2011 Personal Histories
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An Oral History of Primatology at Cambridge
Part I
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As some of you may know, the Personal Histories Project was set up by Pamela Jane Smith of the McDonald Institute for Archaeological Research. It is student led and entirely funded by donations. I am Hanna Sainsbury, a second-year archaeology student, and I am one of many members of this university and of other universities who are part of the team who create events such as these. I would like to, first of all, on behalf of the team, thank you all for coming today. We are extremely grateful for your support and delighted to have you here. We'd also like to thank our panellists for joining us today. We are extremely fortunate to have you here and honoured.

Before I leave the stage and let you listen to the people you want to be listening to, I have a few safety announcements. Firstly, the fire exits can be found, two at the back and one down here on the right. There are toilets in the foyer where you entered and could you please make sure that all mobile phones are switched off. We are filming today so if anyone has a problem with being in the film please make sure that they are sitting behind the cameras. It is our returning film crew who are going to make an educational film for students which will be available on iTunesU. There is a question and answer session at the end of this talk. If you do have a question, please wait until we can get you a microphone before asking the question. Any student who asks a question will receive a bottle of wine. (Audience laughter)

May I please introduce Professor William McGrew.

Professor William McGrew

Thank you very much. Welcome from us as well. I hope we will be able to provide what you need to hear or wish to hear. You never know with these occasions. We will see how it all roles out. I am not going to spend time on lengthy introductions. We have better things to do in terms of story telling and those sorts of things that wouldn't be in the books or the papers or the articles. Without any further, taking more time, I would like to introduce Professor Robert Hinde who will lead with personal history recollections of primatology at Cambridge University.

Professor Robert Hinde

I feel as though I really shouldn't be here because I am not a proper primatologist. I was an ornithologist when I started and I spent nearly all my time when I was an undergraduate on the Cambridge sewage farm which was the most wonderful place for studying wading birds. Then I went my first appointment was to go and work with David Lack at the Edward Grey Institute of Field Ornithology in Oxford. That was, as its name implies, entirely an ornithological enterprise. I was studying Great Tits and all I used to do was to wander around

the wood with a pencil and paper and write down what I saw. That was a wonderful place. It is White and Wood outside Oxford, I don't know how many of you know it, but it is full of blackberries and sweet chestnuts, primroses and everything. You couldn't have a nicer place to work except some people would say Gombe. Right? (Audience laughter)

The good thing that happened to me during that period was that Niko Tinbergen came to Oxford and I learnt more from him, to be honest, than from David Lack. And, when I left Oxford, I called myself an ethologist rather than an ornithologist. At that time, W.H. Thorpe, an entomologist, was setting up a station outside Cambridge, part of the Department of Zoology to study the relation between instinct and learning, as it was called; that was the terminology of the time. He really wanted me to work on bird song but I didn't want to do that. He said that I could do what I liked and I was appointed as Curator which was really a ridiculous title because it was only me and the lab assistant and a field with nothing in it. We spent the first year making aviaries out of metal that was left over from the beach defenses during the war and wiring them up, creosoting them and all that sort of thing.

At first we had no money at all. I had to write down every ha'penny stamp I used in a special stamp book and every quarter pound of nails that I bought in another book. It was really on a shoestring. But, a few years later, it became much better and the United States Air Force supported us for six years to study the nest-building behaviour of canaries. They insisted that I should say in my application that I had been a pilot in the RAF and that made it quite clear that I knew the relevance of this to the United States Air Force. (Audience laughter) Actually, when it came up for renewal a few years later, he insisted that I should tell them that I took the nest material away from canaries sometimes in experiments and we called it 'sensory depravation in canaries'. That was just getting into the space age and that made it all right.

I did various studies there, one of them was the study of imprinting which, for those of you who don't know, is the learning process whereby young birds know what their parents look like and respond to them. At that time, John Bowlby, a London psychoanalyst and psychiatrist heard about ethology and we met at a meeting and he had a great influence on what happened subsequently. He was interested in what happened to children who were separated from their parents. He had done a study which he called "Forty Four Thieves" of juvenile delinquents and found that a very high proportion of them had had separation experiences in their youth. So, he asked me to join his seminar. That was one of the most wonderful experiences of my life. We used to meet once a week in a dirty basement, down in Beaumont Street in London. The seminar had a Freudian analyst, a Kleinian analyst, a Hullian learning theorist, a Skinnerian learning theorist, a Piagetian psychiatric social worker, an anti-psychiatrist and me as the animal chap. And, we had absolutely nothing theoretically in common and that was the most wonderful thing. It taught me that what matters is not the theory but the data.

John Bowlby wanted to show that maternal separation mattered and he couldn't do experiments with children so he helped me set up a monkey colony to do experiments with Rhesus monkeys. We chose Rhesus Monkeys because that was the easiest primate to get hold of at the time. This was about 1958. We set up a colony in which there was six long outdoor cages, each, I don't know, about 20 feet long I should think, each connecting to an indoor room. In each group there was a male, three or four females, and their young. In, I think it was 1961, we had our first babies, whom we called Yuri and Vostok, because Gagarin had just been around the world and we thought those names were appropriate.

We had to learn a lot about monkey communication and we wrote about their signals, their vocalizations, their facial expressions and bodily postures, and so on, in a very primitive sort of way. But, it was a start in the study of primate communication. Then, we got on to the separation experiments and we separated the baby monkeys from their mothers for ten days and were able to show at the end of two years that it had had some affect on their ability to cope with strange situations. I can't go into all the experiments but they involved, "baby goes to hospital and mother stays in the group", "mother goes to hospital and baby stays in the group", "they both go to hospital, the same hospital" and "they both go to different hospitals." So, we could tease out what was important in the separation experience. I should say that the response of Rhesus monkeys to separation was very, very similar to that of human children. That is how I got into primatology not for monkeys in their own right but to try to solve a human problem.

Enter Louis Leakey, the archaeologist, palaeontologist, whatever you would like to call him; he, at that time, had the view that the way to understand the origin of man was through the behaviour of the great apes. He felt and was convinced that young women would be better at this task than young men. So, he got young women out to East Africa and put them through tests to see whether they could stand up to his demands. None of them passed until Jane came along. And, then, Jane did a wonderful study of, was it vervet monkeys on an island in Lake Victoria, and that met his criteria and she was established with Van, her mother, on the Gombe Stream in Lake Tanganyika. A little later, Leakey established Dian Fossey on the Virunga Volcanoes in Rwanda to study gorilla. What was nice for me was that I was appointed their supervisors because he wanted them all to get Ph.Ds to be respectable. Jane had no first degree and Dian was a physiotherapist, I think. We found a regulation, it was actually Bill Thorpe who did it, a loophole in the Cambridge regulations that if you have done something distinguished, like being the master of a ship for instance, then you would go straight into a Ph.D programme. (Audience laughter) Both Jane and Diane passed that criterion.

I used to go on visits to Gombe and to the Virunga Volcanoes and it was absolutely wonderful for me because I had all the fun of the fieldwork and all the fun of seeing the students at work and none of the hard work. I used to go down to the lake and swim in the afternoons much to Jane's disgust instead of watching the chimpanzees all the time. (Audience laughter)

There was a succession of graduate students who came to Gombe and to the Karisoke Research Centre in the Virunga Volcanoes and I was lucky to supervise a succession of them. So, I gained experience with a number of species of primate and also with elephants. It all sort of oozed over into elephants at one point. It was all wonderful for me.

I claim to be the first person who has actually been hit by a stone thrown by a chimpanzee. (Audience laughter) And, I was also the occasion for Jane recording for the first time the use of leaves to wipe, to make themselves clean, because a baby chimpanzee went up in a tree just above my head and stamped on it and then swung away and then carefully picked some leaves and wiped the bottom of his feet (audience laughter) to take the stink off. (Audience laughter)

I also had a nice experience in Dian's camp because the first time we saw the gorilla, there you had to track them and find them. The first time we caught up with them, as soon as they saw me, they took off. Jane said (my hair had just gone grey) Jane said, "They think you are a silver-back.

Put your hat on." (Audience laughter) It is my great failure as a scientist that I put my hat on and I never took it off again to do a properly designed experiment. (Audience laughter)

My great achievement at Gombe, which nobody has ever yet emulated, was that I convinced the bureaucrats in Cambridge, the Accounts Department, because all the Gombe accounts at that time went through Cambridge, I convinced them that a thumbprint was an adequate receipt for £3,000 worth of bananas. (Loud audience laughter and clapping) I don't think anybody will ever do the same again.

Well, about my relations with Jane. I think I ought really to let her say what she thinks. I mean it was marvellous for me but what she records in her little chapter that she wrote on her time as a student at Cambridge, was this that every time after she saw me, she write "back to my digs, I would haul everything into the corner of the room, page after page, written so carefully, now marked all over with Robert's comments and criticisms. How desperately I longed to give it all up and go back to the chimpanzees in the forest." (Audience laughter) The thing is that Jane was going through what I had gone through with Great Tits. I had originally taken handwritten notes and had to turn them into scientific material. And, she had done the same. So, part of my role with Jane was to help her turn her notes into scientific material.

I also helped future record keeping by introducing check sheets into the method. And, I think it was the first time check sheets were used. They have been improved in many ways since, of course, but it did enable the recording to go on, to some extent, systematically year after year.

One of the questions that came from the students beforehand, was, "Was I the Father of Primatology?" and the answer is certainly, 'No'. But, there was an American student who tried to trace out the genealogy of primatologists and it turned out that Sherry Washburn and myself had had more students than anybody else, more academic descendents. But, when she published it, she decided to publish only Americans. That is how it goes. (Audience laughter)

After a few years, I thought I could do more good in the world by studying humans, rather than monkeys, and I turned over to studying four year olds. We studied the mother/child relationship in four year olds and how the four year olds behaved in preschool. Joan Stevenson Hinde did the work in the homes and I did the work in the preschools. It was marvellous because I was the only male in the preschool. So, it was me who was sent out to clear the snow out of the gutters or mend the fuses at the same time as being the white-haired professor who comes to watch the children. Joan Stevenson Hinde did wonderful work and became one of the leaders in 'Attachment Theory', especially in the US, whereas I made a foolish error and thought I could use the same type of recording methods that I had used with monkeys and chimpanzees with four-year-old children. That was just a big mistake. I did a few years without doing anything really, in the long term, of any use.

One of the questions, and here I will finish, one of the questions that came up was, "Did my work with monkeys trigger, or contribute to my work, the last 20 years since I retired, in the peace movement?" And, the answer is No. When I started in research I was happy for a few years to have been a RAF pilot, there was sort of kudos attached to that, then I slowly came to realize how terrible war was. My brother had died of wounds very slowly in an open boat and my best friend was killed in bomber command and a lot of other friends died. I joined an organization called ex-Servicemen CND, which tried to take the hippie image out of CND,

because when CND was portrayed in the media, and it is still the case, it is always portrayed as a bunch of hippies with torn jeans and all that sort of thing. So, we used to go along on the marches with our medals and proper suits, ties and all that, and try to make it respectable. After that I got into Pugwash, an organization primarily concerned with abolishing nuclear weapons. I have had a wonderful retirement in which I have been able to work both for peace and for which I felt my time as a biologist and psychologist gave me some insights into the nature of religion.

William McGrew: Thank you very much. May I call Professor Richard Wrangham to the podium

Richard Wrangham

It is a great honour to be here. It is slightly daunting. I have two of my major advisors here, Robert Hinde, who was my academic advisor, and Jane Goodall who was my field advisor. But, I am here to talk about the oral history of primatology in Cambridge and I can tell you that all is just exactly as it should be. It is all exactly right. Before I go onto the Cambridge history, we could think about a fairy tale in which the ultimate deep meaning is that everything is all right; a fairy tale seems so appropriate for us here. It is Goldilocks and Three Bears. (Audience laughter) It is obvious who Goldilocks is, and we have a couple of spectacled bears (looking at Robert Hinde) but I learned quite a lot about fecal analysis of chimpanzees from Pooh Bear (looking at William McGrew). (Loud audience laughter)

What I want to talk about is the 1970s. The big picture for me is that, whereas primatology now has become, from my point of view at least and from many people's point of view, something that is really intimately involved with humans, the primatology at Cambridge in the 1970s was much more a part of the study of mammals in general. This is somewhat ironic given that Robert has just talked about how he became a primatologist through John Bowlby and thinking about humans. But, in general at that time, the study of primate behaviour was seen through the lens of many different species, very much from a biological perspective. At the same time, just as Robert had been introduced into the field by John Bowlby's question about using monkeys as a model for understanding a problem in humans, so there clearly was a human interest that justified trying to understand primates in general.

The 1960s were an incredibly exciting time. For those people who are interested in the ultimate enlightenment question of who humans are, where do we come from and what is responsible for our social behaviour, you had writers like Robert Ardrey and Konrad Lorenz who were synthesizing the nascent results of various different field studies of animals into a picture that included, 'Well, maybe this is how humans fit in'. At that time, there was very little field primatology indeed. I remember Irven DeVore saying that, when he started his Ph.D in, I think it was 1959, on his way to his Ph.D site in Kenya, he went to eight out of the ten field studies that then existed in the world. And, when he produced his 1965 book on primate behaviour, almost all of the field studies in the world were represented in it. His book contained only about a dozen chapters.

There was beginning to be an understanding of the question that really interested me, namely, what is responsible for species differences in social organization. That understanding came in a sense ultimately from Robert, because a student of his, John Crook, had studied birds in West Africa and was trying to put together a story about why different species of birds lived in groups of different sizes and different stability. That was the background which fascinated me when I graduated from Zoology at Oxford, thinking about, "What can we learn from animals to understand the evolution and diversity of human social systems?"

I know that I had that interest because I can remember what I tried to do. I thought, well, humans are a diurnal, social carnivore. (Scholars were focused on meat-eating as an influence on human evolutionary ecology.) So, where were the diurnal social carnivores? I ended up wanted to study the banded mongoose, not many people's first choice of a model for human social evolution maybe (audience laughter) but actually it is coming around again now. I tried to get a grant, and I didn't, so I ended up having graduated, going to my Tutor in Oxford and saying, "Woe is me, got any great ideas?" And, he said, "Well, you know my daughter." "I sure do." (Audience laughter) My Tutor was called Harold Pusey and he was the world expert on the condrocranium of the frog. I decided not to go that route. (Audience laughter) His daughter was Anne Pusey and she and I were classmates in Zoology at Oxford. She had already lined up a position working with Jane Goodall. He said, "Why don't you do that?" So, that is how "hard" it was to get to work with Jane in the 1970s. I wrote to Jane and I can't remember how long the letter was but that was something like July 1970, and she said, "Oh, I'll be in London in September, come and see me." And, I had tea with her, and she said, "Yes, absolutely splendid, why don't you come in November?" And, there I was. (Audience laughter)

The wonderful, relaxed, way in which Jane introduced me to Gombe was a perfect introduction and representation of the opportunities that she gave when she was there. This was the first week of November in 1970. I think Jane's son, Grub, was about three and Jane was wanting a few research assistants to help develop the study for her because she was having to move out of Gombe since it was not safe for Grub to be there. She spent a lot of her time in Serengeti, which was great for people like me, because she came back with wonderful stories of all of the social carnivores that she and Hugo were studying, adding enormously to what we understood about the diversity of social behaviour. As it was, six or eight pretty raw students were working together in Gombe. Jane gave me the opportunity to follow the relationships of siblings. So, for a year, I had no duties other than to take the eight siblings that were there and follow them wherever they went.

This was a very exciting time at Gombe because it was the phase after Jane had decided to stop the banana feeding. Provisioning bananas had been a really wonderful tool for enabling Jane and her team to get very close-up observations of chimpanzees for the first time in the wild. But, now was the time to cut back the, what was it, the £3,000 banana bill that Robert talked about and allow the chimps to be less focused on this provisioning area. So, it was an exciting time for us because, whereas Jane had been following chimps wherever they went prior to the banana feeding, as I understand it, she was not able, at that stage, to follow individuals all day. She had rarely reached the point where the chimpanzees were having interactions with their neighbours. That is what those of us who were there in the early 1970s were able to do on a regular basis for the first time.

So, we saw the way in which different classes of individuals behaved differently from each other. Chimpanzees live in a fission/fusion society which means that individuals do not stay in stable groups. This system presents a wonderful opportunity to make sense of the relationship between ecological pressures and social behavior. The great majority of the relatively few studies of primates, at that time, trying to relate ecology to social behavior, were working with stable groups which meant that they were not able to dissect the separate interests of the females and the males or the young and the old. But chimpanzees were voting with their feet; each individual went where it wanted to go. So, if you were there for a year, you saw that there were times when the groups were large and times when they were small. There were times when the females did one thing and the males did another. When Robert Hinde and David Hamburg, a professor from Stanford, visited in 1971 and I met them both for the first time, it was incredible to be able to go to them and say, "Is there any chance that I could do a further study?" And, Jane, bless her heart, supported this and David Hamburg provided the funding and Robert said, "Yes, okay." (In those days you were not allowed to come to Cambridge unless you had funding already.)

So, I went to Cambridge which was an extraordinary privilege because of the group of people that were already there. Not just chimp people, like David Bygott and Pat McGinnis . . . there was Tim Clutton-Brock, who had been studying red colobus ecology in Gombe and then there was the gorilla group with Dian Fossey, Sandy Harcourt and Kelly Stewart. There was a baboon group, Dorothy Cheney and Robert Seyfarth (and in Gombe there would be Anthony Collins) and there was a rhesus monkey group. The point about this is that all of these people came through at this time with pretty much new information as far as primatology was concerned. And the essential point about the new information was the consistent discovery that individual relationships really mattered. For the first time, the questions about primate social organization were not so much about just group size and stability, it was "Why is it that in species X, the females are really concerned to stay with their kin, and in species Y, the males will do so at one time of year and not at another?" And so on.

Also in Cambridge, I want to mention, there was another strain of primatology which was led by David Chivers, presenting a very important ecological background with some of his students working with gibbons. So, for me, this mixture of specialists was a thrill.

There was a complementary intellectual input which had happened in Gombe when Robert Trivers, from Harvard, visited in 1972. For those of you who know the history of 'inclusive fitness theory', you will know that it wasn't really popularized until Ed Wilson's book of 1975, *Sociobiology*. But, when Trivers came to Gombe in 1972, he was coming with many of the essential stories of the growing field of 'inclusive fitness theory'. He came to Gombe just after he had published a paper entitled, 'Parental Investment and Sexual Selection', which was the paper that drew people's attention to the fact that from a biological perspective you can analyze females and males as having different kinds of interests. Nowadays, this is completely routine. At that time it wasn't because most scholars focused on the 'group' as the unit of adaptation. When Hans Kummer published an important 1971 book called "Primate Societies," he sub-titled it "Group Techniques of Ecological Adaptation". That was 1971. By 1972, we were starting to think much more deeply about females and males experiencing different selection pressures from each other.

While I was in Cambridge, I had a visit from Peter Rodman who had been studying Orangutans. He had been a student at Harvard with Robert Trivers. Orangutans are very solitary. Their

females and their males can be very easily understood in terms of the strategies of the females to maximize their access to food all the time and the strategies of the males to maximize their access to the females. Chimps proved similar to the orangutans in important ways but also different because the males were much more gregarious. Peter and I were able to build off each other's work in a very satisfying way as we saw the similarities and differences between our findings.

In 1971, I finished my Ph.D. After that, I was away from Cambridge for a year or two. I went to study gelada baboons in Ethiopia as a post-doctoral student of John Crook's. While I was sitting in Addis Ababa, I got the news that four students had been kidnapped from Gombe. I think it was a week before that I had been threatened with kidnapping by people in Ethiopia, and about three weeks before Stephanie Tyler, who had been a graduate student studying horses under Robert Hinde, was kidnapped with her husband and two children. She spent a year with the Tigrean People's Liberation Front. By the way, I had said to her earlier, "I think I am going to leave. I just don't like being threatened with kidnapping." And, she said, "What kind of wuss are you?" (Audience laughter) So, when she was kidnapped, I thought, "Ah!" (Loud audience laughter) I think I did wait a day until after she was released before calling her just to see what she was going to say. (Audience laughter) Robert keeps telling me to pay more attention to birds. She said it was absolutely wonderful. The great thing was that, "We were kidnapped together with our binoculars!" (Loud audience laughter) She had been the Secretary of the Ethiopian Orthontological Society. She said, "We got into areas where people haven't been able to get into for years!" (Loud audience laughter) I realized, at that point, that I couldn't really hack it as a field primatologist. (Audience laughter)

So when there was an opportunity to join the Sociobiology and Behavioural Ecology research group in Cambridge in 1977, I did so. I joined Tim Clutton-Brock, Brian Bertram, Dan Rubinstein, Robin Dunbar, a group of people who, by now, at the end of the '70s, were not so much developing, in some ways solidifying some of the early gains in understanding the evolutionary pressures on social relationships. It was at that time that I put together the growing information on some species having bonds that are more important among females, and others having bonds that are more important among males. I developed the notion that the female-bonded species were those in which, thanks to the distribution of their food, females are able to cooperate with each other to defend against the food. The ones that are not female-bonded are the species in which there is no particular advantage to females to defend their food in that way, leaving males the opportunity to compete with each other by harassing the females or committing infanticide or killing members of neighbouring groups or whatever other charming things they wanted to do.

So, that was my Cambridge story. But, I do go back to Cambridge in a way. In 1980, I left Cambridge, partly to go to spend nine months working with pygmies in the Congo. Elizabeth, who is here, and I were married in that year and we thought it would be nice to do something new for both of us. Our Congo research was my introduction to anthropology, as it was hers. I thought it would be interesting to go and compare what pygmies were doing in a forest, compared to what chimpanzees were doing in the same forest. As it turned out, I actually didn't have much time to make that comparison. I had thought, "I am really interested in people" in the end so why not study people. But, then, I discovered the problem with studying people. They talk to you! (Audience laughter) It is not that I am inherently anti-social; it is just that it is so much harder to get an objective take on what is going on because you are just writing down what

someone is doing, and they are saying, “What did you have for breakfast?” That year was as long as my research with people lasted.

I went to America after that and joined an anthropology department because I was a postage stamp in Dick Alexander’s collection. Dick Alexander was a zoologist who was very taken with ‘inclusive fitness theory’ and I think I was the 7th person to be collected by him and deposited somewhere in a department at the University of Michigan. I went to anthropology and started learning something about anthropology.

In 1983 I went to Stanford and joined Bob Smuts, Robert Seyfarth, Dorothy Cheney, old friends, and Tom Struhsaker, and we edited a book together called *Primates Societies* which we are all very proud of because, in about six months time, a successor will be published, some 25 years later to be edited by John Mitani and colleagues. It was as a result of a suggestion from Tom Struhsaker, that in 1984, I visited Kibale National Park in Western Uganda. I was looking for a site to study chimpanzees because, by this time, I had studied vervet monkeys, people and gelada baboons and realized that what I really wanted to do was to build on what Jane had established in Gombe by establishing a new study of chimpanzees. At that time, the number of chimpanzee studies was growing. But, it was clear that there was room really to try and represent this extraordinary species by as many studies as possible to capture its cultural and behavioural diversity. And Tom Struhsaker had a student, Isabiriye Basuta, who had been working in Kibale and had habituated a community of chimpanzees to some extent. I made a couple of pilot study visits there in ’84 and ’86 and started permanently in ’87. I think the first ten times that we saw the chimpanzees they just jumped down from the tree and ran. It was four years before we saw them regularly on the ground. It was 11 years before we started getting high-quality data and it has been completely worth it.

In 1989, I came to Harvard. This is where the Cambridge connection comes back, because David Pilbeam, who started his career at Harvard, has probably been my most important colleague there. It was he who had urged Charles Sibley, an ornithologist interested in DNA to turn his techniques on apes and who had discovered that humans and chimpanzees are more closely related to each other genetically than either is to gorillas. And given the morphological similarity between chimps and gorillas, the only reasonable interpretation, or the most reasonable interpretation of this is that chimpanzees provide a remarkably good model of the last common ancestor of chimps and humans. For me that is an extraordinarily exciting observation because of the amazing things that we have learned about chimpanzees through Jane’s eyes and those of the many colleagues that I have worked with.

The sum for me is that we have come full circle. Primatology in the 1970s in Cambridge was very much rooted in the study of diverse mammals. It did not come from anthropology, unlike American primatology. It had three great things going for it. The 1970s were this tremendous age of discovery where, for the first time, people were really getting to grips with what basic animal social relationships were like especially in the social carnivores and primates that many of us were particularly interested in. That series of basic discoveries was combined with the fact of ‘inclusive fitness theory’ being developed for the first time. Finally, for the first time, there was a serious effort at systematization of data collection. Robert was very much responsible for that. For those of you who are modern primatologists, the great paper that everyone cites in this respect Jeanne Altman’s 1974 article on the methods of behavioural observation. In many ways,

her paper introduced a way to collect data that people were unfamiliar with. But, those of us who had been at Madingley with Robert Hinde knew those methods already.

So those things combined to make for a very exciting time. I just feel incredibly lucky to have been part of it. Thank you.

William McGrew

Thank you. I shall introduce myself. I am here, somewhat as an imposter, because my Cambridge history is very shallow. I have been here only for five years and my formative years and my doctoral studies were at the Other Place, which has been mentioned before by the two previous speakers, where the external examiner of my D.Phil. was Robert Hinde. At Gombe, I started in 1972, but as you have already heard, they were ahead of me again. All three of the panellists were there long before I was. So, the only thing that I can say that might justify my standing before you for the next 15 minutes is that I believe that, apart from Jane, I'm the only person to have collected data on chimpanzees at Gombe in the '70s, '80s, and '90s. Perhaps, I will say a bit about the '80s and '90s, but I cannot avoid going back to the '70s for reasons that Richard mentioned.

When I came to Cambridge, I succeeded Phyllis Lee as the primatologist in residence in Biological Anthropology. Oddly enough she went on to go my old job at the University of Stirling in a kind of strange, delayed swap. But, when I came here, my goal was to establish a small, focused group of chimpanzee-chasers, people who were keen on Ph.Ds and were willing to chase wild chimpanzees right across the continent of Africa. No, I don't mean that. I didn't send Paco out to chase them from Senegal to Uganda. (Audience laughter) What I meant was to place people across the continent of Africa. So, folks like Kat Koops, is now back in the Nimba mountains of Guinea and Paco Bertolani who worked both in Senegal and then later with Richard at Kanyawara, Fiona Stewart, who did a two-site Ph.D, working both in Senegal and in Tanzania, Caroline Phillips also working in Nimba and in Kanyawara and Susana Carvalho who has worked in Guinea at a couple of sites there. And, when these five complete, and you ARE out there, and I know you are all going to nod your heads and say, "when they complete this year," when I retire in September, the last one will be the 15th Ph.D in chimpanzee chasing that has come out of the University of Cambridge in field primatology. We can put that up against Richard at Harvard and see who has got the most. (Audience laughter) What this comes down to, to cite a point that Robert made, is that the folks who have been these 15 Ph.Ds in chimpanzee chasing are the academic inheritors, the grandchildren of Robert and Jane. That is an important thing in terms of academic genealogy.

So, what about some Gombe memories? Well, Richard has alluded to Stanford University which was important in the '70s as a source of funding and as a source of young people. Undergraduates studying Human Biology at Stanford came out in cycles over a period of a few years, and many of them went on to make their mark in primatology at later points, people like Craig Packer, Jim Moore and so on. But, these studies were cut short in May 1975 by the Gombe kidnapping which involved all expatriate research being shut down because of the security issues that Richard has alluded to.

So, the 1980s were a lean period with regard to expat researchers. There was in effect a ban for some years. But, in 1981, Ant Collins and I succeeded in slipping in and Ant is somewhere in the audience, thank goodness. We were allowed to study only for brief periods as long as we kept ourselves inconspicuous. And, so, we tried to do. Gombe was very quiet and it was the loveliest period to be there of all my times there. However, it was a time when Tanzania was economically challenged, let us say, getting supplies was difficult and every time we went to town we never knew what we would come back with. I remember a period when we had to cook with cottonseed oil, which I don't recommend. It is a recipe for perpetual (I won't say constipation) indigestion. (Audience laughter) Most of the time we were there, we spent transecting. We replicated the methods of Tim Clutton-Brock. I wish to know this for sure but I believe he is the first Gombe Ph.D in primatology. So, it wasn't a chimpologist with the first Ph.D from Gombe. I think it was Tim on the red colobus study. But, what saved Gombe as far as I can see during the 1980s was the stalwart fidelity of the Tanzanian field staff who kept the studies going, who maintained the continuity of the research in the absence of the expats for a number of years. This is an appropriate time to pay tribute to Hilali Matama, the head person, the senior field assistant, who died recently.

In the 1990s, by the time I came back in 1992, things had picked up, enlivened particular by Janette Wallis' time as research director. I came with Linda Marchant and Craig Stanford. When we got there, the old housing that we had lived in, in the old days, had all disappeared and new additions had appeared in staff camp in the field assistants' housing. So, we had to do a little bit of new building and got a new hut. It was only a one-room hut but it was spacious and close to the beach. The real estate market was so crowded that another person working there, Ph.D student called Charlotte Uhlenbroek, had to build her house to the north of Jane, along the shore, which was previously *verboten.* The thing that I remember most, and this is a sad one, was that Craig came home one day in a very grumpy mood and said, "Your chimpanzees have eaten 25% of my colobus." (Audience laughter) And, I thought is this some cumulative total that he has been keeping over the years and he has now reached the threshold? No, in ONE day they had eaten seven of the 28 of his monkeys from one of his main study troops. I think he had good reason to be upset.

So, now in contrast to the very erudite tone that Richard has taken, covering some profound issues in primatology, I'm going to go down-market and tell some stories and I hope you can put up with that. What I have discovered is that the older you get, it is easier and easier to tell stories because there are fewer and fewer people around to contradict you. (Audience laughter) Right. The people who at one time knew aren't there to be a problem and as one remembers less and less, it is easier to make up more and more. (Audience laughter)

So, in Gombe's heyday, in the early 1970s, as Richard has referred to that period, we were crowded with researchers. Literally, there were times when 20 people would gather in the mess for a three-course dinner (of course!). They were wonderful meals produced by the redoubtable Dominic and his assistants and many of those present were Stanford undergraduates. These keen, fit, bright, California surfer-type kids. So, it was inevitable that this would lead to water skiing. (Audience laughter) Now, I defy you to find in any of Jane's many books mention of waterskiing. (Audience laughter) I think this is because we never did it when you were there, Jane. (Audience laughter) Where does this fit in? It does fit in. Gombe is a site that is reachable only by boat. There is no road travel into Gombe. There is no airstrip, so you can't land a small plane. There is a rail connection to Kigoma but that is still some distance away from Gombe.

So, either you would have to walk in, which would be inconvenient, or you come by boat. And, Gombe like all places along the lake had this standard, local-style boat for shifting goods and people up and down the lake. But, for reasons, well whatever, there was a speedboat, a fibreglass speedboat that was bought for emergencies, in case someone had to get into town quickly or to retrieve visiting dignitaries and bring them in comfort to Gombe. So, given the Stanford students and the Beach Boys music of the time, it was inevitable that some Stanford student would bring water skis to Gombe. What gave it an extra *frisson* in terms of actually taking part was, "Would this be the day when you were water skiing and you met a crocodile or hippopotamus?" It was an aspect of Gombe life that needs to be revealed even at this late time. (Audience laughter)

For you young people in the room, I know this is very hard to imagine, but there was a time before CDs and DVDs and Kindles and iPods *etc*. What we had in those days were cassettes. I know you don't know what these are, (audience laughter) but they are little plastic things and there were two spools and this thin thing that went round and round. But, often it didn't go round and round, (audience laughter) because in Gombe, in the dry season, they filled up with dust and jammed. And, in the wet season, they expanded with the humidity and jammed. So, you had a fresh cassette and it lasted for a while and that was that. So, we turned to homegrown entertainment.

I am just going to cite one example of the good old days, when we devised amateur theatricals. A Stanford student remembered very fondly, Julie Johnson, wrote a skit called, "The Great Gobber" which was loosely based on Scott Fitzgerald's *The Great Gatsby*. All the characters were chimpanzees played by human researchers. (Audience laughter) It was essentially a coming-of-age drama with Goblin, the adolescent male, seeking his place in adult society. I played Mike who was the wise, old, technological innovator chimpanzee, and Richard played Figan, who was the confident, rising male, the object of Goblin's hero worship. Now, it must have been hard for Richard (audience laughter) to play such a role. He, of the shortest of all short-shorts by the way, which is another story, (loud audience laughter) being such a shy and retiring guy. The only problem with these skits, these productions, was that everyone wanted to take part. So, we had precious little in the way of an audience. We would sort of dragoon some of the Tanzanian field assistants to come along and watch in a bemused fashion as these people behaved in a strange manner. Or, Jane would sit there patiently, sitting through these things, (audience laughter) probably wanting to get down to the beach and have her sunset whiskey.

What I really want to say is that I am asked many times what is Jane really like? This is a common question. The subtext is, "Is she really so saintly as she seems?" Now, some things about her are predictable. The love of animals that is mentioned in her own autobiographical writings and also in Peterson's big biography certainly applied to Gombe because in addition to the subjects of study, like the chimpanzees, the baboons and the colobus, we had Minnie, the gender-challenged mongoose. We had Crescent, the best-fed genet there ever was. We had Stump Tail, the highwayman olive baboon, and so on. All parts of our daily lives.

Jane has many talents that have not been revealed in the scientific literature. At one time, and I suspect still, she has extraordinarily prehensile toes. I remember you, Jane, picking up objects in the mess and demonstrating this thoroughly. I remember that you were a maker of very tasty sherry trifles, heavy on the sherry and tasty accordingly. But, one more thing comes to mind. Someone told me recently, Jane, that you had been interviewed on television by a young woman

who asked if you were a bit flirtatious. Apparently, you said that you were. I don't know what that means, but it did remind me of a story. (Audience laughter)

When the Canadian High Commissioner came to visit Gombe, Mr. Barker, take note of that surname, Mr. Barker . . . This needs a bit of background of information. In those days, with such a demand for Gombe, there were many people who wanted to come and Jane was always saying yes, "Add one more person." Gombe constantly needed new resources, new equipment, new accommodation, new facilities. And, sometimes when visitors came, if they had a good time, they would feel inclined to write a cheque maybe help out a bit or to show their generosity in some other manner. I do remember an evening when Jane somehow ended up sitting on Mr. Barker's knee in the mess (audience laughter) and she said something like, "This is so much fun; we are having such a good time. From now on I am going to call you Woof Woof." (Loud audience laughter) Get it. Mr. Barker, woof, woof. And, he just beamed. I mean, he just beamed. Sure enough, not long after that, down the lake in a water taxi came a great big load of furniture which ended up in the mess. (Audience laughter)

So, with that parting image, I will yield the stage now to our fourth panellist, the person who has led more pant-hoot choruses than anyone else in history I am sure. The Dame with the name and the fame, who the apes did tame, (audience laughter) always game, shows no shame and is blessedly after 50 years still the same, Jane Goodall.

LOUD APPLAUSE

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