

RACE RELATIONSHIPS: COLLEGIALITY AND DEMARCTION IN PHYSICAL ANTHROPOLOGY

PETER SACHS COLLOPY

In 1962, anthropologist Carleton Coon argued in *The Origin of Races* that some human races had evolved further than others. Among his most vocal critics were geneticist Theodosius Dobzhansky and anthropologist Ashley Montagu, each of whom had known Coon for decades. I use this episode, and the long relationships between scientists that preceded it, to argue that scientific research on race was intertwined not only with political projects to conserve or reform race relations, but also with the relationships scientists shared as colleagues. Demarcation between science and pseudoscience, between legitimate research and scientific racism, involved emotional as well as intellectual labor. © 2015 Wiley Periodicals, Inc.

The early 1960s were a pivotal period in the history of scientific racism in the United States. In 1961, Carleton Putnam published *Race and Reason* (Putnam, 1961), applying racial anthropology to the segregationist cause. Both the American Anthropological Association and the American Association of Physical Anthropologists passed resolutions condemning it, the latter over the objections of its president Carleton Coon, who was Putnam's cousin. The next year, Coon published *The Origin of Races* (Coon, 1962), a massive contribution to evolutionary biology in which he argued that some human races had evolved from *Homo erectus* to *Homo sapiens* earlier than others. The book was immediately employed as evidence of white supremacy by segregationists (Jackson, 2005, pp. 157–162).

Of several scientists who criticized the book, geneticist Theodosius Dobzhansky was the most vociferous, disagreeing with Coon not only about whether races represented distinct biological groups that had evolved into *H. sapiens* in parallel, but also about whether scientists were responsible for the political implications of their work. Before this clash, though, each had already researched race for decades. Indeed, Coon and Dobzhansky had long collaborated by commenting on each other's drafts, and had both worked similarly with anthropologist Ashley Montagu, who pioneered anthropological antiracism in *Man's Most Dangerous Myth: The Fallacy of Race* (Montagu, 1942b) and as rapporteur for the United Nations Educational, Scientific and Cultural Organization's 1950 Statement on Race (Montagu, 1951b). When Coon coauthored *Races: A Study of Problems of Races Formation in Man* a decade earlier (Coon, Garn, & Birdsall, 1950), Dobzhansky described it as "invaluable" (Dobzhansky, 1951, p. 265) and Montagu praised it as an exemplary study of human variation (Montagu, 1955, p. 23).

One source of conflict between these men was the increased significance that the civil rights movement and segregationist backlash gave their research. While historians of science have shown that scientific research on race was consistently intertwined with political projects to conserve or reform race relations (Barkan, 1992; Tucker, 1994; Baker, 1998; Jackson,

PETER SACHS COLLOPY is a doctoral candidate in the Department of History and Sociology of Science at the University of Pennsylvania, where his research brings together the histories of science, technology, media, and the American social movements that have made use of them. His dissertation, "The Revolution Will Be Videotaped: Making a Technology of Consciousness in the Long 1960s," explores how video recording was used to think about mind and society by following the technology, and the discourses of feedback and creative evolution that accompanied it, through institutions of art, psychiatry, and radical politics. In 2013, the Forum for History of Human Science awarded him its John C. Burnham Early Career Award for this paper. Correspondence concerning this paper should be sent to collopy@sas.upenn.edu.

2001b; Jackson, 2005; Baker, 2010; Farber, 2011), these new social movements were not the only forces changing the science of race. John Jackson has paid close attention to how the reception of Putnam's tract shaped readings and reviews of Coon's more scholarly tome (Jackson, 2001a); here I argue that attempts to purify anthropology of racist thought were also shaped by biological controversies beyond the validity of race. The rise of Lysenkoism as a theory of heredity in the Soviet Union, for example, led American scientists—most crucially Dobzhansky—to become suspicious of science that served authoritarian political agendas. At the same time, physical anthropologists increasingly came to believe that the future of their subdiscipline lay in an alliance with genetics and the evolutionary synthesis that was reshaping biology, but disagreed about the terms of that alliance. It was thus possible for critics to interpret *The Origin of Races* as a rejection of the synthesis, while Coon and several prominent biologists saw it, like his earlier book *Races*, as a contribution to it.

As these men debated the proper role of the concept of race in studies of human evolution, they engaged in what philosophers since Karl Popper have termed “demarcation” and sociologist Thomas Gieryn terms “boundary-work” (Popper, 1959; Gieryn, 1983; Gieryn, 1999; Nickles, 2013). Both segregationists and antiracist scientists characterized their opponents as pseudoscientific on the grounds that they were politically compromised by either racist or egalitarian concerns, and thus incapable of scientific objectivity. Since Coon, Dobzhansky, and Montagu shared a professional community and had long recognized each other as accomplished scientists, though, this tactic did not fit the situation. Rather than attacking their opponents as unscientific from the start, they accused each other of betraying science by politicizing it. Privately, they suggested that such betrayals were not only professional offenses but also personal ones—that camaraderie was at stake as well as truth. To accuse a colleague of betrayal was therefore an emotional act, and the alliances and conflicts between these three men illustrate how projects of demarcation involve such sentiments as exasperation, contempt, anger, and even a sort of amusement.

Concern for the social order was not a specific trait of fringe or pseudoscientific research on race, though, but a pervasive aspect of racial research, whether white supremacist or egalitarian in its conclusions. The concept of race was itself what Bruno Latour terms a hybrid, an entity existing simultaneously in the realms of nature, politics, and discourse (Latour, 1993, pp. 3, 10–11). In order to construct a persuasive theory of race, scientists had to integrate these varieties of knowledge about human diversity, while simultaneously insisting that race as a phenomenon was only natural or cultural. While Montagu maintained that race was purely discursive, a “fallacy” referring to nothing in nature, Coon and Dobzhansky each argued that it was a natural category. They also agreed that race could be made political, a transgression of which each accused the other in attempts to purify race of its social significance.

CONSTRUCTING AND DECONSTRUCTING RACE

Carleton Coon was a colorful figure even among the anthropologists of his day, writing novels based on his adventures (Coon, 1932; Coon, 1933) and serving in North Africa as a spy and arms smuggler for the U.S. Office of Strategic Services, a precursor to the CIA, during World War II (Coon, 1980). He idolized explorers like Henry Morton Stanley and Richard Burton, and began traveling and adventuring himself as a Harvard undergraduate, visiting Morocco in 1924 (Coon, 1981, pp. 10, 21, 27–29). After he graduated, Coon remained at Harvard, earning a PhD in 1928 and appointments as lecturer, instructor, and professor over the next decade (Wolpoff & Caspari, 1997, p. 158). In 1948 he moved to the University of Pennsylvania, where he remained until retiring in 1963. Coon's field of study was broad, encompassing

cultural anthropology and archaeology as well as anthropometric physical anthropology; as anthropologist Jonathan Marks writes, “Carleton Coon was probably the closest to a generalist anthropologist the discipline had seen since [Franz] Boas himself” (Marks, 2008, p. 246).

Although most of Coon’s research took place in the Middle East and North Africa, in 1929 Ernest Hooton, his advisor, sent him to Albania to research “the wilder whites” (Coon, 1981, p. 84). Coon—himself a Massachusetts native of English descent (Coon, 1981, p. 1)—thus had some experience studying “the white race” when economist William Z. Ripley asked him to revise his book *The Races of Europe* (Ripley, 1899). The result placed the physical anthropology of Europeans in the contexts of archaeology and history, retaining only the title from Ripley’s earlier work (Coon, 1939). Like most scientists of his time, Coon concluded that modern races are mixed, not pure; more specifically, he wrote that Europeans were descended in part from Asians and Africans “of basically Mediterranean racial form” (pp. 2–3). The rest of their ancestry was derived from earlier Europeans, who were themselves hybrids of *H. sapiens* and “some non-*sapiens* species of general Neanderthaloid form.” Among those Coon thanked in his acknowledgments for reading and commenting on drafts of the book was “Professor M. F. Ashley-Montagu” (p. x).

By this point Montagu—who had received a PhD under Boas at Columbia University in 1936 and was teaching anatomy at Hahnemann Medical College in Philadelphia—had developed the key arguments he would use against racism for the rest of his career (Lieberman, Lyons, & Lyons, 1995, p. 835). Typical of Montagu’s antiracism was a 1939 letter to *The New York Times* in which he argued that racial differences are superficial and that the cause of racism “is not their physiognomy but the values, the culturally determined ideas in my own mind which have taught me to react in this way” (Ashley-Montague, 1939). Montagu also suggested that the cultural problem of racism could be solved by education, and specifically that people should be taught that many of the things they associate with race—accents, facial appearances, expressions—“are things which for the most part are culturally determined, and not biologically determined qualities which reside within the people themselves.”

Montagu’s own self-fashioning demonstrated the plasticity of identity: Born Israel Ehrenberg in London’s working class East End, he would later attribute his interest in race to the antisemitic bullying he experienced as a boy and to seeing sailors from around the world at London’s ports (Lieberman et al., 1995, p. 835). As a young man, he adopted the name Montagu Francis Ashley-Montague along with a “tweed jacket, perpetual pipe, and Oxbridge accent” (Sperling, 2008, p. 21). This bourgeois reinvention, writes Susan Sperling, “allowed him access to elite scientific training.”

Montagu’s analysis of race reached his colleagues in 1940, when he presented a paper on “The Genetical Theory of Race, and Anthropological Method” to an annual meeting of the American Association of Physical Anthropologists (Montagu, 1942a). While Montagu accepted that there was variation between human groups, he argued that those differences generally do not covary. “The common definition of ‘race’ is based upon an arbitrary and superficial selection of external characters,” wrote Montagu, but more rigorously scientific studies of blood groups or cephalic indices were not much better (p. 374). This paper and a similar one Montagu presented to the AAPA the next year with the bolder title “The Meaninglessness of the Anthropological Conception of Race” became two chapters of *Man’s Most Dangerous Myth* (Montagu, 1942b), which eventually went through six editions. A foreword by Aldous Huxley testified to the book’s political currency, while suggesting a more constructivist epistemology than Montagu himself embraced: Montagu recognized, wrote Huxley, “that facts do not speak for themselves, but only as men’s socially conditioned passions dictate” (p. vii).

Montagu borrowed another key component of his antiracism from Huxley's brother. In *We Europeans* (Huxley & Haddon, 1936), biologist Julian Huxley and anthropologist A. C. Haddon argued that "the term *race* as applied to human groups should be dropped from the vocabulary of science" because "migration and crossing have produced such a fluid state of affairs that no such clear-cut term, as applied to existing conditions, is permissible" (pp. 82–83). As a substitute, Huxley and Haddon adopted the term *ethnic group*. They did not actually define the term, but Montagu did, writing that "an ethnic group represents one of a number of populations, comprising the single species *Homo sapiens*, which individually maintain their differences, physical and cultural, by means of isolating mechanisms such as geographic and social barriers" (Montagu, 1942a, p. 375). In contrast to races, Montagu emphasized that ethnic groups were distinguished from each other culturally; physical variation was a subject for "further discussion and research" after the classification of groups rather than a basis for it.¹ Montagu's preference for the terminology of *ethnic group* over that of *race* formed a major part of his antiracist project. He suggested in a 1943 letter to RCA, for example, that they market records by black musicians as *ethnic* rather than "Race Entertainment."²

Another major influence on Montagu was Theodosius Dobzhansky's 1937 book *Genetics and the Origin of Species*, from which he often quoted a passage in which Dobzhansky argued that since "the geographical distributions of the separate genes composing a racial difference are very frequently independent," there are no "complexes of characters," which can be used to attribute a racial identity to an individual (Dobzhansky, 1937, pp. 77–78; Montagu, 1942a, pp. 373–374; Montagu, 1942b, pp. 33, 42, 44). In a 1941 *Scientific Monthly* article, Dobzhansky—who had immigrated to the United States from Russia in 1927 (Adams, 1994, p. 4)—addressed "the perennial discussion of the nature of races" more fully (Dobzhansky, 1941, p. 161). Biologists, he noted, were avoiding "the debate on the 'race problem'" for the "apparently good reason [that it] is not conducted on a scientific plane at all." They were themselves to blame for the nature of the debate, though, having failed to provide it a scientific basis. "The plain fact," wrote Dobzhansky, "is that in biology itself no clear definition of what constitutes a race has been evolved."

Dobzhansky suggested that genetics could provide some insights. He claimed, for example, that the Mendelian conception of genes as discrete particles was incompatible with "the habit of describing races in terms of averages" to which "most taxonomists and anthropologists cling perforce" (p. 161). Drawing on his observations of wild populations of *Drosophila pseudoobscura*, Dobzhansky also argued that "the naive concept of pure races connected by intermediates must be replaced by the more authentic one of the varying incidence of definite genes" (p. 164). Geographically and genetically distinct populations were worthy of a biologist's attention, though, because they could continue to diverge and become independent species. "A race becomes more and more a reality, and less and less an abstraction," Dobzhansky concluded, "as it approaches the species rank" (p. 165). It was with these arguments that he began to develop a concept of race grounded in genetics.³

1. Montagu to Dobzhansky, May 23, 1944, Theodosius Dobzhansky file, box 12, series I, Ashley Montagu Papers, American Philosophical Society.

2. In fact, race music was a genre distinguished by its style and intended black audience as well as the race of its performers. An RCA manager wrote to Montagu and explained that "we have records in our [non-race music] catalog by great negro artists. . . . A race record is a specific kind of entertainment involving both lyrical and musical idioms quite apart from the conventional entertainment." J. L. Hallstrom to Montagu, December 8, 1943, RCA Victor Division file, box 40, series I, Montagu Papers; *Oxford English Dictionary*, 3rd ed., s.v. "Race Music."

3. For more on Dobzhansky's ideas about race and genetics, see Farber (2011, pp. 60–72) and Gannett (2013).

Montagu and Dobzhansky began corresponding with mutual compliments in 1943, agreeing that contemporary anthropological understandings of race were both racist and scientifically deficient, but disagreeing about how to solve the problem.⁴ In particular, Dobzhansky rejected the terminology of ethnic groups. “The only way,” Dobzhansky wrote to Montagu, “is to divest the word race of its emotional contents; and if we biologists can help in this, we shall justify our existence.” He also thought that education in genetics—and specifically in “Mendel’s law and Hardy’s equilibrium”—would combat racism, serving much the same role Montagu assigned to anthropological education.⁵

Dobzhansky carried out his approach of teaching first about genetics and then about race in the 1946 book *Heredity, Race, and Society*, which he cowrote with his Columbia colleague L. C. Dunn (Dunn & Dobzhansky, 1946). The first four chapters covered human diversity and heredity. In the fifth, Dunn and Dobzhansky argued that race definitions are often “ideological, not biological,” but again rejected Montagu’s *ethnic group* terminology (pp. 94–95). “Unfortunately ‘ethnic group prejudice’ is easily exchangeable for ‘race prejudice,’” they wrote, “and one can hate ‘ethnic groups’ just as venomously as real or imaginary races.”

Dobzhansky and Montagu saw their differences as minor, though, compared to their common opposition to typological thinking about race. In “Natural Selection and the Mental Capacities of Mankind,” a coauthored article published in *Science* in 1947, they argued that human evolution had probably selected for “the genetically controlled plasticity of mental traits” rather than specific characteristics (Dobzhansky & Montagu, 1947, pp. 589). This argument built on earlier work in which Dobzhansky suggested that natural selection would favor plasticity when a population evolved in a variable environment (Beatty, 1994, pp. 209–211). “The effect of natural selection in man,” Dobzhansky and Montagu concluded, “has probably been to render genotypic differences in personality traits, as between individuals and particularly as between races, relatively unimportant compared to their phenotypic plasticity” (p. 590).

Both men expected the article to draw “squalls” of criticism, and Dobzhansky went so far as to write to Montagu that “I flatter myself (or fool myself) by believing that this is perhaps the most important single idea that ever occurred to me.”⁶ For decades, though, only its authors cited the article in discussions of race.⁷ When he was compiling the book *Race and IQ* (Mongattu, 1975) in 1974, Montagu wrote to Dobzhansky asking, “Do you think that article had much influence? . . . I have never seen a reference to it in the relevant literature.”⁸

STATEMENTS ON RACE

Coon also began corresponding with Dobzhansky in the 1940s. Catching up on scientific developments he had missed during World War II, he wrote to Dobzhansky in 1946 to praise his article “On Species and Races of Living and Fossil Man” (Dobzhansky, 1944) for “such a clear exposition of the general biological point of view towards human races.”⁹ Their

4. Dobzhansky to Montagu, January 4, 1943, Theodosius Dobzhansky file, box 12, series I, Montagu Papers.

5. Dobzhansky to Montagu, May 22, 1944, Theodosius Dobzhansky file, box 12, series I, Montagu Papers.

6. Dobzhansky to Montagu, June 23, 1947, Theodosius Dobzhansky file, box 12, series I, Montagu Papers.

7. My conclusions are based on an ISI Web of Knowledge search yielding 27 articles, which cite “Natural Selection and the Mental Capacities of Mankind.” The article was cited in articles on human heredity and behavior, beginning with Kubie (1948, p. 16).

8. Montagu to Dobzhansky, June 28, 1974, Ashley Montagu file, box 10, series I, Theodosius Dobzhansky Papers, American Philosophical Society.

9. Coon to Dobzhansky, February 13, 1946, General Correspondence [A–Z] 1946 file, box 1, series I, Carleton Stevens Coon Papers, National Anthropological Archives, Smithsonian Institution.

correspondence continued over the next several years as Coon prepared a new textbook, *Races... A Study of the Problems of Race Formation in Man* (Coon et al., 1950), with his student Stanley Garn—and with fellow Hooton student Joseph Birdsell, who “quite literally wrote no single word of the text”¹⁰ but “let us use some of his ideas and photographs” (Coon, 1981, p. 201). After reading a 1947 draft Dobzhansky wrote to Coon that “I have seldom read anything which interested me more intensely,” explaining that “I believe that the problem of adaptive value of human traits will be the central problem of physical anthropology in the future.”¹¹

For their definition of *race*, Coon and Garn turned to the study of organisms “about which there is no reason for emotional controversy,” and specifically to Dobzhansky’s studies of *D. pseudoobscura* (p. 3). They concluded that races are populations “definitely associated with a place or region, habitually interbreeding, and possessing an historical continuity in the reproduction of a general type” both physically and behaviorally. In humans, they explained, citing Montagu, that “perhaps 90 per cent [of genes] are held in common by all groups of men” (p. 11). Variation between races could be understood, they argued, as adaptation to particular environments accomplished through mutation and natural selection.

Coon and Garn presented an explicit taxonomy of human races only in their last chapter, and only with caveats regarding the uncertainty of classification without “historical evidence in the form of skeletons, sculpture, paintings, etc.” to support a phylogeny (p. 111–112). Furthermore, they noted that since “all races are composite,” individuals can be classified only roughly—an insight they may have drawn from Birdsell’s research on race mixing in Australia, in which, as Warwick Anderson writes, “hybridity came to appear biologically persistent,” implying that “there were no pure types, and perhaps there never had been” (Anderson, 2002, p. 234). Nonetheless, *Races* included a “tentative list” of 30 races accompanied by photographs, including one—representing the “Northwest European” race—of Coon himself (see Figure 1; p. 199). Such uncertainty was not characteristic of Coon, but, as he later wrote, Garn “had a slightly moderating influence on my natural exuberance” (Coon, 1981, p. 201). As Dunn wrote in a review, the book “marks a transition stage in anthropological thinking about race” from fixed types “defined by averages of bodily features” to evolving populations defined by gene frequencies (Dunn, 1951, p. 105).

Races was positively received, and the publisher quickly sold out its print run (Coon, 1981, p. 202). In letters regarding the book, Coon boasted in particular that W. Montague Cobb—a physician, chair of Howard University’s anatomy department, and the sole African-American physical anthropologist of the day (Rankin-Hill & Blakey, 1994, pp. 74–75)—“grabbed the first copy” and told Coon that “it will do a great deal, he feels, to help Negro-White relations.” He was also proud of a letter he had received from Montagu, “that prime objector, who never likes anything,” praising the book.¹² “A few of your ideas I had already independently arrived at—so much the better!” wrote Montagu. “Your definition of race isn’t genetical enough for me—but I like it, nevertheless.”¹³ Five years later, Montagu praised *Races* in print as an exemplary study of human variation (Montagu, 1955, p. 23).

Coon was less enthusiastic about Montagu’s work. In 1946, soon after his return from three years serving in the Office of Strategic Services in North Africa, physical anthropologist T. Dale Stewart asked Coon to review one of Montagu’s books. “I have just done one rather

10. [Birdsell] to Coon, November 5, 1954, General Correspondence [A–G] 1954 file, box 4, series I, Coon Papers.

11. Dobzhansky to Coon, December 25, 1947, General Correspondence [A–H] 1947 file, box 1, series I, Coon Papers.

12. Coon to Edward Kern, May 18, 1950, General Correspondence [F–L] 1950 file, box 2, series I, Coon Papers.

13. Montagu to Coon, May 16, 1950, General Correspondence [M] 1950 file, box 2, series I, Coon Papers.

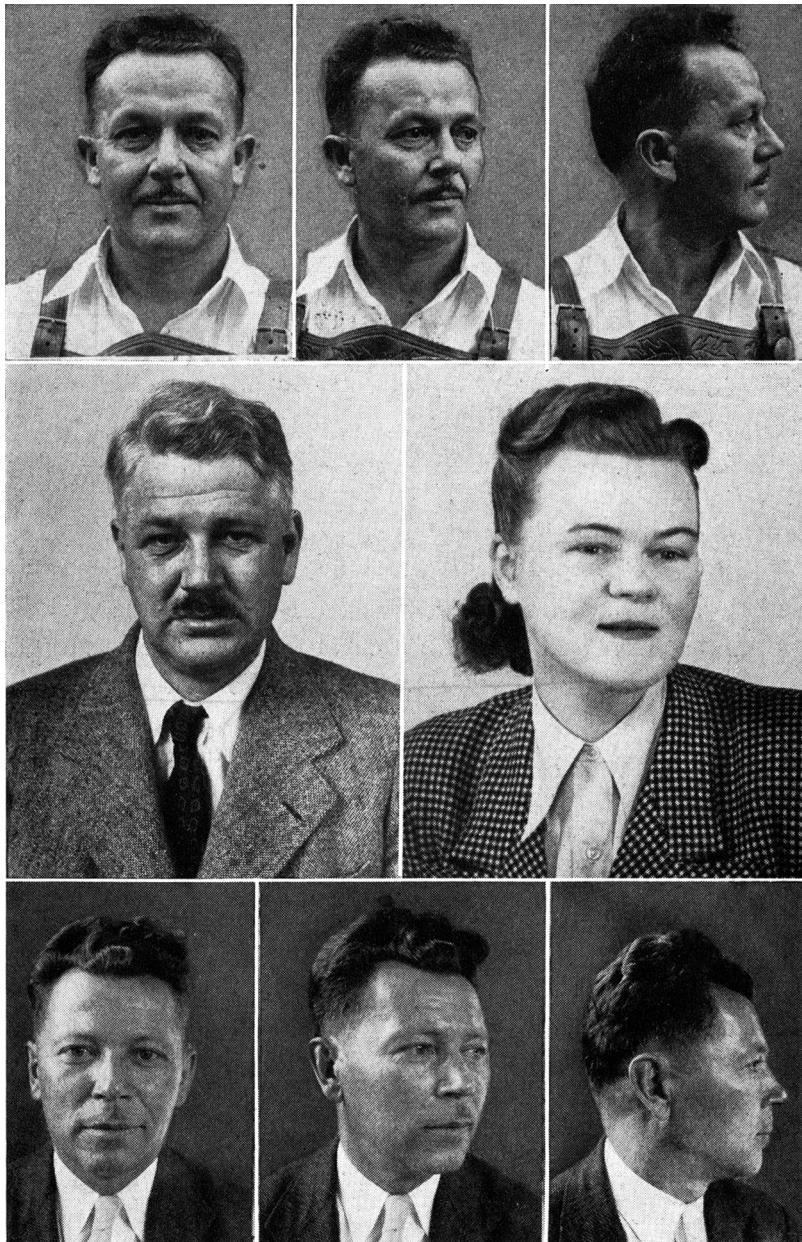


FIGURE 1.

A panel from *Races* illustrating European races, with Coon representing the "Northwest European" in the middle left photograph (Coon et al., 1950, p. 119).

“ticklish job,” Coon wrote back, “and would like very much to be excused from putting my neck out twice in a row.”¹⁴ Twelve years later, Coon explained to another editor that Montagu “has shown an extraordinary forbearance in reviewing MY books for the last few years, and I am a bit loath to break up this ecological arrangement.”¹⁵ (He accepted an opportunity to review Dobzhansky’s *Evolution, Genetics, and Man* [Dobzhansky, 1955], in contrast, writing that “I have a warm spot in my heart for the old guy.”¹⁶) The closest Coon came to writing publicly about Montagu in the 1950s was criticizing anthropologists who, “basing their ideas on the concept of universal brotherhood of man, . . . consider it immoral to study race, and produce book after book exposing it as a ‘myth’” (Coon, 1954, pp. 187–188). Nonetheless, when Montagu asked Coon to read a draft of a text about cooperation, probably *On Being Human* (Montagu, 1951a), Coon wrote back that “this has the bones of a very good book, and a lot of the flesh. Slap it around a little and we will all be proud of you.”¹⁷

What set Coon apart from the mainstream of human biology during the 1940s and 1950s was his resistance to the growing influence of genetics, an attitude obscured in *Races* by Garn’s influence. Coon objected to the scientific hegemony of genetics for precisely the reasons Montagu embraced it: By limiting scientific analysis of populations to particular quantitative modes, geneticists avoided the qualitative descriptions of phenotypes that made up most of the anthropology of race. “Our concept of race should not be hemmed in,” wrote Coon, “by the frontiers of our present genetic knowledge.”¹⁸

Similarly, Coon explained his reluctance to participate in a symposium at Cold Spring Harbor on “Evolution of Man and Population Genetics” by writing to fellow Hooton student Sherwood Washburn that “I feel that the American geneticists have become totalitarian [*sic*]. They have worked out a dogma and anyone who doesn’t fall in with their way of thinking is unthinkable.” Against this “American opposite number of Lysenkoism,” Coon asserted the importance of studying culture in order to understand humanity. He was incensed to find an essay contest on the question of “who marries whom” open only to genetic explanations, for example, writing that “I consider [it] a matter of social anthropology.”¹⁹ When Coon ultimately attended the conference, he contributed a paper arguing that anthropology could not be reduced to genetics because genetics itself sometimes demanded cultural explanations (Coon, 1950). Marriage customs, sexual practices, infanticide, and war influenced not only mate selection, Coon argued, but also fertility, infant mortality, reproductive longevity, and group survival, shaping humans’ differential survival and reproduction.

It’s ironic, then, that when Washburn criticized *Races*, Birdsell attributed his disapproval to a distaste for genetics. “He apparently feels violently emotional on the whole issue,” wrote Birdsell to Coon. “My own feeling is that Washburn primarily approaches race from the point of view of an anatomist rather than a population geneticist, and as a consequence we can hardly expect any other reaction.”²⁰ Washburn—who had recently moved from Columbia University Medical School to the University of Chicago, and whose research was on the comparative

14. Coon to T. Dale Stewart, February 11, 1946, General Correspondence [A–Z] 1946 file, box 1, series I, Coon Papers.

15. Coon to Josef Brožek, February 18, 1958, General Correspondence [A–D] 1958 file, box 8, series I, Coon Papers.

16. Coon to Brožek, December 30, 1955, General Correspondence [A–D] 1955 file, box 5, series I, Coon Papers. The review itself (Coon, 1956) was also enthusiastic.

17. Coon to Montagu, not dated, Carleton Coon file, box 9, series I, Montagu Papers.

18. [Carleton Coon], comments on a book by Bill Boyd, [1954], General Correspondence [A–G] 1954 file, box 4, series I, Coon Papers.

19. Coon to Washburn, March 16, 1950, General Correspondence [T–Z] 1950 file, box 2, series I, Coon Papers.

20. Birdsell to Coon, June 23, 1950, *Races* C, G, & B file, box 61, series VII, Coon Papers.

anatomy of primates (Haraway, 1989, pp. 204–206)—had in fact argued that anatomy was the key to understanding race several years earlier (Washburn, 1944, p. 65). He organized the Cold Spring Harbor symposium with Dobzhansky, though, as an effort to incorporate population genetics into physical anthropology (Smocovitis, 2012, p. S115).

“Evolutionary studies have been revitalized and revolutionized by an infusion of genetics into paleontology and systematics,” wrote Washburn in his 1951 essay “The New Physical Anthropology” (Washburn, 1951, p. 298). “The application of this theory to the primates is the immediate task of physical anthropology.” Among the consequences Washburn anticipated was a new conception of race based on breeding patterns. “There is no way to justify the division of a breeding population into a series of racial types,” he wrote (p. 299), perhaps articulating his actual critique of *Races*. “It is not enough to state that races should be based on genetic traits; races which cannot be reconciled with genetics should be removed from consideration.”

Along with the influence of genetics, Coon opposed what he termed “the current doctrine of racial equality,” particularly with regard to intelligence.²¹ “Races are clearly superior and inferior to each other under given circumstances,” wrote Coon, including among his examples that “a jet black Sudanese is superior, in the Sudan, to a pink-skinned European.”²² Since it had not been proven that races were of equal intelligence, he argued, it was a poor premise for political equality.²³ Even in correspondence, though, Coon avoided explicitly political matters such as whether black and white American should have equal rights. His position, as he wrote to one explicitly racist correspondent, was that scientists should not “express opinions as to national or international policy.”²⁴

This, though, was precisely the role Montagu was adopting. In 1949, Brazilian anthropologist Arthur Ramos invited Montagu to join a UNESCO committee charged with writing a scientific Statement on Race (Hazard, 2012, p. 37). Ramos died before the committee met, though, and the group assigned the task of writing the statement itself to Montagu (Barkan, 1996, p. 99). The resulting document presented a biological conception of race, defining races as “populations constituting the species *Homo sapiens* . . . capable of interbreeding” but with “certain physical differences as a result of their somewhat different biological histories” (Montagu, 1951b, pp. 11–12). It also incorporated Montagu’s idiosyncratic suggestion that “it would be better when speaking of human races to drop the term ‘race’ altogether and speak of ethnic groups” (p. 13), though, as well as the claim that “for all practical social purposes ‘race’ is not so much a biological phenomenon as a social myth” (p. 15).²⁵

The Statement on Race met with widespread dissent from scientists when it was released in 1950. After the anthropological journal *Man* published a number of letters criticizing the Statement, Dobzhansky wrote to Montagu that “the main attack is of course against your suggestion of abolishing [sic] the term ‘race’ in favor of ‘ethnic group’, and you will remember,

21. Coon to Clement W. Meighan, October 5, 1955, General Correspondence [A–D] 1955 file, box 5, series I, Coon Papers.

22. Coon to Dr. Metraux, October 25, 1951, General Correspondence [H–R] 1951 file, box 3, series I, Coon Papers.

23. Coon to Mr. Furnas, June 5, 1955, General Correspondence [E–I] 1955 file, box 5, series I, Coon Papers.

24. Coon to E.L. Anderson, Esq., October 5, 1955, General Correspondence [A–D] 1955 file, box 5, series I, Coon Papers.

25. On UNESCO’s 1950 Statement on Race and its 1951 and 1964 successors, see Provine (1986), Barkan (1996), Gayon (2003), Brattain (2007), Hazard (2012), Selcer (2012). Montagu also appears to have been responsible for the 1950 Statement’s Kropotkinist claim that “biological studies lend support to the ethic of universal brotherhood” (Montagu, 1951b, p. 17). As Nadine Weidman argues, the concept of cooperation was central to Montagu’s understanding of biology (Weidman, 2012). For Montagu’s attitudes toward the work of Peter Kropotkin specifically, see Montagu (1951a, p. 25), Montagu (1952, pp. 39–42), and his foreword to Kropotkin (1955).

my friend, that for the last ten years I have done my damndest [sic] to convince you that this proposition will neither be accepted nor would do any good if accepted.”²⁶ The Statement failed to establish a scientific consensus because most scientists continued to believe that race was a meaningful category even as they rejected its exploitation by explicit racists. Among those “frankly opposed to the statement,” in his own words, was Carleton Coon—though he wrote so only in a letter to a friend.²⁷

Indeed, Coon privately suspected Montagu was a Communist. “Somebody should . . . find out why Ashley changed his father’s name retroactively in Who’s Who,” he wrote to a student in 1960, “[and] whether or not he ever carried a card.”²⁸ Coon was not alone in his curiosity. The FBI began investigating Montagu in 1953, finding that although he belonged to a number of civil rights, antifascist, and professional organizations that they considered Communist fronts, their Communist Party informants did not know him (Price, 2004, pp. 278–279). Montagu later wrote to David Price that he had “always been what was called a liberal,” and had once given a lecture for the New York School for Democracy, “at which time I discovered they were a communist organization at which time I wrote them a furious letter of protest.” He was nonetheless outspoken in his opposition to McCarthyism, resigning from the American Association of Physical Anthropologists in 1953 and the American Anthropological Association in 1955 “because,” he later wrote, “of their inactivity in rising up against the House Un-American Activities Committee and similar organizations.”²⁹

Montagu also left a professorship at Rutgers University in 1955. The conflict with administrators that led to his departure may have involved his opposition to McCarthyism—a prominent alumnus wrote to the president to complain that Montagu’s “blistering attack on Senator McCarthy” in a lecture “coincided with the usual Communistic theme song”³⁰—but it also involved a grant falling through, leaving Montagu without a salary (Radick, 2009). He devoted the rest of his career to books, magazine articles, and television appearances, focusing even more on subjects of popular controversy like gender roles and race relations.

RACE AND REASON

In 1961, aviation entrepreneur Carleton Putnam published *Race and Reason* (Putnam, 1961). Claiming his Princeton undergraduate education in science and his Columbia law degree as credentials, Putnam criticized the Supreme Court’s decision in *Brown v. Board of Education*, which had abolished segregated education in 1954. He contended that the court had been misled by Boasian anthropologists like Montagu, and that people of African descent were not advanced enough culturally or intellectually for “equality of association” with whites (pp. 6–7). “Modern equalitarian anthropology,” Putnam wrote, was “a pseudo-scientific hoax” based on “the doctrines of Franz Boas, . . . a member of a racial minority group” (pp. 22–23).

Putnam emphasized his own northern heritage, even subtitling his book *A Yankee View*, but his audience was largely southern: The Louisiana and Virginia Boards of Education put the book in their high school curricula, and the governor of Mississippi declared October 26, 1961 “Race and Reason Day” (Jackson, 2005, pp. 118–120). *Race and Reason* also influenced

26. Dobzhansky to Montagu, January 26, 1951, Theodosius Dobzhansky file, box 12, series I, Montagu Papers.

27. Coon to Sarah Dees, not dated, quoted in Marks (2008, p. 246).

28. Coon to Harry Turney-High, June 14, 1960, quoted in Jackson (2005, p. 99).

29. Montagu to Price, December 28, 1998, quoted in Price (2004, p. 279).

30. Milo B. Hopkins to Harry L. Derby, January 8, 1953, Rutgers University file 1, box 41, series I, Montagu Papers; [Harry L. Derby] to Lewis W. Jones, January 12, 1953, Rutgers University file 1, box 41, series I, Montagu Papers. See also Sperling (2008, pp. 25–29).

white supremacist leader David Duke, who later claimed his discovery of the book at the age of 13 precipitated his conversion to segregationism from a racial egalitarianism that he had adopted partly through reading *Man's Most Dangerous Myth* (Duke, 1998, pp. 30–33). “*Race and Reason*,” he wrote, “made me realize another legitimate and scientific point of view existed” (p. 37).

Among those who shaped this point of view was Putnam’s cousin, Carleton Coon, who advised Putnam as he wrote *Race and Reason* and was quoted in it as a “distinguished scientist younger than I am, a scientist not a Southerner, who is a recognized authority on the subject we are considering” (p. 50). In addition to providing Putnam with a quotation on the resurgence of hereditarianism in zoology, Coon advised him to cite obscure but respectable authors rather than blatant racists like Madison Grant. When Putnam published *Race and Reason* in 1961, Coon’s contributions to the book were secret. Indeed, the two Carletons had negotiated ways of quoting Coon so that his identity would not be revealed (Jackson, 2005, pp. 100–101).

When Dobzhansky reviewed *Race and Reason* in the *Journal of Heredity* (Dobzhansky, 1961), he argued that scientists had a responsibility to object publicly when the ostensibly scientific became grounds for racism. “Silence,” wrote Dobzhansky, “should not be carried to the point of aiding and abetting misrepresentation” (pp. 189–190). Putnam’s book purported to be a presentation of science, and though the word “means different things to different people, . . . by no stretch of its meaning can Putnam’s book be said to be ‘scientific.’” The biological ideas that were invoked in the discourse of “race prejudice” were rather, Dobzhansky wrote, “pseudo-science.”

As Michael Gordin argues, Dobzhansky’s critique of Lysenkoism in the 1940s and 1950s became “a significant template for how American scientists . . . believed they ought to respond to all suspected pseudoscientists” (Gordin, 2012a, p. 446). After Lysenkoism became official Communist Party doctrine in 1948 and the competing science of genetics was banned in the Soviet Union, Dobzhansky—who counted Soviet geneticists among his friends and colleagues—stopped critiquing it as bad science and began rejecting it outright as “not a legitimate scientific discussion” (Gordin, 2012b, pp. 89–91). As Gordin writes, “on the imagined scale that has excellent science at one end and . . . bad science on the other end, it is *not* the case that pseudoscience lies somewhere on this continuum. It is off the grid altogether” (p. 1).

Although this strategy of rejecting heterodox beliefs as pseudoscience became widespread among scientists generally, Dobzhansky himself applied it only rarely—except to scientific racism. While scientists have implicitly adopted various principles of demarcation between bad science and pseudoscience, for Dobzhansky it was specifically when ostensibly scientific concepts were enlisted in the service of the state—the “prostitution of biology” (Dobzhansky, 1956, p. 22)—that he began to publicly dismiss them as pseudoscientific (Gordin, 2012a, p. 448; Gordin, 2012b, pp. 110–111). As Dobzhansky wrote in his review of *Race and Reason*, “this was most notably true in Hitler’s Germany, and because of this association with Nazism this pseudo-science fell temporarily into desuetude in most of the world” (Dobzhansky, 1961, pp. 189–190). That its resurgence was political was demonstrated, Dobzhansky wrote, by *Race and Reason*’s endorsements from three senators and a letter he received, recommending the book and accompanying a copy of it, from Senator Harry Byrd. For Dobzhansky, then, the work of Lysenko and Putnam was not science because it was political. His critiques were what Latour refers to “purification,” attempts to create “two entirely distinct ontological zones” of nature and culture (Latour, 1993, pp. 10–11).

When the American Association of Physical Anthropologists met in May 1962 and voted to condemn *Race and Reason*, they too were motivated by its political uses. Their discussion began with a motion by Edward I. Fry “designed to place the Association on record again

as regards racism" according to the published proceedings of the AAPA's annual business meeting ("Proceedings," 1963, p. 400), and stating "that all people were exactly equal . . . in intelligence" according to Coon, who successfully tabled the resolution on the grounds that no one present was an expert on the subject.³¹ Nonetheless, a committee formed—made up of Fry, psychologist Josef Brožek, and *Races* coauthor Stanley Garn—to draft a new motion for presentation of the next night.³² Coon was surprised that it was Garn, "of whom I was fond," who presented a resolution on behalf of the committee, and that the resolution specifically targeted his cousin's book (Coon, 1981, p. 335).

We, the members of the American Association of Physical Anthropologists, professionally concerned with differences in man, deplore the misuse of science to advocate racism. We condemn such writings as *Race and Reason* that urge the denial of basic rights to human beings.

We sympathize with those of our fellow teachers who have been forced by misguided officials to teach race concepts that have no scientific foundation, and we affirm, as we have in the past, that there is nothing in science that justifies the denial of opportunities or rights to any group by virtue of race. ("Proceedings," 1963, p. 402).

Garn was also engaged in purification, seeking to legitimate and depoliticize the study of race by ridding it of white supremacy. Neutrality, he suggested, demanded a rejection of the politics of the right as well as the left. Although Garn believed that racial traits represented adaptations to particular environments, he was skeptical of links between race and behavior or intelligence. In explaining why his own 1961 book *Human Races*—intended to update and replace *Races* (Garn, 1962, p. v)—"has nothing to do with racism," Garn wrote that "the history of our species is far too long (and periods of national glory far too short) to direct attention away from race as an evolutionary phenomenon to futile arguments about superiority, inferiority, or moral supremacy" (Garn, 1971, pp. v–vi).³³

According to the minutes of the AAPA meeting, "the ensuing discussion was relatively brief and led to only minor word changes in the motion before it was adopted." Six months later, Coon discussed the meeting with psychologist Anne Roe, who interviewed him as part of a project to revisit the subjects of her 1953 book *The Making of a Scientist* (Roe, 1953). He told her he had spoken out forcefully against the motion:

They proposed to censure Charleton [sic] Putnam for his book, *Race and Reason*. Well it happened that Charleton Putnam is my cousin, and I'm damned if I'm going to censure my cousin. In the first place. In the second place, it's none of their business—to censure anybody. He may be wrong but he's got the right to say what he wants to say—and I said I think your [sic] crazy. I said, how many of you have read the book? One! I said you're going to censure a man's book that you haven't read—and you don't know the man. I said, I know the man, I read the book, and he's my cousin, and I'm damned if I'm going to let you do this.³⁴

31. Anne Roe, interview of Carleton Coon, November 1962, p. 4, Carleton Stevens Coon file, Anne Roe Papers, American Philosophical Society.

32. According to the proceedings "the President appointed a committee . . . to draw up a new motion for presentation at a second business meeting scheduled to be held Tuesday evening following the annual dinner" ("Proceedings," 1963, p. 400) but according to his memoir Coon was surprised by their activity the next night (Coon, 1981, pp. 334–335).

33. Garn also reviewed *The Origin of Races*, praising the book itself but criticizing its appropriation by segregationists in terms that narrowly avoided implicating Coon (Garn, 1963).

34. Roe, interview of Coon, p. 4.

Nonetheless, “the vote for the resolution was something like ninety-one ‘aye’ and one ‘nay’” according to Coon’s former student Gabriel Lasker, who was vice president (Lasker, 1999, p. 148).³⁵ When the resolution passed, Coon resigned from the presidency of the AAPA and left the meeting, only to learn the next day that the members had refused his resignation. He later wrote to Putnam asking that if he wrote about the resolution he “make it clear that it is NOT from me.”³⁶

In Coon’s memoir, this meeting includes one more element, an interaction with W. Montague Cobb. When the question of whether races have equal intelligence came up again the second night, wrote Coon, “I looked at Monty Cobb, who was sitting in the front right corner seat. Monty looked at me and said, ‘But we don’t know, do we?’ I said, ‘No, Monty, we don’t’” (Coon, 1981, p. 335). This story deserves skepticism, both because Coon did not mention it in his interview with Roe and because it fits a pattern in his memoir in which only white liberals protested racism—“I knew that I was in for trouble,” he wrote about publishing *The Origin of Races*, “not from American or other so-called blacks, but from their so-called white protagonists” (p. 344). Earlier in his career, though, Cobb had argued that psychological studies of “innate intellectual capacities” had failed, suggesting that if “the Negro as a biological element in the American population” were instead studied “as we find him, nature and nurture fused,” he would be found “mentally able” (Cobb, 1939, pp. 336, 343–344). For Cobb, then, agnosticism about innate intelligence was an antiracist tactic for dismissing biased psychological testing, while for Coon it preserved the possibility of white intellectual supremacy.

Cobb—who later served as president of the National Association for the Advancement of Colored People—brought up Coon decades later while discussing physical anthropologists “who were bigots” (Rankin-Hill & Blakey, 1994, p. 85). Nonetheless, Cobb told his interviewers, “when he came up for nomination, I supported him.” It is possible, then, that the two men agreed in 1962, and they certainly maintained a collegial relationship despite their differences.

THE ORIGIN OF RACES

Around the same time as the AAPA meeting, Dobzhansky sent Coon a copy of his new book, *Mankind Evolving* (Dobzhansky, 1962), inscribed “with warmest regards from the author” (Coon, 1981, p. 356). Coon had finished *The Origin of Races* (Coon, 1962) but had not published it yet, and was struck by the similarity between the two books. “What you say,” he wrote, “is almost identical with what I am saying in my book, *The Origin of Races*, which is floating somewhere between galley and page proof. We have obviously drawn on the same sources and come up with the same results. This makes me very happy, because now I have much more confidence that I am right.”³⁷ Specifically, Coon shared Dobzhansky’s belief that “as *Homo erectus* spread to new countries and resided there for some time, it differentiated into races,” more than one of which might be ancestral to *H. sapiens* (Dobzhansky, 1962, p. 188). For Coon, this implied “that the races of man had evolved from the *erectus* to the *sapiens* state, rather than having become differentiated later,” and he believed that Dobzhansky had also reached this conclusion.³⁸ Dobzhansky did not state that *erectus* and *sapiens* races were congruent, though, and indeed he implied that the genes of different *erectus* races had mixed into one common *sapiens* gene pool through interbreeding and conquest (pp. 188–189).

35. In Lasker’s account, Coon also denied that he was related to Putnam (Lasker, 1999, p. 148).

36. Coon to Putnam, January 22, 1963, quoted in Jackson (2005, p. 160).

37. Coon to Dobzhansky, May 26, 1962, Carleton S. Coon file, box 3, series I, Dobzhansky Papers.

38. Coon to Dobzhansky, February 26, 1963, Dobzhansky Review [of *Origin of Races*] file, box 72, series VII, Coon Papers.

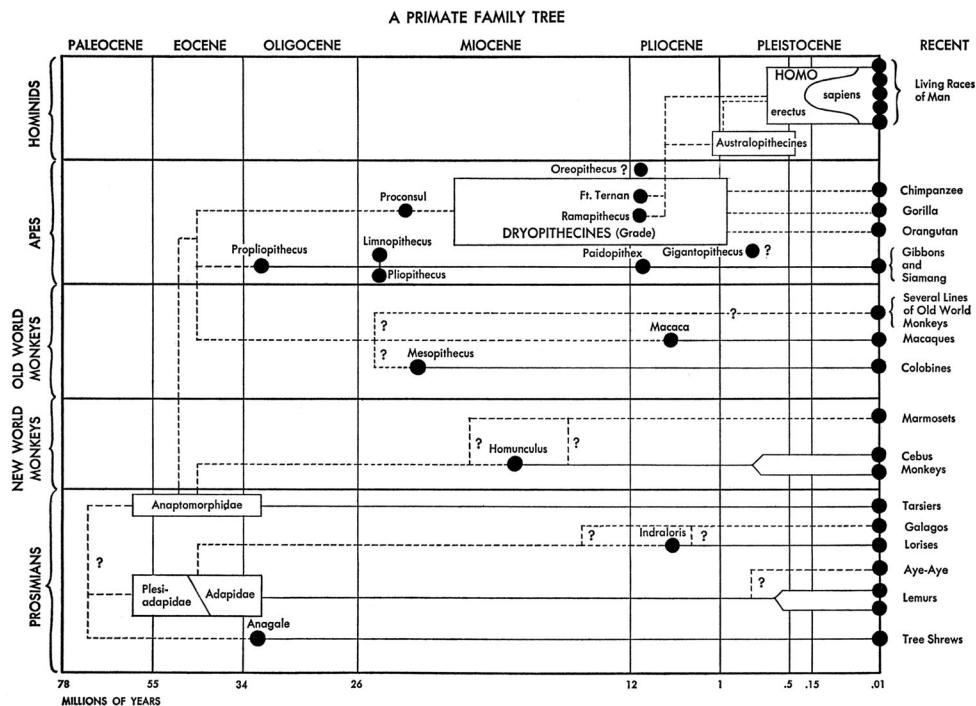


FIGURE 2.

A primate phylogeny from *The Origin of Races*. Note the curved line separating *Homo erectus* from *Homo sapiens* in the upper right corner, indicating some “Living Races of Man” crossed the species boundary before others (Coon, 1962, p. 303).

Coon's own book interwove a comprehensive account of hominid fossils with an argument that five distinct “races or subspecies” of *H. erectus* had evolved separately into *H. sapiens*, “as each subspecies, living in its own territory, passed a critical threshold from a more brutal to a more *sapient* state” (p. 657; see Figure 2 for Coon's illustration).³⁹ He also argued that the cultural development of a race was related to how long ago it had crossed this species boundary: “It is a fair inference,” wrote Coon, “that fossil men now extinct were less gifted than their descendants who have larger brains, that the subspecies which crossed the evolutionary threshold into the category of *Homo sapiens* the earliest have evolved the most, and that the obvious correlation between the length of time a subsequent species has been in the *sapiens* state and the levels of civilization attained by some of its populations may be related phenomena” (pp. ix–x.) In his discussion of fossils, Coon classified 300,000-year-old “Caucasoid” specimens as *H. sapiens*, but “Congoid” fossils less than 100,000 years old as *H. erectus* (p. 335). Frequent climatic changes in western Asia, he explained, had accelerated “Caucasoid” evolution (p. 485).

When Dobzhansky drafted a review of *The Origin of Races* for the literary *Saturday Review* in September, he praised Coon's descriptions of hominid fossils, but objected to the

39. In his memoir, Coon blames this wording on his copy editor, and notes that he changed it in the second edition to “as each subspecies, living in its own territory, underwent advantageous genetic changes due to a process impossible to specify from the meager evidence at hand” (Coon, 1981, p. 343).

idea that subspecies of *H. erectus* evolved into *sapiens* independently.⁴⁰ Coon had suggested that isolated subspecies could evolve in parallel through the simultaneous elimination of unfit genes from multiple populations. Thus, he wrote, “A and B may evolve together into a new polytypic species that retains its original set of subspecies” (p. 16). Dobzhansky rejected this mechanism, writing that such parallel evolution would require a “mystical inner drive that propels evolution.” More importantly, Dobzhansky argued that through his claim that races evolved independently, Coon “gets himself into semantic mischief,” which could easily be taken advantage of by racist propagandists.⁴¹

As a professional courtesy, Dobzhansky sent his review to Coon, attaching an apologetic letter in which he clarified that “it is indeed the unfortunate language which you are using that creates a semantic predicament of a dangerous sort, not any substantive findings.” Indeed, Dobzhansky conceded, “as you yourself have remarked in your letter commenting on my book, we are saying very much the same thing—anagenetic transformation of a single species of *Homo*.” For this transformation to take place independently and asynchronously in different populations, though, seemed to Dobzhansky both scientifically implausible and a politically harmful idea.⁴²

Coon was upset by the review, reading into it an allegation of intent. “It is incomprehensible to me,” he wrote to Dobzhansky, “that a man of your integrity and stature should misrepresent what I said so utterly, turn what was supposed to be a review into an anti-racist tract, and accuse me of ‘mischievously’ furnishing ammunition to racists.”⁴³ At least initially, Dobzhansky did in fact believe that Coon intended to aid segregationists, writing to evolutionary biologist Ernst Mayr of “the uses to which Coon’s book will be put and for which, I am sad to say, it is clearly intended.”⁴⁴ He was surprised by Coon’s defensiveness, though, writing back that “no such allegation is contained in my review. Should I then offer you apologies for what I did not write?”⁴⁵

Dobzhansky wrote to Mayr in order to ask him, and paleontologist George Gaylord Simpson, whether “you think I am unfair to Coon.”⁴⁶ Both Mayr and Simpson, each of whom wrote his own positive review of *The Origin of Races* (Mayr, 1962; Simpson, 1963), replied that he was. “I saw none of the implications which you seem to see,” wrote Mayr, defending Coon’s model of parallel evolution on the grounds that “surely all races of a species possess essentially the same epigenotype and therefore will respond to similar selection pressures in a similar way.”⁴⁷ Simpson conceded that “of course Coon’s book will be misused by the racists, as happens to *any* study of race (including even some by you),” but insisted that “these are not reasons for eschewing attempts to understand racial differentiation and origins.”⁴⁸ Similarly, Julian Huxley wrote to Montagu that Coon “certainly exaggerates some points, but in my opinion he has done a useful service in pointing out the fact that man must have early

40. Theodosius Dobzhansky, review of *The Origin of Races* (not dated), p. 2, series II, Dobzhansky Papers.

41. Dobzhansky, review of *Origin*, pp. 1, 4.

42. Dobzhansky to Coon, October 17, 1962, Dobzhansky Review [of *Origin of Races*] file, box 72, series VII, Coon Papers.

43. Coon to Dobzhansky, October 20, 1962, Dobzhansky Review [of *Origin of Races*] file, box 72, series VII, Coon Papers.

44. Dobzhansky to Mayr, October 23, 1962, series II, Dobzhansky Papers.

45. Dobzhansky to Coon, October 29, 1962, quoted in Marks (2000, p. 5).

46. Dobzhansky to Mayr, October 23, 1962.

47. Mayr to Dobzhansky, November 1, 1962, series II, Dobzhansky Papers.

48. Simpson to Dobzhansky, November 1, 1962, series II, Dobzhansky Papers.

differentiated into a number of subspecies, and that these evolved in adaptive relation with their environment and its requirements.”⁴⁹

Proponents of “the new physical anthropology,” particularly Washburn, characterized Coon as a typological thinker oblivious to the developments of the evolutionary synthesis because he utilized races as units of analysis rather than populations (Washburn, 1963, p. 521; DeVore, 1992, p. 422). As these letters demonstrate, canonical participants in the synthesis judged otherwise. In his review, Simpson recommended Coon’s chapters on evolutionary principles “to anthropologists as an abstract of the most pertinent parts of the massive nonanthropological literature on organic evolution” (p. 269).⁵⁰ Mayr associated the book with populational thinking, identifying it not with physical anthropology’s “intellectually stagnating . . . typological approach” but with “the arrival of a new period” in the discipline (p. 422). If anthropology was omitted from the evolutionary synthesis in the 1930s and 1940s, as Vassiliki Smocovitis argues (Smocovitis, 2012, p. S109), the explicitly genetic “new physical anthropology” was not the only attempt to belatedly incorporate it; Coon’s treatment of “many of the problems of the new systematics and of speciation,” as Mayr described it, represented not a dissent from the synthesis but a contribution to it (p. 420).

Dobzhansky was disappointed that his colleagues disagreed with his critique, but after “some soul-searching and some re-examination of that book,” decided “to stick to my guns, however much I would have preferred to have my guns stacked together with your guns.”⁵¹ There are a number of reasons why Dobzhansky might have been more sensitive than others to the racist implications of *The Origin of Races*, including that he was generally to their left politically.⁵² Also critical, though, was Dobzhansky’s experience in debunking Lysenkoism and more overt scientific racism. While other reviewers focused on the book’s scientific merits, addressing its politics only briefly, Dobzhansky was accustomed to reading biological texts as expressions of authoritarianism. Having recognized that the battle over Lysenkoism was about not only heredity but also control of Soviet science, he appreciated as well that scientific works on race and evolution addressed both heredity and the justice of contemporary race relations.

When Roe interviewed Coon the same month, he attributed other motivations to Dobzhansky. While discussing whether he or Simpson, Roe’s husband, deserved credit for the idea that *H. erectus* had evolved into *H. sapiens* multiple times, Coon said that “Dobzhansky thought that it was his idea and that’s why he’s so mad at me, I think. I wrote him a very nice letter about his book. I thought it was rather trivial, but I wrote him a nice letter about it and in reply I got this nasty attack. I think he’s just a baby.” Coon also described his experience of the criticism: “I’ve been so disturbed by all this integration/segregation business and being shot at so,” he told Roe, “that I haven’t been able to work, I can’t concentrate. . . . And now they’re all yapping and screaming at me and saying I’m a Racist and all this stuff you see, and Dobzhansky’s joined the pack of hounds, which was a terrible surprise to me that he should do that—I thought he was a scholar. And I think actually Montague is behind them.”⁵³

49. Huxley to Montagu, January 17, 1963, Julian Huxley file, series I, box 22, Montagu Papers.

50. Simpson’s review is perhaps the most successful at clarifying Coon’s sometimes imprecise use of evolutionary theory, rescuing the book intellectually if not ethically as a contribution to the synthesis. It contrasts strongly with Dobzhansky’s rejection, not least in that it was not nearly as influential.

51. Dobzhansky to Simpson, Mayr, and Wm. L. Strauss, Jr., November 9, 1962, series II, Dobzhansky Papers.

52. Paraphrasing Dobzhansky’s student Richard Lewontin, geneticist Costas Krimbas describes Dobzhansky as “a social democrat, a strong proponent of civil liberties, vaguely socialist but certainly left-liberal, and not a great defender of capitalism” (Krimbas, 1994, p. 185).

53. Roe, interview of Coon, pp. 3–5.

In the end, the *Saturday Review* didn't publish Dobzhansky's review, perhaps because he had violated their editorial standards by sharing his review with its subject, or perhaps because Coon persuaded the editors not to.⁵⁴ When the review finally appeared in print, in the February 1963 issue of *Scientific American* (Dobzhansky, 1963a), Coon wrote two letters: one to Columbia president Detlev Bronk, asking him to "alleviate this situation" with Dobzhansky, "who was once my friend," and the other to Dobzhansky himself.⁵⁵ "On the advice of an eminent jurist whom I consulted on this matter," he wrote, "I am writing to ask you to end your campaign of defamation against me. . . . When you accuse me of irresponsible writing you forget your own irresponsibility in exposing a fellow scientist to what I have had to undergo as a result of your own actions. Why have you done this? When are you going to stop?"⁵⁶

Of course, Coon had in fact aided Putnam in his work, and Dobzhansky accused Coon of less than he had actually done. As Jackson writes, "Coon's continued public claim that his critics were attempting to politicize his work was disingenuous at best. Putnam, with Coon's blessing and assistance, had transformed Coon's work into a political weapon" (Jackson, 2001a, p. 281).

Dobzhansky's review appeared again that October in *Current Anthropology* (Dobzhansky, 1963b), alongside another review by Montagu (1963) and responses to both by Coon (1963a, 1963b). In this revision, Dobzhansky clarified his attack on Coon's style, writing that "Professor Coon states some of his conclusions in a way that makes his work susceptible to misuse by racists, white supremacists and other special pleaders. This misuse began even before the book was published, and it is continuing" (p. 360). The prepublication misuse to which Dobzhansky referred was a letter by psychologist Henry E. Garrett and anatomist Wesley Critz George published in *The New York Times*, which quoted *The Origin of Races* even though it was dated the day before the book was published (Garrett & George, 1962); Coon had evidently shared a draft with Putnam, who had then sent George useful quotations (Jackson, 2005, pp. 166–167; Marks, 2008, pp. 250–251).

Aside from this point, Dobzhansky's review was a fairly focused attack on the idea that races evolved in parallel, coupled with a proclamation that scientists were responsible for the political uses of their work. Dobzhansky admitted that multiple races or subspecies of *H. erectus* could have evolved into the races of *H. sapiens*—though both *race* and *subspecies* were "not clearly defined biologically"—but only if the subspecies interbred, in which case they would have developed into *H. sapiens* at the same time (pp. 365–366). Coon's idea that populations became sapient at different times, wrote Dobzhansky, "makes *Homo erectus* contemporaneous with *Homo sapiens* for some 200,000 years." They must therefore have been genetically isolated, and yet humans of different races can now interbreed. "For a single species to have arisen from two species that could not interbreed," Dobzhansky concluded, "would indeed be extraordinary." Indeed, such convergence made sense to Dobzhansky only with the framework of orthogenesis, the teleological model embraced by anthropologist Franz Weidenreich—to whom Coon had dedicated *The Origin of Races*—but rejected by the evolutionary synthesis.

54. On the contentious question of why the *Saturday Review* didn't publish Dobzhansky's review, Margaret Mead suggests and Jackson argues that Dobzhansky violated the journal's norms by sharing the article (Mead, 1963; Jackson, 2005, pp. 243–244 n. 51); Coon states and Marks argues that Coon persuaded the editors (Coon, 1981, p. 353; Marks, 2008, pp. 252, 258).

55. Coon to Detlev Bronk, February 25, 1963, Dobzhansky Review [of *Origin of Races*] file, box 72, series VII, Coon Papers.

56. Coon to Dobzhansky, February 25, 1963, Dobzhansky Review [of *Origin of Races*] file, box 72, series VII, Coon Papers.

Coon's reply, and Dobzhansky's reply to it, focused on evolutionary mechanisms that could avoid this charge. Coon pointed out that he had discussed in *The Origin of Races* the role of peripheral gene flow among subspecies in gradually bringing populations of *H. erectus* into *H. sapiens* (Coon, 1963a, p. 366).⁵⁷ He had only presented this as a possibility, though, writing that "we cannot hope to settle the question of parallel evolution versus peripheral gene flow in the evolution of each race by examining fossil bones and nothing else" (Coon, 1962, p. 37). Coon also argued that "Dobzhansky should note that, unlike his fruit-flies, human beings do not mate at random, but are kept apart to a large extent and quite effectively by cultural barriers such as language, religion, and such other customs as feelings about integration and segregation" (Coon, 1963a, p. 366). As a result, human races could in fact be genetically isolated at times and interbreed at others. In response, Dobzhansky wrote that such gene flow undermined the claim that *H. sapiens* had five independent origins: What Coon had now admitted, in his interpretation, was that "the species *erectus* changed into *sapiens* once, but the transition required the flow of many genes over long periods of time" (Dobzhansky, 1963b, p. 367).

Montagu's review was less focused, critiquing several of Coon's claims about genetics and brain size. More incisively, he argued that even if some races became *H. sapiens* first, evolution—and especially cultural evolution—occurs at varying rates. "I altogether fail to see," wrote Montagu, "why a subspecies—granting the very doubtful proposition that it is a subspecies—which has been in the *sapiens* state longer than another subspecies has evolved the most and is obviously going to have a higher level of civilization" (1963, p. 362).

The chain of replies between Montagu and Coon stretched long, but it consisted almost entirely of bickering over minutiae, name calling, and sarcasm. Coon wrote of Montagu, for example, that "were it not for the possibility that some readers who do not know him might take him seriously, I would not bother to answer" (Coon, 1963b, p. 363). To resolve a disagreement about the conclusions of a book on brain size and intelligence, Montagu simply quoted the book's abstract in full (p. 364). Each anthropologist requested that the other's often heated writings be published without revision, believing his own words would discredit him. Montagu's review would "show the world what kind of creature Montagu is," wrote Coon to the journal's editor, while Montagu later wrote that in response to the reviews Coon "wrote virulent and error-filled letters to [Current Anthropology], which upon my advice were published and responded to."⁵⁸ This was scientific debate carried out as much through ridicule and mockery as evidence and argument.

LEGACIES

The *Current Anthropology* reviews marked the nadir of conflict between Montagu, Dobzhansky, and Coon, but their sometimes hostile interactions persisted. According to anthropologist Pat Shipman, when a colleague mentioned to Coon that Montagu had recently visited him in 1977, Coon responded, "You had Ashley Montagu in your office? And you didn't shoot him?" (Shipman, 1994, pp. 283–284).

Coon was kinder towards Dobzhansky. As he was writing his autobiography in 1975, he wrote to Dobzhansky that, regarding their debates in the 1960s, "I would like to say as little

57. Coon also elaborated on the role of peripheral gene flow in unpublished responses to Dobzhansky's review, and in his memoir. Coon to Dobzhansky, October 20, 1962, Dobzhansky Review [of *Origin of Races*] file, box 72, series VII, Coon Papers; Coon (1981, p. 343).

58. Coon to Sol [Tax], January 20, 1963, Dobzhansky Review [of *Origin of Races*] file, box 72, series VII, Coon Papers; Montagu to Stephen Jay Gould, July 25, 1974, Stephen Jay Gould file, box 19, series I, Montagu Papers.

as possible about what happened, and to state that we have buried the now-rusty hatchet.”⁵⁹ Dobzhansky died seven months later without replying, and Coon devoted a chapter to his disagreements with Dobzhansky, revealing “more than I said I would say,” he wrote, “because it belongs to history” (Coon, 1981, p. 356).

Even the relationship between Dobzhansky and Montagu suffered as the former objected more strongly to the wholesale disposal of race concepts. After reading Montagu’s 1964 edited volume *The Concept of Race* (Montagu, 1964), Dobzhansky wrote to Washburn that “it was a bit of a surprise to see there reprinted your splendid Presidential Address [from the 1962 meeting of the American Anthropological Association] together with the trash produced by my friend Ashley.”⁶⁰

Montagu shared his opinion of Coon with paleontologist Stephen Jay Gould in 1974, when Gould passed on an angry letter that Coon had written in response to Gould’s description of *the Origin of Races* (Gould, 1974, p. 21) as “an amusing piece.”⁶¹ “Coon . . . is a racist and an antisemite, as I know well,” replied Montagu, “so when you describe Coon’s letter to the editor of *Natural History* as ‘amusing’ I understand exactly what you mean—but it is so in exactly the same sense as *Mein Kampf* was ‘amusing.’”⁶²

Such allegations of racism offended Coon, as Roe’s interview demonstrates, because he associated racism with exactly the sort of explicit political action he avoided. “He was not a racist in the sense that he wanted to discriminate actively against the underclasses,” write Milford Wolpoff and Rachel Caspari (Wolpoff & Caspari, 1997, p. 169), “but there is no doubt that he had absolutely no sense of social responsibility. In fact, he felt this diluted the objectivity that was necessary in science.” Coon did not, like Putnam, seek out facts to justify his fear and hatred, nor—even when assisting Putnam—did he see himself as an advocate for white social supremacy. He did believe, though, that white biological supremacy was an objective fact, at least in the traits most beneficial to modern humans. “Were the evolution of fruit flies a prime social and political issue,” he wrote in 1968, “Dobzhansky might easily find himself in the same situation in which he and his followers have tried to place me” (Coon, 1968, p. 275).

Privately, Coon recognized that assertions of white supremacy offended his nonwhite associates, telling Roe that he suspected the Wenner-Gren Foundation for Anthropological Research would stop funding his work because its director of research Paul Fejos had “married a colored girl.”⁶³ (Lita Osmundsen, Fejos’ wife, was herself an anthropologist who would soon succeed Fejos as director of research [Douglas, 1986, p. 521].) “I’m sure that this thing isn’t going to make her very happy,” Coon continued. “I’ve got nothing against her, it’s just a sad fate.”

Coon’s was a conservative white American resisting his profession’s increasing liberalism. Although his anger could be intense, he also cooperated with and even befriended those with whom he disagreed. It was this ecumenicalism which made Coon’s leadership role in physical anthropology possible, and which he believed Dobzhansky violated—“In ways unacademical

59. Coon to Dobzhansky, May 9, 1975, Carleton S. Coon file, box 3, series I, Dobzhansky Papers.

60. Dobzhansky to Washburn, February 15, 1965, Sherwood L. Washburn Papers, Bancroft Library, University of California, Berkeley.

61. Coon to Gould, May 23, 1974; Gould to Montagu, July 23, 1974; both in Stephen Jay Gould file, box 19, series I, Montagu Papers.

62. Montagu to Gould, July 25, 1974, Stephen Jay Gould file, box 19, series I, Montagu Papers.

63. Roe, interview of Coon, p. 6.

/ And anything but oecumenical," as Coon wrote in a poem—by suggesting *The Origin of Races* was intended to provide support to segregationists.⁶⁴

Both Montagu and Dobzhansky believed that racism was essentially a bad idea, to be defeated through critique and scientific explanation. In fact racism, like race, was also a matter of complex and constructed identity, built of relationships as well as concepts.⁶⁵ In writing *The Origin of Races*, Coon sought to embrace the evolutionary synthesis without disavowing his cousin's segregationism or the scientific traditions of Hooton and Weidenreich. Unlike Washburn, whose betrayal of Hooton he saw as "oedipal," Coon successfully produced a model of the evolution of races that maintained these intellectual (and crypto-political) commitments—but he did not persuade his anthropological colleagues to adopt it (Coon, 1981, p. 204).

Although Dobzhansky's critique of Coon was compelling, it was not scientifically definitive, leaving Coon with plausible counterarguments. What made Coon's work marginal was that anthropologists generally agreed with Dobzhansky about the political responsibilities of scientists. Proclamations of white supremacy that had been commonplace in the field were becoming exceptional, and the segregationism with which Coon's work was associated was particularly abhorrent to his fellow anthropologists.⁶⁶

As explicit white supremacists like Putnam, Garrett, and George embraced *The Origin of Races*, other scientists distanced themselves from it.⁶⁷ "Scientists will always demarcate," argues Michael Gordin, "because part of what science *is* is an exclusion of some domains as irrelevant, rejected, outdated, or incorrect" (Gordin, 2012b, p. 209). This is exactly what Washburn did in his presidential address at the American Anthropological Association's 1962 meeting, when he announced that "*The Origin of Races* is a reversion to 19th-century typological thinking and is of no use to the profession whatsoever" (DeVore, 1992, p. 422).

It was in this address that Coon's reputation as an outmoded racialist anthropologist became firmly established and *The Origin of Races* excluded from the anthropological canon. Washburn left his attack out of the published version of his paper, which Coon rightly suspected was "watered down,"⁶⁸ instead writing merely that "a contrary view has recently been expressed by Coon in *The Origin of Races*" (Washburn, 1963, p. 521). If his address insisted that *The Origin* be remembered as an irrelevant, rejected, outdated, and incorrect book on race rather than a contested and appreciated one on evolution, his published paper suggested that the boundary-work involved was a matter of simple scholarly disagreement.

Establishing a scientific consensus that Coon's work was typological and racist required not only carefully reasoned argument but also moral judgment and heated invective, as did

64. [Coon], "He's Dead But He Won't Lay Down" (not dated), *Origin of Races—Letters [1962–66, 1968–69, 1975]* file, box 71, series VII, Coon Papers.

65. For example, in 1970 Montagu wrote to linguist Noam Chomsky that "I know the character of Arabs and I know that of Jews and Israelis, and as between the one group and the other I put my faith in the Israelis." This was not a scientific claim, but a statement of ethnic loyalty—and a demonstration that ethnic group prejudice could replace race prejudice as Dobzhansky predicted. Montagu to Chomsky, September 4, 1970, Noam Chomsky file, box 8, series I, Montagu Papers.

66. When the American Association for the Advancement of Science met in Atlanta in 1955, for example, Cobb protested that black members would be unable to stay at the conference hotels and Lasker refused to organize sessions for the anthropology section (Rankin-Hill & Blakey, 1994, p. 85). The Association voted to hold future meetings "only in places where there will be free association among scientists."

67. Dobzhansky was right that Coon's work appealed to white supremacists, not only in his lifetime but beyond. It is on their internet forums Stormfront and Metapedia, for example, that one can now find discussions of *The Origin of Races* as well as *Race and Reason*. See, for example, <http://www.stormfront.org/forum/t486892/>, <http://www.stormfront.org/forum/t490528/>, and http://en.metapedia.org/wiki/Carleton_Coon.

68. Coon to Putnam, September 14, 1963, quoted in Jackson (2001a, p. 277).

the scientific study of race and the collegial relationships that developed around it more generally. Demarcation involved emotional as well as intellectual labor. If the science of race in particular was never isolated from its politics, it was also never isolated from the sentiments and relationships of its practitioners.

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MANUSCRIPT COLLECTIONS CONSULTED

Carleton Stevens Coon Papers, National Anthropological Archives, Smithsonian Institution.

Theodosius Dobzhansky Papers, American Philosophical Society.

Ashley Montagu Papers, American Philosophical Society.

Anne Roe Papers, American Philosophical Society.

Sherwood L. Washburn Papers, Bancroft Library, University of California, Berkeley.

REFERENCES

- Adams, M. B. (1994). Introduction: Theodosius Dobzhansky in Russian and America. In M. B. Adams (Ed.), *The evolution of Theodosius Dobzhansky: Essays on his life and thought in Russia and America* (pp. 195–218). Princeton, NJ: Princeton University Press.
- Anderson, W. (2002). The cultivation of whiteness: Science, health, and racial destiny in Australia. Melbourne, Australia: Melbourne University Press.
- Ashley-Montague, M. F. (1939). Prejudices held cultural, to be overcome by education [Letter to the editor]. *The New York Times*, August 13.
- Baker, L. D. (1998). From savage to negro: Anthropology and the construction of race, 1896–1954. Berkeley: University of California Press.
- Baker, L. D. (2010). Anthropology and the racial politics of culture. Durham, NC: Duke University Press.
- Barkan, E. (1992). The retreat of scientific racism: Changing concepts of race in Britain and the United States between the World Wars. Cambridge, England: Cambridge University Press.
- Barkan, E. (1996). The politics of the science of race: Ashley Montagu and UNESCO's anti-racist declarations. In L. Lieberman & L. T. Reynolds (Eds.), *Race and other misadventures: Essays in honor of Ashley Montagu in his ninetieth year* (pp. 96–105). Dix Hills, NY: General Hall.
- Beatty, J. (1994). Dobzhansky and the biology of democracy: The moral and political significance of genetic variation. In M. B. Adams (Ed.), *The evolution of Theodosius Dobzhansky: Essays on his life and thought in Russia and America* (pp. 195–218). Princeton, NJ: Princeton University Press.
- Brattain, M. (2007). Race, racism, and antiracism: UNESCO and the politics of presenting science to the postwar public. *American Historical Review*, 112, 1386–1413.
- Cobb, W. M. (1939). The Negro as a biological element in the American population. *Journal of Negro Education*, 8, 336–348.
- Coon, C. S. (1932). *Flesh of the wild ox: A Riffian chronicle of high valleys and long rifles*. New York, NY: Morrow.
- Coon, C. S. (1933). *The Riffian*. Boston, MA: Little, Brown.
- Coon, C. S. (1939). *The races of Europe*. New York, NY: Macmillan.
- Coon, C. S. (1950). Human races in relation to environment and culture with special reference to the influence of culture upon genetic changes in human population. In K. B. Warren (Ed.), *Origin and evolution of man* (pp. 247–257). Cold Spring Harbor symposia on quantitative biology 15. Cold Spring Harbor, NY: Biological Laboratory.
- Coon, C. S. (1954). *The story of man: From the first human to primitive culture and beyond*. New York, NY: Knopf.
- Coon, C. S. (1956). Review of *Evolution, genetics, and man* by T. Dobzhansky. *Human Biology*, 28, 376–378.
- Coon, C. S. (1962). *The origin of races*. New York, NY: Knopf.

- Coon, C. S. (1963a). Comments [on T. Dobzhansky's review of *The origin of races*]. *Current Anthropology*, 4, 366.
- Coon, C. S. (1963b). Comments [on A. Montagu's review of *The origin of races*]. *Current Anthropology*, 4, 363.
- Coon, C. S. (1968). Comment on "Bogus science" [Letter to the editor]. *Journal of Heredity*, 59, 275.
- Coon, C. S. (1980). A North Africa story: The anthropologist as OSS agent 1941–1943. Ipswich, MA: Gambit.
- Coon, C. S. (1981). Adventures and discoveries: The autobiography of Carleton S. Coon. Englewood Cliffs, NJ: Prentice-Hall.
- Coon, C. S., Garn, S. M., & Birdsell, J. B. (1950). Races: A study of the problems of race formation in man. Springfield, IL: Thomas.
- DeVore, I. (1992). An interview with Sherwood Washburn. *Current Anthropology*, 33, 411–423.
- Dobzhansky, T. (1937). Genetics and the origin of species. New York, NY: Columbia University Press.
- Dobzhansky, T. (1941). The race concept in biology. *Scientific Monthly*, 52, 161–165.
- Dobzhansky, T. (1944). On species and races of living and fossil man. *American Journal of Physical Anthropology*, 2, 251–265.
- Dobzhansky, T. (1951). Race and humanity [Review of *Genetics and the races of man: An introduction to modern physical anthropology* by W. C. Boyd, *Races: A study of the problems of race formation in man* by C. S. Coon, S. M. Garn, & J. B. Birdsell, and *This is race: An anthology selected from the international literature on the races of man* by E. W. Count (ed.)]. *Science*, 113, 264–266.
- Dobzhansky, T. (1955). Evolution, genetics, and man. New York, NY: Wiley.
- Dobzhansky, T. (1956). The biological basis of human freedom. New York, NY: Columbia University Press.
- Dobzhansky, T. (1961). The bogus "science" of race prejudice [Review of *Race and reason: A Yankee view* by C. Putnam]. *Journal of Heredity*, 52, 189–190.
- Dobzhansky, T. (1962). Mankind evolving: The evolution of the human species. New Haven, CT: Yale University Press.
- Dobzhansky, T. (1963a). A debatable account of the origin of races [Review of *The origin of races* by C. S. Coon]. *Scientific American*, 208, 169–172.
- Dobzhansky, T. (1963b). Possibility that *Homo sapiens* evolved independently 5 times is vanishingly small [Review of *The origin of races* by C. S. Coon]. *Current Anthropology*, 4, 364–367.
- Dobzhansky, T., & Montagu, M. F. A. (1947). Natural selection and the mental capacities of mankind. *Science*, 105, 587–590.
- Douglas, M. (1986). Lita Osmundsen and the Wenner-Gren Foundation: An appreciation. *Current Anthropology*, 27, 521–525.
- Duke, D. (1998). My awakening: A path to racial understanding. Covington, LA: Free Speech.
- Dunn, L. D. (1951). [Review of *Races... A study of the problems of race formation in man* by C. S. Coon, S. M. Garn, & J. B. Birdsell]. *American Anthropologist*, 53, 105–106.
- Dunn, L. C., & Dobzhansky, T. (1946). Heredity, race, and society. New York, NY: Penguin.
- Farber, P. L. (2011). Mixing races: From scientific racism to modern evolutionary ideas. Baltimore, MD: Johns Hopkins University Press.
- Gannett, L. (2013). Theodosius Dobzhansky and the genetic race concept. *Studies in the History and Philosophy of Biological and Biomedical Sciences*, 44, 250–261.
- Garn, S. M. (1962). Human races (rev. ed.). Springfield, IL: Thomas.
- Garn, S. M. (1963). [Review of *The origin of races* by C. S. Coon]. *American Sociological Review*, 28, 637–638.
- Garn, S. M. (1971). Human races, 3rd ed. Springfield, IL: Thomas.
- Garrett, H. E. & George, W. C. (1962). Findings on race cited [Letter to the editor]. *The New York Times*, October 24.
- Gayon, J. (2003). Do biologists need the expression "human races"? UNESCO 1950–51. In J. Z. Rosenberg (Ed.), *Bioethical and ethical issues surrounding the trials and code of Nuremberg: Nuremberg revisited* (pp. 23–48). Lewiston, NY: Mellen.
- Gieryn, T. F. (1983). Boundary-work and the demarcation of science from non-science: Strains and interests in professional ideologies of scientists. *American Sociological Review*, 48, 781–795.
- Gieryn, T. F. (1999). Cultural boundaries of science: Credibility on the line. Chicago, IL: University of Chicago Press.
- Gordin, M. D. (2012a). How Lysenkoism became pseudoscience: Dobzhansky to Velikovsky. *Journal of the History of Biology*, 45, 443–468.
- Gordin, M. D. (2012b). The pseudoscience wars: Immanuel Velikovsky and the birth of the modern fringe. Chicago, IL: University of Chicago Press.
- Gould, S. J. (1974). The nonscience of human nature. *Natural History*, 83, 21–24.
- Haraway, D. (1989). Primate visions: Gender, race, and nature in the world of modern science. New York, NY: Routledge.
- Hazard, A. Q., Jr. (2012). Postwar anti-racism: The United States, UNESCO, and "race," 1945–1968. New York, NY: Palgrave Macmillan.
- Huxley, J. S., & Haddon, A. C. (1936). We Europeans: A survey of "racial" problems. New York, NY: Harper.
- Jackson, J. P., Jr. (2001a). "In ways unacademical": The reception of Carleton S. Coon's *The origin of races*. *Journal of the History of Biology*, 34, 247–285.

- Jackson, J. P., Jr. (2001b). Social scientists for social justice: Making the case against segregation. New York, NY: New York University Press.
- Jackson, J. P., Jr. (2005). Science for segregation: Race, law, and the case against *Brown v. Board of Education*. New York, NY: New York University Press.
- Krimbas, C. B. (1994). The evolutionary worldview of Theodosius Dobzhansky. In M. B. Adams (ed.), *The evolution of Theodosius Dobzhansky: Essays on his life and thought in Russia and America* (pp. 179–193). Princeton, NJ: Princeton University Press.
- Kropotkin, P. (1955). Mutual aid: A factor of evolution. Boston, MA: Extending Horizons.
- Kubie, L. S. (1948). Instincts and homeostasis. *Psychosomatic Medicine*, 10, 15–20.
- Lasker, G. W. (1999). Happenings and hearsay: Experiences of a biological anthropologist. Detroit, MI: Savoyard.
- Latour, B. (1993). We have never been modern (C. Porter, trans.). Cambridge, MA: Harvard University Press.
- Lieberman, L., Lyons, A., & Lyons, H. (1995). An interview with Ashley Montagu. *Current Anthropology*, 36, 835–844.
- Marks, J. (2000). Human biodiversity as a central theme of biological anthropology: Then and now. *Kroeber Anthropological Society Papers*, 84, 1–10.
- Marks, J. (2008). Race across the physical-cultural divide in American anthropology. In H. Kuklick (Ed.), *A new history of anthropology* (pp. 242–258). Malden, MA: Blackwell.
- Mayr, E. (1962). Origin of the human races [Review of *The origin of races* by C. S. Coon]. *Science*, 138, 420–422.
- Mead, M. (1963). Scientist reviewers beware. *Science*, 141, 312–313.
- Montagu, M. F. A. (1942a). The genetical theory of race, and anthropological method. *American Anthropologist*, 44, 369–375.
- Montagu, M. F. A. (1942b). Man's most dangerous myth: The fallacy of race. New York, NY: Columbia University Press.
- Montagu, A. (1951a). On being human. New York, NY: Schuman.
- Montagu, A. (1951b). Statement on race: An extended discussion in plain language of the UNESCO statement by experts on race problems. New York, NY: Schuman.
- Montagu, A. (1952). Darwin: Competition & cooperation. New York, NY: Schuman.
- Montagu, M. F. A. (1955). Time, morphology, and neoteny in the evolution of man. *American Anthropologist*, 57, 13–27.
- Montagu, A. (1963). What is remarkable about varieties of man is likenesses, not differences [Review of *The origin of races* by C. S. Coon]. *Current Anthropology*, 4, 361–364.
- Montagu, A. (Ed.). (1964). The concept of race. New York, NY: Free Press of Glencoe.
- Montagu, A. (Ed.). (1975). Race and IQ. New York, NY: Oxford University Press.
- Nickles, T. (2013). The problem of demarcation: History and future. In M. Pigliucci & M. Boudry (Eds.), *Philosophy of pseudoscience: Reconsidering the demarcation problem* (pp. 101–120). Chicago, IL: University of Chicago Press.
- Popper, K. R. (1959). The logic of scientific discovery. London, England: Hutchinson.
- Price, D. H. (2004). Threatening anthropology: McCarthyism and the FBI's surveillance of activist anthropologists. Durham, NC: Duke University Press.
- Proceedings of the thirty-first annual meeting of the American Association of Physical Anthropologists (1963). *American Journal of Physical Anthropology*, 21, 399–415.
- Provine, W. B. (1986). Geneticists and race. *American Zoologist*, 26, 857–887.
- Putnam, C. (1961). Race and reason: A Yankee view. Washington, DC: Public Affairs.
- Radick, G. (2009). Ashley Montagu: A Darwin critic at Rutgers in the age of McCarthy. Paper presented at Rutgers University.
- Rankin-Hill, L. M., & Blakey, M. L. (1994). W. Montague Cobb (1904–1990): Physical anthropologist, anatomist, and activist. *American Anthropologist*, 96, 74–96.
- Ripley, W. Z. (1899). *The races of Europe: A sociological study*. London, England: Kegan Paul, Trench, Trubner.
- Roe, A. (1953). *The making of a scientist*. New York, NY: Dodd, Mead.
- Selcer, P. (2012). Beyond the cephalic index: Negotiating politics to produce UNESCO's scientific statements on race. *Current Anthropology*, 53, S173–S184.
- Shipman, P. (1994). The evolution of racism: Human differences and the use and abuse of science. New York, NY: Simon & Schuster.
- Simpson, G. G. (1963). [Review of *The origin of races* by C. S. Coon]. *Perspectives in Biology and Medicine*, 6, 271–272.
- Smocovitis, V. B. (2012). Humanizing evolution: Anthropology, the evolutionary synthesis, and the prehistory of biological anthropology, 1927–1962. *Current Anthropology*, 53, S108–S125.
- Sperling, S. (2008). Ashley's ghost: McCarthyism, science, and human nature. In D. M. Wax (Ed.), *Anthropology at the dawn of the Cold War: The influence of foundations, McCarthyism, and the CIA (17–36)*. London, England: Pluto.
- Tucker, W. H. (1994). The science and politics of racial research. Urbana: University of Illinois Press.
- Washburn, S. L. (1944). Thinking about race. *Science Education*, 28, 65–76.

- Washburn, S. L. (1951). The new physical anthropology. *Transactions of the New York Academy of Sciences*, 13, 298–304.
- Washburn, S. L. (1963). The study of race. *American Anthropologist*, 65, 521–531.
- Weidman, N. (2012). An anthropologist on TV: Ashley Montagu and the biological basis of human nature, 1945–1960. In M. Solovey & H. Cravens (ed.), *Cold War social science: Knowledge production, liberal democracy, and human nature* (pp. 215–232). New York, NY: Palgrave Macmillan.
- Wolpoff, M., & Caspary, R. (1997). *Race and human evolution*. New York, NY: Simon & Schuster.