The Science Studio Interview
With Horace Barlow

Roger Bingham: We are in the auditorium of the Salk Institute in La Jolla, California where later today today's guest, Horace Barlow, of the University of Cambridge, will deliver the Francis Crick Nobel lecture. This is one of the institute's ongoing series of Nobel lectures and Francis Crick, of course, was co-discoverer with James Watson of the secret of life, the double helix structure of the DNA molecule in 1953. And, although most of Crick's earlier career was spent at the University of Cambridge, he was a professor here at Salk, from 1976 until his death in 2004. So, there are two immediate, obvious connections between Francis Crick and this year's Crick lecturer.

First of all, Cambridge, where Horace Barlow spent most of his professional life working visual neuroscience, physiology, perception, and you are in fact, as I recall, a Royal Society Professor of Research in physiology at the University of Cambridge, and you are now at Trinity.

And secondly, those subjects that we have just been talking about were the kind of primary entrée to the study of consciousness, which Francis Crick was largely responsible for making a respectable scientific enterprise. What consciousness is for, the making of minds, those are also subjects, I know, that greatly interest you, but first, let's do Cambridge and Crick. Do you recall meeting Francis? Was this "pre-DNA" or was it even on his radar at this point? Do you recall the circumstances?

Horace Barlow: Yes, I recall very well. I'll tell you the story. I got a call from Francis Crick, well, not from Francis Crick, I think it was from a friend of his, saying would I like to meet Francis Crick, who wanted to talk about neuroscience. This was, I think, in 1951, as far as I can date it. And we met, we talked and I told him what I was doing. But then I - he was a physicist, who would take not biology and who had already done one thesis project, but he was casting around for a more interesting area to work in, and he was wondering about neuroscience.

So we chatted at a café in Cambridge for I suppose an hour and a half, maybe two hours, and I talked about what I was doing for half an hour or so. And he asked, as he always did, a whole lot of surprisingly penetrating questions, considering that he ostensibly knew nothing about neuroscience. But he obviously had been reading about it. And then I said, "But what else are you considering doing?" And then, for the next good hour, I think, he talked about the possibilities that he saw arising from analysis of the structure of DNA. This is in 1951, a couple of years before they got anywhere, or got the solution rather. And actually, before he'd started working with Jim Watson. And I was actually bowled over and I was amazed at what he saw as possible. I told him at the end, "Look, if just one percent of what you're talking about comes off, it's going to be ten times as important as anything I can see happening in neuroscience for the next ten years". And he said, "Well, what about consciousness?" and so I said, "Well, you're not going to get far with consciousness until you know about the origin of life. Do that first!" Which, of course, is what he did.

Bingham: So this is very interesting. So these subjects were there from way at the beginning, because he also did some stuff with Sydney Brenner on the notion of the origins of life, possibly panspermia and so on. But the idea that Francis was already thinking about this whole notion of consciousness and indeed of DNA in such depth is quite surprising I think.

Barlow: Yes, it is. But of course, he's largely responsible for where we are in molecular biology and his vision of what might transpire from finding out more about this molecule is absolutely amazing. I never ceased to wonder about it. He told me that, last time I saw him he said that he himself, whenever he thought about those early years, in the early fifties and so on, he was astounded at how rapidly it all developed. And how it followed quite closely what, more or less, what he was expecting. It wasn't, not exactly of course, some things he got wrong in his imagination, but he had an amazing ability to see what the possibilities were just when discovering the structure of DNA.

Bingham: Just before we shift on to, just one last thing about Francis since this is of course the Crick lecture. What were some of the things he was setting out to you in that conversation, as possible futures?

Barlow: Well, once again I remember we talked about quite a lot. It was Chargaff's discovery of the non-random variation of the different bases in DNA from different sources. And, because he initially connected that with the possibility of DNA pairs being coding elements and so on. My memory's not up to it, remembering exactly what he suggested then, and I must have got some of it wrong because I can remember he talked about solving the actual coding problem, which, of course, wasn't solved until very much later. So I must have added that on to my remembered story of meeting him in that café and talking about these things. And ever since then, of course, I've seen him quite frequently and regularly. I never got anywhere in explaining my own ideas about consciousness to him, by the way. He was very resistant to them.

Bingham: What was he resistant to? What were your ideas?

Barlow: Well, it's always seemed to me that humans are social animals, and that without being able to have the kind of conversation we're having now, where I explain my ideas to you and you reciprocate. It's so basic that we expect to be able to ask each other, "Why did you do this? Why are you holding your glasses in your hand now? Why are you doing this?" And you give me an answer. And that's such a natural part of our life that we don't stop to think of what an extraordinary, complex mechanism is required for my brain to be able to explain to your brain what is going on inside it. And that is consciousness, what consciousness does. It enables, it makes a medium of communication between us.

Bingham: So, the biological function of consciousness would be…?

Barlow: Well, to oil the wheels of social life, so to speak, to make it possible. Without me being able to explain to you why I, why you, what I've done, reasons for what I've done, and you being able to explain to me what's going on in your mind, we couldn't have the kind of social life we do. Nor could we have the field of external knowledge that we have, stored knowledge that is in libraries and universities. Nor, for that matter, would we have the means of communicating which are behind building up the material side of social structure, buildings and so on.

Bingham: Now, are you saying that, are you limiting consciousness to humans?

Barlow: Not necessarily, I don't, it's quite clear that animals do communicate with each other, and sometimes very effectively, as when dogs manage to herd a group of sheep in cooperation and know what each other's doing and what they're going to do and so on. So they must have some consciousness then. And a dog and its master obviously have some communication with each other which is, if we give ourselves consciousness then we've got to give the dogs consciousness too, to some extent. But the importance of this component in the activity of our brains must be so much greater than the ideas in any other animal.

Bingham: And I'm thinking now that the title of Francis's book on consciousness was *The Astonishing Hypothesis*. And I think the subtitle was *Science in Search of the Soul* or *The Scientific Search for the Soul.* And yet, somewhere in the book, which is, would be called extremely reductionistic, I suppose, he basically says that you, your feelings, your desires, and so on and so forth, are nothing but a pack of neurons. Do you agree with that?

Barlow: Is this, are you asking me for a value judgment, or, I don't think –

Bingham: Well –

Barlow: I think it's true but I don't think it's a very good summary of the brain, to say that it's a pack of neurons.

Bingham: Okay, well, where I was going to go with this, and we will elaborate this into your own work, and the notion of the neuron doctrine, the neuron, the idea that neurons in fact do, we will now say that there is neural correlates. I was trying to, I would like to have a discussion with you since this is a great deal of the work that you have done over fifty years of the study of vision and perception, about where knowledge resides and how we store it. And specifically, the cover of *Discover* magazine for June 2009, in other words, next month's cover, has a storyline here that says, "Can a single neuron recognize Jennifer Aniston?"

So in other words, in here, Carl Zimmer, the science writer, has written a couple of pages discussing something we will elaborate on, grandmother cells, the possibility of grandmother cells, the possibility of there being cells specified to recognize only individuals like Jennifer Aniston in this particular case, and in certain situations. And this is based on a long review paper that just came out in the *Psychological Review.* Now, how knowledge is stored, whether it's localized in individual neurons or as most constructivist people would think in terms of parallel processing and so on, are very different views. And perhaps these are being put at the extremes here on the spectrum, but this is a, I was surprised to see this because this was a story that I thought was over and done a long time ago, and the verdict was in terms of parallel processing and networks and so on, but the idea of a neuron actually as being smart enough to know whether you're looking at Halle Berry or Jennifer Aniston couldn't possibly be the case.

Now this trip takes us straight back to your early work, your career work on discussing where knowledge is stored and the whole concept of grandmother cells. I wonder if you could elaborate on that, bring it up to date, give me the history and the context.

Barlow: Yes, well, historically the ability to record from single neurons in the periphery, that only became possible with the use of valve-amplifiers and things like that in, roughly speaking, the post-First World War, when people started using amplifiers to pick up action potentials and they could record the action potentials of single nerve fibers in the periphery. That was the work of the school largely originating from E.D. Adrian in Cambridge but of course it spread and people sought or investigated all senses, I think with the exception of olfaction. And we were able to record sensory impulses from these single fibers. Then, at about the time of the First World War, people started being able to record from single ganglion cells in the retina. Now the retina is an outbranch from the brain, it is really part of the brain, so at this point one was able to record the action potentials of single nerve fibers inside the brain. This work was carried out in many countries, many places, many parts of the brain, but it was interrupted by the Second World War. It was really only after the Second World War that people could do it, and the earliest that people could record from neurons in the cerebral cortex was about 1950 or 1960. So it was really quite a slow development.

Bingham: There is a group then, also, working at MIT as well, Jerome Lettvin, who wrote a classic paper "What the Frog's Eye Tells the Frog's Brain", right? But you were also working on frog retina?

Barlow: Yes, well, I was working quite a long time before that, actually, 1949 and 1950 and up to 1953. And the main gist of that work on single neurons was to show that they were much more powerful and reliable elements than had previously been supposed. Working with my colleagues I discovered that the, you could recall from the ganglion cells in the cat retina, for example, and it shows that they responded to the absorption of single quanta of light. One quantum of light caused not just one impulse, but in many cases a whole cluster of impulses, so that by recording from one nerve fiber you could detect when a threshold of stimulus arrived in the, for the cat. And this was a great surprise to a lot of people who had only supposed that the well-known high sensitivity of the eye, for example, resulted from the fact that it was interpreting a message along a very large number of nerve fibers. So in the case of the human eyes, about a million nerve fibers.

But it turns out that the element of sensitivity, so to speak, single impulse, was of the same order of magnitude as the minimum amount that you could detect. And many, many other examples of this in touch and in other fields showed that the single neurons in the brain were significant elements, and you didn't have to have millions and millions of them in parallel to get any sensations. Well that was all right up to a point, and it was a surprise to find elements which are quite selective in their responses. For example, in the cortex itself, you had units that you could hardly, it was very difficult to excite at all, until you had found out what we call the trigger feature, the element in the sensory stimulus that actually causes it to respond. That was Hubel and Wiesel's discovery, to find out that this was depending critically on the orientation of that stimulus and until you got an oriented stimulus you couldn't get these cells to respond.

Very frustrating, because you'd gone through the process, quite elaborate process, of putting an electrode, recording from a single neuron, and you know you've got that because every now and then it gives a spontaneous action potential which you recognize as an action potential, so you know you're recording from the neuron. But nothing you can do, wave your hands, flashing lights on the whole eye, it pays no attention. And then in that case we have a very good, David Hubel gave a very vivid description of how they discovered accidentally. They did all sorts of sensible things to increase their chances of getting a response, but then, quite accidentally, they had little images on the slide and they pushed the slide into their projector, and by chance the edge of the slide was at the correct orientation to stimulate the cell they were recording from. And whereas what they wanted to stimulate was the little spot of light, but they couldn't get by moving the spot everywhere they couldn't get any responses.

But accidentally, they stimulated it with the edge. And of course, they then very rapidly found that if the edge was wrong, they knew that, say, receptive field projected out to this region here and by accident they got the right orientation and said, "Oh, right, that was what was wrong," and when the shadow of the right orientation created by the glass edge of the slide fell in the receptive field they were able to show that it also had to be the right orientation. And from that point on, of course, easy going. Well, twenty years of easy going.

Bingham: Again, one of the great serendipity stories, of course, but how do we move from that, this whole notion of how knowledge is stored, and why am I back with the cover of *Discover* magazine having apparently a debate that I thought was over? So, the whole notion of the grandmother cell was something that Lettvin used in a kind of pejorative sense, right? Because he didn't believe that at all, it was originally Sherrington who talked about, in almost ecclesiastical language he talked about pontifical cells, right?

Barlow: Well, I think that Jerome Lettvin did believe, he wrote me a letter. People often accuse me of having invented the grandmother cell, and I didn't, so, and I knew that of course, so I searched around to find who did. And John Muller, my colleague, suggested to me that I should write to Jerome Lettvin and find out, and I did. He wrote me back a long letter, which was an admirable sample of Jerry Lettvin's mode of thinking, because it was a long story about a Soviet neurophysiologist who believed that there were such things as mother cells and so on and so forth. And then he got into political trouble, so he had to change it to grandmother cells. But anyways, he got things right, because he pointed out that it's not just the selectivity of these neurons which is remarkable, it's the fact that they respond to the same selective feature in spite of changing circumstances of a very big range. You change the angle at which you're looking at grandmother or Jennifer Aniston or whatever it may be, and it still responds the same. Even the written name excites it. So here you are having something of a much higher level than just the physical stimulus, which is still selective for the features which enables it to respond to the grandmother or whatever it may be. And it also has a full range of variances which enables it to go on responding in spite of your changing the point of view, the clothing, everything except the essence of grandmother.

Bingham: Well, where is the essence, where is the notion of grandmother, say, or Jennifer Aniston contained? Is that not contained in networks of permutations?

Barlow: Oh yes.

Bingham: How do we have this –

Barlow: We don't have grandmother cells in the retina, we have far central nerves, that cell is connected through a network to the retina. So there's a network there alright, but the final representation is not necessarily in just one example of grandmother cell, because if that was the case you wouldn't really have any chance of, for example the case of Christof Koch and his colleagues when they were finding these remarkable cells in humans awaiting operations. You wouldn't happen to have a photograph of Jennifer Aniston, and have a chance of hitting a Jennifer Aniston neuron. Either there's a large number or there's something there must be something there that we don't yet understand.

Bingham: Yes, because these were patients who were having depth electrodes implanted in an effort to cure some intractable epilepsy. And so they were able to explore, as it were, the neural terrain to find out which bits appeared to be the neural correlates of outside percepts.

Barlow: Yes, that was the, but their recording was such that could only record from something like, I don't know what the number was, but ten neurons at a time at the most. And if there are ten to the ten neurons in the brain and you are recording from ten of them, and just one of the neurons in the brain responds to it, you're not going to be able to find it. There's a mystery there which we do not yet understand.

Bingham: To make a slight leap, one of the other issues that comes up, and when we're talking about the brain and the organization of knowledge, is whether we construct representational networks or whether we have inherited, to some extent, modules, or whether there is some territory in between. How much of the brain is modular, in a sense? And there do seem, some people do argue that there do seem to be some areas dedicated, to face recognition for example. But the question again becomes are those areas which become face dedicated areas, do they result during development or are they there as it were in the genome? Do you have any thoughts on that?

Barlow: Well, yes, another person who was responsible for thoughts along these lines was Konoski, a Polish, a student of Pavlov's actually, who is best known for his work on conditioned reflexes, but he was also a clinician, very interested in aphasias, well, people's disability to speak, to recognize words and to speak and to use these things. He became aware, as I think many other clinicians did, that there were often very highly specific aphasias, people could not recognize and talk about animals, for example, or moving or small or manmade objects, these are two categories I happen to remember. But, in a very large number of these patients you come to recognize that the specific agnosias were quite common and often for the same classes of object in different people. And that must surely be determined by some genetic predisposition for there to be such classes. Which by no means excludes the possibility that they also learn a lot, it's also an acquired characteristic that depends upon the dearth of things that the person has spoken about and of the words we have to recognize these things.

Bingham: Konoski, I think, called them Gnostic cells.

Barlow: That's correct. What he in fact called them, first of all, Gnostic regions, and within them, as soon as he heard about Huber and Wiesel's results he talked about Gnostic neurons, and the idea of these being formed by a category of, or hierarchy of operations.

Bingham: The reason I pursue this particular line is that one is struck by the, especially as consciousness becomes a subject that is more respectable and so on, and as we develop technologies such as functional magnetic resonance imaging. There is a burgeoning industry now in finding neural correlates for behaviors, like empathy and so on and so forth. And there have also been some recent papers published which have criticized what appears, to some people at least, to be a kind of neo-phrenology, where the statistical methods are not strong enough to support the claims that are being made for the use of the magnet. Is this, do you think, just one of those things as we go ahead in progress that the results will get better, or is there a genuine sense that the technology is being asked to deliver and carry too much freight in terms of understanding what we can about the human brain and the mind?

Barlow: Well, it's a wonderful technique, the fMRI imaging, but a lot of the early work was in fact not repeatable. But that didn't stop it getting a lot of publicity. So, I'm always skeptical when I hear about these results, but most of them have, I think, been verified, but there are certain technical things here which worry me very much. The main one is that it depends upon detecting changes in blood flow, but if you measure changes in blood flow accurately in the periphery you'll find that the blood flow through a muscle, for example, depends very much not just on whether it's being exercised, but on whether you expect it to be exercised. So if you have a person with two arms in this crude method of measuring blood flow in muscles and they may be asked to exercise the left hand or the right hand, and if you devise the situation so that you expect them, the person is expecting it to occur in the left arm, then that's where the blood flow starts increasing first. You get a massive increase in the blood flow in the muscles of that arm even before it starts working. So this same effect presumably must occur in the control of blood flow in the brain.

It's always interpreted as being a direct response to the activity in that particular region, but that's a very insecure assumption. It could well be that the rest of the brain says, "We know that something is going to happen, is going to cause activity in that region – we better put some more blood there". So that it's the expectation of activity, not the activity itself that is causing the change. And this kind of thing is not adequately controlled, except in very, very few studies. And in many of those studies it shows what I was saying, that there's evidence that the blood flow response is due to expectation, not what actually happens.

Bingham: If you were explaining to, do you have grandchildren or something?

Barlow: Yes.

Bingham: What makes them tick, and why they think the way they possibly do, and so on. Do you stress the neuron doctrine in the sense that, the extreme statement of it that is "you're nothing but a pack of neurons"?

Barlow: No, I would stress strongly that the brain is more than just a pack of, a bunch of neurons. First of all, it's a very highly organized bunch of neurons, not just randomly connected with each other, there's an enormous amount of structure there. Just look at Kahal's pictures, more than a hundred years old, showing incredible complexity and regularity of connections in these things. Now, that can't be there just for nothing. All that organized structure must be doing something. So I think that the idea that it's a disorganized mess that somehow manages to organize itself won't get you very far.

Bingham: You mentioned, we talked about family there, I know that you are, although this is the year of Darwin, but Darwin's bicentennial, and you've been roped into a great deal of stuff about this and you're a bit Darwined out, but I think we should at least mention the fact that you are one of Darwin's great-grandchildren.

Barlow: That's right, yes.

Bingham: And this is worth dwelling on. Because if one looks at the family tree and so on, it's an extraordinary, distinguished, and widespread family, starting with the Darwins and the Wedgwoods. You would be the sixth generation if you count-

Barlow: Going back to Josiah.

Bingham: Josiah Wedgwood and Erasmus Darwin, who were, obviously, they were living in the mid late-eighteenth century and were members of a group called the Lunar Society, which was a group of businessmen and scientists who got together to meet on a regular basis. It included Matthew Boulton, Priestley, and so on. So there was this great community that built up, and the Darwins and the Wedgwoods there were also intermarriages with the Huxleys at some point, your own father was Sir Alan Barlow and your mother is Nora Darwin who became Nora Barlow. She, in fact, edited or re-edited the autobiography of Charles Darwin, right?

Barlow: Yes, she was one of the earliest people to put Charles Darwin really on a pedestal and say, "Look, what he did was remarkable and has had remarkable effects". She was also a geneticist herself. She didn't have a university education, she worked with William Batesman, who was a geneticist, has her name actually on some quite important papers I believe. But she took it upon herself to try and collect together as many Darwiniana as she could. She eventually succeeded in persuading the university library to act as a repository of this. So, she's in some sense the founder of the Darwin industry because the university library now has hordes of people coming and looking at this material every year.

Bingham: She actually lived to 104 as I recall.

Barlow: That's right.

Bingham: So she had a long time putting the business together.

Barlow: So I've also been hearing about Darwin all my life. She was a good botanist and good naturalist and she would always recognize birds quicker than anybody else. So she taught me a lot of, and she also was very encouraging, you know, about ones powers of observation and asking Darwin what that means and so on and so forth. So she was very, she did that on top of having six children and running an enormous house with many frequent visitors there.

Bingham: So, did you always have the sense that you would be some sort of scientist? When you were growing up with this, because there were huge numbers of the Galtons as well that were intermarried and so on, there's an awful lot of people in the extended family and the intermarried family who are extremely well known scientists in their own right.

Barlow: Yes, yes. Well, I always wanted to be a scientist, because it seemed to me that, I suppose just as doctors' children tend to regard doctors as being the highest form of creation, we were brought up to believe, at least my mother's side encouraged the belief that being a scientist was the highest form of life. My father's side wasn't quite like that. My grandfather was a very successful Victorian physician, so I actually did medicine too, as well. That's partly because I didn't seem to be, well, there was a great demand for doctors at the beginning of the Second World War and it seemed like a good thing to do. But I found I preferred the scientific aspects of medicine to the actual clinical aspects. So, I went into that, that's when I came back to Cambridge and worked with E.D. Adrian as his student.

Bingham: Now, if one goes to Google and put in "Neurotree" and then somebody's name you can find these sort of genealogical tables, as you know, of scientists, of neuroscientists in this particular instance, and who their fathers were, who their children are. You are situated in the midst of a galaxy of extremely well known neuroscience names. Lord Adrian, of course, was one of your mentors. Sherrington is somewhere off in the corner, and he ultimately gets to T.H. Huxley. J.Z. Young is somewhere in the ferment. And you had students that included Pettigrew, who then-

Barlow: Colin Blakemore.

Bingham: Blakemore; Pettigrew then became the teacher for Ramachandran. So there's this amazing community of scholars which I think is one of the things that Francis Bacon was talking about when he talked in *The New Atlantis* about Solomon's house, this whole notion of communities of scholars. Can you think of any other industry or any other business where there are all these connections of people who are-

Barlow: Well, banking for example.

Bingham: Banking.

Barlow: Well, I suppose in, I mean in the old days, you can tell from peoples' names, you know, they must have been a father-son passing on the trade, as well as the name.

Bingham: Is there another business you might have ever gone into instead of science? Have you thought about it?

Barlow: Well, I remember my father at one point explaining that his side of the family was connected with the cotton industry and that they were always looking for family members who would take on the responsibility, and so on and so forth. But this wouldn't be an easy job; that you would have to start on the factory floor and so on and work your way up. And I think, without exception, we all turned down this offer. But other family members did go in, did follow that course. Not in my branch of the family, but in our relations.

Bingham: You grew up in Cambridge.

Barlow: My grandparents lived in Cambridge, my Darwin grandparents. So we visited here frequently, but I didn't come here and live here until, in Cambridge, until I came up as an undergraduate.

Bingham: Okay. Apart from being this 200th anniversary of the Darwin anniversary, later in this year, of course, in the 150th anniversary of the publication of *The Origin*. But there's also an anniversary just about now, which is the 50th anniversary of C.P. Snow's "The Two Cultures" lecture that he gave in Cambridge, and there's been quite a bit of press about that. Do you have any thoughts about this, the sciences and the arts?

One of the reasons I ask, by the way, just to interpolate this is that I also read in the current newspapers that there is a race on for the Chair of Poetry at Oxford and one of the leading contenders is Ruth Padel, who would be, by my reckoning, your niece. A niece of yours?

Barlow: That is correct, yes.

Bingham: So she's part of the whole Darwin industry as well, business, descent. And, in fact, her latest book of poems is a cycle of poems about Darwin, Darwin's life. So there's this admixture of arts and sciences, I thought perhaps-

Barlow: Well, my two parents – one was definitely on the arts side, my father read classics but then became a civil servant so, but he's always been very interested in origin of words and language and things like that, whereas my mother was a scientist through and through. So I was always exposed to the opposition of these two views and the collaboration, too. And I, indeed, a good scientist is as much an artist as he is a, well, what corresponds to the popular view of a scientist, who is dispassionate and objective and unconcerned with the emotional aspects of what work he is doing. That's not true of any good scientist that I've ever known. They're always passionately involved in their work, and emotionally involved as well.

Bingham: Who is the most outstanding scientist in your mind? Who do you recall as having been the smartest, or the wisest? Those are not the same things, but –

41:35 Barlow: Well, the person who has obviously had the biggest effect on the state of science, is Francis Crick, I mean there's no doubt. When I look back at the people I was taught by in Cambridge, I mean it's Adrian, the elder [unintelligible] and the younger, and there was Alan Hodgkin and Andrew Huxley and the person who I worked quite closely with, William Rushton. I mean, they're all quite exceptional people, and I suppose it would be possible to rank all of them in order of importance, but it's, they're all so exceptional that I'm reluctant to do so. And they excelled each other in different respects. I mean, the smartest of the lot is Andrew Huxley in many ways, who having solved the problem of nervous conduction with Alan Hodgkin then went on and made very major contributions in solving the problem of muscle contractions. He was absolute with objects, and I remember putting a problem to him in objects. It was in one of those Christmas cards that comes around from the publishers of *Perception*, and –

Bingham: *Perception* the journal?

Barlow: Yes, *Perception* the journal, yes. And it beat me, and it beat John Robson, and nobody else could explain it to me, but I showed it to Andrew and he was very puzzled by it to begin with. He said, "Well, can I take it away?" The next day I got a single page of hand written explanations saying exactly how it worked. And he invented, or co-invented and developed the interference microscope. So he's really very good at that. And in designing mechanical devices he had a microtome that he used with electron microscopes which he developed.

Bingham: The journal *Perception*, which you mentioned, which is one of the primary journals in your field, obviously, started in 1972 by Richard Gregory

Barlow: Yes, that's correct.

Bingham: Who, I think, is still the editor-in-chief.

Barlow: Yes.

Bingham: And, indeed, has a paper with Stuart Anstis in the, almost the last most recent journal, copy of the journal. Is this something about being petitioned, it seems to be there was a huge interest in visual perception and particularly illusions, visual illusions, which has now spread over here of course.

Barlow: Yes, well, the country which is equally distinguished in vision and hearing is Holland. They've always had groups of people working on these problems, who attained quite distinction. I think also there are lots here.

Bingham: Yes. This same issue of *Discover* magazine happens to also have in it a nice piece, an ad from the National Science Foundation saying the evolution of evolution, telling you how you can read some more stuff about Darwin everywhere, but there's also on the following page a two page spread here by the John Templeton foundation. And it's an advertisement, and it says it's celebrating the bicentennial of the birth of Charles Darwin, and it asks the question "Does evolution explain human nature?" And then there's a series of responses from various people, including the primatologist Frans de Waal who says, obviously says, "The monkey". There's a comment from Joan Roughgarden who is Professor of Biology at Stanford, who says, "Not yet, and almost surely never". There is a comment from Lynn Margulis, there's several of these remarks. Two things: where do you stand on these kind of issues; and plainly the intersection of religion and science was an issue in Darwin's day, it appears to be one of the reasons that he took so long to publish the *Origin*, the fact that his wife was so religious and so on, and he didn't wish to harm her feelings or indeed almost anybody else in those days. This issue is still with us. We have done meetings here, the Beyond Belief meetings and so on, do you have a comment on these sorts of issues?

Barlow: Well, of course, Dawkins is the most extreme example, and though I find myself agreeing with both his factual and his emotional arguments, he's tinged obviously with very deep disrespect for religion. But it's clear that religion was not only responsible for transmission of knowledge on which science is based through the ages, but also for encouraging its, the development of new knowledge. All the academic establishments in the older universities are strongly connected with religion. So, it seems to be foolish to say that it's, even if you only believe in the merit nowadays of the scientific side of an argument, just for historical reasons one has got to give some respect to the religious tradition. So I don't think one should be too rude to them, except when they are rude to us! I appreciate the fact that Dawkins gives as good as he gets, and usually seems to come out on the winning side of the argument, in my way of thinking, but I just feel that you don't win arguments by being rude, but hopefully by the actually merit of the argument itself. So one should concentrate on that.

Bingham: There have been some issues, obviously, as you know, you're member of the Royal Society, Fellow of the Royal Society. The Royal Society has had issues internally about whether it should accept grants from bodies which have religious connections, and so on and so forth.

Barlow: We should do without them, much better.

Bingham: Let me go back to this notion of reductionism. Although to explain is not to explain away, there is nevertheless a general feeling that we are being reduced to just material goods. One hears from certain people, there is V.S. Ramachandran, in his book *Phantoms in the Brain,* has a footnote at the back where he had a little story in which, let me just remind you of it, he says here, "This misapplication of a reductionism leads us to a perverse and tenacious belief that somehow reductionism itself will tell us how the brain works, when what is really needed are attempts to bridge different levels of discourse. The Cambridge physiologist Horace Barlow recently pointed out at the scientific meeting that we have spent five decades studying the cerebral cortex in excruciating detail, but we still don't have the foggiest idea of how it works or what it does. He shocked the audience by suggesting that we are all like asexual martians visiting earth who spend fifty years examining the detailed cellular mechanisms and biochemistry of the testicles without knowing anything at all about sex."

Barlow: I can't recall saying that, but I think it's very true when he says there are different levels and there are different levels when we examine things scientifically. In vision, which is my own specialty, one of the levels of examining it is to look at the responses of human observes to carefully controlled physical stimuli, the so-called psychophysical approach. Many of the most important things about vision were discovered using that technique. For example, the extreme sensitivity of the eye to light. People didn't realize that it got down to the quantum level until they started looking, until they started showing that human observers could detect very small packets of quanta entering their eyes, and many people did certain calculations showing how it was lost in the different stages in the media and only a fraction of it was absorbed. It turned out that only perhaps a dozen or so quanta are absorbed, and that was proved purely by psychophysical methods.

And of course you want to supplement that, which has now been done, by recording from different elements in the pathway. So this is a case where the whole problem was stated by the, was settled on the map so to speak by the psychophysical approach. But also, you need it carried out also with the reductionist approach and find out, well, how do the single cells respond and so on. So, even in a narrow field like that there are many different levels and you can examine it scientifically, and that's what I think we should do. The reductionist approach, when you can get down to the, well, for example in the case of light you can examine the methods whereby the quantum of light absorbed in a single photoreceptor causes changes in that, which eventually result in an impulse being transmitted back to the brain. But somehow, those details, intricate details, are fascinating in the photoreception itself, but they don't have any general implications, whereas the way that you can detect a single quantum does, because that leads you to the question of single ratios and statistical efficiency of the brain.

It's been fashionable in many parts of psychology to try this and say that we're hopelessly inefficient at doing statistical judgments. But that's not true! You give the brain the right statistical judgments to make decisions about, it turns out to be highly efficient. Which means that in crude terms if you were to take an exact record of what went into the eye and pass it to a statistician, or take two records and ask if they belong to the same population or different populations, and bring forth statistical methods to bear on this and use optimal procedure for deciding such questions, the human can do very nearly as well. It can't, of course, do better, that would be impossible, but it does seem to make use of all, almost all, of the evidence that is available, provided you ask it the right question.

So we've got a very efficient statistical machine up here, not a crude or a poor one. And that's a new reach, by looking at a problem from the more global approach, not from the, not in the first place from the reductionist approach. So you need to, all levels, you can and then the evolutionary approach, which as you were mentioning, that's yet another level. Well, how did this amazing equipment between our ears evolve? And of course it did evolve, we have it, we know that. But we don't know, we can't follow the individual steps, how each particular mutation was favored or not favored, we are a long way away from being able to do that. But how does belief in God help one to have the kind of equipment we do actually turn out to have?

That's a valid question. I don't know what the answer would be, but one of the answers must be that it's a means of ensuring the preservation and propagation of knowledge, that you build up both a literature, so to speak, and a verbal tradition in constancy so on with traditions and ceremonies which somehow establish a mode of behavior within a community, or tend to establish it. That has beneficial evolutionary results. Though I'm neither an expert in understanding these things on a social level, but I think one can see there's a lot of truth in the idea that religious beliefs in a community do serve a function of that sort, of making it more homogenous and more mutual comprehension between people. This is why family businesses exist, for example, because the shared family connection means an increase of trust among members. And similarly belonging to the same religious community has that affect, you end up with social communities in which people can cooperate more effectively.

So there are so many things that we don't understand in any reductionist detail and yet one can see that there is maybe an evolutionary advantage to it. Though I tend to be, I'm unradical and a unconservative and most details I think there are good reasons to be somewhat conservative about, throwing away all aspects of religious belief, and I don't know that we should necessarily get a better world by driving religion out.

Bingham: One of the, this is on roughly the same point, one of the things that I was going to ask you about was the way in which science is perceived by the other members of the public who aren't scientists. And I was noticing in a couple of journals and in a couple of newspapers recently, yesterday for example a new study has shown that the melting of the ice-caps or something will produce a rise in sea levels which is only half of what the preceding report had said. So, and I could cite dozens of examples like this, and it does seem to me that one of the things that a general public seems to like is certitude, so they can, it feels as though it's getting the goods on something, it can enact appropriately in a certain situation. Whereas, science, as we've just been saying, is not in the belief business, it's in the doubt business.

Barlow: Yes, absolutely, yes.

Bingham: And there seems to be this disconnect between the two, I don't know if you…

Barlow: Well, people, students too, prefer certainty. If you give a lecture which expresses doubts about certain conclusions your students will hate you!

Bingham: They want to pass the exam.

Barlow: They want to be given certain, simple facts, you know, and they don't want to have, be instilled with doubt about these things, which makes it much more difficult for them to write and to absorb new facts and so on. They've got a point. They've got to absorb a lot of knowledge, and they want to absorb the certain knowledge first. I mean, their bias in favor of certainty is justified. And perhaps one should not lecture firstly to students saying, instructing them to doubt every statement they hear, or encouraging them to doubt.

Bingham: That then goes back to Huxley, of course, doesn't it?

Barlow: Yes.

Bingham: Stand before fact like a child. On that point, that was written as a response to a letter from Kingsley, after Huxley's son had died. And Kingsley was inviting him to take refuge, if you like, or comfort from a religious perspective, and this is the famous letter in which Thomas Henry Huxley wrote back and made his theoretical stand about being an agnostic and how fact was important and so on and so forth.

Barlow: I've never read that.

Bingham: It's one little letter. But, that entire generation, we tend to forget that your great-great-grandfather, Charles Darwin and Emma Wedgwood, his wife, how many children these Victorians had. These were enormous families. And there were, routinely two or three children would die. Randall Keynes has talked about the effect on Darwin, Charles Darwin, of Annie's death, his daughter Annie at the age of, what, was it ten or something, it was devastating. So, these issues obviously were very much the flaw even as you have somebody there who is figuring out the mechanisms of how all this works. So tell me, it is remarkable, to be a member of that family must just be somewhat interesting.

Barlow: Yes, well sometimes a bit intimidating, too. Well, I mean, one of the things about Charles Darwin is that unlike many scientific and other heroes, he seems to have been a very amiable, straightforward gentleman, and also much loved by all his children, but that's true of almost everybody. If not by their children, but even very much loved and admired by almost everybody who knew him I think. And, the way he dealt with Wallace for example, it's very easy to make that into a, it would have been very easy to make that into a story that Wallace really discovered it and Darwin merely pitched his idea and gave it publicity, which is of course untrue – he didn't give it any publicity at all. Wallace came along, but then he actually handled it a bit in the details, sort of honorably and well it seems to me. So it's difficult to make a real villain out of him, which is unusual.

Bingham: Who would you, is there anybody who you would have liked to have had dinner with and had a conversation with, historically or even now? Who would it be?

Barlow: Well, his grandfather, Erasmus Darwin, was obviously a very fascinating person. It'd be nice to meet everyone in the family, and Galton, too. But I've had my fair share of conversations with quite a lot of distinguished people, so I don't know whether I would very much like to know Mozart, for example. I don't think I would enjoy Beethoven's company so much. But other great artists would have been interesting to know, seen, got to know them a bit more.

Bingham: Is music a major factor in your life, or

Barlow: Well, yes, I enjoy it very much, yes. But I don't think many other people enjoy my music.

Bingham: Oh, you play?

Barlow: Sorry?

Bingham: You play?

Barlow: I do play, yes. But not at a performance standard, should we say.

Bingham: Which instrument?

Barlow: I play various instruments, at various times, I tend to take one out and as soon as I find out I can't play it very well I give it up. It doesn't work.

Bingham: So, what would you say is the biggest mistake you've ever made, and what did you learn from it?

Barlow: Well, I always regret very much that I wasn't a more meticulous observer, I mean more meticulous recorder of what I was doing, because obviously Charles Darwin himself was, and many of my contemporaries keep meticulous notes and so on of what they are doing at any given time, and why they are doing it. Speculations and so on. And that would have been a very good habit to have acquired, I wish I had, someone had drilled that into me when I was at university. But as far as actual errors, so to speak, quite a lot of my publications have things in them which I would not, I would now regard as wrong. I recall one particular thing which I remember I was, when I was working on the frog's retina, and one of the sort of lessons from that was to suggest that a frog, the story of "What the Frog's Eye Tells the Frog's Brain", but this was actually done several years before that.

It was becoming clear that many of the things that we used to regard as going on in quite high centers of the brain were actually going on in the retina, like the surround arrangement and so on. And one of these I came across was, the fact I came across from behavioral studies was that the backward to forward motion in the visual field is much more effective in eliciting nystagmus than the forward to backward motion. So I thought, well it would be interesting to see if this exists in the retina. So I made some observations of this and the results came out negative. It seems that one's recording, one's responding selectively to motion in one direction were as frequent as one's selectivity to motion in the opposite direction. So I never published anything on it.

But of course, the actual effect that in the retina there was directional selectivity that the neurons responded better to motion in one direction than the other was staring me in the face, but I didn't see how important it was, as an example of a kind of mechanism which is much more elaborate than anything that people had, up to that point, expected to be going on in the retina. This dawned on me suddenly, actually by a conversation with Keffer Hartline outside of the swimming pool. I went and worked with Keffer in America in the fifties, this was before the publication of Jerry Lettvin's article, and he said, what do you think of these observations about things that happened in the frog's retina that Jerry had been talking, he must have talked to him about it at a small meeting or something like that. And the example that Keffer Hartline gave was, he says that some of the neurons respond to motion in one direction and not the other. And I suddenly realized that this was observations that I myself made and I knew to be true.

But I was so embarrassed I don't think I brought it up to enlist the significance of the error. So I don't think I brought it up with Keffer Hartline, actually. But that was a blunder, an omission of the first order, and an interesting one, because it shows how theory can mislead. I mean, I had a theory I was testing, and it put me completely off track. Making pertinent observations is our first job, you know, and I'd failed to record this observation which was really quite important, because my eyes were diverted to another possibility. Though I have always been rather in favor of theories as opposed, you've got to make guesses! If you don't make guesses you won't get anywhere, but you've also got to record the unexpected things you find.

Bingham: And be especially critical of your failed hypothesis.

Barlow: Yes, yes.

Bingham: And as Popper would say, you can't just falsify it.

Barlow: Yes, yes absolutely.

Bingham: You have to go and prepare for your lecture now, but let me ask you one last question here. What are you optimistic about?

Barlow: Well, I'm usually optimistic about my own latest hypothesis, so to speak. I'm optimistic about that, but we don't have time to go into it here. Well, I'm optimistic, actually, about the current state of America really. Obama seems to be a new phenomenon, you know. Let's hope it all goes well. But I'm not so optimistic about the state of Britain. But I'm also optimistic about the state of science in America. It seems to be, in the areas that I know about, in a flourishing state. Whereas neither is it well supported in England, nor is neuroscience recruiting such good people, and so on, so I'm a bit depressed about that.

Bingham: But you think here there's a-

Barlow: Yes, yes.

Bingham: I think I mentioned to you that we're planning to do a meeting on the subject, using the old Bacon phrase about "the Merchants of Light", so perhaps there's more light on the horizon here.

Barlow: Yes, I think there is.

Bingham: Horace Barlow, nice to see you. Thank you very much.