Personal reflections on the neuropathology of kuru

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My involvement with kuru began towards the end of 1963 while I was a Fellow at the Massachusetts General Hospital (MGH). During the autumn of that year I was contacted by Dr Michael Alpers, who was on his way to Boston from the National Institutes of Health (NIH) in Bethesda, Maryland with a carload of New Guinean artefacts to be delivered to the Peabody Museum in Salem, Massachusetts on behalf of Dr Carleton Gajdusek. We had been students together at Adelaide University and St Mark’s College in the 1950s and our friendship was thus rekindled.

Sometime after this I was approached by representatives of NIH to investigate the Parkinsonism–dementia (PD) and amyotrophic lateral sclerosis (ALS) neurological complex prevalent on Guam, where it was the main cause of death among the Chamorro people. At that time, the cause of this devastating neurological complex was subject to a great deal of conjecture. These discussions led to visits to Bethesda, where I met Carleton and Joe Gibbs. This was at the exciting time when the first chimpanzees developed symptoms of kuru. Lifetime friendships were thus established with Carleton and Joe and with Jake Brody, as well as with Dr Asao Hirano in New York, who was the first to describe the neuropathology of the Guam disorders.

As I was committed to the University of Western Australia, being on leave from an academic post in the medical school and feeling that I had a mission to set up neuropathology in my home country, I was not able to pursue the very attractive Guam opportunity and returned to Perth in 1965.

However, earlier in that year, while deeply involved in a very busy schedule at Harvard Medical School working with my mentor in muscle diseases Dr Raymond D. Adams, Chief of Neurology at MGH, I was approached by an enthusiastic French-Canadian neurologist André-Roche Lecours. He told me about two kuru brains that had been cut in serial section at the Warren Museum of Harvard Medical School under the watchful eye of the distinguished developmental neuroscientist Dr Paul Yakovlev. Roche had concluded that I had the expertise to study and developase these brains to work up neuropathologically. The purpose of these studies was to establish incubation periods and species susceptibility for both kuru and Creutzfeldt–Jakob disease (CJD). By this time, Colin Masters had returned to Perth as a National Health and Medical Research Council (NH&MRC) Fellow and he joined the studies. These investigations resulted in the unexpected finding that typical kuru and CJD changes were well established in these brains before symptoms appeared (Masters et al. 1976) thus demonstrating a high degree of ‘cerebral reserve’, which neuropathologists had theoretically suggested existed.

As a matter of interest I was also asked to report on a large number of Guamanian ALS–PD cases by the NIH authorities. I was joined by Prof. Henry Urich in these studies and later by Dr Dan Perl. Not only did these investigations establish a large overlap in the distribution of lesions between the three entities but also consolidated the toxic cycad hypothesis as being the cause of the neurological degeneration underlying the Guam conditions. Although the affected Chamorro patients clinically manifested mainly either Parkinsonism–dementia on the one hand or ALS on the other, the lesions were found to be much more widespread in most patients and involved the spinal cord, midbrain and cerebral cortex in each, but to a varying degree, thus demonstrating again a preclinical ‘incubation’ period and a threshold effect prior to symptoms and signs occurring.

Incidentally, I should mention that all this work took place in parallel with my main interest in muscle disease and spinal injuries. My gratitude is expressed to the organizers for the opportunity to participate in the End of Kuru meeting.

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REFERENCES


‘Today I am so happy to see friends I once worked with many years ago’

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I was approximately 16 years old when I first met Dr Carleton Gajdusek in Wanitabi village, situated south of Okapa in the Eastern Highlands Province. I worked with Shirley Lindenbaum when she came to live in our village, and I helped Michael Alpers with his research. I was asked to work as a translator, also to assist with the fieldwork and carry personal things like camera, books and film. The older men carried the heavy boxes from one village to another on kuru surveillance patrols.

There were other medical scientific officers who came later with whom I worked as well, such as Dr Hornabrook and John Mathews. I was trained by them to perform autopsies on kuru dead bodies. Though my position with the project was as a translator, sometimes it was my duty to take human samples collected from the field to Goroka by plane from Tarabo airstrip and return back to the field by the same route.

One of the colleagues who helped me was Tosetnam from Miarasa village; we both shared the workload and helped in the fieldwork. Some of our comrades are not here owing to medical reasons and some, like Tosetnam, have already died. Today I am so happy to see friends I once worked with many years ago, in the 1960s and 1970s.

Kuru fieldwork in 1981 … and beyond

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In 1981, I was fortunate to be able to conduct epidemiological fieldwork on kuru and the experience forever changed me. At the time, the prevalence and incidence had both declined markedly. Yet, clusters of cases still occurred in various villages and questions arose of whether these were the results of the last feast held in each of these areas. I trekked throughout the kuru region, examining current cases and collecting genealogies on 65 recent patients. As described more fully in a paper in Neuroepidemiology (Klitzman et al. 1984) and in a book about my fieldwork in Papua New Guinea, The trembling mountain: a personal account of kuru, cannibals, and mad cow disease (Klitzman 1998), I identified and described three clusters of patients, with patients in each developing kuru virtually simultaneously after having been infected at the same one or two feasts that occurred close together in time. The three pairs had incubation periods of 21, 24 and 28 years, and members of each pair did not vary by more than a year. This research suggested that the disease could therefore follow a uniform course of incubation in two or more people, even when the incubation period is over two decades. It was thus possible to determine when exposure occurred, and hence calculate precisely natural incubation periods for prions in humans—which had not been done before.

Yet I found, too, that some participants at each of these feasts had much shorter incubation periods. Hence, age and viral strains did not determine incubation period. Perhaps the initial dose of the agent or the genetics of the infected individual did.

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