

“Why Did They Die?”

Biomedical Narratives of Epidemics and Mortality among Amazonian Indigenous Populations in Sociohistorical and Anthropological Contexts

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In the second half of the twentieth century, Indigenous populations from different parts of the world were identified as key subjects in a wide range of investigations into patterns of human biological variation. Focusing on Amazonia, a prominent region for such research, this paper explores some of the complex relationships between biomedicine, anthropological knowledge, political regimes, and Indigenous rights. We focus on the roles of Francis L. Black (Yale University) and James V. Neel (University of Michigan), leading American scientists working in lowland South America from the 1970s to the 1990s, in knowledge production and scientific disputes at the intersection of human biology and susceptibility to infectious diseases, as well as the use of specific biomedical technologies (e.g., vaccines). During the Cold War, arguments concerning genetically determined susceptibility to infectious diseases and the role of biomedicine in health care for Indigenous populations became highly disputed, as scientists were concerned about how scientific knowledge could be used in the implementation of public policies. We argue that analysis of unpublished debates about the political implications of the trajectory of biomedical research about Amazonian Indigenous peoples helps to broaden and complexify the global history of human biological diversity research in the post-World War II period.

Introduction

In 1992, 500 years after the beginning of Indigenous population decline that coincided with the arrival of Europeans on the American continent, scientist Francis L. Black from the Yale School of Public Health published a two-page article in *Science* attempting to answer the question “Why did they die?” In this paper, he synthesized three decades of theoretical proposals that he had made concerning the relationship between genetics and immunology in Indigenous populations and the associations with biological responses to introduced infectious diseases (Black 1992).¹

Black, a renowned expert in virology, genetic immunology, and epidemiology, had played a major role in developing the measles vaccine during the early 1960s and subsequently initiated innovative biomedical research in the Amazon, investigating the immunological and clinical response patterns of

Indigenous populations exposed to the vaccine. In all of his Amazonian studies, Black analyzed the biomedical effects of the exposure of Indigenous communities to infectious diseases and, during the final decades of his life, described the radical environmental and sociodemographic changes taking place in Amazonia (Francis L. Black, unpublished manuscript, 1997). In his short 1992 paper, he looked toward the past, offering an explanation for the history of Indigenous death in the Americas as well as anticipating a future in which Indigenous peoples, of Amazonia in particular, would be doomed to a similar fate.

Black’s biological perspective pointed him to a controversial strategy for preventing the disappearance of these communities: increasing levels of admixture of genes between populations. He acknowledged that this solution would not come without trade-offs, noting that “intermarriage between populations reduces the problem, but an unfortunate consequence of intermarriage is often the loss of Indigenous culture” (Black 1992:1740). In contemplating Indigenous futures, Black extrapolated from his science to wager that culture might have to be sacrificed in order to salvage biology.

Like many other researchers setting out in the field of population genetics and biological anthropology in the early 1960s, such as University of Michigan human geneticist James V. Neel—whose work in Amazonia has been widely discussed in anthropological circles and who was also involved with

1. Unlike other scientists such as James V. Neel and Francisco M. Salzano, whose works on the topic of human biological diversity in the Amazon have been analyzed extensively (de Souza and Santos 2014; Lindee 2001, 2004; Salzano 2000; Salzano and Hurtado 2004; Santos 2002; Santos et al. 2014), Black’s work has only recently come under systematic scrutiny (Dent and Santos 2017). Brief comments on Black’s research can be found in Jones (2003, 2004).

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assessing the impact of radiation on survivors of the nuclear bombing of Hiroshima and Nagasaki (see AAA 2002; Borofsky 2005; Lindee 2001, 2004; Neel 1994; Santos, Lindee, and de Souza 2014)—Black belonged to a generation of scientists who focused their investigations on Indigenous peoples from various parts of the world (at the time often referred to as “primitive” human groups). Influenced by ideas emanating from the “new evolutionary synthesis,” a body of theories connecting genetics to evolutionary theory developed in the 1930s and 1940s but investigated extensively in human populations only after World War II, these scientists reframed conversations about race by talking about the production and maintenance of biological variability (Bangham and de Chadarevian 2014; de Chadarevian 2015; Kowal, Radin, and Reardon 2013; Radin 2013, 2014a, 2017; Santos 2002; Suárez, García-Deister, and Vasquez 2017).

It is important to make clear that, until the 1960s, most human biologists working in this paradigm were building on studies primarily conducted on nonhuman organisms like fruit flies. As a consequence, their view of human groups was overdetermined by knowledge that did not, by design, accommodate the complex realities of human life, including kinship, reproduction, and, especially, politics (Murphy 2017; Radin 2017; Smocovitis 2012). Recent historical and anthropological studies have argued that these so-called primitive peoples were identified as key subjects for this wide-ranging intellectual project for a number of reasons, including the assumptions that they possessed genetic evidence of optimal adaptation to the environments in which they lived; that they could, therefore, serve as controls in comparisons with humans exposed to ionizing radiation and other forms of pollution; and that their survival was endangered by these forces, making scientific research urgent (de Chadarevian 2015; Kowal, Radin, and Reardon 2013; Lindee and Santos 2012; Radin 2013, 2017; Radin and Kowal 2015; Reardon 2005 [2002]; Santos 2002; Santos, Lindee, and de Souza 2014; Suárez, García-Deister, and Vasquez 2017).

Despite the underlying values and epistemological similarities uniting these researchers, their views on the appropriate means of intervention varied significantly, in ways that have been elided by the consensus documents produced by the initiatives that coordinated their research, such as the World Health Organization (WHO) and the International Biological Program (IBP; de Chadarevian 2015; Radin 2014b, 2017; Santos, Lindee, and de Souza 2014). Of central significance to our analysis is that Neel and Black disagreed not only over the genetic and biological explanations for the supposedly higher vulnerability of Indigenous peoples to infectious disease epidemics but also over how knowledge of biology “could play directly into the hands of those who do not wish to make adequate provision for the transition of these peoples and rather would prefer to see amalgamation,” as Neel stated in a 1993 letter to Black.²

2. Letter from James V. Neel to Francis L. Black, March 24, 1993, James V. Neel’s papers archived at the American Philosophical Society in Philadelphia.

In this paper, we excavate this specific history—of Neel’s disagreement with Black’s conclusions about Indigenous futures—in order to pose a question of broad relevance: How do scientists reckon with the political implications of the knowledge they make about living human groups? To even pose this question is to acknowledge a state of affairs that is uncomfortable for those who hew to an understanding of scientific knowledge as disinterested and apolitical.³ Nevertheless, since its inception as a science linking the laboratory and the field, human biology has been a crucible for negotiating this and other complex ethical issues (summarized in Radin 2018).

It is imperative for us to situate ourselves as authors in order to make clear how our respective investments in these questions, as well as in the ongoing project of making knowledge in contexts of Indigeneity, implicate us. Ricardo Ventura Santos and Carlos E.A. Coimbra Jr. are Brazilian researchers who, over the past three decades, have been deeply involved in research at the interface between anthropology and public health, focusing mainly on the Indigenous peoples living on land colonized by Brazil. After an initial background in biology, each obtained a doctorate in anthropology in the late 1980s to the early 1990s, specializing in biological anthropology and medical anthropology, respectively. When they returned to Brazil in the early 1990s, Santos and Coimbra became involved with the Oswaldo Cruz Foundation in Rio de Janeiro, affiliated with the Brazilian Ministry of Health, where they continue to work today.

Santos also became a professor at the Department of Anthropology of the National Museum. In the 1990s, issues related to the history of research on human biological diversity in Brazil became a topic of interest for Santos (de Souza and Santos 2014; Santos 2012; Santos and de Mello e Silva 2006; Santos, Lindee, and de Souza 2014). As curator of the biological anthropology archive of the National Museum, he has researched and addressed issues related to the history of the formation of biological anthropology collections in the institution starting in the nineteenth century (see Lindee and Santos 2012; Santos 2012). Subsequently, his interests in the field of the history of science and science studies deepened, coming to include reflections on more contemporary issues, in particular the trajectory of research on the health and biological diversity of Brazilian populations in the period following World War II (de Souza and Santos 2014; Lindee and Santos 2012; Santos 2002, 2012; Wade et al. 2014).

While Coimbra’s research has focused primarily on the field of public health, at various points in his career he became interested in topics widely informed by historical dimensions, including the impacts of epidemics on the demography and

3. The notion of science as disinterested and apolitical was consolidated in the work of Robert K. Merton and is a position that historians, sociologists, and anthropologists of scientists have since demonstrated is untenable. Notable examples include Haraway (1988), Jasanoff (2004), Latour (1987), and Shapin (2010). For a specific critique of Merton, see Hollinger (1996).

social organization of Indigenous peoples, the diachronic transformations associated with the occurrence of infectious parasitic diseases in the Indigenous peoples of South America, and the dynamics of health transitions in these populations (Coimbra 1988; Coimbra et al. 2002). As anthropologists and researchers in the field of public health, Santos and Coimbra share the perspective that historically informed approaches are fundamental for analyzing and participating in contemporary debates; indeed, they are an important aspect of the science of human biology.

Joanna Radin is an American historian of science at Yale who began her career in the twenty-first century. Her research has focused on the ways that postwar human biological projects undertaken by scientists like Neel and Black created unexpected circumstances for the ethics of knowledge production decades later (Radin 2013, 2014a, 2017, 2018). In particular, she investigated the Cold War circumstances that led human biologists to identify Indigenous peoples as destined to disappear and therefore especially important to study. Her work, which is grounded in the history of the IBP, a decade-long project to sample and archive human biological variation, led her to the work of Santos, who had demonstrated important continuities between the IBP and the controversial Human Genome Diversity Project of the 1990s (Santos 2002). Among other things, what linked these two enterprises was what Radin has termed “salvage biology,” the desire to accumulate and preserve traces of Indigenous bodies for “as yet unknown” scientific purposes (Radin 2017). A decade of conversation and collaboration with Santos has been vital for her reckoning with the enduring legacies of this enterprise.

We began this collaboration by puzzling over the emotional intensity that surrounded what, on the face of it, appeared to be a technical debate between two scientists. As we brought our collective expertise to bear on the published and, especially, the unpublished archival record, we came to understand that what initially read as emotions of anger and frustration were traces of how the high political stakes of human biological knowledge had been negotiated through the channels of peer review, only to be expunged from the final publication.⁴ Far from celebrating peer review as a technology of objectivity that ensures the production of the “best” knowledge, we recognize it as a crucial site for understanding how the political and moral commitments of scientists are adjudicated—with varying degrees of success—out of view of most members of the scientific

4. For the purpose of this paper, we have analyzed published academic products (articles, book chapters, monographs, etc.) and documents obtained from Francis L. Black’s papers in New Haven, CT (hereafter referred to as FB-Papers), and from the files of James V. Neel archived at the American Philosophical Society in Philadelphia, PA (hereafter referred to as JN-Papers). At the time that we carried out archival research (2012–2013), both Black’s and Neel’s papers were still unprocessed, with no specific archival identification in each document. When we refer to the archival documents in this paper, in addition to the identification of sender, recipient, and date sent, we list the identification number used in our personal cataloging of the documents (indicated with #).

community (Baldwin 2015, 2018; Burnham 1990; Csiszar 2010).⁵ This, we argue, has led to a misleading assumption that scientific knowledge production does not or should not involve debate over politics (as perpetuated by Merton 1973; Zuckerman and Merton 1971).

In bringing this case to light, we hope to encourage a form of human biological knowledge production that neither subsumes such vital negotiations as epiphenomenal to science nor circumscribes them to back channels of publication, accessible only posthumously through the archival record. Human biology and the anthropological tradition of which it is a part have an important role to play in moving such conversations from the back stage to the scientific front stage (Goffman 1959; Hilgartner 2000) in efforts to make knowledge that is of tremendous consequence to those it has cast as its subjects.

The article is divided into six parts, including this introduction and the concluding “Final Considerations.” Black’s career has not been much investigated from either a historical or an anthropological perspective.⁶ Therefore, in “In Search of ‘Virgin Soil’ Populations,” we provide a brief description of his trajectory, situating it within recent scholarship that has investigated the reasons why “primitive populations” became of interest to post–World War II biomedical science. The other three parts are closely interrelated, examining different instances of intellectual confrontation between Black and Neel over the role of genetic and biological factors in the supposedly higher susceptibility of Indigenous Amazonian populations to infectious diseases. These debates span a period of nearly four decades (1970s–2000s) and were influenced by Neel’s and Black’s respective understandings of sociopolitical contexts in Brazil and how they related to governmental initiatives and policies toward Indigenous peoples. We conclude the paper by calling attention to the fact that, within the sphere of biomedical science in Amazonia in the 1960s–1980s, the interplay between genetically determined susceptibility to infectious diseases and the role of biomedical medicine was highly disputed, at the same time that scientists were concerned about how scientific knowledge might be interpreted and used.

In Search of “Virgin Soil” Populations

Francis L. Black was born in Taipei, Taiwan (formerly Formosa), in 1926 to Canadian parents who had moved to Asia in the 1910s to work at a Presbyterian medical mission. A few years before the outbreak of World War II, the young Black returned to Canada

5. In the 1960s and through the 1980s, when Black and Neel were publishing widely on their research in Amazonia, the peer review system in biomedicine was often anonymous (on the reviewer side). More recently, an increasing number of biomedical journals use what is called an “open peer review process”; i.e., authors and reviewers do not stay anonymous to each other, as named reviewer reports are published alongside the article (Smith 2006).

6. For an exception, see Dent and Santos (2017).

to complete his educational studies. He received a degree in chemistry at the University of British Columbia in Vancouver in 1940 and later obtained a doctorate in biochemistry at the University of California, Berkeley. Black became interested in the then-emerging science of virology during his time as a graduate student and would build his entire career in this area. In the 1950s, he completed a postdoctoral fellowship at the Yale University School of Medicine, working mainly on human viruses, particularly poliomyelitis, and subsequently took up a position as a virologist at the Laboratory of Hygiene in Ottawa, Canada. In 1955, he returned to Yale, where he retired as professor of epidemiology (microbiology) in 1997.⁷ Black died in 2007 at the age of 81.

In the 1950s, Yale possessed one of the most prestigious research centers in the field of serological epidemiology, headed by John Rodman Paul, an influential scientist who proposed the use of serum banks for the purpose of epidemiological surveillance (Paul and White 1973; Radin 2014b). Black would donate the samples he collected to the Yale serum bank. In his early years as faculty at Yale, Black studied the measles virus, focusing on both its biological and epidemiological aspects, while carrying out fieldwork mostly in the United States (Black 1959; Francis L. Black, unpublished manuscript, 1997). After attending a meeting sponsored by WHO on the measles vaccine in the early 1960s (WHO 1963), Black became increasingly involved in vaccine field trials and epidemiologic research in several countries, including Brazil (Black and Gudnadóttir 1963; Black and Rosen 1962, 1966; Niederman et al. 1967).

In addition to studying the clinical and immunological responses of vaccinated individuals, Black was also concerned with the relationship between response patterns and the genetic and demographic history of human populations. His insights led to the formulation of an influential hypothesis set out in a paper published in the *Journal of Theoretical Biology*, where he argued that measles must be a recent infection in humans since it requires large population sizes to persist. He estimated that measles could not have “predate[d] the rise of the great river valley civilizations some 5000 to 6000 years ago” (Black 1966:210).

Black realized that field trials aimed at investigating the safety, efficacy, and applicability of the measles vaccine (WHO 1963:5) could offer an opportunity to better understand clinical and immunological responses to measles infection in previously unexposed human populations. His theoretical ideas were closely associated with issues related to the use of a vaccine

developed using attenuated live measles viruses in human populations that had never previously been exposed to this pathogen but that were increasingly exposed to epidemics of introduced infectious diseases as a result of industrialization and modernization projects in many parts of the world. Working at the interface between theory and practice, Black went on to conduct studies based on his idea that “the vaccine thus provides a model of natural measles which can be ethically administered at a time and place chosen to minimize adverse extraneous influences and to facilitate observation of the effects” (Black et al. 1969:169).

A key aspect of his strategy was to locate purportedly isolated communities that were thought not yet to have suffered from measles epidemics or that had been exposed to the virus only in the distant past, meaning that present-day individuals no longer exhibited immunity against the virus through the presence of antibodies. This is an approach that was referred to and has since been critiqued as the search for “virgin soil” populations (Jones 2003).

The first major study by Black to follow this line of investigation involved analyzing samples from people native to Tahiti, part of French Polynesia. However, it became apparent that the region had already suffered a measles epidemic before the field research could be undertaken (Black and Rosen 1962), prompting Black (unpublished manuscript, 1997) to conclude that “we were too late and measles got in first” (31).

Black subsequently took part in field trials in Iceland, where different types of measles vaccines were being tested (Black and Gudnadóttir 1963; Gudnadóttir, Black, and Jonsdóttir 1966). Although the country’s detailed health records confirmed the absence of a measles epidemic among the rural population for many decades, Black (unpublished manuscript, 1997) was forced to admit that “by virtue of the past epidemics and their European heritage . . . they [the Icelanders] were not truly ‘virgin soil’” (33).

An important opportunity for Black came in 1966 in the form of an invitation from virologist Jack Woodall to join a research team working at the Instituto Evandro Chagas in Belém, Pará State, in the Brazilian Amazon region (Francis L. Black, unpublished manuscript, 1997:37). Some months earlier, Woodall had taken part in an expedition to the Tiriyo, an Indigenous group living in the border area between Brazil and Suriname. Analysis of blood samples had indicated no previous exposure to measles. Within weeks of receiving the invitation, Black was ready to take part in a field study in the Amazon designed to test “measles vaccine reactions in a virgin [soil] population” (Black et al. 1969:168). This field study represented the first of many expeditions to Brazil that Black would undertake over the next three decades.

From a methodological standpoint, the study design for the Tiriyo population was similar to the methods employed in Iceland, with one group being vaccinated against measles at the start of the study, while the second (control) group did not receive the vaccine until data collection had been completed (Black et al. 1969; Van Mazijk, Pinheiro, and Black 1982).

7. Biographical data obtained from the unpublished memoir *Inconveniences Properly Viewed* by Francis L. Black (unpublished manuscript, 1997). As explained by Black, the title is inspired by the phrase “An adventure is an inconvenience properly viewed” by H. L. Mencken. Black wrote that the memoir was “prepared on the occasion of our fiftieth wedding anniversary and my mother’s hundredth birthday.” We thank Douglas and Peter Black for providing a copy of this important document. See also Francis L. Black’s obituary at <http://archives.news.yale.edu/v35.n17/story19.html>, accessed November 11, 2019.

Clinical conditions (temperature and signs of vaccine reactions, including the occurrence of fever and rashes, where applicable) were closely monitored. Blood samples were subsequently collected from the vaccinated group for later immunological analysis in order to assess the levels of seroconversion.

In contrast to the high level of predictability of the research conditions in Iceland, Black’s team faced a number of unanticipated problems in Brazil. Not only were the Tiriyo people in a constant state of mobility, but also a flu outbreak affected the population during the vaccination trial, confounding the results. Consequently, the researchers were left unable to explain why the vaccinated group exhibited a higher frequency of fever compared with the control group. Although the results suggested that vaccination of the population was safe, thus confirming findings from previous trials in various other parts of the world, it proved impossible to establish with a reasonable degree of certainty whether the differences observed between the groups were due to genetic factors: “A genetic difference is, of course, only one of the several possible explanations for this remaining difference in reaction rates” (Black et al. 1969:174–175).

Following the Tiriyo study, immune responses to the measles vaccine among Indigenous peoples in the Amazon became a central focus of Black’s research, even though a fundamental criterion for such studies, namely, the availability of “isolated” native populations, was becoming increasingly difficult to fulfill (see Black et al. 1971; Francis L. Black, unpublished manuscript, 1997).⁸ Between the late 1960s and the early 1970s, the Brazilian government, at that point a military dictatorship, launched major development projects in the Amazon, leading to “permanent contact” with dozens of Amazonian populations and, in most cases, to epidemics of infectious disease and high mortality rates (Coimbra 1987; Hemming 1987). In many cases, epidemics occurred even without direct contact between Indigenous and non-Indigenous peoples (Black and Jacobson 1986; Coimbra 1987; Posey 1994). For Black, who was active throughout the 1960s in WHO working groups on measles, his science had become a race against time.⁹

8. Black worked in various South American countries, including Chile, where he investigated the immune response of the Mapuche tribe to the measles vaccine. In Chile, Black faced many logistical difficulties in the wake of the military coup against President Salvador Allende (Francis L. Black, unpublished manuscript, 1997:41–44).

9. In this paper, we focus primarily on the research carried out by Black and Neel, but it is important to mention that several Brazilian researchers and physicians, in particular, were also investigating the clinical and immunological responses to measles vaccines in Indigenous populations (see Baruzzi et al. 1971; Vieira-Filho 1970). Because of the high mortality rates observed in Indigenous populations, the relationships between exposure to endemic diseases and the measles vaccine immune response persisted as a topic of research in the early 2000s (Spindel et al. 2001). See Strebel et al. (2013) for an overview of the development of measles vaccines over the past decades.

“May I Congratulate You . . . But . . .”

After concluding the Tiriyo study and various other projects, mainly epidemiological in nature and involving different Amazonian populations, Black became increasingly interested in the interface between immunology, health status, and population genetics.¹⁰ Among other topics, his investigations in the early 1970s focused on establishing whether patterns of genetic variability, assuming that nutritional conditions were adequate, could help explain immune responses to infectious diseases in general and measles in particular (see Black et al. 1977). These were questions that were of central interest to human biologists during this time. For these researchers, Indigenous peoples were primarily research subjects who would help to demonstrate fundamental biological principles. It was their purported scarcity, associated by scientists with forces of modernity (forces that they were, of course, a part of) that first jeopardized Indigenous peoples as a biomedical resource, at the same time that scientists often emphasized that they deserved protection and medical attention in their own right (Radin 2017; Radin and Kowal 2015; Santos 2002; Santos et al. 2014).

Black presented the first synthesis of his Amazonian studies in 1976 at a CIBA Foundation symposium held in London and titled *Health and Disease in Tribal Societies* that was fairly representative of contemporary biomedical research on Indigenous populations in the 1970s. The instrumental dimensions of concern for Indigenous peoples were made clear by the organizer’s hope that:

we may formulate ideas about working with primitive people, where to look in order to find them, how to protect them when we go there, what information we ought to collect, how to collect and record it and why to preserve these people (not only ethically, because that is obvious), but because there is much to be gained for ourselves. (Hugh-Jones 1977:3)

In his contribution, Black presented the results of the Tiriyo study conducted in 1966 along with findings from more recent studies on the Kayapó, Parakanã, Asurini, Arara, and others (Black et al. 1977). These Indigenous groups in Pará State had recently come into permanent contact with non-Indigenous populations as a result of the expansion of demographic and economic frontiers in central Amazonia (fig. 1). Black developed his presentation around the argument that “the vaccine reaction was stronger in all three virgin-soil groups [the Tiriyo and the Kayapó subgroups Xikrin and Mekranoti] than in any of the more cosmopolitan populations” used as comparisons (Black

10. It is not our aim in this paper to provide a detailed account of Black’s research trajectory in the Amazon. For a personal account of his more than three decades of research in the region, see his unpublished manuscript from 1997. Some of the most influential theoretical perspectives derived from his Amazon research are presented in Black (1975, 1994a) and Black et al. (1974). See also Dent and Santos (2017).

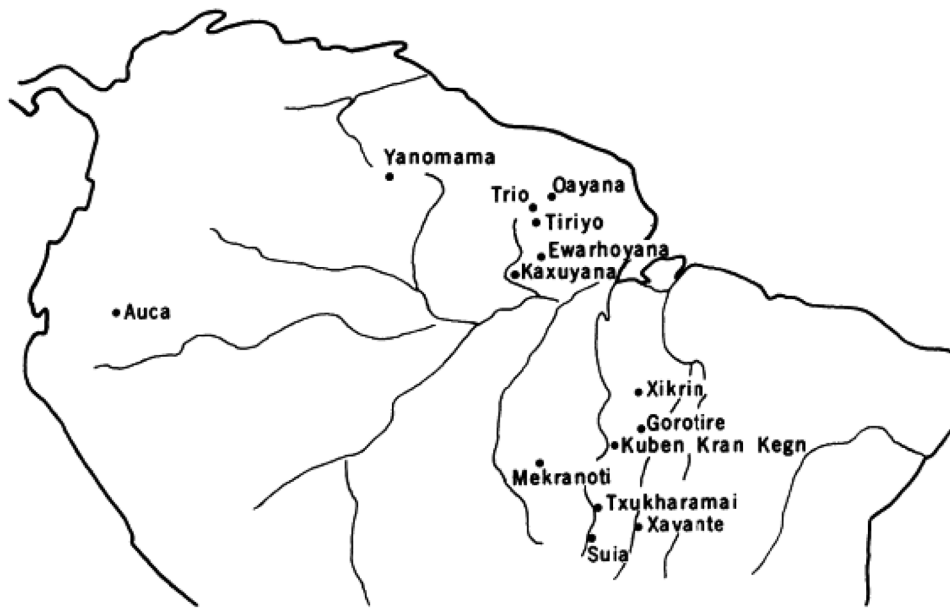


Figure 1. Map of northern South America, showing location of Amazonian Indigenous populations researched by Francis L. Black in the 1960s and 1970s (source: Black 1975:516; reproduced with permission).

et al. 1977:122–123). The idea that “virgin-soil populations are poorly nourished and this leaves them ill-equipped to resist measles virus” (Black et al. 1977:123) was one he rejected in favor of genetic explanations. In particular, he pointed to emerging research on a then-new genetic system associated with immune response, the human leukocyte antigen (HLA) system.¹¹ Black had been working in collaboration with Brazilian geneticist Francisco M. Salzano, and the two scientists posited that:

if . . . homozygosity of the HLA region is associated with enhanced susceptibility to certain infectious diseases, these people will continue to require special medical consideration, by comparison with more diversified populations. (Black et al. 1977:128–129)

In other words, Black was suggesting that the genetics of Indigenous peoples made them more biologically vulnerable compared with “more diversified” populations. In his view, under the circumstances of modernization, the fragility of Indigenous biology would demand continuous “medical consideration” in the face of exposure to introduced infectious diseases. In his presentation at the CIBA symposium, Black was doing more than just stating the findings of his genetic research. He had also begun to articulate a framework that included public health recommendations for acting on these findings that would be made most explicit almost two decades later in “Why Did They Die?” (Black 1992; see also Black 1994a, 1994b). The first step, resulting from this HLA research, was to suggest that, if his and Salzano’s hypothesis of “enhanced

susceptibility” were to be proven, special medical intervention would be necessary. However, by the 1990s, when discussing Indigenous health in the Americas on a much broader scale, Black proposed a more controversial, far-reaching social and demographic measure. He argued that promoting miscegenation or intermarriage between groups would diversify the gene pool, enhancing immunity at the level of the genome. That is, through time, Black came to encourage alterations in biology as a strategy for promoting Indigenous survival, a kind of positive eugenics oriented toward optimization of the body in accordance with circumstances of colonial modernity (Comfort 2012; Kevles 1986).

The 1976 symposium discussion of Black’s presentation commenced with James V. Neel (1977a) remarking, “May I congratulate you [Black] on a very cautious statement of the issue of genetic susceptibility!” (130). This apparent compliment belied Neel’s deeper concerns with Black’s analysis of the preliminary HLA data. While some participants aligned with Black over the potential relevance of genetic factors in explaining mortality levels (as British virologist David A. J. Tyrell [1977] mentioned, “I would be astonished if there was *not* an important genetic element in the way such isolated populations behave in response to infections of various sorts” [132; emphasis in original]), others called attention to the potential role of impaired nutritional status and “breakdown of life in the community” resulting from disease exposure. Neel (1977a) did not participate much in the discussion about Black’s paper compared with other participants, but at some point in the debate, he anticipated what he would stress in his own paper: “I shall be at some pains to challenge the theory that the apparently great susceptibility of primitive peoples to the infectious diseases of civilization is primarily genetic in origin” (132).

11. See Thorsby (2009) for a short account of the history of research on the HLA system.

In contrast to Black's field studies, those carried out by Neel in the Amazon in the 1960s did not focus specifically on the immune responses of Indigenous populations to the measles vaccine. However, an event toward the end of the decade drew his attention to this topic.¹² In 1968, while planning a trip to study a Yanomami group living near the Brazil-Venezuela border, Neel received news of a measles outbreak in the area (Neel et al. 1970:421). Consequently, he took with him approximately 2,000 doses of "Edmonston strain measles vaccine, with the intention of vaccinating as many Indians as possible towards the end of the expedition's more scientific objectives" (Neel et al. 1970:421). Having noted that "the arrival of our expedition in Venezuela coincided with the introduction of measles to the Yanomama of the Upper Orinoco by a young Brazilian" (Neel et al. 1970:421), Neel and his team spent several weeks visiting various villages where, in addition to conducting scientific studies, they vaccinated a small group of individuals, recording their body temperature measurements and postvaccination symptoms.¹³

Neel and his collaborators analyzed their observations concerning the Yanomami measles epidemic in an article titled "Notes on the Effect of Measles and Measles Vaccine" (Neel et al. 1970:418). Unlike Black's investigations involving the Tiriyó and other Amazonian populations, which were directly oriented toward understanding measles, Neel's broader research was not focused on measles, hence his remark that "our observations are thus of necessity disjointed and fragmentary" (Neel et al. 1970:421).¹⁴ In fact, the Yanomami study had no "control group," and postvaccination temperature and clinical data were obtained from just a few dozen individuals. Neel et al. (1970) concluded that "the combined evidence suggests that the reaction of the Indian to measles vaccine is somewhat greater than that of Micronesians, Africans, or United States Caucasians" (427). However, they stressed that their findings could not necessarily be attributed to genetic factors since the pattern of fever and other clinical symptoms may have been "surely due to secondary factors" (Neel et al. 1970:427), indicating their "skepticism concerning the primary [innate] susceptibility of the Indian" (428).

The most significant implication of this conclusion was that there was "no theoretical basis for accepting less than optimal

results in the management of these diseases in newly contacted groups" (Neel et al. 1970:428). Neel wanted to be clear that his scientific findings should not be used to deny medical care, such as a measles vaccine, to these groups once they had been contacted, either by scientists or by settler colonial administrators. He was disturbed by an assumption that "the well-documented collapse of their health, such as it is, which often follows contact with civilized populations" (Neel 1977c:155) was genetically determined. He was not convinced that the members of these groups were genetically destined to die upon contact with "the epidemic diseases of civilization to which they have not previously been exposed and to the 'stresses' of civilization" (Neel 1977c:155).¹⁵

Neel did not present his own scientific evidence against the hypothesis of Indigenous genetic susceptibility but sought instead to highlight the lack of quality evidence in support of any such argument. He also did not discount the potential role of genetic factors. Indeed, he attributed a fairly considerable influence to them. His "guesstimate" was that the figure was close to 20%, as "at least 80% of the high mortality among some primitive groups from measles, smallpox, influenza, tuberculosis, and so on is the consequence of their socioeconomic and epidemiological structure, not their genes" (Neel 1977c:155–156). In Neel's (1977a) view, given the methodological difficulties involved in carrying out any systematic study of the question and the absence of convincing evidence in the literature, it was better to assume that genetic susceptibility was not the major influencing factor, a position that would be "much less conducive to complacency" (166) on the part of health authorities.

Neel was resisting what historian David S. Jones (2004) has documented as a long and pervasive tendency for settler colonists to "rationalize epidemics," to justify as preordained the demise of groups whose way of life impeded the colonial project. "So long as we believe in innate susceptibilities on the part of Amerindian populations," Neel (1977c) lamented, "we can mentally justify poor medical statistics" (166). What Neel was at pains to do was not to demonstrate why Indigenous peoples appeared to die after being confronted with settler colonialism but to make it impossible for his genetic knowledge to be used to avoid preventing death. He was also concerned

12. In addition to Neel's (1994) autobiography, there is a growing historical and anthropological literature regarding his studies in South America (see Lindee 2001, 2004; Salzano 2000; Salzano and Hurtado 2004). More recently, Santos et al. (2014) have analyzed Neel's research within the context of the Cold War.

13. Later in this paper we will analyze, on the basis of the documents in FB-Papers, Black's involvement in debates that followed publication of Patrick Tierney's book *Darkness in El Dorado*, in which his staunch defense of Neel is revealed.

14. Other researchers investigating clinical and immunological responses to measles vaccines in Amazonian Indigenous populations at the time included Baruzzi, Abdala, and Black (1982), Baruzzi et al. (1971), and Vieira-Filho (1970).

15. At least since the 1940s, several Brazilian and foreign anthropologists, including Franz Caspar, Gioconda Mussolini, Darcy Ribeiro, and Charles Wagley, just to mention a few, described in their ethnographic accounts the strong consequences of measles epidemics in Indigenous communities in Brazil (see an overview of these accounts in Coimbra [1987]). Starting in the 1960s with the development of the measles vaccine, the issue of health care attention during measles epidemics became central to practical initiatives going on in several Indigenous territories in Brazil, with major health policy implications. For example, working in Xingu Indigenous Park, physician Noel Nutels reported that, during measles epidemics, communities that received health care presented much lower mortality rates (Nutels 1968).

that biological arguments related to “immunological deficiencies” could be used as a justification for the state to deny adequate health services to Indigenous peoples (Neel 1974, 1994).

The stakes of Neel’s position became clear when Black suggested that Neel was pushing the genetic dimension into the background. In his comments at the 1976 CIBA meeting, published along with the scientific presentations, Black attempted to pull Neel’s attention back to the question of genetics (Black 1977a, 1977b). He said:

You made the point, with which I would agree, that the social disruption of an epidemic has an important effect on the outcome, and is something that can be dealt with. You avoided the genetic element, except that you did notice clinically that the cases of measles were more severe than usual. You left open the question of whether there are other factors as well. (Black 1977b:171)

It bears emphasizing that, in view of Neel’s long career of conducting genetics research on members of marginalized communities—African Americans living with sickle cell anemia and Japanese survivors of atomic bombings (see Lindee 1994, 2001, 2004; Neel 1994)—the charge that Neel had “avoided the genetic element” suggests that Black did not share the perspective of the value of tempering the argument in the case of Indigenous Amazonian communities, of erring on the side of assuming, in the face of underdetermined evidence, that genetics were not influencing immunity. Indeed, Neel responded to Black:

As a geneticist I spend much of my life arguing against invoking genetic susceptibility to explain poorly understood situations, and I am not convinced that we need invoke any special genetic susceptibility in this situation, although I don’t exclude it either. (Neel 1977b:171)

The tension between Black and Neel would persist over the next three decades, albeit less publicly than during the 1976 CIBA Symposium. Both Neel’s and Black’s primary motivation for making knowledge about these groups—an epistemological a priori that they shared—was that such groups could serve as “natural laboratories” for producing human biological knowledge (Radin 2017). However, while the debate at the CIBA symposium appeared to focus primarily on epistemological differences—the relative importance that Neel and Black gave to genetic roots of susceptibility to infectious disease—the stakes were also ethical and political. There were consequences of linking virgin soil narratives with genetic determinism in accounting for Indigenous peoples’ ability to negotiate epidemic infectious disease. Neel and Black were among the few with the technical expertise to argue this rather subtle point at the time.

At the same time that the discussions between Black and Neel involved explicit references to the health and living conditions of Indigenous peoples related to contemporary political contexts in various South American countries, including Brazil, they suggest that these two self-described democratic American scientists were differently attuned to the potential ethical and political

implications of their scientific conclusions.¹⁶ Before returning to a discussion of these differences, it is essential to provide an overview of the political circumstances that unfolded in Indigenous Amazonia over the years of the research and associated debates between Neel and Black over the question of “Why did they die?” and what they meant for future survival.

Military Rule and Indigenous Amazonia

For significant periods between the 1960s and 1990s, military governments ruled in a number of South American countries (Dávila 2013; Fausto 2014; Skidmore 1988). In Brazil, during military dictatorship (1964–1985), the issue of Indigenous lands, especially those located on the borders with neighboring countries, gained prominence as a matter of “national security” (Ramos 1984, 1998). The so-called Indian Statute (Law 6001) was promulgated in 1973 during the period in which the Fundação Nacional do Índio (FUNAI), the federal agency responsible for implementing Indigenous policy, was also run by the military (Carneiro da Cunha 1992; de Souza Lima 1995; Garfield 2001; Hemming 1987; Ramos 1984, 1998).¹⁷ One of the key pillars of this law was assimilation of Indigenous people into Brazilian society (Carneiro da Cunha 1992; de Oliveira Filho 1990; de Souza Lima 1995; Ramos 1984, 1998). The notion of assimilation in Brazil dates from the early twentieth century (de Souza Lima 1995) but gained momentum under the military government because the presence of Indigenous populations in the vast Amazonian region was considered an obstacle to development objectives. One of the slogans of the hard-line government was “*integrar para não entregar*” (“integrate to avoid relinquishing control”) since it was judged to be of the utmost importance to occupy the vast “demographic emptiness” of the Brazilian territory, including those areas occupied by Indigenous peoples.¹⁸

Field research projects undertaken from the 1960s to the 1970s by Black, Neel, Salzano, and many other geneticists, physicians, and anthropologists in Amazonia occurred, therefore, at a time when development initiatives were being prioritized by the Brazilian government. New roads and highways were constructed, hydroelectric power plants were built, and programs were implemented to settle migrants mostly arriving

16. This was an issue that also deeply concerned Brazilian scientists working with the health of Amazonian Indigenous populations at the time (see Baruzzi et al. 1971; Coimbra 1987; Vieira-Filho 1970).

17. Created in 1967, FUNAI is the Brazilian federal agency responsible for carrying out public policies aimed at Indigenous peoples. Over recent years, education and health policies are no longer under FUNAI, which at present retains the legal responsibility to oversee issues related to demarcation and territorial and environmental protection of Indigenous reserves and continues to be the government body responsible for issuing permissions for scientific research in Indigenous territories.

18. See de Oliveira Filho (1990), de Souza Lima (1995), and Ramos (1984, 1998) for detailed analyses of the military regime’s policies regarding Indigenous peoples.

from northeastern and southern areas of the country. During this time, thousands of Indigenous peoples who had previously lived in relative isolation were exposed to the devastating impact of contact with the forces of modernization and agricultural expansion (Garfield 2001; Hemming 1987; Price 1989; Ramos 1984).

The field studies undertaken by Black and Neel in Amazonia temporally overlapped, though there were some important differences: Neel’s field studies took place mainly in the second half of the 1960s and the first half of the 1970s, when the geneticist and his associates shifted their research focus from central Brazil (where they had studied, e.g., the Xavante population) to the Brazil-Venezuela border (investigating the Yanomami; Coimbra et al. 2002; Lindee 2001, 2004; Neel 1994; Santos et al. 2014).

Neel’s interest in working with such groups grew from his collaboration with Brazilian geneticist Francisco M. Salzano. As Salzano endeavored to develop a research agenda in population genetics in Brazil, he drew inspiration from Neel’s encouragements to study Indigenous peoples and to seek sponsorship from the Rockefeller Foundation (de Souza and Santos 2014; Salzano 1991; Santos et al. 2014). Before long, Neel had been approached by a delegate of WHO to conduct a pilot project with Salzano. Neel, who was still involved with his studies in Hiroshima (Lindee 2001, 2004; Neel 1994), began to appreciate the possibilities of studying groups—such as those in Indigenous Amazonia—whom he believed to be as yet uncontaminated by ionizing radiation and who could provide important scientific information for the understanding of microevolutionary mechanisms in the human species (see de Chadarevian 2015; Lindee 2001, 2004; Neel 1968, 1970, 1974, 1994; Radin 2013, 2017; Santos 2002). Under WHO’s sponsorship, Neel coordinated an expert committee responsible for producing the report “Research in Population Genetics of Primitive Groups” (WHO 1964). A few years later, with the support of the Pan American Health Organization, he coordinated the thematic session Biomedical Challenges Presented by the American Indian (Neel 1974).

While Neel’s work in South America was undertaken mostly in the 1960s and early 1970s, when the Brazilian dictatorship was intensifying, Black did not become deeply involved in field research in the Amazon until later, throughout the 1970s and 1980s. Liberalization of the Brazilian military regime’s control occurred toward the end of this period, accompanied by a movement toward redemocratization. In the late 1970s and early 1980s, as hydroelectric power plants were planned to be built in the Indigenous territories in which Black had worked for several years, he became more directly involved with some of the many national and international nongovernmental organizations (NGOs) emerging in defense of Indigenous rights.¹⁹

At the same time that Black was supportive of the causes of Indigenous peoples, he was concerned that his political

involvement could jeopardize his scientific research in Brazil.²⁰ In the 1970s, he became affiliated with the NGO Survival International (USA), an Indigenous protectionist organization with headquarters in New York City.²¹ In a 1980 letter to its president, George Appell, Black wrote:

If I can help, it will be more on the technical side. My interests have been in the factors which cause diseases that are well tolerated in cosmopolitan populations [but] cause severe burdens on more primitive people. . . . You mention a wish to be able to give emergency vaccinations to combat epidemics. I might be able to help in planning such enterprises. . . . I don’t know how much I’ve permitted fear of antagonizing my hosts in the South American dictatorships to hush my feeling of concern for the primitive people, but I have generally preferred to work with the national organizations rather than criticize them.²²

In 1979, he wrote a letter to Brazilian physician Roberto G. Baruzzi expressing his interest in joining the Comissão Pró-Índio, a newly formed NGO based in São Paulo and under the direction of anthropologist Manuela Carneiro da Cunha, stating:²³

I am very interested in the Comissão Pro-Índio. This looks— from its membership—like a group that can do some good for the Indians without antagonizing the government and causing an adverse reaction.²⁴

A few years later, Black would express similar sentiments to geneticist Francisco M. Salzano, who was at that point a prominent scientist and who often participated in debates about Indigenous peoples’ rights in Brazil:

You will know that there are various groups trying to work for the welfare of the Indians who often take an adversarial

20. During the Brazilian military dictatorship, there were several instances when anthropologists had difficulties obtaining permissions from FUNAI to carry out fieldwork in Indigenous areas in Brazil or were even expelled from Indigenous territories, as they were seen as a threat to the political regime (see CEDI 1982:85–87, 1983:107; Dent 2017; Ramos 1998:9).

21. There is a specific folder in Francis L. Black’s papers named “Survival International,” with correspondences that span from the mid-1970s to the mid-1980s.

22. Letter from Francis L. Black to George N. Appell, president of Survival International (USA), April 10, 1980, FB-Papers (#01144 and #01145). Appell replied to Black a few weeks later: “[Survival International] is not really a confrontational type of organization. In many instances I don’t think it does any good to be shouting criticisms to the press, and in fact I think that this might even be of detriment to the Indians themselves” (George N. Appell, letter to Francis L. Black, May 22, 1980, FB-Papers [#01141 and #01142]).

23. See also the letter sent by Manuela Carneiro da Cunha, president of Comissão Pró-Índio, addressed to colleagues to communicate the creation of the Comissão, July 3, 1979, FB-Papers (#01057, #01058, and #01059).

24. Letter from Francis L. Black to Roberto G. Baruzzi, November 20, 1979, FB-Papers (#01056).

19. Letter from Francis L. Black to George N. Appell, president of Survival International (USA), April 10, 1980, FB-Papers (#01144 and #01145).

approach criticizing FUNAI and the government for hastening acculturation or other policies which may or may not be right. As a foreigner, I prefer to keep out of these controversies.²⁵

Black's awareness that his research had political and ethical stakes required him to articulate an appropriate stance vis-à-vis his research subjects. He was certainly aware of the differences among the various complex positions shaping debates about Indigenous rights in Brazil. Yet Black's own orientation is telling. At the same time that he demonstrated awareness of the fraught situation of Indigenous populations (e.g., he wrote to Appell that "there is one situation that particularly concerns me in Brasil. The Tocantins dam at Tucuruí will, within five years, flood most of the Paracaná Reserve"),²⁶ he was concerned that being drawn into political controversies could eventually jeopardize his research in Brazil. Indeed, Black would maintain intense fieldwork activity over the next two decades without facing any major barrier from the Brazilian government. However, on another level, he would not be "out of these controversies." Black's sense of where the politics of his knowledge production enterprise lay was, as we will see, ultimately at the root of his tensions with Neel, which reemerged in the 1990s.

Performing Concern: The Politics of Making Human Biological Knowledge

In the early 1990s, Black participated in the symposium American Indian Change after European Contact at the Annual Meeting of the American Association for the Advancement of Science. On the basis of his presentation at the meeting and the fact that he had for decades been concerned about the inadequacy of available explanations for "why the Indigenous people of this hemisphere died in such numbers" following the European colonization of the New World, Black submitted a paper to *Science* titled "Low Polymorphism at HLA Put New World Populations at Risk from Variant Pathogens?" (Francis L. Black, unpublished manuscript, 1992).²⁷

The main argument of the paper submitted to *Science* was that neither "lack of resistance due to inappropriate genetic traits" nor "social disruption caused by the sudden onslaught of each new disease" (Francis L. Black, unpublished manuscript, 1992:3) would be sufficient factors to explain the supposedly increased susceptibility of Indigenous populations to introduced infectious diseases. On the basis of emerging knowledge in immunogenetics, Black (unpublished manuscript, 1992) argued for an "advantage of [human genetic] polymorphism in countering

pathogen mutability" (11). According to him, compared with more cosmopolitan populations, the reduced genetic heterogeneity of Indigenous populations, as expressed in HLA system polymorphisms, could compromise their immunological response to disease agents and in particular to viruses that can swiftly adapt to human populations (Francis L. Black, unpublished manuscript, 1992).

The paper was sent out for peer review but was rejected for publication.²⁸ One of the reviewers drew attention to the "interesting speculation of the dynamic interface between Major Histocompatibility Complex variation and mutability of pathogenic organisms" but concluded that "several pieces of evidence necessary to make it convincing are lacking," in particular evidence that the degree of death caused by epidemic disease in the New World was indeed significantly greater than that occurring simultaneously in the Old World.²⁹ Another criticism was that the summary of genetic data (HLA frequencies) presented by Black to support his hypothesis was "based on serological typing, some of it quite old," thus compromising inferences about genetic diversity profiles of New and Old World populations.³⁰

Yet in recognition of the timeliness of the manuscript in the context of the 500-year anniversary of the arrival of Europeans in America, *Science's* editors offered Black the possibility of publishing a more succinct and less technical article.³¹ This led to the publication of the two-page paper "Why Did They Die?," to which we referred in the introduction. According to Black, this was "an attempt to adopt a more journalistic style" than his customary method of presenting scientific findings.³²

Black also decided to submit a revised and expanded version of the full manuscript, under the new title of "An Explanation of High Death Rates among New World Peoples When in Contact with Old World Diseases," to the journal *Perspectives in Biology and Medicine*. The new version basically included the same genetic and immunological arguments presented in the previous version sent to *Science* (Francis L. Black, unpublished manuscript, 1992). However, Black greatly expanded the comments on the social and public health implications of his ideas for Indigenous populations. He wrote:

This paper presents the thesis that people of the New World are unusually susceptible to the diseases of the Old not just

25. Letter from Francis L. Black to Francisco M. Salzano, July 28, 1984, FB-Papers (#04866 and #04867).

26. Letter from Francis L. Black to George N. Appell, president of Survival International (USA), April 10, 1980, FB-Papers (#01144 and #01145).

27. Letter from Francis L. Black to the editor of *Science*, May 28, 1992, FB-Papers (#01115).

28. Letter from Katrina L. Kelner, *Science* editorial office, to Francis L. Black, August 26, 1992, FB-Papers (#01109).

29. Reviewer's comments to *Science* on Black's manuscript "Low Polymorphism at HLA Put New World Populations at Risk from Variant Pathogens," undated, FB-Papers (#01112).

30. Reviewer's comments to *Science* on Black's manuscript "Low Polymorphism at HLA Put New World Populations at Risk from Variant Pathogens," undated, FB-Papers (#01112 and #01113). Another reviewer wrote, "There is no new data presented in the manuscript and it cannot, therefore, be considered a research article or report" (#01113).

31. Letter from Katrina L. Kelner, *Science* editorial office, to Francis L. Black, August 26, 1992, FB-Papers (#01109).

32. Letter from Francis L. Black to Katrina L. Kelner, *Science* editorial office, September 4, 1992, FB-Papers (#01110 and #01111).

because they lack any special resistance, not only because the introduced diseases are novel, but primarily because, as populations, they lack heterogeneity. They are susceptible because agents of disease can adapt to each population as a whole and cause unusual damage. An implication is that racial mixing is the best way to preserve the genetic traits of the New World people, and the relative success of mixed populations has been noted. If the theory is correct, mixing individuals would serve as well as mixing genes, but, either way, Indigenous cultures are likely to be lost. For the people most affected there is a choice, but it may be a bitter one. (Francis L. Black, unpublished manuscript, 1993:15–16)

Neel happened to be a member of the editorial board and was selected as one of the paper’s reviewers.³³ Reiterating some of the same arguments that he had expounded on two decades earlier in the 1976 CIBA Symposium debate with Black, he wrote in his review:

This manuscript is a grim reminder of what epidemic disease has done to primitive populations, but [this] reviewer cannot accept the author’s thesis [that people of the New World are unusually susceptible to the diseases of the Old World]. . . . This is simply too sweeping and speculative. . . . There can be no doubt of the destructive sociological consequences of a “novel” introduced disease such as measles. This is observation. . . . I would accept a more tempered speculation which guesses that heterozygosity levels may play a role, but surely not the total role Black attributes to this factor [underlining in original]. . . . It is difficult to believe this heterozygosity difference is the sole factor in the calamitous response of the Amerindian to introduced diseases.³⁴

Contrary to the common practice of maintaining anonymity in peer review,³⁵ Neel concluded his report with the sentence “Please transmit this to Frank with my signature.”³⁶ The journal editor proceeded as instructed and sent Neel’s comments to Black. After making his revisions, Black resubmitted the paper and simultaneously wrote directly to Neel. In this personal letter, after the opening salutation “Dear Jim,” he reflected:

33. Letter from James V. Neel to the editorial office of *Perspectives in Biology and Medicine*, February 17, 1993, FB-Papers (#01187).

34. Letter from James V. Neel to the editorial office of *Perspectives in Biology and Medicine*, February 17, 1993, FB-Papers (#01187).

35. On this issue, Black wrote to the editor of *Perspectives in Biology and Medicine*, “The question as to whether reviewers’ comments should be anonymous is an old one, but openness on Jim Neel’s part has always strengthened my regard for him. I have worked in parallel with Dr. Neel on one of his dearest interest for many years and have had discussions of this kind before” (Francis L. Black, letter to Richard L. Landau, editor of *Perspectives in Biology and Medicine*, March 3, 1993, JN-Papers [#01245]). Black also touched on this topic in a letter to Neel himself: “I am grateful for your open way of reviewing, although I must admit I have come to expect it by now” (Francis L. Black, letter to James V. Neel, March 3, 1993, JN-Papers [#01246]).

36. Letter from James V. Neel to the editorial office of *Perspectives in Biology and Medicine*, February 17, 1993, FB-Papers (#01187).

I had seen your name on the editorial board of *Perspectives in Biology & Medicine* and our past discussions on the subject of New World mortality were very much in mind as I wrote the paper. . . . If I had felt our positions were incompatible, I would have submitted elsewhere, but I did anticipate a difference in emphasis.³⁷

Black detailed to Neel the modifications that he had made in the revised version of the paper, in particular the removal of some of the more emphatic assertions such as “homozygosity was the ‘sole’ factor.” In a conciliatory gesture, Black wrote that “a paper which is misunderstood is not well written.”³⁸

When Black’s paper was finally accepted for publication in *Perspectives in Biology and Medicine*, the editor in chief, Richard Landau, wrote to him, “I feel certain that Jim will be satisfied with the revisions you have made; I certainly am.”³⁹ It was unlikely, however, that Neel was indeed satisfied, considering the historical differences of opinion between the two, as he remarked to Black after he knew that the paper had been accepted for publication in *Perspectives*:

We’re still a considerable distance apart, but I would not stand in the way of your publication if other reviewers feel that the article is okay. . . . I would be very cautious about espousing “racial mixing” as “the best way to preserve the genetic traits of New World people,” since this could play directly into the hands of those who do not wish to make adequate provision for the transition of these peoples and rather prefer to see amalgamation, which if pushed without the necessary accompanying immunization programs, will lead to the slaughter of the few Indigenous populations which are left.⁴⁰

In this culmination of the exchange, Neel takes a position that calls into question more than Black’s science but the terms under which he performs concern for “New World” peoples. It is here, in this para-peer review correspondence, which Neel chose to make deanonymized, that the nature of the tension was revealed: it is one of ethics and politics as much as genetic frequencies. Neel is effectively charging that Black has failed to appreciate the potential violent consequences of his knowledge claims and associated public health recommendation.

It is in the context of peer review, made available only through the retrospective project of history, that it becomes evident in this case that discussions of politics and ethics were constitutive aspects of knowledge production, even as their traces were effaced by the norms of scientific publication. Just as ideas of purity do violence to the ways we think about Indigenous peoples, so do

37. Letter from Francis L. Black to James V. Neel, March 3, 1993, JN-Papers (#01246).

38. Letter from Francis L. Black to James V. Neel, March 3, 1993, JN-Papers (#01246).

39. Letter from Richard L. Landau, editor in chief of *Perspectives in Biology and Medicine*, to Francis L. Black, March 15, 1993, JN-Papers (#01273).

40. Letter from James V. Neel to Francis L. Black, March 24, 1993, JN-Papers (#01244).

ideas of purity obscure the inextricable relationship between the production of scientific knowledge and accountability for its political ramifications (Shotwell 2016). From our present-day vantage point, it seems that Black did not understand or was not willing to accept that Neel's real issue was with the conclusions he (Black) drew about what his science meant or that he did not take those criticisms as consequential (not to mention the editor of *Perspectives*).

The ultimate difference between the orientations of the two scientists was that Black's performance of concern appeared to be motivated by a desire to avoid controversy or conflict with the Brazilian military government, while Neel recognized controversy as an unavoidable aspect of a research enterprise that involved knowledge making about Indigenous groups living under conditions of settler colonialism. Moreover, Black's public health recommendation of racial mixing prioritized the salvage and preservation of genetic traits over the cultural integrity and self-determination of Indigenous peoples. To a twenty-first-century reader, this insight should be received with a sense of irony, for Neel's reputation within the anthropological community has been inextricably linked with the worst accusations of violence against Indigenous peoples.

In 2000, a journalist named Patrick Tierney wrote a book called *Darkness in El Dorado: How Scientists and Journalists Devastated the Amazon* (Tierney 2000a). Among the accusations leveraged in the book was that in the 1960s, Neel and his collaborator, anthropologist Napoleon Chagnon, had knowingly infected members of the Yanomami with measles in order to study the natural history of the disease in a virgin soil population. The measles epidemic that Neel attempted to curtail while investigating biological variation would tarnish his legacy with accusations that Neel contributed to their deaths by casting them primarily as experimental objects. Neel was unable to reply to the accusations made by Tierney in *Darkness in El Dorado* (Tierney 2000a) because he had recently died (Lindee 2001, 2004). There is further irony in the fact that Black was left in the position of interpreting and defending himself as well as Neel.

Members of the anthropological community first learned of Tierney's accusations in September of 2000, when Terence Turner and Leslie Sponsel sent a notice to fellow members of the American Anthropological Association (AAA) condemning Neel and his collaborator, Chagnon, on the basis of their review of Tierney's text, a portion of which was to be excerpted in the *New Yorker* (AAA 2002; Borofsky 2005). Black, who received news of this via an email forwarded from his son Douglas Black,⁴¹ who was then working for the Human Relations Area Files, a clearinghouse of anthropological information, was moved to provide an account defending Neel:

I have been active in the field of measles virus since 1955 and South Amerinds since 1966 and central to several studies of the effects of measles and its vaccines in Amerinds. I feel

41. Printed electronic message from Douglas Black to Francis L. Black, September 19, 2000, FB-Papers (#01232 and #01233).

compelled to comment. Neel's choice of Edmonston B vaccine was logical. I used a further attenuated strain at that time, but Edmonston B was in use in the U.S. [underlining in original]. . . . The EB [Edmonston B] vaccine could not have started an epidemic. . . . I forget now, but Neel doubtless had too little vaccine when faced with a massive epidemic. He would have had to use it where he could and let others get wild virus. Simply watching was not only an option; he had no effective treatment.⁴²

What is arguably even more interesting is Black's defense of Neel's genetic theories. He claims that Tierney misrepresented Neel's views about specific genetic factors:

With a whole set of genes involved, Neel would not have expected their frequency to have been altered in a common direction in an epidemic. . . . The variety of genes available in our larger social groups means that this process can go farther here. We have not been able to find any immunological deficit in these tribes that might explain their susceptibility except a limited diversity. I, myself, believe that wild virus adapts to the homogeneous population and when it spreads from one person to another, is preadapted and more virulent [underlining in original].⁴³

That is, when defending Neel, Black referred to the very hypothesis he proposed in the *Perspectives in Biology and Medicine* paper that, as we have seen, involved a tense exchange between the two scientists. Black concluded his missive remembering Neel in a strongly positive way:

I knew Neel for thirty years and the parallelism in our work might make me either collegial or competitive. In fact, he always seemed an extraordinarily nice man. Of course, I could be wrong, but we build our whole lives on such impressions of friends. I do not believe Tierney's charges.⁴⁴

That Black remembered Neel warmly and regarded him as a friend raises interesting questions about whether Black ever truly appreciated the reasons that Neel was so concerned about how he negotiated the ethics and politics of his research,⁴⁵ how he linked the scanty evidence for genetic susceptibility with the suggestion that miscegenation be considered as a viable public health intervention.⁴⁶ Neel was acutely aware that he

42. Letter from Francis L. Black to Carol Ember, September 20, 2000, FB-Papers (#01234).

43. Letter from Francis L. Black to Carol Ember, September 20, 2000, FB-Papers (#01234).

44. Letter from Francis L. Black to Carol Ember, September 20, 2000, FB-Papers (#01234).

45. While in this paper we explore several scientific controversies between Black and Neel, it is worth noting that in a few instances, they coauthored scientific papers (see Biggar et al. 1996).

46. Black was active in defending Neel, to the point of corresponding with the *New Yorker*. Tierney published a piece in the *New Yorker* (Tierney 2000b) a few weeks before the publication of his book (Tierney 2000a). Black read the *New Yorker* piece and faxed the magazine the pages of the yet-unpublished book by Tierney with handwritten comments (FB-Papers #01225). In the fax

might possibly be construed in the ways that Tierney eventually did, so much so that he kept a file titled “Insurance” to protect him from such claims (Lindee 2004). This is a different sign of Neel’s perhaps greater public profile but also, and perhaps relatedly, of a greater savvy in recognizing the complex relationship between human biological research and the ethics of human biological futures.

Final Considerations

The debates between Black and Neel spanned nearly four decades. They happened both in high-profile international scientific meetings and publications and in not-so-explicit arenas, such as in correspondence and peer review documents now accessible only in historical archives. Whether more or less explicit, the stakes were high, and the arguments over the importance of genetics and immunology had major implications for issues related to public health interventions and the ultimate survival of Indigenous peoples.

These stakes are exemplified by the arguments put forward in 1977 by the chair of the health sector of FUNAI, physician Aldo Molina, and the director of Xingu Indigenous Park in central Brazil, anthropologist Olympio Serra, in a short paper on Indigenous health policy in Brazil published in *América Indígena*, a Mexican journal specializing in Indigenous issues that circulated widely in Latin America. Molina and Serra (1977) noted, “Health aspects among tribal Indian groups in Brazil pose thorny problems if one considers the little resistance which they offer against the particular diseases of national society, specially epidemics” (183). Furthermore, they added, “[There are many instances that] attest to their reduced genetic resistance to virus diseases, to the point that viral infections such as the flu, measles, and smallpox might reduce up to 90% of their population size” (180). That is, in the mid-1970s, when flu and measles epidemics were affecting a large number of recently contacted Indigenous populations in Brazil, and at the very time when Black and Neel had their confrontation in London about the role of genetic factors in immunological response to infectious diseases, Brazilian government officials were explicitly referring to the role of an allegedly “reduced genetic resistance” in their assessment of whether Indigenous populations could immunologically cope with contagious diseases with epidemic potential.⁴⁷

sent by Black to Susan Carney on November 2, 2000, he wrote, “I send a copy of pages purported to come from Tierney’s book I have just received (The 6 week delay in publishing suggests that revisions are being made.). These pages quote me, I believe incorrectly. Apparently elsewhere Tierney says he phoned me in 1997 and presumably it is that conversation he quotes. I do not deny he may have phoned me, I have no recollection. I do deny I disbelieved the statement Neel had used the Edmonston B vaccine—Neel had told me that in the late 60’s. . . . I still believe Neel’s choice . . . was logical and disbelieve it as a source of the measles epidemic” (Francis L. Black, fax to Susan Carney, November 2, 2000, FB-Papers [#01224]).

47. As director of the Xingu Indigenous Park, Olympio Serra was a close partner of Roberto G. Baruzzi, a well-known Brazilian physician who started

Swift political changes happened in Brazil from the 1970s to the 1990s. When the article “Why Did They Die?” was published in *Science* in 1992, the sociopolitical environment in South America generally and in Brazil in particular contrasted markedly with that of the 1960–1980 period when Black conducted most of his field studies. In Brazil, the transition from military to civil government occurred in 1985 and was followed by promulgation of the strongly multiculturalist 1988 constitution, with different and specific principles regarding Indigenous peoples (Carneiro da Cunha 1992; Ramos 1984, 1998). The goal of integrating the latter into national society was replaced by principles fostering the prospect that these populations could remain socially and culturally distinct into any foreseeable future. This new perspective would lead to specific public policies concerning, among other issues, health, education, and land rights.

By predicting the extinction of Indigenous peoples and emphasizing the possibility of their survival only by means of “admixture,” Black—as he expressed in his paper “Why Did They Die?” and in other publications (Black 1994a)—conveyed a message of strong pessimism that, to a large extent, was no longer politically palatable in mainstream Brazilian politics in the 1990s and beyond. In the same way that the global dissemination of vaccines had provided a powerful defense against infections in the second half of the twentieth century, Black believed that a shot of miscegenation could be a way to confer the protection needed by Amazonian Indigenous populations against biological (but not social and cultural, as Black himself admitted) extinction. Certainly, the nostalgia that permeated Black’s writings, as well as those of so many other scientists, was a product of the historical moment during which he and other researchers had witnessed the adverse effects of the interaction between Amazonian Indians and Brazilian national society (Davis 1977; Hemming 1987; Ramos 1998; Ribeiro 1976). By the early 1990s, however, by virtue of the sociopolitical transformations in Brazil and South America, the emphasis on “amalgamation” had become radically out of step with the views emanating not only from Indigenous social and political movements but also from the national state.

Coda: Unfolding Archival Revelations and Relations

In the process of researching this paper, Santos and Coimbra found themselves asking when they had first decided to carry out anthropological and historical research on Black’s intellectual

working in the Xingu area in the 1960s. Not only was Baruzzi well acquainted with the research carried out by Black in Amazonia, but also he participated in the 1976 CIBA Symposium (Baruzzi et al. 1977), when Black and Neel had a major public disagreement on the topic of the biological determinants of Indigenous mortality due to infectious disease epidemics, as we detail in this paper.

trajectory in Amazonia. It was likely when Santos and Coimbra first read Black's 1992 *Science* paper, "Why Did They Die?"

As anthropologists working in the Amazon, Coimbra and Santos had long been acquainted with Black's scholarship through his numerous and influential publications on the epidemiology, genetics, and demography of Indigenous Amazonian peoples. Their interest in Black was as a scientific colleague, a fact that is borne out by the archival record we have used to construct a historical account that makes previously submerged questions of ethics and politics as visible as the technical arguments about genetic and immunological knowledge. In this project, Coimbra and Santos came to understand themselves as both historical actors and present-day historians (on becoming historical, see Radin 2014a).

Indeed, Coimbra began exchanging correspondence with Black in the 1980s, as both were carrying out health research on Indigenous Amazonian populations. Santos first met Black in Miami in 1990, at the annual meeting of the American Association of Physical Anthropologists (Coimbra 1991). In 1994, they invited Black to attend a meeting of the Latin American Association for Biological Anthropology, hosted in Rio de Janeiro,⁴⁸ where they also invited him to contribute to an edited volume in Brazil in Portuguese, with papers covering a broad range of topics related to the health of Indigenous populations (Santos and Coimbra 1994). He immediately accepted the invitation and proposed the translation of one of his papers due to appear soon in a US journal. This was none other than the *Perspectives in Biology and Medicine* article (Black 1994a) so central to the present essay.

Then unaware of the fraught circumstances through which this paper had been published, Santos and Coimbra arranged for the text to be translated, and in the upcoming months, they exchanged several letters with Black as part of the process of revising the Portuguese version. Santos, Coimbra, and Black came to the agreement that, for the Portuguese version, parts of the text would be simplified as much as possible, transferring the more technical genetic and immunological aspects to footnotes and trying to render his arguments in a more accessible form to nonspecialists.⁴⁹ This was because the book was aimed mostly at Brazilian students and scholars in the field of medical anthropology and public health rather than experts in immunology, human biology, or genetics.⁵⁰

48. Letters from Carlos E.A. Coimbra Jr. to Francis L. Black, January 31 and February 7, 1994, FB-Papers (#04649 and #04652, respectively). See also letter from Ricardo Ventura Santos to Francis L. Black, March 9, 1994, FB-Papers (#04653).

49. Letters from Ricardo Ventura Santos to Francis L. Black, March 8 and May 26, 1994, FB-Papers (#04629 and #04648, respectively). Letter from Carlos E.A. Coimbra Jr. to Francis L. Black, May 3, 1993, FB-Papers (#04657). Letter from Francis L. Black to Ricardo Ventura Santos, May 30, 1994, FB-Papers (#04646). The adapted translation of Black's paper was published in the edited volume *Saúde e Povos Indígenas* (Black 1994b; Santos and Coimbra 1994).

50. Letters from Ricardo Ventura Santos to Francis L. Black, March 8 and May 26, 1994, FB-Papers (#04629 and #04648, respectively).

Several decades later, after Neel had been exonerated by the AAA but not before profound soul-searching about the ethics of research involving Indigenous groups had been generated (AAA 2002; Borofsky 2005; Salzano and Hurtado 2004), Santos and Coimbra found themselves in the archive. In fact, it was even more amazing to realize that, as part of Santos's and Coimbra's editorial discussions back in the 1990s, they had unwittingly touched on some of the very same issues that had surfaced in the exchanges between Black and Neel over the publication of Black's paper in *Perspectives* explored in this essay. As editors of the Brazilian volume, Santos and Coimbra were concerned with how some of Black's ideas would be received in Brazil, in particular those related to "racial mixing." For instance, Santos wrote to Black:

In the present academic and political contexts [a new Brazilian constitution, intense debates about the demarcation of Indigenous territories, etc.], this question of racial miscegenation as something positive may well have a bombastic impact in Brazil. . . . Given that the book is targeted at an audience interested in Indigenous health where these more complex biological arguments are not necessarily apprehended in the way we would like, certain ideas might be picked out of context and blown out of proportion. The question of racial miscegenation as something positive is a natural candidate for such.⁵¹

Santos and Coimbra sent Black the translated version of the *Perspectives* paper with their editorial comments, and less than a week later, Black replied in a warm and receptive way.⁵² Compared with the assertiveness about the role of miscegenation in the *Perspectives* paper ("An implication is that racial mixing is the best way to preserve the genetic traits of New World people, and the relative success of mixed race populations has been noted" [Black 1994a:301]), the proposed version of the translation was less emphatic and left open room for some uncertainty in its concluding remarks ("While we still lack concluding evidence, it is possible that miscegenation might be positively influencing patterns of immunological response in Indigenous populations. . . . However, it will be necessary to carry out further research in order to clearly define which factors, socio-cultural and biological, relate to the high mortality levels observed in Indigenous populations following epidemics" [Black 1994b:82; translated from Portuguese]). Black wrote to Santos and Coimbra:

Yes, I am well satisfied with your corrections and I think we have a version that both conveys my intent and should make for easy reading. . . . I was concerned about hitting too hard, but not fully attuned to Brazilian sensitivities.⁵³

51. Fax from Ricardo Ventura Santos to Francis L. Black, May 26, 1994, FB-Papers (#04648).

52. Letter from Francis L. Black to Ricardo Ventura Santos, May 30, 1994, FB-Papers (#04646).

53. Letter from Francis L. Black to Ricardo Ventura Santos, May 30, 1994, FB-Papers (#04646).

Our decision to conclude by demonstrating Santos’s and Coimbra’s own presence in the archive reveals that, without knowing it at the time, they found themselves in a position similar to Neel’s in trying to manage potential controversy around a more fundamental aspect of scientific knowledge production. Black’s concession to Santos and Coimbra in 1994 took place just a year after his exchanges with Neel on the *Perspectives in Biology and Medicine* paper. The fact that Black wrote that he was “not fully attuned to Brazilian sensitivities” contrasts with the ways he had responded to Neel’s long-standing criticisms over the years. Santos’s and Coimbra’s presence in the archive demonstrates that questions of ethics and politics have been enduring concerns for anthropological research with public health implications—the concerns that Neel articulated with regard to Black’s research have become more and not less pressing in the wake of contemporary debates, including the *Darkness in El Dorado* controversy.

When politics and ethics are relegated to the back stage of peer review, as opposed to being regarded as fundamental to the content of scientific publication, it puts the very people who serve as the subjects of knowledge at risk and further prevents them from registering concerns about the implications of findings extrapolated from their bodies. The ability to detect genetic variations or a lack thereof should not be seen as adequate justification for granting scientists the authority to promote interventions with profound, life-altering consequences for Indigenous or any peoples. For present-day anthropologists and human biologists who seek to make knowledge that serves Indigenous peoples’ flourishing, debates over ethics—how research findings and public health recommendations are linked—cannot be relegated to the archive.

Today, there have been important calls for regarding Indigenous peoples not merely as research subjects but as participants and even researchers themselves (Benjamin 2013; Smith 2013; TallBear 2013). At stake is precisely the awareness that the situation of the knower has consequences for the shape of knowledge (de la Bellacasa 2017; Haraway 1988). As we have shown, it is always only a matter of time before history catches up to the present.

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Comments

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Heredity and Environment: Making (Non)sense of Human Isolates

The article by Santos, Coimbra, and Radin makes for a fascinating read for many different reasons. Their thorough examination of the long-lasting disagreement between two prominent American biomedical scientists, Francis L. Black and James V. Neel, about the political implications of researching the biology and health of Amazonian Indians, could not be more topical. Neel’s warning, first issued as early as 1976, that Black’s promotion of “miscegenation” as a means for Brazilian Indigenous populations to alleviate their (allegedly) genetic vulnerability to pathogens “could play directly into the hands” of the military dictatorship that ruled the country at the time has gained extra, worrying relevance now that the “amalgamation” of Indian populations has been put back on the political agenda by those who would not accept any hindrance to the exploitation of the Amazonian El Dorado. First among them, the recently elected president of Brazil, Jair Bolsonaro, could not let his inauguration day pass without starting to unravel the legal protections extended to Indigenous populations by the constitution adopted in 1988—a landmark in the return of the country to democracy.⁵⁴

Incidentally, it is also somehow ironic to learn that Neel, who was publicly accused (shortly after his death) of having treated Amazonian Indians as mere human guinea pigs,

54. The first executive order signed by the freshly sworn-in president shifted the responsibility of designating protected lands for Indigenous people from Fundação Nacional do Índio to the Ministry of Agriculture. The Brazilian agribusiness caucus and resource extraction industries have long favored the amalgamation of Indigenous people as a way to gradually reduce the size of the restricted areas (<http://www.pbs.org/newshour/world/brazils-bolsonaro-targets-indigenous-groups-lgbtq-rights-on-1st-day-as-president>; <http://www.theguardian.com/world/2019/jan/02/brazil-jair-bolsonaro-amazon-rainforest-protections>; accessed February 14, 2019).

actually paid more attention to the political implications of his research than one of his staunchest advocates in the controversy that ensued—namely, Black (AAA 2002; Tierney 2000a). Black and Neel were obviously not the first scientists to study human isolates as a window on otherwise inaccessible biological realities—if not exactly “virgin soil”—or to discuss the possible genetic virtue of miscegenation. Long before pioneer population geneticists laid the scientific basis for the kind of research favored by Neel and Black (Dahlberg 1929; Wright 1922), Italian positivist anthropologists had already started researching isolated populations as a means to shed light on the process of human degeneration and the role played by “atavism” in human heredity (Caglioti 2017; Niceforo 1897; Sighele 1890). It is perhaps less well known that their rather crude methodology was later refined by a tiny group of pioneer population scientists led by the prominent statistician and eugenicist Corrado Gini—the Gini coefficient, or index, is named after him—who worked under the umbrella of the Italian Committee for the Study of Population Problems (CISP; Comitato Italiano per lo Studio dei Problemi della Popolazione). The 10 or so scientific expeditions that they launched between 1933 and 1940 aimed either at human isolates—from the Samaritans of Palestine to the Dawada of Fezzan in Libya and so on—or, less frequently, at racial admixtures, including various groups of Mexican mestizos, as a way to document the link between inbreeding and isolation and the symmetrical benefits of “hybridization” between similar-enough populations (Berlivet 2016). The political motivation of the whole enterprise was to scientifically vindicate the criticisms leveled by the “Latin eugenicists”—of whom Gini was a prominent leader—at their British, American, German, and Scandinavian counterparts concerning both the alleged value of race purity and the purported predominance of nature over nurture. Unsurprisingly, considering their fascist inclinations, the question of the “cultural integrity and self-determination of Indigenous peoples” was not even mentioned by the Italian scientists in their praise of what one might call well-tempered miscegenation.

Finally, when one looks beyond the major differences between the politics of science in the interwar periods and during the Cold War, an interesting common thread between the story told by Santos, Coimbra, and Radin and CISP’s investigations is the centrality of an all-too-famous dichotomous conceptual dyad: heredity and environment. There is little doubt that Black’s reframing of isolation in classic Mendelian genetic terms as an issue of increased homozygosity was far more sophisticated than Gini’s.⁵⁵ However, it is striking to note how little Neel, in his dissenting analysis of Black’s hypothesis, elaborated on the kind

of environmental differences that could have explained the differences observed between Indigenous and other populations. In his first paper on the topic, the “socioeconomic and epidemiological structure” that he believed could explain “at least 80% of the high mortality among some primitive groups from measles, smallpox, influenza, tuberculosis” was mentioned without further discussion (Neel 1977c:155–156). Perhaps for the American geneticist it was just a matter of not stating what he believed was obvious, but as a result, in Neel’s reasoning, environmental factors—that most plastic syntagma—were reduced to a black box whose agency was postulated rather than thoroughly analyzed. It would take a few more years before “the environment” took the center stage again or at least started to share it with “the genes.” The study of “gene-environment interactions” would become the new grail of biomedical research, although misunderstandings about the two notions and their relationship did not miraculously fade away (Keller 2010).

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On Backstage Dynamics in Peer Review Publishing

Science is a social process, as this thoughtful piece by Ricardo Ventura Santos, Carlos E.A. Coimbra Jr., and Joanna Radin reminds us. In excavating a fascinating bit of publishing history, they pull back the blind that normally shields peer review from public view to show how two renowned geneticists, Francis L. Black and James V. Neel, confronted the high political stakes involved in extrapolating policy recommendations from research findings.

Santos, Coimbra, and Radin emphasize that peer review is not just a “technology of objectivity” but a backstage site where scientists negotiate the political implications of their research. They argue that even though this may be “uncomfortable for those who hew to an understanding of science as disinterested and apolitical,” “debates over ethics—how research findings and public health recommendations are linked—cannot be relegated to the archive” but should be recognized as considerations in publishing.

Social dynamics also shape other issues that Santos, Coimbra, and Radin do not address about how prestige hierarchies, personal ties, intellectual tribalism, ego, and emotion figure into the ways that authors, reviewers, and editors treat ideas in manuscripts. Black’s lifetime work helped to demolish pernicious assumptions about the inevitability of Indigenous populations’ demise because of the genetic weakness of individual native bodies’ immune responses. But in these writings from the 1990s, he reified another rationale for fatalism, asserting that the genetic makeup of native communities (in which pathogens quickly become more virulent as they spread among genetically similar

55. Although apparently critical of traditional, stigmatic characterizations of Indigenousness as the lack of a specific element existing in more civilized populations, Black’s views rekindled them in a more euphemistic way: while “the newly contacted people” were not plagued by “deficient immune systems” or “inappropriate genes,” they suffered from “less internal genetic diversity” (Black 1992).

individuals) was a problem to remedy by diversifying the gene pool through intermarriage with other populations.

This cavalier leap in logic, from a meticulous description of research on population genetics to a sweeping prescription for ethnocide, is remarkable. In contrast to the care he took to detail his genetic research, Black tossed off this proposal for a eugenic solution with little discussion of whether pathogenic virulence really is a problem in the long run or whether this might be addressed by less drastic means, such as vaccination and medical vigilance. The virulence issue is most relevant only in communities with little prior immunological exposure to a specific pathogen, mostly during the critical weeks of an epidemic. Even in 1992, only a few native Amazonian groups fit this epidemiological “virgin soil” profile, and Black himself acknowledged that virulence was only one limited factor in mortality rates.

Nonetheless, he insisted on extrapolating from his data to a eugenic solution. In the archival correspondence about his articles, it is striking that both Neel, who outed himself as a reviewer of Black’s *Science* piece, and Coimbra and Santos, who dealt with Black as editors on a related chapter a few years later, avoided direct criticism of Black’s reasoning. Instead, they took a less confrontational approach, citing political repercussions as an argument for softening his position.

Would a less established scholar receive the same deference accorded to an eminent professor at an Ivy League university? Neel had the professional clout to challenge Black, though collegial courtesy seems to have tempered the tone of his pushback. Coimbra and Santos, in contrast, were at the beginning of their academic careers when they edited Black’s contribution to their volume. They were young anthropologists based in Brazil, while he was a famous senior American hard scientist.

One wonders how much of Black’s insistence on the policy relevance of his model reflected the well-documented bias in scientific publishing toward valuing positive research results more than negative results. There is more interest in—and greater professional rewards for—demonstrating that a factor makes a difference than for showing that it does not. Black devoted his career to searching for genetic answers to the question “Why did they die?” It was not his “positive” research findings about virulence but his “negative” results—ruling out differences in native peoples’ immune responses—that were most significant for Indigenous health care. In a recent analysis of contact-related epidemics in 59 native Amazonian societies over the course of more than a century, Robert Walker, Lisa Sattenspiel, and Kim Hill (2015) documented native groups’ long-term demographic resilience: over time, mortality from infectious diseases decreases, and conscientious biomedical care saves lives. “While our data do not speak directly to genetic hypotheses,” they wrote, “the rapid improvements in survivorship through time do implicate a strong role for environmental factors, including increased immunity and better health care and conditions” (Walker, Sattenspiel, and Hill 2015:3). On the basis of his experiences in three first-contact situations, Hill emphasized that “the most important lesson learned was that mortality can be

reduced to near zero levels if the contact team is prepared to provide around-the-clock medical treatment on site for a sustained period of time and complement it with food supplementation” (Walker, Sattenspiel, and Hill 2015:6).

For those who remember the explosive accusations that ricocheted through our discipline in the controversy over journalist Patrick Tierney’s *Darkness in El Dorado* (Tierney 2000a), this account of Neel’s argument with Black adds one more piece for rethinking that story. Tierney’s morality tale of biogenetic determinism, US atomic war research, callous American scientists, Venezuelan political corruption, and racially tinged medical malfeasance in a vulnerable native population triggered an initial rush to judgment about Neel’s actions during the 1968 Yanomami measles epidemic. It took painstaking archival research by Santos and others to sort out the facts. The more balanced picture that emerged showed a physician who went to considerable lengths to try to save Yanomami lives. As Santos, Coimbra, and Radin argue, ethical considerations are integral at each step along the thoroughly social path that leads from the practices of field research and publishing to the reception of scientific findings in political milieus that have real consequences for vulnerable native communities.

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The article analyzes the contrasting views of Francis L. Black and James V. Neel, two geneticists who worked on Amazon Amerindians. In the 1960s and the 1970s, biomedical scientists were concerned with the extinction of those populations, such as Amazon natives, who had not been contaminated by the encounter with other peoples. A number of biomedical scholars focused on the genetic heritage of isolated populations who had not previously been exposed to ionizing radiations. Their research was often financed and supported by national atomic energy agencies. This was the case for the French Centre National de la Recherche Scientifique and for the US Atomic Energy Commission (Radin 2013; Rivière et al. 1968; Roche 1959). Scholars were persuaded that the process of extinction of the Amazon natives was developing much faster than it actually was. In order to prevent the loss of the genetic heritage of the Amazon natives, Black proposed increasing the level of admixture of genes among populations, which entailed promoting intermarriage between populations. Although Black did not suggest which social and political measures should be undertaken, his views could be used to support the Brazilian dictatorial government’s plans of assimilating the Amazon natives in order to exploit the resources of the lands they inhabited. Neel opposed Black’s view because he deemed that it could be used to justify the extermination of the Indigenous people. The authors argue that Black aimed to avoid any conflict with the Brazilian military

government, whereas Neel thought that the conflict could not be avoided. They convincingly argue that he was fully aware of the ethical and political dimension of his genetic research. It is implausible that Black was not conscious of the consequences of his proposal. As the authors of the article recall, Neel's work was carried out mainly in the 1960s and the early 1970s, whereas Black developed his research later, from the late 1970s. This difference is quite important, as Neel developed his research within the climate of the development of the Cold War and the Vietnam War, whereas Black carried out his research during the rise of neoliberalism and the end of the *détente*. Alex Coello de la Rosa (2018) has recently argued that Napoleon Chagnon's reading of the Yanomami as a violent population should be understood in light of "the US American obsession with violence, aggression and territorial expansion that began with genocidal wars against the original Indigenous peoples of the North American continent" (520). We could, therefore, also try to understand the contrast between Black and Neel in light of the history of the United States and of the anxieties of the Cold War. It is worth noting that neither Black nor Neel was concerned with the Indigenous point of view, which other scholars have been seriously taking into account since the 1960s. In 1967, Ezio Ponzo, an Italian psychologist who participated in the expedition headed by Ettore Biocca to study the Yanomami, published the results of his research on acculturation, the process of assimilation of one culture into another, and argued that motivation played a crucial role in adopting the customs of another civilization. Having observed that Amazon natives seemed to desire acculturation, Ponzo implicitly suggested that the Westerners should not prevent it but rather that they should put their efforts into making it less traumatic (Ponzo 1967). Later on, a number of Brazilian anthropologists, among whom were João Pacheco de Oliveira Filho, Alcida Rita Ramos, Paulo Freire, and Eduardo Galvão, coined the notion of Indigenism. The Amazon natives were not considered anymore to be passive subjects of measures taken by Western authorities (see de Oliveira Filho 1998, 2006; de Oliveira Filho and da Rocha Freire 2006; Ramos 1998). Neel's failure to understand that he could not store blood samples from the Yanomami without their consent should likely be understood in the light of both the anxieties of the Cold War and the history of the United States, which prevented him from recognizing Indigenous subjectivity (Borofsky 2005:13). Neel did not elaborate on a personal view of the Indigenous question. Although he collaborated with Chagnon, he never endorsed the latter's ideas. One may think, however, that both Neel's past experiences and the history of the United States made him more cautious in extrapolating concrete political actions from his data. He was, indeed, fully aware of the political, ethical, and social dimension of his research. In his autobiographical book, *Physician to the Gene Pool*, Neel recalls that he became interested in human genetics in 1939 and that he embarked on its study in the early 1940s, when the discipline was discredited by eugenics (Neel 1994:24). Neel experienced a cultural clash during his field research on the effect of radiation in Japan. The complexity and difficulty of carrying out studies with human subjects whose

habits were so different, the implicit feeling of guilt of US researchers toward the Japanese, and the interaction with Japanese researchers and authorities likely led Neel to become more cautious in proposing simple solutions to complex problems in human genetics and to be somewhat more conscious of the role that the history of his motherland played in the making of his research, even if he could hardly understand Indigenous subjectivity (Neel 1994).

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Santos and colleagues are to be commended for their exciting dive into the interconnections between the science, politics, research, and writing of Francis L. Black and James V. Neel on topics that range from and interconnect measles epidemiology, genetic diversity, public policy about Indigenousness, and race crossing. Here, I want to focus primarily on two ideas that the authors highlight: Neel's shifting views on genetic explanations and Black's take that the cause of high measles mortality in Amazonia is isolation and genetic homogeneity and that the political solution is miscegenation.

The detailed analysis of Neel's responses to Black's genetic explanations for the high measles mortality rate of Amazonians is fascinating. As the authors show, in public and private, Neel tried cautiously to temper Black's enthusiasm for genetic homogeneity as an explanation for high measles mortality. Those familiar with Neel are probably aware that earlier in his career, he famously postulated what came to be known as the "thrifty genotype" explanation for diabetes in many Indigenous groups (Neel 1962). Neel hypothesized that exposure to past cycles of feast and famine led to a genetic adaptation for storing fat during feast times to survive famines. He furthered that populations such as Indigenous Native Americans in the Southwestern United States that now have high obesity and diabetes rates were differentially exposed to cycles of feast and famine and were more likely to develop the thrifty genotype. Now, with a more constant food supply, that adaptation is maladaptive, putting individuals at risk of diabetes and obesity.

What is fascinating is that Neel, in his postulation of a thrifty genotype, did pretty much exactly what he warned Black away from; that is, he promoted a genetic theory when many alternative explanations were closer at hand. Why, then, did Neel seemingly switch sides on the salience of genetics for an explanation of measles mortality? One possibility is that he learned from his past zealotry. But this might not make sense, as Neel continued to advocate for his thrifty genotype explanation into the 1990s. Another is about the flexibility of views. One can use genetic explanations when they fit one's needs. Perhaps Neel wanted to distinguish himself from Black? I do not know and would love to hear from the authors on this point.

Black, the deeper focus of this piece, promoted genetic homogeneity, particularly in the human leukocyte antigen system, as a prime explanation for the high measles mortality in Indigenous Amazonians. Furthermore, as a public health expert, he promoted miscegenation as a means to increase genetic resistance, even though, as he was aware, it would likely cause irreversible cultural loss. Black promoted a Faustian bargain: race cross with the dominant society or die.

In his recommendation of miscegenation, Black is part of a long chain of researchers who blame the problems faced by marginal groups not on their lack of power and resources, not on subjugation, colonization, and many worse political-economic processes, but on their own self-inflicted inbreeding and isolation. Black's is a moment in the decades-long, complex, and important science and policy discourse about race crossing. At the same time that hybrid vigor was being recognized in agronomy, many human biologists, such as Charles Davenport, head of the Cold Spring Harbor Laboratory and founder of its Eugenics Record Office, wrote consistently about how race crossing leads to malapportioned and maladapted individuals (see Davenport and Steggerda 1929).

Others like Black argue the reverse, for the benefits of miscegenation in “man.” E. A. Hooton, one of the founding fathers of biological anthropology, wrote that the solution to “the Jewish problem” was outbreeding. In *Collier's*, a popular Sunday newspaper supplement, he concluded an article weirdly titled “Why the Jew Grows Stronger” with a solution: “Absorption of the Jews by outmarriage . . . the Jews would have to lose their cherished religion and their peculiar culture. . . . If they did, they . . . would solve their own problems” (Hooton 1939:72). That was written in 1939, six years after Dachau and months before the Nazis invaded Poland.

Ashraf and Galor have it both ways in their 2013 article on comparative economic development and genetic diversity. They conclude in their abstract with a sort of Goldilocks principle of genetic diversity and wealth, with the middle being just right: “While the intermediate level of genetic diversity prevalent among Asian and European populations has been conducive for development, the high degree of diversity among African populations and the low degree of diversity among Native American populations have been a detrimental force in the development of these regions” (Ashraf and Galor 2013:1).

Lessons are typically hard to draw from historical research. Patterns are often hard to discern. Life is messy. That said, two patterns strike me as worth deep consideration. First, I am struck by the flexibility of employing genetic diversity as an explanation. It is a measure (or actually multiple measures) that can be used in almost any which way, first correlated with a problem (the Jew's peculiarities, Jamaican race crossing, wealth of nations, and measles mortality in Amazonia), and then it becomes the explanation. Genetic ideologies search for genetic solutions. Second, in this long century of biological determinism, we are still grasping at straws of genetic explanations when so many more immediate social, political, and economic explanations are apparent. Of course, those more

immediate explanations change the locus of control and blame, and they lead us back into a mess of entanglements in power, ideologies, and materialities.

Indeed, as Neel (1970) titled his *Science* article, there are “Lessons from a ‘Primitive People,’” but they might be different from what Neel imagined. And that is a good thing for history, science, and all of us. Thanks for the lessons.

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Santos, Coimbra, and Radin have provided a multifaceted analysis of how scientists—in this case James V. Neel and Francis L. Black—“reckon with the political implications of the knowledge they make.” This problem bedeviled scientists throughout the twentieth century, from eugenics to gene editing. Even though the science of health inequities has not received comparable attention, it is a critical problem for health and social policy today. As Neel realized, scientists' explanations of health disparities have far-reaching consequences.

Black and Neel each sought to explain the susceptibility of Indigenous populations to new pathogens. Devastating mortality in the Americas, which had begun in the sixteenth century, continued to plague the isolated Amazonian groups with whom they worked. Black emphasized genetics, while Neel (surprisingly, given his lifelong work in genetics) focused on socioeconomic and epidemiologic factors. Black recommended intermarriage to preserve Indigenous genetic heritage. Neel feared that Black's genetic determinism would foster complacency among Brazilian officials and allow them to accept poor health outcomes as inevitable.

The disagreement between Black and Neel was specific to their contexts, from the supposedly “virgin soil” epidemics that struck the Amazon in the 1960s to the political reforms in postdictatorship Brazil in the 1980s. Nonetheless, their debate has echoes of deeper history. Europeans described the appalling mortality suffered by Indigenous populations in the Americas in the sixteenth century (Jones 2004). By the seventeenth century, they had turned this observation to their political gain. When King James (1877 [1620]) granted a patent for the settlement of New England in 1620, he cited a recent “wonderfull Plague” that had struck the Massachusetts coast and concluded that “Almighty God in his great Goodness and Bountie towards Us and our People hath thought fitt and determined, that those large and goodly Territoryes, deserted as it were by their naturall Inhabitants, should be possessed and enjoyed by such of our Subjects and People” (921).

Opportunistic uses of Indian mortality continued for centuries. In 1854, Josiah Nott believed that Indian extinction was inevitable: “It is as clear as the sun at noon-day, that in a few generations more the last of these Red men will be numbered

with the dead" (Nott and Gliddon 1854:69). Z. T. Daniel (1903), who worked as a physician with the Sioux in the 1890s and 1900s, invoked their susceptibility to tuberculosis: "[The] Indian is fading, he is disappearing; one by one they are passing over the divide by the tubercular route" (317). Such declarations of inevitable extinction helped justify the reservation system, which some saw simply as palliative care for a dying race.

Race mixing figured prominently in these writings, as it did for Black. Many nineteenth-century doctors believed that tuberculosis and extinction could be prevented only through intermarriage. Daniel (1894) believed that Indians "will continue to die everywhere they go, of tuberculosis, until the race is so thoroughly crossed by 'foreign blood' that it will stamp out the tubercle bacillus, and when that is done the Indian race in its original purity will be no more" (290). Commissioner of Indian Affairs W. A. Jones (1900) agreed that his Indian charges would inevitably disappear, not by dying but by "absorption into the body politic of this country" (49).

Physicians and government officials in the nineteenth century recognized that their analyses of Indian mortality had political consequences. They grappled with this responsibility throughout the twentieth century. Yet despite this accumulated experience, they could not always control the impact of their work. Neel himself sometimes ran into trouble. In 1962, he proposed the "thrifty gene" hypothesis to explain the high prevalence of diabetes seen in many Indigenous groups. He later recanted this hypothesis (Neel 1989), but by then the idea had spread far and wide. Researchers spent decades scrutinizing the Pima Indians (now the Tohono O'odham Nation) to identify the genetic causes of their susceptibility to diabetes. This work diverted attention away from the social and economic forces that had actually fueled that epidemic.

The writings of historians and anthropologists can also have unintended consequences. Historians pushed the virgin soil theory in the 1960s as a progressive effort to rewrite history. Traditional accounts had attributed European conquest to their superior technologies, tactics, and societies. Revisionist historians argued that Indigenous societies, which were more sophisticated than had previously been thought, would have resisted European incursions if not for the irresistible power of virgin soil epidemics. But this theory simply transfers blame from Indian societies to Indian bodies (Jones 2003). Even Francis Jennings (1975), a fierce critic of European colonialism, succumbed to the allure of genetic determinism: "If there is any truth to biological distinctions between the great racial stocks of mankind, the Europeans' capacity to resist certain diseases made them superior, in the pure Darwinian sense, to the Indians who succumbed" (22). This logic resurfaced in Jared Diamond's *Guns, Germs, and Steel*. Even though Diamond (1997) promised that his book was not "a racist treatise" (9), his core argument invoked human difference: "The main killers were Old World germs to which Indians had never been exposed, and against which they therefore had neither immune nor genetic resistance" (211–212). I do not know what Neel thought of Diamond's work, but he must have recognized that

Diamond's arguments, like Black's, rationalized the terrible health outcomes still experienced by Indigenous people.

Santos, Coimbra, and Radin are careful and reflective as they write about theories of Indigenous susceptibility. Yet even they chose words with (presumably) unintended consequences. Some readers will object to the occurrence of "decline" and "coincided" in their opening sentence. Many certainly objected when Harvard's Lawrence Summers commented that "the vast majority of suffering that was visited on the Native American population as the Europeans came was not a plan or an attack, it was in many ways a coincidence" (Bombardieri 2005:B2). Paul Farmer would name this an "immodest claim of causality" (Farmer 1997). Scholars increasingly call the devastating mortality a genocide (Edwards and Kelton, forthcoming). While this claim remains controversial, none can deny that Europeans created the conditions in which epidemics wreaked havoc. Seen in this light, the authors' use of "coincided" raises almost exactly the same question that they asked about scientists: how historians and anthropologists reckon with the political implications of the knowledge they make.

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This many-layered essay reveals the varying political stakes of biological knowledge about Indigenous peoples through the lens of a 40-year disagreement between two renowned geneticists. At the heart of the dispute were the presence and mechanism of Amerindian genetic susceptibility to the devastating epidemics that accompanied colonial and settler incursions into their territories.

Francis L. Black believed that the primary contributor to genetic susceptibility to infectious disease was homozygosity, that is, a lack of genetic diversity within "tribal" groups. His proposed solution was biological mixing with outsiders. James V. Neel objected to Black's view, as it could be co-opted by authorities, supposedly within the Brazilian government, "who do not wish to make adequate provision for the transition of these peoples and rather [would] prefer to see amalgamation."⁵⁶ Neel instead emphasized the environmental causes for high mortality rates from measles and tuberculosis.

The essay introduces Black, an understudied figure in the history of twentieth-century biology, but it is the depiction of Neel that most stands out. As the authors note, Neel is most well known in the public sphere for his alleged role, now vindicated, in the deliberate spread of measles among the Yanomami in the 1960s. Within the scientific sphere, however, his most lasting

56. Letter from James V. Neel to Francis L. Black, March 24, 1993, JN-Papers (#01244).

contribution has been the “thrifty gene” hypothesis. His hypothesis, first published in 1962, proposed that modern humans were genetically adapted to a “feast and famine” environment of hunter-gathering, the mode of subsistence for the vast majority of human history. In an environment of plentiful food and less physical activity, these thrifty genes produced diabetes, obesity, and heart disease (Neel 1962). Although his initial proposal concerned the genetic susceptibility of all humans, the idea was almost immediately applied to Indigenous people in particular (Johnson and McNutt 1964). His hypothesis has become “one of the orientating concepts in biological anthropology” (Benyshek and Watson 2006:120), and his 1962 paper is by far the most cited of his articles, with over half of its nearly 4,000 citations occurring in the previous decade.

I mention this here because the thrifty gene hypothesis strikes me as the most appropriate point of comparison with the “amalgamation” dispute outlined in the article. Neel’s careful repudiation of the genetic basis of immunological susceptibility to infectious disease is contrasted by his commitment to Indigenous genetic susceptibility to chronic disease.

By the 1970s, Neel would have been well aware that his hypothesis was overwhelmingly applied to non-European populations. In a later paper on the topic, he admitted that there was no good evidence of genetic differences in diabetes susceptibility among ethnic groups (Neel 1982), but this never translated into a concern for the political consequences of the continued and widespread assumption of thrifty Indigenous genes.

Santos, Coimbra, and Radin describe how Neel, in refuting Black, argued that in “the absence of convincing evidence in the literature, it was better to assume that genetic susceptibility was not the major influencing factor, a position that would be ‘much less conducive to complacency’ (Neel 1977a:166) on the part of health authorities.” This same argument has been repeatedly leveled at Neel’s thrifty gene hypothesis over the previous 20 years. Scholars argue that the idea of Indigenous genetic susceptibility to diabetes undermines the efforts of Indigenous leaders and health advocates to encourage government action on preventing and treating chronic disease. The scapegoat of faulty genetics uncomfortably echoes earlier ideas of genetic inferiority, justifying the largely socially determined epidemics of heart disease, diabetes, obesity, and elevated blood pressure as biologically inevitable given the “rapid” transition of Indigenous peoples from the “Stone Age” to the “Atomic Age” (Neel 1958:797). Neel’s wide influence and continued insistence on the validity of his hypothesis are seen to have had a negative effect on Indigenous health policies in many countries, including Australia, the United States, Canada, and Mexico (Fee 2006; Hay 2018; McDermott 1998; Paradies, Montoya, and Fullerton 2007; Poudrier 2007; Saldaña-Tejeda and Wade 2019).

The story of Neel and Black’s long-standing disagreement illustrates Lindee’s thesis of genetics as an “emotional science” (Lindee 2013). (While this particular essay of Lindee’s is not cited in the article, I am sure it is not a coincidence that the subheading “Performing Concern” echoes Lindee’s [2013] chapter title

“Performing Anger.”) It is not clear, however, why Neel passionately argued against Black’s genetic explanations of infectious disease susceptibility but was silent about the potential political implications of thrifty Indigenous genes. It is possible that Lindee, Radin, Santos, Coimbra, or other scholars of Neel could provide an answer.

Neel was at pains to counter Black’s view that high mortality from infectious diseases was due to an innate lack of genetic diversity. A recent review of evidence for the thrifty gene hypothesis in the Pacific suggests that, in different ways, they were both wrong. The authors conclude, once again, that neither anthropological nor genetic data provide support for the presence of thrifty genes. They suggest that a more likely genetic explanation for higher rates of metabolic disease is selection from infectious disease epidemics, including measles and influenza, that caused the death of up to 75% of Pacific populations in the nineteenth century (Gosling et al. 2015). It may be that the lack of genetic diversity that Black proposed as the cause of susceptibility to infectious diseases was actually an effect of their devastating consequences on Indigenous lifeworlds and biology.

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Santos, Coimbra, and Radin present an important history of the debate between Francis L. Black and James V. Neel regarding the disastrous impact of infectious diseases on Brazil’s Indigenous populations. It traces the careers of two scientists who sought to identify the role of genetic and biological factors in the health of “virgin soil” populations and who constructed their careers disseminating their results in international scientific circuits of conferences, publications, and peer reviews. The article reveals that science is not neutral or objective and that knowledge is produced through negotiations in relational networks. As a case study in which the authors also played a part, it raises several issues in the debates in science studies, ethics, and critical global health (Adams and Biehl 2016), highlighting the complex relationships between biomedicine, anthropological knowledge, political regimes, and Indigenous rights. As a cultural anthropologist engaged in research on Indigenous health since 1970 and as a witness of the construction of the Brazilian subsystem of Indigenous health since 1986, I wish to contextualize further the issues and history of the political, ethical field implicated in this article, demonstrating the complexity between local meanings and global power relations.

Neocolonial Research Ethics

In the 1960s and 1970s, few ethical guidelines or principles existed for research with human subjects. Two international

protocols requiring informed consent had been produced: the 1947 Nuremberg International Tribunal and the 1964 Declaration of Helsinki. However, there is little evidence of the compliance of scientists investigating Indigenous peoples (Ramos 2001). As shown in the case of Black and Neel, concerns for research subjects depended more on the investigator's individual consciousness than on ethical guidelines outlined by governments, professional associations, or Indigenous organizations.

I had two experiences in 1970 that highlight the instrumental dimensions of relations between investigators and their research subjects. I was a member of an interdisciplinary study comparing Indigenous and biomedical diagnoses among the Sibundoy peoples of Colombia (Langdon and MacLennan 1979). The physicians made household visits examining family members and collecting material for clinical examination and diagnosis; subsequently, I visited the household asking for the family's diagnosis and the history of the illness. Research objectives had been previously explained to the regional health director, the two local physicians, Capuchin missionaries with influence over the group (Bonilla Sandoval 1968), and Indigenous men, including shamans, who were influential in the community. There was no official Indigenous organization. The investigating physicians had no intention of providing health intervention in this research. Once I emphasized the need to reciprocate with those who agreed to examinations, they obtained medications to treat most of the health conditions found. I should add that expiration dates of the medications, donated by the project's research center, had elapsed, and the Capuchins accused us of distributing birth control pills.

My second experience occurred with the Siona peoples, with whom I had begun my doctoral research. During a two-month absence, Carleton Gajdusek, a biomedical researcher famous for his discovery of the kuru virus (Anderson 2013), visited the Siona. Accompanied by the village *gobernador*, he extracted blood from all the families. When I returned a few weeks later, all were complaining that they had felt forced to submit to his blood taking. It was not a simple case of unwillingness to collaborate. As for other Amazonian peoples, blood is a symbolically charged bodily substance (Belaunde 2005). For the Siona, blood is a substance in limited quantity that maintains corporal vitality. For several years, a number of Siona continued to blame their ill health on the blood taken by the foreign "doctor." The destiny of their blood remains unknown. Gajdusek's research is an example of science as a form of neocolonial injury (Kowal 2013) that occurs when the researcher's social and political position of dominance authorizes him to take a valuable substance without consideration, consent, or respect for the rights of Indigenous people subjected to research.

Political Responsibility

Although Neel and Black showed a certain sensitivity to Brazil's political situation, the limits of their awareness, particularly Black's, reveal inequalities in relations established by scientists from the Global North conducting research among Indigenous

peoples in the peripheral South. In 1969, Norman Lewis published his widely read article "Genocide," which was based primarily on the Figueiredo report documenting the violence against Indigenous peoples by the Service for Protection of the Indian. In 1971, 11 social scientists from Latin America, including two Brazilians, signed the Barbados Declaration, recognizing the political responsibilities of researchers and the right of Indigenous peoples to be protagonists in their future. During that decade, Indigenous assemblies were held. Indigenous leaders became visible, and the Union of Indigenous Nations was founded in 1980. During the 1980s, Indigenous organizations played an important role in the conferences and other forums that led to the Constitution of 1988 (Carneiro da Cunha 2018). In 1986, the First National Conference for the Protection of Indigenous Health was held with substantial Indigenous representation as well as representation from social and biomedical scientists. With the intention of deliberating over the development of an Indigenous health system, the conference initiated a research network of politically engaged anthropologists and biomedical researchers (Langdon and Ghiggi 2018). This network consolidated by the end of 1990 when Coimbra founded the Work Group on Indigenous Health associated with the Brazilian Association of Collective Health.

Ethical conduct in research among Indigenous peoples in Brazil is no longer a matter of individual humanitarian concern or consciousness. In 1996, a law was instituted requiring all research proposals with Indigenous peoples to be examined not only by local ethics committees but also by the National Commission for Ethics in Research (CONEP). It is a bureaucratic process in which Indigenous peoples have a part; however, they are far from being major protagonists. CONEP is located in Brasília and dominated by biomedical professionals in a review process that examines projects for their scientific objectivity and potential for harm to the Indigenous subjects. Political relevance for or protagonism by those to be studied is not a concern of this legislation.

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This paper elucidates a process that usually remains hidden from public attention but can sometimes be reconstructed from correspondences or other documents accessible for historical inquiry. The authors have demonstrated that a study of peer review processes can add to our understanding of "how . . . scientists reckon with the political implications of the knowledge they make about living human groups." Reading the text, I started wondering about which other political discussions would be brought to light if one examined peer review processes more systematically, for example, in the ever-growing field of population genetics and genomics from the 1990s onward. Here, one would anticipate many such disagreements on how to proceed in light of scientific uncertainty and lacking quality

evidence. While the promises of genomics have often stirred high expectations in the public, we know much less about the underlying disagreements between scientists regarding what it means to engage in this research in a responsible and careful way.

As becomes clear from the text, for scientists who study living human groups, there was—and is—much at stake. The way scientists approach and encounter living human groups and how they talk and write about them afterward may not only be judged against standards of scientific conduct such as objectivity, neutrality, reproducibility, validity, and so on but also evaluated as a matter of human-to-human relationships, entailing expectations of respect, trust, and reciprocity. The different standpoints of James V. Neel and Francis L. Black could be related to different personal experiences and relationships. Neel seemed to fear negative consequences for individuals he knew or whom he perhaps felt personally responsible for—or at least individuals he was not indifferent about. What exactly he feared, however, is not mentioned, and the text does not make an attempt at explicating it, either. The argument about how the government could draw on genetic determinism to find excuses for not addressing health problems in Indigenous communities seems rather general; it would be interesting to know whether Neel had experienced a concrete situation where such a strategy was in place and had negative impacts on persons he was acquainted with.

While this correspondence shows Neel in a positive light and depicts Black as a somewhat less emphatic colleague, I would like to emphasize that the historical actors were engaged in relationships in many complicated ways, and certainly both had blind spots in their social engagement. Each of them might have paid attention to some injustices or some communities that they had been more involved with while neglecting others or perhaps did not even recognize other communities or individuals that also deserved advocacy. Historians of science might point to details such as nutrition—how did researchers control for “adequate nutrition” when comparing populations from around the world?—details that are seemingly technical and too practical to be detailed in this paper but that could perhaps tell us more about the empathy scientists developed for the individuals studied.

Neel’s careful attitude and his sense that Black’s suggestion of miscegenation was politically problematic could serve as contrasting guidance in very different constellations. While in the case presented here, Black’s suggestion of miscegenation is seen as utterly problematic, in other cases, the scientists’ celebration of isolated communities can be seen as equally problematic. As the quote by Hugh-Jones (1977) reminds the reader, to “preserve” these “primitive people” is necessary because “there is much to be gained for ourselves” (3). Isolation and preservation were perhaps not the guiding ideals of all isolated communities all over the world, or, put differently, not all of those communities were isolated by their own choice. Hugh-Jones’s “ourselves” leaves open the question of whether he meant “ourselves” as modern humans or, specifically, “ourselves” the scientists. For scientists who favored

constellations that allowed for a clear-cut research design, isolated communities would be better remaining isolated for as long as possible. In this regard, scientists traveling to far, remote places around the world or to communities separate from majority populations to study isolated populations were perhaps confronted with a conflict of interest.

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Wonderful paper. As a critique of scientific production, it has the thoroughness and balance needed to convince the reader of the inescapable political component in research, particularly when dealing with Indigenous peoples. The authors selected an especially suitable case—a dispute, both public and private, between two prominent biomedical scholars—to dismantle the obtuse debate about the pseudoproblem of science versus antiscience that cropped up in US academic circles in the aftermath of the *El Dorado* scandal in the early 2000s. To this effect, the authors “excavated,” as they say, the intimacy of the private correspondence between James V. Neel and Francis L. Black and, to boot, exposed the pitfalls of the peer review method of scholarly evaluation.

I shall focus on four points that I deem central to the article. First point: the authors clearly and elegantly demonstrate that there is no such thing as a neutral scientific activity. Ethics and research can never be torn apart. The chimera of pure science becomes apparent in their discussion of Black’s “scientific opinion” (a sort of oxymoron, for “scientific” and “opinion” do not go well together) that miscegenation would increase the Indians’ resistance to epidemic diseases. Devoid of any supporting evidence, Black’s insistence on this politically loaded interpretation raised many an eyebrow and trapped the author in an ethical quagmire. This point leads to a second consideration, which is the political weight of scientific statements. The inadequacy of Black’s infelicitous opinion illustrates how oblivious a foreign scientist can be to local realities. It is an example of arrogance that blinds an otherwise prominent professional to the possible consequences of what one can simply see as his “absentminded imperialism.” Unconcern for the political and ethical issues that are always present in scientific research can be as damaging as explicit contempt for the fate of the people who, after all, make research possible.

Third, disclosing the private letters of Neel and Black to each other and to editors and colleagues contributes greatly to elucidating the entrails of scientific production. Factors alien to the sheer quality of a text can seal its fate. Personal preferences, self-defense, bickering, interested “opinions,” and other perfectly human but hardly scientific considerations lay bare what otherwise passes as objective evaluations under that double-edged sword that is the peer review system. Similar to the impact of Malinowski’s intimate diary on anthropologists, exposure of

editors' and reviewers' hidden agency, rather than damaging academia's reputation, contributes to a more realistic perception of what it is all about. Worth noting is the authors' remark on the impropriety of relegating politics and ethics to the archive, "to the back stage of peer review, as opposed to being regarded as fundamental to the content of scientific publication." I could state it no better: such procedure, they say, "puts the very people who serve as the subjects of knowledge at risk."

Fourth point: when the authors themselves play an active part in the story they tell, they enhance the integrity of their endeavor. Indeed, the moment we express ourselves, vocally or in writing, we are part of the story. They are felicitous in recognizing this fact. Their choice of subject matter, their commitment to "excavate" hidden agendas, their up-front attitude regarding political and ethical concerns, and, foremost, their respect, not for opinions, but for the hard and continuous work of the professionals they scrutinize, are positive points the authors have won with this meticulous and careful analysis of the role of political awareness in scientific undertakings.

One final remark: Santos and Coimbra come from an anthropological background that has always emphasized the political aspect of the profession. They and I belong fully to the same tradition (Brazilian anthropology, the National Museum, Universidade de Brasília, "periphery," dictatorships, chaotic development, jeopardized citizenship, Indigenous suffering, etc.). This is why I feel so comfortable commenting on their paper and so pleased to become part of their script. After all, we grew up professionally with the adage—applicable to other fields as well—that to do anthropology is always a political act. We speak the same language in more ways than meet the eye. Our archive is open to scrutiny, our political commitment is explicitly stated, and our belief in academic quality is unshakable. Like Santos and Coimbra, I am convinced that serious academic work can advance the Indigenous cause. In the course of their research on the academic trajectories of Neel and Black, they "came to understand themselves as both historical actors and present-day historians." *Mutatis mutandis*, so did I.

Reply

*Composer of destinies
Drum of every rhythm
Time, time, time, time
I do a deal with you
Time, time, time, time.*

("A Prayer to Time," Caetano Veloso, 1979)

For the two Brazilian authors of this text (Santos and Coimbra) especially, perhaps in part because of their lengthy involvement with the themes being discussed, writing this reply proved to be a more difficult exercise than they had first

imagined. To some extent, it feels that the present work began to be "written" (or, at least, a substantial number of its ideas gestated) in 1992, when they had their first encounter with the article "Why Did They Die?" by Francis L. Black. Over the quarter of a century since ("time, time, time, time"), there have been many intersections of trajectories and "destinies" (including developing a collaboration with Joanna Radin) and many sociopolitical transformations, including the difficult political moment that Brazil is currently living through.

We are grateful to the nine commentators for their incisive and inspiring reflections. Though we wish that we had the space to address them all, we have opted to attend first to those insights that cut across multiple responses to the paper. First, it was fascinating to see the ways that the episode we described prompted commentators to draw parallels with equally complex processes ("echoes of deeper history," as Jones puts it) that unfolded in other regions of the world, places as diverse as Italy, Libya, and Mexico (Berlivet); North America at various historical moments (Goodman and Jones); and Amazonia itself (Cozzoli, Langdon). Second, and relatedly, it was encouraging to see commentators reflect on the broader analytical potential of considering the peer review process as a political archive (Conklin, Lipphardt, Ramos). Third, more than one commentator drew attention to the centrality of research ethics as an issue in contemporary anthropology (Conklin, Langdon, Ramos), exploring how it has emerged from historically specific episodes of conflict.

Below we turn to a series of specific additional responses to important aspects of our text raised by the commentators. To start, Conklin asks to what extent Black received differential treatment from both Neel and us because of his prestigious academic position. She questions whether "a less established scholar [would] receive the same deference accorded to an eminent professor at an Ivy League University." Conklin's point implicates us in the treatment we give to particular figures in our analysis. It is worth reiterating that although we had always been concerned with the implications of Black's formulations on Indigenous peoples in the 1990s, his scientific output, begun in the 1960s, and, in particular, his formulations concerning the epidemiology of contagious diseases and the demography of human populations are considered fundamental contributions to the study of the health of Amazonia's Indigenous peoples (Coimbra et al. 2002; Dent and Santos 2017).

Moreover, the relationship that Santos and Coimbra—particularly the latter—maintained with Black was not limited to a specific moment (e.g., joint participation in an editorial project, as in the case of the edited collection *Saúde e Povos Indígenas*; Black 1994b) but covered multiple instances over a time span of approximately one and a half decades. During the 1990s, there was ample correspondence between Rio de Janeiro and New Haven involving the exchange of publications, invitations to take part in scientific events in Brazil, and even Black's involvement in discussions as a member of the editorial board of *Cadernos de Saúde Pública*, a peer-reviewed journal published by Fiocruz for which Coimbra was editor in chief for two decades (Coimbra

2012). Black visited Fiocruz more than once, where, in addition to his contact with Santos and Coimbra, he maintained relations with other research groups in the areas of virology, immunogenetics, and vaccinology. We mention this pattern of multiple interactions, some of them of an everyday kind, to demonstrate that the relation between two Brazilian researchers and an influential foreign researcher was not sporadic and distant. In this sense, we see the “deference” to which Conklin refers as part of a relationship that, in fact, though involving researchers located at different moments of their careers, was fairly horizontal (as well as cordial). As Kowal points out, however, these research relationships were mediated by a strong affective component. Indeed, our analysis of the exchange between Black and Neel owes a debt to the research of historian of science Susan Lindee, who has emphasized how emotions arising out of the high stakes of genetics research must be considered in accounts of knowledge production (Lindee 2013).

More significantly, Conklin’s point raises the issue of whether Indigenous desires would have been or were taken as seriously by either Neel or Black. And, as a corollary, it should prompt us and other scholars today to consider what Indigenous peoples, especially those whose bodies form the basis of research in anthropology, want from science. Historian of science Rosanna Dent’s work with members of the Pimentel Barbosa Xavante, another community that Neel (and Santos and Coimbra) was scientifically engaged with, is instructive in that it demonstrates how research relationships within the same community can change over time (Dent 2016, 2017). This may come to include the possibility of refusal to engage in biomedical research, even if researchers believe that research is ethical (Benjamin 2016). We will return to this issue below.

Goodman and Kowal directly, as well as Jones more indirectly, call attention to an issue that they believe would have been relevant or even fundamental for us to explore in our text—one that might further contribute to clarifying the emotions and motivations behind the exchange between Black and Neel. For all three commentators, our analyses should have included the fact that Neel himself formulated a hypothesis based on genetic predisposition to disease known as the “thrifty genotype,” which he first proposed in 1962 to account for the higher frequency of type 2 diabetes mellitus and other chronic nontransmissible diseases in Indigenous populations (Neel 1962, 1982, 1999). The thrifty genotype became one of Neel’s best-known formulations and has persisted despite strong criticism that it does not consider important sociopolitical and environmental determinants of diabetes (Hay 2018; McDermott 1998; Paradies, Montoya, and Fullerton 2007). In his comment, Goodman argues that “Neel, in his postulation of a thrifty genotype, did pretty much exactly what he warned Black away from; that is, he promoted a genetic theory when many alternative explanations were closer at hand.” For Kowal, “the thrifty gene hypothesis strikes me as the most appropriate point of comparison with the ‘amalgamation’ dispute outlined in the article.”

It is notable that we did not come across any documents in Black’s archives in which he—someone with an in-depth

knowledge of Neel’s work on the genetics and health of Indigenous peoples—made use of this counterargument in his correspondence with Neel. However, we absolutely agree with the commentators that it is both pertinent and analytically productive to address the question of Neel’s own formulations, especially those related to the thrifty genotype, within the spectrum of ideas discussed in our text.

In doing so, it is also important—with reference to both Conklin’s response and Lipphardt’s interest in whom Neel was concerned about protecting—to situate him as one of the most high-profile scientific diplomats of the Cold War. When the two scientists initially diverged in their views in the 1970s, Neel was already deeply embedded in international epidemiological and public health networks, going back at least to his study of the impacts of the atomic bomb on survivors of Hiroshima and Nagasaki (Lindee 1994). Neel’s reputation for conducting scientific research with high stakes for human health had led him to assume leadership roles in various international projects and committees, including the Pan American Health Organization (PAHO) and the World Health Organization (WHO; de Chadarevian 2015; Lindee 2004; Neel 1994). It was his studies of the effects of radiation on human biology that led him to become interested in Indigenous peoples, whom he imagined as relatively unimpacted by radiation as compared with Japanese survivors (Lindee 2004; Santos, Lindee, and de Souza 2014).

As part of their human biological research in Amazonia, Neel and his collaborators conducted community surveys in which they collected a broad range of data related to health status. The findings of those studies were considered relevant to debates about the design and implementation of health care initiatives targeted at Indigenous peoples in South America. This was during a period when many South American countries were under the rule of military dictatorships that, in the Brazilian case, included a regime notoriously hostile to Indigenous peoples’ rights. Neel’s awareness of this circumstance also led him to write, in addition to his highly technical contributions to the science of genetics, works aimed at the South American community of public health planners and professionals, as well as PAHO and WHO officials. In the publications written for nongeneticists, Neel drew on history and called attention to contemporary public health information. He believed that such contextualization was necessary to better understand and prevent the high mortality rates due to infectious diseases experienced by Indigenous communities after entering into permanent contact with surrounding national societies. He advocated vaccination (very limited at the time) and installation of primary health care infrastructure. He also emphasized the importance of paying particular attention to Indigenous peoples’ foodways (Neel 1968, 1974).

In this sense, when Neel wrote Black warning that his ideas “could play . . . into the hands of those who do not wish to make adequate provision for the transition of these people and rather prefer to see amalgamation,” it seems to us that Neel likely felt accountable to the international public health community for the way that Black’s scientific arguments could be used, possibly by

government sectors, in a manner that might directly harm the well-being and survival of Indigenous peoples. It was, perhaps, from this (self-perceived) position of a political actor involved in debates highly charged with immediate practical implications, including the use of certain scientific ideas in health actions by agents of the state, that Neel made his remarks on Black's theoretical formulations and implications.

This makes his relative lack of concern for the same potential consequences surrounding the thrifty genotype all the more interesting and distressing. It remains an open question, worthy of historical study in its own right, whether Neel perceived himself as the proponent of a theoretical formulation that "could play into the hands of" specific sectors within the United States, with implications for marginalized human populations. Concrete answers may be found in his papers, which fill over 100 feet of meticulously cataloged archives at the American Philosophical Society in Philadelphia,⁵⁷ but it is worth a bit of further speculation here about the extent to which Neel's criticisms of Black parallel his own reflections on the implications of the thrifty genotype.

The thrifty genotype hypothesis did not initially receive much attention when it appeared in 1962; in the decade after it was published, it was cited only four times. Yet as new animal experimental models appeared to corroborate his ideas in the 1970s and 1980s, the hypothesis gained traction. In Neel's memoir, published amid the proliferation of criticisms of the hypothesis in the 1990s (as summarized by Paradies, Montoya, and Fullerton 2007), he maintained his belief that there were genetic bases for explaining the higher predisposition among some populations toward the emergence of diabetes over individual life cycles. At the same time, he stated that "there is now little room for argument with the proposal that health (i.e., genotype expression) would be substantially improved by a diet and exercise schedule more like that under which we humans evolved" (Neel 1994:355). The implication is that Neel was willing to acknowledge that, while genes mattered, what one did with one's genes mattered more. In this way, Neel's approach seemed to be one aligned with epigenetic approaches that understand individuals as having agency in altering their genetic destiny (Rozek et al. 2014). While less deterministic than Black's perspective about the role of genetics in immunity, such an approach does not address the ways that structural factors like socioeconomic inequality and environmental degradation often pose more significant barriers to health than individual behavior.

Perhaps more significantly, the case of the thrifty genotype compels us to ask, in a way similar to our treatment of Black's ideas, whether Neel understood himself as promoting scientific ideas that could directly impact the lives of the disenfranchised people he was investigating. There are examples that show that Neel was certainly capable of taking a political position with relation to his own studies. For instance, he was deeply involved in work on the genetics of sickle cell anemia, which disproportion-

ately affects African Americans, in the 1940s, and he allied himself with the civil rights movement in the United States (Neel 1994).

So while Neel was very much aware of and willing to be outspoken about the political implications of scientific knowledge production in at least some circumstances, he did not explicitly develop in his published writing a self-criticism about the implications of the thrifty genotype hypothesis. He also did not reflect on the ethics of casting Indigenous groups as natural experimental laboratories, valuable to genetics for their purported homogeneity. As Lipphardt has pointed out, the kind of miscegenation advised by Black would have been at odds with a research program that privileged the study of groups understood to be isolated and endogamous. If Neel had offered such reflections, we might know more about what motivated his specific concern for the misuse of scientific knowledge in the case of his disagreements with Black. In their absence, it is still possible to emphasize that his interest in Indigenous health was likely inextricable from his engagement in the Cold War geopolitics of international public health and his role as a leader in the human biological research of the era.

With these considerations in mind, we can now turn to Jones's provocation, which hones in on the high stakes of our analysis and provides us with the opportunity to be even more explicit about our own sense of accountability for the kinds of historical knowledge we have produced. Jones calls attention to our use of certain words, mentioning that "some readers will object to the occurrence of 'decline' and 'coincided' in their opening sentence." For Jones, use of the word "coincidence" potentially strips our narrative of the causal link between the arrival of Europeans in the Americas in the fifteenth century and the subsequent demographic reduction of Indigenous populations.

We must accept responsibility for how our language has betrayed our intentions. If there is a lesson to be learned from the debate between Neel and Black, it is that technical knowledge—be it epidemiological, historical, or anthropological—has political consequences. For this reason, we are grateful for the way that the *Current Anthropology* treatment makes important critiques of our work visible to those who will encounter it in the journal when it is published and when it, too, becomes a historical trace. As such, we are grateful to Jones for providing us with the encouragement to self-consciously perform our argument and assert, without ambiguity, that the genocide perpetuated against Indigenous peoples is unacceptable. As Alcida Ramos points out in a comment that particularly touched us, "We are part of the story."

The present political situation in Brazil has made the circumstances in which Neel and Black worked in the 1960s appear frightfully relevant. This point is mentioned by Berlivet, who opens his comments referring to the lurch in the direction taken by public policies in Brazil intended for protection of Indigenous peoples' constitutional rights following the inauguration of Jair Bolsonaro as Brazil's president in January 2019. The notion of the assimilation of Indigenous peoples by national society, which we discuss in our text as a historical moment, has returned to the agenda with force, circulating as a volatile fuel deep within the

57. See <http://search.amphilsoc.org/collections/view?docId=ead/Mss.Ms.Coll.96-ead.xml>.

state (Bolsonaro 2019; ISA 2019). Our discussion of the political developments that had taken place since Brazil’s return to democracy at the end of the 1980s in the original text appears to us now as painfully shortsighted.

No less dramatically, in the precise week that we finished writing this reply, news about the increase in forest fires in Amazonia has become an intense political and diplomatic issue between Brazil and the European community, with the issues of Indigenous lands being central to the arguments (Symonds 2019). Leading scientific periodicals—in a move rarely seen—have published articles that call attention to major setbacks in Brazilian public health policies designed to assist Indigenous peoples (*Lancet* 2019). Perhaps more than at any other moment of the last half century, in Brazil today there is a very concrete risk of regression in the public policies targeted at Indigenous peoples (Carneiro da Cunha et al. 2017; ISA 2019).

Langdon’s and Ramos’s comments connect with the implications of Jones’s observation, whether examining the construction of the current policy of research ethics in Brazil or the participation of the scientific community (and that of anthropologists in particular) in political debates about Indigenous peoples’ rights over recent decades. Again, let us be unambiguous. Indigenous peoples, while perpetually imperiled, are not without agency. Making space for Indigenous self-determination has never been more important, and that includes expressions of what kinds of relationships to science—and other forms of settler knowledge making—they wish to have. And it is in this spirit that we wish to give our last words to them. Sonia Guajajara, a prominent Indigenous leader in Brazil, has loudly decried Bolsonaro’s recent statement before the United Nations (Bolsonaro 2019), which “reinforces his colonialist perspective, from the time of dictatorship, a view that favors assimilation, as if everybody had to be the same. It is a dictatorial statement, that disrespects the diversity of [Indigenous] peoples in Brazil.”⁵⁸

—Ricardo Ventura Santos,
Carlos E.A. Coimbra Jr., and Joanna Radin

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58. From an interview with Sonia Guajajara, available at <http://reporter.brasil.org.br/2019/09/bolsonaro-representa-a-maior-ameaca-ao-planeta-diz-sonia-guajajara/>, accessed October 7, 2019. See also Guajajara (2019). This article addresses the historical and sociopolitical debates surrounding vulnerabilities associated with historic epidemics of contagious diseases, in particular the measles outbreaks that affected several Amazonian Indigenous groups in the 1960s and 1970s. Half a century later, in 2020, the COVID-19 pandemic has rekindled central issues addressed in this article. In May of 2020, when we were finalizing this article for publication, preliminary reports indicated that Indigenous communities were experiencing disproportionately high rates of illness and mortality with respect to the broader non-Indigenous Brazilian population. Because of the timing of the editorial process, we cannot incorporate into the article’s set of core arguments epidemiological and sociopolitical specificities brought by the current pandemic. However, we wish to amplify the coordinated response to COVID-19 being made by members of the contemporary Indigenous ethnopolitical movement (an incipient social phenomenon of the previous century). Indigenous activists have made concrete demands of the Brazilian government for prevention and treatment actions designed to avoid repetition of the scenes of catastrophic mortality seen in the past as a result of contagious diseases, particularly viruses. We offer the current publication in solidarity with Indigenous and other communities around the world who have been historically disenfranchised from health care and denied self-determination.
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