An interview with Horace Barlow

Conducted by Ann Silver, David Tolhurst and Martin Rosenberg on 21 May 2010

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The Physiological Society

Horace Barlow photographed by Martin Rosenberg.

This interview with Horace Barlow (HB) was conducted by Ann Silver (AS), David Tolhurst (DT) and Martin Rosenberg (MR) in the 'small meeting room' in the Cambridge Physiological Laboratory (the Department of Physiology, Development and Neuroscience) on 21 May 2010. This transcript has been edited by Horace Barlow (22 March 2015). A list of his papers is available.

Listen to the interview (1 h 43 min)

AS: First of all, Horace, thank you for agreeing to take part in [this]. And before we get onto your career in physiology, we'd like to hear a bit about your family and where you grew up, and your schooling, your early interests and particularly the Darwin connection. So, fire away …

HB: Well, I was born with a scientific silver spoon in my mouth, so to speak. Both my grandparents were scientists of their day. On my father's side it was Thomas Barlow, who was physician to Queen Victoria's household, and who was a genuine pioneer in applying real science to medicine. He was a well-known figure of that time. In fact, he even had a disease named after him. The name was used in Germany, but rarely in England.

MR: What was the disease?

HB: Das Barlow-Krankheit is scurvy rickets; that's rickets due to scurvy rather than vitamin D deficiency. And he differentiated between the two. He lived to a great age of 99¾ or something. But his son – my father, was not a scientist at all. He read Classics at Oxford and became a senior civil servant, and was a wise old bird who knew how to get round corners everywhere. But my mother was a Darwin – grandchild of Charles, and very keenly interested both in Charles Darwin himself and in Darwinism, all her life. She, like most females of that age, never went to university, but she did quite a lot of scientific work with William Bateson, who was the co-discoverer, or re-discoverer, of Mendel's work. And she was a very good botanist and knew the names of all the plants and tried to teach them to me, without any luck whatever, I'm afraid. But she did teach me about Darwinism, which was obviously always in her mind; she would often point out interesting little botanical details that had evolutionary significance, and I think I learnt a lot from her and that was my first stroke of luck. The other one was being sent to Winchester, which was a school where the scientific teaching was very good. And that meant that it had a lot of very good science students there, so I learnt there what a lot of people only learn when they come up to university, namely that they are really not quite top of the barrel, so to speak, in the things they think they're good at. Because I was in more or less the same vintage as Freeman Dyson and James Lighthill, and several others who far outshone me in my mathematical and physical abilities. I thought seriously about going into biology. But then the war came along, and I came up here two terms earlier than I would normally – as a medical student.

I first took the remaining 1st MBs which I hadn't taken, in Biology, and Organic Chemistry. I think the Organic Chemistry exam was the only one I ever failed. And the Biology one – I didn't learn much that I didn't know already, except I saw the insides of a dogfish, etc. So what more do you want in this line?

AS: We have just about got to the beginning of the war, and then what happened after that? I mean, you left school …

HB: Yes, I came up here, actually in January 1940, and studied for the remaining parts of the first MB, which I hadn't got. And then started the year in October 1940.

MR: So you weren't too young to have been involved in the war?

HB: No, well I was a medical student.

MR: Oh, that was exempt?

HB: Yes, and I did the shortened medical course here. But then I spent, just at the end of that year – I'd just finished Part 2 Physiology – I spent, I don't know, it was probably two or three weeks, perhaps a bit longer, in William Rushton's lab. He showed me how to get action potentials from nerves in the earthworm, and I asked him one day, 'Well, why do we have to do all this dissection? They are very big nerve fibres; can't we just record them from the whole worm?' And he said, 'That's a good idea; we'll see if we can do it.' And

we did it, and we published something in *Nature*, so that was my first publication.

DT: That was just as an undergraduate?

HB: Yes, well just finished the course, yes.

DT: Do you think you impressed the people here? Did they say, 'Wow, this is someone who will be meteoric?'

HB: I don't think enormously. They were used to the likes of Hodgkin and Huxley, don't forget.

MR: Who were your tutors at that time?

HB: Well, I didn't want to be supervised by William Rushton, because I thought his way of thinking was too much like mine, and that I'd learn more in things I didn't know much about from others. So I had – oh I did have one supervision, I remember, from Roughton, but then he went off to Harvard. And another from somebody called Parsons, I think it was, who did some Trinity supervisions.

AS: Dennis Parsons, I think.

HB: That sounds right. He smoked a pipe; all I remember about him. But then I went to Wilhem Feldberg, which was great. He did a lot to improve my English ('break up your sentences and use fewer unnecessary adjectives', he said). He also taught me about neuro-transmitters, which was a subject that I wasn't very good at.

DT: And he was here then?

HB: Yes, yes. And I don't think I ever actually had a supervision with William, but I spent many hours talking to him, of course, both in the Experimental classroom, and later.

AS: Can I just ask you one thing which I think ought to be recorded, if your answer is yes. You mention Roughton: now did he, at that stage, feed his handkerchief into his mouth and feed it out again?

HB: Yes, he did. I never saw him do it, but my brother Andrew did. He was reading engineering, and he wanted to change to medicine – so he had to go and see his Director of studies, Roughton. He did this, explained his problem, and there was then a very long silence. Andrew got a bit worried, and began to wonder if there was some kind of emergency he would have to deal with, but Roughton eventually raised his hand to the far side of his mouth, withdrew a large pocket handkerchief, and proceeded to direct Andrew's studies. When this was complete, Andrew was still a bit worried, intensely curious, and uncertain what etiquette demanded, or allowed, in such circumstances. He decided the direct approach was best, turned to him and said, 'Excuse me, but may I ask why you put your handkerchief in your mouth?' Roughton just said 'I find it helps me to think.'

MR: Comes out the same colour does it, as it goes in?

HB: I wasn't there.

DT: When you'd finished your degree here, your undergraduate degree, you then went to Harvard. Did you go directly to Harvard? Is that when you went to Mill Hill?

HB: No, I went to Mill Hill first. Well, it wasn't Mill Hill then, it was Mount Vernon. It then later became Mill Hill when the MRC built a grand new establishment.

DT: This was in the same part of London?

HB: It was in Hampstead actually. And it was their main laboratory and it had a lot of work going on. G. L. Brown had a unit there doing research on diving for the Naval Personnel Research Committee. It was a few months before D-Day actually, and they were concerned with harbour clearance and one-man submarines and that kind of thing. So it was improving diving gear. The chief problem that we were working on was oxygen poisoning, which is what happens if you breathe oxygen at more one or two atmospheres pressure, and particularly if you're in a diving suit; I don't know if anybody's yet discovered why it's so bad to be wearing a diving suit – but you get convulsions and things like that. And so they were doing experiments on that.

DT: Was this with big helmets and lead boots?

HB: Well no, they actually had a compression chamber. But we also, thought that some cases were misdiagnosed as oxygen poisoning and were actually caused by carbon dioxide accumulation because the diving gear they had – the shallow water diving gear they used – was just a submarine escape apparatus, which had a small canister for absorbing CO_2 and we reckoned that when people were working hard, they just produced more CO_2 than it could absorb.

MR: Who else was at Mill Hill then? Was Harold Lewis there?

HB: No.

MR: Who was the temperature regulation man?

HB: Ah, he was, interested in thyroid. What was his name? He was Charles Harrington, the boss of the place . But there were a lot of other interesting characters there too. There was a man who tested Winston Churchill's cigars. He said it was a very good job.

MR: Wasn't Heinz Wolff there then?

HB: No, he wasn't. I knew Heinz Wolff, he was a friend of my older brother's.

DT: How long were you there?

HB: I think it was just about a full year.

DT: And then you went to Harvard?

HB: Yes, this was a Rockefeller Medical Studentship. I think there were a total of about 20 of us; about six over three years or so went over there.

MR: What year would that have been?

HB: That would have been starting 1943. To 1944.

MR: Oh, it was during wartime.

HB: And that was interesting too, because you see, by that time I was quite seriously interested in physiology as a career, rather than medicine as a career.

MR: How did you get to Harvard in wartime?

HB: By boat.

MR: Quite tricky, wasn't it?

HB: Well it was. Quite a lot of people were torpedoed on the way. The mortality rate in that 20 of us was astonishing: I think two people were torpedoed; another two got TB. And several others stayed behind in the USA

AS: Going across, did you take a sort of a zigzagging course.

HB: Yes, but not in a convoy because it was on the Queen Mary, and the Queen Mary went fast, and they reckoned it would have to be a pretty nippy submarine in just the right place to be able to sink it.

MR: And what did your parents think about this?

HB: Well, I think they were quite glad to see me leave. England wasn't a very low-risk place to be at that time.

MR: And America had just gone in, was it '41 they joined?

HB: No, '42.

DT: Did you do any lab work, or see anybody's labs while you were there?

HB: Yes, that's the closest I got to ever actually being a doctor because they had a policy of giving 3rd year and 4th year medical students, temporary jobs looking after a ward. It was a terrifying experience, but it was quite instructive. But the other aspect of American medicine was that it was very much more science based than English ordinary practice was. I mean I think the research labs of England were every bit as good, but the actual ordinary [US] doctor always did tests almost before they'd done a physical examination or taken a history.

DT: And when you'd finished that, you then came back to Britain again, and the war had finished, and you had a post at, was it University College?

HB: … The Harvard degree didn't actually enable me to practice even in America, certainly not in England, so I had to do another, I think it was 18 months of clinical training here to get the appropriate places in the grey book signed.

AS: Where did you do that?

HB: At UCL.

DT: And that's where you worked with Walsh and Cone?

HB: No, that was actually when I was at Harvard Medical School. Henry Cone had heard a story about people carrying degaussing coils on their shoulder, which had current flowing through them, and they said the had seen flashes of light. So we decided to take that up and quickly discovered this had been discovered about five times before. But we assumed it was probably the induced currents that had stimulated the eye. It is very sensitive to electric currents.

DT: So you think it was the eye rather than the visual cortex, and you tried transmagnetic stimulation?

HB: Yes. That can be done, but you need very high fields …

DT: So while you were officially doing your medical studies, you get two papers in the *American Journal of Physiology*?

HB: Yes, that's right.

DT: First author.

MR: Alphabetical.

DT: Is it alphabetical? Or who knows …

HB: I think both Walsh and I were English physiologists and therefore agreed to alphabetical order, and Henry Cone, yes, I'm a bit surprised he agreed to that because he ought to have been first really, since it was his problem and we were just helping out.

DT: Did that – well that was two papers on different aspects of vision: did that make you think that you'd like to do visual science, or …

HB: Vision had been one of the things I'd been interested in, in the Part II course, so …

MR: Anything in particular that triggered you in the Part II course?

HB: Well, I gave a talk to what was called the Natural Sciences Club when I was an undergraduate, and I think I gave that – I think I gave two actually: one on colour vision and one on absolute thresholds. It's an undergraduate society and we took it in turns to give talks. So I was already quite keenly interested. And then I had the choice when I was qualified, either of entering the serious rat race in medicine, which involved house jobs, residencies, and so on and so forth, for the foreseeable future as far as I could make out, or doing, trying to do, science. And I thought I'd have a go at that first. So I didn't do any more medicine.

DT: And then you came back here and sought out Lord Adrian?

HB: Yes, with some difficulty. He was reckoned to be quite a difficult fish to catch, you know.

DT: Was he still very active, or had he … He'd already got his Nobel Prize by then, hadn't he?

HB: Oh yes, yes. And he was very active on 101 scientific committees and so on.

DT: He had about six years left as head of this department in '47. Did he finish in '53?

HB: That sounds right, yes [Lord Adrian left the professorship to take up the post of Master of Trinity College in 1951].

DT: But you didn't go to him to, in a sense, do his kind of research?

HB: No, he was a very hands-off supervisor. He saw me for five minutes and said that he thought he could get me a research studentship and that was that. And then we talked a little about things that I might do. He knew I was interested in vision and he said that he'd read an interesting article by Marshall and Talbot on the role of microsaccades in keeping vision going [Marshall WH & Talbot SA (1942). Recent evidence for neural mechanisms in vision leading to a general theory of sensory acuity. *Biol Symp* 7, 117–164]. Would it be true, was it really true, that if the eye was completely still you would see nothing? So I spent the first year doing that and decided that I had to find some equipment. By that time I was not looking for Adrian, but for Adrian's assistant, Leslie.

AS: Leslie Hatton?

HB That's right. Yes, yes. He was almost as difficult to find as Adrian actually, but eventually he showed me what recording cameras were available. Of course, there was nothing remotely suitable for recording eye movements but there was one very old movie camera, that had the mechanism for moving film on at any rate and I reckoned that with a little bit of …

MR: And the fascination then was with single fibres …

HB: That was later.

MR: That was later. And Zotterman and Granit, as well yes?

HB: No, I didn't work with either. I knew them of course.

DT: Lord Adrian's assistant technical staff? Was he a batman basically?

HB: Yes.

DT: He probably lit the fire in the lab in the morning.

HB: Probably.

AS: And boiled the kettle.

HB: Yes, yes. And kept visitors away!

DT: I had a peek this afternoon at your paper on eye movements: it's fascinating for all sorts of reasons. But one thing was *The Journal of Physiology* had what – 1500 pages – that year. I was surprised how many. There were three volumes

of about 500 pages each, and I think last year we had one of 6000 pages. But the other thing was, there weren't that many journals about, and there wasn't much literature. Your papers, in those days, had about 10 references, and presumably that was a thorough reflection of the scientific knowledge at the time.

HB: Well, that would depend upon how good a job one did! If one did a really thorough search of the literature in those days, of course you had to be quite fluent in German, because a large part of the physiological literature was in German.

DT: Well, Yarbus – was it Yarbus ? – was Russian. And what sort of date did he publish his pictures of where people looked when they were looking at pictures of people, I think it was.

HB: Pictures of pictures, they were actually. 'The Soldier's Return' do you remember? And there was another one of the subject looking at a portrait of his girlfriend. I got into trouble with that because there was a visit from the Russian delegation and everything had to be translated, of course, even though some of the Russian delegation spoke quite good English. And I asked him to ask Yarbus whether the subject had her clothes on or not.

DT: And you thought that might change where the person would look?

HB: Yes. And where the results showed that it could fit either the face or not, you know. And the others immediately picked this up, and gave a little snigger and said she had her clothes on, you know, but before he could really get that answer in, the interpreter had said, 'I refuse to translate that question.'

MR: Mike Land has done some work recently up north; have you seen that?

HB: Oh yes, yes. That's very nice. People playing ping pong and making a cup of tea, that type of thing.

MR: And reading music: they look at every note and then as they learn the piece …

HB: Well, an experienced sight-reader doesn't read every note.

MR: But you were right at the beginning of that interest.

DT: Did you ever publish another paper after that one on eye movements?

HB: No.

DT: No, it was only the one. Would you like to tell us how you measured the eye movements, because I don't think we'd get permission to do that now.

HB: No. I looked into the sort of optics and mechanics and so on, and people were busy using reflection from the cornea, but if you work out where the image is when you're looking at a reflection of a bright light in the cornea, it's way back inside the eye, only 2 or 3 mm in front of the centre of rotation. So it's clearly very sensitive and easily disturbed by head movements, which you have to get rid of otherwise you'll confuse them with eye rotations. So I thought it would

be at least three times better if I put a bright spot on the surface of the cornea, so I put a little mercury droplet there.

DT: It must have been terribly uncomfortable; we've talked about this – wedges and bite-bars.

HB: Well that was to keep the head movements down because the head movements would completely ruin it. It was very convenient – I was in the room that used to be where Roughton did his blood flow measurements, his rapid reaction things, and it had a concrete slab let into the wall for a balance, for stability. And that was very useful for putting a head on. And then there was, I think, an old microtome or something like that, which had an iron frame, which I could attach to the thing and put a wedge down one side, and head on the other. That kept their heads pretty steady.

AS: Who were the subjects?

HB: Oh, just friends. And other PhD students.

MR: The system may have a contact lens and a stalk and a mirror, was that yours?

HB: No, that was Yarbus. He had a contact lens that was sucked onto the eye, and in addition had a stalk and a lens, which focused on what was on the stalk. So you put a fixed image there – bear in mind you could do other things with it too. And it was Riggs and Ratliff who did the method with the mirror.

MR: So you're good with your hands? You like making things as well as …

HB: Yes, yes. Of course you don't do that in physiology now.

MR: Oh, no, no, no.

DT: I'm sure we would if we could.

MR: But you know your way round a workshop; lathes and things.

HB: Yes. I was reading Dave Hubel's memoirs about his early life in research, and he was obviously an absolute maniac for making things. And always, always throughout his career had a metal-working lathe in his lab and went on making gadgets all through.

MR: I remember seeing Michael Daly's father in Buck and Ryan's [a hardware shop] with a pleasure on his face when he was looking at these tools.

DT: So by the end of sort of '48ish, perhaps, you've done worms; you've done electro-magnetic stimulation of human eyes; you've done eye movements; and somewhere you've thought, 'Let's look at frog action potentials.'

HB: Well, when I came to the conclusion that eye movements weren't going to get very far, I talked to Adrian. He continued, every now and then, to put his head round the door and say, 'Have you read such and such a paper', you know. And we'd chat for, I'd estimate not longer than half a minute at the most, but they were very beneficial half-minutes. And then on one occasion I said I thought I'd come to the end of the eye movement stuff and I wanted to do

some recordings from frogs' retina to repeat some of Hartline's work, to which he said, 'Oh, I wouldn't do that, Hartline's a very clever chap, you know!' But I didn't have time to explain what I wanted to do.

MR: Hartline?

HB: Well, he'd earlier recorded from frogs.

DT: You did that with a, I suppose we'd call it an isolated eye-cup preparation now. Did you perfuse it with anything, or did it just sit in the air and dry up?

HB: It dried up. I tried to keep it from drying up, but I found as soon as we put Ringer's fluid on it, it killed it. So I stopped doing that. Eventually with Adrian – he hadn't said anything about it except I shouldn't do it – so I thought I'd better write out why I wanted to do it. I did that and sent him a little note saying I was planning to go and get some equipment, which one always bought from the government-surplus stores; and got part supplies, I had contacts, chiefly through Pat Merton, with those who knew what came out of which plugs and so on and so forth.

MR: Oh, Tottenham Court Road.

HB: Yes.

AS: Or Lisle Street.

HB: Yes. Lisle Street [off Charing Cross Road in London's Soho area].

MR: Lisle Street, yes. I used to go there, and people used to wonder why. [This is a reference to the well-known reputation for its 'nightlife'.]

HB: But you got stuff there at sixpence a pound, you know. Whatever you took off the shelves, they weighed it and all at sixpence a pound. One of the ways in which I used to make noise was to put a photomultiplier inside a black box with a light, a DC light, and record the output, which is very noisy, which is photon noise, as near as I could get to white noise.

DT: That seems to equate to a page-and-a-half of your CV, doesn't it?

HB: Yes, quite, quite.

MR: You prefer to work on cold-blooded animals rather than move to mammals, or maybe …

HB: Well, I worked later on mammals, but I started off on frogs, because it was simpler.

DT: When you were presumably eventually writing up – the paper was published in '53 so there was probably quite a long time after you did it – were you aware of Stephen Kuffler's work on the cat at the same time? Because your papers are seminal in this, aren't they? Both in 1953.

HB: Yes, actually my first one on this, was not a proper paper but before Kuffler did his, which was …

DT: International Congress [IUPS Copenhagen].

HB: Yes, is that down there?

DT: Yes, it's on my list [of HB's papers], it's 1950.

HB: Yes, that's right. I think that was before his first one, which was 1952. And he was actually at that meeting. Because he told me afterwards that he hadn't come to my talk, but he had been at the meeting.

DT: Yes. No comment. But your interpretation, the way that you wrote the discussion of your paper, where you sort of introduce this idea that maybe the receptive fields were actually designed for particular behavioural tasks, seeing flies at however many inches, I think …

HB: Yes, that was new. And that was because I was very much taken up by the Lorenz–Tinbergen approach to behaviour.

MR: Did you have any antagonism or were your ideas accepted straight away? Did you have any problems?

HB: I think people were interested, but when you publish a paper in *The Journal of Physiology*, my experience is that your post box doesn't actually overflow.

DT: But there weren't many other journals. This was something I was wondering: whether you can have a picture now of how many journals there were, and how many neuroscientists or vision scientists there were in the known universe at that time. It can't have been very large numbers.

HB: No, no. I remember at the International Congress in Denmark in 1950 … Well, I think the talk that I gave there – you know it was just a brief talk, there must have been 20 or 30 people there, and probably I could recognise a good one-third of them, even though it was international. And actually I met quite a lot of people there for the first time.

DT: Has anybody worked on frog retina since? Do they still find the same things as you, or are they interpreted in different ways now?

HB: Lettvin, you know 'What the frog's eye tells the frog's brain'. They weren't actually recorded from the retina; they were recorded from the colliculus. And a few people have repeated that and done it on other animals and so on. But I don't think it really …

MR: While we're on the formation of visual fields in the cortex … Vicky Stirling, I think, at Mill Hill … But that's recent, more recent news.

HB: The projection? Gaze. Yes, that's it. They've done that, yes.

DT: Do you still think, presumably they were off-cells, but those cells are fly detectors? Do you think they really are a specific piece of frog anatomy linked up to its tongue somehow?

HB: The trouble is that the on–off ones, which I think are most like fly detectors, they're not very different from the cat Y-type receptor. And that's always

worried me. If you look at different species of frog, there are quite noticeable differences between the different species, and I could sort of contemplate at one moment, a lifetime of recording from frogs, etc., etc., becoming the world's expert on this, but I got interested in something else for some reason.

DT: So how did you, or was it subsequent to the frog work, or in parallel that you started thinking about retinal noise and Purkinje Shifts and thresholds.

HB: Well, continuing an interest I already had and also because of my interest with the information theory people, realization that noise in all sensitive instruments is always a problem and surely it must be in vision too. So I was interested in that, and in why, for example, you didn't get the results you expect if you just measure the threshold as a function of background intensity. You'd expect there to be a region where you get no change because it's entirely dominated by some internal noise; then a point where the external noise comes in where it will go up as the square root of the background intensity because that's how quantum fluctuations go up. But you won't get that in most cases: you find a lot of regions where it goes up as the square root, which of course fits the idea that, actually, normal vision is quantum fluctuation limited. So I was interested in plugging that idea. And I came across a paper pointing out how very rapidly the expected rate of thermal degradation of a pigment molecule goes up when you lower the activation energy. Only a slight reduction of activation energy, corresponding to a shift of the peak absorption from 500 to 560, is enough to send the thermal degradation rate up by an enormous factor. So I then argued this is why, in fact, the high sensitivity pigments were shifted to the blue because they would have had increased activation energies and lower thermal noise.

DT: Do frogs have red rods, or is it not as red as I think, and they're cold-blooded anyway, so?

HB: Have you ever seen a rod? Because they actually look red anyway. That's because they absorb short-wavelength light, so that shifts what is transmitted toward the red, the long-wavelength side.

DT: So when you said that you were interested in the information theory people, that's Shannon and Weaver and so on: were they sort of publishing in the early '50s?

HB: Shannon's work was coming to light just at the end of the war.

DT: A lot of people in this department of course had been doing electronics and sound during the war, so they probably knew a lot about electrical noise and electronic noise.

HB: Yes they did. Particularly when recording small voltages, you need to know about it because your electrode is limited by noise too.

MR: Radar and … were you involved with [M. H.] Pirenne at all?

HB: Yes, well he came and worked with William Rushton so I got to know him then. And we talked about a lot of these things, and we were influenced by his little book *Vision and the Eye*. Very good little book.

MR: Yes, I have that. He had beautiful handwriting. He wrote me a note once. A good book. He designed an eye bath for demonstrating to students: a big thing with big lenses so you could show the errors of refraction and so on. And he sent me a diagram of this, and he also sent me a stereoscopic picture of it, and he also sent me a stereoscope. A mahogany stereoscope.

DT: So you were doing this noise stuff in Cambridge, and then you went to Stephen Kuffler's lab for a year? Was that so that he could learn what you were doing, or you could learn what he was doing? Or you thought that …

HB: He was very untheoretical. He didn't understand it well, and said, well, if he didn't understand it, he wasn't going to make comments about it of either kind, so his eyes just glazed over, you know. And that was that. So it was no good me trying to tell him what I was interested in, you know. But I had used cats actually, when I was at Mount Vernon. I can't remember quite why we had to use decerebrate cats. I think G. L. Brown had some experiments going on to see whether the spinal cord was also sensitive to oxygen poisoning or something like that. So that's where I learnt how to do that. I don't believe that Kuffler knew how to do it, so I think he probably learnt how to decerebrate a cat from me.

AS: Who else was in Kuffler's lab when you were there?

HB: It was a couple of years before Hubel and Wiesel were there. They went there just after I left. Dick FitzHugh was the person I collaborated with. He was another theoretician – it must have been rather hard on Steve, I think. But he was sort of into Hodgkin and Huxley stuff. And he and I, well, we worked together on the cats, on the absolute threshold and how sensitive it was. And spent hours measuring up photographic records and so on; before the days of computers, you know, did it all by hand. And doing actual experiments.

MR: You felt you'd achieved something, though, didn't you? Doing it that way?

HB: No-one will repeat this, one said to oneself!

DT: Do you think, how would the experiments you did in those days, how different would they have been if you'd had computers to control things and count things and so on?

HB: Well, the big change of course is when people started using awake, behaving animals because one can look through the anaesthetics and see those that had the least effect on sensitivity – we were interested in sensitivity. And I remember the barbiturates, which had the effect of more or less suppressing all the spontaneous activity in the retina at an anaesthetic level, but they were some of the most sensitive animal results we have. But I think – I can't remember what we finally used – I think chloralose and urethane or something. Because urethane was a great …

MR: Well, that suppresses everything, doesn't it? Urethane?

HB: No, you get quite a lively – you know – liveliness is judged by pinching its tail.

MR: But would it be controversial about nitrous oxide?

HB: Nitrous oxide. Yes. Some would wonder if it was anaesthetic at all.

AS: Yes, The Physiological Society published guidance on that, specifically, because it was queried so often.

DT: Yes, I think they decided that it wasn't an anaesthetic by itself. It needed to be with a barbiturate or halothane and so on.

HB: Especially if one was using it in conjunction with curare

MR: Yes, they misinterpreted the laughter.

DT: So you've done frogs; you've done a year with Kuffler looking at receptive fields in the dark and absolute thresholds, and so on. And then somehow this led – or was this just another line – to the sort of 1961ish papers that so many people now quote in the conferences about redundancy and coding principles and things.

HB: That was from my contact with the group in London really. That was called the Ratio Club and we met I think two or three times a term for a number of years – four or five years – there's been a book written about it by somebody in Sussex. And then it sort of petered out – I talked about redundancy there. Donald McKay was in it, and a computer scientist from Malvern, Philip Woodward. Donald McKay gave excellent talks, and I only discovered much later how he learnt to give such good talks: he was a Methodist lay preacher and gained practise by giving sermons every Sunday.

DT: Talking about statistics and redundancy? But do the ideas of redundancy and coding – do they relate to what you did in the frog? The other stuff you told me, the notion of noise and statistical variability of signals leads you into these ideas?

HB: Yes, and there was a paper by Attneave, which actually I didn't know about when I prepared the talk, but somebody kindly drew my attention to it. He knew a lot of the psychological phenomena and so on, and it seemed that you could get insight into things encoding light, for example the adaptation of sensory discharges, the fact that if you put pressure on the skin, it only sends impulses out for a fraction of a second, or a few seconds at most, and then it goes quiet. And that's because there's no more information: once the brain has been told of something, it doesn't need to go on being told it, you know. And in the same way in space, you don't need to respond to the whole of the uniform area, because points round the edge tell the brain that there's something in there. So it linked up with sensitivity to edges, and likewise, to sensitivity for movement. Looking a little bit longer in this direction, one would expect neurons to be selective for orientation, but I did not suggest that, unfortunately.

DT: I notice that there's an article here entitled The brain as a Predictor [Barlow HB (1962), The brain as a Predictor. *Cambridge Opinion* 27. R. Freedman (ed.), p. 18]. That seems very prescient; is that about top-down processing?

HB: That was for a magazine, was it Granta?

MR: *Cambridge Opinion*.

HB: Was it *Cambridge Opinion*? That was something that Jonathan Miller edited. That was actually written when I came back from Kuffler's lab

DT: It was '62.

HB: That was when I was a Teaching Fellow at King's. Jonathan Miller asked me to write that, and he was my student, so I did.

DT: When did computer science start, and when did people start wondering whether they could make computers recognise patterns or see things? Did you meet people like that?

MR: When did you see the light with computers?

HB: Again with this Ratio Club, Turing came and gave us a talk once. Alan Turing. And Dennis Gabor I don't think did, but Donald McKay was I suppose at the beginning of that time. He was a PhD with Dennis Gabor. And Philip Woodward was a computer scientist at RRE, Malvern. So it was easy to see, and the most advanced equipment people had in those days was one of those averaging computers, which of course did add an order of magnitude to the sensitivity for any repetitive signal.

DT: Did you ever do post-stimulus time histograms? I remember somebody once trying to find the history of the histogram, and it was sort of middle '60s where people started – instead of showing individual pictures of spike trains – started averaging.

HB: Well yes, we did. A paper with Myong Yoon and Bill Levick on the sensitivity of cat retina for individual quanta, that must be 1970, I should think. [Barlow HB, Levick WR & Yoon M (1971). Responses to single quanta of light in retinal ganglion cells of the cat. *Vision Res Suppl* 3, 87–101]

DT: Yes, there we go. *Vision Research*.

HB: Yes, and that was well into the histogram era.

DT: So again, sort of around about 1960 you're speculating about what the nervous system is actually for, but you still just go on and measure it, and there's a lot of work about small signals and noise, and a lot of detailed work with Bill Levick. And you said, almost as an accident, you also did rabbit retina in a way reminiscent of the way you'd studied the frog. An accident?

HB: Well that was an accident. That was because I went on sabbatical for a year to Berkeley planning to do experiments on the cat, and the cat colony at the School of Optometry – the School of Optometry is next door to the Law School

– and they had the basement of the Law School which was where they kept our cats. And they had an epidemic of fleas. And the room above the cat colony was where all the secretaries of the high-powered lawyers lived and they started getting bitten. So they complained to their bosses, so the bosses told the School of Optometry, 'Take your cats away.' So we had to. There were other people working on rabbits, but they weren't kept there – so we had to use rabbits instead. So we hastily had to build a head-holder for rabbits and find out how to prepare them and so on. And Dick Hill and I did our experiments on rabbits instead.

DT: How did you hold their heads? Because we used to do lab classes here and when you put the ear bars in, it just pushes the animal off the apparatus.

HB: I don't know – I can't recall how Richard Hill and I dealt with that. Maybe the problem had already been solved by Elwin Marg, who, worked on rabbits, and he may have already resolved that problem.

DT: Somewhere in this, well you went to Berkeley for a year on sabbatical or some such.

HB: Yes, then I came back here.

DT: For not very long?

HB: I think it was about a year. What happened – Gordon Walls was there when I first went but he had a heart attack towards the end of the time, or just after I'd left, maybe. And they offered me his job, so …

DT: You must have made a good impression?

HB: Maybe, but it was Gerald Westheimer who argued my case.

DT: And did you meet Bill Levick in that year when you came back here?

HB: Yes. He'd come over to work with Fergus Campbell. But he wanted to do neurophysiology, and Fergus wasn't into neurophysiology. So he didn't have much to offer him, but suggested he should work with me instead. That was one of the best strokes of luck in my life. And very pleased I was, you know.

DT: So you did lots of rabbits up on the fifth floor. I did my PhD in that room too. There was a purple stain and a crack, and people said this was where Bill Levick was making a front-silvered mirror and the silver nitrate exploded one night.

HB: Yes, that's right.

DT: They painted it; it's gone unfortunately.

AS: Can I just ask one thing? When all this research was going on, were you doing any teaching at the same time, or only just supervising PhD students?

HB: I was supervising in King's, but that was a, you know, College Lecturer job. And I think it was six hours a week or something; it wasn't exactly demanding.

AS: Did you lecture in the Part II course?

HB: I also did the Part II lectures, yes.

MR: On vision?

HB: Yes.

MR: But you didn't teach in pre-clinical? Or did you?

HB: No.

DT: When you went to Berkeley, and then you had to set up a lab or you had to find your rabbit colony again, did Bill Levick go with you?

HB: Yes.

DT: Yes, I thought you had worked longer than just one year.

HB: Yes, we did quite a lot of the motion experiments in that lab there. In fact, I think we did most of them there. And then when we'd got to Berkeley we started working on the cat and trying to do the experiments I'd planned originally to do at Berkeley. But they didn't work out quite the way we expected. It took us a long time to get going properly on that.

DT: Was the result ultimately different or was it harder to get the result?

HB: Well we came across quite a lot of other interesting things in the course of it. I mean, the actual way the retina normalises for mean luminance and perhaps also for variance, or standard deviation of fluctuations as well, I think I had the impression we were a bit slow in the uptake as to what that was all doing. And we got a lot of interesting results about quantum spike ratio and things like that, and how things do shift around, but I don't think we ever made a very good story of it. I could do better now.

DT: Was William Rushton coming back into this? Was he about the same time?

HB: He went over to Berkeley too to work with Gerald Westheimer.

DT: Oh. Did he also go to Evanston, to Christina Enroth-Cugel?

HB: No, he went to Florida. And with Matt Alpern too, didn't he? He worked with Matt Alpern, but I don't think Matt ever went to Florida. I'm trying to think who was at Florida with him. And he spent several years, or most of the time in Florida, didn't he?

DT: There was this debate that I very distinctly remember about adaptation pools and how large an area you got adaptation over. Was it a receptive field, or a rod? Which side were you on at the time, do you think?

HB: Well, I was trying to explain things in terms of dark light, in terms of the idea that the rods were generating pseudo quanta; generating a response that looked as if the rod was absorbing quanta when it actually wasn't; it was regenerating rhodopsin or something. So the regeneration of rhodopsin made it think it would absorb a quantum or something like that. In which case, if that had been the case, you didn't need the mechanism that William had

postulated. I think it finally turned out to be a case where we were both wrong.

MR: What was the origin of the extrapolation down to one quantum?

HB: That was simply that we calculated the number of quanta absorbed from the flashes of light we were putting in, and then plotting histograms, finding a number of impulses from a single neuron was more than the number of quanta that had been absorbed.

MR: So it was actually accepted?

HB: I think so, yes.

DT: So William Rushton had this notion of adaptation pools and networks and things …

HB: Well, I have a great mistrust of those experiments that he did with stabilized images because of the trouble we had, and it's very difficult to know whether the image is sharply focused and …

DT: This was the experiment of, in theory, flashing a bright–dark, bright–dark, square-wave pattern and then showing that the threshold in the darker bit has been affected as well as in the brighter bit? That was …

HB: That was what he would predict, but the trouble is so would we, because the masking effect of the noise from the strongly illuminated bars could easily interfere with the detection of light …

MR: Inhibition?

HB: Well, no, not necessarily by inhibition but by adding noise to the signal, if the system couldn't separate out between the bars then the noise from the bars would hinder the perception of light between the bars.

DT: So you've got three factors limiting reliable detection: what were the three factors. Because there's a paper here a decade earlier saying something about three points, about lateral inhibition, but maybe that's different. So three factors limiting reliable detection …

HB: Yes, that was different. I don't know what the other three factors were, the three points. The three factors were: loss of absorption, bleaching, noise and … can't remember what the other one could be.

DT: I think it was one of the first papers I ever read, actually.

HB: Oh right.

AS: Is it in *J Physiol*? Should I go and get it?

DT: 1969 it was. I seem to remember it was on that strange pink paper.

HB: Is it Volume 200, page 1?

DT: Yes, yes. And it was sitting in the Reading Room when I was starting my PhD, and I avidly read anything. And that was one of the first things that was there. And I think I could do two of the factors affecting, but the third one I found rather difficult. Yes, Changes in the maintained discharge with adaptation level in the cat retina, in '69 with Bill Levick is a very similar sort of idea to what you did with FitzHugh and Kuffler, wasn't it? 'Maintained activity in the cat's retina in light and darkness'. Did you come back to it with another idea?

HB: I think the new – I think it's in that paper – the fact that there are cells which fire regularly.

DT: Were you recording from retina, or from the nerve with Bill Levick?

HB: From the retina.

DT: You really were?

HB: Yes. And those, as we now know, are the … melanopsin-containing ganglion cells.

DT: Right, so you were finding them? Because there was this very interesting thing that sort of happened in the late '60s, early '70s, where people began to realise, unfortunately, that the experimental technology you used actually seriously affected what cells you recorded from. So I could quite appreciate it that you were one of the original finders of w-cells. Of course Bill Levick then went on to do some very seminal work with Brian Cleeland, and then Heinz Wessely, wasn't it? Yes, some extraordinary work in the late '70s. So his direction will, you know, perhaps have started there, to find some slightly odd ganglion cells that people weren't finding.

HB: The main point of – I think, it's so long since I've looked at these papers I've forgotten what's in them …

MR: The work on the Purkinje Shift – was that a digression or was that something that you were …

HB: No, that was the fact that in all animals that have a Purkinje Shift, that is to say when the wavelength changes as you go to lower luminances, it always goes to shorter wavelengths. And we didn't – I hadn't appreciated until that time, that the spontaneous decomposition rate of a photopigment becomes much higher as the activation energy becomes less, which is accompanied by the shift so the pigments that are only sensitive to blue light, have a lower thermal degradation rate. So that seemed to fit in with the high-sensitive pigments of a shorter wavelength. That's all just been wrapped up by King Wae Ya. He's got all that.

MR: One of the first experiments I did was with Geoffrey Arden on the tapetum of the bush baby. It had a riboflavin tapetum, looks like gold leaf, and the idea was that the light comes in in the centre of the rod maximum – sorry – comes in at a higher, a bluer, wavelength than the rod maximum and that shifts it into the green because it fluoresces and pushes it back through the …

HB: Does it actually fluoresce?

MR: That was the hypothesis. I'm not sure we got a proper – it was all done with electroretinography.

HB: But I don't think that the riboflavin fluoresces … so it would shift to peak wavelengths but it would do it by absorbing light, not by creating light. So that doesn't work out.

MR: That was at the beginning of my career!

DT: I've got your three factors [reading from one of the papers]: the quantum to spike ratio, which is the mean number of additional quantal absorptions required to produce an additional impulse, presumably on average. The temporal course of the response, which determines the time interval within which the maintained discharge is modified, and the statistical distribution of the number of impulses that occur in this time interval in the absence of the stimulus. Did you do gamma distributions in this paper?

HB: Yes.

DT: I vaguely – one of the great mysteries for me was Maths, and I think there were gamma distributions. Didn't you tell me that your brother once wrote a table of gamma distributions, or am I completely confusing that?

HB: No, that's not my brother. It was Barlow's Tables. They may well have had gamma distributions in it. Ah, that's interesting …

DT: I was just thinking: you did worms, frogs, ommatidia – we missed out on that – cats, people, pigeons, rabbits – any other animals? Pigeons. How did you write a paper on the pecten of the pigeon's eye?

HB: Yes, that was – who was that with?

DT: Ostwald, T. Ostwald [Barlow HB & Ostwald TJ (1972). *Nat New Biol* 236, 88–90].

HB: That's right. He did a project with me. The retina is concave, so if the sun's image is on one side of the pecten there will be a very bright spot which is illuminating all the rest of the retina on that side of the pecten, whereas the pecten itself is shading the rest of the retina from the light scattered from the illuminated spot.

MR: Is it truly concave, or is it another function?

HB: It's not necessarily spherical, but it is concave, so that, without a pecten, the light from any point would have a clear path to every other point on the fetna, and that would raise the threshold considerably. And the idea was that by having this structure which sticks out, pointing towards the lens, you would intercept that light. So if the sun's image is one side, the whole of the other side of the pecten would be in shade. It's an intraocular eyeshade! And I think that's right: I think that's one of its functions. It also has another important function of providing oxygen – it's how the vitreous is oxygenated so to speak,

so a lot of the receptors get their oxygen that way. I wouldn't quite like to get into a fight with anyone as to which aspect had greater survival value, but I think the one we pointed to there is genuine.

DT: The various things that I guess you started, sort of between '47 and 1970, were there ones which, looking back, you know, you're disappointed that either you or maybe other people haven't been able to pursue, or didn't want to pursue?

HB: I think people ought to pay more attention to the absolute performance of these statistical tasks because the world is full of people who run down what the brain can do, and point out how illogical people are and all that. But in fact, when the brain wants to do a good statistical job, it can do it very well. You have to choose the right job – it can't do every job very well – and there's nothing like an argument from statistical efficiency for knocking down theories. If a particular theory says you're going to need 250 quanta to do a job, and you show that the brain does it with 10, then that theory is in the waste basket. And there's no other way you can knock things out as conclusively as that, that I know of. So I think that people ought to pay more attention to the optimum performance.

DT: Were there any things – you hinted that maybe your work on dark adaptation pools would have been interpreted differently now, or can you think of, with all these modern techniques like reverse correlations and confocal microscopy and calcium imaging, whether anything you did would have been done better or quicker or …

HB: With regards to doing better, I'm certain if I were to submit papers at random from that list, at least 50% would be rejected now, because the standards just are much higher.

MR: But it's the ideas, isn't it? It's the ideas that are important.

HB: Well, yes, but it's very hard to publish an idea without any facts at all supporting it …

MR: I mean these are the groundwork for the work now. Look at these two … they're building on them; they're not doing the thing from the start.

DT: I don't know, but perhaps what Horace is saying is that nowadays it's quite difficult to send in a lot of bricks and build them up. They somehow feel it has to be a completed project. We still don't know how the brain works so we can't publish it …

HB: Ah, but the actual technical standards: you pointed out that I might not have got ethical approval for putting a person's head on a concrete block and wedging it with things and so on. But of course it's what you do to animals that matters.

MR: Tim Bliss – one chapter of his PhD thesis is 'How the Brain Works' – the title!

DT: Your experimental work, apart from one or two digressions – one on binocular disparity – it's almost entirely about the retina, because obviously that's where

vision starts. If it doesn't leave the retina you won't see it, I think. You said to me yesterday that Lord Adrian somehow didn't really believe that you would be able to study individual brain neurons and be able to study how perception or behaviour worked. It was just too difficult and it would be a network.

HB: Yes, that was the generally held view. It was a complicated network, and even if you could record from a single neuron in the brain, you wouldn't be able to find out where it fitted in, which is perfectly reasonable if you believe that everything in the brain depends upon millions of neurons being active. You're not going to get very much from a single one. But they were wrong. The part played by the neurons is bigger than that: big enough so that you can see, as you go from stage to stage, significant changes happening in a direction which makes some sense.

DT: Yes, your experimental work is mostly about the retina but the speculations about what it's all for, and the neuron doctrine, perception paper that is there … these are not just about the retina, are they?

HB: No, they're more grand ideas, yes. Well, I was talking about redundancy reduction and the importance of redundancy in vision, starting actually before any of the first things were published because it was a talk, I mentioned, at a Journal Club. But it wasn't a Journal Club, it was a discussion meeting I went to that was organised by Zangwill and Thorpe and I gave a paper there about the importance of redundancy, which I think was in 1956 or '57. So I've been talking about that for quite a long time, but it's only quite recently that this has been taken up and other people have understood what I was saying.

MR: Did you coin the concept of the terminology 'redundancy'?

HB: No, that's Shannon. Redundancy plays an important part in Shannon's theory of communication and the engineers always understood immediately, because if you understand about redundancy you can send 10 messages where one grew before, if you see what I mean – to mix metaphors. And so they understood that immediately and applied it, so that's why, you know, the web is so cheap and all that, because it has been understood, and you can send information down wires much better. But to some extent, I, and others at that time, were misled by thinking it was only important in getting more information down each channel. So hence I, and others, thought in terms of redundancy reduction. But that's not really what's at stake. What is at stake is the ordinary way we find our way about in the world: if we're dealing with an extremely complex world where everything is interacting with everything else, then in such a world it's extremely difficult to learn what is truly associated with anything else because it's all messed up and everything is strongly correlated with everything else. So you've got to understand the statistical relationships in the external world. Now it's true that understanding those statistical relationships is what enables engineers to make proper use of optical fibres and things like that, that's the importance of redundancy for them. But for the brain, it's devising a representation from which you can learn. You see, from Pavlov onwards, learning theorist have happily assumed that the different stimuli they use are independent of each other. And of

course they're not. Or you have to work hard to make sure they are. And the ones – the messages that come into the brain – aren't like that: you've got to do something to make sure they are decorrelated; lurking in that information, there are whole mass of interdependencies which are going to completely [mess] up your learning because you're going to think something's association with food is an independent fact, whereas in fact that may be strongly dependent on something that has already been taken account of.

MR: We're not talking about lateral inhibition?

HB: Well, lateral inhibition is a step in that direction. But for example if you look at Geoff Hinton's recent paper, about learning vision, I think it's called, in his system, almost the first step that he talks about at all is not to do with light–dark adaptation or anything like that, but is to do with getting a covariance matrix in which the values are low; getting rid of covariances. And that's just the kind of thing which I think is the right model for what goes on in the cortex.

MR: General sensory thing though, prior to tactile, auditory …

HB: Yes, but according to that it should be applied to any other source of information, so you've got to make sure your tactile information is not simply copying what you're getting by visual information and so on. If so, then paying attention to the tactile information won't buy you anything at all, because you may have already taken all the information that's there …

MR: But then you have selective attention?

HB: Ah well yes; then attention comes in then.

DT: You have an article here called 'Is neurobiology totally non-mathematical?' In 1976, in *Lectures on Mathematics and the Life Sciences*, volume 8. Was neurobiology totally non-mathematical? And is it still?

HB: Well, there's a lot of misapplied mathematics in the theories about networks and so on and so forth, partly because of just what we've been talking about. It's not always appreciated just how interdependent all our sensory messages are. But I can't remember what I said in that paper.

DT: Do you think you really did talk about mathematics or about – I don't know – Bessel functions and von Euler things. It's sort of in the days before computational studies and it seems to me a lot of the ideas that you've suggested about redundancy reduction and decorrelation have only really begun to look credible – that's not quite the right word, is it? to see what they really mean – when you can do a computational analysis of how you believe a vast number of neurons would respond. So that means computer modelling, if not mathematics.

HB: I do think that's true. I mean in the old days one could make theories and wave your hands. And some people were convinced.

MR: Some were very good hand-wavers.

DT: Yes, you waved your hands and said, 'If this neuron inhibits that one, then this neuron inhibits that one, then this will sharpen that one's tuning curve.' And it sounded great until someone tried to write a computer program that did it, and it wouldn't work.

HB: And it doesn't tell you anything of the sort, yes.

DT: Where do you think the ideas – or how would you pursue the study of coding and statistical efficiency and so on? How would you pursue it now if …

HB: Well, I am pursuing it now! or trying to.

DT: Maybe experimentally perhaps. What sort of …

HB: Well I think that what we're doing now, you know, is trying to do psychophysical-type experiments on the operations – that it looks as if, in the light of all these ideas about signal to noise and so on, this is likely to be what early stages of visual processing in the cortex are actually trying to do. So we're using the ability of the eye to detect faint streaks against a background of noisy dots, and it turns out this is a task the eye can do quite well. We haven't got the final figures to absolute efficiency yet, but they're going to be of the order of 20 or 30% or something. In other words, the system is not a complete idiot at this; it's doing really pretty well. And one of the things that you can compare with this is how well you can see streaks which are generated by changing the mean density of the dots. If it's in a streak then you can take that, and can signal just going to the left and not the right, and so on and so forth. You can put that into the statistical calculations and show that it's making good use of all the information in those dots.

MR: And these are psychophysical experiments?

HB: Yes. But you can also produce streaks by a different manoeuvre – by generating Glass patterns where – you've probably seen them at some stage in your life – somebody puts up an array of dots which looks like a spiral, but in fact they're only made of pairs of dots. No more than a pair-wise dependency, and yet you see a complete pattern of streaks. You produce dots like that and not only is that also done with a high efficiency, but it's done at the same time as the others. I mean you can do the same experiment, and you get the same result with regard to efficiency, and yet the mechanism required to do it is totally different. So there's another kind of analysis there leading to the visual streaks … which is autocorrelation.

MR: And these are pseudo-movement effects?

HB: Yes, that's right – pseudo-movement effects, but there isn't any actual movement there.

MR: Are there experiments that you can't quite do now that you'd love to do?

HB: Yes. All it needs is someone with full control of Matlab, and understanding of psychophysics, and appreciation of the nature of the tasks that vision has to

perform, and knowledge of cortical neuro-physiology. There are not all that number who can do all of these.

DT: […] If you had a nice, clean lab and legal permission, and you had frog retina again: is there an experiment you would do on the frog retina or would you do something differently? And you had these clever electrodes that can record from lots of things at once and convince you that they're able to separate the signals.

HB: I think the job of the cortex is to exploit the statistical patterns of firing that occur among its neurons. I think here is a chance that these fancy new electrodes may be able to detect and discriminate between these statistical patterns of firing, even if they cannot detect and discriminate between the firings of individual neurons. I'm certainly hoping this will turn out to be the case!

DT: It could be simpler. They might not have adapted or anything … I've just sort of caught a glimpse here of a very important 1976 paper called 'The Languages of the Brain' and I guess that's important for other reasons. [Barlow HB (1976). The languages of the brain. In *The Encyclopaedia of Ignorance*, ed. Weston-Smith M and Duncan R, vol. 2. Pergamon Press, Oxford.]

MR: You haven't yet waxed philosophical and religious like Eccles or Sherrington?

HB: Well, the nearest I've got to this are some ideas about consciousness, which is, so to speak, something I learnt at my mama's knee: the biological why and wherefore of biological structures and functions depends on their contributions to survival. You're not going to understand anything about consciousness until you know what its survival value is. And to many people, including William James, by the way – which rather surprises me: he couldn't think what kind of survival value consciousness could possibly have had. And that's doubly surprising first of all because he could think about a lot of things, and secondly he's of Irish origin And Irishmen are notorious for the way they can wave their hands and talk, and I maintain that we couldn't communicate with each other if we didn't have – if we couldn't also talk to ourselves. And that is what consciousness is: we're describing what's going on to ourselves – what's going on in our own brain. If we can't describe it to ourselves, we can't describe it to other people. So that if we didn't have consciousness we couldn't live the social life we do. We couldn't have this kind of discussion.

MR: But what's the alternative to consciousness apart from unconsciousness?

HB: Well, I once gave a talk about this at Brown in Providence, and to my horror at the end of it somebody got up in the audience and said, 'I'm sure you know Nietzsche's soliloquy, Number 293 (or something)'.

MR: Sounds like Denis Noble.

HB: It wasn't actually Denis, it was Martha Nussbaum. And I didn't know much about Nietzsche or the soliloquy in question, so she told us what he had said. She told us he had said that if it wasn't for consciousness we'd be like solitary

beasts in a jungle that do not communicate with their conspecifics, lead solitary lives, and manages to get by all right with the few social activities they do engage in.

MR: But it's conscious.

HB: It may have some of the attributes of consciousness, but I'm pretty sure it does not have those attributes that have enabled humans to establish human cultures and civilisations.

MR: Of course not. You know, we don't know what's in other people's heads, but …

HB: I don't think it's fully conscious. I don't think it's capable of communicating with conspecifics, in the way we can, and are in fact doing at this very moment (I hope!)

DT: You've got something called 'Nature's Joke: A Conjecture on the Biological Role of Consciousness'. [Barlow HB (1979). Nature's Joke: A conjecture on the biological role of consciousness. In *Consciousness and the Physical World*, ed. Josephson BD and Ramachandran VS, pp. 81–90. Pergamon Press, Oxford]

HB: Yes, that was one. It was pointing out that consciousness is not what we think it is; we think it's concerned with the internal working of our brain, so you have people talking about possible consciousness of a brain slice in a Petri dish, you know. I don't think that's possible at all. I think that the conscious part of our brain is what enables us to be the enormously social animals we are, and we won't understand much more about its nature until we understand in more detail how it can promote the survival of the human race. You know, this becomes more and more important the more you think about it. Even the baby has to cry to get fed, and so on.

AS: You said largely the social animals that we are – so in cases of severe autism, would you think there was some difference in consciousness?

HB: Yes, I think there would, yes. The trouble is when you start saying things like this, you are liable to be accused of thinking that this entitles you to treat an autistic child like an animal or a slave. I do not think this is so, but I do think autism interferes with consciousness, and impedes the communication of thoughts from one human mind to another, and that this activity has obvious and immense survival value to our species

MR: Maybe we should lighten the discussion?

HB: Oh, I think it's already pretty light-hearted!

DT: […] Were you brought up in Cambridge?

HB: No. My maternal grandmother lived here.

DT: Was she the one who wrote the book?

HB: No, that was Gwen Raverat. She and my mother and Frances Cornford sort of grew up together, so I saw a lot of the Cornfords too.

MR: You didn't speak of your father.

HB: He was a civil servant. He was very keenly interested in the origin of words. We always used to have arguments about the meaning of words, and the large version of the *Oxford Dictionary* was always in the dining room, usually consulted at some stage in a meal.

MR: So he worked up in London? So he commuted?

HB: Yes.

AS: And what about the Cambridge Instrument Company?

HB: That was my paternal grandfather – that was Horace Darwin, who was Mayor of Cambridge. And he was Cambridge's equivalent, so to speak, of Bill Gates or somebody. But that was before people like that made as much money as they do now, unfortunately. And he was – I think – it wasn't considered that he really behaved as a Darwin should behave because he started a company and things like that, you know. But of course the Wedgwoods were, you know, not only was Emma – Charles's mother was a Wedgwood and countless other cousin marriages before and after that – but they were very much the same tribe

DT: Was Charles Darwin a Quaker?

HB: No. The Wedgwoods were, and I think they were … [actually they were Unitarians]

DT: It's interesting how many 19th century big industrialists were all Quakers, and they built little towns to keep their workers in.

HB: Yes.

MR: Keep them on the straight and narrow.

AS: Ricenda Huxley was from a Quaker family [the wife of Sir Andrew Huxley].

HB: She was a Pease. That was a proper Quaker family.

MR: So is your family still – is there a big group in touch, or have you all gone your separate ways?

HB: Well, every now and then we meet, usually at a funeral or occasionaly a wedding, something like that. Yes, I think we probably see more of each other than most cousins do. It's quite a large cousinhood.

MR: Is that the Barlow side?

HB: I keep in touch with one or two of the Barlow-side cousins too, but most of them are on my mother's side, the Darwin side. And last year we saw nothing but each other practically.

AS: And Ruth, the poet, she's a … [Ruth Padel].

HB: She's my niece. My sister's daughter.

AS: Ah, so she's Darwin from … yes.

DT: So, I think we've finished.

AS: Can I just ask you: what about your own children? Are they sort of Darwin-minded, or …

HB: They get on quite well with their cousins actually. They like some of them! But I don't know how many generations they'll go on being on speaking terms.

AS: And has anyone taken to science of your lot?

HB: Of my particular lot: one of my daughters was a mathematician – is a mathematician. And she has a 'construction' named after her, I believe; something to do with algebraic geometry. I don't understand it at all. But she had children and found she couldn't give lectures and have children at the same time, so she's given that up. One of my current lot is one of very few people I welcome in my lab because she's got all it takes to be a good scientist, you know. She's very observant and very good with her hands, and has a lot of common sense. And seems to be able to get equipment to work, and even to be able to repair it at times. But she's at Oxford now, and she shows no sign of diverting her talents to science. She's running the Pembroke College Boat Club at the moment. (PS 2015 She's actually doing a PhD on sleep in Zebrafish now)

MR: Is there religion in the family?

HB: I'm afraid there are pockets of religion.

AS: I think I've said what I want to say.

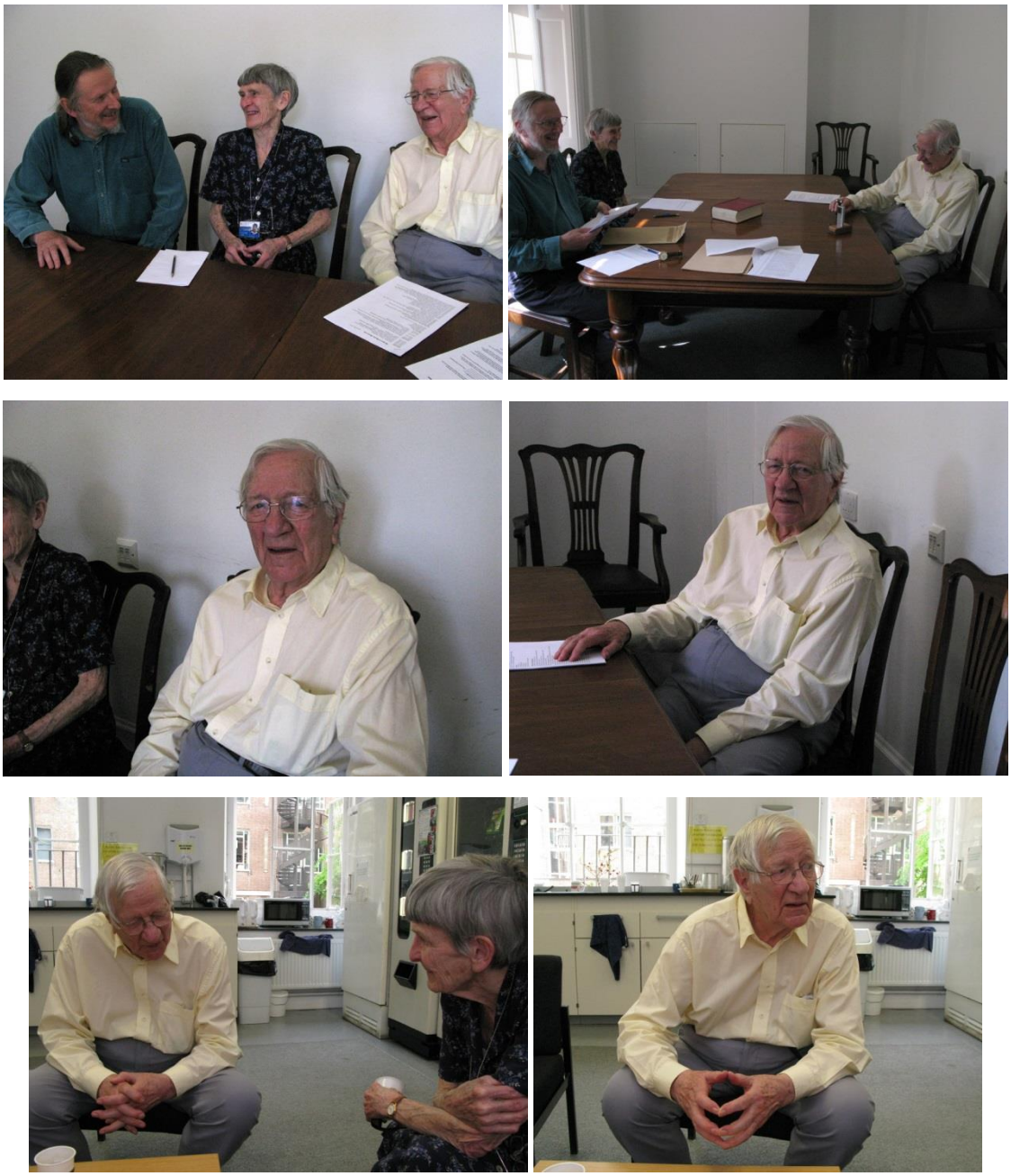
MR: We thank you.

AS: Yes, well, we're very pleased and it's been very interesting and we've learnt a lot. I'm sure other people will enjoy hearing it when it's all transcribed.

HB: Well, I feel sorry for the person who has to transcribe it.

Further reading

Barlow HB (1986). William Rushton. 8 December 1901–21 June 1980. *Biogr Mems Fell R Soc* **32**, 422–459.



Horace Barlow with Ann Silver and David Tolhurst.

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