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B. L. TOZER (Basil)

[redacted] GROVE.

Interview 16th July 1981

T was in charge of EMR at Harwell (but responsible only for the physics and electrical engineering) in the early 1950s. Then in charge of procurement and organisation for Zeta. Then in charge of EMR at Culham. I spent precisely 3 hours with him, and he just talked.

T said that EMR pre-dated Harwell and started when Cockcroft sent messages back from Canada to get British industry started on this, that or the other. It was done by Wilson in London, under the auspices of the old tube alloys group. T's first main work (under Wilson in London) was to use DSIR money to equip British universities with particle accelerators. All this work was on a cost + £ basis, and the universities themselves were not equipped to handle it. There were 6 large contracts with the big electrical firms (M-V, EE, GEC etc). The emphasis then shifted to accelerators for Harwell and through the MRC for medical work in hospitals and research centres.

Other than the accelerator work, other large contracts included that with ICI, and a contract with GEC, dating from the tube alloys days, which gave Harwell access to their research labs for any work on a ~~cost+ basis~~ no profit basis. The difference between cost + and no profit was academic, as the overheads were fixed accordingly, and all the early contracts were essentially based on a gentleman's agreement as to an appropriate overhead rate. At this stage, according to T, things were expanding so fast that anything could be tried. And Cockcroft would support anything, so long as the chap was enthusiastic. A lot of work was started, and there were a lot of dead ends.

Another large field for EMR was instrumentation, again not only for Harwell but ~~the~~ for universities, hospitals, civil defence, etc. These were very extensive contracts, with the firms being encouraged to develop to a broad general specification and sell, Harwell taking a royalty. Other fields covered by large contracts from the pre-Harwell days were for electromagnetic separators, mass spectrometers, and centrifuges (work), the latter conducted on a suck-it-and-see basis and hidden ~~=~~ the GEC contract. This was ended, Kronberger being dissatisfied with it, just before news from Germany sparked off a renewed interest ~~=~~ the field. Fundamental

work on graphite constituted another large chunk of the GEC contract, and was very well done.

Another chunk of work, this time with the universities was on bacteriological research with a view to the possibility of using bacterial preferences to separate out heavy water. This was not successful. There was also liquid metal work at Cambridge University and at ~~[illegible]~~ BTH - EE. In 1954 a substantial contract was placed for a heat transfer in sodium rig into Foster-Wheeler, Bauer being the chief protagonist. It turned out however that Aldermaston were doing almost exactly the same thing (for Lithium) and that Capenhurst were also doing a lot of work of which Harwell had not been informed. Bauer left.

Since Harwell's requirements were very specialised the contracts did not often lead to large scale production. But Philips made some linear accelerators commercially, and they, Vickers, E.J. Cole & Co, and Mullard made instruments. Philips and Mullard posed a problem, since they were commercially inseparable, but Philips were not OK from the security angle.

I said that difficulties began to arise with the cost + contracts when Harwell became more knowledgeable than the firm involved. Arguments

arose as to the design, and the specification kept changing with the result that the contract would get quite out of control. This happened in the mid-1950s with accelerators. On the other hand fixed price contracts had their own disadvantages. The MRC neutron generator made by Philips was 1½ years late and performed well below specification, but they ended up paying for it in full

At this point I left EMR (temporarily) and turned to the fusion programme. His memories of its origins were hazy and inaccurate, but some ideas emerged. Thomson's patent application was for a device to make Photonium, and was passed on the Atomic Energy Office by the Patent Office. They said that Thomson could not take out a patent on this, and passed it to Tozer to sort out. I went to see Thomson with Cockcroft, and an agreement was reached on what he could and could not do at Imperial College – to make matters worse, one of his students working on it was a security risk.

Meanwhile Thoremann was also working on the idea and had gone to Harwell for help. Skinner, who would sign anything, gave him carte blanche to draw anything he liked from the stores and he took chairs and everything.

Thomson was asked to sign away the patent,

and refused, but suggested that if the work could not be done at Imperial it should be done under Allibone at AEI. For a couple of years the AEI work was supervised entirely by Thomson; then Fry insisted on proper control and it was taken over by Ware. But they ended up just bogging along behind Harwell. Craggs at Liverpool had also been involved in early fusion work, but dropped out when it became classified.

One of the biggest EMR contracts I handled was the proton linac. This stemmed from the fact that there was more expertise in the scientific divisions at Harwell than either EMR or industry, with the result that the specification was modified right up to the last minute. I said that Met-Vick were a very hierarchical organisation, in which each department operated under a chief engineer and sales director independently of the others. Big contracts would entail one department actually sub-contracting to the others with resulting chaos. Moreover this sub-contracting was not always on the same costing basis as the original Harwell contract. The whole AEI organisation (M-V, GEC, BTH etc) was similarly disjointed. GEC for example were unable to coordinate design and production, so that having made a prototype they could then - and only then - find that they could not produce it. English Electric were appalling, with their research department run by a benevolent dictator, J.K. Brown, and with a totally

feudal structure which prevented their good engineers from having any say in things. Electronics firms in general were problematic, so they couldn't keep their staff

T said that all firms took on development contracts in the hope of getting a production order. But the development always needed their best men and a very high proportion of their skills, while the requirements were very rarely duplicated, so that the production orders didn't often arise. The firms naturally got fed up with this, and by 1956-7 very few of them were prepared to accept development contracts. Even if a production order did ensue, the chances of early obsolence were high.

T said that there were some pressures on where to place work. Development areas were preferred, and defence requirements always had priority. At one stage the Admiralty even set up an Electronics Research and Development Contracts Coordination Committee to allocate priorities.

High vacuum equipment posed a problem as only one firm (Edwards) was any good at it, and they were getting less efficient as they grew from a family firm (Edwards even said grace at lunch) to a big one, without changing their organisation. Here as elsewhere the problems arose if the expertise being at Harwell and - the late 1950, Harwell were recruiting heavily from the firms with whom they

had been in contact, causing relations.

Back on fusion, T said that Zeta was one of the few successful Met-Vick contracts, and it was successful because it was conducted from Harwell, with men from Met-Vick seconded to join the design teams. T thought that classification, insisted on by the Americans, slowed down the fusion programme quite a bit, and that the effects of sudden declassification slowed it down even more. Talking of the plans for Zeta 2, T said that one problem on the agenda was to determine the project load that could be carried by wood or coal – such was the size of device anticipated.

T said that spectroscopy for the fusion programme was assisted by research contracts with Mullards, Ferranti and Met-Vick, as well as by a contract with Gabor at Imperial College, who extended the field into the ultra-violet and x-ray ranges wholly as a result of AEA support. There were other University success stories too, especially in the development of measurement techniques.

At Culham, T instituted the practice of attaching university staff and especially research students. This was one method of converting money into staff when the latter were limited, and it was often abused, leading to a lot of not-quite-

- relevant research as at Harwell in the Cockcroft days. It also allowed people who could not work for the AEA (mainly foreigners) to be used at Culham, or if the security people got awkward even at Oxford. In general he thinks it had a very beneficial effect, injecting new ideas and promoting close working contacts with the universities. Talking about security, I said that one of his jobs at Culham was to keep secrets out of what were unclassified laboratories. This mainly entailed making sure ex-AWRE people left everything behind them. Martelli he said was very naive. He was on a Euratom attachment to Culham and came fully equipped to handle any secret information he might find – but did not find any.

I said that when the Russians announced a very high temperature in a Tokamak, the Americans did not believe them, but Culham, with their exceptional knowledge of measurement techniques offered their services, went to Russia, and found an even higher temperature than had been claimed. I thought that had Culham gone ahead with ICSE they would have been able to convert to a Tokamak very quickly, and would not, in particular, have been held up as they were by electricity supplies.

Asked about the CTR advisory committee, T said that Schonland just forgot about it.

Back on ICSE, T said that he had been saddled with financial control of the project, when it suddenly became apparent that there was in fact no overall estimate in existence. Indeed such was the uncertainty with respect to each of the components that ~~they asked~~ their estimates had to be graded from A (fixed-price to P (no idea). Most were in the lower end of the scale. Even when the project was finally cancelled, many items had still not been properly costed, and the estimates that did exist were both vague and low.

Going back to accelerators, T told me that he had once worked out the ratios of initial approval to final cost and found values from 1:1 to 1:5, averaging 1:2·8.

At this point, T came to a temporary halt, and I was able to ask a question, to which he replied that before the Authority was set up Harwell was totally autonomous, and that everything that did go through the Ministry of Supply was simply rubber-stamped. He then worked his way round (how I did not note) to his experience

of the physics departments at the universities, and found that those into several professors and a rotating head of department (he cited Liverpool) were the most successful. He then went on to contrast the British and American practices - report of EMIR and the use of industry. The Americans would simply circulate all possible manufacturers and place half a dozen contracts in parallel, one or two of which would succeed. This was effective in stimulating industry, but rather expensive. Harwell would, at least after the early days, do basic research themselves. Only after some time would they think of finding a firm to make what was required, and they would then give a very tight specification as to materials, tolerances, safety, etc.

I did not recall any duplication of EMIR work by different AERE establishments, and he said that a list of contracts was exchanged. Nor was there any real problem of technical feedback between the establishments. There were internal problems though, and it was in particular difficult to stop scientists approaching firms direct.

Back on university research, I said that given the specialised equipment involved work on a given subject had to be concentrated. They therefore tried to build up centres of excellence and did this in many cases: liquid metal chemistry at Nottingham; spectroscopy at Imperial; lasers

at Imperial and Belfast, etc.

I finally brought T back to what he'd said about fixed-price contracts, and he said that the big trouble was that while the contracts and legal branches insisted on extremely tight wording of the Emp R contracts — which in itself put off firms from undertaking the work —, they always backed down when it came to a contractor not meeting the requirements: they would never press their legal claims. This, and the problem of clearing industrial staff for security, were his two big headaches.

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https://en.wikipedia.org/wiki/Peter_Thonemann
https://en.wikipedia.org/wiki/Thomas_Allibone