2. Tasimauri sojourns and journeys
interview with Murray Chapman

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On Saturday night 11 January 2003, as is often the case in Hawai`i, a particular confluence of tides brought David Gegeo, Salome Samou, John Moffat Fugui, Transform Aqorau and Tarcisius Tara Kabutaulaka to a meal at the home of Linley and Murray Chapman. One of the guests was in Honolulu for an international conference on education. David Gegeo, then lecturing at California State University at Monterey Bay had presented a paper with a colleague, Judy Parker. Salome Samou, who was from the island of Santa Cruz but living at Lami, Fiji, had been visiting Washington DC after completing a post-graduate Diploma in Development Studies at the University of the South Pacific, Suva. John Moffat Fugui, from the island of Malaita and a former graduate student at the University of Hawai`i, seems to have been passing through on some political business. Transform Aqorau from the Western Solomons, a lawyer with the Forum Fisheries Agency, was in Hawai`i on fisheries matters; while Tara, then lecturing in politics at the University of the South Pacific, was visiting the East West Centre.

All the guests were from Solomon Islands where Murray Chapman began field work in the 1960s. Much of the talk at dinner was of the Solomons, still to emerge from the civil disorder of 1998–2003. A terrible time for all Solomon Islanders, this conflict only ceased with the advent in July 2003 of the Regional Assistance Mission to Solomon Islands (RAMSI), consisting of armed and police forces from Australia, Fiji, New Zealand, Papua New Guinea, Samoa and Tonga. It was welcomed by the impotent Solomons government and the militants quickly surrendered. The works of war gave way to the works of peace. The Solomons began the slow and painful journey towards recouping the country's losses, with many young tertiary graduates assisting the process. One way or another, all the guests were to assist nation building at home and abroad where all were involved in aspects of administration, politics, higher education, or museum conservation. One way or another too, they were all connected to Murray. We have included this interview largely as recorded in 2003 since it details much of Murray's own intellectual journey.

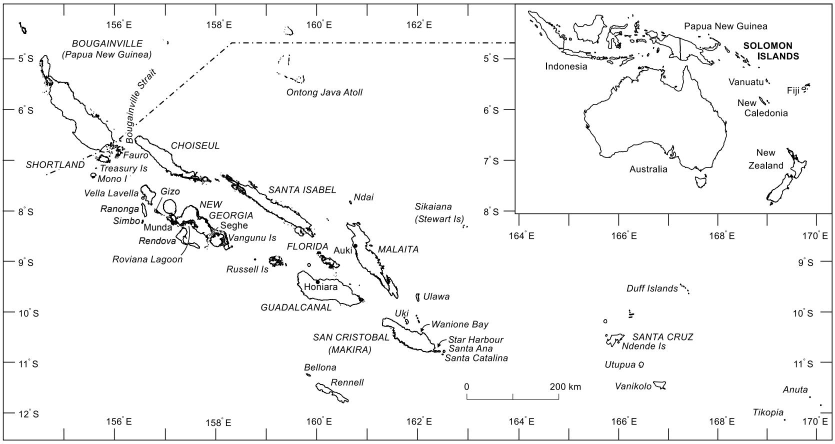
David Gegeo (DG): Today is Sunday 12th of January 2003 and I am here in Honolulu in Lin and Murray's house. I am going to *tok stori* (chat, discuss, or talk about matters) with Murray and I was thinking that we would do the interviews on two topics: your work and research in Tasimauri, the south or Weather Coast of Guadalcanal; then, depending on how much time we have, indigenous epistemology.

I guess the first thing to do is to ask you to give the chronology of your research and work in the Solomon Islands – in Tasimauri.

Murray Chapman (MC): My focus on the Solomons was one of those happy accidents of life. And it began without any choice of mine in an MA Honours seminar at the University of Auckland in 1958. In those days, in many of the social sciences, the Pacific was a great concern to New Zealanders. The geographer Kenneth B. Cumberland wrote a paper called something like 'New Zealand's "Pacific Islands' Neighbourhood"' in a 1949 issue of the *New Zealand Geographer*. That was the Kenneth B. Cumberland, well known as a deeply intelligent and highly opinionated professor, whom we endured at the University of Auckland for about 30 years. So he taught me and he taught many others.

In those days – remember it was 1958 – graduate students walked in, mid-February, to the first meeting of the MA seminar on the Pacific and the instructor said, 'Each of you is going to do four research papers.' I remember one was on population and settlement, another was on agricultural systems. Then he said, 'If you finish those …' and he handed each of us a sheet and explained, 'That's the place you're going to focus on.' Against the name Chapman – because first names were never used in that day and age – against Chapman it was 'Solomon Islands'. Graduate work in New Zealand in the 1950s didn't involve a great deal of choice!

If you think back to the literature on Solomon Islands, it was all written by outsiders. At that time, there was no Solomon Islander with an undergraduate degree, though a few had tertiary qualifications from the Central Medical School in Fiji. So the literature was all written by outsiders and there was very little of it. I spent an enormous amount of time going to museums, archives, anywhere I could put my hands on something about the Solomons. My salvation, I remember, was Colin Allan's opus for the Western Pacific High Commission which had just come out, called 'Customary land tenure in the British Solomon Islands Protectorate'. Without that comprehensive study, my four research papers would have



Map 2.1. Solomon Islands.

been very, very thin. And that's how I first started with the Solomons [see Map 2.1]. As I said, it was one of those happy accidents of life.

If we shift to the University of Washington in 1962, where I went to do a doctorate, everybody wanted to know what this New Zealander, Murray Chapman, was going to do and I talked a little bit about population and resources. Later, they asked me where would I do my field work? Privately, I thought to myself, 'Well, it's obvious I should do it in the Pacific.' From that honours seminar in New Zealand I had become fascinated with the Pacific and particularly Melanesia. I looked at Papua New Guinea but, if you recall the history of islands' work by outsiders in the 1950s: Papua New Guinea was over-run by anthropologists and by archeologists. This was the beginning of the cultural ecology movement and people like Roy Rappaport and Andrew Vayda literally had armies of doctoral students marching around the New Guinea highlands. I also looked at Vanuatu, which was then the Condominium of the New Hebrides, though people called it the Pandemonium.

DG: That's right. The late Walter Lini called it that!

MC: I thought well, if I am very practical and think about intellectual competition, the Solomons should be a good place because it does not seem like anybody is doing much work there. So those were the two happy accidents, if you like, of being told by a lecturer in geography that I had to do four research papers on the Solomons, then of looking, in a practical sense as a doctoral student, where I might be able to go and to make some kind of contribution. To me, that was obviously the Solomons at the time.

DG: So how did you choose Tasimauri as opposed to …

MC: … anywhere else in the Solomons?

DG: Yes.

MC: That again was very interesting. The department of geography at the University of Washington in Seattle was ranked either second or third in the United States. The reason we – Lin and I – went there was because it was one of the departments I applied to on the recommendation of Oskar Spate. He was a remarkable scholar, the foundation professor of geography at the Australian National University, who had done a major study of Fiji as well as of New Guinea. Washington was the only university that had money for my first year. In those days you got a teaching assistantship but nothing else – certainly no support for travel or for family members. So all the Chapmans' savings went into a couple of one-way air tickets. We arrived in August 1962 in Seattle with a total wealth of 13 American dollars in our pockets before we got my first pay cheque the week after.

So why Tasimauri? Why not San Cristobal [Makira]; why not Mono in the Shortlands? Somehow I learned, and I don't remember any longer how, [that] a Dutch ethnographer Rita [Margherita] DeKoster, was working on one of the islands of Gela [Florida Islands], in the central Solomons. This was in the early 1960s, near the pre-war capital of Tulagi. I managed to get in contact with her through Honiara, remembering these were the days when this was not simple. She in turn replied and said she would be passing through Seattle for two or three days on her way back to Europe. She gave me names of people in the Solomons who might be helpful and the most important was of a man called James L.O. Tedder.

Jim Tedder then, I think, might have been district commissioner of Guadalcanal – a man who was very well known by young outside scholars for his ability to nurture research that had to do with Solomon Islanders, rather than of the British colonial system. I wrote to Jim and explained that I would like to study the movement of people in very broad terms. My thinking was to look at three different kinds of communities and so be comparative.

The frame of reference I had was that in one community movement would be largely for customary reasons, like to go to a feast; in another, movement [would be] primarily for wages, like on a coconut plantation. The third would be moving primarily to the main town of Honiara, for a range of largely introduced reasons. So I saw, if you like, three profiles of movement fitted to three unknown or theoretical communities. The idea was to live and work in those communities, to get a comparative and a deeper understanding of the movement of people in the Solomons.

So I wrote all this off to Jim Tedder in a long letter. Basically I said: 'Mr Tedder, I have no idea where the villages are or what their names are. I have been looking at maps, but they don't tell me very much. Could you help me?' After a time, back came a type-written letter, with two or three pages of long paper as an attachment. First of all, he said, 'Your question is very difficult and assumes we know more about Solomon Island villages than in fact we do. Secondly, you have to think about where you are going to live and all sorts of practicalities, because you will not be in the main town. And thirdly, on a page or so, I have listed islands and against the islands the names of villages. I have tried to take your system of movement for customary reasons, for plantation wage labour, and for a range of introduced reasons, then annotated them. See what you think.'

For several days, I would have these long pieces of paper spread out on a map of Solomon Islands, trying to link islands to villages. Of course, most villages were not on any map I could find in Seattle and I had no idea what I was dealing with. But I kept looking and looking, with the idea that each kind of community would be on a different island. Slowly the practicalities dawned: one kind of community on Mono, one kind on Santa Isabel, and a third on San Cristobal [Makira]. That really was not very practical.

Then I started looking more closely at the names and all of a sudden it hit me. There were more names for Guadalcanal. Which, much later, I realised could have reflected the fact that, in the early 1960s, Jim Tedder was working more on Guadalcanal and the surrounding central Solomon Islands than anywhere else. After a while, I began to see a pattern. It seemed there were more village names for the Weather Coast of Guadalcanal, Tasimauri. I wrote Jim Tedder another letter, probably several months later, and said, 'Well, thank you very much. I have done a lot of work on this and I have come down to this place in Guadalcanal which seems to be the south coast. The village names I found were Duidui and several in central Talise, all on the coast. And Pichahila in the Alualu river valley of Birao.

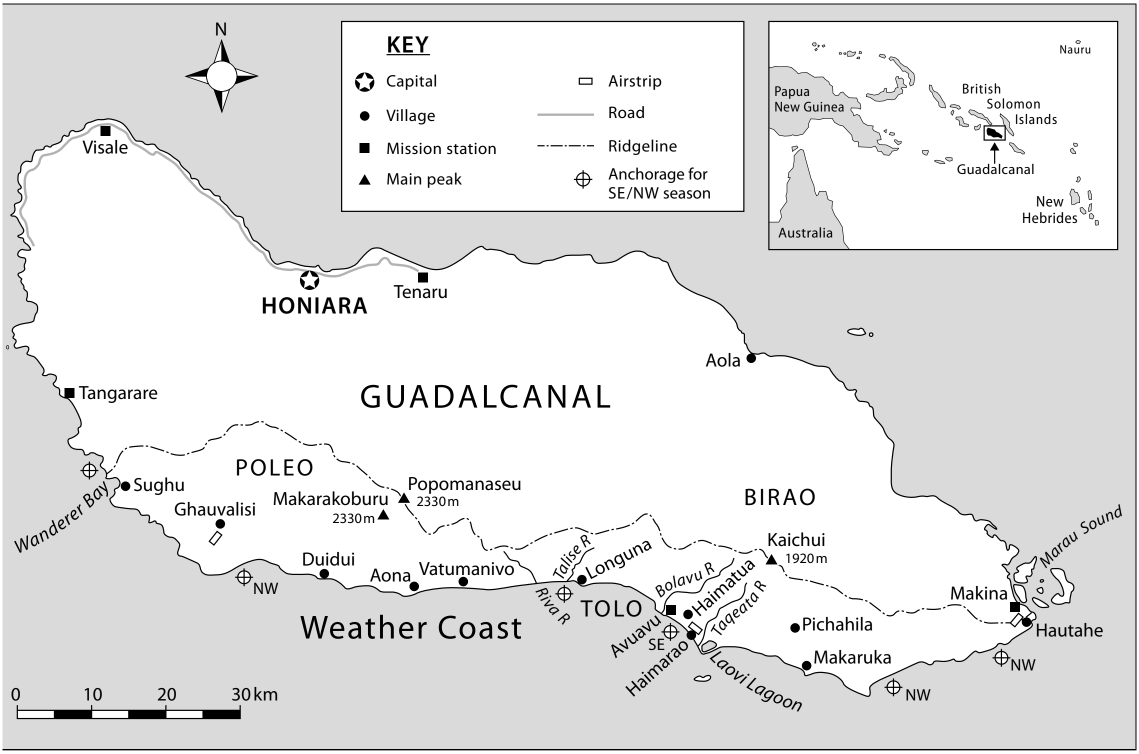
They looked like they would fit what I was trying to do, with Duidui people mainly going to Honiara, men from central Talise communities for wage work, and Pichahila people usually for custom.' I still remember the letter I got back. The first sentence ran something like, and this was probably late 1964 by now, 'Prepare for isolated and simple living.'

Again, this choice of Tasimauri and initially, of those communities, was based on a tremendous amount of luck, because I didn't know what I was doing. And as many young doctoral students, lawyers in training and senior research scholars found over the years with Jim Tedder, I encountered a tremendous amount of understanding, on his part, of what existed on the ground in rural villages. Because most British colonial administrators in those days knew very little about which Solomon Island places a young doctoral student might go. But Jim Tedder did.

DG: Yes, Mr Tedder was the director of the Solomon Islands Broadcasting Services in the early 1970s when I was working there. In fact he and the late Bill Bennett, our Broadcasting Officer, and Welchman Teilo had recommended me for an East-West Centre scholarship to study ethnomusicology at the University of Hawai`i in 1974.

Before we get to the specifics of the research itself, I'd like you to talk a little bit about the rigours or the problems of doing research, especially then in Solomon Islands. I think it is a lot easier today than then. During that time there were hardly any ships going back and forth – and no small airline, no Solair, of course – between the rural areas and Honiara. I think, in your particular case, we're talking about the period between October 1965 and February 1967. Doing field research during that time period, how was it both easier and harder than today?

MC: I think it was easier, ironically because the outside scholar was being parachuted into a colonial administrative system. Whatever the costs and benefits of that system, they at least knew how to administer and cope with strangers, because they too were strangers. For instance, any permit that had to be got or any regulations that had to be met were spelled out very clearly in advance; the documents came, you sent them back, and if money for fees was involved, you did that too. Beyond the main town the district commissioner, which for Guadalcanal was Jim Tedder, did everything else. I didn't understand, as the people commented among themselves, but I only found out years later, 'Oh, they put Murray in two government villages.'



Map 2.2. *Tasimauri*, the Weather Coast of Guadalcanal, c. 1972. Birao, Tolo, and Poleo are some of the language regions on the island (Based on map by Richard T Naito, modified in Chapman 1987, 348, with kind permission).

I didn't know that the people saw Duidui and Pichahila as government villages [see Map 2.2].

Why a government village? Because the district headman for the western half of the Weather Coast was Marcus Pipisi, the big man of Duidui; and the assistant district headman for Birao was Petero Cheni, the big man of Pichahila. It made perfect sense to Jim Tedder. If strangers who were 30 and 28 years old, particularly one a wife, were going to sit in an isolated place for more than a year, where few *ara'ikwao* (outsiders) had ever gone, at least have them in villages where they could be assured of a certain amount of security and safety. Now I didn't know any of that. All I did was follow directions about documents and fees. In that sense, because research was set within a British colonial administrative system, it was much easier than after independence.

By way of contrast, around 1990 when my research in Tasimauri was synchronised with two years at the Solomon Islands College of Higher Education [SICHE], paper work was required by both the national granting authority for a research permit, and by SICHE for a work and residence permit. And the Guadalcanal Province could refuse permission for that research, in the way the regulations were set up. That, in turn, led the director of the Guadalcanal Cultural Centre, Victor Totu, to walk the whole of Tasimauri. Again, I didn't know he was doing that, to make sure this administrative requirement was fulfilled. That means I was in a privileged position in 1990. But if I had been a young doctoral student at the time and I had to deal with that system from Seattle, as I did in 1964, it would have been extremely difficult for me to get a research permit.

I don't think it was any harder in the 1960s, in terms of actually doing the research. In fact, I was struck when I went the third time to Duidui and the second time to Pichahila, from 1991 to 1993, there was very little I had to do differently. Beforehand, a leaf house had to be organised and built, or some living arrangements made. Once the work started, materials not in the local bush had to be got ahead of time. Sawn timber for doors and shelving had to be shipped around to the Weather Coast. Door hinges were requested and had to be found in Honiara. Basic carpentry equipment, like a file, a saw, a plane, had to be bought before the project started and sent to village builders, who kept them once the house was finished.

That was done in 1966, it was done again in 1991. Nothing really had changed. It was the same with stores. Of course you buy as much fresh fruit, leafy vegetables, yam and taro and sweet potato as possible in season. But *ara'ikwao* don't have the ability, perhaps even the patience, to turn baked taro or breadfruit into flour for making bread, which means buckets of processed flour are on the stores list. Drums of kerosene are necessary for the lamps, unless you want to go back to how the people got light in the worst years of civil unrest in the late 1990s – that is to say, stand a wick in a half shell of coconut oil and light it. If you decide to shift from the wood stove of the 1960s to the bottled gas of the 1990s, then a regular system of rotation of small gas cylinders has to be arranged between the Weather Cost and Honiara. For village helpers, this is not easy and drives them nuts!

So a mountain of cargo accompanies your arrival. Pieces of plywood have been cut to shape for a table, nailed onto one of the wooden boxes when emptied, which again was chosen in Honiara to be at table height. All these supplies had to be worked out before leaving for Tasimauri. And

Weather Coast living for an outsider is very unpredictable. What Lin and I decided in 1965 was probably little different from what I did in 1991, except that in 1965 we had never done it before.

Probably one difference in the nineties was better communication. I would have had to walk five or six hours to get to a radio telephone in 1966, because it was a long, long way from Duidui and Pichahila. In 1992 I only needed to walk, say, an hour to get something quickly. I remember shouting into the radio transmitter one day, saying, 'Get some food here. I'm down to a bottle of peanut butter and I've got nothing else. And I don't want to eat peanut butter for the next month!' It's an interesting question, David, because I think the assumption would be so much easier than 25 years before. Perhaps it's a function of the isolation of Tasimauri.

DG: At this point, I'd like to ask you to talk more specifically about your research. You developed a terminology that's very distinct and widely used by researchers in circulation or population movement. Earlier in our conversation you mentioned something about the three types of communities in population movement that you had identified in your research on the Weather Coast of Guadalcanal. Could you talk about each of the three types of communities?

MC: My thinking on population movement within the doctoral programme at the University of Washington reflected those earlier seminars on the Pacific Islands done in Auckland in 1958. By which I mean they gave me a broad but probably not very deep understanding of Oceania. There was something like six other MA students, each like me given a country and topics as the frame of four research papers. Listening to each others' seminars for a whole year gave a limited comparative sense of, say, issues of population distribution, composition and change in about six countries, including the Solomons.

That was the context I arrived at Seattle with. When it came to the study of what was in the literature about population movement, in those days the focus was on internal migration within North America. In an intellectual sense of that broad introduction to Oceania, as well as being socialised within New Zealand as a social democracy, what fascinated me within those personal and intellectual contexts was [that] all the literature was economically driven. Internal migration, the rather settled movements of Americans and Canadians, always occurred for economic reasons: to get a better job, to move to a city of better amenities, to gain more economic status by promotion through a company environment. What struck me about this literature, whether written by economists or sociologists as the dominant figures in the early 1960s, was there was nothing else to look at. There was nothing more to understand. People moved rather permanently for economic reasons. Period.

I think it probably was more my being brought up in a social democracy than my beginning awareness of Oceania that led me to be rather sceptical of such a perspective. And, in private, hostile. I couldn't believe there weren't other reasons for people moving around. That means two things. First, I was not just interested in economic movement. The internal migration literature of that time, so much driven by economic explanations and contexts, was talking about North America, a capitalist economy, and how people moved around within it. Looked at from the outside, from the point of view of a New Zealander, I did not accept those constraints. I wanted to look at any kind of movement and not just internal migration.

Secondly, I was therefore interested in any range of reasons, not just economic ones, and more importantly, a series of reasons. The first chapter of my dissertation is incredibly simple in its guiding questions. It says something like 'In third world or nonliterate societies, people move around for social as well as economic reasons.' That would not be enough for a dissertation in geography in 1999 or 2003, but it was good enough for 1965. I was not only looking at a range of reasons for people's movement but also deliberately locating it outside of North America. Underlying your question, David, is something highly important. My very strong instinct was that, if in fact it was true that people in North America did move internally for economic reasons, that 'truth' was not necessarily transportable to other societies.

That's one dimension of my thinking; what's the other? By looking at a range of reasons for people moving around in many ways in their own societies, I have never used the term 'migration'. Always, deliberately, it is 'population movement'. Of course, that means I have had trouble in publication with, say, the editors of demographic journals who scratch out 'population movement' in my original and insert the word 'migration'. In page-proofs, I always changed 'migration' back to 'population movement' because it was not a minor point, but philosophical. Writing about many kinds of people's mobility is different from discussing internal migration, or focussing on those who move for permanent or semi-permanent reasons and, in doing so, relocate their place of residence. To put it in the colloquial, my interest was on far more than pulling up sticks! My first understanding of Oceanic peoples, from early reading, was that the idea of pulling up sticks was alien.

Much later, I learned why: because ancestral land is everything. Land is being, land is blood, land is genealogy, land is history, land is identity. That was powerful reinforcement to keep shifting beyond the notion of not merely 'migration', but to understand 'population movement'. Not pulling up sticks, but retaining ties to the ancestral or contemporary hearth. Not 'migration', but 'circulation'. So 'population movement' was the broad term and 'circulation' became adopted as the best summary for the constant coming and going I was observing. By the time we left Tasimauri in February 1967, I could see circulation as the theoretical orientation for discussing the mobility behaviors of Melanesian communities I knew best.

As the years went by and I supervised Asian doctoral students from Bangladesh, India, Indonesia, Nepal and Thailand, I learned my understanding of population movement was no different from the experience and knowledge of their own rural communities and interior settlements. Gradually I realised that what I was saying was not specific or limited to the Solomons at all. By the 1980s, in my publications, sometimes I would focus on the Solomons; at other times [I would] be more synthetic and transfer the argument to what seemed as important: the third world in general.

DG: Going back to an earlier point you made about population mobility being based on economic reasons: where do people move seeking wage labour? Could you say something about that?

MC: The way I looked and still look at population movement was contextually, with the village community the frame of reference; that's how initially the villages I hoped to study were chosen. My intellectual orientation was summarised in three different kinds of communities: one to show a range of customary movement, one for a range of wage-labour movement, one for urban-like movement – in those days, largely to the town of Honiara.

Lin and I arrived in Duidui, the first community of study, in November 1965. One of the first things I did, which has been reported in the literature several times, was to record the act of movement as it happened. I had no idea what this would mean, but believed it was the only way to find out about its complexity. In the population sciences, if demographers wish to look at movement in detail over time, they reconstruct it by retrieving details about past actions. This reconstruction is what technically is called a retrospective mobility register. If I did this, I might be able to reconstruct the number of moves in and out of Duidui, their directions, perhaps even the kinds of movement. But those details would tell me nothing about the whole process. Nothing, intriguingly, about the underlying dynamic of those actions. So basically I decided to record everything about mobility when it occurred, or soon after. I sat in Duidui and over the days and months a register of the whole process unfolded in front of my eyes.

For this to be possible, I asked village elders to suggest an appropriate person to assist me. In Duidui I trained Sandy, and in Pichahila, Gabriel Saisudana, because the vernacular of each community was different. Poleo is spoken in Duidui, a coastal settlement, and Birao in Pichahila, a bush or inland place in a valley. I simply asked Sandy and Gabriel, as persons of those communities, to keep their eyes and ears open. If anyone crosses the village boundaries and stays away overnight, make sure you know. Who were they? Where did they go? Why? Any detail, no matter how small or obvious. In Pichahila, Gabriel even put up a notice! Then, as I once said in a paper, at the terribly British time of Monday forenoon, Sandy or Gabriel would sit down with me for at least the morning, if not the whole day, to go through the weekly record. Needless to say, anecdotally I had been doing the same thing, watching for movers, but not as systematically. Usually questions would arise and each went away to amplify the record. With a prospective mobility record, intricate details are being captured as they occur as movement flows. That also means its context, its dynamic, ultimately its synthetic nature too. All that information melds together as it happens before the eyes; it is not at all logical, and full of surprises. But of course, the longer you live in the village community, the better you get at it.

It was about February or March the next year, 1966, and it was time to make a field report to the East-West Centre in Honolulu for what was then called a Predoctoral Dissertation Fellowship in International Development. I was required to have a field supervisor and mine was William [Bill] Davenport, who at his death a few years ago was an Emeritus Curator of the University of Pennsylvania Museum. From the 1950s Bill was well known as an anthropologist in the Solomons. Most of his field work had been done in what was known as the Eastern District, in that part [that is] now Makira-Ulawa Province. At the time of this field report, Lin and I were in Duidui and Bill was in Santa Ana and Santa Catalina, two small islands off the eastern tip of San Cristobal [Makira]. I prepared my report and sent it off from Tasimauri to Santa Catalina which, in terms of the Solomons in the 1960s, was a great act of faith! I noted, 'I had this design, of three communities that are supposed to have a dominant kind or genre of movement within them. But here I am in Duidui and all these three things of mobility, and more, are happening under my feet.' So the first thing I learned was that my thinking about a research framework was terribly simplistic, reflecting the fact that I did not know what I was doing. Which is true of most doctoral students before they start dissertation fieldwork.

There was everything in Duidui movement that I was interested in intellectually, but I also said, 'You know, Bill, I would be uncomfortable to stay with this salt-water community, since one of the communities we chose was inland, in the bush. Why don't I redefine my study as a holistic view of movement in a coastal community and an inland valley?' As a result, the village which was to show going to wage work, in central Talise, was never identified. My published work, as you know David, often reduces to coast or bush people, Duidui as different from Pichahila, Marcus Pipisi the big man of Poleo country and Petro Cheni the big man of the Birao bush.

Marcus Pipisi and Petero Cheni were two of the last three grand *taovia* (big men) of Tasimauri, that is, leaders of great stature gained over years of accomplishment among their people. Always obvious, because they had more of anything and followers who knew and helped with that: food gardens, pigs, planted coconuts or cocoa, money and material goods, local businesses and, before Europeans crossed their shores, more wives than anyone else. The third great *taovia*, Dominiko Alebua of Haimatua, lived in different coastal communities quite close by Avuavu mission station. I often saw him near the end of that long walk from Pichahila, on my way to spend the night at Father William van Duin's place before catching the little plane to Honiara the next morning. Petero Cheni and Dominiko Alebua were the first two *man blong bus* [bush or inland men] to convert to Christianity, and both trained as Catholic catechists in the 1920s and 1930s. Dominiko's grandson, Tarcisius Tara Kabutaulaka, wrote a biography about him.

DG: What was the unspoken dimension of population movement?

MC: From living among Tasimauri people, I learned it was culture. At first, it was more of a vague sense, instinctive. Perhaps that's why it took a long time for aspects of culture to become more visible in my written work. I think that hesitancy was a product of being brought up in a social democracy. I never heard this term 'culture' used in New Zealand in the 1950s, never heard the expression 'a New Zealand culture', although of course there was one!

The whole notion of the culture of a people is critically important. Although anthropologists act as though they discovered or invented this idea, as the years went by I would listen to Pacific Islanders talking in seminars. Their societies had culture, they said. They knew this long before they saw any Europeans, and there were words for it in their own languages. Like *falafala* for the Kawaraʻae of Malaita, as you pointed out in 1992 at a meeting in Honiara on cultural policies in Melanesia. Ironically, my ignorance in the 1950s of this idea perhaps reflected the fact I had never done a course on anthropology, although at the time a few were offered at Auckland [University].

Thinking about a doctorate in the United States was attractive, because I had done all my undergraduate and graduate work in what I call the British derivative system of knowledge. That is, to focus on one discipline and work it to death intellectually. I must have had something like twelve years of geography and little else by the time I finished a Master's degree, so I knew absolutely nothing about any other social science. This also says something about my geography. When applying to doctoral programmes in geography, I noted wanting all the coursework to be in anthropology, sociology and economics. Needless to say, not too many departments answered my letter! In the end, I applied to Berkeley, Chicago and Washington – among the best geography departments in the country, although I didn't know it. Perhaps one's more likely to accept that kind of weird statement from an outsider. Without a fellowship, Berkeley and Chicago could not offer support for a first-year, so I ended up in Washington.

DG: So, in a sense then … you really debunked the long-standing theoretical approach to population mobility that was, as you said, concerned exclusively with economic reasons only. That was ground-breaking, wasn't it?

MC: Perhaps it was and when I went to conferences my approach surprised people. In 1970 I gave a paper at meetings of the Population Association of America, which later became what I think was my best piece of conceptual work: 'Tribal mobility as circulation'. All I saw at the end was a lot of open mouths, with no idea of what I was talking about. I was both amused and amazed. That was one reaction I would get. The other was, if I went to an international conference, either silence – or the demographers, economists, sociologists, even geographers would take the microphone to express their doubts and scepticism.

My first doctoral student was Shekhar Mukherji from India, a hugely sensitive human being and also an incredibly radical thinker. In August 1972 we were at the University of Alberta at a meeting of the International Geographical Union, Commission on Population Geography, where the focus was on internal migration. The session finished. I was sitting near the back; Shekhar came running up the stairs. He said, 'Sir,' – a typically South Asian greeting – 'they didn't answer any of your questions.' And I replied 'No, Shekhar, and they won't until they listen to what I'm saying.' So for the first 10 or 15 years, whenever I emerged from the department of geography at Mānoa in Hawai`i, among colleagues somewhat sensitised to and intellectually comfortable with such issues, professional life could be rough.

What those reactions did was enhance the instinctive bloody-mindedness that every New Zealander is brought up with. Basically, I thought, 'I will show those doubting Thomases that I am right.' And kept plugging away to demonstrate that conventional thinking was so ingrained, there was no willingness to listen. At the time, for some geographers, it was also hard to accept the fact my dissertation was based on two communities. 'You mean you didn't do a pattern analysis of 90,000 Solomon Islanders? … You didn't do a factor analysis of the reasons why people moved?' Hell no!

DG: That is very interesting!

MC: Of course, this was the research style of much of the best-known geography in the 1960s and 1970s. I have some quite good friends who probably have forgotten what they said to me then: 'Well, I suppose you could get away once or twice with that mode of enquiry, but we don't think geographers should do community stuff.'

DG: So, what is the position now in terms of this dichotomy? Is it more acceptable now than then, because of your work?

MC: Well, it's much more acceptable intellectually, but I wouldn't say because of my work.

DG: I would say very strongly it is because of your work! The reason I say this is because many of the students from Indonesia and other Southeast Asian countries you have trained happen to be good friends of mine.

We are coming now to the two volumes that you co-edited with Mansell Prothero: *Circulation in Third World Countries* and *Circulation in Population Movement*. There is a strong current of thought here in looking at population mobility not only from an economic perspective, but also from a sociocultural and political point of view. What do you think?

MC: There are two angles to this. More accurately, two dimensions of time. If nowadays is the context for your question, sometimes my thinking seems terribly old-fashioned. Which is my way of congratulating and encouraging young scholars from the Pacific and Asia for having shifted so far and so fast beyond the thinking of the sixties and seventies. There was an intellectual rigidity in those times. This may be sustained today in particular parts of disciplines like, say, formal demography or even economics. But in the social sciences, there is great scholarly flexibility, tremendous concern with philosophy, huge realisation that the grand theories of this world are not going to be thick on the ground. For the real world throws up too many exceptions.

That is not to say it is impossible to make theory. I always suggest to doctoral students that they aim for theory, but not try and be an Einstein, because it's not going to happen. Don't try for something too grand. Be a little humble, try for middle-range theory and you might be lucky as I was. It might be that the way I and others were thinking and writing in the 1980s has helped encourage some younger scholars to move forward intellectually faster than might otherwise have occurred. But today there is so much energy in the intellectual environment in which they find themselves, it's the case of many leaders who showed the way, not one or two. I not only admire young scholars for their accomplishments, I also find them very exciting.

If we shift the time frame to, say, the 1980s, I had a lot of luck. In this case, the good luck was to be made aware of a social demographer, Sidney Goldstein, who was a university professor at Brown and retired from teaching many years ago. I can't remember how I was given Sid's name but in 1976, as president of the Population Association of America, he gave a stirring address on the distribution and mobility of people. He called it 'Facets of redistribution', when the focus of most scholars was on fertility, natural growth and decline. Around the same time, he made many key suggestions for a meeting I was planning that led to the collection of essays: *Circulation in Population Movement: Substance and concepts from the Melanesian case.* This was the conference held in April 1978 at the East-West Centre in Honolulu, where we had Melanesianists come and talk about the dimensions of mobility in their field studies, mainly from the standpoint of anthropology, demography, economics, geography and sociology.

To put detailed field enquiries in a broader context, we invited leading figures from several disciplines who were comfortable with venturing beyond them. Mansell Prothero, who I had worked with at Liverpool in 1975–76 during my first sabbatical, was the broad-ranging geographer. Sid Goldstein was the broad-thinking sociologist. The career of Clyde Mitchell, from Oxford, had kept crossing the boundaries of social anthropology, quantitative sociology and African studies. As a political economist, Jan Breman from Amsterdam knew about operating in different disciplines. And Everett Lee was the broad-ranging demographer. Having two groups of professionals roll up their sleeves and interact for a whole week worked out very well.

Sid Golstein, even before that 1978 seminar, was tremendously encouraging. In the sixties and seventies he had done a lot of work in Thailand creating and using longitudinal data, which was novel in demography. By longitudinal data, we mean details about people as their numbers and characteristics change or flow through time, rather than, as for any national census, those details being frozen at a particular moment in time. The information Sid had for Thai movement looked back through time, so it was retrospective, not prospective as in my work. That experience led him to see the repetitiveness of mobility, but he never used words like 'the repetitive' or 'the recurrent nature' of movement. He often talked about return or repeat migration. During the eighties, he wrote papers on circulation in Southeast Asian societies, some of which were published by the former East-West Population Institute in Honolulu.

Even today, long after retirement, he is still doing migration surveys and writing papers about the situation in places like Vietnam, Ethiopia, Guatemala and South Africa. Sid did not see circulation the way I do intellectually. But this is the point: if you are a young professional, have even reached the middle ground, there is a lot of luck in the people you find out about or associate with. Through the decade of the 1980s, Sid Goldstein was one of my major supporters outside geography. Frankly, he was one of the key people who at times intellectually protected me, when the nay-sayers and the more narrow-minded of the technical demographers were deeply sceptical.

Another person was Phil Hauser, the Chicago sociologist. Sid and Phil would speak up in seminars at the East-West Centre, at conferences elsewhere and say: 'That concept of circulation is viable and it's important. You people need to be more broad-minded about such things.' So debates about circulation did not simply reflect my thinking, nor necessarily what was happening in geography, my own discipline. It is more that I tended to operate inter-disciplinarily in the study of population movement, which meant I often got support, and very powerful support, from some senior scholars. Frankly, at times, without that I may have just thrown in the towel and said 'To hell with it.' But I didn't.

DG: But it can be rightly said that, despite the fact that that was the thinking, the evolution, you were the one who rather courageously would make, and had made, noises about the urgent intellectual need to broaden or transcend the myopic horizon of the theoretical perspectives informing research in population movement at that time. Wouldn't you agree?

MC: That is true.

DG: That I will argue with you, because it is true, and see the difference it has made! Epistemically, your work is applauded for being ground-breaking in pushing to new horizons the frontiers of research on population movement or mobility. Your work is cutting-edge also in being holistic, arguing, for example, for other social conditions that force or underpin why people migrate. That economics is only one of the social conditions. For the record!

MC: You know my personality, David, as well as anybody. It is to make noises and not give up easily. I think that's a key to this. The other thing I tried to do is put in the published record that circulation was not my idea, which I don't think is acknowledged often enough. Circulation was first proposed by the British social anthropologist J. Clyde Mitchell in the late 1950s, when he was working with the Rhodes-Livingston Institute in what was then Northern Rhodesia, now Zambia. Clyde had published papers on circulation as early as 1959, although he did not always use the word in their titles. In my paper on 'Tribal mobility as circulation', it's very clear how much the theoretical underpinnings of thinking about people in circulation owes its intellectual ancestry to Clyde Mitchell. A lot of people, particularly in geography, thought I invented the concept of circulation. Good grief, absolutely not!

DG: But you put it on the world map, so to speak. Most definitely in the case of the Pacific Basin and Asia.

MC: Well, I think there were several world maps here. When it came to the circulation of African wage labour, Clyde Mitchell put it on the southern Africa map. In the literature of Black or Sub-Saharan Africa, particularly when by Clyde Mitchell and his students, there is a deep concern with the movement of people, especially in the growth of towns, cities and mining centres. That map of circulation is very much Clyde Mitchell's map. For Asia, primarily Southeast Asia, it's Sid Goldstein's map. He's published more about circulation there, initially on Thailand, later China, and more recently Vietnam. If the issue is whose intellectual map this is, which is not usually how I look at it since that seems a bit possessive, Asia would be Sid Goldstein's map.

Perhaps it could be said that some of the Pacific was my intellectual landscape but in 1969, when I arrived in Hawai`i, very few Pacific Islanders were doing Masters' degrees. My first student from Oceania, the Indo-Fijian Shashikant Nair, completed his MA thesis in geography in 1978, and it was another10 years before the Cook Islander, Yvonne Underhill, did the same. By definition, my first doctoral students were Asian, often middle-level professionals and university lecturers in their own right. They took their doctoral experiences back to their home universities and immediately began implementing what they had learned. Those early students were Shekhar Mukherji of India, Rosie Majid Ahsan of Bangladesh, Ida Bagus Mantra of Indonesia, and Anchalee Singhanetra-Renard of Thailand.

Only one, Rosie Ahsan, did an urban study. The others focussed on rural and rural-oriented projects looking at movement on the ground, which usually involved a prospective mobility register. Technically, David, we didn't know how else to probe the ongoing complexity of movement. A prospective mobility register had worked for me, it was very little done, let's try it again. After a while we all got bored with the technique, because it's very time-consuming and hard to write up. In those days, there were few software programs for the simple analysis of such a deluge of information, so we spent endless hours transcribing and coding. This is what happened with my dissertation. In a sense, we were captives when we should have been thinking of other ways to reach the same goal.

DG: Let's go back to the two volumes you edited with Mansell Prothero, *Circulation in Third World Countries* and *Circulation in Population Movement.* Could you say something about them? Two volumes that I keep using, in addition to your other publications, in my own work. Even when I don't use them, I read them for the sake of knowledge. The two volumes to me have steadfastly stood the test to time.

MC: Again, more luck. Even in New Zealand, before Lin and I left Wellington for Seattle in 1962, I knew that Mansell Prothero was a lecturer in geography at the University of Liverpool, who focused on the movement of people – at that time, mainly in West Africa and exclusively in Nigeria. I was fascinated by Mansell's writing and by the understanding it showed. It reflected the time, because, like much work in the 1950s and early 1960s it was richly empirical, richly synthetic and very interpretive. If a theory had passed Mansell by he would have been puzzled by the intrusion!

When I got to Seattle, as I've said, I was intrigued by how different that understanding was from the North American literature. So I began a conversation with Mansell, but didn't know he was viewed as a leader in studying population geography and about to become the chair of its Commission for the International Geographical Union. Later, that correspondence led the family to spend my first sabbatical at the University of Liverpool, in 1975 to 1976. We hadn't been there long when Mansell said: 'Why don't we collect papers together, of your students and mine, on circulation in the third world?' I said, 'Gee that is a great idea.' We drafted an outline and began to think of people, especially less junior colleagues than our students. Then the sabbatical ran out and the trouble began, because we had bitten off more than we could chew. This was the beginning of the first book, *Circulation in Third World Countries*.

At the time, until 1995, I held a joint appointment in the Population Institute of the East-West Centre, now the Program on Population and Health Studies. Intellectually, I was always the outsider in the East-West Population Institute, not so much because I was a geographer, but because my population work was grass roots in orientation. Basically, my thinking on movement and my style of research challenge many of the assumptions that demographers make. This meant life could be difficult because peers had to agree with or to grudgingly accept your work programme. You were reliant on a director, never a geographer, to allocate research funds. Towards the end of the sabbatical year at Liverpool I proposed a focus on circulation, although I don't remember the details; I suspect it was to be through a comparative review of Melanesian work. By then a lot of people had begun to look at population movement on the ground, not just from geography, but also anthropology, a few sociologists, even an economist or two – from Irian Jaya (West Papua) all the way to Fiji. I thought: 'What a wonderful way to get some comparisons going.' Partly because while at Liverpool, off the top of my head and helped by Mansell, I had written a paper that put forward some propositions on circulation. This ended up as the first chapter in the second book: *Circulation in Population Movement.*

After I returned to Honolulu this proposal was revised, sent to the National Science Foundation in Washington DC, and received the magnificent sum of $28,000. An awful lot of money in 1977 for an associate professor, [which] today would be viewed as peanuts by a funding agency! And that provided the basis for the 1978 *tok stori* between Melanesianists and world scholars. What was the impact, the intellectual contribution of those two books? For *Circulation in Third World Countries*, for which Mansell was the lead editor, I think it was the range of papers. We grouped them into different perspectives on circulation: holistic, ecological, social, economic. So we had one paper about a society in northern Nigeria where wandering around was seen in two ways, as if a participant and as if an outside observer. Another paper looked at the religious dimensions of circulation through movement of Hindu pilgrims within both the United States and India. Those papers on circulation went way beyond anything Mansell and I had written about.

In terms of *Circulation in Population Movement*, where I was the lead editor, I think any impact came from the goal to be comparative. In the sixties and seventies, largely outside scholars parachuted in and parachuted out of the Pacific. In some countries, like Papua New Guinea, at times in one village there seemed almost to be more anthropologists and archaeologists than there were indigenous people. An enormous amount of data was being accumulated and it always seemed to be put in papers that resembled ant hills. I kept looking at all these ant hills: 'For goodness sake, can't we talk across the ant hills? Can't we begin to think more broadly?' Starting with circulation seemed a good idea, even if it didn't survive by the end of the conference! For a week in April 1978, it turned out to be an exhilarating journey from beginning to end.

My mistake of course, Mansell's too, was to think we could work on two books at the same time, which involved something like 40 papers written by authors in something like 25 different countries, and get them done in a couple of years. For10 years of our professional lives Mansell and I were taken out of action, as it were, by those two circulation volumes. Years later, as in a 1999 paper by John Connell in the *New Zealand Geographer*, the adjective 'belated' gets inserted when referring to one or other of these books, as a reminder of how authors were badgering me and Mansell about the publication of their essays. Of course, when you get to 2003 and beyond, most people have forgotten about that. It doesn't matter any more.[3]

DG: Now let's come back to a more technical question, which has to do with the doing of research itself, with research being, as I see it, an epistemological activity. That is, an activity through which knowledge, or more correctly data, are collected and then get transformed into knowledge through detailed analysis and application in the world of practical lived experience. I know you have been doing a lot of work with different institutes of higher education in the Pacific, including Solomon Islands. What is your advice? Is research something or an activity of knowledge construction that more Pacific Islanders should be trained in, whether they are geographers, political scientists, linguists or anthropologists?

MC: I think there is an intellectual tendency in me to see those kinds of difficult questions, be a professional coward, and then try to find a simple way to get at them. The Solomon Islands College of Higher Education [SICHE] is a very good example, as you brought up, David. The then new director, Rex Horoi, contacted me late in 1989. He pointed out that the college had been in operation since 1984, yet two fundamental provisions of its charter were not being fulfilled. One was the doing of applied research and the other was the setting up of, at least within Oceania, institutional links. Rex said to me: 'I really would like you, Murray, to focus on the whole issue of applied research.' And he went on to say: 'I want to establish a research culture at the college.'

Now let's think of your question in direct terms. Why does the tertiary institution in a country, the Solomon Islands College of Higher Education, the director of which has a Masters in Applied Linguistics from the

University of Sydney and so knows the external academic world, why does he ask an outsider to get research, applied research, under way? At the time just half of the faculty were Solomon Islanders. There were a lot of expatriates on staff, but by 2003 virtually none. The simple way I approached your probing question, David, was to say: 'Rex, we have to get Solomon Islanders talking with me, initiating the conversation, so that at the end of the process we have a policy they are part of.' You might say my way of articulating the nature of research, of defining a Solomon Islander presence within research, was to listen to what the professional staff had to say.

Years later, I remember being told: 'Do you know, Murray, the most amazing thing about you coming to the School of Education? First, you sat down at the table and we were all in the staff room. Then you put down a yellow pad with a pen on top of it and started talking about what the director wanted. You raised questions of us. Just before you left, one of us looked at your yellow pad. There was nothing on that yellow pad, Murray!' As they whispered to me later, they were stunned I hadn't come in with a ream of notes, I hadn't taken notes, I didn't leave with any notes.

So, beginning to answer your question, David, I was trying to draw Solomon Islanders into the process of defining a research policy. Now shift that experience forward several years. Almost every North American summer from 1997, I would go to SICHE in a voluntary capacity and spend two to five weeks helping with applied research. After 1999, the key counterpart was Gordon Nanau, the foundation principal research officer, who later did a doctorate in international development at the University of East Anglia. In the early days, from the 1990s, there was no such staff member and I worked through the chair of the applied research committee – initially, with Hudson Kwalea, who was then head of the library division, and some years later with John Ipo, head of the school of finance and administration. It was those two Solomon Islander academics who held the applied research initiative together when later I was able to parachute in and out of the college each year.

The other dimension of your question, David, is interesting … Inevitably, I would hold seminars on how to do research, what techniques to use, the history of outside research in the Solomons, what I did in Tasimauri. These sessions were very slow to start because the Solomon Islanders were 'scared stiff' by this notion of research. It was, to many, some imported thing with a glossy label. In workshops, generally speaking, they would open up slowly and, basically, the issue was: 'Murray, I really don't know what I can do that is of any use.' Their doubts reminded me of a Native American at a meeting of the World Bank. At tea break he said to a leading NGO: 'Our village does not have running water. Why should we have running data?'

I had run into these doubts and fears many times, so always had local examples ready. They were obvious, but not to the Solomon Islanders because they weren't thinking about the process in terms of their own people, their own lineage, their land, their political system. They just heard this label 'research' and it was paralysing them.

'Would you happen to have a grandmother who's very old?'

'Yes.'

'Does she happen to know things you don't know? Have you ever talked to her?'

'Yes, we often talk together.'

'Do you take notes when you talk?'

'No, we just sit under the tree, under the mango tree on the big stone. We talk and it's very interesting.'

'Can you remember what she said five years ago?'

'Well, no.'

'What do you think that old woman would do if, before you went home, you went to Chinatown and bought a10-dollar tape recorder and two dollars' worth of batteries? At home, you would explain to her you wanted to *tok stori,* and if she didn't mind you would put this machine beside her. I suspect, that after a while, she would say, "Oh, that's alright. I'm old and I'm going to die soon."'

Then I would say to them, 'You know, if you talk to her and tape it, you know what you've done? You've done research!'

And they were absolutely dumbfounded.

DG: I can understand how the tyranny of the fancy language of western science – even the term research – can blindfold, intimidate and belittle! It happens everywhere and at every level of the research enterprise. This is one of the reasons why I call my research on how the Kwara'ae people construct knowledge 'indigenous critical praxis' and 'indigenous epistemology'. The word 'indigenous' for me is a constant reminder that, despite the fancy and flowery language in which Western-designed research is clothed, what I am doing in my work, fundamentally, is writing about and hopefully perpetuating how the Kwara'ae people of Malaita make knowledge. That simple! In journal articles and book chapters, I used a lot of Kwara'ae words or concepts, despite the tyranny of the Western academic enterprise to publish in English. The purpose, again, is to constantly remind myself of the Kwara'ae people, with whom and for whom I do my work. I have been told many times by journal editors not to use too many Kwara'ae words and concepts as I was not writing for a Kwara'ae audience, but an international audience which reads English. My response, of course, was 'How myopic and what utter rubbish!' What those editors were telling me, in other words, was that it is perfectly alright for an American or a British scholar writing in English to use French, German, Greek, Japanese or Italian words, and vice versa. However, as an indigenous Pacific Island scholar, I am not supposed to use words and concepts from my indigenous languages. Needless to say, I find such epistemic myopia and linguistic colonisation simultaneously amusing and ridiculous.

MC: One of the brave ones in the workshop would ask: 'You mean that's research, Murray?' 'Yes, that's absolutely research!'

For my last three workshops in November 2003, by now theoretically retired, I decided to hit this problem on the head. When academic and professional staff arrived, there was written in large letters at the top of a blackboard: 'What is this thing called research?' Four possible answers followed:

Is it what *ara'ikwao* (outsiders) do?

Is it a colonial plot?

Is it like *vele* or *piro* (two kind of Guadalcanal sorcery)?

Is it found in all societies, under different names? (By that, I meant words in many local languages, to encourage the staff to think about research in their own terms.)

DG: This brings me to the next point … something that has to do with my own evolution as an indigenous Pacific Island scholar trained in Western science. What I see you doing, your approach, through the dialogic activity of *tok stori* – a culturally established method in Pacific societies – is demystifying research and therefore how knowledge is constructed. Something I would have expected from an anthropologist, as anthropologists are the people who have expertise in the art of demystifying the 'mysterious', especially research methods to demystify the 'mysterious' minds of the natives. You deconstruct complex issues in a manner that is readily discernible to Solomon Islanders, without using the flowery and convoluted language of Western science. It's just perfect!

MC: I think there are two things going on here, David. I remember one of my colleagues in population geography in Hawai`i commenting: 'You know what fascinates me about you, Murray? You don't use any jargon.' I said, 'I don't need any jargon for what I am trying to say.' I don't understand why, when academics write, they have to wrap it in such esoteric language that the human brain finds impossible to comprehend on a first reading. That's one dimension. It was the same in the early sixties at the University of Washington. After being asked a few times what kind of geography I was interested in, I realised having no answer ready was not a good idea. So I would say, 'perhaps in population and resources'. After returning to Seattle in March 1967 and giving slide shows of Duidui and Pichahila, I was said to be a 'behavioral geographer'. After a couple of years on the Hawai`i faculty, a colleague labelled me a 'cultural population geographer'. It always puzzled me why I had to be imprisoned in someone else's labels! The other dimension is that if you are brought up in a lively, vibrant social democracy, reinforced by being a product of a working-class family, you call a spade a spade.

DG: Being from the village, I can understand what you mean.

MC: Put plainly, when you're in a working-class household, there isn't time *fo numba wun bulsit* [absolute bullshit]! Things have to be seen as clearly and as quickly as possible, because if you're an electrician, you can kill yourself by getting the voltages wrong. If a timber worker, and make a serious mistake, lose a leg. What do I mean, David, by working-class in New Zealand? My mother's family came from Manchester in England and her father was a station porter for the British railways in the days, in a class-conscious country, where cargo, cases and baggage was pushed and carried for passengers. New Zealand in the early 1900s must have seemed to offer better opportunities and so, very slowly, like many others, they dug a small family farm out of the clay soil and heavy bush on the hills around Auckland.

Family life was simple, neighbours were far away, and survival relied on working successfully with your hands. Everyone had to learn how to deal with the isolation of the rural outback. No one starved, there was no luxury, and everything had a use. It meant my mother's parents, less so my own parents, carried the knowledge of being self-made that gave them an unmistakable identity and a quiet pride. When at Liverpool in the midseventies, I was fascinated by the similarities. The route the double-decker bus took from our apartment to the university passed rows of derelict brick houses empty since the war years, and large open spaces from the demolition of bombed-out residences. People kept to themselves and were hesitant, but they saw that on most days I got on the same bus. Gradually, they would ask why Lin and I had come to Liverpool for the year, in the sense of 'how would anyone choose to be there, given its social history?' When I said I was at the university and we were enjoying Liverpool very much, they seemed a little surprised but quietly pleased. In both Liverpool and New Zealand, there was a clarity about a simple but good life from working with one's hands and getting on with it.

I think this clarity, sometimes a bit slow to come, reflects the way I write, the kind of work I do, and how I react when supervising doctoral students. My first were Asian and the early ones quite status conscious. They would give me a research proposal and I remember saying to one: 'That's an excellent proposal. The problem is, if I shut my eyes, I could be in Reykjavik or Zimbabwe or Auckland, New Zealand. I certainly don't see this as being in Indonesia. Is there any chance you could talk about Indonesia, as an Indonesian?' When I said this, students would look at me, their mouths drop, and seem sad and surprised. I would ask them to think about what I had said, come back in a couple of weeks, and then we could talk about it. And as they left my office, [I would] say: 'I don't want to read something that an American colleague at the University of Chicago would give me. You're an Indonesian, you have something to say, let's hear it!'

For any doctoral student from the Asia-Pacific region, that experience creates tremendous intellectual tension. In the case of this talented Indonesian, he had worked in a second language to meet the requirement of a sophisticated research proposal within the Western intellectual tradition. He had done that successfully, with a lot of blood, tears and sweat. Yet here's his supervisor zapping his scholarly ankles and saying: 'Where's your Indonesianness in this?' I know that being bloody-minded is a feature of New Zealanders! I caused my doctoral students difficulties and it often took them a little longer to finish. But they stuck with it, often delaying for a few months before leaving to go back home and begin research in the field. In itself, another difficult challenge. With their PhDs in hand, all but one of them told me I was right.

I still remember the day, David, when you appeared in my office and asked about indigenous Melanesian epistemology. Did it exist? What was it like? That was years before December 1991, when I returned to the Weather Coast for 13 months to look at the philosophy of Tasimauri mobility through the eyes of Duidui and Pichahila people, because they were the only two Melanesian communities I knew well enough. My luck held, and I did it. My response to your question back in the 1980s was *fo tok stori* about the studies Clyde Mitchell and his colleagues did in Black, or Sub-Saharan Africa from the early 1950s through to the 1970s. They hardly used the term, but saw and described African epistemologies. Basically, I told you to go for it among your own people of Kwaraʻae!

During the 1980s on the Mānoa campus, Hawaiians and Pacific Islanders began to surface more often in applications for admission to graduate study and fellowship support. If asked then, and of course I wasn't, a goal in my lifetime would have been to see some Tasimauri men and women with MA degrees. But that was too conservative. There are at least 23 Solomon Islanders who hold doctorates of philosophy or education, 19 men and four women: first, Joanna Daiwo of Santa Cruz, Te Motu in childhood education (2002), then Alice Aruheʻeta Pollard of ʻAreʻAre, Malaita, in gender and women's studies (2006). More recently, Katy Soapi of Rendova, western Solomons, in chemistry (2008) and Patricia Rodie of Malango, central Guadalcanal, in educational administration (2012).

Far beyond my hopes and predictions, already two from Tasimauri have PhDs: Tarcisius Tara Kabutaulaka of Haimatua in politics and international relations, and Morgan Wairiu of Suʻu, Marau, in soil science. Although for most of the time the country has no independent university, more than half with doctorates have journeyed back and remain in the civil service, in private business, in politics, as NGOs, and at SICHE, which since April 2013 has been in transition as the Solomon Islands National University (SINU). Indigenous scholarship is now part of an intellectual revolution sweeping the social sciences and the humanities, and it will challenge many scholars, including non-indigenous, to embark on new epistemological journeys in their own societies. That's tremendously exciting and an immense privilege to have been a witness at the edges.