

The Legacy of Serological Studies in American Physical Anthropology

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ABSTRACT – Serological data have been used to address anthropological problems since the turn of the century. These were predominantly problems of two kinds in anthropological systematics: the relations of human populations to one another (racial serology), and the relations of primate species to one another (systematic serology). Though they were the locus of considerable debate about the relative merits of ‘genetic’ versus ‘traditional’ data, the serological work had little lasting impact in the field. I attribute this to the fact that the research was carried out largely externally to anthropology, and often interpreted in facile manners. To a large extent the history of this research has been ignored or rewritten following the development of ‘molecular anthropology’ in the 1960s. To some extent, however, contemporary genetic research in anthropology replays aspects of the serological era.

Introduction

To understand the impact of the blood group studies on physical anthropology, it is necessary to understand the nature of the discipline. Anthropology is largely a twentieth-century discipline, whose modern origins in American academia date from the ascendancy of Franz Boas (1858-1942) at Columbia University. American *physical* anthropology in the first part of this century, however, was intellectually dominated by two other figures: Aleš Hrdlička (1869-1943) of the Smithsonian Institution, and Earnest Hooton (1887-1954) of Harvard. Hrdlička set forth the main goals of physical anthropology in 1908.

First, ‘the study of the normal white man living under average conditions, and ... the complete range of his variations’. Second, ‘detailed knowledge of the structure, function, and chemical composition – with their variations – in the primates’.

Third, ‘the determination of development and variation in man’s structure, and also as far as possible in other organic qualities – particularly those of a chemical nature – in relation to time’.

And fourth, 'the human races and their subdivisions'.¹ Operationally the focus of physical anthropology was (and is) the 'physical' nature of the human species, in which the major subjects of analysis are living humans, or the skeletons of their ancestors. Thus, systematics is central to physical anthropology – both in establishing the relationships of human populations to one another, and the relationships of primate species to one another. The expectation is that hereditary continuity exists, and is discernible, between the physical appearances of human populations, and as well between those of humans and the other primates. And the expectation is often met.

Appearances, however, can also be deceiving; and biological anthropology found itself confronted early on by the superficial similarities resulting from convergent evolution, facultative adaptations to common circumstances, and the unmercifully complex and discordant distribution of similarities among the peoples of the world.

The study of blood groups seemed to afford a way around these problems. Not subjected as flagrantly to the selective regimes imposed by the general environment as were the outward anatomical and physiological characteristics, the blood groups appeared to be inherited in simple fashion. They might thus present a simple reflection of human biological history, augmenting the more familiar skeletal data with Hrdlička's 'other organic qualities – particularly those of a chemical nature'.

The First Wave of Racial Serology

Today we use ABO as a paradigmatic demonstration of the absence of discrete racial groupings among the aboriginal populations of the world. Populations differ from one another in a quantitative, gradualistic manner (in fact paralleling their anatomical similarities, appreciated as far back as Buffon and Blumenbach in the eighteenth century). Curiously, though, that clinal pattern was not immediately put forth as inherent in these data by the earliest exponents of blood-group data for physical anthropology.² What they presented as results, rather, was what folk wisdom and contemporary anthropology had led them to expect to find: qualitatively different major groupings of people, or races. Thus, the Hirszfelds segregated the world into

¹ A. Hrdlička, 'Physical Anthropology and Its Aims', *Science*, 28 (1908), 41-42. The first and fourth have since largely been merged and redefined as human variation; the second, primate biology; and the third, paleoanthropology.

² J. Huxley, 'Clines: An Auxiliary Taxonomic Principle', *Nature*, 142 (1938), 219-220.

essentially 'white' and 'other' when they identified three groups (European, Intermediate, and Asio-African), using ABO.³

The number of races quickly doubled in the hands of Reuben Ottenberg, who still used the Hirszfelds' 'biochemical index' (though calling it a 'racial index') to discern now: European, Intermediate, Hunan, Indo-Manchurian, Afro-South-Asiatic, and Pacific American. But by the following year, Laurence Snyder could argue that such indices were inappropriate for the classification of human diversity based on these blood groups. Rather, he maintained, the direct frequencies of the alleles should be used. This fundamental difference in analysis, however, afforded only a trivial modification to Ottenberg's system, yielding: European, Intermediate, Hunan, Indomanchurian, Africo-Malaysian, Pacific-American, and Australian.

The proliferation of data, however, led to a problem. Random similarities between distant populations obliged Snyder to place them in the same group. Thus, Poland ended up with Hunan, China. Harvard's Hooton was openly skeptical about the utility of such unlikely associations, and consequently of the value of the serological studies in racial classifications altogether.⁴

Nevertheless, as his students set out to collect biological data on the world's populations, they began as well to collect blood group data.⁵ Even cultural anthropologists took note: Alfred Kroeber's bivariate analysis reinforced the unclarity of racial associations from these data, ending with a call for 'new data from more populations' and 'restraint from simplistic interpretations'. The 'simplistic interpretations' included the assumption, popular since the Hirszfelds, that human biohistory had involved ancient invasions by 'pure' A and B races, imposing themselves on an indigenous O race. Kroeber's alternative suggestion that multiple independent mutations from O to A might be generating some of the observed distributional patterns prompted a critical response from William C. Boyd, who noted what immunologists themselves had been ignoring: that the A, B, and O

³ L. Hirszfeld and H. Hirszfeld, 'Essai d'Application des Méthodes Sérologiques au Problème des Races', *L'Anthropologie*, 19 (1918-1919), 505-537. L. Hirszfeld and H. Hirszfeld, 'Serological Differences Between the Blood of Different Races', *Lancet*, 2 (1919), 675-679, 18 October.

⁴ R. Ottenberg, 'A Classification of Human Races Based on Geographic Distribution of the Blood Groups', *Journal of the American Medical Association*, 84 (1925), 1393-1395. L.H. Snyder, 'Human Blood Groups: Their Inheritance and Racial Significance', *American Journal of Physical Anthropology*, 9 (1926), 233-263. A.A. Mendes-Corrêa, 'Sur les Prétendues "Races" Sérologiques', *L'Anthropologie*, 36 (1926), 437-445. M. Young, 'The Problem of the Racial Significance of the Blood Groups', *Man*, 28 (1928) 153-159, 171-176. E.A. Hooton, *Up from the Ape*, New York: Macmillan, 1931: 490.

⁵ C.S. Coon, 'Tribes of the Rif', *Harvard African Studies*, 9 (1931). C.S. Coon, *Adventures and Discoveries*, Englewood Cliffs, NJ: Prentice-Hall, 1981.

alleles might in fact be older than the human species itself, since they were present in the apes.⁶

Boyd took the next step in his important paper of 1940: invoking the new theories of microevolutionary population genetics to account for the anthropological patterns. Boyd's argument, though, was still self-interestedly evangelical. While he accused the anthropologists of simple ignorance in their standoffish attitude to the blood-group data, he was quite uncharitable in failing to blame its lukewarm reception within anthropology on the serologists' simplemindedly grandiose interpretations of their own data. Boyd's tirades, it may be noted, were directed most specifically at the unbelieving European physical anthropologists; Hooton and Hrdlička were diplomatically spared his criticisms.⁷

By World War Two, then, two things appeared to have been settled in the anthropological community concerning the use of serological data in the human species. First, in the panoply of biological data being collected on the peoples of the world, these data were worth collecting as well; and second, they did not resolve the nature of the fundamental divisions of the human species any more clearly than other data – in spite of frequent assertions to the contrary by practitioner/advocates.

One point, however, bears emphasis: the first generation of serological data were being collected largely independently of anthropology and were implicitly (and often explicitly) taken to supersede anthropology. A telling example involves the published accounts of a paper given by J.B.S. Haldane at the Royal Anthropological Society on April 22, 1932. Haldane began by observing 'the necessity for a very critical attitude toward any attempt to deduce racial origins from the data of physical anthropology'. He then proceeded to review racial origins from the genetic (serological) perspective, which was 'likely to yield more valuable results than have so far been achieved in that elusive science'.⁸ In contrasting the relative merits of traditional anthropology and the new-fangled serological work, *The British Medical Journal* tellingly omitted

⁶ A.L. Kroeber, 'Blood-Group Classification', *American Journal of Physical Anthropology*, 18 (1934), 377-393. L.C. Wyman and W.C. Boyd, 'Human Blood Groups and Anthropology', *American Anthropologist*, 37 (1935), 181-200. K. Landsdeiner and C.P. Miller, Jr., 'Serological Observations on the Relationship of the Bloods of Man and the Anthropoid Apes', *Science*, 61 (1925), 492-493. The blood groups are in fact found throughout the primates, and A is thought to be ancestral.

⁷ W.C. Boyd, 'Critique of Methods of Classifying Mankind', *American Journal of Physical Anthropology*, 27 (1940), 333-364. Interestingly, Hooton's *Up from the Ape* is listed in the references of the paper, but uncited in the body of the paper.

⁸ Anonymous, 'The Blood Groups in Genetics and Anthropology', *British Medical Journal*, 2 (1932), 26-27. See J.B.S. Haldane, 'The Blood-Group Frequencies of European Peoples, and Racial Origins', *Human Biology*, 12 (1940), 457-480. Contrasted with anthropological data, Haldane would avow, 'The blood-groups on the other hand give information of a more fundamental nature on racial structure, just as do the palaeozoic rocks on geological structure' (p. 477).

Grafton Elliot Smith's critical remonstrations that 'anthropologists did not yet know how to fit these tentative new facts into existing ideas of race', and 'it would be rash to try to formulate any comprehensive interpretation at present'.⁹

The fact is that whatever blood groups had to say about racial origins was not at all clear. Though the work undoubtedly had some implications for anthropology, as anthropologists realized, its application amounted to little more than high-tech science in the service of folk wisdom. The projection that anthropology would someday be superseded by serology was certainly not justified by contemporary data and interpretations, and was, at best, wishful.

Systematic Serology

In parallel to the racial uses of serological data, another line of research developed early in the century. Cross-species comparisons seemed to have properties opposite to the blood-group data – these tests were complex to generate, but reasonably straightforward to interpret. As a result, there was a wide variation in quality of data, a situation that did not seem to exist in the relatively simple blood-type testing.

Following G.H.F. Nuttall's 1904 monograph, the precipitation reactions of the bloods of different species were quickly appreciated to reflect their relationships in the Linnaean hierarchy. Although widely cited for its consistency with Darwinism in the era of the Scopes trial,¹⁰ it had little impact upon anthropology. Essentially Nuttall's work provided no more than an additional 'proof' of the evolutionary kinship of humans to apes, for those who felt it necessary. In Hooton's 1931 textbook, *Up from the Ape*, Nuttall is mentioned (though his name is misspelled as 'Nuttal'); but for the second edition in 1946, Hooton chose instead to reference more recent research from Germany (von Krogh, below). Ashley Montagu's rival physical anthropology text of 1945 does not mention the primate serological work at all.¹¹

The problem, which would be reiterated in succeeding generations,

⁹ Anonymous, 'Blood Groups', *The Lancet*, 7 (1932), 1024-1025. Anonymous, 'Discussion of Human Blood Groups', *Man*, 32 (1932) 142-143.

¹⁰ G.H.F. Nuttall. *Blood Immunity and Blood Relationship*, Cambridge: Cambridge University Press, 1904. L.M. Hussey, 'The Blood of the Primates', *American Mercury*, 9 (1926), 319-320.

¹¹ E.A. Hooton (footnote 4), p. 44. His major source for discussing the blood of primates was the three-page review in C.F. Sonntag, *The Morphology and Evolution of the Apes and Man*, London: John Bale, 1924: 254-256. E.A. Hooton, *Up from the Ape*, 2nd ed., New York: Macmillan, 1946: 44-45. G.G. MacCurdy, *The Coming of Man*, New York: The University Society, 1935. M.F.A. Montagu, *Introduction to Physical Anthropology*, Springfield, IL: Charles C. Thomas, 1945.

involved how to treat discrepancies between serological and anatomical data. The primate anatomist Solly Zuckerman (later, Lord Zuckerman) had been critical of Nuttall's removal of the lemurs from the primates, and devoted some space to that problem in his 1933 book on primate biology. He concluded that since the tarsier also failed Nuttall's precipitin test, and was certainly a primate, 'it is unsafe to lay so much taxonomic weight on this test'.¹²

The comparative serologists self-consciously recognized the quality-control problem they had, articulated strongly in Alan Boyden's 1942 critical review article.¹³ Not only did this research require complex titrated dilutions of antigens and antibodies, but there was often a lack of reciprocity in the results obtained, which made them particularly difficult to interpret.

As shown in Table 1, von Krogh's work¹⁴ does not easily permit the (correct) inference that humans and chimpanzees are most closely related: though human-chimpanzee reacted more strongly than human-orangutan, chimpanzee-human did not react as strongly as chimpanzee-orangutan! Further, the orangutan serum reacted poorly with human and chimpanzee, and no better than with baboon serum. Von Krogh developed an *ad hoc* theory to account for the lack of reciprocity in his results – that humans had evolved new serum proteins above and beyond those of the other primates, such that human serum would match nearly all of a chimpanzee's serum, but a chimpanzee's serum would match only a small part of a human's. Nevertheless, he concluded with a phylogeny equivalent to that of Sonntag (footnote 11, p. 319), and that we would recognize today as essentially accurate (Fig. 1).

TABLE 1. Serological identities among primates, reflecting the percentage of similarity to the 'self' reaction, from von Krogh (1937).

Human antisera		Chimpanzee antisera		Orangutan antisera	
Human-Human	100	Chimpanzee-Chimpanzee	100	Orang.-Orangutan	100
Human-Chimpanzee	84.5	Chimpanzee-Human	62.9	Orangutan-Human	38.8
Human-Orangutan	70.6	Chimpanzee-Orangutan	84.9	Orang.-Chimpanzee	44.3
Human-Baboon	64.8	Chimpanzee-Baboon	72.4	Orangutan-Baboon	47.5

Additionally, Zuckerman had applied serological tests and confirmed Nuttall's conclusion that prosimians did not actually group

¹² S. Zuckerman, *Functional Affinities of Man, Monkeys, and Apes*, New York: Harcourt, Brace, 1933: 51-53.

¹³ A. Boyden, 'Systematic Serology: A Critical Appreciation', *Physiological Zoology*, 15 (1942), 109-145.

¹⁴ Chr. v. Krogh, 'Serologische Untersuchungen über die Stammesgeschichtliche Stellung Einiger Primaten', *Anthropologischer Anzeiger*, 13 (1937), 240-247.

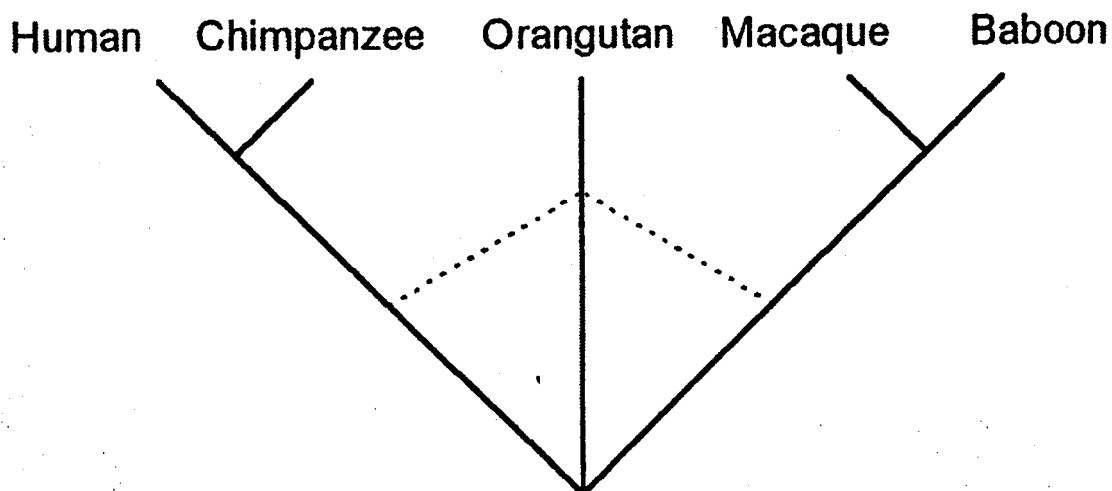


Fig. 1. Von Krogh was unable to position the orangutan with certainty, but recognized the close kinship between human and chimpanzee from his data.

with primates. His interpretation was that the serological data were problematic, while Boyden's, in turn, was that perhaps prosimians were *indeed* not primates.¹⁵ Zuckerman's curious results on catarrhine primate serum were chalked up to simple incompetence by Boyden.

At any rate, there seemed to be little of a revolutionary nature in this work, and as Boyden himself sanguinely concluded:

[C]omparative serology, like comparative morphology, is no simple guide to relationship. The very complexity of the problem demands the use of all possibly pertinent data. The data of systematic serology, where comparable methods are used, are as valid as those of systematic morphology, and the two methods of analysis should be considered complementary to each other.¹⁶

Molecules versus Morphology

The abortive attempt to establish racial distinctions on the basis of blood groups, together with the ambiguous results on primate relationships from serology, combined to establish serological research as marginal to physical anthropology by 1945. Although its relevance was hardly in dispute, and the data were certainly worthy of collection, it did not seem to be saying much that either wasn't already known, or that wasn't obviously inaccurate.

¹⁵ S. Zuckerman, 'The Primate Affinities of the Blood Serum of the Lemurs *Perodicticus* and *Galago*', *Proceedings of the Zoological Society of London*, 11 (1934), 906-613. A. Boyden, (footnote 10), p. 135.

¹⁶ A. Boyden (footnote 10), pp. 141-142.

Somewhat surprisingly, then, the immediate post-war years saw physical anthropology as the locus of a heated exchange over whether serological data should supersede other data, given that the former were, if crude, nevertheless genetic.¹⁷

In his introductory textbook, Ashley Montagu adapted an illustration originally devised by Hooton, in which various human populations were scattered around the perimeter of a circle, like the hours on a clock-face (Fig. 2). Montagu followed Hooton in placing the natives of Australia and Papua New Guinea between the natives of India on the one hand, and central Africa, on the other – calling them ‘brown-black’. On the basis, however, of ‘the virtual absence of Subgroup A₂ and the Rh-negative type’ serologist Alexander Wiener wrote to *Science* to demand that the Australians and Papua New Guineans be reclassified as ‘Mongoloid’, and that ‘the diagram should therefore be revised accordingly’.¹⁸

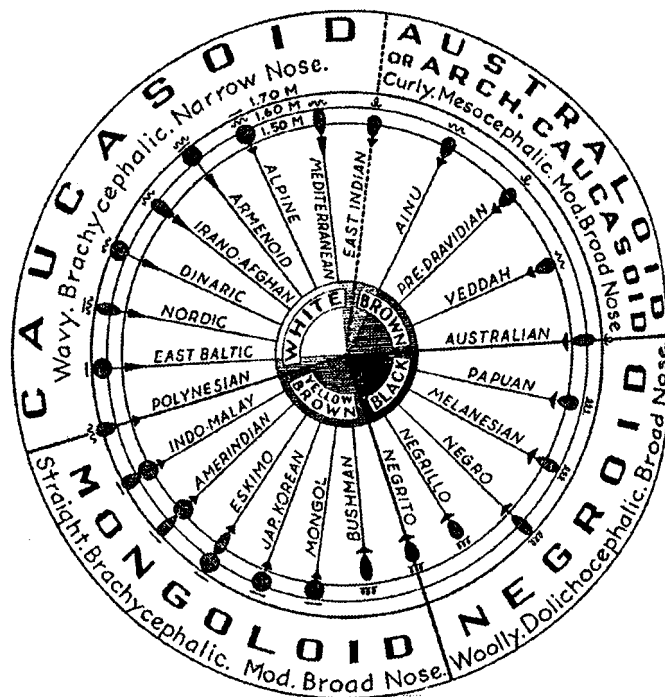


Fig. 2. The racial placement of the aboriginal inhabitants of Australia and Papua New Guinea, here based on anatomical data, seemed to be contradicted by serological data. After Montagu (1945).

¹⁷ This in fact foreshadowed later debates within general evolutionary biology. See C. Patterson (ed.), *Molecules and Morphology in Evolution: Conflict or Compromise?*, New York: Cambridge University Press, 1987.

¹⁸ A.S. Wiener, ‘Blood Group Factors and Racial Relationships’, *Science*, 103 (1946), 147.

The turnaround was striking, for the matter had apparently been settled in the field decades earlier: serological data often seemed to track relationships of populations, and sometimes didn't. Therefore they had no claim to authority. Montagu responded indignantly:

That Dr. Wiener would consider, as he appears to do, blood group factors alone sufficient to indicate the closeness of the genetic relationship between various groups of man is to me nothing short of astonishing. I had thought that all students of the subject, including Dr. Wiener, were agreed that the inferences drawn from such data could, at most, be regarded only as suggestive.¹⁹

Montagu ended with a quotation to the same effect from Wiener himself, which did not forestall another salvo from the serologist. Yet, racial serology had quietly reinvented itself in the intervening decades. Where they had been unsuccessful in *extracting* basic divisions of the human species from their data, serologists could readily *impose* such divisions upon their data, and thus 'confirm the broad separation of mankind into three divisions' – namely, Caucasoid, Negroid and Mongoloid.²⁰

Armed with the argument that blood-groups could indeed yield the 'right' answer in racial studies, Wiener and Boyd proceeded to launch a rhetorical attack upon classical racial anthropology. While Wiener challenged Montagu, Boyd equally confidently proclaimed the general superiority of serological data in anthropology, at least to those with short memories. Thus, '...it would seem hardly too much to say that serology (or rather, genetics), is destined to oust craniometry and anthropometry as the main tool of racial anthropology'.

Needless to say, this did not sit well with mainstream anthropologists. The circularity of Boyd's racial groupings was the easiest target, to one critic:

The question immediately arises: How does he know which are the Negroid and which are the Mongoloid groups if his classification is based on blood groups alone? It seems apparent that Boyd has picked his samples by utilizing the traditional classification of races, which he condemns, and has used blood groups to substantiate the differences between them.²¹

¹⁹ M.F. Ashley Montagu, 'Blood Group Factors and Ethnic Relationships', *Science*, 103 (1946), 284.

²⁰ A.S. Wiener, 'Rh-Hr Blood Types in Anthropology', *Yearbook of Physical Anthropology*, 1 (1945), 212-213. A.S. Wiener, 'Blood Grouping Tests in Anthropology', *American Journal of Physical Anthropology*, 6 (1948) 236-237.

²¹ C.W. Rowe, 'Genetics vs. Physical Anthropology in Determining Racial Types', *Southwestern Journal of Anthropology*, 6 (1950), 199. Boyd's full-length treatment became a classic in the literature of racial anthropology, *Genetics and the Races of Man*, Boston: Little Brown, 1950. However, the asserted dominion of serology was poorly received. See H.H. Strandskov and S.L. Washburn, 'Editorial: Genetics and Physical Anthropology', *American Journal of Physical Anthropology*, 9 (1951), 261-263; T.D. Stewart, 'Objectivity in Race Classifications', *American Journal of Physical Anthropology*, 9 (1951), 470-472; J.B. Birdsell, 'On Various Levels of Objectivity in Genetical Anthropology', *American Journal of Physical Anthropology*, 10 (1952), 355-262.

Yet even the classical tripartite division of the human species did not last for long serologically. By this time Boyd had in fact already doubled the number of races detected serologically by Wiener.²² Just as the anatomy revealed, the closer one looked, the more races one saw. Boyd now distinguished 'Early-European' (represented by the Basques); European (Caucasoid); African (Negroid); Asiatic (Mongoloid); Pacific; and Australoid.²³

He subsequently added a seventh, Indo-Dravidian. And in Boyd's last major exposition of racial classification in 1963, he proposed seven 'groups' comprising 13 races.²⁴ Apparently the serological data provided divisions and classifications which were no more stable than those from classical data.

By this time, however, the ontology of race had already come into question, as had the centrality of racial classifications in physical anthropology. This was precipitated by two publications: Carleton Coon's *The Origin of Races* (more specifically, the reaction against it), and Frank Livingstone's brief essay on the clinal, non-racial, nature of human variation.²⁵ Blood group data were now being marshalled as fundamentally *undermining* the race concept – thus Boyd's entire approach was suddenly fundamentally archaic. Once again, racial serology, which had always been intellectually marginal to physical anthropology, was marginalized.

On the other front, however, systematic serology was undergoing a renaissance. With a simplified technique that permitted qualitative comparisons of two pairs of species, Morris Goodman was able to demonstrate that chimpanzees and gorillas were more similar serologically to humans than to orangutans, and thus the category 'apes' was not a natural category.²⁶ This conclusion would be accepted with little opposition, for example by scholars as 'traditional' as G.G. Simpson.²⁷

²² A.S. Wiener (footnote 17).

²³ W.C. Boyd, 'Systematics, Evolution, and Anthropology in the Light of Immunology', *Quarterly Review of Biology*, 24 (1949), 102-108; W.C. Boyd (footnote 18).

²⁴ W.C. Boyd, 'Genetics and the Human Race', *Science* 140 (1963), 1057-1065.

²⁵ C.S. Coon, *The Origin of Races*, New York: Knopf, 1962; F.B. Livingstone, 'On the Non-Existence of Human Races', *Current Anthropology*, 3 (1963), 279-281; J. Marks, *Human Biodiversity*, New York: Aldine de Gruyter, 1995.

²⁶ M. Goodman, 'Evolution of The Immunologic Species Specificity of Human Serum Proteins', *Human Biology*, 34 (1962), 104-150; M. Goodman, 'Immunochemistry of the Primates And Primate Evolution', *Annals of the New York Academy of Sciences*, 102 (1962), 219-234; M. Goodman, 'Serological Analysis of the Systematics of Recent Hominoids', *Human Biology*, 35 (1963), 377-424. M. Goodman and G.W. Moore, 'Immunodiffusion Systematics of the Primates. I. The Catarrhini', *Systematic Zoology*, 20 (1971), 19-62.

²⁷ G.G. Simpson, 'The Meaning of Taxonomic Statements'. In: S.L. Washburn (ed.), *Classification and Human Evolution*, Chicago: Aldine, 1963: 1-31.

On the other hand, Goodman's phylogenetic work was accompanied by a proposal for reclassifying the apes on that basis. This presaged the school of thought which came to be known as cladism – but which did not exist at the time. Goodman's proposal was based simply on the fact that he did not understand the principles of contemporary systematics, as pointed out by Simpson. The reclassification was roundly rejected.²⁸

Nevertheless, with the serological work augmented by newly-emerging protein sequence data in the early 1960s, chemist Emile Zuckerkandl proclaimed the origin of a new field, 'molecular anthropology'.²⁹ The new work, of course, retained many similarities to the old, without citing it – narrowness of scope, arrogance in interpretation, and marginality to physical anthropology. These properties would be noted, criticized, and challenged,³⁰ but ultimately to a large extent validated by the widely-recounted triumph of molecular anthropology, in the hands of Vincent Sarich and Allan Wilson.³¹

Paleontologist Elwyn Simons and his protégé David Pilbeam in the 1960s were promoting the remains of *Ramapithecus*, which bore noteworthy similarities to the human dentition, as a human ancestor. Since these remains were up to 14 million years old in the fossil record, it stood to reason that the human line had been distinct from the ape lines for at least that long.³² Sherwood Washburn, on the other hand, had emphasized the similarity between human and ape to the extent that he could suggest the lines had been separate on the order of only one million years.³³

Washburn's protégé, Vincent Sarich, applied refinements in quantification of immunological results and the use of minute

²⁸ G.G. Simpson (footnote 24); G.G. Simpson, 'Remarks on Immunology and Catarrhine Classification', *Systematic Zoology*, 20 (1971), 369-370; S.L. Washburn and I. DeVore, 'An Interview With Sherwood Washburn', *Current Anthropology*, 33 (1992), 421.

²⁹ E. Zuckerkandl, 'Perspectives in Molecular Anthropology', In: S.L. Washburn (ed.) *Classification and Human Evolution*, Chicago: Aldine, 1963: 243-272.

³⁰ G.G. Simpson, 'Organisms and Molecules in Evolution', *Science*, 146 (1964), 1535-1538. J. Buettner-Janusch and R.L. Hill, 'Molecules and Monkeys', *Science*, 147 (1965), 836-842.

³¹ J. Gribbin and J. Cheraf, *The Monkey Puzzle*, New York: Pantheon, 1982; R. Lewin, *Bones of Contention*, New York: Simon and Schuster, 1987; M.H. Brown, *The Search for Eve*, New York: Harper and Row, 1990.

³² E.L. Simons, 'The Phyletic Position of *Ramapithecus*', *Postilla*, 57 (1961), 1-9. E.L. Simons and D.R. Pilbeam, 'Preliminary Revision of the Dryopithecinae (Pongidae, Anthropeidea)', *Folia Primatologica*, 3 (1965), 81-152. D.R. Pilbeam, 'Tertiary Pongidae of East Africa: Evolutionary Relationships and Taxonomy', *Bulletin of the Peabody Museum of Natural History* (Yale University), 31 (1965).

³³ S.L. Washburn, 'Tools and Human Evolution', *Scientific American*, 203[3] (1960), 65. S.L. Washburn, 'Behavior and Human Evolution', In: S.L. Washburn (ed.), *Classification and Human Evolution*, Chicago: Aldine, 1963: 202.

quantities of blood, and with biochemist Allan Wilson, published a series of papers in 1966-8 detailing the analysis of these data. Unlike anatomical evolution, there were theoretical reasons to expect biochemical evolution to proceed in a clocklike fashion, and to be less responsive to environmental exigencies than anatomy. This seemed to be borne out empirically as well: two species which separated from a third at the same time, generally appeared to have undergone the same amount of biochemical change.³⁴

Given the amount of immunological distance separating human and chimpanzee, then, they calculated the age of the distinct human lineage at only about 4 million years.³⁵ Thus, as there was no separate human lineage 14 million years ago, *Ramapithecus* could not be a hominid, Sarich would argue, 'no matter what it looks like' (Figure 3).³⁶

The roles were now reversed: serology had caught the paleontologists over-interpreting their data.

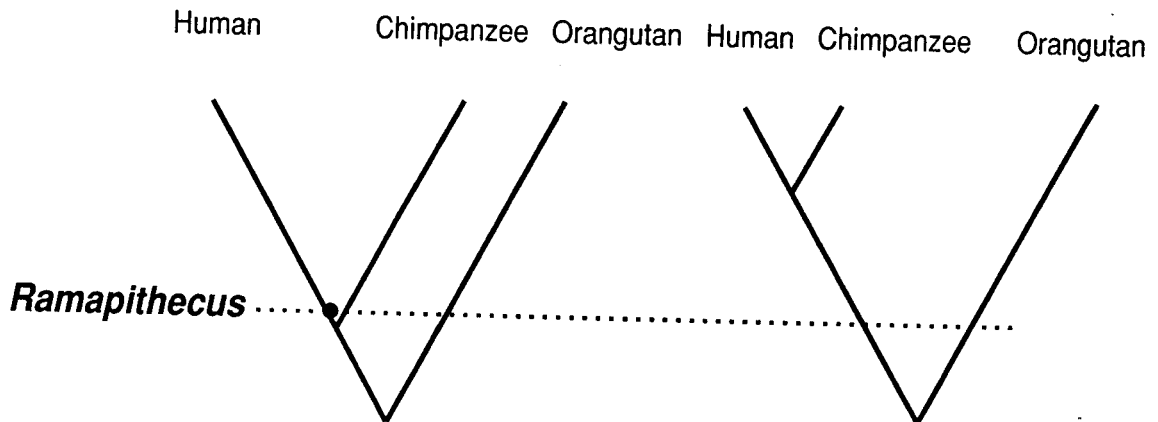


Fig. 3. (Left) Pilbeam's view of *Ramapithecus* as a human ancestor; (Right) According to Sarich, there was no hominid lineage at the time. We now see the fossil as closely allied to the orangutan.

³⁴ V.M. Sarich and A.C. Wilson, 'Quantitative Immunochemistry and the Evolution of Primate Albumins: Micro-Complement Fixation', *Science*, 154 (1966), 1563-1566. V.M. Sarich and A.C. Wilson, 'Rates of Albumin Evolution in Primates', *Proceedings of the National Academy of Sciences, USA*, 58 (1967), 142-148, 1967. V.M. Sarich, 'Appendix: Retrospective on Hominoid Macromolecular Systematics', In: R.L. Ciochon and R.S. Corruccini (eds), *New Interpretations of Ape and Human Ancestry*, New York: Plenum, 1983: 137-150.

³⁵ V.M. Sarich and A.C. Wilson, 'Immunological Time Scale for Hominid Evolution', *Science*, 158 (1967), 1200-1202.

³⁶ V.M. Sarich, 'Primate Systematics With Special Reference to Old World Monkeys: A Protein Perspective', In: J.R. Napier and P.H. Napier (eds), *Old World Monkeys: Evolution, Systematics, and Behavior*, New York: Academic Press, 1970: 199, emphasis in original.

The Rebirth of Serology as Molecular Anthropology

The 1960s also saw one of the most profound revolutions in the history of science, the emergence of molecular biology, and the reduction of heredity to chemistry. Thus, the chemist Emile Zuckerkandl could confidently christen the field of 'molecular anthropology' on a model of what was occurring in biology. Yet, molecular anthropology already existed: anthropologists had been retrieving and analyzing blood samples from the field for decades. If traditional anthropology had not been superseded by genetics (as Boyd had predicted), it had nevertheless integrated genetics effectively, largely through the work of Hooton's students Joseph Birdsell, Frederick Hulse, Gabriel Lasker, Alice Brues, and James Spuhler.

What was new and different about Zuckerkandl's 'molecular anthropology' was actually familiar from the beginnings of the earliest serological research. It was carried out institutionally external to anthropology, by scientists untrained as anthropologists, and thereby unconstrained by anthropological knowledge. The familiar insinuations that anatomy is obsolete, and that prior knowledge is now superseded, were quickly reiterated. To that extent, it promised all the dubious benefits of dilettantism apparent in the serological work of the 1920s, which had left anthropologists suspicious of the application of genetic data, and of the enthusiasm of geneticists.

Especially interesting in the present regard is the historiography of molecular anthropology, which generally presents a six-decade hiatus between the 1904 work of Nuttall and the 1962 work of Goodman. Although genetic data were acknowledged as valuable, and possessed a long history in physical anthropology, Goodman's work in the early 1960s is presented as a virtual apple-falling-on-the-head myth.³⁷ And thus, a popular exposition of the history of physical anthropology gives us two paragraphs on Nuttall, and a segue: 'Nothing much happened for the next sixty years,...'³⁸

Although 'anthropological genetics' of the human species was now well established as a subfield of physical anthropology by the 1960s, its focus was microevolution and demography. As major qualitative divisions of the human species were now starting to be acknowledged as illusory, the legacy of William Boyd's racial serology would thus again find itself intellectually tangential to the main lines of thought in physical anthropology.

³⁷ M. Goodman and J.E. Cronin, 'Molecular Anthropology', In: F. Spencer (ed.), *A History of American Physical Anthropology, 1930-1980*, New York: Academic Press, 1982: 105-146.

³⁸ R. Lewin (footnote 28), p. 106.

An example of this tangentiality is easily seen in the contemporary literature (Fig. 4). The 'mitochondrial Eve' work showed that, when not initially assigned to racial groups, the DNAs of Africans subsumed that of Asians and Europeans. In other words, genetically Asians and Europeans are a subset of Africans.³⁹ It thus makes little sense to ask whether Africans are more *closely related* to Europeans or to Asians, if both of the latter are *subsumed* within the African gene pool. Nevertheless, population geneticists still commonly assign individual genotypes *a priori* into races, and ask their computers about the genetic relationships among the races they have constructed.⁴⁰

The same problem also arises at a more 'micro' level. For example, in a recent genetic compendium, the southern African Khoisan are discussed as a biological, linguistic, and cultural unit. When split and analyzed separately, however, the authors remark that '[c]learly, the Khoi are closer to all Bantu groups than to the San'. This, of course, would immediately suggest that the category 'Khoisan' is a cultural construct, and not a natural category of genetic analysis at all – a point apparently lost on the geneticists.⁴¹

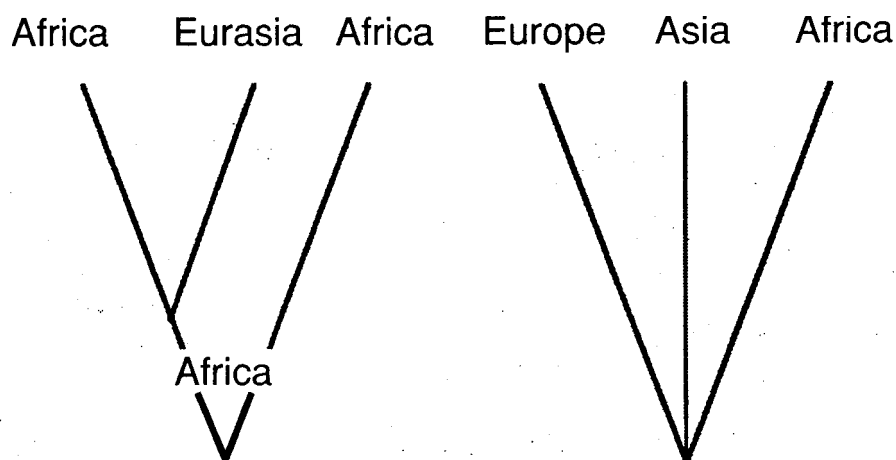


Fig. 4. If all humans are a subset of Africans (left), then artificially dividing the world into continental groups constructs racial categories (right), rather than identifying them in nature.

³⁹ R.L. Cann, M. Stoneking and A.C. Wilson, 'Mitochondrial DNA and Human Evolution', *Nature*, 325 (1987), 31-36. D.A. Merriweather, A.G. Clark, S.W. Ballinger, T.G. Schurr, H. Soodyall, T. Jenkins, S.T. Sherry and D.C. Wallace, 'The Structure of Human Mitochondrial DNA Variation', *Journal of Molecular Evolution*, 33 (1992), 543-555.

⁴⁰ M. Nei and A.K. Roychoudhury, 'Genetic Variation Within and Between the Three Major Races of Man, Caucasoids, Negroids, and Mongoloids', *American Journal of Human Genetics*, 26 (1974), 421-443. L.L. Cavalli-Sforza, A. Piazza, P. Menozzi and J. Mountain, 'Reconstruction of Human Evolution: Bringing Together Genetic, Archaeological, and Linguistic Data', *Proceedings of the National Academy of Sciences, USA*, 85 (1988), 6002-6006.

⁴¹ L.L. Cavalli-Sforza, P. Menozzi and A. Piazza, *The History and Geography of Human Genes*, Princeton, NJ: Princeton University Press, 1994: 176.

One perennially popular, if simplistic, explanation for genetic patterns is to find a known historical migration and to assign it as the cause of a particular genetic pattern. Although this is speculative and thereby very difficult to test empirically, it is a line of thought reaching directly from Candela's explanation for type B blood in Europe (Asiatic invasions in the Middle Ages) to Cavalli-Sforza's conjecture about the effect of the spread of agriculturists upon the European gene pool.⁴²

In some cases, the major historical processes invoked to explain contemporary patterns of diversity are essentially the same as the most quickly discredited ideas of early serologists: mass invasions of pure and qualitatively distinct primordial races imposing their gene pool upon others.⁴³

The Human Genome Diversity Project has now been proposed in part to address such questions.⁴⁴ In a formulation external to anthropology,⁴⁵ a group of prominent molecular and population geneticists sought federal funding to establish a 'gene museum' of the exotic peoples of the world. Approaching human diversity with what appeared to be a threatening composite of archaic ideas, colonialist attitudes, and an exploitative agenda, the project was initially greeted with considerable ambivalence in the anthropological community.⁴⁶ Indeed, it seems to fit snugly into the mold established earlier in the century by racial serology – high technology wedded to folk concepts about human biological diversity and conceptually antiquated approaches

⁴² P.R. Candela, 'The Introduction of Blood Group B Into Europe', *Human Biology*, 14 (1942), 413-443. The reasoning here was a bit more sophisticated than the German attempts to map biochemical 'races' directly on to invasions by distinct morphotypes (see P. Mazumdar, 'Blood and Soil: The Serology of the Aryan Racial State', *Bulletin of the History of Medicine*, 64 (1990), 187-219). It is, however, directly ancestral to the correlational arguments in some modern literature. P. Menozzi, A. Piazza and L.L. Cavalli-Sforza, 'Synthetic Maps of Human Gene Frequencies in Europeans', *Science*, 201 (1978), 786-792. L. Roberts, 'Using Genes to Track Down Indo-European Migrations', *Science*, 257 (1992), 1346. L.L. Cavalli-Sforza, P. Menozzi, and A. Piazza, 'Demic Expansions and Human Evolution', *Science*, 259 (1993), 639-646.

⁴³ M. Nei and A.K. Roychoudhury, 'Evolutionary Relationships of Human Populations on a Global Scale', *Molecular Biology and Evolution*, 10 (1993), 927-943.

⁴⁴ L. Roberts, 'A Genetic Survey of Vanishing Peoples', *Science*, 252 (1992), 1614-1617; S. Subramanian, 'The Story in Our Genes', *Time Magazine*, 16 January 1995, pp. 54-55.

⁴⁵ L.L. Cavalli-Sforza, A.C. Wilson, C.R. Cantor, R.M. Cook-Deegan and M.-C. King, 'Call for a Worldwide Survey of Human Genetic Diversity: A Vanishing Opportunity for the Human Genome Project', *Genomics*, 11 (1991), 490-491. Representation of the HGDP explicitly as an hi-tech alternative to physical anthropology is evident in a recent editorial in a leading scientific journal: 'Bias-Free Interracial Comparisons', *Nature*, 377 (1995): 183-84.

⁴⁶ L. Roberts, 'Genome Diversity Project: Anthropologists Climb (Gingerly) on Board', *Science*, 258 (1992), 1300-1301. F.L.C. Jackson, 'Reflections on the Human Genome Diversity Project', *Anthropology Newsletter*, October 1994, pp. 18-19. G. Lasker, 'Reflections on the Human Genome Diversity Project', *Anthropology Newsletter*, October 1994, p. 19. J. Marks, 'Human Genome Diversity Project: Good For If Not Good As Anthropology?', *Anthropology Newsletter*, April 1995, p. 72.

to the study of the peoples of the world.⁴⁷ Its ultimate success will depend on how (or whether) the project can be reformulated to take advantage of modern anthropological knowledge.

Conclusions

Serology's impact upon physical anthropology was ultimately to establish an asymmetric intellectual relationship between the two fields. New technologies were/are available in genetics, and so long as the interpretation of the data are straightforward, the conclusions that emerge can be obvious. Unfortunately, they have generally proven to be *false*, for the data have traditionally been analyzed with less sophistication than they require, and the results proven decades later to be facile, even if obvious. (One may recall H.L. Mencken's wisdom: To every complicated problem, there is a simple solution – and it's wrong.)⁴⁸

Dichotomizing genetic against traditional anatomical data permitted the latter to be dismissed virtually out-of-hand, along with the training required to collect and interpret them. Ignoring the spectrum of failures and poor interpretations of the serological data permitted later genetic analyses to be rewritten as an intellectual triumph over traditional research in physical anthropology. And to challenge the

⁴⁷ Anonymous, 'Genome Diversity Alarms', *Nature*, 377 (1995), 372. J. Marks, 'Anthropology and Race', *Nature*, 377 (1995), 570. J.H. Relethford, 'Genetics and Modern Human Origins', *Evolutionary Anthropology*, 4 (1995), 53-63. Anonymous, 'Whose Genes Are They, Anyway?', *Nature*, 381 (1996), 11-14. J.E. Terrell and P.J. Stewart, 'The Paradox of Human Population Genetics at the End of the Twentieth Century', *Reviews in Anthropology*, 25 (1996), 13-33.

⁴⁸ Quite possibly the most interesting of these obvious/wrong studies was the Manoiloff blood test, in which the careful addition of a few simple chemicals to a sample of blood could distinguish Jews from Russians, various nationalities from one another, male from female (even in plants), and homosexual from heterosexual. See E.O. Manoiloff, 'Discernment of Human Races by Blood, Particularly of Russians From Jews', *American Journal of Physical Anthropology*, 10 (1927), 11-21; A.T. Poliakowa, 'Manoiloff's "Race" Reaction and Its Application to the Determination of Paternity', *American Journal of Physical Anthropology*, 10 (1927), 23-29; E.O. Manoiloff, 'Blood: Species Reaction', *American Journal of Physical Anthropology*, 10 (1927), 323-328; E.O. Manoiloff, 'Chemical Reaction of Blood for Definition of Sex in Man, Animals, and Dioecious Plants', *American Journal of Physical Anthropology*, 13 (1929), 29-68. The initial claim from German and Russian journals received attention (Anonymous, 'Manoiloff's Chemical Researches on Sex and Race Differences', *Eugenical News*, 11 (1926), 140-141), although Earnest Hooton specifically doubted the ability to distinguish Jews and nations (footnote 4, p. 491). American geneticists under the aegis of Charles Davenport initially reported preliminary results supporting the Manoiloff test's ability to distinguish sex in particular, but abandoned it shortly. See S. Satina and M. Demerec, 'Manoilov's Reaction for Identification of the Sexes', *Science*, 62 (1925), 225-226; S. Satina and A.F. Blakeslee, 'Biochemical Differences Between Sexes in Green Plants', *Proceedings of the National Academy of Sciences, USA*, 12 (1926), 197-202; S. Satina and A.F. Blakeslee, 'Further Studies on Biochemical Differences Between Sexes in Plants', *Proceedings of the National Academy of Sciences, USA*, 13 (1927), 115-122.

intellectual hegemony of genetics is often to bring down accusation of being a troglodyte or Luddite – anti-technology, anti-progress, anti-science.⁴⁹

But the history of serology in anthropology shows that the integration of genetic data into anthropology cannot be adequately accomplished without prior knowledge of the anthropology involved. Unfortunately, it is often not perceived by geneticists to be within their best interests to take the time and effort to acquire the anthropological grounding. One may actually recall the mock diploma constructed by the geneticist Conway Zirkle, lampooning the narrowness of training he perceived in some of his colleagues:

[The university] certifies that John Wentworth Doe does *not* know anything but Biochemistry. Please pay no attention to any pronouncement he may make on any other subject, particularly when he joins with others of his kind to save the world from something or other. However, he has worked hard for this degree and is potentially a most valuable citizen. Please treat him kindly.⁵⁰

The problem, such as it is, has been around for a long time. The tacit authority of genetics and of its students was certainly visible in the eugenics movement, where issues of social and cultural history were subjected to ‘scientific’ genetic analysis. Although the classic expositions by Charles Davenport and Franz Boas were published the same year,⁵¹ the one by Boas had little or no impact in the genetics community. While Boas and Alfred Kroeber publicly assailed the eugenics movement in 1916, geneticists did not begin to break ranks with it for nearly a decade.⁵² And for his critical insights about genetics, Boas faced the usual accusations of narrowmindedness and shortsightedness.⁵³

Thus, the most relevant question for anthropology is unchanged since the 1920s: how to access the new technology and retain the property rights to authoritative interpretations of anthropological problems. This is a two-front war, however: the prohibitive cost of

⁴⁹ P.R. Gross and N. Levitt, *Higher Superstition: The Academic Left and Its Quarrels With Science*, Baltimore: Johns Hopkins, 1994.

⁵⁰ C. Zirkle, ‘Our Splintered Learning and the Status of Scientists’, *Science*, 121 (1955), 516.

⁵¹ C.B. Davenport, *Heredity in Relation to Eugenics*, New York: Henry Holt, 1911. F. Boas, *The Mind of Primitive Man*, New York: Macmillan, 1911.

⁵² F. Boas, ‘Eugenics’, *Scientific Monthly*, 3 (1916), 471-478. A.L. Kroeber, ‘Inheritance by Magic’, *American Anthropologist*, 18 (1916), 19-40. T.H. Morgan, *Evolution and Genetics*, Princeton: Princeton University Press, 1925. H.S. Jennings, *Prometheus, or Biology and the Advancement of Man*, New York: E.P. Dutton, 1925. R. Pearl, ‘The Biology of Superiority’, *The American Mercury*, 12 (1927), 257-266.

⁵³ E.M. East, ‘The Genetic Basis of Eugenics’, *Proceedings of the Third Race Betterment Conference*, Battle Creek, MI: The Race Betterment Foundation, 1928: 41-49. S.J. Holmes, *The Eugenic Predicament*, New York: Harcourt Brace, 1932.

establishing a genetics research program makes it difficult for anthropologists to collect such data themselves; and the utilization by geneticists of essentially a folk knowledge of anthropology effectively marginalizes anthropology where genetics is applied to its questions.⁵⁴ It bears noting that half a century after the Wiener-Montagu feud over the placement of the peoples of Australia and New Guinea, genetics provides no more than utter ambiguity. With one computer algorithm, those populations are genetically the sister group (closest relative) of south Asians – consistent with Wiener; but with another, they are the outgroup to (distinctly separated from) all EurAsia – consistent with Montagu.⁵⁵

It is not 'molecules versus morphology', and never was. It was just a case of complex biological history. To the extent that anything can be said to have been resolved in hindsight, it is that the claim of the serologist to have resolved the issue was incorrect.

History seems to show that the rewards for combining technological prowess and intellectual isolation in genetics are few. But the penalty incurred by anthropology for not coming to grips with the genetic research – either by integrating it or by criticizing and debunking it – has been great. In the future, it will likely be greater.

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⁵⁴ This situation was also apparent in the study of chromosome structure, in which clinical cytogeneticists began to study the chromosomes of nonhuman primates in the 1970s, and to rewrite the anthropology books once again. That literature in retrospect is largely useless, and contemporary research in anthropology is now integrating the two realms more effectively. See J. Marks, 'Hominoid Cytogenetics and Evolution', *Yearbook of Physical Anthropology*, 26 (1983), 131-159.

⁵⁵ L.L. Cavalli-Sforza et al. (footnote 43), 78-79.