

In the Science Zone II: The Fore, Papua New Guinea, and the Fight for Representation

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Four questions—two for Dr. Anderson, the medically trained historian and STS scholar, and two for Professor Anderson, the historian of medicine and anthropology—arise as I read Warwick Anderson’s *The Collectors of Lost Souls*: Who speaks for natural history? Who speaks for the protein or prion? Who speaks for today’s Fore? And who speaks for Asian Pacific science and technology studies, or (given Professor Anderson’s two manifestos on the subject) is there a postcolonial in this text?

But let me begin with an appreciation of how I read what my friend Warwick has achieved so brilliantly in this book already awarded multiple well-deserved prizes. Let me also note in passing the sixty photographs that provide a useful supplement to the “tangles of plaque” in the text. The photographs reveal the humanity of the victims of the neurodegenerative kuru epidemic (relatives helping the afflicted, pp. 111, 113, 130, 156; the juxtaposition of gnarled people “from the stone age or primitive past” with handsome children and adults, pp. 27, 73, 100) and of the well-groomed boys who accompanied Carleton Gajdusek to collect his Nobel Prize and at home in Maryland (pp. 188, 227); track Gajdusek’s maturation from eight-year-old pirate to young scientist to Nobel laureate (pp. 36, 55, 186, 218, 222); portray key scientists and anthropologists in the story, including their work in the field examining patients, taking blood, filming, and posing; and memorialize the primates whose infection and death contributed to our recognizing the disease’s infectious transmission (pp. 140, 158).

Two pictures are particularly layered with meaning for interpretive insight. The cover image, interestingly, is of the Estonian-born physician Vin Zigas (not Gajdusek), admitted among refugee physicians after World War II as an Australian medical officer for New Guinea, and the first to study kuru. Zigas, recumbent, eyes closed, and smiling, is surrounded by happy young Fore men, perhaps turning him into their Whiteman with their gazes, including one turned toward the camera/us.

The other is of “Zigas, [Jack] Baker, and Gajdusek at the dining table/desk/autopsy bench at Okapa. Human brains on the plate, specimen bottles on the window-sill, and census books on a folding table,” with Gajdusek looking through a microscope. The ambiguity or “multivocality” of the photograph lies in its potential on the

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one hand to reveal the backstage of exploratory fieldwork and medical research (often makeshift bricolage, with its multiple diagnostic and correlating tools), or on the other hand to raise questions about whether Gajdusek and others unwittingly were putting themselves at risk of infection from the sticky protein during their autopsies. The latter is very unlikely, though unkind joking sometimes suggested that Gajdusek's erratic behavior in later life was due to such infection. More to the point is that the image enigmatically suggests the adventure and dangers of infectious disease research, and the border between bureaucratic containment protocols and good lab practice in dealing with infectious materials.¹

I divide my comments into four parts: placing the kuru story in conversation both with parallel projects in the 1960s among the Yanomani and others and with today's successor projects; exploring three different meanings in Anderson's subtitle; using my four questions to probe the limits of Anderson's framing in order to suggest complementary directions for future work; and reflecting on Anderson's ethical tact in his ending and on implications for other work in Asian Pacific STS.

1. Carleton Gajdusek and the National Institutes of Health's International Biological Programs

The book *The Collectors of Lost Souls* succeeds far beyond the biography-of-a-Nobel-laureate format that Anderson's extraordinary access to Carleton Gajdusek's journals and confidences might have created.² Indeed, the book aspires to be "a sort of biology and cultural sentinel capturing the culture of biomedical investigation in the second half of the twentieth century" (5). It is partly the story of one of the multiperson, decades-extended research programs of the 1960s, the International Biological Program, in which biologists and anthropologists participated. Using the past tense, and the period convention "twentieth century," marks it as historical, but, I want to stress, its successor projects continue, if no longer energized by Gajdusek, who died two years ago in his Norwegian winter darkness retreat above the Arctic Circle (McNeil

¹ Judith Farquhar remembers (personal communication) that Gajdusek refused to secure his lab at the National Institutes of Health on the grounds of bureaucratic interference. What is needed, he would argue, rather than containment facilities is good lab technique. He did have a Level 4 containment facility at Fort Detrick.

² One of the first games my Massachusetts Institute of Technology students played with Anderson's text was to identify Nobel laureates and how their names would structure the narrative's attribution of value (another way of telling the story). There are eight to eleven: Wendall Stanley (1935, for crystallizing the tobacco mosaic virus, which was important to the Gajdusek story for the irrepressible idea, proved wrong for the tobacco mosaic virus but with staying power for the prion, that it was an autocatalytic protein); Linus Pauling (1954 Nobel Prize in chemistry for work on the chemical bond); John Enders (1954, for cultivating the polio virus), with whom Gajdusek worked at Harvard and Children's Hospital in Boston, learning tissue cultivation techniques; F. Macfarlane Burnet (1960, for work on immunological tolerance), whom Gajdusek first admired and later competed with; Max Delbruck (1969, for work on the replication and genetic structure of viruses), whose phage group coterie at CalTech included Gunther Stent, James Watson, Elie Wollman, and Benoit Mandelbrot as well as Gajdusek; Carleton Gajdusek (1976, for a new mechanism, "slow virus," of infectious disease, shared with Baruch Blumberg for isolation of the hepatitis B virus, using "the Australian antigen," a gift of the aborigines of Australia); and Stanley Prusiner (1997, for the prion). Three more are mentioned in passing: John Eccles (neurophysiology), James Watson (double helix), and Joshua Lederberg (genetic recombination).

2008). And those continuances make me wonder about just what sort of model for STS and particularly for Asian Pacific STS this book makes. How might we write it otherwise, those of us with other points of access, interests, and resources?

For those of us who lived through the kuru and cannibalism debates of the 1960s–70s, among the most vivid debates on which I cut my teeth as an undergraduate at LSE in social anthropology and as a graduate student in anthropology at the University of Chicago, and their successor debates—the “slow virus” versus “prion” debates of the 1980s; the revival of the crystallization idea (à la Kurt Vonnegut’s ice-nine³) and the “mad cow” or bovine spongiform encephalopathy scare of the 1990s; the indignant controversies over bioprospecting and patenting of immortalized cell lines and genes in the late 1990s;⁴ the twenty-first-century exploration of normal and abnormal prions in healthy tissue formation as well as disease processes including bone marrow formation (normal prions) and cancer oncogenes (abnormal prions)—for those of us who lived through all these debates, with their hot accusations and counteraccusations, the publication of Anderson’s *The Collectors of Lost Souls* is a wonderful double journey into (a) *historical epistemology* (à la Ludwik Fleck’s “no epistemology without history” [Fleck 1935])⁵ and (b) *testing terrains* and *ethical plateaus* (Fischer 2003). While modeling interactions among diseases, social organization, and cultural frames, Anderson tracks infectious disease geographies and ecologies made even more changeable by climate warming (malaria, for instance, is now reaching the highlands of New Guinea, no longer confined to just the coasts). These testing terrains and ethical plateaus are sites for managing multidisciplinary subjects, putting multilocal ethnography to work (Marcus and Fischer 1986; Fischer and Marcus 1999), and exercising ethical tact.

Shirley Lindenbaum, one of the anthropologists sent out by the mathematical geneticist John Bennet to do what medical scientists call pedigrees and anthropologists call genealogies, has called Anderson’s book an “important history of kuru research” (2010: 325). Like biography, history is a genre that cannot completely discipline this book. Or rather, the multidisciplinary and decades-long evolving research programs that are Anderson’s shape-shifting subjects force the generic conventions into mutating open-ended forms. At key points, aesthetics and ethics motivate authorial decisions that operate on the level of “sound and sentiment” of unseen and occult worlds, notions to which I will return.⁶ Lindenbaum’s role vis-à-vis John Bennet parallels both that of Napoleon Chagnon’s role vis-à-vis geneticist James Neel’s interest in the Yanomami and Gajdusek’s vis-à-vis Joseph Smadel (who sent him to collect blood samples in Iran, Turkey, South America, and Melanesia).

³ From Vonnegut’s 1963 novel, *Cat’s Cradle*. Vonnegut variously attributed the idea to learning about Nobel laureate Irving Langmuir’s idea about a form of solid water stable at room temperature, with which Langmuir entertained H. G. Wells; and to Gajdusek’s notion of an ever-expansive process of crystallization.

⁴ Including genes from the neem tree in India, John Moore’s hairy leukemia cell line in California, Craig Venter and the National Institutes of Health’s early effort to patent genes produced by mapping the human genome, and a cell line from blood taken from the Hagahai of Papua New Guinea with a variant virus promising resistance to, and vaccine against, adult T-cell lymphoma.

⁵ Appropriately, the book won the 2010 Ludwik Fleck Prize awarded by the Society for the Social Study of Science.

⁶ “Sound and sentiment” is a reference to Steven Feld’s ethnography of that name about the way in which natural sounds of the forest and songs of the Kaluli of Papua New Guinea make up an unseen world but also a vocabulary of perception and emotion and a sensorium built more on sound than sight (Feld 1982).

Lindenbaum, of course, is pleased that Anderson addresses “the complexity of Gajdusek’s reluctance to adopt the cannibalism hypothesis” (2010: 325), for which she, along with her then-husband, Robert Glassie, collected much of the sociocultural and genealogical data (Lindenbaum 1971, 1976, 1979, 2001, 2004, 2010). Other old timers have also been writing remembrances and accounts of the multidisciplinary efforts to unravel the mystery of degenerative neurological diseases like scrapie, kuru, Creutzfeld-Jakob disease, and other transmissible neuroencephalopathologies. Robert Klitzman’s memoir (1998) vividly captures some of the on-the-ground feel of a decaying postcolonial setting, and the rural lives of the uneducated among the Fore, in the late 1990s—a sharp contrast to tropes of medical, economic, and political progress (compare Anderson’s similar, if very brief, passage: 214).⁷ Klitzman’s account resonates with fears that “mad-cow disease” in Britain was a harbinger of plagues of transmissible encephalopathologies with long incubation periods and with a dawning realization that infectious diseases were not receding into the past but were reemerging in new as well as resurgent resistant strains interacting with chronic disease and fostered by increasing inequalities. Anderson is considerably less vivid (in the book) about the endemic violence in Port Moresby and even in the town of Goroka (population about sixty thousand), and the degree to which current conditions are less secure than they were previously, a matter of both tact and lack, to which I will also return.

Today the most prolific and important of the old participant-observers is the anthropologically trained physician Michael Alpers, who did many of the early autopsies in Fore villages, who was the first to analyze striking age- and sex-related patterns revealing the historical and spatial epidemic onset and decline, and who with James Gibbs inoculated chimpanzees at the National Institutes of Health to prove that the disease was transmissible, clinching the cannibalism hypothesis at the gross tissue level (Alpers 1986, 2008; Gajdusek, Gibbs, and Alpers 1966; Klitzman et al. 1984). (The puzzle continues at the molecular protein level.) Alpers is a possible foil to Anderson’s account of the imperial and subimperial struggles to control the field of kuru research, a struggle between the United States’ National Institutes of Health (NIH), represented by Gajdusek, and Australia, represented early in Anderson’s account by Sir Frank Macfarlane Burnet, whose slim mid-twentieth-century book *The Natural History of Infectious Disease* (Burnet and White 1972) Anderson calls (without comment, as if it were self-evident) “the most important monograph in twentieth-century medicine” (48). Anderson stands by this hyperbolic claim, but it surely would bear some substantiation of the influence that this book exerted, again a matter to which I will return in the first of my four questions.

Alpers, also Australian, but one who worked closely with Gajdusek, returned to Papua New Guinea to become for twenty-seven years (1977–2005) the director of the Institute for Medical Research (IMR) there, building it up to a 250-person local staff, continuing the epidemiological surveillance of kuru to its final victims and maintaining his half-century-long involvement with the Fore village of Waisa begun in 1961 (Alpers 2008; Mueller et al. 2010; Mead et al. 2009a). As director of the IMR, he initiated several projects on local diseases including malaria, diarrheal diseases, typhoid, asthma, filariasis, and pneumonia; attempts to design a malaria vaccine for

⁷ Klitzman has a particularly scathing account of local elections.

the tropics; and genome-wide association studies (Mead et al. 2009a, 2009b; Mead, Stumpf, and Whitfield 2003; Mueller et al. 2010). Gajdusek thought genomics to be a waste of time and money; Alpers's IMR produces genomic research. Institutionally as well as cognitively, Alpers might be an interesting foil to Gajdusek, because Alpers is a regional institution builder and because he consistently combined cultural, social, clinical, epidemiological, and biomedical approaches in his research protocols, demanding that all IMR staff take an anthropological approach to their clinical work. This multidisciplinary approach contrasts with that of Gajdusek, whom Anderson describes as shifting away from his early enjoyment of anthropologist and medical geographer personae, pursuing something Anderson mysteriously calls "integrated if importantly not holistic" studies. For various reasons, including age and time, Gajdusek focused increasingly on biological pathogenic causes. Alpers, by contrast, produced one of the earliest articles (first presented in 1967 and published in 1968) on the cannibalism hypothesis (derided by many colleagues at the time), and more recently in 2008 a lovely article (with Whitfield, Pako, and Collinge) on the cosmology and ritual process of Fore mortuary rites (Alpers 1968, 2008; Whitfield et al. 2008).

In this article care is taken to describe possible ways of recycling the material remains of the dead as *transumption* (other ways are burial for transumption by worms, practiced also by Europeans; and hung in exposed baskets for transumption by birds, practiced also across parts of Asia and North America) rather than cannibalism to protect the mortuary rites and their cosmological rationales from the continuing media circus, primitivization, and appeal to perverse middlebrow sublime pleasures of exotic enchantment and claim to horror (Whitfield et al. 2008). Alpers and his fellow authors also make a point of having asked and received permission of the Papua New Guinea government for their study and of acknowledging their Fore informants (of whom author Pako is presumably one), Alpers's former Fore patients, and the chimpanzees he and Gibbs inoculated and cared for.

Alpers thus registers the historical shift in ethics—both in institutional requirements and in general expectations—about medical and ethnographic research. As I described in my *Anthropology Today* review of the Yanomami debates:

1960's science and 1990's science are institutionally quite different enterprises. They are different along at least three axes: the ethics and institutional contexts of science; the way in which activists of all sorts, including scientists, become media players, complicating questions of who speaks for whom; and the palimpsest continuities and differences [between] the human biology research projects (population genetics, sociobiology, human genome diversity projects [Cavalli-Sforza went to Papua New Guinea too and drew blood], health and epidemiological transitions) of the 1960s and 1990s (Fischer 2001: 3).

So far I have listed ten topoi or key issues, a decalogue, for rereading the kuru story in terms of access to (1) *historical epistemology* (what did we know, when, in relation to what other knowledges and frames); (2) *testing terrains* (how to model complex interactions); (3) *scientific imperialisms and subimperialisms*, or turfs, and control over information flow; (4) medical research *institution building* in independent Papua New Guinea (since 1975); (5) the *sequence of genetics-behavioral-ecological projects* (population genetics and sociobiology to genomics, biomics, and syndemics) from, as

the Brazilian biological anthropologist Ricardo Santos (2002) put it regarding the Yanomami debates, watching blood, feces, and urine samples flow from the Brazilian Amazon to Penn State and the University of Michigan in the 1960s and watching the flow repeat again in the 1990s, only now called DNA; (6) the development of *institutions of ethical accounting* (IRBs, formal consent protocols); (7) use of *comparative animal models* (sheep scrapie, mice, hamsters, mink, monkeys, chimpanzees, yeast; and today comparative genomics); (8) *anthropological* methods, archives, and perspectives (genealogies, story elicitation and interpretation, *attention to belief and local cultural frames for clinical effectiveness*); (9) *the role of the media* (who speaks for whom, public relations, exoticizing moral panics, representations of science); (10) *shifting logics of exchange* in various spheres (gift economies; commodity exchange and capital accumulation in symbolic, power, or economic currencies; cosmological exchange cycles).

2. Exchange Systems and Appropriations of Anthropological Theory

Anderson's book is subtitled "Turning Kuru Scientists into Whitemen," and much of his meditation is about exchange systems. There are at least three registers of reference in the harmonics of the subtitle.

First there is the stress on Fore categories of apprehension, a matter of cultural analysis or cultural anthropology. Alpers highlights this register both in his account of Fore mortuary rituals as concerned with the socialities of each of the five souls of Fore personhood, and in his insistence that all members of the IMR adopt anthropological attitudes of openness to patients' beliefs and narratives as well as to the information about ecological, behavioral, interpersonal, and psychological interactions and implications that are carried along with and encoded in those beliefs or narratives. The funerary rites of exchange and transumption embody and encode obligations and memory. They affirm and rearrange the affective, social, and juridical ties of kinship, as classically described, and analyzed, in male terms, and reanalyzed in female terms, by Bronislaw Malinowski (1922) and Annette Weiner (1976) for the Trobriand islands (further expanded upon for New Guinea by Marilyn Strathern [1975, 1988]), showing how funerals, in particular, disassemble and return cosmic elements temporarily entangled in marriage relations between Trobriand matrilineages. Roy Rappaport's once famous, now period piece, *Pigs for the Ancestors* (1968), perhaps has renewed salience here. That study was done in an ecological-energy-nutritional framework popular in studies of the Yanomami and of the San Bushmen as well, in an enthusiasm for homeostatic models of complex ritual and marriage cycles, functioning also as self-organizing population-spacing mechanisms, in which warfare and sorcery were regulatory devices. It was an early systems biology and ecology approach but at a still crude macro, not molecular, scale.

Anderson stresses the magic and sorcery component of Fore conceptual worlds and invokes Strathern's claim that Melanesian modes of personhood are paradigmatic in the same way that Marcel Mauss (1925) claimed Maori notions of the *hau* (the spirit or surplus value of a gift requiring a larger than equivalent return), Trobriand proliferation of juridical kinds of gifts and steps in gift giving, and Kwakiutl aggression and strategic compulsion in gift exchange are paradigmatic of the moral foundations of the

obligations of exchange systems. Anderson, following Bourdieu (1977) on the strategies of gift giving and following Strathern on sorcery (1975, 1988), claims that the magic of sorcery cuts social relations and accumulates power in ways that circumvent legitimate and normal accumulation and social exchange. He suggests that laboratory work is a parallel sorcerer's site of alienation of persons into things, and in that sense works the magic of the scientist's accumulation of power: blood is ideally stripped of its relation to the person from which it comes to yield up pure (genetic, biological) information. But in fact this alienation can never go to completion (the blood would become informationally useless), just as gifts are never separated from their givers, and it is through the networks of exchange, strategically played, that Anderson is more persuasive about how Gajdusek became a big man in the kuru sweepstakes. Gajdusek, he points out, was too emotionally wrapped up in his relations with the Fore to ever really turn them into objects or things. Gajdusek, moreover, seemed happier traveling than in the lab, and Warwick's account is consequently thin on events in the lab, although a colorful passage from then-lab assistant (now University of Chicago professor of anthropology) Judith Farquhar and from a *Look* magazine reporter says more about the ethos of the 1960s than the scientific work.

The second register in Warwick's subtitle refers to and stresses the social anthropology of exchange relations of the Fore and Melanesians in general (and in fact all of us, as Marcel Mauss famously taught us to interrogate the sources of moral power), in this case of trying to socialize into local cultural terms the operations of the foreign scientists turning them into what in pidgin is called, not white men, but whitemen, an effort linguistically as well as morally to enroll white outsiders into Fore systems of moral exchange and obligation. At the end Warwick dissects the demands of these exchange relations as one of Gajdusek's psychodynamic flaws, quoting from one of the journal entries Gajdusek's own locating of his need to get away from a claustrophobic (however loving) relation with his mother to parallel desires to escape too-demanding relations with his younger scientific colleagues in the lab as well as with the Fore. At other times, Anderson begins to sketch out a shift from gift-giving scientific exchange networks to commodified capitalist ones (paralleling those that Hannah Landecker [1999, 2000, 2007] has sketched for immortalized cell technology beginning with the free exchange of HeLa cells in the 1960s to John Moore's demand for commodity control over his hairy leukemia cells in the 1990s, and Henrietta Lacks's family's claim for some monetary compensation in the 1990s era of intellectual property claims). Gajdusek still represents the earlier morality, while Stanley Prusiner (at Stanford) and Charles Weissmann (in Zurich) represent the post-*Chakrabarty* Supreme Court decision (1980) and post-Bayh-Dole legislation (1980), morality that initiated an era of patenting, spinning off research into biotech start-up companies, and hiring public relations and marketing people. By invoking this shift Anderson can avoid painting either Prusiner or Gajdusek as immoral. As in many scientific fields, prion studies was an interesting field of competition over naming rights, credit, and the cultural capital that so accrues, but just as noted, there are larger systemic shifts involved.⁸

⁸ Pat Merz's scrapie-associated fibril (SAF) was called prion rods by Prusiner. The scrapie incubation gene (Sine) of Alan Dickinson et al. was called Prn I by Prusiner. Gajdusek's protein crystallization theory was called infectious prions by Prusiner. Gajdusek acknowledges Prusiner's gaming but philosophically said that

Third, of course, *sotto voce*, is perhaps the loss of Carleton Gajdusek's multiple souls. He, one of the Collectors of Lost Souls of the Fore loses his own multiple souls (which the Fore perhaps try to help him re-collect by their demands on him): first his multidisciplinary soul, focusing ever more on the biological pathogenic causal factor; second the loss of his public aura soul (through a very public arrest and prosecution for child abuse and perversion, a scandal caused in part by Western moralism and moral panic against homosexual and homosocial patterns undergoing change both in Melanesia, where ritual patterns were being eroded, and the United States, where homosexuality was slowly coming out of the closet);⁹ and third perhaps what Erving Goffman (1959) would call loss of the soul-controlling impression management over his own erratic and irascible character, and thus over the exchange systems in biological objects, collaborative relations, and personal relations.

As an anthropologist, I am pleased by Anderson's nods toward the importance of social and cultural anthropology and toward the subfield of medical anthropology, giving a very brief and tantalizing sketch of the rivalries among Australian centers of anthropology, even though sometimes he calls the social anthropology old-fashioned and impractical; he jokes that the old social anthropologists busied themselves collecting Fore fireplace stones, and one does not know whether he is referring to Ronald and Catherine Berndt or to Beatrice Blackwood, who did study the stone technology of the Kukukuku under trying conditions (11). It turns out he is talking about what eventually he identifies as "the dodderly old Reo Fortune," hardly a fair evaluation of anthropological methods.¹⁰ It is true, though, that (along with many other seemingly culturally specific syndromes) kuru was taken in the 1960s as one of many high-profile puzzles on which medical anthropology should have something to say, and in that sense helped hone that subfield's arguments about how to integrate biological with cultural or social dynamics.

I am bemused by his phrase "integrated if importantly not holistic." Methodological holism (or methodological functionalism) remains arguably one of the great strengths of social anthropology, the methodological obligation to follow the implications of changes in one part of society for other institutional parts of society, the ecological rule that you cannot change only one thing (Gellner 1959, 1970; Fischer 1997). In any case, Anderson evaluates neither Burnet's research program ("the most important monograph in twentieth-century biomedicine") nor directs attention to more recent integrative theory arguably represented by Alpers, and signaled by

he, Gajdusek, was in good company with Newton, who similarly lost out to Leibniz with respect to the terminology of calculus. (See Rhodes 1997 for these disputes. Rhodes's title, *Deadly Feasts*, plays upon both kuru and the fears in the 1990s that a series of encephalopathies seemed to haringer slow incubation infections that would become plagues because they would be impossible to detect in time. See Prusiner 2008 for his reflections.

⁹ This is among the ethically and legally delicate terrains of the book. Although a weak defense of Gajdusek's libidinal attachments to boys was that in New Guinea homosociality and homosexual rites of initiation were culturally appropriate, Anderson says that, while such practices were culturally validated across the river in the groups studied by Roy Wagner (1967a, 1967b, 1978) and Gil Herdt (1981, 1982), such initiation rites were not practiced among the Fore.

¹⁰ The names of not just Reo Fortune, Margaret Mead, and Ronald and Catherine Berndt go by, but also A. P. Elkin and S. F. Nadel, all storied names in anthropological lore and of quite checkered and colorful reputations. The training of government anthropologists is mentioned, if only in passing, with contrasting commitments intellectually and otherwise.

notions such as the third epidemiological transition and ecosyndemics (Alexander et al. 2003; Barrett 2010; Singer 2010). (Anderson, since the publication of the book, has contributed to a collection on this topic edited by Herring and Swedlund [2010].)

3. Kuru Legacies, the Fore, and the Fight for Representation: Four Questions

My four questions, two for Dr. Anderson and two for Professor Anderson, in the light of the ongoing successor projects, ask just what sort of model for STS and particularly for Asian Pacific STS this book makes, or how might we write it otherwise to find the limits of Warwick's text in order for us collectively to build upon it in further and alternative directions? To use a simple convention: Who speaks for natural history? Who speaks for the prion? Who speaks for the contemporary Fore? And who speaks for the anthropology of science and technology studies, or (given Professor Anderson's two manifestos on the subject [Anderson 2002, 2009b]) is there a post-colonial in this text (or does it remain firmly in the colonial modality of discussion of exploitation, appropriation, and orientalism or primitivization and denial of the other's coevalness)?

1. What happened to the natural history of disease? Who (and how many) speaks for the shifting relations among field science, lab science, and simulation science in our world of biomes, ecomes, and syndemics? And what role might Papua New Guinea's IMR play, along with such new initiatives as efforts to set up genomics centers in Africa (a current initiative of the National Institutes of Health and the Society of African Genomics) and international efforts to improve estimation of disease burdens, monitoring of pathogenic outbreaks, and new modes of transmission?

As we digitize Alfred Wallace's and Charles Darwin's archives (thanks to the facilities housed now, note, not in the Department of History but in the Department of Biological Sciences at the National University of Singapore), how do we remap the history of infectious and transmissible disease? Anderson gives us two historical points in time. First is the temporary defeat of the Australian regional empire to control the kuru and infectious disease field in New Guinea by the more global mapping and tissue collection of the U.S. National Institutes of Health. Later there is the presumptive defeat of Gajdusek's pregenomic biology by Prusiner's prion and commercializing biology. In the latter period, the prion laboratories in Edinburgh and London, stimulated by the bovine spongiform encephalopathy scare, had to negotiate across both gift-giving regimes in Papua New Guinea and the new intellectual property regimes (Collinge et al. 1996; Collinge 2001; Collinge, Whitfield, and McKintosh 2006). Today war and disease metaphors again intermingle. Infantry divisions (ID) and infectious diseases (ID) are causes for funding biosecurity concerns, new kinds of biology, cultural sentinel structures for emerging diseases, and in general promotion of biological sensibilities, attention to the intelligence of companion species, and other signaling feedback from the biosphere.

In Brazil, as STS scholar Myanna Lahsen (2004, 2008; Lahsen and Nobre 2007) has been describing, the Large-Scale Biosphere-Atmosphere research program, funded originally by the National Aeronautics and Space Administration (NASA) and the European Union as well as Brazil, was designed to build a Brazilian scientific capacity that would not be reliant on Harvard and Edinburgh tower experiments or Raytheon

and NASA surveillance systems for decision-making about the Amazon forests and global carbon cycles. As STS scholar Langdon Winner (1986) might have asked, do Tower experiments have politics? The answer: You bet. So what does the world look like from Papua New Guinea's IMR today, or from Melbourne and Sydney, or from Michael Alpers's perch in western Australia?

Or perhaps closer to home, what might it mean, in the wake of the sudden acute respiratory syndrome and avian flu scares, to have Singapore and Sydney as rapid detection and analysis sites for emerging new threats, including the encephalitis outbreaks for which Burnet saw New Guinea as a vast avian reservoir?

Transmissible spongiform encephalopathies are not traditional infectious diseases, and yet as we have learned from the bovine form, they can come from species crossings and new relations of production, as can infectious avian influenza. (In the 1970s before the mad cow outbreak, there were iatrogenic infections from transplanted cornea, from infected electrodes used in brain surgery, from hormone growth factor derived from cadaver pituitaries; it is not only the Fore who recycle the dead.)

2. Who speaks for the protein? Or can the infectious amyloid or scrapie-associated fibrils or prions speak? Obviously the names themselves index research teams competing for priority and for the rewards of coproduced authority and power. But the prion research program only now begins to unfold in places like the Whitehead Institute at Massachusetts Institute of Technology and other basic biology labs not involved earlier with the kuru story (see Garrity et al. 2010; Steele et al. 2009; Tyedmers et al. 2008, 2010).

The question here is whether journalist Richard Rhodes's book on kuru and Creutzfeld-Jakob disease correctly characterizes a series of discoveries in successive species crossings as a coming plague with unknown incubation times—the fear encapsulated in the old term “slow virus.” (It turned out not to be a virus, but the scary feature was the long incubation with deadly outcome.) As we begin to unravel more mechanisms—crystallization cascades (from which Vonnegut in part modeled *ice-nine*) but now using the help of polymer chemists to track protein misfolding triggered by variants of what otherwise are normal and necessary prions for health, each mutant leading to different strains of dysfunction, and to follow enucleation around tiny nanoparticles of aluminum silicate traveling along nerve axons into the brain. The kuru story here interdigitates with other amyloid plaque diseases (Parkinson's, Alzheimer's) and thus perhaps other research genealogies.

Indeed as Warrick suggests toward the end of the book, at the end of his life, Gajdusek was investigating diseases in Yakutia (Siberia) and Guam. In both cases he was more focused on biological mechanisms, having less time and fewer resources for anthropological inquiry. He was still full of ideas about mechanisms, looking at one point to the potato spindle tumor virus (not a true virus but an infectious agent that seemed to have no proteins) and tracking rat encephalitis from Siberia to Maryland.¹¹

¹¹ Judith Farquhar (personal communication). Farquhar recalls that at Gajdusek's funeral a loyal East European colleague eulogized Gajdusek by noting that he was less a virologist than a biologist interested in everything from plants (the potato spindle tumor, for which he would wander over to colleagues at the Department of Agriculture) to tick-borne encephalitis carried in the fur of rats, a problem that came up during the Korean War and that prompted a trip to Siberia to collect rats. He then speculated that rats around his home in Frederick, Maryland, were similar enough to Siberian rats that they might also carry the same

Among these, cascading crystallization and metal nanoparticles are still on the agenda. He was less enthusiastic about genomics, insisting it was a waste of time and money. That stance, too, might provide alternatives to current master narratives not just for overproduction of mere brute genomic sequencing, but also for more patient-directed and patient-focused research.

3. Who speaks for the contemporary Fore and for Papua New Guinea? It is here that the past tense becomes a particular problem, and the impression can be left that the Fore remain noncoeval “primitives” (Fabian 1983). It is perhaps here also that a peculiar vision of anthropological methods can too often be fetishized as a rhetoric of othering rather than, as anthropologists themselves practice their evolving research programs, as investigations into contemporaries who inhabit many differently situated conceptual perspectives and ways of valuing and culturally accounting. Despite the many Fore whose voices murmur throughout the book about memories of former mortuary practices, about Karaton (Carleton [Gajdusek]), about fear of kuru and sorcery, and about relatives lost to the disease (although almost always filtered through the voices of others); and despite the photographs and descriptions by the scientists of the ravages of kuru on the motor system of individuals they have come to know as people; still there is very little sense of how the Fore or how others from Papua New Guinea think about (a) the research projects on kuru, (b) the medical research from the IMR in Goroka, or (c) their place in the larger pluralist and global world. For the past, Shirley Lindenbaum provides a vivid account of meetings called in south Fore by male orators and village headmen during the height of the kuru epidemic and their reference to similar meetings further north that seemed to them to help dampen the epidemic. But neither for the past nor especially for the present is there detailed consideration of how Fore think about the work of the IMR.

Two ethical problems complicate Warwick’s task. The anthropologist of the contemporary world would immediately be interested in the insider-outsider perspectives represented by the thirty-eight adopted sons Carleton Gajdusek brought to the United States for education, one becoming a physician, one an artist, others returning to Papua New Guinea, and so on. There is also the Iowa-educated Fore pastor mentioned only in passing (213). To protect Gajdusek’s sons, Anderson has chosen to mention by name only one of them, Mbaginta’o (Ivan Gajdusek), who was the first to come to the United States, and he only because he is now deceased. The account of Mbaginta’o is a bit garbled: he is said to have done well at first, refusing to speak Fore or pidgin, but then to have had trouble with English, speaking it less well than his age mates at home, yet he went to Swarthmore and worked in the Salem Museum before returning to be a curator in a Papua New Guinea museum. He died in 2005 after writing a letter expressing love for Gajdusek. His presence in the book thus functions as a partial defense for Gajdusek, given the public scandal of the Maryland court case. All but one of Gajdusek’s sons remained loyal to him throughout the ordeal. But we learn nothing about what he thought about his “papa’s” work or the challenges of his Fore people. One can understand Anderson’s wish to protect the privacy of these young men, but surely one could, without naming or identifying them, get a sense of their perspectives on the

encephalitis-bearing ticks. They did. Farquhar says she remembers all this because fur combing was a common lab technique at the time.

modern world and their understanding of the epidemic and research projects in which their communities of origin participated. But beyond the Fore, other people from Papua New Guinea might have perspectives equally likely to throw light on what has transpired in the last decades in terms of medical research and medical capacity building.

One name I happen to know from the world of climate change debates, who has spoken on the topic at the Massachusetts Institute of Technology, is Kevin Conrad, Papua New Guinea's ambassador of environment and climate change, sent as a child to boarding school in Los Angeles, educated as a banker, and now a prominent voice for Papua New Guinea in forums on climate warming, such as the 2008 Bali Summit, when he dramatically held the United States to account.¹² An impressive and urbane spokesman for Papua New Guinea, he seems very different from a "stone age" "primitive" who cannot be expected to understand or speak in informed ways.

Again it seems unlikely that Gajdusek and colleagues, Michael Alpers and the IMR in particular, have not helped to increase biomedical research capacity in Papua New Guinea. Are things only as decayed and grim as Robert Klitzman described in 1998, or Anderson alludes to briefly in his book (2009a: 214)? How can Goroka be producing genomic studies, or what is the role of the 250-strong Melanesian staff in those studies: are they only the data collectors, or do they contribute more creatively, and do they get any scientific credit? And what do they understand about what they are doing; how do they understand their role within their communities?

Is this, in short, a story of whiteman's colonial science, done for the benefit of the whitemen, if not only for Nobel prizes, still with no local agency to speak of? Is this the kind of STS we wish to write?

And finally, for Professor Anderson, the historian of medicine and anthropology:

4. Who speaks for anthropology and anthropological STS? Is there a postcolonial in this text (particularly because Anderson has written manifestos for something he calls postcolonial STS)?

I want to commend Anderson for his multitiered use of the Maussian (and Biagioli, Bourdieu, Strathern, Weiner) framework of the sociology of exchange (Biagioli 1993; Bourdieu 1977; Mauss 1925; Strathern 1988; Weiner 1976). He sketches the transitions from gift economies (among scientists, and between Fore and scientists) to commercial economies (the biotech revolution, the introduction of coffee and then coffee plantations from which the Fore earn some cash, and their induction into the Australian colonial labor schemes through which they gained experience beyond their localities—much to the horror initially of some of the infectious disease doctors who feared spreading kuru more widely), though Anderson tells us little about these transactions and their effect on hierarchy, authority, morality, upward mobility, urbanization, and so on, confining himself briefly to mentioning the gender shifts away from the women who were the key players in the mortuary rituals to a patriarchal pattern

¹² At the Massachusetts Institute of Technology, he jokingly confronted *New York Times* reporter Andrew Revkin, reminding him, "You called me a mouse," to which Revkin smilingly retorted, "Yes, the mouse that roared": he was referring not to Conrad but to Papua New Guinea as a tiny country confronting the superpower. See Revkin's 22 January 2008 column, as well as a video clip of Revkin interviewing Conrad titled "The Mouse That Roared in Bali" (Revkin 2008). See also the account of the Massachusetts Institute of Technology event in Callison (2010: 270–71).

imposed by whitemen. And I want to commend above all the sensitive appreciation of Gajdusek's ambivalence about personal emotional entanglements and of his desires and disappointments in creating an experimental family to partially repay the social debt to the Fore his science incurred.

That said, I would, of course, wish for a more nuanced appreciation of the commitments and research programs of the anthropologists Anderson lightly divides into autonomous social anthropology versus more practical medical anthropology. I hope that a new Asian Pacific STS would encourage anthropologists of science and technology who can be reflective about the scientific agendas and exchange systems, as well as about local institutional structures in relation both to global networks and in relation to local knowledge of all kinds. I am waiting for a really good STS account of medical practices and ecological knowledge in Papua New Guinea. That would start perhaps with a serious rereading and recontextualizing of all the New Guinea ethnographies produced in the last five decades along with their field notes, using a framework of historical epistemology and testing terrains and ethical plateaus, of the sort that Anderson has brought to Gajdusek, Burnet, Alpers, Prusiner, Glasse and Lindenbaum, Collinge, and others in the kuru story. His ability to enlist the help of Alpers, Burnet, Farquhar, Lindenbaum, Mathews, and others, and to deal with their competitive cooperations and feelings of being slighted by one another bespeaks of enormous tact, as well as the continuing commitments by the principals to the projects that began in the 1960s and continue to unfold through dramatic changes institutionally, ethically, and in our understandings of the biologies involved.

4. Genre Twists: Toward a Cosmo-political STS

A really good book elicits complementary work that extends its initiatives in other directions. I hope my rereadings suggest a few such directions. This book is great fun to read, is worth exploring for its footnotes as well, and ends with an enigmatic literary twist that is aesthetically pleasing but also worth an anthropological recontextualizing. Gajdusek finally is cast in the role of Lord Jim. This romantic ending functions also to remind us of Anderson's tact and personal relation to Gajdusek (as well as to the many others he interviewed and who contributed to the editing, helping him to avoid any inadvertent rubbing salt in old wounds of competition for credit and for standing vis-à-vis Gajdusek). Lord Jim, says Anderson (personal communication), is a character that Gajdusek identified with, as he did with Nikolai Gogol's *Dead Souls*, a novel he read in Russian in the New Guinea highlands, and the writing style of André Gide.

But for the historically minded anthropologist, Lord Jim reminds us that in South-east Asia, too, we have lots of parallel work to do with the Bugis, Malays, and others in the vast Indonesian archipelago and Malay peninsula, crisscrossed with Meccan pilgrims, Hadrami merchants, and today massive construction projects that are changing the face of that part of the world. The so-called third epidemiological transition, increasing inequalities, and interaction of multiple technological strata will lead to many new terrains and ethical plateaus for STS work on emerging and reemerging infectious diseases as habitats, ecologies, and implications of climate change affect these local habitats.

Although *The Collectors of Lost Souls* is much more than a biography of Kaoten (the Fore name for Carleton Gajdusek), it remains indebted—and no one should wish otherwise—to Anderson’s remarkable access to Gajdusek’s journals and conversations with Gajdusek. Such an opportunity comes but rarely, and Anderson has crafted a great gift for us all. Still, it is to be hoped that the future of STS will bring other equally remarkable texts that will lay out other bits of the mosaic of what I call the cosmo-politics of the scientific enterprise over the past half century (Fischer 2009), increasingly dependent upon switching points of knowledge that are globally distributed, always attentive to national (and other levels of) politics as well as to the cosmopolitanism of the scientific enterprise with its transnational mentoring relationships, travels for knowledge (Arabic, *rijal*), back-channel openness across partially closed political boundaries, care of data and intellectual resources, and friendships forged across all kinds of parochial perspectives and interests.

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