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David Parkin in interview: Anthropology, language & diversity

by

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**David Parkin in interview:
Anthropology, language and diversity**

An interview conducted
by
Dr Karel Arnaut and Dr Roxy Harris

David Parkin was Professor of African Anthropology at the School of Oriental and African Studies, University of London until 1996, and then Professor of Social Anthropology at the University of Oxford until 2008, when he retired and became Emeritus Professor at Oxford and Honorary Fellow at SOAS. In 2009, he became Research Professor at the Max Planck Institute for Religious and Ethnic Diversity in Göttingen, Germany, focusing on medical and sociolinguistic processes of diversification.

This interview was conducted on Wednesday 5th June 2013 at the first international conference on Language and Superdiversity held at the University of Jyväskylä, Finland <https://www.jyu.fi/en/congress/superdiversity>. At this conference, David gave the opening keynote lecture Revisiting Cultural Concepts through Cycles of Keywords and Phrases, and he refers to this in the interview below. The interview itself was intended to explore the comparisons between David's earlier work on language practices in multiethnic, multilingual urban contexts in Africa, and contemporary perspectives on urban language and superdiversity.

KA: Hello David. It is nice to have the occasion to talk to you. I would like to start off with a very general question. You have been in Anthropology now for more than half a century. My question is - what brought you into Anthropology?

DP: I think when I was in the sixth form of my high school, at the age of 17, I became very conscious of events in Africa. This was the time when countries were pushing for independence from colonial rule. And also the time when in Kenya there was, what was called the Mau Mau Movement, which in fact was the liberation movement, which occurred from 1952-1959. So at the tail-end of it, I was made very conscious of what was happening. I was not alone in worrying about what was happening in Africa. We were obviously radical in our views, quite rightly so, and I became interested in Africa because, quite clearly, a lot of dramatic change was taking place. I was encouraged by a few teachers to think in this way. And the great alternative to a usual university education was to go to the School of Oriental and African Studies (SOAS), which offered a degree that combined the study of Swahili and Bantu Linguistics at SOAS, with that of Social Anthropology at the LSE. It was a combined degree – those two subjects and two colleges – principally SOAS, but also the LSE. So it combined a young person's concerns about particular events in Africa and the world at a time of tremendous change – anticipated change as well as actual change – including the liberation struggles and the emergence of political independence throughout the colonial world. That, coupled with the possibility of learning about these places and developing a disciplinary approach via Social Anthropology as well as learning the

languages, was very appealing. So that was my way into the general area of Anthropology.

RH: I was wondering what drew you into the language question specifically, given your plenary talk at this conference about the UK end of things not particularly paying attention to the language question in Anthropology. How come you got into the language question?

DP: At school I was one of those singled out to study languages – French and German among my A Levels, and Latin and Spanish at O Level. I then specialised in Swahili at university, where we had some very good teachers and great facilities. And Bush House, the BBC venue for broadcasting, included lots of Swahili speakers from East Africa, who lived in London. And I got to know them. We were encouraged to do so, and made great friends. So I was already speaking Swahili actually quite fluently by the end of my degree without ever having been to East Africa. So that was a continuation of my interest in language. And then when I got into Social Anthropology proper, I was struck by the fact that I was myself interested in the language as an object of socio-cultural study, but that quite a lot of my colleagues were interested in it only as a tool for doing fieldwork. Whereas, it seemed to me that language, in and of itself, was fascinating. And a very dear mentor and friend, Wilfred Whiteley, who was professor at SOAS, had a project in the late 60s studying language in Kenya as part of a Ford Foundation Project, a so called survey of African languages. And he thought that I might like to join this, which I did. So fairly early on, therefore, I was institutionally drafted into a theoretical look at language not just as a tool for fieldwork, but as something which was integrated in social process and could be studied as such.

KA: Could you perhaps tell us a bit more about this survey? Because as far as we can see, the survey as such, had quite a number of innovative dimensions in terms of the study of language and social change. And within that, your position was quite prominent. Also in publication afterwards, you take up a number of chapters in that book, *Language in Kenya*¹. So could you tell us a bit, what gave you this special role in a project in which, if I'm not mistaken, you were a junior researcher?

DP: Yes, I certainly was a junior researcher, but Wilf Whiteley gave me plenty of scope to do what I wanted. And I had lots of say. So, yes, I took up four chapters out of the book. But before that there had been another conference and publication called *Language Use and Social Change*², which Whiteley edited. I had written a paper there, which was about

¹ Parkin, D. (1974). Chapters 5 – 8. In W. Whiteley (Ed.), *Language in Kenya*. Nairobi: Oxford University Press.

² Parkin, D. (1971). Language choice in two Kampala housing estates. In W. Whiteley (Ed.), *Language Use and Social Change: Problems of Multilingualism with Special Reference to Eastern Africa*. Oxford: Oxford University Press.

some people in a bar in Kampala, people of different ethnic groups who switched between different languages at certain points in their interaction. When there was a crisis, they would revert to their vernacular. But when they were trying to talk amicably without crisis, they would use Kiswahili or even English as a kind of lingua franca. So this struck me and I wrote a paper on that. And John Gumperz and Dell Hymes especially liked it. They were at the conference. It was my first experience meeting these great people. And they were very encouraging, so I thought there was something in this. And then when Wilf Whiteley drafted me into the African language survey, I already had some idea of what perhaps I could do.

The African language survey, you asked me about that, this was a remarkable project I have to say. It was very well funded principally by the Ford Foundation, and covered a number of Central and Eastern African countries. From the middle or late 60s, until the early 70s, each of these separate country projects produced at least one book and sometimes more than one book. The work ranged from a rather classificatory approach to language – basically identifying languages and their differences and uses; this was a rather essentialised view of language in some cases – to our project which was much more concerned with language as a form of social interaction, and with trying to see social context as part of language transactions. I think, perhaps, we emphasised that more than any of the other projects, and that was because Wilf Whiteley was a trained anthropologist from Oxford, with myself also an anthropologist. As you say, I was a junior member, but I was able to take that on board and look at language as a field of social activity. There was still, even in my work, a somewhat more essentialised approach than what I would adopt now. I distinguished the different vernaculars from each other, as if they were self-contained. At the same time, I would make remarks to the effect that there was a lot of borrowing. But it was a secondary concern to that of code-switching and language choice. But it was there. So I think that was our distinctive contribution. It was probably quite ahead of the time. We were looking at the kind of issues that we have been talking about today actually, i.e. so-called superdiversity. But we were also breaking down barriers between the reified view of languages and treating them as areas of communication with much borrowing and crossover talk, to use the modern phrases. So one would say that, yes, 50 years ago, we were dealing with these kinds of issues. But, of course, we didn't have the particular terms that we have today. We certainly didn't talk about superdiversity. But we did talk about diversity, it's there.

RH: I want to just follow up a bit on the research methods that were in this survey. You say that you weren't too comfortable with essentialisms. But in the volume, more than one of your chapters actually has quite a lot of this stuff in it – about numbers of people who speak a certain language, who belong to a certain ethnic group.

If you were uncomfortable with it, how did that express itself at the time?

DP: You are quite right. I was juggling in a sense the two approaches. One of which was, indeed, to try and count people who claimed to speak one language as against another. These were based on self-reports for the most part. That was part of the essentialising dimension. It was an attempt to try and get some idea of how people themselves saw their linguistic environment, especially in a city like Nairobi, which was developing enormously, demographically and in other respects. Because people would talk about this. I actually lived on a housing estate in Eastern Nairobi, and people would actually talk about language as a recurrent feature of concern and as important in their lives. They were displaying their consciousness of the fact that, yes, there was ethnicity, and they were certainly talking in these essentialist terms, which, of course, they inherited to some extent from the colonial system of demarcation of ethnic groups. So they would talk in this way. This was very much for them a reality. Even though we speak of these as no more than essentialised contours, they were also talking in these terms. They would say: I'm a Luo speaker or I'm a Luhya speaker or Kamba, Kikuyu, or even a Bantu speaker (see Parkin 1971). So it seemed important, therefore, to have some idea how they themselves saw their linguistic environment. But you're quite right, the reality was, of course, that people were borrowing and using terms. And so in a couple of chapters in that book, and also the preceding book, I tried to show how, in fact, there was this transactional element.

RH: I notice that you've made some very small comments about the difficulty of that kind of survey approach. One of them was the problem of self-report, where there are status differences and inequalities, so what people say that they speak depends on the status of the code. Would you like to say a little bit more about that?

DP: Yes, I was worried about the whole notion of self-report. I still am. I think it has got to be highly qualified. I'm not against it as a way of getting some idea from large samples of how people see their linguistic environment, as I said earlier. But one has to guard against the fact that, as we found, people who saw themselves as having high socioeconomic status might stress their use of English, and claim it as their language, much more than others. And it was difficult to quantify this. With a sample of hundreds, you couldn't of course check out every single case. So that is definitely a bias built into self-reporting. But it is also part of reality in that it is the way people see themselves at that time. In those days, if you had higher educational status, and despite speaking just as much Swahili as someone of lower educational status, you would nevertheless tend to emphasise your command of English as being greater than that of Swahili. Because, after all, it is social contexts that shape, perhaps even determine, how much of it you speak or see yourself as speaking.

RH: I'm going to stick to this theme about research methodology, an issue that came up in the discussant's response to your keynote this morning about language competence of the anthropologist. There's this very interesting paper by Borchgrevink (2003)³ suggesting that anthropologists had some kind of sleight of hand about their actual language competence. And I just wanted to ask you a bit more about that. Inasmuch as you reporting in one of your chapters about having an assistant, you didn't really say much about what the assistant did. And then you talked about your competence in the Giriama version of Swahili. You said you learnt it. Would you like to say a little bit more about this question on language competence?

DP: For the African language Survey of 1968-9 in Nairobi, Kenya, I did know Swahili fluently, I had no problems with that. But I didn't know the other vernaculars. I had come straight out from London, so I didn't know the main vernaculars in Nairobi: Luhya, Kamba, Kikuyu, and had only a limited knowledge of Luo from my earlier fieldwork in Kampala - I could hold a halting conversation in it. And while I was there in Nairobi, it seemed worthwhile to try and focus on Luo. So I did actually learn much more, rather more systematically. So I could use Swahili and Dholuo. Swahili, as I say fluently. Dholuo, never fully fluently, but enough to get by and to talk to people, it would flow quite freely at times. But I never learnt Luhya, Kikuyu or Kamba, to be honest. I decided that it was good to have a rather Luo-centric view of the situation in Nairobi, while taking some account of other peoples' language use.

Now, the use of assistants. Actually in the case of Luo, I didn't use them exclusively for this sampling. As regards sampling Luo respondents, the Luo assistants were three people I knew very well. And the purpose in using them, was really to cover the large number of households that were involved in the survey. This was alongside my actual knowing intimately a much smaller number of households and people. In the case of the other languages - Luhya, Kikuyu, Kamba - the assistants I used would generally not be Luo. They would be of those languages. I didn't have the same close rapport, because I didn't speak any of those languages.

RH: I was thinking about the bit about the transactional research you did. Not the survey, the transactional bit, where you do talk about the days when you didn't have digital recorders. And you describe being in social places and overhearing conversations, and then straight afterwards, you and the assistant writing them down. Can you explain how you did that?

DP: Yes, it would have been much better to have been able to record them, so that you could actually go back and listen to them. I didn't have that opportunity. But I think all anthropological fieldworkers develop this

³ Borchgrevink, A. (2003). Silencing language: Of Anthropologists and Interpreters. *Ethnography* 4 (1), 95-121.

ability to remember – if not verbatim, sometimes verbatim –whole conversations. What I used to do was to listen to people and write these conversations down as soon as I could, fairly openly. No one objected to this, so I didn't have to run off to some corner or anywhere. I just had to do it rather quickly. So that became the basis of the data. Yes, you could say it's flawed insofar as it really ought to be the full recording. But one doesn't always have the conditions or the opportunity to do that. But I was reasonably confident that the points I was trying to make about the transactional languages, the way in which people switched, and the analysis I gave, did actually represent the situation as it happened.

RH: The transactional stuff you wrote in the book, one of those chapters, you had a number of episodes. And embryonically, it's very like what people now do in superdiversity research, where they are capturing naturally occurring conversation, and as the late Jens Normann Jorgensen talked about, identifying language features, not languages. It seems to me that, in your own way, you were sort of reaching for that in those analyses.

DP: Definitely. I was very aware of it, the way in which bits and pieces of languages were combined together, but in some emerging pattern or rules. I refer to this today in the lecture.⁴ One example I gave was: *nita win you kabisa*. That was just listening to kids talking to each other as they played football outside the house, which I stayed in on the housing estate. And I do have some recordings of some of these kinds of conversations. There's nothing very special about that. It's basically, as I say, the use of Kiswahili pronominal subject prefixes (*ni*) and tense prefixes (*ta*), but use of English pronominal object suffixes (*you*), and then with adverbs going back to Swahili (*kabisa*). So *nita win you kabisa*, is an example of a regular pattern. I was struck by that. So that was an indication of how, even among kids of age 11-13, they were developing their own language.

RH: But you were also fitting the other African vernaculars into this pattern as well.

DP: That's true. The whole bit about transactional analysis is that it is often in the context of jocular exchanges. Say a Luo goes to a market, where the market stall is managed by a Kikuyu. There's always a certain edge in ethnic relationships, especially among people who don't know each other, or perhaps only among people who don't know each other. So there would be a series of concessions. The Kikuyu stallholder might recognise him as a Luo perhaps, and greet him in the Luo vernacular. So, in a sense, this was the Kikuyu stallholder's concession. And then the Luo customer would then reply either in Luo, or perhaps at some point in Kikuyu. People would share these bits and pieces of each other's languages. So it's a series of concessions. You could say the stallholder wants to sell things. So of course, his initial concession (greeting in Luo) is a kind of a gift. And

⁴ David Parkin refers to his plenary lecture at the superdiversity conference (see: <https://www.jyu.fi/en/congress/superdiversity/keynotes>).

the customer is prepared to play the game. Maybe he will get a discount after all. So it's all set in this game-like context, in which bits and pieces of language from different vernaculars were transacted.

RH: The last thing I'd like to say at this stage is, I'm very struck by the use of the term "jocular", and you said tension in interethnic relationships. I think it's really very important. For example, in the work of Ben Rampton, in contemporary times in language and superdiversity, that potential for people of different ethnicities who are mixing together, sometimes under conditions of ethnic tension, to actually do language playfully, is an important element. So I was really struck by your pinpointing that in your research all that time ago. How important do you think this question of playfulness is?

DP: Ethnic relations were traditionally often placed in a framework of what one calls joking relationships. There are even Swahili and indigenous terms for this, where there are certain ethnic groups with whom you'd have a playful relationship; others where you'd perhaps have a less playful relationship, even an avoidance relationship. But of course in the modern context it wasn't quite like that. Nevertheless, this idea of jocularly overcoming ethnic tensions, I think, is a perennial problem. If it's the case that Ben has found the same sort of thing in London, then I'd suggest it's an immediately accessible resource. This idea of knowing one or two bits of other people's languages and using them. And we get this all over the world. If you go to another country, say Hong Kong. I don't know Chinese, but I can pick up one or two words of Chinese and use them to a stallholder in the market. He may laugh, and smile, and know for the world that I don't speak the language. Nevertheless, it's a concession I'm making and it breaks the ice. There's no tension in the first place, perhaps, unlike the situation we wanted to talk about. But it is an illustration of how we can overcome barriers through the use of language, which is after all a free resource. That's the amazing thing about language. Anyone can use anyone's language, within limits I suppose. And can use it to that effect. I'd go so far as to say it's bound to be, at some point, a universal feature.

KA: Perhaps following up on that, and coming a bit nearer to the present, you moved on in the 1980s and 90s. You ventured into a whole number of terrains of anthropological research. Certainly, the interest in language very much stayed with you. We can come back to that in a minute. But first of all, picking up on the idea of "features". In your work on performance, which I think is very innovative as well, I'm particularly thinking of this contribution to the edited volume by de Coppet⁵. There you were actually trying to see, what I now would say, the diversity going on in a ritual setting.

⁵ Parkin, D. (1992). Ritual as Spatial Direction and Bodily Division. In D. de Coppet (Ed.), *Understanding Rituals*. Abingdon-on-Thames: Routledge. pp 11-25

What was your purpose? How did that speak to the existing approaches to ritual that you were seeing around you?

DP: I suppose I thought rituals had been treated very much in terms of the way in which people talked about them. The use of prayer, for instance, which I later came to want to study. And also the invocations. So there was always a verbal element in the analysis of rituals, as well as a performative element. But I wanted to try and tease out the possibility theoretically. I was thinking of rituals without words. To what extent could ritual be seen as purely performative in the sense of physical action? It was really an idea taken from the notion of *rites-de-passage*. The idea of passage, of moving through, presupposes a physical movement. I was at the time very interested in spatial movement, and the extent to which rituals depend, fundamentally, not on words, but actually on physical movement. I think I still hold to that. It is the case that most rituals do have or are accompanied by some kind of verbal dimension. People do speak, as it were, with each other or to the spirits or whatever happens to be the context. But there are plenty of rituals which don't need words. You could argue that ritualistic action, by virtue of the fact that it is not only performative but based on repetitive steps and movements and spatial occupation, that it doesn't need words. The ritual can be conducted without words. And it was also a riposte to, perhaps unfairly I don't know, Levi Strauss' privileging of words over ritual. Actually I probably would privilege words over ritual myself, but I just wanted to tease that one out. So that one article stands alone in saying that language is not so important in one sphere of life, which is that of ritual.

KA: In other work of that same period, as I said, language was very prominent. There is an important article in the *Annual Reviews in Anthropology*, 'Political Language'⁶. And this seminal volume, *Semantic Anthropology*⁷. Could you just say, in general, what were the leading ideas to pursue your interest in language, but outside the interactional transactional sphere you were focusing on in the 60s and 70s?

DP: Well, take the article 'Political Language' first. It's the result of some earlier publications on speech-making and public rhetoric. I still remain interested in this. That particular article was really a review of other people's approaches, with mine added on or repeated. It's trying to look at the extent to which one could claim, as some have claimed, that the use of a particular form of public rhetoric stifles creativity. And I think it's more complicated than that. One or two others have now agreed with this, that in fact, you set up a kind of a dialectic. You speak in a certain way of formalised language. But that inevitably generates its opposite. Maybe not at that particular point in time while you are there on the platform speaking, but at some point. The use of certain terms may trigger a

⁶ Parkin, D. (1984). Political Language. *Annual Reviews in Anthropology* 13, 345-365.

⁷ Parkin, D. (Ed.) (1982). *Semantic Anthropology*. London: Academic Press.

reaction. So it's not just the case, it seemed to me then, that you have command through your use of key terms and public rhetoric over the total social situation. There will always be some kind of reaction. One of my best examples was the article by Borgstrom⁸ on the use of "democracy". Once the Nepali monarchy used the term "democracy" and told the people that one day they will have democracy. According to him, once people had got this idea, they would react: "Hey this is not a bad idea, we want it now! Not in the future." I thought that was a wonderful illustration. And we know that this was, in fact, what happened. The monarchy was overthrown. Now, is that analysis correct? The introduction of a key concept like democracy was obviously a whole plethora of ideas. It's more than just a word. It's an opening up of new possibilities. If that can trigger, literally, a revolution, then clearly we are on to something where it is not possible to say that you can ever command others through the use of language whilst stifling creativity. There will always be a riposte or even resistance. And that was really what the article was about.

Now *Semantic Anthropology* was somewhat different. It was related, but it was a collection of articles for the most part, with myself giving an introduction. It was trying to show how language itself is society. It's not just that language is part of society, but that our very interactions are communicative. No one, perhaps, would dispute that. But that language is also an essential part of the communication. This actually relates to another work – Tim Ingold's *Key Debates in Anthropology*. So, language is society and language is interaction, but you cannot do without language *qua* language. It's not just that as humans we are linguistically gifted or enabled in a way that other animals are not. I think that language at some point comes back at us, whatever our situation. It demands commentary. And so, however silent you may remain in a situation, or however much it may depend upon physical actions to carry out tasks, at some point there's a kind of linguistic or verbal response or reaction from someone somewhere, even if it's only yourself. So I didn't want to disconnect the two.

KA: You expressed that nicely by saying how *Semantic Anthropology* could have been 'Critical Anthropology' or 'Reflexive Anthropology'. So you opened it there, and it's like an attempt at reinventing Anthropology.

DP: Yes, that's quite right. 'Semantic Anthropology', 'Critical Anthropology', 'Reflexive Anthropology' could have been alternative ways of referring to it. The book came out in 1982 at about the same time as Marcus and Clifford's work⁹, which obviously was very important. I think we were saying similar things, but I think ours was definitely more linguistic,

⁸ Borgstrom, B. (1980). The Best of Two Worlds: Rhetoric of Autocracy and Democracy in Nepal. *Contributions to Indian Sociology* 14 (1), 35-50

⁹ Clifford, J. & Marcus, G. (Eds.) (1986). *Writing Culture: The Poetics and Politics of Ethnography*. Berkeley: University of California Press.

whereas their approach was much more general, asking significant questions about reflexivity in the field and in the writing up or field reports. But I think ours was part of the same genre. I can't remember which came first. About the same time, I guess.

KA: Also, a bit later on in the 1990s, you moved on to Oxford. You became head of department. As I remember, you were sort of headhunted to go there and, perhaps not the right word, to 'help out' an institute which was trying to reinvent itself in a new era after the big men had left – Evans-Pritchard, Godfrey Lienhardt, and Edwin Ardener and so on. So it was a new era, not only for Oxford itself, but also the anthropological landscape in general. How had that changed, since you started being interested in Africa and Anthropology, back in the 1960s? Had decolonisation changed Anthropology? Was Anthropology properly decolonised?

DP: Let me start with the Oxford move. I think it has to be remembered that Oxford's Institute of Social and Cultural Anthropology (ISCA) had a full-time equivalent staff of only seven, believe it or not. I think it was six plus two halves or something of that kind. There were some anthropologists in other departments, but the institute itself had only a FTE of seven. So on a purely pragmatic basis, I realised two things. One is that, being so small, it would not survive. This was a time when there were amalgamations of small departments throughout the country, in the UK. Many 'nights of the long knives'. A really hard time actually. And governments which just wanted to make cuts just about everywhere. So I realised we had to expand or else we will be properly amalgamated with Sociology, which at the time seemed to be favoured over other social sciences, apart from Economics. So I set myself the mission of expanding.

The second thing, therefore, was how to expand. What attracted me to Oxford actually, was the fact that they did have Biological Anthropology, and also Material Anthropology, including, of course, Museum Ethnography through the Pitt Rivers Museum. There were other possibilities of amalgamation, of expansion. And so the idea was, if not to create a four fields approach in the American style, to do something quite similar. A few years earlier, on a UGC visit, I had proposed, with ISCA's representative, Peter Riviere, to create a School of Anthropology and Museum Ethnography, which was adopted. When I arrived in 1996, in Biological Anthropology, we had Professor Ryk Ward, who was initially very cooperative and helpful. But unfortunately, he died, and we were rather stranded. Anyway, he'd moved Biological Anthropology into Zoology, which might not have been a problem, but one never knows whether we could have retained our hold on the subject. So we started afresh, gradually. By recruiting people into what we'd then call Evolutionary Anthropology, prominent people like Robin Dunbar. And in Cognitive Anthropology, Harvey Whitehouse later. By recruiting people into Material Ethnography, very good people, and being fortunate in having Michael O'Hanlon as the new director of the Pitt Rivers Museum,

who was obviously very inspiring and innovative. And also, most importantly, by building up Medical Anthropology, which is now very prominent, and I think could arguably be the best group of medical anthropologists in the country, possibly in Europe. And there's the leadership of Elisabeth Hsu and Stanley Ulijaszek. So we built up these different arms. And then, of course, Steve Vertovec brought in the Transnational Communities Programme, which eventually became Compass. And that was another arm of Anthropology. So migration and transnational settlement had their own institute, and of course it has expanded since then.

So that was to your first comment on Oxford – what to do with it going there. It grew from being an interesting but rather small group to a School of over 40 people divided into these different sections. And if you include all the add-ons and the post-docs, many more than 40. I'm not suggesting that when I arrived there was not quality. Of course there was quality. There were people like Peter Rivière and others. So that wasn't the problem. The problem really was the future of Anthropology. This is related to your other question – Anthropology in the UK. The future of Anthropology, not only at Oxford –where we had resources to build, if not a four fields approach, something similar – but also the future of Anthropology in the UK as a whole. I thought on this and used to give one or two talks on the fact that we were on the cusp of having to enlarge Anthropology. But also of intellectually wanting to enlarge Anthropology. To take account of Evolution, Material Culture and Museology and of Linguistics and the medical. And of inevitably, therefore, having to encourage some degrees of specialisation, not all of which was welcome, let me make that clear. So it was a project for Anthropology Oxford, but also a project for the UK as a whole. And I think it's had some effect. There already were places like University College London, which already had a more holistic approach to Anthropology. And Durham. And to a lesser extent, Cambridge, which was perhaps not well coordinated in quite the same way, but it was there, though more fragmented. And since then, I think there have been attempts to try and bring in Museum Ethnography, for instance, and Medical Anthropology, certainly, in other departments, and even Evolutionary Anthropology. So I think it is an inevitable way in which Anthropology, as a whole, will go. So it's as if we are coming back to an integrated holistic Anthropology, rather than just calling ourselves social anthropologists as against physical anthropologists or biological anthropologists.

KA: How had the image or the mission of Anthropology changed, in a world that was dealing differently with the exotic, which was one of the issues you addressed in your lecture *Nemi in the Modern World*¹⁰?

¹⁰ Parkin, D. (1993). Nemi in the Modern World: Return of the Exotic?. *Man* 28 (1), 79-99.

DP: In a sense, this is something we all shared – the realisation that we were part of The Other. It was the inevitable shift at that time. The realisation that we were not, as it were, disengaged or dissociated from nor have no effect on peoples we studied. I had the image of going out in the field and the realisation, of course, that one's very presence there had an effect. One was part of that which one observed. I think that was a general message we all came to share. And so that inevitably meant that, if we were part of that which we observed, then either we were part of the exotic or there was no exotic to be observed. And, of course, it is a fiction. Human beings are human beings, and we have similar aims. And what we should recognise through our involvement in the intensive interaction that fieldwork involves – over a long period of time, using and speaking the language of people who we're working with – is that we should make explicit our comments on and for humanity. The problems 'others' have may be cast in a different language, and perhaps even expressed culturally in a particular way, but they are the same problems that you and I have. And I think that kind of existential element became very important, for me at least. It was more than phenomenology. It was not just observing what was going on there as the basis of any interpretation. It was actually sharing their humanity. Sounds perhaps a bit over-sentimental, but it was quite important at the time to try and get over that idea of The Other out there / and We here, studying them by going out to them, which after all was an extension of the old colonial form of approach, which perhaps was inevitable at that time but was no longer relevant to the modern day. And, of course, more and more anthropologists, students especially, were from these very places. I think that was something that happened throughout Anthropology. It was very exciting.

KA: After Oxford, you moved on, among other things, to Göttingen, the Max Planck Institute for the Study of Religious and Ethnic Diversity. What made you make this step? What did you find interesting about this young institute, which is trying to deal differently with diversity?

DP: The first thing to say is that my wife, Vibha Joshi, was offered a fellowship there to work on Northeastern India. Her director was Peter van der Veer. But Steve Vertovec, the other director, got to hear of this. He and I, of course, had known each other at Oxford. In fact, I'd appointed him to his position at Oxford before he went to the Max Planck Institute. And he suggested I come as well. More than that, he wanted me to build on my interest in Medical Anthropology, and to set up a Medical Anthropology group. And I was very happy to do that because I had been interested in comparative Medical Anthropology, and this seemed to be a wonderful place where one had the resources – that was the main thing, I think. At that time, we were instructed as well to build up a library. Well, how often do you get that opportunity? There was money to use to build up a library. Since Medical Anthropology was one of my fields, I wanted to be able to set up the working group, which is what happened. Already before

then, Gabriele Alex had been working in the field at Max Planck, so it wasn't as if I came entirely alone. And then of course later, Kristine Krause became appointed to a position, while she was there as a doctoral student working in Medical Anthropology. So that was the basis of it. Steve felt I could help out there, which I was happy to.

And then while there, we had the visitation by Ben Rampton and Jan Blommaert, I think within a few weeks of my having arrived. They gave a very interesting, almost an applied linguistics course – or a course in how non-native language could be useful to you while you do fieldwork – to the various people at Max Planck. It was a brilliant idea on the part of Steve to invite these people. And they were very good, I have to say, and inspiring. We had lots of jousting. It was two, maybe three days. We didn't always agree, but in the end I did feel I'd learnt a lot, actually, about the importance of certain approaches they were adopting. They adopted at the time a Goffman-esque approach. I'd always been slightly concerned about Goffman, because it seemed to me that it was too un-ethnographic. It shows how traditional I was. "Lots of ideas, but where's the substance? Where's the real material?" But they demonstrated the use of his concepts with actual material – with tapes and examples. So then we thought – I'm not sure if it was my suggestion or whose suggestion – why not have a working group in 'Sociolinguistics', 'Linguistic Anthropology', whatever you want to call it? 'Sociolinguistics' would have been the right term since it wasn't just anthropologists. And then, of course, because Steve was developing his notion of superdiversity, to fit that 'Sociolinguistics' into superdiversity. And so the working group in sociolinguistic superdiversity was born. And that, of course, also tapped into my interest in language very directly. It's still my favourite. I still have more interest in language than anything else.

RH: It's very interesting how these connections go, because some years before when Steve Vertovec was at Oxford, he invited Ben, including myself, to go and do a presentation. We had written a paper on creole metaphors and sociolinguistics¹¹. I think it was something about creolisation. Ulf Hannerz was there. There's an interesting link here, because in your concluding commentary in the latest *Diversities* journal, you talk about parallel pluralisms versus fuzzy boundaries or no boundaries at all, as a way of understanding language and cultural practices in multiethnic and multilingual societies, which superdiversity is concerned with, which may be something to do with what you speak (called creolisation or not). I wonder if you'd like to say a bit more about this idea of parallel pluralisms versus fuzzy boundaries, in comparison with your East African research and contemporary work on Language and Superdiversity. In particular, were there any fuzzy boundaries in your earlier East African work? Or was it all parallel pluralisms?

¹¹ Harris, R. & Rampton, B. (2002). Creole Metaphors in Cultural Analysis: On the Limits and Possibilities of Sociolinguistics. *Critique of Anthropology* 22 (1), 31-51.

DP: Let's go to your earlier comments that many of us, including myself, still thought initially in terms of different languages. In other words we were borrowing the framework or worldview of the people themselves who we were talking to. And as a Swahili speaker, I would come across this. As I mentioned earlier, people would differentiate themselves, by saying I'm a Luo speaker or even I'm a Bantu speaker. People were using such auto-classification. So we started with that. So yes, our initial canvas, if you like, was essentialist and therefore pluralistic. And then, of course, the idea was to try and show that that wasn't always the case, hence the interest in transactional language. But I suppose it is still the case that people, even nowadays, at present times, still talk in terms of different separate languages, while at the same time engaging in all sorts of linguistic exchanges which make it very clear that the boundaries between these so called separate languages are, in fact, fuzzy.

RH: I wondered whether there are particular locations which lend themselves to parallel pluralisms, and particular locations which lend themselves to fuzzy boundaries? I say this because it's come up in Language and Superdiversity discussions we've been having for the last few years. I'm based in London, and in our discussions, we've often felt that in lots of locations in London, it's dominated by fuzzy boundaries, linguistically and in terms of cultural practices and ethnicities. But in some of the other locations in our Language and Superdiversity group, it's parallel pluralisms. I was thinking about it recently with the UK reports on the rioting in Sweden, where the reports said, "oh the riots took place in immigrant areas." And Karel earlier on said, "I've lived in this area in Brussels, which is the black area." In a location like London, in very many places you can't say that they are immigrant areas or not. They are places of many ethnicities and many language practices. So were there any places like that in your earlier work?

DP: Oh yes. In Nairobi, where I did my study of Luo, 1968-69, the area where I got a house and spent most of my time, was in a housing estate called Kaloleni, which was 90% Luo. And there were a number of other areas in Nairobi which were also associated with distinctive ethnic groups. But there were also other areas which were very mixed. So you did have that kind of pattern of ethnic separateness, but also ethnic mixing. So there were such areas. And in my case, I chose the Luo area partly because I'd come to know a bit of the language through my experience in Kampala. I was able to extend it and take this decision, so it made sense to be there. But I certainly went to other places, and there were two beer bars in the estate, which I describe in my book on Luo. One was ethnically mixed, and the other was entirely Luo. So I'd go to both bars; different kinds of beer being served; completely different ambience; fascinating. So there were these. What was interesting is that people would also recognise this difference. They would talk in terms of mixed areas. But more

importantly, it's always the marked one, I suppose, that stands out. They'd particularly talk about the ethnically separate or distinctive areas. "This is a Luo area, this is a Kikuyu area." In a sense, they marked their map of spatial or ethnic understanding. Whereas, regarding the mixed areas, their knowledge was less prominent in conversations.

RH: What about the language practices in those mixed areas? Were they like contemporary superdiversity and with fuzzy boundaries?

DP: Yes, when they are really mixed, they are Swahili. This is why Swahili is such an important language in Eastern and Central Africa, and its development over time. Take the case of Kampala. When I first went to Kampala in 1962, this will surprise you, I spoke a form of what people often thought was coastal Swahili, which is the 'high' and 'pure' form, only because I'd been talking to coastal people for the most part when I was doing my three-year degree in Swahili in London. And in Kampala they couldn't believe I didn't come from the coast. As I said, I was fluent, but it was definitely a coastalised standard form of Swahili, quite different from the Swahili spoken in Kampala. And it used to be said in Kampala, "oh it's only these Kenyans who speak Swahili, and maybe some other people from northern Uganda. We southerners don't speak Swahili" and so on. That's completely changed now, as you know. Swahili is not as widely spoken as English, perhaps, in Kampala city, but certainly very widely spoken. It's part of that East African, Central African territorial spread of the language. So I think that, yes, in the mixed areas, Swahili was coming along. What is also interesting – I mentioned today in the lecture – the whole question of Sheng, which we talked about. I'd love to do more research on that. There hasn't been that much research done. And, of course, there is always the question of the extent to which there is something we can call Sheng, rather than just being an urban, mainly young person's vernacular, which mixes Swahili, English and whatever ethnic vernaculars are to hand. I think that's probably true. I haven't done the research recently, but it's probably a rapidly evolving mode of communication, which now has a name, Sheng. But which has very fuzzy boundaries indeed. Probably the most fuzzy of all.

RH: I was just wondering about a research problem that affects contemporary researchers. In your keynote you talked about things like 'the unsaid', which can only be properly apprehended if you've got some kind of historical perspective linked with some ethnographic insight. Contemporary researchers have this problem of recording naturally occurring speech, often between urban youths of different ethnicities, and identifying language 'features', but not picking up the nuances. How would they be able to pick up concepts like 'the unsaid' and all of those things you referred to today, from these stretches of talk? Would they have to do it as a team?

DP: I think it could be done as a team. I haven't really thought much about that. I think the critical thing is that 'the unsaid' is always relative, in a

sense. Nothing is entirely unsaid. There are always allusions to a phenomenon. If there's an occasion or an event, or a sense of there being something among us – whether it's witchcraft or fear of spirits or even just someone's jealousy – we may not refer to it directly. But there's a sense of it being there. So in that way, it's unsaid. But it's also relative because there are degrees of unsaid-ness. And you can only really observe that relativity over time. This seems to me the great methodological problem, to go back to your point. I was lucky having worked in one area using Luo and Kiswahili, and also among other people like the Giriama, whom I haven't talked about because it'd have gone on for too long. Had I in mid-career moved, say, to China, learnt Chinese and worked in China, then I wouldn't have had that long term perspective. I'm not suggesting one shouldn't switch, because it's very tempting to switch of course. But I've had that 50-year long period in which I've been able to observe and take notes on, and indeed in some cases in later years, record conversations and the use of these terms. So I've been able to see over time the relative significance of 'the unsaid', and the way in which 'the unsaid' becomes 'the said', and then becomes stigmatised, and then goes underground again.

RH: So would you recommend to younger scholars that they take, what seems to be an approach taken by certain anthropologists, of returning over and over again to the same place? Would you recommend it to younger scholars in Language and Superdiversity to have that historical sense, and try and go back over time?

DP: I think it's an option. I think they'd need to think about it, and of course, their interest has to be sustained. It may well be that some people think that they need to look at other social situations, cultural situations to sustain their interest. But it certainly is an option to do what I did. The other possibility, is one you alluded to, which is teamwork. It seems to me, it should be possible for people who interact reasonably easily with each other – within the department or across departments as part of a network or consortium – to be able to share that task. And for younger people to relate to older people or more senior people who worked in the field. So why not? That should be possible. If you've worked in a certain part of the world, and you are younger than me, and I'm older, why not come together? I think, in fact, it'd be quite a good option, because it would bring about collaboration, which is not just for its own sake, but is focused on a particular problem – the longevity of language use.

RH: Perhaps more senior scholars could open up their historical material to younger scholars, or go into the same location to achieve that same effect.

DP: That's right. There are archives and there are recordings, quite a lot of stuff that people do have. And increasingly so, as time goes on. In that way you share it. This is why I think InCoLaS or the consortium is very important, because we are in a position to do that. It doesn't matter

where people go as long as they remain connected to each other through this focus, which a department can't necessarily provide because departments consist of people who move on to other departments, or retire. Paradoxically, the department is not the best way for people to keep in communication with each other. Whereas an organisation like InCoLaS probably is, as long as it is sustained. Which is why one is very keen to do that. So I think I'd prefer that second option of people sharing their resources, and their notes. I don't think people worry about such concerns as that "This is my data" or something of this kind. The only slight worry that I have encountered in the past, with one or two students, is when I offered to give them my notes on Giriama or Luo, sometimes they'd said, "I'd rather not have them, I'd rather arrive at my own conclusions, my own ethnographic discoveries." And I'd have to respect that. I can understand why they'd want to do that. So then I said, "If at any point you want to use them, in fact, I'm going to put them on some website so they can be open to public access." That would be the only obstacle. But maybe then one can say if it's only language data, then that doesn't matter, because after all it doesn't affect their ethnographic observations. But that's obviously something to be negotiated.

RH: One bit that's linked to this senior scholar-more senior scholar issue is, earlier on you talked about this stage in your career when you wrote a paper, and you said Dell Hymes and John Gumperz liked it. Could you say a bit more about that scenario and how you saw those people at that time? What was the context in which you said they liked it? How did they take it? What kind of figures were they in your eyes?

DP: It was a very exciting time. This was, I can date it, December 1968 or January 1969, around Christmas or the New Year. It was at a conference organised by Wilf Whiteley, and it took place in Dar es Salaam in Tanzania. Apart from Whiteley himself – who was really quite brilliant in his own way, I know he's not much heard of, but actually he was quite inspiring – invited were a number of people including Dell Hymes, and John Gumperz, and Fishman. So you had, you know, the big three. It was incredible to be with these people. I was just a whippersnapper in my twenties. And they were all very impressive, I have to say. And I gave my paper. It was just a descriptive account, really, of people of different ethnic backgrounds, all men it so happens, in a bar at one point joking with each other, and then one of them says something that offends the other. And so there was tension, if you like, a 'crisis'. So I analysed it like a ritual crisis: something happens, what do you do with it? As I said earlier, when there was a crisis, people would revert to their own vernaculars and withdraw from their collective amity, whereas the amity itself was expressed in the use of Kiswahili the lingua franca, or in some cases English. It was a simple point, but the fact that it was illustrated through a case study, is why they liked it. I remember John Gumperz telling Dell Hymes, " This is a good illustration of the ethnography of speaking", and Hymes agreed. That's the very phrase he used, which I remember because

I was very pleased. Great John Gumperz speaking to the great Dell Hymes. And Joshua Fishman was very articulate, a very intelligent man, producing a different perspective in his work, a different agenda if you like, a different approach. And he and John Gumperz argued ferociously, which is good to see too when you are in your twenties and you see these great stags locking horns. The paper was later published in, I think it was called, *Language Use and Social Change*.

Another influential person, by the way, who actually figured in that conference, was Aidan Southall, who was one of my supervisors. Again, he's probably unheralded and yet should be much, much better known. He wrote a paper – which was one of a number – which tried to show the fuzziness, to go back to that term, of language use and boundaries. He had this formidable knowledge of Nilotic languages, not just that of the people he worked among, the Alur, but some related Nilotic languages, including Luo. And he also had an understanding of Bantu languages and linguistics. He had some knowledge of Luganda and Kiswahili, because he lived in Uganda. He was one of those rare people who went out in 1946 and stayed there until the mid-60s, continuously. He wasn't one of those who came back after first fieldwork to a position in the UK or the USA, only doing so much later. So he had 20 years or so of actually living in Uganda. Fantastic. He knew the languages. So he gave this wonderful paper in which he tried to show how key concepts or key words floated around the different communities, which were extensively different linguistic communities, but which were in fact sharing notions common to them all without necessarily acknowledging this. And he did similar kinds of analysis in other publications.

RH: Was that published?

DP: Oh yes, it's published. It's a very good book, *Language Use and Social Change*. Fashions come and go, but it's a book which is still relevant today.

RH: Well, thank you very much for that really illuminating, interesting survey of some of your influences and experiences. I think I have enjoyed it and I'm sure Karel has. Thanks very much.

DP: Thanks.