REMINISCENCES OF MY EARLY CAREER IN WAVES

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For my friend Touvia Miloh on his sixtieth birthday.

My first research project was on water waves, and it began in June 1944, nearly 60 years ago. Since then there have been big changes in our subject and in the practice of research, and it may be of interest to recall the situation as it was during my early career. Everything was on a small scale and involved few people. Engineers used the slide rule, the drawing-board, and model experiments using the towing tank. Applied mathematicians showed how solutions could in principle be calculated, but actual numerical calculations were very slow ; the most powerful aids were electromechanical calculating machines, or analogue calculators, or special finite-difference graphical methods like relaxation which do not work for waves. There was no reason for engineers and mathematicians to collaborate closely.

I had come to England as a refugee in 1937 from Germany. I spent three years (1941-43) at Cambridge University, reading mathematics and getting a bachelor's degree. (This was in accord with British war-time policy, young scientists were needed for the war effort.) In my last year my main interest was functions of a complex variable. Then at the end of 1943 I was told to join the Admiralty (British Navy Department) as a scientist in Teddington, West London.

The landings in Normandy took place in June 1944. Wave forecasts were made for these: allied troops had to land on beaches. These wave forecasts were accurate because the local waves were generated by the local weather; distant weather had little effect. For the landings in Japan, due in 1946, the waves on the beaches would be ocean swell generated by distant storms.

A new group of about 5 scientists and 5 technicians was formed in June 1944 to formulate rules for forecasting waves in Japan, and I was one of its members. We asked how we might discover these rules, we were told that Nobody Knows. The head of the group was Dr George Deacon, Fellow of the Royal Society, much too distinguished for this position. He put all of us (including himself) in one large room, so that we could talk freely. No ideas resulted. Some of us went to Cornwall, South West England, to observe waves on the only English Atlantic Coast, no ideas resulted. On our return to London I looked at Lamb's Hydrodynamics, the chapter on surface waves, and found the problem of Cauchy and Poisson (1815), I was the only member of the group who could understand the mathematics.

Cauchy and Poisson considered an instantaneous point disturbance on the free surface of water. Their theory tells us that a given period (say a 10-second wave) will travel out from the centre with the group velocity appropriate to 10-second waves (one-half of the wave velocity). We can therefore from our observations on the beach find the time and distance of the disturbance by tracing any given period backwards with its deep-water group velocity (known from theory). The propagation lines for all the periods meet at the instantaneous point disturbance at the appropriate time. A disturbance over a finite area and a finite time would similarly give rise on the beach to a spectrum changing with time. SO WE DECIDED TO STUDY WAVE PROPAGATION. We measured wave pressure at Lands End, taking records of 20-minute length every two hours. Norman Barber was able to show how the spectrum of a 20-minute record could be found in 20 minutes, so the work could be done in real time. When the spectrum was correlated by us with weather information we found that the Cauchy-Poisson hypothesis was confirmed. THE PROPAGATION PROBLEM WAS SOLVED. We could now hope to correlate the wave spectrum with the wind strength in the storm and we did this. However, the war was now over (1945). Nevertheless, our methods have become the basis of modern wave-forecasting. This was my first wave problem and is perhaps the most important problem in which I was ever involved.

What happened next ? Barber and I wrote a draft paper for publication. This was re-written by George Deacon in a brilliant manner. According to Admiralty rules he should have appeared as the sole author, but in fact he refused to have his name mentioned at all. Famous scientists visited us from all over the world, in 1946 and 1947. Deacon would explain our joint work to them, then he would say: " Of course, I was merely the administrator, the work was actually done by A,B,C,D, and E," and then he would mention all our names. Deacon later became the first director of the British National Institute of Oceanography. It remained his policy never to take any credit for the good work over which he presided. He was a great success.

Because of government policy I had to stay in the Admiralty until 1947. During this time I decided not to become an oceanographer, too little was then known about the ocean for the application of mathematics. There were however many interesting mathematical water-wave problems which I had heard about and which interested me. Here are some of them, described very briefly .

There was the work of W.R.Dean (a lecturer at Cambridge University) on the submerged circular cylinder subject to a normally incident wave. He found that the reflection coefficient (a function of two dimensionless parameters) was always zero. Surely there must be some mistake ? I used a different method and found that the result was correct. I also proved that there could not be a trapped mode for any values of the parameters, this was new mathematics.

For my next problem I tried to find the waves due to a heaving half-immersed circular cylinder without forward speed. There were no known results, but there were standard methods,(not known to me). I invented a new method, using wave-free potentials. The coefficients in the infinite system of equations were explicit and computable by the methods available in 1947 and were actually used to compute for the first time the virtual mass and damping as a function of frequency.

My work was noted by the Admiralty, and George Deacon was summoned by our chiefs in London. The Director of Research complained that he could not understand my mathematics. Deacon told them that my work was of fundamental importance, and his great prestige prevailed. When he returned to our lab, he told me that I was permitted to continue my work. He added: " I did not tell them that I also could not understand your mathematics." My methods also worked for heaving and rolling of cylinders of arbitrary section and were used soon after in calculating the heaving and pitching of ships (in Japan and in the Netherlands).

Later in 1947 I was appointed to a post-doctoral fellowship in applied mathematics at Manchester University (but still without a doctorate). I could have had a similar position in Cambridge, but I could see that Manchester was better. The Manchester department was at that time becoming the leading applied-mathematics group in England under Sydney Goldstein, and their work was mainly on supersonic aerodynamics.

I have no space here to describe my work in the 1940's and 1950's at length, it can be found in my Collected Papers. There was the heaving circular cylinder at high frequencies. The conventional theory breaks down at an infinite set of irregular short periods. I developed a theory which did not break down, the first time such a theory had been found for short waves in hydrodynamics, acoustics or optics (apart from a small number of explicit solutions) . I later extended this to acoustics and was followed by many others.

Then I went on to trapped modes. For a submerged circular cylinder and normal incidence I had shown that there were no trapped modes, but what about oblique waves ? There was a famous trapped mode, the Stokes Edge Wave on a sloping beach. Was this a property of the slope of the beach ? I tried a submerged circular cylinder and oblique waves and proved the existence of a trapped mode.

At this point (1950) Professor Sydney Goldstein decided to move to Israel, to the Haifa Technion. He invited me to come with him but I did not accept. I had lived in difficult conditions since 1933, and I wanted a quiet life. If I had accepted, probably I would have met Touvia earlier than I did, perhaps I would have lectured to him. In fact Sydney Goldstein did not stay long in Israel but went on to Harvard.

Instead I became a lecturer at Cambridge University. Cambridge did not provide me with an office in which to work. I had to give six lectures per week, and

I was expected to do research, but there was no professor to keep an eye on my work. There could be no promotion, there were a few endowed professorships but there were many University Lecturers who were mathematicians of world renown and never became professors.

Soon after my return to Cambridge I happened to meet G.I. Taylor, Yarrow Research Professor of the Royal Society (and one of the greatest scientists I have ever met). He had read my theoretical paper on trapped modes in oblique waves, and he insisted that I ought now to demonstrate these by experiment. I protested: where would I do this, I had no experimental facilities, I was no experimenter. He brushed this aside, I could informally join his small group. In the end I did experiments on sloping beaches with the essential help of other members of G.I. Taylor's highly talented group. I showed that Stokes's edge wave is the first member of a sequence; the number of trapped modes depends on the angle of the beach.

This was a strange and inspiring experience. G.I Taylor had no special interest in trapped modes, but he felt that he must contribute to my science education at some expense and inconvenience to himself, he provided everything, I did not need to apply for financial support.

I will only mention that I went on to work on short-wave asymptotics in acoustics, and on the Kelvin ship-wave pattern. This led to interesting developments on the asymptotics of integrals. In Cambridge I had a number of very talented students, among them Roger Thorne, Richard Holford, A.M.J.Davis.

In 1957 I spent a year at MIT, at the Hydro Lab in Civil Engineering, the invitation came from its director Arthur Ippen , a true scientist and engineer whom I came to admire greatly.(No British engineer would have asked me to spend a year in a British engineering department.) I was involved in experiments with highly talented students, and I gave graduate lectures and seminars on mathematical aspects. Among the graduate students (in naval architecture however) was Nick Newman. He obtained permission to spend one year with me in Cambridge. On my return to Cambridge there was Ernie Tuck from Australia who wished to work with me, and who followed me to Manchester after I had moved there in 1961. In Manchester the flow of talented graduate students continued, among them David Evans (and many others whom I do not name for lack of space). They changed the face of ship hydrodynamics, they inspired colleagues and students, as a result much of my own work has been overtaken. This makes me very happy.

Science now plays a much bigger role in the world, and the life of scientists has become more demanding and less agreeable. The computer has enlarged the role of mathematics, and many engineers are now excellent mathematicians. I was very lucky to have come into science when I did, and to have had such wonderful colleagues and students.