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EVOLUTION IN CULTURAL ANTHROPOLOGY:
A REPLY TO LESLIE WHITE

By ROBERT H. LOWIE

LESLIE White's last three articles in the *American Anthropologist*¹ require a reply since in my opinion they obscure vital issues. Grave matters, he clamors, are at stake. Obscurantists are plotting to defame Lewis H. Morgan and to undermine the theory of evolution.

Professor White should relax. There are no underground machinations. Evolution as a scientific doctrine—not as a farrago of immature metaphysical notions—is secure. Morgan's place in the history of anthropology will turn out to be what he deserves, for, as Dr. Johnson said, no man is ever written down except by himself. These articles by White raise important questions. As a victim of his polemical shafts I should like to clarify the issues involved. I premise that I am peculiarly fitted to enter sympathetically into my critic's frame of mind, for at one time I was as devoted to Ernst Haeckel as White is to Morgan. Haeckel had solved the riddles of the universe for me.

ESTIMATES OF MORGAN

Considering the fate of many scientific men at the hands of their critics, it does not appear that Morgan has fared so badly. Americans bestowed on him the highest honors during his lifetime, eminent European scholars held him in esteem. Subsequently, as happens with most celebrities—Aristotle, Darwin, George Eliot, for example—the pendulum swung in the opposite direction. The reaction overshot its mark at times, as when Americanists doubted even Morgan's Crow findings. Nevertheless, appreciation has been frequent and ample even in later periods. Haddon calls Morgan "the greatest sociologist of the past century"; Rivers hails him as *the* discoverer of the classificatory system; Radcliffe-Brown rates *Systems of Consanguinity and Affinity* a monument of scholarly, patient research"; Mitra pictures Morgan as an anthropological colossus, a greater Tylor; Marcel Mauss and Paul Radin are avowed admirers.² What, precisely, does White expect? An academic muezzin at every center of learning who shall lead anthropologists in daily Rochester-ward obeisances and genuflections?

MORGAN AND "BOASIANS"

The term "Boasian" is misleading. Of the great physiologist Johannes Müller a one-time disciple said: "There is no school in the sense of common dogmas, for he taught none, only a common method." This holds for Boas.

¹ White, 1943, 1944, 1945.

² Haddon, 1934, p. 127. Rivers, 1914, p. 4 f. Radcliffe-Brown, 1941, p. 1. Mitra, pp. 109–120.

His students have often differed from their teacher and from one another. Kroeber, Sapir, Radin have repeatedly expressed their dissent from cardinal "Boasian" views, and even I have uttered misgivings on certain points. Laufer incidentally, was not trained by Boas at all and was doubtless more deeply influenced by Eduard Hahn.

Speaking *pro domo*, I find White's procedure curious. He virtually accuses me of plagiarizing Morgan on the subject of animal domestication³ when I merely state matters of long-established common knowledge. Morgan's reference, incidentally, is so casual as hardly to merit notice, and Francis Galton's full discussion of the point, which I have duly registered, is much earlier. Again, in White's latest article I am referred to fifteen times, but only three of the publications cited appeared after 1922, though I have twice dealt with Morgan rather fully in much later years.⁴ In these recent discussions, as in my *Primitive Society* long ago, I explicitly mention Morgan's use of diffusion,⁵ yet White finds it "difficult to see how Lowie could have read the passages in Morgan" concerned with that principle.

I do not, however, impugn White's good faith; the obsessive power of fanaticism unconsciously warps one's vision.

As a matter of fact, my conscience is clear on the subject of Morgan. In 1912, in the face of Americanist skepticism, I substantiated his discovery of matrilineal clans among the Crow. In 1916 I referred to his "superb pioneer achievements"; in 1917 I took pains to show that he was right and I wrong on an important point in Crow kinship nomenclature. In 1920 I called *Ancient Society* "an important pioneer effort by a man of estimable intelligence and exemplary industry"; in 1936 I commended his acuity as a field worker and credited his Systems with "a magnificent and valid conception."⁶

To be sure, eulogistic comments are balanced by harshly critical ones. As my treatment of Boas indicates,⁷ I am not an idolater. In proof of my malevolence White likes to quote a sentence dating back to 1920: "It may be said categorically that even at his worst Morgan never perpetrated more palpable nonsense, and that is saying a good deal."⁸ *More suo*, White fails to explain what evoked such violence. Since in 1877 there were already trustworthy reports on African Negroes and Polynesians, I regarded Morgan's denial of monarchic and aristocratic institutions among primitive peoples as inexcusable. Nowadays I should use more temperate phraseology, but as to the substance of my remark I remain adamant. Indeed, by way of amendment, I should say

³ White, 1943, p. 339.

⁴ Lowie, 1936, 1937.

⁵ Lowie, 1937, p. 59; 1920, p. 147.

⁶ *Id.*, 1912, p. 186; 1916, p. 293; 1917, p. 56; 1920, p. V; 1936, pp. 170, 180; 1937, p. 92.

⁷ *Id.*, 1937, pp. 151-155; 1943.

⁸ *Id.*, 1920, p. 389.

that Morgan is guilty of still less defensible propositions. How could any ethnographer ever put the Polynesians into the same category with Australians and *below* the Northern Athabaskans? Again, Morgan finds that "the discrepancies between them [Seneca and Dravidian kinship systems] are actually less . . . than between the Seneca and the Cayuga." This does not deter him from inferring a *racial* affinity between Dravidians and Seneca because of the identity of their kinship terminologies.⁹ The Seneca, by implication then, must be racially closer to the Tamil than to their fellow-Iroquois. Is this line of argument to be rated brilliant, profound, sensible, dubious, or is it palpable nonsense? How does Professor White grade it?

Professor White may say that we ought to judge a scholar by his positive contributions, and I heartily concur. Yet fairness to other scholars viewed in historical perspective demands that we should not gloss over such flagrant delinquencies, especially when their perpetrator regards them as cardinal discoveries. I should like to see some realization on White's part that sporadic impatience with Morgan may have an objective basis.

Certainly irritation at him is not necessarily bound up with anti-evolutionism. How, otherwise, does White explain the generally tender treatment of Tylor? I herewith offer some purely personal remarks to explain why, notwithstanding my appreciation of certain aspects of Morgan's work, I cannot take kindly to him as a scientific personality. For one thing, I resent his dreary schematism; for another, I find little evidence in him for that sympathetic projection into alien mentality which anthropology is supposed to foster. He avoids one of the major departments of culture because "all primitive religions are grotesque and to some extent unintelligible."¹⁰ He dogmatizes thus on the subjective feelings of our indigenes: "The passion of *love* was unknown amongst the North American aborigines of pure blood. The fact is sufficiently established by their marriage customs. They were given in marriage without being consulted, and often to entire strangers."¹¹ Were, however, any doubt possible as to Morgan's narrow-mindedness, it is dispelled by White's admirable edition of his hero's travel notes. A few gems suffice for demonstration: The frescoes of Michael Angelo are "substantially absurd," the Sistine Chapel is "a poor specimen of a Pagan Temple." The Roman Carnival is "an unutterable piece of nonsense and levity," proving the frivolity of the population. Catholic ceremonial evokes "inexpressible disgust". The people of southern Italy "are utterly worthless," Italians in general "degraded beyond all other peoples called civilized." On the other hand we learn: "Our country is the favored and the blessed land. Our institutions are unrivalled and our people the most advanced in intelligence. . . ." ¹²

⁹ Morgan, 1871, pp. 166, 508.

¹⁰ Morgan, 1877 (Kerr ed.), p. 5. ¹¹ *Id.*, 1871, p. 491.

¹² White (ed.), 1937, pp. 285, 290, 303, 311, 315, 327.

Taylor, I feel, could not have written in this strain. Probably White does not acquiesce in his idol's Bilboesque sentiments. If this is a correct surmise, why does he dissent from Radcliffe-Brown's apt characterization of Morgan's provincialism?¹³ Why does he ascribe criticism of Morgan to anti-evolutionism or other sinister motives when such obvious reasons are at hand? Evolution has very little to do with the case.

ANTI-EVOLUTIONISM

As there is no Boasian sect, so there is no Boasian "reactionary philosophy of anti-evolution", nor a "philosophy of planless hodge-podge-ism."¹⁴ The former phrase naturally suggests the degeneration theories which Tylor refuted in *Primitive Culture*. Contrary to what might be regarded as the implications of White's phrases, Boas and his disciples nowhere question the established facts of prehistory (nor does anyone else), witness his inclusion of Nelson's section on "technological evolution" in *General Anthropology*.¹⁵

It is, indeed, not easy to discover the meaning of White's accusations. On the one hand, he tries to clear Morgan of *unilinear* evolutionism, which is the butt of Boas's strictures. If he were correct (see below), it would merely prove that Boas misunderstood Morgan, not that he had an anti-evolutionary philosophy. On the other hand, White summarizes evolutionary doctrine in his "Energy and the Evolution of Culture," laying down propositions which Boasians may find trite and futile, but which do not arouse the Bryanesque ardor imputed to them when evolution is propounded. Boasians do not deny that man requires food, controls his environment with the aid of tools, improves his control by invention and discovery, and alters social structure as a result of technological evolution. I refer White to a paragraph of Boas's in his general text.¹⁶

As a matter of fact, no reputable scholar challenges either the demonstrable findings of prehistory or the economic truisms proclaimed by White, least of all, the Austrian school whose writings are evidently on his *Index librorum prohibitorum* since he contents himself with a garbled sentence borrowed from Kluckhohn concerning their views. It is, however, patent that Fathers Schmidt and Koppers are not anti-evolutionists in White's esoteric sense. They do not, to be sure, like the word "Evolution," but the reality they fully recognize. As Morgan generally speaks about "development," so they have plenty to say concerning "*Entwicklung, Fortentwicklungen, Weiterbildungen*;" and when Father Schmidt somewhere traces *Stufen der ganzen Entwicklung*, what are these

¹³ *Id.*, 1944, pp. 218 f., 230.

¹⁴ *Id.*, 1943, p. 355; 1945, p. 354.

¹⁵ Boas, ed., 1938, p. 150 *et seq.*

¹⁶ White, 1943, p. 354. Boas, 1938, p. 678 f.

but evolutionary stages? Such section headings as "*Der Schritt vom niederen zum höheren Jägertum*" or "*Von der Jagd zur Tierzucht*" indicate that an evolution of some sort is definitely assumed within the several Kulturkreise. It is *unilinear* evolution that the Austrians and the Boasians reject, but since White has latterly discovered that Tylor and Morgan are not unilinear evolutionists at all¹⁷—a matter to be discussed below—what is the row about from White's point of view? It would seem that then "he is right and we are right, and all's as well as well can be." If he deigned to read the Catholic scholars he so airily dismisses, he would discover that in the field of empirical inquiry (as opposed to metaphysics) they are as technologically oriented as himself. Stressing economic conditions as *von ganz hervorragender Bedeutung*, they explicitly accept historical materialism as an excellent and even indispensable heuristic principle.¹⁸

Thus White's gloomy picture of most contemporary anthropologists plunged into Cimmerian darkness, unrelieved by a single lambent ray of evolution, is preposterous. He ought to realize that Thurnwald, Radcliffe-Brown, Radin, Lesser, Malinowski are professed evolutionists, and that even I have spoken kindly enough of neo-evolutionism.¹⁹

The questions which worry White, viz., "why Boas and his disciples have been anti-evolutionists" and what may be "the source and basis of the anti-evolutionist philosophy of the Boas group," automatically disappear. In order to infuse sense into such queries they must be re-formulated: Why have Boas and his students attacked not evolution, but Morgan's and other writers' evolutionary schemes?

Characteristically White does not attempt to answer the question genetically. He makes a great to-do about Morgan's never alleging the priority of animal husbandry to agriculture (as if anyone had made the charge); he triumphantly points to Morgan's placing pictography before the alphabet (a matter not in dispute). But he preserves a discreet silence on virtually all matters that are relevant to the debate.

Boas began as a unilinear evolutionist. In 1888 he defended "the current view of a necessary precedence of matrilineal forms of family organization."²⁰ I also recall his telling me how deeply he was impressed by a first reading of Tylor's "adhesion" study. All problems then appeared solved, at least in principle. What was it, then, that made him alter his convictions? That is a worth-while psychological problem.

Before directly answering this question it is well to digress and take up certain startling discoveries announced in White's second article, for they explain in part why he does not understand what the discussion is about.

¹⁷ White, 1945, p. 347.

¹⁸ Schmidt and Koppers, pp. 382, 396 *et seq.*, 625 *et seq.*, 636.

¹⁹ Lowie, 1937, pp. 246, 289. Malinowski, p. 17. Thurnwald, 1931, 1: p. 16.

²⁰ Boas, 1940, p. 635.

NOVEL DISCOVERIES IN THE HISTORY OF SCIENCE

Between 1859 and 1881, we learn, thinking people were divided into two hostile camps: they championed either science or theology. A devout Christian could not be a scientist or a Darwinian. "*Those who opposed Darwinism did not labor for, or make contributions to, science. . . . If you were for Theology, you were against Science.*"²¹

This statement happens to be wrong in every particular and from every conceivable angle. Darwinism and science never have been interchangeable terms. Pious Catholics, witness Pasteur and Mendel, made epoch-making researches during Morgan's lifetime; Clerk Maxwell and Kelvin were, I believe, devout Protestants; Julius Robert Mayer was beyond any doubt a deeply religious man—and an opponent of Darwinism.²² The critics of Darwinism included towering figures in the history of science—Karl Ernst von Baer, Louis Agassiz, Rudolf Virchow, Albert Kölliker, Sir Richard Owen. Some of them were religious, others mere skeptics. On the other hand, Christian believers by no means uniformly rejected evolution; they included the geologist Joseph Le Conte and the botanist Asa Gray, whom Darwin held in high esteem.

The Manichaeic picture of Darwinian atheists as angels of light pitted against a Satanic brood of Christian obscurantists is merely more of Whitean melodrama. As might be expected, so revolutionary a doctrine as Darwinism evoked a variety of responses. Darwin's, Wallace's, Huxley's correspondence and Haeckel's polemical writings furnish ample illustrations. True scientists were thrilled over having so many obscurities illuminated for the first time. This group, as noted, comprised Christians, who promptly set about to harmonize their religion with their scientific convictions, revising what seemed unessential articles of faith. Materialists and other radicals just as naturally used the new ideas as grist for their mill, yet not all of them did so indiscriminately. David Friedrich Strauss, e.g., hailed Darwinism as a great achievement, yet found it still "highly imperfect". The inadequacies felt even by so sympathetic a scholar were naturally aggrandized and stressed by fundamentalists. Thus, there was by no means a clear-cut division into two hostile camps.

For present purposes we are interested in the *scientific* opposition. Why, we ask, did a man like Virchow maintain so reserved and at times hostile an attitude towards Darwinism? Why, half a century later, did experimentalists like Thomas Hunt Morgan remain critical? Whence the skepticism of Jacques Loeb,²³ whom even White can hardly brand as a fundamentalist? Were all these men reactionary philosophers, enemies of Science? The answer is clear. They objected to evolutionary theories on the ground that they were not

²¹ White, 1944, p. 219. White's italics.

²² Ostwald, pp. 73, 82.

²³ Loeb, in H. Schmidt, ed., vol. 2, p. 15.

scientific enough. Some of them cannot, nevertheless, qualify as "anti-evolutionists", for they gratefully recognized the widening of the intellectual horizon due to Darwin. But they rejected phylogenetic speculation, for which they sought to substitute the demonstrable findings of the laboratory.

DIFFUSION AND EVOLUTION

Reverting to Boas, his critique of evolutionary schemes is the psychological equivalent of the experimentalists' critique of "the biogenetic law." The facts did not fit the theory, hence the theory would have to be modified or discarded. To cite concrete instances, L. H. Morgan teaches that the individual family is an end-product, preceded by various stages including that of a clan organization; Morgan, Bachofen, and Tylor teach the priority of matrilineal descent. Boas found that in the interior of British Columbia clanless tribes with a family organization and a patrilineal trend adopted from coastal neighbors a matrilineal clan organization.²⁴ Diffusion thus disproved the universal validity of the formula that Boas himself had been defending in 1888.

Subsequently it turned out that borrowing had played a far greater part among primitive groups than most anthropologists had supposed. The difficulty of establishing universal laws of sequence seemed correspondingly increased. In this sense I came to maintain that diffusion laid the axe to the root of any theory of historical laws.

Professor White in dissent blares forth a sennet of defiance: (1) Tylor and Morgan both accept diffusion in concrete cases, hence Diffusion and Evolution lie together as the leopard and the kid in Isaiah's peaceful kingdom. (2) Tylor "does not state, nor do his remarks imply or even allow of the intimation 'that every people must pass through all the stages of development,' as Boas claims." Similarly does Morgan nowhere "declare or even imply that each tribe, everywhere, must go through the same stages of cultural development."²⁵ (3) The Boasians fail to discriminate between cultural evolution and the culture history of specific tribes or peoples; Tylor and Morgan are never concerned with the history of tribes or peoples, only with that of cultural traits or complexes, such as writing, metallurgy, social organization. Hence the criticism that their formulae do not fit particular tribes is irrelevant, they were never meant to do so.

Let us scrutinize these allegations. That Tylor and Morgan knew about diffusion is, indeed, a patent fact, which I have taught for over twenty-five years, as White admits with reference to Tylor.²⁶ The point is irrelevant, for it is failure to integrate diffusion with evolution that is charged. To repeat a twice-told tale, Tylor offers the formula: (1) patrilocal residence; (2) taboo

²⁴ Boas, 1909, p. 16. Swanton, p. 173.

²⁵ White, 1945, p. 347.

²⁶ Lowie, 1920, p. 147; *id.*, 1936, p. 173.

between wife and husband's kin. If the correlation in a dozen cases evolved independently, there is presumably an organic tie-up. But what if there has been a single historic center of origin for the trait couple? As a transcendent entity there might still be a law of sequence, but there would be no way of demonstrating it. Similarly with Morgan. If clans arise independently the world over out of similar antecedents, the clan may reasonably be put into a definite place in a chronological series. Not so if, as Morgan argues, it sprang up a single time and was thus diffused over the globe. Metaphysically, here also the sequence postulated is conceivable; empirically, however, it ceases to be demonstrable. To amend White's phraseology, "diffusion negates [the possibility of proving] evolution."

The criticism is that Tylor and Morgan fail to resolve this logical difficulty, not that they ascribed an independent development of Christianity to the Seneca or believed in an independent origin of maize-growing in the Balkans.

Contrary to White's allegations, moreover, Tylor and Morgan much more than *imply* a faith in parallelism. Both strongly believed in psychic unity. In accepting this principle Morgan declares: "It was in virtue of this that mankind were able to produce in similar conditions the same implements and utensils, the same inventions, and to develop similar institutions from the same original germs of thought." And in one of his most famous publications Tylor, having likened human institutions to stratified rocks, thus continues: "They succeed each other in series substantially uniform over the globe independent of what seem the comparatively superficial differences of race and language, but shaped by similar human nature. . . ." ²⁷ Now, this last statement, to be sure, says nothing about specific peoples. The important thing, however, is not how a writer formulates his principles in the abstract but what he does with them.

Now, White to the contrary notwithstanding, Tylor and Morgan both apply their magical formulae to particular peoples. Tylor correlates cross-cousin marriage with exogamy. His schedules listed 21 peoples as practising cross-cousin marriage, only 15 of whom were described as exogamous. Sure of the general validity of the formula, he boldly places the remaining six tribes in the exogamous category.

Morgan finds Ponca, Winnebago, Ojibwa and Menomini with patrilineal institutions, but from his formula he infers that they were once matrilineal. Similarly with the Greeks, despite "the absence of direct proof of ancient descent in the female line." Correspondingly, Greeks, Romans, Celts, Germans and Hebrews are credited with a pristine punaluan stage. ²⁸

I suggest that this is no longer dealing with the cultural process, it is verit-

²⁷ Morgan, 1877 (Kerr ed.), p. 562. Tylor, 1889.

²⁸ Tylor, *ibid.* Morgan, 1877, pp. 48 f., 59, 159, 161, 170, 175, 357, 388 f., 438.

ably "pseudo-history". Morgan is speaking of the past of Siouan and Algonkian *tribes*, and he defines it not on anything known about Siouan and Algonkian history, but as a deduction from his formula. And when a punaluan stage is ascribed to the remote ancestors of Greeks and Hebrews, it can be only on the assumption that these *peoples* have passed through the same stages as other peoples. Morgan does not ask in how far unique happenings in the past—say, alien influences—might have deflected these peoples from their predestined path.

Incidentally, White nowhere explains how he supposes the formulae to have been ultimately derived. Are they empirical inductions? In that case they must rest on observations of the history of specific tribes. Or are they all *a priori* constructs like the precious notion about the uncertainty of fatherhood as the cause of matrilineal reckoning in early times?

CONCLUSION

Leslie White misunderstands the status of the problem. It is false that any reputable anthropologist nowadays professes an anti-evolutionist philosophy in the sense alleged. The "anti-evolutionism" of the Boasians and of the Kulturkreisler has nothing to do with, say, the degeneration theory of de Maistre. It implies bad faith or bigotry to suppose that either Boas or Schmidt denies the findings of prehistory concerning the sequence of stone and metal tools, of simpler and of more complex economic systems.

But the Boasians do claim the right to check evolutionary generalizations by the facts they are meant to explain, precisely as the experimental zoologists checked the recapitulation theory. A dogmatist naturally cannot understand that true scientists are not interested in proving a preconceived system. There are such. Psychologically viewed, Boas's attitude is simply that of Virchow towards Haeckel's phylogenetic hypotheses, that of Loeb in preferring demonstrable truths to fictitious genealogical trees. It is of a piece with Newton's "*Hypotheses non fingo*"; with Mach's aim to purge science of metaphysics; with Virchow's, Ostwald's, Mach's prescription to observe without preconceptions.

In Virchow's valedictory speech at Würzburg in 1856, so young Haeckel reported to his parents, the great pathologist explained to the students that his whole life's aim was devoted to discovering the unvarnished truth, to recognize it free from bias and to disseminate it unaltered. He exhorted them to get rid of all prejudices, "with which we are unfortunately crammed full from infancy on" (*mit denen wir leider von Kind auf an so vollgepfropft werden*) and to view things as they really are ("*die Dinge so einfach und natürlich anzusehen, wie sie sind*").²⁹ Some years previously, in one of his most famous papers, Virchow

²⁹ Haeckel, p. 200 f.

had already warned against the dangers that lurk in any system:

“Dann kommt jeden Augenblick der Konflikt zwischen dem System und dem einzelnen Fall, und gewöhnlich wird der einzelne Fall dem System geopfert.”⁸⁰

Boas, too, refuses to sacrifice individual observations to a preconceived scheme, *voilà tout*.

In conclusion I reiterate that I am altogether convinced of White's good faith. But as Voltaire explains:

“La chose la plus rare est de joindre la raison avec l'enthousiasme: la raison consiste à voir toujours les choses comme elles sont. Celui qui dans l'ivresse voit les objets doubles est alors privé de la raison.”

POSTSCRIPT

I have just encountered inexplicably forgotten passages in Father Schmidt's *Handbuch der Methode der kulturhistorischen Ethnologie* (Münster, 1937), which demonstrate beyond a doubt that he accepts not only the concept, but even the term “evolution”:

“Nun, Professor Lowie und andere werden aus meiner Darstellung der Methode ersehen, dass ich nicht nur das Wort, sondern auch den Begriff und die Tatsache der Evolution nicht vermeide, sondern, mit der ganzen kulturhistorischen Schule, frank und frei mich zur *Evolution* bekenne, aber nach wie vor in der Ablehnung des *Evolutionismus* verharre.” (p. vii)

“ . . . wer den Evolutionismus bekämpft und ablehnt, bekämpft und verwirft damit nicht die Evolution, die (innere) Entwicklung.” (p. 10)

The case against White is thus even stronger than previously indicated.

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BIBLIOGRAPHY

BOAS, FRANZ

- 1909 Die Resultate der Jesup-Expedition (Separat-Abdruck aus den *Verhandlungen des XVI Internationalen Amerikanisten-Kongresses*). Wien.
- 1938 *General Anthropology*. New York.
- 1940 *Race, Language and Culture*. New York.

HADDON, ALFRED C.

- 1934 *History of Anthropology*. London.

HAECKEL, ERNST

- 1921 *Entwicklungsgeschichte einer Jugend*. Leipzig.

LOWIE, ROBERT H.

- 1912 Social Life of the Crow Indians (*Anthropological Papers of the American Museum of Natural History*, vol. IX, pp. 179-248).
- 1916 Historical and Sociological Interpretations of Kinship Terminologies. (*Holmes Anniversary Volume*, pp. 293-300). Washington.
- 1917 Notes on the Social Organization and Customs of the Mandan, Hidatsa, and Crow

⁸⁰ Virchow, 1849, p. 18.

- Indians (*Anthropological Papers of the American Museum of Natural History*, vol. XXI, pp. 1-99).
- 1920 *Primitive Society*. New York.
- 1936 Lewis H. Morgan in Historical Perspective (*Essays in Anthropology in Honor of Alfred Louis Kroeber*). Berkeley.
- 1937 *The History of Ethnological Theory*. New York.
- 1943 Franz Boas, his Predecessors and his Contemporaries (*Science*, vol. 97, pp. 202-203).
- MALINOWSKI, BRONISLAW
1944 *A Scientific Theory of Culture and other Essays*. Chapel Hill.
- MITRA, PANCHANAN
1933 *A History of American Anthropology*. Calcutta.
- MORGAN, LEWIS H.
1871 *Systems of Consanguinity and Affinity of the Human Family*. Washington.
1877 *Ancient Society*. New York. (Kerr edition, Chicago.)
- OSTWALD, WILHELM
1910 *Grosse Männer*. Leipzig.
- RADCLIFFE-BROWN, A. R.
1941 The Study of Kinship Systems (*The Journal of the Royal Anthropological Institute of Great Britain and Ireland*, vol. LXXI).
- RIVERS, W. H. R.
1914 *Kinship and Social Organisation*. London.
- SCHMIDT, HEINRICH (ed.)
1914 *Was Wir Ernst Haeckel verdanken*. 2 vols. Leipzig.
- SCHMIDT, W., AND W. KOPPERS,
1924 *Völker und Kulturen*. Regensburg.
- SWANTON, JOHN R.
1906 A Reconstruction of the Theory of Social Organization (*Boas Anniversary Volume*, pp. 166-178). New York.
- THURNWALD, RICHARD
1931-1935 *Die menschliche Gesellschaft*. Berlin & Leipzig. 5 vols.
- TYLOR, E. B.
1889 On A Method of Investigating the Development of Institutions; applied to Laws of Marriage and Descent (*Journal of the Royal Anthropological Institute*, vol. 18, pp. 245-269).
- VIRCHOW, RUDOLF
1849 *Die Einheitsbestrebungen in der wissenschaftlichen Medicin*. Berlin.
- WHITE, LESLIE A.
1937 (ed.) Extracts of Lewis Henry Morgan's European Travel Journal (*Rochester Historical Society Publications*, vol. XVI). Rochester, New York.
1943 Energy and the Evolution of Culture (*American Anthropologist*, 45: 335-356).
1944 Morgan's Attitude toward Religion and Science (*ibid.*, 56: 218-230).
1945 "Diffusion vs. Evolution": an Anti-Evolutionist Fallacy (*ibid.*, 339-356).