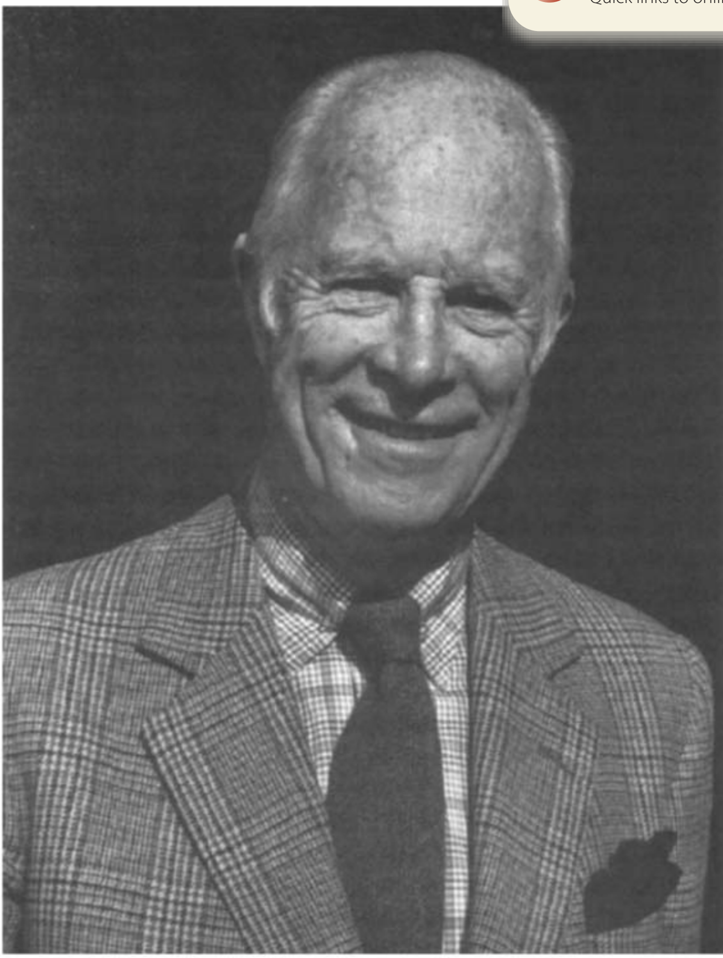

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YESTERDAY, TODAY AND TOMORROW

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Although attuned to dinosaurs and cave men as a boy, I never heard of anthropology. Certainly not as a college freshman tentatively pointed for English literature. That pointing was stopped dead by the list of summer readings I was handed. Concealed from consciousness at first was an older friend's mention of enjoying a course in anthropology, and now a light went on in the attic. I went to see Professor Tozzer, the anthropology chairman, who made it plain that the department did not need every Tom, Dick, or Harry, and admitted me as a concentrator only after determining that my freshman grades were above a certain not very high minimum.

Like some others, I was captured for good by the essential appeal and viewpoint, both intellectual and aesthetic, of anthropology as a whole. Naturally, this viewpoint was reflected in our teachers. None of the three limited himself to one subdiscipline. Roland B. Dixon's courses were mostly labeled "Races and Cultures of" Oceania, North America, etc, which included archaeological outlines of these areas; he also taught the prehistory of China, India, and Mesopotamia, as well as a seminar in linguistics. Alfred Tozzer taught social anthropology and religion, although his own field was Middle American archaeology. Earnest Hooton supplemented physical anthropology with a very good course on Africa, the only gap in Dixon's perimeter, and also a course on European prehistory good enough to capture a Movius. We were supposed to cover all the fields, and even in my graduate years I took Tozzer's seminars on Mexico and the Mayas, to my great gratification then and later.

From Tozzer and Dixon I learned, or was at least exposed to, the beauties of organization. Tozzer was serious and methodical in lectures. He wrote nicely arranged outlines on the board, but in a hand that did not distinguish well

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between i's or o's or n's or u's, so that when he wrote a word like "commun-ion" it was apt to look as much like a healthy cardiogram as like actual handwriting. His seminars were different: Held in his office, they were also appealing presentations of the solid information of the day, but laced with anecdotes about Middle American archaeologists reputable or disreputable, delivered with a delighted half chuckle. It is easy and pleasant to remember his face in action and the sound of his voice—the things that live on in the memory of one more generation after you die, before they are gone forever.

Dixon had to be experienced to be known. He was reserved and, I think, shy; but on the other hand he was kindly, amiable, and helpful in course contact. His erudition was enormous: He knew a number of European and American Indian languages, without linguistic training; and he had wide first-hand ethnographic knowledge. He was a walking HRAF: Each subregion in one of his area courses got exactly the same treatment; he needed no outline on the board, because by the second week any new student knew what was coming next. First, remarks on physical anthropology. Then a capsule archaeology. Then material culture: food-getting, house form, clothing, artefacts of every kind, weaponry offensive and defensive, and so on, followed by similarly structured social organization and religion. Dixon had only to take a breath, and the student wrote the next heading down in his notes. But this approach was not dry; it was interesting, and the student left the lecture with sore tendons in his hand but satisfied that he had gotten his money's worth.

Dixon was extraordinarily meticulous and immensely systematic. His systematic nature also expressed itself in the organization and cataloging of the Peabody Museum library (now the Tozzer Library of the Harvard University Libraries, and always unequaled in anthropology). It was he who planned the exhibits in those halls that were devoted to ethnography: beautiful epitomes of all culture areas, without concessions to artistic canons of display. He must have collected a great many of the materials himself. Now thoroughly out of date and dismantled, they were nevertheless gems of visual information.

Hooton, of course, was a phenomenon. He was genuine, forceful, and reflective. Entirely without self-consciousness, he never gave a thought to presenting a persona. He was not one to look over his shoulder to see if any were following him. Nor would I call him merely charismatic. I do not think his students had a "Cher Maître" attitude toward him. All this may seem strange, considering his public image. Based upon my first memories of him, I doubt he was originally aware of his gift for the vivid and the comic; but gradually he became a formidable public speaker. Owing to his richness of expression, and to the fact that he knew his own mind, Hooton was much in demand at Harvard alumni clubs and elsewhere. He felt strongly that in tastes, ideals, and breeding habits the American public was going downhill—has he been proved wrong? wait and see—and he said so constantly enough to create a misanthropist image. Despite his deep pessimism (e.g. 19), he illustrated many of his general writings with humorous cartoons. He was not a first-class draftsman like Adolph Schultz, but he was good at making his point, and his blackboard drawings were excellent.

Hooton never expressed his dark views in the classroom. He was a completely practical teacher, who could in fact dictate a lot of numbing statistics in his courses. In the midst of such a delivery, naturally, he might suddenly give way to a humorous or colorful diversion. His witticisms were not set pieces, asterisked in his notes, but spontaneous and appropriate excursions.

He was patient, listening seriously to his students and never brushing anything off. He was not a glad-hander. He laughed easily, and liked to, but he didn't go around smiling all the time. Behind a practical exterior he was warm and hospitable. He and Mrs. Hooton had tea for all comers every day, and everyone was welcome. At the first department meeting after he died, someone asked who would now offer daily afternoon tea? The answer, alas, was that nobody would. We had lost an institution along with the man.

He was always mindful of his graduate students, past and present. It was probably only later that they realized how much thought he gave them, and how many opportunities, large and small, he put in their way. In my youth I was asked by a publishing house to write a popular book on human evolution. There were not many such books then. The publishers had asked Hooton to do it but he, being too busy, recommended me for the job. To show me what they had in mind, they gave me another of their books—of the "Story of Chemistry" brand of glop, as I remember. I thought I could do better, fell to work, and sent them half a dozen chapters. They said sorry, not really what they wanted, there's a war on, etc. I tried another publisher; no luck. I thought my work was not really so bad, and asked Hooton if he would look at it. In a way I was actually competing with him, at his behest. He read it over and on his own hook advised the publishers to reconsider it. Properly awed, they told me they had heard from Hooton how much my book had been improved, and requested another look. Along with the same old chapters I now sent additional ones, and after some further dithering they brought the book out as *Mankind So Far* (23). By any modern standard this work is old-fashioned, of course, as such books get; but it was successful then and did me a lot of good. However, it was Hooton who started me on it and helped it through by taking a little trouble. That was by no means the only time he gave me substantial help or put me onto interesting things. Nor did I butter him up; far from it. I was too naive for that.

He was simply large souled. He was incapable of taking offense—or at least he never did so in my presence. He couldn't give offense either, even when (being no Mr. Chips) he spoke sharply. In that remote day it was permissible to turn in hand-written papers. He forcefully told me several times that my handwriting was dreadful, which it was; the criticism never seemed harsh to me. Later, when I was the new Secretary of the AAPA, I was with him one day in his study at home discussing the next meeting. He suggested having a session on new methods of body-typing. I was not conscious of the fact that he had recently become interested in Sheldon's approach. In my callow way I suggested that such a session would only open the door to a lot of constitutional stuff. Mrs. Hooton had just come into the room; she laughed heartily and went out again. I don't know why Hooton didn't kick me. He simply gave a kind of dismissing grunt and passed it off. Only later did I realize what I had said.

Around this time Hooton showed his unflappability at a meeting of the AAPA. Ashley Montagu had considerable gifts in anatomy and anthropological history, as well as one for treading on his seniors' toes. His listed paper was on the pyramidalis muscle in the primates, but he announced a change of title and launched into an attack on some recent writing of Hooton's. Hooton, seated right in front of him, listened with utter amiability, thanked Montagu and offered to give a paper on the pyramidalis muscle.

It is commonly said that a whole generation of physical anthropologists was "trained by Hooton." This does not sound right: If there had been more coaching, his students would have tended more to follow parallel tracks. Instead, they set off in many directions. As he said himself, he was pleased that none of them were yes-men. Of course, anthropometry was the available technique, whether for measuring growth (Garn), for evaluating the effect of environment (Shapiro) or, in the bulk of cases, for comparing and describing populations to various ends. He even influenced some cultural anthropologists to bring back highly useful data—e.g. Lloyd Warner, Douglas Oliver, Clyde Kluckhohn. In his *Up From the Ape* (15), he included instructions in measuring; but I do not remember any drill in such work, of the kind implied by "training." The only process that could be called training involved practice in bone identification, along with a little desultory work in bone measuring, and some rudimentary statistics.

Rather, he educated us. He profoundly respected education in the basic sense, from his own experience. Beyond that, the effective element was the way he treated us, in conversation and suggestion, and in putting in our way opportunities for thesis work and later projects and jobs. And behind it all lay his attitude: his verve and his honest interest in his own work and that of others. Some of his students could speak well, and some could write well, but he excelled them all at both. Although we did not actually see him at it, I think he was also more industrious than any of his students, possibly excepting Carl Coon. He helped and influenced archaeologists as well as physical anthropologists. He showed his gift for synthesis in his books, above all *Up From the Ape* (15). With that book and *Man's Poor Relations* (22), he introduced primates to the public at large, showing an engaging affection for his subjects. Such writing was a major gift to both the profession and the public.

Naturally, it was course work that drew undergraduates and graduates under the Hooton spell. The mainstays were his human evolution and bone courses, the latter having some coverage of the living. His other offerings involved his own interests, race mixture and anthropology of the criminal.

These may seem bizarre choices now, but given the embryonic state of human genetics at the time, I suppose the study of racial hybridization seemed a useful approach.

I can think of only one student who took up "race mixture": Harry Shapiro worked with the descendants of the *HMS Bounty* mutineers. Nobody followed Hooton into the anthropology of criminals, although he employed some students in his work. In a major lifelong investment of time and energy, Hooton pursued connections between soma and psyche, body and behavior. He lectured at length on Cesare Lombroso, who claimed to distinguish physical marks of criminal man, and on Charles Goring who, in *The English Convict* (11), employed biometry instead of ear lobes, and satisfied himself by statistical analysis that few if any measurable aspects distinguished malefactors from benefactors. Hooton felt strongly that this was a wrong conclusion, and once said in a radio talk: "See what a rent the envious Goring made [in Lombroso's mantle]."

Yet it was Goring's system of statistics that Hooton taught us then, and which he used for the rest of his life. It had been suggested to Goring by Karl Pearson, consisting essentially of the *t*-test for the signficance of a difference between two samples. (Hooton got the correlation coefficient from Kendall and Yule, and taught us that also.) Is has to be said that Hooton had no mathematical background and, more important, little natural feeling for the relation between statistics and data. He taught us how to compute the standard deviation but said nothing about either the normal curve of error which it expresses or the whole matter of sampling. All this, I think, cost him dearly in effort. He mounted a massive project on US prison populations in a number of states and believed he had detected correlations between physical differences and types of crime (20). Both his selection of samples and his ideas of control were denounced by statisticians right and left. He never thought simply to compare convicts with their unincarcerated siblings or to use similar controls. Such a technique might not have been easy, but even limited samples would have yielded better answers than he got from his massive but rather ill-chosen bodies of data.

Statisticians were not alone in belaboring him. He was constantly under attack by those whose dogma absolutely denies all connections between biology and behavior. Such connections were indeed among Hooton's avowed interests, but he was never, as I think many believed, a racist. One need only peruse his many public pronouncements, as in *Apes, Men and Morons* (18), or *Why Men Behave Like Apes* (21), to see that this was not the case. On the other hand, he berated social scientists for ignoring the variable nature of the human animal.

He eventually felt frustration with the methods prevalent in his early career. He became discontented with types and measurement, out of his own philosophy. He thought there should be a holistic science of body, function, and behavior, and that physical anthropology should, as the Chinese say, serve the people. (This has not been a general attitude among us, then or since.) He made his view known repeatedly, especially in public lectures. "In its beginning physical anthropology was pure science, in the sense of being completely useless," he said. He did arouse the interest of various local researchers in child development and dental medicine, but what to offer in more positive terms remained a problem. He wrote: "A few medical scientists turned to physical anthropology.... They asked us for a comprehensive scheme of body build and we gave them the cephalic index—which is the scientific equivalent of asking for bread and getting a stone. Medical science looked at the data of physical anthropology and saw a mass of unrelated measurements, coefficients and probable errors. It turned away in disgust, leaving the anthropologist to rattle his calipers in the Valley of the Dry Bones."

So it was that, in about 1940, he looked to Sheldon's methods of body typing, which had a double appeal: scales in place of measurements or types, and a promise of body/behavior connections. But Hooton soon discarded Sheldon's formalized scales of temperament, simplified and objectified his scales of body typing, and, in his late work, addressed himself to possible physical variations in military personnel by function and performance.

For practical ends, of course, things have now changed at many points. One need only think of growth studies, or the introduction of more sophisticated insights coming from anthropology into medicine.

Hooton's earliest principal field of endeavor was craniometry. The work itself did not last beyond the 1930s, but the interest persisted in his osteology course and in his view of race and populations, and, I think, best reveals his train of thought. As his other interests show, he was a self-made anthropologist. His mentors at Oxford and elsewhere were anatomists, but he did not follow that line. And, strange to say, he took no note of Karl Pearson's school, of which more below. So there was not much to guide him outside of the French and German formalized methods of measurement. He got some instruction in the Broca school from Hrdlička. He practiced these techniques industriously in major works, but I think in the end he found them rather sterile.

Unlike the continentals, he was not interested in straight racial classification. (He did suggest some rather tortuous racial genealogies, but not as a result of his own research.) Both he and the Europeans can certainly be called typologists, but there is a distinction: The Europeans were more interested in distinguishing among whole populations as types or as representing preconceived "races." Hooton was more concerned with the dynamics, with looking at the variation within populations, although in such work typology was indeed prominent.

His first major work, on Canary Island skulls (13), was an attempt to find significant subdivisions within that population. Let him tell it (17): The Canary Island study taught him "that the analysis of a racially mixed population whose antecedents are individually unknown, can best be approached by the division of skull series into morphological impressional types, on the basis of general resemblance. Such types may then be validated by statistical tests." This was actually written after his well-known Pecos Pueblo study (14), which was carried out exactly along those lines.[1] The Pecos cranial types were assigned designations partly with suggestive connotations: "Pseudo-Negroid," "Long-faced European," "Plains Indian." Given the process of selection, it is not surprising that the statistics appeared to support the distinction among types. But he also found that his "Pseudo-Australoids" and "Plains Indians" failed to approach their proposed affiliates. This work founded his perception of American Indian racial origins, and of racial history generally. However, his interpretation (e.g. 16) was by no means as simple or specific as the above labels might suggest.

With our latter-day wisdom it is easy to see the a priori flaws—i.e. using statistics to validate what were nonrandom samples to begin with, and departing from the assumption that skeletal populations, whether Canary Island or Pecos, were racially mixed, not normal breeding populations. Hence the types. But it was Dixon, in an unlucky foray into physical anthropology (5), whose racial types were based strictly on divisions of three cranial indexes—e.g. long skull + broad nose + long face would be one of eight such possible combinations, each of the eight in search of a primeval parent race. In a mammoth analysis Dixon subjected cranial series the world over to such subdividing, and created a scenario of migrations of the eight primeval races that was not persuasive. Chastened by the reviews that met the publication, he retreated into his excellent work on cultural evolution and diffusion, and he ever after spoke of his *Racial History of Man* as The Crime. It is interesting that Dixon's Crime was too mechanical for Hooton. They worked independently, even though they shared certain assumptions and were similarly vague about the ultimate origins of the supposed racial components involved.

However the results may look today, the Pecos study was impressive as a thoughtfully designed and fully executed major project of craniology, and it was correspondingly influential. Like others of the day, I used means of measurements and indexes for description and comparison, trying to relate populations in this way. I gave Hooton's approach a try with two cranial series, one Irish and one Melanesian, but was unable to discern anything persuasive as to types; also, I was dubious about dissecting populations in this way, having some idea of normal variation. I take no credit for this; it was a limitation that seemed to enforce itself. In comparing populations, running the

[1] I am here addressing only the matter of types. The Pecos study was also a detailed tabulation of the characters and variations of the series of the whole; it included statistical testing of the type series against known sets of African, Asian, etc skulls, and also statistical comparisons of stratigraphic subgroups. The impressive combination of inventiveness and much detailed work is the reason for the general effect of the Pecos project.

eye up and down columns of means was minimally rewarding but seemed to me the only recourse. For statistical support there was Pearson's Coefficient of Racial Likeness, which entailed a lot of labor, and Mahalanobis's D^2 distance, which entailed a great deal more, although on the face of it a better statistic. I became disappointed and disenchanted with available methods. So, of course, did Hooton, who had been the most inventive in his own endeavors, both with crania (Pecos) and with the living (Irish; the data having been collected on a large scale by Wesley Dupertuis).

The second major figure of the time was, of course, Alex Hrdlička. Tenacious of purpose, he made great contributions: founding the *AJPA* and supporting it partly out of his own pocket; bringing the American Association of Physical Anthropologists into being; building up the cranial collections of the Smithsonian and indefatigably traveling to collect data on crania elsewhere; and cleaning up many dubious claims of ancient American human remains.

He was certainly a diligent measurer of skulls. At one dinner of the AAPA Raymond Pearl toasted Hrdlička with a rather wooden jest about how many skulls Hrdlička could measure in a day, beginning with six before breakfast. Hrdlička, as I remember, was not amused. As to methodology, he was indeed keeper of the flame. He was true to the methods he had learned in Paris, and he explicitly saw firm standardization of measurements as a gift to the future. In the 1930s he endeavored to reinforce standardization by means of special small studies, published in the *AJPA*, on particular points of craniometric methodology. I was obliged to do one by the sheer force of his personality, brought to bear on this very junior colleague. I remember riding back from a New Haven meeting on the train, observing Hrdlička trying to persuade Adolph Schultz to do a similar thing, with Schultz uncomfortably but successfully managing to evade the job. I think Hrdlička also approached Hooton, also unsuccessfully.

Hrdlička compiled and published large numbers of measurements and their means but did not use them effectively to demonstrate anything. Statistics flowed easily from Boas; Hooton used them lavishly but did not understand them well; Hrdlička detested them and warned the young away from them. As Alice Brues says (1), Hrdlička suffered from "math anxiety," a temperamental disposition not unknown in later anthropologists.

Biological anthropology in America might have a different face today if Franz Boas had been teaching in a university with more students interested in that field and in evolution, and if Boas had not eventually been so occupied with the Kwakiutl. At Columbia he had only one PhD in physical anthropology, Marcus Goldstein, who tells me he thinks Herskovits may also have started in physical. Boas actually did more in our field than might generally be remembered today: influence of the environment (changing cephalic index in immigrants), growth studies (spotting different rates in individuals, etc; see

32), and biometrical genetics (unexpected stature values in hybrids; importance of variation among family lines within a population).

His German education was strong, beginning with classics. Boas was fascinated by physics and mathematics, which obviously came easily to his intellect. He thus saw the biological and the mathematical aspects of problems and data at the same time. He admired Karl Pearson and his work; he did not go as far as Pearson in actually formulating statistics, although he adumbrated analysis of variance before Fisher introduced it. Though he used and warmly advocated measurements, specifically for purposes of classification of existing humanity, his classification was broad and loose, unlike those of his contemporaries. In 1899 he wrote (2): "The function of measurement is … solely that of giving greater accuracy to the vague verbal description … . Measurements must be selected in accordance with the problem that we are trying to investigate." Put that with what Fawcett (see below) wrote a couple of years later, and anthropology would seem to have been poised for a more rapid advance along such lines than actually ensued.

I wish I knew more about Boas. As with Hooton, I am inclined to think his influence was not that of simple charisma (in spite of his being "Papa Franz" at Columbia), but rather came from personal force, ideas, and industry. He never downplayed the biological aspect vis-à-vis the cultural, although many of his students tended to do so. That is to say, I think some members of the "Boas school" developed attitudes that were not those of Boas himself. At the same time his mathematical viewpoint defended him against ideas of "types" and fueled his devastating review of Dixon's *Racial History of Man* (5).

In the early part of the century, craniometry seemed to be the only game in town as far as bones were concerned. Of the major players, Boas sat osteology out, allowing Bruno Oetteking to give courses at Columbia that were pure and dense methodology. Boas seems only to have taught a grueling course in statistics.

With respect to developments then and later it is a pity that there was not more transatlantic communication, specifically between Americans and the Biometric Laboratory under Karl Pearson. Pearson proceeded from statistical principles to the measuring of crania as test material, instead of the other way around. He got his good friend Flinders Petrie to harvest large series of skulls in Egypt, where Petrie was the leading archaeologist of the time. In due course Pearson and such coworkers as G. M. Morant and Miriam Tildesley became more interested in the anthropological problems, and used various sets of crania for comparison. Pearson devised the Coefficient of Racial Likeness as the first multivariate measure of distance among samples or populations.

At the turn of the century Cicely Fawcett, under Pearson's guidance, published on the Naqada crania (8). This paper introduced the measurement designations and definitions used henceforth in the Biometric Laboratory, borrowed with modifications from the Frankfurt Agreement of 1884. [In Hooton's very first cranial reports, on the Madisonville (12) and Turner (36) mound burials, he noted that the methods in use in the Peabody Museum and in the National Museum conformed instead to the Monaco Agreement, implicitly acknowledging Hrdlička's influence.]

Fawcett had this to say: "We do not contest the value of anatomical appreciation in the hands of the master, but we do contest the cloaking of such appreciation by an apparent array of craniometric data, which are statistically inadequate," going on to assert that most data published by anatomists were hopelessly inadequate and further, as Pearson had made clear, that large numbers of specimens would be needed. This statement seems to answer beforehand some objections made later, especially those about the relative virtues of anatomical versus statistical assessments.

But the main point is this: The Biometric Lab people knew why they were measuring, whereas many craniometrists—and I would have to include Hrdlička—did not. There remained the problem of computation: The CRL was a makeshift. R. A. Fisher, in his well-known paper (9) of 1936, pointed out that the CRL was a measure of significance of difference, not a measure of likeness or of distance. He also complained that things were in a primitive state if specialists were not being taught even simple statistics, and said "the fact is, whether it be necessary or accidental, that the majority of anthropologists, as of biologists, feel so unfamiliar with statistical reasoning as to accept, in some cases, alleged statistical conclusions with something akin to credulous awe, or in others to reject them with indignation as introducing unnecessary confusion into otherwise plain issues." By this time Fisher had already formulated analysis of variance and the discriminant function, and he knew what he was talking about.[2]

Mahalanobis (25) in the 1920s tried to point out to Pearson another defect in the CRL: that it did not take account of the correlation among measurements, as Fisher also noted later (9). He did not prevail, but then himself introduced D^2, the accepted best measure of distance today. Lionel Penrose in 1954 (28) published his size and shape distances, also subject to the effects of correlation; but because of greater ease of computation several people argued that they were close enough in results to D^2 to be preferable. Computers have naturally made that appeal pointless.

Thus for the first half of the century there was a certain stagnation at the center of this kind of work, owing to lack of both computers and communication. Again, Boas practically invented analysis of variance himself but only in the form of a mathematical expression to point out the importance of variation within populations, not only the differences between them. To Hrdlička all this

[2] I met Professor Fisher late in 1961 in Adelaide, where he had retired. In the course of conversation he said he thought Africans were most distant among modern populations, essentially today's Out-of-Africa position. To my lasting regret I did not press him as to why he thought so; he died a month later.

was obscenity. Hooton always employed a small staff in compiling and computing, by hand and using punched cards, testing the significance of differences among types, criminals, or army jobs. Even Pearson, who could project multivariate statistics mentally, was cowed by the actual work—hence the CRL. In *Anthropology Today,* published in 1953, John Rowe (29) dealt with technical aids, and for biological anthropology discussed only punchcard methods of data compilation, which Hooton had been using for years. At that time, electronic computers were great vacuum tube dragons, slow and unreliable, though nonetheless effective.

So much for the first half of the century. A very few years, centered on 1950, brought change from many directions. In the 1940s Boas, Hrdlička, and Weidenreich died, with Hooton surviving only until 1954. The same decade saw major books by Dobzhansky (6), Simpson (30), and Mayr (26), followed by the synthesis in evolutionary theory datable to 1947 (24), as well as the recognition of *Australopithecus* as a hominid. In anthropology Boyd (3, 4) and Washburn (33, 34) were particularly effective in exorcising some of the past.

Boyd had a good grasp of the genetic aspects of evolution and was a hard-working blood-group man. The ABO data were well known, but other systems like the Rh were only just coming over the horizon. A mild and pleasant man personally, Boyd nevertheless minced no words in plowing under simpler ideas of multiple races, with their supposedly clear edges and long persistence, as well as any and all ideas of types. His 1950 book was a well-informed and balanced presentation and criticism. His subtitle was "An Introduction to Modern Physical Anthropology"; and perhaps because he came into the field from immunology, and so was not one of the boys, some physical anthropologists were nettled. He was a major force in civilizing the unreconstructed (myself included), even if his effect was not instantaneous.

Washburn has been an acute and articulate critic (33, 34), and had particular effect through helping organize the 1952 Wenner-Gren Conference, and especially the Cold Spring Harbor Symposium of 1950. With clarity of style he pointed out the logical difficulties underlying past conceptions of race, types, and classifications, and emphasized the role of problem- and hypothesis-testing. Later on he was ahead of the curve in fostering studies in primate behavior and molecular biology.

The 1953 publication of *Anthropology Today* is now far in the past. As a sign of how far, we might remember that carbon-14 dating was then brand new.[3] What has been happening to physical anthropology since that distant time? The field is of course far too broad for facile indexing. Nor, unfortunately, is it governed by well-developed theory of its own, as Washburn

[3] Its entrance would have been further delayed if Paul Fejos had not instantly seen the importance of Libby's discovery and called a group of anthropologists together to consider it (7).

apparently hoped it could be—unless such theory is limned in such prevailing controversies as that over the origins of modern *Homo sapiens:* replacement versus regional continuity. Naturally, present work follows the past along the same obvious avenues to the human animal: his primate relatives; his remains, fossil or recent; his living biology.

Weidenreich precipitated the "origin of *sapiens*" argument as early as 1939 (35); however, nobody in 1950 foresaw the avalanche of fossil hominid material coming after 1960, something that is now fodder for a large number of professionals. That development has been important in fusing several main interests in human population history. Not long ago, paleoanthropologists and molecular workers disagreed in dating hominid emergence, and the latter won general acceptance of their solution. In today's work the molecular people, the paleontologists, and the cranial-morphology and distance people are all avidly looking into each other's materials, and symposia show that this has become a natural grouping of workers, informed also by archaeologists and paleoclimatologists.

The other main avenue has been broadened and extended: human biology and variation. This is really Hooton's main interest, freed from its earlier limitations. Blood and serum polymorphisms are perhaps less vigorously pursued than when they were the one new alternative to anthropometry, and before the appearance of DNA analysis.

In population studies especially, Hooton's frustrated hopes are being fulfilled: interaction of anthropology and medicine (in the broad sense). In particular, the Harvard Solomon Islands Project (10), with sizeable teams, has gathered a wealth of biological, anthropological, and medical data, on eight Melanesian communities comprising a spectrum of degrees of contact effects, over a 15-year period of particularly rapid acculturation. (On Bougainville, the team arrived at a reportedly pristine village to be greeted by a boy with a transistor radio clasped to his ear; on Ontong Java, visits spanned the changeover from wood-hulled to fiberglass canoes.) The Project yielded immediate significant findings on the health effects of changing diet, as well as providing a future data base, established at a critical point in time—notably, before the powerful impact of copper mining on Bougainville.

Another well-focused recent example is an assemblage of papers (31), on epidemiological topics from an anthropological perspective, recognizing especially the co-evolution of culture and pathogens, as well as population and individual genetics. Writers specifically recognize that 40 or 50 years ago neither anthropology nor medicine was in a technical or conceptual position to produce such integrated work. Today the vista is one of unlimited progress in the same direction. Again, Hooton felt deeply that this direction was needed, but he could not even envision the shape of things to come.

In general, it seems to me that the 1980s have seen the lifting of physical anthropology to a new plane. The number of workers is greater, of course, but in addition students now serve real apprenticeships in one line or another (see my remarks above about being "trained by Hooton"). Primate behavior has, so to speak, developed stereoscopic vision, now comparing multiple populations of a given species and of many separate species. Molecular workers are doing things I understand only in outline. Analysts of fossils carry interpretations of the functional anatomy of single bones to impressive lengths, often using forbiddingly mathematical methods. Similar studies of cranial form and function also use special measurements and demanding statistical analysis.

As an example of such sophistication let me cite recent studies by Oxnard (27), actually an old primate hand. A longtime form-and-function man, using morphometrics, Oxnard has lately devised continuous numerical scales for behavior, diet, and other aspects of life style, over many primate species, so as to apply factor analysis and canonical variates to the whole. Such analysis elicits fuller detailed information about kinds of adaptation than could be found when simpler categories of locomotion were used; at the same time, almost paradoxically, the analysis appears to sustain the existing taxonomy of species even though species related in this way may arrange themselves differently along adaptive axes. Work like that of Oxnard brings together various approaches—anatomical, behavioral, quantitative—at a new level. Needless to say, such studies could not have been accomplished in the pre-computer period, which constrained even so energetic a man as Schultz to tabulations of osteometric data; he was limited to demonstrating the range of variation in ape species, valuable though that was.

Certain other interests in current physical anthropology, though active, are more parochial: forensics, for example, or paleopathology. These have their special journals, and there is a *Journal of Quantitative Anthropology* to serve the whole field. When Hrdlička retired as editor of the *AJPA* he stipulated that a new series of volumes should be started, to distinguish them from his own legacy. If he could see recent numbers of the journal, containing formidably mathematical work and using ad hoc measurements not covered in the Frankfurt or Monaco Agreements, I feel sure he would be satisfied he had done the right thing.

Foretelling future activity was chancy in 1950 and is chancy now—a warning against pontification. Washburn at that time believed experiment would become important, but it is really not in the nature of physical anthropology to be experimental; non-invasive work like Tuttle's on muscle performance is representative of what can be done. Beyond a safe bet that things will proceed by further intensification of what is being done at the moment, it is highly probable that DNA analysis, nuclear and mitochondrial, still has a great deal of milcagc in it, and might do more to elucidate the history of human populations than blood polymorphisms did.

It is easier to point to historical problems that should find answers. Preoccupation with modern human origins should continue for a while, advancing only by fits and starts (for example, with new datings of the Middle Paleolithic Near Eastern fossil remains). The answers are bound to come eventually, but probably through much painstaking study of existing materials—there are good recent examples—and through piece-by-piece fossil finds, not through a dramatic solution. Mitochondrial Eve has been a stimulus but needs more scrutiny and samples. Long ago Weidenreich, who had plenty of imagination, constantly insisted that fossils, and only fossils, could settle such questions.

Similarly, continuing finds of late Miocene hominoid fossils should close in on an actual ape/australopithecine ancestor, whose emergence may reveal some unprophesied aspects. Here, matters of climate and general environment must invade a problem that was once all anatomy and teeth. These are things that must captivate anthropologists, as dinosaurs do the public. Biomedically important studies like those of human growth, or even reconstructions of language origins, are sure to have a hard time competing with the products of the Rift Valley silts.

Speculation is ultimately idle. More important are questions of education for physical anthropologists. In my own day, the PhD called for a reading knowledge of French and German—now merely pleasant luxuries—and for conversance with Hooton's bone identification. Today, a streamlined course in human and primate anatomy would seem essential. For those who can stomach it, at least a reading knowledge of statistics is needed. This surely means some basic mathematics of the high school level. Unfortunately, streamlined statistics appropriate for anthropologists are hard to come by. I once remarked to M. J. R. Healy, who is good at such things, that the statistics department at Harvard required a year or more of calculus for admission to a course in so relatively simple a method as multivariate analysis, and he was indignant. My own mathematical background does not equip me to understand some of what is being published today, and a higher level of learning is really demanded by computer availability and the opportunities for modern investigation. Like Healy I can see no reason not to teach the appropriate statistics without demanding previous servitude in advanced math.

Further enormous changes over the past 40 years are obvious. At the founding meeting of the AAPA, for example, only a few of the eighty-odd present were physical anthropologists in their own right. And the Association was hardly yet national; it was some years before it met as far west as Pittsburgh—after all, in those train-bound days, baseball's western horizon was St. Louis.

And what about anthropology as a whole? Around the house I happen to have the Harvard course lists for 1891, 1931, and 1991. In the first of these, the complete course offerings of the college occupy 41 pages, devoting the first 20 to ancient and modern languages. On the last page, one paragraph announces a three-year course in Archaeology and Ethnology, for graduate students; no prerequisites—other than knowledge of French, Spanish, elementary chemistry, geology, botany, zoology, drawing, and surveying. By 1931 anthropology had forged ahead, having two and a half pages of course listings all to itself, and three officers of instruction (this was a major department). By 1991 both these numbers had increased tenfold. (These may be ho-hum figures to anthropologists of this moment, but they awe one whose career has spanned the interval.) I do not feel that all this signifies that today's graduating senior is ten times as educated as I am, or, for that matter, as Hooton or Thomas Jefferson were. Still, her or his training and professional focus will certainly be informed and scientific at a much higher level.

As all of anthropology grows, the ties between the biological and cultural domains have become looser—a depressing fact. At least in this country, anthropologists of two generations ago explicitly favored a more unified discipline. Nowadays it is too much to expect individuals to have authority in several fields, but I think we are only the losers when contacts diminish and subjects like sociobiology or population attributes are addressed from *ex parte* positions.

Those earlier physical anthropologists were the frontiersmen, free spirits. Can one imagine a new Hooton, Hrdlička, or Boas? Technology was simple and laboratories hardly deserved the name. Work was individual, and jointly authored papers were almost unknown. Hooton and Dixon, both at Harvard, did not consult over cranial types. Schultz and Straus, both at Johns Hopkins, both working on primates, published jointly only once, as far as I know. Now, collaborative papers are more the rule, although we have not yet reached the point of a paper published this year in *Nature,* on an Einsteinian problem, by 35 coauthors. Charles Darwin, are you listening?

As for myself, I am well content with what I got. Teachers like Boas, Hooton, and Dixon were making their own anthropology, on the grand scale, and we had the luxury of a general view of the field. As an undergraduate I was surprised and delighted to hear so much from Dixon about China, India, Mesopotamia, Majapahit Indonesia, the Incas, and others, and I agree that we should all know a great deal more about such cultural springs. And we should all know a great deal more about Western civilization. The current drive to dilute study of the latter, by which the nation lives, with a sort of cultural bazaar, does not seem to me to be the anthropological perspective at all. We learn about humanity from other cultures but we do not live by them.

In obedience to anthropology's mission, professionalism has led to some particularized and recondite interests, certain of them downright peculiar, and I wonder if some of this baggage will still seem rewarding to today's graduate student 20 or 30 years from now. I had a couple of such courses, in and out of anthropology, but I still benefit and take pleasure from most of my work. I cannot imagine a professional life that would have been more congenial, or one that would have given me more admired and amusing friends.

It is pleasant to be retired. The news from anthropological front lines arrives in ever greater quantities. The discipline of teaching obliges you to try to present important matters in well-rounded, balanced fashion, even as you make your own views known. A nice ideal, but now I can lean back, read without having to revise lecture notes, and tell myself (in private) just what I think of things. It's sort of like spending capital, and why not?

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