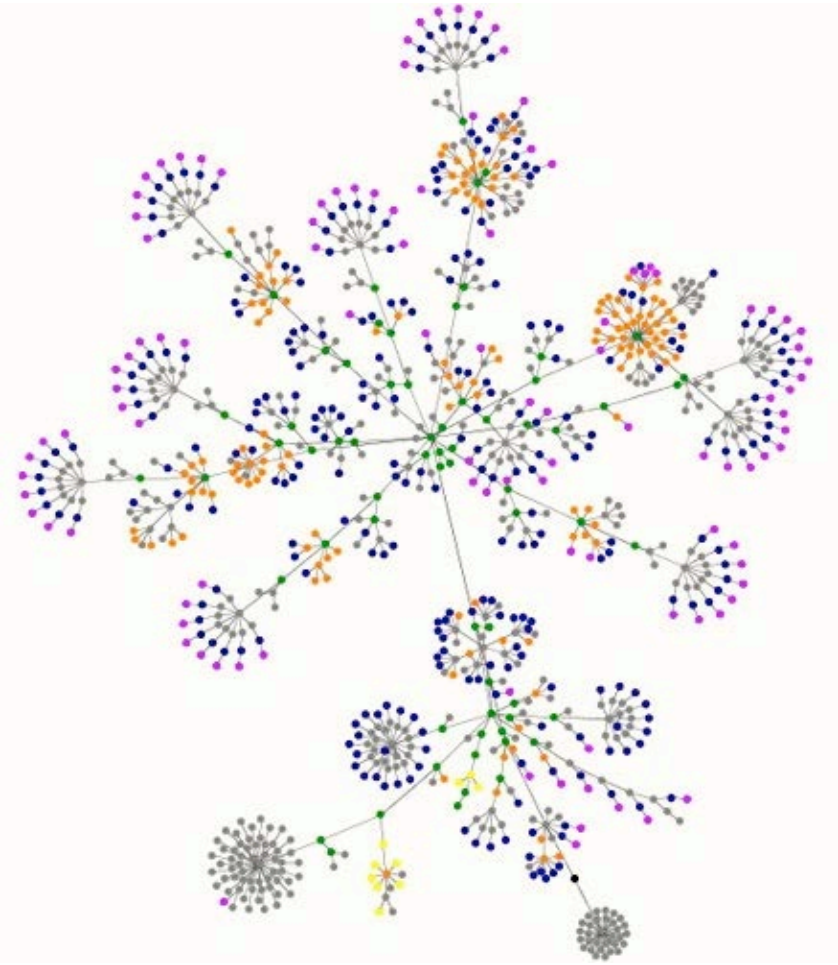
ENCOUNTER WITH

ZOOLOGY AND ETHOLOGY



Sarah Harrison and Alan Macfarlane

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Preface to the series

There have been many autobiographical accounts of the creative process. These tend to concentrate on one level, and within that one aspect, the cerebral, intellectual working of a single thinker or artist's mind. Yet if we are really to understand what the conditions are for a really creative and fulfilling life we need to understand the process at five levels.

At the widest, there is the level of civilizations, some of which encourage personal creativity, while others dampen it. Then there are institutions such as a university, which encourage the individual or stifle him or her. Then there are personal networks; all thinkers work with others whether they acknowledge it or not. Then there is the level of the individual, his or her character and mind. Finally there is an element of chance or random variation.

I have long been interested in these inter-acting levels and since 1982 I have been filming people talking about their life and work. In these interviews, characteristically lasting one to two hours, I have paid particular attention to the family life, childhood, education and friendships which influence us. I have let people tell their own stories without a set of explicit questions to answer. This has led them to reflect on what it was in their lives which led them to be able to do their most interesting and rewarding work. They reveal the complex chains which sometimes lead to that moment when they discovered or made something new in the world.

I started for some years mainly in the disciplines I knew, anthropology, history and sociology. But after 2006 I broadened the project out to cover almost all fields of intellectual and artistic work. I have now made over 200 interviews, all of them available on the web. Future volumes based on these interviews are outlined at the end of this volume.

How to view the films

The films are up on the Internet, currently in three places.

Alan Macfarlane's website, www.alanmacfarlane.com

The Streaming Media Service in Cambridge:

http://sms.cam.ac.uk/collection/1092396

On both of these, the full summary of the interviews are available.

Most of the interviews are also up on the 'Ayabaya' channel of Youtube.

The films can be seen from within a free PDF version of this book by pressing on the image. You will need to download an Adobe Acrobat PDF reader (free) from the web if you do not have it. If you right click on the film, other options open up. The free PDF version can be obtained by going to Dspace at Cambridge and typing Macfarlane Encounter followed by the name of the book, for example 'computing' or 'economics'.

Technical information

Unless otherwise specified, all the interviewing and filming was done by Alan Macfarlane, mostly in his rooms in King's College, Cambridge.

The detailed summaries, with time codes to make it easier to find roughly where a passage of special interest is to be found, were made by Sarah Harrison, who also edited and prepared the films for the web.

The cameras improved with time, but there are occasions when both the early cameras and microphones were less than satisfactory. We have had to wait for the technology to catch up. It is hoped one day to improve this if funding and technology allow.

Introduction

Cambridge is famous for its zoology and ethology, from Charles Darwin to David Attenborough. And within Cambridge my own College, King's, was the home of much of their academic career of several important figures in this field in the second half of the twentieth century. Their friendship and partnership is explored in this volume.

I met both Pat Bateson and Gabriel Horn when I came to King's in 1971 and I have remained quite close friends with both of them since then. The relations are not just ones of friendship, but also lie in the fact that Pat Bateson was for more than a dozen years Provost of my College, while Gabriel Horn and his wife Prill moved to the fenland village where we live shortly after we moved there.

Pat Bateson was Provost of King's College, Gabriel Horn was Master of Sidney Sussex College, and the third of the triumvirate, Robert Hinde, was Master of St John's College. I had known Robert from the 1980's through a shared interest in kinship and mating patterns.

It was at the suggestion of Pat Bateson that I started the science interviews and the interview he did with Gabriel Horn at the start of 2007 was the first experiment in this field. Along with Gabriel Horn, he then became one of my three main advisors on whom I should interview, along with Herbert Huppert, also of King's.

Bateson, Horn and Hinde represent Cambridge zoology and ethology in the second half of the twentieth century, but the tradition of course continues. A younger representative, again a friend in King's, is Barry Keverne, who gives the concluding interview.

The zoology museum in Cambridge, with the whale. Many of Darwin's objects are here. Alan Macfarlane explains the connections to Robert Malthus, another Cambridge figure.

http://downloads.sms.cam.ac.uk/1290276/1290281.mp4

Gabriel Horn

16th January and 3rd April 2007

http://downloads.sms.cam.ac.uk/1121995/1122003.mp4

Extracted from Wikipedia 22 August 2014

Sir Gabriel Horn, MD, ScD, FRS, FRCP (9 December 1927 – 2 August 2012) was a British biologist and Emeritus Professor in Natural Sciences (Zoology) at the University of Cambridge. His research was into the neural mechanisms of learning and memory.

Horn's first academic position was in 1956 at the Department of Anatomy, University of Cambridge as a Demonstrator in Anatomy. He became a Lecturer and then a Reader, before leaving to become Professor of Anatomy at the University of Bristol in 1974. In 1975, while at Bristol, he obtained his DSc degree. In 1977, he returned to Cambridge to head the Department of Zoology. He retired in 1995 and was Emeritus Professor. He was Master of Sidney Sussex College, Cambridge from 1992 to 1999 and Deputy Vice-Chancellor of the university from 1994 to 1997. He remained a fellow of Sidney Sussex College after 1999 until his death, and was a fellow of King's College, Cambridge, from 1962 to 1974 and from 1978 until 1992.

He was elected a Fellow of the Royal Society in 1986, receiving their Royal Medal in 2001. He was given an Honorary Doctor of Science degree by the University of Birmingham in 1999 and by the University of Bristol in 2003. He was knighted in the 2002 New Year Honours "for services to Neurobiology and to the Advancement of Scientific Research".

INTERVIEW SUMMARY

Gabriel Horn interviewed by Patrick Bateson 16th January and 3rd April 2007, filmed by Alan Macfarlane

0:09:07 Father was a tailor, a lovely man; mother managed in difficult circumstances, but also a lovely person; during the war living in poor housing, father ill and mother having to help him in the shop; had three elder brothers, oldest brother about nine years older; mother born in the East End of London and we used to go from Birmingham to see my grandmother there; hated seeing the extreme poverty there; father's parents would have died in Poland before my birth; mother's parents came from Austro-Hungarian Empire; father born 1878 and mother in 1892; I was born in 1927; felt that in terms of the life my family lived we were rather well off compared with mother's family in the East End

5:09:10 My father despised both liberal and orthodox Jews; we did not live in a Jewish community in Birmingham but had their business near the centre of town and later moved to Handsworth; whilst my brothers went to the Hebrew School when the family lived in the centre, there was no such school for me to go to when we moved; they were also too far from the Synagogue to go but rarely; mother did keep a kosher kitchen which was a bit of a problem for me when later I was evacuated; by the time I went to university I had given up entirely

7:13:14 My primary school, Westminster Road Junior School, was next to my home; nothing very remarkable happened there; I was bullied but never remember any anti-Semitism though my older brothers did experience it; my parents did not want me to be exposed to the New Testament so did not go to the morning congregation at school or to religion lessons; I was the only one not to go but it was accepted without comment; I failed entrance examination to the grammar school, perhaps due to disruption of being evacuated for a few months and then returning to find the school closed; went to a school of sorts for one hour a week in the basement of a church; actually did get through the exam but did

not get a place; my older brother had been to a Commercial school and one did the exam for that at thirteen; I took it and got into a Technical school and in my first year was near the bottom of the class in most things; in the second year I remember the gym master reading 'The Lady of Shallot' to us; in the culture I came from people didn't read poetry and certainly not role models like him; something sparked an interest and I went from being bottom in maths and physics to top; even almost remember the day when I suddenly realized how algebra worked; later, when I was about to leave school the maths master asked me what I intended to do; I had chosen a Technical school rather than Commercial as I wanted to be a civil engineer; I had seen in Arthur Mee's 'Children's Encyclopaedia' the bridge over the Victoria Falls and I decided then I wanted to build bridges in Africa; after I left school at fifteen I applied to Stewart and Lloyds in Birmingham for a job but didn't get it; my father got a job for me as a draftsman in a big engineering company and I stayed for a week as I thought it an awful job

15:02:19 At that time my older brother, Henry, had been called up; he had worked for my father who was by then getting quite ill and needed help; he asked me to help him and become a tailor; I agreed on the condition that I could get off one day a week to study for the National Certificate of Mechanical Engineering; during that two year course I was also going to youth clubs; at one we were playing table tennis in a school hall where the elderly woman in charge was reading Huxley and Wells 'A Science of Life' to a small group; I listened and that was almost the decisive moment; I knew nothing of biology but decided to do medicine so that I could help people, also there was no one to tell me what I should do if I wanted to study biology; another problem was that to get into university you needed to matriculate and then the Higher Schools Certificate; decided this was what I wanted to do and asked my parents; could see that they were rather pleased although my father tried to persuade me to be a tailor as a safe job; he was, however, prepared to support me; I went to a woman who was a biology teacher at the King Edward's Grammar School for Girls in Handsworth; she offered to arrange teachers for me in February when the exams were in June; in fact it was a two year course and the cost of five shillings an hour would be a lot for my parents;

realized it was fruitless and went back to the Technical college to do the mechanical engineering exams; in the following September I went to night school intending to accelerate the two year course into one year; the following January my father died and the family needed a breadwinner; still had the shop and went on selling clothes; I was good at making women's skirts but not jackets; put up a notice advertising 'alterations', a skill which served me well later in micro-surgery; made a living but still went on at night school, managed to matriculate in the following June and then embarked on the equivalent of 'A' levels - chemistry, physics and biology; I was seventeen and this was a two-year course which I was trying to do in one year; was an extraordinarily exciting learning year; due to be called up for armed forces at eighteen and asked for a deferment until I had finished my exams; only gave a short deferment and refused to extend it to the following July; I wrote to my MP and he wrote to the Minister of Defence, Mr Arthur Henderson, who refused to interfere in the process of the call up 'but it was unlikely it would be implemented in time before Mr Horn's examinations'; many years later I met Mr Henderson in the House of Lords where I sat next to him at lunch and told him the story; he professed to remember me saying 'we thought it was a neat solution'; passed my exams; before I did matriculation, didn't know who to talk to about medicine but wrote to the Sub-Dean and the Birmingham School of Medicine, Professor Charles Smout; he agreed to see me but was quite violent saying I had no qualifications and should give up the absurd idea; wrote me a letter in the same vein; some years later after I had got my Higher Certificate I asked to see him again; he upbraided me once more and told me to leave but he did ask what I was carrying in my briefcase; it was my certificate and he told me to come back and offered me a place to read medicine

25:51:15 Politics was not discussed in my family; mother said she was Liberal but I never knew what that meant; as an undergraduate I met Anne Soper and in our last year we went out together; we talked a great deal about politics; there were a number of factors but it was not family background that interested me in politics; in those years of enlightenment for me when I had decided to read medicine, my mind was opened by Huxley and Wells 'A Science of Life' and I began to read, particularly philosophy; remember

reading C.E.M Joad's 'Guide to Philosophy' and that brought a huge change intellectually; it was an extraordinary experience to question all certainties; with my background at a school which gave a training for skilled artisans where all the answers were known; from Joad I went on to Bertrand Russell, I came across Einstein's theory of relativity, A.J. Ayers 'Language, Truth and Logic'; these opened up my mind while at the same time I was seeing the social conditions of people; in my course at the university there was a course in social medicine and saw the poverty around one; Anne Soper was the daughter of Donald Soper, a very left-wing Christian politician; by then I was an agnostic but was more and more conscious of the needs of the underprivileged and the injustices of the distribution of resources in the country; the waste of talent in many young people while for others who went to the right school it was given; it should be drawn out by the school but for the majority it wasn't; think those that were the factors that drove me to the left of politics

30:31:10 At Birmingham I started to develop an interest in the brain; I was beginning to be aware of perception and sensation and the philosophical issues around that and consciousness even then; I remember one day in 1946 reading an article on the surgery of the brain in a magazine called 'Discovery' and it was that moment that I knew I wanted to work on the brain; that and the philosophical background was driving me towards the central nervous system at an early stage; I enjoyed my time at Birmingham; I plunged into undergraduate life and got involved in the debating society and became chairman of the society at the end of my first year; that made me also chairman of the political society; that first year was a great year for exploring beyond the bounds of anatomy and physiology as was to some extent the second year; the third year was fabulous; a remarkable man in the medical school called Solly Zuckermann, the Professor of Anatomy, although anatomy was not actually taught us by him; he was in London a good deal of the time advising the Government, but he would come on Fridays and stay until Monday; somehow the whole atmosphere changed in the Medical School on Friday afternoon; everyone was alert and they remained so until Monday afternoon; I knew that I could work in the Department of Anatomy with a very small group where quite a lot of work was being done on the brain; I took a scholarship exam

which would allow an extra year without an exam but a dissertation; I was so keen to have a research project and it was one of the most exciting years of my life; this was the reason why when I came to Cambridge I wanted a project in the third year; it was also the year I met Anne and married her; she was reading zoology and helped me with my zoological knowledge; we connived to marry without telling anybody and did so the day after our first paper; we kept our marriage secret from Donald and his wife for many years but I am pretty certain they would have opposed the marriage; I had no background, no money, and they had aspirations; very middle-class which I was not

36:43:12 I loved Solly and he was very much a father figure to me; when I got the scholarship to do a B.Sc. in a year I also got an opportunity representing Britain in the debating society in an all-India tour in 1951; a fantastic opportunity and I wanted to go on this six-week visit but it was at the start of the B.Sc. course; I went to the department and saw Peter Crow, a distinguished endocrinologist, who was Solly's right hand man; he said I should see Solly who advised me not to go; during that year I did a lot of research and then I was to go on to clinical work; I really wanted to continue research in the Department of Anatomy but I was also supposed to be attending ward rounds; I was absenting myself from ward rounds and the Professor of Surgery, Stammers, apparently contacted Zuckerman and complained; Zuckerman put out an instruction that 'Horn should not be encouraged to stay in the laboratory, but you don't have to discourage him either'; later on I really didn't want to come to Cambridge but to stay in his department; I went to see him and he hadn't got a job for me; by then I had decided to stay in medicine and had published some papers in endocrinology; Zuckerman said he would write to his friend Dixon Boyd at Cambridge which he did in my presence; it was a glowing letter and a job at Cambridge did come up and Dixon Boyd asked me if I was going to apply; that is how I got into Cambridge

40:34:09 Having spent five or six years studying medicine, a new rule had been introduced requiring one to spend a year at a hospital in order to become registered which I did; I still went on doing research though my poor wife and children suffered; was

also thinking what future research to do; applied for the job in the Department of Anatomy in Cambridge and hoped to go into fulltime research; however I still clung to medicine; to eke out a living I used to supervise quite a lot for King's which was the beginning of my association with the college; I also found I could also earn nearly as much by doing the surgery for the local GP in Histon on one or two evenings a week and occasional weekends; also did occasional weeks as a locum in the Fens while I was a demonstrator; kept this association going until finally I realized that there were plenty of people to do good and then focussed more and more on my research

43:48:09 The huge influence from my philosophical interests led me to wonder how one could study consciousness from an experimental point of view; became aware of the selective nature of consciousness and attention and the things you do attend to are apprehended in a self-conscious way; seemed to me that if you could understand how the different signals are treated in the nervous system, the tended and unattended signals, you could find the routes whereby the attended signals are passed and treated in the brain and then see how that differs from the unattended signals; that seemed to me a potential attack on the problem of consciousness from the experimental point of view; there were already some neurophysiological correlates of this in the work of Hans Berger in 1928 when he studied the human electroencephalogram found a rhythm at the back of the skull called an alpha rhythm and they disappear if the subject is attending to a stimulus and reappear if the subject is not attending; so something going on in the brain which is quantifiable; Lord Adrian and Brian Matthews were able to repeat Berger's work and take it forward in the 1930's; Adrian was a sensory physiologist and was interested in how signals were passed from the skin to nerve fibres and up to the brain; he discovered the nerve impulses that we use; he wondered whether the signals that are not attended to are blocked in being transmitted from the eye or skin to the brain; I thought it a great thing to study and my intention was to come to Cambridge and set up an electrophysiological laboratory and study attention in animals; there was no one in Cambridge who was studying the neurophysiology of the central nervous system, nor very little in Britain; thought I must be trained somewhere so asked

Professor Boyd if I could take leave the next year and go to work with Herbert Jasper at the Montreal Institute of Neurology; Dixon Boyd agreed but couldn't pay me and Herbert Jasper had said I could come if I raised the money; I applied to the Wellcome Trust and got a grant; Herbert knew Adrian and asked him if he would see me; saw him in his laboratory one morning and he was simply wonderful; he was then Master of Trinity, a powerful man; he was in his basement lab all blacked out so he could take photographs and he was filing something; we spoke for next two hours on attention

50:29:16 Had a similar experience with another august person, A.J. Ayer; I had written an essay on the neurological basis of thought during my year in Zuckerman's laboratory; Zuckerman had read it and sent it to Ayer; Ayer asked to see me; Zuckerman had thoroughly read and commented writing Hebb, a famous neuropsychologist, all over my manuscript; Hebb had published a paper in 1949 on how memory might work and unbeknown to me I had written in my essay almost exactly the same; saw Ayer in his apartment at Mayfair where he harangued me on how wrong I was and that physiology had nothing to say about sensory perception; I occasionally drew his attention to what Bertrand Russell had said which only inflamed him all the more; Adrian was most encouraging; he did understand the blocking mechanism of the mind; said 'the doctor asleep at night does not often hear the call of his child but he will wake to the ringing of the telephone'; selectively blocking signals but allowing some SOS messages through

54:34:21 Began to teach for King's in 1956 though can't remember well; 1957-58 I was away; in 1958 King's asked me to take on all the supervisions which I did but I was still only a demonstrator and as such colleges are usually reluctant to take on; they did offer me very generous dining rights which I exercised; Provost Sheppard was extraordinarily nice to me; I remember the very first evening coming into college much too early and there was only one person there, hidden behind a newspaper; he asked me who I was and I returned the question which amused him greatly; coming from Birmingham to Cambridge although a doctor I only had an M.B. and so was called 'Mr'; at one dinner was placed next to Sheppard

and noticed my card said 'Dr Horn' and I commented to him that I was only 'Mr' to which he replied that I was the only proper doctor in the college; met John Griffith dining in hall after I came back from Canada and fortunately applied for and got a very substantial sum of money from the US National Institute of Health which enabled me to set up a first class neurophysiological laboratory with the help and encouragement of Professor Boyd who was a classical anatomist and thought I had come from a different planet; very sweet to me and gave me space; there I was recording from nerve cells in the cerebral cortex and was telling John Griffith about this and he said he would like to see what I was doing; he was fascinated though a mathematician and theoretical chemist and had never really thought about the nervous system; after that he came frequently, often carrying a tightly rolled umbrella, a shy upright person, unsure of himself; we would watch the oscilloscope screen together; you can tell when you are recording from a single nerve cell because the cell generates an electrical current which you can measure the voltage associated with it; the voltage generates a wave that you can see on the oscilloscope; any one neurone has a particular wave form; sometimes we could see one wave form accompanied by another little wave form which had to mean it was two neurones; John had been reading some work by Eccles, a great neurophysiologist; he said that if I could tell him how neurones worked in the brain he would tell me how the brain worked; those were the days when it was thought that there was only one kind of neurone and that was in the spinal cord and that you could extrapolate from that to every single neurone in the brain; he saw these two neurones on the screen and wondered how they interacted with each other

Second Part

0:09:07 Collaborated with John Griffith and published at least one paper together; taking forward how the two neurones interacted electrically because if you could understand that you would begin to understand how networks and nerve cells interact; no mathematics available to study it so he had to design the mathematical tools for studying it; there were no computers then; these were electrical pulses and you couldn't analyse the time elapsing between each pulse so had to film it and then the film was

analyzed by a woman who was also measuring film of pulsars at that time; took months for the measuring the intervals; became a fellow in 1962 and we spent much time in my room here, in his room, and my laboratory, and formed a great friendship; and going beyond our own work together he was a very original scientist and I think would have got a Nobel Prize

3:11:19 Working on attention where there was always two stimuli I had found some nerve cells in the visual part of the brain that responded not only to visual but also tactile stimulation; there were very few of them and the responses were quite weak but visual physiologists did not like this; there was a group in Germany let by Baumgartner who had found similar things for auditory input, neurones in the visual cortex responding; Dave Hubel and Vernon Mountcastle really didn't like that and they were very influential figures in the US at the time; coming from it from attention and finding nerve cells in the visual part of the brain responding to a tactile stimulus whether the animal was attending or not; there was a basic level of interaction going on which was independent; Peter Venables and I did an experiment in humans with similar results; not only did you get switching between modalities, like audio and visual, but they actually interfere with each other at quite a low level of the cerebral cortex; in 1963 Charles Shute and Peter Lewis in the Department of Anatomy where I was working had shown that there were pathways that had never been discovered that were actually not visual but they went into the visual pathways from other parts of the brain; I thought this was the key; there was a lot of evidence that this extraordinary high level of brain function, attention switching, for example, and what one would call clinically, consciousness, is controlled deep down in the very primitive parts of the brain called the brain stem; I wanted to spend some time putting electrodes into the brain stem and seeing whether there were lots of nerve cells there that responded to more than one sensory modality as that is what I would predict; I had written a chapter on some neurocorrelates of perception and gave it to Horace Barlow to read; he liked it and when he could not go immediately to take up a Chair at Berkeley he suggested I go for a term teaching on the physiology of vision; when I arrived I was told I would have to work with Dick Hill as there was not enough equipment for me to work by myself on brain stem research;

working on anaesthetised rabbits we put an electrode on the brain stem and came across nerve cells that responded beautifully to visual stimulus; I remember one day dropping a pin on the floor while we were monitoring the activity of the nerve cells and getting an immediate response; further research found lots of nerve cells that responded to sound and touch as well as to visual input; however, when we came to look at where the electrode had been it was not in the core of the brain stem but in the roof; the roof of the brain stem is the most ancient visual centre in vertebrates called the optic tectum; we looked through the literature and found nothing published and so the world didn't believe us; remember coming back to Cambridge and telling Dixon Boyd who asked for proof which I gave him; then a lot of people started working on this; I then also found that neurones in that part of the brain responded differently to infrequent and frequent stimuli; for the former there was always a response but it declined with frequency, showing habituation; this had been observed in behaviour and I had heard about from Bill Thorpe at the Thorpe-Zangwill club; in animals, they give a response to a novel stimulus but if not rewarded with food, for example, they don't respond; I suddenly realized that we were dealing with a nerve neural counterpart to behavioural habituation so I saw my life retrospectively opening up in two directions; saw I must follow this elementary form of learning as here was a clue to understanding what the neural basis of learning might be; the other was sensory interaction

14:19:03 In 1967-68 Dick Hill wrote to me and said he would like to come and spend a summer in Cambridge; I had read about the vestibular stimuli in the visual cortex and I'd already been to Makerere to work with Hugh Fraser-Rowell where I'd worked on locust brains; came across the work of a Dutch man, Wiersma, whom I met, who was recording from the optic nerve of crayfish and found that the nerve fibre was looking at the field of vision above the horizon; every time a visual stimulus was below the horizon there was no response; found that if he tilted the animal the nerve cell went on responding in the same area and didn't tilt; suggested to Dick that we see if something like this existed in the visual cortex of a mammal; we found this was so and we sent off a letter to 'Nature' which was published; it caused intense controversy; the following year I was joined by Gerry Steckler and

we spent a year working on it once again and we published the full paper in 1972; could never understand the vitriol that was heaped on me in meetings in the visual forum and it took many years for other people to confirm; it was a difficult experiment to perform and also took courage to do it in the States; there was a group there which published a paper in 1981 saying in a sense that I was wrong in underestimating the number of these as there were more, but it still went dead and is a lacuna in the scientific system that this still isn't quite respectable

19:07:00 [PB: I will tell you why. It is because people trained in a certain kind of science do an experiment where you vary one thing and keep everything else constant, and if you get an effect that is the cause. People still think in this very linear way and if someone like you comes along and says it is not the cause but one of the causes, people hate this systems approach to biology. I have encountered many physiologists who hate this]

19:42:13 I remember I had some interaction with Colin Blakemore about this and he said 'The trouble is we physiologists don't know how to handle parallel systems and therefore it's not popular'; I don't know if that's it or the explanation for it; I read a review by someone in Stirling University about this kind of influence and she had never heard of this earlier work; I wrote to her and she said she was pleased to know about it but was facing terrible trouble getting her work published; I felt then that the quicker I get out of the field of visual physiology the happier I should be in another field

20:40:17 You and I had already started in 1965 before I went to Makerere; when I came back we began to think of ways of dealing with that problem; on the habituation front, I went on to study it in the insect brain and work out its characteristics and a year after Dick Hill and I published in 'Nature' showing these lovely habituating curves of neurones and showing that these nerve cells had so many of the properties of behaviour; this paper was 1964 and in 1966 a paper appeared from Tauk and Brunaire in France showing that you could get this kind of response decrement and showing many of the properties of behavioural habituation across a single junction between nerve cells and the next neurone; the

trouble was they didn't know it was any one cell to the next nerve cell and it could have been many nerve cells interacting and they hadn't got any system that did it; I thought that I needed a system to do that; I knew there was one system where there was unequivocally the case that there was one nerve cell sending a signal to another nerve cell and that was the squid giant ganglion; thought that is the place if I can show this habituation then I know its occurring in the synapse and you have got to look within the synapse to understand it, you don't need to look much further; it happened and I remember giving a presentation of these results to a conference Robert Hinde and I organised in 1969 on short term change and neural activity; Eric Kandel was there and I showed these slides of this lovely waning of a response and it had to be on the pre-synaptic side not on the post-synaptic side and I said I thought that calcium was probably involved; Kandel showed later on that calcium was involved and it did happen at the synaptic junction but he never referred to that work, except when I met him after he got the Nobel Prize referring to my great paper on the squid stellate ganglion

24:26:23 I had been in the Department of Anatomy here in Cambridge for eighteen years and I really wasn't interested in running anything but just doing my research; I'd been pulled onto the research committee by Robert Hinde and began to see that the way the University was running was important for my research too so I began to look beyond it; I was then teaching six hours a week and had been for years here in King's; I had been appointed Reader; was not very impressed by the way Professor Harrison ran the Department; used to go on family holidays to Cornwall and Devon, driving through Bristol which I loved; when the Chair of Anatomy came up at Bristol I applied; I was extremely lucky in that Barry Cross had not been a traditional anatomist and had done an extremely good job in organising that department; he had allowed a young junior lecturer to revise the teaching of topographical anatomy so it was going extremely well when I arrived; the teaching looked good so I was able to concentrate on research; I always had a love of developmental biology and embryology; the field of molecular developmental biology was just beginning; I had a vacancy in the department and I managed to get a very bright young man, John Knowland who was working with Fred Sanger at

the MRC Laboratories here in Cambridge; he came and began molecular developmental biology; sadly he only stayed about as long as I did in Bristol; felt that if I am going to populate a department of anatomy with scientists who had no medical training they ought to know a bit about anatomy; needed a clinical input and thought I could get John Knowland to get familiar with these things; too big a job and he opted out; I brought in quite a lot of neuroscientists to the department; I encouraged the development of biomechanics with research by Lance Lanyon; became quite strong in oral biology with Bernie Moxham; really a matter of appointing the right people to jobs and encouraging them; also, I as head of department did research; felt that no head of a science department should just be a manager; there was some discretionary money that I could use as head of department; one thing it brought me was the opportunity to bind the people in the department together so I used to spent some of that money on a party once or twice a term for all the staff, including technical and secretarial, and their partners, which was appreciated; two of the people I appointed, one Malcolm Brown who became Fellow of the Royal Society and remain in Bristol; Bernie Moxham went to become head of department at Cardiff; both those two departments got five stars in the Research Assessment Exercise, the only two to do so

33:52:22 Had a telephone conversation with Robert Hinde about applying for the chair in the Department of Zoology in Cambridge; thought it was absurd as I was not a zoologist and had no real interest in natural history and knowledge of biology was limited; gave all the reasons why I shouldn't apply; he countered with my work on squid, locust, all sorts of animals and even humans; I had great esteem for the department with which I had had links in the past; thought there were brilliant people there; in the end agreed that Robert could put my name forward and finally I was offered the Chair and it was far from clear that I should take it; I was well known in the department for my work with neuroscientists there; my research cut across boundaries and the one thing I saw as positive in going to zoology was the fights I continually had to mount in the medical faculty and the science faculty in Bristol about intellectual territory; to me they were senseless arguments as I didn't see the world that way and being forced to teach student that way was anathema; I then got the reputation in Bristol of

wanting to take over the physiology department which was the last thing on my mind; the attraction of zoology in my discussions with you and Robert was the awareness that it didn't matter whether you worked on behaviour or cells it was all one; zoology cut across everything to do with animals which included humans; at that time the department was not in good shape so I knew it would be an uphill task while everything in Bristol was simply wonderful; remember going away with the family and we discussed it endlessly; in the end they left it to me to decide; on the basis of this challenge I suddenly thought what an opportunity to abandon all these constraints and step into the wide world of real biology

Third Part 3rd April 2007

0:09:07 Didn't quite appreciate how much needed to be done in the department; the first thing that I did was to try to absorb the atmosphere of the department; at the time Donald Parry was head of department and was doing a marvellous job but he didn't have the authority that was required to help in critical ways; I got my research group going as I felt strongly that the head of department ought to lead not only by teaching and administrating, but ought to do research themselves; first priority was getting myself established and finding out what people wanted; I was not then head as I left Donald in charge; found that people did not want more neuroscience and there was a real fear that I was going to take the jobs that became vacant and fill them with my own kind, which is what you might do in a department of anatomy to strengthen one area; realized it would have been disastrous so tried to find out what were the strengths and weaknesses of the department; also my own experience of zoology was 'A' level and it was a new world for me; looked around for someone, not an ethologist as we were strong there; had been told of a man called Nick Davis who was at Oxford and managed to get him; having strengthened behavioural ecology; [PB: had a project on that at King's at the same time which was renamed sociobiology, which meant we could establish a strong group. Tim Clutton-Brock came back as a fellow, Robin Dunbar was here and Richard Wrangham. That may have helped to get Nick Davis here]

5:34:18 Moving a few years on when I was then head of department and on the council of the School of Biological Sciences; on the needs committee one had the fights for vacant offices; since the mid 1970's when the University has been under pressure from external forces to slim down; there was a big fight over one vacancy and I spotted that Tim Clutton-Brock was coming to the end of the sociobiology group at King's and I felt we ought to get hold of him; I pressed the needs committee who were mainly medics and they didn't understand what Tim Clutton-Brock was doing; in the end got the vacant post; the John Humphrey Plummer Professorship became vacant and also another Chair in Biological Sciences; both could go to any department; I thought these two professorships could be used to extract John Gurdon and Ron Lasky from the Laboratory of Molecular Biology; Richard Keynes, then Head of the Physiology Department, suddenly became very supportive; had heard that neither was very happy in the Laboratory and I went to have tea with them there and it was clear to me that they were interested; what they really wanted to do was to establish an outstanding department of developmental biology and I think they thought that they themselves weren't sufficient; there was another Chair coming vacant, the Quick Professorship; the person who got it in the end was Chris Wylie from University College; I don't want imply in any sense that you could short-circuit the University but what we had was some quite remarkable people who were willing to allow their names to go forward; Alan Hodgkin was on the panel of electors and he was very enthusiastic; Gurdon and Lasky got the Chairs; Chris Wylie and his wife, Janet Heasman, who is a remarkable developmental biologist, worked together; couldn't get one without the other; Chris got the Chair and Janet applied for a job in the department; difficult as there was a very strong internal candidate; in the end she did have the edge; so the four of them were in post

12:09:09 I was able to take over space in the department; Torkel Weis-Fogh had wished to promote cell biology and had brought in a group run by Bob Johnson, supported by the Cancer Research Campaign; managed to get space for Bob Johnson; Cancer Research and possibly Wellcome gave money for the refurbishment of laboratories for Gurdon and Lasky and the thing

took off; later had the opportunity to appoint Mike Bate also a young, distinguished, developmental biologist; had this extraordinary cluster and that development brought the Department of Zoology up to the highest possible level; we also had in Charlie Ellington in comparative physiology who worked on locust flight; formed a marvellous group with Martin Wells; buoyant department but clear that many people were after Ron Lasky and John Gurdon was keen to keep him; they developed a plan to get more space by getting a building; that in itself was a big problem because the department would lose people attracted to new facilities; we got round the problem by ensuring that anyone who worked in the new institute would also be a member of a department and have teaching obligations to that department; their research contributions would be attributed to their department so that departments would actually gain; this is now the model for interdisciplinary facilities in the University; from that Wellcome-CRC came the Gurdon Institute and now the stem cell centre that has recently been established; all flowed from Gurdon and Lasky coming in the first place

18:00:17 Meanwhile there were inter-departmental things to consider; when I arrived we had 28 people reading Part II Zoology and I could see that as the department got stronger more people would want to come; there was a reluctance to expand because of the work load; when I finished there were over 80 Part II students; we in Cambridge were becoming stronger and stronger in neuroscience; 1969 I had got a group of people together in different departments in response to a then Science Research Council feelers to establish a neurobiology institute somewhere in the UK; planning meetings chaired by Oliver Zangwill but hostility from Alan Hodgkin etc.; SRC did not take it up but it did bring departments together and out of that came the Neuroscience Club which used to meet on Saturday morning; in 1987-8 the Medical Research Council said they did want to establish a neuroscience centre somewhere in the UK; this time we got the Brain Repair Unit; at that time we established the annual Cambridge Neuroscience Seminars; at that time spoke to Nick Mackintosh in King's who was then head of the Department of Experimental

Psychology suggesting a joint Part II in neuroscience which was successfully started

22:01:23 Don't know how good a head of department I was; I was always accessible when there was serious trouble but not all the time; my schedule in full term was Monday, faculty boards and administration; from Tuesday afternoon onward I would work in my lab; Thursday afternoon would do a little administration; people did not disturb me in my lab; Fridays I would write papers at home and analyse data; pretty rigorous schedule; lectured usually in the Lent term, enjoyed teaching

25:15:12 The museum is one of the great collections in the UK but was not being well supported by the University; I did not have much in the way of resources for it and heads of departments explicitly wanted to close it and disperse its contents around the country; the Professor of Pathology was talking about dispersal but had never been to the museum; decided to have a meeting of the needs committee of the School of Biological Sciences in the museum; before the meeting took the heads of departments round the museum and they were simply amazed by what they saw; that changed the whole atmosphere and no longer any talk of closure; Ron Oxborough and I were worried about the position of museums in the University and managed to get the General Board to agree to appoint a committee to look round all the museums in the University which resulted in the key operating system for museums since

28:00:24 Remember there was a devolution of finances from the centre and departments were charged according to number of students, staff, space, and income they were receiving from research councils; if you did no research you did not get a deficit; Physiology had very little by way of income from research grants, with a lot of staff and minimum of teaching; Zoology had £500,000 deficit; departments manoeuvred their staff-student ratios or introduced new lecture courses to give the impression of heavy teaching loads; caused deep hostilities with struggles across departments; as head of department had struggles with Keith Peters who wanted to take over the School of Biological Sciences; later he expressed relief that I had blocked him as he was able to put all his

energy into the Clinical School; despite all our public arguments we always remained friends which is academia at its best; this is one of the attributes of Cambridge colleges that instil a sense of respect and the opportunity to disagree while retaining friendship

35:01:22 When I had retired as Professor of Zoology I was approached by what is now DEFRA to look at origins of BSE; when I was on the council of Agriculture and Food Research Council I had been chairman of the committee that distributed financial resources into the whole of the effort on transmissible encephalopathies; it is not my field but I was only given two days to think about it as it needed to be reported to the House of Commons before the end of the Michaelmas session; agreed to do it and I appointed the committee members; Nick Phillips had already reported that the origins of BSE was a mutation in cattle, possibly in one animal; much debate whether that was right or plausible; more to the point he excluded scrapie as a source; clearly the Government wanted to have some update on what the current view was; a strange task as I knew nothing about it; I got John Webster from Bristol Clinical Veterinary School; when talking about the feeding of cattle described some changes in the sixties and calf feeding; I wondered whether there could be a sensitive period for eating infected material; we then began to look into the diet of calves and found that round about the critical epidemiological time there was a change in the way calves were fed and from then were fed meat and bone meal; cattle had been fed it before but it had never been given to calves; the epidemiological evidence also began to fall into place; it had been thought by the Phillips Commission that meat and bone meal were the trigger; it had been fed to cattle since the 1920's or even earlier; rough calculations on the numbers of cattle since suggested it would be a very rare mutation; why in the UK in the 1960's or 1970's and never in the rest of the world where the same technique was used; we thought it advisable not to exclude scrapie although it was taken by the press to declare that scrapie was the cause of BSE; given just six months to produce our report

42:18:20 Never felt it right for the head of a large department to take on the headship of a college; nearing retirement from the department realized that Mastership of Sidney Sussex would give me a further seven years; retired a year early as head of department; very different experience as in a department you have power to direct and to control space; in a college there is much less of a hierarchical structure where you have to both reflect the fellowship's desires and to guide it; I took with me the King's notion of a Research Centre as I thought it a good way of using research fellowships; I had mentioned this in my interview and had suggested the crumbling Soviet Union as a subject among others; after election was approached by a fellow which resulted in a project on post Soviet states in transition which was very successful; run by a fellow called Graham Smith who was a geographer; they had produced a landmark book but sadly Smith died just a week before publication; also presided over the 400th centenary celebrations; asked to start an appeal and managed to raise £6,500,000 and a system for donations; Sidney Sussex was the college of Oliver Cromwell so a tension between it and the Crown; no member of the Royal Family had ever visited the college; I had a good relationship with the Vice-Chancellor, David Williams, and he encouraged Prince Phillip to come to the college; he enjoyed his visit; wanted to invite the Queen for the 400th anniversary celebration; was invited to meet the Queen at Emanuel College and asked her, then followed this invitation with a letter and she came; also got a building put up; fund raising continued through system of annual giving

Fourth Part

0:09:07 Science policy meetings started with a conversation with Alan Hughes shortly after the 1997 elections; he said that L.S.E. had arranged a programme of seminars for Ministers on matters to do with government; these did not actually happen but wondered whether we should put the experience of Cambridge in science and technology at the disposal of the Government; never crossed my mind that I should approach the chief scientific advisor but instead wrote to the Vice-Chancellor, Alec Broers; sent a copy to John Eatwell who had just come to Queens and was a member of the Upper House; failed to get a reply from Broers but when pressed

said it was not possible; John Eatwell said it was possible; Cambridge is a devolved system where one can do this without the University; invited Dave King, Master of Downing and head of chemistry, Bob Hepple, Master of Clare and professor of English law to join us; at that time great interest in the press in cloning which prompted intense moral, religious and ethical debate; I drafted an abstract on cloning; John Eatwell contacted David Milliband who was head of the Prime Minister's policy group; later learnt that Milliband took it to Richard Wilson who was then Cabinet Secretary who took it to the Prime Minister; they all thought this a good idea; that was how the seminars started; we were able to do this independently of the University; all we had to supply were sandwiches as we got local people to speak; Martin Evans and Anne McLaren spoke among others; Richard Wilson came as did the Permanent Secretary to the Treasury and a minister; later other Permanent Secretaries came as did Alec Broers; paid for food and non-alcoholic drink from our college entertainment allowances; have been roughly two seminars a year on topics to do with science and society; they have been very successful; the Scottish Government has become interested in having them there and I took one seminar group to Scotland to show how it was done; have also taken a group to China; it is beginning to fan out as a means of bringing scientists together with those who take government decisions for the later to understand the nature of science; about halfway through David King said he thought he had got the Prime Minister interested and would I get a group together to go to Number 10 and make a presentation; took four outstanding speakers on a variety of topics: Andy Hopper on computer science, Trevor Robbins on drugs of addiction, Sir Richard Friend on physics and Katy Core on epidemiology; before we have our seminars we will have got the speakers to meet and discuss their abstracts so the thing fits together; they communicate on a level that a non-technical person can understand; we do a dry run the day before; I went with the four to Number 10 and sat beside the Prime Minister for three hours; the four talked for half an hour each; the Prime Minister was enraptured and I was very impressed by his summary of the talks at the end; David King had warned me that if it failed then the Prime Minister would not have gone along the scientific route; clearly it didn't fail and the Prime Minister gave a speech supporting science; Gordon Brown is also

strongly in favour; in part I feel it derives from this programme where we not only look at science but its ethical, social and moral implications; foresight programmes where the Government look at future developments in science and decide expenditure are based on our seminars; did one on migration last Friday where I argued there was science in migration for which we had the largest turnout of Permanent Secretaries ever, eight out of fifteen; getting very positive responses with requests for materials for their departments; the Minister responsible chaired the general discussion and promised to take forward to Government policy the outcomes of what had been said that day; on this topic needed to convince the speakers on migration in animals that they are uncovering general rules that are applicable to humans; does entail an enormous amount of work but will continue if there is pressure from the Government to do so

19:09:15 The Chief Scientific Advisor when this started was Bob May; he was very put out as he thought it undermined his position with the Government; some weeks later he apologised and said that in fact I had strengthened his position; he then always came to every seminar both as scientific advisor and later as President of the Royal Society; all Presidents since have come

21:39:16 Received a letter from Keith Peters saying that the Government had been looking at the output of a substantial review on brain science, addiction and drugs; the field was so important, the developments so nascent, that they wished for a committee to be established to look into the implications and to carry this foresight project forward; to make recommendations to the Government on development in brain sciences, addiction, medicines for mental health and all implications; Government had decided the lead department should be the Department of Health; gave the investigation to the Academy of Medical Sciences; Keith Peters asked me to chair a working group which I agreed to do so long as I could appoint whom I liked; that went well and we have been going now for a year and a quarter; hoping we will be able to report by the end of the year but may be difficult; looking at the development of brain sciences and how they are likely to impinge on the development of medicines for the treatment of addictions; however, the brief goes beyond that; the benefit of having read

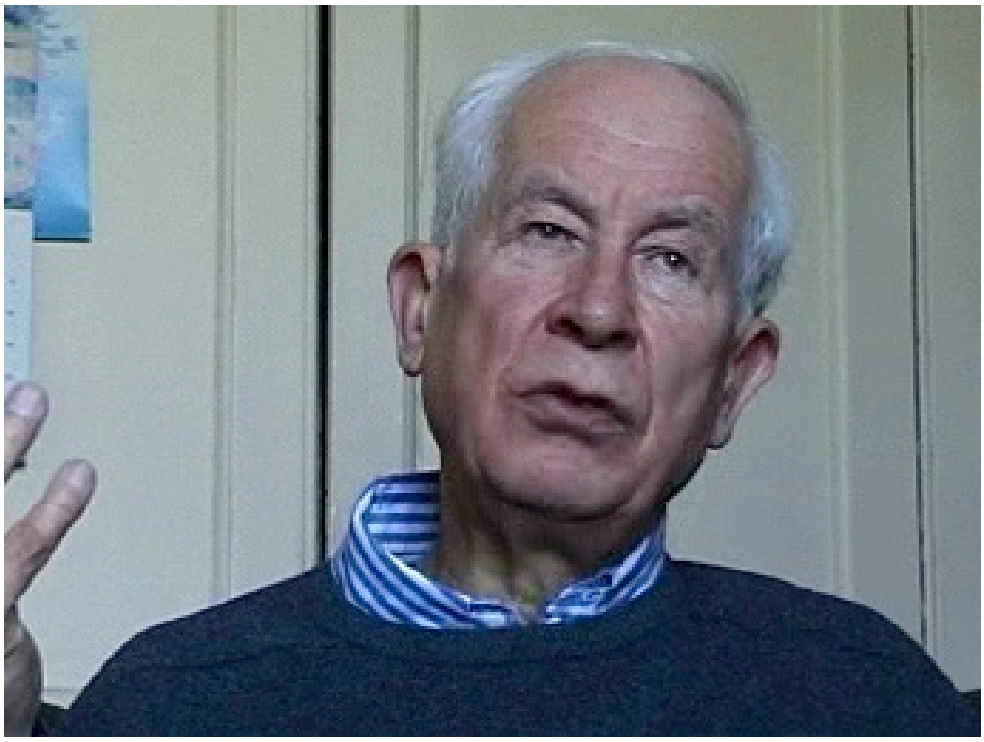
medicine meant that I saw the approach to this as one of public health; rather than see addiction as something that is essentially criminological, although there is a huge criminological component, as a public health problem you ask what are the causes and how could you prevent it; got a child psychiatrist and child psychologist on my committee and they have been reviewing the risk factors for the later addictive behaviour; I am sure it will have implications right the way through Government policy; I have already submitted an interim report and I gave a talk to a cross Government meeting last December; the Minister who chaired it wanted my report so that she could act on it; I said she could not as the report was not due for another year; big problem arose as there are clear implication from what we have done so far for the wellbeing of children and care of pregnant women; Minister sat up when I got round to public health and what the Government could do apart from becoming more efficient on the legal side by way of prevention; Dave King asked for an interim report which I agreed to, subject to modification; have noticed in the Chancellor's budget a massive increase in expenditure on the welfare of children although I do not know whether my document fed into that level although it may well have done so; regret that the press has failed to pick up the massive shift in support for children

28:31:23 Gambling does come into it as the brain areas involved in gambling are the same as those involved in addiction; Eric Taylor's report includes a reference to twin studies and fostering studies in which the behaviour of the genetic parents in respect of drug taking is a larger determinant in their offspring's drug taking than the foster parents

30:53:15 Coming back to Cambridge settled in a village as Prill wanted a place where she could keep her horse; found a barn and land and later got another horse which I learnt to ride; pleasures of college fellowship and the rewards of companionship are affected by living outside Cambridge; however, wonderful to live in a completely different world; our next door neighbours are farmers and this broadens one's horizons; have had some serious illnesses when I nearly died; was true to myself and never took to religion; very concerned about Prill and my children and their sense of loss; Prince Phillip was coming to Sidney Sussex to formally open a

building for which I had raised the money; the Queen had unveiled the foundation stone in 1996; formal opening was to be in March 1999 in my retirement year and I was very keen to be present; Tim Cox was my physician and helped me to get there; not sure I was driven as illness focuses one on things like family and friends; as one gets better then the old drives reassert themselves and I got back to work as before; reflections on nearness of death when very ill

Patrick Bateson



13 December 2007

http://downloads.sms.cam.ac.uk/1112567/1112574.mp4

Patrick Bateson & Robert Hinde, *Growing Points in Ethology* (1976)

Extracted from Wikipedia – 21.8.2014

Sir Paul Patrick Gordon Bateson, FRS (born 31 March 1938) is an English biologist and science writer. Bateson is emeritus professor of ethology at Cambridge University and president of the Zoological Society of London since 2004.

Bateson's grandfather's cousin was the geneticist William Bateson, and his daughter is Melissa Bateson, also a professor of ethology, at Newcastle University. Patrick Bateson received his BA degree in zoology and Ph.D. degree in animal behaviour from Cambridge University. Previous academic positions include a Harkness Fellowship at Stanford University and ten years as head of the Cambridge sub-department of Animal Behaviour. He was elected a fellow of the Royal Society in 1983. He retired as the biological secretary to the Royal Society after five years and Provost of King's College, Cambridge after fifteen years in 2003. He retired from his Cambridge Chair in 2005. He was made a knight bachelor in 2003. He received an Honorary Doctorate from St Andrew's University and an Honorary Fellowship from Queen Mary College, London.

Bateson has written many books and articles on ethology, animal welfare, behavioral development and evolution, gives public lectures and broadcasts.

INTERVIEW SUMMARY

Patrick Bateson interviewed by Alan Macfarlane 13th December 2007

0:09:07 Born in 1938 on the Chiltern Hills; father designed the house where I was born; had a brother five years older; William Bateson, the biologist, was a cousin of my grandfather; he coined the term 'genetics'; he had been working on inheritance for a long time and then Mendel's book became available and he suddenly realized how important his work was and became a champion of Mendel; although I never knew him he was a figure in the family; he was Professor of Biology in Cambridge for a while and then became Director of the John Innes Institute which was in London at the time; he was prolific and got the whole subject of genetics going; fiercely opposed by group of biometricians; he was to some extent a role model for me; remember being fascinated by natural history lessons as a small boy; the local school had a very good teacher called Mrs Truscott; had a number of people in our house during the war as mother was Norwegian and as a result had Norwegian refugees, including Karen Spärck Jones; had a very happy childhood

5:59:07 Mother was extraordinarily vivacious and everybody loved her; I hardly knew my father who was wounded and captured at Dunkirk; I used to write to him in the Prisoner of War camp; he was not very well when he came back after the war and only lived for another ten years; he had been an expert in timber drying before the war and during the war took a degree in architecture in the Prisoner of War camp; he had great charm; he had a brother, F.W. Bateson, who was a don at Oxford, an English literary critic; Gregory Bateson, the anthropologist, was a son of William Bateson; I did not meet him until I was a graduate student; I was taken to a conference in U.S. by Robert Hinde as his student and through an error Gregory was also invited; he was astonishingly like my father even though they were second cousins; we still don't understand why these likenesses occur even though the genetic relationship is not very great; he was a powerful, curiously

inarticulate man though a coiner of terms; had a cult following in California though think his book 'Steps to an Ecology of Mind' is a dreadful book; I did meet Margaret Mead at a conference who flirted with me as a seemingly younger version of Gregory

13:11:10 After my first school I went to a Prep school in Sussex for five years; I didn't like being sent off as a boarder at eight and to begin with was unhappy; eventually settled down and had a happy time; from there went to Westminster, initially as a day boy as my parents were living with William Bateson's brother in Chelsea, looking after him; became a weekly boarder in my second year; initially I didn't do too well but rose steadily up and by the end was in top sets and had started to do biology, which I loved; also rowing and spent two years in the first eight; hard training and chemistry master was furious at time lost; there was a status advantage in sport that gave me confidence; at fourteen had started going to a bird observatory on the Northumberland coast in my holidays where we caught birds and ringed them to study their migration; we had a neighbour on the Chilterns called Richard Fitter who was a well-known naturalist who had suggested I go there; I had already made up my mind that I wanted to do zoology and wanted to go to Cambridge; there met a schoolmaster who described doing a Ph.D. which sounded like heaven

19:33:06 At school had a very good biology master who had also taught Andrew Huxley; I read 'Apes, Angels and Victorians' which I looked at again recently and it is a very good biography of both Darwin and Huxley; reading it at school was the first time I realized how important Darwin was; Westminster had a liberated feel about it; the then headmaster, Walter Hamilton, used to run an essay society which I wrote for and was very helpful in learning to write well; made some good intellectual contacts there, some of which have persisted; I played the cello though not very well

24:06:18 Came to Cambridge for the scholarship exam; should mention that uncle Ned Bateson had been at King's and keen that I come here; earlier he had take me to see Cambridge and he wanted to take me to his father's house (father had been Master of St John's) to show me a little chestnut he had planted; found an enormous chestnut tree had grown; failed to get a scholarship at

that point but got a place at King's in December 1955 and then went to Norway; had a wonderful grandfather who had been Chief Justice when the Germans invaded; the King and Government left but because he stayed he became officially the Government of Norway; he went undercover and ran the resistance; he was never caught by the Nazis and became a hero after the war; I went to live with him and he got me a place in the Natural History Museum in Oslo where I worked every day, learning systematics and how to skin birds; when summer came he introduced me to the Norsk Polarinstitut where I got a place as a deckhand doing hydrographical work and went on an expedition to the north of Spitsbergen where I had masses of time for bird watching; came back and started at King's in October 1957; that winter I went to a conference organised by David Lack at St Hugh's Oxford for undergraduates; he started as a schoolmaster but then wrote a famous book on the life of a robin and then got a place at Oxford; Niko Tinbergen gave a talk about gulls; I met a fellow Cambridge undergraduate called Chris Plowright who had been to Spitsbergen as a geologist; he was also an ornithologist and we wanted to go there to look at the Ivory Gull; talked to Tinbergen who was very enthusiastic as nothing was know about it; a student of his, Esther Cullen, had just published a brilliant paper on the adaptations of the kittiwake, a cliff-nesting gull, and he was very keen that other cliff-nesting gulls should be looked at; Tinbergen had spent a year in Greenland as a young man and was very keen to go back to the Arctic; the plan was that he should lead this expedition but a few months before we left he developed an ulcer which meant he couldn't come; I exploited my contacts with the Norwegian Polar Institute and they took us round to the north-east part of the archipelago where we were dumped on an island with all our provisions and two boats; we couldn't get into the fjord as it was still full of ice but eventually the ice cleared and we could get in and found these pure white gulls nesting on a cliff about 1000 feet up; very exciting to see them; lugged our stuff to the top of the cliff and had five weeks to work on them; found they did not have the adaptations of the kittiwake but it was the first description that had ever been done of them; we nearly got stuck in a blizzard when the boat that was coming to collect us warned us that we were in danger of being iced up in the fjord; Tinbergen was delighted with our

results, including very good film; I spent next six months writing up our results

35:47:13 This did conflict with my undergraduate work and only got a 2.2 and feared I'd never be able to do research; George Salt was my supervisor in my first year; he really inspired me, rather severe, but with a formidable mind; after second year did final year zoology which I loved; taught by Donald Parry and John Pringle; Donald Parry was a kind man but not a very good teacher; at the end of my third year got a first and a University prize; George Salt then suggested I aim at a research fellowship at King's; by that time I had met Robert Hinde who was willing to supervise me but didn't want to risk his friendship with Tinbergen by poaching me; did stay in Cambridge; Hinde was Steward at St John's but soon after got a Royal Society research fellowship; he had come back from Oxford to look after the field station which Bill Thorpe had set up at Madingley; in my first year as a graduate student a Lab was built there and it was renamed as Sub-Department of Animal Behaviour which it still is; Robert has a very sharp mind and reads at an astonishing rate; he was a fantastically good supervisor but very critical; he was like that in seminars where he would tear eminent speakers to pieces; he was unrelenting and people who had experienced this remembered it for years after; as students it taught us to be very critical; do remember that Tinbergen was treated very differently as Robert had a very high respect for him; his paper was on the sorts of questions you can ask about behaviour; question of why gulls removed egg shells from their nests; experimented by putting shells at varying distances from dummy nests and found that the greater the distance from the nest the lower the chance that it would be preyed upon by crows and hedgehogs; suggested this was the reason for the gulls action; this was one of the first attempts to look at the functional significance of a behaviour pattern; he made the point that if you know what the current function of something is it doesn't tell you why it evolved; it could be that when this habit evolved they were then nesting in marshes where the presence of an egg might have actually encouraged disease; possible that this behaviour pattern evolved for one reason but was co-opted for another later in evolution; he was clear about this but many people still muddle it; Robert a marvellous supervisor and

had a number of distinguished student such as Jane Goodall and Dian Fossey, Tim Clutton-Brock and many others

47:38:22 I applied for a Harkness Fellowship and got one to go to California and wanted to work on the mechanisms of behaviour; went to the Lab of a man called Karl Pribram who was a neuro-surgeon and neuro-psychologist; quite different from Robert Hinde; he just loved ideas and was constantly coming up with wild theories; I wanted to work on the neural basis of behaviour and spent two years there; got a rather liberating way of thinking about theory whereas Robert was rather anti-theoretical; an interesting thing to come out of that was an experiment that I did which was to put illuminated panels of letters in monkeys cages which they were exposed to for some time; later they were taken to some apparatus where if they pressed panels letter came up and if they got the right one they got a peanut; discovered that if the monkey had seen one of the letters before then it learnt very quickly; if it had seen two of them then they took much longer to learn to discriminate them than animals that had never seen them before; in a sense they had classified them together and when they had to discriminate between them they had to unlearn the categorisation; this was a new observation and that became the work of one of my graduate students when I came back to Cambridge; I resumed work on behavioural imprinting which I'd done for a Ph.D.; while I was in California I submitted a dissertation to King's and got a junior research fellowship; an important moment as it changed the course of my life; got this in 1964 and that summer had to interrupt the work I was doing as the Harkness insisted that we spent three months travelling round the States; they gave us a car to travel to all the main regions; at the time I was a bit fed up as I wanted to get on with my experiment; of course we had a wonderful trip; then continued working there for a final year; had just married before leaving for California

53:29:20 On coming back to Cambridge had a job lined up for me at Madingley as well where I became senior assistant in research and had the responsibility of looking after day to day administration; Bill Thorpe was my boss and Robert had by that time got a Royal Society research fellowship and fairly soon after became director of an MRC unit within the Sub Department;

decided to resume the work I had been doing on behavioural imprinting; about that time came into King's for dinner and sat next to a delightful neuro-scientist working in the Anatomy Department and discovered that he was very interested in the neural basis of learning; that was Gabriel Horn and we started on a long period of collaboration; we think quite differently in many ways but at the same time complemented each other in a really important way and got a great deal out of it, including a long friendship; think that dining together is a very important way for intellectual ideas to flow; to get this coming together between different disciplines is much more difficult in universities where departments have little opportunity to meet; in Oxford and Cambridge we have this fantastic opportunity to meet people; often in the interstices between disciplines that exciting things happen

57:46:24 At that point Gabriel went on sabbatical to Uganda but when he came back we started working together; we didn't know what to measure at that time and teamed up with a pharmacologist called Les Iversen; he left after a while as he thought we were getting nowhere; Gabriel and I went down to give seminars to a group that Steven Rose ran in London; we wanted to find a biochemist who was keen to collaborate and Steven had been interested in the effects of experience on the nervous system; started an important collaboration between the three of us; then designed increasingly complicated experiments which I think were very important; when you try to look for changes in the brain associated with experience there are all sorts of things that can be going on - the animals can become more active, stressed, attentive, stimulated; if you want to know whether the things you are measuring are specifically related to the laying down of the memory you have to do a whole set of experiments, each of which includes a sub-set of possibilities; devised a kind of triangulation approach which I still think was intellectually very important; sometimes when I see people doing work on the neural basis of behaviour they have not gone through the rigour of excluding alternative explanations; famous example is all the imaging that people do; took us several years but finally able to say pretty confidently that there was an area in the brain which was necessary for the laying down of a memory and specifically related to that; we then needed to identify it much more precisely and at that point Gabriel was

moving to; still needed to identify the precise point and developed a technique whereby you take two groups of chicks one of which had learnt as much as it will learn about the imprinting object and a group that has just started to learn; you wait a day and then train both groups for the same amount of time; one group has learnt everything and the other has a lot to learn; they are both stimulated in the same way and at that point you introduce your biochemical marker; then you kill them, slice their brain and see where the activity is occurring; using this technique of under-training or over-training then re-training them next day we were able to find an area of the brain which was particularly related to the laying down of memory; very important as once found the area could be lesioned before imprinting or after when memory of the imprinting object would be destroyed; that became the basis for a lot of work, some of which I was involved with, which Gabriel built on which became a very important starting point for a whole programme of research

Second Part

0:09:07 Experience with Tinbergen and his interest in function still going on in the back of my mind so asking myself what is the function of imprinting; the usual answer is that animals imprint in order to know what their species looks like; I developed the thought that it enables you to know what your mother or father looks like so you can avoid the hazards of responding in a filial way to someone who is not your parent; if you look at ducks you will see that a female will attack a baby that is not her own; sexual imprinting takes place later in development; developed argument that this enables you to identify close kin and when adult you chose a mate who is a bit different but not too different; strikes a balance between inbreeding and outbreeding too much; followed with experiments on a quail colony at Madingley and found they had a very strong preference for individuals who were their first cousins that they had never seen before; whether this goes on in the wild is another matter but it indicated that imprinting provided a standard to offset mating preference against; echoes the preference for cross-cousins in human societies throughout the world; did not like the suggestion that these findings explained the incest taboo which I see as conformist behaviour; another thing I was interested in was the development of mother-offspring relationships in cats and also

play in cats as little good systematic work had been done on play; love cats and breed them at home; two interesting things came out of this work, one is that play is heterogeneous i.e. play with each other before play with objects; secondly, there had been idea of parent-offspring conflict which was particularly marked at the time of weaning but I felt this was wrong; found that if the mother was in bad shape the offspring pick this up and wean themselves and go onto solid food; conversely the mother has to be sensitive to the condition of her offspring and if they are in poor shape she will spend much longer looking after them if able to do so; now interested in the ways in which cats develop depending on conditions in the environment; David Barker's work on the life histories of babies recorded in a midwife's notes from the early part of the twentieth century; found that small babies were much more likely to get heart disease as adults; association of a mismatch between the environmental conditions at birth and the subsequent change which meant they were not adapted to deal with them; problem acute in places like India where heart disease and diabetes are at epidemic levels; conversely, big babies are poorly adapted for famine conditions; reflections from demography; effects on subsequent generations spawned whole new field of research, epigenetics, where the mechanisms of transgenerational adaptation are explored; no actual change in the DNA but suppression of some parts of the genome and activation of others which are appropriate for the lived in environment and for their offspring; human migration and adaptation subjects of great interest now because of the health implications

15:00:11 Was critical of Richard Dawkin's selfish gene hypothesis as it misled people into thinking genes actively determined development although he himself did not actually think that; he went on to argue that communication did not involve the transmission of useful information but was merely manipulation; still people who believe in the selfish gene rather than cooperative behaviour but they has been a shift toward the latter

17:29:16 By the mid-eighties I had been in Cambridge for a long time and had seen how refreshed colleagues were by going elsewhere; then it was suggested that I should be a candidate for Provostship when Bernard Williams retired; I was a bit reluctant

but agreed; odd election as a lot of the younger fellows wanted a Provost from outside and had I been one I would have agreed with them; found the campaigning uncomfortable and feared it would divide the college; I was just elected by a few votes; there was a lot of ill-feeling afterwards and I quite often had a really difficult time on the governing body which persisted for quite a few years; about halfway through things changed and it became much easier; that said, it was a very interesting period for me as it allowed Dusha, my wife, and I to work together because the job requires a lot of entertaining; she was a marvellous hostess; it also allowed us to meet people we would otherwise never have met; the most startling of these was Princess Margaret; shortly I took up residence in the Lodge, Jack Plumb, the historian, asked me to invite her to the Advent Carol service; when she accepted, Plumb said she would stay for the weekend; she came with entourage; we had to treat her as royalty and she was a little awkward about coming into an academic household; it went well and having done it once, Plumb again asked us to host her; she came about seven times in all; over the years we got rather fond of her and it became quite a nice relationship; on another occasion the Dalai Lama stayed; he was wonderful, with an extraordinary warmth about him; he brought two monks with him and we were told he would not have any food after midday though he did join us when we ate; was very interested in science and quizzed me about my work; talked about Tibet and the Lama system; was not very keen on the latter though thought the culture of Tibet was important; the Lama system was, in fact, fairly recent; interesting that when the Chinese moved up to Tibet their the women had to move down to lower altitudes to have their babies; set me wondering whether there could have been selection over the centuries to allow Tibetans to survive at high altitude; we had a question and answer session with him and our students and he was asked who did he most admire in the world and he said Gorbachev; by that time (c1991) Gorbachev was very unpopular in Russia, but the Dalai Lama thought he had done more for world peace than anybody; later on John Barber invited Gorbachev to come to a conference in King's and he stayed with us; when Bukovsky who had been persecuted in the Brezhnev era and had got out, he became a student here, stayed on in Cambridge and became involved in extreme right-wing movement; he wrote to me saying it was intolerable that I was going to host Gorbachev in the

Lodge; told him that what was good enough for the Dalai Lama was good enough for me; found Gorbachev fascinating

30:19:19 Another visitor was Salman Rushdie who was then in hiding but we heard that he wanted to give an address in the Chapel; we agreed; incredible police procedure as they feared for his life; they minutely inspected the Chapel and the Lodge garden; Dadie Rylands complained about rough looking types with dogs in the garden; Salman came for lunch and all the guests were carefully vetted; behind every curtain was an armed Special Branch man; sitting in Provosts stall in the chapel, looking up at ceiling bosses which are Tudor roses, seen from the side, look like the face of an angry man; not sure how intentional it was but once you see it you see it all the time; notion that God scowling at me as a non-believer; think I am an atheist as I really don't believe in a god; was brought up in Church of England tradition and I love a lot of the ceremony; music in the Chapel is wonderful and am a great supporter of the choir and Stephen Cleobury; thought that this was one of the truly great things about this college; I had no compunction about taking part in ceremonies though could have been accused of hypocrisy; predecessor were of similar opinions; coaching for bible reading from Dadie Rylands was just to breath deeply

38:49:47 When I had to take on role of fund raiser that meant quite a lot of travel in the US and meeting absolutely delightful people; there were aspects of the job which I liked very much; also in the second part of my Provostship I instituted the Provost Seminars which were also very good; brought students and fellows together and we had some marvellous speakers; also Dusha and I used to have musical evenings in the Lodge; students would organise the music and we would give them a meal afterwards

40:57:16 One of the changes that occurred during my career as a behavioural biologist was the increasing rigour in the way people worked; had a very bright graduate student, Paul Martin, who came back to Cambridge as a post-graduate for a while; wrote 'Measuring Behaviour' with him which has been very successful; marked a change in the subject where people were getting increasingly careful about how they measured and did experiments; downside was that

it is very easy to measure things in a trivial way and stop focussing on the big questions; at the same time I was also editing a series with a man called Peter Klapfer where we were trying to encourage people not to be constrained by tight methodology; we invited essays for this series and sometimes got pieces that were incomprehensible so had to strike a balance; many good students at Madingley, particularly in the 1970's, like Tim Clutton-Brock and Richard Wrangham; also some very good research assistants, one of whom was Prill Barrett now wife of Gabriel Horn and she worked with me on play in cats; Nick Humphrey had been a student of Larry Weiskrantz and gone with him to Oxford; applied for a job at Madingley and he was a delightful and stimulating man, doing interesting work; however he felt increasingly that very few people would actually read his papers and wanted to get at a much wider audience; decided to get involved in television and mistakenly, I thought, gave up his job to make films; in the end the films were not very well done and he had a difficult time getting back into the academic world; in a book that Robert Hinde and I edited celebrating twenty-five years since Madingley was established, Nick wrote a wonderful chapter on the social function of intellects, the most cited chapter in the book which started to become a whole new field on brain development

46:38:23 I had been doing a study for the National Trust on the hunting of red deer by hounds; suspect they thought I would do a whitewash but I decided it was to be done properly; reported to the Trust in great secrecy and on the strength of my report they banned such hunting immediately; I became the object of hatred to hunting people and they did their utmost to ruin my reputation; just at that time I was elected to become Biological Secretary of the Royal Society; I enjoyed the role as it enabled me to encourage the Royal Society to be much more positive in getting science across to the public; Aaron Klug was President when I joined and in his quiet way did a lot; he is a shy man who doesn't relish a public profile, but a wonderful man; his successor, Bob May, was quite different and very good at projecting science; as Biological Secretary I had to sit on all the sectional committees which deal with biological candidates for the Royal Society; was worried about tactical voting and decided to institute a new procedure which made the voting records transparent and encouraged honesty

51:37:07 When Gabriel Horn retired from the headship of the Zoology Department to become head of Sydney Sussex I agreed to take it over; thought it would not be for very long as we had a candidate from the US, Jared Diamond, who everybody wanted; he wanted to come but the pension arrangements are so much worse here and after a long period of indecision he decided not to; the next candidate also failed to come so my period of headship extended; at the same time I was Provost of King's; the difference between the department and King's was striking; department meetings were businesslike and efficient; at King's there would be long discussions which appeared to be reaching a consensus when someone who had been quiet until that point suddenly lobbed in a hand grenade and shattered the consensus and you would have to start again

54:47:04 Noel Annan was Provost when I was an undergraduate and later became a good friend; an important Provost who got the Research Centre going; Edmund Leach, his successor, was also interesting and we had good discussions on the whole business of nature and nurture; towards the end of his Provostship he was torn about whether he should give up so he appointed a committee of three to give him advice on when he should retire - Bernard Williams, myself and Ross Harrison - three future Provosts; Bernard Williams was totally different in style; very quick and intelligent but could lose patience with people; it was he who persuaded me to take over as Provost

59:45:06 Advice to a young scientist is to enjoy it

Patrick Bateson in conversation with Gabriel Horn

http://downloads.sms.cam.ac.uk/1783971/1783974.mp4

13th July 2007

INTERVIEW SUMMARY

Patrick Bateson in conversation with Gabriel Horn

13th July 2007 Filmed by Alan Macfarlane

0:07:09 AM: How did your collaboration begin?

PB: Did PhD on process called imprinting in young birds; went to Stamford to work with Karl Pribram on the neural basis of behaviour; back in Cambridge resumed work on imprinting; memory of meeting with Gabriel in King's and discussed interest in neural basis of imprinting; Gabriel was interested in habituation; began to see the possibility of combining our interests; Gabriel went to Uganda so we didn't start our collaboration until 1967, two years later; we started by looking at a particular enzyme of birds that had been imprinted and found some changes; looked round for a neural chemist who could work with us and started collaborating with Leslie Iversen; did several experiments but not very conclusive; then we both gave a talk to a group in London hosted by Steven Rose; he had been working on exposure to light in rats and was very keen to collaborate; we started this collaboration in about 1968; we were looking at changes in the synthesis of protein and got some results that were quite interesting; then we moved to the precursor of protein, RNA, that needs to be synthesised in order to build protein and that gave more interesting clear cut results; at that point we really started to think clearly about the issue and realized that there were so many other things going on when a young bird starts to learn about the characteristics of the pseudo-mother; they run around more, are stimulated more visually, they get excited; all sorts of things could be producing these changes in the brain; then we started to think about what sorts of controls we could do; even at that stage we realized that no one experiment was going to solve all the problems

5:48:12 GH: My recollection of our meeting is almost exactly the same; I had been supervising in College and I was late for Hall; I rushed in and there was a queue, and there were you in the queue; so we stood together and walked in together; that was purely chance, a wonderful piece of coincidence, arising out of the collegiate structure; in those early days when we didn't know what to look for there was a lot of interest in this enzyme and there was a technique available for studying it, staining it with a dye; we had a new microscope in the Department of Anatomy where you could compare two images, so we did see differences; after that remember after the meeting, discussing whether we should ask Steven Rose to collaborate; you were going to another meeting at the Royal Society and asked him there; another thing that was important for me was that Robert Hinde and I had organized a conference, on short term changes in nervous behaviour, at King's in 1969; you gave a paper on the huge difficulties of interpreting a brain change from the behaviour; you compared all sorts of traditional controls that were being used in the behavioural sciences at that time and showed that many of them were totally inadequate

PB: I think my chapter was called 'Are they really the product of learning'

GH: That was the time we had to think; we would sit in my room discussing strategies for designing experiments

8:56:01 PB: Going back to that conversation at high table; the Nobel committee have made a film about prizes; they came to interview me at King's about the importance of the collegiate atmosphere in the winning of Nobel prizes; I gave the example of this long collaboration with Gabriel which started at a meeting over dinner; the sociality of it and the alcohol, all adds together to create a sense of stimulation and enormous interest in the person you are talking to; it was a very important context for starting a long collaboration

GH: I had this collaboration with John Griffiths; he was a Fellow of King's and we developed a friendship; I remember him coming into hall one day having read a book on the nervous system, he was

a theoretical chemist, saying "If you can tell me how a neuron works I'll tell you how the brain works"; we jostled over this ridiculous remark as there is no singularity of neuron, but he became very interested in the nervous system and we published a paper together; another instance of how collaboration takes place; I think that when you have informal circumstances, one develops friendships

PB: To continue the story; one of the first controls that we used a crucial structure of the bird's brain; all the fibres from one eye go to the opposite side of the brain; there is a connection between the two sides of the brain but it occurs later in the neural pathway; Gabriel was able to develop a technique for splitting the two halves of the brain without cutting the visual input; one could have one part of the brain which was receiving input and the other part, not, if you cover up one eye; it was a marvellous bit of surgery for developing a split brain animal where you train one side of the brain and the other side is untrained; that procedure produced an extraordinary result; a very clear difference was found in one specific region of the brain when we compared the trained with the untrained side

GH: Had to develop a special knife and an instrument on a framework; it had of course been done on other mammals and even in humans, but their size makes it easier

PB: You have to be careful because if you cut too far back you blind the bird, so it has to be done with great precision; anyway, it worked, and that was our first control; the difficulty still was that the trained side of the brain was more stimulated visually than the untrained side; we didn't know that it is necessarily related to something to do with the storage of a memory, it could be something entirely non-specific just to do with exciting the brain; so we had to develop other techniques; one was just to exploit the variation you get in chicks; some are much more active than others and some learn more quickly; we then simply relied on a correlation between the things that we could measure in behaviour and the changes in the brain; that came up with the result that again, when it was to do with learning there was a strong correlation, but how active the chick was or how quickly it started

to run were not correlated; so we could use the correlation technique to say there was something about this bit of the brain that was special; this was the second technique that we used

14:16:19 GH: We didn't know which bit of the brain it was; if you give the theoretical background to this, Karl Lashley had proposed that the memory is distributed throughout the brain; any idea that memories were localized in a particular region was simply not on; he had spent his life working on it; when we began our work this climate very greatly influenced the thinking; we were in some sense tainted by it; what we did was to divide the brain up into three bits; although the size of a chick brain is 1cm from front to back it contains many millions of nerve cells, so we were looking at a pretty large chunk; nevertheless it was in that chunk that we got these effects of changes in biochemistry which were very strongly related to learning

PB: The final experiment we did before we went on to much more detailed analysis was an ingenious experiment in which we trained one lot of birds for a little bit of time and another lot for a long period; then a day later we trained both groups for the same amount of time; that was when we were measuring the synthesis of RNA in the brain; the beautiful result there was that the over-trained birds which had already learnt as much as they could showed much less synthesis than the under-trained birds; this control showed that biochemical activity in the specific region of the brain that we had previously implicated in the imprinting procedure was actually related to the learning process and not to just being active as both groups were equally active; I am still very proud of that experiment

GH: So am I, but it was the three together...

PB: The three together... one bearing on a mountain does not tell you where the mountain is, you need to have more; each experiment was eliminating a different subset of possibilities; putting them all together then led to a very robust conclusion; the bit of the brain that was active and leading to structural changes, was the one most likely to be associated with the storage of

information; that conclusion led on to much more careful studies to localise....

17:26:03 GH: It is worthwhile saying that those experiments took a long time to do; we first published in 1969 and the last of that series was published in 1975; of course, the 1969 paper was built on work done from 1967, so we were refining our methods; I remember that the experiments were done and the samples sent off coded to Steven Rose at Imperial College, and we never knew what the answer was until the results came in and Pat had decoded the biochemical results

PB: They were done blind...

GH: That we regard as necessary; stemmed in part from the days we worked with Les and I don't think we were doing it blind in that way; afterwards we realized that we had to, and ever since then we have always worked blind; I think it is crucial; this work is sensitive to all sorts of variation and you must diminish them

PB: There is a massive amount of work done by psychologists on if you have an expectation about the result it affects how you handle the data; in my field, many people don't do their work blind; they know which way the animals or people have been treated, and you get the extraordinary halo effects that influence the results enormously; I think it crucial; knowing how the animals have been treated can sometimes account for all of the variation; there is a big lesson to learn there

GH: I remember you used to phone me and say these are the results and I was always uncertain that we could replicate the previous results; it always came as a surprise to me because at that time there was such consistency in the data; the next step was that, given we had found these rather crude biochemical changes, all it meant was that if new proteins were being made there will be more component parts needed for these proteins; we took a component part which in the case of ribose nucleic acid is uracil and made it radioactive, so in areas where new proteins were being made, in that area where they were being made more quickly, you would see a higher level of incorporation into that tissue; it was all pretty

crude stuff, but what we needed now if we were going further on the localization, is the memory localised to a particular brain region, would be to do what is called auto-radiographic technique which is to try to find out where that radioactive probe is in the brain and to do so precisely; that was a rather massive undertaking; by that stage I had gone to Bristol but we still collaborated; Steven had gone on his own way and there was a gap of a year or so; while I was in Bristol we imprinted chicks using that two day technique that you described; then we had to cut sections through the brain; in what we call the roof part, I had to measure some twenty-nine different regions using the microscope to make the measurements; the slides were randomized so I did not know which was which; I would not let anyone else do it although I was still running the department; every afternoon I would go and sit at this awful task at the microscope; the results came out that there was an increase in a very particular region of the brain which we called IMHV; I remember Steven Rose phoning me and he asked where the region in the brain was; I said that I couldn't tell him until we had published the data; occasionally these things get out and someone scoops it; when you submitted a paper to a journal then it took eighteen months; I finally capitulated and let Steven know what the region was and he then went off and referred to it in a lecture; in the audience were one or two people from Japan, Takamatsu was one, and he went off and replicated the experiment and found the same result, and he published in the same journal as us; however, he published a short note which took six months so he got it published in the same year; often I read some of the Japanese literature and it refers to that result rather than to ours; to give him credit, somehow he had got hold of the reference to us and did refer to us; it is an example how one can make a mistake but it is difficult when you have a friend pressing you.

24:30:18 AM: There must be a tension between collaborative work and hiding your results

PB: It is a real tension and it is much worse in some subjects than in others; molecular biology is horrific because of fear that someone else will do your experiment and publish before you; in my field nobody cares that much because they are all doing different things

GH: In what is called the meat sciences there is a lot of competition; the tensions are very real; Steven was working on another form of learning in chicks so left us with something else; he immediately went to see whether his form of learning involved the same region as ours; they published six years after us; interesting that they tend to refer to IMHV and their paper as the source of it; occasionally I have to say that the first paper was published in 1979 and not 1985; after that we realized that the neurobiological world would not be very interested in what we were doing unless we were able to show that the findings held true if we destroyed the area as all this was correlational, however good our controls were; suggest we should put a lesion, destroy the piece of tissue and stop the animals from learning; it did; the next question was if you put the lesion after they had been trained and memory formed, you should abolish the memory; it did; did had a total of four lesion experiments and they all pointed to exactly the same thing; I think many people in the world are not persuaded yet and I don't know how one can; in the mammalian world there is very little evidence for localization except in olfactory work; I was talking to Brenda Milner who was the person who was involved in the first lesion experiment in humans; the man had epilepsy and they removed the region of the brain called the hippocampus and he had a very severe effect on memory; he could not acquire new memories; this triggered off a huge amount of work which is still going on in the animal as well as human world; when I ask Brenda what she thought the hippocampus is doing she had said that she hasn't any idea; it remains a mystery; Malcolm Brown has shown from electrophysiology that what was claimed that it would do in memory it does not do; it is some other region that was hooked up in the lesion when Brenda Milner's surgeon did the operation on this human being they necessarily could not restrict the lesion to suck up that bit of brain but took quite a bit more; it turns out that it was the other bit that they sucked up that was doing the damage rather than the hippocampus itself; I think the issue very far from being solved; the only way to persuade anyone that we do have localization is to encourage them to read the whole build-up; it is not only the biochemical work which was a self-contained unit, the

predictions that are made from it about lesions, for example, to a restricted area but not others are also consistent; since then, as you gradually withdrew a little from it, we began to think of theoretical aspects of it as well; we went on to other molecular biological aspects; what is going on in this brain region and electrophysiological studies recording from single nerve cells in there; but in our collaboration we did go on to think out how you might model the function of this brain region

31:10:13 PB: Going back one step, nowadays we have all sorts of imaging techniques that are available to see where bits of brain are active - PET scans etc. - and they generate wonderful pictures, but we don't know whether the site of activity is upstream or downstream or to the side of the effect in which we are interested; people forget that and don't do all the necessary controls and as a result there is a vast amount of literature that we simply can't interpret properly; at a very early stage in our collaboration we had been talking about how we might start to model the effects we had discovered, however we did not publish anything

GH: When I was in Bristol we used to see each other about once a month; we did that experiment of stimulating the brain, talked about theoretical work, and used to work long hours on this model; I said I didn't think it was very interesting as I thought there was nothing new in it, so it was entirely my fault that we didn't move on; then, several years later when I had come back from Bristol to Cambridge, I was at a meeting in Oxford on networks; I saw the main speaker put up a model that I could have sworn had been rifled from our files; I was shocked, and came back grovelling as I felt I had done you a great disservice; had we published that paper at the time we were writing it we would have scooped the field; it was absolutely new; but for me I did not see any new principals of neurobiology, what I failed to grasp was the fact that there were new ideas for artificial intelligence

PB: I spent an awful lot of time developing computer programs to simulate this which were realistic, both at the behavioural level and at the neural level; these programs were actually very remarkable because they were simulating exactly what we had been finding, and simulating a lot of the biology; they were also throwing up things we

hadn't really expected; there is a remarkable process which we are all familiar with, known as developing polymorphous concepts; there may be a variety of things by which you recognise something but not all of those things will be present all of the time; it is very difficult for humans if you only have a subset to categorise them; our neural net model would also have difficulty with it unless it had been exposed to pure sets which had all the components of one category and of the other category; you train it for quite a long time and then you start to take away some of the characteristics, and then it generalizes very well; what was regarded as an extremely difficult cognitive problem actually turns out to be simple principals of generalization; neither of us expected that feature of the model to come out, but it did; the other thing that I thought was very exciting was that we know that very detailed little things about the mother are important in distinguishing her from another mother; what we found with the model was that even things that weren't very exciting to the model to begin with, if they were associated with things that were exciting and had high value, by degrees the things that had low value became more and more important; by the time the model was well trained it would do what an expert birdwatcher will do in recognizing a type of bird as it flashes past; that was another nice thing that came out, that you could train it to recognise very fine features, which an animal has to do on the basis of rather limited information

GH: The computer model was based on what we knew of our chicks so had a very strong neural basis as well as behavioural basis

PB: It did have a lot of interesting consequences; we eventually published it long after we might have done

PB: This work was published in the early 1990's, when there were lots of people working on neural networks

GH: We had begun when I went to Bristol in 1974 and we were onto this

PB: I have a sketch of the model that we finally used drawn ten years before

GH: Mine was all dusty and yellowing when I went back to it

PB: I guess that was our last bit of collaboration, but that was about thirty years after we had started

39:45:09 AM: How long did you spend with each other?

GH: In the early days of our collaboration we spent hours and hours, walking and talking, or in our room; I remember one lovely walk alongside the brook that runs under Brooklands Avenue..

PB: I had an allotment there...

GH: That was one occasion when we thought up an experiment in which the question was could you implant a memory in a brain; always thought it was impossible to do, but we knew that chicks were able to discriminate lights flashing at different frequency; we did do an experiment which involved planting electrodes into what we now know to be IMHV (IMM as it is now known), and giving pulses at the frequency of a flashing light; one group of chicks had a frequency of 4.5 per second the other at 1.5; you match it for the number of pulses; then after you finish that you give the chick a choice between a real light in the outside world which is flashing at 4.5 per second and a light which is flashing at 1.5; the chicks with the 4.5 stimulation went for the 4.5 in real light and the chicks trained at 1.5 went for 1.5; it did not happen when we put the electrodes in another bit of the brain which it should have done had it been a trivial observation; that also did not elicit much interest; one day, many years afterwards, I had come home from the Department of Zoology; my wife, Prill, said it was wonderful being married to a famous person - obviously ironic; what had happened was that a thriller had been published, the centrality of which was that someone had had a memory implanted in their brain by electrical stimulation; the key was, according to the detective, that if you wanted to know the answer you better read the paper by Horn et al, Cambridge scientists; Prill had just read this book

PB: It was such an implausible experiment but it did work

GH: Implausible except people had been saying that if you want to demonstrate an area brain involved in memory you ought to be able to implant a memory in that region and that is precisely what we did

PB: That was the result of a long walk; we also used to walk along Devils Dyke

GH: In Bristol we would walk around Clifton, the tall figure of Pat in an utterly disreputable overcoat

PB: My tramp coat...

GH: Otherwise it would be intense discussion with bits of paper; I don't remember alcohol when we were talking about experiments; after we had done an experiment at Madingley we used to go out to a pub at Dry Drayton and have a ploughman's lunch and half a pint of beer; that was the only drinking that was done; on the drinking at King's when we first met - he is younger than I and had just become a Fellow; the beauty of King's is that it is non-hierarchical, sitting next to someone, having a glass of wine, and they ask you what you do; then it begins to flow more easily; I think it is just made for intellectual interactions; I have had more than one but Pat's is the longest and most sustained

46:28:17 AM: How did you deal with the difficulties of co-publishing your work?

PB: We alternated authorship

GH: And where it was clear that one had done more than the other, for example the brain lesion stuff, that my name would come first, and for the ...radiography; for many of the behavioural papers, your name came first; the order of name was important for us

PB: It is a potential source of conflict and is particularly difficult when you have more junior people involved; I had a practice of allowing my graduate students to publish on their own as felt that

my name being there would actually interfere with their subsequent careers; not all people feel that but I did

GH: I did not do that; when I had research students I spent a tremendous amount of time teaching them the techniques, and designing the experiments for them, particularly in electrophysiology; they needed help all the time and it was also part of my own research activities, but I always put their name first; certainly with my junior colleagues now, where I will have designed the experiment, taught them what to do and analysed the results....

PB: There has been a change of culture with Research Assessment Exercise, now you have to do it to claim credit for your department; that has been imposed on us really

GH: I think in the behavioural sciences there was very much more the tradition of letting the person write...

PB: And there would be much more of their own individual effort, working on their own; I would spend time with my students but don't think I ever spent as much time as you did training them

49:38:23 AM: Would it be true to say that without your collaboration many of the things you were trying to do would have been impossible?

GH: I would say unequivocally that the things I have done could not have been done without that collaboration with Pat; was Pat unique in his knowledge - if I had got someone else with the skills in imprinting in the behavioural sciences, in principal I could have done it - but it so happens that Pat was very nearly unique in the world at that time; it was my good fortune; the other thing was that we clicked, we liked each other; if there had been another Pat intellectually in the knowledge of imprinting, I might not have got on well with him and the whole thing collapse; the fact that we got on well together, that our families got on well, and we are very close friend still, and had the community of the College to keep us together in our professional lives; we kept together in the laboratory, even though initially we had our own laboratories; when I went to Bristol we had to contrive our meetings; then when I

came to Zoology we were in the same department; there was a different dimension of the College where we would meet and have a common interest within it which was the College; the third dimension was the joint work, but I think it was the amalgam without which it couldn't have been sustained

PB: It was interesting that because we spent so much time together we realized that quite often we were punning, using words in slightly different ways, so we would sometimes pass each other by in our discussions; I think it happens often when people from different backgrounds start to work together, they are using the same words in different ways and it takes time to discover that; you need to work together for a long time before you know that actually you are using language rather differently from each other; I find that very interesting and I see it too now with people who are trying to collaborate and falling out because they don't understand each other; can't remember whether we built up a language for communicating...

GH: The acronym that we had for the bit of the brain, that was a code, but it is known throughout the world by people who know about this region of the brain...

PB: Not consciously, but I am sure that if I had been describing what we were doing it may have been opaque to the listener, but we knew what we were talking about; I had an interesting experience when I gave lectures as a University Lecturer, and I thought it important that the students acquire some of the jargon of the subject; the lecture was studded with jargon and we have a questionnaire at the end of our lectures and the students complained about the jargon; decided to do my lectures in plain English the following year; when I started to turn my lectures into plain English, even I did not understand some of the terms; it was quite salutary

54:29:22 AM: Is there any truth in Einstein's remark about the need for absurd ideas?

PB: We did have long discussions, very often with Steven Rose, and Steven didn't properly take the point about the difficulty of

sorting out the things that were always confounded; we had almost philosophical discussion on how could you be sure that what you were dealing with was both necessary for and exclusively related to the storage of information

GH: That was about linguistic difference and the use of language, and those views on not needing to control for these various factors is still widespread

PB: Having absurd ideas I am not sure about, certainly having inchoate ideas is another matter; certainly we would have ideas that were not properly formulated so in a sense they might have seemed absurd, but more likely they would seem undeveloped and need work; that is much more a feature of the conceptual problems that one can have in a subject; you have a glimmering of what it might be like to do certain things and then as you think more and more about it, it crystallises; that is more like the pattern than you would come out with ideas that were ridiculous

GH: I think the only time we did something that we thought was ridiculous was that brain stimulation experiment; as we were walking along and this idea comes up in the conversation, I can't be certain about it but it did seem a bit way out, but it wasn't conceptually absurd; people had spoken about it and couldn't do it and we thought of a way that we could; Einstein's remark applies more to physicists, cosmologists, mathematicians perhaps than it does to biologists; certainly our work on imprinting was radically different from what anyone else was doing; it was probably the first way forward after the years of depressing work by Lashley putting in brain lesions where they couldn't find any effects in mammals; our work could not be described as absurd but just very logical

PB: Gabriel rather implied that people haven't taken any notice of this work, but I think it worth saying that Gabriel got the Royal Medal of the Royal Society, which is one of its most distinguished medals for this work and all the detailed molecular biology that followed from it, so it was recognised

1:00:18:08 GH: It is slightly puzzling to me that though the work is quite well known in Europe, Japan, India, China and people know

about it in the States, but very little research has been done on imprinting and therefore our work on the memorial aspects of it, we have many firsts in advancing the understanding of how that image that the chick acquires as it learns, we have a very good idea now of what actually goes on in the brain and in the memory; our electron-microscopy study was the first to show that the junctions between neuron actually change and specifying actually what the change was; that is almost never referred to in the U.S

AM: Is this a problem with American perceptions of what is past?

GH: They did pick up the habituation work from Europe and taken to the States and was developed in this way; they are well known for not citing European literature in general, it is not just neuroscience;

AM: Were either of you ever tempted to move to the States?

PB: I was tempted to go to the States in the late 1980's and felt I had been in Cambridge for a long time; I did do a post-doc in the States and had a number of visiting chairs, but I did feel that I wanted to get out of England at that point; I went to Berkeley and was quite tempted to stay there but then the election for Provost came up here and I decided to stay in Cambridge

GH: I would not have gone; after my father's death all my close family migrated to the States; when I went there I experienced both academia and the life my family had; they were not rich and I realized the problems of not having health insurance or pensions; I would have not necessarily suffered as they might, but had I a prolonged illness I would have lost my insurance; realized that the US was a wonderful place to go to if you were single, young, healthy, and if married, don't have children; given the vagaries of life, I couldn't have an expectation of all those things, so I was not attracted; the American dream favours 30% of the people, and with its inequities is a return to nineteenth century Europe

PB: I questioned a surgeon friend in US on how much he would charge when he operated on a patient. He replied $10,000

1:08:10:06 PB: I am not sure I would have gone to the States but I have a lot of rather good experiences of working with colleagues there; it is probably correct that the kind of atmosphere that we have in Cambridge is extraordinary; I agree too about productivity, certainly in the past all the innovative ideas in my field were coming out of England and very little out of the States

GH: Although I sound anti-American, I am talking about the social structure; that is not to say that we don't have problems here but we do have a system for catching people who are deprived; however, I used to love going to the States; I liked the academics there and the discussions I had, and did like the ambience; but asking a colleague at Berkeley what technical assistance he got from his department, he got nothing; I particularly need a lot of equipment and assistance for my research, some of which is provided by my department; in the States there was nothing; your stipend was for nine months of the year and you spent the other three months hoping to get money from the research institutes and you had no technicians; if your grant expired you had nothing; even a phone call was charged; this was in the 1960's and 1970's; we don't have that situation here; on the other hand, if you go to a large research institute where everything was funded and you could do your own thing, that would be great as the equipment would be superb

PB: If you look around Europe, the German way of doing things is to take their best scientists and put them into Max Plank institutes; some of them work very well but I can think of quite a few where there are extremely good scientists with everything they need, and they go to sleep; in the French system, if you become part of the CNRS you have a job for life and people become unproductive after a while; there is a kind of stimulus in a place where you are in touch with the young who will come and question your assumptions

GH: I think that is crucial; the exposure to young minds, and especially the young minds that come to Cambridge, they are very bright young people; I have been challenged in a most marvellous way teaching first year undergraduates; of course, in a research

institute you have all the equipment, you don't have to apply for grants, but you are deprived of this source of intellectual stimulation; I am in this situation because I chose to be; I find that when I contrast working in a laboratory at CNRS in Paris, there were no students, and for me that sort of place was hollow; the interactions were rather inconsequential, the people would be coming in extremely late and not being sure what they wanted to do; no stimulus to do very much and no challenges; this can't be true all over but there is a risk; in this atmosphere, especially in the collegiate system, where you meet people from other disciplines as well; those other disciplines - you were in behavioural science, I was in anatomy, how would I have come across a behavioural scientist? - the same is true for John Griffith, a mathematician - how would I have come across such people without some melting pot of intellectual discourse which a college is; you get so bored with students at times, especially after doing it for a number of years, but there is a real benefit

PB: There is a trade-off; I see colleagues who go into universities where there is a mass of teaching and too much student contact, and they suffer and don't produce anything ever again

GH: I remember Berkeley I taught a pretty heavy load for the period I was there but the assistant and full professors had huge teaching loads and laboratory classes; although some of the labs were run by their PhD students, they were responsible for them; the contact time they had with students was huge; I asked one of them how he got time to do any of his own research; he was in his late thirties and had published some good papers; he said he never went in the lab but told his research students what to do and they would bring the results; he had no time to learn new techniques

1:17:23:00 AM: What about going further east?

PB: Singapore is incredible; an enormous amount of work is going to move there; one company is going because they are so harassed by animal rights people; the Singapore Government pours an enormous amount of money into science

GH: It took Sydney Brenner there and other distinguished people...

PB: They have very good students as the education system is so good; I have no experience of Japan or China, but I was very impressed by Taiwan where they have an extremely good schooling system and they are very well trained; a lot of things we are loosing through sheer incompetence they are taking a lot of trouble with

GH: Our own colleague, Michael Bate, a very distinguished developmental biologist, spends a great deal of time in Bangalore and loves it; they have a research institute there headed by a King's man; I employed a Chinese post-doc; he had done a medical degree in China, then did a PhD in Brazil, then he came to work with us, and now he is in the States; there is a Sinification of science, especially in the States; the fact that 9/11 reduced the intake of people from the far east into the States has had a near catastrophic effect on science there; I remember attending a meeting here about three years ago when the president of the American Academy of Sciences who aired this problem of the restriction on foreign graduates; a colleague here, a professor of Chinese, told me about the Sinification of science and I realized that a lot of papers in my field were by lead scientists who are Chinese; I don't know much about science in China myself, but do know that Tom Blundell thinks very highly indeed of their biotechnology and they are moving at tremendous speed

PB: They are turning less and less to the West now they feel sufficiently confident about what they are doing

GH: Many are going back; that is a deliberate policy; I had the pleasure about five years ago of meeting the Chinese Minister for Education; I asked him about investment and he said that 15% GDP was going into higher education - an astonishingly high figure; they are pouring money into scientific institutions with the clear intention of making first class science facilities, but also drawing back those who have gone abroad

PB: A Chinese Research Fellow was here who, during the Cultural Revolution worked on a pig farm, where he had taught himself English, higher maths, and advanced physics, in his spare time; he had got to Australia and then came here; an incredibly nice, very bright man; although he had all these ghastly experiences as a young man, he went back to China

Robert Hinde

7th and 20th November 2007

http://downloads.sms.cam.ac.uk/1121517/1121555.mp4

INTERVIEW SUMMARY

Robert Hinde interviewed by Alan Macfarlane 7th and 20th November 2007

0:09:07 Born in 1923 in Norwich; father was a GP who had married a nursing sister at Guys where he had trained; youngest of four children; mother was Isabella Taylor from a family in Kirkby Lonsdale, Westmorland; her father had died early and her mother had brought up six children by starting a cake shop; several of the girls had courageously left to work elsewhere; my mother was very loving and I got a great deal of her attention as the youngest; think my security stems from having had a very good relationship with her; father was a conscientious GP with an intense interest in natural history and also the ancient history of the Middle East where he'd served in World War I; sure that his encouragement of my natural history instincts were important

3:35:20 Went to Oundle school at eleven which was too young and I hated it; I made no real friends there; known from early on as "the professor" as I wore glasses; not myopic but father mistakenly thought I had a squint aged two and had to wear glasses; not very good at games; I was entered for School House as the head of house, Kenneth Fisher, had an acquaintance with Kirkby Lonsdale; first boy to go into a science school certificate form where the form master, Ian Hepburn, to whom I owe an enormous debt; moved to his house where he was a wonderful bachelor housemaster where there were concerts on Sunday nights; he was a keen naturalist and wrote a good book on flowers of the coast for the New Naturalist series; took boys rock climbing and took me bird watching a lot; Fisher was also a keen birdwatcher and also took me out; at the end of my time at Oundle the war was beginning and the wife of his son, James Fisher, the famous ornithologist, came and taught me English and was a great influence; she made me not just a scientist by supplying me with books to read during the war

8:56:07 Hepburn was a chemist; I was not allowed to do biology at school as that was for farmers; no good at Latin but did chemistry, physics and maths; other hobbies included fishing; collected butterflies; another master who was good to me was Capt. Collier M.C. who had retired from the Indian Army and was a butterfly collector; from Oundle got a great deal of encouragement from the staff; made few friends and was bullied a bit by the boys

11:03:00 Attempted a scholarship to Emmanuel College, Cambridge, and did reasonably well in chemistry and physics but failed on maths; after I had been in the Air Force for about six months the Headmaster arranged a closed exhibition to St John's; had signed on for air crew at seventeen and a quarter but not called up until the end of 1941; stayed on at Oundle and then worked for the Young Men's Christian Association, driving a van taking tea round to the anti-aircraft sites in Norfolk; also worked with James Fisher who was doing rat control research in the Port of London; while working with him remember going to the telephone in London Zoo and being told that my brother was missing; he was the eldest in the family and an RAMC doctor and his troop ship was torpedoed; parent had no news of him for several months and eventually found a letter in a Liverpool newspaper listing the survivors; father got in touch with one of them and learned that my brother had died a very horrible slow death of wounds and exposure in an open boat in the Atlantic; effect on my parents was exacerbated a couple of years later when they heard that I was missing; the effect of bereavement is not often talked about in the context of war; in my childhood I lived next door to a family with children about the same age and were very close; Graham, my immediate contemporary also died in the war, shot down in 1944, and this influenced own later interest in war

16:24:24 Later interest in religion was not influenced by work with YMCA at that time; called up and sent to Southern Rhodesia to train as a pilot; went on to flying training school and flew tiger moths; group of us were selected for Coastal Command and sent down to George in South Africa to train as a navigator; came home on a troop ship via South America; took months and months despite being on a fairly fast troop ship without an escort, having to keep watch for submarines; I was then a mild sceptical Christian

but the man I was on lookout duty with was a passionate atheist; talked for weeks and weeks and when we got to England, he was a Christian and I was an agnostic; eventually joined a Catalina crew, flying boats, and posted to a squadron operating in the Indian Ocean from Ceylon with an outstation in the Maldive Islands; we were looking for the Japanese fleet and submarines; meant very long trips over the sea of eighteen hours or so; not especially dangerous; glad to say I was never involved in killing anybody; the dangers were either running out of fuel, bad weather, or while in England, flying into a mountain in bad weather; one crew did find the Japanese fleet and just lasted long enough to get the radio message out; came back to England and trained as a Sunderland pilot; did a few operational trips but then the war was over; because I had an exhibition at St John's I persuaded the Air Force to give me early release and came up to Cambridge in January 1946

21:49:08 Did a lot of bird watching and the first academic lecture I gave was to the Oxford Bird Club on the birds of Southern Rhodesia; I had started to take an external London degree by correspondence in zoology and subsidiary chemistry and I took and passed the latter so I only had zoology to take; eventually finished it immediately after my Cambridge degree and got a class lower in London than I got at Cambridge; thought I would get an extra vote but they had abolished university seats in Parliament; at Cambridge I read Natural Sciences, initially physiology, chemistry and zoology, then part 2 zoology; I got a 2:1 in my first year then a first in the two following years; spent a great deal of time on the Cambridge sewage farm watching migrant birds; organized watch at different sewage farms all over the country to see how migrant waders behaved; during that time fortunate to come across a bird on the Cambridge sewage farm that had never bred in Britain before, or so was believed, and that was the moustached warbler; the record was accepted by the British Ornithologists Union committee until last year when it was questioned and has been removed from the British list; was important to me as it got me my first job; talking of Cambridge, I did make a number of friends among the ex-servicemen; what was extraordinary was that we never talked to each other about our war experiences; the man with whom I shared a room had shot down three Junkers 88 in fifty minutes and was a D.F.C. and an Air Force Cross which he got

after the war for flying planes through the sound barrier; another chap had no legs and I have a vivid memory of us all going to the pub and his not being able to keep up and calling him peg legs, but never knew how he lost them; I have reflected on this more recently; why? partly not wanting to shoot a line, partly guilt at being a survivor or having killed

27:25:17 I had two supervisors in St John's in zoology, Frank Hollick who was very shy, and each would sit on the edge of our chairs in his room and neither of us say a word for what seemed ten minutes at a time; he was very interested in the aesthetics of animals but not a very good teacher; the other was Colin Bertram who had been in charge of fish cultivation in Cyprus during the war to feed the troops with carp and about that time became secretary of the British Genetical Society and was always talking about people being from good breeding stock; lovely man, Palmer, taught me chemistry; was incredibly old; Benians was the Master at that time, a classicist; Jack Goody was there but we didn't know each other then

30:04:06 David Lack, the ornithologist, had been to see the moustached warbler and offered me a job as a research assistant in Oxford with the possibility of doing a D.Phil.; he wanted me to do a comparative study of the feeding behaviour of rooks and jackdaws; struck me as boring, it was in relation to Gause's hypothesis that no two species with the same ecology could exist together at the same time; David was moving from robins to studying the ecology of great tits at that time; suggested instead doing a behavioural study of the great tit and that is what I did; lovely time, working in Wytham Wood just outside Oxford; I wandered round with a notebook and field glasses writing down what I saw; so happens that my daughter is now a research fellow at Newnham and is also studying great tits but she has incredibly complicated apparatus; what has been nice for me has been to work as her unpaid assistant counting eggs in nest boxes; Lack was not a good supervisor; he corrected the English but wasn't especially interested in what I was doing; lucky in Oxford because Niko Tinbergen came to Oxford when I was a D.Phil. student and as he had no students of his own I had his undivided attention for a

while; he was the person who really influenced me; he was a very charismatic man who did everything himself

34:02:13 Lorenz, an Austrian, did some very interesting work in 1930's and in a sense got ethology going; Tinbergen was working independently in Holland and got into contact with Lorenz shortly before the war; Lorenz was taken prisoner of war by the Russians; Tinbergen was taken into a hostage camp by the Germans and had a rough time; I was at the first conference where they met after the war and they tried to have a good relationship with each other, but Niko said to me that the thing he couldn't bear was hearing German voices in the corridor outside his bedroom; although they were both passionately interested in animal behaviour, Lorenz was an ideas man, Tinbergen was an experimenter; there were really two ethologies, Lorenzian and Tinbergian; I was very much influenced by Niko;; Lorenz was very kind to me until I wrote a paper criticising his model of motivation and then he used to speak of Niko and me as the English speaking ethologists as though we were a separate race

36:41:05 At Oxford I was newly married and didn't have much time for college life although I was a member of Balliol; did a D.Phil. in eighteen months; another piece of luck, Bill Thorpe of Jesus College, Cambridge, was wanting to start an ornithological field station to study the relation between instinct and learning and he asked Lorenz to come and be the curator but he had another job; he then asked Reg Moreau who also turned it down, and then he asked me; I came in 1950 to Madingley and worked there until I retired; we spent the first year putting up a perimeter fence and aviaries; we had no building except for a small Nissan hut which had been left by the Home Guard which we used as a food store; the University had just acquired the whole of the Madingley estate and that is why Thorpe had been able to establish it there; I did more work on great tits and finches for the first few years; then did a little work on imprinting; Thorpe was interested in how birds learnt to build nests or whether they did so instinctively; a problem still not fully solved; I got slightly diverted into studying the endocrinological basis of nest building because I wanted to have canaries that would breed all the year round; we could show the interaction between changes in the external world, like daylight, the

endocrine changes in the bird, changes in the behaviour of the bird which produced new stimuli to which it responded, which produced new endocrine changes in the bird; I was working at the same time as Danny Lehrman who was an important influence on comparative psychology and ethology was working in Rutgers in US and he became one of my closest friends; he was a very overweight New York Jew, very clever, very verbal, and his work on that topic has been carried on in the Institute of Animal Behaviour in Rutgers to this day; at that time we used to see each other about once a year; remember once arriving at five in the morning and Danny keen to go bird watching and by ten o'clock we'd seen fifty-one species

42:15:19 I was extremely fortunate that it was so easy to get money then and I had one grant application turned down because I had not asked for enough money; was in contact with a Colonel in the United States Air Force based in Brussels and he came to see the work that I was doing with canaries; as a result he wrote a grant application for me to the American Air Force and I got support from them for three years; he did a follow up application on sensory deprivation in canaries; he was a cardiac physiologist whose aim in life had been to see what he could put across the generals in Washington and I was his second best; the best had been a study of Indian fakirs on the grounds that they could help American airmen if they came down in the sea

45:38:50 During this time I was existing for several years without a proper job, then I was made a senior assistant in research and eventually made assistant director of research, but I never had a proper lectureship; applied for a demonstratorship in psychology and didn't get it which was all to the good as it would have involved a lot of teaching; much freer to do research of the sort that I wanted to do; had been working on imprinting in parent-offspring relationship in birds; this came to the ears of John Bowlby who was a psycho-analyst; at that time he was very concerned with the fact that parents were not allowed to visit their children in hospital except in visiting hours; felt this was bad because before the war he'd worked a lot with adolescents who had got into trouble and found that nearly all of them had had a separation experience from their parents; this was only clinical evidence and he wanted

experimental evidence to try and confirm his thesis; I used to go to seminars at the Tavistock Clinic in London which he ran; we had nothing theoretically in common but an interest in the parent-offspring relationship; taught me an important lesson that it is not the theory but the problem; after a year or two John helped me get money to set up a rhesus monkey colony in Cambridge; we had six groups of rhesus monkeys each with a male and three or four females and their young; we spent a lot of time working out methods of recording behaviour which were subsequently used by primatologists in the field; we were able to show in the end that ten days separation could produce effects that we could pick up two years later in their inability to cope with stressful situations; this research took about ten years and we were able to contrast what happens with situations comparable with mother goes to hospital-infant stays at home, infant goes to hospital-mother stays at home, both go to hospitals together, both go to different hospitals; symptoms were exactly the same in monkeys as in young children but the details differed because of the different social structure of monkeys; made me realize both the value of comparative studies and their great danger; in humans the child is less affected if mother goes to hospital and he stays at home in a familiar environment; in monkeys it was the other way round because when the mother came back she had to re-establish her relationships with all the other group members and didn't have any time for a demanding infant; the social factor made the difference; however it did help Bowlby to get the hospital regulations changed; my second wife works in attachment theory still; have not personally looked at bringing up children in different cultures; Mary Ainsworth's work on the Buganda; [Macfarlane comment on Western child rearing compared with Japanese]

54:37:18 During that time offered a Royal Society Research Professorship which has allowed me to follow my research interests wherever they have led me; got it in 1963; Louis Leakey had come to the conclusion that the secret of man's origins lay with the great apes and that women were better at studying them than men; he had found Jane Goodall and wanted her to get a Ph.D. and wanted someone to supervise her; Professor Hall at Bristol was the only other person working with monkeys in the United Kingdom and he had died from a monkey bite; I got the opportunity to supervise

her and later Dian Fossey on gorillas; spent time in their camps getting all the excitement and pleasure of their research on chimps and gorillas without having to do any of the hard work; subsequently advised many students who worked in their camps; although I never did any fieldwork on non-human primates I have had a considerable number of students, some of whom became distinguished, who worked on their sites; memories of supervising Jane Goodall and Dian Fossey, neither of whom liked writing academic papers; they taught me that animals are individuals and not just members of a species

Second part

0:09:07 Memories of Dian Fossey; tracking gorillas; Fossey's phobia of heights; this period of my life was a wonderful time at Madingley with researchers coming in and out all the time, passionately interested in animals and in their research, being able to communicate with each other; Pat Bateson was almost my first student; this period began in 1968 I was 45 and continued for the next ten to fifteen years; personally happy too as I had remarried

7:37:23 I have always wanted to make the world a better place; when first a fellow of St John's there were five fellows, all historians, who teased me about this; decided it would be better to study children than monkeys; recruited Judy Dunn to the unit and she helped me to get interested in child development; I was interested in mother-child relations but we didn't agree on attachment theory; I started a study with my second wife, Joan Stevenson-Hinde, on four year olds and their mothers; I worked in nursery schools with the four year olds and she worked in the homes; became in effect part of the nursery school; Joan got interested in shyness in children and has gone on to do remarkable work; I tried too hard to use the recording techniques which had done so well with monkeys and they weren't really applicable for children, thus not especially fruitful years

12:12:05 Had thought that if we understood how people behaved then could help to make the world a better place; [Macfarlane on own work on children in different cultures]; during that period started the Cambridge Association against Nuclear Warfare; Pat

Bateson was partly involved; then got involved in ex-servicemen's C.N.D. to alter the perception that C.N.D. was just peopled by long-haired hippies; from that graduated into Pugwash, an organization that grew out of the Russell-Einstein manifesto in 1955 against nuclear weapons; it gave rise to conferences on science and world affairs, the first meeting of which was held in 1957 and involved scientists from both sides of the iron curtain; has had many subsequent meetings and workshops, mostly on nuclear weapons but on other subjects too; the movement got the Nobel Peace prize with Sir Joseph Rotblat in 1995; when I joined in the 1980's it worked primarily at the political level, trying to get the right scientific insights across to politicians; politicians have listened because of its impeccable scientific integrity; this has taken up a lot of my time in retirement

19:31:12 In 1989 I was elected Master of St John's which cut down research for five years; I really enjoyed being Master because at the centre of things; spent a lot of time raising money for a new undergraduate library; did about sixty meetings all over the world; although I resented the time spent, these meetings could be fun because the people who came were the loyal Johnians; their questions reflected the nostalgia they felt for the college; successful in raising the money; debates on whether the library should be open all night and later noted students working there at 4am; John's is a lovely college and very cooperative at that time; did write some papers at that time but began to feel itchy for research; did lecture a little each year and from the mid-nineties have been lecturing on personal relationships to SPS; gave that up when I was eighty as I thought it was improper to be lecturing on love to young students; Joan and I are still involved in a course of lectures in psychology; quite enjoy lecturing; in the 1950's when I was very insecure without a proper job, St John's made me steward and I was in charge of food in the hall and the undergraduates' wine cellar; they did not realize I had no interest in either; two or three years later they made me a tutor instead which I enjoyed as I had contact with undergraduates; gave it up when I got the Royal Society research professorship in 1963

27:51:22 Reflections on the causes of war; two things essential with institutionalised war, supply of weapons and young people willing to carry them; my view is that although aggressiveness plays a part, the real issue is duty; [further reflections on war including Macfarlane's]

34:19:12 Interest in religion stemmed from my upbringing; as mentioned before I became an agnostic on the wartime voyage but put off further thought on it until retirement; when I was Master I wrote a book on relationships and another on war, and also wrote a book called 'Why Gods Persist' which is trying to use all the biology, psychology and social science that I have learnt to bear on the question why people believe in gods and why religion is so powerful; not keen on Richard Dawkins denigration of religion as a lot of people get a lot out of it; he treats religion as a unitary entity whereas following Malinowski I would want to divide it into structural beliefs, narratives, ritual, moral code, social aspects and religious experience; each of these are mutually supportive but can be to the advantage of individuals, although less clear about religious experience; language people use to describe it is very like describing an aesthetic experience except for the interpretation; don't get Christians who have had visions of Buddha; began to feel that morality is the most important thing in the world today; wrote a book 'Why Good is Good' trying to show that what is basic in society is something like do as you would be done by; that is compatible with the importance of exchange and how moral precepts are passed on by interaction between what people do and what people are supposed to do; why are people nice? Recent thought is because early humans lived in competing groups that had to be cooperative with each other but not with another group; groups devised their own precepts and the ten commandments are not universally applicable; passed down through a dialectic about what people do and what they are supposed to do; in my lifetime that dialectic has led to divorce becoming more acceptable

42:55:00 Have just finished another book which goes a bit further and is trying to use an objective approach about how morality actually works in the real world; we have rules but in nearly every case we have a means of excusing yourself from them; in many institutions in our society behaviour that is incompatible is either

condoned or encouraged; in business ethics you no longer do as you would be done by but do the best for yourself; this is justified by the economists saying it is good for the consumer; in war soldiers are encouraged to kill; politicians by political necessity have to lie; in law you can't have a legal system without barristers who defend people whom they may suspect are guilty

45:44:23 Memory of J.B.S. Haldane at a disastrous early lecture I gave at University College; John Maynard Smith and G.P. Wells let a bat go in the auditorium and it flitted in and out of the projector beam and the audience was in hysterics; Haldane kindly took me off for a drink afterwards; brief reference to incest avoidance

Postscript 20th November 2007

0:09:07 Luck at being an early entrant to subject of ethology; it insisted you always start with description, that you must study the behaviour of animals in their natural environment, that you must ask four questions: causal, developmental, functional, evolutionary (demonstrated with thumb); this came into collision with a group in the American Natural History Museum led by Schneirla who had a pupil, Daniel Lehrman, who wrote a vicious attack on ethology; we tended to picture Danny Lehrman as an ogre until he came across to Europe and talked with us; turned out to be an absolutely charming man; eventually led to a rapprochement between comparative psychology studied in the US and ethology as studied here; regret that I paid too little attention to the functional question which has become the centre of behaviour studies in recent years largely due to the work of W.D. Hamilton; very exciting to study the courtship of finches, for example, and argue from that how the displays evolved and what the relations between the different species were

4:27:10 Then moving into mother-child relations was a very exciting time as I felt I was doing something that might make a real difference in the world; also gave me a chance to get into the primate fieldwork; I was the only person who knew about non-human primates in this country at the time; role in teaching Jane Goodall was how to make precise recordings in the field which I had learnt by studying captive monkeys; then got involved in pre-

school behaviour reflecting work of John Bowlby; led on to studying human relationships; at that time no science of human relationships only of human interactions; now a flourishing branch of psychology; tried to set up a scientific framework based on observation and analysis for studying all the complexities of human relations; thoughts on Desmond Morris; after this worked on religion and morality

9:13:04 I have never had to take any decisions in my life; had the choice of being a B.O.A.C. pilot or an undergraduate but fear of tutor [dislike of living out of suitcases RH] decided the latter; failed to get a demonstratorship in psychology; only other decision I have had to take was whether to let my name go forward as biological secretary of the Royal Society; decided against getting involved in administration to allow me to continue with research; wonderful to have had a Royal Society Professorship as it gave room to follow my interests; also lucky with my mentors like David Lack, Niko Tinbergen, Bill Thorpe; in the 1950's met Gabriel Horn and we edited a book together called 'Short Term Changes in Neural Activity and Behaviour'; think we met through a seminar run by Thorpe and Zangwill, Professor of Psychology, which was an effort to break disciplinary boundaries; Gabriel and I ran a conference and one of the Dutch participants gave us a bottle of geneva which we drank as we edited the manuscripts; Gabriel, Danny Lehrman and Jay Rosenblatt, also from the American Museum of Natural History, have been my three closest male friends; lucky in my second marriage to Joan Stevenson as she is also a psychologist and brought a new perspective to my work; Frank Beach, Professor of Psychology at Newhaven, wonderful man and very kind to me; Ernst Mayr, the biologist, with whom I did a paper; Danny Lehrman and Jay Rosenblatt, the latter became a psychoanalyst who continued also as an animal behaviourist

16:42:05 Have had some wonderful students; started with Pat Bateson who took up the study of imprinting; for some time he was director of the sub-department of animal behaviour at Madingley and I was director of the Medical Research Council Unit for the Development and Integration of Behaviour within that sub-department; Jane Goodall and Dian Fossey, two very remarkable women; Peter Marler and John Crook were both students of Bill

Thorpes at Madingley though I supervised Peter Marler for a while; John Crook underrated as he really made a breakthrough in behavioural ecology through a comparative study of weaver birds showing the relation between their life history and their nesting habits; he went to Bristol as a lecturer and then got more interested in Zen; later students include Richard Wrangham, now professor at Harvard; Dorothy and Robert Seyfarth, now at Pennsylvania; Sandy Harcourt and Kelly Stewart who studied gorilla, in Dian Fossey's camp

20:47:11 A question I sometimes ask myself is do I have any regrets about my academic career; I spent a lot of time doing things that were tremendous fun but were they really doing any good to the human race; studying canary and finch courtship don't see that the human race is much better for it though I sometimes kid myself that any advance in science is worthwhile; it is for that reason that I have turned more and more in the last thirty years towards studies of aggression and war and how to stop it, and religion and ethics; I sometimes kid myself that it is some use; I am very lucky to have got involved in Pugwash, an organization primarily of scientists which by maintaining an impeccable reputation is able to influence governments; that sort of meets everything I want to do

22:28:07 Advice to student to always do something you passionately want to find out about; no good being set a particular problem; own experience with David Lack wanting me to work on the comparative behaviour of corvids but allowing me to follow my own interest instead; have always tried to do the same with students although Pat Bateson was a bit of an exception in following my work on imprinting; there is nothing more dreary than the mechanics of research; I spent five or six winters on my knees in a cold , draughty wooden bird room recording how often a chaffinch chinked when I put an owl in front of it; did try to give fieldworkers good preparation before they went out to the field and most practised recording techniques with our captive monkeys

Barry Keverne

23 March 2009

http://downloads.sms.cam.ac.uk/1124156/1124163.mp4

From Departmental Website – 22 August 2014

Professor Keverne has long standing experience in behavioural neuroscience and has, in the past 10 years, brought molecular genetic techniques to focus on brain development and investigate how genetic perturbations of the brain influence brain function. In particular he has employed androgenetic and parthenogenetic chimeras to understand how the imprinted genome influences brain development and has extensively investigated the adult phenotype of mice carrying a mutation in paternally expressed genes. These studies have led to a co-adaptive evolutionary theory of brain and placental development through genomic imprinting. Pheromonal influences on behaviour and endocrine responses in mice is also a long standing interest and in recent years, together with Piers Emson, he has investigated pheromonal signalling via Erk and Akt phosphorylation to enhance vomeronasal neural regeneration survival.

INTERVIEW SUMMARY

Barry Keverne interviewed by Alan Macfarlane 23rd March 2009

0:09:07 Born in Walmsworth, Yorkshire, in 1942; the only industry in the area was coal-mining and my father was a miner; I was the first male in four generations of my family not to work in the mines; my father was injured in a fall in the mine; with his compensation he bought a small hotel business which went well and expanded; as my father was at war in 1942, I was brought up in a house full of women - mother, grandmother and great-grandmother; remember my great-grandmother as a matriarch whom everybody listened to; she was extremely generous to me; I was born prematurely and weighed just over 4lbs; there were no incubators and the nurses told my grandmother that I had no chance of survival but she brought me through; still remember my great-grandmother's Co-op number as I used to buy groceries for her, even after we had moved away; I went to the junior school that had only two classes; it had no lavatories, just a hole in a field at the bottom of the playground; remember having to queue up every morning to get a spoonful of cod-liver oil followed by a spoonful of orange juice; I did not seem to have any traumas in my childhood and can't remember ever being unhappy; it was a bit tough for me when my father came back from the War as I had been so spoilt and he introduced some discipline

4:52:07 My parents were working-class but very pleased to have made it into being independent, but that was by the time I went to university; they were not openly loving and rather ashamed of showing emotion, but my great-grandmother was always loving and kind; I was allowed to grow up in my own way as both parents were working; even at junior school I would come home and get myself something to eat; I have a younger brother who went into the family business; around my home there was a lot of countryside; the River Don was nearby though it was very polluted; when at junior school I used to collect stamps, and was interested in sport, sprinting in particular; there were no teachers that influenced me then; the classes were huge - between 50-60 children; I did my 11+ there and out of the two classes that took the examination, only

nine went to grammar school; I passed for Doncaster Grammar School which was in the school catchment area, but because we lived outside that area I went to Mexborough Grammar School; it was a massive co-educational school with over a thousand students and five streams in each year; I got into the top stream but never came first in the class as it was always girls who came top; the highest I ever came was third; I sailed through school without problems; I was caned twice, once for whistling in the corridor and once for eating in the street; discipline was strict but I never thought anything of getting the cane; there were teachers that I liked very much, particularly the biology teacher; found chemistry pretty dull but we all had to do it, and physics; we were taken on biology field courses which I enjoyed very much; I also played chess, both for the school and then for the county; when I went to university I played for it; with seventy others we played one of the Russian grand masters; I lost, but nobody beat him; I realized at that point that I was not as good at chess as I had thought, and from then on only played for pleasure; at university I took up mountaineering and climbing; used to go quite often to Harrison's Rocks in Tunbridge Wells, to Wales and the Lake District; we used to go to Scotland every winter to go walking and mountaineering; one year we rescued the S.A.S., four of whom were out on a mission; after university I took a year out and went to Africa, to the Mountains of the Moon, to do mountaineering and a biological project; I went by boat via the Suez Canal; when we arrived we were told not to go up there as there was war in the Congo and guerrilla forces were coming up from there; warned that they would shoot us for our supplies; we therefore transferred our project to Mount Kenya as it has the same kind of Afro-alpine zone; once we ran out of food except for peas as the porters bringing it up failed to arrive; there were four of us working there and we have remained close friends ever since; the biology was asking questions about both fauna and flora because of the high fluctuations in daylight temperature; we were interested in how any flora could possibly survive and why the plants grew to such a gigantic size; what we found was that at night the leaves closed up and that the core temperature didn't go below zero; we found it was a continual dying process which made the plants grow, as the leaves on the outside die at night, then during the day the metabolic process gives growth to the rest of the plant; it got me interested in thinking about biological research

19:34:22 At school I was in the school play and also played the violin; I started the latter at junior school where my music master thought I was destined to become a famous musician; when I went grammar school I did not reveal that I played the violin because I realized that to be good you had to put a lot of time into it; I was trying to do male-like things and excel at them; I have retained an interest in music and now my favourite is Bach; I do like Gregorian Chant, Mahler, and a whole range of composers both old and new; it is something I do to relax to rather than as an intellectual pursuit; at university I did spend a lot of time getting to understand music; on religion - I was never confirmed; my parents were not overtly religious although they believed in God; I was not brought up in any religious way; I found out after my mother had died that her father, who lived into his nineties and was a First World War hero, fell out with his father who was a Rabbi, and married a non-Jew; he also signed up for war without his father's agreement so was literally cast out of the family; I guess my mother was not brought up in any religious way, nor was my father; I am not of the Dawkins' persuasion as I think he is just as evangelical as the evangelicals; I believe in beliefs, and that they are enormously important; I would never deny anyone something they believed in that gave them a sense of comfort or wellbeing; my beliefs are very much in humanity and the future of mankind, trying to ensure that we keep our environment as habitable and ecologically normal as possible; I don't think, like Dawkins, that Darwin has disproved religion; Darwin was a religious man, and it was more the death of his daughter that made him disaffected; his wife certainly remained very religious; I think that if he had been really anti-religion he would never have got on with his wife and family; he, too, was tolerant of religions even though latterly he may not have believed; I do not believe in special creation and do believe in evolution, and that it is infinitely more complex than we might imagine; in the last few years most of my thinking time has been thinking about evolution; one of the things that particularly interests me is how human beings have evolved into something quite different from other creatures, particularly in our brain development

28:48:24 At school I was in the sixth form science stream though I had been in the arts stream earlier; in consequence I had to do

both 'O' and 'A' level chemistry in two years; the school was bent on my going to university although I was the first person in my family to go; I have cousins who have also gone but they were younger; I went to London University, to Queen Elizabeth College which was part of King's College; I thought it absolutely fantastic; being in London and being independent meant I did not put as much effort into my academic work as my social life, and involvement in university societies; this was 1961-64; I then took a year out; when I came back I decided that I was really interested in research; I had read physiology; in those days, the person I admired was J.Z. Young; thought he was a wonderful biologist with a very evolutionary approach which stimulated me to think in that direction; because I was interested in brain and behaviour I did my Ph.D. at the Institute of Psychiatry, at the Maudsley; I was interested in the way in which hormones influence behaviour; they have a powerful effect in rodents and mammals but nobody had really looked at their influence in large-brained mammals, and I looked at monkeys; I was very interested in what motivated females in terms of their sexual activity; the problem was that the work done in the field was incomplete and any work done in the Lab had suggested that monkeys mate at all times; because the male is so much bigger they tend to dominate, particularly in captivity; I emancipated a female from a male by having a partition across their cage with a door that moved when the female pressed a lever; got it to the point where the female had to press five hundred times to open the door; the door was too small for the male to get through and on the other side there was a lever she could press when she wanted to leave; the outcome was that the female pressed the lever to get to the male every day of a cycle; she did seem more interested to get to the male around mid-cycle when she ovulated; I was also interested in the male's interest in her and all of the evidence suggested that the sex skin colour was all-important; found that one could artificially create the colour by use of oestrogen cream, but the male was then not interested, but it was the smell of the female that was important, the pheromones; did a converse experiment with males seeking females, using females that were ovariectomised and unreceptive to males, but changed their attractiveness by changing their odour; males would pursue the ones that smelt right; looking at the hormones and seeing how they are impacting on the brain behaviour and on the periphery on the

behaviour, generally speaking hormones are synchronizing both things together throughout most of biology; I then started to look at what happens when you do this in a social group; if you make a female attractive with artificial pheromones in a social group it had no impact whatsoever; then went to look at social hierarchy and its effects on behaviour; found that an animals status was much more important than the hormones, even if the male was castrated; conversely, if the male lost rank socially, sexual behaviour stopped immediately; rank was correlated with male hormone levels, testosterone, but if a male lost rank the testosterone would go down; taking a low-ranking male and filling it with testosterone had no impact on its behaviour; after spending five years working on how hormones have an impact on behaviour, I suddenly realized you had to get these into the context of the social situation; social hierarchy is most important in both male and female; low-ranking females were less likely to ovulate, even when challenging the brain with hormones which normally would bring it about; thus a there was social suppression of reproduction in low-ranking individuals

40:28:07 That work was supervised originally in London by Richard Michael; there were people in the States who were interested in the work I was doing and I applied for an American fellowship; I did not get it so remained with Michael; I had six or seven papers in Nature and Science in the course of three years in the late 1960s, early 1970s; about a year later, Richard Michael invited me to go to the States as he was moving there; he offered me a tenure-track senior post-doc research position; I was keen to move although he had been very helpful to me; I came up to Cambridge to see Joe Herbert, one of his former students, and he invited me to come to Cambridge; I brought my money to Cambridge, then I got a research fellowship for research in psychiatry; I got a lectureship within two or three years in the Department of Anatomy; I then had to learn anatomy, how to dissect the whole body, and I learnt a lot about the brain; teaching medics in those days meant they spent a whole year on the brain; it was a complete new discipline but I love being on a learning curve, and still am, so had no problems with teaching and supervising; the brain is challenging and you can't treat it in isolation; you have to know how it fits in with the body, and responds to bodily needs and tells it what to do; I got my lectureship purely on the basis of

my publications; the Professor of Anatomy was Richard Harrison who was really a zoologist and was interested in dolphins; his attitude was that anybody could learn anatomy; I gave lectures on the sympathetic nervous system, the brain, but also on topographical anatomy; I came here in 1972; Harrison was relaxed but his second in command was Max Bull, a died in the wool anatomist at Queens; he set up teaching experience for me with physiotherapist who used to come once a week from Bedford to learn anatomy, before I progressed to teaching medical students; I do enjoy teaching; long after I became a professor I supervised all the second-year medics until the College appointed a medic to a fellowship here, part of which meant he had to teach; he was a neurologist so taught the brain, which was what I had been teaching; I do not supervise PhDs any more as this is officially my retirement year; I still have grants so will be continuing with my research

49:53:22 Because I had not been an undergraduate here I did not get a fellowship, but I did teach for King's in 1973-5; Charlie Loke was very much in favour of someone having a medical qualification and the College appointed a pharmacologist, so I didn't get a fellowship here; I became sub-director of studies for medical sciences at Sidney Sussex and I taught at Newnham; after the pharmacologist King's appointed Matt Kauffman as a College lecturer in anatomy; after three years he went off to a Chair in Edinburgh; by this stage Charlie was a bid disenchanted with people who were not around very long and he asked me if I was still interested, so it was in 1985 that I became a fellow at King's; Joe Herbert was the antithesis of Richard Michael, completely informal and totally disorganised, but a brilliant mind; what he taught me to do was really to think about things until it hurts; he always acted as devil's advocate; he did not pull his punches at lab meetings, the kindest of people at a personal level, but rigorous intellectually, and I learned a lot from him; I also learned a huge amount from Gabriel Horn; he has real leadership qualities, and is absolutely brilliant at motivating and encouraging people; I was asked to take over from Pat Bateson; going back, I did overlap for a few years with Gabriel in Anatomy; I was the first non-medic to be appointed to the department apart from Martin Johnson, and the other medics were not exactly warm and welcoming - Gabriel

not like that at all but was keen on everybody doing research; I was the first person to introduce use of radioactivity into the department and this excited him; he made me feel important whereas nobody else in the department ever spoke to me; Harrison, who had been very welcoming, was busy being head of department but was otherwise detached from it

Second Part

0:09:07 Robert Hinde has been very encouraging; our research overlapped a little as he has been interested in mother-infant and family relationships, which has interested me at a mechanistic level in terms of how the brain works to regulate that behaviour, and the importance of hormones in maternal behaviour; I am talking about animals here, because what is interesting about all mammals is that you have two generations developing in one individual - the infant and placenta, which is also foetal, that actually communicates with the mother to extract resources, but also to tell the mother's brain what to do; it tells mother to eat more food in early pregnancy so it can be stored for the latter part when she can't eat enough to satisfy the foetus; it also primes the brain to be ready for maternal care; what is interesting about human behaviour is that women don't have to go through pregnancy to be perfectly good mothers - it may help, but it is not necessary; in all other mammals, except large brained animals like primates, it is deterministic; primates learn maternal care; one thing about the human brain in particular is that it continues to develop until late puberty, from seventeen up to twenty-one; most of the brain development occurs postnatally, and for a large brain this requires careful development in an appropriate environment; another thing that interested me about maternal care are the neural mechanisms, in particular the way the hormones act on the brain to prime neuropeptides, in particular, oyxtocin, for maternal care; what is interesting in what is acting relatively mechanistically in small-brained mammals, is also the same mechanism that is acting in humans and monkeys in terms of their social interaction; I first worked on maternal interactions with a relatively small-brained mammal like sheep, where you get the mother bonding with her lamb; secondly, how living socially with a large brain, most of it developing postnatally, what changes have had to occur in brain evolution to encompass that; it is interesting

that the basic mechanism, in sheep for instance, are very much driven by olfactory cues; when you get to high primates and humans, olfactory mechanisms play much less of a role, but the brain's reward mechanism kicks in; there is the same basic structure in terms of the hypothalamic mechanisms and the oxytocins and receptors and the brain's reward mechanisms, but what has happened as the brain has got bigger, the reward system now responds to a lot of input from other parts of the brain, and is not just regulated by these neuroendocrine mechanisms; the brain has got larger to such an extent that it is becoming self-regulatory, although in regulating itself it has to go through a very long social learning process; we kind of think of everything we do as being straight-forward and natural but unless you have watched a mother interacting with a young child, as I see with my grandchildren, how much effort mothers put into their children - to get them to stand, feed themselves, walk - the amount of positive reinforcement and encouragement is just obsessional, but it works; you find that kids that have been neglected and brought up in a nursery don't learn to walk or talk; we think of it as a natural process but it is learned

8:11:11 From seventeen to twenty-one the brain reaches the age of reason, where you are less driven by your emotions as the brain can control them; it is the pre-frontal cortex that is maturing at this time; there is a bit of a worry for modern kids who are now coming into puberty in junior school as they are coming into reproductive maturity long before they have reached rational and emotional maturity which has not changed; much more insight has come from MRI scans; essentially what it is is a pruning and rewiring, it is not growth in the sense of new neurons forming; neurons are in fact dying and the biggest increase is in the white matter which is converting those neurons; what interests me is that biology is very conservative in that it has taken the mother-infant interaction which is deterministic, but that is the same mechanism which has expanded into social bonding, the same neural mechanisms have been called into play in terms of social bonding and social cohesion; it is also the same mechanism, incidentally, which is usurped by addiction - smoking, alcohol, drug abuse; what is sad about those drugs is that they also destroy the maternalism; heroin addicted mothers, usually within a year of birth, half of their children are taken into care, either by a relative or the State; by the

time the child reaches school age it is 90%; what I was interested in research-wise was trying to figure out how things are put together at a molecular-genetic level; that is an enormous task as there are so many genes that you could pick on; I was lucky in a way because I knew Azim Surani and he had just discovered genomic imprinting in the context of the placenta; I was interested in it in the context of the brain although there were no imprinted genes at that stage; what we did was to get the whole of the genome imprinted; imprinted genes are normal autosomal genes which are expressed according to parent of origin; there are some of your genes that you have inherited from mum and dad, but only you father's are expressed, and for other genes only your mother's are expressed; you can try and figure out what the whole of the imprinted genome is doing by making pathogenetic or androgenetic animals which you can do by putting the nucleus from an egg into an egg and you get a haploid organism of all the genes which have come from mum; you do the same by taking out the female nucleus and putting two male nuclei, but then it is lethal very early in development because of the placenta primarily; you can make chimeras where you can take cells and make them pathogenetic or androgenetic, and providing they don't exceed 40% of the total they will survive; that has a very big impact on both brain growth - size at birth - and whereabouts they are going to in the brain; since then I have been following through with imprinted genes and am going back to that maternalism, and how important the matrilines have been in this context; I am particularly interested now in how the brain and placenta have co-adaptively evolved; I am also still interested in olfaction as for small-brained mammals that is the most important sensory system they have, and how adaptive that might be according to the environment they are in; their olfactory and pheromone receptors are continually turning over, and are thought to be the only neurons in the brain which regenerate; the question is that if they are continually turning over, are they coming up with the same repertoire or is their turnover being selected in such a way that they are better able to respond to the environment in which they find themselves; both of these things are interesting, not so much from a genetic point of view, but from an epigenetic point of view; genes which are regulated and expressed as a result of environmental influences, that epigenetic process that makes them do that; evolution is in part dependent on environmental,

selective, things, but it may not be just a passive selection but much more active than we previously thought

15:28:06 The difference between science and chess is that your only opponent is yourself; it is an intellectual challenge where you don't always come up with the right answer but you are continuously challenging yourself; if you try and think about genes and behaviour, how do you know what time and part of the brain is crucially important, or which of the genes are the ones to try and get a handle on this very big problem; how to narrow things down without losing track of the big picture; that is what has really interested me; this has been helped by discussions with colleagues, Joe, Azim and Charlie; neither Azim nor Charlie know about the brain, but they have in-depth knowledge that I have had to generate myself, and the easiest way to generate knowledge about a completely new area is to talk to somebody who is interested; especially if it is a fellow scientist, you then get the social contact and enthusiasm, and sowing of seeds that make you want to know more; it is not just the thought that something is interesting and you go and read about it, but you need some direction and discussion to continue; Azim is the most modest person you could ever imagine, but has been extremely stimulating

19:11:00 The collegiate structure of King's (where both Azim Surani and Charlie Loke are fellows) has made these sorts of conversations easier; I would also say that supervising is also a wonderful stimulus because it forces you to keep broad; every year I used to find students would ask me questions in a way that I hadn't really put together in that way before; it is not that I didn't know the answers or the bits, but hadn't put them in quite that format before; never a year went by when I didn't at some point get some kind of stimulus; always rewarding to teach young minds, but also rewarding for yourself; in the college, Sidney Brenner is a person I have enormous admiration for; when I was admitted to my fellowship I sat next to him and came away feeling very small; I know him well enough now to know that he is not a very good listener, but with a few prime words you can get a monumental amount of knowledge, information and ideas from him; Sidney is just pure crystallized genius; he is the cleverest man I know and I am proud to have been at the same college with him; I took over

from Pat Bateson, but then he came here to become Provost; I came into Madingley to take it in a slightly different direction to Pat; since he stopped being Provost he has come back to Madingley and become much more involved with myself and post-graduate students, and we have been doing things together; Pat is now very interested in epigenetics because he likes that way of being able to engage the environment in terms of gene-environment interaction in developing the brain and behaviour

23:52:05 The human brain is different because of the long developmental process it undergoes postnatally; in terms of belief systems, we all need them in the sense that we understand what we mean by guilt, shame, or blame; you need an internal representation of self to be able to feel guilty, what you should be and what you are not being, so not matching up to the standards that you set for your internal representation of self; some people put God in that slot to set that standard; I don't feel you need any external reference point; I think it is an internal reference point, but you do need it; it is something that comes to you when you reach maturity, being able to value relationships and what you put into them, and all the things you do in terms of what you expect of yourself; all of the things that I have achieved, I feel that I have had a lot of influence from other people but I don't thank God for it; I think it has come about, partly through my own direction but especially through being in an environment like Cambridge; Cambridge is just such a wonderful place for anyone who wants to learn and be curious; you could live a thousand years and go off in all different disciplines and never come to an end of it; what you have got to be careful about is losing focus because there are so many things that are intellectually stimulating

27:17:21 A crucially important part of my life is my family; my wife is Spanish and I have three children who are now grown up; my two daughters have children; that has been my secure base for the way I have operated, always there, always welcoming, always understood me, allowed me to do things that might not generate a lot of income; I could not have done anything without it; I don't force my family to engage with what I do academically, though if they are interested that's great; I did not notice the mother-child interaction so much with my own children but then one is too

involved, emotional and close, and can't stand back and observe; it is much easier to do so with grandchildren; I was made a Fellow of the Royal Society in 1997, an Honorary Foreign Fellow of the American Academy of Arts and Sciences two years after, a Fellow of the Academy of Medical Sciences, and the Wiersma Professorship at Caltech, among other awards

Other possible volumes

Sciences

Biology, zoology and ethology: Patrick Bateson, Gabriel Horn, Robert Hinde, Michael Bate, Alison Richard, John Gurdon, Horace Barlow, Ken Edwards, Barry Keverne, Vittorio Luzzati, Azim Surani [2 volumes]
Physiology and medicine: Andrew Huxley, Richard Keynes, Yung Wai (Charlie) Loke
Chemistry and biochemistry: Sydney Brenner, Dan Brown, Hal Dixon, Aaron Klug, Frederick Sanger, John Sulston, John Meurig Thomas, John Walker, David King [2 volumes]
Astronomy and cosmology: Antony Hewish, Martin Rees, Neil Turok, Owen Gingrich, Edwin Salpeter
Physics and mathematics: Richard Friend, Dan McKenzie, Brian Pippard, John Polkinghorne, Herbert Huppert, Julian Hunt, Professor John Coates, Sir Peter Swinnerton-Dyer, Jeremy Sanders, Haroon Ahmed, John Simpson [2 volumes]
Computing and technology: Andy Hopper, Ken Moody, Jean Bacon, Hermann Hauser, Keith van Rijsbergen, Ben Shneiderman, Maurice Wilkes

Arts and humanities

Anthropology: currently there are 84 people whose interviews and/or lectures are up on the web. [probably about 10 volumes]
History: 19 historians on the web [probably about 4 volumes]
Sociology: Michael Banton, John Barnes, Andre Beteille, Ronald Dore, Ronald Frankenberg, Stuart Hall, Geoffrey Hawthorn, Michael Mann, David McLellan, Garry Runciman, Richard Sennett, M.N. Srinivas, Peter Worsley. [2 volumes]
Economists: Partha Dasgupta, Wynne Godley, Geoff Harcourt, James Mirrlees, Robert Rowthorn, Richard Smethurst
Literature: Peter Avery, Gillian Beer, Frank Kermode, Christopher Ricks, George Steiner, Toshi Takamiya
Explorers: Ursula Graham Bower, Owen Lattimore, David Snellgrove, John Cross

Musicians and artists: Stephen Cleobury, John Rutter, Antony Gormley, David Willcocks
Demographers: Luigi Cavalli-Sforza, Akira Hayami, James Lee, Osamu Saito, Richard Smith, Tony Wrigley
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