



Roxane Winburn









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THE HISTORY OF  
ETHNOLOGICAL THEORY

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*By*

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*To my Sister  
Risa Lowie*



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## PREFACE

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There are two histories of the entire science of man, but neither of them offers an adequate survey of ethnological development. That justly esteemed veteran, Professor Alfred C. Haddon, has crowded into the 140 small pages of his *History of Anthropology* (London, 1934) an amazing number of names and dates connected with prehistory, physical anthropology, linguistics, and ethnology; but space limitations barred any attempt to trace the progress of thought. T. K. Penniman's *A Hundred Years of Anthropology* (London, 1935), on the other hand, gives a one-sided emphasis to biological problems and is highly capricious in its admission and rejection of matters pertaining to ethnology.

The present treatise is explicitly devoted only to that part of anthropology (as the term is understood in English-speaking countries) which concerns culture. Within that sphere it attempts to indicate the course of theoretical progress; but since theory must rest on fact, the growth of knowledge through the perfection of techniques for gathering information receives proper consideration.

A glance at the Table of Contents suffices to show that there has been no undue emphasis on American theories; indeed, Morgan and Boas are the only writers to whom extended discussion has been granted. On the other hand, in illustrating points by special instances, the author has inevitably leaned heavily on American material, with which he happens to be most familiar.

|         |   |
|---------|---|
| BAE-    | Bureau of American Ethnology—   |
| -B      | Bulletins.  |
| -R      | (Annual) Reports.   |
| CNAE    | Contributions to North American Ethnology.                                    |
| CU-CA   | Columbia University, Contributions to Anthropology.                           |
| FL      | Folk-Lore.  |
| FMNH    | Field Museum of Natural History—  |
| -M      | Memoirs.  |
| -PAS    | Publications, Anthropological Series.   |
| IAE     | Internationales Archiv für Ethnographie.                                      |
| ICA     | International Congress of Americanists<br>(Comptes Rendus, Proceedings).      |
| IJAL    | International Journal of American Linguistics.                                |
| JAFLL   | Journal of American Folk-Lore.  |
| JRAI    | Journal of the Royal Anthropological Institute.                               |
| MAIHF   | Museum of the American Indian, Heye Foundation—                               |
| -C      | Contributions.  |
| -IN     | Indian Notes.   |
| -INM    | Indian Notes and Monographs.  |
| PM      | Peabody Museum (of Harvard University)—                                       |
| -M      | Memoirs.  |
| -P      | Papers.   |
| -R      | Reports.  |
| PMM-B   | Public Museum (of the City) of Milwaukee, Bulletin.                           |
| Rev     | Revue d'Anthropologie.  |
| SAP-J   | Société des Américanistes de Paris, Journal.                                  |
| SI      | Smithsonian Institution—  |
| -AR     | Annual Reports.   |
| -CK     | Contributions to Knowledge.   |
| -MC     | Miscellaneous Collections.  |
| UC-PAAE | University of California, Publications in American Archaeology and Ethnology. |
| UPM-AP  | United States National Museum—  |
| -R      | Reports.  |
| -P      | Proceedings.  |



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|---------|---|
| UW-PA   | University of Washington, Publications in Anthropology.                                 |
| Ver     | Verhandlungen der Berliner Gesellschaft für Anthropologie, Ethnologie und Urgeschichte. |
| ZE      | Zeitschrift für Ethnologie.   |
| Z vgl R | Zeitschrift für vergleichende Rechtswissenschaft.                                       |



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THE HISTORY OF  
ETHNOLOGICAL THEORY

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# II

## INTRODUCTION

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Ethnography is the science which deals with the "cultures" of human groups. By culture we understand the sum total of what an individual acquires from his society—those beliefs, customs, artistic norms, food-habits, and crafts which come to him not by his own creative activity but as a legacy from the past, conveyed by formal or informal education.

The relation of ethnography to sister disciplines is thus clear. It is that part of anthropology (in the English sense of the word, the whole science of man) which is not primarily concerned with races as biological divisions of *Homo sapiens* and does not interest itself in the psychology of individuals except insofar as it reflects or influences society. On the other hand, prehistory is simply the ethnography of extinct social groups.

At times ethnography shares its subject matter with literature, but its attitude is distinct. An exotic milieu, say, Tahiti, impresses itself on the sensibilities of a Pierre Loti, whose talent may convey similar thrills to the reader. An ethnographer does less and more. He renounces aesthetic impressions except as a by-product;

he does not select his facts for literary effectiveness since his duty lies in depicting the whole of cultural reality. As a naturalist cannot confine himself to beautiful butterflies, so the ethnographer must ignore nothing that belongs to social tradition. He records a boys' game on stilts as faithfully as he does the cosmogonies of Tahitian priests: both are part of his theme, and children at play may reveal as much of basic cultural process as does the metaphysical speculation of their elders.

The ethnographer also parts company with the antiquarian who collects odd customs with a philatelist's zeal about his stamps. From raw facts a scientist proceeds to orderly arrangement and interpretation. How have cultures come to be as they are? Why do remote peoples share similar ideas and usages? Why does a certain group fail to make an adequate adjustment to climate? Why does another perpetuate a custom no longer appropriate? These are among his problems; and in proportion as they engage him, the descriptive ethnographer turns theoretical ethnologist.

But theory can proceed sanely only on a wide foundation of fact. That is why all branches of anthropology necessarily lagged behind until geographical discovery enlarged their scope. A map of the ancient Greek or Roman world at once shows why its makers were precluded from sound conceptions of man as a species: they lacked elementary knowledge of his varieties. Eratosthenes (ca. 200 B.C.) knew nothing of Australia or Oceania or America; his view of Asia did not extend beyond India, and in that quaint triangle of his which stands for Africa the sources of the Nile are placed at the southernmost extremity. Ignorant of major races, the Greeks naturally were in no position to subdivide mankind on a rational basis. The Orient was no better off. When in 126 B.C. General Chang Kieng returned to China from his western travels, he brought "to his astonished countrymen a



glowing account of the new world which he had discovered, and which was nothing less than the Hellenic-Iranian civilization inaugurated in those regions by the successors of Alexander the Great" (B. Laufer).

Adequate knowledge of the globe is amazingly recent. New Zealand was not so much as sighted before 1642, and a fuller acquaintance with Oceania sets in only with Bougainville and Cook. Little more than half a century ago our best maps showed a blank space for the Belgian Congo. How could anyone survey humanity even superficially until at least its location and its range of variation were determined? The discovery of Australia, for example, disclosed a new race and many distinctive social practices.

What is more, even when the facts are established, it takes time for sound concepts to mature. The philosopher Meiners, writing towards the end of the eighteenth century, had read the accounts of the great explorers of his day. As a result of his studies, however, he classifies man into two main stocks—the Mongolic and the Caucasian *or* Tartaric, the latter being subdivided into (a) the Gothic *or* Celtic; and (b) the Slavic. Meiners derives the Australians from "the lower caste or the oldest inhabitants of Hindustan," which at first blush seems an anticipation of modern classification. But he mars his scheme by crediting these primeval natives of India with a Mongolic origin. His Mongolic stock is thus ancestral to the Eskimo, Chinese, Japanese, Indo-Australians, Papuans, Australians, and some of the African Negroes. As for the higher castes of India, they are indeed Caucasian, but "Slavic"—a category that includes also Armenians, Arabs, Persians, European Slavs, and many Germans.<sup>1</sup>

If it took time to correct grosser misconceptions as

<sup>1</sup> C. Meiners, *Grundriss der Geschichte der Menschheit*, 17 sq., 30 sq., Lemgo, 1785.

to physical affinities, the situation was inevitably worse for an understanding of culture, especially in its less tangible aspects. Travelers who admirably elucidated externals failed to go deeply into native beliefs and custom. Captain Cook was accompanied by such scientists as Banks and Forster, whose observations remain inestimable. But the time spent on his voyages permitted no thorough study of religion or family life. On such points missionaries, fur traders, and others whose calling enforces long residence are often superior even to modern specialists. The religion of Brazilian aborigines emerges more clearly from the reports of early Portuguese, French, and German visitors than from the works of such reputable ethnographers as Karl von den Steinen and Fritz Krause.<sup>2</sup> And the meager or confused reports of Farabee and Koch-Grünberg on South American marriage customs do not approach the accounts of André Thevet (1575), Gabriel Soares de Souza (1587), and Father F. S. Gilij (1781).<sup>3</sup>

The simple truth is that professional training, while important, cannot perform miracles. It can make an investigator note what an equally good unschooled observer would neglect. The most brilliant amateur cannot divine that at a particular stage of science apparently trivial details, like a basketry technique or the number of tent poles, may assume crucial importance. On the other hand, the elusive facts of social life and religious belief cannot be ascertained by the best specialist without long and arduous inquiry. It is when a talent for observation accompanies both protracted residence and contact with professional ethnography that we obtain such superb results as mark the work of Snr. Nimuendajú.<sup>4</sup>

<sup>2</sup> A. Métraux, *La religion des Tupinamba*, Paris, 1928.

<sup>3</sup> Paul Kirchhoff, "Die Verwandtschaftsorganisation der Urwaldstämme Südamerikas," *ZE*, 63:55-193, 1931.

<sup>4</sup> Curt Nimuendajú Unkel, "Die Sagen von der Erschaffung und Vernichtung der Welt als Grundlagen der Religion der Apapocuvaguarani," *ZE* 46:284-403, 1914.

From the preceding remarks it is clear why the very material for ethnographic interpretation was hard to obtain, and slow in coming in. Yet, in principle, sound theory presupposes ample information on every phase of life from every society in the world. However, even this demand is insufficient, for cultures vary in time as well as in space. Here we meet a second reason for the tardiness of ethnology. Until recently its time perspective was even more defective than its spatial vision. What vistas hitherto undreamed of were opened with the decipherment of the Rosetta stone and of the Behistun inscription! Yet Champollion presented his discovery to the French Academy no earlier than 1822, and Rawlinson published on Old Persian cuneiform in 1847. And these discoveries, startling as they were, carried us only a few millennia before Christ. The real revolution came with the recognition of Boucher de Perthes. When at the joint meeting of the Austrian and German Anthropological Societies at Innsbruck Rudolf Virchow explained the organization of the great European associations devoted to our science, it was to this event that he rightly assigned the first place.<sup>5</sup>

Jacques Boucher de Crèvecoeur de Perthes (1783-1868) was an amateur antiquarian who as early as 1836 argued that man was contemporaneous with extinct mammals, a thesis he defended before the local Société Impériale d'Émulation at Abbeville. At first proceeding a priori, he soon (1838) submitted stone hatchets in proof of human craftsmanship in the Pleistocene period. His was the common fate of prophets: "Practical men disdained to look; they were afraid; they were afraid of becoming accomplices in what they called a heresy, almost a mystification: they did not suspect my good faith, but they doubted my common sense."<sup>6</sup>

<sup>5</sup> Rudolf Virchow, "Eröffnungsrede," AGW-M, 24:70-77, 1894.

<sup>6</sup> Boucher de Perthes, *De l'homme antédiluviennne et de ses oeuvres*, 11, Paris, 1860.

A treatise *De l'Industrie primitive* (1846) made no impression on the learned until Dr. Rigollot (1854), a former antagonist, examined the sites from which the tools had been secured and announced his conversion. Still the guild of savants remained unconvinced. There were those who pronounced the strata of the hatchets as hardly older than the advent of the Romans. Some thought the tools had sunk to Pleistocene depths by their own weight. Others doubted the human origin of the flints, assigning their shape either to volcanic or to glacial action.

At last, in 1858, the British paleontologist Hugh Falconer examined Boucher de Perthes' collection and expressed himself satisfied by the evidence. His compatriots, Joseph Prestwich, John Evans, and Charles Lyell, followed suit; and at the Aberdeen meeting of the British Association for the Advancement of Science in 1859, Lyell announced his complete acceptance of the new views. The results harmonized with researches on British soil reported at the preceding meeting and led to the reinvestigation of stone finds previously registered but neglected.

The recognition of Boucher de Perthes' thesis marked a new era because it implies that culture dates back to the Pleistocene: the flints were not only made by man, they were obviously more than random freaks and worked in conformity with a social tradition. What is more, Boucher de Perthes was, in modern parlance, something of a functionalist (see Chap. XIII). That is to say, he understood that the artifacts discovered could not be isolated products of some technological instinct, but fitted into a larger context. He maintained stoutly and convincingly that the ancient stone knappers must have had not merely knives and hatchets, but a language, an art, social customs—in short, an equivalent, however

rude, of a complete culture.<sup>7</sup> In other words, culture dated back not a few paltry millennia, but to a period considerably prior to the geologically Recent era. The problem thus extended from the social existence of all living peoples to that of their cultures throughout all the thousands of years intervening between the Pleistocene and the present. The very question could not have been raised until about seventy-five years ago. Now it suddenly dawned upon students of civilization that what they had hitherto known was merely the final scene of a lengthy drama, that they had been trying to “reconstruct a book from its last chapter.”<sup>8</sup>

<sup>7</sup> *Op. cit.*, 52-59.

<sup>8</sup> Ch. Letourneau, *La condition de la femme dans les diverses races et civilisations*, 3, Paris, 1903.

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# III

## PIONEERS

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A point of departure is always arbitrary. We shall start with Meiners and Klemm because both had a tolerably clear conception of the central core of ethnography. Yet each recognized predecessors with aims akin to his own. Among those cited by Meiners are Iselin, Falconer, Goguet, and Montesquieu; while Klemm concedes priority to Voltaire: "Voltaire was the first to push aside dynasties, series of kings, and battles, and to seek what was essential, Culture, as it manifests itself in customs, faith, and governmental forms."

### MEINERS

In Meiners' *Grundriss* (1785) <sup>1</sup> this concept is adumbrated, but not yet distinctly conceived. As already pointed out, this author treated man's bodily diversity along with his social characteristics, but—what was far worse—he arbitrarily selected for discussion certain phases of social tradition to the exclusion of others. Avowedly for mere convenience' sake, he eliminated religion and the later stages of scientific development and

<sup>1</sup> See page 5 for precise reference.

still less defensibly disregarded all "but a few of the most remarkable customs—for to describe and explain all the practices of all peoples would be an enterprise equally foolish and thankless: foolish, because one can never include the infinite number of observances; thankless, because most of them can be no more satisfactorily explained than the formation and derivation of most words and because even the most probable explanations yield little for a knowledge of human nature." Meiners is thus very far from postulating a science that shall on principle deal with the whole social heritage of human beings.

Nevertheless, Meiners did sense the need of a new branch of learning to be set over against political history, a science to be dubbed "the history of humanity." Nor can he be accused of vagueness as to its contents. He lists as topics "food and strong beverages, dwellings, dress, and adornment of all nations" . . . ; "the . . . opinions of wild and barbarous peoples about the most important phenomena and effects of Nature and finally the history of the beginnings of the most necessary sciences. . . ." Elsewhere he adds "remarkable practices, the education of children, treatment of women, forms of government and laws, customs, notions of wealth and decorum, of honor and shame." This is, indeed, a formidable roster; and when Meiners pleads for a view of man as he has been at all times and all places, he voices the aims of modern anthropology. What we miss is a clear statement of what unites all these several disparate aspects of human life.

#### KLEMM

Clearer in his formulation and a more potent influence on research was Gustav Klemm,<sup>2</sup> to whose "in-

<sup>2</sup>Gustav Klemm, *Allgemeine Cultur-Geschichte der Menschheit*. 1. *Die Einleitung und die Urzustände der Menschheit enthaltend* (Leipzig,

valuable collection of facts" E. B. Tylor paid his respects in his first general treatise (1865).

Gustav Klemm (1802-1867), a native of Chemnitz in Saxony, was from childhood a passionate collector of specimens and an eager student of museums, without, however, slighting religion and social structure. An omnivorous reader, he offered what the accessible sources supplied, a veritable treasure-trove of facts, often significant, sometimes at least entertaining, from all known regions and periods. Even today one could not readily find a fuller compendium on the cuisine of all ages. He ransacked recondite chronicles for details of human interest, sometimes suggesting Berthold Laufer's writings and perhaps still more Alfred Franklin's books on medieval France. Sometimes, to be sure, Klemm turns a mere gleaner of trivialities, as when his discussion of swords merges in a complete list of famous Toledo smiths of the sixteenth century.

But Klemm did more than offer masses of raw material; we must credit him with anticipating Tylor's classical definition of culture, which is virtually our own. Klemm makes it comprise "customs, information, and skills, domestic and public life in peace and war, religion, science, and art." "It is manifest in the branch of a tree if deliberately shaped; in the rubbing of sticks to make fire; the cremation of a deceased father's corpse; the decorative painting of one's body; the transmission of past experience to the new generation." A tiny twist to the last phrase would have turned the trick of an adequate definition.<sup>3</sup>

The notion of progressive development was familiar to Klemm, even though his main works preceded Darwin's *Origin of Species*. The idea itself is, of course, both

1843; here quoted as *C-G. Allgemeine Culturwissenschaft* (quoted as *Cw.*), in two parts: *Werkzeuge und Waffen*, Leipzig, 1854; and *Das Feuer, die Nahrung, Getränke, Narkotika*, Leipzig, 1855.

<sup>3</sup> G. Klemm, *Cw*, 217, 1854; *C-G*, 1:21; *Cw*, 37, 1855.



ancient and primitive. It occurred among the Greeks and has been recorded from the lips of reflective sages among illiterate aborigines. The forms it took sometimes closely resembled the results of sober research, but we emphatically agree with Virchow that they must not be confounded with them. When a Chinese compilation of 52 A.D. presents the sequence of a Stone, Bronze, and Iron Age, this is not a case of genius forestalling science by two thousand years; an alert intelligence is simply juggling possibilities without any basis of facts or any attempt to test them.<sup>4</sup> The Chinese Stone Age became a scientific problem when other civilizations turned out to have had premetallic periods, and a reality when, two decades ago, Andersson excavated sites of stone workshops.

Klemm did not have to go far back for an evolutionary conception of man. Whether he was influenced by it or not, there was Condorcet's *Esquisse d'un tableau historique des progrès de l'esprit humain* (1795), outlining how primitiveness rose through stages of animal husbandry and agriculture to alphabetic writing, and ultimately enlightenment.<sup>5</sup> Klemm recognized three stages—savagery (*Wildheit*); tameness (*Zahmheit*); and freedom. He is clearest in defining the first of these: The savage roamed about, owning neither herds nor land and recognizing no paramount authority. On the second plane, families are consolidated into tribes with rulers by divine sanction. Here develop writing, pastoral life and farming, but with the limitations imposed by priestly domination. Freedom comes only when nations shake off this yoke, thereby gaining a chance to develop their mentality in all directions. The Persians, Arabs, Greeks, Ro-

<sup>4</sup> B. Laufer, "Jade," FMNH-PAS 10:71, Chicago, 1912.

<sup>5</sup> J. Salwyn Schapiro, *Condorcet and the Rise of Liberalism*, 234-270, New York, 1934.

mans, and, above all, the Germanic stock illustrate this highest step.<sup>6</sup>

However, it would be an anachronism to impute to Klemm either an anti-clerical or a nationalistic attitude in the present sense of these terms. Christianity is for him the mainspring of progress toward freedom precisely because it dissolves the national hierarchies. On the other hand, his views on race, though curious, bear no relation to those of Günther or Hitler. His system does, indeed, involve a division of humanity into active and passive races, the latter being mere copyists who transmit what has been handed down from the past or imposed upon them by conquerors. However, the passive group comprises not only Mongoloids and Negroids, but even Egyptians, Finns, Hindus, and the lower strata of European society. Secondly, Klemm likens his two races to man and woman, respectively: as the sexes are mutually complementary, so the active race is incomplete without the passive, and vice versa.

On this subject our author is somewhat obscure. He visualizes the earliest human beings as spreading over the globe, first as hunters and fishermen, later as herders, "until increasing population necessitated agriculture." At this point one might reasonably object that mere imitators do not inaugurate momentous economic changes. As for the active race, it somehow arose independently near the Himalayas, traversing its own periods of savagery and tameness, of stock raising and farming. Active peoples naturally subjected the passive ones, whence that fusion which Klemm considers essential for the ultimate ends of Nature.

The foregoing principles—this dichotomy of our species and the three evolutionary stages—underlie the organization of the *Cultur-Geschichte*, which begins with "passive humanity" and passes on to the active race;

<sup>6</sup> Klemm, *C-G*, 21-23.

while in each of these two sections the author proceeds from the lowest to the most advanced members according to the categories of savagery, tameness, and freedom. Klemm is prone to accept uncritically the psychological judgments of travelers and is equally naïve in connecting mentality with geography. The tropical South American forest Indians lack "the finer sentiments of friendship, love, and modesty"; and though exceptional cases suggest that the Creator designed even them to progress, as a rule they are infinitely indolent and lethargic. As denizens of the woods they grow up with a limited horizon, while coast dwellers reflect the constant changes due to the sea, changes which foster their powers of concentration. That is why the fishing tribes of Australia excel the South American forest Indian in alertness, reflectiveness, and intellectual independence. Fuegians are rated as peers of the Australians, thus assuming higher rank than the "far cruder" forest tribes. In fairness to Klemm we must add that he is thinking rather of the hunting Botocudo than of the horticultural Arawak, so that the comparison, however unconvincing, is not quite so grotesque as it appears at first. He does refer to the use of bitter manioc, but without realizing the ingenuity required for eliminating its virulent poison.<sup>7</sup>

Klemm's intellectual limitations appear when we compare his treatment of certain topics with Tylor's. He knew as well as the British ethnologist that the Kamchadal cooked meat in wooden troughs filled with water into which they threw heated rocks. But it remained for Tylor to bring the custom into line with North American and Polynesian usages, to conceptualize the operation as "stone-boiling," and to assign it a definite place in the history of cookery. Again, Klemm has excellent remarks about fire—its universality, its being an exclusively human possession, the impossibility of investigat-

<sup>7</sup> G. Klemm, *C-G.*, 1:196-200, 234, 280, 287 f., 327-332, *Cw*, 241, 1885.

ing its origin. He recognizes friction as an older technique than percussion and from a passage in Pliny infers its presence in pre-Christian Europe. But his typology marks no advance over Pliny's: the rubbing together of sticks, strike-a-lights, and the burning lens exhaust his inventory.<sup>8</sup> Once more it was Tylor who defined the fire-plough as a distinct implement and determined its distribution; Tylor who indicated drills managed with a thong, bow, or flywheel as diverse and superior forms of apparatus.

While his was hardly a great intellect, Klemm nevertheless remains a noteworthy figure. His comprehensive and clear conception of what culture is, as well as the wealth and variety of his knowledge, assures him an honorable position among our pioneers. He also spread technological information by the excellent and ample illustrations of his books at a time when museums were rare, inadequately equipped, and less accessible than they are today.

#### W A I T Z

Theodor Waitz (1821-1864) represents a wholly different approach. As professor of philosophy at Marburg he was interested above all in psychological questions, and his *Anthropologie der Naturvölker* (Leipzig, 1858-1871), a six-volume work, in part published posthumously, is largely a treatise on primitive mentality. He explicitly refrained from technological detail on the plea that Klemm had amply dealt with that phase of the subject. On the other hand, he is strongest where his predecessor is weak—in the critical analysis of sources and the depth of his psychological insight. His position is defined in the first volume, which bears the sub-title *Über die Einheit des Menschengeschlechtes und den Naturzustand des Menschen*. It was this part that was

<sup>8</sup> G. Klemm, *C-G* 1:178 f.; *Cw* 66-70, 260.

reissued in 1876 and in 1863 had been singled out for translation by the London Ethnological Society as the most representative continental treatise on man. The geographer Georg Gerland, who prepared the two final portions of the whole work for the press after the author's death, rightly praises the "strict, cautious, sober method" of this first volume. Considering its date (1858), we must pardon occasional errors and recognize it as a worthy forerunner of Boas' *The Mind of Primitive Man* (page 131), which closely parallels its argument.

Waitz deprecates rash verdicts on racial disabilities, but his calm intellect remains undimmed by propagandist fervor. He is equally free from exaggerations of the geographical order, explicitly refuting popular extravagances. Not all mountaineers love liberty, nor does grand scenery suffice to evoke aesthetic thrills. Environment does not automatically stimulate a specific adaptation, which is itself a function of a people's culture: the sea offers opportunities only to mariners, not to people who lack boats. We must thus distinguish between effective causes and mere occasions or minor factors of progress. Geography may inhibit but does not necessarily create. It is overshadowed by historical and social determinants—by migrations and the consequent diffusion of traits. A dense population is both a result and a cause of advancement. Native capacity, Waitz undogmatically concludes, is at least roughly the same in all races. If some have shown a greater trend toward civilization, it is because of favorable circumstances. The degree of culture is thus far less an index of innate endowment than of the vicissitudes of history. Waitz is especially cogent in comparing civilized and uncivilized conditions. He stresses—though with characteristic moderation—the role of genius and insists that important discoveries were made on ruder as well as higher levels, but that in both cases they probably had to be repeated before people at

large were able to profit from them. Essentially, Waitz's insight into the determinants of progress is as clear as that of today.<sup>9</sup>

Waitz appears to advantage in the second volume (1860), which applies his principles to Negroes—*Die Negervölker und ihre Verwandten*. Its compact résumé of accessible data, with its resolute exclusion of tourists' fancies, is surpassed only by the writer's incorruptible judgment on what was then a hotly disputed problem. He does not regard Negroes as merely excellent imitators who are thereby nearer to the ape; but neither does he accept at its face value the exaggerated estimate of well-meaning humanitarians. Throughout there is discriminating appraisal. Individual Negro geniuses are cited, such as the inventor of the Vei alphabet and the rulers of great Negro states. A race representing a lower *species* could not produce any individuals of such exceptional ability. On the other hand, among us, too, it is the outstanding genius who creates group progress. In answer to the charge of cruelty as an innate trait of Negroes, he adduces the mild treatment of African slaves. And considering how inadequate his sources were on such topics, he forms remarkably sound conclusions about Negro religion. Rejecting then current notions, he recognizes the affinity of the African "fetich" with the American "medicine"; and sees the natives hovering on the brink of monotheism.<sup>10</sup>

In 1860 a student who had mastered the writings of Klemm and Waitz would thus be in a somewhat better position than might at first seem conceivable. With a tolerable survey of the material equipment of human groups he might unite a fair perspective of the forces controlling progress.

<sup>9</sup> Th. Waitz, *op. cit.*, 1:408-424, 428 f., 447, 473 f., 482.

<sup>10</sup> *Ibid.*, 2:167, 175, 216, 222, 228-232.



## BIOLOGY, PREHISTORY, AND EVOLUTION

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As we have seen, the idea of progressive development from savagery to civilization was much older than Darwin or even Lamarck. However, when evolution became not merely an approved biological principle but a magical catchword for the solution of all problems, it naturally assimilated the earlier speculations about cultural change as obviously congruous with its own philosophy. Similarly, the discoveries of prehistory neatly fitted into the evolutionary picture. Both biological theory and archaeological research powerfully stimulated the study of culture, but not without creating grave misunderstandings.

Evolutionary doctrines implied that complex organisms had slowly developed from extremely simple forms. Pitt-Rivers,<sup>1</sup> transferring the notion to the sphere of human arts, postulated Spencerian changes "from the simple to the complex, and from the homogeneous to the heterogeneous." Moreover, not only evolutionary but Darwinian, he assumed continuous modification by *minute*

<sup>1</sup> A. Lane-Fox Pitt-Rivers, *The Evolution of Culture and other Essays*, Oxford, 1916.

steps. Stonework, for instance, he traced from the crudest flint implements to the polished celts of modern savages by "numerous intermediate gradations." And in deriving diverse Australian contrivances, such as the mushroom club, the shield, and the boomerang, from a single prototype—a simple cylindrical stick—he obviously patterns his procedure on the phylogenetic hypotheses of contemporary zoologists.

One problem that seems to loom disproportionately large in the pages of Victorian anthropologists is that of degeneration. But if Lubbock, Tylor, and Pitt-Rivers devoted page after page to that subject, there was an excellent reason. Influential writers, theological and otherwise, were contending that primitive peoples had retrogressed from a higher state. In rebuttal the evolutionists reiterated with unremitting emphasis that the dominant note in the history of the species was an upward movement, decline being exceptional.<sup>2</sup> Their general point of view is concisely stated by two of its enthusiastic champions. Says Letourneau: "All the civilizations past or present had their barbarous or savage infancy, out of which they have slowly and painfully evolved . . . ; the rude contemporary races, the lowest of which border on animality, picture for us, in general fashion, the slowly progressive phases which were traversed by the ancestors of civilized peoples."<sup>3</sup> Similarly, Pitt-Rivers declares that "the existing races, in their respective stages of progression, may be taken as the bona fide representatives of the races of antiquity. . . . They thus afford us living illustrations of the social customs, the forms of government, laws, and warlike practices, which belong to the

<sup>2</sup> See e.g. E. B. Tylor, *Researches into the Early History of Mankind and the Development of Civilization*, 150-190, London, 1865; Lord Avebury (= Sir John Lubbock), *The Origin of Civilization and the Primitive Condition of Man*, 6th edition reissued with a new preface, 524-552, London, 1911.

<sup>3</sup> Ch. Letourneau, *op. cit.*, 3.



ancient races from which they remotely sprang, whose implements, resembling, with but little difference, their own, are now found low down in the soil. . . ."<sup>4</sup>

Biologists had in part direct proof for their evolutionary schemes, paleontological findings; the anthropologist's counterpart was prehistory. Let us, then, examine wherein its value lay.

Boucher de Perthes had shown that stone work, hence culture, dates back to the Pleistocene, but he was not the first to propose a Stone Age on empirical grounds. That honor belongs to a Danish historian: in his *Aperçu sur les périodes les plus anciennes et les plus remarquables de l'histoire nationale* (1813) Vedel-Simonsen had already argued for three periods of Scandinavian antiquity—a Stone, a Copper or Bronze, and an Iron Age. Of his immediate successors, Christian Jurgensen Thomsen accepted the idea, and Worsaae (1821-1885) extended it to other European countries.<sup>5</sup> This generalization was not more readily accepted than that of Boucher de Perthes. As late as 1875 German scholars categorically denied a Bronze Age, and bitter controversies were waged over this point, the Scandinavians—Worsaae and Sophus Müller—opposing their German colleagues.<sup>6</sup>

The Stone Age, however, had been definitely established by that time, and its local developments were being brilliantly illuminated by new excavations and conceptualizations. The Danish zoologist Japetus Steenstrup demonstrated a premetallic age of hunters and fishermen living under climatic and arboreal conditions prior to those of written records. Others divided the Stone Age into two main epochs, that of chipped stone tools (paleolithic) and that of ground implements (neolithic). The former was again susceptible of division

<sup>4</sup> Pitt-Rivers, *op. cit.*, 53.

<sup>5</sup> Ingwald Undset, "Le préhistorique scandinave, ses origines et son développement," *Rev.* 3<sup>e</sup> série, 2:313-332, 1887.

<sup>6</sup> L. Lindenschmit, *Ar-A*, 9:152, 1876.

according to the artifacts produced, viz., into the Acheulean, Mousterian, Solutrean, and Magdalenian.<sup>7</sup> We must not omit the Swiss lake dwellings, which disclosed a complete neolithic culture with relics of farming, animal husbandry, earthenware, weaving, and ground stone axes, for the inability of scholars to link these finds with the products of any historically known peoples deeply stirred the minds of European scholars.<sup>8</sup>

Undoubtedly, then, prehistory proved evolution by the rigorous technique of geological stratigraphy at a time when ethnographers were still groping for proper methods of investigating living aborigines. No wonder that ethnographers leaned heavily on the staff of archaeology. People who raised cereals and ground stones were equated with the neolithic Swiss; a knife-chipping Australian horde was set down as Mousterian.

What the evolutionists as a class failed to see was the limited range of cultural facts for which progress could be directly demonstrated. Prehistory reveals only material phenomena, and only part of them. Only under exceptionally favorable circumstances of preservation can it teach anything about even such tangible but perishable objects as bows or basketry. In short, it determines accurately certain phases of technology and nothing else. A picture derived solely from archaeological sites is often grotesquely distorted: the most skillful woodcarvers and bark-cloth beaters, leaving little evidence of their craftsmanship, must suffer in comparison with potters and metallurgists.

A mechanical transfer of the prehistoric categories in vogue fifty years ago is thus fatal even for sound appraisal of *technical* progress. It is true enough that all metal-working groups have passed through a premetallic

<sup>7</sup> Gabriel de Mortillet, "Classification des diverses périodes de l'âge de la pierre," *Rev.* 1:432-442, 1872.

<sup>8</sup> Cf. R. Virchow, *op. cit.*

stage, but it does not follow that all peoples without metals make stone tools. As Von den Steinen insisted, many South American natives find this impossible for lack of stone.<sup>9</sup> In order, then, to apply the concept "Stone Age" here, we must divest it of its original meaning and make it include bone, shell, wood, and the like. Again, the separation into a Paleolithic and a Neolithic period can be kept useful only if we completely alter the primary sense of these terms. Some Australians merely chip stone, their neighbors not otherwise a whit more advanced grind axes because they have access to diorite; the difference implies only a difference in material resources, hence it cannot serve as a major line of demarcation. In order to infuse significance into the term, investigators have perforce redefined "Neolithic" to indicate primarily the status of potters and farmers. Thus, prehistory, instead of being an infallible guide, required correction and conceptual purging by ethnographic treatment.

But if prehistory left gaps and sometimes even misled scholars on points of technology, it had nothing whatsoever to offer on the growth of supernaturalism and social organization. That was intolerable for the evolutionist mentality, which demanded the sequence of events for every phase of human activity. The biologist, similarly handicapped by the defectiveness of the geological record, had recourse to embryology. Anthropologists eked out results of excavation by falling back on a corresponding law of growth. As *Homo sapiens* was zoologically at the peak of the animal kingdom, so Western Europe in 1870 marked the goal of civilization. As the single cell was the hypothetical starting point for evolution, so a savage hovering on the border of bestiality must serve as the point of origin for culture. Since, however, that primeval man could no longer be observed, modern sav-

<sup>9</sup> Karl von den Steinen, *Unter den Naturvölkern Zentral-Brasiliens*, 2nd ed., 196, Berlin, 1897.

ages were lightly substituted insofar as they differed from Victorian Europe. On the other hand, usages of modern Europeans not in keeping with their advanced status were like those rudimentary organs of animals which Darwin had compared to the letters of a word that are no longer pronounced.

A fatal fallacy of all this reasoning lay in its naïve equation of modern primitive groups with the primeval savage, as in the sentences quoted from Letourneau. This led serious writers into absurd underestimation of recent tribes and uncritical acceptance of tourists' tales. Sir John Lubbock was one of the most versatile minds of his age, an eminent prehistorian, a writer who ranged over the whole field of anthropology and thought independently on all of its phases. Yet he is no better than Klemm at assessing poor evidence.<sup>10</sup> The Andamanese have "no sense of shame"; "many of their habits are like those of beasts." The Greenlanders have no religion, no worship, no ceremonies. The Iroquois have no religion, no word for God, Fuegians not the least spark of religion. ". . . there can be no doubt that, as an almost universal rule, savages are cruel."

This last sentence illustrates another major error—the complete abandonment of objective criteria. What is cruelty? Is the cannibalism of Oceania worse than the wholesale massacres of modern warfare? Sir John's writings teem with subjective judgments, naïvely passed on the basis of resemblance to or deviation from European standards. The Hottentots are "disgusting," the Australians "miserable" savages. Occasionally he exhibits insight, as when he corrects Prescott for ascribing human sacrifice to "fiendish passions."<sup>11</sup> But, generally

<sup>10</sup> John Lubbock, *Prehistoric Times*, 430, 437, 448, 509, 511, 536, 570, London, 1872.

<sup>11</sup> *The Origin of Civilization*, 384.

speaking, he is himself constantly mortified, shocked, horrified, by the savage scene.

The modern scientific procedure is to refrain from *all* subjective pronouncements; to recognize that while material objects and rationalistic schemes are "higher" or "lower"—better or less suited to their purpose—this does not hold for art, religion, and morals, where no universally recognized standards exist. The anthropologist as an individual cannot but respond to alien manifestations in accordance with his national and individual norms; as a scientist, however, he merely registers cannibalism or infanticide, understands, and if possible explains such customs.

Lubbock's egocentrism appears most oddly in a subject that certainly does not bear directly on ethical attitudes—the designation of relatives. He appreciatively taps the Eskimo on the shoulder for "correctly" recognizing uncles and aunts, that is using true equivalents of the English terms, while the Hawaiians, who have no special word for these relatives, are credited with the most savage nomenclature known.<sup>12</sup>

The resemblance of modern savages to a primeval ape-man is so important a tenet that we must explicitly expose the error. It lies in failing to understand that even the simplest recent group has a prolonged past, during which it has progressed very far indeed from that hypothetical stage. To look for any living people without religion, for instance, is like trying to observe life evolving out of inorganic matter. As we shall see, the greatest of the evolutionists avoided this pitfall.

Finally, as to "survivals," the rudimentary organs of social groups. Unquestionably civilization in all its stages teems with illustrations. Instances are offered by the most determined opponents of the evolutionist system. Thus, among some of the Eskimo Boas notes the

<sup>12</sup> *Ibid.*, 183, 202. Also in JRAI, 1:11, 1872.

curious women's stockings, which bulge out enormously just below the knee. Whence, he asks, such an odd fashion? An old source testifies that in 1750 the women wore huge boots kept open by whalebone hoops, children being put inside these pouches. Thence comes Boas' conclusion that "the wide ankle-pouch of the long stocking of the west coast of Hudson Bay may be a survival of this wide boot."<sup>13</sup> The question is not, then, one of accepting or rejecting survivals, but whether an alleged survival is genuine or spurious. For in culture as well as in biology, there may be alternative explanations for a "useless organ." More particularly, the utility may be merely masked, the feature fully functioning in some unexpected way. For example, a favorite argument of the period was to point to the avunculate, i.e., the special powers enjoyed by a maternal uncle, in societies that reckoned descent through the *father*. How, it was asked, could such a disharmonic trait be interpreted? Surely in only one way: as a legacy from an earlier period in which the tribe had traced descent through the mother, hence had emphasized the role of maternal kinsmen. This argument, however, no longer convinces because the maternal uncle may have become important in other ways. If, for instance, a society with or without patrilineal clans made a husband do service for his bride, he would naturally settle in her home or village, and his offspring would automatically fall under the sway of her kin. There is thus no need at all to infer matrilineal descent from avuncular authority. In fact, the alternative cited is only one of several plausible explanations.

The survival argument would be uniformly trustworthy only if there were a fixed law of sequence. Such laws were indeed repeatedly affirmed but hardly ever demonstrated outside of technology. The evolutionists,

<sup>13</sup> F. Boas, "The Eskimo of Baffin Land and Hudson Bay," AMNH-B, 15:105, 356, pl. III, 1906.

assuming that they knew the course of development as predestined, considered themselves able to interpolate missing links in the chain of events. In this they were far too optimistic, and their interpretation of survivals was correspondingly faulty.

So far the discussion of the doctrine before us sounds like an arraignment. But it would be gross injustice to underestimate its services. To the insight of our hypothetical student of Klemm and Waitz is added for the first time a synthesis of all cultural data, combining from a single point of view the results of ethnography and prehistory. If the lesser apostles of the theory rashly distorted the facts to eke out lacking evidence, saner spirits avoided such gross blunders. Innumerable new problems were broached, some of them were even solved under the impetus due to the luminous concept of progressive development. For, notwithstanding the qualifications cited, evolution is a positive fact in material culture and freely conceded by the most determined critics of its Victorian champions.<sup>14</sup> To admit this, together with the possibility that material conditions may affect other phases of life, is to open the way for a fixed sequence of social and religious phenomena. Actually, these writers themselves postulate stages of development (*Stufen der Entwicklung*), in other words, evolution.<sup>15</sup> From entirely distinct starting points such contemporary anthropologists as Thurnwald and Radcliffe-Brown are also rehabilitating the concept. It is thus very far from dead, and our duty is merely to define it with greater precision.

Finally, another word of caution seems indicated. It has become customary to oppose cultural evolutionism to the principle of diffusion. This is by no means a fair

<sup>14</sup> W. Schmidt and W. Koppers, *Völker und Kulturen*, 45 f., Regensburg, 1924.

<sup>15</sup> *Ibid.*, 264 sq.

view of the matter. To be sure, given a fixed law of development, the same beginnings might lead to an indefinite repetition of the identical stages. Crude stone-fracturing might thus be followed everywhere by the same series of techniques first recorded in France, so that Southern Spain and Africa and China would all exhibit a sequence of Chellean, Acheulean, Mousterian, Solutrean, and so on; and correspondingly with traits of other categories. Actually, the early evolutionists differed individually in their attitude towards this problem. Some were inclined to explain resemblances by independent multiplication due to an identical law of growth. But this was probably not wholly true of any writer, and demonstrably held for neither Tylor nor Morgan, the most influential thinkers of all. So orthodox an evolutionist as Pitt-Rivers was emphatically not a parallelist. He did not, to be sure, categorically deny that mankind might have "independently designed the same forms of tools in various parts of the world." But he explicitly derives the boomerangs of Australia, the Deccan, and Egypt from a single center; he holds that in the development of the bronze celt "each new improvement was communicated from tribe to tribe and from nation to nation"; and his final words (in 1864) are that "by means of intercommunication, no less than by spontaneous development, have been formed those numerous combinations which so greatly puzzle the student of culture at the present time."<sup>16</sup> A generation later, Pitt-Rivers' commentator is not less emphatic in vindicating the importance of dissemination: "Cases of independent invention of similar forms should be considered to have established their claim to be regarded as such only after exhaustive inquiry has been made into the possibilities of the resemblances being due to actual relationships."<sup>17</sup>

<sup>16</sup> Pitt-Rivers, *op. cit.*, 145, 153, 183, 228.

<sup>17</sup> Henry Balfour, Introduction to Pitt-Rivers, *op. cit.*, xii, xix.



This is very nearly the principle of modern diffusionism (page 158).

As a matter of fact, biological evolution would *not* suggest parallelism. Zoologists did not assert that man or the horse had again and again risen from humbler prototypes, but they believed in unique happenings culminating in so many end results. Parallelism was possible only on the principle that the psychic unity of mankind constantly impelled societies to duplicate one another's ideas. That, however, was not a corollary from evolution, but the doctrine of a staunch critic of Darwinism, Adolf Bastian.

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# IV

## ADOLF BASTIAN

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Adolf Bastian was born in Bremen in 1826. He studied law, natural science, and medicine in no less than five German universities, spending his last semester in Würzburg under the great pathologist Rudolf Virchow. Having graduated as a doctor of medicine in 1850, he promptly secured a position as ship's surgeon and spent the ensuing eight years voyaging to Australia, Peru, Mexico, California, as well as to various Asiatic and African countries. In 1859 he published *Ein Besuch in San Salvador*, and the following year *Der Mensch in der Geschichte*. In 1861 he was off again to Farther India and Eastern Asia, and henceforth his life was punctuated by lengthy travels to remote corners of the globe, with intermediate sojourns in Berlin, his chosen headquarters. There he became Curator of Ethnography (1868), subsequently founding what remained for decades the largest emporium of ethnographica in the world, the Königlich Museum für Völkerkunde (1886). In 1869 he helped Virchow organize the Berlin Society for Anthropology, Ethnology, and Prehistory, became co-editor of its journal, the *Zeitschrift für Ethnologie*, and

took an active part in geographical activities. Bastian, paradoxically, was a successful promoter, a "fisher of men," an astute buyer whenever a chance offered to enrich his beloved Berlin collections, and simultaneously a shy bookworm, if not a recluse. In 1903 von den Steinen asked him when he had last visited a theatre; after some reflection came the answer, "In 1859." His meteoric appearances and disappearances became proverbial in Berlin; and he died in Trinidad, off the coast of Venezuela, in 1905, at the ripe age of seventy-nine. Karl von den Steinen epitomized his career in one sentence: "No German scholar has traveled more, none read more, none written more."<sup>1</sup>

Two factors invested Bastian with a comic halo in the judgment of irreverent posterity—his determined opposition to Darwinism and his style, a combination that evoked Haeckel's vituperative title of "Geheimer Oberkonfusionsrat." Both manifestations, however, have been misunderstood.

Bastian's attitude towards biological problems reflected not theological prejudice but a puritanical empiricism. Let us not forget his association with Virchow. That great pathologist embodied, above all, the reaction of triumphant natural science against the speculations of the German *Naturphilosophen*. What could not be determined by direct observation or experiment savored of metaphysics. Like Virchow, Bastian regarded transformism as untenable so long as no one had ever seen one species changing into another. He spoiled his case when, decrying the homologies of the evolutionists as "scientifically undefinable similarities," he pretended to see no difference between them and the analogy between a tulip stalk and a swan's neck. But the basic objection that he leveled at Darwinism—and Darwin himself he held in

<sup>1</sup>Karl von den Steinen, "Gedächtnisrede auf Adolf Bastian," ZE 37:236-249, 1905. Also: Von Richthofen, *ibid.*, 249 sq.

high esteem—was that of the modern experimentalists, of Jacques Loeb and Thomas Hunt Morgan. His position may be narrowly unhistorical, barrenly skeptical, but it was not lacking in scientific respectability.<sup>2</sup>

About Bastian's style, it is also wiser to discriminate instead of joining the chorus of cheap gibes. At its worst it is surely inconceivably crabbed. To confront Bastian in some of his lucubrations is a never-to-be-forgotten experience. The astounded reader runs into sentences twenty lines and more in length and hacks his way through bracketed quotations in Latin, Greek, or Polynesian, only to find that he has yet to extricate himself from the maze of some major parenthesis. Bewildered by recondite allusions and unheard-of authors, he is distracted by footnote after footnote that lend but mediocre illumination until a full stop at last affords a breathing spell. Nor are bizarre figures of speech an aid to understanding. Geography is suddenly introduced as the "many-breasted mother of . . . ramifications spun over the globe"; she works "to level the soil for Ethnology," who in turn traces "the roots of Psychology embedded in Physiology"; as a result of all which, "Materialism is to see the hitherto amorphous torso of her world-view perfected . . . by her consecrated wedding to Idealism."<sup>3</sup> The following purports to explain why the Tree of Humanity may be glimpsed more clearly on primitive levels: "We meet it just and barely as light shoots sprout out of the earth, as the little stem puts forth its leaves, displays blossoms, is decked in floral splendor, at times perchance even affording little fruits . . .; and wherever we meet it, we can seize, grip and touse, strip and pluck it on be-

<sup>2</sup> A. Bastian, *Die Völker des östlichen Asien*, 5: xlf., Leipzig, 1869. Cf. his review of Haeckel's *Anthropogenie* in *ZE*, 7:203, 1875; and his remarks *ibid.*, 3:133 sq., 349, 355f., 1871; 8:394, 1876.

<sup>3</sup> *Der Völkergedanke im Aufbau einer Wissenschaft vom Menschen und seine Begründung auf ethnologische Sammlungen*, 2, Berlin, 1881.

half of science, even vivisect it.”<sup>4</sup> Restraint from premature generalization is preached thus: “Thereby would be tailored for us a beggar’s cloak of mottled shreds and patches (*Bettelmantel aus buntscheckigem Stückwerk*), whereas if we wait calmly for the facts to be gleaned for a definite survey, a magnificent peplos will be woven, as though spread by Zeus over a sacred oak, as a radiantly reflected image of reality.”<sup>5</sup> One further example must suffice: “He [an individual] would perish without society, without that unifying community of spirits that, swelled by the billowing thoughts of the past, roars along in the current of history and in foaming spray surges around the barriers” (*schäumend im Gischt die Schranken umbrandet*).<sup>6</sup>

All these illustrations are taken from a single work. The cumulative effect of such diction is easily imagined. Yet matters are far worse than described. For there is no intelligible organization: ideas turn up on the principle of free association, with favorite propositions recurring at irregular intervals like the *leitmotifs* of a music drama. This is not the biased verdict of a single critic, but the general consensus of opinion; Karl von den Steinen’s obituary reference to the undammed stream of ideas (*Überquellen der Vorstellungen ohne jede notwendige Hemmung*) defines the identical impression. Add to this that Bastian rarely deigned to give an exact source reference, and the joys of consulting him will be fully appreciated.

Bastian was not unaware of the obstacles he put in his readers’ path. In rejoinder he argued that the primary duty of the day was to garner facts, that his travels often led him to places remote from libraries, that even from his meager indications experts would be able to verify his statements.

<sup>4</sup> *Ibid.*, 8.<sup>5</sup> *Ibid.*, 91.<sup>6</sup> *Ibid.*, 135.

Why, then, our insistence on qualifying the popular verdict? First of all, if this were the whole story, the nature of his influence would be an enigma. Yet he did enjoy the esteem of men like Virchow and Tylor; and for a generation his ideas loomed important in German ethnology. Let us, then, approach him from another angle, let us peep into his six-volume travel book on *Die Völker des östlichen Asien*; at once a very different Bastian appears. The narrative is straightforward, often vivacious, nay, spiced with humor. Here is a traveler sympathetic with his hosts, avid of information wherever he can get it. He watches processions of Burmese nobles with their henchmen bearing betel boxes and holding parasols, and listens to the itinerant astrologers in the market places. The same man who at home shunned the theater attends interminable native performances, persevering through their masquerading, conundrums, and ribaldry. He chuckles over the pious Buddhist fishermen who never kill their catch but merely compensate them for their submergence by drying them in the sun. Equally entertaining is the story of the would-be cook: after Bastian had painfully persuaded his parents, uncles, aunts, and cousins not to restrain the young man from accompanying him, the chef himself developed scruples: "Squirming like a worm on the ground, he protested he could never murder an innocent fowl; he would cook as many chickens as I might order, would roast or chop them into ragout, but never would he consent to slaughter poultry himself." The theological debates with the king of Burma on the right of self-defense are likewise tinged with a sense of the grotesque.<sup>7</sup>

While such items become rarer in subsequent volumes of the work, they are not lacking there. Bastian, trying to explain points of anatomy to a princely presi-

<sup>7</sup> Bastian, *Die Völker des östlichen Asien*, 2:16, 48, 53, 71, 92, 157, 504, Leipzig, 1866.

dent of the Siamese medical college, raises his arm above that noble's skull. At once this breach of etiquette is brought home to him "by the menacing growls swelling like muffled rumbling from the mouths of the crawling vassals, for in Siam there is no graver affront than to touch a superior's head." Again, there is the banquet with the minister of the exchequer at which a bigoted abbot glorifies Buddhism in stilted phraseology, taking a thinly disguised fling at the barbarian fellow guest who seeks wisdom without years of monastic seclusion. Finally, in Japan the traveler views dramatic scenes "that we could not describe either in Latin or Greek script, but which the Japanese, in the company of their wives and daughters, viewed with dispassionate equanimity."<sup>8</sup>

Stylistically, then, Bastian led a double life. It is true that a chronological factor enters, since the later writings are unquestionably more distorted than their predecessors. But this is not the whole story. In the last two volumes of the *Reisen* the clear, even, sprightly diction of the narrative contrasts sharply with the trope-laden, labyrinthine manner of the prefaces, where the author expounds his creed, stressing the danger of system-mongering and the dependence of psychology on ethnology. Bastian, then, became confused in proportion as he discussed theory; and his progressive obscurity was probably due to his increasing concern with general principles.

What are these principles? The one that most directly affected contemporary thought was unquestionably his belief in independent development. He did not, indeed, deny diffusion, but he insisted that in each case it must be proved by detailed evidence. His position was thus diametrically opposed to both Graebner's (page 158) and to Balfour's (page 28) since he contended that by a general law the psychic unity of mankind every-

<sup>8</sup> *Op. cit.*, 3:71, 82, 1867; 5:325, 1869.

where produced similar "elementary ideas" (*Elementargedanken*). Only different external stimuli would evoke different responses, whence the origin of geographical provinces. At a higher stage, contact with other cultures may supersede such physical conditions as a stimulus, whence history and cultural development proper (*Geschichte und eigentliche Kulturentwicklung*). But compared with the basic laws, these historical causes are of subordinate significance.

Bastian's faith in supposedly uniform laws of growth, in a "genetic principle" through which lower and simpler phenomena become higher and complex, shows that cultural evolutionism may very well go hand in hand with a repudiation of biological transformism. Further, notwithstanding appearances, Bastian never preached the chaotic accumulation of raw data as an ultimate goal. He clamored for the harvesting of facts because they were in danger of being irretrievably destroyed by the leveling of modern civilization. And this was deplorable because for a proper perspective science needed samples of *all* cultures, past and present.

It was Bastian's belief in a law of growth that emboldened him to herald an "applied anthropology." Insight into normal processes would stave off pathological deviations, safeguard the national exchequer, avert "the formidable miscarriages" of Anglo-Indian administration exposed by Maine, affect the weal and woe of millions. Bastian's idea thus foreshadowed the work of the Africa Institute today.

Nor should we overlook Bastian's views on psychology. Decades before Rivers (page 169), he argued that a science of mental life must take cognizance of ethnographic data, because the "individual's thinking is made possible only by his functioning in a social group."<sup>9</sup>

<sup>9</sup> Bastian, *Der Völkergedanke*. . . , 1881.



Thus, Bastian anticipated many of his successors. His gospel of saving vanishing data is Haddon's; his insistence on *proof* of assumed historical connections coincides with Boas'; like Thurnwald and Radcliffe-Brown, he postulates laws of sequence; like Malinowski he would apply anthropology to colonial government. And what are his geographical provinces but the culture areas of later research? Add his unchallenged achievement of founding a great ethnographic museum, and it becomes intelligible that he loomed as a major figure of his time.

At present we can hardly assign to him quite that distinction. Who now reads Bastian? It is one of the numerous paradoxes in his career that this untiring preacher on the need of complete data has left not a single standard monograph. Neither his early book on West Africa nor the learned tomes on *Die Kulturländer des alten Amerika* (1878) can be considered indispensable to workers in these fields. As von den Steinen put it, "he was not an ethnographer in the narrow sense of the term." He was too restless to settle down in one spot and immerse himself into the life of a particular people. For in practice his interests were not nearly so broad as in theory. At bottom what lured this scorner of metaphysics was the world-view of men in different ages and places, their conceptions of cosmology, cosmogony, and eschatology. On his Eastern Asiatic travels he observed keenly enough, but the one thing he studied systematically was Buddhism.

Theoretically, we find a host of sound and stimulating ideas with an equal dearth of definitive results. Why did he not *define* the geographical provinces of the world? Why do we constantly hear of a law of development without ever seeing the proof for its existence? How are pathological modes of growth to be distinguished from normal ones? Bastian offers no answers. To formulate a

problem, experiment in thought as to possible solutions, amass relevant material, and then logically to exclude one conclusion after another, was obviously beyond him. That is why he is a forerunner—a forerunner with a variously stirring message—but not a leader to salvation.

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# V

## COMPARATIVE LAW

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The decade following Boucher de Perthes' recognition was a period of feverish activity; and the characteristic trends, comparative and evolutionist, are nowhere better exemplified than in the studies of early law. Naturally enough, they were pursued first by historians and jurists, but these influenced anthropological thought even when they themselves made little or no use of primitive data. For example, Numa-Denys Fustel de Coulanges (1830-1889) in *La cité antique* (1864) stressed the differences between classical and later forms of jurisprudence. Thus names, he pointed out, had developed quite differently in antiquity and in medieval times. In the Middle Ages the baptismal name was the true individual name and patronymics only evolved much later, while among the Romans the patronymic was earlier and more important. The early Teutons owned individually what they harvested, but not the soil on which their crops were raised; in Greece, land was always private property. Whether these views are borne out by later specialist research or not, they gave a hint of the range of variation in legal institutions, enlarging the bounda-

ries of juridical provincialism. Fustel de Coulanges' results influenced Morgan's treatment of the clan. Moreover, we note that his position is militantly functionalist: institutions are declared unintelligible except in their context, law cannot be understood apart from religion.

#### BACHOFEN

A Swiss jurist, J. J. Bachofen (1815-1887), exerted a more powerful influence on ethnographic thought, for he was the first to throw into relief the existence of matrilineal descent. Moreover, while primarily concerned with classical antiquity, he also utilized material on primitive peoples.

Though by profession a lawyer, Bachofen was, above all, a philologist who sought to penetrate to the essential core of classical life. "Roman law," he writes, "always appeared to me as a part of ancient, especially of Roman philology, hence as a section of a larger whole, embracing the study of classical antiquity in its entirety."<sup>1</sup> Indeed, according to an admiring expositor, Bachofen's chief claim to fame rests on his magnificent vision of ancient culture as a connected unit, while his contribution to sociology is secondary and incidental. However that be, Bachofen's only work that concerns us, *Das Mutterrecht* (1861), teems with references to classical mythology and is studded with Greek and Latin quotations.

The essence of the argument is set forth in a lengthy introduction (pp. v-xxxiii). Starting from Herodotus' account of the Lycians as matrilineal, Bachofen deduces from it a coherent system of law antecedent and antithetical to the patriarchal principle of antiquity. Not only did children take their mother's name in Lycia, but

<sup>1</sup>J. J. Bachofen, *Selbstbiographie und Antrittsrede über das Mutterrecht* (ed., Alfred Baeumler), 13, 1927.

women ruled the household as well as the state. Applying the principle of survivals, the author interprets mythological references to outstanding women as relics of a one-time gynaecocracy. Further, he argues, the very rigor of Roman patriarchy implies an inimical principle that had to be combated and ousted.

Like Fustel de Coulanges, Bachofen is an aggressive functionalist: "The hegemony of maternity in the family can not be conceived as an isolated phenomenon." Hence a rule for reckoning descent is necessarily only one link in a chain of ideas. Matrilineal descent, in contrast to patrilineal, exalts the left above the right side; night above day; the moon above the sun; the youngest above the oldest child. Even the notion of general liberty and equality naturally flows from "child-bearing maternity" (*aus dem gebärenden Mutterthum*). But how could the weaker sex attain ascendancy? Bachofen answers: Through woman's aptitude for religion. Specifically, woman in the flesh represented the Earth goddess (*tellurische Urmutter*); where there is feminine dominance there is also a chthonic faith, connected with Demeter or an equivalent figure. It is a basic tenet of Bachofen's that secular gynaecocracy merely reflects the primary phenomenon, viz., the cult of a female deity.

In his chronology Bachofen is a typical evolutionist of the old school. Once more a belief in progressive stages appears independently of modern biological theory, for it is improbable that the Swiss jurist should have been affected by Darwinism when he wrote *Das Mutterrecht*. Gynaecocracy, we must note, was not the earliest social condition: it came as a reform superseding promiscuity (*Hetärismus*). Yet this antecedent stage sprang not from sheer lust but from an idea-system decreeing that woman was not created to fade in the arms of a single mate. Exclusive possession by a male was an offense against God, to be expiated by periodic ceremo-

nial prostitution. To Bachofen, then, the historic worship of Dionysus was a step backward: woman, naturally sensual, hence fascinated by a phallic system, relapsed from the severe gynaeocracy of the Amazons into the lowest form of tellurism, a fleshly venery.

In principle, however, Bachofen postulates universal stages: Primeval promiscuity was followed by a revolt of woman, who craved delivery from such humiliation. The result was Amazonian assertiveness. Once in the saddle, however, women devoted themselves more and more to peaceable pursuits, among other things inventing agriculture. At this point, it is assumed, development diverged in detail; in some regions women lost their superiority in domestic affairs, elsewhere they yielded political power. Thus, paternity, a higher principle, came to triumph, ushering in not merely a social change but a revolutionary world-view: the celestial Apollonian idea gained the victory over tellurism; the right side came to outrank the left, day conquered night, spirit subjected matter.

Though our author rests his case preponderantly on classical data, he culls parallels from primitive areas, such as African instances of feminine prestige (Balunda) and Negro as well as American cases of inheritance by a sister's son. Here again is an illustration of his functionalism; for nepotic inheritance can be interpreted as a vestige of pristine gynaeocracy only on the assumption that a trait must fit into a logically linked setting. Bachofen also treats a phenomenon that looms large in subsequent discussion, the "couvade," i.e., the custom of the *father's* taking to his bed when his wife is delivered of a child. Bachofen foreshadows some later interpreters by suggesting that the husband must be made to appear as a fictitious second mother in order to lay claim to his offspring.<sup>2</sup>

<sup>2</sup> J. J. Bachofen, *Das Mutterrecht*, 105-110, 255 f., Stuttgart, 1861.

At first blush, a bald outline of Bachofen's scheme may provoke a wholly negative reaction. Much of it is fantastic, the whole of it shot through with more than a flavor of mysticism. Specifically, he thoroughly confused the phenomenon of matrilineal descent with a matriarchate, which has never been authentically demonstrated as its concomitant. Nevertheless, something can be said on the positive side. However specialists of today may appraise his classical studies, Bachofen applied to this field ideas which modern ethnography accepts as axioms for the investigation of alien societies. Like Fustel de Coulanges, he is emphatically a functionalist in rejecting the study of a single aspect of civilization and especially in connecting social structure with religious practice. Further, Bachofen, while full of emotional evaluations, was at least willing to contemplate all the facts of Greek culture—including its crudities—which the classicist guild of his day ignored from aesthetic snobbishness. Again, he repudiated interpretation of myths in terms of individual psychology, because they were the product of an unconscious social activity, the *Volksgeist*. Whatever we may think of this conception, its critical half forestalls error, and its positive side at least harmonizes with some modern trends.<sup>3</sup>

To sum up, Bachofen was the first to direct attention to matrilineal descent as a problem; and he combined this service with some valid general principles for approaching alien cultures.

#### M C L E N N A N

Independently of Bachofen, though somewhat later, J. F. McLennan (1827-1881), discovered the importance

<sup>3</sup> Alfred Baeumler, in *Der Mythos von Orient und Occident, eine Metaphysik der alten Welt; aus den Werken von J. J. Bachofen mit einer Einleitung von Alfred Baeumler herausgegeben von Manfred Schroeter*, celix-eclxxx sq., München, 1926.

of matrilineal descent; and though a chasm separated the combative, logical Scotch lawyer from the mystical Bâle patrician, several of their speculations happen to coincide.

Like so many of his contemporaries, McLennan was essentially a parallelist. "All the races of men have had, to speak broadly, a development from savagery of the same general character."<sup>4</sup> Consistently with this, he stresses survivals, in fact, his interest in them prompted his first essay on *Primitive Marriage* (1865), subsequently republished in the *Studies in Ancient History*: "wherever we observe symbolical forms, we are justified in inferring that in the past life of the people employing them, there were corresponding realities."<sup>5</sup>

In particular it was the ritual of bride-capture that stimulated McLennan. In the same year Tylor briefly listed instances of sham bride-kidnapping, summarizing the facts in these words: "In these cases the abduction is a mere pretence, but it is kept up seemingly as a relic of a ruder time when, as among the modern Australians, it was done by no means as a matter of form, but in grim earnest."<sup>6</sup> Like McLennan, in other words, Tylor assumed *systematic* wife-capture; but what remained an incidental remark of Tylor's became the very keystone of McLennan's thinking.

As a purely logical construct that scheme inspires respect. McLennan's was a vigorous intellect, eager to read meaning into the oddments of available ethnographic literature and quite unimpressed by authority. Unfortunately his independence was coupled with a pugnacity that barred fairness to opponents. Worse than that, he loved to practice dialectics *in vacuo*, to speculate about conditions not only unknown but unknowable.

<sup>4</sup> J. F. McLennan, *Studies in Ancient History*, 301, London, 1886.

<sup>5</sup> *Ibid.*, 5.

<sup>6</sup> E. B. Tylor, *Researches into the Early History of Mankind*, 284, London, 1865.



Two terms coined by McLennan have become common scientific property. "Endogamy" defines a condition in which "members of a family or tribe are forbidden to intermarry with members of other families or tribes" (p. 24); "exogamy" designates "prohibited marriage within the tribe" (p. 27). According to the scheme, early tribes were exogamous and chronically at war with one another. Thus, they could get wives only "by theft or force." Later, when intertribal relations grew more amicable, abduction ceased to be necessary and was preserved as a mere symbol of past reality.

At this point we must ask what is to be understood by a "tribe." In a spirited rejoinder to McLennan's attack on *Systems of Consanguinity and Affinity*, Morgan (page 62) insisted that such a thing as tribal exogamy did not exist; it was merely the clan ("gens"), a tribal subdivision, that was exogamous.<sup>7</sup> This certainly holds for most American cases, but McLennan's error is less crass than it at first sight appears. When he treats of Australians, for instance, he envisages their exogamous tribes as "exceedingly numerous and exceedingly small, being a species of family groups" (p. 41). In other words, he foreshadows Radcliffe-Brown's "hordes" even though he does not know that these units are patrilocal and patrilineal. Likewise, when hypothetically picturing Scotch developments, McLennan conceives the primeval tribe as a localized clan on the Australian pattern, while the equivalent of Iroquois organization is considered a later type, due to "the interfusion of clans" but with exogamy persisting for persons sharing the horde name (pp. 56-58, 77 f.). McLennan fails to prove the alleged sequence and to discriminate clearly between the local exogamy of Australia and the clan exogamy of the Iroquois; but apart from this, his position is intelligible and even logical.

<sup>7</sup> Lewis H. Morgan, *Ancient Society*, Note to Part III, New York, 1877.

Later research has supported McLennan's claim that exogamy is a widely prevalent principle of law. On the other hand, there is nothing to show that it regularly accompanies bride-abduction. His own data do not bear out the correlation: for America he cites capture among natives of the Orinoco and Amazon, while his exogamous instances are from the Eastern United States and the Chaco. His logic errs in another argument. Granting universal hostility and local exogamy, bride-capture would indeed become a necessity; but McLennan argues backward that wherever there is abduction, real or symbolic, exogamy also must have been present if it is no longer there. This does not follow, because conceivably bride-kidnapping might be due to an alternative cause.

For McLennan exogamy was not an ultimate datum, but resulted from a general practice of female infanticide. Rejecting the idea that an instinct prevented early man from mating with his kin, he substitutes social causes for this biological factor. Tribes surrounded by enemies would find girls a source of weakness, whence wholesale destruction of female infants. The entire course of evolution, then, assumes the following shape. In the beginning there were promiscuous hordes, which killed off the majority of baby girls so that males came to preponderate. However, promiscuity came to be mitigated by arrangements whereby a small set of men attached themselves to a particular woman. This is "archaic" polyandry, wherein the male partners of a woman bore no necessary relation to one another. It evolved into the "fraternal" variant when only sons of the same *mother* shared a wife. This ushers in another cardinal feature of the system, viz., that under primeval conditions kinship could be reckoned only through the mother because paternity was uncertain.

While not yet introducing the terms "matrilocal" and "patrilocal," McLennan utilizes the concepts they

represent. Under archaic polyandry the wife lives with her mother and brothers, her children being born in this matrilineal household. Later she has a house of her own, where her husbands visit her according to fixed rules, the offspring remaining affiliated with her kin. In the subsequent stage of fraternal polyandry, woman is detached from her own family and passes into her husbands', where her children are born and henceforth belong. To use modern parlance, patrilineal descent results from patrilocal residence with fraternal polyandry.

The levirate—that widespread custom by which a brother inherits a widow—is conceived by McLennan as a natural part of the polyandrous scheme and as a significant survival of it. Only, he avers, where there is or has been polyandry do brothers succeed in preference to sons. Similarly, the American practice of calling a paternal uncle by the same term as the father, as well as correlated ways of classing kindred “bear the stamp of a polyandrous origin.” Our author firmly believed in a strong bond uniting the several traits that especially interested him. Thus, he infers that where there is polyandry, there must once have been a rule of “kinship through females only”; that all exogamous races must have been originally polyandrous, hence must once have “recognized blood-ties through mothers only.” Here is another pioneer with a marked functionalist bias and, logically enough, with a proclivity for the survival argument.

McLennan must also be noted as one of the very first to see totemism in broader perspective. In a series of papers he endeavored not to explain the origin of totemism but to prove that the civilized nations of antiquity had all passed through a totemic stage, which was thus conceived as a universal step in human civilization. Consistently with his general position he would not accept the view that totems were mere emblems but contended

that here, too, a reality must have preceded the symbolism. In other words, he held that there was an ancient worship of animals and plants which subsequently evolved into higher cults but sporadically left relics in the form of emblems. The misconceptions in these essays are profound. Apart from the parallelist faith in universal stages, we note the erroneous idea that totemism generally implies worship and the refusal to ascribe to early man any belief in benign beings apart from the totemic cults. The confusion of totemism with animal worship, while pardonable at the time, led many others astray and had to be dispelled three decades later in a masterly paper by Tylor.<sup>8</sup>

Why was this vigorous intellect so frequently misled in its conclusions? The explanation is simple: they lacked the support of facts; and their author was too enamored with his reasoning to entertain rival hypotheses. Empirical data do not bear out either the universal enmity of primitive groups or the prevalence of female infanticide. Nor, as Morgan rightly contended in the rejoinder already cited, is there any reason to suppose that polyandry—decidedly rare as a fixed institution—ever characterized an epoch of human development. This critic likewise correctly repudiated McLennan's notion of "kinship through females only," seeing that primitive tribes regularly designate relatives on both sides by specific terms irrespective of whether the rule of descent is matrilineal or the reverse. As for bride-capture, Thurnwald admirably summarizes the essential facts about this "scientific myth." A new status in the life cycle evokes a social demand for some change in conduct, whence a ritual expression of the stage reached. A transitional state brings with it mental inhibitions, which may be

<sup>8</sup> J. F. McLennan, "The Worship of Animals and Plants," *The Fortnightly Review*, 6: 407-427, 562-582, 1869; *Ibid.*, 7:194-216, 1870. E. B. Tylor, "Remarks on Totemism with Especial Reference to Some Modern Theories Respecting it," *JRAI* 1:138-148, 1899.

dramatized, as a result of which we find McLennan's symbol of capture. As E. C. Parsons has shown, there are instances of reluctance on the part of the groom, not the bride; and more recently Thurnwald himself records groom-abduction in New Guinea.<sup>9</sup> To concentrate on bride-kidnapping as *the* phenomenon to be studied was to emphasize unduly one extreme variant of the natural context requiring interpretation.

Notwithstanding these strictures, McLennan's place in the history of ethnology remains important. He added to the concepts and nomenclature of nascent comparative jurisprudence; his speculations, even when sterile from want of material, stimulated his contemporaries by the vigor with which they were set forth; and his trenchant criticisms, however unfair in part, sometimes hit the mark.

#### M A I N E

In 1861—the year of *Das Mutterrecht*—an English author published the book that above all others laid the foundation of comparative jurisprudence, *Ancient Law*. Henry James Sumner Maine (1822-1888) was Regius Professor of Law at Cambridge, and went to India as Legal Member of the Supreme Council of the Governor-General in 1862, afterwards becoming Vice-Chancellor of the University of Calcutta. Returning to England in 1869, he became Corpus Professor of Jurisprudence at Oxford and later (1887) Whewell Professor of International Law at Cambridge. His principal works, besides that already cited, are: *Village Communities in the East and West* (1871); *Early History of Institutions* (1875); and *On Early Law and Custom* (1883).<sup>10</sup>

<sup>9</sup> Richard Thurnwald, *Die menschliche Gesellschaft*, 2:104 sq., Berlin u. Leipzig, 1932. Elsie Clews Parsons, "Holding Back in Crisis Ceremonialism," AA 18:44 f., 1916.

<sup>10</sup> M. E. Grant Duff, *Sir Henry Maine: A Brief Memoir of His Life*, London, 1892.

Maine enlarged the boundaries of traditional jurisprudence by comparing Roman law and modern Western systems with those of India and Eastern Europe. Unlike McLennan and Morgan, he dealt only incidentally with primitive usage so that his predominantly legal training puts much of his discussion beyond the ethnologist's competence. In *Ancient Law* the reader constantly faces the unfamiliar problems of traditional jurisprudence. The very terminology is forbidding, the facts lie beyond his scope. He is unable to judge conclusions on the process of feudalization, Salic law, or Brehon usage. Nevertheless, what he does carry away is part of his most valuable intellectual equipment.

While McLennan impresses us by the bumptious vigor of his intellect, Maine is the embodiment of serene wisdom coupled with unusual subtlety. He thus achieved a series of concepts which turned out to hold valid for primitive law no less than for the Indo-European peoples whose legal development was his primary field of study. Among the most important of his ideas is the antithesis of the blood-tie and the territorial bond, a distinction adopted by Morgan and of great significance for subsequent ethnological thinking. Maine—and Morgan after him—erred in denying that early and primitive peoples ever united on the basis of territorial grouping, but certainly kinship bonds are much more potent on that level than those of mere contiguity. What is more, the conceptual distinction of the two types of social solidarity remains valid and illuminating. Maine further brought out the contrast of tort and crime; of status and contract; of the inalienable real estate of early times and the merchantable land of higher civilizations; of archaic formalism and modern emphasis on substance. Every one of these distinctions is definitely applicable to ethnographic data and brings enlightenment. The same holds

for the principle of legal fiction, which Maine also treated at length.

What astonishes us, above all, in Maine is his thoroughgoing historical-mindedness. At a time when Lubbock was forever undergoing emotional spasms over "revolting" primitive customs, Maine wrote: "It is not the business of the scientific historical enquirer to assert good or evil of any particular institution. He deals with existence and development, not with its expediency." Neither a mystic like Bachofen nor a romanticist like Rousseau, Maine, through the incorruptible medium of his common sense, sees things as they are and were: "Although there is much in common between the Present and the Past, there is never so much in common as to make life tolerable to the men of the Present, if they could step back into the Past."<sup>11</sup>

Maine was a true historian in another sense. McLellan speculated about the unknowable social conditions of a remote past and traced a generalized scheme of development; and we shall find in Morgan's work an even more elaborate system of this category. Maine may occasionally drop a word of homage to "continuous sequence, inflexible order, and eternal law in history," but this sop to regnant fashion agrees neither with his practice nor with his philosophy. "So far as I am aware," he writes elsewhere, "there is nothing in the recorded history of society to justify the belief, that, during that vast chapter of its growth which is wholly unwritten, the same transformations of social constitution succeeded one another everywhere, uniformly if not simultaneously." He thus explicitly rejects the idea "that human society went everywhere through the same series of changes." He is avowedly interested in "the real, as opposed to the imaginary, or the arbitrarily as-

<sup>11</sup> *Village Communities*, 3d ed., 230, 289 f., London, 1876.

sumed, history of the institutions of civilised man.”<sup>12</sup>

In a period of evolutionary schematism such a position proved unbelievable. Because Maine dealt with early Indo-Germanic law he naturally stressed patriarchal features; as a result he was attacked for championing the theory of universal development from a patriarchal stage. Yet he explicitly repudiated this view along with McLennan's and Morgan's because both are “open to considerable objection as universal theories of the genesis of society.” This is what he really said: “There are unquestionably many assemblages of savage men so devoid of some of the characteristic features of Patriarchalism that it seems a gratuitous hypothesis to assume that they had passed through it.” Also: “There has been room . . . for many courses of modification and development, each proceeding within its own area.” Finally, since he knew actual history, Maine recognized the force of diffusion: the Belgian constitution, he pointed out, is not an independent parallel of the British but was copied from one of its copies.<sup>13</sup>

As already explained, Maine made only sparing use of ethnographic data and was doubtless imperfectly acquainted even with those available in his day. He exaggerated the power of sexual jealousy as a psychosociological constant—a factor real enough in savagery but often quite differently manifested there. But as a rule Maine's superb insight enabled him to see clearly even where he was not particularly conversant with the material. He showed that exogamy and endogamy were not at all mutually exclusive except with reference to the same unit; and he saw through the flimsiness of much so-called evidence from primitive societies. Thus, he rejected the cock-and-bull stories about the immorality of the Andaman Islanders and was triumphantly supported

<sup>12</sup> *Ibid.*, 266; *On Early Law and Custom*, 219, 192, 201, London, 1890.

<sup>13</sup> *Ibid.*, pp. 204, 218, 285.



by later research. His closing words on that subject should still be the field investigator's motto: "There is no subject on which it is harder to obtain trustworthy information than the relations of the sexes in communities very unlike that to which the inquirer belongs. The statements made to him are apt to be affected by two very powerful feelings—the sense of shame and the sense of the ludicrous—and he himself nearly always sees the facts stated in a wrong perspective. Almost innumerable delusions are current in England as to the social condition in regard to this subject of a country so near to us in situation and civilisation as France."<sup>14</sup>

Finally, like Fustel de Coulanges, Bachofen, and McLennan, Maine was a functionalist in treating phenomena not as discrete but as interrelated. He recognized the contacts of archaic law with religion and morality, and pointed out the military weakness inherent in a system of matrilocal residence. Among the Southern Slavs, he suggests, the assertion of individual property rights, at first exceptional, sapped the whole scheme of the house community. In India, again, the establishment of local courts *unintentionally* altered Indian law because legal rights, obligations, penalties, and political superiority are all interwoven.<sup>15</sup>

A picture of the pioneers of comparative law would be incomplete without Lewis H. Morgan; but his influence has been so potent that it warrants separate treatment.

<sup>14</sup> *Ibid.*, 215 sq., 222, 278, 231.

<sup>15</sup> *Ibid.*, 288, 253; *Village Communities*, 67-76.

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# VII

LEWIS H. MORGAN

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By a freak of fortune Lewis H. Morgan, whose main original contribution to ethnology lies in its most aridly technical field—kinship terminologies—has achieved the widest international celebrity of all anthropologists. Naturally this is not due to his solid achievements, but to a historical accident: his *Ancient Society* (1877) attracted the notice of Marx and Engels, who accepted and popularized its evolutionary doctrines as being in harmony with their own philosophy. As a result it was promptly translated into various European tongues, and German workingmen would sometimes reveal an uncanny familiarity with the Hawaiian and Iroquois mode of designating kin, matters not obviously connected with a proletarian revolution. Even in America Morgan's book has long been most readily accessible in the inexpensive reprint issued by a Socialist firm in Chicago. In the immediate past the bourgeois lawyer, who never severed his connections with Christian orthodoxy, has been officially recanonized by the present Russian regime. Its spokesmen declare his work "of paramount importance for the materialistic analysis of primitive com-

munism"; its Academy of Sciences has published a translation of *Ancient Society* for its "Classics of Scientific Thought"; and there are rumors of veritable pilgrimages to Rochester, New York, as the focus of Morgan's new dispensation. Neither falling under the hypnosis of such hero-worship nor recoiling from its quaint manifestations into the opposite extreme, we shall endeavor to see Morgan against the background of his time.<sup>1</sup>

Like McLennan and Maine, Lewis H. Morgan (1818-1881) was a lawyer, but unlike them he was not merely a philosopher of institutions but also a first-hand observer of aboriginal custom. His *League of the Ho-de-no-sau-nee or Iroquois* (1851) is one of the best earlier descriptive reports of Indians; and his brief visits to other tribes produced much valuable material on social organization. This, indeed, was the topic that especially attracted him in the field or library, while he unaccountably neglected other aspects of culture. In the opening chapter of *Ancient Society*, for instance, he dismisses supernaturalism with the dictum that "all primitive religions are grotesque and to some extent unintelligible." However, he did not ignore Iroquois technology, to which indeed he paid considerable attention. We must also credit him with a sympathetic attitude towards the Indian, such as was not by any means general at the time. Theoretically, Morgan's direct contacts with tribes east of the Rocky Mountains were not an unmixed blessing, for they inclined him to see all American Indians in the image of those he knew, i.e., with uniformly democratic government and organization. Had he begun his studies among the Eskimo or Paiute, his general views might have been different.

<sup>1</sup> Bernhard J. Stern, *Lewis Henry Morgan, Social Evolutionist*, Chicago, 1931; also, *idem*, "Selections from the Letters of Lorimer Fison and A. W. Howitt to Lewis Henry Morgan," AA, 32:257-279, 419-453, 1930. ✓✓

What, then, were these views? His aim was to furnish a complete scheme of institutional progress, with special attention to marriage, kinship, government, and property. It resembled the systems of his contemporaries in being evolutionary and avowedly opposed to the theory of degeneration as applied to savages. But it was laid out on a vaster scale, being much more inclusive than Maine's in dealing with humanity at all periods and places, while the facts, in no small measure first noted by Morgan himself, were better authenticated than McLennan's. What is more, though Morgan intensively studied only relatively few aspects of group life, he defined these in relation to the rest. He divided all history into three main stages—Savagery, Barbarism, and Civilization—and correlated each with economic and intellectual achievements. Savagery was the period before pottery; Barbarism, the ceramic age; Civilization began with writing. The first two major periods were subdivided each into a lower, middle, and upper status, each provided with its signpost. Thus, the upper status of Savagery is heralded by the use of bow and arrow; the upper status of Barbarism, by iron tools. So far the periods and sub-periods are defined by single traits. But actually Morgan characterized them by a whole series of features, so that major inventions appeared as the correlates of such and such economic activities, social customs, and political institutions. Thus he offers a comprehensive scheme of cultural wholes far beyond anything attempted even by the systematic McLennan. Morgan's *Ancient Society* was a synthesis of sociological material that for the first time brought together material on Australian and American natives, on ancient Greece and Rome; and all this in an orderly arrangement prescribed by an evolutionary doctrine. No wonder the book was hailed in many quarters as a classic.

In judging this landmark we must recollect that in

1877 immense areas of the globe were imperfectly described. Yet the gaps in Morgan's knowledge about facts thoroughly determined in his time are amazing; and, as usual, it is largely ignorance that accounts for glaring errors in theory. Along certain chosen lines he was incomparably ahead of his period; in others he neglected data that should have been at his fingertips. His treatment of the Polynesians is inconceivable. Though Captain Cook's observations must have been accessible, Morgan puts this horticultural, sophisticated people in the same class with the Australian hunters. Lacking bow and arrow, both races are degraded to the middle status of Savagery—below the level of the rudest North Americans. This is taking a classificatory device far too seriously! Even so, it is hard to understand how Morgan could have missed the social strata of the caste-ridden Oceanians. Similarly, enough about Negro societies was available to suggest the ever-recurring establishment of autoocracy on the Dark Continent. Nevertheless, Morgan, seeing primitive mankind in Iroquois terms, denied that monarchy could appear prior to Civilization, i.e., before literacy: monarchy and a clan system, he averred, were incompatible. Aristocracy, again, could not evolve before the later period of Barbarism, i.e. the Iron Age. This dogmatism happened to yield a valuable by-product—the scrutiny of Spanish chronicles with their extravagant descriptions of an Aztec empire. But the total picture of ancient society that resulted was curiously distorted. It not only did violence to African and Polynesian facts, but misrepresented in part even the American aborigines since slavery and class stratification were thoroughly established for British Columbia.

Morgan's discussion of Polynesia at last reveals one pioneer who was not a functionalist at heart, who was willing to ferret out the minutiae of Oceanian kinship

systems although he apparently remained in utter darkness as to other facts of their society.

One other important factual deficiency must be noted. Morgan, who had himself described fraternities among the Iroquois, found no place in his system for clubs or any other primitive associations based on voluntary affiliation. Yet on the American Plains the "Dog Soldiers" were widespread in his day and several decades before had been accurately described in a famous travel book. While according to Morgan the only important unit in the career of a Mandan or Hidatsa would be the clan, he could have learned from Prince Maximilian of Wied-Neuwied that the military organizations of these Indians largely dominated social life. Their police activities, indeed, bore directly on that "Idea of Government" to which fifteen chapters are devoted in *Ancient Society*. That Morgan omitted Melanesian and African secret societies is more intelligible, but his neglect of well-known and by no means obsolete phenomena in the United States is hard to understand. The skew vision of primitive life imposed on ethnologists by Morgan's disregard of associations was not corrected until Heinrich Schurtz's *Altersklassen und Männerbünde* (p. 99).

In his general approach to culture history, Morgan was a typical evolutionist firmly convinced that the highest known, i.e. mid-Victorian, condition of society had been gradually achieved through a series of stages starting with the very antithesis of the glorious present. In other words, believing in a law of progress, he failed to maintain the detachment prescribed by Maine and gave ethical appraisal to the facts he encountered. Essentially, Morgan held conditions of society to be "substantially similar" at any particular stage of development. Insofar as he recognized differences he traced them to diversity of physical environment, a point thus shared with Bastian. Specifically, he often mentioned "the unequal

endowments of the two hemispheres" to account for the lack of Old World elements, such as livestock in America. Yet even in the Old World the very same domestic beasts were quite differently utilized by Egyptians, Chinese, and Turks, so that "equal endowments" may evoke unlike responses.

Occasional qualms were not lacking. "The phrase 'similar conditions of society,' which has become technical," he writes, "is at least extremely vague. It is by no means easy to conceive of two peoples in disconnected areas, living in conditions precisely similar."<sup>2</sup> Nevertheless, as a rule he adhered to the principle that "the experience of mankind has run in nearly uniform channels." And, so believing, he freely extrapolated where authentic information was wanting; although admittedly he had no documentary proof of female descent in Greek and Latin societies, the supposedly universal law that any patrilineal organization necessarily grows out of a matrilineal one made him infer that Hellas and Rome must once have been matrilineal. In other words, Morgan credited himself with possessing a generally valid scheme of sequence by which unknown events could be safely deduced. This is the point at which the historians of culture level their critical batteries: culture, they declare, is far too complex to be reduced to chronological formulae; its development is mainly divergent, not parallel.<sup>3</sup>

Morgan was not unduly disturbed by cultural loans, though he freely admits them. The ancient Britons had iron tools, hence should be assigned to the upper status of Barbarism. But because their social organization is rude, they are put into the *middle* status: "The vicinity of more advanced continental tribes had advanced the

<sup>2</sup> *Systems of Consanguinity and Affinity*, 472 f., Washington, 1871.

<sup>3</sup> B. Laufer, *Dokumente der indischen Kunst; erstes Heft: Malerei, das Citralalshana, nach dem tibetischen Tanjur herausgegeben u. übersetzt*, 32, 192, Leipzig, 1913.

arts of life among them far beyond the state of development of their domestic institutions." As Stern has shown, this exposes the weakness of the whole scheme. We can logically grade either by chosen "inventions or discoveries" or by sociological criteria, but we cannot propound a system on one basis and at our convenience shift its basis later on. If iron can be borrowed, so can other "tests of progress." The proximity of archers would account for the shooting of arrows by Northern Canadians, who on general grounds might well be degraded to the middle status of Savagery; and contacts with potters explain earthenware among the various simple peoples. In short, diffusion plays havoc with any universal law of sequence. This difficulty, however, Morgan does not face.

However, Pitt-Rivers' case has shown us that derivation from a single focus and a theory of evolution are not mutually exclusive. Actually, in two major problems Morgan's general parallelism was superseded by its exact antithesis. He regarded the clan as practically universal at a given stage, but the principle of reckoning certain blood-relatives as kin and excluding others seemed so abstruse that he postulated a single origin. To explain the spread of the concept he fell back on natural selection: the rule of marrying outside one's clan would be so advantageous as to "propagate itself over immense areas through the superior powers of an improved stock thus created."<sup>4</sup>

On the subject of relationship terminology Morgan was still more extreme. A system of designating kindred, he argued, cannot be borrowed; it can merely be spread by migration. The Hawaiians and Zulu classified certain relatives in the same way, hence Polynesians and Kaffir must spring from the same stock. The Tamil of India and the Iroquois of New York share the same system, hence:

<sup>4</sup> *Ancient Society*, Part II, Chap. 15.



“When the discoverers of the New World bestowed upon its inhabitants the name of *Indians*, under the impression that they had reached the Indies, they little suspected that children of the same original family, although upon a different continent, stood before them. By a singular coincidence error was truth.”<sup>5</sup>

This was, of course, mystical nonsense that could be readily refuted from Morgan’s own schedules. Lubbock at once demolished it. Not only the Tamil but also the Fijians and Australians resemble the Iroquois in their kinship categories. Are the last, then, specifically allied in race with all these biological groups? Worse still, the several Iroquois systems given by Morgan are far from uniform, one of them being closer to the Polynesian than to the Seneca type. In other words, some Iroquois, on Morgan’s contention, would have to be classed as racially Polynesian!<sup>6</sup>

The instance is enlightening because here Morgan was dealing with material he knew better than did any of his contemporaries. His amazing inferences illustrate his deficiency in historical sensitiveness.

Notwithstanding such lapses, Morgan’s influence on comparative sociology was not only tremendous but in many respects beneficial. We must simply discriminate between his services and his blunders, his original achievements and what he took from others. Various views commonly associated with him were in no sense peculiarly his. It was Bachofen who first proclaimed the priority of matrilineal descent, and Morgan simply joined the chorus of McLennan, Lubbock, and Tylor. Similarly, the idea that rude savages could not live in individual wedlock was contemporary patter. Again, primitive communism was part of the scientific credo of

<sup>5</sup> *Systems*, 508.

<sup>6</sup> Lord Avebury, *The Origin of Civilization*, 6th ed., 179, 1911.

the period.<sup>7</sup> But Morgan rendered a real service in giving early currency to Maine's distinction between territorial and kinship units; he helped clarify the concept of exogamy (page 45); and he repelled McLennan's notion of polyandry as a generally significant social phenomenon. What is more, he was the first and, unfortunately, the last to summarize in scholarly manner the North American data on clan organization, material in large measure secured in the field by himself or through personal correspondence.

However, Morgan's unique distinction lies in literally creating the study of kinship systems as a branch of comparative sociology. His fame, we may confidently predict, will ultimately rest on his *Systems of Consanguinity and Affinity* (1871). As the most active reviver of interest in the subject has written, "it was he who collected the vast mass of material by which the essential characters of the [classificatory] system were demonstrated, and it was he who was the first to recognize the great theoretical importance of his new discovery."<sup>8</sup> Indeed, Morgan did more, for he gathered not merely samples of the type Rivers here mentions, but described as adequately as he could all the kinship nomenclatures of the world, considerably over a hundred in all. Any ethnographer who has tried to secure even a single complete terminology of relationship from natives can appreciate what this means. Morgan collected a large number of systems by direct inquiry in the field; for others he enlisted the aid of missionaries, traders, and consular agents, to whom he gave the requisite instructions. In this part of his work the factual deficiencies are wholly excusable. South American information along this line was buried in recondite sources only quite recently made available; African relationship

<sup>7</sup> See Lord Avebury, *op. cit.*, 103, 478.

<sup>8</sup> W. H. R. Rivers, *Kinship and Social Organisation*, 4 f., London, 1914.

terms had never been methodically recorded. Morgan spared no pains, and his achievement was colossal.

A mere compilation of raw data, however, can never confer the title of greatness; and of course Morgan would never have undertaken the task unless spurred on by the hope of deeper insight. Here discrimination is again necessary. We have seen that his interpretation of the schedules in racial terms was not merely wrong but absurd. However, immersing himself for years in the welter of fact, he came to sort out his data so as to furnish a solid basis for important historical conclusions even though they were quite different from those he primarily had in view. For example, he recognized the criteria of what is now known as the "Omaha" system and indicated its occurrence among the Algonkian as well as the Siouan family. He was equally successful in defining the criteria of the "Crow" type and in tracing them among at least three distinct linguistic stocks—a determination fraught with significance irrespective of his failure to make the most of it. Still more suggestive was the similarity of American with Old World features, though a racial explanation never was so much as debatable. When a long series of relationship classes found in New York State turns out to be almost exactly duplicated in Southern India, such coincidence clamors for an explanation. By bringing under a common category all comparable phenomena the world over, Morgan advanced the typological and the distributional aspects of the subject; and in exploiting the resemblances for his own purposes, he at least drew attention to a very real problem.

His two basic categories, the Classificatory and the Descriptive, have long since proved inadequate, but here again the fault was partly due to the unavailability of information from certain crucial areas. Broadly speaking, Morgan regarded most European systems as "de-

scriptive" and those of primitive peoples as "classificatory," i.e., they grouped together many relatives of a certain type under a common *class* term. Typically, a primitive man would apply the same word to his father, his father's brothers, and at least certain of his father's cousins; and the term for "son," "mother," and so on, had correspondingly wide extension. We now know that in America by itself many tribes unknown to Morgan are as definitely nonclassificatory as the English or French.<sup>9</sup> With the pertinent anomalies among the Eskimo Morgan wrestled manfully, though in some bewilderment, recognizing the differences from his standard American types, yet unable, for want of enough comparable material, to see the variants in proper perspective. On the other hand, Kroeber has rightly urged that Indo-European languages are not lacking in classificatory terms, such as "cousin." As to "descriptive," Rivers pointed out that while the Scandinavian *farbror* for paternal uncle *describes* the relationship, this does not hold for the French *oncle*, English *uncle*, and so on. Such terms merely denote the relationships in question without describing them; and even if Morgan meant his rubric for the hypothetical proto-Indo-European terminologies, this would leave virtually all modern European nomenclatures without a place in his scheme.

A still more vital objection may be made. "Descriptive" relates to a technique for defining relationship, "classificatory" to a mode of grouping. A descriptive term like *farbror* might very well be extended to a large class of the father's kinsmen. The two basic concepts of Morgan's scheme are thus not complementary, but belong to different logical universes.

In justice to Morgan we must add that even today a

<sup>9</sup> Leslie Spier, "The Distribution of Kinship Systems in North America," *University of Washington Publications in Anthropology*, 1:69-88, Seattle, 1925. Ronald L. Olson, "The Quinault Indians," *ibid.*, 6:91, 1936.

satisfactory all-embracing classification of kinship terminologies has not yet been achieved. It does seem certain, however, that a particular terminology is rarely, if ever, a unified logical system. As Kroeber has shown, it is rather the result of several mutually intersecting principles; and it is these that must be determined before a complete typology is possible.<sup>10</sup>

Besides the historical explanation in racial terms, Morgan advanced a sociological interpretation of far greater significance. In an original manner he used his systems to prop up the evolutionary series, by no means peculiar to him in essence, that led from promiscuity to compulsory monogamy. His simplest set of terms came from Polynesia; hence he assumed it to be the most ancient, a conclusion generally held untenable now since in classification simplicity often comes late. He further noted that these Oceanians used the same word for a father and a mother's brother. This, Morgan argued, implied that at one time a man mated with his own sister, since on that assumption the children would have no reason to distinguish their maternal uncle from their father. In other words, the classification of kin survives from a period in which the closest blood-kin regularly cohabited.

The fatal error here lies in reading an unwarranted meaning into the facts reported. For what the schedules tell us is not that the uncle is called "begetter," but that the uncles and the begetter are all designated by a single term which, strictly speaking, has no European equivalent. The custom postulated by Morgan would indeed logically produce the given terminology; but there are other possible explanations. For example, the Polyne-

<sup>10</sup> A. L. Kroeber, "Classificatory Systems of Relationship," *JRAL*, 39: 81, 1909. On the problem of classifying the systems, cf. Bernard W. Aginsky, "Kinship Systems and the Forms of Marriage," *AAA-M*, 45:1-102, 1935; also Kingsley Davis and W. Lloyd Warner, "Structural Analysis of Kinship," *AA* 39:291 sq., 1937.

sians simply may have used one term for all kin of equal status with reference to the speaker.

Nevertheless, a splendid and fruitful idea remains. Though the special interpretations of terminological features offered by Morgan are erroneous, his principle that they are correlated with social factors of some sort, with forms of marriage and rules of descent, is largely true. It has already led to important discoveries by Tylor, Rivers, Radcliffe-Brown, and others; it holds out promise of the most exact interpretative findings in the range of cultural anthropology.

Morgan's *Systems*, then, blazed the way for two avenues of research: distributional studies of kinship, which automatically merge in important historical problems; and inquiries into the organic nexus between terminologies and associated usages.

A total judgment of Morgan hinges on one's attitude towards all-embracing systems. Men who are willing to hail Herbert Spencer as a greater Newton inevitably gaze upon *Ancient Society* as an unrivaled synthesis. Minds that discriminate what is achieved from what is attempted must dissent. Recognizing the value of the book at the time of its appearance, they cannot overlook its strange neglect of vital sociological phenomena and the consequent distortion of the picture offered. They see many of its views as the product not of the author but of his generation. *Their* abiding faith in Morgan's greatness rests on the *Systems of Consanguinity and Affinity*. Imperfect in form and matter, like everything from Morgan's pen, it remains a towering monument. The case invites comparison with another evolutionist. Lubbock's versatile mind ranged over the whole field of civilization; he thought much and independently, but rarely with concentration. Outside of prehistory he has left little mark; though he gives considerable space to the growth of supernaturalism, for

example, a historian of comparative religion might easily leave him unread. Morgan expressed no enlightening ideas on art, language, or religion; but he can never be ignored by the student of kinship. His was not a flashy intellect, but one of unusual honesty, depth, and tenacity; and prolonged groping rewarded his real, if drab, intelligence with glimpses of true insight. There is no better illustration of Darwin's saying, "It's *dogged* does it."

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# VIII

EDWARD B. TYLOR

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In the heroic period of modern British civilization no one represented ethnology more worthily before fellow scientists and the laity than did Edward Burnett Tylor (1832-1917). He was the peer and comrade in arms of Huxley, Galton, Spencer, and Wallace; he was cited by psychologist and historian, biologist and philosopher, by everyone interested in primitive thought or behavior; and the lapse of time has merely confirmed the earlier judgment of his greatness.<sup>1</sup>

Tylor had been privately educated, and after a brief business career, in 1856 he visited Mexico in the company of Henry Christy, the prehistorian associated with Lartet in the exploration of the Périgord caves. This journey resulted in Tylor's first book, *Anahuac; or Mexico and the Mexicans* (London, 1861). Several years later came a major work, *Researches into the Early History of Mankind and the Development of Civilization* (London, 1865), which was followed by the two volumes on *Primitive*

<sup>1</sup> For biographical details, see Andrew Lang, in *Anthropological Essays Presented to Edward Burnett Tylor in Honor of His Seventy-fifth Birthday*, 1912; also A. C. Haddon's obituary notice in *Nature*, Jan. 11, 1917; p. 373; and R. R. Marett, *Tylor*, New York, 1936.



*Culture: Researches into the Development of Mythology, Philosophy, Religion, Language, Art, and Custom* (London, 1871). A popular textbook on *Anthropology: An Introduction to the Study of Man and Civilization* (1881) rounded out the number of Tylor's books, many articles being previously or later contributed to the *Journal of the Anthropological Institute*. Though not a university graduate, Tylor was associated with Oxford as keeper of the University Museum and advanced from a readership in 1884 to professorial status in 1896. By then he had long enjoyed an international reputation, his principal books having been translated into several languages.

Tylor was not technically a field worker, yet he was the very opposite of an armchair anthropologist. That he saw Mexican natives in his early manhood and later (1884) paid a brief visit to Pueblo villages counts for something, but more important is his unremitting tendency to study culture in the very heart of a metropolis. He receives a Tasmanian skin-scraper and forthwith has it tested by his butcher; he peers into shop windows for a parallel of the Oceanian pump-drill; in Somersetshire he watches a weaver throw her shuttle from hand to hand; and discerning a problem in aboriginal gesture languages, he learns hundreds of signs in the Berlin Deaf and Dumb Institution.

This inclusiveness of his interests is one of the conspicuous facts about Tylor. Most of his compatriots, both contemporaries and successors, limited their inquiries to particular topics. Lang, Frazer, and Marett cultivated religion and folklore; Rivers, social organization; Pitt-Rivers, Balfour, and Haddon worked on technology and art. Tylor embraced the whole field, even though he could not contribute equally to all its divisions. He kept abreast of prehistory, in which his interest had been stimulated by Christy; and he steadfastly concerned himself with linguistics, as the references to Steinthal and Lazarus

indicate. It is merely necessary to compare Tylor's remarks on language with the pitiable treatment of the subject in Rivers' *The History of Melanesian Society* in order to appreciate the greater seriousness of Tylor's approach to the problems of civilization.

Equally noteworthy is Tylor's uncanny sense of fitness in dealing with sources and extricating from them a solid core of knowledge. When he began to write, much of the globe was unknown ethnographically. Otherwise excellent reports skimmed over matters now—largely thanks to Tylor—methodically investigated by the veriest novice. Able travelers mingled fancy with observation, indulged in the superficial psychologizing that duped Klemm, and otherwise twisted the facts from initial bias.

Here, again, comparison is illuminating. Lubbock was not a man of mean caliber. He was scientifically trained; he made original contributions to prehistory, including the distinction between the Paleolithic and Neolithic periods; in breadth of interests he rivaled Tylor; and his critical acuity is attested by his strictures on Morgan. Yet, in approaching savage man, he lacked two requisites, detachment and a sense of probability. Himself forever emotionally affected by exotic custom, he timidly apologizes for describing what may be "very repugnant to our feelings," fearful "lest I should be supposed to approve that which I do not expressly condemn."<sup>2</sup> And his eagerness to fill in the gaps in an evolutionary system constantly deflects his judgment. On both counts Tylor's superiority is clear. It is interesting to see Tylor spurning the very evidence which Lubbock readily swallows. Lubbock accepts the word of "sailors, traders, and philosophers, Roman Catholic priests and Protestant missionaries" that there are races of men "altogether devoid of religion"; and among these he

<sup>2</sup> Avebury, *The Origin of Civilization*, xviii, 212.

includes some Eskimo and some Canadian Indian tribes, Brazilian aborigines, Andamanese, Hottentots, East Africans! Tylor in masterly fashion exposes these fantastic stories, revealing their psychological causes and concluding that "the asserted existence of the non-religious tribes in question rests . . . on evidence often mistaken and never conclusive."<sup>3</sup> Again, Lubbock does not, indeed, hold that savages use gestures because they lack words, but he quotes the famous tale about the Arapaho Indians unable to converse in the dark as though he half believed it. Tylor explicitly rejects it: "Captain Burton only paid a flying visit to the Western Indians, and his interpreters could hardly have given him scientific information on such a subject."<sup>4</sup>

It is impossible to exaggerate Tylor's services in separating the dross from the gold of early chronicles and thus rescuing a substantial body of authentic fact on every phase and period of civilization.

Tylor, however, went far beyond a mere assemblage of trustworthy material; he derived new concepts from these data and formulated problems which bear both on the history of civilization and on the interrelation of its traits. To quote Clark Wissler, it was he who "blocked out the essential processes of primitive fire-making"; Dr. Walter Hough has indeed added much detail, but he adhered to Tylor's categories, which marked a definite advance on Klemm's. We have already pointed out the fruitful definition of "stone-boiling" as a stage in culinary development. This, too, was not a mere matter of coining new words, but of creating a new concept by segregating one set of data from the rest. Similarly, all subsequent discussion gained definiteness by Tylor's minimum definition of religion as animism, "the belief in

<sup>3</sup> Avebury, *ibid.*, 219 f., Tylor, *Primitive Culture*, 1:425.

<sup>4</sup> Avebury, *op. cit.*, 432-434; Tylor, *Researches*, 78 f.

Spiritual Beings.”<sup>5</sup> Again, facts of social organization hitherto un-co-ordinated or inadequately classed were ticketed “local exogamy,” “teknonymy,” “cross-cousin marriage.”

Tylor’s epigoni have been reproached with amassing facts under a common head without special regard either to their provenience or to local differences. The substance of their material might thus properly be condensed into tabular form, indicating that such beliefs or customs were found here, and others elsewhere. Tylor’s own treatment, however, is often explicitly geographical, as when he traces the distribution of the Stone Age from continent to continent or passes from North American to Kamchadal stone-boiling.<sup>6</sup> Such a survey logically merges in some historical interpretation, for resemblances are naturally interpreted either as evidence of contact or of some inherent law. Tylor wrestled with this problem throughout his career; indeed, his *Researches* revolves largely about this question; and he applied both principles of explanation—historical connection and Bastian’s concept of psychic unity.

There is a strange legend about the development of ethnological theory. It represents ethnology as sunk in the slough of Bastian’s elementary ideas until rescued in 1887 by the geographer Ratzel (page 119). He, it seems, denounced the independent evolution of culture as equivalent to spontaneous generation in biology and for the first time demonstrated the complexity of culture due to the migration of its elements.<sup>7</sup> A still more picturesque account is offered by the eminent anatomist G. Elliot Smith (page 160). After vehemently ridiculing biological analogies in culture, he ecstatically hails

<sup>5</sup> *Primitive Culture*, 1:424.

<sup>6</sup> *Researches*, 203 sq., 263 f.

<sup>7</sup> W. Schmidt and W. Koppers, *Völker und Kulturen*, 32 f., Regensburg, 1924.

Ratzel's attack on "spontaneous generation"<sup>8</sup> and assigns to the Leipzig geographer much the same part as do Fathers Schmidt and Koppers. What is, however, still more interesting, he portrays Tylor as a double personality, now guided by the benign patron saint of Diffusion, now tempted by the sinister demon of Bastianism until he finally succumbs to the lure of the Evil One.<sup>9</sup> A few lines in an extremely brief Encyclopaedia article written in 1910 towards the close of Tylor's life and probably never seen by most of Tylor's admirers are taken to blot out his influence on the side of historical connection, and this despite the admission that throughout his life he had steadfastly championed diffusion. Absurdity can go no further.<sup>10</sup>

The actual facts bear no resemblance to this clap-trap. Bastian himself never denied the dissemination of culture, he merely demanded specific proof of it. Had Professor Elliot Smith been more conversant with anthropological literature, he might have recalled the two collections of essays issued in celebration of Bastian's seventieth birthday (1896). One contains an essay by Tylor, the other by Boas, two men intimately associated with Bastian. Both articles avow a faith in diffusion—Tylor's in the most uncompromising manner. Did these two friends of Bastian deliberately attempt to insult the man they were ostensibly honoring by flaunting views in mockery of his own?

In a widely read work, Richard Andree's *Ethnographische Parallelen und Vergleiche* (1878), Bastian's position is clearly set off in the abstract and illustrated by concrete examples. Andree is quite willing to entertain the view that Sudanese, Somali, and Bantu derive the

<sup>8</sup> Incidentally he errs in asserting that the comparison originated with Ratzel. It occurs in the good old parallelist Richard Andree's *Ethnographische Parallelen und Vergleiche*, p. iv, Stuttgart, 1878.

<sup>9</sup> G. Elliot Smith, *The Diffusion of Culture*, 66, 116-183, London, 1933.

<sup>10</sup> Cf. R. R. Marett, *Psychology and Folk-Lore*, 81, 1920.

rule of mother-in-law avoidance from a single source; it is the American and the Australian occurrence that he feels compelled to treat as separate instances. A rejection of diffusion on principle is out of the question. Indeed, the same author subsequently traced to a single center of origin all cases of shoulder-blade divination, ranging from Great Britain and Morocco to Bering Strait.<sup>11</sup> Undoubtedly there were writers who took Bastian's elementary ideas very seriously, but it is safe to say that from 1860 to 1887 there never was a time when any responsible writer denied contact as a factor in culture history.

But whatever may have been the situation in Germany, ethnologists who could read English required no Ratzel to teach them about historical connection. As early as 1864, Pitt-Rivers—an intimate associate of Tylor—held strong views on that subject (page 28). As Professor Elliot Smith is compelled to admit, Tylor himself was constantly producing proof of contact. He was, in fact, the very antithesis of a strict parallelist, even if he viewed the facts with scientific poise instead of falling prey to a cheap diffusionist dogmatism. In short, he was thoroughly convinced of the force of borrowing in human history and expressed this faith both abstractly and with respect to special cases. "Civilization," we read, "is a plant much more often propagated than developed." Again, "Most of its phenomena have grown into shape out of such a complication of events, that the laborious piecing together of their previous history is the only safe way of studying them. It is easy to see how far a theologian or lawyer would go wrong who should throw history aside and attempt to explain, on abstract principles, the existence of the Protestant Church or the Code Napoléon." And in the introduction to a translation of Ratzel's *History of Mankind* he contrasts "the small

<sup>11</sup> Richard Andree, "Scapulimantia," in *Boas Anniversary Volume*, 143-165, 1906.

part of art and custom which any people may have invented or adapted for themselves" with "the large part which has been acquired by adopting from foreigners whatever was seen to suit their own circumstances."<sup>12</sup>

Let us now consider some of Tylor's specific ideas on the subject. First we note his introducing diffusion into a discussion of prehistoric technology. After pointing out the amazing similarity of stone tools in different parts of the world, he argues that with all due allowance for psychic unity "it is very doubtful whether it can be stretched far enough to account for even the greater proportion of the facts in question." While not ruling out independent origin, he concludes that the observed uniformity "may some day be successfully brought in with other lines of argument . . . which tend to centralize the early history of races of very unlike appearance, and living in widely distant ages and countries."<sup>13</sup> With regard to various inventions, Tylor urges the same point. He derives the piston bellows of Madagascar from Indonesia; argues from the distribution of North American pottery to a single source; and at least favors a single world focus for the bow and arrow.<sup>14</sup> Of nonmaterial elements, the Australian, African, and American theory of disease as due to an extraneous object which the physician must suck out is treated as having a common origin; and the same explanation is offered for various mythological parallels between America and the Old World.<sup>15</sup>

What distinguishes Tylor from the extreme diffusionists is simply his serene willingness to weigh evidence. Refusing to assume a priori that all resemblances result from dispersal, he applies definite criteria for settling the question. They have never been improved

<sup>12</sup> Tylor, *Primitive Culture*, 1:53, *Researches*, 4.

<sup>13</sup> *Researches*, 203.

<sup>14</sup> *Ibid.*, 167-169, 366; *Primitive Culture*, 1:64.

<sup>15</sup> *Researches*, 277, 335, 360.

upon. In the case of American pottery he applies the principle of continuous distribution: earthenware vessels occur not sporadically "as if a tribe here and a tribe there had wanted it and invented it" but "in a compact field" from Mexico northwards. In comparing the Tortoise myth of India with its New World counterparts, he cites the combination of three specific features as evidence of connection.<sup>16</sup> He thus forestalls by decades Graebner's "qualitative" and "quantitative" criteria (page 158). Quite formally, in the discussion of American lot-games, he marshals the reasons for a common center. The Hindu game of pachisi is connected with the Aztec patolli because both share not a single trait but a series of independent features—divining by lot, a sportive wager, an appreciation of the law of chances, transfer of the result to a counting board, and rules of moving and taking. Tylor concurs in the opinion that "highly special or complex phenomena" are less likely to be duplicated than "the obvious and simple" and thus concludes that the games are related, i.e., he infers communication across the Pacific from Eastern Asia.<sup>17</sup>

This statement implies conditions in which resemblance is not held sufficient evidence of connection. Elsewhere Tylor is more explicit: whenever we see "like grounds" from which the similarities could have grown, it is permissible to suggest independent development.<sup>18</sup> So far Tylor is wholly right. A priori a resemblance may just as well be due to the same antecedent as to historical contact; and the logical value of the two explanations is identical. But Tylor does at times lapse into the vagueness of Bastian when he speaks of similarity as assignable to "the like working of men's minds under

<sup>16</sup> *Ibid.*, 335.

<sup>17</sup> E. B. Tylor, "On American Lot-Games, as Evidence of Asiatic Intercourse before the Time of Columbus," in *Ethnographische Beiträge*, Supplement zu IAE 9:55-67, 1896.

<sup>18</sup> *Researches*, 296.



like conditions," of "general laws" that explain phenomena as "direct products of the human mind."<sup>19</sup> We must, of course, recall that Tylor's prime preceded the epistemological purging of natural science that is associated with Poincaré and Mach. The laws of physics were still "eternal and inexorable," and any new branch of knowledge aspiring to recognition simulated the ritualism of its elders. At times, to be sure, Tylor's reference to "laws" in civilization seems to imply no more than a recognition that its phenomena are causally determined; but he obviously has more in mind when he compares these principles with the law of magnetic attraction.<sup>20</sup>

Now, this position requires much modification in order to retain any validity. The attraction of iron filings by a magnet is a predictable phenomenon, but the savage tendency to explain fossil remains by myths of giants is not similarly predictable.<sup>21</sup> We are merely wise *after* the fact; Tylor's caption "Myths of Observation" marks a useful descriptive category, but we can neither be sure that a particular people will have developed such tales nor what may be the specific plot if they have. Also, granting the specific unity of the human mind, when can we be sure of "like conditions"? Unless "the uniform action of uniform causes" can actually be traced, parallelism of culture traits is an empty allegation.

That is why, as a rule, historical connection accounts so much more satisfactorily for resemblances than the rival hypothesis. Whether probable or not, former contact does *explain* how remote peoples come to share customs and beliefs. The champion of parallelism scores only if he can demonstrate the same specific determinants in both areas, and this he usually fails even to attempt. For general "psychic unity" will not do: on that assump-

<sup>19</sup> *Ibid.*, 3, 5, 325.

<sup>20</sup> *Primitive Culture*, 1:1-4, *Researches*, 3.

<sup>21</sup> *Researches*, 3, 299.

tion all the societies of the world should share the features in question; at least, the parallelist must point out the particular circumstances that militate against the invariable reproduction of the same result by a common mentality.

Let us illustrate by reverting to Andree. This typical parallelist cites many instances of African, American, Australian, and Asiatic parent-in-law avoidance, treating them as so many spontaneous effects of the same mental disposition. There is, however, no attempt to account for the absence of the phenomenon elsewhere; and insofar as the author extricates from his sources any cause for the feature compared, it is not uniform. The Kaffir are said to refrain from intercourse with a mother-in-law for fear of committing incest even in thought; but in the Argentine the rule springs from the practice of sacrificing old women to a deity, and this offering is facilitated if son-in-law and mother-in-law remain strangers.<sup>22</sup> Here, then, the logical treatment of the problem is not at all satisfactory from the point of view of parallelism. Not only does it remain unexplained why psychic unity is so capricious in creating the rule in some places while not in others; but the specific determinants are not the same, so that actually the same mentality produces like results under unlike conditions! Granting the reality of such convergence, the logic remains deficient within the parallelist scheme.

It is here once more that Tylor's superiority appears. While sometimes he employs the vague phraseology of his period, he repeatedly brings forward specific determinants of specific effects. Why, for example, does he not use diffusion to explain the general belief in a hereafter? Because of the general occurrence of dreams apparently showing the continued existence of deceased

<sup>22</sup> R. Andree, *op. cit.*, 159-164, 1878.

kin.<sup>23</sup> We may doubt the facts—though in my judgment Tylor correctly represents them—but the logic of tracing a ubiquitous effect to a ubiquitous antecedent is unasailable.

Tylor, however, deserves still greater credit for his treatment of the more difficult instances of restricted distribution that baffle the parallelist. Again we can illustrate by the parent-in-law observance. In his earlier discussion Tylor inclines to a single focus, for “it is hard to suppose that the curiously similar restrictions . . . can be of independent growth in each of the remote districts where they prevail.”<sup>24</sup> This we have recognized as a logically valid interpretation. When Tylor subsequently produced a substitute theory, he did not fall back upon the inadmissibly vague phrase “psychic unity,” but requisitioned a definite sociological determinant, matrilocal residence, to explain the mother-in-law taboo; while by patrilocal residence he explained the reverse rule for a woman and her father-in-law.<sup>25</sup> In the same paper he similarly treats as causally linked other customs, such as exogamy and one of Morgan’s Classificatory types of nomenclature. Moreover, he tried to support his conclusions by the theory of probabilities, comparing the actual associations with those to be expected on chance.

These investigations, merely summarized in the paper cited, were unfortunately never presented in full and seem to be irretrievable, so that we cannot satisfactorily check the conclusions by the evidence on which they rest. The statistical technique has been proved inadequate in several respects. To concentrate on a single point, it is difficult to define the basic group to be selected as the unit in such an inquiry. Tylor does not seem to

<sup>23</sup> *Researches*, 5-8. *Primitive Culture*, 1:450.

<sup>24</sup> *Researches*, 296.

<sup>25</sup> E. B. Tylor, “On a Method of Investigating the Development of Institutions; Applied to Laws of Marriage and Descent,” *JRAI*, 18:245-269, 1889.

distinguish between cases reducible to a single place of origin and others where several or many foci are probable. In its published form the paper fails to strike the balance, then, between diffusion and independent origin. Apparently Tylor here inclines to the latter position, for he writes: "The institutions of men are as distinctly stratified as the earth on which he lives. 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 acting through successively changed conditions in savage, barbaric, and civilized life." If transmission is as potent as Tylor elsewhere recognizes it, would it not seriously disturb the uniform action of any law of sequence, or at least render impossible its demonstration? Thus, if twelve patrilineal Siberian tribes restrict speech between a woman and her husband's father in complete independence of one another, the case for an organic nexus between the taboo and the residence rule is much stronger than if all of these peoples borrowed the custom from a single source. On this latter assumption the accidental contact of two groups would seem far more influential than any immanent law of development. In any case, how could a law be inferred from a single occurrence?

Tylor's achievement, however, remains unaffected by such statistical foibles, for it rests on a number of absolutely sound logical principles. In the first place, he eliminates vague psychologizing in favor of specific social factors. The objection to Andree's type of explanation is thus overcome; human nature produces such and such a restriction on social intercourse, *but only in specified circumstances*. Further, Tylor recognizes the complexity of cultural phenomena when, in his explanation of teknonymy, he accepts more than one determinant of the same effect. He thus substitutes for the antiquated

metaphysical concept of cause the mathematical concept of function: teknonymy is no longer the inevitable effect of matrilocality or of an avoidance rule; it is simply more probable with these concomitants than without them. The notion of "law" as thus purified is thus nothing more than what it has become in the definition of philosophical physicists—a limitation of our expectation in the light of experience.

The essential thing is this: ethnologists have constantly, consciously or unconsciously, affirmed causal connections; but they have rarely stooped to justify their assertions. Tylor's scientific conscience prompted him to offer proof; and while from the published fragment of his schedules the validity of his demonstration cannot be established, at least one major conclusion seems to be borne out by later research: some correlation, though not so high as he thought, between exogamy and the Iroquois type of kinship terminology. Apart from specific results, we must insist that Tylor remains one of the few scholars whose championship of independent evolution is not a sterile, however warranted, denial of diffusion. For our sense of causality is satisfied only when the conditions of the problem are met in his spirit—by the demonstration of specific determinants that tend to produce like results in historically independent centers.

One point of Tylor's logic, however, must be exposed as vulnerable. Any statistical treatment can prove only correlation, not a time sequence; it yields propositions of the order that the side of a triangle varies with the opposite angle, but can never establish the primacy of either angle or side. But in his eagerness to prove maternal societies earlier than paternal, Tylor ignores this limitation. He begins by defining three social systems. In the maternal type matrilineal descent is coupled with avuncular authority and nepotie inheritance; at the opposite pole the paternal society has patrilineal descent,

paternal authority, filial succession; in the intermediate condition the features of the two extremes are variously blended. Tylor inquires how certain selected usages, such as the *couvade*, are correlated with these three types. The *couvade* is absent from the maternal "stage," occurs twenty times in the intervening, and only eight times in the pure paternal system. This, he argues, shows the priority of the maternal stage, for had it come later it—and not the paternal—would show survivals of the practice.

But observation does not reveal "stages," only certain combinations of traits. The facts show merely negative correlation of the *couvade* with features *a, b, c*, of the matrilineal system, and positive correlation with *e, f*, of the patrilineal and the intermediate system. A sequence is smuggled in only by assuming from the start that the classificatory differences have chronological meaning, viz., by assuming what is to be proved.

What doubtless misled Tylor here was the contemporary preconception that what differs from modern civilization is *ipso facto* inferior and earlier. Swanton has shown (page 145) that in North America the rudest peoples are either patrilineal or without fixed rule of descent, while many higher tribes, like the Hopi, are strictly matrilineal; and comparable evidence has accrued from other continents. The priority of "mother-right" as a general principle has thus been pretty generally abandoned.

The same bias produced a corresponding error in Tylor's *Primitive Culture*, when he denied high gods to the simpler peoples. In other words, he assumed that such conceptions could arise in civilization only as the result of slow evolution out of primitive soul beliefs. On this subject Tylor was challenged by Andrew Lang, a versatile man of letters who had shown far more than a mere dilettante's interest in comparative religion and

folklore.<sup>26</sup> Lang insisted that deities did not necessarily improve in dignity with advancing civilization and that extremely rude peoples, including some Australians, shared with Christianity the conception of a primeval and morally pure Creator. This view has been vigorously championed and elaborated by Father Schmidt.<sup>27</sup> The truth certainly seems to be that sundry unequivocally simple tribes,—certain Negritoes, Californians, and Fuegians—have the conception of a Supreme Being free from the undignified traits characteristic of many mythological figures; and in most of these cases the notion is clearly not inspired by Christian missionaries.

Here, as in the treatment of rules of descent, Tylor's mistake sprang from the difficulty of applying the evolutionary scale to elements of nonmaterial culture associated with values. As pointed out above, he kept himself on the whole remarkably free from this type of error; yet, like other evolutionists, he sometimes felt he had a key to the law of progress, confidently speaking of anthropology as "essentially a reformer's science," as "active at once in aiding progress and in removing hindrance." But such subjectivism is not at all obtrusive in his writings.

Like so many of his distinguished compatriots, Tylor was averse to systematization. His most ambitious scheme, that of animism, has nothing like the illusively clear-cut classification of Morgan's *Ancient Society*. As Issaurat said in reviewing the French translation of *Primitive Culture*: "What one notes above all is the abundance of documents. One finds them by piles, by heaps, by mountains, and when these are cleared there are still others."<sup>28</sup> Swamped with facts, careless readers

<sup>26</sup> A. Lang, *Magic and Religion*, 15-45, 224 sq., London, 1901; *The Making of Religion*, 160-190, 3d ed., London, 1909.

<sup>27</sup> Wm. Schmidt, *Der Ursprung der Gottesidee*, 6 vols., Münster in Westfalen, 1926-1935.

<sup>28</sup> Issaurat, in *Rev.*, 6:133 sq., 1877.

who must have bold outlines for complex phenomena inject into the treatment generalizations that do not exist. Thus, Elliot Smith imputes to Tylor the view that "all people instinctively regard the universe as alive, and regard the objects of it—the mountains, the trees, the rivers, objects of wood and stone,—as animate beings possessing souls which make the whole world akin."<sup>29</sup> Undoubtedly that is how Professor Elliot Smith would have simplified matters if he had constructed a theory of animism. Tylor's intellect, however, was of a different category. He did not confuse the belief in spirits with the universal animation of nature. He definitely ascribes the latter view only to Algonkians, Fijians, and Karens, merely contending that "many other peoples, though they may never have stated the theory of object-souls in the same explicit way . . . , have recognized it with more or less distinctness."<sup>30</sup> Even in his survey of religion, then, Tylor's parallelism is limited by his knowledge of tribal variation, precisely as his general belief in progress does not preclude explicit statements that advancement fails to be uniform in all branches and that degeneration is a reality, even though overshadowed by progress.

Tylor, we noted, did not escape the imprint of his time. He lapses into comparisons of the savage with the child of civilized countries, yet he remains free from the extravagances of Letourneau and Lubbock with their constant blurring of the line that divides man from other species. His position in practice approaches Waitz's, for he recognizes that, whether real or not, racial differences are negligible in the study of civilization.<sup>31</sup> If his psychology erred, it was in another direction; he did slight the emotional in favor of rational factors, and here

<sup>29</sup> G. Elliot Smith, *The Diffusion of Culture*, 172, London, 1933.

<sup>30</sup> *Primitive Culture*, 1:476-484.

<sup>31</sup> *Ibid.*, 7.



correction has been imperative. With Waitz, Tylor further shared a critical sense in evaluating testimony. The student of Tylor in 1890 could thus profit from a vast mass of thoroughly sifted and authenticated material, interpreted from a unifying, evolutionary point of view, tempered with sanity. Problems were set forth in definite terms, as was the logic that must underlie their solution. There were no capricious escapades of fancy; a clear intelligence was seeking order in a vast and largely virgin field. The words which Virchow pronounced on the death of the physiologist Johannes Müller might well be applied to Tylor: what evokes admiration is "the methodical rigor of the investigator, his temperate judgment, his secure serenity, the ample perfection of his knowledge."<sup>32</sup>

<sup>32</sup> "In dem Physiologen Müller bewunderte man nicht so sehr das Genie des Entdeckers, nicht so sehr den bahnbrechenden Flug des Schers, sondern vielmehr die methodische Strenge des Forschers, das maassvolle Urteil, die sichere Ruhe, die reiche Vollendung des Wissens." Rudolf Virchow, *Johannes Müller; eine Gedächtnisrede*, 15, 1858.

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# VIII

## PROGRESS

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### PREHISTORY, TECHNOLOGY, FIELD WORK

Notwithstanding the colossal achievement of Tylor in his two main works, a great deal remained to be done. First of all, the inadequacy of factual knowledge became patent. For example, the universality of a Stone Age throughout the world had been made probable on general grounds, but science required positive proof, and for Africa as well as Southern Asia the evidence was avowedly meager. Apart from this broad problem there were innumerable special questions as to the precise sequence of cultures in every region of the globe. Thus there developed a feverish activity among the prehistorians of Europe and America, of which the learned journals of the period give ample evidence. In part the findings were only of local antiquarian interest, but in part they were basic. Stone implements continued to be reported from Swaziland and the East Horn, establishing with increasing cogency a premetallic age in all of Africa. On the other hand, a critical scrutiny of American data led to the conclusion that man was not nearly so ancient in the

New World as in the Old. With respect to later periods, Scandinavian scholars, such as Oscar Montelius and Sophus Müller, traced the relations of Northern Europe with the South and even the Near Orient.<sup>1</sup>

Technology developed both from archaeological and ethnographic investigations. A prehistorian, in order to discuss intelligently the origin of bronze work in a given region, inevitably went into refinements far beyond a layman's competence. He had to ascertain where tin could have been procured at all and what percentages of it entered the alloy; also the impurities of particular bronzes demonstrated a specific relationship between the countries that shared them. For all such determinations the anthropologist was forced to requisition the aid of chemists and other specialists. Corresponding detail was found necessary for other branches of early or primitive industry, as a sound basis both for connecting peoples with one another and for appraising their skill. It was clearly not enough to say that such a tribe had arrows, and even their use of compound bows only vaguely placed them as archers. A detailed structural study was required for establishing the essential facts.<sup>2</sup> The United States, where Indians could still be seen fashioning arrow points and tanning hides, formed an unusually favorable area for testing archaeological interpretation by the practice of living tribes. Here, accordingly, technological studies flourished, especially with the development of museum collections. Frequently they were combined with experimentation. Walter Hough, not content to enlarge on Tylor's assemblage of data on fire-making, determined by actual trial how quickly a native might produce fire with a simple drill. A mechanically gifted inquirer like F. H. Cushing would

<sup>1</sup> E.g., Oscar Montelius, "On the Earliest Communications between Italy and Scandinavia," *JRAI*, 3:89 sq., 1900.

<sup>2</sup> Henry Balfour, "On the Structure and Affinities of the Composite Bow," *JRAI* 19:220-246, 1890.

himself manufacture arrow points in order to gain an insight into the inwardness of the stone-knapper's technique.<sup>3</sup>

The accumulation of facts cannot be dissociated from the progress of theory. It was only by new material that the generalizations of Tylor and others could be confirmed, and in turn it prompted novel interpretations. Tylor had declared the gesture-language to be "essentially one and the same in all times and all countries," but only a much wider range of observations, such as Garrick Mallery began to adduce in the early Reports of the Bureau of American Ethnology, could settle the point. Morgan had suggested a stage of group-marriage, which Fison and Howitt thought they could still observe in full swing among Australian blackfellows.<sup>4</sup> Miss M. H. Kingsley's observations in West Africa were guided by her reading of Tylor; on the other hand, R. H. Codrington's findings in Melanesia turned out to give a new direction to speculations on the origin of religion.<sup>5</sup>

Gradually there rose the demand for regional studies, undertaken not incidentally to a naturalist's or missionary's main interests, but as complete investigations of particular peoples by professional anthropologists. In 1884, the British Association for the Advancement of Science appointed a committee, of which Tylor was a prominent member, for investigating the Northwestern tribes of Canada; and from 1888 until 1898, Franz Boas was connected with the relevant reports. These investigations doubtless stimulated the Jesup North Pacific Expedition (1897-1902), organized by Boas for deter-

<sup>3</sup> W. Hough, "Aboriginal Fire-Making," AA 3:359 sq., 1890. F. H. Cushing, "The Arrow," AA 8:307 sq., 1895.

<sup>4</sup> See e.g., A. W. Howitt, "The Dieri and other Kindred Tribes of Central Australia," JRAI 20:30 sq., 1891.

<sup>5</sup> R. H. Codrington, *The Melanesians, Studies in their Anthropology and Folk-Lore*, Oxford, 1891.

mining Siberian-American connections. Comparable in intensiveness and roughly contemporary was the Cambridge Expedition to Torres Straits, led by Dr. A. C. Haddon, assisted, among others, by Dr. W. H. R. Rivers and C. G. Seligman, each collaborator devoting himself to a special topic. Here may also be cited the description of the Arunta in Baldwin Spencer and F. J. Gillen's *The Native Tribes of Central Australia* (London, 1899), which precipitated infinite discussion, mainly because of the aberrant type of totemism discovered by the authors. Equally stimulating were Karl von den Steinen's two expeditions to the upper Xingu in 1884 and 1887, which culminated in the discovery of unsuspected Arawak and Carib tribes in the interior of Brazil. This gifted observer clarified many phases of the daily life of the natives, though his residence was too brief for intensive investigation. Nevertheless his brilliant reconnaissance, set forth in a dashing style saturated with humor, exerted a deep influence and illustrates the possibilities as well as the limitations of pioneer work.

Such detailed studies naturally fostered regional comparison and delimitation, which implied a rigorous typology.<sup>6</sup> Field work also brought home the inadequacy of the techniques hitherto employed. Thus, it proved far from easy to secure a kinship nomenclature by direct questioning; and Rivers' experiences in the Torres Straits led to a method employed ever since—that of first ascertaining a subject's genealogy and then asking how he addressed such and such an individual whose relationship to him had been independently determined by the pedigree.<sup>7</sup> In the United States linguistic studies led Gatschet and others to transcribe tales phonetically in the aboriginal tongue; and it soon became clear that the

<sup>6</sup> E.g., A. C. Haddon, "A Classification of the Stone Clubs of British New Guinea," *JRAI* 3:221-250, 1900.

<sup>7</sup> W. H. R. Rivers, "A Genealogical Method of Collecting Social and Vital Statistics," *JRAI* 3:74 sq., 1900.

philological approach was the only safe method for recording native prayers, literary forms, and kinship terms.

### D I F F U S I O N

Theoretical interpretation was by no means lacking in this period. And once more we must repudiate the hoax that in this era diffusion was taboo. In France Hamy was ready to trace the Copan monuments of Honduras to China; in Germany Schultze-Sellack favored a connection of Toltec ideas with China and Japan.<sup>8</sup> Nephrite objects led to prolonged debates as to the possible provenience of the raw material.<sup>9</sup> The great Scandinavian archaeologists held strong views as to Near Oriental influences on the whole of Europe. In England, the supposed hotbed of evolutionism, Miss Buckland from 1878 until the end of the century presented paper after paper in defense of as radical a diffusionist doctrine as has ever been broached. According to her, civilization was *never* independently acquired; foreshadowing Elliot Smith, she believed that sun- and serpent-worshippers had spread agriculture, weaving, pottery, and metals over the earth; she adduced ceremonial haircutting and sweating among the Navaho as evidence of intercourse with Japan.<sup>10</sup> Whatever we may think of her evidence, she was certainly not lynched by her audience.

Why, indeed, should she be? Evolution, as we have seen, lay down amicably beside Diffusion in the An-

<sup>8</sup> E. T. Hamy, "An Interpretation of one of the Copan Monuments," JRAI 16:242-247, 1887. Carl Schultze-Sellack, "Die amerikanischen Götter der vier Weltrichtungen und ihre Tempel in Palenque," ZE 11:209 sq., 1879.

<sup>9</sup> E.g., Ver 15:211, 478, 1883.

<sup>10</sup> A. W. Buckland, "Primitive Agriculture," JRAI 7:2-18, 1878; "Prehistoric Intercourse between East and West," *ibid.*, 14:222-232, 1885; "Points of Contact between Old World Myths and Customs and the Navaho Myth entitled 'The Mountain Chant,'" *ibid.*, 22:346-355, 1893; "Four as a Sacred Number," *ibid.*, 25-96 sq., 1896.

thropological Institute. Pitt-Rivers, during this period, reaffirmed with emphasis his faith in a connection of Australian and Egyptian boomerangs. Yule brought out cumulative evidence—head-hunting, aversion to milk, bachelor dormitories, pile dwellings, piston bellows, water ordeals—on behalf of intercourse between Indo-China and Indonesia. In a paper read in 1878 and published the following year, Tylor himself suggested the Asiatic origin of a Mexican game. He followed this up with evidence for the origin of cat's cradle in South-eastern Asia, and traced Polynesian kites to the same region. Using the identical argument of modern diffusionists,—the uninventiveness of rude peoples—he even pleaded for a Scandinavian origin of Eskimo clothing and such games as cup-and-ball. In America, to be sure, Brinton inclined to an intransigent parallelism, but his views were being vigorously combated by F. W. Putnam and O. T. Mason. Mason was quite willing to swallow a moderate dose of diffusionism; even to interpret the specific features shared by canoes on the Amur and the Columbia as proof of contact.<sup>11</sup>

#### COMPARATIVE ECONOMICS

To turn to other developments, there were whole departments of culture perforce neglected by earlier writers that were at last being seriously attacked. Prominent among these was primitive economics. Prehistorians, of course, taught that agriculture and animal husbandry were preceded by hunting and gathering, but the history

<sup>11</sup> Pitt-Rivers, "On the Egyptian Boomerang and Its Affinities," *JRAI* 12:454-463, 1883. Colonel Yule, "Notes on Analogies of Manners between the Indo-Chinese Races and the Races of the Indian Archipelago," *Ibid.*, 9:290-304, 1880. E. B. Tylor, "On the Game of Patolli in Ancient Mexico and Its Probably Asiatic Origin," *Ibid.*, 8:116-129, 1879; "Remarks on the Geographical Distribution of Games," *ibid.*, 9:23-29, 1880; "Old Scandinavian Civilisation among the Modern Esquimaux," *ibid.*, 13:348-356, 1884. Report on World's Fair in *AA* 6:425, 1893. O. T. Mason, *ibid.*, 8:113, 1895.

of cultivated plants and domesticated animals was very inadequately known. A. L. P. P. de Candolle's *Origine des plantes cultivées* did not appear until 1883, Eduard Hahn's *Die Haustiere und ihre Beziehungen zur Wirtschaft des Menschen* until 1896. It was the generation before the turn of the century, then, that brought major enlightenment. Miss Buckland specifically concerned herself with primitive agriculture and, along with some more questionable views, advanced the sound idea that cereals were not necessarily the first species to be brought under cultivation but might well have been antedated by roots and fruits. She also stressed the prominence of women "exclusively in agricultural pursuits among the lower races"—an idea that attained great prominence in subsequent discussion. Roth, who pursued the same subject at somewhat greater length, likewise regarded women as the first cultivators.<sup>12</sup> Illuminating ideas on the psychology of animal domestication were thrown out by Francis Galton, whose researches tended to show that all species domesticable had actually been domesticated.<sup>13</sup> Scholars were also recognizing the importance of cultivated plants and domestic animals as evidence of historical relationship. Obviously a plant that was not a part of a regional fauna must have been imported in its cultivated form, as was argued for the species found in Swiss lake dwellings. Thus, botanists and zoologists came to be impressed into the service of anthropology.<sup>14</sup> There appeared monographic studies of the uses to which domestic species were put in different regions, such as Von Tschudi's paper on the llama and

<sup>12</sup> A. W. Buckland, "Primitive Agriculture," *JRAI* 1:3, 17, 1878.  
H. Ling Roth, "On the Origin of Agriculture," *ibid.*, 16:102-136, 1887.

<sup>13</sup> Francis Galton, *Inquiries into Human Faculty*, 243 sq., London, 1883 (Originally in *Trans. Ethnol. Soc.* 1865).

<sup>14</sup> See, e.g., A. Braun, "Über die im Kgl. Museum zu Berlin aufbewahrten Pflanzenreste aus altägyptischen Gräbern," *ZE* 9:289 sq., 1877.



Radloff's contributions to early volumes of the *Zeitschrift für Ethnologie* on Turkic beasts, later reissued in his book on Siberia.<sup>15</sup>

## ART

Within our present period also fall the first systematic investigations of primitive art. In the early eighties Hjalmar Stolpe of Stockholm began copying the ornamentation on objects in the principal European museums and later added to his materials during a voyage around the world. His general conclusions, first issued in the Swedish journal *Ymer* in 1890, were made accessible in English a year or two later, under the title *On Evolution in the Ornamental Art of Savage Peoples*. To these he added in 1896 *Studies in American Ornamentation—A Contribution to the Biology of Ornament*.<sup>16</sup>

Stolpe rendered an unquestionable service by his resolute attempt to define regional styles in Polynesia, which incidentally brought out the amazing diversity of decoration within this circumscribed area inhabited by closely related peoples. Of less value, though at least equally influential, was his attempt to trace the development of ornament from realistic figures to pure geometrical designs "through a series of intermediate forms." In this he had been anticipated by Pitt-Rivers and F. W. Putnam, but Stolpe applied this biological conception on a larger scale. According to him, the savage hunter, on attaining periods of leisure, would begin to decorate his implements, carving into a more realistic image a piece of wood offering a chance resemblance to the beast he pursued. "These animal figures, at first realistic, would become in time such invariable adjuncts that a mere in-

<sup>15</sup> J. J. Von Tschudi, "Das Lama," ZE 17:92, 1885. W. Radloff, *Aus Sibirien*, Leipzig, 1893.

<sup>16</sup> The English translations have been reprinted as *Collected Essays in Ornamental Art*, Stockholm, 1927.

dication would be sufficient to satisfy the craving for their presence, and this change, through the awakened desire for symmetry and for covering the entire surface may have been so great that the figures pass into mere lines, or what used to be called geometric ornament." Practically all primitive ornament was thus derived from zoomorphs, plant originals being assigned to a higher level. In the most emphatic way Stolpe denied that primitive man knew any geometric figures as such: "When at any time he employs similar figures they have for him an entirely concrete significance. He sees in them either a sign of the object from which they evolved, or else a picture of the thing itself."

It is important to note a psychological assumption that underlies Stolpe's reasoning. Denying to primitive man a purely aesthetic or play impulse, he sought some deep—especially a religious—meaning behind the decoration and applied this idea to tattooing no less than to the designs or artifacts. Stolpe did not dispute the influence of textile techniques on ornamentation, especially in producing rectangularity; but he assigned to this cause merely transforming, not creative, potency.

Whether through Stolpe's influence or independent thinking along the same lines, this basic view was widely accepted, dominating von den Steinen's study of Brazilian designs and A. C. Haddon's Papuan researches, the latter, however, also embodying a noteworthy attempt to define regional styles.<sup>17</sup> A quite distinct position was taken in America by W. H. Holmes, who stressed the effect of technical processes. Basketry techniques, more particularly, automatically produce decorative patterns which would extend their sway when copied on pottery

<sup>17</sup> A. C. Haddon, *The Decorative Art of British New Guinea*, in Cunningham Memoirs, Royal Irish Academy, Dublin, 1894; *Idem*, *Evolution in Art*, London, 1895.

or in carving.<sup>18</sup> This fruitful idea evidently had important implications. If the very act of making a basket yielded geometrical forms, Stolpe was no longer warranted in tracing such designs to a representative motive. Further, an important historical conclusion resulted. For, on this hypothesis, remote peoples could independently evolve similar decoration from similar techniques; there would be parallelism of art development even though the techniques might conceivably have come from a single source.

Art, of course, is much broader than ornamentation; and actually there was one attempt—Ernst Grosse's *Die Anfänge der Kunst* (1894)—to cover the entire field. But the time was not yet ripe for a significant discussion of either literature or music because the basic facts were as yet unavailable. In order to discuss primitive literary effort a mass of linguistically secured texts was prerequisite, and these were only beginning to accumulate in appreciable number. As for comparative music, the even greater dearth of trustworthy raw material was fatal. What Dr. von Hornbostel says of the Fuegians largely holds true of all primitive music before the systematic use of the phonograph: "Of Fuegian native music scarcely anything was known until recently, except a few poor notes scattered in ethnological literature, and half a dozen musical examples recorded by ear and hence of doubtful reliability."<sup>19</sup>

#### SOCIETY AND RELIGION

On social organization the period hardly reveals any advance comparable to that achieved by Maine, Morgan, or Tylor. Edward Westermarck (1862- ), a Swedish Finn who has taught at Helsingfors, London, and Abo,

<sup>18</sup> W. H. Holmes, "Origin and Development of Form in Ceramic Art," BAE-R 4:443-465, Washington, 1886.

<sup>19</sup> Erich M. von Hornbostel, "Fuegian Songs," AA 38:357, 1936.

aroused great notice by his treatise on *The History of Human Marriage* (London, 1891; 5th edition, 1921), which was translated into several languages. Its main propositions are concisely set forth in *A Short History of Marriage* (London, 1926). Heavily documented, *The History of Human Marriage* on its first appearance impressed many as a definitive counterblast to the more questionable aspects of Morgan's teachings. It rejected primitive promiscuity, describing the family, with a paternal protector, as the earliest form of social unit, one already prefigured among anthropoid apes. Westermarck suggested reasonable alternative explanations for phenomena often explained as relics of promiscuity or group marriage; and he repudiated the dogma that matrilineal had uniformly preceded patrilineal descent. However, Maine had previously voiced similar points of view.

More original is Westermarck's theory of incest. In his later formulation he epitomizes it as follows: "Generally speaking, there is a remarkable absence of erotic feelings between persons living very closely together from childhood. Nay more, in this, as in many other cases, sexual indifference is combined with the positive feeling of aversion when the act is thought of. This I take to be the fundamental cause of the exogamous prohibitions. Persons who have been living closely together from childhood are as a rule near relatives. Hence their aversion to sexual relations with one another displays itself in custom and law as a prohibition of intercourse between near kin."<sup>20</sup> This idea, while plausible, is not easily tested, and certainly the ultimate verdict on its value rests with psychologists.

Reverting to Westermarck's sociological ideas, we unenthusiastically note his essential agreement with

<sup>20</sup> Edward Westermarck, *A Short History of Marriage*, 80, London, 1926.

present views. Westermarck is very widely read, and his original researches in Morocco,<sup>21</sup> though only appraisable by Islamists, bear the earmarks of scholarship. His use of aboriginal data, however, is unsatisfactory. Indeed, his own account of the procedure when preparing one of his later books is not apt to arouse confidence: "I made use of the same method as I had employed in my book on marriage. I made my excerpts on slips of paper, which I numbered according to subject-matter, so that afterwards I should be able so much the more easily to group together all data bearing upon the same question: homicide, theft, love of truth and falsehood, adultery, cannibalism, and so on. . . ." <sup>22</sup> This approach is doubly suspect: first of all, the classification does not grow naturally out of the material but is imposed on it; secondly, the collector is likely to concentrate only on what seems to fall under his rubrics, omitting correlated phenomena of the utmost significance.

There is an obvious reason for these deficiencies. Westermarck is not primarily interested in culture; he is a philosopher who uses its phenomena to illustrate his points. Unlike Tylor, therefore, he makes no effort to assimilate *all* the relevant data. When Tylor cites a North American fact, there is in the back of his consciousness a picture, accurate in the light of what was then known, of all pertinent aspects of life. With Westermarck the reader has the uncomfortable feeling that nothing interests him less than to comprehend primitive tribes as a culture historian would like to understand them. Sweeping generalizations of his, chosen here and there, will illustrate his ethnographic inadequacy. In divorce, we learn, "among a large number of peoples all the children generally follow the mother. This is especially the case where descent is matrilineal, and among

<sup>21</sup> E.g., E. A. Westermarck, *Ritual and Belief in Morocco*, London, 1926.

<sup>22</sup> Edward Westermarck, *Memories of my Life*, 101, London, 1929.

the native tribes of North America it seems to be the general rule."<sup>23</sup> But though descent through the mother is common enough, there are numerous instances of North American tribes with the reverse law or no definite rule at all. In North America, again, "the wife must carefully keep away from all that belongs to her husband's sphere of action,"<sup>24</sup>—a statement so vague as to mean nothing and certainly not correct in the absolute way indicated. Indeed, three or four pages further, we read that Pawnee women figured at tribal councils; that Puget Sound wives were always consulted before a bargain was closed; and that an Omaha husband would not give away anything without his spouse's consent. How these specific utterances are to be harmonized with the preceding general statement is not easily seen.

Throughout we painfully miss Tylor's careful sifting of evidence. The profusion of Westermarck's documents has blinded some critics to his amazingly uncritical use of them. Not only are there inconsistencies of the kind just cited, but bad, good, and indifferent sources are cited indiscriminately. Even good sources are abused: "Lewis and Clarke," writes Westermarck, "affirm that the *status* of woman in a savage tribe has no necessary relation even to its moral qualities in general." And he goes on quoting his authorities to the effect that "the tribes among whom the women are very much debased, possess the loftiest sense of honor, the greatest liberality, and all the good qualities of which their situation demands the exercise."<sup>25</sup> Tylor would have inquired into the opportunities the writers had for such comparative judgments and into their educational equipment for forming, let us not say an *objective*, but

<sup>23</sup> *Idem*, *A Short History of Marriage*, 279.

<sup>24</sup> *The Origin and Development of the Moral Ideas*, 1:636, London, 1906.

<sup>25</sup> *Ibid.*, 1:647.

at least a reasonably convincing, view in so delicate a matter.

In short, Westermarck neither appraises his evidence discriminatingly nor becomes absorbed in his cultural phenomena; and while his views on early family life largely coincide with current doctrines, we are not able to discover any signal advancement of ethnology due to his writings. After all, the study of culture is not likely to be greatly promoted by those uninterested in its data. Nevertheless, Westermarck, by boldly challenging what he supposed to be the general view of anthropologists, at least helped to concentrate attention on vital issues. Tylor's contemporary review of *The History of Human Marriage* acknowledges its industry and independence and, as was then natural, praises its blending of biological and sociological points of view. On the other hand, it exposes the one-sided underestimation of matrilineal institutions and makes it clear that Westermarck was not the first to reject primitive promiscuity, which, indeed, Tylor himself had never sponsored.<sup>26</sup>

While we cannot attach outstanding significance to Westermarck's bulky tomes, another author appearing shortly after the turn of the century marks a real epoch in the study of social organization. Heinrich Schurtz (1863-1903) was a pupil of Ratzel (page 119), and absorbed his master's ideas on diffusion without, however, relinquishing the older form of parallelism. But his importance lies in an entirely different direction. His *Altersklassen und Männerbünde* (Berlin, 1902), while replete with adolescent subjectivism on a variety of irrelevant topics, for the first time summarized those associational activities independent of blood-ties which previous treatises had ignored. The picture of primitive society was thus radically altered. A person was no longer to be conceived as merely a member of a family

<sup>26</sup> *The Academy*, 40:288 f., 1891.

or clan; he belonged simultaneously to an age-class, a club, a secret fraternity. It is true that Schurtz committed a pioneer's errors. He underestimated the associational capacity of women, he offered an artificial genetic scheme, he committed quaint errors of detail, as when he connected the age-societies of the Plains Indians with totemism. But his faults are more than compensated by the new insights he afforded, the new problems he broached or suggested. Among these we note the influence of men's organizations on political structure and their relation to the kinship groups. The timeliness of Schurtz's achievement is indicated by two facts. Independently of Schurtz, though several years later, the American sociologist Hutton Webster similarly united relevant material in his meritorious book on *Primitive Secret Societies* (New York, 1908). On the other hand, the need for more descriptive data led to the first intensive professional investigation in the field—Kroeber's researches among the Arapaho of Oklahoma and Wyoming.<sup>27</sup> This, in turn, prompted a whole series of comparable studies by the same institution, largely with the view of testing Schurtz's theory of the age factor as a determinant of social solidarity.

In a widely read work<sup>28</sup> Van Gennep (1873- ) hardly does justice to the enlargement of perspective due to Schurtz. But he suggestively supplements it, as well as Tylor's somewhat summary treatment of ritualism, by concentrating on the rites of initiation connected with age-classes and secret organizations. These he aligns with ceremonies of admission into castes and professions and even with coronation ritual. Initiation, indeed, figures in Van Gennep's system as only one of a large series of ceremonies linked with the life cycle and

<sup>27</sup> A. L. Kroeber, "The Arapaho: Ceremonial Organizations," AMNH-B 18:151-229, 1904.

<sup>28</sup> Arnold Van Gennep, *Les rites de passage*, Paris, 1909.



other critical periods. He is concerned with disengaging from the multiplicity of detail a common sequence, "le schéma des rites de passage." Essentially there are three steps: *rites de séparation, de marge, d'agrégation*. The purpose of this category of ceremonies is to lead the individual from one status to another. Sometimes, as in mortuary observances, the preliminary separation is emphasized, sometimes, as in initiation, the intermediate step (*de marge*) of the novitiate looms important. But quite generally the ceremonial performer is first segregated from his or her normal social setting; then remains for a while in a neutral state; and is at last formally reintroduced to a recognized social position. This would be typically illustrated by the seclusion of a prospective primipara; her continued separation with definite taboos; and her final promotion to the status of a "mother."

Van Gennep himself seems aware of the schematism into which such a classification might degenerate and explicitly recognizes that the rites in question have aspects beyond their transitional elements. With these qualifications his book is a welcome contribution, for it defines a large set of phenomena and assists in their descriptive analysis. The relative validity of his basic concept is certified by its repeated application to field data by such observers as E. C. Parsons, H. Junod, and A. W. Hoernlé.

Within the latter part of the Tylorian period also falls the rise of Sir James George Frazer (1854- ). His little book on *Totemism*, published in 1887, was the precursor of the gigantic *Totemism and Exogamy* (4 vols., London, 1910), itself ultimately dwarfed by the final edition of *The Golden Bough* (1st edition, 1890; 3d edition, 12 vols., London, 1911-1915). Among his other works may be cited *Folk-Lore in the Old Testament* (London, 1918).

Frazer's reputation is probably unparalleled by that of any contemporary writer on anthropological subjects. If our treatment seems unduly curt, there is a reason. Undoubtedly Frazer's assiduous compilation of data, often from recondite sources, is very helpful; but after Maine and Tylor and McLennan, he disappoints by his failure to grapple with problems in a thoroughgoing fashion. To put it briefly, he is a scholar, not a thinker—and a scholar, moreover, who in his eagerness to assimilate descriptive data has somewhat perversely ignored the strides of theory. Thus, he has hardly kept abreast of the attempts to supplant the older parallelist schemes with a more critical insight into the effects of tribal intercourse; and his interpretations suffer from an a priori use of vulgar psychology, with constant lapses into a false rationalism. With his intellectual limitations we could hardly expect novel insight into social organization nor in any department the creation of many significant new concepts. Some of his confusions were, indeed, already pointed out by Tylor.<sup>29</sup>

But in the study of comparative religion, which is evidently most congenial to Frazer, we must concede to him more than a mere accumulation of raw fact. To be sure, he champions a number of indefensible propositions. He contrasts magic with religion, following Tylor's rationalistic classification of the former as a pseudo *science* on the ground that it attempts to coerce nature in conformity with an immutable law of causality. This is psychologically objectionable, because magical beliefs are commonly saturated with the same reverential attitude towards the associated rites which Frazer restricts to religion. As Marett (page 109) and Goldenweiser suggest, both are properly classed as forms of

<sup>29</sup> E. B. Tylor, "Remarks on Totemism, with Especial Reference to Some Modern Theories Respecting It," *JRAI* 1:138-148, 1899.

“supernaturalism.” Chronologically, Frazer also errs: he supposes magic to precede religion because it is psychologically simpler than the conception of personal agents—a wholly a priori contention; and, further, because its uniformity as contrasted with the multiple forms of cult implies priority. However, there is no such homogeneity of magical practice: magical formulae, prominent in Oceania, Siberia, and Arctic America, are lacking over large areas of North America; contagious magic is not found in Central Australia; and so forth. Besides, homogeneity does not logically involve antiquity; one would rather suppose that the more ancient of two systems of thought would be liable to more manifold metamorphoses.<sup>30</sup>

Nevertheless, Frazer’s discussion has solid merit. It contrasts with the utmost clarity two antithetic attitudes. On the one hand, a worshiper supplicates superior powers; on the other, the possessor of extraordinary knowledge uses it to effect desired results, with or without the aid of spirits he controls. It is true that in practice the two antagonistic principles may be joined. Thus, the occult information may itself be granted by a god in answer to a humble prayer. Nevertheless, there are these two extremes about which supernaturalism revolves, and Frazer’s formulation has been of great value in classifying relevant phenomena and defining them in particular cases. For example, Frazer himself has correctly pointed out the “conspicuous predominance of magic over religion” among the Melanesians of New Guinea; and according to Bunzel what among the Pueblo Indians formally represents a prayer “is never the outpouring of the overburdened soul,” but “more nearly a repetition of magical formulae” with no trace of humiliation

<sup>30</sup> *The Golden Bough*, 11-60, New York, 1922. Cf. W. Schmidt, *Der Ursprung der Gottesidee*, 1:510-514.

before divine beings.<sup>31</sup> On the other hand, the Plains Indian typically invokes the commiseration of the supernatural powers by drawing their attention to his miserable plight. Similarly, Frazer's explicit subdivision of magic into "imitative" and "contagious," while hardly exhaustive, helpfully ranged masses of pertinent material. Finally, his conception of taboo as negative magic, staving off misfortune as positive occult techniques ensure good luck, is both novel and stimulating if not wholly convincing.<sup>32</sup>

Frazer, then, can certainly not be ignored in the study of the development of thought on comparative religion. But in my opinion his proper place is in the history of English literature and of the intellectual classes of Europe. His style, overornate for some tastes, is unquestionably a thing of beauty; and this gift, coupled with remarkable erudition, has enabled him to imbue his readers with that sense of perspective in envisaging the phenomena of civilization which ethnology conveys to its votaries. The anthropologist assumes this vision as a foregone conclusion and asks for an illumination of special problems; and in that respect Frazer's services, while not negligible, shrink to moderate proportions.

#### PRIMITIVE MENTALITY

Racial psychology, which Waitz had examined in its broader aspects, required a more technical investigation. The first adequate systematic research on any one primitive group was conducted by Rivers, who subjected his Torres Straits Islanders to the tests perfected in the psychological laboratories of Europe (see page 170). But rather earlier there was a partial step in this direction,

<sup>31</sup> Preface to Br. Malinowski, *Argonauts of the Western Pacific*, xiv, London, 1922. Ruth L. Bunzel, "Zuñi Ritual Poetry," BAE-R 47:615, 618, Washington, 1932.

<sup>32</sup> See R. R. Marett, *Psychology and Folk-Lore*, 192 f., London, 1920.

oddly enough stimulated by the British statesman Gladstone, who piqued himself on his classical scholarship. From the dearth of Homeric color designations he inferred an inferior color sense, which precipitated a very lively discussion as to possible racial differences.<sup>33</sup> The upshot was to rule out deficiencies in vocabulary as indicative of racial inferiority—a minor contribution to the general belief in psychic unity.

More significant for ethnology was the revolution of psychology inaugurated by Francis Galton's concept of individual differences in his *Inquiries into Human Faculty* (London, 1883). Boas was profoundly impressed by Galton's personality and soon introduced his idea into ethnology, with consequences both for theory and field investigation (see page 134).

Another reform of anthropological psychologizing came from sociological considerations. As pointed out, Bastian had already clearly seen that individual psychology could not cope with the problems of thought and behavior because men's mentality is largely determined by their cultural setting. But the explanations of primitive mentality long continued to ignore this factor, to treat belief held by a particular savage, for instance, as though it had sprung from his individual psyche in response to certain experiences.

Still more important was the type of mental response that was almost invariably offered. Tylor, himself the child of a rationalistic period, tended to represent the savage primarily as a *reasoner*, as basically moved by intellectual promptings and merely misled by ignorance. "Human custom," we read, "is hardly ever wilfully absurd, its unreasonableness usually arising from loss or confusion of old sense." In identically the same spirit, Elie Reclus defines Australian supersti-

<sup>33</sup> E.g., A. S. Gatschet, "Farbenbenennungen in nordamerikanischen Sprachen," ZE 11:293 sq., 1879. Rabl-Rückhard, "Zur historischen Entwicklung des Farbensinnes," *ibid.*, 12:210, 1880.

tions: "They are the consequences reasoned out and logically deduced from premises, which are admittedly false, but justified by appearances: mere optical illusions due to an as yet imperfect camera."<sup>34</sup> This intellectualism, which minimized the emotional and generally irrational determinants of civilization, required revision, and one of the influences in administering the corrective was the French thinker Tarde.

Gabriel de Tarde (1843-1904), magistrate, *chef du service de la statistique* in the ministry of justice, and professor of modern philosophy at the Collège de France, was a voluminous writer on criminology and the philosophy of law as well as on sociology, but we are concerned with only one of his books, *Les lois de l'imitation* (1890, 2d ed. 1895), which profoundly impressed Boas and, through him, dozens of anthropologists in the United States. It was also translated into English by the American ethnographer Elsie Clews Parsons in her earlier, sociological phase.

In judging Tarde we must remember that he was a sociologist, and that the essence of his book largely dates back to the eighties, when several chapters appeared in the form of articles. In other words, while he knew Tylor and Lubbock, he was not saturated with ethnographical information; like Maine, whom he repeatedly cites, he largely relies on historical and contemporaneous data. Accordingly, prepared for lapses in the treatment of special anthropological problems, we merely smile when he proclaims the pristine universality of bloodthirsty gods and sketches religious evolution through human sacrifice, animal sacrifice, and vegetable offerings up to spiritual symbolism.<sup>35</sup>

Where Tarde sees more clearly than the contempo-

<sup>34</sup> E. B. Tylor, in JAI 23:236 f., 1880. Elie Reclus, "Contributions à la sociologie des Australiens," in Rev. 3<sup>e</sup> série, 1:240 sq., 1886.

<sup>35</sup> *Les lois de l'imitation*, 2d ed., 296, Paris, 1895.

rary evolutionary anthropologists is in his objective attitude towards the civilization of his period. Here there is no trace of smugness, no suggestion that in 1885 man had reached a peak from which he might look down pityingly, if not scornfully, upon his predecessors. Tarde does not accept the traditional fetiches of modern life, such as the jury system, but aligns it with defunct juridical methods of procedure: "It is stupefying to see how quickly at certain periods there are diffused criminal procedures equally odious and absurd, such as torture, or equally inefficient and unintelligent, such as the jury system." His picture of revolutionary movements tending to sink into as rigid a dogmatism as that against which they rebelled is unexcelled: "The most profound revolutions tend to become traditionalized, as it were . . ." "The *moliéristes* . . . with their devout attachment to minor traditions of the French theater, ought not to make us forget that their idol, Molière, was in his century an artistic innovator, a man most openminded toward innovations, most antagonistic to fetichism."<sup>36</sup> This sane position reacts on the judgment of savagery. Unlike Lubbock, who minimizes moral sentiments among primitive peoples, Tarde convincingly shows that they are identical on their and our level, being simply more narrowly applied at the earlier stage.<sup>37</sup>

The basic concept of the book, however, is the force of imitation: "Society is imitation, and imitation is a kind of somnambulism." That is to say, imitation proceeds *irrationally*, through prestige suggestion, the inferior individuals or classes aping their betters—and that, irrespective of practical considerations. Men follow tradition and sometimes flout it in favor of contemporary innovations from without; but whether dominated by *la coutume* or *la mode*, to use Tarde's terms for this

<sup>36</sup> *Ibid.*, 266, 343, 320, 370.

<sup>37</sup> *Ibid.*, 376.

antithesis, in neither case do they submit their views to intellectual scrutiny. Nearly half of Tarde's book is devoted to these extralogical influences. Contrary to appearances he announces that imitation proceeds "du dedans au dehors," i.e., ideas are imitated first, behavior later. This questionable aspect of his theory will engage our attention in another context.<sup>38</sup>

Consistently with his major postulate, Tarde emphasizes diffusion in all periods of history. Like other adherents of this principle, he assumes sterility of imagination and spreading of ideas even without vast migrations or conquests. Even in the Stone Age tools passed from country to country, and the same holds for pottery. If early flint implements are strangely uniform, it does not follow that "this similarity was due to the spontaneous appearance of like ideas and like wants among these primitive men. Nothing could be more arbitrary than this conclusion. . . ." The facts merely indicate wide dissemination. If even the Incas were unable to invent the wheel, how can we credit ruder peoples with an innate tendency to evolve ceramics? "Thus it seems to me fallacious to see in the almost universal distribution of this art, the proof of the necessity, the innateness of certain discoveries." However, he accepts a number of separate culture centers in the light of contemporary knowledge, "des foyers encore indécomposables de civilisation."<sup>39</sup>

Imitation, however, presupposes at some time an invention that serves as a model. Tarde by no means neglects this aspect of the matter. Invention is the fusion of two or more pre-existing ideas into a new synthesis; its laws belong essentially to individual logic, while the laws of imitation are in part social, largely extralogical. Since such creative synthesis is not calculable, Tarde as-

<sup>38</sup> *Ibid.*, 95, 205-394.

<sup>39</sup> *Ibid.*, 50 f., 53, 105, 109, 352.



signs a role to historic accident. He acutely recognizes, however, the logical interrelation of ideas, whence the irreversibility of intellectual progress.<sup>40</sup>

Many of Tarde's generalizations are not capable of rigorous proof, and certainly none such was attempted by their author. He presented, however, fresh principles that could be tested by ethnographic material precisely because they were avowedly timeless in their applicability, holding for societies as such. To sum up, we recognize two principal contributions emanating from him: a detached view of modern civilization, and a psychology of social man that did justice to nonintellectual motives of behavior. The latter influence presently became effective when the young ethnographer Boas investigated the growth of secret societies on the coast of British Columbia. The multiplication of ritual was traced to the prominence that went with membership in an organization, leading tribesmen to seek admission. When this was no longer feasible, native imagination created comparable societies under the spell of prestige suggestion. "These are the strange phenomena treated by Stoll in his book on suggestion, and rather more profoundly by Tarde in his book on the Laws of Imitation."<sup>41</sup> Incidentally, we note here the birth of the "pattern" theory, which played a prominent part in later discussion.

The emphasis on nonrational determinants of group behavior and belief, while an ever-recurring principle of Boas', was far from being restricted to him. Among his approximate contemporaries we may here single out R. R. Marett (1866- ), Tylor's successor at Oxford. Steeped in classical and metaphysical studies, this urbane and balanced spirit has persistently shed light on the primitive mind, and especially on the psychology of

<sup>40</sup> *Ibid.*, 109, 411, 413.

<sup>41</sup> Franz Boas, "Die Entwicklung der Geheimbünde der Kwakiutl-Indianer," *Festschrift für Adolf Bastian*, 442, 1896. Cf. *idem*, *The Mind of Primitive Man*, 114, New York, 1911.

faith. That the savage is "not perpetually spook-haunted"; that religion serves the function of restoring confidence in crises; that it is too complex to be reduced to a single root; that "the bane of the psychological study of human belief is a shallow intellectualism"—these are utterances that many ethnological writers might well have taken to heart. Specifically, Marett wisely distinguishes the savage's workaday world of normal experience from the transcendental phase of his being, where a sense of mystery supplants common sense. Stressing the subjective states of the latter category, Marett finds them identical, irrespective of whether animistic notions occur or not, and hence he unites Frazer's "magic" and "religion" in the wider category of "supernaturalism." Recognition of the supernatural, he is careful to point out, involves no conception of "nature" in the sense of modern science. The savage "does not abstractly distinguish between an order of uniform happenings and a high order of miraculous happenings. He is merely concerned to mark and exploit the difference when presented in the concrete."<sup>42</sup>

Marett thus passes beyond the intellectualistic notion of magic that ensnared Tylor and Frazer. He likewise introduced the useful distinction between "animatism" and "animism."<sup>43</sup> Tylor tended to see in all personification of inanimate objects the assumption of a spirit, a being modeled on the human soul. This is, of course, plausible in many instances, but Marett showed that we cannot assume it as a logical corollary. To yell at a hurricane is indeed to treat it as though it were alive, but it is not the same as to imagine a being of refined essence dwelling within the tempest and directing

<sup>42</sup> R. R. Marett, *The Threshold of Religion* (1st ed., 1909), 11, 102-121, London, 1914.

<sup>43</sup> *Ibid.*, 14, 18.

it. Here, then, there is "animatism" but not, without further evidence, animism.

In a sheaf of essays, some of which date back to over twenty years ago, Marett clearly forestalls contentions latterly supposed peculiar to the functionalist school. He deprecates *mere* antiquarianism in the study of folklore, aptly asking, "How and why do survivals survive?" And in another context he returns to the matter more specifically. The phenomena described under that label *may* indeed "be referable to antecedent historical conditions"; but they may also be explicable from "conditions operating here and now." This is the precise position of men like Malinowski except that in their less temperate moments they would ban all history.<sup>44</sup>

With characteristic poise Marett makes of primitive man neither a logic-chopper nor a chronic mystic: "The savage turns out to be anything but a fool, more especially in everything that relates at all directly to the daily struggle for existence . . . common sense is no monopoly of civilization." Repeatedly our attention is called to that solid core of accurate information acquired by early man that contrasts so sharply with his fanciful theories and looms as the basis of our modern science. Marett's psychology is also thoroughly up to date in recognizing both the force of rationalization and the importance of individual differences.<sup>45</sup>

Marett has been willing to forego ethnographical field research and has never engaged in literary enterprises of Frazerian dimensions. But in post-Tylorian England for poise in the judgment of theories or for a sympathetic grasp of primitive values there is no superior to this philosophical humanist.

<sup>44</sup> *Psychology and Folk-Lore*, 13, 123-127, London, 1910.

<sup>45</sup> *Ibid.*, 198. *Anthropology*, 227, 242-246.

## H A H N

Not even Tarde stressed the irrational factors of civilization more vigorously than did a German theorist of economic development whose most important publication falls within our period. Eduard Hahn (1856-1928) received his training in geography under the celebrated explorer of China, Baron Ferdinand von Richthofen. To Hahn we owe by far the fullest treatment of domesticated animals.<sup>46</sup> Later writers have dealt with the subject from a mainly zoological angle and have omitted species unimportant in our own economic system. Hahn's interests embrace zoology, geography, economics, and culture; he neglects neither the silkworm nor the muscovy duck, neither the guinea pig nor the bee.

According to Hahn, domestication involves free breeding in captivity, and it is this condition that rendered the process enormously difficult. All sorts of species have been kept as pets or even, like the elephant, put to practical tasks, but they fail to multiply when removed from a state of nature. As for the original motives of animal husbandry, Hahn convincingly eliminates several that would occur to a naïve speculator. Sheep could *not* have been raised for shearing because in the wild condition they lack wool, which is a by-product, not an antecedent of domestication. Similarly, a cow naturally yields milk only for calves, any surplus having originally been too insignificant to warrant impounding; milch cows, too, are an end result. Correspondingly, wild fowls do not lay eggs in such abundance as to tempt early man into keeping them for such purposes. What is more, we know various chicken-breeding tribes that never eat either eggs or poultry, while Eastern Asiatics and Indonesians are averse to milk drinking. Hahn suggests that

<sup>46</sup> *Die Haustiere und ihre Beziehungen zur Wirtschaft des Menschen*, Leipzig, 1896.

people kept poultry originally as alarm clocks or for cockfights—both noneconomic motives. This subordination of practical consideration Hahn carries to an extreme, as one of his admirers freely admits.<sup>47</sup> At times it almost appears from his account as though primitive man had no technical problems to solve, as though he viewed nature exclusively from the angle of supernaturalism; and one marvels at his being able to survive with so complete an aversion from common sense. The truth is, of course, intermediate between the intellectualism combated and the irrationalism put in its place. Hahn's ideas on domestic animals, however, form only part of a system and must be understood in that setting.

A cardinal principle of the theory is the repudiation of the traditional three economic stages: hunting, herding, and farming. Hahn refutes this sequence simply by pointing to the innumerable pre-Columbian Indians who farmed without owning live stock. Animal husbandry is thus certainly not a *necessary* antecedent of tillage. For this idea he yields priority to Alexander von Humboldt, who had already cited the same evidence. As a matter of fact, in a meritorious historical study Koppers has shown that the great naturalist was in turn preceded by the Swiss historian I. Iselin, who as early as 1786 challenged the accepted views from Polynesian data.<sup>48</sup>

From this critique emerged a positive conclusion simultaneously arrived at by Ratzel, though more amply elaborated by Hahn.<sup>49</sup> If American and Oceanian tribes tilled without live stock, such cultivation represents a distinct type. Thus was conceived the antithesis of primi-

<sup>47</sup> Hahn, *op. cit.*, 79, 154, 300. Ulrich Berner, "Rationales und Irrationales in der Wirtschaftsentwicklung primitiver Völker," ZE 62:210-214, 1930.

<sup>48</sup> Wm. Koppers, "Die ethnologische Wirtschaftsforschung," A 10-11:611-651, 971-1079; 1915-1916.

<sup>49</sup> Eduard Hahn, *Die Haustiere*, 388 sq.; *Von der Pflücke zum Pflug*, 37, Leipzig, 1919; *idem, Das Alter der wirtschaftlichen Kultur*, 28, Heidelberg, 1905.

tive "hoe-culture" and "plough-culture," the latter being the exclusive mark of higher civilizations. Each is a set of correlated traits. The simpler type implies not merely the hoe but its use by women in relatively restricted plots. This sociological point, already foreshadowed by Bachofen, Buckland, and Roth, attains great prominence in Hahn's scheme, ploughs being characteristically linked with men. Preferably Hahn identifies "agriculture" (*Ackerbau*) only with ploughing, beasts harnessed to a plough being guided by men to draw furrows in an extended field.

The revision of the three-stage scheme rests on this dichotomy. "Agriculture," by definition, follows domestication; hoe-culture is independent of it. Primeval farming grew directly out of feminine gathering in the earliest economic stage, which Hahn describes as omnivorous but with a flesh diet rather subordinate. There is thus not so much a "hunting" stage as one of hunting *and* gleaning, with women responsible for the vegetable fare. Women invented work, for early man was an idler, occupying himself now and then with useful labor and rather as a pastime than with serious intent. Ultimately men did come to control various livestock species, but this could not happen on the gleaning level because the instability of a hunting-gathering life precluded the keeping of animals until they would reproduce. Agriculture rose when primitive hoe-culture was combined with animal husbandry, the ox (which Hahn considers the primary livestock species) being made to draw a plough. This yields, then, the sequence of (a) hunting-gathering; (b) hoe-culture; (c) hoe-culture with stock-breeding; (d) "agriculture." What, then, about pastoral nomadism? Here is another characteristic element of the scheme. Hahn refuses to regard pastoralism as an independent economic type because herders, in all but a

handful of exceptional instances,<sup>50</sup> depend on neighboring farmers from whom they get the vegetable food to eke out their diet. Herding peoples, then, are in essence degenerate representatives of stage (c) who under special circumstances have lost hoe-tillage, making shift with their herds, yet leaning as far as possible on near-by tillers.

Hahn's later writings are unfortunately flavored with a persecution mania and a Messianic complex, which strangely enough is tempered with a redeeming modesty. Irrelevancies abound; the unsuspecting reader learns what the author thinks about the celibacy of Catholic priests, British free trade, and the wicked German socialists. Even disregarding these digressions, we are often repelled by fantastic hypotheses and dogmatic assertions. For reasons not at all clear Hahn finds the focus of his "agricultural" complex in Babylonia rather than in Egypt, blandly admitting the absence of proof in the cuneiform inscriptions. With still greater *sangfroid* he confesses his complete ignorance as to Babylonian pig-breeding, only to add that it certainly originated precisely in this area (*gerade in diesen Gebieten*) and certainly in connection with the cult of the great national goddess. He cavalierly denies any appreciable antiquity to the civilization of India—a view now refuted by the excavations at Mohenjo-Daro.<sup>51</sup>

Turning to the essential aspects of his scheme, we find that rather serious exaggerations mar the account of every one of his basic types. He describes excellently woman's economic contributions in the pre-horticultural stage, rightly stressing her knowledge of complex procedures for rendering vegetable food possible or palatable; but he minimizes beyond all reason the

<sup>50</sup> *Die Haustiere*, 132 sq.

<sup>51</sup> *Das Alter . . .*, 107, 159, 195.

complementary activities of men.<sup>52</sup> What is worse, his restricted ethnographical perspective leads to an underestimation of mankind at this stage. Hahn spurns the idea that "hunters" could have domesticated livestock, because he fails to note the fishermen occupying permanent villages, as in British Columbia, who are thus hardly less stable than many rude tillers.<sup>53</sup> Gudmund Hatt, Wilhelm Schmidt, and Koppers have justly criticized this feature of the theory. Further, the hoe is not the universal implement of simple farmers, since many Oceanians and Americans wield dibbles exclusively. But even if we widen the concept to make it include whatever is not a plough, this abstraction still fails to be preponderantly a feminine tool. Women did probably invent farming as a consequence of earlier root-digging, but in the historic era many Africans, Americans, and Oceanians assign farming wholly or in part to men. The reverse proposition—that ploughs are exclusively masculine, comes much nearer to the truth—yet it is not without important exceptions, such as Arthur Young observed in traversing France just before the Revolution. Again, the ox may have been the first stock animal, but prehistoric evidence does not yet prove its priority to the ass, the pig, the sheep, or the goat. Finally, it is indeed true that herders, like other human beings, crave a varied diet, so that they readily trade or extort grain from farming populations, but this does not in any sense make them dependent on their neighbors. Arabs can subsist for weeks on camel's milk, the Turkic and Mongolic peoples of Asia for months on fermented mare's milk. Tribes that have large flocks and herds of horses, cattle, sheep, goats, and camels, milk the females of all these species, prepare cheeses,

<sup>52</sup> *Von der Hacke zum Pflug*, 27 sq.

<sup>53</sup> *Das Alter . . .*, 92 sq.



and eat the flesh of their beasts are certainly as self-supporting as any society can be.

However, Hahn's absurdities and dogmas constantly jostle ideas that are sound, striking, and original. As his survey of domestication remains unrivaled, so he has defined more problems and thrown out more suggestions than any other writer on comparative economics. Though renouncing priority, Hahn more than anyone else eliminated the old three-stage theory from serious discussion, and his concept of hoe-culture marked a tremendous step in advance. It involved far more than a segregation of simpler from higher cultivators. Hahn realized that his system, like others, could not simultaneously distinguish types and take cognizance of transitional forms. But he compensates explicitly for this defect inherent in all classification. His merit lies precisely in detecting both essential difference and essential likeness. He sees that in rude farming it is root crops and vegetables, not cereals, that preponderate; but he traces hoe-culture down to our kitchen orchards that persist alongside of "agriculture." He contrasts Occidental fruit-growing with that of primitive peoples on the basis of its grafting technique. He distinguishes South Chinese "horticulture" (*Gartenbau*) from "agriculture" and assigns to it the highest place in his scheme because its use of irrigation and fertilizers ensures a more intensive exploitation. But he derives horticulture from hoe-culture and is willing to credit aboriginal Peru and Mexico with at least an approach to this highest plane of tillage.

Hahn's discussion of domestic animals is full of theoretical import. Like Tarde, he anticipates the "pattern" principle: the milking technique, for example, once invented, was transferred from one species to another; the horse was ridden only after riding had been developed with the ass and camel; reindeer breeders modeled their procedure on the experience of cattle and

horse husbandry. Saturated with the difficulty of domestication, Hahn inclines to single, or at best a few, centers of diffusion. This he does not necessarily identify with an actual importation of beasts; the mere idea of subjecting a particular species may have been passed on. Thus he does not positively assert that China derived its pigs from Western Asia, or vice versa, but he considers some connection conceivable by way of "a possibly very weak stimulation."

Hahn strikingly sets forth the contrast between Eastern Asia and the Near Orient in the repudiation and use of dairying. If he erred in representing herders as parasites on horticulturists, he at least helped us see the problem of nascent pastoralism in a new light. Obviously, the first herders could *not* subsist on milk; insofar as they developed—contrary to Hahn's theory—from a hunting condition, they could have utilized their beasts only for transport and for their flesh.

Hahn certainly tried to correlate the several aspects of a culture, say, the social position of the sexes with the division of labor. In this spirit he also approached technology. In what sort of setting, he asks, could a beast have been first harnessed to a plough? It is inconceivable that a hitherto untrained ox should pull a cultivator; we must assume that he had already been accustomed to drawing a wheeled cart. In visualizing the origin of the wheel, to be sure, Hahn is at his worst. Eager to vindicate the role of supernaturalism, he assumes that the cart originated as "a model by which the votaries of the Babylonian astral faith imitated on earth the movements of their celestial deities." This miniature conveyance, sprung from the brain of "an idle priest," was constructed of a spindle with whorls. In enlarged guise it was later taken beyond the temple precincts, and streets were built on "which the gods might roll along in chariots. Very gradually, like many other things, the

divine carriage was degraded to a utensil of daily life.”<sup>54</sup> This far-fetched association of ideas recalls Bachofen, but it likewise demonstrates an essential functionalism. Hahn, however, was also a historian. Imbued with the complexity of culture, he deprecates simple evolutionary schemes, a position strengthened by his stress on irrational motives. Finally, while not an extremist, he constantly applied diffusionist principles, as already noted.

Hahn's contribution is easily summarized. He raised comparative economics to a new plane; and for his work as a whole there is not yet an adequate substitute. He stimulated Boas and a host of other writers. Specifically, he exerted a deep influence on Laufer, some of whose most characteristic views are admittedly derived from Hahn. Thus his correlation of handmade pottery with women, in contrast to the association of men with the wheel is obviously modeled on Hahn's allocation of the hoe to woman and the plough to man (page 116). Notwithstanding psychological disparity, Hahn bears in his reputation some resemblance to Morgan. Both had a restricted range of interests, a crotchiness that at times led to absurdity; but each concentrated on his favorite field with unflagging zeal, and both remain landmarks.

#### RATZEL

Friedrich Ratzel (1844-1904) shared with Hahn not only a similar starting point but even one essential result, independently arrived at—the antithesis of hoe and plough cultivation. Nevertheless, the totality of his work lies in quite a different direction. Trained at first as a zoologist, he soon turned to geography and came to occupy the chair for that subject at Leipzig. His approach to civilization thus had a broad scientific basis,

<sup>54</sup> *Das Alter* . . . , 122-127.

and his literary productivity—second only to Bastian's—ranged over several disciplines.<sup>55</sup>

Contrary to some of his expositors, Ratzel did not exaggerate the potency of physical environment. Indeed, he repeatedly warns against this pitfall and is still further removed from those geographers who see in climate an overshadowing determinant. What saves him from such naïveté is recognition of the time factor: recent immigrants into an area cannot be so well adapted as natives of longer standing. Two further considerations, he contends, preclude an automatic response to environment: the incalculable effect of the human will; and man's limited inventiveness, of which Fuegian dress is cited as an illustration. No one could emphasize more than Ratzel the force of past history. At a pinch, he argues, early New England could be understood apart from the country, but never without reference to the Puritans who settled it. Again, he asks whether without Indian contact the lotus flower could have become the symbol of Buddhism in arid Mongolia. Ratzel also knows that sentimental factors deter men from exploiting available resources and make them reject alien offerings.<sup>56</sup>

Unquestionably there are lapses. Ratzel does interpret Pueblo Indian life in terms of their country without explaining why their immediate neighbors in identical circumstances enjoy a different culture. But, as a rule, a crass environmentalism is foreign to him. In principle he approaches modern anthropology and the unexceptionable anthropogeographic position formulated in Brunhes' discussion of dwellings: "If houses are far from wholly explicable by geography, this category of

<sup>55</sup> Vid. Viktor Hantzsch, "Ratzel-Bibliographie," in Fr. Ratzel, *Kleine Schriften*, 2:v-lxii, München und Berlin, 1906.

<sup>56</sup> F. Ratzel, *Anthropogeographie*, 1:13-110; 2:713, Stuttgart, 1899. *Idem*, *Kleine Schriften*, 2:36, 63, 407 f., München und Berlin, 1906. *Idem*, *Völkerkunde*, 1:91, 101, 322, 351, Leipzig und Wien, 1894.

human phenomena cannot, at least, be perfectly understood without recourse to geography.”<sup>57</sup>

It should be noted that scholars naturally start from the phenomena most familiar to them. A geographer, approaching civilization with the concepts of the desert, steppe, and the ocean, asks: What can I explain by these elements? The ethnographer, primarily facing cultures, asks: How can the differences between such and such societies be interpreted? At times geographical conditions may fit the case; often they are ruled out because, being identical, they cannot explain variations.

Temperate on geographical causation, Ratzel was equally moderate in appraising biological heredity. He strongly believed in the unity of our species, explicitly repudiated Gobineau and Chamberlain, and denied to no branch of humanity the capacity for furthering the common aims of mankind. Not that he held all races to be equally endowed, but he rejected *major* differences. On this question he is not completely consistent. Ratzel is unprejudiced, but since he accepts psychological judgments far less critically than Waitz, his suggestibility involves him in contradiction. There is caution in the abstract, a neat exposition of the entanglements of Nature and Nurture, a defense of the Chinese against the charge of unimagination. On the other hand, the Negro figures as a childish, imitative creature rarely capable of “intellectual heights” (*geistigen Höhen*); and the Canadian Athabaskans are introduced as “vigorous, but poorly endowed” (*wenig begabt*). Our objection is not that these statements are wrong, but that they rest on unscientific evidence. What reconciles us in a measure is the fairness that shines through Ratzel’s opinions.<sup>58</sup> In this spirit he likewise treats the relations of colored

<sup>57</sup> Jean Brunhes, *La géographie humaine*, 111, Paris, 1912.

<sup>58</sup> *Völkerkunde*, 1:470, 671, 1914; 2:12-15, 661 f., 1895. *Kleine Schriften*, 2:127, 406, 409, 462-487, 495-497.

peoples to white civilization, defining the aims of "applied ethnography" in 1900—after Bastian, but long before the Africa Institute.<sup>59</sup>

Ratzel's *Völkerkunde*, first issued in three volumes (1885, 1886, 1888) and reduced to two in the completely revised second edition (1894-1895), is unquestionably a significant work. The lavish and excellent illustrations from museum collections far surpassed anything hitherto presented; and it gave precisely what one could not get from Tylor—a geographically arranged description of living peoples. When, some fifteen years later, Georg Buschan edited the next German equivalent, his "Illustrierte Völkerkunde" (1910), specialization had advanced so far that even this first one-volume edition required five collaborators. Ratzel still surveyed the whole field unaided, and, judged by what was then known, he offered a balanced picture. The higher civilizations were necessarily treated briefly, and if Africa received disproportionate space—a whole volume in the first edition—this amplitude is warranted by the epoch-making discoveries recently made by Schweinfurth, Pogge, Stanley, and Nachtigal. Partly it was due to the dearth of material from other areas, which extenuates the skimpy treatment of America and Siberia.

Such a survey afforded Ratzel opportunity for both geographical and historical reflections. Here, then, are some of his distinctive ideas, drawn, however, from other writings as well.

As previously indicated, Ratzel did not invent the principle of diffusion, which Tylor and Pitt-Rivers applied quite as radically (page 72). But Ratzel did give special emphasis to the uninventiveness of mankind, though this idea, too, was clearly enough voiced by Tylor.<sup>60</sup> Ratzel

<sup>59</sup> *Kleine Schriften*, 2:402-419.

<sup>60</sup> "Old Scandinavian Civilisation among the Modern Esquimaux," JRAI 13:348-356, 1884.

added the notion that the globe, so far as inhabitable, is actually everywhere occupied by man, whence he inferred far-reaching migrations dating back to a very early period. "The earth is small" and must have been again and again traversed by primitive groups, whence the constant spread and observable leveling of culture. Ratzel thus transfers to civilization his doctrine of the essential biological unity of *Homo sapiens*. Granted these conceptions, he naturally reverses Bastian's principle that resemblances are merely evidence of a common mentality. Accepting psychic unity, Ratzel will have none of it as an interpretation of similarities. The uninventive human beings that were constantly migrating hither and yon simply transported what they had picked up as their cultural inventory. Consistently, Ratzel dropped the requirement that diffusion can in the main be inferred only by a continuous or otherwise traceable distribution. Bows on the Kassai may be affiliated with those from New Guinea irrespective of whether the path of migration is ascertainable.

Given this attitude, a world survey must prompt many specific historical hypotheses. The most remote Australians and Africans, we learn, have traditions that go back to India and Egypt; Australian religion suggests decay of a higher form. South American bows are connected with those of Oceania; the head-protectors of the Gilbert Islands with those from the Northeastern Siberians; the ornamental art of British Columbia—indeed, the Northwest of North America as a whole—with the Arctic regions of the Old World, as well as with Polynesia. Negro Africa is related to India, Southern Asia, and New Guinea. Extremely characteristic are Ratzel's ideas on the higher American cultures. He envisages no wholesale importation of Toltec, Maya, or Quechua elements by priestly Asiatic colonists. These complexities come not from a specific center in the Old World; their roots go

back to a primeval community (*uralten Gemeinschaft*) of cultural goods, carried here and there over the earth in the course of many prehistoric millennia (*im Laufe vieler vorgeschichtlicher Jahrtausende*).<sup>61</sup>

This view shows typically good intuitive sense in the rejection of flimsy attempts to derive American high cultures *in toto* from a particular region, and a less admirable but equally typical vagueness. For what requires an explanation is how Mexico and Peru came by their pyramids, their developed agriculture, their bronze; these features are *not* a common heritage of mankind and are known from nowhere *many* millennia ago. A corresponding stricture often holds against Ratzel's views. It does not suffice to indicate even striking resemblances; their *history* is established only when we know the several stages, and until then there is merely a problem that may or may not lead to a sound reconstruction of what happened. A generic theory of incessant migration with potential contacts of all peoples with all other peoples is a meager substitute for those specific relations that have actually obtained.

This vagueness may spring from one of Ratzel's most acceptable doctrines, the unity of human history, because he oddly exaggerates it into a uniformity of culture. He has no more sense for differences than the more naïve parallelists, a failing he shares with later diffusionists. "At some period," we read, "an ingenious (*sinnreiche*) mythology was thought out and imagined: parts of it are found scattered everywhere . . ." There follow similarities from Indo-Germanic, American, Polynesian, West African peoples. In a strange inventory of man's common heritage are included objects and ideas Ratzel perfectly well knew to have a restricted distribution, a fact he evidently ignored under the spell of his leading principle. It includes, e.g., spear-throwers and bows,

<sup>61</sup> *Völkerkunde*, 1:38, 138 f., 352, 353, 499, 525, 533, 595-597, 668-670.



farming, puberty rites, and shamanism. Indeed, from the *Völkerkunde* we learn that North Asiatic shamans and African rain makers, American medicine men and Australian magicians, are alike in essence, aims, and even part of their means. "To speak at length about the priests of these [American] peoples, would be repeating with minor variations what has been described for Polyne- sians and remains to be described for Africans."<sup>62</sup>

In the abstract, of course, Ratzel knew the danger of equating what is only superficially alike, and he specifically warned against it,<sup>63</sup> but his ethnological practice was little affected by this knowledge. Evidence of glaringly different cogency is constantly offered as equally grist for the mill. But without a well-developed sense for significant differences it is impossible to develop a sound typology of separate traits and still less to define culture areas. What we painfully miss in Ratzel, then, is precisely what might be expected from a geographer—sharp demarkation of regional boundaries. The African data, which he controlled best, are admittedly refractory, and little was known of New World cultures when he published the *Völkerkunde*; yet something better might have been expected than the perfunctory classification of Negro groups, while the segregation of at least the Pueblo tribes from the category of "Forest and Prairie Indians" was imperative even in 1894.<sup>64</sup>

In one of Ratzel's essays there occurs a curious comparison of Darwin with Herder, in which the German classic is described as "much deeper and precisely for that reason less popular. . . , but also, to be sure, less successful in the solution of specific problems than the English savant." To us it seems that Ratzel's phrase

<sup>62</sup> *Kleine Schriften*, 2:136-138, 230 f. *Völkerkunde*, 1:54, 583, 669.

<sup>63</sup> *Kleine Schriften*, 2:519.

<sup>64</sup> Virehow, reviewing the first edition, makes the same basic criticism, illustrating by Ratzel's juxtaposition of New Foundlanders and Araucanians, Tupi and Apache (ZE, 18:291, 1886; 20:248, 1888).

fits his own scientific character—a capacity for conceiving comprehensive ideas coupled with a comparative deficiency in the formulation of definite problems. This statement is meant to describe, not to deny, his contributions. He certainly supplemented the topical surveys of the evolutionists by his areal description, introducing factors naturally suggested by his training, but not at all obvious to the nongeographer. Thus, he properly stresses a people's situation on the earth's surface, correlating isolation with poverty and developing the idea of "marginal zones" (Randländer), which has been utilized by later writers.<sup>65</sup> His systematic regional consideration also threw into relief some historical views completely vindicated by later research, such as the influence of India on Africa or the connection of Australian with Oceanian puberty rites. And while many of his suggestions suffer from looseness, he was not uniformly averse to formulating definite problems. He did consider the resemblance of Congolese and Papuan bows sufficient proof of former contact, but he saw the value of confirmation by a study of the associated arrow types and other features. In other words, he grasped the "quantitative criterion" as a tool for the comparison of complete cultural provinces. Ratzel's wide reading further affords his readers information on a great diversity of topics. He brings out the role of the pastoralists in Old World history, the instability of primitive farming, the tendency of emigrants, illustrated by Scandinavian-Americans, to settle under familiar climatic conditions.

From the foregoing remarks we should not expect an abundance of rigorous new concepts. Too often Ratzel is content with a traditional classification. Folk tales are mainly fragments of myth; totemism remains unanalyzed; and a barely existing or even lacking totemic sys-

<sup>65</sup> See e.g., F. Boas, "Die Resultate der Jesup-Expedition," Separat-Abdruck aus ICA 16:10 f., Wien, 1909.

tem is made responsible for the animal names of Plains Indian military societies; the animation of all nature is assumed as a universal trait notwithstanding Tylor's discriminating prophylactic analysis; matrilineal descent is confused with gynecoeracy. On the positive side, Ratzel shares with Hahn the fruitful distinction between hoe- and plough-farming.<sup>66</sup> But his chief contribution probably lies in certain more general ideas—the conception of humanity as a unit, the tempering of environmentalism with a historical perspective, the demand for a conversion of space into time relations, the deprecation of spectacular migrations in favor of slow, continuous infiltration, the postulation of marginal peoples.

<sup>66</sup> *Kleine Schriften*, 137 f., *Völkerkunde*, 1:39, 113, 561, 564. Schmidt and Koppers, *Völker und Kulturen*, 390.

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# IX

## FRANZ BOAS

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Since Tylor no one has exerted on ethnology an influence comparable to that of Franz Boas (1858- ). Born in Minden, Westphalia, he studied physics and geography at Heidelberg and Bonn, whence he followed Theobald Fischer, his major professor, to Kiel. There he took his doctor's degree in 1881, his dissertation dealing with the color of sea water. Fischer himself had turned to geography from physics, and Boas acquired a natural scientist's control of mathematics which enabled him to follow the development of biometrics. His earlier writings include a discussion of psychophysics in a physiological journal and a proof of Talbot's law in the *Annalen der Physik und Chemie*. We must note, however, that Fischer's interests embraced anthropogeography. In the year of Boas' doctorate his teacher published a long essay describing the role of the date palm in North African and Western Asiatic life.<sup>1</sup> This paper describes aboriginal methods of raising the tree, of preparing dishes and

<sup>1</sup>Theobald Fischer, "Die Dattelpalme, ihre geographische Verbreitung und culturhistorische Bedeutung," in *Ergänzungsheft No 64 zu Petermann's Mitteilungen*, Gotha, 1881.

stimulants from it; it defines the commercial intercourse fostered by the spread of the palm, and fixes Arabia as its probable home. The treatment lends conviction to Fischer's own statement that he directed his pupil's interests towards ethnography.<sup>2</sup>

A decisive factor in determining Boas' lifework was an expedition to Baffin Land in 1883-1884, where contact with the Eskimo yielded a rich ethnographic harvest. From 1885 to 1886 he was assistant at the Royal Ethnographic Museum in Berlin and *Privatdocent* at the University. Thus came relations with Bastian and also with Virchow, the dominant spirit in the Berlin Society for Anthropology, Ethnology, and Prehistory. In 1886 Boas began those investigations of British Columbia tribes that engaged his attention for a long time to come. They also brought contacts with Tylor, then on a committee of the British Association for the Advancement of Science that promoted the exploration of that region. Three years later came Boas' first American position at Clark University, followed from 1892 to 1894 by work on the anthropological exhibits of the Chicago World's Fair. In 1895 began a decade's connection with the American Museum of Natural History, overlapping a lecturership that grew into a professorship at Columbia University, from which Boas retired in 1936.

Boas' historical position is unique. He is the first anthropologist who combined ample field experience with an unrivaled opportunity to train investigators. A. L. Kroeber, A. B. Lewis, F. G. Speck, R. H. Lowie, A. A. Goldenweiser, P. Radin, E. Sapir, F.-C. Cole, L. Spier, M. Herskovits, G. Herzog, A. Lesser are among those who took their degrees under him, but his immediate influence extended much further. It includes men like A. M. Tozzer, R. B. Dixon, C. Wissler, S. A. Barrett, J. A. Mason, J. R.

<sup>2</sup> See Fischer's letter to Laufer, the editor of *Boas Anniversary Volume*, viii, New York, 1906.

Swanton, R. Linton, who either studied under him for a limited time or pursued field research under his guidance. Still another category is made up of those who, like B. Laufer, P. E. Goddard, E. C. Parsons, G. Hatt, T. Michelson, came to Boas as mature scholars. Boas' curatorial duties at Berlin and New York further enlarged the scope of his activities, making him a practical administrator, a theorist on the functions of museums, an organizer of expeditions and of publication series. Sharing Tylor's abiding concern with philology, he had the added opportunity to record and analyze the speech of many illiterate peoples, so that this geographer-physicist grew into an investigator of language whose work commanded the respect of the linguistic specialist. And as there is nothing amateurish in his philological treatises, so he has done a full-fledged professional's work in physical anthropology, criticizing the methods of its votaries, measuring innumerable Indians, investigating growth by biometrical techniques, independently establishing the fact that the stature of mixed breeds is not intermediate between that of the parental stocks. He contributed even to archeology by stratigraphic excavations in Mexico, while his ethnological work, descriptive and theoretical, is in its totality monumental.

Such manifold achievement of high quality, coupled with his German origin and relationships, his frequent trips to Europe and regular attendance at scientific congresses, has made Boas a towering figure in international science. On the other hand, his influence on the world at large has not been commensurate with his intellectual stature. This is due to several circumstances. Boas is a man of research and has never been interested in rendering truth palatable. He appeals neither to the masses nor to that part of the cultivated public which looks for aesthetic enthrallment. He is thus essentially a writer of monographs, not of books. From the point of view of the

laity, the few slender volumes that pretend to address the general reader are not so much books as negations of the idea of a book. The best known of these, *The Mind of Primitive Man* (New York, 1911), is in the main a collection of essays previously issued in technical journals; one-third of *Primitive Art* (Oslo, 1927) is a highly technical analysis of specimens from a single area. Indeed, the inspirer of dozens of anthropologists has never composed a single textbook. Finally, Boas is not linked with an easily condensed or soul-stirring scientific message. All this, set down by way of description, not criticism, explains why Boas has not left a deeper mark on the intellectual life of the world than that made by many of lesser stature.

#### FIELD WORK

Boas must be understood, first of all, as a field worker. Here, too, we are struck by the all-inclusiveness of his interests. He himself has referred to "bad gaps" in his early account of the Eskimo, but what impresses us as truly remarkable is the multitude and kind of detail this novice, schooled in another discipline, succeeded in recording. He noted string-figures long before the cat's cradle game had become a fashion in ethnographic investigation and took down the music as well as the words of Eskimo songs. From such observations sprang his conviction that the savage "is sensible to the beauties of poetry and music."<sup>3</sup> As a matter of principle, Boas came to insist more and more on the thoroughgoing description of all cultural data as the sole warrantable scientific attitude. House types, basketry, social structure, beliefs, and tales must all be registered faithfully and with the fullest detail possible.

<sup>3</sup> F. Boas, "Das Fadenspiel," *AGW-M* 18:85, 1888. "Poetry and Music of Some North American Tribes," *Science*, 9:383-385, 1887.

So far there was not yet a sharp cleavage between his procedure and that of competent predecessors. But Boas raised field work to an entirely new level by demanding that the ethnographer's technique must equal that of a student of Chinese, Greek, or Islamic civilization. This implies some control of the aboriginal tongue, for which neither pidgin English nor an interpreter's rendering is a suitable substitute: ". . . we must insist that a command of the language is an indispensable means of obtaining accurate and thorough knowledge, because much information can be gained by listening to conversations of the natives and by taking part in their daily life, which, to the observer who has no command of the language, will remain entirely inaccessible."<sup>4</sup>

This ideal, Boas admitted, could not be fully realized by most ethnologists because native languages are so difficult that hardly any trained investigator is able to master them. But fortunately there are substitutes that approach perfection. The field worker can at least learn as much as time permits and, above all, he can secure authentic records of native thought by phonetically transcribing tales, prayers, poems, set speeches; by then reading them to his informants and rereading them for revision; and by carefully translating such documents with the aid of an interpreter. Thus, Boas' passion for texts springs from the need for material vying in documentary value with our sources for Periclean Athens or the Italian Renaissance. The natives' *ipsissima verba* represent an ultimate datum of reality without the blurring screen of a free translation, which should be used only as a supplement to the interlinear rendering. Of course, an intelligent nonprofessional observer familiar with the language and collaborating by correspondence with an ethnologist may learn to interpret native life "from within" while simultaneously answering the spe-

<sup>4</sup> *Handbook of American Indian Languages*, 1:60 (Washington, 1911).



cialist's queries. Boas early recognized this possibility, and his encouragement of James Teit, a squaw man settled in British Columbia, led to a series of splendid monographs on Salish tribes.

Another approach yields even better results. A native who has become literate and even educated can be taught to write *spontaneously*, preferably in his vernacular, what he knows or can gather from qualified elders about tribal lore. Boas accordingly not only himself recorded innumerable texts in the aboriginal tongues, but stimulated an enormous amount of high-grade recording by Indians. Foremost among his earlier students was the part-Fox William Jones, who transcribed a superb series of Fox and Ojibwa texts. In continuance of this work, the use of a syllabary by the Fox Indians subsequently enabled Dr. Truman Michelson to amass a tremendous wealth of material sent in by tribesmen. In recent years Boas has trained Miss Ella Deloria to take down Teton stories among her people and to furnish them with a set of linguistic, stylistic, and ethnographic notes that make her work a classic of descriptive literature. The Nez Percé myths dictated by an old native woman to her college-bred son form another notable instance.<sup>5</sup>

Material of this sort has the immeasurable advantage of trustworthiness, authentically revealing precisely the elusive intimate thoughts and sentiments of the native, who spontaneously reveals himself in these outpourings. Boas aims at ascertaining the true inwardness of aboriginal life, not by the uncontrollable intuitions of romantic outsiders but by objective documentation. Better than by answering direct questions, a blue-blood in the caste-ridden Kwakiutl society displays his attitude toward the

<sup>5</sup> William Jones, "Fox Texts," AES-P 1, New York, 1907; *idem*, "Ojibwa Texts," AES-P 7, New York, 1917, 1919. Ella Deloria, "Dakota Texts," AES-P 14, New York, 1932. Archie Phinney, "Nez Percé Texts," CU-CA 25, New York, 1934.

successful upstart in Boas' dramatic tale of the parvenu's rise and ultimate humiliation.<sup>6</sup>

Again, how do aboriginal artists approach and solve their problems? Dozens of scholars had been working on museum specimens when Boas started this totally new lead. He attacked the problem by getting James Teit, long resident among the Salish Indians, to interview dozens of native basket-makers about the details of their artistic careers, the effect of home training or alien contacts, their judgment of the efforts of others. The same urge to see aboriginal mentality in all its phases has made Boas encourage work by trained women. Since primitive peoples often draw a sharp line between the sexes socially, a male observer is automatically shut out from the native wife's or mother's activities. A woman anthropologist, on the other hand, may naturally share in feminine occupations that would expose a man to ridicule. Women have made important contributions independently of Boas, but probably nowhere have they achieved so much work as under the stimulation of the Columbia atmosphere—witness the publications of Drs. Elsie Clews Parsons, Ruth Benedict, Ruth Bunzel, Gladys Reichard, Erna Gunther, Margaret Mead, Gene Weltfish, Ruth Underhill.

To consider another topic, Boas translated his acceptance of Galton's individual differences into field practice. The scientist dealing with culture must exclude nothing on sentimental or aesthetic grounds. He is interested in the speculations of a native metaphysician but not a whit less in the gross drolls of a primitive raconteur. Only if we know the whole gamut of individual responses to the social setting—the varying mediocre participants as well as the creative leaders—can we understand that complex entity we call a tribal culture.<sup>7</sup> This point of view, among

<sup>6</sup> F. Boas, "Ethnology of the Kwakiutl," BAE-R 35:1104 sq., Washington, 1921.

<sup>7</sup> F. Boas, "The Ethnological Significance of Esoteric Doctrines," *Science*, 16:872 sq., 1902. The growing interest in individual psychology

other things, justifies and even requires transcription of several variants of important myths.

Another technique is that of securing the reminiscences of informants, not as a substitute for the customary methods but as a valuable supplement. An autobiography fills gaps in our information by naturally bringing out facts no inquirer would dream of asking about; notably it elucidates personal relations and subjective responses to the cultural setting, enlarging once more our knowledge of primitive individuals in relation to their society. To be sure, remarkable attempts in this line have emanated from other schools. Mrs. Gudmund Hatt (then Miss Demant), for example, induced a Lapp to write out his recollections, which she subsequently translated into Danish, whence English and German editions have been issued.<sup>8</sup> It is also reported that nowadays Russian ethnographers are extensively applying this method. But probably nowhere has so much material of this type been systematically collected as in America, under the direct or indirect stimulus of Boas. No one has applied the technique with more zest or skill than Radin, while to Truman Michelson we owe illuminating autobiographies of Indian women.<sup>9</sup> In the same category, though not identical in scope, belongs the Tewa Indian's diary kept at Dr. Elsie Clews Parsons' suggestion. As she points out in her introduction, Pueblo data, overabundant in some respects, had suffered from one serious deficiency, a view of the culture "from within," whence the restriction of "our insight into the ideas, feelings, and

among writers of all schools is brilliantly exemplified in Hilde Thurnwald's *Menschen der Südsee*, Stuttgart, 1937.

<sup>8</sup> Emilie Demant, *Das Buch des Lappen Johan Turi*, Frankfurt am Main, 1912.

<sup>9</sup> Paul Radin, *Crashing Thunder*, New York, 1927 (originally published as "The Autobiography of a Winnebago Indian," UC-PAAE 16:381-473). Truman Michelson, "The Autobiography of a Fox Indian Woman," BAE-R:40:295-349, 1925; *idem*, "The Narrative of a Southern Cheyenne Woman," SI-MC 87, No. 5, Washington, 1932.

volitions of the people, into those collective representations, as Lévy-Bruhl has called them, which are a significant factor in social behavior. Therefore this journal is peculiarly welcome; it fills psychological interstices."<sup>10</sup>

We can imagine how Tylor would have rejoiced over material of the caliber garnered by the Boas school. Perhaps he would not have preferred to ignore the emotional phases of supernaturalism if he had enjoyed access to adequate collections of native prayers and visionary accounts. A scholar is dependent for his interpretations on the available techniques, the facts made known by them. As refined instruments of precision open new realms to the astronomer and physicist, so every enlargement of knowledge about man in society brings novel insights to the investigator of culture.

#### VIEWS ON MENTAL PROCESSES IN RELATION TO CULTURE

Let us, then, turn to Boas' general views. In his estimate of races he has commonly been cited as an egalitarian. This, however, is an error of indiscriminating readers. He explicitly rejects the dogma that "there are no differences in the mental make-up of the Negro race and of other races. . . . On the contrary, if there is any meaning in correlation of anatomical structure and physiological function, we must expect that differences exist." What Boas insists on is that certain differences have been alleged without evidence and, further, that whatever differences may ultimately be determined are minor, hence do not "unfit an individual of the Negro race to take his part in modern civilization."<sup>11</sup> For obvious reasons Boas' championship of the simpler peoples and his exposure of race charlatanism or immature biological arguments have

<sup>10</sup> Elsie Clews Parsons, "A Pueblo Indian Journal," AAA-M 32:6, 1925.

<sup>11</sup> *The Mind of Primitive Man*, 271 f.

attracted wider attention than other aspects of his work. While sympathizing with his position, we cannot, however, attach importance to it for an estimate of his achievement, because it represents no vitally new idea. A critical attitude towards the "evidence" for inequality had been repeatedly anticipated, notably by Waitz. Boas must be credited, however, with bringing the argument up to date; and, it is necessary to repeat, the extravagances of his opponents do not betray him into equally dogmatic egalitarianism.

Boas' originality appears when he elucidates primitive mentality, especially as compared with that of civilized man. Expounded at length, his views on the subject might have gained him a larger following. As it is, he has in the main uttered his principles aphoristically, with a minimum of illustration. The student who has sat under him can supply the missing instances, but the layman is hard put to it in trying to invest these ideas with positive content.

In order to appreciate the advance due to Boas, let us once more revert to Tylor. In Tylor's treatment savages are essentially intellectuals grappling with their problems under the handicap of limited information. He describes games as largely "only sportive imitations of the serious business of life"; and while admitting the existence of ecstatic conditions accompanying supernaturalism, he explicitly restricted his survey to "the intellectual rather than the emotional side of religion."<sup>12</sup> Moreover, not even Tylor wholly rose above the smug conviction that Western nineteenth-century civilization was the only conceivable goal of social development.

Tarde, we noted, had transcended evolutionary complacency and had also attained a proper sense of human irrationalism. Boas shares both points of view and supports them with the amplitude of his wider ethnographic

<sup>12</sup> E. B. Tylor, *Primitive Culture*, 1:72; 2:329.

knowledge, indicating and solving a host of special problems by the way. To begin with his approach to Western civilization, he writes: "In no case is it more difficult to lay aside the 'Culturbrille'—to use von den Steinen's apt term—than in viewing our own culture."<sup>13</sup> That, however, should be precisely our supreme aim—not merely to see other peoples from their own angle but to see ourselves as others see us. This "non-Euclidean" approach is immanent, if often latent, in Boas' work. Particularly he exposes the popular fallacy that the individual civilized person is more rational in his processes than the savage; actually both accept traditional judgments uncritically, our conclusions being better founded only because the tradition of our civilization has become progressively more scientific.<sup>14</sup>

The "irrationalism" of simpler cultures consists largely in their classifying concepts differently from ourselves and especially in their curious association of—to us—disparate phases of mental activity. With them fashion and ethics, myth and history, music and poetry fuse in a manner strange and in part intolerable to us; ritual, in particular, pervades all their social activities, entering innumerable queer combinations.

Such linkages, Boas contends, as now confront us in primitive life are for the most part not primary realities but secondary associations which constitute an extremely important anthropological phenomenon. What is more, in the majority of cases these associations involve secondary *interpretations*—rationalizations in current psychiatric parlance—of customs or thoughts that arose independently and in the main unconsciously. To take a hypothetical case, a people's economic life may prevent simultaneous eating of venison and seal flesh, but when

<sup>13</sup> F. Boas, "The History of Anthropology," in *Science*, n.s. 20:517, 1904.

<sup>14</sup> *The Mind of Primitive Man*, 204-206.

an individual breach or a contact with an alien group teaches the hitherto unconceived possibility of transgression, the rule rises into consciousness, whence the need for justifying it. Thus, a matter-of-fact usage is sanctioned *ex post facto* by virtue of some supernatural decree.<sup>15</sup>

Boas himself applied this principle to the field of art. As noted, his predecessors accepted the names given to decorative designs as proof of a former effort to portray the species or object designated (page 93). Boas asks: What right have we to assume a primary connection? Possibly the geometric forms arose automatically from technical processes, as Holmes suggested, or from a craftsman's craving to play with his technique—another recognition of nonrational factors. On this assumption, the name or symbolic meaning may be simply a secondary feature, an afterthought which bars conventionalization.

But how can we prove the reality of this process? The most satisfactory attempt is probably the discussion of Eskimo needle cases.<sup>16</sup> A survey of available specimens demonstrates a fixed nonrealistic type of needle case, with flanges and tiny knobs, as the model floating before the craftsman's mind when he sets out to carve the container. A needle case as a whole is never conceived as the image of an animal, yet individual pieces show the miniature knob of the "standard" form transmuted into a seal's head, while sporadically the flanges turn into walrus heads with tusks, or into lemmings. The artists certainly did not severally try to create walruses, lemmings, and again seals, with the miraculous result that these diverse forms were all conventionalized into the identical traditional tube with its flanges and knobs. Rather must the geometrical portions of the implement have stimu-

<sup>15</sup> *Ibid.*, 197-243.

<sup>16</sup> "Decorative Designs of Alaskan Needle-cases: A Study in the History of Conventional Designs," USNM-P 34:321-344, 1908.

lated the artificer into altering them realistically as momentary fancy suggested.

Corresponding research by Kroeber and Wissler under Boas' direction yielded similar results among the Plains Indians. Kroeber found that the Arapaho assigned ten distinct interpretations to a rhomboid. Wissler's male informants gave a military significance to designs embroidered by their wives, who associated them with quite different ideas.<sup>17</sup> Here, secondary reading in of interpretations is an established fact.

Just as symbol and decoration join as an afterthought, so do ritual and myth. Here, once more, Boas' theory grows out of early field experience. The same ceremonial, he observed, had spread over British Columbia, but each tribe had a different explanation of its origin. Evidently, the rituals are not dramatizations of the aetiological myths, which on the contrary are merely rationalizations sanctioning the performance. The primacy of ritual, though not universal, has since been abundantly confirmed in other areas. Further, complex ceremonials prove to represent no basic unity, being welded together of historically diverse elements.<sup>18</sup>

In mythology it had been fashionable to interpret the plots in cosmic terms—as the adventures of solar or lunar heroes. With the advent of Boas' principles these facile explanations dissolved into nothing. A given tradition turns out to be primarily a story which may *sometimes* acquire a cosmic flavor by being ascribed to the sun or moon. The association is *proved* secondary whenever the same plot is linked with totally distinct heroes,

<sup>17</sup> A. L. Kroeber, "The Arapaho," AMNH-B 18:144, 1902. Clark Wissler, "Decorative Art of the Sioux Indians," *ibid.*, 273, 1904. F. Boas, *Primitive Art*, 88-143, 1927.

<sup>18</sup> F. Boas, "Die Entwicklung der Geheimbünde der Kwakiutl-Indianer," *Festschrift für Adolf Bastian*, 441, Berlin, 1896. Cf. Paul Radin, "The Ritual and Significance of the Winnebago Medicine Dance," JAFI 24:149-208, 1911. Leslie Spier, "The Sun Dance of the Plains Indians: Development and Diffusion," AMNH-AP 16:451-527, 1921.



so that in at least all variants but one the association could not possibly be primary and on analogy became suspect for the residual case. Here, also, the phenomenon hitherto naïvely accepted as a unit assumed a quite different appearance. Any particular variant was analyzable into episodes borrowed from hither and yon. A direct interpretation in, say, solar terms is not permissible because it would be quite arbitrary to single out as the original form that version which happens to refer to the sun. As Boas vindicated the potential primacy of ornament, so he showed that the plot—the equivalent of our novelette—could well be the basic phenomenon which might or might not later unite with an explanatory element.<sup>19</sup>

In the same spirit Boas approached totemism, on which befuddlement had reached a maximum—notwithstanding Tylor's admirable words of warning. Totemism, too, appears as an artificial unit; the catchword has been applied to diverse phenomena presenting superficial analogies. In reality, these several associations have neither a single psychological nor a single historical origin. Boas' ideas were elaborated by Goldenweiser with some individual additions.<sup>20</sup>

Some scholars felt the dissolution of traditional concepts as a loss; Boas' procedure seemed mere criticism, not constructive work. Nothing could be further from the truth. When a design and its name, a folk tale and its cosmic hero, a ritual and its origin myth, a clan and its totemic designation, are once recognized as spurious units, the sham problem of a generalized origin for any of these phenomena vanishes; the facts are seen in a

<sup>19</sup> F. Boas, *Indianische Sagen von der Nord-Pacifischen Küste*, Berlin, 1895. T. T. Waterman, "The Explanatory Element in North American Mythology," *JAFL* 27:1-54, 1914.

<sup>20</sup> F. Boas, "The Origin of Totemism," *AA* 18:319-326, 1916. A. A. Goldenweiser, "Totemism, an Analytic Study," *JAFL*, 1910, reprinted in his *History, Psychology and Culture*, 213-332, New York, 1933.

more natural arrangement; and a series of legitimate special problems is unfolded. The question how "Totemism" evolved is recognized as nonsensical; instead we are led to ask how and why such and such clans bear animal names, why they are linked here with magical rites, there with artistic carvings, and so forth. The intellectual liberation due to Boas is the same that we gain whenever science substitutes a sound phenomenalism for an arid conceptual realism based on premature classification.

### FUNCTIONALISM

Analysis, however, has for its complement the postulate that cultures are not mere aggregates of separate elements but integrated wholes. In his functionalist approach, Boas—though, of course later than Bachofen and Fustel de Coulanges—antedates others by decades. As early as 1887 we find him warring against the curatorial practice of synoptic museum exhibits, because if a specimen is isolated "we cannot understand its meaning." A rattle, for instance, may be a musical instrument or an implement of ritual; two objects identical in outward appearance may thus have vastly different connotations. "The art and characteristic style of a people can be understood only by studying its productions as a whole." To this conception Boas has steadfastly adhered in principle, and it formed the central theme of a treatise by one of his favorite pupils.<sup>21</sup>

In registering Boas' functionalism we are simply chronicling plain facts, not expressing unqualified assent. Doubtless Boas' warning was timely and served a useful purpose, but the doctrine lends itself to exaggeration in the hands of less cautious followers. Certainly many of the cohering elements in the life of a people are not chance

<sup>21</sup> F. Boas, "The Occurrence of Similar Inventions in Areas Widely Apart," *Science*, 9:485 f., 1887; *idem*, "Museums of Ethnology and Their Classification," *ibid.*, 587-589. Herman K. Haerberlin, "The Idea of Fertilization in the Culture of the Pueblo Indians," *AAA-M* 3:1-55, 1916.

concomitants, but it has never been proved that *all* the traits are linked, and the attempts hitherto made to demonstrate integration have never gone beyond showing that, say, art is connected with supernaturalism, or economic pursuits with magical ideas. Further, how can we ever attain knowledge of the underlying quintessence of a culture? Evidently only from an intensive study of its elements; there is no royal road unless divination is to supersede sound scientific analysis. Finally, while it is true that no isolated fact is significant, it does not follow that the only vital correlations lie within the supposedly closed system of a particular culture. These fairly obvious reflections, directed not against Boas but against distortions of a relatively valid position, vindicate studies which are anathema to the militant functionalist of today. A rattle may, indeed, be invested with all sorts of meanings, but there remains a reality describable under that head; and this can be profitably, though not exhaustively, investigated from a technological and distributional angle. Otherwise we are arbitrarily restricting the scope of our investigations. This is obviously Radin's intention when he prescribes for study only such part of the data "as bears directly upon the culture as a whole."<sup>22</sup> But if ethnology is the science of culture, it cannot rest content with this approach. The tribal life then appears as merely a segment, arbitrarily delimited for convenience' sake, of human culture, and correlations are permissible in *all* directions, with associated intratribal traits, with neighboring cultures, with physical environment.

To return to Boas, his functionalist position must be understood in relation to the ethnological practices that evoked it. Many of the *lesser* anthropologists were at bottom antiquarians who collected curious oddments of custom or belief, placing them in convenient pigeonholes

<sup>22</sup> Paul Radin, *The Method and Theory of Ethnology*, 27, New York, 1933.

according to a rule-of-thumb classification. Frequently disparate phenomena were thus brought together—as in the case of totemism—because of a superficial resemblance. Refusing to follow the path of least resistance, Boas, with a keen sense for differences and for the complexity of social life, was able to distinguish like and unlike features. He insisted that before equating phenomena we must first be sure of their comparability, which could be determined only from their context. Specifically, he insisted that we must not group together peoples on the strength of similar behavior but that the associated sentiments are an essential part of any phenomenon to be studied. Thus, the sacrifice of a child on behalf of one's community cannot be properly set down with other instances of "murder," but with forms of self-abnegation.<sup>23</sup> This is evidently very different from totalitarian mysticism.

#### GEOGRAPHY AND HISTORY

The quality of Boas' mind appears nowhere more clearly than in his attitude towards geography and history.

Starting as a geographer, he was disillusioned by his Eskimo experience as to the potency of physical environment, to which he has since then ascribed a preponderantly limiting rather than creative importance. He points out the association of vastly different cultures with the identical environment and, like Ratzel, but more consistently, the overlaying of geographical by historical factors. In one of his very early papers he contrasts two scientific tendencies: the physicist tries to reduce reality to simple elements, while the cosmographer dispenses with generalization, seeking to comprehend a complex phenomenon as a whole. Both tendencies are equally

<sup>23</sup> F. Boas, *The Mind of Primitive Man*, 192; *idem*, "The Limitations of the Comparative Method of Anthropology," *Science*, 4:901-908, 1896.

legitimate, but with the intricacies of geography—and, by extension, ethnology—the latter is the more fruitful procedure. To quote Boas: “I aligned myself with those who are motivated by the affective appeal of a phenomenon that impresses us as a unit, although its elements may be irreducible to a common cause.” Because of the complexity of cultural data, Boas deprecates the quest of laws: “Cultural phenomena are of such complexity that it seems . . . doubtful whether valid cultural laws can be found.” Such laws as may be propounded will be “necessarily vague and . . . so self-evident that they are of little help to a real understanding.” Tylor, we noted, also recognized the complication of events in most of cultural phenomena, but he reserved an undelimited residue to which general laws were applicable.<sup>24</sup>

From Boas' point of view, however, a cultural phenomenon is intelligible only from its past; and because of the complexity of that past, chronological generalizations, like those of physics, are as impracticable as are timeless generalizations. Paternal descent, for example, need not spring out of maternal descent, as Morgan and McLennan assumed. Stimulated by Boas' fresh outlook on this question, which apparently had been settled once and for all, Swanton undertook an investigation that culminated in a rejection of the traditional sequence and remains a landmark in the study of American Indian societies.<sup>25</sup> He proved beyond a reasonable doubt that in North America the clanless condition preceded unilateral descent. Methodologically, he refused to accept the position frequently held as axiomatic that Australian phe-

<sup>24</sup> F. Boas, “The Aims of Anthropological Research,” *Science*, 76:611, 1932; *idem*, “History and Science in Anthropology,” *AA* 38:137, 1936; *idem*, “The Study of Geography,” *Science*, 9:137-141, 1887. E. B. Tylor, *Researches*, 3 f.

<sup>25</sup> John R. Swanton, “The Social Organization of American Tribes,” *AA* 7:663-673, 1905; *idem*, “A Reconstruction of the Theory of Social Organization,” *Boas Anniversary Volume*, 166-178, 1906.

nomena are the rudest extant and that the succession of events in Australia represents the norm for all human groups. In insisting that first of all "each region must be worked out by itself so far as practicable," Swanton voiced an essential feature of Boas' programme of study. This sense of diversity separates Boas on principle from the parallelists. A closely connected point of his is the reality of convergence: in culture not only like causes but also unlike causes produce like effects. "It is of very rare occurrence that the existence of like causes for similar inventions can be proved, as the elements affecting the human mind are so complicated; and their influence is so utterly unknown, that an attempt to find like causes must fail, or will be a vague hypothesis."<sup>26</sup>

Boas' historical outlook must be considered in the light of this overpowering sense of cultural diversity, for it explains his reluctance to accept the generalized pictures of extreme diffusionists as well as his repudiation of parallelist schemes. For these diffusionists resolve culture history into the interaction of a very few ultimate culture complexes. Surveying the whole span of human existence, Boas sees no warrant for such simplicity of formulation. Each group has its own unique history, due partly to inner causes, partly to extraneous influences; and these differentiations must date back to an extreme antiquity. If, for instance, Elliot Smith interprets practically all arts and customs of savages as decadent relics of Egyptian civilization that spread as complexes with the rise of navigation, Boas objects that while single detached elements may persist indefinitely, "the coherent survival of cultural features that are not organically connected is exceedingly rare." Thus, in the course of centuries Elliot Smith's complexes would inevitably be torn

<sup>26</sup> F. Boas, "The Occurrence of Similar Inventions in Areas Widely Apart," *Science*, 9:485 f., 1887; *idem*, "The History of Anthropology," *ibid.*, 20:513-524, 1904.

asunder and rearranged differently in whatever region they might reach.<sup>27</sup>

As a result of this attitude Boas does not reconstruct history on a major scale. Probably more familiar than any of his contemporaries with the ethnography of the world, he has never traced the sequences of culture history as a whole; and even for America he has merely sketched the course of development with utmost brevity in an after-dinner speech to which he himself attaches no importance.<sup>28</sup> Given a diversification of culture myriads of years ago and an instability working differently on every group because of specific conditions and specific contacts, precise reconstruction is possible only after the most intensive investigation. Hence no one scholar can establish sequences for more than a minute fraction of the total number of peoples; and Boas prefers connecting North-eastern Siberia with British Columbia to formulating a scheme for all of the New World, let alone for both hemispheres. His restraint is partly due to his rejection of certain principles applied by others in reconstructing the past. He does not believe that the area of distribution is proportionate to the antiquity of traits; nor does he consider the area of greatest intensity to indicate the original focus of dissemination.

Boas deprecates the criticism that such abstention from historic synthesis implies indifference to "the ultimate problems of a philosophic history of human civilization." He doubtless feels that a satisfactory synthesis may some time emerge from a sufficient number of intensive regional studies. In the meantime his approach offers two compensations—a rigorous demonstration of his-

<sup>27</sup> F. Boas, *Primitive Art*, 6 f.; *idem*, "The Methods of Ethnology," AA 22:311-321, 1920; *idem*, "Evolution or Diffusion?" AA 26:340-344, 1924; *idem*, "The Social Organization of the Tribes of the North Pacific Coast," AA 26:323-332, 1924.

<sup>28</sup> "The History of the American Race," *Annals N. Y. Academy of Sciences*, 21:177-183, 1912.

torical relationships and new light on the processes involved in such contacts.

Boas' approach to concrete historical problems is best exemplified by his mythological investigations and by the work of the Jesup North Pacific Expedition.

As repeatedly pointed out, there was never a period in which diffusion was generally denied. But in mythology parallelism was carried rather far by Brinton, who explained resemblances even between neighboring tribes on the principle of psychic unity. Boas's early work in British Columbia completely demolished this interpretation. If Brinton's position were tenable, proximity would be a negligible factor, hence the most remote variants of a cycle would not differ any more than those among contiguous tribes. Actually, the Raven myth turned out to be most elaborate in the north and to taper away progressively as one proceeds southward. Hence, the plot was not the independent product of a common psychology but had been evolved in one center and transmitted from tribe to tribe. In other words, the combination of adventures found in the subdivisions of this area is a function of geographic position, not of a common mentality. The beauty of this logic has never been surpassed either by Boas himself or anyone else. What impresses us is the conversion of barren abstract dispute about parallelism versus diffusion into a scientific problem that admits of definitive solution.<sup>29</sup>

It is the capacity for defining problems so as to prepare a clear-cut solution that distinguishes Boas from Ratzel. Not content with general migratory movements of peoples and ideas that would lead to an indiscriminate international communism of culture, he is interested less in the possibility than in the demonstrability of contact.

<sup>29</sup> Daniel G. Brinton, *The Myths of the New World*, 172 f., Philadelphia, 1868. F. Boas, *Indianische Sagen von der nord-pazifischen Küste Amerikas*, 329 sq., 1895.



Methodologically, proof consists not in indicating isolated resemblances, but in tracing a number of complex cultural phenomena over a continuous territory beyond which they are lacking or fragmentary. Only cogent evidence warrants assuming a break in geographic continuity. Thus, Boas rejects Ratzel's theory of an Oceanian-American connection not as inconceivable but as undemonstrated, in the same spirit refusing to ally certain mythological incidents of Oceanian and American mythology: "We desire to find uncontestable evidence of transmission, not alone the possibility or plausibility of transmission; and for this purpose our safeguards [complexity of the traits compared and continuous distribution] must be insisted on."<sup>30</sup>

What, then, are the results based on these premises and the investigations of the Jesup Expedition? The tribes of Northeastern America are proved to have had at one time intimate relations with the Paleo-Asiatics. Especially is there a parallelism of complex folktales between East Siberians and the Indians of British Columbia, while no such similarities ally the East Siberians with the Eskimo, who now occupy an intermediate position. Hence, Boas infers, the Eskimo, coming from the east, must have driven a wedge between these two groups which once formed a continuous major block of population. This comparatively recent contiguity of the Eskimo and the East Siberians explains their sharing many features of economic life. True, the Eskimo did not take over the Asiatic idea of domesticating reindeer, but that is because the tribes near Bering Strait had themselves only acquired reindeer a few centuries ago. Correlating all the findings with those from other sections of the New World, Boas lays down in broad terms the history of American

<sup>30</sup> F. Boas, "Die Resultate der Jesup-Expedition," ICA-P, Separat-Abdruck, 6 f., Wien, 1909. *Idem*, "Mythology and Folk Tales of the North American Indians," JAF 27:381 sq., 1914.

cultures. Basically there are only two areas: the marginal tribes of the Arctic, Northwest, and California, representing the primeval hunting culture in its several local variants; and the Middle American farming civilizations from Peru to Southern Mexico. The remaining cultures are due to Middle American influences blending with the archaic features in varying degree.

These are historical inferences of no mean import, and they have remained fundamental to Americanist research.

Characteristically Boas was not content with his theory of Eskimo migration, but proposed a crucial experiment. Eskimo skulls happen to have several highly distinctive features. Hence, it is proposed, we must excavate Alaskan sites in search of calvarial remains; and should older strata harbor non-Eskimo types, the intrusion of the Eskimo from the outside world would be a demonstrated fact.

One further result of the Jesup Expedition must be cited. Various tribes of the Salish family were shown to have become secondarily assimilated to the higher coastal cultures of British Columbia. Specifically, they had borrowed a clan system to supersede their older family organization. Hence, Morgan's sequence is reversed for this series of peoples and refuted as a general law. Again we note that vital conclusions are the direct outgrowth not of abstract reasoning but of concrete research.

Before leaving Boas' attitude toward diffusion, we must note one other point. In contrast to those satisfied with establishing the *fact* of a historical connection, Boas regards this as merely an initial step. It is important to ascertain *why* traits were borrowed and *how* they were incorporated into the borrowing cultures. What, for instance, is the role of captives, male or female, in introducing novel ideas? Why are some cultural features rejected while others are readily adopted? How are bor-

rowed elements remodeled? What innovations do they evoke? These are the questions Boas studies as "the dynamic conditions of change," "the dynamics of primitive life." They lead to still another extension of traditional field research; in addition to the pigeonholing of facts on the model of our standard monographs we need supplementary data "on the way in which the individual reacts to his whole social environment, and to the differences of opinion and of mode of action that occur in primitive society and which are the causes of far-reaching changes."<sup>31</sup>

#### SUMMARY

It is one of the most difficult tasks to expound Boas' greatness to those who have failed to come into personal contact with him. His achievement is full of paradoxes. Here is a man lacking and scorning any artifice that might attract students, yet he has trained and influenced the greatest number of professional anthropologists. Here is a scholar who controls the ethnographic literature of the world as well as anyone, yet he has never summarized his views in a comprehensive treatise comparable to Ratzel's *Völkerkunde*. His critics suggest an incapacity for synthesis; his intimates know that he forms opinions on all anthropological questions but refrains from utterance when the evidence seems indecisive. That even the provisional syntheses of this independent and erudite thinker would shed floods of light is unquestionable; it is not, however, Boas' method of procedure. It is still more paradoxical that this indefatigable collector, who has consistently preached the totalitarian view of culture, has produced not a single full-length monograph of any one tribe. Notably he has published thousands of pages on the Kwakiutl, but he has never integrated his data in a single

<sup>31</sup> F. Boas, "Evolution or Diffusion," AA 26:341, 1924; *idem*, "The Methods of Ethnology," AA 22:315 sq., 1920.

work. Here, too, he is doubtless deterred by puritanical motives. On the one hand, our duty is to gather the raw facts before they disappear; on the other, when can one be sure of having all the data that would warrant definitive interpretation?

It is this deliberate aversion to systematization that is the despair of many readers and precipitates misunderstanding. The craver of systems cannot understand a scientist's progress from problem to problem without at once generalizing a particular solution achieved. For example, Boas once propounded the view that in British Columbia totemism had evolved through allowing inheritance of the individual guardian spirit. This hypothesis was discussed as though it were a theory of totemism as a whole, an interpretation indignantly rejected by Boas: ". . . it is entirely opposed to the methodological principles to which I hold to generalize from the phenomena found among the Kwakiutl and to interpret by its means all totemic phenomena."<sup>32</sup> This attitude is the scientist's as opposed to the philosopher's; it has been lucidly set forth by Ernst Mach: "To the scientist who always detects new features in every major solution of a problem systematizing and schematizing always appear premature, and he gladly leaves it to the more practised philosophers."<sup>33</sup>

More disturbing to those who share Boas' conception of anthropology is his failure to expound at length the reasons for a change of opinion. Thus, in 1909 he still considers it conceivable that America might have been peopled by the transatlantic migration of ancient Mongoloids coming from Europe. In later discussions this possibility is ignored or eliminated.<sup>34</sup> Again, his earlier

<sup>32</sup> F. Boas, "The Origin of Totemism," AA 18:320, 1916.

<sup>33</sup> Ernst Mach, *Erkenntnis und Irrtum*, p. vi, Leipzig, 1906.

<sup>34</sup> F. Boas, "Die Resultate der Jesup-Expedition," 15. *Idem*, "The History of the American Race," *Annals N. Y. Academy of Sciences*,

accounts credit the British Columbians with a belief in possession. Nothing could be more explicit than the statement: "In the second dance the novice appears wearing a mask, which represents the spirit which possesses him." In later publications this phenomenon figures as belonging distinctively to the Old World: "On the other hand it seems quite foreign to the beliefs of American tribes. . . . The spirits may attack man, but they do not enter his body."<sup>35</sup> So far as I am aware, this discrepancy is nowhere explained.

A more vital matter relates to contradictions in explanatory principles. We have already cited Boas' view that primitive cultures are not stable. This is unquestionably correct, but his application of it to a critique of diffusionism appears puzzling in the light of one of his own major conclusions. He chides the British diffusionists for assuming that "ancient Mediterranean customs could be found at the present time practically unchanged in different parts of the globe."<sup>36</sup> Yet we have found him arguing for an exceedingly ancient connection (*uralte Verbindung*) between the Indians and the Paleo-Asiatics on the basis of mythological resemblances. This obviously implies that the tales have remained stable over a period of thousands of years. If there is a way of harmonizing these two positions, it has not been indicated. Finally, there is a curious indefiniteness with regard to areal range as a criterion of antiquity. On the one hand, Boas objects to making a general principle of this point of view "which, with due caution, may be applied here and there"; and specifically he protests against Spinden's, Wissler's, and Kroeber's reconstruction of American chronology on

21:178, 1912. *Idem*, "America and the Old World," ICA 21:21, 1924. *Idem*, *Scientific Monthly*, 110, 1929.

<sup>35</sup> F. Boas, "The Tribes of the North Pacific Coast," *Annual Archaeological Report*, 1905, 246; Toronto, 1906. *Idem*, "America and the Old World," ICA 21:27, 1924.

<sup>36</sup> F. Boas, "The Methods of Ethnology," AA 22:317 f., 1920.

this assumption. Yet because songs and tales are found universally, they figure in Boas' account as "the primary form of literary activity" and in the same spirit he sets down exogamy as older than totemism. "The recognition of kinship groups, and with it of exogamy, is a universal phenomenon. Totemism is not. It is admissible to judge the antiquity of an ethnic phenomenon by its universality. The use of stone, fire, language, is exceedingly old, and it is now universal. On this basis it is justifiable to assume that exogamy also is very old." From a more recent statement we gather that: "In a few cases it seems justifiable to infer from the world-wide diffusion of a particular cultural achievement its great antiquity. This is true when we can prove by archaeological evidence its early occurrence."<sup>37</sup> However, Boas evidently does not mean that only archeological evidence has cogency since he admits the antiquity of language and song. We are thus left without guidance as to the applicability of the principle. "Here and there" and "in a few cases" provides no acid test without further specification wherein trustworthiness may lie. Repugnance to systematized exposition may thus be carried a bit too far. Like Tylor, then, when he fails to define the sphere of "general laws" as against that of specific history, Boas leaves us here in a methodological quandary.

An objective recital of Boas' achievements is one thing, an appraisal involves as much personal taste as an artistic judgment. Those who cannot divorce ethnology from belles-lettres will find nothing to attract them in Boas, whose bald exposition never aspires to literary graces. But even among those who view anthropology as a science, responsiveness has varied. As Boas wrote in his obituary of Virchow: "There are but few students

<sup>37</sup> *Idem*, *Primitive Art*, 5 f., 301. "The Origin of Totemism," AA 18:323 f., 1916. "The Aims of Anthropological Research," *Science*, 76:609, 1932.

who possess that cold enthusiasm for truth that enables them to be always clearly conscious of the sharp line between attractive theory and the observation that has been secured by hard and earnest work.''<sup>38</sup>

Those ethnologists who crave bold generalizations are certainly doomed to disappointment. Boas' greatness lies not in the systematic elaboration of facts, but in his independent approach to that material, his novel classification of it, his capacity for defining problems hitherto undreamt of, his insistence on a methodologically rigorous solution. From that angle he stands unrivaled, and all his contemporaries seem shallow in comparison.

<sup>38</sup> F. Boas, "Rudolf Virchow's Anthropological Work," *Science*, 16:441-445, 1902.

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# X

## HISTORICAL SCHOOLS: BRITISH DIFFUSIONISTS

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### HISTORICAL SCHOOLS

“History” is a term legitimately applied in several senses. It signifies either the course of events or its description, some scholars restricting it to written reports. The historical ethnologists, however, must largely dispense with documents because they mainly deal with illiterate tribes whose past is at best fitfully illuminated by written sources. These scholars have accordingly been reproached with relying on mere conjecture, but they command a twofold defense. Archeology, so far as it can be used, yields more objective evidence than the biased narrative of ancient chronicles. Secondly, every historian synthesizes his documentary evidence by his interpretation; and the ethnologist may plead for a similar latitude provided that he uses canons of inference which ensure reasonable accuracy.

Our historical schools in anthropology, however, must be viewed historically, i.e., with reference to the condition, actual or putative, that evoked them. They are



in conscious revolt against "evolutionism," rejecting its doctrines as subjective simplifications, hence distortions, of the real events. Actual history is too complex for such neat summaries as Lewis H. Morgan's. As Laufer put it, "Development does not take place according to the subjective classificatory scheme of the ethnological school that has gone astray in evolutionist paths."<sup>1</sup> In principle the avowed historian recognizes that development is intricate, that each people experienced a distinctive set of influences, specifically as a result of unique contacts with neighbors. To determine the nature of such intercourse, then, is certainly an initial and, according to Graebner, the basic problem.<sup>2</sup>

Now the first thing to note once more is that this doctrine was not invented when Ratzel challenged Bastian's elementary ideas, let alone when Graebner and Ankermann delivered their lectures on African and Oceanian culture circles.<sup>3</sup> Tylor, we cannot too frequently insist, explicitly accepted the complexity of culture and repeatedly suggested how the life of peoples had been molded by importations. To be sure, his theory of religion allocated certain beliefs to definite stages in a hypothetical scale, but various less bulky though not less important publications exemplify his faith in diffusion. As for Boas, his work in British Columbia shows above all the interplay of tribal groups; and it is precisely because of such influences that he rejects "simple" explanations of myths as though they had been conceived on the spot as a direct response to nature.

Recent movements known as "diffusionist" par excellence, then, were not the first to propound a metamorphosis due to alien contacts. Nor can we easily distin-

<sup>1</sup> B. Laufer, *Dokumente der indischen Kunst*, 31, 192 f., Leipzig, 1913.

<sup>2</sup> F. Graebner, *Methode der Ethnologie*, 107, Heidelberg, 1911.

<sup>3</sup> F. Graebner, "Kulturkreise und Kulturschichten in Ozeanien," ZE 37:28 sq., 1905; B. Ankermann, "Kulturkreise und Kulturschichten in Afrika," *ibid.*, 54 sq.

guish the moderates from the radicals by the criteria used to establish relationships. Graebner, the methodologist of the German school, has announced two means of determining cultural connection—a formal resemblance neither inherent in the nature of the phenomena compared nor due to geographical causes; and the chance association of a whole series of elements in two regions (“quantitative criterium”). Now these are criteria clearly set forth and applied in Tylor’s discussion of the patolli game. This is not remarkable, for they have probably been applied ever since man began to institute comparisons at all.

The extremists, however, do stress two additional principles. The uninventiveness of the human mind, to which Tylor alludes only incidentally, becomes a cardinal dogma. While these scholars do not deny that a duplicated invention is conceivable, they are so skeptical about human originality as virtually to exclude the assumption. Graebner ingeniously adds the auxiliary point that there are only criteria of connection, none of independent development; hence, at best, parallelism might be inferred at the very close of investigation, from the failure to prove transmission. The second principle, which dates back to Ratzel, is the irrelevance of distance or continuity. Not of course that even the radicals allege action at a distance; but unspanned remoteness is not felt as an obstacle in the face of similarities. As easy-going investigators of faunal distribution invent land-bridges to suit their purpose, so the diffusionists decree at their convenience a former continuity no longer visible.

So far, then, moderates like Boas differ mainly by their greater caution. For them the degree of human or racial inventiveness is as yet unknown, hence cannot be invoked as an ultimate principle on either side; and they treat intermittent resemblances not as worthless, but as

lacking cogency so long as the paths of communication remain obscure. However, we must note another major difference among ethnological historians that is not quite coterminous with the division into a right and left wing. Tylor is content with establishing the transmission of particular features; Boas connects whole cultures but only as a rule in adjacent regions. Ratzel abandons contiguity but concentrates on single elements—types of bow or armor. But a disciple of Ratzel, Frobenius, going beyond his master, traced to a common origin the whole culture of two remote areas, West Africa and Oceania.<sup>4</sup> Graebner, starting from a similar survey, came to work out a scheme for the whole of culture-history. Similarly, Father Schmidt, acknowledging his indebtedness, revised and amplified Graebner's doctrines so as to create a veritably new system; and a recent attempt, elaborating Graebner's and Schmidt's principles, is due to Montandon. Completely independent of the Germans is the pan-Egyptian theory of Elliot Smith and his disciple Perry.

There are thus at present two main schools, a German and a British, which purport to define the course of culture throughout the globe and throughout human existence. To that extent they supplant Morgan's doctrines with a system similarly all-embracing; but avowedly their procedure differs in reconstructing history inductively. The question is how far they have succeeded.

Lengthy discussions of methodology are avoided at this point because the relevant treatises, while estimable contributions to the logic of science, seem ethnologically unimportant.<sup>5</sup> The crucial point in practice is not whether

<sup>4</sup> Leo Frobenius, *Der Ursprung der afrikanischen Kulturen*, Berlin, 1898.

<sup>5</sup> Cf. H. Pinard de la Boullaye, *L'étude comparée des religions*, 2:183-282, Paris, 1925. F. Graebner, *Methode der Ethnologie*, Heidelberg, 1911. Schmidt and Koppers, *Völker und Kulturen*, Regensburg, 1924. E. Sapir, *Time Perspective in Aboriginal American Culture; A Study in Method*, Ottawa, 1916. W. Schmidt, *Handbuch der Methode der Kulturhistorischen Ethnologie*, Münster, 1937.

the phenomena compared should be similar, but whether alleged formal resemblance actually exists. Graebner himself admits the subjectivity of this criterion when he rejects Von Luschan's alignment of Papuan headrests with the Ionic capital. He contends, however, that the difficulty is overcome by the quantitative criterion. Yet obviously this is significant only if the several comparisons hold water: two dozen features of dubious character are less cogent than two beyond cavil. As Graebner says in another context, everything hinges on whether a scholar is facing natural units or artificial constructs of his brain. And Sapir, almost duplicating his phraseology, writes: "The constant danger that besets the investigator is to make historical or psychological actualities out of merely conceptual abstractions."<sup>6</sup>

Accordingly, diffusion is best considered not in the abstract, but in the light of several schemes. In the interest of simplicity rather than of chronology we begin with the British school.

#### ELLIOT SMITH

G. Elliot Smith (1871-1937), a justly distinguished anatomist, at some time decided that ethnology was in a bad way and required a savior. Being a forceful personality possessed of a vigorous style, he soon attracted a group of apostles. To plunge into his writings is unquestionably a stimulating experience. Let no one, however, approach with the picture of British science formed on a reading of Faraday or Darwin. Here there is no humble quest of the truth, no patient scrutiny of difficulties, no attempt to understand sincere criticism. Vehement reiteration takes the place of argument. Elliot Smith is more cocksure than Haeckel, more contemptuous of opposition than Dr. Samuel Johnson. Everything is

<sup>6</sup> Graebner, *op. cit.*, 86, 118, 155. Sapir, *op. cit.*, 38.

grist for his mill, everything is either black or white. This procedure is not limited to ethnology, but there is a vital difference: in physical anthropology Elliot Smith controls the facts, hence—right or wrong—his judgments command respect, while in ethnography his crass ignorance darkens counsel.

Being at one time stationed in Cairo, this reformer of ethnology decreed that Egypt must be the source of all higher culture. Had he tarried on the Euphrates, we may reasonably surmise him to have fathered a pan-Babylonian theory. Be that as it may, his actual scheme rests on a few dogmas that are easily summarized:

(1) Man is uninventive; hence culture arises only in exceptionally favorable circumstances, practically never twice independently.

(2) Such circumstances existed only in ancient Egypt; hence elsewhere culture, except some of its simplest elements, must have spread from Egypt with the rise of navigation.

(3) Civilization is naturally diluted as it spreads to outposts; hence decadence has played a tremendous role in human history.

Of "Natural Man," unstimulated by Egypt, we get an engaging picture. This idyllic creature, honest, peaceable, improvident, was "almost wholly devoid of anything worthy of the name of culture." In 4,000 B.C. religion and social organization, marriage and burial ceremonies, houses and clothes, all arts and crafts except those used to make hunting equipment, were lacking everywhere outside of Egypt and vicinity. Human beings, we learn, lived essentially like the anthropoid apes.<sup>7</sup>

The ancient Egyptians were favored by the growth of wild barley in their country, which led to its deliberate cultivation, the inundations of the Nile prompting the

<sup>7</sup> G. Elliot Smith, *In the Beginning; the Origin of Civilization*, 20-31, New York, 1928.

natives to imitate its process by irrigation. Having to store food, the people invented pottery and granaries, the latter evolving into dwellings. "The leisure enjoyed by men who stored up food in their settled homes" was devoted to inventing basketry, matting, and weaving; and, incidentally, cattle came to be domesticated. Religion rose out of the embalmer's art: the king-engineer who controlled fate by accurately predicting the movements of the Nile was mummified and henceforth treated as immortal. The practices performed to ensure the royal corpse against corruption gave rise to drama and ceremonialism, to dancing and music, also stimulating architecture and carpentry.<sup>8</sup>

This account of Egyptian history can be definitively judged only by Egyptologists; it impinges on ethnography because Elliot Smith virtually denies any independent developments. The American Indians, according to him, lived like apes until the beginning of the Christian era; their first pyramids were erected five or six centuries later as copies of Cambodian and Javanese models, themselves traceable to Egyptian prototypes.<sup>9</sup> American initiation ceremonials and secret societies go back to the mummification ritual of the Nile; Australian totemism and social organization are "the degraded and otherwise modified results of the adoption of alien [i.e., Egyptian] practices and beliefs."<sup>10</sup>

To start with the beginning: What about the ape-like condition of man sixty centuries ago? It is so unwarranted an assumption that at lucid intervals Elliot Smith's unguarded statements completely nullify this cardinal principle. Thus, on prehistoric evidence he credits the ape-like Natural Man who on one page has "neither arts nor crafts beyond the making of implements of the chase" with "an aptitude for pictorial art and craftsmanship." Obviously the second statement di-

<sup>8</sup> *Ibid.*, 30-46.

<sup>9</sup> *Ibid.*, 83.

<sup>10</sup> *Ibid.*, 25, 67.

rectly contradicts the first. As for supernaturalism, Elliot Smith admits that "long before the use of gold," i.e., long before Egyptian civilization could travel round the earth, men believed in imitative magic: "The teeth of ferocious animals were believed to protect the wearer, since they conferred powers of aggression upon their original owners."<sup>11</sup> If so, why could not the Australians have arrived at such conceptions independently? Yet according to the chief apostle of the creed it is probable that "prior to the coming of this [Egyptian] civilization, the native peoples were devoid of any magical or religious practices or ideas."<sup>12</sup>

Obviously an Egyptian origin for any trait does not follow from even the most rabid insistence on human un-inventiveness. What does follow is simply a single center somewhere; the feature, however, might be of Chellean antiquity, a parallel signifying merely the persistence of an extremely ancient cultural element on both the Nile and the Darling. This, for instance, might explain the distribution of animism in time and space without recourse to multiple origins.

It is true that Elliot Smith and Perry categorically deny an "original idea of a soul that persisted after death" except in association with mummies; but they offer no evidence for this amazing allegation.<sup>13</sup>

The pan-Egyptian obsession of these writers runs counter to established historical facts. When two peoples meet there is not an automatically irreversible stream of culture from the higher to the lower. Europeans brought livestock and wagons to the American Indians, but in return they acquired maize, squashes, potatoes, and a host of other plants. The Chinese were not uniformly donors in relation to ruder neighbors, but took over

<sup>11</sup> *Ibid.*, 48.

<sup>12</sup> W. J. Perry, *The Children of the Sun*, 480, New York, 1923.

<sup>13</sup> *Idem*, *Gods and Men*, 69, London, 1927.

seafaring from the Malays, felt from northern herders. As for the ancient Egyptians, in the nascent stage of their civilization, they were little superior to surrounding tribes; hence contact with them certainly implied an exchange rather than one-sided benefactions.

Yet the extremists constantly argue that: (a) some tribe has demonstrably borrowed a particular trait; therefore (b) its inventory of crafts and customs must come from the same source. The logical fallacy is patent. Admitting that the Mexican pyramids are Indo-Chinese and that the patolli game is Hindu, this would prove nothing more than contact of America with Asia; it would imply that other traits *might* have been borrowed by the New World, not that they actually were. In the light of the instances cited, loans in the opposite direction are wholly plausible; indeed, reputable botanists believe that the sweet potato was carried by the Polynesians *from* South America. The reverse conclusion does not follow from contact, but only from the dogma that the aborigines of America, being chimpanzee-like, had nothing to give.<sup>14</sup>

This gross fallacy pervades the entire treatment of the Western hemisphere. Perry proclaims: "It is well-known . . . that the agricultural tribes of the United States owe their customs to Mexico and the neighboring countries. The proof is simple and conclusive. All these tribes cultivate maize, and other southern plants, such as squashes and gourds. Maize is indigenous in Central America, so its cultivation must have been propagated thence in all directions."<sup>15</sup> The preposterous inference is simply an a priori deduction from the principles already expounded. American agriculture comes from Mexico, hence everything in the New World comes from Mexico; but agriculture came to Mexico from the outposts of

<sup>14</sup> Elliot Smith, *In the Beginning*, 29.

<sup>15</sup> Perry, *Gods and Man*, 18.



Egypt; hence ultimately Egypt is the universal fountain-head. As a matter of fact, Americanists increasingly doubt that Mexico is the center of aboriginal husbandry. It now appears wholly plausible (a) that maize may have been raised first in South America; (b) that another staple, such as manioc, may have preceded maize in the New World; (c) that American tillage has several independent foci.<sup>16</sup> The speculations of the British diffusionists collapse on any of these postulates. No doubt they will deny the last and assert that the others involve only minor details: the essential fact for them is "agriculture," and agriculture, no matter where or how it originated in *America*, must have come from abroad. This is, of course, again a priori reasoning, but even apart from that it reveals a further cardinal error.

To the diffusionists "agriculture" is an ultimate irreducible verity; to unprejudiced minds the term illustrates an artificial construct of the scholastic classifier. There is more resemblance between the Ionic capital and a Papuan headrest than between the sowing of cereals and the planting of a banana shoot; and when cultivation implies the laborious extraction of poison, we are again dealing with quite a distinct matter. For some purposes a common label may be convenient, just as at times we may speak of "keeping animals." But such classification does not prove an underlying common reality; bee-keeping is not the same as training elephants or herding horses; and sowing seeds is not equivalent to planting a side-shoot or a tuber, let alone ridding a tuber of its prussic acid. If, then, bitter manioc should prove the oldest cultivated species in the New World, it could not be derived from alien forms of "cultivation" that share with it nothing but the arbitrarily assigned name.

<sup>16</sup> A. V. Kidder, "Speculations on New World Prehistory," ALK, 150 f., 1936. Carl Sauer, "American Agricultural Origins: A Consideration of Nature and Culture," *ibid.*, 291 f. J. Eric Thompson, *Archaeology of South America*, 16, Chicago, 1936.

Elliot Smith and Perry misapply the form criterion still more gravely when they trace a "dual organization" over the whole globe and back to Egypt. They actually equate the division of a society into intermarrying halves with *any* stressing of the number "two": the halving of a village community by a street running north and south is evidence of a "dual organization";<sup>17</sup> and so is the very division into opposing sides at games. How competitive games were to be played without such arrangements is unexplained. Here their claim for a single historical phenomenon can be directly refuted. Whenever one of three exogamous clans becomes extinct—as happened in a Hopi village—two clans remain as a secondary phenomenon that is evidently an independent growth. Similarly, the ranging of Crow Indian men in two rival military organizations about 1870 was simply due to the extinction of several societies recorded among them two generations earlier. The dual grouping is a consequence of special conditions, and is neither conceptually nor historically related to the normal exogamous moieties. Again, the Angami Naga of Assam are divided into moieties that are not now but traditionally were at one time exogamous. However, in one village recently inhabited only by members of one of the old moieties, the people all belonged to either of two clans which intermarried freely but tabooed marriage of clansfolk. That is, these local clans were simply exogamous moieties! Further, in a still more recent period one of these newfangled moieties was broken up into six segments, while the other remained undivided, with the result that a member of one of the new seven clans might marry into any one of six others. Thus, the "dual organization" is demonstrably a fluid phenomenon which disappears and reappears in the course of history.<sup>18</sup>

<sup>17</sup> Perry, *op. cit.*, 56.

<sup>18</sup> J. H. Hutton, *The Angami Nagas*, 110 sq., 125-132, 418 sq., London, 1921.

This case leads us back to the problem of inventive-ness. Obviously, when the extinction of one clan leaves two, the consequent dualism is not a sign of ingenuity but an automatic end result. Accordingly, the question of human or primitive originality does not enter at all. Diffusionists persistently fail to discriminate between difficult technological achievements which are by general admission rarely, if ever, made independently and cultural features that impose little or no strain on the intellect. They further belittle the inventiveness of primitive men, stressing their failure to originate devices that would add to their comfort. It is true that the Australians and Fuegians make shift with inadequate shelter and clothing; but other primitive groups have been more resourceful and certainly did *not* get their technique from Egypt. How could the Egyptians who have never felted themselves, transmit felting to the Central Asiatic nomads? Is it seriously suggested that the Siberian ski and snow goggle, the tailored clothing of Arctic peoples, the snow house of the Eskimo are directly or indirectly patterned on Egyptian models? An intensive study of aboriginal technology reveals many processes that command our profound respect.<sup>19</sup> If Messrs. Smith and Perry neglect them, it is because of their unfathomable ignorance of elementary ethnography. Incredible as it is, when Elliot Smith lists the ape-like peoples of the present time ("almost wholly devoid of anything worthy of the name of culture") he includes the reindeer-breeding, butter-churning, cheese-making Lapps!<sup>20</sup>

Our conclusions as to inventiveness may then be summarized as follows. Human groups, the most sophisticated as well as the rudest, have often failed to invent what in retrospect seem obvious improvements; even the

<sup>19</sup> See, e.g., Erland Nordenskiöld, *Comparative Ethnographical Studies*, 8:1-124, Göteborg, 1930.

<sup>20</sup> Elliot Smith, *In the Beginning*, 20.

Greeks muddled along with a wretched arithmetical notation. Accordingly, we cannot for any people assume an unlimited stock of creative ideas. But from this it is a far cry to deny all significant originality to mankind at large, or to limit it to the ancient Egyptians. Especially should we beware of ascribing to higher civilizations social features and cosmic or religious conceptions which may arise spontaneously, either like the dual organization from external factors or by a simple association of ideas akin to the classifications of language. No one argues that the Shoshone Indians of Wyoming derived their dual number from Greek grammar; in both instances, paired occurrence of phenomena happened to strike the primary speech-making minds as significant and established the dual category. In the same way, such celestial phenomena as eclipses may quite readily prompt a similar explanation in distinct parts of the world. This is not to assert that they *have* evoked independent resemblances, but to insist that the analogy of an eclipse with a devouring and regurgitating monster is not an intellectual feat to be put on the same plane with bronze-casting or the domestication of livestock.

After the preceding remarks it is hardly necessary to comment on the exaggerated role ascribed by Elliot Smith to degeneration as the normal process everywhere outside Egypt. But it is worth while to point out certain chronological implications of the scheme. Since the Indo-Chinese pyramids are used for dating higher American civilizations, our diffusionists are obliged to crowd an incredible number of novel developments within the period of, say, 500 A.D. to 1492. They must assume that a profusion of distinct pottery styles suddenly sprang up in Peru, Mexico, and the southwestern United States; also that innumerable varieties of plants peculiar to America were domesticated during a few centuries, a conclusion at variance with botanical evidence. Further,

the study of tree-rings in the southwestern United States has achieved a trustworthy chronology for this area. It appears that in northern Arizona the hypothetical apemen made pottery by at least 500 A.D.; in the southern part of the state possibly by the beginning of the Christian era. As for the higher civilization of Yucatan, the best authorities find the Maya calendar in full swing in 300 A.D.<sup>21</sup> To turn to another aspect of the case, it is alleged the Indians of the United States did not become farmers until several centuries ago, i.e., they remained in the unclad, houseless, and nonreligious ape-like state until then. If so, their cultural development subsequent to southern stimulation was staggering, for they certainly could not acquire from Mexico what Mexico itself lacked. To single out religion, they might take over Mexican ritual, but not that highly personal faith which the Aztec failed to display. The tremendous differentiation of relevant beliefs and observances would thus have to be crowded into a pitifully short span. And even if this were conceded as possible, we should still not be dealing with decadence but with a wholly novel series of original developments. *Sapienti sat.*

Perry requires no separate treatment. Better read in ethnography than his master, he is incapable of seeing the evidence naïvely so that all the items gleaned are dropped into the preordained pigeonholes. On basic facts of culture history he is as prone to error as Smith: we merely note his crediting the Maya with an "alphabet."<sup>22</sup>

#### RIVERS

Towards the end of his life W. H. R. Rivers (1864-1922) came to form a triumvirate with Elliot Smith and

<sup>21</sup> Kidder, *op. cit.*, 148.

<sup>22</sup> Perry, *Gods and Men*, 75. For an appreciative survey of Elliot Smith's anatomical work, see T. Wingate Todd, "The Scientific Influence of Sir Grafton Elliot Smith," AA 39:523 sq., 1937.

Perry, but he deserves consideration for real achievements which were independent of this association. Rivers was medically trained, investigated the physiology of the senses, and thence turned to experimental psychology. In 1898, aided by C. S. Myers and W. McDougall, he was the first to subject aborigines, to wit, the Torres Straits Islanders, to a thorough set of laboratory tests. Of these researches a recent specialist says that they "might well serve as a model for present-day investigators."<sup>23</sup> They established certain sensory racial differences, but none that indicated a wide chasm between Papuans and other races. Above all, irrespective of particular findings, the approach was exemplary in its scientific caution, its analysis and rejection of popular errors.

Unlike his ultimate allies, Rivers did notable ethnographic field work. In the Torres Straits he developed the most useful technique known for recording kinship nomenclatures and systematically studied the sociology of the Islanders. Later he published a valuable tome on the Toda, one of the simpler peoples of southern India, and a considerable body of new material on Melanesia.<sup>24</sup> These investigations must be rated high, but they suffered from certain deficiencies. Rivers views natives as an outsider, so that his writings lack the intimacy of Rasmussen's accounts of the Eskimo or even of the better monographs on American Indians which consciously strive to afford glimpses of the inwardness of tribal life. Moreover, with a one-sided emphasis on sociological phenomena there goes the virtual neglect of material culture and even of religion. Finally *The Todas*, falling into Rivers' earlier period, ignores the possible influences

<sup>23</sup> *Reports of the Cambridge Anthropological Expedition to Torres Straits*, Vol. II, Cambridge, 1901. Florence L. Goodenough, "The Measurement of Mental Functions in Primitive Groups," AA 38:1-11, 1936.

<sup>24</sup> W. H. R. Rivers, *The Todas*, London, 1906. *The History of Melanesian Society*, Vol. I, Cambridge, 1914.

of higher civilizations in India, where almost any naïve observer would have suspected them.

Topically, Rivers was right in assigning first place to his sociological researches. These must not be judged by the posthumously published work on that subject<sup>25</sup> (based as it was on meagre lecture notes) but by sundry special essays. Rivers veritably revived the sociological study of relationship terms, which had virtually ceased through revulsion from the rash of Morgan's theses. In 1909 Kroeber, while fruitfully paving the way for work on the linguistic categories embodied in kinship systems, denied any social determinants. Five years later Rivers took up cudgels not on behalf of Morgan's scheme but in vindication of his basic postulate that the nomenclatures had sociological correlates.<sup>26</sup> *More suo*, Rivers was carried away by his enthusiasm, proclaiming that every detail of relationship terminology was determined by social conditions, a conclusion certainly at best unproved. But he advanced our insight in several ways. He criticized Morgan's typology—the inappropriateness of the concept of "descriptive" systems as applied to the common Indo-European terminologies. He proved beyond question that Melanesian forms of cousin marriage are functionally related to the designation of kin. Finally, he formulated in a new way the problem, previously broached by Tylor, of how far exogamy was linked with the more usual of Morgan's "classificatory" types. The definitiveness with which such questions could be attacked led to a rebirth of interest in relevant researches, so that vast bodies of material bearing on the basic issues were accumulated and remain valuable irrespective of

<sup>25</sup> *Social Organization*, edited by W. J. Perry, London, 1924.

<sup>26</sup> W. H. R. Rivers, *Kinship and Social Organization*, London, 1914. A. L. Kroeber, "Classificatory Systems of Relationship," *JRAI* 39:77-84, 1909.

their relation to Rivers' conclusions.<sup>27</sup> Naturally, since the terms were presumptively linked with social institutions, all kinship customs, taboos and privileged familiarity, for instance, were scanned so that our knowledge of them progressed by leaps and bounds.

Medically and psychologically trained, Rivers did army service during the War, treating cases of shell shock. His alert and suggestible mind was affected by the rise of psychoanalysis, and on that basis he attempted to ally ethnology with psychology.<sup>28</sup> Whatever he may have added to psychological science in this way, he hardly advanced ethnology; to us, at least, he does not seem to have done more than to paraphrase ethnographic facts in psychiatric argot.

With his ability and energy, his fusion of theoretic interests and field experience, Rivers at one time loomed as the prospective leader of British anthropology. Yet for a variety of reasons he failed to wear Tylor's mantle. First of all, manifold as were his interests, he never made them converge upon culture as such. Unlike Tylor and Boas, he never devoted himself intensively to the study of language; he never mastered the history of culture as a whole, never advanced the study of either technology or comparative religion or art. His rather late incursion into ethnology left him a relative stranger, and he never took steps to acclimatize himself. He fostered the common delusion that classical English anthropology had been unaware of diffusion; and only personal Oceanian experiences converted him to a belief in the complexity of culture.<sup>29</sup> Whether Rivers ever read Tylor's *Researches* or his discussion of the patolli game is a fair question.

<sup>27</sup> E.g., E. W. Gifford, "California Kinship Terminologies," UC-PAAE 18:1-285, 1922.

<sup>28</sup> "Conservatism and Plasticity," *Folk-Lore*, 32:10-27, 1921; *Mind and Medicine*, Manchester, 1919; *Dreams and Primitive Culture*, Manchester, 1917-'18.

<sup>29</sup> *The History of Melanesian Society*, 2:1, 1914.



Even in the special field of sociology he strangely ignored Tylor's treatment of exogamy in relation to classificatory systems. Rivers was often like a modern mariner shouting with glee over the discovery of America.

Another handicap was a lack of mental poise that perhaps still more sharply distinguished Rivers from his great predecessor. In his later work he displayed an amazing autosuggestibility. An idea that sprang into his mind forthwith assumed the character not of an assumption to be tested, but of an axiom from which startling conclusions might be deduced. Thus, Rivers cites several interesting cases of the disappearance of useful arts in Oceania. In one island, which must of course have been reached by water, the natives no longer manufacture canoes; elsewhere pottery, revealed in archeological sites, is a lost art; while in still other localities the bow, once an important weapon, has been abandoned.

Certainly the instances—especially the loss of canoes—are striking; and Rivers made a contribution here because, though Tylor recognized the reality of decline,<sup>30</sup> he rather minimized its likelihood in case of "arts which belong to the daily life." Further, Rivers plausibly explains such decadence by irrational factors: if a craft is practiced—as in Polynesia—by religious officials, their death would suppress it because the proper ritual, a priestly privilege, is a prerequisite. Thus, Rivers gave a new support to Hahn's insistence on the potency of non-utilitarian motives.<sup>31</sup>

These conclusions, however, were not deemed sufficient. With exemplary candor Rivers explains his interest in the matter. He wants to analyze Oceanian culture into several complexes carried over wide regions of the globe, and for such analysis it would often be nec-

<sup>30</sup> *Researches*, 180 sq.

<sup>31</sup> W. H. R. Rivers, "The Disappearance of Useful Arts," *Festschrift tillägnad Edvard Westermarck*, 109-130, Helsingfors, 1912.

essary to assume loss of elements; "and the probability and stability of any analytic scheme will be greatly promoted if one is able to assign motives for the disappearance. . . ." Because even an art so useful as navigation *can* disappear, Rivers at once assumes that it has disappeared over and over again: the Tasmanians may have reached their island in craft superior to their rude balsas, the South Americans may once have owned boats of which no vestige remains, and so forth. The fallacy of this argument is clear. Though men *have* abandoned utilitarian industries, they have not done so everywhere and all the time. As Tylor rightly felt, the *general* course of technology has been progressive rather than retrogressive. The contrary assumption, consistently carried out, might lead to the inference that the rudest hunters of today were once superior to ourselves in the material arts—a conclusion not supported by their prehistoric remains.

In a supplementary paper<sup>32</sup> Rivers advances the idea, equally useful for extremist inferences, that a few immigrants possessed of a superior technology can impose their customs on a large autochthonous population. In characteristic fashion our author combines this conception with his earlier theory of decadence, applying his synthesis to Australia. Australians inter their dead, or put them on platforms, or embalm them, or cremate the corpse. Whence this amazing diversity of an emotionally tinctured practice? exclaims Rivers; only a succession of distinct immigrations could account for such variety, and only a superior material culture would in each case cause the aborigines to adopt the new form of burial. Yet how can this repeated infusion of new strains be reconciled with the racial homogeneity of the present Australians? And if the technology of the newcomers was

<sup>32</sup> "The Contact of Peoples," *Essays and Studies Presented to William Ridgeway*, 474-492, Cambridge, 1913.

superior, what has happened to it? Here is indeed a paradox, a set of intricate problems to delight the heart of a puzzle-solver.

The solution is ingenious. If the immigrants were uniformly few in number, their racial strain would fail to assert itself in the total population. Their material arts must indeed have transcended the level of the natives, but of course useful arts disappear! Each time, then, a group of new immigrants came they degenerated technologically, whence the persistent lowliness of Australian culture as we know it. “. . . it is essential to the argument . . . that this disappearance of useful arts should have taken place in Australia, and on a scale perhaps unrivaled anywhere else in the world.” Burial rites and other irrational elements remained, whence the combination of sociological and religious complexity with technological crudity and physical unity.

This argument is characteristic of Rivers' "historical" thinking. From beginning to end it rests on pure fantasies ingeniously interwoven. The coexistence of several burial practices, with which the discussion starts, is no cause for amazement because it is a common situation among very primitive peoples. To cremate or bury a corpse is not the same as inventing pottery. Californian tribes both inter and burn the dead; Canadians have platform and tree disposal, inhumation, cairn-burial, and cremation. The attitude of peoples toward death varies so much that we cannot predict how tenaciously they would cling to a traditional form of burial. Indeed, Kroeber has plausibly argued that the disposal of the dead is largely a matter of fashion.<sup>33</sup> Certainly the appearance of a superior technology is not the only factor that would cause a change in ritual; the Ghost Dance of 1891 started from the lowly Paviotso of Nevada and was eagerly borrowed by the superior Plains tribes. Further,

<sup>33</sup> A. L. Kroeber, "Disposal of the Dead," AA 29:308 sq., 1927.

conceivably the same early band of immigrants introduced all the modes of disposal, fitting each to a special circumstance, and so forth. In short, alternative solutions abound. There is no real problem, but a sham problem resting on the dogma of aboriginal uninventiveness; and it is solved by interweaving possible but undemonstrated determinants into a scheme supported by not a single verifiable fact.

After this comparatively harmless sample we may forego the intricacies of Rivers' ambitious scheme as elaborated in the second volume of *The History of Melanesian Society*. Hypotheses are here reared upon hypotheses until the bewildered reader asks himself in what possible sense this could be called "history."

Let us, then, briefly summarize the contributions of British diffusionists. Those of Elliot Smith and Perry are probably nil. Rivers did excellent work, but independently of his "historical" reconstructions. He looms in the annals of ethnology for his pioneer work on the psychology of a colored race and, above all, for his investigations of relationship systems and kinship customs.

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# XII

## HISTORICAL SCHOOLS: GERMAN DIFFUSIONISTS

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In the range and solidity of their knowledge, the German diffusionists are incomparably superior to their British counterparts. Prolix and pedantic as Graebner frequently is in the *Methode*, his special papers reveal a wealth of ethnographic information; and his "Ethnologie," discussing the whole gamut of civilization, geographically and topically, explodes many of the traditional criticisms leveled against him. It certainly neither considers only museum objects nor ignores the psychology of diffusion. As for Father Schmidt, he constantly astonishes us by the width of his reading, while his interests embrace technology no less than sociology, comparative religion, linguistics, and prehistory. The mutual relations of these two leaders have been repeatedly set forth in Schmidt's writings; their differences, from some angles significant, may be provisionally ignored when their position is to be contrasted with that of other schools. Let us, however, first note some points of agreement with their contemporaries.<sup>1</sup>

<sup>1</sup> For an appreciation of Graebner, see: Julius Lips, "Fritz Graebner," AA 37:320-326, 1935, and Wm. Schmidt, *Der Ursprung der Got-*

Like Boas, Rivers, and Ratzel, then, the modern German diffusionists oppose as oversimplified the evolutionary schemes of an earlier day and explain the complexity of real history in terms of contact metamorphosis. Development, they further find, is not uniform, so that a people with simple technology may have an advanced social structure or form of worship. We must shun a priori schemes and determine the actual course of events.

With the British school, but to a much lesser degree with more moderate diffusionists, the Germans share a belief in the uninventiveness of man. Independent origins, while abstractly conceivable, are therefore almost always ignored, though Schmidt and Koppers explicitly except the conical roof of Asiatic nomads and of higher hunters.<sup>2</sup> Unlike Elliot Smith, the culture circle theorists propound a multiple development, not a single evolution on the Nile, followed by universal degeneration elsewhere. What unites the Graebnerians with the pan-Egyptians is merely the attempt to present the totality of culture history, an attempt not made by such historians as Boas, Nordenskiöld, Hatt, or Wissler.

The German diffusionists, then, are not the only diffusionists or historians. Their differentia lies in a particular system of culture strata, by which they explain the growth of civilization in all periods and all regions.

Graebner and Schmidt picture primeval man as living in small groups, presumably somewhere in Asia. Isolated and without means of transportation, these populations evolved a number of distinctive cultures (*Kul-*

*tesidee*, 1:743 sq., Münster i. W., 1926; *idem*, A, 1935. For a vindication of both wings, see Clyde Kluckhohn, "Some Reflections on the Method and Theory of the *Kulturkreislehre*," AA 38:157-196, 1936.

Graebner's most significant works, besides those already cited, probably are: "Ethnologie," in *Die Kultur der Gegenwart*, Teil 3, Abteil 5:435-587, Leipzig, 1923; "Die melanesische Bogenkultur und ihre Verwandten," A 4:726-780, 998-1032, 1909; *Das Weltbild der Primitiven*, München, 1924.

<sup>2</sup> Schmidt and Koppers, *Völker und Kulturen*, 488, 533

*turkreise*). As modes of travel improved, the influence of these centers began to radiate mainly in the aggregate rather than by the spread of single elements. When two of these systems clashed, they either blended or one destroyed the other. Lacking boats, the earliest immigrants into America and Africa were obliged to enter by narrow passages, whence they were successively pushed farther by newcomers into the most remote and inhospitable regions, to persist there as marginal populations. Such wanderings successively carried primitive modes of life *as complexes* to the four quarters of the globe. Losses and modifications were inevitable, but they do not preclude identification of the cultural totalities even today. Thus, Graebner relates the "Melanesian bow culture" to the Neolithic of Central Europe because both share pile dwellings, a rectangular ground plan, coiled pottery, a special mode of hafting adzes, and spoons. The lack of cattle and other traits in Oceania is ascribed to geographical deterrents.<sup>3</sup>

These writers are aware of the intricacy of intertribal influences and repeatedly allude to secondary complications. Though Graebner, like Elliot Smith, derives the civilizations of Mexico and Peru from the outside, he does not imagine an irreversible stream of elements pouring in from a single western focus. He concedes that some Polynesian traits may have been imported from America; and the residual parallels of the New and the Old World are not derived from a single source. Again, the African distribution of totemism, headrests and conical roofs is not explained by one early dissemination, but is partly ascribed to fairly recent diffusion.<sup>4</sup>

This very fairness, however, involves the school in a

<sup>3</sup> The exposition given above follows mainly Schmidt and Koppers, *op. cit.*, 64 sq., 71 sq., 111.

<sup>4</sup> Graebner, "Ethnologie," 464, 496.

quandary. They grant that during the thousands of years of culture history any people has been exposed to varied alien ideas. By what process, then, can we recognize the several basic complexes as integral units? And to this epistemological query there is no satisfactory answer. The *Kreise* are before us as ultimate axioms and by ingenious shuffling of their constituents the whole of civilization is explained. But what led to the definition of the complexes?

Graebner began his researches with Oceania, where he recognized six successive layers, which he labeled, respectively, the Tasmanian, Old Australian, Totemic, Moiety, Melanesian Bow, and Polynesian.<sup>5</sup> Of these his first and last are geographically defined; and all scholars would agree that the Tasmanian culture is ancient, the Polynesian recent. But what about the intermediate parts of the scheme? Let us select two distinctive complexes for scrutiny—the Totemic and the Moiety culture.

In Australia the Totemic complex is primarily peculiar to the central and eastern areas. It embraces the following material elements: penis-sheath, stiff bark girdle, conical-roofed huts, dugouts, headrests, spears with stone points or wooden barbs, and spear-throwers. Sociologically, there is totemism, always associated with patrilineal hordes. Burial is on platforms, boys' initiation rites probably involve circumcision, in the decorative style looms a band with marginal triangles or semicircles. Mythology is astral, the sun playing a major part.

Now, first of all, how does Graebner arrive at this combination of traits? Avowedly, he is no longer describing a continuous geographical unit as in the case of Tasmania. On the one hand, he admits that elements of the complex occur probably all over Australia "because of secondary movements." On the other hand, criteria of this culture are said to be lacking in Australia, viz.,

<sup>5</sup> Graebner, *op. cit.*, 449 sq.



the penis-sheath, headrests, and bark girdles; that is, they never spread from Asia beyond Melanesia. The conical roof, too, is not found in Australia, but Graebner suggests that the center post of Australian dwellings may be a survival of the true Totemic type of hut. For such phenomena he offers alternative explanations. Features extending from the north may simply have originated after the main southward movement of the complex; or they may represent local Melanesian differentiations; or they did migrate to Australia but proved unviable because not in harmony with the pre-existing Australian culture. All these explanations are plausible *if* we admit the reality of the Totemic complex to begin with. But why Graebner assumes that this arbitrary association of traits once marked a coherent area in Oceania is not clear. When he traces the complex to remote regions, the persistent elements naturally diminish; for Africa Graebner admits its extreme attenuation. Totemism itself, he concedes, is far rarer there in its hypothetically primary local and patrilineal form, because it has spread widely through secondary transmission.<sup>6</sup> But how do we know empirically what is primary and what is secondary diffusion? We observe merely that certain specific features cohere in particular localities. Only subjective abstraction from the immediate data establishes the hypothetical complex; and other scholars might well combine different features into an equivalent *Kreis*. This is not pure empiricism, but empiricism largely diluted with a priori speculation.

To turn to the Moiety complex, according to the scheme moieties are primarily matrilineal and go with cultivation of the soil. The bearers of this culture raise yams, navigate in plank boats, build gable huts, saw fire instead of drilling it, and wield heavy clubs. The dual organization further accompanies men's secret so-

<sup>6</sup> Graebner, *op. cit.*, 464.

eties, whose members don masks to impersonate and worship the dead. Myths are predominantly lunar, decorative art curvilinear, musical instruments include the Panpipe. Here, again, Australians have been selective borrowers, rejecting farming, masquerades, and musical instruments. When the matrilineal and the totemic circles fuse, the totem groups are arranged in complementary moieties, which may be either patrilineal or matrilineal. Some Melanesians thus have totemic moieties, others multiple totem groups of the matrilineal type.<sup>7</sup>

A West African counterpart is recognized, though this is conceived as a union of the Moiety complex with another matrilineal culture kept separate in Melanesia. Weaving, the varied forms of musical instruments, and metallurgy mark the African equivalent as more recent; it lacks the Oceanian clubs and plank boats. Sociologically, the western Negroes have achieved distinctive innovations by blending totemic and moiety ideas, their totems remaining patrilineal, while exogamy is regulated through the mother's line.<sup>8</sup> Corresponding explanations are offered for America.

Here we face one of the fallacies of the system. It is dogma to treat totems as primarily patrilineal and moieties as matrilineal, and then to say that deviations from this norm must be due to blending.<sup>9</sup> Empirically, North America alone presents so many formidable exceptions that such explanation becomes incredible. Thus, the Southern Siouans have patrilineal moieties; the Hopi lack moieties but have multiple clans with totemic names and matrilineal descent. Of course, the facts can be fitted into the scheme by an auxiliary hypothesis for each deviant, but such supplementary assumptions progres-

<sup>7</sup> Graebner, *op. cit.*, 452 f.

<sup>8</sup> *Ibid.*, 464 sq.

<sup>9</sup> Schmidt and Koppers, 87.

sively weaken the dogma. Thus, Father Schmidt suggests that in America matrilineal moieties emanate from the Athabaskans, who transmitted them in the north to the coastal tribes of British Columbia and in the south to the Pueblo tribes,<sup>10</sup> who are conceived as formerly patrilineal.

Now such an explanation would hardly occur to most Americanists. The Northern Athabaskans, so far as untouched by coastal influences, are notoriously simple folk who completely lack any clan system. This holds specifically for people as far west as the Great Bear Lake. Dr. John M. Cooper, one of our most trustworthy investigators, informs me that his detailed inquiries as to a clan organization among the Chipewyan of the Great Slave district yielded wholly negative results. Indeed, so experienced an observer as Father Morice generalizes the denial for all the eastern tribes from Hudson Bay to the Rockies and ascribes matrilineal clans only to those Far Western Athabaskans "who live in regularly constituted villages, had adopted matriarchy, with all its consequences, *after the example of the coast Indians.*"<sup>11</sup> As for the Pueblos, there has indeed probably been a fusion of the moiety idea with a clan system, but according to a quite different pattern from that assumed by Graebnerians. The Southern Athabaskans—Navaho and Apache—are matrilineal but have multiple clans; it is the Hopi with an "all-penetrating matrilineal clanship system" who lack moieties; while some of the Eastern Pueblos have moieties but with patrilineal descent.<sup>12</sup>

<sup>10</sup> *Ibid.*, 231.

<sup>11</sup> C. B. Osgood, "The Ethnography of the Great Bear Lake Indians," Annual Report, 1931, National Museum of Canada, 74, Ottawa, 1933. A. G. Morice, "The Canadian Dénés," Archaeological Report, 1905, 201, Toronto, 1906; *idem*, "Are the Carrier Sociology and Mythology Indigenous or Exotic?" *Transactions Royal Society of Canada*, Section II, 109-126, 1892.

<sup>12</sup> E. C. Parsons, "The Religion of the Pueblo Indians," ICA 21 (First Part): 140, The Hague, 1924.

In short, though the *Kulturkreis* theorists allow for greater complication than the parallelists and Elliot Smith, they still oversimplify the facts in practice, if not in theory. Tribal crosscurrents are constantly producing effects that cannot be defined by means of a few cultural complexes. Only intensive work in each major area can determine what really took place.

This leads to another point. All the diffusionists from Ratzel on are right in treating the history of mankind as one unit; this implies *ipso facto* that no Monroe Doctrine can segregate America from the rest of the world. Actually Boas and his collaborators on the Jesup Expedition ignored continental boundaries and proved far-reaching relations between Siberians and American Indians. Similarly, Speck and Cooper have traced scapulimancy from northern Europe through Asia to eastern North America; and Gudmund Hatt connects Lapp and Asiatic traits with New World phenomena.<sup>13</sup> If a divinatory practice in Albania can be derived from the same source as its parallel in Labrador, there can be no objection to establishing contacts over any distance whatsoever. The essential thing, however, is to compare only strictly comparable phenomena. Here lies the error common to all the extreme diffusionists: they mistake analogies for homologous features.

This is of course the fallacy underlying Perry's treatment of the "dual organization," an error not even shared by his ally Rivers.<sup>14</sup> Graebner and Schmidt are not so crassly crude as to subsume under one head whatever can be considered in pairs; but they do fail to see that a moiety system need not be conceptually the same in Australia and in New York and that it can arise independently in distant regions. And as with moieties, so with a

<sup>13</sup> F. G. Speck, *Naskapi*, 158 f., Norman, Oklahoma, 1935. John M. Cooper, "Scapulimancy," ALK, 29 sq., Berkeley, 1936. Gudmund Hatt, *Arktiske Skinddragter i Eurasien og Amerika*, Copenhagen, 1914.

<sup>14</sup> W. H. R. Rivers, *Social Organization*, 31, New York, 1924.

number of other concepts. One feature shared by the marginal Fuegians of South America with Graebner's Old Australians is the type of dwelling, a wind-screen or a dome-shaped hut.<sup>15</sup> These structures, however, are so crude and architecturally undefined that their designations are only classificatory labels; and because the dwellings are so simple they may have been invented over and over again. When Koppers pictures the very origin of human habitations, he asks, "What was more obvious than to perfect these natural protective roofs" (viz., trees and shrubs)? If the procedure is so obvious to primeval man, it can evidently have occurred also to his descendants, so that from its distribution we can infer nothing as to dissemination. To take another Graebnerian trait, "skin clothing," a hunter is very likely to utilize the skin of his quarry, and no historical conclusion seems warranted from this common practice.

To summarize the main objections to the scheme: (1) It is not clear how the fundamental complexes are established as historical realities; (2) the component traits—including such vital elements as the moiety principle—are in part classificatory devices without historical reality; (3) some of the elements may very well have arisen independently; (4) the complexity of actual events is too great to be described by the interaction of a small number of cultures, though their deft dialectic manipulation can of course give a self-consistent scheme through the introduction of auxiliary hypotheses *ad hoc*.

At this point we must take cognizance of an interesting French offshoot of the Graebnerian movement. It emanates from Professor George Montandon of the *École d'Anthropologie* at Paris.<sup>16</sup> Originally a physical anthropologist, this author broaches ideas obviously tintured

<sup>15</sup> Schmidt and Koppers, *op. cit.*, 80, 439 sq.

<sup>16</sup> George Montandon, *L'ologénèse culturelle; traité d'ethnologie cyclo-culturelle et d'ergologie systématique*, Paris, 1934.

by his earlier training. To him the *Kulturkreise* appear as the conceptual equivalents of races, though he expressly repudiates the idea that races and cultures must coincide. As somatology no longer clings to a geographical limitation of race, so ethnology must break with the tradition of defining its major units in territorial terms. Thus he accepts the notion of *cycles culturels* consisting of elements that remain associated *par compagnonnage traditionnel* rather than by an organic affinity. In short, Montandon deliberately enrolls himself under the Graebnerian banner.

The biological flavoring of this variant appears in the distinction of "higher" and "lower" samples of civilization. Beyond the primeval stage, Montandon ranges all cultures in either the *rameau tardif* ("backward branch") or the *rameau précoce* ("precocious branch"). The former grow slowly but surely, culminating in modern civilization; the latter are capable of much complexity but lose themselves in blind alleys. Under the spell of phylogenetic theories the author is evidently applying to ethnology what zoologists tell us about anthropoids in contrast to hominids.

This logically precipitates a relapse into the subjective evaluations of the Lubbock period. As proof of the merely relative and specialized development of higher American Indian cultures Montandon cites "the spiritual aberration attested by excessive human sacrifice." He concludes: "Such a civilization was bound to collapse." But, apart from the absence of major human sacrifices in Peru, the ritual killings of the Aztec seem paltry indeed beside the wholesale massacres of European warfare. Science has nothing to do with such rank subjectivism.

Disregarding these quaint idiosyncrasies, we readily concede to Montandon some of the strong points of German diffusionism. He, too, views mankind as a connected

unit and seeks to combine ethnographic with archaeological facts. He deserves credit for being less intransigent than his compeers on the subject of independent development. In principle he even regards it as an open question whether whole cultures may have become similar independently. As to special parallels, his discussion of the coiled basketry (*vannerie spiralée*) of Australia and Fuegia is instructive: they could be safely assigned to one center only if there were resemblances of complex varieties and shades (*nuances compliquées*). With this principle not even the most moderate of ethnological historians could quarrel. Montandon likewise escapes the crass monism of Elliot Smith by insisting that a culture, instead of arising in one definite point, may result from a fusion of traits within a larger area. The documented facts about Egypt, Babylonia, and China support this view.<sup>17</sup>

On the other hand, Montandon shares the characteristic errors of diffusionists. He lightheartedly assumes that "totemism"—at best a convenient label—is a phenomenon of unitary character and origin. Surely its variations are more remarkable than those of Australian and Fuegian basketry? Finally, Montandon is no clearer than the German theorists in explaining how the cultural cycles are derived and why it would not be equally legitimate to combine other features into comparable complexes to be traced over the globe.

But we must not allow skepticism to blind us as to the real merits of the Graebnerian movement and we must specifically repel certain unfair strictures. For example, it has been alleged with vehemence that Graebner's outlook is mechanical, excluding an understanding of the dynamics of diffusion or indeed of any psychological aspect of social life. This is a grave error. Minds that see only heroes and villains in life naturally indulge

<sup>17</sup> Montandon, *op. cit.*, 41, 501.

in such unrealistic antitheses as History *vs.* Functionalism or History *vs.* Psychology. Graebner and Schmidt are avowedly interested, above all, in the sequence of events, but this does not automatically bar a functionalist or a psychological approach in principle.

Looking, for example, into one of Graebner's later publications, we find him facing—precisely as did Boas, Thurnwald, and Radcliffe-Brown—the process of contact metamorphosis.<sup>18</sup> He shows that mechanical juxtaposition does not inevitably evoke a transfer of arts and beliefs; that borrowing proceeds selectively, with varying speed for elements of distinct categories; that some phenomena have an inherent affinity for others, predisposing to assimilation, while other traits are negatively correlated and bar or hinder adoption. Finally, Graebner indicates how borrowed elements acquire novel significance among the recipients: how lunar myths turn into vegetation myths, how curvilinear designs adapt themselves to a rectilinear style, and so forth. The constantly reiterated stricture that Graebner deals with transmission in a purely mechanical way is as baseless as the gibe about his exclusively museological outlook. Goldenweiser, for instance, alleges that the interrelation of associated traits is “quite beyond Graebner's horizon”; and by contrast he praises Rivers for observing “the psychological interplay of cultural features.”<sup>19</sup> As we have just indicated, these are precisely the points made by Graebner himself, and Goldenweiser's critique rests on ignorance of the “Ethnologie.” All that can be urged is that Graebner evinced a disproportionate preference for historical reconstruction; but that surely is a matter of taste.

In Father Schmidt's writings, too, psychological

<sup>18</sup> Graebner, “Ethnologie,” 577 sq.

<sup>19</sup> A. A. Goldenweiser, *History, Psychology, and Culture*, 84, 150, New York, 1933.



points of view are far from negligible. While he is not always willing to curb irrelevant ethical appraisal, his attitude toward primitive races is exemplary in its general appreciativeness. In the matter of native endowment he is on the side of Waitz as against Klemm. More significant still is his unremitting insistence on the individual variability of primitive man—in opposition to the traditional view that on simpler levels personality is merged in society.<sup>20</sup> Here once more Schmidt's views coincide with those of Boas; and it may be noted that Graebner expresses himself to the same effect, if less obtrusively.

Further, both Graebner and Schmidt—contrary to superficial criticisms—recognize an interrelation of cultural phenomena. This is an inevitable consequence of their following Grosse's correlation of family types with economic activities,<sup>21</sup> a position that verges on economic determinism. A *Kulturkreis* is not a random series of traits; in part the organic bond uniting the concomitants is specially stressed. Masks, for instance, are treated as a natural correlate of a spirit cult; the realism of African carvings is derived from their mimetic magical purpose; the totemistic world-view naturally expresses itself in animal figures.<sup>22</sup> Again, whatever one thinks of Father Schmidt's account of the Moiety complex, it bristles with correlations on the model of that good old functionalist, Bachofen. In its primary form he derives matrilineal descent from feminine tillage and links it with feminine ascendancy, girls' puberty rituals to the exclusion of boys' initiations, and female deities.<sup>23</sup>

Finally, in spite of themselves, these arch-historians

<sup>20</sup> See e.g., W. Schmidt, *Der Ursprung der Gottesidee*, Band II, 2. Abteilung: 173 sq., Münster i. W., 1929; *Völker und Kulturen*, 39, 59.

<sup>21</sup> Ernst Grosse, *Die Formen der Familie und die Formen der Wirtschaft*, Leipzig, 1896.

<sup>22</sup> Graebner, "Ethnologie," 556, 562 f.

<sup>23</sup> Schmidt and Koppers, *op. cit.*, 86 sq., 264 sq.

and arch-critics of parallelism firmly believe in evolution. For, to recognize that cultures change in time *and* to see single traits as organically related is to admit the possibility of a definite sequence. Schmidt, to be sure, has a phobia of the word "evolutionism," but when he lays down "the stages of the whole development" (*Stufen der ganzen Entwicklung*) of matrilineal institutions, it is mere quibbling to deny that he throws himself into the arms of Evolution. Schmidt differs from Morgan mainly in denying universal parallelism, *unilinear* evolution. In other words, he believes that only certain peoples pass through a matrilineal condition. Within these groups, however, his scheme implies clear-cut evolution. Feminine tillage ushers in an economic revolution, producing matrilineal descent; men first retain their independence by mere visits to the homes of their wives; later they adapt themselves to the new conditions by matrilocal residence, which in turn favors monogamy, the cult of a female deity, girls' puberty rites, gynaeocracy; there follows bride-service with strengthening of avuncular authority; service, however, passes into bride-purchase, with a reversal of parts, woman being now degraded to the status of a chattel. This result is promoted by subversive masculine organizations, the "secret societies," whose mummers terrorize women and substitute worship of male ancestors for the cult of a human goddess.

This sequence, to be sure, does not impress us as valid. Until these events are actually demonstrated somewhere on the globe we must reject the scheme as not a whit more empirical than Morgan's. But unconvincing as it is, the underlying idea remains sound: certain phenomena *are* connected so that the presence of one renders the occurrence of another more probable. Only this reacts on the basic assumption of radical diffusionism, the virtual exclusion of independent multiple occurrences. For let us take the nascent stage of the assumed Moiety

complex. Women have just begun to plant; then the species planted will begin to spread, as in historic times; and naturally they will do so as an aspect of feminine responsibilities. Now, *ex hypothesi*, tillage by women evokes matriarchy; hence the transmission of the first species cultivated prior to the development of the whole Moiety complex in the original focus will *repeatedly* produce similar consequences and "the steps of the entire development" may be traversed not once but over and over again. It is not possible to couch functional relations in temporal terms without postulating the possibility of parallelism, a limited rather than a universal parallelism, yet parallelism for all that.

Thus Graebner and Schmidt are by no means so intransigent as appears from selected facets of their writings; and a reconciliation with the views of many contemporary and supposedly hostile colleagues is not at all barred.

In justice to Father Schmidt, he must not be considered solely or even mainly as a follower of Graebner. Even in his championship of the culture circle concept, he is no satellite, but an independent thinker who materially departs from his predecessor's tenets. Thus, he regards not Tasmanians but the Pygmies as the nearest group to cultural origins among races extant. Let us note that Schmidt excels Graebner in his sense of cultural totality so that when he deals with a geographically delimited population—say, the Central Asiatic nomads—his picture is at once vivid and sound. His conception of the rise of higher civilizations is also noteworthy from the same angle; and their derivation from a fusion of hoe-tillage, animal husbandry, and specialized craftsmanship seems essentially convincing.

Among the important by-products of Schmidt's anti-parallelism is his conclusion that very primitive groups may have a conception of a Supreme Being, the core of

the argument in his notable work *Der Ursprung der Gottesidee* (6 vols., 1926-1935). In our opinion the contention is borne out by the facts; and though Schmidt had a forerunner in Andrew Lang, the Austrian scholar's amazing erudition has done much to elucidate this point. One qualifying comment is necessary. We must not ignore the significant fact that the notion frequently fails to dominate religious consciousness. The Ona, e.g., admittedly solve most of their problems by an appeal to shamanism rather than to their otiose high-god. In other words, their capacity to conceive a Supreme Being stands vindicated, but it is rather an intellectual achievement than a phenomenon of practical importance. As Kierkegaard might say, Schmidt examines the natives in philosophy and then gives them a high mark in religion.

Ethnology owes much to Schmidt for the establishment of *Anthropos*, a journal second to none in the field. With unsurpassed energy Schmidt enlisted the services of missionaries scattered over the globe and thereby secured priceless descriptive reports from men resident in remote regions for a long span of years, hence thoroughly conversant with the customs and language of their native parishioners. Major contributions of this sort have been published in monographs of the "*Anthropos-Bibliothek*." The journal has not by any means been restricted to missionaries, but has numbered among its contributors such scholars as Birket-Smith, Ernst Grosse, Berthold Laufer, A. L. Kroeber, F. G. Speck, and Erich von Hornbostel.

While Schmidt has never studied natives in the field, he has directly prompted some very important investigations. Realizing the significance of the most primitive cultures, he has sent some of his disciples to the Pygmies, others to the Fuegians. The most conspicuous result of these researches is Gusinde's magnificent monograph on

the Ona,<sup>24</sup> now supplemented by his parallel study of the Yaghan. It is worth noting that Schmidt's theoretical interest in individual variability has also stimulated observations along these lines.<sup>25</sup>

Anticlerical critics have suggested that the field work undertaken under Father Schmidt's auspices has been unduly colored by Catholic or personal prejudices. This is an unfair criticism; let him that is without bias cast the first stone. Schmidt has naturally exerted a deep influence on his pupils, as any scholar of strong personality and marked attainments is bound to do. There is no evidence, however, that the results have been twisted except through the common foibles of humanity. Certainly some of his disciples' findings do not harmonize with his conclusions. For example, it is one of his pet views that the Pygmies represent one basic culture, yet in the single Congo area Father Schebesta has observed much diversity, due to borrowings from different Negro groups, so that the possibility of reconstructing a proto-Pygmy world culture seems definitely lessened.<sup>26</sup> We must therefore exonerate Schmidt of an unjust accusation. He doubtless errs at times from overindulging with a virtuoso's gusto a natural gift for dialectics, but it is unfair to impugn his good faith.

Our final balancing of the books thus leaves the German diffusionists with very considerable assets. What Elliot Smith attempted with amazing lack of information, they have attacked with much fuller knowledge; and what Ratzel left largely undefined, to wit, a generic world-wide intercourse, they have formulated in a set of definite historical problems. Their initial postulate is acceptable. Man did at one time occupy a restricted territory; and when he spread he unquestionably carried

<sup>24</sup> Martin Gusinde, *Die Selk'nam*, Mödling bei Wien, 1931; *idem*, *Die Yamana*, 1937.

<sup>25</sup> W. Koppers, *Unter Feuerland-Indianern*, 216 sq., Stuttgart, 1924.

<sup>26</sup> Paul Schebesta, *Bambut; die Zwerge vom Kongo*, Leipzig, 1924.

with him part, at least, of his cultural inventory. Americanists such as Boas, Wissler, and Kroeber explicitly concur in this assumption as applied to the earliest immigrants to the New World. When, therefore, the Graebnerians allege the survival of some of these traits in Tierra del Fuego, they are not talking arrant nonsense. It has, indeed, been objected that cultures, being fluid, could not maintain their status through millennia; but the matter of stability is a moot problem, and Cooper has argued cogently that very rude cultures have a tendency to persist, as suggested by prehistoric findings.<sup>27</sup>

Let us, however, mention specific questions illuminated by the Graebnerian approach. One problem to which the German diffusionists have fruitfully directed attention is the possibility of an ancient contact between Tierra del Fuegian and Californian (or Great Basin) culture.<sup>28</sup> Some of the parallels cited are of the most specific nature, notably, the obligatory use of a head scratcher at a ceremonial occasion and the notion of pristine immortality, ensured by washing a dying person. We may add the striking parallels of a Yaghan myth and the Californian story of Wolf and Coyote, both of which contrast a benevolently inclined hero with a marplot brother, resemblances not nearly so pronounced between the Yaghan Two Brother Myth and that of other South Americans. Possibly still more striking is the occurrence of the Lecherous Father motif among the Ona of Fuegia and the Californians (and other North Americans); in both areas a man, lusting for his daughters, feigns illness, predicts his death, directs his survivors to leave his body unimpeded, and urges them to leave and marry a man he describes; after their departure he rises, meets

<sup>27</sup> John M. Cooper, "Analytical and Critical Bibliography of the Tribes of Tierra del Fuego and Adjacent Territory," BAE-B: 227, Washington, 1917.

<sup>28</sup> W. Schmidt, *Der Ursprung der Gottesidee*, Band II, 2. Abteilung: 1031 sq., 1929.

them from the opposite direction, and attains his end. These are not vague hints of intercourse in all sorts of directions after the fashion of Ratzel; here a differential relationship is alleged for two widely remote areas and supplementary evidence in support has been accumulated.

It is also directly to the influence of the *Kulturkreislehre* that we owe a number of other promising researches. Von Hornbostel supports the Fuegian-Californian relationship on musicological grounds; and he has made of Melanesian-Peruvian intercourse at least a debatable question by his comparison of the absolute pitch of the Panpipes played in these regions.<sup>29</sup> Solid investigations have thus resulted from the stimulus of Graebner and Schmidt.

Our conclusion, then, is not to forego the reconstruction of human history in its entirety, but to pursue Graebnerian aims after purging his scheme and his methodology of their unsound elements. We must use only rigidly definable concepts and realize more keenly how complex is historical reality. Ours should be the caution of a geologist who, having established his strata in one continent, does not promptly leap to their identification with comparable phenomena elsewhere. What Graebner and Schmidt have failed to do is to work out American, Asiatic, and African history *naïvely*—independently of the originally constructed Oceanian stratification. The sounder approach will be to work out the sequence for each area in complete independence of the pigeonholes suitable elsewhere and ultimately to combine all findings in a world scheme.

<sup>29</sup> Erich M. von Hornbostel, "Fuegian Songs," AA, 38:357 sq., 1936. *idem*, "Ein akustisches Kriterium für Kulturzusammenhänge," ZE, 43:601-615, 1911.

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# XII

## FRENCH SOCIOLOGY

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In France, anthropology took a course of development distinct from that in other countries. It was a Frenchman, Boucher de Perthes, who inaugurated the epoch-making advances of prehistory; and his continuators, from Lartet and de Mortillet to l'abbé Breuil, have remained pre-eminent. Man as a biological organism has also stirred French enthusiasm for many decades, as the names of Broca, Topinard, and Boule testify. But for some inscrutable reason the arts and manners of living peoples have attracted little interest. There were French colonies with Oceanian and Negro populations, but the accounts published of them long remained few in number and inferior in quality to the comparable reports of British or German officials. As for scholars trained to observe in the field—until lately there were none. The remedy came from an unexpected quarter. It was not ethnography that stimulated the theory of culture, and through it other disciplines. On the contrary, the impulse to field research finally emanated from philosophy. The Institut d'Ethnologie, whose *Travaux et Mémoires*, issued since 1926, at last represent the equivalent of publications in



other countries, was sponsored by three men,—Professors Lucien Lévy-Bruhl, Marcel Mauss, and Paul Rivet. Of these, only Rivet can be reckoned an ethnographer. Lévy-Bruhl is a philosopher by training, while Mauss ranks as the successor of Émile Durkheim, who was indeed the founder of a sociological school but had both studied and taught philosophy. It is, above all, Durkheim, Mauss, and Lévy-Bruhl who have affected theoretical discussion in other countries, and accordingly to them we now turn.

#### DURKHEIM

Émile Durkheim (1858-1917) looms as the author of several significant sociological treatises, such as *De la division du travail social* and *Le suicide, étude sociologique*. Ethnologically he became important in 1898 by founding *L'Année sociologique* which, while embracing a variety of topics, consistently stressed the cultures of illiterate peoples. It presented detailed and extremely careful analyses of ethnographic literature, as well as original articles on such definitely ethnological subjects as magic, Australian marriage classes, and the origin of incest. While most of these essays expounded the tenets of the editors, outsiders were not barred, even Ratzel figuring in one of the earlier volumes. Thus, Durkheim provided a yearly survey of the literature on all phases of civilization, not excluding technology and linguistics; and in that period after Tylor's prime when theory was languishing or sprouting in hidden nooks, *L'Année sociologique* offered a welcome opportunity for the airing of basic problems. Durkheim's own contributions, both as memoirs and reviews, are distinguished for their range of knowledge and penetration, as in his comments in the initial yearbook on Kohler's treatise *Zur Urgeschichte der Ehe*.

The preface to the opening issue of the series clearly

expounds the founder's programme. He wished to familiarize sociologists with those concrete findings of the social sciences which might provide materials for a sociology of the future. On the other hand, the benefits were not to be one-sided. History, more particularly, was suffering from the lack of a comparative point of view to be supplied by the sister discipline. "It is thus that Fustel de Coulanges, despite his profound insight into historical matters, misunderstood the nature of the *gens*, seeing in it only a vast agnatic family,—because he knew nothing of the ethnographic parallels of this family type." A primary aim of Durkheim, then, was to synthesize the two sciences, to combine the perspective of the one with the documentation of the other. "Fustel de Coulanges was fond of repeating that the true sociology was history; nothing is more certain provided that history is worked sociologically."

Durkheim, then, does not disdain history; but his special interest lies in the determination of types and laws, without which the facts would lack significance. This position explains, respectively, his admission and his rejection of historical material. He deliberately excludes what is not amenable to comparative treatment—historic individualities, "innovators of every type," the biographical element—for these, we are told, lack utility for the sociologist. Here is a highly distinctive element of Durkheim's philosophy.

Apart from *L'Année*, Durkheim's contribution to ethnology is mainly embodied in *Les formes élémentaires de la vie religieuse* (Paris, 1912), a work that has played a curious role. It has profoundly influenced several English-speaking writers, yet Father Schmidt correctly states that "possibly no book has reaped so many eulogies in detail, yet has been so generally repudiated in its main propositions."<sup>1</sup> Goldenweiser, for instance, after

<sup>1</sup> Wm. Schmidt, *Der Ursprung der Gottesidee*, 1:579, 1926.

paying court to the author's "wisdom, scholarship and noted brilliancy," categorically rejects his conclusions on the origin of religion and the relation of the individual to society: "Sharp as is the author's wit and brilliant as is his argumentation, one closes the book with the melancholy assurance that Durkheim has left these two perennial problems where he found them."<sup>2</sup> Why, one naturally asks, is there no illumination from brilliance? This is surely the sort of critique that fails to explain what we wish to understand,—the nature and cause of a writer's influence. In order to penetrate the paradox we must go back to Durkheim's basic aims, more obvious from *Les règles de la méthode sociologique* (1894; 6th edition, Paris, 1912) and some shorter articles than from his major work; and we must try to bring him into relation with his contemporaries and immediate predecessors.

Culture had been defined by Tylor as embracing those "capabilities and habits acquired by man as a member of society." This implied, then, a distinguishable field of knowledge demanding a separate science with distinctive procedures. But ethnologists were by no means quick in visualizing the implications. Their tardiness becomes apparent when we scan the literature of about 1915, which confronts us with a veritable cornucopia of pronouncements, each vociferously contending for the autonomy of ethnology. Rivers, while not in principle spurning an ultimate psychological interpretation, insisted that sociological (= cultural) phenomena must be explained first in sociological terms, not in those of a science dealing with simpler data. Hocart argued that "the ever-changing and endless variety of custom and belief" could not be derived from the constant mental traits of humanity. According to Wissler, psychology could teach the ethnolo-

<sup>2</sup> A. A. Goldenweiser, *History, Psychology, and Culture*, 373, New York, 1933.

gists what is innate but could never solve his specific problems: "All the knowledge of the mechanism of association in the world will not tell us why any particular association is made by a particular individual,"—why some one invented the bow or inaugurated exogamy. In poignant sentences Kroeber outlined the sole end of ethnology as the study of culture regardless of organic phenomena; its sphere is the social, the individual having merely illustrative value.<sup>3</sup>

Durkheim had proclaimed this independence of sociology more than two decades earlier and had drawn the logical inferences from it. Because sociology concerns itself with a distinctive sphere, explanations by any science dealing with simpler data are inapplicable. Specifically, psychology is as impotent for the purpose as are physics and chemistry to explain organic facts. Hence the conclusion: "The determinant of a social fact must be sought among antecedent social facts, not among the states of individual consciousness."<sup>4</sup> This explains the recurring emphasis on *group* ideas in contrast to individual ideas, a point later amplified by Lévy-Bruhl. To quote: ". . . whatever is social consists of representations, consequently is a product of representations."<sup>5</sup> At this point we parenthetically recall that Tylor partly conformed to Durkheim's principles in correlating parent-in-law avoidance with forms of residence, employing one cultural datum to shed light on another. It is only when he goes behind this correlation that he resorts to explanations of a psychological order, such as abound in the writings of lesser anthropologists of the era.

<sup>3</sup> W. H. R. Rivers, *Kinship and Social Organization*, 92, 1914. A. M. Hocart, "Psychology and Sociology," *Folk-Lore*, 115-137, 1915. Clark Wissler, "Psychological and Historical Interpretation for Culture," *Science*, 43:193-201, 1916. A. L. Kroeber, "Eighteen Professions," *AA* 17:283-288, 1915.

<sup>4</sup> *Les règles de la méthode sociologique*, 124 sq., 135.

<sup>5</sup> Durkheim, "La prohibition de l'inceste," *L'Année sociologique*, 1:69.

To return to Durkheim, he not only offered a new branch of knowledge, but placed it on a footing of dignity. It was to rival the objectivity of the older sciences, starting with directly observable facts, eliminating the subjective judgments of values to which Lubbock had fallen prey, rigorously applying such methods of proof as were consistent with the nature of the data. Such objectivity had indeed been achieved by some earlier and contemporary ethnologists; but probably none of them had so explicitly set it forth as a matter of principle. The appeal of such an approach to scholars of a certain mentality is thus readily understood.<sup>6</sup>

But *Les règles de la méthode sociologique* offered more than a programme. There were specific conclusions. Though classed as an "evolutionist," Durkheim at least in theory revolted against the idea of unilinear evolution, which he deprecated as oversimplified: there is not one species of society, but a number of qualitatively distinct types. Historic development breaks up into "a multitude of fragments which, being specifically different from one another, cannot be united continuously." Durkheim certainly does explain in parallelist fashion resemblances among remote peoples as symptoms of a definite stage (*symptomatiques d'un certain état social*).<sup>7</sup> Nevertheless, he realizes better than many of his contemporaries the diversity of primitive culture, hence the absurdity of lumping together Homeric Greeks, Zulus, and Iroquois. This position is well expressed in Durkheim's review of an essay by Steinmetz: "People argue as though the so-called savages or primitives formed a single identical social type."<sup>8</sup>

With this at least intermittent historic sense is coupled an insight into cultural dynamics. Durkheim knows

<sup>6</sup> *Ibid.*, 52, 54, 159 sq.

<sup>7</sup> *Ibid.*, 26, 96, 147, 117.

<sup>8</sup> Durkheim, *L'Année sociologique*, 4:341, 1901.

that social phenomena are not normally created by deliberate planning. He also neatly distinguishes between the utility of an institution and the historical causes of its origin. Phenomena may exist without serving a vital need, either as survivals or because they never had a useful function. What is more, the function may change secondarily. In this connection we may add that in a paper already referred to there is a timely warning against accepting aboriginal explanations as anything but afterthoughts devised to sanction a pre-existing practice. "We know how these theories are fashioned; they are required not to be adequate and objective, but to justify practice."<sup>9</sup> Thus Durkheim expresses the very idea elaborated by Boas.

As a philosopher, moreover, Durkheim viewed current concepts with keener criticism than was common among the ethnologists of the nineties. He saw both the irrelevance of race for the elucidation of culture and the lack of any instinct to achieve progress. He warned against vague catchwords that mask vital differences: because two peoples avoid plural marriages, it does not follow that their "monogamy" represents the same phenomenon; everything hinges on whether such restraint is obligatory on principle or merely the result of natural conditions.<sup>10</sup>

There are, it is true, less attractive features in Durkheim. The desire to free sociology from the yoke of other sciences precipitates a quite unnecessary antagonism to the psychological approach. There is also a curious anomaly. Though the primary purpose is to vindicate the autonomy of sociology, though only certain types of logical procedure are held compatible with the nature of sociological data, the writer cannot rid himself of the prejudice that the new science, like its predecessors, must

<sup>9</sup> Durkheim, *L'Année sociologique*, 1:55, 1898.

<sup>10</sup> *Les règles*, 48, 112 f., 120, 129.

aim at general laws. Finally, he postulates as the simplest hypothetical form of society the unsegmented "horde,"—"a social aggregate comprising none other that is more elementary, but which splits up immediately into individuals." This sort of society may not be demonstrable now or in the historic past, but from the combination of such independent hordes must have sprung the historic clan system, each horde becoming one segment or clan of the new totality. What is curious about this is the deliberate exclusion of the individual family as a definite unit preceding the clan organization, a dogma contrary to all recent research, but constantly recurring in Durkheim's writings. Here, as in his conviction, elsewhere set forth, that matrilineal necessarily precedes paternal descent, Durkheim remained an evolutionist of the old school, relapsing into the parallelist error of treating Australian and American societies as rungs of one ladder.<sup>11</sup>

These defects, however, must be viewed in historical perspective. Durkheim's championship of sociology naturally led to militancy against psychology as the science that threatened to keep the new discipline under its thumb. At the same time scientific respectability had to be maintained at all costs—hence the insistence on laws. As for the misconceptions of the clan and family, they formed the common creed of the period. On the credit side, Durkheim was on several vital points abreast of the insights in process of achievement by Boas and very definitely in advance of most post-Tylorian ethnologists. Here was a man of high seriousness, filled with the sense of his mission, supported by a wide familiarity with the social sciences, and striving for major generalizations such as had apparently ceased to appear from the ranks of ethnology itself.

This, then, is the background for Durkheim's major

<sup>11</sup> *Ibid.*, 102 f. Cf. E. Durkheim, "La prohibition de l'inceste et ses origines, *L'Année sociologique*, 1:1-70, especially 10 f., 22 f., 28, 53, 1898.

treatise. He attempts to interpret religion as a *social* phenomenon, choosing for his specific theme the religion of Australians because to them he ascribes the lowest and simplest social organization, viz., a clan system.<sup>12</sup> Accordingly, the essence of the phenomenon, masked on higher planes, should appear with supreme clarity there. What, then, does Durkheim understand by religion? In contrast to received ideas, he treats as immaterial the belief in personal supernatural beings. A basic dichotomy everywhere divides what is sacred from what is profane. But it is not merely spirits and deities that loom as sacred, but also the impersonal force Melanesians call "mana"; indeed, when people venerate the sun or the moon or the souls of the dead, it is solely as reservoirs of this mysterious impersonal entity, so that it becomes a matter of indifference whether this force resides in a personal being. Since mana is contagious, diffusing like an electric current, any object, any rite, is potentially sacred, i.e., of religious nature. Thus, a ceremony for increasing totems without worship of spirits or gods is *not* purely "magical" but truly religious because invested with sanctity. Durkheim does not, however, confound magic and religion under one head. Though magic has sprung from religion and borrowed its technique therefrom, he conceives it as its very antithesis. Religion invariably presupposes a communion of the faithful, while magic, even when officially practiced, lacks a church: the magician is neither bound to his clients, nor are his several clients bound to one another, by any moral ties;<sup>13</sup> his laws are rules of expediency devoid of holiness.

How, then, did this sense of the sacred arise? True to his principle of eliminating psychological factors, Durk-

<sup>12</sup> *Les formes élémentaires* . . . , 88, 238, 255.

<sup>13</sup> *Ibid.*, 58 sq., 65, 284 f., 430, 455 sq., 459 f., 490, 516 ff. The ideas on the sacred and profane are foreshadowed in the opening essay of *L'Année sociologique*.



heim spurns the influence of dreams and of natural phenomena on individual minds. What impressed primitive man was the overshadowing force of society, the clan of which he was a member, to which he owed protection and knowledge, without which he was a nobody. Only by symbolizing their clans could totems—often singularly inconspicuous in themselves—become sacred; indeed, characteristically, it is the totemic emblem that is more sacred than the animal it stands for; and essentially it is the social unit, the clan, that is God. The sentiment towards this entity was born at those periodic tribal reunions which alternate with the humdrum existence of hunting and gathering in minute family units. Practically all of Australian religious activity is restricted to these major assemblies, which evoke a regularly recurring state of effervescence; while the complementary season of separatism is filled with profane tasks. In the major assemblies, then, arose the notion of the sacred as contrasted with the profane world then left behind; and this notion, identified with the clan, was totemically symbolized. All spiritual beings are simply derivatives. The soul, which Tylor had put at the base of religion, is merely the totemic principle incarnated in each individual member of the clan. The Australian high-god is nothing but the product of tribal sentiment transcending that of the clan. For totemism is not elaborated by a single clan but by a whole tribe that had attained some consciousness of unity. “Or c’est ce même sentiment de l’unité tribale qui s’exprime dans la conception d’un dieu suprême, commun à la tribu tout entière.”<sup>14</sup>

Let us pause to examine this remarkable scheme. As a general explanation of religion it is at once ruled out by several fallacies. The clan is not the oldest type of social unit, having been preceded by the family. Australian society is not the simplest known, but it is manifestly

<sup>14</sup> *Ibid.*, 238-424.

more complex than that of the Fuegians, the Basin and California Indians, the Eskimo, or the Andamanese. To learn about the origins of religion from them is accordingly hopeless; the attempt is a relapse into unilinear evolutionism. But even were we to waive this point, an unbiased inspection of the Australian data would at best show totemism as *one* ingredient of native religion; its ascendancy over others is unproved; that other phases are derivatives is an arbitrary assumption. At the present stage of knowledge it is thus useless to refute in detail the ingenious arguments adduced in support of these theses.

Equally serious is Durkheim's aversion to psychology. Healthy as is his protest against vulgar interpretations in terms of mental states, his devotion to sociology as an autonomous science becomes doctrinaire and misleading. There is no hard and fast line between one branch of knowledge and another. What should we think of a prehistorian who investigated the origin and spread of bronze-casting without knowing that bronze is an alloy of tin and copper? The most vital problems would escape his attention. But metallurgy is much farther removed from culture history than is psychology! The sociologist simply cannot get away from states of consciousness, and Durkheim does not really eliminate them but smuggles them in at his convenience. The *état d'exaltation*, which he finds at the base of religious emotion is evidently a psychological condition; and the part of wisdom—precisely if one considers the state fundamental—would be to trace it systematically through all its contexts. Then, however, it appears at once that comparable exaltation is a frequent concomitant not of periodic tribal gatherings but of lonely vigils. The chronological priority of the crowd phenomenon is simply a corollary of Durkheim's primary axiom—the invariable precedence of the social over the individual. This is, of course, true today since

the individual of any society finds a ready-made traditional system of belief and practice to which he must somehow adapt himself; and it is a merit to have thrown the fact into relief. But in speaking of *origins* the dice are no longer loaded against the importance of the individual. Not that he can create something out of nothing, but that his deviation from the norm may become significant in ushering in innovations, as has demonstrably happened in recent aboriginal messianic movements.

Durkheim ignores this set of facts because he disregards the immense importance of individual variability among simpler peoples. He writes: "The group displays an intellectual and moral uniformity of which we find only rare instances among higher societies. Everything is common to all. Movements are stereotyped; everybody executes the same ones in like circumstances and this similarity of behavior only reflects that of thought. All minds are swept away by the same eddies, hence the individual merges in the generic type."<sup>15</sup> This is precisely the fallacy refuted by Boas, Schmidt, Hilde Thurnwald, and diverse naïve field workers, such as the late Reverend Junod. To take down five versions of the same folk tale in one community suffices to explode once and for all the dogma of psychological uniformity on simpler levels.

Durkheim's weakness along these lines naturally appears in his treatment of dreams.<sup>16</sup> He criticizes Tylor's approach as intellectualistic, but lapses into the same fault in a more extreme form. Why, he asks, would a native interpret the dream vision of a distant friend as a real visit if all he had to do in order to check the conclusion was to ask whether his camp mates shared the experience? "During the same time they, too, have had dreams, but they are quite different. They have not seen themselves taking part in the same scene; they believe

<sup>15</sup> *Ibid.*, 7 f.

<sup>16</sup> *Ibid.*, 78 sq.

that they have visited quite different places." Had Durkheim paid more attention to these subjective phenomena he would have noted that a native who treats a dream as veridical is often stirred to the very depths of his soul. The experience is an immediately convincing datum not in need of corroboration, sharing the ineffableness of mystic revelations, too sacred to be communicated to one's fellows, let alone to be submitted to logical scrutiny.

Reverting to Durkheim's cardinal proposition, we may further expose his psychological insufficiency by citing a legitimate stricture by Goldenweiser.<sup>17</sup> If a crowd situation precipitates religious behavior and sentiment, why does this not hold for *all* crowds? What distinguishes the secular from the ceremonial American Indian dance? Why does the Australian corroboree remain a form of entertainment distinct from initiation and totemic rituals? Evidently there is a determinant over and above the mere factor of assemblage.

As to the definition of religious phenomena, any writer is, indeed, at liberty to define his terms, hence at first blush to identify religion with sacredness may seem permissible. What Durkheim does, however, is rather different. He asserts that all societies dichotomize the universe into a sacred and a profane half, the sphere of religion coinciding with that of the "sacred, i.e. segregated, forbidden things" (*choses sacrées, c'est-à-dire séparées, interdites*) recognized as such by the community of believers. Now such an antithesis is, indeed, reported from Polynesia, where "noa" and "tabu" express precisely the antagonism between secular and holy things. But if such formalized dichotomy represents a general phenomenon, Durkheim fails to present the evidence. Let us add, for the sake of clearness, that he expressly does not identify the sacred with the supernatural, mysterious,

<sup>17</sup> A. A. Goldenweiser, *History and Culture*, 371.

unknowable.<sup>18</sup> Ingeniously and speciously he argues that the notion of the supernatural can appear solely in contrast to that of a natural order of the universe, such as has only arisen recently with the advancement of science. Once more his nonpsychological orientation has played him a trick. For psychologically what is implied is simply that the native irrationally responds to a given experience as transcending the ordinary routine: thrilled and awed, he labels the source of his emotion by some word describing the extraordinariness he feels, and cares not in the least about accurately conceptualizing it in the abstract.

Nevertheless, from another angle, Durkheim's approach is not devoid of merit. The traditional restriction of religious faith to that in personal beings may well exclude what is better classed under the same head. As we properly consider philosophical rather than religious the arid description of divinity furnished by metaphysicians, so a response strictly comparable to that otherwise associated with gods may be evoked by other phenomena. If the Australians consider their rites for the propagation of totem animals, in the words of a missionary, "as a sort of divine service"; if a Bantu soothsayer refuses to sell his set on the ground that it is to him what the Bible is to the Reverend Junod, then the ceremonials and the divining bones are properly classed as "religious," irrespective of their connection with sentient supernatural beings.

On the other hand, little can be said for Durkheim's conception of magic. Right, for the reasons just given, in classing many magical procedures under the head of religion, he is once more misled by his fatal penchant for neat bisection of a logical universe. Magic becomes in his eyes an antisocial, churchless mockery of religion. "Magic puts a sort of professional pleasure into profaning sacred things; in its rituals it inverts religious

<sup>18</sup> *Ibid.*, 33 sq.

ceremonial." That, however, such an attitude characterizes any considerable number of primitive groups remains an unproved allegation. The primitive equivalent of the Black Mass, to which Durkheim alludes, is not apparent. On the other hand, there are many instances of the very same person figuring ambivalently as social shaman and antisocial sorcerer, in both instances using precisely the same methods and being judged merely by the results.<sup>19</sup>

So far we have said nothing to explain our initial paradox, the enthusiasm for the book and the deprecation of its doctrines. Yet the solution is clear, though not apparent from Goldenweiser's review. Durkheim's contribution lies not in those points on which he piqued himself but along quite different lines. It is not pure accident that the ethnologists of English speech who have been particularly drawn to Durkheim—Radcliffe-Brown and William Lloyd Warner—are field workers and specialists in the Australian field. Durkheim misconceived the place of Australian culture in historical perspective, but having once persuaded himself that it presented the simplest form of social life, he immersed himself in the relevant material with exemplary assiduity and appraised it with critical acumen. When Strehlow, for example, adduced facts additional to those published by Spencer and Gillen, some writers were inclined to treat the earlier sources as superseded. Durkheim fairly weighed the evidence, partly reconciled the contradictions, and reduced them to their proper, minor proportions.

This mastery of the raw material, however, enables Durkheim to draw a number of conclusions that, whether wholly new or not, were sound and important. Thus, the contrast between the routine and the ceremonial phases of the annual cycle is real, even if it cannot explain the origin of religious feeling. His intimate knowledge of

<sup>19</sup> *Ibid.*, 59 f. Cf. Wm. Lloyd Warner, "The Social Configuration of Magical Behavior: A Study of the Nature of Magic," ALK 405-415, 1936.

the area further leads Durkheim to indicate phenomena of wider import. He demonstrates that ritual has a less serious aspect, merging into a form of entertainment. Again, he clearly illustrates the "pattern" phenomenon of American authors: the totemic ceremonials of the several clans differ in detail but there is a core of ideas common to all their practices, e.g., the bull-roarers. What is more, even the initiation festival and the totemic ceremonies, otherwise distinct in purpose, share vital formal elements. Thus, like Boas, Durkheim recognizes that the reasons alleged by natives as underlying their performances are mere rationalizations, ritual being primary and its ostensible aims secondary. It is probably no accident that Durkheim, with the behavioristic outlook stressed in the exposition of *Les règles*, should be most successful in the discussion of ceremonial.<sup>20</sup>

Two other points merit attention. Durkheim was sufficiently independent in his outlook to admit the native origin of the Australian high-god idea when it was still hotly contested. Finally, his excellent treatment of symbolism convincingly shows that a symbol may succeed in concentrating upon itself all the fervor that properly belongs only to the ultimate reality it represents.<sup>21</sup>

In summing up Durkheim's work so far as it bears on ethnology, we thus note several positive points. He saturated himself with the ethnographical data on the areas that interested him, critically sifted them, and when unaffected by his special bias, arrived at valid theoretical conclusions. Some of these had been reached by others, but Durkheim attained them independently and illustrated them by other sets of data. He was a thinker who wrestled with general ideas. It is true that his a priori conceptions led him astray, and part of his argument suffers from scholasticism. But if he sometimes

<sup>20</sup> *Ibid.*, 406, 540 f., 542 f., 550.

<sup>21</sup> *Ibid.*, 415, 314 sq.

lost himself in arid dialectics, he also penetrated beyond the obvious. Marett has well defined the contrast between him and a famous British author in their treatment of religion: "Sir James Frazer's method is simply to ask whether a given body of associated beliefs and practices is signalized by the presence of a certain concept. But M. Durkheim's method, going deeper, considers whether it fulfils a certain social and moral function." And the comparison concludes with the ironic comment: "Sir James Frazer's principle of classification has at least this virtue—that it is not over-subtle."<sup>22</sup> We have pointed out that part of Durkheim's doctrines coincided with those of Boas. But that does not detract from his historic significance, for we have seen that Boas' principles are not easily disengaged from his writings. Besides, minds to which Boas' procedure is radically uncongenial cheerfully accept the very same ideas when presented as part of a coherent system.

#### DURKHEIM'S FOLLOWERS

The scholars whom Durkheim rallied round his banner adhere so closely to his principles that a detailed exposition is unnecessary. Nevertheless, several of their productions cannot be passed over in silence.

Probably the most influential of these is Henri Hubert and Marcel Mauss' "Esquisse d'une théorie générale de la magie,"<sup>23</sup> whose conclusions, indeed, already figure in their master's work. Its erudition and insight have been acknowledged by writers of other schools.<sup>24</sup> With Schmidt, however, we must repeat the stricture that the sociological school, here as elsewhere, fails to recognize the influence of the individual. This is all the stranger because under magic the authors include sha-

<sup>22</sup> R. R. Marett, *Psychology and Folk-lore*, 188 f., London, 1920.

<sup>23</sup> *L'Année sociologique*, 7:1-146, 1904.

<sup>24</sup> W. Schmidt, *Der Ursprung der Gottesidee*, 1:514 sq., 1926.



manism,<sup>25</sup> which so clearly reflects the impress of powerful personalities and of individual differences. If it is true that "the magician is normally a sort of maniac" and only a limited number of people in a community are subject to such experiences, it is absurd to argue that public opinion *creates* the magician. As with other cultural data, we have to face the interaction of society and the individual, and no one can foresee the scope of the latter's influence on events in a given situation.

Hubert and Mauss deserve credit for their admirable critique of the overintellectualistic interpretations of their predecessors. They rightly contend that without mysticism magic simply merges in science; that this mystical factor is never lacking in magical belief. It can never be reduced merely to the laws of association, which the magician operates and limits in accordance with his momentary wishes: he blinds a frog to transmit its blindness to the victim, but does not sympathetically transform him into a frog. In other words, he does not put his trust in the indefinite automatic functioning of trains of association once set in motion.

Successfully avoiding the error of confounding magic and science, Mauss and Hubert fail properly to distinguish magic and religion. They are hindered by an initially assumed antithesis between these two sets of phenomena, religion being conceived as social, magic as typically antisocial or asocial. Not that the two necessarily incarnate these contending principles, but that essentially magic tends towards evil sorcery.<sup>26</sup> But since shamanism, included by our authors under magic, constitutes the very essence of innumerable religions, the artificiality of this polar contrast is obvious. Actually, they conclude by treating mana, or impersonal magic power, as the source of religion no less than of magic. At

<sup>25</sup> *Op. cit.*, 22, 28, 30, 35, 37, 94.

<sup>26</sup> *Ibid.*, 17 et passim.

bottom, then, there is no such sharp demarkation. To illustrate the crowd origin of magic, they cite the war dance of Dyak women, interpreted by the actresses into efficient collaboration with their actually fighting husbands and brothers. Thus, we learn, mana is born. "On the other hand, we do not detect in their spirit this definite notion of sacred things which is the sign of the religious state."<sup>27</sup> But how common is such a *precisely* defined notion of sacredness in religion? And why do some social gatherings precipitate a notion of mana, and others of sacredness?

As is usual with the members of their school, Hubert and Mauss evolve excellent ideas so far as their thinking is not overshadowed by sociological bias. They recognize not merely emotion and desire, but *unconscious* ideas; they realize that magic presents regional differences, that a given society has a restricted number of ritual forms—in other words, tribal patterns; they properly emphasize the importance of incantations, *rites oraux*, complementary to the generally recognized *rites manuels*.<sup>28</sup>

A very different contribution is represented by Mauss' "Essai sur les variations saisonnières des sociétés Eskimos; étude de morphologie sociale."<sup>29</sup> This admirably documented paper defines the sharp dualism of Eskimo life, with its alternations of caribou hunting and the quest of sea mammals. In correlating these economic aspects of Eskimo life with social and religious seasonal differences, Mauss produces a striking "functional" picture and is suggestive in the truest sense of the term, i.e., in suggesting parallel inquiries on other tribes.

Finally, we must consider Mauss' "Essai sur le don; forme et raison de l'échange dans les sociétés archa-

<sup>27</sup> *Ibid.*, 137 f.

<sup>28</sup> *Ibid.*, 51, 53, 57, 116.

<sup>29</sup> *L'Année sociologique*, 9:39-132, 1906. The late H. Beuchat assisted in the preparation of this memoir.

iques.”<sup>30</sup> It had been one of Durkheim’s favorite ideas—in contrast to many of his contemporaries—to stress the quality of the documentation rather than the number of societies compared. “The essential thing,” he wrote, “is to unite not many facts, but facts at once typical and well studied.”<sup>31</sup> This admirable principle guides Mauss in his study of primitive gifts no less than in his earlier essay on Eskimo seasonalism. On the primitive level, he argues, gifts are rendered not freely but as an obligation, and there is an obligation likewise to accept the proffered present. Further, there is not an individual exchange of goods, but one between clans, tribes, or families; and what is exchanged represents not purely economic utility but a whole system of courtesies, rites, feasts, military services, all of which Mauss groups together under the head of *prestations totales*.

The familiar potlatch of British Columbia, in which the contracting parties vie with each other in quest of prestige, forms a competitive subtype of this category, the *type agonistique*, which also includes Melanesian practices, while elsewhere occur intermediate forms between such rivalry and the simple obligatory exchange of gifts. Mauss cites Polynesian parallels, hitherto neglected, as illustrating the entire potlatch theme minus its exaggerated rivalry. For Oceania, generally, he argues that though the exchange of goods plays a large part, the forms and rationale differ from ours: there is no purchase, no sale, no barter in the strict sense of these terms. These forms necessarily involve the ideas of credit and honor so marked in the transactions of British Columbia Indians. The principle of the exchange-gift is treated as representing a definite evolutionary stage between that of total group prestation and the late stage of purely individual contract.<sup>32</sup>

<sup>30</sup> *L'Année sociologique*, nouvelle série, 1:30-186, 1925.

<sup>31</sup> *Ibid.*, 4:341, 1901.

<sup>32</sup> Mauss, *op. cit.*, 126.

While the Durkheimian propositions as to primitive clans and the dominance of society reappear in this treatise, they are not obtrusive. On the other hand, Mauss duly stresses the irrational, nonutilitarian motives underlying primitive negotiations that take the place of our business transactions. With Thurnwald and Malinowski he thus becomes one of the leading students of primitive economics. Finally, the concluding section of the essay explicitly demands the study of cultures as integral wholes, indicating that "economic" facts have their social, religious, aesthetic aspects. The point had, indeed, been made before, but it is none the less sound when voiced not merely as a mystical shibboleth but with concrete illustrations. No wonder this timely production has met with a more generally cordial reception than have most works of the school.<sup>33</sup>

#### L É V Y - B R U H L

Lucien Lévy-Bruhl (1857- ) is even more definitely identified with philosophy than are the preceding French authors. He has published a treatise on the *History of Modern Philosophy in France*, as well as monographs on individual thinkers—Comte and Jacobi. Naturally he is attracted by the primitive equivalents of such concepts as causality and the logical law of contradiction. Quite intelligibly, too, his interest takes a somewhat different direction from that of Durkheim and Mauss. Perhaps as widely read, Lévy-Bruhl does not evince that intensity of concern with particular regions which we recognize, e.g., in Mauss' essay on the Eskimo cycle. In other words, his orientation is less ethnographical, notwithstanding the wealth of his documentation; and though his main works were issued as *Travaux de*

<sup>33</sup> See e.g. Richard Thurnwald, *Die menschliche Gesellschaft*, 5:48 f., Berlin u. Leipzig, 1934.

*l'Année sociologique*, it is not clear to what extent he can be reckoned a full-fledged member of the school.

Lévy-Bruhl's two most important and mutually complementary books, *Les fonctions mentales dans les sociétés inférieures* (Paris, 1910; 5 edition, 1922) and *La mentalité primitive* (Paris, 1922), certainly share in part Durkheim's position. Though their aim is to define the primitive mind, this is expressly and emphatically not attempted by leaning on psychology. Lévy-Bruhl, like Durkheim, ignores the individual mind and deals only with "group ideas" (*representations collectives*). In harmony with the leader of the French sociologists, he thus spurns all explanations of primitive attitudes and actions that treat their originators as rational beings seeking an explanation. In conscious opposition to Tylor, he must be placed with Tarde and Boas as among those who allow full scope to the emotional side of primitive man. He illustrates the importance of the affective factor by a wide range of data and not infrequently arrives at important reclassifications, as when he shows convincingly that the primitive concept of the soul is by no means so simple and uniform as the followers of the animistic theory assume; and he rightly treats the *couvade* not as an isolated instance of aboriginal *bizarrie*, but as merely one of a large set of taboos imposed on both parents at childbirth. Again, he demonstrates very clearly the nature of mystic numbers; if Malays speak of seven souls for each individual, it is not that they actually distinguish seven aspects of a spiritual essence; "It is, on the contrary, because in their eyes the number seven enjoys pre-eminently mystic virtues, becoming a kind of category, to which conform not only their magical operations, but also their representations, including those of the soul."<sup>34</sup>

Thus, Lévy-Bruhl forcibly brings home to his read-

<sup>34</sup> *Les fonctions mentales . . .*, 83-93, 296-302, 250.

ers the way in which a pre-existing norm determines individual thinking. Like other members of the school, he also displays a sense of cultural dynamics, as when he discusses the possible alterations in the native interpretation of funeral rites or the tortuous route of mythological development.<sup>35</sup> A less attractive feature shared with Durkheim is the naïve evolutionary assumption that Australians are the lowest of known peoples.<sup>36</sup> Still more objectionable is the ever-recurring use of the term "law," a veritable *idée fixe* of the French sociologists.

For Lévy-Bruhl believes that he can summarize the *représentations collectives* by the "law of participation," to which all of primitive mentality conforms. Now it is not easy to define this law because the author himself nowhere furnishes a clear-cut definition. But by piecing together statements in different parts of his works we can make his meaning sufficiently clear. Primitive people perceive nothing as we do, conceive nothing as we do, dispense with the principles of contradiction and of causality. Their thinking is prelogical, though not on principle antilogical, in that it is a matter of indifference whether the law of contradiction holds or not. In other words, the primitive mentality makes an inseparable jumble of logical and nonlogical procedures. What, however, is the positive meaning of "participation"? According to our author, logically distinct aspects of reality tend to fuse into one mystic unity. An Australian horde does not *own* its hereditary land, for it cannot conceive separation from it. "Between its members and this locality there is a mutual participation: it would not be what it is without them, nor they without it." Similarly, a South American Indian who declares himself a parrot means precisely that, viz., an inexplicable mystical identity of himself and the bird. In primitive hunting, what

<sup>35</sup> *Ibid.*, 383-391, 439 f.

<sup>36</sup> E.g., *ibid.*, 329.

is essential is not the practical pursuit of the game but the magical appurtenances of the chase: "What is essential are the mystical operations that can alone ensure the presence and capture of the quarry. If they are lacking, it is not worth while trying." This trend culminates in the group ideas of death. For, in such activities, as the chase, a minimum of rational adjustment of means is essential to attaining the end sought. Into the notions about the dead this logical factor does not intrude, hence prelogic here runs riot untrammelled. In his résumé Lévy-Bruhl stresses the point that the very term *représentation* is inaccurate in speaking of group ideas. The primitive mind does more than represent its object: "It possesses it and is possessed by it. It participates in it, not merely in a representative, but simultaneously in the physical and mystical sense of the word. It does not only think but live it."<sup>37</sup>

Even in his earlier work Lévy-Bruhl ascribes only infinitesimal significance to the notion of causality among simpler societies. In his later treatise he defines the equivalent among them as indifferent to *secondary* causes: "The connection between cause and effect is immediate. Intermediate links are not admitted, or at least, if recognized, are regarded as negligible and receive no attention."<sup>38</sup>

In these discussions we accept the emphasis on irrational associations of ideas on ruder levels, the view that a merging of logically irrelevant notions precedes their analytical recognition as so many distinct phases of reality—incidentally a favorite point of Boas. But we resent the term "law" because Lévy-Bruhl nowhere provides a clue to the nature of the associations expectable in a given situation. *Why* does the "law of participation" make the Malay invest with a mystic halo the

<sup>37</sup> *Ibid.*, 77, 130, 242, 263, 332, 334, 355, 378, 426 f.

<sup>38</sup> *Ibid.*, 78. *La mentalité primitive*, 92, 518.

number 7, while this same "law" makes the Pueblo ascribe a similar significance to 4? Why does this principle precipitate the full-fledged *couvade* in South America and merely mild dietary and other taboos in California?

But, waiving the point, we find serious objections to Lévy-Bruhl's basic conception of the primitive mind—objections we share with such diverse thinkers as Pinard de la Boullaye, Schmidt, Goldenweiser, and especially Thurnwald, whose masterly critique we indorse in every point.<sup>39</sup> In the first place, Lévy-Bruhl roams over the whole primitive world and then presents a composite picture of the "primitive" mind, a category that is made to embrace even the highly sophisticated natives of China and India. Second, there is a complete neglect of individual variability in any one society; yet every society includes not only those who follow along the predetermined paths of tradition, but also leaders who in part break away from the past and found new "group ideas." Third, primitive man, irrational though he often appears in his abstract formulations and the religious phases of culture, is often as keen an observer as civilized man, and as logical a reasoner from his observations. Intermittently, Lévy-Bruhl himself admits this, but he tries to argue the fact away by ascribing aboriginal skill to a sort of intuition such as guides the billiard player "who without knowing anything of geometry or mechanics, without requiring reflection has gained a rapid and sure intuition of the movement to be executed for a given position of the balls."<sup>40</sup> But, as Thurnwald notes in supporting Nieuwenhuis' observations in Borneo by his own in Melanesia, the longer one associates with "savages," the more intimate one's acquaintance with their language, the more do the differences initially felt between their mode of thinking and ours tend to disappear. The

<sup>39</sup> R. Thurnwald, *Deutsche Literaturzeitung*, 486-494, 1928.

<sup>40</sup> Lévy-Bruhl, *La mentalité primitive*, 92, 518.



“savage” thinks rationally as we do in comparable situations, that is, he uses the rule-of-thumb logic that suffices for everyday use. What Lévy-Bruhl describes as the “law of participation” is the common foible of all humanity, not the peculiarity of primitive minds. He establishes his contrast not by comparing civilized and primitive man, but, in Thurnwald’s apt characterization, “the highest achievements of the modern intellect”—*nota bene*, only in its professional activities—“with a rather vague ‘primitiveness.’”

The net result of Lévy-Bruhl’s discussions—apart from some of the fruitful corrections of earlier classifications mentioned above—is to bring home once more the importance of social tradition in molding individual responses to experience and to stress the overwhelming significance of irrational factors *not* in primitive, but in human thought.

#### RADCLIFFE-BROWN

Though an Englishman trained at Cambridge by Haddon and Rivers, A. R. Radcliffe-Brown (1881- ) is best considered with the Durkheimians, whose influence he freely acknowledges. He has lived in the South Seas, taught at the universities of Sydney, Cape Town, and Chicago, and since 1937 has been professor of social anthropology at Oxford. In contrast to Mauss and Lévy-Bruhl, he has had abundant contacts with primitive men, yet the resulting difference is less than might be supposed. For Radcliffe-Brown is most emphatically not a field man by temperament. The only complete description of a tribe he has ever attempted is based on his maiden trip, which took him to the Andaman Islands (1906-1908). His report, unduly delayed by the War, is creditable enough, but in no way remarkable as an increment of factual knowledge if compared with the account of his predecessor, E. H. Man. Unlike the mono-

graphs of other modern investigators, it lacks the personal touch: statements are consistently offered in a generalized form without reference to individual informants' experiences. This procedure, as we shall see, reflects more than merely a novice's deficiency in technique. Later Radcliffe-Brown did significant work in Australia, but none of his relevant writings touch more than selected phases of native life. Of original investigation among the natives of Polynesia and South Africa as a result of prolonged residence in these areas there is no record. Paradoxically, this widely traveled scholar is at heart an armchair anthropologist who formulates problems in the study to be solved in the field by his followers. This is said by way of characterization, not of criticism, for he has thus stimulated important work by others. Indeed, the meagreness of his total output is intelligible from the compensatory energy he has expended on teaching and the organization of research.

Radcliffe-Brown's printed work divides into two categories: a series of confessions of faith outlining the proper procedure of anthropological research; and his actual contributions, which by no means wholly harmonize with the spirit of his manifestos.

In his general pronouncements<sup>41</sup> Radcliffe-Brown clearly and avowedly stems from Durkheim. "Social anthropology" or "comparative sociology"—the study of group behavior—is independent of psychology and ignores as irrelevant the individual as an individual. Its aim is to discover *laws*. Since history explains its phenomena only by "finding wherever possible the particular cause or occasion of each change that has taken place,"

<sup>41</sup> A. R. Radcliffe-Brown, "The Methods of Ethnology and Social Anthropology," *South African Journal of Science*, 20:124-147, 1923; "The Present Position of Anthropological Studies," *Brit. Assoc. Adv. Science*, Section H, Presidential Address, 1-32, 1931; "Applied Anthropology," *Australian and New Zealand Association Adv. Science*, Section F, Presidential Address, 1-14, 1930.

it cannot yield the generalizations current in natural science, hence at best the historical method is of comparatively slight value; in proportion as it becomes conjectural it approaches worthlessness. Where definite historical data are extant as to the origin of an institution, that knowledge may indeed be most significant for social anthropology, but actually the historical ethnologists provide only "a few, a very few" such facts. The social anthropologist, on the other hand, observes and explains customs and beliefs by showing "how each one of them is an example of some general law of human society." To elucidate totemism, for instance, he demonstrates it as "a special instance of a phenomenon or at any rate of a tendency which is universal in human society." Finally, when laws are once determined, they can be applied practically to regulate the course of social development, in parallelism with the procedure of other sciences.

On another essential point Radcliffe-Brown agrees not only with other Durkheimians but also with Boas, and, as we shall see, with Thurnwald and Malinowski. Any particular culture is "normally a systematic or integrated unity in which every element has a distinct function." This implies skepticism, again shared with Boas, as to the comparability of single traits from different areas. Specifically, Radcliffe-Brown opposes the "atomic" view of studying the distribution of isolated features apart from the context that gives meaning to them. He is further alive to the process of selective borrowing, thus repudiating the notion that contact automatically precipitates diffusion.

As in the case of other writers, Radcliffe-Brown's real achievement lies in rather different directions from those suggested by himself. This herald of the "systematic unity" of cultures has not essayed a single integrated cultural picture since his avowedly immature treatise on

the Andamans.<sup>42</sup> And the "laws" he propounds must be repudiated. The following is a sample: "Things that have important effects on the social life necessarily become the objects of ritual observances (negative or positive), the function of such ritual being to express, and so to fix and perpetuate the recognition of the social value of the objects to which it refers." From this principle our author infers that hunter-gatherers will perform rituals concerning their game animals and edible plant species; that where there are clans, the clans will perform generically similar but specifically distinct rituals of this type, that clanless societies will not have such totemic ceremonials but will rather display "a general undifferentiated relation between the society as a whole and the world of nature as a whole."<sup>43</sup> In the first place, these propositions remain quite as unverified as the historical hypotheses against which the author inveighs. Secondly, the primary generalization offered is a trite statement of certain descriptive facts. It does not even pretend to explain those very important rituals which do not purport any bearing on the food supply. Certainly, hunters execute rites to ensure capture of the game, but the Plains Indians also performed the Sun Dance, their most conspicuous ceremony, for a variety of other reasons, such as revenge, securing the recovery of a sick relative, and so on. A *law* of ritual would have to define what species would figure ceremonially; under what conditions nondietary factors determine ritual; why frequently it is not the clan at all but some other unit that conducts ceremonies; and a dozen other things.

The grandiloquent use of the term "law" is most regrettable and in some circumstances leads to absurdity, as when Radcliffe-Brown writes of "a universal socio-

<sup>42</sup> *The Andaman Islanders: A Study in Social Anthropology*, Cambridge, 1922.

<sup>43</sup> "The Methods of Ethnology and Social Anthropology," 135 f.

logical law though it is not yet possible to formulate precisely its scope, namely that in certain specific conditions a society has need to provide itself with a segmentary [clan] organization.”<sup>44</sup> Whoever heard of a universal law with an as yet undefinable scope, of a law that works in certain specific *but unspecified* conditions? Is it a law that some societies have clans, and others have not? Newton did not tell us that bodies either fall or rise.

Like other authors, Radcliffe-Brown achieves his most solid work when he forgets about his abstract profession of faith, immerses himself in a set of data, and extricates sense out of chaos irrespective of any doctrinaire principles. Thus, he points out the highly important fact that African ancestor-worship is patrilineal, regardless of matrilineal descent; and he explains most cogently why the Northern Thonga of Portuguese East Africa use a single word for the maternal uncle, the maternal uncle's son, and the mother's father. By native usage, it is my maternal grandfather that sacrifices on my behalf to my matrilineal ancestors; on his death the duty devolves upon my maternal uncle, and after *his* death, upon his son. Representatives of three distinct generations thus share an important ceremonial function and are appropriately designated by a single term.<sup>45</sup> Nothing could be better than this interpretation. In the same article he clarifies the concept of avuncular authority, showing that it may be counterweighted by the status of the paternal aunt.

Most important of all, however, is Radcliffe-Brown's contribution to Australian social organization. His field work led to a brilliant discovery, viz., that cross-cousin marriage proper invariably accompanies one type of kinship nomenclature, while cross-cousin marriage of the

<sup>44</sup> “The Social Organization of Australian Tribes,” *Oceania Monographs*, No. 1, p. 109, Melbourne, 1931.

<sup>45</sup> “The Mother's Brother in South Africa,” *South African Journal of Science*, 21:542-555, 1924, esp. pp. 552, 554.

second degree goes with another type.<sup>46</sup> This is infinitely better than a sham law, for it is a real, verifiable correlation, revealing a connection hitherto unguessed between two sets of isolated facts.

It is in this field, too, that Radcliffe-Brown marks the most definite advance over the Durkheimian creed. He breaks with the dogma of Australian ultra-primitiveness; he accepts the family, not the clan, as the basic social unit; he no longer assumes the primeval character of totemism, nor the necessary priority of matrilineal descent. Apart from these deviations from French sociological doctrine, he establishes a well-defined series of concepts to cover Australian phenomena, summarizing the essential facts with exemplary terseness and a sane appraisal of all sources extant.

Finally, Radcliffe-Brown's theoretical intransigence on the subject of history wanes before data with which he is thoroughly familiar and, notwithstanding some qualms, he stoops to chronological hypotheses. The Yarlalde kinship system "cannot reasonably be supposed to have developed independently of those [Aranda systems]. . . . we must certainly assume some historical connection between them." Again, the Kumbaingeri type is a stepping stone from the Kariera to the Arunta form.<sup>47</sup> Surely this is conjectural history!

One aspect of Radcliffe-Brown's historical thinking, rather adumbrated than fully limned, is his notion of cultural evolution. It differs from Morgan's scheme in renouncing a universally valid sequence, but it does imply definite social trends, notably the tendency of wider integrations to supersede those of narrower scope.<sup>48</sup> This neo-evolutionary phase of Radcliffe-Brown's philos-

<sup>46</sup> "Three Tribes of Western Australia," JRAI 43:143-194, 1913.

<sup>47</sup> "The Social Organization of Australian Tribes," 51, 63, 113, 120.

<sup>48</sup> *Ibid.*, 113-120.

ophy of culture suggests ideas broached by Thurnwald and Schmidt.

Two recent utterances clarify Radcliffe-Brown's present position,<sup>49</sup> especially his conception of "function," which he defines as "the contribution which a partial activity makes to the total activity of which it is a part." He explicitly concedes that the implied unity of any social system is a mere hypothesis to be verified, a conclusion in which we heartily concur. In other words, he supplants the dogma that "everything in the life of every community has a function" with the legitimate statement that "it *may* have one, and that we are justified in seeking to discover it." He admits that at the present time it is impossible "to establish a purely objective criterion" for the degree of functional unity of a particular society, but merely hopes for greater enlightenment in the future. Further, Radcliffe-Brown rejects the dogma that "there are no discoverable significant sociological laws."

In this form the doctrine is much more acceptable, for what is inadmissible as a set of axioms may remain a legitimate programme. *If* every item of culture has a function, *if* comparative sociology has valid laws to offer, this will be of great interest to all ethnologists. In the meantime we take cognizance of the message and shall watchfully lie waiting for what may come in its wake.

Of more immediate interest, however, are two other points. Radcliffe-Brown now overtly recognizes study of the individual as "an essential part of the task"; and historical explanations are now regarded as complementary to those of the functionalist order, though naturally, authenticated events are preferred to merely inferential ones. "A sociologist who neglected [documented] history . . . would be gravely at fault"; and in accordance with this principle a member of the school has recently

<sup>49</sup> "On the Concept of Function in Social Science," AA 37:394 sq., 1935; "Kinship Terminologies in California," AA 37:530 sq., 1935.

examined the kinship structure of Southeastern Indians as recorded at different periods.<sup>50</sup> It appears that we not only may but ought to study the changes going on before our eyes: a "synchronic" approach must be combined with a "diachronic" one. The only comment required is that this thoroughly sound principle is not new. Fifteen years ago Dr. Elsie Clews Parsons collated data gathered among the Hopi in 1883, 1890, and 1920, respectively, concluding her diachronic survey with these words: "Here under our eyes has gone on an immensely interesting process of cultural change of which we have as yet but the barest record—to so many of us study of the past is so much more appealing than study of the present, even the present in which the past repeats itself, in terms clearer and more pregnant than archaeology can ever use." Parsons' researches on the Pueblo are invaluable precisely because they transcend the static view of a culture often presented by investigators and show exactly what did happen, inevitably revealing not only a chronology but also determinants of social behavior. Thus, when a Hopi chief died in 1892 there was no automatic succession to office by the theoretical heir. Two genealogically possible claimants were frowned upon because of their foreign wives; some candidates aroused opposition because of their fraternity membership; and finally the hierarchy installed a reluctant grandson of the late chief's eldest sister.<sup>51</sup>

We note, then, with satisfaction the far-reaching agreement of Radcliffe-Brown with other workers.

To sum up, Radcliffe-Brown is a relatively independent offshoot of the Durkheim philosophy who does not differ nearly so much from his fellow workers of other

<sup>50</sup> Fred Eggan, "Historical Changes in the Choctaw Kinship System," AA 39:34-52, 1937.

<sup>51</sup> E. C. Parsons, "Contributions to Hopi History," AA 24:253-298, 1922; *idem*, (editor), *Hopi Journal of Alexander M. Stephen*, 1047, New York, 1936.



schools as his earlier utterances might lead one to suppose. While he is not primarily interested in the reconstruction of the past, his practice in a modest way belies his theory as to the uselessness of even conjectural history. Topically, he has concerned himself almost exclusively with problems of social organization, which he has advanced in several important respects, while keeping abreast of contemporary progress. His earlier ukases as to what social anthropology ought to do we greet with a shrug; what he has actually done, restricted as it is in scope, we welcome with genuine respect.

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# XIII

## FUNCTIONALISM: PURE AND TEMPERED

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M A L I N O W S K I

Bronislaw Malinowski (1884- ), professor of social anthropology in the University of London, is often paired with Radcliffe-Brown. He himself has not rejected the classification: Radcliffe-Brown's "tendency to ignore completely the individual and to eliminate the biological element from the functional analysis of culture" is said to constitute "really the only point of theoretical dissension" and the only one on which Durkheim's principles require supplementing.<sup>1</sup> Notwithstanding, however, Radcliffe-Brown's recent inclusion of the individual as a legitimate object of inquiry, the resemblances between the two English scholars should not be overrated, and we have deliberately divorced them in our treatment. They share with each other—but also with Boas, Bachofen, and Fustel de Coulanges—a concern with the interrelation of the several elements within a given society. Further, both have avowed a disdain, largely but by no means uniformly indulged in practice, for history. In every other respect their lifework diverges radically. For the difference con-

<sup>1</sup> Introduction to H. Ian Hogbin, *Law and Order in Polynesia*, xxxviii, New York, 1934.

ceded by Malinowski bears not on a casual division of opinion but on a chasm between two distinct personalities.

In contrast to Radcliffe-Brown, Malinowski is first of all a field investigator. His intercourse with savages has indeed been less varied, but those three expeditions of his to New Guinea, which included a two years' stay in the Trobriand Archipelago, marked an epoch in his life. All his subsequent publications, descriptive and theoretical, stand rooted in that major experience. His field technique conforms to Boas' standards: he learnt his Trobrianders' tongue, tried to live their life, garnered concrete rather than abstract statements from his informants, and recorded them in the vernacular. Rivers had approached natives as an outsider; Malinowski reformed British methods by stressing the unformulated phases of aboriginal culture, "the imponderabilia of actual life and of typical behavior." As Boas had insisted on registering the exoteric no less than the esoteric side of primitive communities, so Malinowski noted as equally significant the norm and the deviations from it. Endowed with an unusual literary sense, he thus succeeded in creating a "flesh and blood" picture of his Melanesians. For the traditional systematic monograph he has substituted a series of books, each devoted to a central theme, which is exhibited in its relations to the tribal life as a whole. Thus, an account of ceremonial barter merges in considerations of the traders' canoes, of property rights, class distinctions, rules of inheritance, magic. Yam cultivation emerges as the basis of wealth, power, and law and as inextricably bound up with magic. Inevitably there is repetition, but the reader becomes saturated with the Trobriand atmosphere, sees the aborigines as human beings, not as puppets designed to produce kinship nomenclatures or to illustrate some sociological law. Most important of all, this picture is not

conveyed by rank impressionism: subjective as the interpretations in part appear, there is an estimable mass of textual material—interlinear renderings of native statements eked out by free translations.<sup>2</sup>

Immersing himself into the native scene, Malinowski is dominated by it, and his theoretical tenets largely emanate from direct observation. In justice we must note that even before his trip to the Trobriands he had grasped the basic character of the family from a library study of Australian sources, thus admittedly anticipating Radcliffe-Brown's conclusions.<sup>3</sup> This idea was, however, intensified and amplified by Trobriand life; and allying himself with a host of modern writers, such as Westermarck, Boas, Swanton, Schmidt, and Kroeber, Malinowski came to regard the family as the fundamental unit in all human society.<sup>4</sup>

It was field experience that led Malinowski to one of his characteristic doctrines—the conflict of practice with theory. Here he was preceded by Boas, yet many recent authors were still speaking of primitive law as functioning with automatic precision. Malinowski by striking case material illustrated how individual natives chafe under social restraint to the point of defying tradition. Specifically, he has again and again emphasized the Trobriand father's predicament in trying to reconcile paternal love with the matrilineal law that favors his sister's son to the detriment of his own son.<sup>5</sup> In the ensuing conflict of loyalties the decision will hinge on the parent's personality—the strength of his character and of his fatherly sentiments. Such individual differences,

<sup>2</sup> *Argonauts of the Western Pacific*, London, 1922. *Crime and Custom in Savage Society*, London, 1926. *The Sexual Life of Savages in North-western Melanesia*, New York, 1929. *Coral Gardens and their Magic*, 2 vols., New York, 1935.

<sup>3</sup> Br. Malinowski, *The Family among Australian Aborigines*, London, 1913. Radcliffe-Brown, *The Social Organization of Australian Tribes*, 103.

<sup>4</sup> *Sex and Repression in Savage Society*, 243.

<sup>5</sup> See especially, *Crime and Custom in Savage Society*, 101 sq.

then—ignored on principle by the Durkheimians—not only help us understand what happens in special situations but reveal factors capable of overthrowing the social tradition itself and of producing its successor.

Again, the peculiar network of mutual obligations typical of Melanesia led Malinowski to herald reciprocity as a basic principle of human society: “The duty of one person is inevitably the privilege of another; services rendered are boons received; gifts and tributes presented by one side can be demanded by the other.”<sup>6</sup> From this set of relationships he further inferred the precedence of civil over criminal law in society generally.

Observations in the Trobriands further made Malinowski reject certain fallacies about primitive man’s economic behavior and helped him clarify the non-utilitarian side of economics. Hahn had already demonstrated the religious and sportive facets of animal husbandry. Malinowski widened the outlook by exhibiting the motives that underlie production and exchange among his people, and analyzed such concepts as labor, wealth, money, and value in correspondence with savage realities. Value turned out to rest not on utility alone or on utility plus rarity, but rather on the fancy character of a product, on which the craftsman lavishes a disproportionate amount of effort. Correspondingly, the elaborate ceremonial barter of the natives revolves about the irrational transfer “of two meaningless and quite useless objects.” And, as our rationalistic notions cannot be transferred to Trobriand economics, so the division of labor there assumes a quite distinct meaning.<sup>7</sup>

No doubt the avowed core of Malinowski’s philosophy of culture, his “functionalism,” is in one of its aspects also the reflection of experience in Melanesia,

<sup>6</sup> E.g., introduction to Hogbin, *op. cit.*, xxxiii sq.

<sup>7</sup> *Argonauts of the Western Pacific*, esp. 49-104, 166-194. *Coral Gardens*, 21, 41.

where the interlocking of a priori disparate phases of activity, such as magic and industry, are insistently brought home to a sensitive observer.

As a positive achievement stimulated by reading rather than direct observation may be registered Malinowski's synthesis of psychoanalytic concepts with his ethnographic findings. We have previously noted the sterility of Rivers' attempts in this direction. Malinowski goes further, broaching a new and legitimate problem. Granting that the type of family known in our civilization precipitates repressions and conflicts, may we assume identical psychological situations as a universal human phenomenon? Malinowski answers in the negative, pointing out most suggestively that in a matrilineal community of the Trobriand type the sentiment directed against the father would become an avuncular complex. Right or wrong, he has here paved the way for investigating a possible correlation—the nexus between a type of institution and its psychiatric concomitants.<sup>8</sup>

With Malinowski, as with Radcliffe-Brown, an estimate of achievement must not be warped by restiveness over apocalyptic utterances on points of principle. In messianic mood Malinowski is forever engaged in two favorite pastimes. Either he is battering down wide open doors; or he is petulantly deriding work that does not personally attract him. From first to last he intermittently taunts the antiquarian "gloating over isolated and outlandish anomalies of human behavior." Yet from McLennan and Tylor to Spier and Birket-Smith not a single professional of standing has treated stray items of behavior or craftsmanship as anything but a means to an end. In the same spirit Malinowski thumbs his nose at technology, flouts distribution studies, sneers at recon-

<sup>8</sup> "Mutterrechtliche Familie und ödipus-Komplex; eine ethnologisch-psychoanalytische Studie," Sonderabdruck aus *Imago*, X Band, Leipzig, Wien, Zürich, 1924. Also, *Sex and Repression*, 135 sq.

struction of the past. The only worthy aim is to study "the part which is played by any one factor of a culture within the general scheme." At times the more elusive aspects of social life are made to loom large. Malinowski goes so far as to suppress his own data from islands where a brief sojourn precluded ideal intensiveness of investigation. As though a naturalist would fail to report the existence of the okapi because he had been unable to trace its embryonic development.<sup>9</sup>

In short, Malinowski's functionalism is avowedly antidistributional, antihistorical, and treats each culture as a closed system except insofar as its elements correspond to vital biological urges. Unhesitatingly rejecting the intransigence of the creed, we accept its main positive postulate, but with important reservations already indicated in our discussion of Boas (page 142).

First and foremost, a science of Culture is not limited to the study of so many integrated wholes, the single cultures. This is doubtless important, but it constitutes neither the whole nor even the preponderant part of the ethnologist's task. A science of culture must, in principle, register every item of social tradition, correlating it significantly with any other aspect of reality, *whether that lies within the same culture or outside*. In defiance of the dogma that any one culture forms a closed system, we must insist that such a culture is invariably an artificial unit segregated for purposes of expediency. Social tradition varies demonstrably from village to village, even from family to family. Are we to treat as the bearers of such a closed system the chief's family in Omarakana, his village, the district of Kiriwina, the Island of Boyowa, the Trobriand archipelago, the North Massim province, New Guinea, or perchance Melanesia? The attempt to adhere rigorously to any one

<sup>9</sup> E.g., *Argonauts*, 509, 517. Introduction to Hogbin, *op. cit.*, vii. *Myth in Primitive Psychology*, 34 f., New York, 1926. *Coral Gardens*, 457.

of these demarkations precipitates absurdities. There is only one natural unit for the ethnologist—the culture of all humanity at all periods and in all places; only when the functionalist has, at least implicitly, defined his particular culture within that frame of reference, does he know what he is talking about. Why are anthropologists forever harping on the Maya system of notation? The most significant thing is not that we find it embedded in mystical associations. But that these Americans achieved the abstract notion of the zero, thus intellectually triumphing over Greece and Rome—*that* is indeed a matter of moment. Yet in order to assess the fact we must know its distribution: imagine the same notation among Ituri Pygmies, Central Australians and Andamanese, and its significance radically changes.

But the very spatial arrangement of our data leads to factors of another order. Without the physical environment in which they are rooted, many of them are unintelligible. Here lies the extraordinary significance of Nordenskiöld's investigations. In short, spurning isolated facts as superciliously as does Malinowski, we shall seek meaningful relations in all directions, not within the supposedly watertight compartment of a single body of social tradition.

As already noted, Malinowski does emphasize one extracultural set of determinants: each phase of culture corresponds to "some fundamental tendency of the human organism." Marriage satisfies the sexual need; legal parenthood stems from parental emotion; prescribed emergency behavior is derived from the urge of self-preservation.<sup>10</sup> Whether these are truisms or statements of deeper significance, the propositions are too vague to interpret what we wish to have explained. We take it for granted that all forms of marriage have some connection with sex. What we want to know is why the Toda practice

<sup>10</sup> Introduction to Hogbin, *op. cit.*, xxxvii.



polyandry, the Bantu polygyny, the Hopi monogamy; and that cannot be explained in terms of generic human tendencies, as Hocart well recognized.

Concentrating, then, henceforth on the aspect of functionalism that has unquestioned explanatory worth, we must sharply define what has been proved from what has been alleged. It has been demonstrated that magic interlocks with economic attitudes, these again with social structure, and so on. But neither Malinowski nor any other "totalitarian" has ever shown that all the hundreds of descriptively separable traits play a definite role in tribal life, are all manifestations of one grand mystic unity. This, we saw, is now freely conceded by Radcliffe-Brown. As soon as functionalism is reduced to what it is—a worthy programme for ascertaining what intracultural bonds may exist—the neglect of other methods appears as solely a matter of personal preference. That is to say, Malinowski may legitimately ignore for his purposes what he cannot interpret in functional terms, but he cannot deny the existence of "accidental or fortuitous" complexes. It is nothing but a clever lawyer's dodge when he turns the tables on Graebner, contending that chance connections can be inferred only after we have exhausted "all the possibilities of explaining form by function and of establishing relationships between the various elements of culture." Similarly, it is sheer dogmatism to declare: "The better a certain type of culture is known, the fewer survivals there appear to be in it."<sup>11</sup> How many cultures has Malinowski ever examined from this point of view?

Finally, Malinowski's recurrent emphasis on the ineffable aspects of our field of inquiry seems exaggerated. Culture certainly includes untranslatable features, but so does all human experience. Science cannot deal with the incommunicable as such, however explicitly it takes

<sup>11</sup>"Culture," in *Encyclopedia of the Social Sciences*, 4:620-645, 1931.

cognizance of its existence. What if a myth is "not merely a story but a reality lived?"<sup>12</sup> We cannot relive the reality, but we can study that textual rendering which Malinowski disdains as merely the "intellectual" aspect of the tales divorced from their mystic aura.

Actually Malinowski's practice soars above the limitations of his doctrinaire philosophy, an intuitive sense of fitness preserving him from its extreme implications. He will not "move one inch from my intransigent position that the study of technology alone and the fetishistic reverence for an object of material culture is scientifically sterile." But in the same breath he admits that "a knowledge of technology is indispensable as a means of approach to economic and sociological activities and to what might be adequately called native science." What is more, he gives the customary details about Trobriand yam houses; and though much is made of the emotions of native mariners towards their boats as "the deepest ethnographic reality," empathetic sentiment presently yields to a thorough consideration of the craft as an adaptation to obvious needs.<sup>13</sup>

Very far from treating, say, Omarakana village as an impermeable entity, Malinowski defines Trobriand distributions in the spirit of any competent up-to-date ethnographer. He marks off the several economic districts of the archipelago, correlates local emphasis on fishing and stone polishing with environmental peculiarities, points out the regional differences in regard to shipbuilding. He even steps outside the magic circle of the Trobriands to include Kitava because all Boyowan canoe mythology is associated with that island. Indeed, the ceremonial trading to which he devotes a major treatise could not be described without discussing other northern and even some southern Massim tribes. Where,

<sup>12</sup> *Myth in Primitive Psychology*, 18, 24, New York, 1926.

<sup>13</sup> *Coral Gardens*, 460. *Argonauts*, 105-145.

however, shall the line be drawn? Boyowa is intelligible only with reference to Massim, Massim cannot be understood apart from Melanesia, from Oceania, from all culture history as the one and only true whole.

Better still, this scorner of history himself reconstructs the past. He does so self-consciously, mumbling the purifying spell that he is "discounting any undue antiquarian or historical bias." Actually, he infers—convincingly—that taro preceded several kinds of yams in native cultivation because of its preponderance in horticultural magic. It thus appears that "the ethnographer ought to keep his eyes open for any relevant indications of evolutionary lag or historical stratification."<sup>14</sup> But, if so, there is functional disharmony: the small yam is economically more important than taro, hence it and not the taro ought to be the primary center of magical usages. The "evolutionary lag" of taro magic is evidently an instance of what the wicked evolutionists called "survival." And if historical stratification is not only permitted but prescribed when relevant, the taboo against a chronology of the past is mitigated in the obvious sense: sound reconstruction is admitted, and only fanciful reconstruction remains outside the pale.

To sum up, Malinowski's practice fortunately does not bear out the negative excrescences of his principles. He is certainly within his rights in studying most what interests him most; he becomes a dogmatist only when laying down the same tastes for others; and since he absolves himself from orthodoxy when common sense so dictates, others will do well to follow what he does rather than what he prescribes.

As for the valid core of his doctrine, we reiterate our faith in its importance. In fairness, however, it cannot be considered wholly new. Bachofen, Fustel de Coulanges, and Boas, among others, presented "several

<sup>14</sup> *Coral Gardens*, 459.

aspects (of culture) closely intertwined and influencing one another"; probably everywhere scholars have followed the practice intuitively. In his ostensibly pure description of Californian Indians, for example, Kroeber expounds the integration of Mohave life by dreams, songs, and myth. Among the Northwest Californian tribes, he finds the caste spirit reflected on etiquette, money-madness on class distinctions, caste sentiment on marriage forms. In the latter area Du Bois, too, has shown how the prestige of wealth lies at the core of tribal ideology. Among the Yokuts of central California, A. H. Gayton discovers a social utility of sorcery that strictly parallels Malinowski's findings in the Trobriands.<sup>15</sup> Representing another school, Kirchhoff has unearthed significant South American relations: matrilineal residence, bride-service, and the menial status of the son-in-law form a connected whole. Correlated with them is a special marriage form, for a husband's lot is mitigated if his daughter marries his brother-in-law, who is thus freed from the necessity of seeking a mate away from his natal village.<sup>16</sup>

In short, Malinowski is hardly the father or the sole exponent of functionalism. We gladly hail him as its most articulate, its most persuasive herald. Others have either preached *or* practiced the faith; he has done both.

There is one grave error of omission in Malinowski's achievement, the narrowness of his ethnographic approach. His rejoinder to critics who reproach him with viewing all savages in the light of Trobriand is fair enough: His generalizations purport merely to provoke

<sup>15</sup> A. L. Kroeber, *Handbook of the Indians of California*, 1-99, 754-795, Washington, 1925. Cora Du Bois, "The Wealth Concept as an Integrative Factor in Tolowa-Tututni Culture," *ALK*, 49-65, 1936. A. H. Gayton, "Yokuts-Mono Chiefs and Shamans," *UC-PAAE* 24:361-420, 1930.

<sup>16</sup> Paul Kirchhoff, "Die Verwandtschaftsorganisation der Urwaldstämme Südamerikas," *ZE*, 63:85-193, 1931.

parallel inquiries in other regions; he has, indeed, expressly demanded a "fuller testing in the various anthropological provinces."<sup>17</sup> Nevertheless, he remains an ethnographic provincial, unable to wean himself from the Trobriand, or at most the Australo-Oceanian, scene for that massive comparative attack on civilization that characterizes a Tylor or a Boas. It is not, of course, that he is actually ignorant of other areas, but that he cannot bring himself to institute intensive comparisons, such as Radcliffe-Brown, for instance, broaches when discussing the South African avunculate. Yet some of Malinowski's most important results fairly clamor for precisely this sort of checking. Why are not the Trobriands compared with other matrilineal regions, with Northwest America, Angola, Arizona? Is it of no interest to ethnology that nepotic succession, avuncular authority and paternal devotion in northern British Columbia closely parallel the Melanesian picture?<sup>18</sup> Is it not worth investigating whether that soul-stirring conflict of duty and love so graphically portrayed by Malinowski would recur in like conditions elsewhere?

The quality of Malinowski's contribution is presumably clear from the foregoing remarks. By precept and—better—by the example of his superb field work he has thrown into relief the importance of uncrystallized aspects of native life, of correlating so far as possible its several phases instead of separating them in distinct rubrics. His intolerance of other approaches, his adolescent eagerness to shock the ethnological bourgeois—that figment of his fancy, the mere technologist and oddity-monger—must not blind us to his soundness on problems of social organization, his vital ideas on primitive law and economics. On the other hand, the brilliance of his results must not dazzle us into brushing aside as inferior

<sup>17</sup> Introduction to Hogbin, lviii. *Sex and Repression*, 139.

<sup>18</sup> E.g. Franz Boas, "Tsimshian Mythology," BAE-R 31:425 f., 1916.

the methods and topics he rarely touches and, in the abstract, arbitrarily taboos.

#### T H U R N W A L D

Richard Thurnwald (1869- ), like Malinowski, is distinguished both as an observer and as a theorist. His first expedition (1906-1909) led him to the Bismarck Archipelago and the Solomon Islands, a second (1913-1915)—interrupted by the Great War—to the interior of German New Guinea, and in 1933 he resumed work in Melanesia. In 1930-1931, Thurnwald, assisted by Mrs. Thurnwald, investigated several East African tribes under the auspices of the Africa Institute. As a teacher he has been active at Halle, at Yale, and at his present headquarters, Berlin. Internationally known by his explorations, his repeated sojourns in the United States, and his innumerable publications, Thurnwald became one of the foremost liaison officers of the social sciences when he founded the *Zeitschrift für Völkerpsychologie und Soziologie*, a journal that gave full scope to his interests, at once wide and deep, in economics, sociology, jurisprudence, and psychology.

Since Thurnwald and Malinowski worked in the same general region, it is worth while comparing their results, which actually coincide to a gratifying extent. Thurnwald's earliest reports<sup>19</sup> anticipate Malinowski's emphasis on the network of mutual services and counter-services (*Leistungen und Gegenleistungen*) as a characteristic of Melanesian communities. They also set forth the barterers' tendency to reckon as equivalent only certain definite types of objects, a pig, e.g., being exchanged for a knife, not for a spear or an armlet. Thus

<sup>19</sup> R. Thurnwald, "Im Bismarckarchipel und auf den Salomoinseln 1906-1909," ZE 98-147, 1910. *Idem*, "Ermittlungen über Eingeborenenrechte der Südsee," Z vgl R 23:309-364, 1910. *Idem*, *Das Rechtsleben der Eingeborenen der deutschen Südseeinseln, seine geistigen u. wirtschaftlichen Grundlagen*, Berlin, 1910.

they foreshadow Malinowski's critique of the concepts of "value" or "money" as applied to aboriginal conditions. Theoretically significant are the recurring functionalist sentiments in these early papers. Thurnwald connects real-estate law with food-getting activities; explains inheritance rules from the social structure, including the sexual division of labor; shows how these usages are interwoven with mortuary customs and how the penal code reflects religion as well as the political organization.

But the differences are no less striking than the resemblances. Less intent on mirroring the native's inwardness, Thurnwald presents a comparatively sober picture. He insists on the importance of personal differences, but does not demonstrate them in the flesh. Less subject to the hypnotic charm of his material, he arranges it under convenient captions derived from extrinsic knowledge and views it in the light of current theory. In *Argonauts* Malinowski merely refers to the *Kulturkreis* school; Thurnwald proceeds to test its specific conclusions. Are bows really associated with pile-dwellings? Thurnwald scrutinizes the several types of dwellings seen in New Guinea and finds but a partial confirmation of Graebner's assumption.<sup>20</sup>

Thurnwald's mind, moreover, has an encyclopedic range that carries it far beyond the ethnographer's purlieus. He not only classifies his natives linguistically, but studies their somatic traits, noting albinism and pygmy statures; and defines their habitat in geographical terms. Nor is this trait confined to the sphere of observation. In a treatise on Papuan society<sup>21</sup> a painstaking exposition of marriage and kinship practices merges into a theoretical discussion from linguistic, psychological, biological, and even ethical angles. And

<sup>20</sup> "Vorläufiger Bericht über Forschungen im Innern von Deutsch-Neu-Guinea in den Jahren 1913-1915," ZE 147-179, 1917.

<sup>21</sup> *Die Gemeinde der Bânaro; ein Beitrag zur Entstehungsgeschichte von Familie und Staat*, Stuttgart, 1921.

more amazing than the scope of the author's interests is the specific way in which he considers publications in these several branches of knowledge—disquisitions on the nature of the state, reports of the Eugenics Laboratory, or monographs on American Indian relationship systems.

Polymathy, infused with an urge to systematize, has prompted both topical<sup>22</sup> and regional<sup>23</sup> surveys such as are obviously uncongenial to Boas, Malinowski, and Radcliffe-Brown. Schmidt presents possibly the closest recent parallel, but with a characteristic difference of another order. Schmidt, we found, though by no means opposed to a psychological approach, directs his main constructive efforts toward historical ends. Thurnwald, on the other hand, while professing no hostility to history, is primarily attracted by problems of another category. To be sure, he demands historical perspective lest the functional picture be distorted; amply cites documentary records; accepts and applies the contact of peoples as an explanatory factor; and even chides the Graebnerians for paying too little attention to *specific* events. So, when he sketches paleolithic society, neither his principle of reconstruction nor his essential result ostensibly deviates from Schmidt's. Nevertheless, even here there is a noteworthy difference. Schmidt strives for as particularized a picture as possible, Thurnwald is concerned with what is typical either as process or sequence. In other words, his orientation is primarily sociological.<sup>24</sup>

This preference looms large whenever Thurnwald discusses diffusion. Borrowing, he is never tired of

<sup>22</sup> *Psychologie des primitiven Menschen*, in Gustav Kafka, *Handbuch der vergleichenden Psychologie*, 1:147-320, München, 1922.

<sup>23</sup> *Die menschliche Gesellschaft in ihren ethno-soziologischen Grundlagen*, 5 vols., Berlin u. Leipzig, 1931-1935. Esp. vol. I, *Repräsentative Lebensbilder von Naturvölkern*.

<sup>24</sup> *Ibid.*, 1:15 f., 91-93, 1931; 2:280, 308, 1932; 4:297, 315, 1935.



teaching, is not a mechanical phenomenon, but depends on the recipients' as well as on the donors' culture. It has psychological correlates: not even a paddle or an arrow can be taken over without affecting the borrowers' mentality; further, there may be vital alterations in the form and meaning of what is received. Again, so far from there being an automatic transfer of traits when peoples come in contact, different loan-elements are assimilated with varying rates of velocity. All this is unquestionably sound, though these propositions are no longer peculiar to the functionalists. Since Thurnwald, on the other hand, is not on principle so intolerant of chronological reconstructions, the antithesis between him and the historical ethnologists reduces largely to a difference in emphasis or personal interests.

Such divergence, however, remains vital, for it affects what a scholar does and how he responds to a given problem. Thurnwald, while not intransigent, is at times quite orthodox in his functionalist convictions. Thus, he confronts with skepticism evidence that kinship nomenclatures have been disseminated: such diffusion, he argues, can take place only under highly favorable circumstances and implies modification, nay, a revolution in the whole social structure; never could it rest on "mere sport or idle mimicry" (*blosser Spielerei oder sinnloser Nachäffung*).<sup>25</sup> Yet Tarde has pointed out the strength of prestige suggestion; and, apart from that, as already emphasized, the doctrine that the elements of a particular culture must be organically related is a useful heuristic hypothesis but most emphatically not a demonstrated proposition. However, the purpose of our example is not to criticize but to define: what concerns us is that where others joyously explain a given distribution as intelligible only by dissemination, Thurnwald remains dissatisfied unless the fact of borrowing

<sup>25</sup> *Die Gemeinde der Bánaro*, 176 sq.

can be harmonized with his general notion of cultural integration.

Not concerned with the spread of elements as such, Thurnwald is willing to recognize independent development of similarities found in widely separated regions. He spurns the old unilinear parallelism; sensible of the intricacies of cultural growth, he concedes the distorting effect various factors might exercise on a "normal" trend of events. Notwithstanding this, however, there are residual regularities, which science must trace to their adequate antecedents. This purged evolutionism approaches Radcliffe-Brown's and—intermittently and *malgré lui*—even Schmidt's views, but Thurnwald's is the amplest elaboration, perhaps best exemplified in his discussion of the state. With due caution as to variants, he presents a series of stages, each of which purports to be the logical and psychological successor of its immediate antecedent. They are:

(1) The origin of fixed groups of families (bands, classes, settlements).

(2) Their crystallization around permanent families of leaders.

(3) Differential estimation of families and family groups according to their descent and culture (ethnic stratification).

(4) Mixture and assimilation among the ethnically distinct and diversely estimated groups.

(5) Exceptional status of mixed-breeds and assimilated persons coupled with general resentment because of segregation and difference in ranking.

(6) Aristocracy is superseded by dynastic despotism, with transvaluation of values according to dynastic relationships (castes and guilds, bureaucracy).

(7) Individualization of society, democracy and plutocracy.

(8) Upstart rulers, whose powers rest on the personal de-

votion of their military retinue. At this stage there sets in a trend toward creating a secondary homogeneity.<sup>26</sup>

To this aspect of the doctrine we shall return presently.

Thurnwald presents a rare blend of field experience, ethnographic erudition, theoretical interests, and systematic thinking. The last-mentioned trait appears to advantage in his discussion of primitive economics in Volume III of his great work. Malinowski had, indeed, advanced our insight by his investigation of selected topics, and Mauss had intensively treated the exchange of gifts. Thurnwald, however, offers the first competent ethnographic survey of the entire field from the sociopolitical point of view, thus supplementing Hahn's discussion of farming and animal husbandry. Apart from once more throwing into relief the nonrational motives in simpler societies, Thurnwald suggestively defines all pertinent concepts, such as "trade," "money," "demand," and "capital," in relation to primitive conditions.

An outstanding trait of Thurnwald's mentality is his poise. Except for occasional anti-Graebnerian flings, he avoids partisanship, judicially weighing pros and cons. We may cite his penetrating comments on Lévy-Bruhl (page 220), his utterances on the value and the limitations of direct testing techniques in estimating primitive capacity.<sup>27</sup> He fights for principles, not for shibboleths: even "functionalism" fails to hypnotize him into unqualified enthusiasm. Like Boas, he warns us against catchwords that purport to exhaust reality but merely distort it. The *couvade*, he shows, is only an extreme type of natal taboo systems and must not be treated by itself. Similarly, "marriage by capture"

<sup>26</sup> *Die menschliche Gesellschaft*, 1:9 f., 16, 24 f.; 4:24 sq., 236, 290 sq., 302 ff.

<sup>27</sup> *Psychologie des primitiven Menschen*, 174.

should not be divorced from groom-abduction, as E. C. Parsons had already indicated.<sup>28</sup>

Thurnwald is most felicitous in handling broad general principles. He sets forth admirably the role of leadership, the straining and preferential retention of definite personality types by definite forms of society. With equal clearness he contrasts the irreversibility of technological progress—where each step presupposes its predecessors—with the “cyclic” sequences not subject to hierarchical grading. While the plough, for example, must be preceded by a hoe or a dibble, matrilineal descent is merely one of a limited number of possibilities and by any objective criteria neither higher nor lower than its alternatives.<sup>29</sup>

But it is a commonplace that men have the defects of their virtues. No one could cover the range of Thurnwald’s material without factual lapses and inconsistencies, which in some volumes of his major treatise were unduly increased by his remoteness from libraries at the time of printing. On the other hand, the concern with empirical data sometimes betrays Thurnwald into over-ample description that no longer strengthens any general point, being apparently jotted down solely for the author’s future convenience.

More keenly than such peccadilloes we feel the frequency with which Thurnwald propounds correlations of the utmost significance without attempt at proof. Probably the second volume of *Die menschliche Gesellschaft* alone would furnish subject matter for two dozen doctoral dissertations. Woman’s higher status is connected with her economic independence, negatively correlated with masculine preponderance in food-getting, positively with matrilineal descent; the levirate is at-

<sup>28</sup> *Die menschliche Gesellschaft*, 2:105 sq., 4:248.

<sup>29</sup> *Ibid.*, 4:266 sq., 288 ff. “Sozialpsychische Abläufe im Völkerleben,” ALK 383 sq., 1936.

tached predominantly to clanless tribes, and so on.<sup>30</sup> Irrespective of inherent probability, such propositions cannot be accepted without the fullest inductive demonstration.

Thurnwald's weakness is thus quite different from that of many other ethnologists. He is wise and learned, he has abundantly proved his skill as an observer, he is empirically and yet also theoretically minded. The trouble is that he often fails to concentrate his facts on the crucial point of the argument. To revert to his outline of political evolution,<sup>31</sup> there are facts galore and there is a plausible scheme, but the two are very loosely connected. Thurnwald does not use his historical data to demonstrate that there actually has been a repeated succession of events in independent areas. Here, and in combining with such evidence that for the correlations alleged, but not proved, lies the greatest gap to be filled in Thurnwald's thinking.

<sup>30</sup> *Die menschliche Gesellschaft*, 2:34, 192, 246.

<sup>31</sup> *Ibid.*, 4:23 sq., 251 sq., 302 ff.

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# XIV

## RETROSPECT AND PROSPECT

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Nothing would be more remote from the truth than to conceive ethnologists as ranged in hostile camps. There are noteworthy differences of opinion, yet they often resolve themselves into a difference of emphasis or obvious misunderstandings. Some insist on disagreeing where there is perfect harmony lest they forego the dramatic role of the prophet preaching in the wilderness; others criticize their fellows not for what they believe but what in the opinion of the writer should logically be their belief.

But turning back a generation or two is to become aware of general advancement in which virtually all ethnologists share. No one now relapses into the environmentalism of Klemm; the danger lies in the opposite direction. To quote one who started as a geographer: "Neither the world distributions of the various economies, nor their development and relative importance among particular peoples, can be regarded as simple functions of physical conditions and natural resources. Between the physical environment and human activity there is always a middle term, a collection of specific objectives and

values, a body of knowledge and belief: in other words, a cultural pattern."<sup>1</sup>

Similarly, whatever divergences exist as to the innate endowment of races, responsible writers unite in regarding these native differences as comparatively small; and certainly no one nowadays would derive, say, the profusion of secret societies in Africa and their lack in Siberia from simple hereditary mental differences between Negroes and Siberians. Again, Lévy-Bruhl's provocative thesis has been examined and rejected by workers of diverse schools and most varied opportunities for observation—by Thurnwald and Boas, Driberg and Spier. As Seligman puts it, it is "in contradiction to the experience of field workers, who are after all the best qualified to judge."<sup>2</sup> At the same time the potency of irrational determinants championed by Tarde, Boas, Lévy-Bruhl, and others is an integral part of modern teaching.

In short, four *simpliste* errors—environmentalism, racialism, the notion of a prelogical primitiveness, and that of primitive intellectualism—are definitely discarded.

Turning to the much mooted question of historical connection versus independent development, we find again substantial progress since Bastian's day. There is general agreement that Bastian's "genetic law," undefined as it remained, cannot explain specific coincidences. While no one ever denied diffusion *in toto*, its importance has been established beyond cavil and what nowadays divides scholars is merely the intensity of their concern with this principle, the nature of their methodological safeguards against error. We have seen that an avowedly antihistorical functionalist like Malinowski may sporadically turn historian *malgré lui*. On the other hand, there is no unescapable conflict between functionalism and

<sup>1</sup> C. Daryll Forde, *Habitat, Economy and Society*, 463, London, 1934.

<sup>2</sup> C. G. Seligman, "The Unconscious in Relation to Anthropology," *British Journal of Psychology*, 18:373, 1928. Leslie Spier, "Havasupai Ethnography," AMNH-P 29:331, 1928.

atomism. The same student who is not a doctrinaire shifts his ground with the nature of his problem. Spier, at one time intent on plotting the distribution of isolated elements, insists most vehemently that comparisons based on the resulting tables remain inadequate. So Nordenskiöld, whose charts are a thorn in functionalist eyes, gives the most intimate picture of native drinking bouts and juvenile games. This is as it should be. As Parsons remarks, "Wisdom in ethnology, as in life, lies in having more than one method of approach."<sup>3</sup>

But harmony extends beyond matters of general approach into the field of special interpretations. Within the space of half a century a number of questions have been settled—so far as we can judge, definitively. No one now defends the three-stage theory of economic progress; and Hahn's distinction of plough-farming from hoe- and dibble-farming stands unchallenged. That promiscuity now exists nowhere and is an unproved hypothesis for the past is the view of Radcliffe-Brown, Malinowski, Schmidt, Thurnwald, Brenda Z. Seligman, and all Americanists. Swanton, Schmidt, Radcliffe-Brown, and Malinowski—to mention only a few—have definitely supplanted the idea of clan priority still held by Durkheim with the recognition of the family as a basic social unit. Even in comparative religion there is at least far-spread convergence toward acceptance of a high-god concept on rude levels: what was still unthinkable to Tylor is now cheerfully accepted by Americanists like Radin and Cooper, Africanists like Baumann.<sup>4</sup>

<sup>3</sup> E. C. Parsons, *Mitla; Town of Souls*, 479, Chicago, 1936. Leslie Spier, "Havasupai Ethnography," AMNH-P 29:83-392, 1928; *idem*, "Cultural Relations of the Gila River and Lower Colorado Tribes," *Yale University Publications in Anthropology*, No. 3, New Haven, 1936. Erland Nordenskiöld, *Comparative Ethnographical Studies*, 9 vols., Göteborg, 1919-1930; *idem*, *Indianerleben*, Leipzig, 1912.

<sup>4</sup> Paul Radin, *Primitive Man as Philosopher*, 342-374, New York, 1927. Hermann Baumann, *Schöpfung und Urzeit des Menschen im Mythos der afrikanischen Völker*, 5, 164, Berlin, 1936.



Of course it would be foolish to deny sharp differences on any number of special problems. But such diversity of opinion should be seen in proper perspective. Some of it is inevitable and desirable in a live and growing branch of knowledge; some of it is illusory—the figment of controversial would-be Messiahs who obscure issues by a melodramatic contrast between the elect and the doomed.

In the introductory chapter we saw that sound ethnology was impossible until geographical discovery had paved the way for at least a rough charting of the gamut of social variability. For some time past exploration has taken a turn towards intensiveness of survey. Whatever may be feasible in practice, our theoretical aim must be to know all cultures with equal thoroughness. How intensively a particular problem shall be studied at a definite stage varies with the circumstances, precisely as does the decimal place to which a physicist shall carry his calculations. For certain purposes it suffices to characterize a dwelling as round; in Samoa a close analysis reveals that what is so described is not a true round structure at all, but a rectangular house with shortened middle section and terminal apses. Any inferences from the occurrence of “round” dwellings in Samoa are therefore fallacious.<sup>5</sup> If technological and genealogical particulars of forbidding aspect loom large in modern monographs, they should not be interpreted as meaningless trivialities. They may be essential for the broaching of new problems, for the definition of the observed phenomenon itself. It may be a boresome detail whether the fragments of a Peruvian fabric were originally of one web, but it is of the utmost importance to ascertain how these textiles rank among

<sup>5</sup> *Te Rangi Hiroa, Samoan Material Culture*, 16 sq., 665 f., Honolulu, 1930.

those of the world; and only technical considerations of the textile expert can tell us.<sup>6</sup>

Ethnologists are not always sufficiently conscious of the assistance rendered to them by techniques and concepts extraneous to their own discipline. Yet such dependence is no cause for abasement. There are no hard and fast lines between culture and the rest of reality. For specific tasks, zoological, botanical, psychological, historical, metallurgical facts may prove more important than other phases of culture. How can we know that the aversion to incest is not innate? Only if the psychologist assures us that no such instinct exists. How could Rivet demonstrate the existence of Mexican bronze and its affinity with Peruvian metalwork? How could Nordenskiöld ascertain the efficacy of the Colombian tools made from an alloy of gold, silver, and copper? Only by requisitioning the services of a metallurgist. We cannot gauge a people's utilization of their natural resources without knowing the character of the fauna, flora, and topography, i.e., without the help of natural history and geography; and so theoretical a matter as Lévy-Bruhl's thesis can be settled only in the light of such ecological insight. This is the justification for the development of ethnozoology and ethno-botany.<sup>7</sup>

Again, cultural phenomena vary in time; and as Radcliffe-Brown's recent statements show, it is now generally recognized that we cannot understand process without a grasp of chronological relations. We have noted the revolution in thought, the sudden flood of light that emanated from Boucher de Perthes' discoveries. But whence did

<sup>6</sup> Lila M. O'Neale, "Wide-Loom Fabrics of the Early Nazca Period," ALK 215 sq., 1936.

<sup>7</sup> Paul Rivet et H. Arsandeaux, "Contribution a l'étude de la métallurgie mexicaine," SAP-J XIII, 1921. E. Nordenskiöld, *Comparative Ethnographical Studies*, 9:101-112, 1931. Erna Gunther, "A Preliminary Report on the Zoological Knowledge of the Makah," ALK 105-117, 1936. Walter E. Roth, *North Queensland Ethnography Bulletins*, Brisbane, 1901-1906.

relation of other  
sciences &  
disciplines.

this new perspective come? Evidently from the geologist's technique of stratigraphy. Repeatedly this intrinsically extraneous method of procedure has been applied with almost equally spectacular success. In the hands of Kidder and Nelson it transformed a jumble of unintelligible facts about the Southwestern United States into an orderly system. Nor should we forget that the greatest subsequent advance in this region is due to an astronomer's concern with climatology. By studying the growth of tree rings in drought and in moisture, A. E. Douglass found that "definite ring patterns recorded specific year groups and as a consequence developed a system whereby he can tell the year when a log was cut from a living tree. Beginning with trees whose actual cutting date was known he has been able to devise a type ring chart going back to about 700 A.D." Material antedating living trees thus furnished an archeological time scale: against Douglass' charts the investigator checks the rings on beams in his ruins and determines the year of the cutting. Though timber need not have been used immediately after felling of the tree or may have been used more than once, these minor deviations can be corrected and hardly detract from the accuracy of the dates computed for the erection of the buildings.<sup>8</sup>

Primitive literature (page 95) could be studied comprehensively only when ethnographers had borrowed the phonetician's technique for writing down the hitherto orally transmitted tales and poems of illiterate peoples. This loan made possible such superb works as Thalbitzer's studies of Eskimo lore and Bunzel's collection of Pueblo chants. Similarly, comparative musicology, hampered by inferior techniques until quite recent times, rose to a new

<sup>8</sup> N. C. Nelson, "Pueblo Ruins of the Galisteo Basin," AMNH-P 15, 1914. *Idem*, "Chronology of the Tano Ruins," AA 18:159-180, 1916. A. V. Kidder, *An Introduction to the Study of Southwestern Archaeology*, New Haven, 1924. F. H. H. Roberts, Jr., "A Survey of Southwestern Archaeology," AA 37:1-35, 1935.

plane with phonographic recording, and is promising to bear effectively on some of the central problems of ethnology (see page 195).<sup>9</sup>

Ethnology thus leans on her sister sciences and advances with their progress. Its autonomy no longer requires the bumptious assertiveness that marks a sense of inferiority. There is no danger of absorption in a "more fundamental" discipline, for the reality of social tradition as a separate aspect of the universe is no longer doubted. What we need is a clearer recognition of how cultural phenomena interlock with others; let us then examine the relations of ethnology with geography and psychology, since these sciences bear upon culture not intermittently but inevitably and constantly. For, however inelegant may be a people's adaptation to their surroundings, *some* solution of the environmental problem is a prerequisite for survival, hence a co-determinant of every culture studied. And still more pervasive is the psychological factor which enters not only every culture but every item of culture. For, as Boas long ago pointed out, the artifacts of a museum collection differ from the inert objects of a mineralogical cabinet because they invariably symbolize a social tradition—the interplay of minds.

#### GEOGRAPHY

Geography supplies the student of culture with a technique as well as with certain definite results. The facts he studies vary in space, and to determine their spatial relations is the first and most obvious of his tasks. A

<sup>9</sup> Wm. Thalbitzer, *The Ammassalik Eskimo*, 115-559. Copenhagen, 1923. Ruth Bunzel, "Zuni Ritual Poetry," *BAE-R* 47:611-835, 1932. Helen H. Roberts, "Melodic Composition and Scale Foundations in Primitive Music," *AA* 38:79 sq., 1936; *eadem*, "Musical Areas in Aboriginal North America," *Yale University Publications in Anthropology*, No. 12, New Haven, 1936. George Herzog, "Plains Ghost Dance and Great Basin Music," *AA* 38:79 sq., 1936; *eadem*, "Musical Styles in North America," *ICA* 23:455-458, 1928.

-spatial relations

distributional approach does not solve all problems but it is a first step towards understanding. It is fashionable to deride this pedestrian procedure, but its prophylactic efficacy remains unchallengeable. Without accurate information about distributions we are unable to appraise any given achievement. If the zero concept were the common property of the Maya and the Australians, its presence would bear a wholly different significance. So we cannot regard moieties as a general phase of social evolution when they are so conspicuously lacking in Africa. Because the range of one trait regularly coincides with another we suspect a significant bond between them. On the other hand, a given distribution may suggest all sorts of historical problems. It was the geographical tabulation of mythological episodes within a continuous area that enabled Boas to prove diffusion of tales beyond the shadow of a doubt. On the other hand, an intermittent occurrence raises questions of another type. The hockey game of Plains Indians turns up in the Gran Chaco, but it is lacking in the intervening territory. Nordenskiöld plausibly suggests that the game was shared by the ancestral groups which were once in contact but later drifted apart. The story of a benevolent and a marplot brother, the latter introducing death and labor into the world, occurs in California and in Tierra del Fuego, while the tales about twins in such regions as the Brazilian interior are basically different. Hence neither psychic unity nor pan-American unity accounts for the facts, which are again most easily explained—whether correctly or not—by early intercourse.<sup>10</sup>

Naturally cartography is a mechanical instrument, not a master. If we plot distributions in ignorance of vital facts, sham issues are inevitably raised. Thus, it is one thing to imagine that coiled baskets are lacking in South

<sup>10</sup> Erland Nordenskiöld, *Comparative Ethnographical Studies*, 9:90, 1931.

ambiguous vs  
intermittent  
distribution

America outside of Fuegia and quite another to discover them also in Panama, Colombia, Brazil, Peru, and on the Chilean coast. Similarly, the absence of hoes from all of South America would be of real interest were it not that their undoubted use among the Quichua, past and present, eliminates the problems otherwise raised. Naturally, the sane cartographer must pay attention to chronological differences emerging from his sources. Thus, Nordenskiöld finds that in the Upper Amazonas the earlier spear throwers were supplemented by the blowgun. This, in turn, stimulates the question of why such a change took place; and we learn that it hinged on new trade relations that brought in curare since without this poison the darts blown from a blowgun remain ineffectual missiles in warfare. Naturally, it is important to define one's items so that only the same concept receives the same label. Here, Nordenskiöld, whose procedure is generally most commendable for the exhaustiveness of his survey, both spatially and temporally, is not always convincing. While "hockey" is undoubtedly—irrespective of interpretation—a clearly defined feature, this no longer holds for the rubric "tents of animal skins"; since their shape is admittedly quite different from that of the North American tipi, no good purpose is served by the common label.<sup>11</sup>

Such sporadic lapses by students of distribution in no way warrant the slurs cast on their endeavors in recent times. "Atomistic" investigations, when based on a conceptually sound typology, on the complete spatial distribution of traits and on their ascertainable temporal modifications, are fully as justified as the aesthetically more attractive "totalitarian" approach. Ethnology, we repeat, is not merely the science of cultures but of culture—of every fragment of the universe pertaining to the social heritage of all human groups.

If the cartographic approach imperceptibly leads to

<sup>11</sup> Nordenskiöld, *op. cit.*, 2:24, 168, 1920; 3:59-64, 1924; 9:77-94, 1931.

questions of deeper import, the consideration of the physical conditions confronting a society forms an integral part of any clear conception of their culture. It is not at all a matter of identifying culture with an automatic response to environment—an error long since exposed and buried. What is involved is partly again the elimination of sham problems. As Nordenskiöld has so beautifully shown, certain traits are simply barred by environment: south of Santa Cruz, Bolivia, bark cloth and fish-drugging disappear for want of suitable species of trees and narcotic plants, respectively. With navigable rivers are eliminated boats, with the absolute lack of the raw material, the whole art of stonework.<sup>12</sup>

Discussion of many problems is bound to remain sterile without the application of geographical insight. Farming looms large in the history of mankind, but the problem of its origin and spread involves matters no ethnologist can settle without recourse to principles not derived from the social heritage alone. Given the general equipment of incipient cultivators, there is the matter of possible soils. In Northern Mexico settlements were restricted mainly to flood plains and to rough mountain lands to the exclusion of rich clay and clay loam tracts, such as primitive dibbles or even bone and stone mattocks cannot cope with. The Californian aborigines resisted the impulse to adopt farming after the fashion of the Colorado River peoples partly because of their soils, partly because those crops which were available to them could not be profitably raised in regions with winter rain. "The Pacific Coast of the United States, as a land of Mediterranean climate, had to wait on the introduction of crops from the European Mediterranean." For maize thrives in areas that are warm and humid during the initial months of growth and not, as some ethnographers have suggested, in arid surroundings. More generically, Sauer finds nas-

<sup>12</sup> E. Nordenskiöld, *Indianer und Weisse*, 15-20, Stuttgart, 1922.

cent farming must be sought in forest lands, not in brush or grass areas, which present insurmountable impediments to any but advanced forms of tillage.<sup>13</sup>

Investigators of Arctic peoples have consistently stressed environmental conditions not because they determine these cultures any more exclusively than elsewhere, but because they are so obtrusive as co-determinants. Eskimo life is a constant interplay of geographical and cultural factors. The Southampton Islanders, lacking the soapstone other Eskimo use for their lamps, substitute vessels of limestone slabs painfully cemented together, yet tenaciously adhering to the traditional form. The immense length of sledges in certain Eskimo tribes, the substitution of wood for bone, the importance of intertribal trade relations—all these and a dozen other features depend directly on local circumstances. Obscure and mooted as the origins of Eskimo economy and its relations to that of the Indians remain, no sane theory will ever dispense with thoroughgoing ecological considerations. Birket-Smith, Speck, and others have thrown into relief the contrast between the subarctic Indians dependent on the timber lands for winter subsistence and the litoral Eskimo capable of dispensing with the shelter of the forest. Hatt had previously pointed out the revolutionary importance of snowshoes, which enabled the Indians to hunt freely over the inland area of Canada, while hitherto they had been obliged to hug the lakes and river courses and maintain themselves by fishing there. Thus, a magnificent perspective opens up on the remote past of an as yet undifferentiated subarctic culture, which subsequently evolves in divergent directions, the adoption of snowshoes leading the Northern Indians along one line of evolution, while adaptation to hunting from the ice produced the

<sup>13</sup> Carl Sauer, "American Agricultural Origins: A Consideration of Nature and Culture," *ALK* 279-297, 1936; *idem*, *Aztatlan, Ibero-Americana* 1:58 sq., Berkeley, 1932.



distinctively Eskimo economy.<sup>14</sup> These suggestive views are all rooted in a geographical orientation.

Siberian culture is likewise unintelligible without a proper understanding of natural conditions. Why is the mode of settlement so different in corresponding latitudes of America and Asia? Because, Bogoras explains, the Asiatic litoral differs: except in the northeast the shallowness of the ocean and the low swamps of the coast are unfavorable to habitation. How, again, can one understand the spread of man and his works in Northern Siberia without knowing of the network of connected rivers which preclude walking but allow travelers to paddle from one river system to another? Similarly, our outlook must be wholly askew unless we realize the enormous abundance of edible animals, the incredible ease with which fish are taken.<sup>15</sup>

The aversion from geography is historically comprehensible, but none the less unjustifiable. No ethnologist now shares the delusion that culture is man's inflexible reaction to his physical surroundings, a view repudiated by Ratzel himself. Everyone admits the *tertium quid* described by Forde (page 250), everyone sees that pure environmentalism would imply an exclusively rational attitude of humanity such as has been amply refuted. But when every possible allowance is made, the obvious fact still stands that societies have attained solutions of practical problems. Further, this adaptation to the conditions of life constitutes the outstanding intellectual achievement of mankind. Everything, then, that contributes to our insight into the conditions offered by nature deepens

<sup>14</sup> F. Boas, "The Eskimo of Baffin Land and Hudson Bay," AMNH-B 15:75, 357, 1901. K. Birket-Smith, *The Caribou Eskimos*, 1:233; 2:212-233, Copenhagen, 1929. Gudmund Hatt, "Moccasins and their Relations to Arctic Footwear," AAA-M 3:151-250, 1916. F. G. Speck, "Culture Problems in Northeastern North America," *Proceedings of the American Philosophical Society*, Vol. LXV, No. 4, Philadelphia, 1926.

<sup>15</sup> W. G. Bogoras, "Elements of the Culture of the Circumpolar Zone," AA 31:579-601, 1929.

our insight into the character of culture. "Human geography," says Forde, "demands as much knowledge of humanity as of geography."<sup>16</sup> He is right, but he is addressing geographers; for the student of culture the maxim should be reversed.

### PSYCHOLOGY

In this context we are neither concerned with the psychologizing of the man in the street nor with the interpretative guesses of the closet philosopher. "Psychology" designates the results and—at least, nascent—concepts of a branch of learning specializing in the inborn attitudes and behavior of human beings. Precisely because psychology is in principle concerned with what is not culture, its interests and those of ethnology must overlap in practice. For no one knows intuitively what is and what is not part of man's "original nature," everyone's judgment being warped by his personal experience. Positive facts ascertained by either science thus prove a corrective for the other, helping to delimit both fields.

The general trend of research has apparently been to narrow the psychologist's sphere of influence. It is not "natural" to point with the index finger, for Pueblo, Basin and Plains Indians commonly do so by protruding their lips. African and Oceanian observation suggests that "even at the crawling and toddling stage the primitive child can seldom be left alone and must spend a large part of its time *balanced on its mother's hip.*" But contact with American Indians limits the generalization and exposes the practice as conventional. Tylor, though recognizing handshaking and kissing as far from inevitable human responses, still believed in a gesture-language that was "essentially one and the same in all times and all countries." But the signs of Queenslanders and Sioux

<sup>16</sup> C. D. Forde, *Habitat, Economy and Society*, 465.

lend little support to psychic unity. Again, an American native draws a knife towards his body, the African Negro whittles in the opposite direction. Sometimes even neighboring groups of the same race display striking diversity in simple everyday matters: the Pueblos carry loads on their heads, the Havasupai never do; most American natives chip stone tools, but the coastal tribes of the Northwest only peck and polish them. Emotional expression varies as much as do motor habits. On the Plains of North America black betokens not grief but victory; in many tribes weeping reflects not sorrow but a ceremonial duty; and so forth. Boas thus sums up an immense range of observations when he declares that the plasticity of the human organism makes it follow the cultural pattern with which it has become identified.<sup>17</sup>

At first blush such conclusions seem to make the cooperation of psychology and ethnology a one-sided affair. Men all live in society, and if society so deeply affects their outlook on behavior, all the illumination seems to come from the cultural side, with nothing given in return. Thus, Wissler argues that individual differences of motor habit are not a cause of relevant tribal differences in basket-work: "Culture differentiation and psychological differentiation . . . run in relatively independent cycles," the former explicable only in historical terms.<sup>18</sup> But even if we fully accept such a dictum, it would not prove the futility of psychology for our purposes. While the hierarchical scale of the sciences does not properly express their relations, matters are not mended by simply revers-

<sup>17</sup> Leslie Spier, "Havasupai Ethnography," AMNH-P 29:329, 1928. Brenda Z. Seligman, "The Incest Barrier: Its Role in Social Organization," *British Journal of Psychology*, 22:259, 1932. E. B. Tylor, *Researches . . .*, 45, 53 f. F. Boas, "The Effects of American Environment on Immigrants and Their Descendants," *Science*, 84:522 sq., 1936; *idem*, *Primitive Art*, 145 sq. A. L. Kroeber, "The Arapaho," AMNH-B 18:417, 1907. Otto Klineberg, "Notes on the Huichol," AA 36:459, 1934.

<sup>18</sup> Clark Wissler, "Material Cultures of the North American Indians," AA 16:501, 1914.

ing the ladder nor do we progress by clinging to a single rung. Actually the analogy is misleading because the interrelations of the sciences are of a quite different character. Of course culture cannot be subsumed under mental processes, otherwise there would be no ethnology at all—no more than there would be a biology if organisms grew and bred merely by the laws of gravitation and of chemical affinity. When the autonomy of our subject is once granted, however, declarations of independence grow repetitious. Our position towards psychology should correspond to that assumed towards geography: we cannot explain all of our phenomena through it, but neither can we explain them fully without it.

In the first place, then, psychology, working from the opposite end, rules out interpretations that lure and ensnare the unwary ethnologist. Can exogamy be reduced to an innate incest sentiment? The modern critique of the instinct concept explodes the theory and restricts the problem to its proper cultural sphere. On the other hand, mental manifestations bear constructively on such perennial themes as the diffusion controversy. How did the figments of mythology take form? Dreams parallel them and provide a possible source. That they themselves in part reflect the regnant folklore offers no ultimate explanation. But their content recurs in remote areas with analogous interpretations yet in conditions virtually precluding dispersal from tribe to tribe. Such distribution, then, supports the possibility of independent origin.<sup>19</sup> This is not a relapse into reliance on a vague "genetic law": specific ideas of mythology are connected with specific mental phenomena or their antecedents. Naturally, what we need for clearer insight is fuller information on savage dreams.

However, a general human explanation breaks down when diversity, not likeness, is to be explained. Here psy-

<sup>19</sup> C. G. Seligman, "The Unconscious in Relation to Anthropology," *British Journal of Psychology*, 18:377 sq., 1928.

chology might still conceivably render service by tracing group differentiae to original nature, but to nature racially circumscribed. Actually, to be sure, any differences of endowment that may exist are not congruous with cultural differences. This, however, is far from maintaining that the races are alike. To quote Thomas' reasonable conclusion: "It is to be emphasized . . . that there are no proofs that the mind is of precisely the same quality in all races and populations, and no such claim is made by anthropologists. It is not improbable that there is a somewhat different distribution of special abilities, such as mathematics, music, etc."<sup>20</sup> Here is the rub. Such variations may even jointly account for only a small fraction of culture, but insofar as they exist they are *verae causae*. The findings of the Cambridge Expedition to Torres Straits, while largely inconclusive, do suggest a higher threshold of pain for the Papuans than for the Caucasians—a result later paralleled among the Vedda. More fully corroborated, such inferences would help account not indeed for the mutilations to which many savage societies subject their initiates, but for the fact that such disfigurement could ever have been conceived and could ever have succeeded in persisting. Again, while the development of African as against native American music may be largely due to borrowing from Egypt and India, we cannot a priori exclude a higher innate aesthetic sensitiveness. Everything urged against overenthusiastic testers may be granted. If native ability is to be determined, the groups compared must be relatively pure; we must discount for environment in choosing tests; affective and motor factors may be at least as potent as the intellect. But when all this is not merely conceded but emphasized, specific racial differences remain a possibility with potential consequences for cultural differentiation. The only science equipped to deal with this question is psychology;

<sup>20</sup> W. I. Thomas, *Primitive Behavior*, 799, New York, 1937.

and its controlled results are equally important to ethnology, whether they culminate in a proof or in a refutation of racial faculties and disabilities. In the one case we acquire a potential explanation of empirical differences; contrariwise we are thrown back upon historical accident as the sole determinant.

Original nature may differ individually as well as racially, and here psychology offers a positive contribution of incalculable importance. Galton's notion of congenital individual variability, seized by Boas and others, has revolutionized the outlook on savage life. What had hitherto been a static phenomenon now appeared instinct with the germs of change; automata obedient to custom gave way to human beings paralleling the gamut of emotional and intellectual values familiar in civilization. These deviations, moreover, are sometimes demonstrably significant for society, and it is the joint task of psychology and ethnology to define the interplay of personal and social determinants.

One clear-cut result has been attained. There is a social selection of personalities, discussed by Thurnwald under the caption "Siebung" (sifting) and independently by several British and American authors.<sup>21</sup> The madcap hero of a horde of warriors is the ruffianly bravado of a more staid society; the musing sage of one group is a maladjusted milksop in a mining camp. Indeed, as Seligman remarks, savages invest with prestige persons we should clap into an asylum for the insane. Since this is a general human process, the records of literate peoples might well be scrutinized from this angle. Savage society presents no more striking case than the ascendancy of

<sup>21</sup> R. Thurnwald, article "Siebung" in M. Ebert's *Reallexikon der Vorgeschichte*, Berlin, 1929; *idem*, *Die menschliche Gesellschaft*, 4:264 f., 309 sq., Berlin u. Leipzig, 1935. Barbara Aitken, "Temperament in Native American Religion," *JRAI* 60:363-387, 1930. Ruth Benedict, "Configurations of Culture," *AA* 34:1-27, 1932. Ralph Linton, *The Study of Man*, 443-487, New York, 1936. C. G. Seligman, *op. cit.*, 374.

Samuel Johnson in British life contemporaneously not only with Lessing and Voltaire on the Continent, but with Adam Smith, Hume and Gibbon in Scotland and England. For the British strainer, to use Thurnwald's analogy, these towering intellects were mere dregs.

Social sifting may effect biological selection. Unremitting preference for preferred traits, say skill in the chase, might culminate in the elimination of the "unfit," hence lay the foundation of hereditary group differences. If so, these would be demonstrable only by psychological techniques. In the light of present knowledge, meagre as it is, an alternative result seems more plausible, viz., the survival of the "inferior" types through protective mimicry. They assume naturally lacking virtues, persist in ordinary circumstances, and fail only when unmasked by a major crisis.

Stimulated by psychology, then, the ethnologist has shown that individual variation exists in rude societies; and that societies, recognizing diversity, respond differently to the same variation, one group exalting the very deviation that is derided by its neighbors. Further, the occurrence of deviants accounts for the undoubted fact of change. As Morgan remarks, personal experiences—rooted in mental idiosyncrasies—may "deeply condition the individual, sometimes so deeply that if the experience is at variance with a tribal . . . belief, the individual will retain his own variation. There can be no doubt that this is a very significant means of modifying a culture." Whether such an alteration is achieved depends obviously on the strength of the aberrant personality, with the social receptiveness as the co-determinant. The ethnographer takes cognizance of the deviation regardless of whether it actually leads to a new tradition. Thus, in northwestern California, where women are the shamans, supernatural revelations normally exclude the military tenor so dominant on the Plains. Yet a sporadic

gallant will seek a lonely spot, acquiring as a spirit's blessing strength as well as wealth. Because of the tribal ideology, "most men's ambition did not lie in this direction"; yet even the rare presence of the impulses typically expressed in Eastern visions has cultural meaning, because here lies the possible germ of a traditional change of shamanistic purpose and routine. Other cases are less hypothetical. Among South American aborigines the Guarani are noted for their periodic migrations in search of an earthly paradise. The prophets mimicked the behavior of mythical heroes and to that extent were conditioned by their environment. But why did such characters arise only now and then? And what made their tribesmen welcome these "véritables acteurs d'un drame mythique"? There were evidently leaders and followers; further, the leaders themselves differed in the specific nature of their message, which was thus *not* traditional. More clearly, though not by any means clearly enough, we recognize the force of qualitative variations in the personalities of the North American "messiahs" who periodically arose soon after the first contact with civilization.<sup>22</sup>

Why does our insight into such events remain imperfect? Evidently because our reports are inadequate on the psychological side. The biographical details, as a rule too sketchy even for a layman, hardly ever suffice for a "clinical" picture of the actors. Unfortunately the scientific understanding of personality is as yet inadequate. The field has been largely cultivated by psychiatrists, often phenomenally penetrating in their intuitive grasp of a patient's needs, but weak in conceptualization

<sup>22</sup> Wm. Morgan, "Human Wolves among the Navaho," *Yale University Publications in Anthropology*, 11:40, New Haven, 1936. A. L. Kroeber, *Handbook of the Indians of California*, 67 f. Curt Nimuendajú, "Die Sagen von der Erschaffung und Vernichtung der Welt als Grundlagen der Religion der Apapocúva-Guarani," *ZE* 46:284-403, 1914. A. Métraux, *La religion des Tupinambá*, 217, Paris, 1928.



and critical judgment. Their classifications, possibly of great therapeutic and even theoretically of heuristic value, must be treated with reserve. Thus, we cannot follow Seligman in his wholehearted use of the psychoanalytic distinction between "extravert" and "intravert" types for explaining shamanism. In the first place, for cautious psychologists these categories strictly apply only to extreme forms of mentality, normal personality displaying the commingling of extravert and intravert elements. Secondly, ethnographic experience does not bear out the contention that savage peoples are predominantly extravert but rather suggests the very same dual character among them as among ourselves. To quote one of our keenest observers about the Maricopa of Arizona: "Like all Indians, these people can sit endlessly saying nothing and looking fixedly into space . . . On the other hand, they can become excessively talkative and are at all times ready to joke and laugh,"<sup>23</sup> Brooding and boisterous self-expression are thus not at all mutually exclusive.

But if the psychiatrists are precipitate and the academic students of personality overdilatory, is not the ethnographer left in the lurch? Yet the situation is less desperate than it seems. Once freed from the fallacy that psychology is to explain culture without residue, the ethnographer profits, first of all, by the psychologist's "case method." He must not forget that the culture he investigates is a living reality only as mirrored in its bearers; the two are as inseparable as the sides and the angles of a triangle. In other words, the culture by itself is an abstraction; the reality is adequately described by exhibiting samples of personality responding to the social setting. The correct procedure is to give an adequate definition of both. The better observers have—sometimes quite intuitively—groped towards such char-

<sup>23</sup> Leslie Spier, *Yunan Tribes of the Gila River*, 327, Chicago, 1933.

acterization. Rasmussen's demonstration of an Eskimo skeptic helps delimit shamanism as a *cultural* phenomenon, as does Bogoras' account of Siberian inverters as shamans. Radin's biographies of Winnebago Indians serve the same purpose. Hallowell's frankly programmatic study of Western Ojibwa consciously applies modern clinical concepts to individual cases. In one of his instances a powerful wonder-worker satisfies his incestuous cravings and justifies them in terms familiar to his group; he is merely carrying out the dictates of a tutelary spirit, which cannot be disobeyed without a sense of sin. His rationalization has the strongest efficacy from the aboriginal viewpoint, yet it is interesting that even in this exceptionally favorable instance the flouting of custom was unable to quell some sense of guilt.<sup>24</sup>

Here, as usual, the picture is blurred by the meagre description of the actor's personal traits. The merit of Hallowell's approach lies in its concentrating on the issues involved. Addressing psychiatrists, he emphasizes the significance of ethnographic data when garnered by an observer "aided in formulating his problems by the psychiatrist sensitive to the implications of culture." Our point is that such formulation may be profitable for the student of culture provided the psychiatry is sound. This means that it must progressively fight shy of catchwords, substituting empirically definable descriptive traits. Radin cogently argues for the reality of an intellectual class among primitive groups, and on this assumption plausibly deduces the possibility of monotheism as an individually recurring creed.<sup>25</sup> But the psychological identification of such believers is less satis-

<sup>24</sup> W. Bogoras, "The Chukchee," AMNH-M 11:415, 426 sq., 441, 450 sq., Leiden, 1909. Knud Rasmussen, *Intellectual Culture of the Caribou Eskimo*, 58 sq., Copenhagen, 1930. A. I. Hallowell, "Psychic Stresses and Culture Patterns," *American Journal of Psychiatry*, 92:1291-1310, 1936.

<sup>25</sup> Paul Radin, *Primitive Man as a Philosopher*, 342 sq., 366, New York and London, 1927.

factory. They are supposed to combine an eminently religious temperament with speculative capacity; but what precisely *is* eminent religiousness? Do the historic figures commonly credited with this characteristic conform to a single mental type? What is the common denominator of the saintly recluse and the propagandist, the bold inventor and the zealous preserver? The psychology of the future will, we hope, supplant vague terms drawn from vulgar experience with the subtler concepts of a more refined observation.

Although personality stands in the foreground of current discussion, the contacts with psychology may be viewed from another angle. Psychology may help even when it fails to shed direct light on an ethnographic phenomenon. According to Wissler we saw that cultural diversity has a solely historical basis. But the subject Wissler discusses is the direction of movement in basketwork, in other words, motor processes, a concept ethnographically significant because cultures vary with regard to such processes, yet obviously one transferred from psychology. Further, Wissler cites evidence to show the difficulty of an individual change from sewing in a clockwise to counterclockwise direction or vice versa, even though the initial choice of either alternative be accidental. He is thus invoking another psychological concept, habit; and habit is not without pertinence to our cultural problems. According to Weltfish, all modern American Indian basketry weaves occur prehistorically within the same areas. This amazing stability is surely to be connected with the persistence of habits once formed, as Boas suggests. In other words, the varying processes in basketry may well have a historical basis and yet be related to vital facts of psychology.<sup>26</sup>

<sup>26</sup> Gene Weltfish, "Problems in the Study of Ancient and Modern Basket Makers," AA 34:108-117, 1932; *eadem*, "Prehistoric North American Basketry Techniques and Modern Distributions," AA 32:454-495, 1930.

In this connection may be cited Fechner's ideas on experimental aesthetics.<sup>27</sup> He may be quite wrong in postulating his "golden section"—an ideally pleasing ratio of the sides of a rectangle. But rectangles appear as a distinct feature in Plains Indian decoration; we can measure their sides, and determine tribal preferences. What if Fechner's ratio turns out to be fallacious? What if the observed preferences are due to historical chance? If real, they aid towards a finer discrimination between regional geometrical styles: the psychological stimulus proves ethnographically profitable.

Psychological factors loom large in primitive religion even apart from the obvious phenomena of leadership. Dreams are of vital relevance, but again mental variations of definable character may assume ethnographic importance. The dream reports of Colorado River Indians are steeped in a mythological atmosphere; those of their kinsfolk on the Gila describe actual sleep imagery. The latter can no more be disregarded than the former when the tribal contrast hinges precisely on this distinction. But we are confronted not merely with free sleep experiences and those traditionally patterned. There are visions as well as dreams, and whole areas differ in whether they stress one or the other form of experience. As a matter of fact, "visions" are sometimes either nothing of the sort but hallucinations of an auditory nature, or at least are associated with nonvisual components. This is not a trivial point considering the incredible frequency of sacred, i.e., revealed, *songs*. Among the Navaho, again, the capacity to pass into a trance characterizes the culturally recognized class of diagnosticians. Those aspirants fail who cannot "withdraw sufficiently from conscious awareness

F. Boas, *Primitive Art*, 149. L. Spier, "Havasupai Ethnography," AMNH-P 29:136, 1928.

<sup>27</sup> Gustav Theodor Fechner, *Vorschule der Aesthetik*, Leipzig, 1876. Ch. Lalo, *L'esthétique expérimentale*, Paris, 1908.

to allow any stream of unassorted ideas to pass through their mind, or for a picture to form itself." The ethnographer cannot well make shift without some recourse to psychology.<sup>28</sup>

But the psychology should be accurate. The failure to attach a precise and accepted meaning to the terms "trance," "frenzy," "orgy," largely vitiates Benedict's interesting contrast of non-Pueblo and Pueblo cultures of North America. The supposedly Dionysian worship of the former, rooted in their "ecstatic" visions, constitutes a major differentia of the scheme. But the revelations in question conform to this pattern only to a moderate degree. The Crow Indians do employ the word for "intoxication" when describing the hypnotic state of a Sun Dancer after protractedly riveting his gaze on a sacred effigy. It also embraces the condition of a person whose indwelling supernatural threatens to pass out of the host's mouth. But the term is not extended to the more customary supranormal phenomena, and the narratives of such experiences only rarely imply ecstasy. Still less is the ecstatic timbre discernible when an Ojibwa boy seven years of age retires to fast at his elders' prompting and after meticulous surveillance acquires the sort of guardian spirit his instructors permit him to accept.<sup>29</sup>

The following principles, then, hold. Neither science can be reduced to the other, but cross-fertilization is practicable and helpful. Ethnology enlarges the psychologist's ken, demonstrating the scope of social patterning in individual behavior. The ethnologist, inevitably bound to use terms relating to the mind, uses the de-

<sup>28</sup> A. L. Kroeber, *Handbook of the Indians of California*, 753, Washington, 1925. Leslie Spier, *Yuman Tribes of the Gila River*, 238, 257, Chicago, 1933. William Morgan, "Navaho Treatment of Sickness: Diagnostics," AA 33:390-402, 1931.

<sup>29</sup> Ruth Benedict, "Psychological Types in Southwestern Culture," ICA 23:572-581, New York, 1930; *idem*, *Patterns of Culture*, New York, 1935. Paul Radin, "Ojibwa and Ottawa Puberty Dreams, ALK 233-264."

terminations of scientific psychology prophylactically against the snares of vulgar psychology; and constructively in refining his analysis of regional differences and of particular processes.

#### META-ETHNOGRAPHY

The anthropological expeditions that followed on the heels of the Tylor period yielded publications which differed notably from earlier accounts. Addressed to a scientific public, they generally lacked literary quality, which indeed could hardly be preserved because of the technical detail that demanded registry. Since, furthermore, the aim was to render the material available for comparative work by scholars, a stereotyped arrangement evolved to facilitate reference. To take at random two representative monographs on very different peoples, the Maidu (California) and the Yuchi (Southeastern United States), both authors begin with certain generalities as to habitat, demography, and history; then proceed to material culture, art, social organization, and the life cycle; and close with religion and mythology.<sup>30</sup> It was both natural and essential that field workers, apart from any chance observations they could make, should inquire into matters emphasized by the great theorists of the preceding period. The results inevitably varied with the writer's ability: born observers were not hampered by the fashionable procedure, which in the hands of others still yielded worth-while though uninspiring fruits. Its dangers are being vastly overrated at the present time: the true creative spirit is never cramped by formal restrictions and the dullard remains petty whether he devotes himself to the uncrystallized phases of culture or to free verse.

<sup>30</sup> Roland B. Dixon, "The Northern Maidu," AMNH-B 17:119-346, 1905. Frank G. Speck, "Ethnology of the Yuchi," University of Pennsylvania, *Anthropological Publications of the University Museum*, 1:1-154, 1909.

With growing insight into savage existence it became clear that the traits obvious either through the natives' or the theorists' emphasis did not cover the whole of social life. Boas early drew his students' attention to such amateur records as the Lapp Turi's reminiscences and Rasmussen's first book. In 1922 some twenty Americanists under the leadership of E. C. Parsons, recognizing the deficiency of current monographs, deliberately set out to correct this by fictitious biographical and impressionistic sketches based on observation of their favorite tribes. In her preface the editor thus criticized contemporary technical literature: "The commonplaces of behavior are overlooked, the amount of 'common sense' is underrated and the proportion of knowledge to credulity is greatly underestimated." In the same year Malinowski proclaimed the need for going beyond "the collection of crystallized, ethnographic data." When, therefore, a decade later Margaret Mead issues a clarion call for recognition of "all parts of a culture, and not merely those which present the superficial appearance of having greatest form," this manifesto is somewhat belated. The technique she prescribes for such researches, viz., the systematic observation of countless concrete instances, is excellent; but her goal is not new and she seems strangely unaware of previous striving towards it. Our nonhistorically minded younger generation often rediscover America, and it is perhaps cruel to disturb their illusions.<sup>31</sup>

While Mead throws into relief aspects that escape notice because unstressed by the natives, Benedict calls attention to the "dominant drives" of cultures. Certain integrative principles shape the raw material of custom; no more mysterious than art styles, they "are as

<sup>31</sup> E. C. Parsons (editor), *American Indian Life*, 2, New York, 1922. ✓  
Br. Malinowski, *Argonauts* . . . , 20, 1922. Margaret Mead, "More Comprehensive Field Methods," AA 35:1-15, 1933.

characteristic for individual areas as are house forms or the regulations of inheritance,"<sup>32</sup> hence demand recognition.

This view has both philosophical and ethnological affinities. In the history of psychology its cognate is the critique of associationism, with its insistence that the whole is more than a summation of its parts. In the study of civilization Benedict recognizes such forerunners as Dilthey and Spengler, but urges that the higher cultures they have investigated are too complex to reveal their essence. Ethnographically, she must be grouped with all who study the connection between elements rather than the discrete elements. Thus, contemporaneously with Benedict's earliest essay in this field an article by Barbara Aitken distinguished Pueblo from Eastern Woodland religion not in point of content but for its difference in emphasis. The Easterners had allowed the individualistic temperament to set the tone, while the Pueblos yielded precedence to the social temperament.<sup>33</sup> While this gives a psychological character to the cultural differentiation, several earlier writers had demonstrated patterns especially in ceremonialism without recourse to basic mental facts. Boas' pupil Haeberlin, for example, showed that Pueblo religion was characterized by the ever-recurring idea of fertility, which sharply set off the identical ritual as performed by Pueblo and Navaho, respectively.

Benedict's aim, however, transcends her predecessors' in envisaging the totality of culture. This would seem to bring her into the functionalist camp, but she insists on marching ahead of its vanguard. True, Malinowski has shown each element playing its part in the whole; but what manner of whole is it within which the

<sup>32</sup> Ruth Benedict, "Configurations of Culture," AA 34-1-27, 1932.

<sup>33</sup> Barbara Aitken, "Temperament in Native American Religion," JRAI 60:363-387, 1930.



traits function? And while he has shown reciprocity as a "basic behavior trait" of Melanesians, Benedict would like him to disclose the "fundamental attitudes" of which such behavior is the outward symbol.

The goal, then, is to fix the stylistic peculiarities of cultures and to express them in psychological terms. Benedict disarms criticism by admitting that many cultures have failed to achieve a thoroughgoing integration; and by narrowing the scope of her principle so as to exclude technology. She thus seems to vacillate between a consistent totalitarianism and the recognition of "dominant drives." Moreover, to date she has fully discussed only three peoples from this point of view.<sup>34</sup> All this makes it peculiarly difficult to assess her contribution.

Against the totalitarian version of the doctrine we repeat the criticism already advanced against Malinowski and Radcliffe-Brown. Even if technology is barred, it remains an unproved hypothesis that all the residual items are integrated. On the other hand, the search for dominant drives is a fruitful idea; and the picture Benedict herself offers of a Pueblo people is both vivid and suggestive. The subject has not, indeed, been so uniformly neglected as she implies: the wealth obsession of northwestern California, the lure of military glory on the Plains, are ethnographic commonplaces. But Benedict's gospel will make us seek *leitmotifs* in more recondite spots and should prompt scrutiny beyond their gross manifestations. In any case, her discussion concentrates thought on several worth-while questions. In what circumstances do people achieve a distinct ideology? Are configurations, for instance, to be found on the Fuegian-Basin—Negrito level? And what is their total influence on the several cultures that disclose it?

Patterns, then, if not all-pervasive, may be accepted as potentially extending over major cultural blocks. How

<sup>34</sup> *Patterns of Culture*, New York, 1935.

are they to be defined? Here we feel Benedict has been somewhat lacking in caution. The reality she seeks, which she aptly compares to an art style, shares with such styles a comparative elusiveness. The danger of impressionism is averted only by the ample and concurrent testimony of several good witnesses. That is why the Eskimo, described in detail by so many first-rate investigators, would form an ideal subject for configurational treatment, one whose omission is hardly intelligible. That is why Benedict's account of Zuñi, resting as it does on most ample documentation by herself and other observers, is so satisfactory; why the contrast between Pueblo and Eastern Indians, sensed as it is by Aitken, Kroeber, and others, ranks as a fact, not mere fancy. But it also indicates why Benedict erred in choosing for one of her three major subjects the Melanesian Dobu, whose supposed pattern is derived from a single source with unimpressive documentation.

There is another pitfall. In setting off configurations against one another there is an inescapable tendency to overweight differences: the writer, intent on distinguishing, distorts the total picture in favor of his specific criteria. This is one of the errors Parsons' symposium was designed to correct. As she wrote in her preface: "Commonly the interesting aspects are those which differ markedly from our own culture or those in which we see relations to other foreign cultures we have studied." A recent Chinese visitor to Zuñi offers the same comment: he finds that Benedict's differentiae are correct but that their emphasis throws the picture out of focus.<sup>35</sup> Yet if anything should give a true perspective it is the pattern that animates the whole.

Yet Benedict's description of Zuñi has found favor with the best judges. How is the contradiction to be

<sup>35</sup> "Li An-che, Zuñi: Some Observations and Queries," AA 39:62-76, 1937.

reconciled? The point is that Li An-che's stricture touches not the picture as given but Benedict's epitome thereof. She resembles a physicist seeking enlightenment about electricity not from the empirical relations of electrical phenomena but from a concise dictionary definition. Borrowing, for example, a Nietzschean antithesis, Benedict defines the Pueblos as "Apollonians" who institutionalize "sobriety and restraint in behavior," while the "Dionysians" of the Plains favor "abandon and emotional excesses." Hence, the contrast of ritual formalism with personal shamanism; of subdued and frenzied mourning ceremonial; of diffident hiding from public notice and boastful competitiveness.

Now this is, indeed, to oversimplify a real antithesis. We have already repudiated the notion that the Eastern Indian's vision conforms to the pattern of orgiastic excess. Going further, we find in much of Plains religion a ritual formalism that attains Apollonian degree. The opening of a Blackfoot sacred bundle has no element of frenzy, let alone orgy. The celebrants assume positions not to be altered before there is ritual dispensation. They burn incense, shake rattles, and sing interminable chants. Even the mildly dramatic imitation of buffalo is highly stereotyped. The performance is as solemn and sober as imaginable.<sup>36</sup>

Our judgment, then, culminates in qualified approval. There are cultural leitmotifs, and their scope should be ascertained, regionally and within particular cultures. But an adequate definition of these patterns is as yet a thing for future research.

#### THE OUTLOOK

Being a science, ethnology implies an orderly arrangement of its data, the verifiability of its findings, a

<sup>36</sup> Clark Wissler, "Ceremonial Bundles of the Blackfoot Indians," AMNH-P 7:204 sq., 1912.

logical basis for its conclusions. But in conforming to the canons of all science, it must not adopt the particular techniques of physics, biology, or geology except where the cultural data as such demand such recourse.

Scientific procedures are not gadgets preconceived and thrust into reality in the hope of a catch. They evolve and are applied spontaneously as problems arise in the mind of a thinker saturated with his theme; and then no arbitrary boundary will stop his sally into the unknown. When Laufer concentrates on porcelain, it is not in the spirit of sinological antiquarianism. "Porcelain" is conceptually defined as a pottery type with distinctive glaze. Its history, then, is that of Chinese glaze and earthenware. The differentia can be studied only by the aid of chemistry, physics, mineralogy, and the history of the Near Orient; and it is thus that its rise is determined. Curiosity is not yet exhausted, and as a by-product we get a dissertation on the potter's wheel and its sociological correlates. No one could have foreseen the questions opened by the incipient inquiry nor the techniques required for answering them.<sup>37</sup>

The student of culture, then, will be unable to predict what he may need, let alone what his colleagues may require for their purposes; but his inescapable duty is to define his concepts and arrange them in a spatial, temporal, and causal context.

Concepts must be clear and rigid, rising above the fluidity and vagueness of the raw phenomena. Here lies a major difficulty for all thinking. "Bronze" is copper deliberately alloyed with tin to enhance the hardness of the artifact. How sure, however, can we be in a given case that the percentage of tin is not due to chance? "Tanning" is a process involving chemical as well as mechanical changes in the skin. But what if excessive fat

<sup>37</sup> Berthold Laufer, "The Beginning of Porcelain in China," *FMNA-PAS* 15:79, 183, Chicago, 1917.

stimulates an unplanned chemical action? And, if so, who can be sure that the natives fail to recognize the effect of this factor? A "moiety" is etymologically one of complementary halves; most commonly, one of the two exogamous clans of a tribe, but the term has been extended, e.g., to two complementary ceremonial groups not affecting marriage. How can the concept do service among the Timbira of Brazil, who bisect all the tribe on two distinct principles and its male constituency on still other lines? Again, in the Southeastern United States one tribe has exogamous halves, while its neighbor's moieties do not regulate marriage, yet otherwise share the same functions. The "moiety" label does not matter, but the associated concept is all-important. For an exogamous moiety is a species of the genus "clan." We may well ask, then, whether a coexisting moiety and clan system are genetically related. But to link nonexogamous with exogamous moieties is warranted only if extraneous considerations suggest the connection; otherwise the common name deceives us into assuming real unity.

The clarification of concepts, then, directly gauges scientific progress. Morgan's tilt against MacLennan brought into relief the nonlocal character of the clan as then known. Later, in California, E. W. Gifford and Wm. D. Strong discovered landowning and politically autonomous groups reckoning descent through the father and imposing exogamy. If the emphasis is put on unilateral descent and exogamy, these "lineages" are clans; yet evidently such independent units differ from their non-localized Iroquois counterparts. Along lines already anticipated by MacLennan, Gifford and Strong explained the ordinary form of clan by the fusion of lineages. In Australia Radcliffe-Brown found similar patrilinear "hordes" coexisting with a clan system of the usual type. Steward, recognizing the affinity of Ona bands with Californian lineages and Australian hordes, propounds the

question of a common underlying antecedent. He visualizes these "bands" as one subtype of the general "unilateral" category, the "clan" representing another.<sup>38</sup> Goldenweiser injected another point: An Iroquois clan includes both actual and assumed descendants of one ancestress through females; and ethnologists had been wont to speak of inheritance following clan lines. With nice discrimination Goldenweiser showed that the Iroquois distinguished the actual kin within a clan, privileges being primarily transmitted from blood relative to blood relative. In other words, the Iroquois "clan" comprises several matrilineal groups of true kindred, and these are the rightful claimants to title and office. This notable contribution to our stock of sociological ideas has proved applicable to Pueblo society, where the same distinction is vital.<sup>39</sup>

But this once more raises a problem. Goldenweiser's "maternal families" are the matrilineal equivalent of Steward's patrilineal bands inasmuch as both are exogamous units reckoning descent unilaterally. But whether on this basis they are best classed together as "lineages" or are to be distinguished by virtue of autonomy is a debatable question. Thus, new knowledge offers an ever-recurrent challenge to the ethnologist's acumen. His data, complex and oscillating, constantly menace received categories. Precisely for that reason it is one of his noblest tasks to bring order into this hodgepodge, barring sham problems and smoothing the path for real ones.

<sup>38</sup> E. W. Gifford, "Miwok Lineages and the Political Unit in Aboriginal California," AA 22:389-401, 1926. W. D. Strong, "An Analysis of Southeastern Society," AA 29:1-61, 1927. A. R. Radcliffe-Brown, "The Social Organization of Australian Tribes," 29. J. H. Steward, "The Economic and Social Basis of Primitive Bands," ALK, 331-347.

<sup>39</sup> A. A. Goldenweiser, *On Iroquois Work; Summary Report of the Geological Survey*, Ottawa, Canada, 464-475, 1912; 365-373, 1913. Elsie Clews Parsons, *Hopi Journal of Alexander M. Stephens*, 1:xxxiv, New York, 1936.

Apt concepts, then, are a primary goal. Without them even a simple charting of distributions is invalidated. But from another angle the spatial extent of a phenomenon defines the concept itself. Our notion of the Trobriand Islanders differs from our notion of the Liliuputians or the Yahoos because the first are at least implicitly thought of as rooted in a definite part of the globe; and what is true of whole peoples applies to the single items of their social existence. What any one of them signifies in the total course of human history depends on where it occurs and does not occur. Bronze, stone masonry, a position system of arithmetical notation—all these would denote utterly different values if typical of the Andamanese, Fuegians, and Tasmanians.

And as distribution influences appraisal, so it leads directly to questions of causality. In California, the Paiute of Owens Lake prize the pinenut as their staple food, while their brethren of the Oregon boundary line consider it of minor importance. Why this difference between subdivisions of a tribe? Because in the north *Pinus monophylla* is very nearly lacking.<sup>40</sup> Is the explanation trivial? Well, for a philosophical inquirer it involves an attack on the riddles of the universe. Here are two groups linguistically so close that they separated hardly more than several centuries ago. To compare them is thus to measure the rate of cultural differentiation. Further, according to a popular theory, economic factors primarily determine the whole of social life. Here, then, we can directly test the effects of a shift from or to reliance on the pinenut.

Knowledge of distribution regularly merges in historical interpretations. The American Indians are essentially a single race that gradually came to occupy all

<sup>40</sup> J. H. Steward, "Ethnography of the Owens Valley Paiute," UC 33: 241, 1933. I. T. Kelly, "Ethnography of the Surprise Valley Paiute," UC 31:99, 1932.

of the New World. From British Columbia to Cape Horn countless modern tribes prescribe sticks as head-scratchers in ceremonial situations varying in detail but without overthrowing the basic unity of the concept. It is, then, no perverse historical mania but a normal impulse to give temporal meaning to the distribution. Is it due to recent propagandist fervor? That would be psychologically curious since the reasons for such spread are not at all obvious. Or are we—more probably—dealing with an extremely ancient trait that has persisted in many places? If so, here is evidence of the incredible tenacity with which inconspicuous elements may be retained.

We have seen that scholars differ widely in their chronological tastes. Some are content with vague indications; others insist on precise time fixation; still others profess reliance on only written records, or even disdain history on principle. But even the extremists smuggle a chronology into their systems for the simple reason that the time category is inescapable, hence the need for making our chronology as accurate as we can. There is a further reason. Conceptual, spatial, temporal, and causal aspects of culture are not so many distinct realities; insight into any one of them enhances our comprehension of the rest. Here, if anywhere, the functionalist point of view should be applied.

The point is important enough to warrant ample illustration. Let us consider, then, how our understanding progresses with our knowledge of space and time relations. To Ratzel the plate armor of Bering Sea natives was a copy in bone or hide of Japanese metal laminae, first, because he ignored the wide occurrence of plate armor in Asia; secondly, because lacking faith in the originality of primitive peoples he barred them from a creative part. Laufer corrected the distributional statement: plate armor occurs in China, western Asia,



Turkestan, and ancient Siberia, hence even on Ratzel's psychological doctrine Japan need no longer figure as the focus. Further, there is written history which Ratzel neglects. Chinese annals prove that the maritime Sushen wore bone plate armor in 262 A.D.—from six to eight centuries earlier than the hypothetical Japanese prototype, which is thus eliminated. Let us further drop the psychological dogma, and the relation of the cruder armor to that of *any* higher civilization appears in a new light. The primitive peoples may have borrowed greater regularity for their laminae, but such imitation does not preclude the independent invention of armor as such.<sup>41</sup> Note here the functional query that entered by a side door: In what technological setting can armor be invented? But corresponding issues go with any chronological determination. Conspicuous in Pueblo ritual is the masquerading in its rain cult, which a naïve observer accepts as an integral part of native religion. Parsons, starting from other Spanish influences on Pueblo life, at first traced the masks to the white intruders. Forthwith the concept of aboriginal Pueblo ceremonial is modified. But another metamorphosis ensues: an archeological site, dating back to the fourteenth or thirteenth century, harbors an unmistakable drawing of a Pueblo mask, evidence supported by Sahagun's account of an Aztec rain priest's costume. The pre-Columbian antiquity of the masks is re-established; only their efflorescence, not their origin, can be derived from the invaders.<sup>42</sup> But if certain traits *have* cohered at any time, they are at least not mutually exclusive. Thus, our functional advances with our historical insight.

Again, Beals' ransacking of early chronicles proves

<sup>41</sup> B. Laufer, "Chinese Clay Figures," FMNH-PAS 13:258 sq., Chicago, 1914.

<sup>42</sup> E. C. Parsons, "Some Aztec and Pueblo Parallels," AA 35:611 sq., 1933. E. C. Parsons and R. L. Beals, "The Sacred Clowns of the Pueblo and Mayo-Yaqui Indians," AA 36:510, 1934.

Tamaulipas an enclave of farmers surrounded by Mexican nomads, but of farmers lacking Aztec traits beyond earthenware and agriculture. The archeological evidence Beals cites, however, including temple mounds, stone structures, and metallurgy, leaves no doubt of southern affinities at an earlier period. Similarly, in the Plains Strong and Wedel find a succession of cultures, very early hunters receding before horticulturists, who are superseded by post-Columbian equestrian hunters.<sup>43</sup> These temporal data have an infinitely deeper significance than appears on the surface. What are the conditions that lead to decadence? What factors specifically determine relapse into nomadism? And what is the functional significance of the horse in the Plains? Is it perchance comparable to that of the snowshoe in subarctic regions?

Every scheme of developments in the New World bears on vital matters of theory that go beyond mere dating. According to Perry and Elliot Smith, until several centuries ago the Indians north of the Rio Grande lived in more or less simian fashion, only acquiring their customs and beliefs along with maize from Southern Mexico. Here we have the psychological dogma of human uninventiveness; the functional dogma that practically no culture is possible without farming; the diffusionist dogma that traits are dispersed in large blocks from a single center; and by implication a second psychological dogma that maize, once injected into the life of a people hitherto ape-like, produces a frenzied burst of creativeness by which they forthwith grow the plant in completely novel ways and originate usages and beliefs

<sup>43</sup> R. L. Beals, "The Comparative Ethnology of Northern Mexico before 1750," *Ibero-Americana*, 2:136, 143, 149, Berkeley, 1932. W. D. Strong, "The Plains Culture Area in the Light of Archaeology," *AA* 35: 271-287, 1933. *Idem*, "An Introduction to Nebraska Archaeology," *Smithsonian Miscellaneous Collections*, 93: No. 10, Washington, 1935. W. R. Wedel, "An Introduction to Pawnee Archaeology," *BAE-B* 112:94-102, Washington, 1936.

never known in Mexico. To single out one phase of the scheme, the simple archeological determination of pottery and farming in southern Arizona about the beginning of our era explodes the incredible rapidity demanded by the theory. Substituting Kidder's minimum dates of 400 B.C. for the crystallization of Maya civilization, and 1000 B.C. for incipient American agriculture, we arrive at a quite different and more accurate notion of the process of diffusion.

Again, there are the stone tools of Folsom, New Mexico. Even as recent artifacts they would arouse interest, but Penck's estimate of an antiquity of 9,000 years establishes primitive hunting as a concomitant of extraordinary stonework. If, on the other hand, the age were geologically proved several times as great, the superiority of the settlers over their Eurasiatic contemporaries would be of interest.

Even intelligent description is inseparable from a time perspective. As Sapir convincingly pointed out, the several culture areas into which Americanists divide their natives are by no means classificatory equivalents. The Plains, e.g., differ far less from the Eastern Woodlands than either region differs from British Columbia. And a proper weighting of these units implies a relative chronology. There can, indeed, be few more fundamental questions than those concerned with the factors making respectively for stability and for modification; and without a time scale this matter remains inconclusive. Settlement of the New World implies, geographically, adaptation to at least six distinct zones. According to Penck, even 25,000 years would be inadequate for the successive acclimatizations consequent on migrating from Alaska to Tierra del Fuego. This may be sheer conjecture, but any definitive confirmation or qualification would teach us how rapidly a culture can be created. Verily, the time factor is not a thing adventitiously

linked with culture, but is always present and defines the very essence of our phenomena.<sup>44</sup>

We have repeatedly mentioned correlations. They are, in our opinion, the closest approximation ethnology is likely to achieve to the ideals of exact science. Some scholars, indeed, postulate "laws," feeling that otherwise ethnology would lapse to an inferior status. This is aping ideals applicable only to a part of physics and in no way incumbent on other branches of learning. Every science formulates its data according to their nature and may even differ in its procedure and results from problem to problem. Biology uses mathematics sparingly and for limited purposes; astronomers predict eclipses but are unable to calculate the gravitational pull exerted by all bodies on all others. The ethnologist, correspondingly, may never discover laws; yet his scientific respectability remains unimpaired so long as he co-ordinates with a maximum of attainable efficiency, the particular phenomena he studies.

The reality of correlations on the pattern of Tylor's adhesions is unshaken. However, we must beware of sham regularities. The proper method is to note empirical associations that force themselves on our attention and then test whether they are random or organic. Lesser has admirably set forth and exemplified the logic of the procedure: "Interrelation in an area cannot establish causal connection by itself, but if this interrelation occurs elsewhere independently, it becomes probable that a functional relation is present which can be used as a working hypothesis." The most cursory inspection of our literature shows that ethnologists are forever postulating functional relations without vouchsafing any

<sup>44</sup> Edward Sapir, "Time Perspective in Aboriginal American Culture: A study in Method," *Canada Geological Survey*, Memoir 90:45, Ottawa, 1916. Albrecht Penck, *Wann kamen die Indianer nach Nordamerika*, ICA 23:25, 28 f., New York, 1930. A. V. Kidder, "Speculations on New World Prehistory," ALK, 143-151.

reason for their conjectures. Progress will consist largely in substituting demonstrated or demonstrable correlations for idle allegations.<sup>45</sup>

Given the complexity of social phenomena, we certainly cannot as a rule expect a simple causal relation: any established correlation must be treated as overlapping others; or, to put it differently, our correlates do not fully determine one another, the presence of some simply renders the others *more* probable than would otherwise be the case. That is the simple meaning of the much misunderstood relationship between matrilineal descent and certain kinship classifications; the latter are to be expected rather with than without matrilineal clans; no one asserts that the rule of descent automatically evokes the classification.

Such correlations justify a greatly tempered parallelism or neo-evolutionism. If four or five features are organically linked, it is probable and sometimes certain that they have not always synchronized. That is, one of them may be not merely an equivalent concomitant but a significant antecedent. If so, its presence anywhere favors the appearance of the same consequents. Thus, Steward finds a common ecological cause underlying patrilineal bands in the four quarters of the globe. Definite decorative patterns recur with definite basketry techniques, and certainly the angularity of life forms in woven fabrics is not the cause but the effect of weaving. Among the Pueblos, woman's importance in ritual varies directly with the absoluteness of feminine house ownership and the strength of the clan. Parsons' analysis indicates not merely a functional tie but an irreversible sequence: women as house owners; matrilineal residence; matrilineal lineages; matrilineal clans. The causal role of matrilineal residence agrees with Tylor's

<sup>45</sup> Alexander Lesser, "Kinship Origins in the Light of Our Distributions," AA 31:716, 1929.

earlier ideas and might well be checked in all comparable regions.<sup>46</sup>

Naturally, correlations can be established only if the several regions concerned are historically independent of one another. In other words, their determination is inconsistent with an extreme diffusionism. But the messianic ideas precipitated by European contacts among North American, South American, and African natives clearly prove that similar notions can be evoked by similar antecedents where there is demonstrably no diffusion. In principle, then, the possibility of independent repetition is vindicated.

What, then, is the prospect of ethnology? Its hope lies in maintaining the universalist and the objective approach of Tylor and Boas: whatever preferences the individual worker may gratify, our science as a whole can neglect no aspect of social life as intrinsically inferior to the rest. Specifically, material objects must be studied as embodiments of their makers' craftsmanship, aesthetic taste or spiritual aspirations. Subjective attitudes and personality must also be investigated as social symbols; what is obscure must be made clear, and results can be reckoned scientifically valid only if verifiable by subsequent investigators. This topical breadth will be matched by the massiveness of the regional approach, which must include all past and present cultures from the highest to the lowest. Breasted's Egyptian researches and Laufer's ransacking of the sinologue's treasure-trove are an earnest of what may be expected from a thorough examination of written sources by scholars steeped in the ethnologist's point of view. This regional and topical universalism implies an ever-widening and deepening culture history which will provide matter for an increasing number of significant cor-

<sup>46</sup> J. H. Steward, *op. cit.* E. C. Parson, "The House-Clan Complex of the Pueblos," *ALK*, 229-231, 1936.

relations in Tylor's fashion, an ever-active epistemological critique of concepts in the spirit of Boas.

The discipline we have described is bound to rank co-ordinate with other sciences insofar as it continues to investigate objectively and intensively that segment of reality which falls to its province. Spatially it will arrange its data after the fashion of geography; chronologically, it will use—according to particular exigencies—the logic and techniques of geology, historical astronomy, political history; causally it will establish an indefinite number of valid correlations, thereby attaining the degree of generalization compatible with its own section of the universe.





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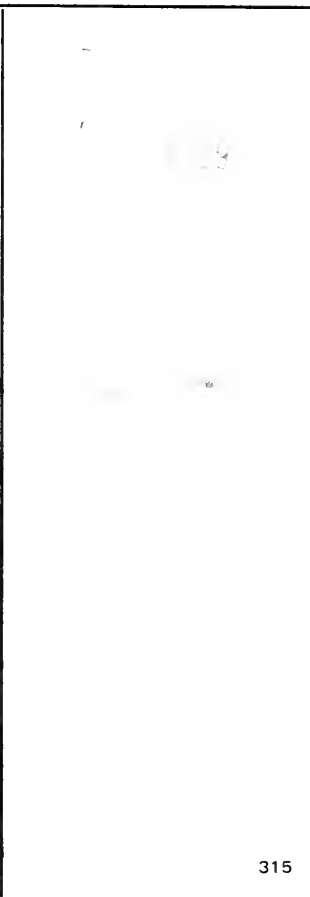




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