A Conversation Between Harold Barlow and Alec Cullen, on November 24, 1983

Cullen: How did you first join the academic staff at University College?

Barlow: Well, I started life in industry with my father, and I was working in his business (he was an electrical contractor) when I received a letter one day out of the blue from Ambrose Fleming offering me a post as an Assistant Lecturer in the design of electrical machinery. This seemed a good opportunity for me at that time to get into research, which I had always been interested in, and after talking to my father I decided to take up the post. I have never looked back; I would not have wanted to change that decision as it gave me opportunities I should not have had otherwise. When I started, I had never contemplated the possibility of going into University teaching, it just was not one of the things that I thought I could possibly do, but there it was, the opportunity came. Fleming made me a Research Assistant in the Department working for him on some of his early ideas, mostly using thermionic valves with different applications. I remember very well working on a variety of valves, some of which were called French valves; they were like the ordinary light bulb in structure but with a filament and a plate, and a grid inside, and these were quite useful in various applications that he was interested in. We also had valves that were made by the Marconi Company and these (V.24's) were little tubular structures with the grid and anode connections taken out throughout the cylindrical surface of the glass so as to reduce the length of the wire connected to them. One of the serious problems of the early days of the thermionic valve application was coupling between grid and anode circuits setting up oscillations. Everybody was trying to reduce that to a minimum and make it possible to use the valve as an amplifier without, at the same time, oscillating. It intrigues me to see how the new developments in micro-circuitry chips have come along, and this of course enables one to get over all the unwanted coupling problems in one go. It is so easy now that everything is on the chip, but the things we learned, for example, controlled feedback, from those early difficulties were quite important.

C: One thing I remember seeing was some early microwave experiments that Fleming did with spark gaps. Were you involved in those at all?

B: Yes, I used to set these up for Fleming to show at various public lectures that he gave, and it was really a repetition of Hertz's work on the spheres with the spark gap and the oscillations that these produced when using the spheres as a sort of capacitance of the circuit; and of course they did generate exceedingly high frequencies.

C: There is one illustration I remember in, I think, one of Fleming's books—it had a little thing that looked like a waveguide, a rectangular tube with a spark gap inside it.

B: Yes, it was not the sort of waveguide that we think of today in microwaves but it was a form of discharge that we used to set up oscillations, and we found it convenient to enclose it in a sort of rectangular guide. But of course in those days the generation of high-frequency power was mostly done by Poulsen arcs, and I recall that when I was in the Navy at Portsmouth we had a Poulsen arc setup at Horsey Island, at the back of Portsmouth Harbor, which was generating at 15 kHz or thereabouts and putting about 100 A into the aerial from this Poulsen arc.

Sometimes in the laboratories at Portsmouth we used to have enormous sparks jumping across from one metal conductor to another due to the radiation from Horsey Island! Our work was directed mainly towards receiving wireless signals underwater, and I was engaged on this as my main job. I used to go out in submarines; mounted on the conning tower of the submarines were two teak boxes that were about a meter square and about 2 in thick, containing coils of wire (filled up with pitch), and these were erected at right angles on the two sides of the conning tower. We received morse signals at 15 kHz, which was roughly the frequency, and this could be done underwater when the submarine dived. On a number of occasions we used to get signals from Horsey Island down to about 20 ft under the water. I think that these were the very earliest experiments that were done on underwater signaling by radiation at 15 kHz.

C: You could not go very deep I suppose without losing the signals?

B: No, the field attenuated very rapidly as we went down, and I recall that we used to go down until we could not see through the periscope. At small depths when the boat was just submerged you

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could see the water surrounding the submarine and any nearby ships, but we used to dive below the depth of the periscope and still get signals from the station at Horsey Island.

C: I was just thinking further ahead during the 1939–1945 war years; that was, I suppose, when you had a renewed interest in microwaves.

B: The Second World War brought me into touch once again with microwaves; the work on radar particularly, and of course on the centimeter point-to-point links. The No. 10 set used by the army in France and on the Continent was an enormous asset, I believe, at the time, and of course it was in many ways the prelude to the microwave point-to-point developments all over the world.

C: But you always had a feeling, it seems to me, that one should not use radio if one can use cable. Is that right?

B: That is quite right. I have always felt that we have unfortunately relied too heavily on ether waves and not sufficiently on guided waves such as cables and waveguides. Of course, as time has gone on, the ether has become so congested that many services that would be valuable to us today—between mobile units, trains, ships, motor cars, and all the rest of it—cannot be properly accommodated with the frequencies available. I have always said that, if possible, one ought to distribute between fixed stations, that is, transmitter and receiver, by some guided-wave system, and this applies of course to broadcasting. I remember trying to persuade the BBC on many occasions not to use so many radio waves, but it was an uphill task and the principal difficulty was that it generally cost more to run cables than it did to employ radio.

C: The optical-fiber business may be providing a part of the answer.

B: I think the optical-fiber business will, as you say, rectify this situation to a large extent. I think if you look back in the URSI records you will find that at one of the Commission VI meetings we had a discussion about the problem of trying to conserve radio waves for purposes for which they must necessarily be used. I drafted some recommendations which were subsequently accepted by URSI. This has always been one of the things that I have felt we have not done very well in this country. We now have our satellites which are of course valuable, but the U.K. is a closely knit community where cables and guided-wave systems can be employed very efficiently and effectively. In a vast country like Canada or India you must have a satellite for broadcast distribution, but in the U.K. I do not think it is so important, excepting of course the European satellite programs. One has to take into consideration the particular conditions and circumstances of the country concerned.

C: Your interest in the long-distance waveguide must have been based on these sorts of ideas.

B: Certainly, yes. When the war came to an end many of us who had been in Government Research and Development Establishments turned our attention to ways and means of applying to civil use some of the developments that had taken place during the war in military applications. One of the things that I was always interested in was communicating from one place to another, and particularly in the use of a waveguide of some kind to do this. The obvious guided-wave system was the metal tube carrying a TE_{01} mode with its unique characteristic of falling attenuation with increase of frequency, and that is what I put forward.

C: Even though it did not in the end get used in this country, you were saying the other day that a lot of the ideas that were developed and the techniques that were developed were used for optical fiber work later.

B: Yes. That I think is true. A lot of the work that was done on the tubular waveguide was an enormous help in the subsequent development of optical fibers, and many of the techniques are very similar, though the frequencies are different. Much of the basic work is very much the same and one development helped the other.

C: But of course it is a big stimulus to millimeter-wave techniques, which are used now for a lot of other things.

B: Yes, that is right. One of the things that I feel we have to do in optical-fiber work is to utilize the spectrum capability more effectively. It is very, very, inefficient as it is now. As is well known, there are two important windows, one at about 1.3 μm and the other at about 1.55. These are on opposite sides of an absorption band. Both have been applied, but I do feel that the good old tubular waveguide utilized its spectrum much more effectively than the optical fiber!

C: Yes, I am sure that is right. Brian Davies said to me the other day that he had calculated for our M.Sc. course that if an optical fiber utilized its spectrum efficiently there would be enough capacity for half the world to talk to the other half!

B: Because we have so much capacity readily available, I am afraid we are not using it as effectively as we could, and we do need ideas as to how this might be done.

C: But the surface-wave business, that is another link with optical fibers, because there was a lot of surface-wave work done at UCL. How did that really begin? How did your interest in surface waves originate?

B: Well I met George Goubau during the war. He came over for some special discussions and he had been working on his single-wire transmission line at the time, which was the copper wire coated with dielectric or corrugated. It was really quite remarkable how well you could guide waves by this arrangement. The point I had not realized at that time, which carried so much importance for communications engineers, was the fact that any discontinuity or bend in the surface waveguide caused radiation, and likewise made it susceptible to interference. This simple fact really prevented the single-wire transmission line from being taken up as a major telecommunications link. The telecommunications engineers insist that the circuit must be completely screened, and of course the various types of single-wire transmission line were set up in the United States using single-wire transmission lines and they operated for a great many years, quite successfully, but they never really developed commercially, and the company that George Goubau set up to make applications and to develop practical systems unfortunately went into liquidation. He was a man who had enormous insight into physical behavior, as well as being able to appreciate to the full the basic concepts. I had a great admiration for him and I thought he was a great man.

C: It was here at UCL that I first met him.

B: Yes, he came to give a course of Advanced Lectures. It was his thinking that started us on surface waves, and the waveguide we used to call a G string. I never really understood whether G was for Goubau? I suppose it was. It was certainly the Goubau stimulus that started it all, and then of course we were fortunate in recruiting you and Tony Karbowiak to the research that followed. You both did a great deal of valuable work on surface waves.

C: We used to have furious arguments about the theory! He's a marvellous chap.

B: Yes, Karbowiak had very strong views and it wasn't easy to get him to see other approaches to the problem differing from his own. But he was always worth listening to.

C: Another area of work was the Hall effect. How did this start?

B: What interested me was that the Hall effect is essentially a multiplier effect, while the Poynting vector represents multiplication of electric- and magnetic-field components. It seemed to me that the Hall effect was an ideal way of measuring power if you could get a sufficient output to operate an indicating instrument. This we tried very hard to do, but I'm afraid it did not succeed completely because, although in principle it was right, every Hall plate has to have attached to it metal conductors which set up rectifier action, and in most cases the rectifier action was larger than the Hall effect. So, although it worked in principle, it was not really a practical development.

C: But it certainly gave rise to a number of papers you published, one or two with Lawrence Stephenson, which demonstrate it *could* be made to work.

B: Yes, it certainly worked when special arrangements were made. There was no question about that, but the output one could reasonably get from some of the semiconductors available giving quite large Hall effects wasn't really sufficient to overcome the disturbance. That was a shame.

C: Do you think that modern techniques with semiconductors may be able to rectify this?

B: Well, since those days, I believe that means have been found to reduce rectifier action very effectively, and it is possible that some of the diffusion techniques could make the device successful. It would be interesting to see if that were possible and so provide for a practical power measuring instrument.

C: Certainly there have been a lot of advances in measuring extremely small voltages too.

Your current interests are really in surface waves in the form of optical fibers, as well as low-loss cables. Is that still true?

B: Yes, the interests I've had recently assume first of all that, in spite of the optical fiber development, coaxial cables will still be required. The TEM coaxial cable, in large sizes at any rate, has an inner whose diameter screens quite a large cross section of the cable as a whole. It seemed to me, therefore, that by using parallel wires in place of the solid inner conductor, and operating in the dipole mode you could bring into use the cross section of the inner structure. This might be expected to reduce power densities and so possibly to give lower attenuation in the dipole mode. When this cable was set up we were encouraged to find that, with certain structures, we could get attenuations which were little more than half that of the corresponding attenuation for the ordinary TEM coaxial cable.

Experimental laboratory-fabricated lengths of cable were very encouraging, and after some discussion we persuaded a well-known company in Japan to make this multiconductor cable. It was based on one of their commercial productions, employing ordinary TEM coax but with the significant difference that the inner, originally a tube of copper, was replaced by a multiconductor structure giving an anisotropic surface impedance.

This structure is inductive and resistive to axial current and capacitive and resistive to the circumferential current as required for the dipole mode. An ordinary dielectric rod has the same kind of surface impedance when supporting a dipole mode, so it seemed a good way to make a cable in coaxial form, transmitting in the dipole mode instead of the TEM mode, and at the same time utilizing for some of the power transmitted the cross section area of the inner of the multiconductor structure. We are still trying to complete the measurements on this cable and if it should prove as successful as we hope, the company might make it commercially in due course.

A student in the Department is working on the project. A particular problem is that every high-frequency supply is in TEM coax, so that we have to transform to the dipole mode in order to examine the operation of the cable, and this requires a transducer. Thus, we are applying a dielectric rod, initially excited in the TE_{11} mode while contained in a circular metal tube filled with the dielectric rod. Then, by gradually removing the outer tube, the wave goes over to a dipole mode surface wave on the dielectric rod. The dipole mode surface wave is then injected into the cable giving a convenient flexible arrangement which can be rotated to alter the polarization.

The transducer operates in the 3-GHz region, and some reduction of loss in the cable is expected to come from improved power-density distribution in the dielectric over the cross section.

Another particular interest I have at present is in the guiding properties of optical fibers. A year or two ago I proposed the use of a higher refractive index tubular structure supporting an HE_{11} mode, instead of the usual core-type of fiber in the same mode. The tubular fiber comprises an inner core of lower refractive index covered by a thin layer of rather higher refractive index and then cladded by a medium of slightly lower refractive index. Such a structure will support an HE_{11} mode, as well as TE_{01}, TM_{01}, etc., but by reducing the thickness of the wall of the tube all modes except the HE_{11} can be cut off, giving a single-mode fiber. Tubular fibers can easily be 100 μm or more overall diameter, easing the problem of joints and coupling into the fiber.

I am also interested in the power transmitted along conventional core-type optical fibers. Calculations have previously been made by Gloge of the Bell Telephone Co., and by Unger in his well-known book. I have been looking more closely at power behavior as cutoff is approached. Apparently, power is transmitted right up to the cutoff point, both within the core and the cladding, but a very large proportion of the total power is transferred from the core to the cladding at near cutoff. In this vicinity, the decay of field in the cladding becomes very small, so that the field carries right on at high strength to the outside of the cladding. In effect, what you get in these circumstances is a fiber that transmits in the HE_{11} mode, using both core and cladding together as the supporting medium, while the air outside provides for the evanescent field. To overcome such behavior, the addition of a second cladding with a rather higher refractive index should be effective. A summary of this work will be published shortly in the *Journal of the Institute of Physics D*.

Postscript

At the end of the discussion, Harold Barlow said that the thing he most wanted to say was how fortunate he felt he had been in his colleagues. He mentioned—off the cuff—those whose names appear below. We, for our part, thinking of ourselves as a small sample of hundreds of people who are similarly indebted, feel how fortunate we have been to have had the opportunity of working under his leadership and learning from him how to approach new problems, and above all for the inspiration of his unquenchable enthusiasm for microwaves.

Eric Ash Alec Cullen Ian Stephenson
John Brown Geoff Sims

A Short Biography of Harold Barlow

Harold Barlow (SM'54–F'56–LF'78) was born in Islington, London, in November 1899. His father, and his father's older brother, were professional electrical engineers, the younger brother being a chemist. All three were educated at City and Guilds Technical College in London. Harold's father had always envisaged him joining him in his business, Barlow and Young Ltd., and from an early age he encouraged proficiency in the use of tools. On leaving school, Harold was awarded a Mitchell Scholarship and entered City and Guilds College, Finsbury, in October 1915, to study electrical engineering. He was then only 15 years old, the average student at entry being aged 16 or 17. The Head of the Electrical Engineering Department was Professor Sylvanus P. Thompson, FRS. Both he and his successor, Professor W. H. Eccles, FRS, had a great influence on Harold. Soon after Harold graduated, the First World War broke out.

He then joined the RNVR as a subaltern attached to the Signal School, Portsmouth, and worked on the problem of communicating with submarines underwater using radio waves of around 15-kHz frequency. During the war years, Harold mixed with a number of recruits from other universities, and then realized how little he really knew about electrical science. He, therefore, entered University College London for the engineering degree course and graduated with first class honors in 1920.

In the succeeding three years, he worked at University College London as Research Assistant to Professor Ambrose Fleming, then Head of the Electrical Engineering Department and renowned as inventor of the thermionic valve. He carried out research on the application of these valves, particularly assessing the merits of amplitude and frequency modulation techniques, and was awarded the Ph.D. degree in 1923. He then felt in duty bound to join his father in Barlow and Young Ltd. He was not entirely happy feeling that he had abandoned many of his real scientific concerns, and after a year or two, out of the blue, came an invitation from Sir Ambrose Fleming to a post at University College London as an Assistant Lecturer in the design of electrical machines. He decided to accept, and now looks back on that decision to enter academic work as the most crucial in his career and one which he has never regretted. The teaching work at the College was very demanding, and, with a very small staff, Harold had very little time for research, though he had published eleven papers up to the outbreak of the Second World War.

In August 1939, he was one of a small group assembled at the White Hart in Halesworth, Suffolk, to hear about the secrets of radar and to take part in the daily operation of a CH Station not far away on the Suffolk Coast. After several subsequent moves, he eventually arrived at the Radio Department of the Royal Aircraft Establishment in Farnborough, and apart from temporary breaks, he remained at RAE until 1945, eventually becoming Superintendent of the Radio Department.

After the war, he returned to University College London having been appointed Professor. (I had been a member of his staff at RAE Farnborough from 1940 and was fortunate enough to follow him to University College London a year later.) On his arrival, Harold was determined to create at University College a center for microwave engineering. His knowledge of microwave techniques, acquired during the war years, he now proposed to apply to civil rather than military ends, and his first publication after the war was entitled "The Exploitation of Microwaves for Trunk Waveguide Multi-Channel Communications." This paper has become a classic, and the ideas it contains form the basis of much of his work and the work of his associates up to the time at which it became clear that optical fibers would provide a more economical solution. Harold Barlow has always had a strong belief in the interplay between research and teaching, and it is significant that his very next paper described an adjustable skeleton waveguide model to demonstrate the synthesis of a waveguide mode of propagation in terms of transmission-line theory. This, together with his excellent short book on microwaves, was an immense help to those who had mastered transmission-line theory but found it difficult to reconcile the familiar idea of propagation of waves on a two conductor system with a propagation of waves in a hollow metal tube.

Harold's particular research interests have mostly concerned waveguides of various kinds, but especially the circular electric mode for long-distance transmission, and surface waveguides, including the Goubau line. He also initiated a series of researches into the high-frequency Hall effect applied to microwave power measurement. A major part of Harold Barlow's work on the circular electric mode concerned the problem of mode conversion at bends, and various ingenious schemes were put forward by him to help solve this problem. He did work on the helical waveguide, but latterly was a strong advocate of the dielectric-lined waveguide, and with a small group of researchers carried out extensive theoretical and experimental investigations on the effect of imperfections of various kinds, including mechanical tolerances on mode-conversion in the straight portions of the waveguide run. With support from the British Post Office, he established a small laboratory at Martlesham Heath in advance of the creation of the British Telecom Research Centre there, and his work contributed significantly to the total British Telecom effort at that time.

Harold has always been on the lookout for new applications for physical phenomena, and his Hall effect wattmeter developed for a whole range of frequencies from 50 Hz to microwaves is a good example of this. He also developed a Hall-effect microwave mixer, again exploiting the multiplication phenomenon intrinsic to the Hall effect. Another characteristic of Harold Barlow's work is his interest in taking devices or phenomena well known in a certain frequency range and then applying it to an entirely different frequency range. A good example of this is his development of a microwave electrostatic wattmeter. Together with a number of collaborators, he carried out extensive researches on surface-wave structures beginning with the Goubau line, referred to earlier, in the form of a dielectric coated wire, but later going on to study the properties of a corrugated rod. This led him on to a series of fascinating theoretical and experimental studies, in which surface-wave concepts were combined with hollow waveguide concepts in an endeavour to obtain structures exhibiting lower loss and other desirable properties. This has been a major part of his research in recent years, coupled with a renewed interest in surface waves on dielectric materials, particularly in relation to optical fiber transmission.

Harold Barlow still has a room in his old Department at the College and is still active in research, especially in the two last-mentioned areas. His interests, however, are as wide as ever, and he will always find time to talk to any colleague or research student who has something interesting to say or to show and who can always be assured of enthusiastic encouragement and invaluable advice. No one can have benefitted more in this way than the author of this brief and, I am afraid, inadequate biography.

Alec Cullen (M'56–SM'60–F'67) was born in London, England, in 1920. He was educated at Lincoln School and Imperial College of Science and Technology.

On graduating, he went to the Royal Aircraft Establishment, Farnborough, where he worked on radar during the war years. In 1946, he took up a Lectureship in the Department of Electrical Engineering at University College London, where he worked with Professor H. M. Barlow in building up microwave research in that Department. In 1955, he was appointed to the Chair of Electrical Engineering at the University of Sheffield. In 1967, he returned to University College to succeed Professor Barlow in the Pender Chair of Electrical Engineering.

Professor Cullen was appointed OBE in 1960. He was elected a Fellow of the Royal Society in 1977

from The Inventor and the Pilot Russell and Sigurd Varian

DOROTHY VARIAN

from Chapter Twelve The Research Laboratory

SIG WAS INTENSELY interested in ways to make flying safer and talked about the problems they encountered with inadequate instruments. The pilots lacked instruments that could locate mountains hidden by clouds where Mexican maps indicated swamps, detect planes approaching in overcast or at night, or guide a plane to a safe landing when visibility was obscured. These and many other navigation aids were badly needed. Since this was a subject Sig knew well, he could foresee a promising future for them if they were able to develop instruments of this kind.

He spoke often of the vulnerability of the Panama Canal to enemy attack. He was sure he could fly over a city or a military target at night or in heavy overcast without being detected by any defense system then in use, drop his bombs, and get away unscathed, and if he could, so could any other competent pilot.

Russell began thinking about how such planes might be detected. He knew what would be required: radio waves, which were able to penetrate clouds. They would have to be very short waves (now called microwaves) in order to locate the plane with any precision, using equipment of a reasonable size. To do this, the radio waves would have to be many times higher in frequency than the highest obtainable from any practical tube then in existence. The problem was how to generate these short radio waves and obtain substantial power.

At the time, Russell knew nothing about the research on pulsed radar then being carried on in secrecy by the military. He began to visualize a system that amounted to an outline of what was later known as Doppler radar. Such a system would need a practical source of short waves. He knew that the generation of short waves by conventional means was limited by the difficulty of building suitable resonant circuits attached to conventional tubes and that at the shorter wavelengths the efficiency of the resonant circuits was very low. He concluded that if practical requirements for generating microwave power were to be met, a new type of resonator would be needed.

There was another problem, one that both he and Bill Hansen overlooked at first, which was the limitation imposed on the generation of high-frequency power by the transit or flight time of electrons across the electron tube. At that time it was considered necessary that the flight time be small compared with the period of a single cycle of the radio frequency wave. The result was that electron tubes became smaller and smaller as the intended frequency increased. Because of this design limitation and the low efficiency of resonant circuits at these high frequencies, it appeared to be impossible to generate a sufficient amount of power to be useful in most applications.

Although the klystron's roots went back much farther, as has been mentioned earlier, the klystron project itself had its beginning with a letter from Bill Hansen in early February 1936. When Russell left Stanford in September, he and Bill had been discussing the concentric line hollow

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