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# Voices of the Dead: James Neel's Amerindian Studies

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## Voices of the Dead: James Neel's Amerindian Studies

### **Abstract**

During his 1967 fieldwork, James V. Neel, professor of human genetics at the University of Michigan, spent a good deal of time collecting chicken dung. He scraped up dirt and chicken waste from the ground around the Yanomamö villages. He sought out dirt from the floors of the Yanomamö houses, where parrots were kept as free-roaming pets. He crawled under chicken coops, filling seventy-five labeled plastic bags with samples, using a fresh plastic spoon for each sample, and he worried about getting this soil and bird waste safely back to Atlanta, Georgia, for testing at the Centers for Disease Control (CDC).<sup>1</sup>

### **Disciplines**

Anthropology | Cultural History | Genetics and Genomics | History of Science, Technology, and Medicine | Science and Technology Studies

## 2

# VOICES OF THE DEAD: JAMES NEEL'S AMERINDIAN STUDIES

M. Susan Lindee

Spokesmen for the investigating commission and expert psychiatrists established that the members of the criminal gang were hypnotists of extraordinary power. . . . They were able to remove from the field of vision things or people who were in fact within that field of vision. . . . And so almost everything was explained, and the investigation came to an end, as everything in life comes to an end.

—Mikhail Bulgakov, *The Master and the Margarita*

During his 1967 fieldwork, James V. Neel, professor of human genetics at the University of Michigan, spent a good deal of time collecting chicken dung. He scraped up dirt and chicken waste from the ground around the Yanomamö villages. He sought out dirt from the floors of the Yanomamö houses, where parrots were kept as free-roaming pets. He crawled under chicken coops, filling seventy-five labeled plastic bags with samples, using a fresh plastic spoon for each sample, and he worried about getting this soil and bird waste safely back to Atlanta, Georgia, for testing at the Centers for Disease Control (CDC).<sup>1</sup>

Chicken dung was only one of many things extracted from the Yanomamö and their environment during Neel's field research in the 1960s and 1970s. Blood, stool samples, saliva, urine, viruses, parasites, dental casts, human milk, tissue samples, and texts describing physical examinations and family relationships moved from the villages along the Orinoco River to the CDC in Atlanta, to the University of Michigan, and eventually to other laboratories and universities and tissue banks.

Neel's decision to continue to collect these objects and materials in early 1968—despite a major medical crisis in the form of a measles epidemic—is one of the aspects of his fieldwork that may be particularly jarring to twenty-first-century sensibilities: the blood samples remained a very high priority for Neel, even as those around him were dying. Indeed, throughout his field research in Central and South America in the 1960s and 1970s, he measured his success by the number of blood samples he sent back to Ann Arbor or to Atlanta. He celebrated when his group collected more samples than expected, and he was distressed when the yield was lower.<sup>2</sup> Furthermore, he built a network of South and Central American agents—anthropologists, missionaries, physicians, government officials—who could collect blood samples and other materials by proxy for his research program. The vacutainers would be shipped from Ann Arbor ready to fill. The return flights would be announced by cable so that someone from Neel's lab could be ready to track and collect the samples when they arrived. Neel engineered a system of social and bodily resources that brought blood and many other field materials into his laboratory for analysis and interpretation.

These things were signs, clues, evidence. But signs of what? In 1966, Neel told a mycologist at the CDC, “I believe I can assure you that from your standpoint, these specimens come from people who have had absolutely minimal contacts with the Western world; i.e., they come about as close to reflecting the disease pattern of the primitive Indian as we are going to be able to find.”<sup>3</sup> The extracted materials were signs of the “primitive,” of the original state of man, and of the conditions under which human evolution had occurred, when “man” became “man.” And they were the primary justification for the field research: “Although we collect much information in the field through genealogies and physical examinations, the real medical pay dirt comes from the careful and intensive study of blood, saliva, urine, and stool specimens which we collect in the field.”<sup>4</sup>

In his collecting, his analysis, and especially his theorizing, I would suggest, Neel was engaged in a resurrection project, in which the bodily and environmental traces drawn from the populations living around the Orinoco could be used to reconstruct the experiences of people who had been long dead, the people who had experienced the forces that shaped human evolution 10,000 or more years ago. In chicken dung, which might contain the fungus that caused the respiratory illness histoplasmosis, he saw the human gene pool and, by extension, the human future. Stools, urine, hair samples, and saliva were prosaic and cosmic, waste and evidence, contemporary and ancient.

In this chapter, I explore how and why Neel and his collaborators gathered the diverse objects they extracted from tropical field sites. I notice particularly what Neel and his

diverse collaborators sought to learn from the many things that came from the field. I assess why Neel chose to work in Brazil and Venezuela, what he thought he was looking at when he looked at the Xavante and Yanomamö, and what his field practices and the material legacy of those practices (the blood samples stored in State College, Pennsylvania) suggest about changing human subjects research protocols over the last forty years. I propose that the bodily traces taken from Amerindian groups constituted a form of testimony that could be used to reconstruct the experiences—with some literary license, “the voices”—of the dead. The Yanomamö, Xavante, Cayapo, Seminole, Cuna, Creek, Bokota, Caddo, Machusi, Wapishana, Guaymi, and other groups tracked by Neel and his colleagues spoke of disease and survival directly through their blood. But the dead who wandered through these scientific texts also included all the humans and protohumans who had experienced “human evolution,” people who had been dead so long they spoke only through the bodies and antibodies of living “primitives,” through the titers compiled in Atlanta laboratories, or the fecal parasite counts prepared by a New Orleans specialist in tropical medicine.<sup>5</sup> The dead in my text also include James Neel, who died in February 2000, eight months before the Tierney controversy, and who left behind a remarkable textual record, both published and unpublished, of his life and his work.

## **Beginnings**

Neel may have been interested in isolated South American populations before 1955, but it is clear that his professional relationship with Francisco Salzano played a critical role in

the initiation of actual field research in Brazil, which then led to other studies in Panama, Costa Rica, and Venezuela.<sup>6</sup>

Salzano first contacted Neel in December 1955, with a request to spend a year in Neel's laboratory. He had been reading the *American Journal of Human Genetics*, he said, and had found the work coming out of Neel's lab particularly relevant to his own interests. He had tentative Rockefeller Foundation funding and hoped that the year at the Heredity Clinic in Ann Arbor would prepare him to develop a program in human genetics at the University of Rio Grande do Sul, where he was on the faculty of genetics. A young *Drosophila* geneticist, Salzano had published several papers on chromosomal polymorphism in South American *Drosophila* and on a new species of *Drosophila*.<sup>7</sup> Neel replied almost immediately that he was happy to hear about Salzano's plans, but that certain points needed to be considered. First, he wondered if Salzano had sufficient mathematical training. Second, he said a year was not long enough to carry out a research project in human genetics. "When you shift your attention from *Drosophila* to man (as I have also done), you must adopt a different time scale. Problems in human genetics move much more slowly than those utilizing fruit flies."<sup>8</sup> Meanwhile, Neel wrote to a contact at the Rockefeller Foundation to ask his opinion about Salzano. "I rather prefer to have a candidate sell himself to the investigator with whom he wishes to have a fellowship experience," this contact said, "but can tell you that Salzano has a very nice personality and would, I am confident, fall in 100 percent with any program you might lay down for him."<sup>9</sup>

Salzano arrived in Ann Arbor in early September 1956. One of his projects that year was to prepare a survey or summary

paper on studies of Amerindian groups, and this paper suggested some of the inadequacies of earlier studies (Salzano 1957). He noted that at the time there were already ninety-five published scientific papers dealing with blood groups in South American Indians. Beginning in the 1930s, investigators had tracked down isolated groups, convinced individuals to submit to a blood test, labeled the blood to be sent in to the laboratory for processing, and aggregated the findings to reach conclusions about human history. The Carib, Guajiro, Piaroa, Guahibo, Arawak, and Caramanta had all been bled and tested—439 Pijao Indians in Colombia subjected to blood testing for a paper published in 1944, and almost 3,000 Andean Indians of various groups in Ecuador in 1952. The Quechua of Peru, Tucano of Brazil, Alkuyana of Surinam, Mataco of Argentina, Macá of Paraguay, and Panzaleo of Ecuador—all bled and thereby physically brought into technical explorations of race, migration, mutation, and “white admixture,” their blood providing signs, clues, evidence. The blood samples analyzed in the laboratory revealed the presence of the Diego factor, different proportions of the ABO groups, the MN groups, S antisera, and Rh factor, and presumably historical relationships between Amazonian and Andean groups.

But Salzano was dissatisfied with these data and research programs, observing that simply collecting blood samples did not provide enough information for the results to be analyzed in a meaningful way. Blood sampling and laboratory results left out too much about culture, about health, and about known historical relationships that sometimes contradicted what might be assumed based solely on blood tests. “It would be highly desirable that future surveys would include data on the mating system of the tribes under study, the degree of



consanguinity of the individuals studied, and the effective population number of these populations. This of course, would require much more time and energy than is now devoted to these surveys, but would greatly increase their value” (Salzano 1957: 575). His acknowledgment in this paper thanked both Neel and James N. Spuhler, a University of Michigan biological anthropologist interested in blood groups, human behavior, and human biology.

Why did Neel himself begin to work with isolated populations in Brazil and Venezuela? There are several possible explanations. Neel was certainly attracted to the romanticism of the “vanishing world” of the Amerindian, at least partly because he seems to have interpreted fieldwork as a test of character and masculinity (Neel 1994). As Oreskes (1996) suggested in her exploration of scientific heroism, the field was seen by many scientists as a particularly masculinized research site, a place where heroic efforts might be required and physical strength was necessary. Rossiter (1995) has documented the systematic exclusion of women scientists from many kinds of fieldwork, in some domains as late as the 1980s. The field was a place where a male scientist might test himself and demonstrate masculine resolve. In one particularly revealing letter from 1965, Neel told one of his administrative assistants:

Our man in Venezuela [a reference to Napoleon Chagnon, then on his first field trip with the Yanomamö] is in a situation guaranteed to test the strongest. It’s difficult to describe to one who has not experienced it, the strange mixture of exhilaration and foreboding which quickly develops when suddenly all those familiar props you grew up with are gone and you are very much alone, or virtually alone,

in a completely strange culture which doesn't regard your life or interests as much concern of theirs. Many men can't take it. These include even professed anthropologists, which explains why PhD theses in that field often do not get the follow-up they should.<sup>10</sup>

Thirty years later, in his autobiography, he made a similar point, evoking the “elaborate rationalizations as to why we do this or that” and suggesting that his Amerindian work was an opportunity to test himself, and his character. “Mine had thus far been a rather safe life . . . in a setting where help in case of miscalculation was close at hand. This would be different” (Neel 1994). A historian interested in psychological motivations could therefore propose that the Amerindian work began because Neel reached middle age. Like many others before and since, he felt compelled to prove himself, in this case through a highly masculinized notion of physical trial in the field and in contact with “natural” man.

From a different perspective, a historian could propose that the Amerindian work began because of Neel's sense of history and his concerns about the long-term impact of science and technology on human health. For someone deeply committed to the legitimacy of the technoscientific worldview, Neel could be quite skeptical about the impact of “progress.” He commonly invoked the threat of “civilization” as an urgent justification for his Amerindian studies: “The relatively few remaining primitive populations of the world were so rapidly being disrupted that ours was almost surely the last generation to encounter any of them in relatively undisturbed condition,” he wrote in his autobiography (Neel 1994).

Neel's first major field research, of course, had focused on a population as devastated and temporally limited as any group of isolated forest dwellers—and one directly damaged by technoscientific progress. These were the Japanese survivors of the atomic bombings at Hiroshima and Nagasaki, where Neel began work in the spring of 1947. In Japan, aided by local physicians, nurses, midwives, and scientists, and working with his long-term collaborator William J. Schull, he developed an elaborate and complicated genetic study of the survivors' offspring, in whom germ-line mutations resulting from atomic radiation might be presumed to be expressed and visible. His subjects were uniquely vulnerable products of history and of science, the focus of worldwide sympathy and outrage, at the center of vituperative unfolding debates over nuclear weapons in the 1940s to the 1960s, and they were victims of what was arguably a technology that changed “what it means to be human.”<sup>11</sup> A young M.D. and Ph.D., with a strong grounding in *Drosophila* genetics, Neel was suited not only by training but also by temperament and political skills to take over the genetics project. He handled field research well, could manage the cultural tensions of working in occupied Japan, and tolerated the ambiguity of much of the data analysis, at times almost reveling in the complexity of the problems before him (Lindee 1994).

Like the atomic bomb survivors, Amerindian groups were a limited resource, a human database that was available only for a brief historical moment, the moment Neel interpreted as the period between first contact and assimilation. Neel believed that the 1960s constituted this moment. He said there was a great need for parallel studies of selection in both “advanced and primitive societies,” and that such work could be important in “our efforts to understand man's past and predict

his future” (Neel 1958: 59). Neel then presented a research justification that could be applied to his later work with the Xavante and the Yanomamö:

Over the world, primitive people are being projected in a few generations from a Stone Age to an Atomic Age culture. In making this transition, they will be called upon to telescope into a few generations biological and cultural adaptations which have extended over a period of thousands of years in Europe. To the extent that these adaptations involve genetic systems, here is a priceless opportunity to study biological selection. One of the most exciting opportunities today is that of collaborative studies along these lines between anthropologists, geneticists and physicians all over the world. (Neel 1958: 59)<sup>12</sup>

Neel’s first visit to Brazil was, appropriately enough, in his guise as radiation expert. In 1961 he attended a World Health Organization meeting in Rio de Janeiro, where the group considered the feasibility of biomedical studies in areas of high natural radiation in Brazil. But on that same visit he met with his former post-doctoral fellow, Salzano, to discuss possible studies of Brazilian Indians,<sup>13</sup> and Salzano provides my third explanation for Neel’s Amerindian studies: his relationship with a culture broker. While there do seem to have been both psychological and temporal factors shaping Neel’s interest in indigenous groups in South America, a historian more interested in social networking might propose that Neel began to work with Amerindian groups because he began to work with someone who could be a translator, a local contact. Salzano led him directly to the Mato Grosso and the Xavante, and from there to the massive research agenda he constructed around this first fieldwork in Brazil.

It is clear that in the early years Neel and his American collaborators depended heavily on Salzano's judgment. They deferred to him on the question of which groups should be studied, and they often seemed to assume that he could and should direct their selection of field sites. For example, less than four weeks before the departure date for the second field trip to Brazil in 1964, Neel still did not know which tribe he would be studying. He expected to be in Brazil with a large interdisciplinary team by April 4, and on March 10 he wrote to Salzano saying, "Please drop us a quick note as soon as a tribe has been selected, so we can read what is available here. [Terry] Turner has just spent a day with us—the Gorotire and Kuben Kran Keng still look good, but the Xavante and the Xingu tribes perhaps even better."<sup>14</sup> Salzano helped Neel make the necessary contacts to start field work in Brazil, which in turn prepared him to expand into Venezuela and other locations.

Research with Amerindian groups suited Neel. It suited his intellectual interests, his concerns about modern culture, his personal sense of what it meant to be male and masculine, and his social networks. Brazil and Venezuela were close and convenient, easily accessible from Ann Arbor. Neel later wrote that one of the advantages of working with Brazilian groups was "their relative proximity to the laboratory facilities of the Department of Human Genetics in Ann Arbor. This was important, because in conjunction with the field work we would be sending a stream of biological samples back to Ann Arbor for analysis."<sup>15</sup>

## The Total Population Concept

If the question were posed in this way—What exactly was James Neel *doing* in South America?—one perfectly reasonable answer would be “collecting blood and other human bodily materials and fluids, and shipping them back to Ann Arbor.” His letters and notes are filled with details about specimen handling, identification, storage, shipment, and assessment. His field notes, particularly, constantly refer to the numbers of blood, saliva, urine, or stool samples collected that day and to the frustrating uncertainties of getting those materials out of the forest and back to Ann Arbor. Some examples, of no particular note, taken from his 1968 field trip: “Up early, organized and here in village today get pedigrees, 87 ACD, 76 clots, and 82 salivas, and Chas teeth and Arends special studies. Work until 2:00. Pack the specimens, get Boris off” (23 January 1968); “Up at dawn and draw 44 bloods by 10:15” (25 January 1968); “Spent the AM finishing obtaining blood and saliva spec. the Maks here at Santa Maria 64 specimens” (27 January 1968); “We are in our first real bind, with a fridge full of blood but no transportation” (27 January 1968); “A good day—73 bloods and salivas” (28 January 1968); “Full day at the Dojo—33+ physical exams, anthro’s, salivas, bloods, 9 urines, BCGs. Busy, busy” (30 January 1968).<sup>16</sup>

Neel was looking for invisible things, for *Streptococcus mutans* (dental caries) and *Histoplasma capsulatum* (histoplasmosis), and for copper, lead, cadmium, and mercury, for antibodies against salmonella, streptococci, the enteroviruses and arboviruses, for hepatitis B, diphtheria, malaria, and for roundworm, hookworm, pinworm, and

amebic dysentery. These invisible things, detectable in the laboratory, covert in the field, would provide clues to the disease pressures that might be shaping reproductive success in the Yanomamö. The individual life story—the medical history—was written in antibody response. “Medical histories [in these groups] are almost worthless,” he told a specialist in infectious disease who was helping him track down relevant sources relating to the streptococcal viruses. “There is of course no qualified observer in there continuously and, further, the moment you begin to introduce qualified observers the change in living habits is such that your observations from then on may not reflect the past. In other words, we are pretty well driven to a careful study of the antibodies to get a true bill of goods on what has gone on.”<sup>17</sup> The “true bill of goods” was not in whatever the Yanomamö or the Xavante might say about their health but in the evidence that their bodies had been exposed to diseases.

The antibodies revealed not only events that had occurred within living memory, however, but also events that had occurred during “the long period of human evolution.” “Basically what we are attempting to do is to obtain a picture of the Indian in pre-Columbian times,” Neel told a missionary at Manaus in September 1966.<sup>18</sup> Disease pressures, manifest in blood and stool samples and tracked through immune response tests at the CDC, could, he proposed, “make an extremely important contribution to our knowledge of the disease pressures operating on primitive man during the long period of human evolution.”<sup>19</sup> The Yanomamö and other Amerindians, he wrote later, presented “an imperfect mirror of the later stages of human evolution, the mirror cracked and dusty, but as accurate as any we have” (Neel 1994: 139). They were, in some fundamental way, like the Indian in pre-

Columbian times. They could stand in for the dead. Blood was a sign of the medical experiences of people both dead and living. In its responses, human history was revealed.

The framework guiding Neel's fieldwork and analysis of bodily materials was the "total population" concept, the idea that studies should include attention to the biological, biochemical, anthropological, linguistic, historical, and sociological characteristics of a population. This multidisciplinary approach was needed in order to understand human evolution, which Neel believed had occurred primarily in hunting-and-gathering groups that were very much like twentieth-century "primitives." "If we would understand modern man, we must study such primitive groups as still remain in a way in which they have rarely if ever been investigated to date" (Neel et al. 1964).

The first trial of this concept in the field occurred in the summer of 1962, when Neel, Salzano, the anthropologist David Maybury-Lewis, and several others visited a Xavante Indian village near the Rio das Mortes in the state of Mato Grosso, Brazil. Maybury-Lewis had worked with the Xavante in 1958 and knew the language and social structure. Salzano provided the local expertise. Neel considered the Xavante an ideal group, not because it was isolated and untouched—to the contrary, the Xavante had been interacting, often through warfare, with Europeans and outsiders since at least the eighteenth century—but because it was "at that critical point in its relations with the outside world when it is approachable but yet culturally intact." The Xavante were neither so remote and untouched that working with them would be difficult nor so acclimatized to the outside world that they could no longer



provide a model of the “primitive.” Furthermore, there was “a small airstrip near the post, providing the means for rapid transportation of blood samples to a base laboratory at Rio de Janeiro” (Neel et al. 1964: 53). Airstrips and pilots mapped onto research plans, and the “stream of biological materials” bound the research group to the islands of the Mato Grosso that were embedded in flight technology and Indian Protective Service programs.

On this field trip Neel and his colleagues set up their examination room at the post of the Indian Protective Service, which was one kilometer from the village and had better facilities for examinations. “For the first several days in the field all Xavantes looked alike, by virtue of their broad faces, their large noses, and their striking uniformity in skin, eye and hair color and style. But as familiarity grew, so did the ability to recognize differences, an ability increased by later study of the photographs obtained in the field” (Neel et al. 1964: 68). Ninety-one individual tribe members came to the post to be examined and interviewed about their families (for pedigrees) and bled. They were measured, and their skin, eye, and hair color codified on the Kruse scale and the Fischer-Saller table. Five standardized photographs were taken of each subject. The breadth of the inner zone of the iris was estimated. The thickness of the lips was measured. Fingerprints and palm prints were taken from some of these participants. The bodies of these ninety-one Xavante, ranging in age from newborn to approximately sixty, were thus packed into numbers and transformed into charts. The charts compared Xavante and residents of Hamburg, Germany, for a range of parameters, including the presence of the “Mongolian fold,” general form of head and face, dermatoglyphics, and intrapopulation variability. The

Xavante were also compared to other speakers of related languages, though as the authors pointed out in their 1964 paper, “We are aware that language is acquired rather than inherited, ie that these are not necessarily biologically related peoples” (Neel et al. 1964: 66).

In this anthropometric phase, the Xavante were constructed as a genetic whole, a breeding population that could be compared to “Europeans” or other groups in Brazil for phenotypic qualities of face, body, hair, and eyes. At the next level of analysis, the level of the blood, the Xavante were genetically diverse, differing in the MNSs, RH, Duffy, Kidd, and Diego traits. Neel saw blood as a way to get around the “problems inherent in eliciting accurate pedigree and demographic data from a short-lived, illiterate people where marriage occurs at an early age and the resulting clan affiliation is regarded as more meaningful than biological descent” (Neel et al. 1964: 89). Blood typing could reveal nonpaternity or inbreeding; analysis of blood antibody response could reveal disease exposure through time; tracking parasites (treponema, malaria) could reveal biological vulnerability or environmental variation.

After their second field trip in 1964, Neel and his collaborators began preparing an elaborate set of papers to be published together. Neel felt overwhelmed by the enterprise in the summer of 1966.<sup>20</sup> Each aspect of the multidisciplinary study seemed to need its own paper, and finally, in November 1966, Neel submitted ten papers about the Xavante—a linked series of texts laying out a vast empirical and theoretical frame for studies of the genetics of “primitive peoples”—to the editor of the *American Journal of Human Genetics*. These 153 pages of text could disturb any editor, and Neel expected

H. Eldon Sutton to be taken aback (though Sutton had been warned some time before that this was coming). “Before you throw up your hands in horror, let me make several points,” Neel told Sutton. He then mapped out a grand agenda:

I suspect these papers are going to help initiate a whole-scale reevaluation of many aspects of human population genetics. The field must move beyond the aimless accumulation of gene frequencies which now occupies so much space in the journals. . . . As I indicated in our earlier conversation, there are two papers here which are largely non-genetic, namely number VIII and IX, although even these have some interesting implications for population problems. I very much hope it will be possible to accept them also (assuming acceptance of the others) since they illustrate our concept of the need of the total study of selected populations if we are ever going to understand the interplay between the genetic and the environmental.”<sup>21</sup>

Eight of these papers finally appeared in the journal under the joint title “Further Studies on the Xavante Indians” (*American Journal of Human Genetics* 16 (1967): 463–574). Two others went to the *American Journal of Tropical Medicine and Hygiene*, apparently because Sutton concluded they were not relevant to a journal of genetics (Neel et al. 1968a,b). It was still a huge publication package, and an ambitious presentation of a research agenda that Neel and Salzano expected to have broader applications in human population genetics.

In their final summary paper, they proposed that it seemed “self-evident that much about contemporary man can be truly

understood only in the light of his past.” Some critics of population studies of this kind had questioned whether any of the surviving primitive groups were truly typical—given that all had been to some extent influenced by “contacts with higher cultures.” Neel and Salzano countered that

to denigrate these studies simply because the Garden of Eden is no longer pristine is surely not the scientific approach. There is a clear challenge to the geneticist to join in the study of these cultures in the greatest depth possible while the opportunity still persists, mindful of the presence of cultural contamination, striving (in the case of the American Indian) to detect post-Columbian influences, and searching among groups for common denominators which by their constancy suggest their primordial nature. (Neel and Salzano 1967: 555)

The theme of this final paper, which was intended to bring home the central point of the entire enterprise, was that “primitive” man in some ways surpassed “civilized” man. Here, Neel’s romantic ideas about balance, simplicity, and social order were fully expressed. Regarding Xavante controls on reproduction (including abstention, abortion, and infanticide), the paper noted that “at a time when the world is increasingly concerned with fitting its numbers to its resources, it is cause for contemplation that these ‘primitive’ people have met the issue of reconciling numbers to resources and way of life.” Regarding Xavante resistance to disease and parasites, they said “primitive man was in better equilibrium with his environment than is civilized man” (Neel and Salzano 1967: 567). They hoped that population genetics could help man “develop the society most consistent with both his present genetic endowment and his continuing evolution” (569). Neel had used the word “primitive” without

articulating any reservations in his earliest papers, but by 1967 he seems to have begun to recognize some of its political implications. In his paper with Salzano they said the word was employed “as a convenient synonym for a nonmaterial culture whose economy is based on hunting and gathering with or without simple agriculture or pastoralism” (Neel and Salzano 1967: 555).

After the Xavante studies, Neel and his colleagues went on to study the Yanomamö, in a series of field trips to both Brazil and Venezuela, from 1967 to 1975. They also collected blood and materials from Native American groups,<sup>22</sup> and from other isolated populations identified and sampled by their network of contacts.<sup>23</sup> Some of this blood yielded the “rogue cells” with heavy chromosome breakage that became the subject of his later research (Neel et al. 1996).

## **Conclusion**

Neel spent much of his professional life collecting blood and other materials from groups that were uniquely victimized by history—those isolated by culture or environment, those devastated by nuclear weapons. Sometimes he wondered if he himself were a technical extension of that victimization: “As we examined the Indians and collected our samples, all this the basis of learned papers that would ultimately contribute to our professional reputations, were we only the latest of the exploiters, now for scientific reasons? Students have on several occasions raised this point when I have lectured on these studies” (Neel 1994: 171). In another context, he wrestled with the possible conflict between the needs of the Yanomamö and the needs of technical knowledge systems.<sup>24</sup>

As early as 1969—perhaps even earlier—both Neel’s prodigious collecting of human bodily materials and Chagnon’s field practices had attracted some negative attention. Chagnon wrote to the head of the New Tribes Mission in October 1969 that he had been accused of “coming like a thief into the country” to study tribal groups without giving all his information to local scientists. He had also heard “from reliable sources that there are now people in the country [Venezuela] who are trying to make it difficult for ‘certain foreign medical people’ to perform their harmful craft and drain the blood out of all of the Indians in the country, taking it, again like thieves, back to the United States.”<sup>25</sup> In 1970 the missionary Jim Bou told Neel and Chagnon that the Guaicas and Makiritare “still talk about the people who came to take their blood in order to do witchcraft on them”!<sup>26</sup> Neel had been attacked much earlier, during his field research in Japan, in ways that echoed the criticisms in Brazil and Venezuela. Some of the atomic bomb survivors studied by the Atomic Bomb Casualty Commission had expressed a sense of violation when autopsy materials were sent to Washington, D.C., to the Armed Forces Institute of Pathology, for storage. The same commission had been criticized by Japanese activists for its failure to provide medical care to the survivors who were the subject of study (Lindee 1994).

In a climate of shifting attitudes among indigenous groups and growing awareness of possible exploitation, Neel, like many others who collected human bodily materials from the field, had to accommodate and adapt to these concerns. After changes in human subjects practices and rules in the 1970s and 1980s, field researchers stopped collecting the huge numbers of blood samples—6,000 to 10,000 in many

studies—that had characterized an earlier era. It was difficult and complicated to acquire large numbers of informed consent agreements, and some indigenous groups had become uncooperative, “commercialized and resistant to being studied.” Appealing to a Costa Rican colleague in 1983 to collect blood, samples from the Cuna, Neel noted, “I think we would have to be prepared to reimburse them for blood samples; we need your suggestion as to how much this should be per individual.”<sup>27</sup>

At the same time, some population geneticists believed that collecting thousands of blood samples was both too expensive and unnecessary, and that samples of twenty-five to forty from any population would be sufficient to assess biological and historical relationships.<sup>28</sup> Neel favored expansive blood collection, but his style of field research was not the model for the proposed (but not realized) Human Genome Diversity Project.

In Neel’s papers in the archives of the American Philosophical Society in Philadelphia, there are some bodily traces of the Xavante: thick, dark clumps of hair folded into a file, wrapped in plastic and numbered.<sup>29</sup> The blood samples are not in Philadelphia but a few hours’ drive away at the Pennsylvania State University laboratories in State College, Pennsylvania. Neel’s sometime collaborator Ken Weiss, a biological anthropologist, stores them there. They could in theory be used in research, embedded as they are in a paper trail of origins and pedigrees, and relevant to explorations of migration and population shift in the Americas. In practice, however, they are not currently being used by researchers. They are stored, frozen, indefinitely.

They are not the only blood samples thus stored. Thousands of others, collected by many investigators in the 1950s, 1960s, and 1970s, are now sitting in laboratory freezers in many institutions, often uncataloged and unused. These materials are physical manifestations of a lost world of biomedical research: they are dramatic evidence of the changing social relations and theoretical perspectives of researchers and human subjects over the last forty years. In a paper promoting what he called “freezer anthropology,” D. Andrew Merriweather, a biologist at the University of Michigan (at Neel’s home institution in Ann Arbor) has proposed that scientists should exploit these blood samples, collected in the heyday of large-scale anthropological fieldwork. He proposed that they might contain DNA that could allow investigators to “look back in time at a unique snapshot in the history of human populations” (Merriweather 1999). Merriweather himself has used such samples to conclude that there were four founding lineages in the peopling of the New World across the Bering Strait, and that the Pacific Islanders migrated out of Southeast Asia through Indonesia, New Guinea, Melanesia, and out into the Pacific. Human blood tells many stories and contains many voices.

Another voice appears in this chapter, quoted in the epigraph. Mikhail Bulgakov was a Russian writer whose novel *The Master and Margarita* James Neel was reading in the airport when he left for the 1968 field trip to the Orinoco. The novel tells several interwoven stories—of the Crucifixion of Christ, seen through the eyes of Pontius Pilate; of the decapitation by tram of the editor of an important literary journal (who then wires his relatives to inform them of his own death); of a large black cat that drinks vodka; and of Satan, a visitor to the modern Soviet Union, who wears a gray beret and passes for



a “foreigner.” The master is a troubled writer; Margarita is his muse and married lover, who sacrifices everything for him. The devil causes much trouble, and in the end all the rumors and hysteria are dispelled by a formal investigation, which concludes that none of the events chronicled in the novel happened at all. They were the products of mass hysteria, of hypnotists, magicians, and tricksters.

Bulgakov was a skilled social critic with a wry sense of humor, attuned to the ways that public outrage can be manipulated to suit the needs of power and sensitive to the ubiquitous and mundane presence of evil. He is perhaps a proper commentator on the Yanomamö controversy—posthumously, of course, and entirely unintentionally. Neel, reading Bulgakov’s novel, thought it “at once a work of colossal arrogance and conceit and ego, but tempered by the integrity that would permit him to polish and polish even believing it would never be published, as well as the superb craftsmanship and in places penetrating insight. Tonight, if we get to it, we start Yanomamö language lessons.”<sup>30</sup> Thus begin the field notes for what has become one of the most famous or infamous field trips in the history of genetics and anthropology.

In May 1968, as the measles epidemic continued in Yanomamö populations, the missionary Robert Shaylor told Neel that his work fighting the measles outbreak earlier in the year had been inspired by God: “We thank God through Jesus Christ our Saviour for supplying all this vaccine and the medicine you helped with while you were here. You are the man God used to meet real needs at a very important time.”<sup>31</sup> The man God used had been dead for seven months by the fall of 2000, when the controversy over his work with the

Yanomamö began, but he had preserved and left behind a vast collection of papers and correspondence. The Amerindians he studied say very little in these texts, except by virtue of their parasites and antibodies. Because of the structure of knowledge-making (in history, science, journalism), some people's voices come through their bodies, their chicken coops, their diseases. Some people's voices appear in scholarly papers like this one. And some people's voices are conjured by the hypnotists, who can "remove from the field of vision things or people who were in fact within that field of vision" (Bulgakov 1967).

In his book *Darkness in El Dorado*, Patrick Tierney conjured up a story of the heart of darkness. In this chapter, I have done some conjuring myself, not of hearts but of urine, blood, chicken dung, and Yanomamö hair, of the material culture that brought "primitive man" into evolutionary time and that resurrected the experiences of the dead. I propose that Neel's romanticism about the Xavante and Yanomamö could be read as a stark technoscientific rejection of twentieth-century technology and culture and a deployment of the tools of quantitative and laboratory analysis to show that "man" was making devastating biological and social choices, and that "culture" (as in the culture of Western civilization) was not particularly good for the gene pool. Neel clearly and passionately believed that modern society had set in motion forces that were damaging to the genetic health of the human species. And he believed that groups living in a primitive or natural state were exemplars of what was genetically natural. Neel's scientific project with Amerindian groups was thus informed by an impassioned critique of progress and a deep skepticism about modern culture: he drew on all the standards of the scientific

method to call into question the evolutionary benefits of science and technology.

## NOTES

1. “I am obviously disappointed that no histoplasma turned up in these soil samples, since I personally spent considerable time scraping around in chicken coops, not to mention a number of miscellaneous places”; Neel to Libero Ajello, chief of the mycology section at the National Communicable Diseases Center (later Centers for Disease Control), 4 December 1968. The reference was to a fungus with worldwide distribution that causes histoplasmosis when inhaled. The infection can lead to chronic progressive lung disease or acute fatal disease, and the disease may mimic tuberculosis. Earlier serological studies of the Yanomamö had suggested that they had been exposed to this fungus. See also Ajello to Neel, 21 December 1966 (“I am sending you 75 plastic bags and spoons for the collection and shipment of the ‘soil’ specimens. The specimens should be collected in bat and bird habitats, where guano accumulated and enriched the soil. The upper layer of the guano-soil mixture should be collected at a depth not to exceed one inch. The bags are numbered V1–V75. They should be filled completely to ensure enough material to carry out the intended tests. A fresh spoon should be used for each bag.” And see Ajello to Neel, 12 January 1967; Neel to Ajello, 6 March 1967. All in Papers of James V. Neel, American Philosophical Society, Philadelphia (hereafter Papers of JVN, APS).

2. The number of specimens collected was linked to the expense of the trip—more specimens cost more, fewer could

not justify the cost of the field research. See, for example, Neel to Moacyr A. Mestriner, Department of Genetics, University of São Paulo, Ribeirão Preto, Brazil, 1 September 1976. Indians: Macushi and Wapishana (Alpha Helix trip), Papers of JVN, APS.

3. Neel to Leo Kaufman, of the Mycology-Parasitology Section of the Communicable Disease Center in Atlanta, 1 December 1966, Papers of JVN, APS.

4. Neel to Wayne Miller of the Servicio Geodésico Interamericano, Caracas, Venezuela, 2 November 1965, Papers of JVN, APS.

5. Neel corresponded with and sent samples to Stanley H. Abadie, assistant professor of medical parasitology at the School of Medicine of Louisiana State University Medical Center, Papers of JVN, APS.

6. Salzano has himself reconstructed this long-term collaboration in a commemorative essay, Salzano (2000).

7. Salzano to Neel, 17 December 1955, Papers of JVN, APS.

8. “Accordingly I think it might be to your advantage if you did not come with the idea of carrying on any specific line of investigation, but rather with the idea of devoting your time to learning the techniques which would be of most benefit to you when you return to Brazil”; Neel to Salzano, 28 December 1955, Papers of JVN, APS. Salzano wrote back that he had statistical training but would be willing to train more before he came, and Neel said he should do so. Neel also said he could not give Salzano an office: “We here are

working under quite crowded conditions, and I am unable to promise you the luxury of an office of your own. Rather it seems quite probable that it will be necessary to share space either with some other visiting Fellow, or one of our graduate students. I hope you will not regard this as too much of an inconvenience”; Neel to Salzano, 10 January 1956, Papers of JVN, APS.

9. Harry M. Miller, Associate Director, Biological and Medical Research, Rockefeller Foundation, to Neel, 3 January 1956, Papers of JVN, APS.

10. Neel to Frankie Davidson, 17 February 1965, Papers of JVN, APS.

11. The massive literature on the cultural and social impact of the development and use of nuclear weapons includes Smith (1965), Sherwin (1977), Easley (1983), Boyer (1985), and many, many others. The use of nuclear weapons has been commonly construed as a fundamental break, a turning point in human history, by many commentators.

12. In this paper, Neel focused on differential fertility studies (a few carried out in various locations around the world) and on sickle-cell anemia in Africa, which provided a compelling story demonstrating the power of the environment (or of environmental disease pressures) to shape human heredity. “For the time being, this is probably the clearest example in all human genetics of the effect of an environmental change on the future evolution of man” (Neel 1958: 66).

13. “This would be a very different sort of undertaking from the traditional fieldwork of the single cultural or physical

anthropologist, or the dash of a geneticist to a remote area to obtain blood samples,” Neel wrote in his autobiography (1994: 121). For a useful comparison on Neel’s research in South America—particularly his involvement with the International Biological Program—and the Human Genome Diversity Project, see Santos (2002).

14. 10 March 1964, Neel to Salzano, Papers of JVN, APS.

15. Neel 1994: 121. Also: “How often have I envied the members of expeditions whose collectibles—plants, insects, artifacts—could be preserved indefinitely permitting a much less driven schedule. Each time—no matter how often we had done it—that we got a shipment out in good shape, it was an occasion for a small celebration, albeit, since I ran a dry camp, nonalcoholic. The alcoholic celebration came later, when we returned to Ann Arbor, and learned the specimens had not only gone out in good shape but arrived in good shape. In all, we only lost one shipment” (Neel 1994: 147).

16. All quotations are from Neel’s field diary for the 1968 trip, which is in his papers at APS.

17. Neel to Gene H. Stollerman at the Northwestern University Medical School, 24 July 1963. Neel told Pentti Kokko at the CDC three years later, in September 1966, almost the same thing, that antibody titers were the only way to get medical histories in these populations: “Since there are no trained medical observers stationed in these areas, and since it is impossible to get meaningful medical histories, we have come to rely heavily on antibody titers in defining disease pressures.” Kokko was chief of the laboratory branch

of the Communicable Disease Center, Atlanta. Neel was seeking his collaboration on their very large samples; Neel to Kokko, 23 September 1966, Papers of JVN, APS.

18. Letter, JVN to Macon C. Hare, missionary at Manaus, 20 September 1966, Papers of JVN, APS.

19. “However, they also tell us to what extent the diseases of civilization have already reached these Indians, with a possible modification of the primordial disease and reproductive patterns. Since we are doing our best to get good morbidity-mortality data, from which we are attempting to build population models for primitive man, it is of the utmost importance for us to have the best possible insight into whether these mortality-morbidity patterns might have been altered already by their fleeting contacts with our culture”; letter, 30 August 1967, to Joseph Schubert, Bacterial Serology Unit, CDC, Atlanta, asking if he could spend a day at CDC that fall discussing his work among the Yanomamö, Papers of JVN, APS.

20. His correspondence with Salzano and others in this period contains many references to his anxieties about getting the papers out and getting all the details straight—the results of blood tests, the identities of individuals, the credits; Papers of JVN, APS.

21. Neel went on to suggest reviewers—Jim Crow, because he expected Crow would be critical of some aspects of the study, and René Dubos, because he viewed Dubos as “the most knowledgeable person of whom I know in this ‘total population’ concept”; Neel to Sutton, 9 November 1966, Papers of JVN, APS.

22. For example, in May 1966, William S. Politzer of the Department of Anatomy of the School of Medicine, University of North Carolina at Chapel Hill, sent James Neel 264 blood samples collected from Seminole and Creek Indians at Wewoka, Oklahoma, including that of three-year-old Bill Davis Jr., and eighty-five-year-old Ida Factor. Each person was listed with race (Seminole, ½ or Seminole, Creek or Seminole Caddo, Choctaw, Chick. Navajo, Chickasaw). “The great majority of the people appear to be full blooded Indians, or close to it”; Politzer to Neel, 28 May 1966, Papers of JVN, APS.

23. “This laboratory would indeed be interested in typing blood specimens from representatives of any of the *pure*-blooded Indian tribes left in Paraguay. While there may be no such thing as a completely pure tribe, we are reluctant to spend very much effort on tribes with the admixture estimated at greater than 5 percent, because then it becomes difficult to make many of the kinds of inferences in which we are interested.” Neel to Ricardo Moreno, 22 February 1974, Papers of JVN, APS. Moreno was a professor of Genetics in the School of Medicine at National University, Asunción.

24. “These people pose a special challenge to our conscience,” he told a Pan American Health Organization group in 1974, “and as symbols of past violations of the brotherhood of man call for a particular effort on our part. Their treatment should of course be dictated by humane considerations, but the investigator in me can’t resist commenting on the unique research resource they constitute—our last chance to revisit ourselves evolving”; Neel, “Control of Disease in Amerindians in Cultural Transition,” p. 7, presented at the thirteenth annual meeting of



the Pan American Health Organization Advisory Committee on Medical Research, 24 June 1974, Papers of JVN, APS.

25. Chagnon to Paul Dye, Field Director, New Tribes Mission, Puerto Ayacucho, Venezuela. 1 October 1969, Papers of JVN, APS.

26. Jim Bou, New Tribes Mission, to Neel and Chagnon, 27 April 1970, Papers of JVN, APS.

27. Neel to Ramiro Barrantes, Escuela de Biología, Universidad de Costa Rica, San José, Costa Rica, 19 December 1983: “We would like to raise at this time the possibility that you might be able to go to Panama, and obtain samples from the Cuna as well as repeat samples from the Bokota (from a different area). I realize that this may be very difficult and perhaps impossible given the various political problems of Central America at the present time. If, on the other hand, you thought it was a feasible undertaking, we would try to supply the funding from here in Ann Arbor. We are thinking in terms of perhaps 100 samples from the Cuna and another 100 from the Bokota”; Papers of JVN, APS.

28. This was the perspective of Luca Cavalli-Sforza, and it was accepted by many population geneticists.

29. In one file are records of physical examinations, with photographs and descriptions of the intelligence and personalities of the people photographed. There is one particularly striking photo, of a young Xavante man wearing a string necklace supporting a paper card on which the number “15” has been written. His hair is short, curly, and black. He has a dark line of stain, a decorative marking,

straight across his cheeks and mouth. He stands before a stone surface broken by fractures. Later, after I had encountered it in the archives, I realized that this same photograph appeared in one of the 1967 papers, with the caption “Young Xavante male (0121015) short (154.3 cm), curly-haired, mentally dull with dental abnormalities as described in text. Digits appear stubby. Right parotid enlarged. No specific diagnosis was reached.” In the text, the authors said, “One, a 19-year-old, was the most unusual appearing Xavante. He was short, stocky, had curly hair (the only such individual seen), a left Simian crease, and quaint, dull, smiling, sleepy facies” (Weinstein et al. 1967).

30. Yanomamö, 1968, Field Notes, Papers of JVN, APS. The book is a surrealist critique of Stalinist Russia, and the sort of novel that inspires entire courses. Bulgakov’s work was banned in Russia, and he spent his later years as a stagehand in the Russian theater. But he kept writing.

31. Missionary Bob Shaylor to James V. Neel, 20 May 1968, in a letter describing the continuing impact of the measles epidemic, Papers of JVN, APS.

## REFERENCES

Boyer, P. 1985. *By the Bomb’s Early Light: American Thought and Culture at the Dawn the Atomic Age*. Pantheon, New York.

Bulgakov, M. 1967. *The Master and Margarita*. Translated from the Russian by Mirra Ginsburg. Grove Press, New York.

Easlea, B. 1983. *Fathering the Unthinkable: Masculinity, Scientists and the Nuclear Arms Race*. Pluto Press, London.

Lindee, M. S. 1994. *Suffering Made Real: American Science and the Survivors at Hiroshima*. University of Chicago Press, Chicago.

Merriweather, D. A. 1999. Freezer anthropology: New uses for old blood. *Philosophical Transactions of the Royal Society*, London, Series B 354: 121–129.

Neel, J. V. 1958. The study of natural selection in primitive and civilized human populations. *Human Biology* 30: 43–72.

———. 1994. *Physician to the Gene Pool*. Wiley, New York.

Neel, J. V., A. H. Andrade, G. E. Brown, W. E. Eveland, J. Goobar, W. A. Sodeman, G. H. Stollerman, E. D. Weinstein, and A. H. Wheeler. 1968b. Further studies on the Xavante Indians. IX. Immunologic status with respect to various diseases and organisms. *American Journal of Tropical Medicine and Hygiene* 17: 486–498.

Neel, J. V., E. O. Major, A. A. Awa, T. Glover, A. Burgess, R. Traub, B. Curfman, and C. Satoh. 1996. Hypothesis: “Rogue cell”-type chromosomal damage in lymphocytes is associated with infection with the JC human polyoma virus and has implications for oncogenesis. *Proceedings of the National Academy of Sciences (USA)* 93: 2690–2695.

Neel, J. V., W. M. Mikkelsen, D. L. Rucknagel, E. D. Weinstein, R. A. Goyer, and S. H. Abadie. 1968a. Further studies on the Xavante Indians. VIII. Some observations on

blood, urine, and stool specimens. *American Journal of Tropical Medicine and Hygiene* 17: 474–485.

Neel, J. V., and F. M. Salzano. 1967. Further studies on the Xavante Indians. X. Some hypotheses-generalizations resulting from these studies. *American Journal of Human Genetics* 19: 554–574.

Neel, J. V., F. M. Salzano, P. C. Junqueira, F. Keiter, and D. Maybury-Lewis. 1964. Studies on the Xavante Indians of the Brazilian Mato Grosso. *American Journal of Human Genetics* 16: 52–140.

Oreskes, N. 1996. Objectivity or heroism? On the invisibility of women in science. *Osiris* 11: 87–113.

Rossiter, M. 1995. *Women Scientists in America: Before Affirmative Action, 1940–1972*. Johns Hopkins University Press, Baltimore.

Salzano, F. M. 1957. The blood groups of South American Indians. *American Journal of Physical Anthropology* 15: 555–579.

———. 2000. James V. Neel and Latin America—or how scientific collaboration should be conducted. *Genetics and Molecular Biology* 23: 557–561.

Santos, R. V. 2003. Indigenous peoples, the atomic bomb and human genome research: Reflections on late twentieth century human biology in/from Amazonia (1960/2000). In *Genetic Nature/Culture*, edited by A. Goodman, D. Heath,

and M. S. Lindee. University of California Press, Los Angeles (in press).

Sherwin, M. J. 1977. *A World Destroyed: The Atomic Bomb and the Grand Alliance*. Vintage, New York.

Smith, A. K. 1965. *A Peril and a Hope: The Scientists' Movement in America, 1945–47*. MIT Press, Cambridge.

Weinstein, E. D., J. V. Neel, and F. M. Salzano. 1967. Further studies on the Xavante Indians. VI. The physical status of the Xavantes of Simões Lopes. *American Journal of Human Genetics* 19: 532–542.