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SPECIAL PIECES

An annotated history of kuru

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This version of the history of kuru research departs from many earlier accounts, including my own, which often cling to a particular sequence of research and publication. The story usually begins in the early 1950s when several patrol officers and two anthropologists working in New Guinea reported the disease.[1] It then unfolds with the first medical assessment in 1957, the anthropological investigations that began in 1961, and the mix of anthropological and medical research that unraveled the case in the 1960s. The narrative reaches a high point with the report of two Nobel Prizes in Medicine: one to Carleton Gajdusek in 1976, for transmitting the infection to laboratory primates, and the other to Stanley Prusiner in 1997, for identifying the nature of the infectious agent. It soon becomes clear that no one born since 1960 (when cannibalism ended) had come down with the disease, confirming the hypothesis that consuming the dead was the mode of transmission. The last case of kuru in 2009 then brings an end to the epidemic that once threatened the survival of the Fore people. The lessons learned during the epidemic are directed now at cultural and medical issues associated with diagnosis and care for patients with all forms of prion disease, and for the promotion of public and animal health worldwide.

[1] The historians Hank Nelson and Warwick Anderson are exceptions. They both provide complex cultural histories of kuru research, which have greatly enhanced my own understanding. New Guinea is now called Papua New Guinea, following independence in 1975.

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This story of kuru – told from the perspective of the history of medicine – tends to underplay the fact that medicine did not bring an end to the epidemic. Instead, the disease was arrested by those who told the Fore to abandon their mortuary practice of consuming deceased relatives. For the government it was illegal; the missionaries deemed it sinful. This was an inadvertent benefit of the colonial encounter, since the relationship between cannibalism and kuru was not known at the time. The medical version gives little attention to the Fore experience of the epidemic and their own efforts to halt the death toll that they understood imperiled their future. This story is told elsewhere (Lindenbaum 2013).

In this essay, I tell a different story about kuru research.[2] Part one looks at the various ways in which anthropologists and medical investigators studied the disease. Negotiations within and across disciplinary borders were marked by a mix of skepticism, rejection, and acceptance of theories, reflecting the intellectual histories and practices of both anthropology and medicine. Their ideas about what counted as 'truth' were complicated by different research methods and practices, and different styles of recording and publishing data. This account is consistent with Charles Rosenberg's (1992, 5) observation that science and medicine should be seen as generation- and place-specific practices, not accumulations of knowledge advancing ineluctably, if somewhat erratically, toward a deeper understanding of nature.

My focus here is on the network of anthropological and medical investigators and others who worked and lived with the Fore people in Papua New Guinea for significant periods of time. Much of the material comes from the journals, diaries, letters, and publications of kuru investigators, administrators, historians, and anthropologists, and is focused on the 1950s and 1960s, the key years of kuru inquiry. These documents reveal a community of investigators who were in touch with one another, their social and professional lives intertwined as they exchanged ideas and information. The second part of the essay looks at the research practices that shaped the scientific personae of both anthropologists and medical investigators, as illustrated in their signature publications: the ethnography and the scientific article. The essay ends with some observations about use of visual technologies by kuru investigators, anthropologists, and the Fore that illustrate different ways of seeing and knowing the world.

[2] My thanks to Kate Crehan, Gavan Daws, Erin Martineau, and Jane Schneider for their insightful comments on an earlier draft.

History retold

Early reports from patrol officers and scientists

During the early 1950s, Australian patrol officers mentioned kuru in their official reports to the Department of Native Affairs.[3] Their reports combined ethnographic description, clinical observation, and an account of their administrative activities. Arthur Carey was the first to draw attention to what appeared to be a new disease. His 1951 report noted that the Fore pattern of early marriage for girls had to some extent been determined by women's vulnerability to kuru in their childbearing years. Using the term 'kuru' for a disease that the Fore said was mainly killing women, Carey described the clinical symptoms and called for a medical diagnosis that could provide immediate treatment (Nelson 1996, 188).

Nelson also notes that patrol officer John McArthur described the disease in 1953,[4] followed by William Brown in 1954. McArthur's report confirmed that kuru sorcery was practiced, stated that he had sent some sorcery offenders to Kainantu (the center of government administration for the Eastern Highlands in colonial New Guinea), and surmised that kuru must surely be psychological (Mathews 1976, 84). In 1955, John Colman sent a typical case of the disease to Kainantu for medical observation, eliciting Vincent Zigas's provisional diagnosis of acute hysteria in an otherwise healthy woman, an interpretation that preceded the first wave of feminism that changed our views about the ready association of extreme emotional states with illnesses in women. Frank Earl, an emergency medical practitioner who accompanied Colman, also described the disease in 1955, suggesting that kuru might be a form of encephalitis (Radford 2012, 241).

In 1957, Charles Julius, an anthropologist working for the government, sent his survey of South Fore sorcery beliefs and practices to the Department of Public Health, which included the significant observation that in most areas women and uninitiated men had both the right and duty to eat dead relatives. Listing six different types of sorcery, he noted the Fore 'obsession' with kuru, but made no connection between the gendered and generational patterns of cannibalism and the epidemiology of the disease. Keeping in mind the public

[3] For my discussion of these reports, I have relied on the historical accounts by Nelson (1996), Anderson (2008), and Mathews (1971), but these reports are also available here: http://libraries.ucsd.edu/collections/about/collections-of-distinction/melanesian-studies/papua-new-guinea-patrol-reports/papua-and-new-guinea-patrol-reports-19221955.html

[4] McArthur's 1953 observations were included in his 1965 Okapa Report.

health audience for his report, Julius observed that the Fore considered kuru to be beyond the scope of European treatment.[5]

In 1958 Ronald Berndt, an anthropologist who worked from 1951 to 1953 in the North Fore with his wife Catherine (also an anthropologist), observed that he had earlier taken note of kuru symptoms, which he considered to be 'psychosomatic', based on his observation that physical symptoms appeared to be directly associated with the threat and fear of what was believed to be a particularly malignant form of sorcery (Berndt 1958). His assessment reflected a medical view current at the time that death often resulted in persons who believed themselves to be bewitched In a personal note to Berndt, Gajdusek, a young American virologist who had just begun to study kuru, dismissed this diagnosis:

> We have recently had the assistance and advice of Dr. Sinclair, Director of Psychiatry from the Royal Melbourne Hospital, and he agrees with our current opinion that fatal kuru ... cannot by any stretch of the imagination be identified with hysteria, psychoses or any known psychologically induced illnesses. ... [T]he evidence for direct nervous system damage is far too great in the strabismus, and pictures ... of advanced neurological disease shown by the advanced cases. (cited in Berndt 1958, 22–23)

Publications, reputations, and disease definition

In 1957 Gajdusek and Zigas published two articles in which they referred to kuru as a disease syndrome new to Western medicine. The article in the *New England Journal of Medicine* came out before its companion in the *Medical Journal of Australia*, a priority that irritated the Australian scientific community. In correspondence with Joseph Smadel, his sponsor at The National Institutes of Health in Bethesda, Maryland, Gajdusek said he had submitted the article to the Australian journal 'for political reasons', but was counting on the American article appearing first and was ready to 'urge' and 'beg' for that to happen (Nelson 1996, 194n78). Professional and national territoriality had begun to affect the course of scientific research.

The question of who should conduct such studies was already complicated by nationalities and personalities. Gajdusek had been sending material to Melbourne for testing, and writing to the Public Health Department in Port Moresby for medications, reference books, equipment for use in postmortem examinations, and buildings where he could carry out autopsies. He had ended his ties with the Walter and Eliza Hall Institute in Melbourne, and

[5] Julius's 1957 report is reprinted in Farquhar and Gajdusek (1982, 281–88).

had no position with the Department of Health, but was behaving as if he had standing with both institutions. The incoming director of the department radioed a message saying that Gajdusek should withdraw his requests. The assistant administrator followed with a letter accusing him of falsely claiming to represent the Hall Institute, as well as participating in a research project allocated to someone else without approval, which the administrator considered 'grossly unethical'. Gajdusek did not seem greatly disturbed, although his journal entries record a sense of frustration. In public, he said there was enough for a dozen scientists to explore, and he continued working, but sent more of his material for laboratory analysis to America, where he had now secured a position as a visiting scientist at the National Institutes of Health.

The 1957 medical publications had emphasized the familial aggregation and high frequency of kuru, its localization to the Fore and adjacent people with whom they intermarried, and the apparent vulnerability of children of both sexes and adult women. This suggested a possible genetic etiology, or at least a hereditary predisposition closely related to the ill-defined group of heredofamilial[6] neurologic degenerative disorders of the central nervous system. Clinically, it was said, kuru presented as a progressive neurological disease, an attribution of animism to disease states that Latour might recognize (see Latour 2014). With so little known about kuru at the time, these clinical observations were not medically contested; the epidemiological information established a basis for extensive data collection in later years.

In 1958, however, Berndt had addressed what he identified as the limitation of both the clinical and genetic hypotheses, as well as the difference between medical and social research. Recalling his earlier view that the manifestations of kuru sorcery were 'psychosomatic', and Gajdusek's response that the evidence pointed toward a group of chronic progressive heredofamilial degenerations of the central nervous system, he asserted that 'any attempt to speak of "cause" in this context must bring us to the core of a recurrent controversy':

> The problem of 'how much' responsibility should be given to organic (or 'intrinsic') as against environmental (or 'extrinsic') factors in any given case is not easily resolved, particularly when the social setting of this kind of enquiry itself helps to structure both question and answer. The institutionalized division between (e.g.) medical research and social or psychological research, apart from the subdivisions within these fields, means that workers within them are influenced by training, interest, and status considerations to seek out one sort of 'explanation' rather than another. In spite of an increasing degree of communication and even collaboration between

[6] Occurring in more than one member of a family.

> them, and the demand that a one-sided approach should be resisted as far as possible (cf. the insistence in some areas that psychiatrists should have adequate medical qualification), for most practical purposes and in reference to most of the research and teaching personnel concerned, the rapprochement between them is still potential rather than actual.
>
> I do not want to perpetuate this controversy. ... In any case, I do not think we are in a position to speak dogmatically of 'causes' or origins here. On the one hand, there is visible evidence of physical deterioration in the victims of kuru, together with positive post-mortem findings of extensive neurological damage. ... [T]he medical findings in this situation make it clear that more than functional disturbance is involved, that organic damage of a severe kind is an empirical 'fact' and not an assumption which remains to be proven. On the other hand, the social and cultural evidence, intangible as it is, is no less real. ... My contention is that the medical evidence is not sufficient to rule out psycho-social factors in contributing to the presence of kuru as a disease. It seems to me that here we have a situation, a problem of mutual interest, in which medical and social scientists could very well collaborate, in order to arrive at a satisfactory 'explanation' which would take fully into account their respective points of view. (Berndt 1958, 22–27)

Because kuru seemed to run in families and was localized to a small interrelated population, the role of genetics attracted the attention of Henry Bennett, Frank Rhodes, and Norrie Robson (1958), who proposed that kuru was a hereditary disorder, determined by a single autosomal gene, recessive in males and dominant in females. Given the somber public health implications of the hypothesis, the medical findings were monitored closely. Since the mid-1950s, men in the Highlands had been encouraged to join the Highlands Labour Scheme, signing two-year contracts to work in regions where labour was scarce. The administration considered, but then rejected, a plan to quarantine Fore men to prevent the spread of the notional kuru gene.

Ethnographic complications

Early investigations were hampered by the lack of information about Fore social life, especially kinship. In 1961, Robert Glasse and I were sent to collect this data, supported by a grant from the Department of Genetics at Adelaide University. We chose to live in the South Fore community at Wanitabe, which had the highest reported incidence of the disease. Our focus on Fore kinship led us to question the genetic hypothesis. We had been charged to collect 'pedigrees', not 'genealogies' – a concept that acknowledges that notions of relatedness are subject to historical and cultural construction. Our genealogies showed, however, that many of the supposedly closely related kuru victims were not closely related

biologically, but were kin in what we would call a social sense. In addition, we gathered evidence that the Fore considered kuru to be of recent origin. On a ten-day patrol through Fore lands from south to north, and into the borderlands of the Keiagana, we recorded local accounts of kuru 'first sightings'. Together, these depicted an epidemic that had spread within recent memory, emerging on the North Fore border around the turn of the century, then spreading along a traceable route until it reached the southern border in the early 1930s (see map in Lindenbaum 2013, 18).

This history was a challenge to the Mendelian model of genetics, as John Mathews, a physician who joined the kuru project full time in 1966 observed:

> The purely genetic model assumed that the disease must have been of remote evolutionary origin and that it ought to have been in epidemiological equilibrium. It was soon apparent that kuru was too common and too fatal to be a purely genetic disorder unless the hypothetical kuru gene was maintained at high frequency by a mechanism of balanced polymorphism. There was no evidence to support the latter suggestion. (Mathews 1971, 13–14)

A fatal genetic disorder could thus not reach the incidence that kuru then had without soon killing off the host population, unless the hypothetical gene for kuru conferred a selective survival advantage in some way.[7]

Writing in his personal journal while in the South Fore Rest House on 20 December 1961, Gajdusek was already mulling over our findings, recalling his recent visit to our Wanitabe fieldhouse, during which we discussed our research. '[Robert and Shirley Glasse (later Shirley Lindenbaum)] have been doing intensive, careful work', he writes, but then introduces a note of skepticism:

> The Glasses are very impressed about the failure to elicit any reported cases of kuru earlier than forty years ago. That too, was my experience: however, I am not as convinced as they are, by any means, that this indicates that there were none. The accuracy and availability of recall earlier than four to five decades ago is hard to evaluate. I doubt that informants can supply much information of events prior to their fifteenth to twentieth year and informants over sixty are rarely dependable! (Gajdusek 1961–1962, 1:30)

[7] Geneticists have introduced the concept of balanced polymorphism to indicate that along with an unfavorable disease state, some characteristic favorable to survival is simultaneously inherited.

On the same day, he records the following account of our journey to the Gimi region, where we had observed Suave, a Gimi therapist treating kuru patients:

> They have been following Suari [*sic*] from Ivaki, who is travelling about the area, curing kuru by shooting small arrows from a small bow into the extremities, causing bleeding and later feeding pig meat in a very involved ritual. They have some dramatic photos of him in action, and a few patients claim dramatic recoveries—but not cures by any means. In fact, they tell me of some seventy cases he has treated, sixty-five are now dead. (Gajdusek 1961–1962, 1:130)

In a later passage Gajdusek writes that he has just sent a corrected manuscript of a Pacific Science Congress paper on kuru given recently in Honolulu, and communicates his understanding that the conference proceedings would be published in *Eugenics Quarterly*. In contrast to the drive in medicine to publish quickly, and first, anthropologists may take years to publish their data. This ethnographic account of Suave, the Gimi therapist, would be published fifteen years later (Lindenbaum 1979).

Searching for a biological solution

On 6 February 1962, now in the South Fore village of Agakamatasa, Gajdusek (1961–1962, 1:292) records the visit of Michael Alpers, a physician who had just arrived to begin an epidemiological study of kuru: 'Before he left I gave him the full list of so-called "recovered" kurus of the entire region since 1 January, 1957, which will be his major focus of interest during his two years of field work and sojourn here'. The same journal entry includes a note that reveals again the race in medical science to publish findings:

> I was up before sunrise typing up the Slow Virus Infection abstract—the second of two for the Society for Pediatric Research which must be in by February 24th and which I sent out for Marion to type in final form. ... I have submitted one paper on Kuru in Childhood, and another on Slow Virus Infections in which I shall develop the possibility of kuru-scrapie, slow virus and temperate virus analogies and our current approach to the problem. (Gajdusek 1961–1962, 1:293)

Gajdusek had long imagined a life of scientific distinction. Alpers's obituary for Gajdusek indicates that from the age of ten, Gajdusek was determined to follow a career in medical research. In his family house in Yonkers, New York, he had written the names of famous microbiologists from Paul de Kruif's *Microbe Hunters* on the steps leading to the attic. When Alpers visited, Gajdusek enjoined him to 'climb the stairs to stardom' (Alpers 2010, 54).

By 27 February 1962, now back at the Purosa Rest House, Gajdusek (1961–1962, 1:339) notes that Robert and I had joined him: 'We had had a conversation for about two hours before they again rushed off to make a dinner appointment at Okapa with the Alpers'. Still mulling over our account of the history of the epidemic, he leans toward a biological solution:

> The Glasses are convinced that there was no kuru here before 1925, and this puts grave doubt on genetic origin. However, an environmental change, or an ethnic-social change in environmental exposure could still be coupled with genetic susceptibility or lability, and account for the pattern arising anew over a broad region and rather recently. I must admit that our own experience has never turned up a case over 35 or 40 years ago which stood cross-checking, but I have always discounted the negative reports in the prehistory of this preliterate people. However, their concern is obvious, and it only makes our search for an inborn error of metabolism or a temperate virus all the more pressing. (Gajdusek 1961–1962, 1:340)

Later on the same day, he returns again to the question of the validity of the assertion that kuru was new: 'Genetic origin as a fully genetically mediated trait would be very unlikely if the Glasses are right. ... However, our aim to continue to pursue infectious and intermediary biochemical leads is fully in harmony with all possibilities and, thus, our approach is unaltered' (Gajdusek 1961–1962, 1:341). Alpers, however, wanted him to reconsider. Writing nine months later from his new fieldhouse in the South Fore village of Waisa, he urged Gajdusek 'to take seriously the Glasses' argument that kuru was a recent, emerging disease' (Anderson 2008, 172).

'All this holding of meetings'

In 1962, Robert and I took a short break from fieldwork, staying for a time with my parents in Melbourne. Medical historian Warwick Anderson's (2008, 177) interview with John Mathews indicates that I gave John a copy of our genealogies and that we discussed our work on cannibalism. Robert and I then attended a kuru conference at Adelaide University, along with kuru investigators and local laboratory scientists, where we spoke informally about our work on cannibalism and presented papers on the recent spread of kuru among the Fore and the social impact of the disease. Robert provided this information in papers he wrote for our annual report to John Gunther, the Director of Public Health (Glasse 1962a, 1962b). Robert's 1963 report provided more detailed information about the association of cannibalism and kuru throughout the region (Glasse 1963).

We returned to New Guinea to find the Fore holding public meetings to denounce kuru sorcery. In one place or another, from November 1962 to mid-March 1963, male orators condemned the activity of kuru sorcerers, and suggested that the sorcerers were killing so many women that the Fore might not survive. On 22 December, Mert Brightwell, the assistant district officer at Okapa, sent us a letter he had just received from Mildred Cervinka, a missionary at Oriesa in the South Fore:

> The facts as I cd gather them ... [are that] the couple working on kuru in Wanitabe had called out for the LL's and TT's [Luluais and Tultuls, government-appointed leaders] and said that 'the kiap' [Brightwell] said they were to kill and eat all the women with kuru; then men and boys only wd be taken in an airplane by the kiap to a new locality where they cd have a fresh start without kuru, only that they wd have to swear they wd not start working kuru nor poison this new locality. The women and female children wd be left behind here.
>
> The more I hear of this, the more ridiculous it sounds, but you cannot convince the natives that it is a lie, nor can you convince them to take off to Okapa and find out the real facts.
>
> I am afraid that all this holding of meetings and getting themselves worked up over this will develop into some serious situations and uprisings if they are not soon straightened out.
>
> Any help you can give to make the truth known to them and get them to stop this terrible lie abt 'eating the women with kuru' etc. will most certainly be appreciated. Instead of thinking now of Christmas and the Christmas message, they are totally occupied with this talk. ... It is not that I desire to interfere with native affairs, but to let you know that the affair is really getting serious.

Administrators had memories of similar disturbances. The meetings, directed at demographic and economic conditions unsettling the region, resembled gatherings held in the past when the Fore had attempted to redefine their troubled relationships with one another and with the colonial administration.

The cannibalism hypothesis

In addition to learning that kuru was a new disease, our 1961 genealogical survey had revealed the possible role of cannibalism in the transmission of kuru, a hypothesis we continued to investigate. The Fore, we were told, consumed the first kuru victims, whose names they remembered. All body parts were eaten, except the gall bladder, which was considered too bitter. Significantly, not all Fore were cannibals. Some elderly men rarely ate

human flesh, and small children residing with their mothers ate what their mothers gave them. Youths, who were initiated around the age of ten, moved to the men's house, where they began to observe the cultural practices and dietary taboos that defined masculinity. Consuming the dead was considered appropriate for adult women but not men, who feared the pollution and physical depletion associated with eating a corpse. The epidemiological information provided by Gajdusek and Zigas in 1957 – that kuru occurred among women, children of both sexes, and a few elderly men – seemed to match perfectly the Fore rules for human consumption.[8]

In April 1963 we sent our annual fieldwork report to John Gunther, the director of public health and the source of our grant money for the second year of research (Glasse and Glasse 1963). We wrote that we were continuing to gather information on a set of topics begun earlier: the origin and spread of cannibalism and kuru; women's life and child-rearing practices; kinship, myths, folklore, and history; and concepts of disease treatment – a range of subjects that reflected anthropological research at that time. We noted that extensive data had been collected on the possible association of cannibal practices and kuru, and that the practices varied considerably in the kuru region and in adjacent areas. An attempt would be made to relate these findings to variations in kuru prevalence. The borders of kuru were of particular interest.

One month later, a group of Australian scientists with an interest in kuru visited us at Wanitabe. The visitors included Alpers, who had begun his own research, and MacFarlane Burnet, a recipient of a Nobel Prize in 1960 for his contribution to immunology. As director of the Walter and Eliza Hall Institute in Melbourne, Burnet was familiar with kuru research, and we discussed with him our cannibalism hypothesis. He later observed that 'some, like the present writer, initially found the suggestion incredible, but must confess now to at least an open mind on the matter' (Burnet 1972, 587). Unknown to us, Burnet had been losing faith in the genetic theory and was secretly receptive to the idea that cannibalism might be implicated (Anderson 2008, 168).

Also in 1963, Richard Sorenson, working at that time as a photographer with Gajdusek, recorded his thoughts about anthropology and cannibalism. His journal entry, while mistakenly attributing a paper's authorship to me rather than Robert, indicates the degree to which kuru research was discussed locally, one way or another:

[8] Young boys who participated in mortuary consumption as children, and who died later would, over time, change the ratio of male-female mortality.

> I ran into another of Carleton's old acquaintances, Blu Russell [a road-building contractor], at the hotel [in Goroka]. He tells me that kuru is caused by eating spiders and frogs and the reason men don't get it is that they leave these delicacies for the women and children. Everyone seems to have his own theory. Shirley Glass [*sic*] has just written a paper implying strongly that it is transmitted by cannibalism—particularly by eating of the brain of the deceased relative. (Sorenson 1963–1964, 2)

Sorenson would return later to the South Fore as an anthropologist. In *The Edge of the Forest: Land, Childhood and Change in a New Guinea Protoagricultural Society* (1976), Sorenson uses a method of ethnographic research perhaps influenced by his close association with Gajdusek and the scientific studies at the National Institutes of Health. Combining aerial photography, blood typing, and the historical reconstruction of land use, he describes Fore society as 'protoagricultural', occupying an evolutionary phase between hunting and gathering and agricultural settlement patterns. A section on kuru provides a summary of the state of medical and anthropological research in 1976; a summary of the experimental transmission of kuru to chimpanzees, confirming an infectious etiology; acceptance of the cannibalism hypothesis; and the epidemiological evidence that the disease had been slowly declining, due perhaps to the cessation of cannibalism (Sorenson 1976, 36).

Early in 1963, we had a conversation about kuru with Richard Hornabrook, a New Zealand neurologist who had just arrived in Okapa to begin work on the clinical course of the disease. He posed the key epidemiological question: 'What do adult women and children of both sexes do that adult men don't do?' We said, 'They ate the dead'. In our many encounters with medical investigators we continued to present our views about cannibalism, but were receiving little positive feedback.

John Mathews was the first to give public support to our findings. Mathews began to study kuru in 1963 – while in his residency year at the Royal Melbourne Hospital – and arrived in New Guinea in 1964. His 1965 *Lancet* paper, based on an analysis of our genealogies, confirmed that kuru was of limited time depth, which he considered hard to accommodate with the genetic hypothesis but consistent with the cannibalism theory. (Gajdusek's 1965 diary takes note of this publication.) Mathews (2008) later observes that he continued to examine the epidemiological data for evidence that was consistent with the view of kuru as a slowly spreading infectious disease.

The transmissibility of kuru, however, was still considered a major medical problem. Many years earlier, William Hadlow (1959), a veterinary pathologist, had discerned the connection between the neurological features of kuru and scrapie, a disease in sheep. Moreover, he said, scrapie was transmissible. This observation had inspired Gajdusek and his colleagues to test the possibility that kuru could be transmitted to chimpanzees, a project that came to fruition

in 1966 (Gajdusek, Gibbs, and Alpers 1966), and was the basis for Gajdusek's Nobel award. Kuru, like scrapie, was now considered to be an infectious disease caused by a 'slow virus' of very long incubation, a term introduced by Gajdusek.

Until this time an appropriate medical model for assimilating the idea of cannibalism was not available, but some medical investigators were now ready to turn to anthropology. Robert Glasse (1967) presented the case for cannibalism as the mode of transmission to the Division of Anthropology at the New York Academy of Sciences, and in the following year, Robert and I joined John Mathews, the lead author, in a publication that would reach a wider medical audience (Mathews, Glasse, and Lindenbaum 1968). Our paper showed that the cannibalism theory was supported by a wealth of epidemiological and ethnographic data consistent with the stories about named individuals who had taken part in mortuary feasts and who had themselves died from kuru, and it predicted what was likely to happen if the cannibalism hypothesis was true. In 1968, Alpers also published epidemiological data that pointed to cannibalism as the mode of transmission.

A history of skepticism

Some still doubted the cannibalism hypothesis. Perhaps the most surprising resistance came from Gajdusek (1971), who proposed an alternative route of transmission. The mechanism, he argued, lay in the contamination of highly infectious tissues through cuts and scrapes on the hands of mourners, as well as from brain tissue purposely rubbed on the body during mortuary ceremonies, a practice the Fore have denied. These denials were collected by the anthropologist Jerome Whitfield, with the assistance of a young generation of Fore research assistants, who interviewed elderly Fore men and women who had consumed deceased kin (Whitfield et al. 2008). While the handling of infectious body parts could perhaps be a means of occasional self-inoculation, Gajdusek had begun to overstate the case.

Gajdusek's resistance to the idea of oral ingestion as the route of transmission has elicited speculation from many who knew him. The sensitivity of the topic may explain some reluctance to discuss the role of cannibalism, given the image of the cannibal as an icon of primitivism. A more likely explanation is that Gajdusek and his colleague Ralph Garruto, a biological anthropologist, are said to have found it difficult to imagine how social and cultural studies could be rendered commensurate with contemporary biological research and pathological findings. Their collaborative work on infectious disease is said to have 'made only small gestures toward incorporating sociological analysis' (Anderson 2010, 265). It is also the case that Gajdusek's data about cannibalism were inaccurate. The former assistant

administrator of the territories, now Sir John Gunther, recounts a meeting with Jack Baker, Gajdusek's partner in the early kuru patrols. Baker tells Gunther that he and Carleton 'kicked themselves for not following up on cannibalism better than they did', and added that 'the men gave their oath that the brain was not eaten' (personal communication, 22 May 1979). Women, the main consumers, were not consulted.

Gajdusek did not doubt that the Fore were cannibals, although skepticism about the practice has a long history in Western thought (Avramescu 2009). The notion that cannibalism as a social custom did not exist, however, was finding a receptive anthropological audience (Arens 1993). Although discredited today, the denial of cannibalism was kept alive during the 1980s and 1990s by a generational shift in the human sciences, glossed as postmodernism, which studied metaphor and representation, providing new life for the idea that cannibalism was nothing more than a colonizing trope and stratagem, a calumny used by colonizers to justify their predatory behavior. Postcolonial studies proposed that the figure of the cannibal was created to support the cultural cannibalism of colonialism through the projection of Western appetites onto the cultures they consumed (Kilgour 1990). A common factor in the history of cannibal allegations was said to be the combination of its denial among ourselves, and its attribution to those we wish to defame, conquer, and civilize. In an atmosphere of postcolonial guilt, denial about cannibalism was extended to denial on behalf of those we wished to rehabilitate and acknowledge as our equals (Rawson 1997). Unfortunately, cannibal denial – and the debate it engendered – diverted attention at that time from a deeper analysis of prejudice, race, and racism, topics that had received renewed critical attention from anthropologists in the 1990s (Lindenbaum 2004).

Anderson's (2010) assessment of Gajdusek's scientific career identifies 1963 as the pivotal point when he shifted from considering ecological and cultural explanations of the disease toward an effort to discover an infectious agent. Entertaining the audience at the Royal Society with stories of picaresque adventures among 'primitives', Anderson (2010, 251) observes, Gajdusek:

> expatiated on possible genetic causes and the ecological background of the disease. Soon, however, he would begin transmission experiments at the US National Institutes of Health (NIH), in Bethesda, Maryland, seeking an infectious source for the bizarre neurological deterioration he had witnessed with such distressing frequency. ... On first encountering the Fore in March 1957, Gajdusek had found their primitivism and isolation captivating: he quickly assumed the personae of ethnographer and geographer, discarding for a time his microbiological training. Six years later, he was contemplating a return to microbe hunting.

His 1963 address to the Royal Society outlined a number of aetiological possibilities that had been considered and rejected, including 'such an outlandish hypothesis as the possibility that,

in the course of ritual cannibalism (the consumption of dead relatives, particularly by women and children as an act of respect and mourning), the infantile gut may have permitted passage of undenatured homologous brain antigens which might have initiated a state of auto-sensitization' (Gajdusek 1963, 162). However, as Anderson notes, kuru brains showed no evidence of any immune response, and no antibodies to brain tissue were detected. The search for a virus would displace further exploration of the cultural and ecological explanations.

'Virus' or 'prion'?

In the end, the nature of the unconventional infectious agent that caused kuru and other transmissible spongiform encephalopathies (TSEs) was finally shown to be a pure protein, called a 'prion' by Stanley Prusiner, a discovery that led to his own Nobel Prize (Lindenbaum 2001). It was Prusiner's award, not Gajdusek's, that drew skepticism from the scientific community and the press, illustrating the erratic pathway leading to the acceptance of scientific truth. Citing Clayton Christensen's proposition that new technologies are either innovative or disruptive, Prusiner (2014) notes that the former are said to rely on incremental improvements or small advances that nourish an established technology; the latter are transformational and discontinuous, creating disorder by disrupting the status quo, and are often greeted with profound skepticism. The prion, Prusiner (2014, x) writes, was an example of transformational thinking about proteins and how they cause disease, an example of a scientific anomaly that reproduces and infects but contains no genetic material, constituting 'a disruptive transition in our understanding of the biological world'.

The two Nobel awards also signal a generational divide that separated the two scientists and their scientific practices. Gajdusek received his medical degree in 1946 and was awarded the Nobel Prize for successfully transmitting kuru to chimpanzees, confirming the status of the disease as infectious. He and his collaborators had inserted their clinical observations of kuru and other TSEs into larger frames in order to make sense of the diseases. Prusiner believed that his biochemical training following receipt of his medical degree in 1968 gave him a technological advantage in discerning the nature of the infectious agent based on molecular analysis and the biochemical logic of these conditions (Anderson 2008, 191–198).

Prusiner and Gajdusek found it difficult to agree on a common language to describe their findings. 'To me', Prusiner (2014, 145) said, 'there was so much evidence against a virus causing scrapie that it seemed misguided to continue using the word. If people didn't like my word, they could use the word "agent". ... I argued that it was time to retire the word "virus"

in this context and suggested the term "prion"". Gajdusek, however, 'adamantly refused to use the term "prion" and always called these agents unconventional viruses or infectious amyloids' (Alpers 2010, 58).

Prusiner made two very short visits to the Fore region. During the first in 1978, he examined eight kuru patients, and in 1980, seven more, but his research kept him in the laboratory. Gajdusek also worked in the laboratory, but spent stretches of time in far-away places where he led a nineteenth-century kind of research life that was possible in colonial New Guinea in the 1950s and 1960s. He wrote about this experience in his personal journals, which, like *Alice's Adventures in Wonderland*, described the 'almost endless curiosities' with which Lewis Carroll filled his writings (Gardner 1960).[9]

On wonder

Lorraine Daston and Katharine Park's (1998) sumptuous and illuminating *Wonder and the Order of Nature* explores the changing sensibilities of scientists toward the strange objects and the phenomena they observed, collected, and wrote about from 1150-1750. For medieval and early modern intellectuals, they propose, wonder was a cognitive passion, as much about knowing as about feeling (Daston and Park 1998, 15). Since the Enlightenment, however, wonder has become a disreputable passion in workaday science, redolent of the popular, the amateurish, and the childish: 'Scientists now reserve expressions of wonder for their personal memoirs, not their professional publications. They may acknowledge wonder as a motivation, but they no longer consider it a part of doing science' (Daston and Park 1998, 14–15).

Their message also puts to rest the assumption that wonders and monsters were marginalized by the 'rise of science' and the triumphs of rationality, or that secularism buried the wondrous for European elites. Marvels and wonders fell from grace in European high culture, as rabble rousers' claims of divine inspiration, expressions of demonic possession, and 'enthusiasms' of various sects all came to be viewed as a threat to the princes' and the church's monopoly on the power of wonder. The response of intellectuals after 1660, especially among those whose memories of civil and religious disturbances were still vivid, was to disarm a weapon that had fallen into the wrong hands (Daston and Park 1998, 32). Wonder and wonders became vulgar, the antithesis of what it meant to be elite, a profound

[9] The journals were distributed originally to colleagues and friends, and are now available in the archives of the National Institutes of Health.

mutation in the self-definition of the intellectual. In Enlightenment morality, monsters offended taste not truth.

Wonder in the field

Gajdusek's journals and letters from the field are filled with wonder. In a letter to Smadel at the National Institutes of Health, written on 15 March 1957, Gajdusek (1977, 109) describes his first encounter with the disease:

> I am in one of the most remote, recently opened regions of New Guinea (in the Eastern Highlands), only contacted in the last ten years and controlled for five years – still spearing each other as of a few days ago, and cooking and feeding the children the body of a kuru case, the disease I am studying – only a few weeks ago. ... It is so astonishing an illness that clinical description can only be read with skepticism. ... To see whole groups of well-nourished healthy young adults dancing about, with athetoid tremors, which look far more hysterical than organic, is a real sight. And to see them, however, regularly progress to neurological degeneration in six months, usually three, and to death is another matter and cannot be shrugged off.

Eight months later, wonder, amazement, and a sense of adventure have been excised from his account of the disease in the *New England Journal of Medicine* and the *Medical Journal of Australia*. His personal journals, however, continue to provide a place for awe and the allure of strange things. Writing on 3 September, Gajdusek's 1957 journal begins:

> For the past two years I have kept a personal journal in a haphazard, irregular, and half-hearted fashion. My Gidean phase of journal enthusiasm subsided as I finished reading all of Gide. … I feel this omission has been a loss to me – loss of richness in the experiences … for journal-keeping tends to add condiment to them. Again, I make a start. (1957, 1)

Gajduesk had first come to the Fore region on 14 March for a brief visit, and had remained in the region for almost half a year, much of the time spent on patrol. On 11 September, he reflects on the particular allure of having chosen Okapa, rather than Lufa, as a base camp for kuru research. (Lufa, also in the Eastern Highlands, is located among Hua-speaking people.)

> The Highlands are the show-places of New Guinea, the great mystery of the huge island, the great attraction to adventure seekers, the Shangri-La for the colonial pioneer settler. ... Moke [Okapa Patrol Post] and Lufa Patrol Post are the two show-places of the Highlands – choice inaccessible outposts which it is a privilege of a few to see. ... Apparently Kuru has brought me to the best of the lot. Lufa is impressive – that is easy to see – with its 7,000-foot mountainside location below the 11,500 summit of Mt. Michael, an isolated 'stone age' man station, administering a huge, interesting, recently-contacted, and still 'uncontacted' and unpatrolled population. Nothing could be more interesting, except our Moke! (Gajdusek 1957, 18).

He writes lyrical passages about the grandeur and beauty of the landscape. The journals are a compelling mix of personal reflection, literary elegance, medical observation, and descriptions of the marvels he now encounters at the margins of the world. On 18 August 1959, he observes:

> We left Awarosa hamlets … and descended several thousand feet … the mist rose slowly and left the Lamari Valley clear with a ceiling of clouds at the level of the rain forested mountain tops. This was such a mythical, aurorean scene – with the beautiful valley set in a nirvana-like atmosphere of breaking cloud and haze – that the mist enveloped early morning departure was more than compensated for. … The immense cliffs forming the eastern canyon wall gradually came into view across the Lamari as we gazed down into the canyon. Before us, and still far below, loomed a steep mountain which forms the eastern cliff-like bank at the mouth of the Aziana valley. The Aziana flows westward toward this mountain, but the mountain blocks its passage to the Lamari and the Aziana is forced southward, leaving the narrow and steep mountain as a separating wall between the two great rivers. To the south of this 'dividing range' the rivers join. (Gajdusek 1959, 86)

He writes again about the wonders of the environment. While the entry begins with the dispirited note that the day was 'virtually devoid of accomplishment' (with little scientific work), he finds that it was still such an inspiring day that there was nothing on the face of the earth he would rather have done:

> After passing through Kamira we climbed higher into the forested ranges and finally reached the ridge between the Kamira valley and the slopes descending toward the Lamari. … [A] half-hour later we suddenly broke from the rain forest onto a small kunai shelf, like a flying carpet, high above all the nearby ranges, all the geography of the upper Lamari lay stretched out clearly before us. … Across the valley, downstream, lie the slopes with gardens of Abomatasa which Vin and I visited on our second kuru epidemiological patrol. (Gajdusek 1960, 131)

While in the Awa village of Iauna, he enters the men's house and discovers four naked youths sitting cross-legged on one side of the small round central area, with low sleeping

cubicles arranged in a circle, and with a roof about four feet high. He noted that the boys were initiated, as could be surmised from their nakedness, their ceremonial posture, the uninterrupted chanting in unison, and the huge boar's tusks through their noses. They sat stoically and chanted to the accompaniment of several village elders, who sat opposite them. He was told that they had been in seclusion for three days, and that the initiates' nakedness would eventually be covered with new, exquisite, ceremonial decorations. Four pages of a journal entry in 1961 are devoted to a detailed account of the various stages in Kukukuku[10] male initiation practices, provided by a man who had experienced the arduous and complex ceremonies (Gajdusek 1961–1962, 1:178–81). These sensitive and detailed ethnographic passages are not interpreted or given analytic attention, as would be the case in anthropology.

Rare, strange, and beautiful things

The 1957 patrols have the trappings of 'first contact'. Writing to Jack Baker, Vin Zigas, and Lucy Reid, members of the Kuru Project back at the government station at Okapa, Gajdusek (1957, 47) indicates that he has been 'keeping maps, field notes, and compass sights, which will help in getting a first draft map ready when I get back and go over the data with you'. He treats local people with yaws, cuts, and sores; collects samples of their blood and urine; and tends to illnesses in his own line, the six youths who are his research assistants and close companions. In October 1957, the party arrives at the hamlet of Ureba, 'our first camp with the Kukukukus, a party of 72 carriers, 6 boys, 11 police and a contingent of a dozen or more Mobutasa men and boys who have come with us. Thus our line is 100 strong' (1957, 67).

He begins to record rare, strange, and beautiful things: 'On the way back to our camp, the Kuk youngsters picked up a large 4 cm. long cricket-like orthopteran with long antennae, which they eat. Further down, they attacked a decaying log with their hand axes to find a horde of larvae of these insects, at least 5 cm. long, with an abdomen one to two cm. in diameter', which they considered a dietary luxury (Gajdusek 1957, 126). He observes that 'the Kuks with us now have taken and eaten sugar, salt, buttered and jam-covered buns, so their reluctance is no insurmountable taboo', a note that reflects his research interest in child growth and development. Leaving the ridge, the boys find 'a highly aromatic yellow-to-purple colored leaf, ovate in shape and eight to twelve inches long, which they roll up and

[10] Anthropologists no longer use the name Kukukuku to refer to people in this Anga-speaking region, using instead local names to distinguish the different social groups.

insert through their nasal septal hole almost one cm. in diameter, large enough to pass the colored drawing pencils into it when they are drawing for me' (Gajdusek 1957, 126).

At a bush camp en route to the Yar people two days later, he notes that the leeches were fierce and the patrol encounters nests of stinging native bees and large ants. As they ascend to elevations where the sago plant thrives, he writes: 'The insect life has suddenly become lush and abundant. … Walking sticks, numerous grasshoppers, leaf hoppers, ants of every sort, large and small bees and various Homopterans [order of 'stink bugs'] … abound. I hope to start collecting insects shortly, as I did on the last trip to the Yar people' (Gajdusek 1957, 128). He keeps a record of animal and bird life and collects exotic objects – bows and arrows, stone tools, penis covers, and woven armbands – purchasing them with a colonial inventory of salt, beads, mirrors, and *laplaps* (cloth skirts). The route from the mountain range at Agakamatasa to Purosa, he notes, 'is one of the richest regions in the Okapa Sub-District for cassowary, cuscus, and bird of paradise', and the boys tell him there are tree-climbing kangaroos and python down toward the Lamari (1961, 1:140). The boys bring him:

> a remarkable snake over an inch in diameter and perhaps eighteen inches long. … It was golden brown or almost orange in color, with white, almost silvery, streak-like markings, really very beautiful, with a triangular head. … I photographed the snake and then placed it in a plastic jar. … I shall try to fix it in formaldehyde and send it to Bogaert at the American Museum in New York. … After the first crab at Morandugai my interest called forth a rash of crabs, and they appeared to be of two different species. I have sent one to NIH already and I have kept a few others as well. (Gajdusek 1957, 265–266)

A cabinet of curiosities

The Eastern Highlands had become his twentieth-century 'cabinet of curiosities'. Like Gervase of Tilbury, the thirteenth-century writer who dedicated his catalogue of marvels to Emperor Otto IV, Gajdusek's collections reveal the 'tight links between wonder, pleasure and the insatiable human appetite for the rare, the novel and the strange' (Daston and Park 1998, 23). He sends his insects, animals, and artefacts to various museums, the modern cabinets of curiosities. Collections of blood, urine, and autopsy material are also carried by an unlikely chain of couriers from the New Guinea Highlands to a different destination, his laboratory in Bethesda, Maryland, a feat Gajdusek describes as 'no trouble' (Farquhar and Gajdusek 1981, 67).

Warwick Anderson's (2013) cultural history of valuation in kuru research shows how the transit of body parts from their New Guinea locations, where the autopsies were conducted,

to Gajdusek's laboratory in Bethesda, and from there to the laboratories of colleagues around the world, resulted in the gradual severing of personal links associated with the specimens. Their value for scientific knowledge, and for the renown of the scientist, depended on the alienation of specimens from their Fore bodies. Gajdusek, not the Fore, was now seen to be the donor – a shift in scientific sensibilities considerably more rapid than the centuries-long process of objectification documented by Daston and Park.

For Gajdusek, the scientific value of his work remains central. He uses his network of runners to send letters and manuscripts to be typed and forwarded to scientific journals, and to receive mail, journals, and the latest information about kuru research. 'The Japanese Dr. Shoho, now in Africa, keeps suggesting helminthic infection' [parasitic worms], he notes, 'and we have not done enough on this line!' (Gajdusek 1961–1962, 1:294). At the same time, he continues to record his travels on film, both still and moving, driven by an urgent need to document and collect rare things and local customs before they disappear. He is attracted also to scientific research that requires what he calls 'opportunistic investigations of epidemiological problems in exotic and isolated populations' (Nelson 1996, 191). Before arriving in Australia to join the Hall Institute, he had been wandering the borders of Afghanistan looking for an appropriate disease. Soon after arriving in Melbourne he set out to investigate an outbreak of measles in northern Queensland, staying long enough to learn about Aboriginal child development, especially the secondary sexual changes in boys and the age of menarche in girls. At the same time he collected more than five hundred samples of blood, which he claimed was the most extensive serum collection ever made on Australian Aborigines (Anderson 2008, 51). Gajdusek sometimes wondered if he was turning into Lord Jim (the character Joseph Conrad describes as leaving earthly failings behind) as he finds a new set of conditions for his imaginative faculty to work upon (Anderson 2008, 9). Mathews described him as 'a latter-day Richard Burton' (Anderson 2008, 179).

In March 1962, Gajdusek visits Leonard Glick, an anthropologist living among the Gimi people, neighbors of the Fore. Glick had begun to study the ways in which cultures classify the natural environment, the 'grammars' of culture, an early contribution to the field of cognitive anthropology. By the time of Gajdusek's visit, however, Glick (1963, xvii) had shifted his focus to study native medicine, a rationale he later explained in his doctoral dissertation: 'Eventually it became apparent to me that native medicine though perhaps only peripherally relatable to what I had learned as a physician, led into some fundamental concerns of cultural and social anthropology, and there seemed to be room for further exploration of this subject'. Gajdusek (1961–1962, 2:43) had reservations about both projects:

> Len has worked extensively on ethnobotany and ethnozoology, especially on the taxonomy and classification in the native system. … He had intended to write his doctoral thesis on the native system of classification in zoology and botany but has given that up. The weak point in his work seems to me to be his failure to get either full collections of the botanical and zoological specimens he is working on, or to arrange for a full taxonomic identification. … I am a bit worried at the lack of sound anchoring in our scientific taxonomy.
>
> Len is not doing any medical or clinical investigation at all. He has, however, made very extensive anthropological study of disease classifications, treatments, and explanations, and intends to write his thesis on the medical lore of the people. It is a good idea, but I am disappointed to find here again another study of primitive medicine without the proper necessary tie-ins with modern scientific medicine. Perhaps he will rescue this matter before leaving, but I have my fears.

Glick's dissertation, completed in 1963, provided a comprehensive account of Gimi medical beliefs and behaviors. Gimi history, subsistence, religion, social organization, kinship and marriage, the fluid nature of alliance and enmity, as well as gender relations, were set out in five chapters, providing the anthropological context for his analysis of local medical theory and practice. Appendices include clinical observations about medical ailments not considered to be caused by sorcery, and a case study of the dynamics of a psychotic event that illustrates a Gimi approach to mental illness. His dissertation is an early example of the critical stance in medical anthropology, showing that people with conditions that would be identified in biomedicine as neuroses are not cut off from others, but are immersed in an environment that urges them to re-establish or maintain a semblance of normality.

The other or otherness?

At the time of his encounter with Glick, Gajdusek was not in a position to view the completed dissertation, but his comments suggest that he does not fully comprehend the nature of ethnography. Alpers's (2014, 21) recent criticism of anthropology also suggests a misreading of the discipline and its history:

> It is hard to separate the discipline from its original core … [which is] based on the concept of the Other, and there is no Other. There is but one human race. … The concept of ethnicity provides an inclusive combination of genetics and culture to describe human groups, but there are no exclusive 'races' into which human-kind can be divided. … The core of anthropology should be our humanity … [and adherence to the original core] continues to constrain the way that inherently fascinating ethnographic information is written up.

These well-meant views – Gajdusek, the virologist, calling for more science and Alpers, the epidemiologist, for more humanism – draw attention to the predicament of anthropological identity, a question that has long energized anthropologists. In the 1960s, Eric Wolf's (1974 [1964], 13) answer was that anthropology had a foot in both camps:

> Anthropology is both a natural science, concerned with the organization and function of matter, and a humanistic discipline, concerned with the organization and function of mind. … Just as the subject matter of anthropology is dual, so the concern of the anthropologist is dual: he must mediate between human biology and ecology on the one hand, and the study of human understanding on the other. Necessarily, he must be both outside observer and participant in the internal dialogues of his informants. By definition, therefore, anthropology is less subject matter than a bond between subject matters, and the anthropologist will forever find himself translating from one realm to another.

Beginning in the 1960s, anthropologists had begun to question their basic concepts and motivations, as well as their own cultural, class, racial, and gendered positions as ethnographers, reflecting the ethos of the time. The debates led to a more refined understanding of the moral and ethical nature of ethnographic research and writing, with a consensus forming that ethnographers should expose, analyse, and critique human inequality and discrimination, and appreciate other ways of being human (Knauft 1996). The debate about anthropological identity gained momentum again in the 1980s. A number of scholars, anthropologists among them, turned their attention to the nature of ethnographic writing and representation at a moment of postmodern sensibility. The more recent focus on 'anthropological engagement', a research method that might include a challenging mix of social critique, advocacy, and activism, views research as co-produced by the anthropologist and members of the community being studied (Low and Merry 2010). As Marcus and Fisher (1986) had proposed earlier, ethnographic writing would provide the descriptive foundation for anthropological theory, a crucible for theoretical play which allowed for the reconceptualization and rebuilding of theory.

As a result of this exercise in disciplinary self-analysis, some scholars have turned away from research based on the concept of the 'Other', a distancing term that both leaves little room for exploring the intimacies and complexities in human relationships, and places a limit on the entities that are considered to belong in a common world (Descola 1996; Viveiros de Castro 1998; Kohn 2013). We are called instead to encounter what might be called 'otherness', a kind of 'third place' achieved mid-stream by the swimmer who crosses a river, to adopt Michel Serres's (1991, 5–6) metaphor. The real 'passage' occurs in the middle space between two thresholds, when the swimmer abandons all reference points.

While our articulation of this middle space may be recent, the experience of it is not. Kenneth Read (1965, 6), one of the first anthropologists to work in Papua New Guinea's Eastern Highlands characterized this middle space in 1965: 'Looking back now, I believe I was permanently elated most of the time I was there. At least this is the only name I can give to a state of mind in which certainty in my own abilities and discovery of myself joined with a compassion for others and a gratitude for the lessons in acceptance that they taught me'. Following a return to the Eastern Highlands, thirty-one years after he saw it for the first time, Read (1981, ix–x) writes that the Gahuku-Gama are:

> a people to whom I owe a possibly irreparable debt of gratitude. They were the largely unknowing mentors in my journey toward the discovery of myself. … I have never known why this admission generates such suspicion … but I venture to say that if you can show me an anthropologist who has not been moved and in some way changed by the experience of fieldwork, then you have not brought me a good anthropologist. The goal of objectivity is not subordinated by acknowledging that one must of necessity establish personal relationships with those who are also your subjects.

Anthropologists who worked with the Fore were also drawn into the world of wonder as they explored local concepts of sorcery and theories of disease causation (Julius 1957; Berndt 1962; Lindenbaum 1975), and the enchanted environments where humans and bush spirits live together (Lindenbaum 2002).

Anthropological entry into this middle space depends in large part on the often-undirected nature of ethnographic research. A typical day in the South Fore might begin by setting off with research assistants to attend a distant court hearing, a marriage payment, or a funeral. Others join us on the road. Our companions comment on significant things and events associated with particular locations – pandanus trees planted by their ancestors, the site of a battle with people they once considered their enemies. Taking a different path on the way home, we pass through a stand of undomesticated forest inhabited by bush spirits, our voices subdued to avoid attracting the attention of these capricious beings.

Gajdusek (1960, 133) had also acquired a sense of otherness and self-understanding while in the Eastern Highlands:

> If the weather is with us, we shall again behold the great Lamari valley, which I love as perhaps no other spot on earth. It is here that I have found myself and lived my most satisfying days, I have no excuses nor apologies to give my fellow civilized men … for I am proud of the fact … and with Roosevelt High School, University of Rochester, Harvard Medical School, Cal Tech., the Boyce Thompson Institute and Walter Reed, this has been one of 'My Universities'. … It is only when one lives closely to these ethical, very moral, and emotionally sensitive people, who

> have a great respect for person and property, a rigorous yet 'enlightened' sexual code, a keen and proud individuality coupled with a strong sense of family responsibility and community solidarity, that one realizes how little we have progressed in five millennia.

His scientific publications, however, provide no trace of this exuberant sense of wonder and illumination. Prusiner's scientific publications similarly give no sense of the awesome experience of working in the laboratory. His recent memoir, written after the Nobel award, however, anticipates excitement about the 'wonderful experiments' he now contemplates and the captivating structure of biological entities: 'There is a remarkable beauty to the complexity of proteins and the simplicity of their assembly. … After chains of amino acids are assembled, they must fold into a particular shape before the protein can become biologically active. Sometimes proteins require help to fold properly – other proteins called chaperones, provide this help' (Prusiner 2014, 175). Latour might draw our attention again to the agency attributed to chaperone proteins, the scientific act of animation before the entities become de-animated.

Gajdusek retained a sense of wonder and wrote about it while in the Highlands, his 'university' for self-discovery, nurtured by personal relationships with his research assistants and with people at Agakamatasa where he had his own fieldhouse. His inner world was nurtured also by literature. The journals refer to Camus, Conrad, Dostoevsky, Faulkner, Gide, Gogol, Hesse, Joyce, Kafka, Kierkegaard, Mann, Melville, Musil, Nietzsche, Proust, Rimbaud, Salinger, and Tolstoy, and he carried the works of many of these authors with him. Like Prusiner, however, his scientific self was molded in the laboratory, and sustained by the institutional practices and rewards achieved as a member of the scientific community with which he maintained contact even while living in New Guinea.

On seeing and knowing

This essay began by situating ethnographers as participants in disputes among medical scientists about what they accept as truth, and turned then to a related topic: the ways in which members of each discipline wrote about their observations, the theoretical assumptions that undergird their work, and the fashioning of the scientific self. Anthropologists and medical researchers both cultivated a certain kind of scholarly self, molded at a particular time in a particular place, and their personae are revealed best in their publications, part of the process of 'doing science' that Daston and Galison (2007) describe for nineteenth-century scientists.

The elimination of awe and wonder in the scientific publications of Gajdusek and Prusiner is a legacy of the creation of the scientific self associated with the quest for achieving scientific objectivity. As Daston and Galison observe (2007, 204), scientific selves were not called into being by free-floating norms and types alone: 'A Self must be practiced, not simply imagined, admired (or castigated), as a public persona'. To demonstrate the norms or codes of conduct associated with the scientific self, they select three images from the long tradition of scientific atlases, the dictionaries of the eye designed to represent scientific objectivity: an eighteenth-century drawing of a typical plant species, a late-nineteenth-century micrograph of snowflakes, and a mid-twentieth-century image of the magnetic field of the sun, the mixed output of sophisticated equipment with a 'subjective' smoothing of the data (Daston and Galison 2007, 20–21). The three images are said to represent truth to nature, mechanical objectivity, and trained judgment, enforced by an appeal to these ethical values, as well as to pragmatic efficacy in securing knowledge. Considered by their producers to be faithful representations of nature, Daston and Galison (2007, 53) note that they are also 'nature perfected'.

Just as nineteenth-century worldviews wedded to particular research strategies resulted in a new conception of the scientific self, anthropologists in the 1960s questioned their own concepts and motivations, an exercise that provided a new understanding of the moral and ethical nature of ethnographic research resulted in a change in the perception of the anthropological persona. In the current ethos of environmental humanism, we see this happening again. Eduardo Kohn and Philippe Descola, among others, question the scientific distinction between nature and culture, as well as between humans and things. Adopting a modern form of animism, Kohn's ethnography *How Forests Think* (2013) and Descola's *In the Society of Nature* (1996), break with our inherited dualisms, question our assumptions about what it means to be human, and provide a register of our shifting sensibilities. They also retain a sense of otherness, enhanced during fieldwork among people for whom the idea of nature as the domain of all phenomena occurring independently of human action is foreign.

Some anthropologists are also searching for forms of representation that go beyond linguistically and culturally mediated ways of thinking, choosing a technological solution favored by scientists in the nineteenth century. Kohn (in Golub 2014) suggests that photography could convey the nondiscursive, representational logic he came to comprehend while living with the Runa in the Upper Amazon. Similarly, the work of Sebastião Salgado, a Brazilian documentary photographer of the mountains, deserts, oceans, animals, and peoples that have so far escaped the imprint of modern society, is informed by 'the absurdity of the idea that humanity and nature can somehow be separated' (Salgado and Salgado 2013, 6). The current quest for technologies designed to represent the world made visible, also seems an echo of nineteenth century hopes that technologies of the eye could create images untainted by subjectivity.

When the Fore first encountered the imaging technologies introduced by kuru investigators, they said that photographic negatives would reveal the sorcerers holding poisonous kuru bundles in their raised right hands, and that the ophthalmoscope, a scientific instrument for viewing the interior of the eye, would reveal secret identities and hidden motivations. The Fore belief that 'to see' is 'to know' brings into sharp focus different visions of what constitutes truth, the issue that energized the debates among anthropologists and medical scientists during the early years of kuru research. Anthropologists recognize that technologies are not neutral (Lock and Nguyen 2010). Located in both the sciences and the humanities, we understand also that a knowing producer, employing a specific epistemic strategy, stands behind every image, and viewers bring to each image their own ways of seeing and knowing.

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