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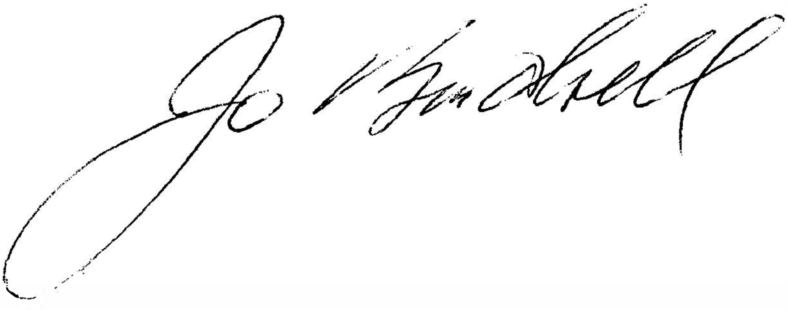

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SOME REFLECTIONS ON FIFTY YEARS IN BIOLOGICAL ANTHROPOLOGY

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Looking back over 50 years has tempted me to offer an overview of the changing intellectual scenery of the subject, even though some subjectivity is involved.

The dominant figure intellectually in physical anthropology in America for more than 40 years was Earnest Albert Hooton. He was a remarkable man personally, offering his students all the virtues of charity, warmth, support, and a genuine friendliness. His failing lay at a professional level, where he championed the anthropometry of the individual and never accepted the development of the new systematics as it emerged during his lifetime.

As his students we knew that he had started out in the classics. We were not aware that having qualified for a Rhodes scholarship in 1907, he deferred going at once to Oxford and instead attended the University of Wisconsin to do graduate work. He received a degree of M.A. in the classics in 1908, and a Ph.D. in 1911. This classical background, combined with his native abilities, helps to explain his superb manner of communicating both in writing and in speech.

Entering University College in Oxford in 1910, he attended a seminar on Sophocles and found it unrewarding. An earlier informal interest in anthropology led him to R. R. Marett, and so he took his diploma in anthropology in 1912. He was appointed instructor at Harvard University in 1913, where he was to remain until his death in 1954.

During his four decades of active teaching at Harvard University, Hooton trained the great majority of professional physical anthropologists who came

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to practice in this country during that period. These first-generation products in time became established in their own academic departments and went on to produce a second generation of professionals. The training provided by Hooton at Harvard included a stimulating series of lectures, both in regional problems and in methodology; but he was perhaps best remembered for his exacting bone laboratories. Rigorous and detailed, these succeeded in imparting to each student a permanent knowledge of bony anatomy. Over and above his strictly academic duties he served as a sympathetic father figure, and was much admired and liked by his students.

Physical anthropology has been defined in various ways. Some consider it to be the study of human evolution, and I am one of those. Others believe it to involve a methodology applicable to many types of investigations. The development of applied physical anthropology rises out of this latter concept. Hooton was perhaps the first professional to bring this discipline to maturity when in 1945 he helped the Heywood-Wakefield Corporation to improve the comfort of their passenger seats. Shortly thereafter he was called in as a consultant by the Air Force, who sought to fit personnel to their machines. In his initial project he investigated the restricted space in the ball turret on the all-important B-17 bomber. As Hooton pointed out, the compartment was so small that only a modest fraction of gunners could fit into it and function. From this study grew the important shift in emphasis to design machines to fit men, which has subsequently been pursued by both the Air Force and the Army. His techniques were quickly taken up by the armed forces of European countries. Hooton must be given full credit for this development, which stands as one of his most important and permanent professional contributions.

Hooton, like others working in the field in that period, was grounded in the conceptual framework of descriptive anatomy. It is not surprising that as a brilliant and ingenious man he carried this heritage to new levels of development, which suffered in not being based upon population concepts. All of his professional career he devoted his energies to what might be called the physical anthropology of the individual. As early as 1916, only three years after assuming his position at Harvard, he examined the physical characteristics of the crania of the extinct Guanche of the Canary Islands. This study indicated he had already formalized his own method of typological analysis using the characteristics of individuals to attempt to unravel racial origins (4). In 1926 in *Science* he reached a broader audience through his paper on "Methods of Racial Analysis" (5). This formalized his typological method, to which he remained loyal to the end of his research activities.

Very briefly, Hooton felt that variations among individuals in a series offered definite taxonomic clues to their origins. While he first worked out the method on crania, in time it became his standard technique for analyzing living populations. He had an extraordinarily sensitive morphological eye.

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His practice was first to spread out the whole population of crania on the tables of the bone laboratory in Peabody. He then sought to identify individuals among them that resembled each other in detailed ways. When he had broken the series down into what he considered morphological types, he proceeded to validate these types statistically by testing their metrical values against those of the total series. If the subtypes differed significantly from the total series, he considered his type selection justified. When in the literature he found other populations in which the indices were similar to those of his subtypes, he judged that he had identified the original population contributing to his test subset.

Hooton conceived of man first in terms of primary races, those of long standing in broad continental regions, and second in terms of composite races formed thorugh hybridization among the primary groups. He judged that his primary races were based upon traits considered "nonadaptive." Microevolution was little discussed in his day, and he did not consider evolutionary processes in any detail. His method of analysis was successfully taught to all his students, took firm hold among physical anthropologists practicing in Eastern Europe, and today still characterizes what is called the Polish Anthropological School. Hooton's confidence in his method was fed by its seeming successes. In "The Indians of Pecos Pueblo" (1930) he made composite photographs of his subtypes, which do in fact show distinct morphological attributes (6). I recently reexamined these photographs and found that Hooton's "Pseudo-Australoid" and "Pseudo-Negroid" do give an overall impression of morphological similarity to Australoid or Negroid populations. Small wonder that Hooton felt he was dealing with biological realities in a subtle and definitive fashion.

Armed with a sorting machine provided by Thomas Watson, Sr., President of International Business Machines Corporation, Hooton had the facilities with which to apply to large problems his belief that the individual was the source of evolutionary information. He initiated a massive study on the Irish, involving nearly 10,000 men, and sorted them into his typological components of European racial groups (8). The nearly equally large study of the Chicago World Series was never published but was conducted along similar lines. In defense of Cesare Lombroso, the Italian criminologist, against the criticism of Goring, Hooton undertook an elaborate study of the American criminal, both white and black. He felt that criminals should differ morphologically from the normal population—should show stigmata, as contended by Lombroso. Using metrical and morphological traits he compared criminals with the general population and examined differences among criminals by crime category. When he published his volume on the white American criminal (7) it was severely criticized professionally, and he did not publish the proposed second volume on the Negro criminal.

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When Sheldon introduced his constitutional typology (9), Hooton became an immediate and vigorous supporter. His dedication to the anthropology of the individual is best shown in a statement written certainly after 1951, but published after his death (3).

> We had thought that physical anthropology was through with this hoary sinner—the fictitious average type—but unfortunately such is not the case. There has arisen a group of geneticists who are interested in physical anthropology, but know little about it, and another group of physical anthropologists who are interested in genetics without knowing much about that, who have revived the old idea of talking about "populations" as if they were races or subspecies. These workers concern themselves with isolated variables and attributes because they are afraid to use the term "race," in any except the most generalized application, lest they be accused of "racial discrimination" or of being "racists." They are willing to have "races," but they are loathe to assign any individual to a race because they think of "races" as being "populations" or "groups." This is absurd. If there is a Negro race, there must be Negroes. The same thinkers, if they can be so designated, are equally opposed to individual constitutional "types"—and for the same reason.

It is remarkable that a scientist of Hooton's evident brilliance and breadth of interest failed to probe the relevant developing fields of biology. He had little use for and no real knowledge of the emerging field of genetics. Although the new systematics was being formulated by English workers in the 1930s, Hooton paid little attention to it. He never changed his original conviction that the secret of human racial classification was to be uncovered in the individual.

His method involved two serious biological faults. For ancestral racial types to "mendelize out" in descendant individuals, all of the racial attributes would have to be carried by genes at a single locus. This is of course impossible. Further, the bare bones of genetic resegregation makes it totally unlikely for the bulk of the population to show ancestral types. From genetics each population would be expected to comprise mixed phenotypes, with very few reconstituted ancestors. Yet one further difficulty stood in the way of Hooton's typological approach. The idea that indices, sometimes relatively few in number, might indicate ultimate relationship totally ignores the reality that in human populations proportions do not come in an infinite variety but in a limited one. It can be shown that populations having no possible recent relationship sometimes show quite similiar indicial values. The writer was the only student of Hooton's to point out these realities in print (1) during his life (1951).

The decline of interest in racial analysis among professional physical anthropologists can be traced to a number of coincidental factors. The Pearsonian School's coefficient of racial likeness failed to analyze fully the biological differences among regional populations, for it depended upon the improper assumption that relationship was measured by likeness. The collapse of Hooton's method of typological analysis finished an era. The swing

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to the analysis of populations continued among younger workers. Additionally, awareness of racial abuses in society increased, and the trend against racism set in. Whether the Marxist aversion to the concept of races was important in this trend is moot, but it certainly influenced the frame of mind of many cultural anthropologists. Today, save when craniologists and geneticists are seduced by computers and software into taxonomic exercises concerning population differences, the trend is toward studies in what is now called biological anthropology.

The American Association of Physical Anthropology was organized in 1929 by Ales Hrdlicka. Its founding members numbered several dozen, who delivered a like number of papers at the first meeting. Today the association lists almost 1300 members, and at meetings in 1986 no fewer than 356 papers were given in multiple sessions. Today, as originally, membership consists of a wide variety of persons with interests in human evolution and the biology of man. Members associated with medical institutions were among the founding fathers and still comprise about one fifth of the membership. They are sufficiently numerous that there continues today a professional interest in growth studies, with focus primarily on variations in economics, nutrition, or altitude. A new field of biomedical anthropology is developing. Forensic anthropology continues to play its role and has incorporated high-tech approaches to its problems. Many biological anthropologists at this time focus on the evaluation of the health, longevity, and causes of death among archeological populations. The advent of high technology allows the use of scanning microscopes and even newer instruments to investigate the shape of enamel prisms in teeth, elements of dietary intake, and a whole series of newly defined problems. If concern these days is with issues of lesser scope than those previously investigated so fruitlessly, there is a wholesome trend toward hypothesis framing and testing.

In the older phases of biological anthropology questions of taxonomy predominated. Now there is much more concern with process, a more difficult area of inquiry. The rise of population genetics in the broad area of biology in time affected the study of human evolution. In mid-century, genetic variants began to be discovered at an increasingly rapid rate. There was still then a feeling among biological anthropologists and human geneticists that genes could serve as race markers—an attitude that persists in a few quarters today. But it became evident with time that a population's gene frequencies were influenced by factors in addition to descent. Most mathematical models from population geneticists and empirical results from field workers indicated that evolutionary change was not in all cases linear and determinate but contained a large stochastic element. As soon as chance enters the equation indeterminancy arises. Today an important issue among students of human microevolution is the degree to which chance events influence population

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gene pools. Enough work has been done to indicate that this influence is considerable. For this reason we now question the old belief that likeness between populations, whether it be morphological or genetic, serves to measure their relationship.

One of the least changed areas of study in biological anthropology involves fossil man. The field has traditionally involved anatomists, and still does today. But the ramifications of fitting early hominids into their environment, dating their remains, studying their lifeways, and evaluating their nutrition, have created a complex of other workers focusing on the topic of early human evolution. Today, as always, the students of fossil man focus on taxonomy and tend to classify fossil finds in a simple, deterministic timeframe involving only adaptive changes. Owing to popular interest and massive funding, the number of recovered human fossils has increased from a few to many hundreds. This is true of the Old World generally, but dramatically so in East and South Africa. Paleoanthropologists have been less affected by the changing perspectives of overall evolution than other biological anthropologists. They remain convinced that (*a*) the degree of likeness between populations measures their relationship and (*b*) the differences between earlier and more recent fossils are always a direct manifestation of adaptive trends. While the latter is undoubtedly true in the long-term record, chance factors tend, regretably, to be ignored. As a consequence, in spite of the vast increase in fossils available for study, their classification has become yet more confused. Even the principles of cladistic analysis have not clarified Plio-Pleistocene taxonomy in the hominids.

The anatomical approach has not always produced evolutionary truths. For example, there had been long and bitter discussion of whether the australopiths were effective bipedally. Various reservations about pelvic structure and the like were voiced. All was resolved, however, when Mary Leakey at Laetolil discovered the remarkable series of hominid footprints more than 3 million years old, which conclusively show that the hominids of that period were effective in bipedal locomotion. These data resolved the problem without resort to anatomical judgments.

The groundswell in the study of animal behavior has dramatically influenced biological anthropology to date. The first field studies of primate behavior were undertaken by Ray Carpenter in the 1930s and stood for several decades as isolated classics of their kind (2). Since then, beginning in the 1950s, research in ethology (including that of the primates) has taken off at a remarkable rate. In the 1960s sociobiology began its formulations and has remained of primary importance. Sociobiology has the great virtue of providing testable hypotheses relating behavior to biology. Most primate ethology these days reflects this tendency. It has even appeared rather cryptically in the

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work of some social anthropologists, an area in which the importance of biology has usually been denied.

Traditional departments of anthropology today all have their quota of primate ethologists. It is difficult to decide whether the dominant sociocultural anthropologists who direct the destinies of these departments are primarily intrigued with the behavior of other primates as a simpler expression of proto-human tendencies, or whether they are really prepared to concede that sociobiological interests belong in their institutions. In any case it should be pointed out that whereas primate ethologists initially profess to be investigating behavioral aspects that may have direct bearing on human evolution, they frequently do not persist with that orientation in their research. George Schaller, for example, one of the first and best primate ethologists, no longer pretends to be providing materials for the study of human evolution. He has in succession studied the ethology of African lions, Indian tigers, the snow leopards of the Himalayas, blue sheep in the same environment, and recently has spent four years investigating behavioral patterns of the giant panda. All of his studies are extremely interesting, contributing much to general ethology; but they hardly fall in the direct line elucidating human evolution. This trend is evident among some of the other pioneer primate ethologists, and it simply means their loyalties are to ethology and sociobiology.

Here it may be appropriate to digress in the tradition of oral history. A somewhat subjective recollection of life in the department of anthropology at Harvard University in the 1930s may interest students in this age.

Harvard University, of course, has an awesome reputation, which certainly helps graduates find their careers in life. But for me, the attraction of Harvard resided in the person of Professor Earnest Hooton. As a graduate in aeronautical engineering from the Massachusetts Institute of Technology with two dismal years' experience as a financial analyst in New York City with Dunn & Bradstreet, I suspected the world of anthropology was more or less a closed book. But Hooton had made news in the *New York Times,* appearing there as an authoritative student of human evolution. He looked to be an altogether stimulating person, and thus he beckoned to someone badly needing a change in career direction.

When I arrived in the department of anthropology at Harvard University in 1935 a strange but interesting new world opened. The department had barely recovered from the death of Roland B. Dixon, its prime mover in the field of ethnology. Dixon had instituted an approach involving area courses jammed with detailed descriptive material, and these continued under institutional momentum. Professors Hooton and Alfred Tozzer occupied the key positions in the department, and below them at untenured levels were a number of younger men who taught a variety of area courses and other undergraduate

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subjects. The manner in which Harvard University exploited its untenured faculty maximized competition among the insecure younger personnel. Two of the top competitors for the next step to tenure were Carlton S. Coon and Clyde K. M. Kluckhohn. The tension generated by this situation spread downward throughout the entire graduate student body. It seemed necessary to side with one or the other of the candidates. The graduate students were badly split, as were the younger members of the faculty. A majority entered the camp of the Coon supporters, while the cultural-anthropological candidates, fewer in number, rallied around Kluckhohn. Both of the aspirants for tenure were warm, personable, and intellectually stimulating. Perhaps the best strategy for a new graduate student was to avoid firm commitment to either camp. Although maintaining neutrality was difficult, I succeeded in remaining on friendly terms with both Coon and Kluckhohn, not only within the department but also in after years.

I never entered Kluckhohn's courses formally, but just before I left for field work in Australia in 1938 Clyde asked casually in the hall whether I planned to do any blood grouping. Such a project had never been mentioned by Hooton, who did not include genetics in his interests. Clyde immediately called William C. Boyd by phone and arranged that I receive laboratory instructions within days. Boyd not only taught the subject effectively, for it was still simple in those days, but provided test sera throughout my two years of field work in Australia. Further, in 1947 Clyde was instrumental in obtaining me a teaching position at UCLA, which occupied the rest of my professional career.

Course requirements for the degree of Ph.D. were not unduly strenuous, even for students with no undergraduate training in the field. Professor Hooton taught a variety of courses in physical anthropology and two semesters in European archeology, which he justified by explaining that the real archeological professionals made the course too complicated in detail. Coon was not allowed to teach any courses in physical anthropology, but he handled a variety of area courses. There were no departmental seminars in those days, and in fact the intellectual content of the department's offerings stressed description rather than theory. As in most departments, the graduate students taught each other a good deal and their society was agreeable, even if native New Englanders remained reserved toward the aliens from west of the Hudson. The department used its prestige in raising funds for field work for its most promising candidates.

Norman T. Tindale, who has spent more time in the field with the Aborigines than any other anthropologist, had lectured briefly at Harvard University. Influenced by him, Hooton determined that Australia would be a favorable site for the study of race mixture. (Tindale thus entered my life even in graduate student days.) Hooton obtained a sizeable grant from the Carnegie

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Corporation in New York to provide for two years of study in the Australian field. By great good fortune I was delegated to do the field work. This generosity of Hooton's affected the rest of my professional career.

Field work in Australia in 1938–1939 was in many ways filled with good fortune. Tindale was present the entire time, taught me how to work effectively with Aborigines and part-Aborigines, and took the genealogies of each individual. He guided our work to maximize the number of hybrid individuals we could study up and down the entire east coast of the continent and across the southern coast to the southwest corner. We were further helped by the fact that economic conditions in Australia had driven most of the hybrid peoples back to government support stations of one kind or another, concentrating them where they could be examined. Since check samples from full-blooded Aborigines were needed for the study, the Aborigines of the east and south coasts were included in the series even as they were proceeding in most areas to extinction. (As an aside it may be pointed out that I entered the field with full confidence in the typological approach used by Hooton. After the first few months of field work with real populations, that set of assumptions was necessarily discarded.) The results of this trip have been published in terms of the trihybrid origin of the Aborigines. But the all-important hybrid series, containing almost 400 first-generation crosses between Europeans and Aborigines, is just now undergoing analysis for reasons that seem sufficient and proper.

It is something of a truism that the field research undertaken for a doctoral dissertation permeates the rest of an individual's research career. Series of desert Aborigines examined in Western Australia in my first field work in time provided the conviction that microevolutionary studies of process were still possible in that portion of Australia and that population structure was eminently suitable for the project. I therefore spent two further years in the field in 1952–1954. When I was preparing for that trip to Australia, Carl Coon rather chidingly said, "You're going back to Australia, but you've been there." Coon was essentially a taxonomist of human populations and did not concern himself deeply with processes.

At the present time data from the second field trip are being prepared in monograph form for our "Microevolutionary Patterns in Aboriginal Australia. A Gradient Analysis of Clines." It contains many new and exciting aspects of microevolutionary processes.

The population structure in Aboriginal Australia had to be defined in basic terms before evolutionary studies could proceed further. A good deal of anthropological inquiry had been conducted by A. P. Elkin and his students, but their interests focused almost exclusively on social structure and kinship. Few data existed about Aboriginal ecology, demography, and the detailed structure of these simple human populations. Such variables had to be an-

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alyzed. Consequently I spent some years in these endeavors. There was a high correlation, in excess of .80, between the amount of annual rainfall and the tribal area occupied. This is of course a rudimentary kind of environmental determinism of population densities. An analysis of the function of family, band, and tribe showed clearly that the tribe was the breeding unit, or deme, in genetic terms. Throughout Australia homeostatic tendencies kept population units in equilibrium with their environments, even after short-term disturbances. This could be demonstrated in an experiment in nature owing to the disruption of tribal units after the acceptance of the initiation rights of circumcision and subincision. Systematic female infanticide was used as a stabilizing force to keep numbers below maximum carrying capacity. Between 25% and 50% of female infants suffered this fate. Tindale's excellent work demonstrated that intertribal marriage was relatively uncommon—14% or less in the regions covered in the first trip. It fell to less than 12% in the desert tribes researched on the second trip. These data provided the basis for a model showing how tribal boundaries did inhibit intertribal marriages and presumably other cultural transactions. As opposed to the expectations of the basic band geometry, fewer than one third of the expected intertribal marriages occurred.

With the homeostatic tendencies broadly evident in Australian populations it is possible to define three so-called "magic numbers." Families tended to number 5 persons at any given time, bands showed a surprisingly tight distribution about 25 in number, and there was a general tendency for tribal populations to approach 500 persons. These population properties made Australia an ideal laboratory for the investigation of microevolutionary processes.

The effective breeding population of each tribe was relatively small—about 100 persons. The rate of gene flow between these tribal demes was low. The relations between men and their land allowed the development of the concepts of genetic distance and genetic space that facilitated modeling of processes on this continent in pre-contact times. Not surprisingly, the dynamics of biological processes under these conditions contained many interesting features. My field work in 1952–1954 was designed to obtain biological data on as many tribal demes as could be salvaged in Western Australia. It was not certain in advance what processes should become evident. Population structure suggested that random genetic drift and founder effect would be identifiable in the data. These expectations were realized—and a great deal more besides.

All of the more than 150 biological traits surveyed showed clinal patterns in space. This finding required the development of a space calculus to evaluate the steepness of clinal gradients. This was achieved in a simple fashion that allowed the combining of slopes for different traits into a composite gradient.

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An overview of the results indicates that chance processes are more visible than adaptive linear trends. Many characters exhibit north-to-south gradients, but these are better explained as residues from biological differences among the three incoming waves of peoples than as local adaptive processes. While adaptive changes are clearly the major feature of long-term evolution, these data (with their time depth of ~10,000 years) suggest that in shorter time spans such changes are not so evident in simple human populations.

Several thousand genealogies taken in the field indicate that inbreeding, the supposed characteristic of small populations, does not occur among these Aborigines. As Tindale has reported, they consciously arrange their marriages so as to avoid it completely. This not only throws doubt on some of the most important formulations in population genetics, but also reflects badly upon certain aspects of social anthropology. The belief that the 4-section system and the 8-class system forced inbreeding depression upon the populations practicing them is clearly an artifact of playing kinship games without recognizing that the rules of these maritial systems involve classifying terms of kinship and not biological ones. With careful management, genetic inbreeding can be avoided in very small populations.

The intensive analysis of good data provides unexpected profits. It has been possible to insert time factors into what is essentially a space calculus. Two traits involving transient polymorphism are useful here. One, the mutant tawny hair color, can be shown to have spread over roughly the entire continent in a few thousand years. Another genetic trait, nasal cartilage anomaly, spread rapidly in northwestern Australia in an even shorter time. The coefficients of selection for these traits are not .01 or .05 as has usually been postulated; they are much greater. The distribution of another trait, the blood group type R_z, in 3 abrupt disjunct clinal peaks in Western Australia allows a modeling calculation of how far back in prehistory the trait originated, from founder effects under extreme climatic conditions. Population structure is regular enough to show that this series of dramatic peaks was probably formed no more than 4 or 5 centuries back in prehistory.

Several other interesting exercises have become feasible. The use of composite biological gradients leads to analyses involving other systems. For example, a series of tribes in the Fitzroy River valley can be compared with the adjacent desert tribes parallel to them in the south. Whereas ecological advantage might be expected to produce more frequent interdemic interactions, apparently it has not. The degree of biological differentiation among the desert tribes is smaller than that among the well-sited tribes on the river. In another instance, the rate of differentiation of language can be compared to that of biology. In the extreme northwest of the Kimberleys a series of tribes speak prefixing languages. These have always been considered very different from the normal Australian language patterns to the south and

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on the rest of the continent. Biological data were collected on both groups. Using composite gradients it is possible to show that the substantial differences, as measured by linguists, may occur in a matrix of tribal demes in which biological differences have not become highly developed.

Two other findings are of biological interest. In the northwest of Western Australia a small pit frequently occurs at the base of the helix of the ear. The differences between a bilateral, a unilateral left, and a unilateral right presence occur in the whole number ratio of 1:2:3. This is statistically significant, and the same laterality of expression is true for large series of Europeans and East Africans. It is an example of directed, genetically determined asymmetry. Since the phenomenon results from an embryonic failure of the fusion of the ear buds, it may be a clue to basic processes in fetal development. Another new finding, based upon MN blood types, is that the frequency of the gene *N* varies between the sexes and with age. This is evidence of a new form of sex-influenced selection that operates in one direction in males and in another in females. While we cannot at present explain the phenomenon, its observation does open new doors to the analysis of selection in basic biological attributes in human populations.

If the above paragraphs sound enthusiastic, they are meant to. I owe an initial debt of gratitude to E. A. Hooton, for allowing me the opportunity of working in Australia, and another to N. B. Tindale, for facilitating my work there, educating me in the realities of the field, and providing intellectual support and helpful data through the half century of our relationship. Biological anthropology at this level is completely rewarding.

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