MAT *Medicine Anthropology Theory*

THINK PIECES

# Epidemiology, social history, and the beginnings of medical anthropology in the highlands of New Guinea

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## Abstract

Shirley Lindenbaum's study in the early 1960s of the origins and transmission of kuru among the Fore people of the eastern highlands of New Guinea is one of the earliest examples of an explicitly medical anthropology. Lindenbaum later described her investigations as assembling 'an epidemiology of social relations'. How might the emergence of medical anthropology, then, be related to the concurrent development of the social history of medicine and global epidemic intelligence? Are these alternative genealogies for medical anthropology?

## Keywords

medical anthropology, kuru, disease, epidemiology, social history, Lindenbaum

In a striking passage of her book *Kuru Sorcery*, published in 1979, Shirley Lindenbaum claims that in eliciting the responses of Fore people to the plague of kuru she was inevitably tracing

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'an epidemiology of social relations' (vii). What might this mean, this curious phrase, 'an epidemiology of social relations'?[1] To me, it suggests a means of disentangling the threads of history, sociality, and subjectivity entwined in the early years of medical anthropology. It allows us, I believe, to unravel metaphor and method in one of the origin stories of the field.

Lindenbaum's ethnographic engagement with Fore began in the early 1960s, coinciding with the rise of what came to be known as 'epidemiological intelligence', or surveillance, and with the emergence of the social history of disease. Charles Rosenberg's *The Cholera Years*, published in 1962, heralded the new social history of medicine. Rosenberg treated nineteenth-century cholera epidemics as social 'sampling techniques', the means of revealing the economic structures, patterns of behavior, and otherwise latent or obscure cultural assumptions and discriminations in the antebellum United States (see also Rosenberg 1966, 1967). Was this a historical epidemiology of social relations? Perhaps the qualifier 'historical' is redundant: is not epidemiology, the changing pattern of disease over time, inherently historical? Later, Rosenberg (1992) urged fellow social historians to view epidemic disease – such as kuru – as 'configuration', emphasizing the structural and relational aspects, rather than simply as 'contamination', as the technical mechanism of transmission. To what extent, then, was the epidemiology of social relations another way of talking about historical configurations of disease? What might such framing entail in representing suffering and intersubjectivity in nascent medical anthropology? In 1961, Shirley Glasse, as she then was named,[2] entered the eastern highlands of New Guinea – an Australian-mandated territory newly opened up and subject to the imperial gaze – just as epidemiology and the social survey were taking off, going global, and just as the social history of medicine was developing in the northern hemisphere. What happens when we place the medical anthropology she fashioned there into this broader intellectual context and personal emplotment?

Growing up in Melbourne in the 1940s, Shirley Inglis had initially been drawn to the study of literature. During the 1950s, the Department of English at the University of Melbourne acquired a reputation for staunch adherence to Leavisite doctrine and attendant high moral

[1] Nancy Rose Hunt (2016, 238) echoes this phrase when she refers to the 'colony-wide epidemiology of subversion' in the Belgian Congo.

[2] Born Shirley Inglis, Lindenbaum was first married to Robert Glasse, with whom she conducted research on the Fore. She has published under both 'Shirley Glasse' and 'Shirley Lindenbaum'. In view of these name changes, and the anachronism (and possible sexism) of projecting 'Lindenbaum' back in time, I have chosen to refer to the Inglises and Glasses by their first names, except where the distinction is obvious.

seriousness, but Shirley seemed more attracted to the remnant historical aspects of the curriculum. Somewhat listless and adrift, she spent the summer after graduation camping on a beach with her older brother Ken and his wife Judy Betheras Inglis. Recently returned from studying history at Oxford, Ken was preparing to write a commissioned history of the Melbourne Hospital, a book with an anthropological tinge called *Hospital and Community* (1958). Over the years, his anthropological interests would deepen, especially during his stint as professor of history at the University of Papua New Guinea (Inglis 1998; Davison 2017). Judy had completed a diploma of anthropology at Oxford, working with E. E. Evans-Pritchard and focusing on so-called cargo cults. Later, she went on the study urban Aboriginal communities in Adelaide (Kerin 2007). Previously unaware of anthropology, Shirley was inspired through her conversations with Judy and Ken, setting off the following year to study in the Department of Anthropology at the University of Sydney.

Although less prominent internationally than in its prewar apogee with A. R. Radcliffe Brown and Raymond Firth at the helm, Sydney was still the major Australian center for training in social anthropology. A. P. Elkin, the recently retired (though still guiding) professor at Sydney, had studied in London with diffusionists Grafton Elliot Smith and W. J. Perry and thus developed interests in culture contact and applied anthropology (Jones and Anderson 2015). During the 1940s and early 1950s, he sent most of his students out to document the social lives of mixed-race Aboriginal 'fringe dwellers'. Prewar figures such as W. H. R. Rivers and sociologist Robert E. Park also had shaped his commitment to studying culture contact. While deeply unfashionable in postwar social and cultural anthropology, Elkin's intellectual concerns appealed to governments seeking practical advice on how to assimilate or even to 'civilize' subject populations (Peterson 1990; Gray 1994; McGregor 1999). Yet it seems Mervyn J. Meggitt, a dashing young anthropology lecturer, exerted a stronger influence on Shirley Inglis. Meggitt had just returned from fieldwork with the Enga in the northwestern highlands of New Guinea, and his exciting lectures, according to Inglis, 'provided a fine-tuned appreciation of lineage theory and British social anthropology'.[3] From him, Inglis learned about the intricacies of highland kinship systems, as well as how to deal with conflict (Meggitt 1958, 1962). Additionally, she found another lecturer, H. Ian Hogbin, who had studied with Bronislaw Malinowski, a 'captivating presence' who also unraveled the complex entanglements of New Guinea kinship.[4] Hogbin (1963, 1996) startled the former Methodist Ladies College student with his tales of the 'islands of the menstruating men', among other dramatic set pieces. In summer, Inglis met a visitor to the department, Robert M. Glasse, an American graduate student in the competing anthropology program at the

[3] Email to author, 18 November 2017.

[4] Email to author, 18 November 2017.

Australian National University, first led by Siegfried F. Nadel, then John A. Barnes (Gray 2007). Glasse was studying the combative Huli people of the southern highlands of New Guinea (see R. Glasse 1959). Soon Inglis and Glasse were married and looking for work.

From the mid-1950s, reports filtered out of a mysterious illness among the isolated Fore people in the eastern highlands of New Guinea (Anderson 2008, 2010; see also Lindenbaum 2010, 2015). The first *kiaps*, or patrol officers, in the region – newly 'opened up' – began to see women and children who trembled and shook uncontrollably, with involuntary spasmodic jerking, and difficulty swallowing and speaking. Although Fore attributed the affliction, which they called 'kuru', to rampant sorcery, it seemed to the outsiders some sort of neurological disorder. As much as they could tell, kuru led inevitably to death. In 1957, the flamboyant, roguish American virologist D. Carleton Gajdusek ventured into the region and commenced redefining the place and the people in terms of 'kuru', making Fore visible through the lens of a fatal brain disease of unknown etiology – an affliction soon, it seemed, to eliminate them altogether. He could find no likely cause for the epidemic, no conventional infectious agent, no nutritional deficiency, no toxin (Gajdusek and Zigas 1957). He dismissed the claims of anthropologists Ronald and Catherine Berndt (1959), protégés of Elkin (Gray 2005), that it might be psychosomatic, arising from the stress of first contact with outsiders. By 1960, a distinctly unflashy rival team of medical scientists from Adelaide, led by J. Henry Bennett, had concocted an elaborate genetic explanation for the disease (Bennett, Rhodes, and Robson 1958). They needed further genealogical investigation – the collection of pedigrees, applied anthropology – of the Fore to confirm their theory.

Barnes wrote to Bennett faintly praising Robert Glasse as a 'sound and reliable' young anthropologist, fit for the job.[5] When the geneticist interviewed the couple he was more impressed with Shirley Glasse, the sister of his old friend Ken, who struck him as 'a very alert young lady, an enthusiast with much good sense'.[6] And so, in June 1961 the Glasses arrived at Okapa in the Fore region, where the Falstaffian *kiap* Mert Brightwell gruffly told them they could borrow his Land Rover to look for a place to live. As the vehicle rumbled over the mountain divide towards Wanitabe, in the south Fore, the noise reverberated, drawing a crowd. 'We saw a large group of people by the road', Lindenbaum recalled, so 'we stopped to have a conversation. . . . We said we were looking for a place to stay. And they

[5] J. A. Barnes to J. H. Bennett, 7 February 1961, John T. Gunther papers, MS 3722, National Library of Australia, Canberra, ACT.

[6] J. H. Bennett to J. Gunther, 28 February 1961, Gunther papers.

said: "Well, stop here!""[7] Local women clustered around her, rubbing her breasts and gesturing as though to take the flesh into their mouths, as a supplication for intimacy and friendship. Everyone wanted to join in the new practice of shaking hands too. The Wanitabe people promised the couple a house. 'It was an area that we knew was a hot spot for kuru', Lindenbaum later told me, 'and people were very welcoming and said: "Come in!" . . .' 'So we were theirs', she went on, 'We were *their* people'.[8]

Inquiries among the Fore suggested that kuru was a recent scourge, having come from the north probably in the 1920s. As they accumulated Fore memories, the Glasses became intrigued by the coincidence of endocannibalism. It became hard to exclude cannibalism from their calculations. Not long before kuru emerged, it seemed, Fore had taken up the ritual consumption of loved ones after natural death, a practice they ceased more or less voluntarily around 1960 (R. Glasse 1967). Women and children, those most likely to succumb to the disease, had been the principal consumers of dead bodies. 'The present evidence', Robert Glasse noted in 1963, 'is suggestive, but far from adequate' (n.p.). Implying some transmissible agent, the association of kuru and cannibalism was inconsistent with any primary genetic explanation. The Glasses discussed this with Michael Alpers, an epidemiologist and associate of Gajdusek based nearby at Waisa, a village engaged in long-standing feuding with Wanitabe. He relayed the hypothesis to Gajdusek who was skeptical, unwilling to magnify the taint of cannibalism (Anderson 2008). In 1965, that reluctance vanished. Two chimpanzees in the animal house at the US National Institutes of Health, inoculated a few years earlier with Fore brain tissue, began to manifest signs of kuru, thereby proving the disease could be transmitted (Gajdusek, Gibbs, and Alpers 1966). Gajdusek postulated a 'slow virus', a novel indolent pathogenic agent, for which he received the 1976 Nobel Prize in Physiology or Medicine.

But how had this enigmatic, invisible agent spread among the Fore? In 1965, Alpers determined that there were fewer and fewer instances of kuru among children born after 1960, suggesting that whatever the mechanism of spread might be, it had disappeared around that time. By 1968 he was arguing explicitly for cannibalism as the mode of transmission of the putative slow virus (Alpers 1968). The same year, Robert Glasse and Shirley Lindenbaum, together with John D. Mathews, an epidemiologist based at Okapa,

[7] Interview with Shirley Lindenbaum, 17 March 2005, New York City. The Glasses stayed at Wanitabe from July 1961 until March 1962, then again from July 1962 until May 1963.

[8] Interview with Shirley Lindenbaum, 17 March 2005, New York City. In The Collectors of Lost Souls (2008), I describe in detail the Glasses' stay at Wanitabe, their exchange relations with local people, and Lindenbaum's special rapport with the women, including how she became the stori missus.

published an article in the *Lancet* that consolidated all the anthropological and epidemiological evidence, demonstrating definitively that cannibalism was responsible for the spread of kuru among the Fore (Mathews, Glasse, and Lindenbaum 1968; see also R. Glasse 1963 and Mathews 1967 for earlier speculations). 'Medical anthropology' was gaining currency in the 1960s as the term for a new discipline and collaborative formation, and the Glasses had provided one of the most potent and persuasive examples of this new mode of inquiry.

From the beginning, the magical thinking of the Fore had captivated Shirley Glasse. Following Evans-Pritchard (1937), she was especially interested in the social and psychological functions of sorcery beliefs (S. Glasse 1964). As early as 1962, she observed: 'Fore gain relief from anxiety by their belief in sorcery: it gives them a course of action when faced with catastrophe. Yet their inability to avert or counteract it gives rise to further stress. They meet this stress by reaffirming their belief in sorcery' (S. Glasse 1962, 16). Her reading of Mary Douglas (1966) on the body as social metaphor and of Victor Turner (1974) on conflict, ritual, and social drama served to enrich her analysis. In a series of articles and in her 1979 book, *Kuru Sorcery*, Lindenbaum examined Fore attribution of disease to malign human agents and disturbed social relations. Nothing shook the conviction of her friends and neighbors at Wanitabe that men made kuru; if it had been a sickness, they reasoned, it would have been gone long ago. In practice, the obsession with sorcery, Lindenbaum wrote in 1979, 'simultaneously registered and aggravated the social inequalities and demographic imbalances' of Fore communities (146). The study of kuru sorcery thus permitted the anthropologist to assemble an 'epidemiology of social relations' (Lindenbaum 1979, vii).

Close encounters with epidemiologists in the field, such as Mathews and Alpers, might have influenced Lindenbaum's thinking. Interactions with Gajdusek, too, must have attuned her to epidemiological reasoning: he always was boasting of what he called 'medical snooping' in exotic places. Indeed, his mentor Joseph Smadel, at the National Institutes of Health, was an associate of Alexander Langmuir, the founder of the Centers for Disease Control, and a resolute advocate of disease surveillance and epidemic intelligence. In the 1960s, epidemiology, like medical anthropology, was starting to emerge as a global enterprise (Langmuir 1965; Susser 1985; Declich and Carter 1994; Fearnley 2010). But what did 'epidemiology' really signify in Lindenbaum's anthropological work? She seems to be referring to how patterns change over time, to be using the term as a proxy for 'history'. Thus, it is hard to discern where, in her work, medical anthropology ends, and the social history of disease takes over. Like social historian Charles Rosenberg, her contemporary, she was always more concerned with making configurations or ecological patterns visible, rather than simply tracking contamination. While Lindenbaum would attribute her interest in the historical framing of disease mostly to later engagement with the work of her friend and

colleague Eric R. Wolf (see Wolf 1983), it seems to me that this commitment was there from the beginning. Just as her brother drew on anthropological concepts to shape his historical narratives, Lindenbaum was always a social historian, mining deep historical seams in her anthropological research. In an out-of-the-way place conventionally deemed not to have history, Lindenbaum respected her friends and neighbors as historical agents – not simply ethnographic subjects – as she went about creating what became known as medical anthropology.

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