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AMERICAN CONTRIBUTIONS TO ANTHROPOLOGY[1]

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AMERICAN anthropology has been in large measure shaped by its opportunities, which implied duties. The New World presented an apparently distinct variety of the human species with great physical, linguistic and ecological diversity. After their discovery these indigenes were threatened at times with extinction or miscegenation, everywhere with a possible obliteration of their mode of life. The obvious task was "to save vanishing data." As a result sheer collection or description bulks large in American anthropology. One thinks of the impressive series of annual reports and bulletins issued since 1879 by the Bureau of American Ethnology; the vast collections of crania and skeletons amassed by the late Dr. A. Hrdlička in the U. S. National Museum; the intensive reports on Californian tribes due to A. L. Kroeber and his disciples.

[1] Address of the retiring vice-president and chairman of the Section on Anthropology, American Association for the Advancement of Science, Cleveland, September, 1944.

Of course, Americans have not shied away from other areas on principle. Honolulu, with its Bishop Museum, has been a natural spring-board for Oceanian investigations, and other institutions have now and then financed transoceanic research. A. B. Lewis, Margaret Mead, Hortense Powdermaker have studied Papuans and Melanesians; Martha Beckwith, H. L. Shapiro, E. W. Gifford, Ralph Linton are associated with various Polynesian projects; Wm. Lloyd Warner and D. S. Davidson have, respectively, investigated Australian sociology and technology; Raymond Kennedy, E. M. Loeb, Cora DuBois are specializing in Indonesia; and George Herzog, Melville J. Herskovits, Wm. R. Bascom are reckoned Africanists. However, in the nature of the case most of us have remained predominantly Americanists.

Our saturation with the concrete data of the New World has given a distinct flavor to American work during the last half century. To outsiders we have

often appeared, plausibly enough, as intelligent—and not always over-intelligent—collectors of raw data. In requital we have tended to look with the empiricist's scorn upon European theories as the lucubrations of closet anthropologists. Probably most of us would concede nowadays that both attitudes are one-sided.

In my remarks to-day I shall endeavor to sketch the nature of American work in the main branches of our science, though it is obviously only on a small segment of anthropology that I can speak with anything like authority.

Physical Anthropology

On this subject more particularly I speak as an interested spectator rather than as in any sense an expert. Our compatriots seem to me to have contributed in three diverse ways. On the one hand, they have described our aborigines, fixing their status within the human species. Secondly, but for obvious reasons less frequently, they have examined other races and synthesized relevant findings. Finally, they have grappled with problems of what may broadly be termed "human biology."

Samuel G. Morton (1799–1851), who has been called the first physical anthropologist of America, collected 968 Indian skulls, on whose study rests his amply illustrated work, "Crania Americana" (1839). He postulated the unity of all American Indians, including the Mound Builders, but gave to the Eskimo a distinct position. From dearth of Asiatic material he failed to see the connection between the American natives and the Old World Mongoloids, but later investigators—to mention only Hrdlička and Boas—amply demonstrated the derivation of our Indians from eastern Asia. Of subsequent treatises I need mention only Dr. Bruno Oetteking's "Craniology of the North Pacific Coast" with its "splendid mastery of anthropometric technique" and "technical perfection."[2]

Under the second head we may note that as early as 1844 Morton had extended his survey to the ancient Egyptians, whom along with their neighbors and descendants he rightly classified as Caucasian. In later periods the desire to place the American race within the species automatically led to comparison with Asiatic types, as already suggested in my reference to Boas and Hrdlička. Roland B. Dixon's "The Racial History of Man" (1923), however liable to criticism in its basic assumptions, had the unquestionable merit of envisaging humanity as a whole. The more restricted, yet sufficiently comprehensive synthesis of Wm. Z. Ripley, "The Races of Europe" (1899), and its more recent namesake by C. S. Coon (1939) naturally come to mind as dispelling the charge of provincialism. Nor should we forget Coon's field work in Albania and Arabia; E. A. Hooton's monograph on "The Ancient Inhabitants of the Canary Islands" (1925); and H. L. Shapiro's investigations in Polynesia.

Falling simultaneously under the head of research in extra-American physical anthropology and in human biology may be reckoned studies that bear on evolutionary problems. Jeffries Wyman is credited with the first accurate osteological description of the gorilla (1860), and in recent years Adolph H. Schultz has gained eminence by his studies of primate ontogeny. Naturally all writers on human evolution are bound to grapple with the interpretation of fossil finds, but my colleague, Dr. Theodore D. McCown, himself unearthed a series of finds in Palestine which shed light on basic evolutionary processes.

In a third category I should place investigations bearing on such fundamental phenomena as growth and heredity. Since Henry P. Bowditch's pioneer efforts (1877) a considerable number of American students have concentrated on the factors of growth, including such environmental conditions as economic status. (For bibliography, see Franz Boas, "Race, Language and Culture," 1940, pp. 49–52.) Boas was especially concerned with the correlation of descriptively diverse physiological phenomena—such as that between early menstruation and early eruption of the molar teeth.

As to problems of heredity, Boas's paper on "The Half-Blood Indian"[3] in a sense paralleled Von Luschan's observations on segregation in Asia Minor and, of course, considerably antedated the rediscovery of Mendelism. Boas's much misunderstood "Changes in Bodily Form of Descendants of Immigrants" (1912) should be regarded as a study of modifications rather than as an attack on current views concerning the germ-plasm. H. L. Shapiro's "Migration and Environment" (1939) is devoted to a similar theme, culminating in the proposition that "man emerges as a dynamic organism which under certain circumstances is capable of very substantial changes within a single generation."

Speaking under correction, I register my impression that, over and above the obvious descriptive tasks, our physical anthropologists have shed light on significant biological problems related to man.

Prehistory

Though our archeologists as a group may seem especially liable to the charge of narrowness, we should not forget the veteran George Grant MacCurdy's

[2] See E. A. Hooton, in *American Anthropologist*, 33: 444*f*., 1931.

[3] *Popular Science Monthly*, October, 1894.

persistent interest in Old World prehistory, culminating in his fine two-volume work on "Human Origins" (1924), nor N. C. Nelson's research in Mongolia and Th. D. McCown's in Palestine.

Archeological provincialism is easily understood: exacting preliminary work and sound local interpretation had to precede syntheses of wider import. As knowledge grew, basic chronological questions came to the fore. What was the actual sequence of events in the various major regions of the New World? Could the more ancient finds be considered synchronous with corresponding European specimens? When did man first set foot on American soil? In part such questions affect a general view of human culture history, so that quite naturally European scholars have shared our concern with the results.

For a long time Wm. H. Holmes (1846–1933) loomed as the grand old man among American archeologists. He rendered excellent service in dispelling the fantasies of amateurs and over-enthusiastic professionals prone to correlate crude samples of stonework in this country with the transatlantic Chellean. Holmes's studies "demonstrated that all these chipped stones labelled Paleoliths were only the rejects of the native implement makers thrown away on the workshops because of flaws in the stone or shapes not suitable for making finished implements."[4] Though opinions are still divided as to whether man reached America in the Pleistocene, Holmes's insistence on geological evidence for great antiquity was a healthy corrective to earlier speculation, and probably no reputable investigator would nowadays suggest that American man was contemporary with the Chellean or Acheulean of Europe.[5]

In apparent refutation of Holmes's skepticism, cases of Pleistocene fauna associated with artifacts have been established, but the query remains whether individuals of Pleistocene species can not survive long into the Recent period. On the other hand, in Patagonia Junius Bird[6] has not merely demonstrated tools alongside of skeletal remains of extinct sloths and horses, but also indicated an antiquity of from 3,000 to 5,400 years on geological grounds. Since Indians could hardly have traversed the distance between Bering Strait and Tierra del Fuego, with its variation of geographical zones, in the twinkling of an eye, a respectable historical antiquity for human occupancy of America seems vindicated. Precisely how to assess the time span required is still a matter for debate. Various students have grappled with the problems bound up with the Folsom finds; and F. H. H. Roberts, Jr., has periodically summarized the archeological, paleontological and climatological evidence bearing on the antiquity of man in the New World.[7]

If I understand the course of development, our prehistorians have made great strides in the matter of technique during the last generation or so. Whereas much of the earlier work was dictated by esthetic motives, A. V. Kidder and N. C. Nelson ushered in the severely stratigraphical approach typical of geology in their investigations of the Southwest; and H. B. Collins, Jr., has similarly investigated the sequence of Eskimo cultures. In the Plains Wm. D. Strong has demonstrated the value of combining a stratigraphic technique with a historical approach, applying the sound principle of working from the known backward to the unknown.

As for the higher civilizations, their study has profited both from the perfection of technique and the widening of horizons. "The Maya and their Neighbors" (1940), the Festschrift in honor of A. M. Tozzer, indicates that the disciples and collaborators stimulated by him have progressed to a pan-American conception of Central American prehistory; and Wm. D. Strong's recent survey of Andean research suggests that our Peruvianists, too, view their problems in hemispherical terms.[8]

LINGUISTICS

The amazing diversity of speech in the Western Hemisphere offered a rich field for exploration. Accordingly, scholars soon began to describe and classify native languages. Albert Gallatin (1761–1849), diplomat, Secretary of the Treasury and founder of the American Ethnological Society (1842), was a noteworthy pioneer. Subsequently John W. Powell (1834–1902) published the classification (1891) that was to remain the starting-point of all later efforts.

Thanks to Boas's influence, many workers primarily concerned with ethnography were trained to record native texts, the result being a vast mass of material for philological analysis. Naturally, they were not always up to the standards nowadays exacted by the professional linguist, but Boas also lured into our fold such Indo-Europeanists as Edward Sapir and Truman Michelson, and established a *rapport* with other specialists in language, such as Leonard Bloomfield. In consequence, we can now claim a fair number of "anthropological linguists"—men who have undergone the severe discipline of traditional comparative language training, but who work mainly or largely with aboriginal tongues.

Consummate craftsmanship, linked with an anthro-

[4] Walter Hough, in *American Anthropology*, 35: 753, 1933.

[5] Holmes, "Handbook of Aboriginal American Antiquities," I. Washington, 1919.

[6] *Geographical Review*, 28: 250–275, 1938.

[7] *E.g.*, "Evidence for a Paleo-Indian in the New World," *Acta Americana*, 1: 171–201, 1943.

[8] "Cross Sections of New World Prehistory," 1943.

pological perspective, has yielded excellent results, which are by no means limited to the Americanists' domain. Boas himself passed far beyond its bounds, Sapir constructively grappled with African and Asiatic stocks, and Emeneau's field research was devoted to the Dravidian family.

On the whole, our linguists have manifested greater audacity than is commonly associated with American anthropologists. Dixon, Kroeber and Sapir all attempted to supersede the conservative Powell classification. Sapir adumbrated possible connections between North and Central American stocks, even between Canadian and East Asiatic languages—not to mention his ambitious (though in my humble opinion sterile) global classification of speech into four types.

In the matter of genetic relations Powell and Boas were conservatives, but this does not mean that they lacked interpretative aspirations. They were merely concerned with other questions, such as the processes of mutation and the psychology of speech. Boas, more particularly, intent on seeing each language in the light of its distinctive genius, was forever defining the categories of thought which each of them reflected.

By and large, our linguists have probably been more conspicuous in displaying a fruitful combination of empirical knowledge, technical expertness, breadth of view and insight than any other group of American anthropologists.

Cultural Anthropology

The history of cultural anthropology in the United States presents several main phases. Among these the period of fantastic theories can not be ignored, for they were broached by reputable writers. Catlin cited bull-boats as evidence of a Welsh colony on the upper Missouri, and others pointed to menstrual taboos to prove the descent of our Indians from the Ten Lost Tribes of Israel. Subsequent restraint in historical hypotheses may in part be explained as a justifiable revulsion against such weird fancies.

In quite a different category, of course, belong the schemes of unilinear development of which Lewis H. Morgan's "Ancient Society" (1877) is the outstanding example. Grandiose in comprehensiveness, it rested in part on scientific facts and has been aptly compared to the biological theories of phylogeny that blossomed forth soon after the appearance of "The Origin of Species." As field work in particular regions failed to support Morgan's generalizations, Americanists rebelled against the system and rejected it on principle—much as the experimental biologists and geneticists spurned Haeckel's phylogenetic speculations. Sometimes—as in the case of the Crow clan organization—they even doubted Morgan's findings where later research established their correctness.

What followed was, for one thing, the type of intensive regional surveys linked with the names of, say, Boas, Wissler, Kroeber, Swanton. It is clear that in the nature of the case the resulting historical reconstructions could not vie in impressiveness with Morgan's laws of social evolution. Any one who craved shorthand formulae for cultural sequences was bound to view even the bolder efforts of later times as pedestrian alongside of "Ancient Society." In much the same way a fervid Haeckelian would not be content with the contempt for genealogical tables displayed by Jacques Loeb and Thomas Hunt Morgan. Yet it would be preposterous to deny that these scientists were pathfinders as well as critics and that their positive work is the more significant. The Boasian phase of our science should be regarded in similar fashion. Its contribution—which can not, of course, be attributed solely to my revered teacher—lay in a quite different direction from that of the unilinear evolutionists, but that does not mean that its champions were inferior in originality and mental grasp.

Two concrete examples may illustrate my meaning. When Boas began the study of primitive art, the regnant theory of decorative designs, typically championed by H. Stolpe, conceived all such motifs as conventionalized representations of living beings. Boas's refutal of the universal validity of the explanation doubtless struck some ethnologists as an example of his negativism. He had eliminated a view that, admitting all reservations, had accounted for a good deal and had failed to put anything in its place. To me this is as though a zoologist blamed Thomas Hunt Morgan for not supplying an *Ersatz* for Haeckel's phylogenetic scheme. As T. H. Morgan's work on heredity was in a distinct and new direction, so Boas's contribution was incommensurable with that of his predecessors. He demonstrated that savages read meaning into meaningless patterns, that the primitive artist, too, copes with an esthetic tradition, that the urges of a virtuoso may lead to artistic results. He disappoints the seeker of a formula for sequences, but inspires those capable of enjoying new vistas.

For my second illustration I will choose Kroeber's paper on "Classificatory Systems of Relationship."[9] Challenging Lewis H. Morgan, it comes close to denying any correlation between kinship terminologies and sociological usages. I dissent strongly from part of the argument, and Kroeber himself no longer holds his views of thirty-five years ago in unmodified form. But the merit of the paper does not lie in its critique of Morgan; it lies in shifting consideration

[9] *Jour. Royal Anthrop. Inst.*, 39: 77–84, 1909.

to aspects of nomenclature which neither Morgan nor his followers took into account and in suggesting a mode of approach that, although tentative in its concrete form, has proved exceedingly helpful in subsequent research.

To think of the Boasian phase, then, in terms of pedantic soundness devoid of inspiration is radically wrong-headed.

The new insights spring in the main directly from wider and deeper knowledge of fact. In this respect we in the United States enjoyed singular good fortune. It was far easier to go from New York to Montana than from Cambridge University to the Torres Straits, or from Berlin to the Matto Grosso. For us it was possible to return again and again to particular tribes, checking and rechecking results, sometimes having them reexamined by a new investigator. Not only did museum specimens readily accumulate, but aboriginal craftsmen were still extant to illustrate their manufacture. The language handicap proved less onerous for us because some Indians had already learned to speak English; in a few instances they were academically, and could be anthropologically, trained. The inwardness of native life could thus be interpreted for us authentically by "marginal men," of whom William Jones remains a shining example. Even where that was not possible, the conditions of intercourse with natives facilitated intimacy of contact. The respectably long series of biographical sketches ushered in by "The Autobiography of a Winnebago Indian" (1920), edited by Paul Radin, may be cited as the sort of material obtained.

Not that such documents surpassed in authenticity Knud Rasmussen's books on the Eskimo or the reminiscences of the Lapp, Johan Turi. But because of our favorable situation they became more abundant in the United States than elsewhere. In this connection we may recall Malinowski's clarion call in "Argonauts of the Western Pacific" (1922), summoning anthropologists to study "the realities of human life," "the imponderabilia of actual life." *More suo,* his demeanor was that of a prophet preaching in the wilderness, but a reader of Rivers's "The Todas" will understand why the manifesto struck a responsive chord in Britain. Now, in the very same year appeared a symposium on "American Indian Life," and in her editorial preface Elsie Clews Parsons expressed the credo of her twenty-odd collaborators in much the same terms as Malinowski. Through our good fortune, learning about the true inwardness of native thought was no longer with us the goal of a solitary messiah, but the aspiration of the rank and file.

This statement is made in anything but a spirit of jingoistic braggadocio. For one thing, there is a gap between aspiration and fruition. As a matter of fact, I am extremely ambivalent about the later developments of the phase I am describing. Thanks to our opportunities and the organization of anthropological training, we have collected a vast body of sound information, perfected techniques, and produced many competent craftsmen. Unfortunately, however, the last decades also coincide with a deterioration of general education in the United States that elsewhere would hardly be believed. In consequence, we have graduating seniors who do not know the meaning of common words, and Ph.D.'s who write in a style unworthy of a high-school pupil. I can not wax ecstatic over science that is practised as a trade. Mere competence in the handling of field techniques may make a good ethnographic craftsman, but not a good cultural anthropologist.

There is still another, a contemporary, phase to be noticed. A goodly number of our younger colleagues concern themselves primarily with personality in its relations to culture, which they approach by a fusion of ethnographical with either psychoanalytic or sociological outlooks. My reaction to these movements is not at all negativistic. When "youth is knocking at the gate," I am not, indeed, prepared to follow Hilda Wangel's advice in Ibsen's "The Master Builder" and throw the door wide open, but on the other hand I have no wish to slam it in their faces.

As for the psychoanalytic approach, an obvious merit lies in its stressing aspects of native life that had hitherto received inadequate recognition. This is not a slur on the often admirable observations of previous investigators. But precisely as Professor L. M. O'Neale, being both an anthropologist and an expert on textiles, sees significance in fabrics that eludes the ordinary ethnographer, so a clinically trained colleague is bound to note much that would otherwise remain unrecorded or misunderstood. From this point of view Gregory Bateson and Margaret Mead's "Balinese Character; a Photographic Analysis" (1942) must be heartily welcomed. What disturbs me is the conceptual framework within which they and others in comparable studies present their material. I can not gauge how far the authors reflect the generally recognized findings of modern psychiatry, how far merely the shibboleths of an enthusiastic sect whose creed may some time rank as sound doctrine, but can not yet be regarded as established. Not in a spirit of captiousness, but as a layman seeking enlightenment I should like to know, for instance, whether it is orthodox psychology to believe that "fear of many sorts becomes a pleasant emotion . . ." (*op. cit.,* 147). Again, I can not without further instruction understand how any amount of watching could demonstrate a sex difference stressed by the same writ-

ers (p. 24, *f.*): What manner of sense impressions can convey the legitimate inference that a man handles a fighting-cock "as an extension of his own body," whereas a woman handles her child "as something separate"? I deny nothing, but I want Adolf Meyer, Thorndike, Woodworth, Nolan D. C. Lewis, *et al.*, in short, any fair cross-section of scientists concerned with mental phenomena, to assess such statements for me.

To make myself clear, the new approach seems to me full of possibilities. Obviously it can aid in the understanding of motor habits, methods of teaching, trance and visionary conditions, shamanistic personalities—all subjects as important for us as they are to a psychologist pure and simple. But before assimilating pertinent findings I should like to be sure of their validity. The late paleontologist, W. D. Matthew, once told me that he would like to write a treatise on land-bridges in which he would indicate where they must be assumed; where they can be inferred with probability; where they are possible; and where there is no ground whatsoever for postulating them. It is such nicety of discrimination that for my guidance I should like to find among our psycho-ethnographers. In my judgment they have something to give me; I want to know, how much.

To turn to the writers who combine sociological with ethnographic techniques and points of view, they are presumably best represented in modern acculturation research. On this subject, as one who has both witnessed and in his own person experienced acculturation, I speak with greater confidence. What impresses me is that I have derived the maximum illumination in this field not from anything produced by our guild, but from W. I. Thomas and Fl. Znaniecki's "The Polish Peasant in Europe and America" (1918–1921); from the second volume of Theodore C. Blegen's "Norwegian Migration to America" (1940); from Theodore Jorgenson and Nora O. Solum's biography of "Ole Edvart Rölvaag" (1939); and from the correspondence of Friedrich Kapp (in Wilhelm Bolin, "Ludwig Feuerbach" 1891). The reason seems to me clear. The study of acculturation along customary lines usually relates to the impact of our civilization on a comparatively unknown culture. With some distinguished exceptions—I can off-hand recall only Hilde Thurnwald's "Menschen der Südsee" (1937)—even our better monographs fail to delineate varieties of personality in relation to new conditions; and at best the documentation is meager, stimulating rather than quenching our thirst for information. How many Kwakiutl or Omaha Indians do we know as human beings after reading thousands of pages about these tribes?

Obviously the case is quite different for a study of acculturation in higher civilizations, where abundant evidence is either ready for use or can be secured without insuperable difficulty. I therefore venture to suggest that our specialists in acculturation examine the vast material of this order that awaits exploitation from an anthropological point of view.

As I see it, there has been in the past some danger of narrowing the field of inquiry, although the "Symposium on Acculturation"[10] indicates that broader points of view are maturing. Dr. Mandelbaum's discovery of tribes in India living in conditions externally ideal for exchange of customs, yet such as to inhibit cultural osmosis strikes me as highly suggestive. Again, Dr. Ethel John Lindgren's paper, "An Example of Culture Contact without Conflict" (*ibid.*, 40: 605–21, 1938) shows that where Caucasians are not numerically preponderant nor over-sophisticated the relations with an aboriginal people may develop in an exceptionally genial way. We shall get at the core of the matter only after having before us a great many different samples of contact metamorphosis, otherwise there is danger of mistaking special instances—say, of technologically simple and hopelessly outnumbered populations confronted by Western civilization—as typical. I venture to point out a few themes that in my judgment have not yet been adequately considered by anthropologists.

In the first place, I should like to see a synthesis of what is known about the Islamization of various peoples. The suggestive bits in Reuben Levy's "An Introduction to the Sociology of Islam" (2 vols., 1931, 1933) merely whet my appetite. Secondly, some reading prompted by the study of Japanese evacuation undertaken by my colleague, Professor Dorothy S. Thomas, convinces me that the conventional picture of what happened in Japan between 1853 and, say, 1894 is thoroughly distorted. Specifically, there was no initial wholesale repudiation of Western values. At some opportune time I should like some one conversant with the language to revise current opinion by examining the periodical literature of the crucial decades. Third, Americanization requires more intensive study than has been accorded it. It can not, *e.g.*, be viewed in terms of "inferior" immigrants who either crave absorption in the "superior" Anglo-Saxon tradition or perversely resist assimilation from motives of self-interest. Many of the newcomers look with scorn upon the American scene, which they regard as unequivocally "low-brow" compared to what they have known at home. Others, like Rölvaag, nostalgically try to salvage the values of the homeland and thereby to enrich their adoptive country.

[10] *American Anthropologist*, 43: 1–61, 1941.

To take the illiterate peasants as the norm can only distort our total picture of what is involved, for even though numerically preponderant these immigrants are swayed by their leaders. Finally, there is the generic problem, in Europe not less than in America, of a rural population adapting itself to an urban culture. I agree with Messrs. Conrad M. Arensberg and Solon T. Kimball[11] that such problems can not be solved in purely economic terms. But what is it, apart from economic motives, that drives individuals from the security of a cozy familial, communal, rural existence into domestic service in the towns, and from domestic service into factories? "Back to the land" has more than once been a rousing slogan; why does its effectiveness peter out?

CONCLUSION

In conclusion, I should like to repeat that we in America have been fortunate in our past opportunities. The probabilities are that we shall not be less so in the future. Disturbing as our "ethnic minorities" potentially are to our body politic, they offer rewarding and as yet inadequately utilized fields for research. Further, the World War is expanding our interests. As our archeologists have already turned pan-Americans, let us cease being Americanists and turn global anthropologists.

SCIENTIFIC EVENTS

THE ALTON OCHSNER MEDICAL FOUNDATION

THE Alton Ochsner Medical Foundation in New Orleans is a non-profit corporation chartered under the laws of the State of Louisiana for the following general purposes:

1. To conduct research in the causes, prevention and treatment of disease.
2. To provide fellowships whereby selected young physicians who have completed their internships may be trained in the various special branches of medicine and surgery.
3. To provide instruction for practicing physicians and surgeons.
4. To provide diagnostic facilities and hospital care for selected indigent patients.

The charter under which the foundation operates provides that no part of its net earnings shall inure to the benefit of any private individual, but that those earnings shall be devoted exclusively to the advancement of medical science, and to educational, charitable and literary purposes.

The foundation maintains paid fellowships for young physicians who have had at least one year of hospital training in order to enable them to continue their studies and research. These men are accepted for training in various medical and surgical specialties for periods of from one to three years.

Another purpose of the foundation is to conduct laboratory and clinical research into the causes, prevention and treatment of disease. Facilities in this direction have been enlarged considerably through the recent organization of laboratories for chemical research. The new laboratories provide means for close cooperation between clinicians and chemists on questions of mutual interest. It is expected that a frank and mutual exchange of ideas between members of the two professions will stimulate joint investigative work on diagnostic and therapeutic problems.

The research activities of the chemical laboratories will be concerned especially with the therapeutic aspects of medical enzymology. By this is meant work dealing with the role of extracellular and intracellular enzymes in disease and with disturbances in physiological mechanisms linked to abnormalities in enzyme activity, balance, concentration and function. Whenever desirable for therapeutic purposes, attempts will be made to restore or to modify normal enzymatic activities of the diseased organism. According to the official statement:

> It is evident that investigative work in this direction opens up new approaches to a large variety of therapeutic problems. The development of this field has a logical companion in present trends in clinical analytical chemistry, where determinations of biological catalysts are just beginning to replace successfully the use of functional and tolerance tests and the measurement of a variety of metabolites. The program of the chemical research laboratories as described above constitutes perhaps the first instance of a joint attempt by clinicians and chemists to use recent advances in enzymology for the solution of therapeutic problems.

According to equipment the chemical research laboratories of the Ochsner Foundation consist of three units. The first unit contains modern technical facilities used in biochemical research work, including equipment for the characterization of minute amounts of material. The second unit is fully equipped for work in synthetic organic chemistry. The third unit has the instruments required for research in clinical analytical chemistry. The work of this last unit will deal with the improvement of present clinical laboratory procedures in order to approach more closely the high standards of reliability which are characteristic for other branches of analytical microchemistry.

[11] "Family and Community in Ireland," 1940.