Mus. of Nat. Hist., vol. 11:13. New York, 1916); L. Spier, *The Sun Dance of the Plains Indians: Its Development and Diffusion* (Anthrop. Pap. of the Amer. Mus. of Nat. Hist., vol. 16:7. New York, 1921).

Driver<sup>34</sup>, and Kroeber made an important contribution to the understanding of North American culture areas<sup>35</sup>. Nevertheless, after the second World War it seemed as if the diffusion studies had come to an end.

Let us pause here and try to find out, firstly, why historical research became diffusionistic, and secondly, why it went out of fashion.

As already said Boas used the diffusion approach as an instrument in his fight against what he called a pseudo-scientific law, the idea of unilinear evolution. The diffusion method enabled him to disprove evolutionism, to show that one cultural form in one place was connected with a similar cultural form in another place. However, in his view it did not grant him any means to prove historical seriation in a very definite sense. No doubt he could have gained more insight into specific historical sequences had he tried, but his rigid matter-of-fact inclination prevented any efforts of this sort. The "history" he reconstructed was horizontal rather than vertical, spatial in the sense in which for instance Kroeber understood it<sup>36</sup>. Boas' followers showed less inhibition in their use of diffusionistic methods, and evidently applied points of view which had been introduced into archaeology to make the most of them. But it is characteristic that they could not supplant diffusionism by other methods. As Kroeber himself has noted, theirs was a "flat" perspective of history<sup>37</sup>. We may, as Kroeber did, refer this to the unhistorical American mind, or to the shortage of white man's history in America and the lack of old traditions<sup>38</sup>. Europeans have often noticed that American scholars avoid the historical perspectives because these invite to antiquarianism, arbitrary judgments and escapism from today's urgent issues. It is no coincidence that, after the passing of the great, European-trained generation of American anthropologists (Boas, Lowie, Radin, Wallis and others), American anthropology turned into a social science of sociological, psychological and similar problems.

Still, this explanation is not sufficient. Diffusionistic research offered itself as the only tool to history, since at that time neither archaeological finds nor documentary sources could be used without difficulties in the study of American Indians - and this study was quite naturally the main concern of American ethnologists. Only in rare cases, and particularly in the studies of Indians East of the Mississippi, were documents consulted<sup>39</sup>. The "flat" perspective was

34. See below for further details, pp. 108 ff.

35. A.L. Kroeber, *Cultural and Natural Areas of Native North America*. Berkeley, 1939.

36. A.L. Kroeber, *History and Evolution* [*Southw. Journ. of Anthropol.*, vol. 2 (1946), p. 11].

37. Kroeber, *The History and Present Orientation of Cultural Anthropology*, p. 151; W.N. Fenton, *The Training of Historical Ethnologists in America* [*Amer. Anthropol.*, vol. 54 (1952), pp. 335, 337].

38. Cf. also Mead in: Mead and Bunzel, *op cit.*, p. 2.

39. See further below, pp. 114 ff.

thus unavoidable. Those who, like Boas, Dixon and Goldenweiser, doubted its value for historical reconstructions stayed away from it. A scholar like Radin tried to use the "internal evidence" of the information he had obtained from the Indians to achieve the time-depth he wanted; but few endorsed this method since it presupposed the unconditioned historical truth of oral tradition<sup>40</sup>.

In this connection we must not forget one essential circumstance: namely, that the culture-area concept from which the age-and-area theory proceeded had been invented as a device to arrange museum objects. The same scholar who had formulated the first-mentioned concept, Otis T. Mason, curator of ethnology at the Smithsonian Institution during the last decades of the last century, had also introduced the evolutionistic serial arrangements in museum exhibits. Wissler then developed the culture-area concept into a historical tool and supplanted Mason's evolutionistic arrangements of objects with his arranging them according to concentric diffusion. It is true that folkloristic themes, ceremonies, cults and other intangibles also could and were investigated this way, but the point of departure was the material objects, houses, household goods, means of transportation, ceremonial bundles, artistic creations, and the like<sup>41</sup>. Boas' interest for myths and tales in distributional perspective was also partly shared by the protagonists of the age-and-area school, but their main emphasis was on the spread of material culture, with ceremonial and social systems closely following.

The age-and-area approach, as well as the Boas period, came to an end in the 1920's<sup>42</sup>. There were many reasons for this. One was that by this time the North American aboriginal cultures had ceased to exist in their assumed uncontaminated state, and their last remains had been thoroughly investigated<sup>43</sup>. Interest now turned towards the ethnographical fields in other continents where more primitive conditions still ruled and where the reconstructive, looking-backwards perspective was of no immediate concern. At the same time new trends appeared, partly inspired from Europe: the studies of psychoanalysis and the personality in culture, the studies of patterns and values, and the community studies. Functionalism and structuralism slowly replaced historical investigations. History gave way to process, as shown in the studies of culture contact (acculturation), and the accidental historical events were less intriguing for the new

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40. Cf. R.H. Lowie, *Oral Tradition and History* [*Journ. of Amer. Folk-Lore*, vol. 30 (1917), pp. 164 ff.]. Cf. Radin's counter-argument in his *The Method and Theory of Ethnology*, p. 50 n. 18. The truth value of aboriginal traditions was taken for granted by earlier American scholars such as J.W. Fewkes.

41. Cf. for example Wissler's monographs on Blackfoot culture.

42. Cf. Meggers, *op. cit.*, p. 176.

43. Mead and Bunzel, *op. cit.*, p. 574.



anthropologists than recurring themes and regularities investigated by cross-cultural surveys. Fields which once were the laboratory of historical methods, such as kinship and other aspects of social organisation, became the chief domain of structuralistic methods. Finally, applied anthropology entered the scene.

This widening of the ethnographical approaches, and the diversion from the American Indian ethnographical field, would not in themselves have impeded historical research if not other factors had been influential as well. It proved fateful that Franz Boas and his pupils had not been able to impress ethnological research outside America. They themselves worked incessantly with the ideas and theories provoked by their European colleagues, but their impact abroad was little indeed<sup>44</sup>. One reason for this was, perhaps, that Boas never attained a coherent ethnological theory, and that his pupils among the age-and-area theorists had a too mechanistic vision of culture spread. In any case, the historical methods were not so ingrained that they could stand the changes of time (and times change quickly in America). Furthermore, there was a change in the philosophical outlook on culture, Boas' key-concept: the old notion of culture as "a self-contained class of events" was superseded by the idea that it was "an 'abstraction' or a sort of accessory to man that can be explained only in terms of his nervous system"<sup>45</sup>. Sapir, Linton and Hallowell paved the way for this new outlook which, for a time at least, seemed to paralyze descriptive and historical ethnological work. Just as the psychologically oriented anthropologists atomized the historical culture concept the students of acculturation (sometimes identical with the former) denied the mechanistic implications in Wissler's age-and-area theory. They could show that diffusion is a most complicated process the direction and success of which depends upon many factors<sup>46</sup>. Likewise structuralists pointed out that elements of social organisation could not be treated on a distributional basis<sup>47</sup>.

A debate about the validity of the age-and-area theory took place. American linguists found that it was applicable to their particular subject<sup>48</sup>. On the other hand, critical anthropologists such as Hodgen and Wallis considered it inappropriate in ethnology<sup>49</sup>. Wallis said, "No one who has asserted the hypothesis that magnitude of distribution is an index of age has, so far as the

44. Mead and Bunzel, *op. cit.*, pp. 6 ff.

45. Meggers, *op. cit.*, p. 196.

46. See e.g. R. Linton, *The Study of Man* (New York and London, 1936), pp. 324 ff.

47. G.P. Murdock, *Social Structure* (New York, 1949), pp. 58, 192 ff.

48. G. Bonfante and Th.A. Sebeok, Linguistics and the Age and Area Hypothesis [*Amer. Anthropol.*, vol. 46 (1944), pp. 382 ff.].

49. M.T. Hodgen, Geographical Diffusion as a Criterion of Age [*Amer. Anthropol.*, vol. 44 (1942), pp. 345 ff.]; *idem*, Similarities and Dated Distributions [*Amer. Anthropol.*, vol. 52 (1950), pp. 445 ff.]; W.D. Wallis, Inference of Relative Age of Culture Traits from Magnitude of Distribution [*Southw. Journ. of Anthropol.*, vol. 1 (1945), pp. 142 ff.].

present writer can discover, offered any evidence that it is true"<sup>50</sup>. Facing the criticism some adherents of the distributional approach tried new methodological solutions. John Cooper, the eminent anthropologist at the Catholic University in Washington, proposed a method for establishing cultural sequences which partly reminds us of the method applied by the Swedish ethnologist, Nordenskiöld<sup>51</sup>: to analyze the total cultures in an area and establish common traits in its marginal sub-areas which are not found in intervening non-marginal regions. If these traits cannot be explained in terms of independent origin or cultural transmission they should be understood as a common cultural heritage, antedating the corresponding features in the intervening cultures<sup>52</sup>. Cooper's theory found little resonance, and the same may be said about Loeb's efforts to introduce techniques reminiscent of those of the Vienna school<sup>53</sup>.

The final result of the emerging criticism was that on the eve of World War II historical research among primitives, and thus in ethnology, had fallen into some sort of disreputation<sup>54</sup>. Most American ethnologists now considered all historical research without dated foundations to be "conjectural history"<sup>55</sup>. Historically inclined ethnologists became archaeologists, for in archaeology new materials and new methods seemed to open fruitful historical perspectives. Some distribution studies went on, particularly in material culture. But in the main historical ethnology seemed to have gone out of fashion.

## 2.

A COMPARISON WITH EUROPEAN ETHNOLOGY, both general and regional, at the end of the 1930's, gives clear evidence that the all but complete extinction of historical ethnology in the United States had no counterpart in continental Europe. Now it is true that debates around concepts and methods always have been more intense and more critical in America (although Murdock, in his

50. Wallis, *op. cit.*, p. 146.

51. E. Nordenskiöld, *En jämförelse mellan indiankulturen i södra Sydamerika och i Nordamerika* [Ymer (Stockholm), vol. 26 (1926), pp. 298 ff.].

52. J.M. Cooper, *Temporal Sequence and the Marginal Cultures* (Catholic University of America, Anthropological Series, No. 10. Washington, 1941); *idem*, *The Culture of the Northeastern Indian Hunters: A Reconstructive Interpretation* [F. Johnson (ed.), *Man in Northeastern North America*. Andover, Mass., 1946, pp. 272 ff.].

53. Cf. for instance E.M. Loeb, *Tribal Initiations and Secret Societies* (Univ. of Calif. Publ. in Amer. Archaeol. and Ethnol., vol. 25: 3. Berkeley, 1929).

54. This criticism was strengthened by the severe reaction, in Europe as well as in the United States, against the historical reconstructions by Father Schmidt and Elliot-Smith.

55. Concerning the term conjectural history, cf. A.I. Hallowell, *The Beginnings of Anthropology in America* [F. de Laguna (ed.), *Selected Papers from the American Anthropologist*. Evanston and Elmsford, 1960], p. 12 n. 32; A.R. Radcliffe-Brown, *Structure and Function in Primitive Society* (London, 1952), p. 104.

*Social Structure*, charges the "Boas school" with their negligence of not having developed the concept of culture — a strange accusation in view of Kroeber's literary production on just this topic). However, even if in certain theoretical issues European ethnology was lagging behind, its historical orientation was founded on the traditions of the discipline on this continent, and, as concerns regional European ethnology, in the nature of the subject-matter.

The situation was different in America. Herskovits has rightly emphasized the fact that in American anthropology there has been, and still is, an antihistorical tendency which stresses structural laws and ignores historical depth<sup>56</sup>. Also Kroeber was, as we have seen, aware of this non-historical outlook, and referred it to the lack of old historical traditions. However, there is much that speaks for Washburn's assumption that the lack of significant and easily available historical evidence for most Indian societies created the unpopularity of the historical approach<sup>57</sup>. Fenton, a first-rank ethnologist in present-day American ethnology, thinks that the main reason why ethnology has matured so slowly as an historical science "is the long-standing preoccupation with diffusional studies and the neglect of direct history. Since studies of diffusion have passed out of fashion, direct history has been forgotten"<sup>58</sup>.

Indeed, most anthropological studies in America in the 1940's and 1950's were not concerned with history or development at all, unless they were purely archaeological (and archaeology was to a large extent occupied with taxonomic classifications which largely alienated it from European archaeology). The popular fields were acculturation and personality studies, whether in Freudian form or in the newly developed personality-and-culture pattern. Radcliffe-Brown had been a professor at the Chicago University between 1931 and 1937, and stimulated by his teaching thorough investigations on social structure, in particular kinship studies, took form. Statistical devices were introduced in the cross-cultural surveys guided by the research scholars of the Human Relations Area Files<sup>59</sup>.

56. M.J. Herskovits, *The Ahistorical Approach to Afroamerican Studies: A Critique* [*Amer. Anthropol.*, vol. 62 (1960), pp. 559 ff.].

57. W.E. Washburn, *Ethnohistory: History "in the Round"* [*Ethnohistory* (Bloomington), vol. 8 (1961), p. 39].

58. W.N. Fenton, *The Training of Historical Ethnologists in America* [*Amer. Anthropol.*, vol. 54 (1952), p. 328].

59. Concerning American anthropology after the War, cf. R.H. Lowie, *Gegenwärtströmungen in der amerikanischen Völkerkunde* [*Mitteilungen aus dem Museum für Völkerkunde in Hamburg*, vol. 23 (1951), pp. 7 ff.]; *idem*, *Contemporary Trends in American Cultural Anthropology* [*Sociologus* (Berlin), vol. 5 (1955), pp. 113 ff.]; R. Beals, *Current Trends in the Development of American Ethnology* (*Selected Papers of the Fifth International Congress of Anthropological and Ethnological Sciences*, Philadelphia, 1960, pp. 11 ff.).

To a European observer it is conspicuous that the new methods of dealing with folk cultures in Europe, United States, Mexico, Indian and Japan scarcely brought forth any historical perspectives. American folk-culture students differ in that respect most obviously from their European colleagues in folklife research, or regional ethnology. One reason seems to be that the American folk-culture research was partly initiated by a social anthropologist and doctor of law, Robert Redfield, whose field-work, as we know, was concentrated on modern conditions in Central American communities<sup>60</sup>. Like the authors of the so-called community studies (Warner, Arensberg, Lynd and others) the authors of folk-culture studies have often been as much sociologists as anthropologists<sup>61</sup>. It is only when aspects of culture contact have come into focus that some type of a historical perspective has been resorted to<sup>62</sup>.

Whether the historical study of folk culture will gain ground in the discipline of folklore is difficult to tell. Many years ago Thompson considered it both desirable and possible to widen the folklore research to research on folk cultures<sup>63</sup>. Such an expansion of the scope would most probably have resulted in a transference of the historical methods in the study of oral traditions to the studies of material and social culture. As we all know diffusionistic methods loom large in American folklore research<sup>64</sup>. There is as yet no sign, however, that Thompson's vision will be realized<sup>64a</sup>.

In the late 1950's the future of historical ethnology seemed rather hopeless. The great leaders with a European heritage, such as Lowie, Radin and Kroeber, were rapidly disappearing from the scene, and with them as it seemed a deeper concern with historical procedures. True, emigrants from Europe, working in folklore, anthropology and related disciplines, continued in the United States the historically slanted research which they had conducted in Europe<sup>65</sup>. But they did not considerably influence the trends of American ethnology. Even in 1966, when the winds have changed, Fenton declares that "ethnology, in the

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60. R. Redfield, *The Folk Culture of Yucatan*. Chicago, 1941.

61. Cf. in this connection the following works: R. Redfield, *The Folk Society* [*American Journal of Sociology* (Chicago), vol. 52 (1947)], and R.T. & G. Anderson, *Sexual Behavior and Urbanization in a Danish Village* [*Southw. Journ. of Anthropol.*, vol. 16 (1960)].

62. G.M. Foster, *Culture and Conquest: America's Spanish Heritage*. Vik. Fund Publ. in *Anthrop.*, N° 27. New York, 1960.

63. Cf. Hultkrantz, *General Ethnological Concepts*, p. 138.

64. Cf. R.M. Dorson, *Historical Method and American Folklore* [*Indiana Historical Bulletin* (Indianapolis), vol. 23 (1946), pp. 84 ff.].

64a. Cf., however, the recent article by N.F. Riedl, *Folklore and the Study of Material Aspects of Folk Culture* [*Journal of Amer. Folkl.*, vol. 79 (1966), pp. 557 ff.].

65. In this connection we may mention an agrarian anthropologist like Paul Leser and a folklorist and anthropologist like Sven Liljebblad. Cf. Leser in *Anthropos* (Freiburg), vol. 49 (1954), pp. 729 ff.; S. Liljebblad, *The Indians of Idaho*. Boise, 1960.

European sense of culture history, today has few devotees and almost no masters. ... To be certain, as we have become more 'scientific' we are less 'humanistic' " 66.

Indeed this does not sound very promising. However, let us not forget that Fenton thinks here of the broad, synthetic and impressionistic works, not of the thorough analyses of limited space and time.

For the winds have changed.

### 3.

THE NEW APPROACHES TO HISTORY have been groping and cautious. They have developed as a necessity, for no science of culture is complete without an historical perspective. To a certain extent it is possible to say that the new growth of historical ethnology has taken place within the lines of research which came in vogue after the Boas era. However, also old methods of historical research have been renewed and revitalized. The new perspectives are no longer solely based upon American Indian cultures, or even the cultures of primitive peoples. In some measure the civilizations of the Old World now provide some of the factual foundations.

The rigour of the new methods and the skill exhibited in their application has made an impression upon such an anthropologist as Lévi-Strauss who cannot be charged with being too dedicated to historical procedures. "Conjectural history", he says, "has perfected and refined its methods... One may well ask oneself, then, if Radcliffe-Brown's mistrust of historical reconstructions did not correspond to a stage of scientific development which will soon have passed" 67.

Let us find out what has happened.

As just mentioned some of the old methods have been recreated and refined. To many anthropologists the classical distributional studies represent a dead end. As Lowie says, "this sort of thing is simply no longer done" 68. Yet, there is still such work being done, and it will apparently also keep its hold in the future, for it reveals — if nothing else — spatial cultural connections. The foundations for the new diffusionism were laid in the 1930's (see above) and originated with Kroeber. Under the sponsorship of the University of California a "Culture Element Survey" was carried out by 13 field investigators among 254

66. W.N. Fenton, Review of "The Paths of Culture: A General Ethnology, by K. Birket-Smith" [*Amer. Anthropol.*, vol. 68 (1966)], pp. 530 f.

67. C. Lévi-Strauss, The Scope of Anthropology [*Current Anthropology* (Chicago), vol. 7 (1966)], p. 115.

68. Lowie, *Contemporary Trends*, p. 119.

tribes and bands West of the Rocky Mountains ("the Californian school")<sup>69</sup>. "The prime purpose of the Survey", writes Kroeber, "was to insure greater comparability of cultural information than existed in the published works of ethnographers. ... We originally expected that more evenly comparable data would make possible statistical treatment defining more authoritatively than heretofore the similarity groupings of a long array of tribal cultures, their geographical variation, and inferences as to their historic growth"<sup>70</sup>. As Kroeber points out, this project created little attention. The anthropological interests had turned to acculturation, culture and personality, integration and pattern, and the questionnaire approach and atomization of cultural materials did not appeal to the new generations of cultural anthropologists<sup>71</sup>. At the same time, however, the statistical treatment pointed to the future, and not only in historical research: cross-classification tables with statistical tests of significance (by correlation coefficients, mean square contingency coefficients and chi-square) were subsequently applied by social anthropologists in cross-cultural analyses of social structure.

The results of the Culture Element Survey were handled statistically by, i.a., Kroeber himself, Klimek, Milke and Driver. It is mainly due to the efforts of the latter that diffusion studies have survived, although they have by no means the same response today as they had forty years ago. Together with a colleague, Massey, Driver has worked out an "atlas" comprising of the distribution of important elements of material and social culture in aboriginal North America<sup>72</sup>. He has even tried to analyze elements of social structure, such as kinship systems and avoidances, from a diffusionistic point of view<sup>73</sup>. His approach to historical reconstruction is very cautious. "Too little is known about time differences among non-literates who have left no documentary records, but spatial relations may be shown on maps"<sup>74</sup>. If, however, reliable criteria are at hand certain valid reconstructions may be made. For instance, in his distributional analysis of girls' puberty rites Driver has shown a great number of similarities between the Apache rites in the Southwest and those of Northern California and the Northwest Coast. He concludes that all three complexes have a single origin in

69. See the series Culture Element Distributions in Anthropological Records (University of California), edited by A.L. Kroeber.

70. A.L. Kroeber, *The Nature of Culture* (Chicago, 1952), p. 263.

71. For further criticism, see my remarks in *Temenos* (Helsinki), vol. 1 (1965), p. 101.

72. H.E. Driver and W.C. Massey, *Comparative Studies of North American Indians* [*Transactions of the American Philosophical Society* (Philadelphia), vol. 47 (1957), pp. 165 ff.].

73. H.E. Driver, *An Integration of Functional, Evolutionary, and Historical Theory by Means of Correlations* (Indiana University Publications in Anthropology and Linguistics, Memoirs, vol. 12. Bloomington, 1956); *idem*, *Geographical-Historical Versus Psycho-Functional Explanations of Kin Avoidances* [*Curr. Anthropol.*, vol. 7 (1966), pp. 131 ff.].

74. H.E. Driver and P.R. Sanday, *Factors and Clusters of Kin Avoidances and Related Variables* [*Curr. Anthropol.*, vol. 7 (1966)], p. 176.

the North, since the Apache once came from Canada<sup>75</sup>. There is nothing new and revolutionary in this approach; but it stands out positively from earlier diffusionist studies in the almost hesitating, cautious application of historical inferences.

It is difficult to prophesy the future of the diffusionistic method. In showing distributions of cultural materials and spatial relations between groups of people it will probably be of a certain import. With additional criteria it can, cautiously, be used for historical reconstructions. Diffusion studies have their greatest prospect in the fields of oral traditions and material culture. Although the occupation with material culture has never quite ceased among American ethnologists<sup>76</sup> such studies have been rather scarce since the 1920's. There are at present certain signs that they will regain part of their popularity. Thus, the American Anthropological Association is now sponsoring a program of fellowships intended to support research on ethnology based on the study of museum collections<sup>77</sup>. This program is mostly concerned with American Indian materials and does not at all touch on collections of the white man's folk cultures. Attention has recently been drawn to the fact that both anthropologists and folklorists have neglected the material aspects of American folk culture<sup>78</sup>. Here is a field where diffusion studies could easily be tested against the testimony of written sources.

Diffusionism thus represents an old tradition in American anthropological research. Otherwise the growing historical interest has proceeded from sources rather different from the traditional ethnology of continental Europe. Awkward as it sounds, the new historical trends stem mostly from the non-historical research lines mentioned above, and from the inspiration from the national files of documents in archives, as well as from the new progress made in archaeology.

One point of departure has been the functionalistic and structuralistic research on social organisation according to the British model. As has already been

75. H.E. Driver, *Culture Element Distributions: XVI, Girls' Puberty Rites in Western North America* [*Anthropological Records of the University of California* (Berkeley), vol. 6 (1941)].

76. Cf. for instance the recent contribution by E.T. Adney and H.I. Chapelle, *The Bark Canoes and Skin Boats of North America* (Smithsonian Institution, United States National Museum, Bulletin 230, Washington, 1964). Other important post-War works include Castetter's paper on Yuman Indian agriculture (1951), Davidson's later articles on Australian fire-making (1947), footwear (1947) and churingas (1953), as well as his supplement to the earlier published snowshoe-study (1953), Heizer's investigation of fish poisons (1953), Hunt's survey of American Indian beadwork (1951), and Rostlund's paper on freshwater fish and fishing in native North America (1952).

77. Fellow Newsletter, *Amer. Anthropol. Ass.* (Menasha), vol. 7 (1966), pp. 1 ff.

78. N.F. Riedl, *Folklore vs. Volkskunde: A Plea for More Concern with the Study of American Folk Culture on the Part of Anthropologists* [*Tennessee Folklore Society Bulletin* (Athens, Tenn.), vol. 31 (1965), pp. 47 ff.].

mentioned, Radcliffe-Brown was for a time professor at the University of Chicago where he inspired many young anthropologists — for instance, Eggan and Tax<sup>79</sup>. However, in contrast to their teacher several of these anthropologists, in particular Eggan, combine functional-structural and sequential perspectives in the study of kinship<sup>80</sup>. Eggan says, "we need to adopt the structural-functional approach of British social anthropology and integrate it with our traditional American interest in culture process and history"<sup>81</sup>. Also a well-known anthropologist outside this circle, George Peter Murdock, applies similar perspectives in his comparative investigation of kinship systems<sup>82</sup>. Lowie sees this change of outlook from the static to the dynamic, temporal perspective as "a natural consequence of their establishing functional relationships, since the priority of one of two or more correlates implies a 'causal' sequence"<sup>83</sup>. In other words, we have here an evolutionistic sequence according to White's view of the evolutionistic procedure.

It is a moot point if we should consider the evolutionistic perspective "historical" or not. Leslie White who, during the last World War, resumed the evolutionistic approach from the oblivion into which it had fallen under Boas and his pupils has the definite opinion that there are three kinds of interpretation of culture: history, evolutionism and functionalism. History, he says, is a temporal process which deals with the particular and unique, whereas evolution is a temporal-formal (functional) process in which cultural phenomena are presented as a temporal sequence of forms<sup>84</sup>. Of course everything is dependent upon whether we want to define history in this narrow way; Kroeber who had a broader, "spatial and horizontal" view of history meant that White simply reduced cultural history to generalized formulas<sup>85</sup>. Steward who attempts to establish regularities of developmental sequences in limited cultural fields (whereas White studies the cultural forms in the culture of mankind as a whole) is aware that he is dealing with historical reconstruction<sup>86</sup>. Even if his main attention is centered on the cultural process his finds are definitely of historical import.

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79. Cf. F. Eggan (ed.), *Social Anthropology of North American Tribes*. Chicago, 1937; 2nd ed. 1955.

80. F. Eggan, *Social Organization of the Western Pueblos*. Chicago, 1950.

81. F. Eggan, *Social Anthropology and the Method of Controlled Comparison* [*Amer. Anthropol.*, vol. 56 (1954)], p. 745.

82. G.P. Murdock, *Social Structure* (New York, 1949), pp. 323 ff.

83. Lowie, *op. cit.*, p. 120.

84. L. White, *History, Evolutionism, and Functionalism: Three Types of Interpretation of Culture* [*Southw. Journ. of Anthropol.*, vol. 1 (1945)], pp. 221 f., 243.

85. A.L. Kroeber, *History and Evolution* [*Southw. Journ. of Anthropol.*, vol. 2 (1946)], pp. 13 ff.

86. J.H. Steward, *Theory of Culture Change: The Methodology of Multilinear Evolution*. Urbana, 1955.



This emerges clearly from, for instance, his scheme of parallel development for the great civilizations<sup>87</sup>. Steward arrives here at the formulation of the independent recurrence of synchronic and sequential interrelationships of cultural phenomena, based on the developmental regularities of early civilizations in Peru, Meso-America, Mesopotamia, Egypt and China. The idea of such a pattern of sequences (which reminds us of earlier developmental analyses by Spengler, Toynbee and Kroeber) may be termed evolutionistic, but the establishment of regularities summarizes historical findings, and the method involved may be judged as historical<sup>88</sup>.

In his evolutionistic analyses Steward proceeds from an ecological perspective. His intensive preoccupation with the natural conditions of Great Basin social organisation during field-research in this area in the 1930's directed his attention to the ecological determinants of cultural process<sup>89</sup>. Ecological considerations have for some time been of great importance to archaeology; they now enter ethnological research and provide new tools for culture-historical reconstructions<sup>90</sup>. Anyone who has studied American anthropological journals in this decade will be convinced that the combination of ecological and neo-evolutionistic perspectives is by no means a rare phenomenon in present-day anthropology.

Another root of modern historical studies may be found in the investigations of acculturation, initiated in the 1930's. In the beginning acculturation was almost another word for diffusion. It was used in this way already eighty years ago by Holmes<sup>91</sup>, and Herskovits, one of the authors behind the famous *memorandum*<sup>92</sup>, considers acculturation to be "but a specialized form of the diffusion process"<sup>93</sup>. More and more, however, acculturation was apprehended as the process of change in complete culture-contact<sup>94</sup>, and theorists differentiated between degrees of process and degrees of change<sup>95</sup>. Acculturation takes place when two (or more) complete cultures meet, and the following-up of the fusion or

87. J.H. Steward, *Cultural Causality and Law: A Trial Formulation of the Development of Early Civilizations* [*Amer. Anthrop.*, vol. 51 (1949), pp. 1 ff.]. See also *Theory of Culture Change*, pp. 178 ff.

88. I am aware that some European ethnologists, Heine-Geldern for instance, do not share my opinion on this issue.

89. Cf. Steward, *Theory of Culture Change*, pp. 30 ff.

90. See e.g. E.N. Ferdon, *Agricultural Potential and the Development of Cultures* [*Southw. Journ. of Anthrop.*, vol. 15 (1959), pp. 1 ff.].

91. W.H. Holmes, *Pottery of the Ancient Pueblos* (*4th Ann. Rep. of the Bur. of Ethnol.*, Washington, 1886), p. 266.

92. R. Redfield, R. Linton and M.J. Herskovits, *Memorandum on the Study of Acculturation* [*Amer. Anthrop.*, vol. 38 (1936), pp. 149 ff.].

93. M.J. Herskovits, *Some Comments on the Study of Culture Contact* [*Amer. Anthrop.*, vol. 45 (1941)], p. 8.

94. Hultkrantz, *General Ethnological Concepts*, p. 17.

95. Cf. e.g. B.P. Dohrenwend and R.J. Smith, *Toward a Theory of Acculturation* [*Southw. Journ. of Anthrop.*, vol. 18 (1962)].

resistance involved is therefore sometimes equivalent to a chronological study of culture-contact. The presupposition is, of course, that there are documents at hand which can show the development. A typical case of this form of acculturation studies is Padden's paper on culture-contact in Chile<sup>96</sup>. Here we are reminded of Fenton's statement that "cultural history offers the best opportunity for testing theories of pattern growth and decline, for demonstrating cultural change, and for explaining stability"<sup>97</sup>.

Padden's paper may justly be called ethnohistorical, and with this term we are confronted with the latest, and seemingly enduring, approach in American historical ethnology. It is even possible to say that ethnohistory has salvaged this ethnology from the fate Kroeber had, despairingly, assigned for it: a recent death<sup>98</sup>. To many researchers this new trend has appeared as a great surprise, as something unexpected in the apparent ahistorical anthropological milieu in America<sup>99</sup>. An experienced ethnologist like Fenton sees it as the fulfilment of Maitland's prophecy that anthropology has to make a choice between becoming history or nothing<sup>100</sup>. European ethnologists will in ethnohistory recognize an approach similar to what they have been dealing with for many decades, a technique which they take for granted. In America, it has not been so, due to the scarcity, until recent times, of the necessary materials, as well as to their inaccessibility.

Ethnohistory as a concept was introduced by Clark Wissler in 1909<sup>101</sup>. To him it meant the search for ethnological information in historical documents<sup>102</sup>. Today, when a whole journal is dedicated to the research on ethnohistory, it is defined as "original research in the documentary history of the culture and movements of primitive peoples, and related problems of broader scope"<sup>103</sup>. Literary documents from previous periods are thus the key to the understanding of historical time-depth, not the reconstructions on the basis of diffusionistic methods. This does not prevent the adherents of ethnohistorical approach from stressing

96. R.C. Padden, Cultural Change and Military Resistance in Araucanian Chile, 1550-1730 [*Southw. Journ. of Anthropol.*, vol. 13 (1957)].

97. Fenton, *The Training of Historical Ethnologists*, p. 336.

98. Cf. above, footnote 3, and F. Eggan, *Some Anthropological Approaches to the Understanding of Ethnological Cultures* [*Ethnohistory*, vol. 8 (1961)], pp. 3 f., 8.

99. Cf. for instance P. Leser, Review of "Africa: Its Peoples and Their Culture History, by G.P. Murdock" [*Technology and Culture* (Cleveland), vol. 1 (1960)], p. 257.

100. W.N. Fenton, *Ethnohistory and Its Problems* [*Ethnohistory*, vol. 9 (1962)], p. 4. Cf. also above, p. 99.

101. C. Wissler (ed.), *The Indians of Greater New York and the Lower Hudson* (Anthrop. Pap. of the Amer. Mus. of Nat. Hist., vol. 3. New York, 1909), p. XIII.

102. D.A. Baerreis, *The Ethnohistoric Approach and Archaeology* [*Ethnohistory*, vol. 8 (1961)], p. 49.

103. Washburn, *Ethnohistory*, p. 31. This definition will be found as an explanation on the covers of each separate number of the journal "Ethnohistory".

the importance to the study of historical stratification of a cautious investigation of continuous diffusion<sup>104</sup>. Furthermore, although ethnohistorians rely heavily on documents they also take into consideration archaeological evidence, inferences from the study of social organisation, linguistic reconstructions, genealogies, etc. Since the majority of American ethnohistorians are concerned with American Indian cultural history the time-span covered by documents is very short, 400 years at the most, for the western part of the continent not more than 150 years (the pueblo settlements and the Californian Mission Indians excepted). This means that archaeology supplies a quantity of information, and since both archaeology and ethnology are parts of anthropology the passage from one to the other is not the same step as for instance in Europe. Often an investigator is archaeologist and ethnologist in the same person.

It is not too bold to say that the growth of ethnohistory was partly inspired by archaeologists. The great expansion of American archaeology after 1930, in finds, theories and techniques, provided the presumptions. Archaeologists developed a sense for time-depth which the age-and-area specialists seldom did<sup>105</sup>, and new dating procedures (analysis of pollens, dendrochronology, radiocarbon dating, fluorine dating) made their time-measurements fairly reliable. Julian Steward formulated, in 1942, the so-called direct historical approach by which cultural complexes could be carried backward in time from the historic to the proto- and prehistoric periods<sup>106</sup>. The method had earlier been used by such archaeologists as Nelson and Strong, but was now made a major instrument for culture-historical reconstruction. (Cf. Fenton's "upstreaming", below.) By 1950 archaeology had attained such importance that, as Eggan reminds us, at the Wenner-Gren International Symposium in 1952 "culture history was mainly in the hands of the [American] archaeologists, both with regard to method and theory and also results"<sup>107</sup>. Good examples of ethnohistorical research through the combination of ethnography, historical records and archaeology are Heizer's and Mill's history of the Yurok Indian village, Tsurai on Trinidad bay, and Eggan's reconstruction of Plains Indian history<sup>108</sup>.

The main stream of ethnohistorical studies refers however to the analysis of written documents. Although the idea of "ethnohistorical research" is fairly recent, ethnological studies based on documents occurred at an early date. The

104. Fenton, *op. cit.*, pp. 14 f.

105. Cf. I. Rouse, Culture Area and Co-Tradition [*Southw. Journ. of Anthropol.*, vol. 13 (1957), pp. 123 ff.].

106. J.H. Steward, The Direct Historical Approach to Archaeology [*Amer. Antiquity* (Menasha), vol. 7 (1942)], p. 337.

107. Eggan, *op. cit.*, p. 3.

108. R.F. Heizer and J.E. Mills, *The Four Ages of Tsurai*. Berkeley and Los Angeles, 1952; F.R. Eggan, *The Ethnological Cultures and Their Archeological Backgrounds* [J.B. Griffin (ed.), *Archeology of the Eastern United States*. Chicago, 1952], pp. 35 ff.

foremost pioneers were John Swanton and Frank Speck who, working among the almost extinguished Indian cultures East of the Mississippi, by necessity had to have recourse to old written records. It is well-known among Americanists what excellent results they reached<sup>109</sup>. Still, ethnohistorical research was hampered in those days because of the difficulty in bringing forth the documents, and anthropologists working among the living cultures in the West preferred the direct recording techniques. Nevertheless, as the criticism of the diffusionistic historical methods towered even Wissler himself turned to direct history and started to analyze documentary sources re the Plains Indians<sup>110</sup>. Other anthropologists followed suit, notably Vernon Kinietz whose main concern has been to publish documents on the Iroquois and Algonkian Indians.

Ethnohistorical research became an established approach when, after the war, the National Archives were organized and the new journal "Ethnohistory" appeared<sup>111</sup>. As Lurie has noted, some of the modern interest in ethnohistory relates to the fact that there are more data to work with — and, we may add, more easily accessible data<sup>112</sup>. New ethnohistoric techniques were applied. Fenton adapted the direct historical approach to ethnological and documentary materials and called his method "upstreaming", a term which had been used by Sir Flinders Petri<sup>113</sup>. Fenton associates his method with the techniques applied by Speck when the latter "aroused the dim memories of boyhood experience in the minds of old Indian men"<sup>114</sup>. He describes his method as follows: "In essence, the method utilizes patterns of culture existing in the living culture for reinterpreting the earlier sources and proceeds by linking these to earlier patterns in a direct sequence, but against the tide of history, going from the known present to the unknown past"<sup>115</sup>. As Fenton points out, his method rests on the premise that major cultural patterns are stable over long periods of time<sup>116</sup>. This method was first used in 1941 and first formulated in 1949<sup>117</sup>.

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109. Cf. A.L. Kroeber, *The Work of John R. Swanton* (*Smithsonian Miscellaneous Collections*, vol. 100. Washington, 1940), pp. 1 ff.; Fenton, *The Training of Historical Ethnologists*, p. 334; Eggan, *Some Anthropological Approaches*, p. 6.

110. Fenton, *Ethnohistory and Its Problems*, p. 7.

111. Perhaps it should also be mentioned that "applied anthropology" has promoted the growth of ethnohistory. The Indians Claims Commissions entrusted anthropologists to work out maps of tribal boundaries and reconstruct Indian holdings of territory on the basis of written documents.

112. N.O. Lurie, *Ethnohistory: An Ethnological Point of View* [*Ethnohistory*, vol. 8 (1961)], p. 81.

113. Fenton, *Ethnohistory and Its Problems*, p. 12.

114. Fenton, *The Training of Historical Ethnologists*, p. 334.

115. Fenton, *Ethnohistory and Its Problems*, p. 12.

116. Fenton, *The Training of Historical Ethnologists*, pp. 334 f.

117. Fenton, *Ethnohistory and Its Problems*, pp. 12, 21; Fenton, *Collecting Materials for a Political History of the Six Nations* [*Proceedings of the American Philosophical Society* (Philadelphia), vol. 93 (1949)], p. 236.

There are of course many other methods involved in the ethnohistorical research<sup>118</sup>. Although most ethnohistorians are ethnologists, some of them have been trained as historians — and, as Radin once told me, what is ethnohistory if not history? Some ethnohistorical works are already classical, for instance, Ewers' study of the Blackfeet Indians<sup>119</sup>, Spicer's analysis of Southwestern Indian acculturation<sup>120</sup> and Murdock's reconstruction of African culture history<sup>121</sup>. We are still waiting for ethnohistorical monographs on American folk cultures. For obvious reasons they should be easier to write than the works on American Indians. The latter are restricted to a short time-span, as we noted above; in the Southwest, the oldest part of the United States in the documents, history began with the arrival of the Spanish explorers<sup>122</sup>, but it is a fragmentary and not quite clear history. Furthermore, the documentary history of the American Indians is just a history of cultural contact<sup>123</sup>. The only documents North of Mexico were those of the white conquerors.

These circumstances motivate a certain caution in our being too optimistic about documentary ethnohistorical research on the former "primitives". This kind of historical reconstruction will, in the long run, need the assistance of archaeology, linguistics and trait-distribution analyses. Documentary research on American folk cultures promises to be more yielding. On both scores we may agree with Fenton when he, quoting Sapir, expresses his conviction that "cultural anthropology in America has not yet realized its potentialities as a strictly historical science"<sup>124</sup>.

118. See in this connection Ph. Dark, *Methods of Synthesis in Ethnohistory* [*Ethnohistory*, vol. 4 (1957), pp. 231 ff.].

119. J. C. Ewers, *The Blackfeet*. Norman, 1958.

120. E.H. Spicer, *Cycles of Conquest: The Impact of Spain, Mexico, and the United States on the Indians of the Southwest, 1533-1960*. Tucson, 1962.

121. G.P. Murdock, *Africa: Its Peoples and Their Culture History*. New York, 1959.

122. Cf. Ch. H. Lange, *Plains-Southwestern Inter-Cultural Relations during the Historic Period* [*Ethnohistory*, vol. 4 (1957)], p. 166.

123. S. Pargellis, *The Problem of American Indian History* [*Ethnohistory*, vol. 4 (1957)], p. 115; Fenton, *The Training of Historical Ethnologists*, p. 336.

124. Fenton, *The Training of Historical Ethnologists*, p. 328.