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Bullard, Edward Crisp 1907-1980

Edward Crisp Bullard Oral History, 1973.

Physical Description: 1 audiocassette tape; 1 transcript

Description: This collection consists of an audiocassette tape and a transcript which records a one hour interview conducted with Sir Edward Crisp Bullard on August 5, 1973 in La Jolla, California about the history of the heat flow probe, an instrument designed and used by Bullard to make measurements of heat flow through the ocean floor. Bullard built the instrument and took the measurements while at the Scripps Institution of Oceanography. The interview was conducted by Scripps Institution of Oceanography archivist Elizabeth Noble Shor and her husband oceanographer George G. Shor. Mrs. Shor transcribed the interview and published it in: Elizabeth N. Shor, "E.C. Bullard's First Heat Probe," EOS Transactions of the American Geophysical Union 65 (February 28, 1984), 73-74.

Edward Crisp Bullard was a British geophysicist born in Norwick, England September 21, 1907 educated at Clare College, Cambridge University. Bullard spent most of his career at the Department of Geodesy and Geophysics at Madingley Rise in Cambridge, named Bullard Laboratories in his honor, and as director of the National Physical Laboratory. In 1949, Bullard visited the Scripps Institution of Oceanography where he designed, built, tested and used the heat probe. From 1966 until his death he had an appointment as professor at the Scripps Institution of Oceanography, University of California, San Diego. He died in La Jolla, California April 3, 1980.

SIO MC42

Memo for the Bullard Control File
September 28, 1983

Elizabeth Noble Shor prepared this exact transcript of her 1973 interview with Sir Edward Bullard in August, 1983 and gave this copy to the Archives on September 27, 1983.

Questions and comments in brackets were made by Mrs. Shor.

D. Day
Archivist

I first became interested in heat flow in the early 1930s. I'll send you a copy of a paper that describes some of the background of the early work on land. It was the British association which is like your triple AS -- had a committee on the subject, with Harold Jeffreys on it. And, in the period just before the war I was developing methods of making heat-flow measurements on land, which had been done extraordinarily badly before, and hardly anything had been done since about 1880. There was a previous British association committee way back in the 1880s, and I have written a historical paper on this.

["Oh --"]

The historical paper is in Willie Lee's book -- Lee and Uyeda on heat flow. He'd asked me to write a summarizing chapter for the book, and I said I wouldn't do that -- because it's too dull a thing to do, and then he wrote to me and said he thought this was most unreasonable and improper of me, and he saw no reason why I shouldn't write a summarizing chapter. And so I wrote an historical chapter which was really a sort of joke chapter and was mostly about the study of heat flow in the seventeenth century.

["I see -- well, I would like to see it."]

But it does give the more recent stuff.

Well, then, shortly before the war I began to think about measuring heat flow at sea. And the war in '39 -- the war overtook the plan, and we didn't do anything about it. And immediately after . . .

["Had you developed an instrument at that time?"]

No, I had developed the ideas for the instrument but we had done no hardware development, but I knew what I wanted to do. And we had two schemes for doing it: one was the one we eventually adopted of putting

a spike in, and the other was the idea of putting a kind of mattress on the sea floor and leaving it there and measuring the temperature gradient through it. Then, after the war I couldn't do this because I couldn't get a ship. Discovery was being used for replenishing lighthouses, and there was no British oceanographic ship available immediately after the war. Then, I tried to get Maurice Ewing to cooperate. He said he couldn't spare any ship time for this purpose. He'd just gone to Lamont.

["His laboratory had just begun then, hadn't it?"]

Just begun then in Lamont, yes. I'd known him -- I'd been to sea with him before the war when he was at Lehigh, in connection with seismic work.

Well, then I went to Canada -- I took a job in Canada, going up there in March 1948. And in the summer of '48 I paid a brief visit to California -- it was my first visit to La Jolla.

["What brought you here?"]

Well, there was a -- Louis Slichter used to run a conference on geophysics every year. I don't know whether it still goes on. Probably not. Anyway, on one of the campuses there was a conference, and this time I think it was at either UCLA or Berkeley, I forget which.

["That was what brought George here . . ."]

I went to that, and Louis brought me down here afterwards, and I came -- I think this is right, and I came down here and met Roger Revelle and Walter Munk.

["Roger would have just returned from the Navy in 1948."]

That's right, yes.

["Spring of 1948 -- so that sounds like the right time."]

Yes, and my memory is that I went to a party at Roger's, and I met a lot of blondes. I remember Mary Flower -- do you remember her? She

was the wife of a man by the name of Russell.

["Oh, yes."]

And Martha Munk, and Nancy Van Dorn, and all those people. I remember La Jolla as being full of blondes.

I got on very well with Walter and Roger. They asked me to come back the next summer. I forget exactly when, but I got a letter inviting me to work for the summer down here, and I said there were several things I'd like to do, and one of them was the heat flow measurement. And I came down here -- I don't exactly remember when (I could probably find out) -- I would have thought it was probably June, July 1949. And I got here and it was suggested that I take on Art Maxwell, who was a graduate student. He was here with his first wife.

["Did he have a first wife?"]

Yes. He and his first wife were living here, in a little house somewhere, and he was a graduate student.

["I typed his thesis later."]

Well, anyway, I decided to do two things: to investigate the westward drift of the earth's magnetic field, which was a data analysis problem, and to try and build an apparatus for measuring heat flow.

Well, then I went to Roger and said we'd like some workshop time. And Roger said he was sorry, the workshop was completely taken up with urgent work, and it was impossible to give any workshop assistance.

["The Marine Life Research Program."]

Well, I don't know what it was. There were lots of other things going on too. The upshot was that there was no workshop assistance. So I decided that we'd build the thing ourselves. So I went around to the fellows ~~and I couldn't get any workshop assistance for~~

fellows and I hadn't got any workshop assistance -- couldn't we work in the workshop, Art Maxwell and myself? This was obviously a slightly embarrassing matter. But we went down to the old workshop in the old shed. Is it still there?

["Yes, just south of T-3."]

Yes, just south of the old aquarium building -- yes, it's the temporary building south of Scripps.

["I believe that's the shop area, or do you mean on the other side?"]

No, I mean right on the beach.

["South of where the Director's Office now is."]

Yes, that's right.

["The shop is still there."]

I got an office -- I shared an office with Walter Munk, over the old aquarium, over the old director's office in that -- whatever that building's called -- the original building of Scripps.

["Scripps Building."]

Yes.

["Where I'm working, where Hubbs is working now."]

Well, we settled down. This was a substantial job. We designed what -- it was the first time I think that anyone at Scripps had had a watertight equipment for use in the deep sea. I remember the expert advice from Isaacs and so on was very much against this. They wanted us to build an equipment that was full of oil -- the recorder, the electronics and everything immersed in oil. And I decided against this. I decided to build a watertight apparatus made watertight with O-rings.

["That was new, brand new."]

That was, I think, brand new. And we had some interesting experiences with it. First of all, we had great difficulty in getting sufficiently strong tubing -- alloy steel tubing seemed not to be available in sort of one-inch thicknesses six inches diameter. We eventually found that the people who made ball races . . .

["Made what?"]

Ball races. Ball bearings -- the races in which the balls run -- made them by slicing them off steel tubes, and we found a man -- I think it was SKS, one of the manufacturers of ball bearings and ball races, supplied us with a piece of tubing. And we got some end plates out of good alloy. The stuff was about six inches internal diameter and about 3/4 of an inch thick, something like that. And we made up things out of this, with O-rings. Of course, the O-ring had been introduced originally for aircraft-hydraulic systems during the war, and was then introduced into the uranium diffusion plant

["Oh."]

as a matter of making it pressure tight.

["I see."]

And I'd known of it from these sources. And we decided to use this. And it proved extremely effective. We went out and tested these containers, and we never had a leak. I've never had a leak from an O-ring container at sea.

["George has had success with O-rings on the --"]

Well, they're used in everything now of course. They're used in cameras and seismic equipment and all kinds of things.

[" -- the air gun."]

Yes, they're used for everything.

And we made a very clumsy apparatus for this purpose. And we decided to do something which I thought I understood rather than something I didn't. So we bought -- at least I got from the British Admiralty galvanometers, which were very robust and which could be used horizontally. And we fixed these and used photographic recording and thermocouples. So it was thoroughly old-fashioned equipment, no electronics, photographic recording, galvanometers, reversing switches for eliminating thermal emf's in the equipment.

And this equipment I afterwards used myself, so it is the equipment I describe in my first paper.

Well, we decided to allow the full pressure to come on the probe itself. So the probe itself was filled with oil, and the recorder was full of air. And we had pressure-tight connections between the two, and, sort of looking back, the whole technique of fitting the connections through -- I remember we had a good deal of trouble with getting the connections through, and eventually I decided there was too much advice in the workshop on this, and I took the end plates home. I was living in Cole's Book Shop.

["Oh."]

And I took this home, and heated it on a gas range and soldered the connections in -- little glass connectors with a little metal skirt, and soldered them in.

["Was Art Maxwell working closely on this?"]

He was working on this -- we were both working on the lathe making this thing. Of course, we had to make this photographic recorder -- you see, we had to make a photographic camera and all the associated supports and things.

Well, anyway, we made this apparatus in the course of about a couple of months. And we took it to sea. We had trouble in getting ship time.

What we wanted was a day or two of ship time to go out and test it, you see. We did all the testing. I remember the first testing we did, we built a sort of gallows down at Mission Bay, which was then a marsh of course -- and strung the apparatus up with the galvanometer and things in it and climbed up a ladder and cut the ropes and let it drop to see if it would go into the mud and see whether the shock would break the galvanometer and all this kind of thing.

["That's very soft mud, though."]

It's pretty soft mud. Well, it's the only mud we had. And the mud's pretty soft out in the San Diego Trough.

Anyway, we then tried without any galvanometers in to see if we could get it into the mud out in the trough off San Diego, and we got -- There was a tremendous amount of sort of testing to be done. It was an entirely new thing really.

["True, true."]

and we discovered the trouble that if you put a thing into the bottom and left it there you couldn't avoid pulling it and bending it, and we tried tubes of various lengths and various diameters, to see what we could do. And we more or less discovered what we could do and what we couldn't do. And finally we took it out for a run with the whole apparatus to try and measure the heat flow -- and we got no records at all.

["Everything worked?"]

Everything worked. Except we didn't get any records. And we spotted what it was. It was condensation on the mirrors of the galvanometer, which we'd not thought of before. So we put drying equipment in and got rid of this. But we never got any results that summer, and I went home. I was director of the National Physical Laboratory. I went home and had another similar apparatus built, and I got results with that.

["Did you mostly build that one yourself, that second one?"]

That was built of course by the NPL workshop. We had some of the best workshop facilities in the world.

["You directed other people to --"]

What I did was -- I was at home. I had superb facilities, and no question as to who was going to use them. You see, I was director of an operation spending many millions of dollars a year. And if I said do something they did it. And so I had proper working drawings made of the equipment and had it built in a proper workshop and so on. And we took it out and it worked every time.

["You tested it then off England?"]

Yes, I'd done all the testing here so I just made a new apparatus and took it out to sea and it worked. And we got our first heat-flow measurements essentially with the apparatus here.

Now that's all been published, so the dates on this are all known.

["Well, published where, Teddy?"]

I'll give you all the references. Our first paper was published in the Proceedings of the Royal Society. You know the two letters in Nature, do you? One from Roger and Maxwell and the other from me?

["No."]

Well, I'd better find these for you. Let me find these references and get you Xeroxes.

["I'd be most grateful. I don't need Xeroxes. All I need is the citation."]

Well, what happened was this. There was a little -- well, not exactly a misunderstanding -- but I understood that the group here and ourselves were going to publish a joint paper. And I was very busy and I held up publication of our work for some time, and eventually the paper -- the

Maxwell letter to Nature went in, and I put a letter in in the same article, in the same number.

["I see."]

We didn't publish a joint -- well, then afterwards we published a joint paper. There was a bit of a muddle about publication.

Do you know -- now I come to think of it, the business about taking the things home to make the go-through connections was at NPL -- it was the NPL workshop that was making a scene about this. I think we did it here the same way by soldering them through. The NPL was making a fuss about this.

["What's the NPL?"]

National Physical Laboratory. It's like the Bureau of Standards. And their workshop was making a scene about go-through connections -- pressure-tight connections in heavy plates, and I took it home and did it on my domestic gas-range. And I think this rather shook the place, to have the director take home the thing and actually do the soldering when they said they thought they couldn't do it. That was in England. It wasn't here. We did it here without any fuss. We did it ourselves in the workshop.

There was a curious incident. There was a certain feeling of holding back in the workshop, and we were told we must never go in the storeroom without asking the head workshop man.

["Here."]

Yes, and I went along with this. It was no trouble to me but there was a certain feeling of strain with the workshop manager -- who shortly afterwards was found to be stealing stuff from the workshop and selling it in San Diego.

["He would not let others come into --"]

He wouldn't let them go into the storeroom because he was stealing stuff out of the storeroom and selling it. He was dismissed eventually,

["I see."]

shortly after I left. And it was obviously the greatest embarrassment to him to have two members of the facilities -- one member of the staff and one graduate student -- every day in his workshop and working late at night and so on.

["Very inconvenient."]

Yes, we got keys to the shop and everything, and it was really to the last degree inconvenient to him.

["I shan't ask you his name."]

I've forgotten his name. He was very good to us and very nice to us. And eventually, actually, some of the heavy machining was done. I said to him one day, I said, "Look, do you want us doing the heavy machining? These are big lathes. We're not professional mechanics. We're turning considerable material, these alloy steels. Wouldn't you rather do this yourself?" And they did -- the actual outer container was machined by the workshop staff in the end. But we machined the rest of the stuff.

Now there is a complete description --. The apparatus we had here is essentially the same as the apparatus I built in England. And I have a very nice photograph of Art Maxwell on the first lowering, Art Maxwell standing there holding the thing over the side on the first lowering.

["I'd like very much to see that."]

I'll send it to you.

Well, now, so that's essentially the story. And then afterwards Art Maxwell, Roger and I wrote a summarizing article on the whole subject.

["After that Midpac --"]

Yes, it was about a year later, I suppose. It was published two

years later. It's a review article about heat flow.

[*That I should read. It occurs to me that there's probably a good summary too that I must have read years ago in Art Maxwell's thesis.*]

Probably is, yes. But I'm saying that the critical things to look at are those two letters in Nature, which are the first publications, then there is my paper in the Proceedings of the Royal Society, my first paper in the Proceedings of the Royal Society describing my own measurements, and there's our joint paper with Roger.