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Lowie, Robert H.

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*Reminiscences of Anthropological Currents in
America Half a Century Ago*

ROBERT H. LOWIE
University of California

THE Editor of the *AMERICAN ANTHROPOLOGIST* has asked me to offer "some discussion and analysis of the intellectual ferment, the various ideas and interests, and the important factual discoveries in their relationship to these ideas, that were current during the period of your early years as an anthropologist." In responding I shall have to go far afield. The task suggested implies nevertheless two noteworthy restrictions. Factual discoveries are irrelevant (except as they influenced ideas), as is administrative promotion of scientific interests. Accordingly, though sharing Sapir's judgment that as a field worker J. O. Dorsey was "ahead of his age," I must ignore him for present purposes. Again, there will be only brief references to Frederic Ward Putnam (1839-1915) and to Frederic Webb Hodge (1864-1956); as to Powell and McGee, only their thinking demands extended notice.

It is well to recall that in 1904, when I began graduate work, only Columbia, Harvard, and California had full-fledged academic departments of anthropology, but the Field Museum, a descendant of the Chicago World's Fair of 1893, had been fostering research, as had the Bureau of American Ethnology and the United States National Museum. The anthropological departments of Columbia and of the American Museum of Natural History were still intimately connected; even closer was the bond between the Museum of Anthropology in San Francisco and the department in Berkeley, both of them being parts of the University of California. Thus, New York, Washington, Chicago, and San Francisco-Berkeley were the chief centers of anthropological activity.

If the following pages seem to give disproportionate mention of my own university, this is not due to parochialism on my part. In 1904 Columbia indisputably provided the most comprehensive training to be obtained in the country. My first seminar there was attended by Alfred M. Tozzer, already a Ph.D. from Harvard. When John R. Swanton had presented a linguistic dissertation at Harvard, the Columbia professor was invited to examine him. Before George A. Dorsey sent Fay-Cooper Cole to the Philippines, he had him spend a semester with Boas, and after his return from the field Cole returned to take his degree in New York.

In the present essay I shall begin by sketching the orientation of men whose thinking developed independently of this particular academic tradition. I shall then attempt to indicate the intellectual movements that impinged on my generation and presumably in large measure on our teachers'. It is hardly necessary to emphasize that even among the small number of prospective professionals at the time the reactions to these impulses varied considerably, in accordance with our greatly varying individualities and equally diverse backgrounds.

I

Whatever may be said in criticism of the scholars to be treated in this section, they numbered among them men of unquestionable talent and enthusiasm. As will be shown, some were unusual personalities, some achieved important scientific results. It is my considered opinion that the less impressive individualities among them did the most useful and most lasting work.

Cushing (1857–1900). During one of my seminars the name of Frank Hamilton Cushing happened to come up. "He was an exceedingly able man," Boas declared. Then he paused. After a brief intermission he resumed: "I'm afraid his work will have to be done all over again."

To a novice the judgment seemed a curious *non sequitur*. Had I known Cushing's writings, I could have filled in the ellipse. Cushing *was* exceedingly able: with rare manual skill he could duplicate aboriginal artifacts; and with rare perceptiveness he recorded elusive Indian usages. But his was an undisciplined imagination; he was able to impart the flavor of the Pueblo atmosphere, but he leaves us wondering how much of his interpretation reflects his own rather than his native hosts' mentality (Cushing [1884–85] 1920). A sober inquirer of later date found his versions of Zufi myth highly suspect; for the most part "the endless poetic and metaphysic glossing of the basic elements" probably "originated in Cushing's own mind" (Bunzel 1932:547 f.).

The one general principle of interpretation Cushing used was evolution, linked with the doctrine of psychic unity. Culture is due "chiefly to the necessities encountered during its development." Nothing seemed more natural than that the ancestral Pueblo entering the Southwest first used gourds and baskets, then by their unaided efforts achieved pottery. Cushing expresses his indebtedness to E. B. Tylor, yet a vital phase of the British anthropologist's thinking eluded him. For Tylor expressly notes the continuous distribution of ceramics in North America from Mexico northward, inferring that the art "spread from a single source" (Tylor 1865:366). Indeed, Cushing conceived the whole of Pueblo culture as a spontaneous local growth: people driven into the Southwest at first subsisted on roots and seeds until they were "spurred on by that great motor of humanity—hunger—to a knowledge of irrigation and horticulture" (Cushing 1920:516 f.). Correspondingly, they began by constructing brush lodges, but "by a series of stages" advanced "to the recent and present terraced, many storied, ceremonial structures" (Cushing 1886:473–481).

It is not surprising that at the World's Fair in Chicago Cushing argued against any evidence for ancient cultural contact between the New World and other continents (Holmes 1893:425 f.)

Brinton (1837–1899). Among the eminent men of his period, Daniel Garrison Brinton was not the least remarkable; and like Cushing he was an uncompromising champion of unilinear evolution. Among his American contemporaries he stands out in several ways. Though, unlike the rest, he did no field work, his reading covered the whole range of our science. Medically trained, he some-

times dealt with physical anthropology, but his greatest effort went into ethnological linguistics, mythology, and comparative religion. He edited an eight-volume *Library of American Aboriginal Literature* (1882–1890). He held a chair of American Archaeology and Linguistics at the University of Pennsylvania. Attending scientific congresses, he became personally known to the leaders of the science abroad; Rudolf Virchow once asked George Grant MacCurdy to convey his regards to Brinton [oral communication of G.G.M. to R.H.L.]. Probably no American-born colleague of his generation was so deeply saturated with the European atmosphere. He had studied at Paris and Heidelberg, and profusely quoted from German, French, and Italian sources. Nor were his interests restricted to scholarship; he haunted European picture galleries, read Browning and Tennyson, admired Ibsen and Zola when their names were still anathema. Altogether he published twenty-three books and innumerable articles—on the Mound Builders, on Anthropopithecus, on the philosophy of language, on Central American guardian spirits, and what not (Smyth et al. 1900).

Of the effectiveness of his teaching I have found no record, but A. B. Lewis once told me that Brinton had been a spirited debater at scientific gatherings, a statement quite credible to a reader of his reviews.

Here, then, was a man of independent mind, unusual erudition, and exceptional cultivation, yet amazingly little profit can be drawn from his writings. Of course, he was not always wrong and he helped dispel some popular fallacies, such as the existence of tribes without religion, the degraded character of African fetishism, or the racial distinctness of the Mound Builders (Brinton 1898:31, 101; 1901:255). But many of the opinions most confidently voiced by him must be read to be believed. Decades after Waitz he quotes old wives' tales about aboriginal mentality: the Australians are marked by "almost brutal stupidity"; their "natural feelings and moral perceptions seem incredibly blunted" (1898:16 f.). There are even incredible ethnographic blunders: the Melanesians, *unlike the Polynesians*, are said to be agriculturists (1901:228, 237).

Confusingly Brinton mingles racial, cultural, and linguistic points of view. One is inclined to praise him for citing types of arrow release as samples of motor habits, but alas! he seems to conceive them as biologically determined since the relevant passage occurs between a comparison of human with simian musculature and a description of steatopygy. The American Indians are characterized by copper color, straight hair, *and* incorporating languages.

Considering that he plumed himself on his linguistic insight, some pertinent thoughts of Brinton's have a curious flavor. Following Horatio Hale, he thus explains the differentiation of stocks: Children are forever coining new words and among themselves soon evolve a distinctive idiom. Barbarians would often leave very young children behind, and "those who survived developed a tongue of their own, nearly all of whose radicals would be totally different from those of the languages of their parents. Thus, in early times . . . numerous independent tongues came to be spoken within limited areas by the same ethnic stock" (Brinton 1901:33, 36 f., 61 f., 63, 65, 74 et seq., 97 f., 237).

The work on *The American Race* exhibits a certain independence of Powell, e.g. in recognizing a Uto-Aztecan family, though it was left for Sapir to provide the demonstration. As a whole, the book is a sad disappointment. Brinton vehemently rejects any affinity between American Indians and the Mongoloids; the former entered the New World by a land-bridge between Europe (or "Eurafrica") and America; they "could have come from no other quarter." Within the Western Hemisphere, parallelism is carried to a ludicrous extreme. Like Cushing, Brinton regards Pueblo culture as "a local product, developed in independent tribes by the natural facilities offered by the locality. . . . The culture of the Pueblos, both ancient and modern, bears every mark of local and independent growth" (1891:17-58, 113-117, 336 f.).

Consistently with this position, Brinton had interpreted the same myth even in neighboring tribes as the result of psychic unity (1868:172 f.). The "universal mythical cycles" were "independent creations of the human intellect, framed under laws common to it everywhere, and which tend always to produce fruits generically everywhere the same" (1898:117 f., 129).

It is a melancholy reflection that Brinton's enthusiasm and learning produced so slight a permanent contribution.

Powell and McGee. John Wesley Powell (1834-1902) and his collaborator William John McGee (1853-1912) are best treated jointly. In a different way from Brinton they were both remarkable men. Notwithstanding the loss of his right arm in the Civil War, Powell intrepidly achieved the descent of the Colorado River (1869, 1871); McGee, while suffering the tortures of cancer, was able to record his subjective experiences for a scientific publication. As scientists, both men were primarily geologists, but became absorbed in anthropology and strove valiantly to make it a *science*, sometimes in a rather naïve way. Characteristic is their emphasis on the biologists' principle of priority in nomenclature. I have heard McGee defend it before a session of the American Anthropological Association as though the matter were of vital importance for the status of our discipline. If a reader of the *Handbook of American Indians North of Mexico* (1907, 1910) who seeks information on the Blackfoot is referred to an article on the "Siksika," it is due to this Powellian crotchet. No less peculiar is the mania of both men for newly coined words. Their writings teem with such terms as "sophiology," "esthetology," "demonomy," "historics." Administratively they have won undying renown, Powell by founding the Bureau of American Ethnology (1879) and its series of publications, McGee both as Powell's collaborator and as the foremost organizer of the American Anthropological Association (Hodge 1912:686). But our concern is with their scientific contributions.

From that point of view we are once more doomed to disillusionment. Neither ranks high as a field investigator. Powell met many Ute and Paiute on his Far Western explorations and claimed a speaking acquaintance with their dialects, but apart from four good versions of myths (Powell 1881) and occasional tidbits, he published nothing of ethnographic value. McGee's most am-

bitious research project was devoted to the Seri, to whom he dedicated a 300-page treatise on the basis of little over a week's observation (McGee 1898). Kroeber, who went over the ground later, gives full credit to his predecessor's extraordinary gifts as an observer. However, McGee's uncontrolled imagination and his strong preconceptions yielded a distorted picture (Kroeber 1931:3, 18).

The intellectual set of the two men is well illustrated by their formulation and solution of a specific problem. After his brief sojourn McGee conceived the Seri as "notably egoistic and inimical toward contemporaries"; thereby they contrasted with the "notably altruistic" Papago, though both tribes share the same type of environment. Other peoples having been examined from the same point of view, the Papago stood "in the front rank of aboriginal tribes as graded by power of nature-conquest," whereas the Seri were at the opposite extreme. What conclusion is drawn from these facts? "The Seri, habitually submitting to a harsh environment . . . merely reflect its harshness in their conduct," while "the Papago, seeking habitually to control environment in the interests of their kind . . . are raised by their efforts to higher planes of humanity" (Powell 1903:XXVIII).

However, as Professor Heizer has pointed out to me, there was another side to McGee that explains the high personal regard in which he was held by such exacting judges as Boas and George A. Dorsey. When a situation fitted into his geological experience he could display exemplary caution. In Pleistocene deposits in Nevada he discovered an undeniable artifact, which a conservative archeologist (Holmes 1919:69 f.) pronounced "the second most important observation yet recorded bearing upon the problems of the high geological antiquity of man in America." But McGee himself refused to draw sensational inferences from a single find (McGee 1889), leaving the matter in abeyance. *Si ita omnia dixisset!*

To return to Powell, his one effective publication in our field is the treatise on North American linguistic stocks (Powell 1891:1-142). Acknowledging his indebtedness to other investigators, such as Gallatin, Henshaw, Pilling, Gatschet, and J. O. Dorsey, he assumed sole responsibility for the classification presented. He accepted as probable the subsequent fusion of some of his stocks, but foresaw no material reduction in the total number (1891:26 f.). Americanists have often chafed at his conservatism, but the scheme has unquestionably helped to bring order into chaos and has probably aided ethnography more than a bolder classification might have done. It is more profitable to seek cultural resemblances between Navaho and Chipewyan than between Ojibwa and Yurok.

As for their philosophy of culture, Powell and McGee fell back on evolution, perpetrating some of the dreariest series of stages ever concocted in its name. To the familiar categories of savagery, barbarism, and civilization, they added enlightenment (Powell 1888). Human thought was said to fall into two major divisions, the mythological and the scientific. Within the former there were four stages: in the beginning men assigned life to everything; next they

anthropomorphized and deified beasts; then they superseded animal gods with the personified and deified natural powers (physitheism); finally they deified mental, moral, social traits, such as war, love, etc. (psychotheism).

Concerning social organization, Powell in no way advanced beyond Lewis H. Morgan. His terminological separation of unilineal descent groups into matrilineal "clans" and "patrilineal" gentes is defensible and gained a following among American scholars. In the classical tradition he assigned clans to savagery, gentes to barbarism. The change in rule of descent had several causes. For one thing, the priestly office was passed on from father to son [Why? one asks], whereby patrilineal reckoning became fundamental. Again, women were separated from their clansfolk when following their husbands to fishing and hunting grounds, by which practice the husbands' and fathers' authority was enhanced to the detriment of maternal, fraternal, and avuncular powers. Agriculture tended to influence developments in the same direction, for women and children would be working "under the immediate supervision and control of husbands and fathers." As is usual with Powell, no concrete examples are given in support of these generalizations (Powell 1896:10 f., 14 f.).

Adolphe Bandelier. To the foregoing galaxy of American "characters" may be added Swiss-born Adolphe Bandelier (1840-1914), an original if ever there was one. As a student in his first class at Columbia I speak from personal acquaintance. His astonishing knowledge of Latin-American sources was coupled with extravagant vehemence, obstinacy, engaging naïveté, and a bizarre sense of humor. My fellow-student Speck he addressed as "Lord Bacon." In the midst of a lecture he once stigmatized a scholar he disliked as "that damned liar, Sir Clements Markham." Seeing me in the hall outside his classroom one afternoon he approached me with an air of mystery, threw an arm around my shoulders, and confidentially asked, "Lowie, can you tell me where the toilet is?" At a social gathering in Boas' house I remember his arriving late, advancing toward his host, and truculently remarking, "*Meine Frau lässt Sie nicht grüssen.*" Then, turning to Mrs. Boas, he said, "*Meine Frau lässt Sie beinahe grüssen.*"

Bandelier did do significant field work in the Eastern Pueblos, in Mexico, and in the Andes. He is especially noted, however, for the effective demolition of the widespread belief in grandiose American empires, though he considerably overshot the mark. As a theorist, he ranks admittedly as a satellite of Lewis H. Morgan (Bandelier 1877, 1878, 1879). With admirable cogency Professor Leslie White has demonstrated how Bandelier, modifying his original conceptions to bring them into harmony with Morgan's scheme, came to represent Aztec social organization in the image of the Iroquois. Mexicans and Peruvians had never advanced beyond a clan system, had retained as the basis of social relationships kinship ties rather than economic or territorial ones (White 1940:11-63).

Some Washingtonians. Paradoxical as it may sound, the most solid contributions came not from the colorful, impressive personalities just treated, but from several unpretentious workers. Who nowadays reads Brinton or Powell or McGee, whether for facts or ideas? But Jesse Walter Fewkes (1850-1930),

Otis T. Mason (1838–1908), and Walter Hough (1859–1935) are still far from negligible in their respective fields of specialization.

Fewkes started as a zoologist, as a one-time student of Louis Agassiz at Harvard, but later turned ethnographer and archeologist. When publishing on protozoa in his early days, he once told me, he found that only half a dozen people in the world would read his papers, so he shifted to anthropology. One cannot help wondering how large a public he acquired by his meticulously thorough, but soporific descriptions of Hopi ceremonial. The fact remains that the specialists have profited from them: Haeberlin cites 32 of Fewkes' papers in his doctoral dissertation, and 15 were used by Elsie Clews Parsons in her work on *Pueblo Indian Religion*. What is more, Fewkes was an undisputed innovator in introducing sound-recording into field work (1889), first among the Passamaquoddy, soon after among the Pueblos; and according to an exacting critic, "few anthropologists since have made such thorough and judicious use of sound-recording equipment" (Rowe 1953:914). He did not shine as a theorist, to be sure, as witness his naïve faith in the historic value of clan migration legends (Fewkes 1900); at all events, one did not have to worry whether Fewkes was substituting the figments of his fancy for aboriginal thinking.

Mason's impress on technological researches is apparent from a glance at the comparative studies of Wissler, Spier, Birket-Smith, and Nordenskiöld. Above all, his *Aboriginal American Basketry* (Mason 1904) has remained a classic, un-supplanted after half a century's investigation. Theoretically, he was indeed capable of dreary evolutionistic patter (Mason 1908), but even concerning interpretation there is something to be said on the credit side. Evolutionist though he might be after the fashion of his period, he by no means shut his eyes to the claims of diffusion. In fact, we owe to him an eminently sound exposition of the logic of the diffusionist problem (Mason 1895a), adducing the detailed similarities between Amur and Columbia River canoes. At Chicago (1893) he was one of those who combated Brinton's intransigent parallelism.

Hough's museum studies, though on a lesser scale, are roughly comparable to Mason's. He, too, broke a lance for diffusion, tracing Northwest American plate armor to Japan (Hough 1895); though Laufer showed that the specific provenience suggested by Hough was untenable, he upheld the broader theory of some Asiatic source and praised Hough's "intensely interesting and valuable study" (Laufer 1914:260 et seq.).

In native capacity William H. Holmes (1846–1933) belongs on a higher level than the three men just considered. This judgment is not due to any personal preference on my part, for whereas Hough and Fewkes were distinctly genial in their attitude to a younger man, I found Holmes stiff and condescending. But Boas—emphatically no friend of Holmes—correctly described him orally as "a very able man." Lacking the flamboyance of Cushing, the imposing personality of Powell, the quaint charm of Bandelier, he was distinctly their superior in sobriety of judgment. An artist by training and endowment, he had the scientific rather than the artistic temperament.

A prehistorian, Holmes claims consideration here only insofar as his in-

quiries bear on cultural theory. This applies especially to his discussion of primitive art. Like the German architect Gottfried Semper, who preceded him by some twenty years but was not concerned with aborigines, Holmes stressed the influence of technique: "Geometric ornament is the offspring of technique" (Holmes 1886c:465). "The more closely the ceramic art of the ancient peoples is studied the more decidedly it appears that it was profoundly influenced by the textile arts, and especially by basketry" (1886a:359). But elsewhere there is complementary attention to the effect of life forms. In the evolutionary era of anthropology it was normal to look for ultimate origins, and Holmes traced the rise of ceramic forms to the imitation of "natural originals," such as gourds, coconut shells, and bladders. Further, "unconscious embellishment" would result if the artificer imitated, say, the spines or ribs of mollusk shells; at a later stage "these features would be retained and copied for the pleasure they afforded" (1886c:446, 454). Though placing the "realistic pictorial stage" later than the appearance of "nonideographic" elements, Holmes did not ignore them and states that their significance tends to be lost, so that they "are subsequently treated as purely decorative elements" (1886c:453-457). How this view can be made to harmonize with the dictum that geometric ornament is the offspring of technique is not easily understood. At all events, Holmes considered various possibilities in the course of his studies. What is more, he must be credited with anticipating, though perhaps not adequately elaborating, certain views associated with Boas. He mentions, in passing, the virtuoso's urge to play with his technique; and he clearly recognizes the tendency to read meaning into primarily nonsignificant designs (1886c:452; 1906:186).

At least one of Holmes' archeological conclusions bears on a basic ethnological issue. He proved decisively that the rude American artifacts suggesting European paleoliths were not to be regarded as their chronological equivalents. They did not indicate a stage of inferior craftsmanship, but individual mis-carriage in the process of manufacturing a tool of superior ("neolithic") grade. Thus, morphological similarity could arise independently by "convergence," though Holmes did not so phrase his inference (Holmes 1919:75, 159 et seq.).

I have heard Holmes described as morbidly cautious, but do not find him so. We have seen that he did not simply brush aside McGee's Pleistocene find; and when confronted with marked coincidences in detailed features he was willing to accept some sort of "ethnic relationships" even if it meant linking an Alaskan wood carving with a clay replica from a grave in the Middle Mississippi valley (1886c:451).

* * *

So far, then, as the interpretation of facts was concerned, the scholars discussed in this section relied on evolution tempered with diffusion, against which latter only Cushing and Brinton took a determined stand. In any case, not one of them approached the stature of Morgan or Tylor, and certainly

none of them advanced beyond these predecessors theoretically. When they turned to generalities, they were likely to fall into empty schematizing. It was when Cushing duplicated aboriginal implements, when Mason analyzed basketry weaves, and Hough demonstrated how rapidly fire could be produced with a simple palm-drill, that they added to our insight, showing what early invention implied and "how much original human thought has been bestowed" on perfecting them (Mason 1895b:229). But estimable as is Mason's essay on parallelism versus transmission, it hardly compares with Tylor's papers on the patolli game and on correlations (Tylor 1878, 1889). Nor does Mason's magnum opus attain the originality and thought value of Morgan's *Systems* (Morgan 1871), notwithstanding its obvious faults. Certainly none but Brinton among the group concerned himself, as Tylor and Morgan did, with most departments of culture on a global scale; and Brinton alas! did not advance any subdivision of anthropology.

II

Diffusion and Evolution, then, constituted the theoretical legacy acquired by the nascent anthropologists about the turn of the century. But these guiding principles came to be qualified, transmuted, and supplemented by the advent of ideas stemming from various extraneous sources. I shall consider the respective influences of geography, biology, history, psychology, and philosophy.

The impact of geography is perhaps most obvious. I am not referring to the theory of environmentalism as reflected in, say, Cushing's speculations on Pueblo origins, for against that aberration the younger generation was adequately inoculated. I have in mind rather the positive advance in distributional investigations.

Such problems had indeed obtruded themselves before. When obsidian tools turned up in an Ohio mound at least a thousand miles east of any possible source of supply, transmission was the only possible explanation (Holmes 1914:427, 430). Sporadically, as in Mason's treatment of the canoes from the Amur River, the intermittent occurrence of highly detailed similarities was solved in corresponding terms. Apart from such specific questions, curators faced the task of installing museum collections and devised arrangements based on geographical proximity. These efforts naturally culminated in the definition of culture areas (Holmes 1914).

What distinguished the new era was the *systematic* determination of distributions for purposes of historical interpretation. A geographer by training, Boas early (1895) applied the method to the study of Northwest Coast mythology (Boas 1940:425-436), thereby setting a pattern widely followed. It was, of course, a natural procedure for anyone geographically oriented, as witness Gudmund Hatt's dissertation on Arctic skin clothing (Hatt 1914). Further, it could be extended to so apparently elusive a phenomenon as the vision quest (Benedict 1922). What is more, whole cultures could be compared for major historical reconstructions. The avowed objective of the Jesup Expedi-

tion was the "precise determination of the geographical distribution of ideas and cultural forces" (Boas 1909:7). The extensive studies by Clark Wissler and Leslie Spier, paralleled by those of Erland Nordenskiöld and Kaj Birket-Smith abroad, may be mentioned as examples of distributional researches envisaging historical objectives.

Insofar as distributional researches tended to establish diffusion, they could not help affecting the theory of unilinear evolution. If the use of obsidian tools by Ohio mound-builders was impossible except through transmission from an outside source; if Columbia River Indians had got their canoes from the Amur; if Pueblo pottery was a consequence of Mexican stimuli, then these particular features in the respective cultures were not due to any laws of internal development. As unexceptionable instances of dissemination multiplied, the conviction gained ground that if such laws existed they were perhaps obscured to such an extent as to become unknowable. We are not at the moment concerned with the correctness of the inference, but with the historical fact that it was drawn.

III

It is a commonplace that the belief in cultural evolution is independent of Darwinism. Not to cite aboriginal myths, the idea was propounded by ancient Greek and Chinese philosophers. Bachofen's *Das Mutterrecht* (1861) appeared after *The Origin of Species*, but the conservative Swiss jurist remained untouched by the scientific currents of his age. In fact, we know that he expounded his basic position at a philologists' congress as early as 1856 (Meuli 1948:1045).

However, it was Darwin's theories that stirred the intellectuals of the world, and among them the ethnologists. Darwin assumed progressive development insofar as descent of a more complex from a simpler form may be called "progress." This feature is indeed eliminated from Radcliffe-Brown's definition of *social* evolution (Radcliffe-Brown 1947), but it pervaded the writings of the *cultural* evolutionists. In analogy to the biological philosophy of the times, Tylor recognized "stages of development or culture, each the outcome of previous history" and sought "to work out as systematically as possible a scheme of evolution." Notwithstanding occasional relapses in culture, he argued, the general course was upward (Tylor [1871] 1889:1, 20 f. 32, 62 ff.). The insistence on advancement is equally pronounced in Lewis H. Morgan and Powell, over-obtrusive in O. T. Mason's lists of change "from stone hammer to steam hammer," "from conch shell and rattle to orchestra," "from tribal deity to the Infinite and Omnipresent" (Mason 1908:187 et seq.).

Since the ethnological theories leaned on biology, a radical shift in biological views inevitably had repercussions in the sphere of cultural anthropology. Such a shift was indeed a reality by 1904.

Not that skeptics had been lacking before; in fact, they included some of the greatest figures of nineteenth century science—Richard Owen, Rudolf Virchow, Karl Ernst von Baer. But about the turn of the century doubt and

discontent came to a head, not as the result of a religious crusade, but of the rise of experimental methods. Jacques Loeb, bent on reducing the phenomena of life to the physics and chemistry of matter, was at one with the neovitalist Hans Driesch in looking with a mixture of pity and disdain on the "speculative and descriptive orientation" of the phylogenists.

A Columbia student who from a boy had accepted Darwinism as a dogma, who had steeped himself as an undergraduate in Herbert Spencer's *First Principles* and hailed Ernst Haeckel's *Die Welträtsel* as a definitive solution of all cosmic enigmas, was profoundly disturbed when browsing in the departmental libraries of Schermerhorn Hall or talking to age-mates who majored in zoology. Bewildering judgments turned up in the new books and journals. Haeckel, it seemed, was an irresponsible hotspur, if not a forger of evidence. For William James, Herbert Spencer was a "vague writer," and in Pearson's opinion the British philosopher cut a sorry figure when using the terms of physics. Darwin himself, esteemed for his monographs, was not always taken seriously as a theorist. In the building where our student spent most of his time Thomas Hunt Morgan, a prophet of the new dispensation, held forth on the weaknesses of the Darwinian philosophy.

To mention some of Morgan's points, he cited Johannsen's experiments on the inefficacy of natural selection: in a pure line the selection of particular individuals for breeding had proved immaterial. As for the "biogenetic law," the embryonic resemblances of higher and lower forms admitted of a simpler, alternative explanation than Haeckel's view that ontogeny rapidly recapitulated phylogeny. The paleontological record, with all its deficiencies, was indeed admitted as evidence of evolution, but it could never reveal "the hereditary units which have made the process of evolution possible." Perhaps most disturbing of all was the critique of the Darwinian's argument from comparative anatomy. Certainly one could make a plausible showing of widely diverging types that were linked by graduated steps. But was this more than a logical arrangement of data, suggesting at best what *might* have happened? Experiments had shown that 125 true-breeding mutants could be produced from the wild fruit-fly, and one could then put them in a series, with normally winged forms at one end and wingless forms at the other. But one extreme had *not* been evolved from the other by many intermediate steps: the several mutants had developed independently of one another (T. H. Morgan 1916:7, 27, 159).

Comparative psychology gave aid and comfort to the skeptics. Before the laboratory experiments of Edward L. Thorndike the alleged proofs of links between animal and human mind shriveled into romanticizing, anecdotal trivialities.

It does not matter in this connection whether, or to what extent, the new attitude was warranted. The point is that by 1900 the intellectual climate had changed. The transports of delirious rapture were succeeded by the mood of the *Katzenjammer*. What had figured as the quintessence of scientific insight suddenly shrank into a farrago of dubious hypotheses. In short, sobriety

reigned once more in professional circles. The revulsion of feeling was of a piece with the nausea evoked among the German laboratory workers of the early nineteenth century by the excrescences of *Naturphilosophie*.

Cultural anthropology could not escape the empiricist trend, which was eloquently defined in the opening pages of the Reports of the Jesup Expedition:

"The history of anthropology is but a repetition of that of other sciences. . . . New facts are disclosed, and shake the foundations of theories that seemed firmly established. The beautiful, simple order is broken and the student stands aghast before the multitude and complexity of facts that belie the symmetry of the edifice that he had laboriously erected. Such was the history of geology, such the history of biology. . . . We are still searching for the laws that govern the growth of human culture, of human thought; but we recognize the fact that before we seek for what is common to all cultures, we must analyze each culture by careful and exact methods. . . ." (Boas 1898:3 f.).

Let us note that, as Thomas H. Morgan did not reject biological evolution in toto, neither did anthropologists reject all cultural evolution. As Morgan accepted the paleontological record, they accepted the testimony of prehistory. Beyond that they were willing to be convinced by evidence. Contrary to some misleading statements on the subject, there have been no responsible opponents of evolution *as scientifically proved*, though there has been determined hostility to an evolutionary metaphysics that falsifies the established facts. To adduce an a fortiori proof of this contention, Fathers Schmidt and Koppers (1924:382, 396 et seq., 625 et seq., 636) consistently speak of "development" (*Entwicklung*), "stages of the total development," "the step from lower to higher hunting," and so forth. They, like their independent associate Heine-Geldern (1937; 1955:7), welcome the teachings of prehistory; nothing is farther from their minds than a relapse into the degeneration theory combatted by Tylor in 1871. Like other critical culture-historians, they repudiate *unilinear* evolution while making due allowance in principle for internal development. To quote Father Schmidt (1937:III, 10): "I neither avoid the word nor the concept and fact of evolution, but . . . freely profess *evolution*, while now, as before, deprecating *evolutionism*". . . ". . . He who combats and rejects evolutionism, is not thereby combatting and repudiating evolution, internal development."

Whoever wishes to understand the psychology of what has been inaccurately called "the reactionary philosophy of anti-evolutionism" in anthropology would do well to ponder the attitude of Jacques Loeb, T. H. Morgan, and H. Driesch toward phylogenetic speculations. Critical votaries of both sciences had simply arrived at higher standards of proof.

IV

In the meantime, skepticism concerning evolutionary "laws" was also being fostered by philosophers and historians.

The "Southwest German" school of philosophy drew a sharp line between

“nomothetic” and “idiographic” branches of learning, i.e. those which sought to establish laws and those which aimed at comprehending phenomena in their totality. Wilhelm Windelband formulated the antithesis in his rectoral address (1894), using for illustration physics and history (Windelband 1915). The essential distinction is doubtless older; it underlies an early paper of Boas' on the two approaches possible in geography (Boas [1887] 1940:626-647).

Windelband has been quoted by Radcliffe-Brown and myself, but his follower Heinrich Rickert seems to have made a deeper impression on ethnologists—possibly because he elaborated the issue at great length (Rickert 1899, 1896-1902). In Kroeber's collected essays there are eight references to him (Kroeber 1952:54, 70, 71, 73, 101, 123, 136, 469), and Sapir (1917:447) paid him a high compliment: “For a penetrating analysis of the fundamental distinction between historical and natural science I strongly urge all anthropologists, and social scientists generally, who are interested in method to refer to H. Rickert's difficult but masterly book on *Die Grenzen der naturwissenschaftlichen Begriffsbildung; eine Einleitung in die historischen Wissenschaften*. I have been greatly indebted to it.”

I surmise that Sapir's disinclination to put phonetic and natural “laws” on the same level stems from his indoctrination with Rickert's ideas. The reader of Windelband or Rickert might certainly conclude that the historical disciplines not only failed to demonstrate laws, but most emphatically did not wish to find any.

During the years under discussion historians themselves were clashing over the logic of their branch of knowledge, and their debates were echoed in our seminars. Indeed, two of the chief combatants, Karl Lamprecht and Eduard Meyer, lectured at Columbia. Lamprecht, scorning traditional historiography, insisted that it must be transformed into a true science, i.e. use the principle of causality as known to physicists and must determine laws. “Percepts” (*Anschaungen*) were to be superseded by “concepts” (*Begriffe*). Whole epochs could be subsumed under such concepts. Lamprecht had allegedly had some obtruded on him by a purely inductive study of the tenth and eleventh century in Germany. A “diapason” (a favorite term of his) penetrated all the psychic phenomena of a period, “all sentiments and actions.” [Attention, Configurationists!] The eras thus revealed for Germany turned out to have correspondences in the past of other great nations; the psychic characteristics of periods succeeded one another in regular sequence and were causally linked (Lamprecht 1900: 14, 16 ff., 25 ff., 33 ff.).

A colleague of Lamprecht's at Berlin deliberately carried the fight into the ethnological arena, dealing with Tungus and Papuans, Chukchi and Australians (Breysig 1904; 1936:433 ff.). He not only believed in fixed sequences, but also preached a return to Bastian and to a universal parallelism.

This new dispensation manifestly ran counter to Windelband's and Rickert's philosophy, which found a valiant champion in Eduard Meyer, the dean of German historians of antiquity. In agreement with Rickert, Meyer declared that throughout his long experience he had never encountered an

historical law. History was indeed a science, but its distinctiveness as such lay precisely in dealing with unique phenomena. In devastating sentences Meyer exposed Lamprecht's reliance on empty catchwords, and exploded Breysig's "laws" as so much pretentious drivel. Positively, he defined history as the science which determined past facts, selecting from their infinite number those which had proved effective, *wirksam* (Meyer 1910:3-67). The issue seems perennial within the guild, as indicated by an eminent Dutch scholar's treatment of it a generation later (Huizinga 1943:24 f., 40, 42, 46, 52).

Directly or indirectly, the idiographic conception of history affected incipient ethnologists. Thus, in 1909 a recent Ph.D. deprecated Schurtz's and Webster's "belief in a law of social evolution" as "unhistorical." Each tribal series of the age-societies he was studying must be investigated as "a unique historical product" (Lowie 1909:75, 98).

Of course, neither Columbia nor America had a monopoly of such points of view. When Wilson D. Wallis returned to the United States from his Oxonian interlude, he made it clear that R. R. Marett had also turned from unilinear evolutionism and was preaching the need for regional specialization (Wallis 1912:178 et seq.).

V

The singularity of past events was not the only lesson to be learned from history. Evolutionists were mostly continuators of the tradition of French encyclopedism. The votaries of *éclaircissement* had gauged past epochs by the norms of Western Europe in the 18th century: earlier periods ranked higher or lower as they approached the assessors' own age, so that medieval times, in particular, received a very low grade indeed.

The investigators of culture in the second half of the last century had only in part emancipated themselves from such complacency. Lord Avebury (Sir John Lubbock) was constantly chagrined and shocked by the "disgusting" Hottentot, the "miserable" Australian, the "cruel" savages "almost entirely wanting in moral feeling" (Avebury 1872:430, 437, 448, 509, 511, 536, 576; 1911:414). Indeed, the evolutionists of the period by no means spared those Western civilizations which happened to deviate from their own. L. H. Morgan's diaries while traveling in Europe illuminatingly exhibit this parochialism. The Italians, he averred, were "degraded beyond all other peoples called civilized," the South Italians were "utterly worthless." On the other hand, the United States ranked as "the favored and the blessed land. Our institutions are unrivaled and our people the most advanced in intelligence" (White ed. 1937:285, 290, 303, 311, 315, 327).

Yet as early as 1774 Herder had proclaimed in unmistakable terms that every people and each period in its past should be judged not by extraneous norms, but in accordance with local and temporal circumstances. The ancient Egyptian was not to be compared with the Hellene or with the eighteenth century *philosophe*. In short, Herder championed what we now call "cultural relativism," and thereby he deeply influenced Western historiography (Herder

1935:153-190. Cf. Clark 1955:187 et seq.). James Harvey Robinson, who fifty years ago was lecturing to large audiences at Columbia on the history of the West European intellectual class, was in his general outlook very far to the left of center, but historical-mindedness was one of his cardinal principles and nothing could be more sympathetic than his account of St. Francis of Assisi. Incidentally, it was Robinson's lectures that fired Radin with the idea of studying the role and the attitude of the intellectual class among primitive peoples (Radin 1927:XII).

Herder was referred to in my first seminar and figures here and there in our teacher's writings. Whether Boas derived relevant principles from the German classic directly or indirectly, I do not know, but he unremittingly preached the necessity of seeing the native from within. As for moral judgments of aboriginal custom, we soon learnt to regard them as a display of anachronistic naïveté.

* * *

History presented ethnologists with more technical suggestions. Paul Radin had majored in history under Robinson before turning to anthropology for his life work. It is probably no accident that, uncongenial as Radin found Boas, he became his most scrupulous follower in the collection of raw documentary material (Robinson 1912; Radin 1933).

VI

Psychological science is still unable to answer some questions which an anthropologist would like to have illuminated for him. But between, say, 1900 and 1915 psychology had clarified several important problems—the relative inborn endowment of the races, the question of individual variability, the mental processes of primitive as compared with civilized man. On some of these questions, to be sure, aid came from outside the circle of professional psychologists.

As early as 1858 Theodor Waitz had judiciously warned against underestimating the innate capacity of unlettered races, but it remained to probe the matter with the help of up-to-date techniques. In 1898 trained psychologists for the first time "investigated by means of adequate laboratory equipment a people in a low stage of culture under their ordinary conditions of life" (Haddon n.d.: 62). The statement refers to W. H. R. Rivers' and his associates' studies among the Torres Straits Islanders, which he summarized some years later (Rivers 1901); the findings were reported on in one of James McKeen Cattell's seminars. At the St. Louis Fair (1904) Cattell's associate, Robert S. Woodworth (1910: 171-186), investigated various ethnic groups and arrived at results comparable to those of the British researchers. Contrary to certain interpretations, these two independent investigations did not demonstrate identity of racial endowment, but they failed to discover radical differences and did not exclude environmental explanations of such differences as turned up. In other words, pending subsequent correction of these results, the

ethnologist felt warranted in ignoring hypothetical differences; and with assurance he could reject the gospel of Gobineau and his followers.

It was otherwise with innate *individual* variability, a phenomenon forcibly brought home by Francis Galton, whose *Inquiries into Human Faculty and Its Development* (1883), according to Wm. James, marked a new era in the history of psychology. The book gave me one of the lamentably few thrills experienced in the course of much professional reading. The environment was indeed favorable to an appreciation of Galton. Cattell, who had worked with him in London, regarded him and James as the two most remarkable men he had known. Boas, I think, had at least met Galton and held him in high esteem. The application of Galton's principle to our field was inevitable. With it collapsed the dogma that aborigines were wholly submerged in their social setting; at the same time new lines of observation obtruded themselves. Ethnographers consciously noted the role of leaders, skeptics, and other deviants; they distinguished between esoteric and exoteric rituals and myths; they made it their business to record several versions of the same tale, to take heed of the individual craftsman's attitude toward his art. The entire problem of the individual's relation to his society loomed as one of major consequence—for English-trained Wallis no less than for the Columbia group (Wallis 1915:647 et seq.).

This new orientation corrected the sociological aversion to dealing with everything biographical and individual, as defined in Durkheim's Preface to his first *Année* (1898:VI). It also ran counter to Wundt's notion that on the primitive level the individual was negligible, *Völkerpsychologie* terminating precisely at the point where "history" began with individuals influencing further developments (Haeberlin 1916:279).

However, American anthropology, far from spurning the lessons of sociology, incorporated them into its stock of ideas. The individual mind certainly was not a blank tablet, as was so commonly assumed in plausible hypotheses of cultural origins. If Japanese carpenters use planes in one way and Western carpenters in another; if different Crow Indians repeatedly experience the same kind of visions while Ojibwa regularly see different things on their fasts, such psychological phenomena can be neither typical of the human species as a whole nor of single individuals, but of cultures. However important may be the individual's psyche, his psychological manifestations are at least co-determined by social standards.

How far back in European thought this realization can be traced, I do not know, but it is already crystal clear in Marx's and Engels' (1888) critique of Ludwig Feuerbach. That philosopher had reduced "the essence of religion to human nature. But human nature (*Wesen*) is not an abstract something inherent in the single individual. As a reality it is the ensemble of social relationships." The religious sentiment, the Socialist thinkers contended, is a social product; the individual analyzed by Feuerbach belonged to a definite social form (Engels 1946:55 f.).

From another starting-point Moritz Lazarus and Heymann Steinthal

postulated a folk-soul, a notion already adumbrated by Herder, and developed it in the *Zeitschrift für Völkerpsychologie und Sprachwissenschaft* (1859). That journal, too, figured in a seminar I attended; and it is interesting to learn that Adolf Bastian, Boas' senior at the Berlin Museum, had been personally inspired by Lazarus (Schmidt and Koppers 1924:28). The notion of the folk-soul was further elaborated by Wundt, who recognized psychic manifestations "not to be explained solely by the characteristics of individual consciousness because they presuppose the interaction of many" (Wundt 1913:3).

If the individual's mental life was largely influenced by his society, this fact by itself implied that his thinking could be only in moderate degree rational. With or without a sociological orientation, a variety of authors kept harping on human irrationality. Gabriel Tarde's *Les lois de l'imitation* (1890) brought home to Boas the force of unconscious imitation and prestige suggestion (Boas 1940 [1896]:382). An economic geographer, Eduard Hahn, showed convincingly that sundry features of economic life could not possibly have originated in logical ratiocination (Hahn 1896, 1909, 1919). His main points were absorbed by Boas and Laufer, who passed them on to the younger generation. Laufer, independently of Hahn, strongly felt that the course of history was nonrational, and hence resistant to any logical scheme: "The unexpected, the unforeseen has always happened, and this is what cannot be supplied or supplemented by the logic of our rational mind" (Laufer 1914:261). Lévy-Bruhl (1910) went so far as to ascribe to primitive, as contrasted with civilized, man a prelogical mentality. The distinction was generally rejected, finally by its author himself, but the argument did convincingly support other evidence against picturing primitive man as formally solving his problems. Again, Wundt's psychology, even apart from his *Völkerpsychologie*, was definitely anti-intellectualistic. Finally may be mentioned the growing conviction among students of religion that ceremonial tended to precede the myth sanctioning it; in other words, behavior preceded thought.

The general import of all these views was to modify to a considerable degree the outline of religious development so impressively elaborated by Tylor. The great British anthropologist had represented early man as excogitating a veritable system of beliefs in answering the problems of human existence. Later scholars regarded relevant ideas as of unconscious growth, logical interpretation setting in only secondarily (Boas 1940:596). Marett presented the case in almost identical terms: The "fairly conscious inferences" ascribed to primitive man by Tylor must have been "preceded by an unconscious attitude of spontaneous behavior" (Wallis 1919:292).

Wundt's name has turned up several times in my discussion, and in view of his having devoted eleven volumes to *Völkerpsychologie* he can hardly be ignored. He was demonstrably read to some extent by Boas, Kroeber, Goldenweiser, Reichard, Haebler, and Lowie; from a conversation with Sapir I recall that he had at least read at Wundt, but was repelled by his diffuseness. Goldenweiser, on the other hand, on whom anything voluminously systematic cast a spell, put a very high value on Wundt's contribution to social science

and dedicated a book to him (Goldenweiser 1933:189-198). He is obliged to admit Wundt's relapses into a superannuated evolutionism, but lavishes praise on his nonintellectualistic approach to the origin of early inventions. However, a scholar interested in culture history could have derived the same insights more naturally from Hahn and Laufer.

Nevertheless, I should not like to minimize Wundt's influence on ethnology, but think that his general psychological position proved more fruitful than his specific treatment of cultural data. Haeberlin was a student of Wundt's in Leipzig and has left us an incisive, though not unappreciative, critique of his former teacher's principles (Haeberlin 1916b:279-302). Although Haeberlin also attended Lamprecht's lectures and may have been stimulated by one aspect of the historian's thinking, I believe that the psychologist's influence was deeper. When Haeberlin formulated the concept of configuration, though not using the word (1916a:1-55), he was probably extending his master's concept of "the creative synthesis" of psychic action with its corollary of the totality transcending the sum of its elements (Wundt 1911:1-113; 1920:152-162).

VII

The period here dealt with was one of intellectual ferment far transcending the range of particular disciplines. The second, enlarged edition of Karl Pearson's *The Grammar of Science* and the second, enlarged edition of Ernst Mach's *Die Analyse der Empfindungen* appeared in 1900; H. Poincaré's *La science et l'hypothèse* in 1903; Wilhelm Ostwald's *Vorlesungen über Naturphilosophie* in 1902. Crystallizing his earlier thoughts, Wm. James published *Pragmatism* in 1907, *A Pluralistic Universe* in 1909. John Dewey's instrumentalism was correctly felt to represent a related point of view. As Pearson remarked at the time, "many minds are being stirred to reconsider the fundamental concepts of science."

Students in eastern universities were not divorced from these currents. Dewey joined the Columbia faculty in 1904 and once, though much later, offered a joint seminar with Boas on comparative ethics. James and Ostwald lectured at Columbia in my day, and I recall an intimate conversation with Ostwald after he had addressed a seminar of Cattell's. Several of us, including Paul Radin, Goldenweiser, and myself, founded an informal "Pearson Circle" for the discussion of *The Grammar of Science* and continued our meetings long after we had left the University. The group naturally included nonanthropologists, among them the philosopher Morris R. Cohen, to whom S. F. Nadel has acknowledged his indebtedness.

What has all this to do with anthropology? Simply this: the anthropology of fifty years ago was not the concoction of "isolationists" (as they have been branded by some younger colleagues). We were not wholly concerned with finding out whether the Plains Indians put up tipis on a foundation of three or of four poles. In philosophical terms, the ethnologists of that era had passed from a naïvely metaphysical to an epistemological stage and in this were reflecting the spirit of the times.

In 1909 I qualified for the position of statistician and editor of the New York State Commission in Lunacy. During the preceding months I had contacts with Dr. Adolf Meyer, the psychiatrist, who had not yet accepted a chair at Johns Hopkins. What impressed me in those days was that in a sense Meyer, Boas, and Mach were doing much the same thing. They were severally scrutinizing such blanket terms as "schizophrenia," "totemism," "matter" and trying to discover their factual basis. When I grappled with Schurtz's notion of "age-society" and later with L. H. Morgan's of "classificatory terms of relationship," I more or less consciously applied the principles of these scientific thinkers. We had learned to view catchwords with suspicion.

* * *

In the present article I have not tried to write a history of intellectual movements; I have merely drawn attention to those currents of thought which certainly or at least probably bore on the history of American anthropology.

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