Interview part 2

Nejm: We left the last session, with you about to enter the village and about to enter a potentially embarrassing situation.

Roy: I was offered a house [in Rouhua] that happened to be vacant. At the time I was collecting zoological specimens with kit given to me by the Natural History Museum in London. This was in support of my work on ethnozoology. I humanely killed things and injected them with a little formalin, and thereafter preserved them in large polythene containers. After about six months in the field, I became a little concerned that when I would go back to my house I would find a reticulate python curled up on my sleeping mat or hanging in the rafters. I mentioned this to Komisi, the head of clan who owned the house. He explained to me that this was obviously going to happen because the principal totem of his clan was the reticulate python. He explained that I would have to remove the polythene containers with the snakes, and would have to pay a fine. This was a classic lesson in participant observation, because I had to learn how to go through the ritual of paying a fine, a plate and five lengths of red cloth that had to be bought at the local Chinese kiosk about 3 or 4 km away.

Nejm: so the fine was for offending the snake spirit?
Roy: The fine was for offending the ancestral snake spirit. And everything was alright after that! The thing about doing fieldwork is that because you’re going into an area where you don’t know the people and you certainly don’t know their cultural rules and values, the scope for embarrassment is enormous. And if you are working with a people who are being incredibly tolerant, because they’re being tolerant you probably don’t learn the rules as quickly as you ought to. So if I’d known immediately that I shouldn’t be keeping snakes in that house I wouldn’t have got into the mess I got into. And there were other things, particularly in terms of gender relations and the use of language. Obviously, when I started out I didn’t know any Nuaulu [language] at all - it just wasn’t a language where there was anything published, just one or two scrappy wordlists that had been published before the 2nd World War. So I had to learn that language from scratch, and Nuaulu is full of prohibitions on using particular words in mixed sex company. So you have to learn that you can’t use the same words when talking to men as when talking to women, or in mixed company. And sometimes that was embarrassing. For example the word for a fresh water crustacean, which I would use in male company, is the same word as for the female genitalia, so you have to remember not to use that when in mixed sex company.

Moreover, Nuaulu are always looking for explanations of misfortune. And most misfortunes appear to be random; they cannot understand particular combination of events. [Have you] read Evans-Pritchard on Azande Witchcraft? It’s the same kind of situation, where a concatenation of circumstances are the problem. They can understand fully to their own satisfaction that the reason the bamboo slats inside a house break at a crucial moment is because there are ants eating through it, but they might want to know why it’s happened to them at that particular time! And it will usually be that something they have done previously has angered ancestral spirits. It’s usually the ancestral spirits. So it could be anything they may have done wrong in the past would be mobilised as a possible explanation. And it’s one of their main preoccupations, trying to prevent these bad concatenations of circumstances and thinking of ways in which they unknowingly have offended the ancestors. It’s a major preoccupation, and they constantly talk about the burden of monne, the burden of custom. They say things like “it’s alright for you Christians and Muslims, you don’t have the burden of custom. Every few minutes ancestors may be interfering in our lives, so we have to make sure they’ve been placated or make sure we’ve done things in the correct way”. So, the important thing about ritual is that it must be done precisely in the correct way. Any deviation from the correct way may result in misfortune.

Nejm: Did you find any conflict with your own belief system or lack thereof?

Roy: Yes, I think there were moments. I had to have some kind of identity, and at least initially my identity was that I was a Christian (I came from a Congregationalist Protestant Christian family). I had also had some sort of previous association with the Catholic mission in Ambon, [and] before I got into the field I would occasionally stay with the Catholic fathers. The nice thing about Catholics is that they like a good drink occasionally, and the Dutch Catholic mission were fairly progressive on both social and theological issues! That association was known, and it didn’t particularly handicap me. You know, I had to have that identity, I couldn’t avoid [it]. Local people needed to place me within some field of understood religious identities, even though there was a certain amount of tension between
Indonesian Christian and animist groups. But the longer I was there, the more I felt confident I could identify with the Nuaulu themselves, and my position as a cultural Christian seemed less important to them.

In Indonesia there is a state doctrine (in fact [David] Cameron when he was recently [2012] visiting on a state visit was banging on about it); it’s called Pancasila. And it’s supposed to be an inclusive doctrine of tolerance, whereby Indonesia can be effectively a secular state and yet take on board the various religious groupings. Or course, it’s an overwhelmingly Muslim country, but in the doctrine of Pancasila they recognise five religions. [However,] it doesn’t recognise animists, or indeed certain kinds of Islam that involve mysticism, Sufi tarikat and so on. There are limits to the doctrine of Pancasila. In colonial times the Nuaulu were classified as ‘Hindu’ together with the Hindu Balinese who in the immediate post-colonial period were recognized under Pancasila. However, in the last 30 years, they’ve tightened up on the definitions and Nuaulu are no longer recognised as Hindu. So if they are to get government jobs, in the civil service, police, army, they have to convert to some other recognized religion. That’s the official position, that’s tightened up and it’s probably got even tighter given the sensitive religious situation that’s developed over the last 10-15 years. There was active conflict in Maluku and other parts of Indonesia between about 1998 and 2001, between Christian and Muslim communities, connected on the one hand to local separatist sentiment and on the other to intrusive fundamentalist Islamist cells. This marginalised animist peoples like the Nuaulu. But I think over time, it didn’t seem to matter so much.

I think that whenever you’re a fieldworker, you question some local beliefs and practices, but if things happen you have to enter into the spirit of it. Because the alternative would be impossible... it’s easier for them to understand that you are a cultural Christian [or a cultural Muslim, Jew or Hindu] than for you to say you don’t have any sort of affiliation at all; they would find that difficult to handle. And for Nuaulu certain kinds of belief, like the belief in spirits, are so self-evident that questioning them is extremely difficult. They just assume that if you’re human you must share in these kinds of beliefs. For them it’s absolutely clear, like they believe that everybody has hookworm and body lice – you’re not human unless you have hookworm and body lice because everybody there has them. It’s part of being human, they really do think that. So, I think there were occasions when the few Christians that were around wanted me to affiliate with them, perhaps in disputes with others, which was a bit tricky sometimes. There was one poor chap [Paulus] who had converted some years before I had arrived, who had to move his house to outside the village because he was keeping bibles in his house. There’d clearly been some sort of séance, and an ancestral spirit said that the reason for a particular misfortune was because Paulus was keeping a bible under his bed. He was given an ultimatum: he could either remove the bible, or he could move his house outside the village. The Nuaulu village has a very clear symbolic structure. The symbolically potent area is right in the centre, and as you move to the periphery it becomes less problematic, and if you’re completely outside it becomes even less problematic. This is what was happening with the bible.

But obviously things can become intolerable and I’m sure there are many fieldworkers who find themselves in very difficult positions occasionally. I always think that I was very lucky. I know that not everybody was happy with me being there, and by and large I just didn’t talk
to those people. But there were a sufficient number of people who were perfectly happy with my being there, and no doubt there were some advantages for them in this arrangement. Anyway, they were incredibly hospitable. And when these problems arise they’re real learning experiences, to such an extent that I wonder if they hadn’t happened - if I hadn’t faced those problems - I would have had as much insight into how the place worked.

Nejm: Are you religious yourself?

Roy: No, I wouldn’t say I am. I’m a cultural Christian. I enjoy certain things about the liturgy of the Church of England and I was brought up as a Christian though not an Anglican, but no, if you were to push me, I would feel I would have to say I’m an atheist. But it’s always a tricky position for an anthropologist who’s often working in societies where there are strong religious beliefs. You must recognise the force of religion in some form or another. And in the context of your fieldwork you can’t reject it. But I don’t accept the position [such as say] Evans-Pritchard, or to some extent Mary Douglas, who were both Catholics. Evans-Pritchard was a Catholic convert. He maintained that you can’t understand a religion unless you’re religious yourself. I don’t hold any sway with that. I mean, I understand the virtues of participant-observation and having been a Christian at one point in my life I can have some sort of empathy, or understanding of what is going on in people’s minds in certain key moments of ritual or belief. But I certainly don’t think you have to be a practicing Christian to understand how religion works. In fact, I probably think it can create a problem in accepting that there’s some black box, that there’s always some area of understanding and explanation into which you cannot stray, that you either believe it or you don’t and there’s no point in going any further!

1998. With Laura Rival in Venice on the occasion of an EU 'Future of the Tropical Rainforest Peoples' meeting in Frioul. Laura (now at Oxford) taught anthropology at Kent between 1994 and 2001, and was a founding member of the Ethnobotany team.
Nejm: I wanted to ask a couple of things. I wonder if we could backtrack to your first few days and your first impressions, and even their impressions of you, your initial entry as a young man?

Roy: Well, I think it helped that I could pass myself off as a student. Nuaulu understand what being a student means. It means being relatively poor, and it means being there to learn, and it means that they [that is the subjects of your research] are there to teach you. And so that’s a very helpful role to have. And I have to say as I’ve got older and I’ve gone back to the field, Nuaulu no longer treat me as a student! And they expect me to have stacks of money to hand over. And they keep on saying when I ask any question that they assume I must already know the answer to: “you know this! You’re always asking us these questions and you know the answers already!” And of course, sometimes I do and sometimes I don’t, but it’s rather an important part of fieldwork technique to repeatedly ask the same questions, to try to triangulate and remind yourself of what the position is. But certainly being a student was extremely helpful. I think I was extremely lucky, I was sort of protected by a bevy of gatekeepers and assistants like Jan [Resmol]. I don’t know what it would have been like if I’d been completely on my own. I suppose you never are really, because people always step in to assist you.

Nejm: Can you describe some impressions of the first few days?

Roy: I suppose you have to get used to a new environment, and Nuaulu villages are different to those of other people on Seram. At that time when I first arrived [early 1970] houses were entirely made of timber and sago leaf stalks and so on, and they were incredibly smoky, with a pall of smoke hanging over the village and the house in particular. It was also very humid, oppressively so. And although most Nuaulu villages are on or near the coast, they’re highly connected with the forest in a way in which other non-Nuaulu villages aren’t. So, if you go to your average coastal Muslim village, the village itself is probably surrounded by coconut groves for some distance until you get to the forest, whereas the Nuaulu, their gardens, their swiddens, tend to be several kilometres away in the middle of the forest, so the whole orientation is to the forest, the presence of the forest is everywhere.

Nejm: Is there any kind of dualistic division between the forest and the village, or are there more grey areas?

Roy: Absolutely. You can almost say that the Nuaulu case presents a defence for the more traditionalist view of how nature is constructed. They don’t have a word for nature, at least they didn’t have, but there is nevertheless a strong conceptual difference between the village and the forest; the forest is sort of a proxy for nature, because the rules governing behaviour in the forest are different from those governing behaviour in the village. So for example, thinking of linguistic rules, there are certain expletives you can use in the village that you can’t use in the forest because they’re considered as mocking animals. So as you pass from the village to the forest you often perform a ritual, make a small offering to the spirits of the forest, so there’s a very clear boundary as you go into the forest. And Nuaulu talk about the village as being like an island. And that’s a metaphor that crops up in all kinds of symbolic contexts. So yes, there’s a strong sense of dualism as between the village and
the forest, and hence between culture and nature.

Nejm: Did those sort of deeper realisations take a while to conceptualise?

Roy: Well, I don’t know at what point that interpretation suddenly occurred to me. They [Nuaulu] certainly didn’t draw a little map and show me how it all worked. Of course, one of the things about doing ethnographic research anywhere is that you do a literature survey that has told you about the peoples in this area. And so you do have certain expectations about how things are supposed to work. In the context of, especially, eastern Indonesia there is this longstanding Dutch structuralist tradition. And so I was expecting these sorts of elaborate conceptual patterns. And so when they came along I was quite pleased! (laughs). One thing it did teach me though was that you have to be careful because it is terribly easy to make your data fall into some kind of neat system of binary oppositions. And a lot of my professional career, when I haven’t been doing ethnobiology, has really been a critique of this Dutch position. Although you could say that the Nuaulu work with some kind of ‘cognitive geometry’ in which they use different kinds of oppositions to think about the world around them, there is no neat, overarching symbolic structure. It’s a very dynamic kind of situation, and the problem with the Dutch structuralists was that they were working with very inadequate ethnographic materials, and they thought there were these rather tight cosmological models where everything neatly slotted together. But that certainly hasn’t been my experience. So if you take the concept of nature: on the one hand certainly, there is this strong contrast between the village and the forest, but then, equally there is a series of symbolic gradations, so the village periphery is more liminal than the village centre, less symbolically charged; and if you go into the forest there are clearly areas that are less ‘part of nature’ than others in the sense that they have been modified by humans: other village sites, sacred groves, all that kind of thing. So there wasn't an easy neat structuralist interpretation of what was going on.

And that was also true of their kinship system. Because when the Dutch structuralists were trying to see a match, they referred to “a total concordance” between the symbolic structure, [incorporating] all sorts of dualisms, and including the kinship structure. But they were only working with what they understood to be the ideal structure. They looked at the terminologies and did the kinship algebra, and they could see that people should be always marrying their mother’s brother’s daughter because the word for mother’s brother’s daughter was the same word as for wife, say. So, it all sort of made sense. But if you do - as Anglo-Saxon ethnographers tended to do more than the Dutch - collect a lot of detailed kinship data on genealogies, and data on marriages, you see that the ideal pattern that approximately reflected the kinship terminology doesn’t actually happen in practice. Only a certain percentage of people marry their mother’s brother’s daughter. So real life is messier than these ideal patterns suggest.

Nejm: How about your methodology? I haven’t managed to look at your thesis yet for that section.

Roy: Well, there’s not a separate methodology section in there I’m ashamed to say. Those were the days, especially in anthropology, when you weren’t expected to have a methodology section. The methodology was implicit. Though I suppose I was more
methodologically conscious or literate than some other people, who simply imbued the ethos of participant observation, simply filling their notebooks with anything they found out as they went along. I’d read all the stuff on Dutch structuralism, and so I was conscious of that, but that wasn’t in tune with things I was more methodologically geared up to working on. Because I had been reading Conklin on how to do research on shifting cultivators, and I had been reading some of the early work on the proper way to conduct ethnobiological research, I think I was more methodologically sensitive and explicit in those specific areas, and on the more technical things. I had talked to people at the Natural history Museum (in London) about how you actually preserved dead snakes and things like that. Even at that stage I had honed-up on particular protocols for doing swidden surveys and transects. But none of that was really part of the anthropology that I’d learned at the LSE! That all had to be acquired because I was doing work on ecology and ethnobiology.

Within my thesis, above the more technical level, the kind of model I was using to try to integrate different kinds of data was in tune with something called ‘generative analysis’, which had been developed by Frederick Barth. In the way I used it, what it most resembled in retrospect was Vayda’s analytic induction. My thesis was called ‘Nuaulu settlement and ecology’ – and its objective was to understand the wider Nuaulu pattern of settlement as an outcome of the various factors that impacted upon it. So clearly if they’re [the Nuaulu] conducting shifting cultivation, then the requirements for the effective conduct of shifting cultivation were part of that: you had to have a certain amount of land in fallow, it had to be rotated and this kind of stuff. And at the same time there were clearly non-ecological factors, almost symbolic factors. We’ve already talked about the symbolic structure of the village, and that’s a very good example. You know, the literal pattern of the Nuaulu village, although you can’t always see this when you walk into a village, is dictated in part by these symbolic considerations. So, at the centre, ideally, in every Nuaulu village is a big ritual house. And then on the periphery are the menstruation huts for the women. Now that’s entirely in accordance with the symbolic expectations. But when you walk into the village you can’t see it as a set of concentric circles of course; but it’s there. Such symbolic considerations are quite important when you are explaining the juxtaposition of different components of the settlement pattern. Why do particular clans live in particular areas? So
the clan *Matoke* is a *primus inter pares* - it provides the ‘lord of the land’, who has certain responsibilities for supervising the matter of land relations, over all other clans. And all their [Matoke] houses have to be located in what we would call, the northeast corner of the village. You can’t immediately see this, it has to be inferred. I mean, the point of the generative analysis was that somehow the visible pattern you could see, was generated by the interaction of these different kind of factors. And that’s what I was wrestling with. How you would resolve the more mundane economic and ecological factors with the more religious and symbolic things. That’s something else that was connected with my methodological ruminations at the time.

Nejm: Did you use particular methods to talk to people to gather ethnobotanical data?

Roy: Whether or not it was Conklin’s approach that I was most dependent upon I do not know. I had [read] both Conklin’s *Hanunoo Agriculture* and his piece on the ethnoecological study of shifting cultivation. This latter is quite programmatic: it actually tells you how to proceed. I found that quite helpful, and of course he used an emic language-based approach. So if you’re interested in the swidden cycle you find out all the different terms first of all. That’s how you get some kind of ethnographic structure. I had also read people like Freeman who had written on Iban in Borneo, a very influential study, and I had read Geertz on agricultural involution, so all that was being sort of churned over in my mind.

Nejm: Of course, these days we have kinds of things like freelists and such.

Roy: We’re much more explicit now [I hope]. I suppose in a sense we did undertake freelisting, but we didn’t do it so rigorously then. The ethnoscience procedures, the idea of the question frame and of always asking people the same questions so that you elicit the same kind of answer, and that sort of thing. And how you structure an interview in order to learn what the correct cultural responses are in particular situations and so on. But a lot of the specific methods that we now associate with ethnobiology just hadn’t been developed. Berlin’s big book [*Principles of Tzeltal plant classification*] hadn’t been published, and you know that was a major benchmark; but although Berlin’s book was absolutely meticulous in data gathering, I don’t think he was using all the specific methods that we would be familiar with today. I don’t think he used freelisting in the structured way that we might associate with the work of Raj Puri [University of Kent]. In fact, Berlin has only rather vague things to say about intracultural variation. He recognises that it’s going on, but he doesn’t offer any kind of method for demonstrating what sort of consensus there might be. That came later, all the stuff on consensus analysis and so on. So my work was very much in the prehistory of ethnobiological methodology. We were still railing then against the kind of ethnobiology/ethnobotany that was simply about making lists of useful plants, we were just trying to get beyond that a bit.

Nejm: Do you think you had a gift for language?

Roy: No. I struggled with language. I did learn Dutch and it’s become an absolutely essential tool for me [because] some of the key historical records are in Dutch. I didn’t *really* pick up Indonesian until I’d got to Jakarta. Having learned that, and some combination of Indonesian and Ambonese Malay, I had to learn Nuaulu, and that was the real challenge.
found out later about the Nuaulu word-lists, but they wouldn’t have helped me at all. I had read some of the linguistic work on other language groups in that part of Indonesia, but again it wasn’t particularly useful. And again, I think it was far better learning it the way I did, which was not as a linguist would learn it (although I’d attended classes in descriptive linguistics in London). I sort of learned it according to my own rules, using my own methods and my own orthography. I subsequently realised there were problems with that. I realise now just how – from a formal linguistic point of view - inadequate my understanding of the language was when I was using it. This is exactly what certain commentators say of Malinowski. He was a brilliant practical linguist of course, but if you look at his field notes which are kept in a library at the LSE, they’re written in at least five different languages: Trobriand, probably pidgin or at least one of those Melanesian lingua-francas, Polish, German and English, that’s five! There’s pages of stuff, nobody else can really read them! My field notes weren’t as bad as that, but there were [only] three languages represented. What informed commentators say about Malinowski was that he was brilliant at picking up languages, but he wasn’t a good analytical linguist. So you’ve got no proper linguistic analysis of Trobriand language, although he gifted us these terms that now trip off the tongue, like ‘phatic communion’, and his other contributions in the area of semantics and socio-linguistics.

So, I struggled. And to a certain extent I suppose my inadequacies in the area of language didn’t matter so much given the kind of work I was doing. My language skills were adequate, not fluent. But when I was in the field I felt that it didn’t seem to be too much of an impediment. You know intuitively when you’ve been integrated into a fieldwork situation for your language incompetence not to matter, it’s when you start dreaming in the local language. It was obviously working, I wasn’t having to think too much about it, but It must have been apparent to Nuaulu speakers as I’d learn enough to make myself understood, to understand other people, but it wasn’t brilliant.

Nejm: Was it an easy place to live?

Roy: You mean in terms of things physical?

Nejm: Well yes, but also in terms of friendliness?

Roy: Oh yes, it was. That’s why I felt I had an easy ride. Again, you hear of fieldworkers who, either because of the kind of society they’re working in or because of micro-political issues, find it very difficult to talk to people. That was never a problem. And again, another one of those important truisms about fieldwork is the importance of children. Children were very interested in any outsiders. They may be a little scared to begin with but that quickly disappears when they interact with you. And of course they’re much more tolerant than adults and so learning the rudiments of language, and indeed other cultural rules is so much easier. It’s a good way to do it, I would recommend it. Physically, it [life] was different, but I think I was expecting that; in fact, in certain respects I found it more tolerable than I might have had reason to expect. The little hut that they’d given me was no bigger than this room [RFE’s office], divided into two. Did I tell you about Evans-Pritchard’s dictum, ‘take two tables?’
Nejm: Yes!

Roy: Well they’d actually provided me with two tables! And there’s this big slab of wood – that was one, and it was the right height, and I could use my typewriter, and have my Tilley lamp hanging up and so on. And then I had one [a table] where I had all my food preparation going on. And it became quite a social centre. During the day I might be out and about, measuring a swidden or attending a ritual as one does. And then in the evening I’d come back and start writing up my notes, light the Tilley lamp, and have something to eat. And of course the Tilley lamp was the brightest lamp in the village and so it attracted everybody. So that was quite productive as well; because people come to you it became a sort of social meeting place. And so I learnt a lot during those evening sessions round my table. People were always very generous with food. I had an arrangement whereby I would buy sacks of rice and sugar and those sorts of things you would get from the stores, salt and so on, and there’d be a rough and ready exchange. People would give me some cassava roots or a bit of meat or something, and I would give them some rice. So that worked very well. I was never short of food or anything. And of course I dispensed minor medical assistance as well. So that was the exchange relationship that made collecting fieldwork data work, really. And I think at that stage people were perfectly happy with that arrangement. On subsequent visits, as I ceased to be a student and became a ‘big man’, they expected more of me, and as indeed the whole area became a great deal more commoditised. From the 1980s the government put through a road, and there were incoming transmigrants, so there was much more contact with the outside world. Nuaulu women would go off to the market and sell tubers and stuff. So as it was much more of a cash economy, so their cash expectations of me were probably correspondingly higher (laughs).

Nejm: So you obviously made good friends?

Roy: Yes, some surprising ones as well - friendships that have lasted over the years. And sometimes that was reflected to some extent as well in naming practices. The female prefix for a name is very often *pina*, which means female. So there was a ‘Pina Roy’ and other people named after me in various ways. And there was a dog called Ellen! Saete, the current village head was only a six year old boy when I first arrived and I got to know him [then], and when his father died, this chap should have become head of the village. However, there was a bit of politicking going on. In fact, he didn’t become head of the village until maybe, six or seven years ago. But now that he is, it’s very convenient for me! Because I was young in the early seventies I tended to bond quite a bit with young men my own age. There was a chap named Nauue who I got to know extremely well; he was a good hunter so I spent a lot of time in the forest with him. And then in 1992 there was a big furore over a head-taking incident. When I first went to the field there’d been no short-term history of headtaking at all, although it’s part of the remembered history of all the people of this area. And there are stories, mainly stories of other people told about the Nuaulu, but also by the Nuaulu themselves. I’m pretty sure, [the Nuaulu] weren’t taking heads at the time and probably hadn’t taken heads systematically since before the Second World War. They may have done so during the instability caused by the Pacific war, and following that there was a secessionist movement in the Moluccas, in which the Nuaulu tended to take up with the people who were anti-Jakarta and pro-independence. And I think there may have been a bit
of headtaking that took place then. After that, I think it just stopped. And when I was asking people about headhunting, they said “there’s no more headhunting, there’re no skulls around the place; when we plant a post for a new ritual house we plant a coconut, we don’t plant a human head or anything like that. Ours is a religion of red cloth and plates.” So this is the currency of ritual exchange today, red cloth and plates all the time, no heads.

And then in 1992, it was all over the local papers. Some Nuaulu had got into a fight with some incomers over sago groves they claimed were theirs. It got into a fight, got out of hand, they killed these guys. They cut their heads off and didn’t know what to do with them. So they went into a sort of ‘cultural default’ mode. And they took the heads back to the village and they asked the ritual elders, “what do we do with these heads?” The elders said, “bury them, bury them underneath the posts of the ritual house!” And that’s what happened. It was very strange! And one of those chaps involved in the headhunting was my friend Napua. He went to prison with two others - for seven years. Since then there’s been one other incident of head taking, but that wasn’t in the village where I lived.

Nejm: I know a little about your engagement with urgent anthropology, and so I wonder how your role has shifted, in terms of roles they’ve asked you to play?

Roy: I don’t think I’ve ever been very successful as an advocate or an intermediary. There are specific things Nuaulu have asked me to do that were problematic. They had to build a new guest house and so I paid for securing an artisan, and paid for the cement and things like that. And they wanted a public address system at one stage, I supplied them with that. And then there was another occasion, it was related to what I was saying about Pancasila. It was much later in the early 1980s when they were getting concerned, partly about the Pancasila thing, not being able to get employment and so on. And also they were getting worried about the depredations of logging companies, having at first been quite keen on logging companies because they left all sorts of useful rubbish behind. And they carved roads through the forest that they could use. But then when the logging companies left they just left a mess! So their views on logging diminished, and they wanted me to intervene on these kinds of things. I found that rather difficult. I helped them produce a video that I then showed to bureaucrats in the department of ‘Masyarakat Terasing’. There’s a special Indonesian government department that deals with ‘remote groups’, and the Nuaulu come within that category. But I think their expectations of what I could do for them were unrealistic, really. And now, they’re much better placed to be their own advocates, for example one woman, Hunanatu Matoke, who co-authored a dictionary with Rosemary Bolton, went to university. And there’s a movement within Indonesia now fronting a sort of renaissance for traditional groups. This is not so active in the Moluccas because there’s not so many of them [animist groups] - there are no more than 2000 Nuaulu. But in parts of Sumatra these groups now have some sort of political power and they have been challenging the government on forestry issues for example. So to that extent it’s encouraging. This movement only really became possible with the overthrow of the Suharto regime in 1997-8, which led to a process of devolution, whereby local governments had more control. And that gave local groups [and NGOs], including the Nuaulu, a lot more power. They can now, as indeed they do, sell to logging companies! And so on. So it’s a two-edged sword: they have more freedom, but a freedom to undermine their own mode of traditional existence.
Nejm: I read how the Nuaulu had taken on certain environmental ideologies?

Roy: Yes, that really wasn’t [down to] me. It was really related to what I was saying about the way their attitudes towards loggers changed. Initially they were very much in favour of them because of the fringe benefits of being close to a logging camp. And it was also [the same with] transmigration: the government settlements that were created in the area around the Nuaulu. Through both these developments the forest was disappearing. Up until that time they had no reason to think that the forest would ever disappear. It was an infinite resource. They knew that although they were swiddening it, eventually it regrew. There were the actions of the logging companies and the government in creating transmigrant zones, but there were also other things going on, [such as] a cement factory that created a bit of a problem. As a result local attitudes began to change. And of course this was in the mid-eighties when the views of certain environmental activists, Indonesian NGOs were becoming more available. So they bought into the ecological rhetoric, honed with their understanding of forest dynamics. And also they had television. For the most part their main interest in television was, for example, English league soccer (laughs), but I remember on one occasion watching them absorb David Attenborough [on tropical rain forest]! It was most bizarre, but I am sure you can make the connection.

2009. With Ria, Clara, Leny and George, at the end of jetty in Debut, a village in the Kei Islands. At the time Roy was conducting field research on cassava diversity and management with the Indonesian anthropologist Hermien Soselisa, funded by the British Academy.
Interview part 3

Nejm: It would be good to know a bit about the interesting and wacky world of the academic. What’s an average day like? You probably start early.

Roy: I do. I’ve always been a fairly early riser. I think I’m a much earlier riser than I used to be when I was younger. So I’ve got into the habit of doing the work that requires the most thinking about first thing in the morning. Because the problem with working at a university, and I think this is increasingly so with the number of students, is the calls upon your time. Once you come into your office you don’t get too much time to yourself. It’s a constant stream of interruptions.

Nejm: You’re up at what time?

Roy: I’m usually at my desk by 6.30am, just to clear the email out of way and get down to something.

Nejm: Are you a coffee man or a tea drinker?

Roy: I’m a coffee man, and again, increasingly so. This may have something to do with fact that I get up earlier and earlier! I drink far more coffee than I’m sure is really good for me. I will have to wean myself off it. But all this is fairly recent.

Nejm: So I assume there’s no average day. It obviously depends if it’s term-time or not. Have you found a way, in your long career of making your research more efficient?

Roy: Yes, getting up early! I don’t know how I manage this, but I’ve always found that I’m a relatively efficient user of time. I know that some of my colleagues really do have to separate out their research time from their teaching time and admin time. And they have to be in the mood to do research. But I’ve found I’ve been able to slip in bits of research in between doing these other things. It’s probably not the most intellectually satisfying kind of research that you fit in, in the interstices between lectures and committee meetings, but of course most research is like that anyway: it’s not the thinking but rather the databases, and the texts and things which you can do fairly automatically after you’ve been doing it for some time I guess. Yes, you’re right. Term time is fairly orderly, there’s a rhythm and you have your teaching, and of course the rest of the time is what you make of it. And certainly, there’s less free time now than there used to be. It’s very difficult for me to compare my use of time when I was first appointed here [at Kent] in 1973, with what I do now. It’s an entirely different ballgame. It was at a much slower pace in the seventies, for one thing. The things
we did by way of administration, somehow *pro rata*, took longer because you couldn’t just zip off an email to someone, and the whole thing had to be written out, and very often you would take it down the corridor to a secretary who would type the memo for you, and you would correct it, and so ... yes it’s a wonder there were academics who were as productive then as they are now.

2012. Roy with Oliver Rackham in the Swingland Room at Kent. Oliver has been a pioneer in the field of historical ecology and a regular contributor to the School of Anthropology and Conservation programme of events.

Nejm: You said it was usually databases and texts you were able to work on during the in between times, because you’ve edited a phenomenal number of texts. What does that involve in particular?

Roy: I always tell myself that the most recent edited volume is going to be the last, because they’re always quite a headache to put together. And we’re told by other members of the profession, and by publishers, that there’s no future in collected editions anymore. So it will be interesting to see what happens. But as you say, I’ve edited a few over the years, and I think there has been a place for these. But they really are hard work. Nevertheless, there is a feeling in anthropology, I think less so in certain other science subjects, that these edited collections really are very serious and considered contributions to particular sets of issues.
The first volume I ever edited was on *Social and ecological systems* in 1978 for the ASA. And that [series] still continues, and is still considered a flagship series for social anthropology in the UK. And of course in those days there were fewer special issues of journals. So if you wanted to make a bit of a splash on a particular issue where you’d got a series of contributors talking about the same thing, you’d have to do it through a collected volume. But now, I think these are increasingly being replaced by special issues of journals, and the way in which periodicals are disseminated now means there is less of a market for that sort of publication. But as you say, it has taken up a lot of my life doing that. One thing about it is, that as an intellectual process, it can be very rewarding, by virtue of having to interact with colleagues and editing their chapters, and in writing a critical introduction which is supposed to draw together the various themes - you learn a lot. I always think that I’m never going to do that sort of thing again, but after six months or a year you forget about all the hair you’ve had to tear out in the process, and you’re looking forward to new projects.

Nejm: I did have one question from Rory [McBurney] which was “what’s the least proud you’ve been of a piece of work you’ve put out?”

That’s a trick question! It’s the sort of question you’d ask at an interview, I think. I know which research project I’m least proud of, that’s an easier question to answer, which was the big European grant we had [APFT], which was an absolute nightmare. I was tempted by the money that was involved, split threeways between Paris, Brussels and Canterbury. There was £4 million tied up. I don’t consider it to have been money well spent as far as the research itself was concerned.

Nejm: That’s pretty good going for the amount of publications you’ve put out.

Roy: Oh, that isn’t too say that I’m consistently satisfied with what I write. I’m certainly not, but there’s nothing apart from that [the APFT grant] that I would wish to disown, and here the problem was project management rather than research quality. And I think when you write things, and this is a rather worrying tendency, it does become part of you, and you become very defensive. And even if you know it’s not perfect, you still defend it. And that’s the nature of a lot of academic output. The other thing I’ve been doing more of recently is collaborative work, co-authoring things. And that for me is a very interesting challenge, because in the past, you know, in social anthropology writing was an intensely private kind of enterprise. And I think social anthropologists on the whole are not good at collaborating on particular research papers. But where it involves fieldwork, I’ve done quite a lot of that, one way or the other. I started off by working with students. I thought that very important. I had a number of Indonesian students, and they needed some support to get things into the public domain. And I found that really instructive. I worked with Johan Iskandar on his Baduy material, and then with Rini Soemarwoto on her Kasepuhan material as well. And I’m doing something now with Yoshimi [Osawa]. And that I like … working with PhD students.
But today I’ve just heard we’ve had an article accepted by the Journal of Ethnobiology, that’s come out of [a British Academy funded] cassava project. There’s three of us who’ve authored this article, and it’s about the way in which cassava is managed, or has been managed in several different localities in eastern Indonesia, and how this has been a response to environmental change. And, of course, cassava is not native to anywhere in SE Asia. And it’s probably only been actively planted since the middle of the 19th century. There are some areas where it’s become really important, and there are some areas where it’s less important, and this may be linked to degrees of aridity and deforestation. So what I’ve been doing is looking at several sites and undertaking collaborative fieldwork with an Indonesian anthropologist, Hermien Soselisa, which I have very much enjoyed. It was good being in the field with somebody else. And then we also had a genetics expert [Asri Peni Walundari] - a sort of Indonesian Jim Groombridge - in Bandung, who was doing DNA profiles of all the cassava landrace samples that we were sending her. So we could trace the relationship between different cassava samples and how effectively they’d been managed and moved around, and to note the enormous differences between two areas. One had fairly high humidity, where people were not really interested in managing cassava, where there was very little diversity, and very little attempt to manipulate it and exchange it. Another area was arid, deforested, with a great deal of diversity, and a lot of active management and exchange. And this is reflected in the DNA diagrams … fascinating stuff. Now, I’ve really enjoyed working on that, to work with an Indonesian colleague in the field. I’ve never really done that in a fieldwork context before, and remember I’ve been going back and forwards to Indonesia since 1969. So I’d left it rather late to collaborate in that way. But I really found that it worked very well.

And then I have had a similar project that has nothing to do with ethnobiology at all, though it did relate to my interests in classification. This was a British academy project on the Eolithic controversy. I was interested in the process of cultural cognition as its been studied by people who study folk classification systems ethnographically, but was attempting to apply it to the way in which mid-nineteenth century archaeologists were thinking about stone tools, because they’d only just decided these were human workmanship. Then how were they going to think about them, classify them? And that’s what interested me as a problem. I collaborated with an archaeologist who did all the work on the collections of eoliths and archive material in Maidstone and in various London museums. And again we did experimental work in the ethnobiology lab. We asked students to pile-sort material, using tests that ethnobiologists use routinely, but doing it with stones that were plausible artefacts but had no history of being artefacts at all. We wanted to understand how people who were new to this kind of exercise would think about these stones, how they would classify them, how they might recognise what might be a tool and what was not. And that was really interesting. So increasingly, I’ve been more collaborative. And I think if I continue … well, perhaps I will… there’ll be more collaboration in the future.
Nejm: I’m glad to hear that, I’ve always wondered why anthropologists don’t collaborate more like the old Rivers-Haddon expeditions.

Roy: Well, it’s interesting you should mention that, because, what happened was, Malinowski arrived on the scene! At that particular period in the development of anthropology the main methodologies involved either working intensively with one key informant, like Franz Boas with George Hunt amongst the Kwakiutl, writing down endless texts, or the Torres Strait model [i.e. Haddon-Rivers], which was the classic 19th century model of the scientific expedition, like the Challenger expedition and things like that. And of course most of the people who went to the Torres Strait were not really anthropologists, although some of them subsequently became so, like Rivers. Even Haddon was trained as a natural scientist. But then partly because fieldwork was very difficult, the logistics of simply putting an expedition together were complex, I think the whole participant-observation style was not conducive to that. You know, the full-immersion style of ethnography where you had to retain your flexibility, and having all these other people around in the field was just making things more difficult and getting in the way.

2006. With Dario Novellino and a Buryat shaman at the International Congress of Ethnobotany is Istanbul. Dario is a Kent PhD graduate supervised by Roy, with considerable experience of working in the environmental NGO field and in advocacy.

Nejm: We’ve already started to talk about some of the projects and the some of the collaborative stuff, and maybe we could discuss what you’re looking forward to doing now that you have some free time?

Roy: This is where I’m going to disappoint you because one thing I’d like nothing better to report, is that I will be doing something completely different. But one of the reasons I’ve decided to retire now is because I was rapidly realising that if I went on with the current set
of responsibilities I would never do some of the things that I have been planning to do for a decade or so (laughs). And I feel a bit guilty because in academic life you’re constantly being diverted by new things that come along. Things that are urgent, as when you’ve got students and you have to respond to the moment. And what you put in for an ESRC grant application, although it may be very interesting, is not necessarily conducive to writing up all that field material you collected when you were a PhD student. Maybe things have changed [in the profession generally], but [speaking for myself] I’ve got an enormous amount of data that I’d like to attend to. And there’s no way in which I could do that unless I retired. So that’s one of my main motives for retiring. Partly, this will involve writing up my ethnobotanical material. In the Nuaulu Ethnobotanical Database I have a very considerable amount of data that probably won’t be terribly scintillating intellectually, but is important given that we still have so few major studies of the folk classifications of plants. [It would be helpful], if I were able to produce something that was comparable to Brent Berlin or Gene Hunn’s monographical treatments, or something like that. I did the animal material some time ago but I never got round to the plants. I had an ESRC grant in 1996 that allowed me to really make progress on Nuaulu ethnobotany, but the work was never quite finished. And it really needs to be made available in some form.

So there’s that, and the other thing I want to return to, and this goes back to maybe the first conversation we had, is to write up more of my kinship data. The [second half of the twentieth century up until the] early seventies was an era when Dutch - and other anthropologists including Rodney Needham – had attempted to progress our understanding of kinship, and [as we have seen] Lévi-Strauss had drawn very heavily on this body of work. Now I’ve got a lot of genealogical data which I’d like to revisit, so although it has absolutely no connection with what I’ve been doing over the last 10 years, the intricacies of kinship analysis is something I’d like to get back to.

Nejm: That’ll certainly keep you occupied. Will that allow for any other activities, I don’t know, are you good at gardening?

Roy: I’m not good at gardening, but I’m told that I will be spending more time in the garden (laughs). And on the house. It’s an old farmhouse and it needs a bit of attention, so I’m going to pay attention to the fabric of the house.

Nejm: So the last question would be... I’ve been going over some of your recent works, and not so recent, such as the 2006 Ethnobiology of Humankind and other syntheses, such as with Eugene Hunn and another one from 2011, and I’d love to hear both where you see ethnobiology going, and where you’d like to see it going. We’ve had some brief reviews such as Pablo Dominguez’ recent presentation relating to a more quantitative direction, a la Viki Reyes-Garcia, whereas some of us are going in a more qualitative and symbolic direction while very much grounded in biology and ecology.
Roy: I would like to think there’s room for both. There’s certainly a lot of interest in ethnobiology. And I think, intellectually, it’s not going to disappear, and I think it’s certainly going to be driven by some of the applied issues. For example, conservation scientists see that it can augment what they’re trying to do, and there’s the whole indigenous rights issues there, so I think it’s inevitably going to be much more applied. What I was explicitly trying to do in that special issue in the JRAI was to bring it back into anthropology where I thought it belonged, because I think ethnobiology as a set of practices, and also the sorts of intellectual issues it raises, can contribute in major ways to those questions we consider to be central to the anthropological project. So that’s what I was trying to do there. But I’m not entirely confident at the moment, despite the interest in ethnobiology, that it’s going to be reproduced effectively in terms of personnel, in the way that we had perhaps once hoped. Maybe things are more encouraging in North America, and as you know there’s a lot of interest in ethnobiology in the Global South. In places like Mexico there’s a very considerable interest. But in the UK, apart from my graduate students, and a few graduate students elsewhere who’ve done very good PhD work, there are not many people working in university departments who claim to be ethnobiologists. And this worries me a bit. Can it reproduce itself? But then publications are fairly vibrant. So, I rather wonder about that. And certainly the particular area that I have always been interested in, folk classification and how this links up to the study of human cognition, I don’t see much interest of continuing interest in either, except in so far as it is a body of knowledge that is helpful for people doing other work in the broader area of cognitive anthropology. So yes, that’s slightly concerning, I think.

Nejm: I’ve wondered about that myself, [areas] such as classification.

Roy: You know, maybe it’s had its day, perhaps we’ve got to move on, and what is most interesting about it has been said. But in ethnobiology more generally, there’s never been as much interest. I’m always immensely satisfied when I go to an international conference [such as the recent ICE in Montpellier]; you really do feel a sense of confidence that what you have been doing is continuing and developing in interesting ways.

Nejm: That’s good at least, so perhaps maybe just in the UK in terms of professors and older professors. Maybe there’s a gap between the graduate students building their own careers.

Roy: Maybe there’s no middle generation. We were rather hoping to attract people who have ethnobiology credentials to take some of the posts that we’ve just been appointing to. But there was nobody who came through that was regarded as being appointable. Perhaps we didn’t try hard enough, but that’s partly a political game. But you may be right there.
Nejm: So thematically we’re talking about the applied stuff. Some people are talking more about the indigenous rights and indigenous ownership of traditional knowledge, development studies, is there anything else in terms of themes? Or perhaps unification, we were talking about drawing things together within anthropology within a co-evolutionary framework, which involved a time dimension. I liked your enthusiasm on the historical ecology.

Roy: Did you see the recent [television] programme, the series on ‘unnatural histories’? Researchers are now finding these incredible structures, earthworks in parts of the Amazon, enormous ceremonial sites that had never been visible before, indicating that there wasn’t forest there all of the time. And of course the work on black earths, and things like this. And then there’s the local stakeholder dimension. What I like about the International Society of Ethnobiology is that there’s always a tension, and it’s an important tension, a positive tension between the science and the activism. It’s an organisation whereby scientists and researchers can actually get together and share concerns with indigenous activists and people who own and wish to protect knowledge, as well as use it and understand it. Ideally you might think these aspects might be mutually supportive, and to a considerable extent they are, but there are tensions because, inevitably, the project of wanting to protect indigenous knowledge is a highly political one, and it doesn’t always sit comfortably within the scientific context and the ways of trying to understand the world that anthropologists, and perhaps academic ethnobiologists would prefer. But you need that kind of tension, and I think intellectually the world would be a far less satisfying place to live in if [these kinds of tensions] didn’t exist.

It’s similar to how I feel about anthropology as a whole. I was trained as a social anthropologist, my PhD was in the area of social anthropology, but I’ve always considered that anthropology was a bigger project than that. It [should be] concerned with asking, and attempting to answer questions about what it means to be human. This involves engagement with the biological at every level. And that’s never easy. And even in an institutional context like ours [at Kent], the biological anthropologists like to do their own thing, and the social anthropologists like to do their own thing, and it’s hard work getting them to talk to each other! But I think it’s necessary and worthwhile to try to do so. I am convinced that despite the tensions that are constantly pulling us apart, the [holistic] part of the academic project I’m committed to is sufficiently robust that a complete split is never actually going to happen. Although institutional and scientific cultures might encourage a split it could never actually happen in the longer-term because the interaction is philosophically essential. And maybe - and I don’t know whether you can see these kinds of inner tensions in other domains of scholarship, or whether it’s a special feature of anthropology – there is always hope when as researchers we are simultaneously sitting on two sides of the fence, as both scientists and humanitarians.
Postscript. On editing this interview, I realised I’d left out a crucial question, one I was interested in asking and that others had asked me to ask also, so I felt I’d be doing them a dis-service if I left it out. It could be quite a personal question, and I think Roy was quite honest in answering it. It’s not long, but interesting nonetheless: What motivates/drives you (to do great works)?

Roy: I don't know how to answer this and I'm probably not the best person to ask anyway. I suppose I'm 'driven' by a combination of short-term and long-term commitments to myself and to others, a mixture of immediate and ultimate causes. As an academic, there are a whole raft of incentives, commitments and responsibilities you enter into - for research assessment exercises, fulfilling promises to senior colleagues, obligations to students, co-researchers and research subjects, peer group pressure, enhancing promotion prospects, flattering invitations, personal vanity etc. I think your question, however, is attempting to go beyond this in looking for explanations based in my upbringing and personal psychology. As I say, I am not best placed to provide answers here, though there must be some. To disappoint you, I cannot imagine that it could be either sibling rivalry or an attempt to emulate by father!

1995. An official staff photograph taken at the time of the launching of the Part 1 module on the Foundations of Human Culture. Roy had maintained a long-standing interest in human evolution since his student days in London. The emerging BSc programme at Kent began by bootstrapping with part-time staff and it was to be a few years before the appointment of Sarah Elton, our first biological anthropologist.