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Reminiscences of Operational Research in World War II by Some of its Practitioners

F. L. SAWYER, A. CHARLESBY, T. E. EASTERFIELD
and E. E. TREADWELL

This is a transcript of four talks given in two sessions at the OR Society Annual Conference 1987, at the University of Edinburgh, to recognize the 50th Anniversary of OR. Some slight editing has been carried out by Professor R. W. Shephard (Centre for Operational Research and Defence Analysis, Long Acre, London), but hopefully each speaker's style and enthusiasm has been preserved.

Key words: history of OR

Professor R. W. Shephard:

I was delighted when I was asked by the Operational Research Society to arrange this session to celebrate the 50th Anniversary of Operational Research, and I am very thankful to my four speakers, Frank Sawyer, Arthur Charlesby, Tom Easterfield and Ted Treadwell, for so readily accepting my invitation to speak to us about their experiences in early operational research. The only guidance I have given the speakers is that they should concentrate on things that happened to them first-hand and try to give us an idea of what it was like working in operational research in the early days.

The chronology of operational research during World War II is not very easy to unravel, and so I shall try when introducing each speaker to give you a thumbnail sketch of the origins of the organizations they joined so that you can appreciate more fully the roles they played.

The origins of operational research are closely entwined with the invention of radar, or radio direction finding (RDF) as it was known originally. In 1934, H. E. Wimperis, Director of Scientific Research at the Air Ministry, became aware of work that was being done by Robert Watson-Watt, Superintendent of the Radio Department at the National Physics Laboratory. Watson-Watt had been studying the properties of radio-wave propagation through the ionosphere. He had noticed that reflections of the radio waves took place sometimes, and thought these might be used to help in the detection of intruding aircraft at night. Wimperis was instrumental, in 1934, in getting the Air Ministry to set up the Committee for the Scientific Study of Air Defence to help develop the methods that might be used to defend against hostile aircraft. The Committee was chaired by Sir Henry Tizard, and included Professor E. Hill, Professor Blackett, H. E. Wimperis and A. P. Rowe among its members. The possibilities of using radio waves to direct energy in sufficient quantities to cause damage to the aircraft themselves were soon dismissed, but those of using Watson-Watt's discoveries to provide an early warning system were quickly endorsed. As a result, Watson-Watt and a small team of scientists were seconded to the Air Ministry and began work on RDF at Orfordness in Suffolk on a disused airfield in 1935. In the following year, Bawdsey Manor, about 10 miles south, was acquired, and the staff moved there and expanded in numbers.

Also in 1936, a team of RAF officers under B. G. Dickens was formed at Biggin Hill airfield to study how an RDF chain might be organized and used to help with the interception of aircraft attacking Great Britain. Their work, which took about 2 years to complete, resulted in the plotting and operations room technique which ultimately helped win the Battle of Britain.

The term operational research appears to have been originated in about 1938 by A. P. Rowe, who was then Deputy Superintendent of Brawdsey Research Station. He applied it to work carried out by teams from Bawdsey under E. C. Williams, who were examining the efficiency of the operations room technique just mentioned in Fighter Command during the summer exercises in 1938 and 1939. These teams were designated the Operational Research Section, Stanmore, at the beginning of 1939, and thus claimed an existence in their own right.

On the outbreak of war, two things happened. Bawdsey Research Station was renamed the Air Ministry Research Establishment and then, a year later, the Telecommunications Research Establishment (TRE) after various geographical removals. TRE has a history stretching to the present day, but carried out virtually no OR work after 1939. The second change on the outbreak of war was that the Operational Research Section, Stanmore, changed its name to the RDF Research Section and then, shortly afterwards, to the Stanmore Research Section. In June 1941, this was redesignated the Operational Research Section, Fighter Command.

Our first speaker, Frank Sawyer, started work in November 1939 at the Air Ministry Research Establishment, and later transferred to the Operational Research Section, Fighter Command.

Mr F. L. Sawyer:

Ladies and Gentlemen, I take it that I am here to answer questions along the lines of, 'What did you do in the war Grandad?'—an extremely difficult question to answer if you had my wartime history and if you were dealing with children. But I take it that you and I have a good deal of common ground and that I can, therefore, use language which I used quite naturally in the old days.

I think that, in order to enable you to understand how I saw things at the time, I must preface my remarks by saying something about myself. This carries no implication whatever that I did not have colleagues from whom I learnt, and leaders who guided me. I would like to pay my tribute now to all those who worked with me, and for whom I worked.

On 3 September 1939 (which I remember was quite an important date), I was almost 21 years old. The previous summer I had got a First in the Mechanical Sciences Tripos at Cambridge and was expecting to do not less than one year's academic research work on what then went under the happy name of 'wireless'. On 9 October 1939, I went before the University Joint Recruiting Board, and they put me down to be 'reserved for research—wireless'. At the beginning of November 1939, I started work at the Air Ministry Research Establishment at Dundee as an Assistant 3 on a salary of £214 per annum.

Dundee was where Bawdsey Research Station, the 'home of radar' as you have heard, flitted to on the outbreak of war. My first fortnight there was quite overwhelming. The science and technology of radar, which had been kept highly secret of course, was fascinating. Also I was glad to learn that such a thing as radar existed when a lot of people were expecting massive air raids to start immediately. But that fortnight I spent at Dundee were the only 2 weeks I spent there. The Superintendent, A. P. Rowe, had dreamed up the concept of what he called 'scientific observers' who would be attached one to each of the various operational radar stations. Their job was twofold: practical research work at the stations, and feedback of operational problems and experience to the establishment. I was to become one of these scientific observers, and after a month's training on a radar station in Lincolnshire under R. A. Smith of TRE (I remember the weather was particularly bad), I was left there on my own—a civilian amongst servicemen!

I moved to other stations and did other jobs, but they were always on the radar stations and never at the research establishment. Because of this, I was not surprised when one of the jobs I got was at the behest of what became the Operational Research Section, Fighter Command, and required me to investigate problems involved in tracking high densities of aircraft. The problem was that the stations were detecting large numbers of aircraft but were having difficulty in reporting more than a fraction of what they saw. My investigation was done at the radar station in the Thames estuary, from which we could see the German aircraft bombing the Army just before, and during, the evacuation of Dunkirk. I did help to work out some procedures which alleviated, but did not solve, the problem I was set.

As a bonus, someone reported me to the police as a suspicious character because I was a civilian who left the pub he was staying at in the evening and did not return until breakfast time the following morning. However, the CO of the station was good enough to bail me out!

Some 2 months later, at the beginning of August 1940, I was formally transferred to the ORS at Fighter Command. The reason for my transfer was as follows. The Battle of Britain was in progress, and one of the several components of our defences that was being tested to the full was Fighter Command's radar reporting system. This system used radar stations along all the eastern and half the southern coasts of the UK, feeding the information via a communications network into the underground filter room at RAF Fighter Command HQ at Stanmore. The output from the filter room was then fed to the various operations rooms, where appropriate reactions by fighters, Ack Ack and Civil Defence were planned and controlled. This reporting system was known to have faults, and thus the ORS were charged with undertaking a comprehensive investigation of it and making recommendations.

A team of three tackled the job. Eric Williams, the leader, was not only an old radar hand and fully experienced in the system as a whole, but he was also very well accepted by the Command's staff and, more important, knew his way around the Command structure. The second member of the team, L. G. H. Huxley, was responsible for studying enemy air activity and identifying its features and detecting how they changed. I came in as the junior member who was supposed to be knowledgeable on radar stations, and capable of making the observations necessary for our study.

Our investigation really was comprehensive, and we published a full report in November 1940. Basically, it was a study of an information-handling system, in which we identified weaknesses and mismatches, and suggested remedies. We called our report 'The Capacity of the RDF System'. One of the key sentences in our summary read like this, and I quote:

> 'This report presents the results of investigations to determine the measures that must be taken to effect improvement.'

Please note the words 'measures that *must* be taken'. Good resounding stuff. There were 15 such measures, relating to organization, equipment, methods of operation, and personnel. Looking back, I suppose we might be accused of arrogance in what we said. For example, one of the 15 measures we recommended was summarized just as, 'The proper selection, training and education of personnel'! Expanding on this in the body of the report, we stated and discussed six reasons why the average skill and ability of personnel were far below a standard which was both desirable and possible. As I say, good resounding stuff when you look back at it; and let me remind you that this related to just one of the measures that we recommended.

But the service took it all, and they went on to involve us heavily in follow-up action. There were meetings with the Command Staff, with the Radar Research Establishment, and with equipment manufacturers. There were countless individual discussions and exchanges of minutes. There were further operating experiments, and we helped to write operating procedures. We were directly involved in carrying out procedures for the 'proper selection, training and education of personnel'!

I enjoyed the education bit: my contribution consisted mainly of giving practical training at a radar station to WAAFs (Women's Auxiliary Air Force personnel) who were to become officers, supervising the operations at the main radar stations. They were great girls, from all walks of life, and their common attributes were intelligence and keenness. Don't misunderstand me when I say that it was a great experience for a lad of 22 to have eight such courses pass through his hands!

Up to this point, I, as a humble 'Indian' rather than a far-sighted 'chief', had taken my work as it came to me, because it obviously needed to be done. I had not thought how to describe the content in general terms, or what my role really was. But we were beginning to talk among ourselves about these things, and were on the look-out for words of wisdom from our leaders. I have always liked Watson-Watt's lighthearted definition of the OR worker as 'the court jester who is paid to be irreverent so long, and only so long, as he is wise'! You probably would not accept that definition now, and even less would you accept another saying of Watson-Watt's, which I liked because it related very directly to my area of work, which was heavily equipment-orientated. He likened OR to system engineering, where you had to determine 'what the user wants; what he ought to want; what he gets; what he ought to get; and what he ought to have and to do in order to get what he ought to want out of what he gets'! (Watson-Watt said that, I believe, at a meeting in 1942.)

We were clearly becoming a bit introspective at this time, and for some of us, myself included, the tendency was aggravated by a growing frustration. I seemed to be getting bits and pieces of jobs which appeared more like time-fillers than worthwhile pieces of research. I had, of course, been spoiled by the intensity and importance of our study of the radar reporting system, and perhaps there was some general feeling of let-down after the Battle of Britain. It might well have been different in other commands, and at other levels of work.

I wonder if there is any OR worker who has not just occasionally wondered whether he can justify his existence? When that doubt occurs to a young man in wartime, there is another factor.

You think of your contemporaries who are actually fighting. My closest friend from university days had read history. He was flying Blenheims, bombing by daylight. Not quite kamikaze stuff, but not far from it. When we met, I never knew what rank he was going to be; he oscillated between Flight Lieutenant and Wing Commander, according to the balance between casualties and replacements in his squadron. He was killed in action in 1943. It was quite impossible for me to be sure that *I* was doing the right thing.

Anyway, I suspect that our chiefs, Harold Larnder and Eric Williams, must have got pretty fed up with their underlings' restlessness. We lectured them, we even wrote the odd memo, pressing for more work of greater significance to the war effort. I am fairly sure they felt much the same as we did, and were seeking ways of reviving our effectiveness and, with it, our morale. I do not know whether our pressure helped or hindered them. I do know that, from around the middle of 1942, I personally felt more satisfied with the work I was doing.

A very large proportion of that work was associated with preparations for the invasion of Europe. Not surprisingly, I was involved in the effort to make an aircraft reporting system mobile—that is, to put radars, observer posts and operations rooms on wheels. Units were being built up to do these things, and I was attached to No. 101 Mobile Air Reporting Unit (MARU) and its associated No. 1 Mobile Operations Room Unit (MORU). These units were sent to participate in the invasion of Sicily, and I was detailed to follow them to see how they got on. Unfortunately there were some delays in unloading, and they lost all their equipment, sunk by enemy action in Syracuse harbour. I did not find this out until I arrived there, but I had a very interesting couple of weeks watching some masterly improvization at the sharp end of warfare. It was not easy to construct a useful report on all this, and even harder when I had to give talks to collections of quite senior officers on what I had seen.

Perhaps this is the moment to talk about how those of us in overseas theatres of war were slotted in with the RAF. On my trip to Sicily I was disguised as a Flight Lieutenant: more formally, I was granted an honorary commission in the Administrative and Special Duties Branch of the RAF for the period of my duty in North Africa. I had to put my uniform away when I returned to the UK, and it only came out again a year later when I moved to Europe—this time at the more exalted level of Squadron Leader. The question of rank was interesting. It was assumed that the nature of our work meant that we would, from time to time, be 'hob-nobbing' with senior officers, and this was true. To make things easier, we were given one rank higher than the one which would have been equivalent to our civilian grade. Needless to say, our pay remained at the civilian level!

I was a bit apprehensive about being an 'instant officer'. You see, to any serviceman of any command, service or nationality, I was indistinguishable from a proper officer. I might be asked to take on *any* task appropriate to my rank. In the event I encountered no problems—which is a pity because I have no corresponding disasters to tell you about. Actually, we did make two concessions to being in the service: we took a course of instruction on how to drive heavy transport and motor cycles; we also took instruction on the handling of revolvers. In fact, the only vehicle I ever had to drive was a jeep, and, although I had to carry a revolver, I never had to use it.

To return now to the preparations for invading Europe (with me back in civilian clothes). Towards the end of 1943 the Second Tactical Air Force (Second TAF, as it was referred to) was being built up in the UK, eventually to go to Europe with British and Commonwealth land forces. For a short time I was attached to one of the forward groups, and I must tell you about one short, simple investigation I carried out. The Group's Senior Air Staff Officer called me in and said, 'Something we must quantify for planning purposes' (I am turning what he said into OR language) 'is the number of sorties per day which we can expect our pilots to make over the beach-head. I am talking about a peak effort while that beach-head is being established. Get the facts on past operations of this sort, make appropriate allowances, and come back with a figure!' So I went away and did what he said. I cannot remember details, but I think I went through records of landings in North Africa, Sicily, Anzio and at Dieppe. I went back with my answer, and said (I think), 'Six sir!' He replied, 'Ha! That is the figure we are working on.' Now why had he given me the job? Did he think he ought to follow the fashion and find something for his OR man to do? Was he testing the OR man? Or was he just wanting to sleep better at nights? At the time I thought of none of these things; I merely thought, 'Here is a man who knows his job.' Most of the responsible people that I have met in the RAF do indeed know their jobs—most of them. You must never underestimate your client.

My next task might be regarded as a retreat from OR. Because of my experience in studying both the performance and the operation of radar, I was selected to join a team who were locating sites for radar stations in the Normandy beach-head and beyond. There were four of us: an expert from the Radar Research Establishment, an American OR man and two British operational researchers, Roy Innes and myself. We were told where the invasion was to take place, and were given the phase lines—that is to say, the expected positions of the front line up until D-Day plus 90 days. This was obviously highly classified information, but we were some of a number of people who just had to know 'where'. There was, of course, another group of people who had to know 'when'. Very few people indeed knew both 'where' and 'when'.

I shall not bore you with details of how we chose sites for radars. Sufficient to say that we used maps and aerial photos, and that we had our own relief model of the beach-head. The work involved a lot of travel between the planning and intelligence organizations. The chief problem was to get our sites incorporated into the overall plan. Normandy was going to be so crowded with personnel and equipment that we had to negotiate quite toughly to get places for our radars. But the work was done, and then, like millions of people on both sides of the Channel, we just had to wait.

For the operations in Europe, I came under Michael Graham at Second TAF headquarters, and I was put in charge of a section detached to 85 Group. That Group was responsible for all RAF work that had to be done in the base area. Its main operational role was air defence by night in both the base and tactical areas. A lot of people who were in Normandy thought there was no air defence by night! This was largely because they could not see it!

The OR section started as Peter Cox and myself. We were later joined by a WAAF Section Officer, M. A. Stokes, and by Joyce Kestin (to whom I apologize because I know she is married and I cannot remember her married name).*

Joyce, for some reason, was not made a WAAF officer, but was supposed to be treated like one and had to wear a plain sort of battle dress. We started under canvas in the orchard of a small chateau near Bayeaux, and then moved up to Ghent, where the Hotel de la Poste was a very excellent officers' mess. I went back there after the war and the old waiter welcomed me with open arms, and produced such a vast meal for my wife and myself in celebration that we had to spend the night walking round our bedroom digesting it!

Our work with 85 Group consisted of a continuing analysis of the Group's night fighter operations and collaboration with staff officers on problems arising with the aircraft control and radar reporting systems. My previous work on sites for radar was helpful here, but I must confess that our day-to-day work was far from earth-shattering and was largely *ad hoc*. We incurred a certain amount of displeasure from our RAF clients by trying to get them to extend the Group's role from that of providing night fighter defence to providing night fighter support for Bomber Command's raids into Germany—we were so close to Germany, our night fighters could have done that. We were possibly being a bit presumptuous in going into such matters of strategy, but we did know what we were talking about in relation to the electronic warfare that was going on at that time, and we thought there was an opportunity to be taken. Also, there was certainly a feeling amongst us that the terms of reference of OR were as wide as we were capable of making them, so we felt we could say these things. The fact that we never got anywhere with them was one of our failures.

As I said, we—mainly Joyce Kestin—produced regular analyses of the Group's night fighter operations, drawing what lessons we could. At the end of the war in Europe, the pressure lifted and we took time to compile a comprehensive report, grandiloquently entitled '85 Group Operations and the Air Defence of the Base and Tactical Area'. We were tidying up, and writing, a tiny piece of history preparatory to disbanding, or possibly going to the Far East—the atomic bomb had not then been dropped. In fact, even before the bomb was dropped, it was clear that, unless you were anxious to go to the Far East, you could probably count on starting your peace-time life pretty soon. And that is, in fact, what I did. But Peter Cox did not. After the end of the war, and a

* It is, in fact, Joyce Earl.

few days after he had been awarded a mention in despatches for his work in 85 Group, he was killed in a road accident in Germany.

Now I have come to the end of my story. What lessons did I learn that might still have some relevance? I really don't think I have any lessons that will startle you. They are standard things. I learnt the value of being in a team containing a wide range of disciplines. None of us were trained in operational research. We came to it from the disciplines in which we'd been trained. The office in which I worked for some time housed a chemist, a physicist, a mathematician and myself, an engineer. Just along the corridor were a lot of other people with a wide variety of disciplines behind them. I am sure that the sparks we struck off each other were brighter and more numerous because of our differences in background.

I also learnt that reliable data can be difficult and tedious to get. A lot of people get bored when you start talking about data, but it really is—I won't say *the* most important—but it is so important that it needs more emphasis given to it than is usually the case. One must make a considerable effort to get data, though not more than sufficient to achieve what you judge to be an optimum. I suspect that this aspect of OR at times languishes under the shadow of elegant and ingenious modelling.

Perhaps my most important lesson was that one must have respect for the abilities of those outside OR with whom one works—all of them: operatives, staff, big white chiefs. There seems to be a narrow path to tread here. On the one hand you have to accept that whoever you are working with knows a lot that you don't; and some of them will be much wiser than you anyway. On the other hand, you have to be prepared to apply your wit and expertise to almost anything. The right mixture of humility and assurance can be quite difficult to achieve. An OR man or woman who gets it right most of the time is someone to cherish, and to emulate.

Professor R. W. Shephard:

You will have gathered that one of the main principles established during World War II was that OR scientists should work in sections geographically close to the operations they were studying. We have just heard some examples from Fighter Command. Additionally, for example, the Army had No. 1 Operational Research Section with the Army in Italy, No. 2 Operational Research Section with 21 Army Group for the D-Day landings and subsequent operations, and Nos 10 & 11 Operational Research Sections in the Far East.

The RAF Middle East set up an Operational Research Section based in Cairo in late 1941 to study the effectiveness of their various missions. This was just about the time that Operational Research Section, Bomber Command was being set up at home, and the air war in Europe was swinging from defence to offence. Also there existed later, in the Mediterranean area—apart from RAF Middle East—the North West African Air Force, which again had its own small Operational Research Section based, I believe, in Algeria. It is to this section that our next speaker belonged. Arthur Charlesby joined it in 1943, about the time it was set up. He remained with it when it amalgamated with ORS RAF Middle East at the end of 1943 and became known as the Operational Research Section, Mediterranean Allied Air Force, which eventually had about 20 to 25 staff.

Professor A. Charlesby:

It is a pleasure to attend this meeting, not only to be able to meet old friends and perhaps make new ones, but also to see how operational research has developed since the 1940s, when it had the greatest impact not only on World War II but also on me! I think that in all my career, this was the most exciting period I ever had, mainly because the value of the work done was being felt almost immediately. It also trained me in the scientific outlook, which meant not just using a lot of computers, but rather asking awkward questions—which I still do! I generally get slung out as the result, but I continue to ask them anyway. It also means having an independence of thought; being able to choose one's own subject for study; thinking clearly on logical grounds; and getting decisions implemented because one has access to top levels. This is vital.

I volunteered for the services in 1938, and again in 1939, but was in a reserved occupation. Eventually I managed to get into the Ministry of Defence in 1942 to do scientific work, and then, in 1943, I was able to transfer to an ORS in the Mediterranean. My year in the MOD gave me some very interesting ideas on how the Civil Service operates and how scientists in it operate. For instance, I discovered that the man who sat opposite me had a first class honours degree in mathematics. He said he was going back to the University of Cambridge after the war to get a first class degree, or some kind of degree, in the classics. Why? Because he wanted to enter the Civil Service after the war. He said it was no use entering the Civil Service with a first class maths degree if you wanted to get anywhere!

I also noticed some very curious things. For instance, it is known that our aircraft did not really know where they were at this stage of the war (about 1942). Their guidance systems, their radar and so on were pretty inaccurate; they tended to 'wander around'. On the other hand, according to military intelligence, the German night bombers knew *exactly* where our AA units were. So the story went that the moment they flew over an AA gun position they would descend to a lower height so that we were always firing at the wrong height! Well, I refused to believe that German intelligence was this much better than our own, and yet there is no doubt that the results from the AA units (the radar units) confirmed that this was what was happening. So I looked into it and found a very curious effect. To find the height of an aircraft you estimated its range and elevation and used a bit of trigonometry to determine the height. But I said, 'Who does these measurements?' Two separate men on the ground! Obviously, if their observations are not exactly co-ordinated in time—even one or two seconds difference—you will get exactly the observed effect. So just because no-one had done a little bit of elementary trigonometry, for several years we had wasted pretty well all our AA shells. We didn't need a computer or anything. We had to know a little big of 'trig' and ask an awkward question: 'Why are the German aircraft so much better at knowing where they are than ours?' But they weren't!

On one occasion I was in Reading, and I had to fly back to London urgently, so they sent a 'plane for me. It was a biplane with an open cockpit, and I am very susceptible to airsickness. We had to go via Gloucester, stopping in Gloucester, and I was violently sick all the way. If you're interested, the air speed of one of these little biplanes was exactly equal to the speed at which you ejaculate when you have had too much to eat. So I wasn't being sick once, I was being sick continuously. When I landed at Gloucester, very, very ill, they wanted to give me a Lancaster to fly back. I didn't take it!

Eventually, as I have said, I was able to get to the Mediterranean, where I served under E. C. Williams, who had the temporary rank of Wing Commander. I had lots of interesting tasks there, particularly on bombing accuracy: finding out how accurate bombing was, how many aircraft you needed to destroy a particular target, and the influence the pilot could have. Now this was more or less parallel to the work that Professor Zuckerman and his team had been doing in the Mediterranean, based in Cairo. In fact, he carried on in his way, on bombing accuracy; we carried on in ours. I'm afraid we did not collaborate as closely as we might have done.

At one stage we landed in Sicily, in search of information there, and the only British unit we could find was one consisting of one Second Lieutenant and two soldiers. This was the only unit in Palermo, which was the Headquarters of the American Army. The west part of Sicily was under the control of General Patton, who was violently against anyone with long hair. He refused to have anyone visit his sector, including Professor Zuckerman, who, the story goes, was 'captured' when he tried it. We dressed ourselves up in enemy uniforms as Italians (but they never shot us!), and we went to visit this Second Lieutenant and asked him what the British Army, consisting of himself and two soldiers, was doing in that area. He said, 'We've got 70 Italian prisoners of war, and it's our job to look after them.' Well, how do you look after 70 prisoners of war with two soldiers? He said, 'It's very difficult, because we have to march them through the streets each morning into the mountains to do some work and then we march them back in the evening. They're native to this part, and they chat with all their friends as they go through the streets.' We said, 'How many do you lose?' 'That's not a problem', he said. 'As the Italians pass through the streets they tell their pals what it's like being a prisoner of war. We go out with 70 and come back with 90!' Clever people, the Italians.

They also taught us something about economics. In those days the Americans were very keen on being highly democratic, and insisted that anyone who worked for them had to be paid at the appropriate American rate. Now the American pay for washing a shirt was equivalent to the money earned for one week's work by an Italian. The result was that all the Italians stopped working normally. All they wanted to do was to find one shirt per week! This completely wrecked the economy of the country!

In connection with our study of bombing accuracy, and the bombing of railway lines in particular, we were interested in estimating the accuracy of our allies, the Americans. The Americans had a very large team working at Massachusetts Institute of Technology on bomb accuracy, in particular computing the best formation to fly. But the results they obtained were completely phoney. They were very good mathematicians, but they knew nothing at all about bombing. They didn't even realize that the bomb is released by a bomb-aimer, who could easily make an error of one second in his release point. The result, regrettably, was that all the voluminous work they did was completely futile. They had got the right mathematics but the wrong model. In fact, we worked out their bombing accuracy for them and recommended the formation they should fly. And now here is an example of how the Americans behaved in those days and still do; it's one of the most impressive things I know about the Americans. I wrote this paper on bombing formations and handed it in. The next morning I met an American officer and asked him if he had seen my paper. 'Oh yes,' he said, 'we've already flown our sorties this morning in the formation you recommended.' Now you compare that with what the British would have done. They'd have said, 'Let someone else try it first and then we will see.' I am afraid this is very often the way it is with us. I have had a number of valuable inventions looked at by British industry, and they say, 'Well has anyone tried this before? Well, we won't try it either, but if it is successful, come back and we will think about it.' The inventions often are successful, but then British industry is too late!

Now here is another true story, and rather an amusing one. I was very interested in the bombing accuracy of American aircraft, including that of the Marauder B26 two-engine bomber. I worked out the bombing accuracy they were achieving in the Mediterranean, sent the papers to HQ, and also sent a copy of them to England. The US had a large number of these squadrons here, but refused to believe my estimates of bombing accuracy. They said that my calculations showed that the bombing in the Middle East was three times as accurate as that for B26s flying from England. 'Something wrong with your calculations.' I checked them over and found them perfectly correct. So there was quite a dispute. Then, fortunately, the invasion of France took place, and the squadrons from the Middle East and the British-based squadrons were all shifted to France. Now we had exactly the same aircraft flying against the same targets. There could be no question of difference in opposition or anything. Was the difference in accuracy I had predicted correct? So I went over to France for two weeks to check up, and found it was in fact true: the difference was a factor of three, which meant that one American aircraft flying from a Middle East squadron by their method was three times as valuable as an identical American aircraft flying in formation with the method used by British-based squadrons. What was the difference? Just a matter of training. The method used in the Middle East was for the commanding officers to say, 'This is your target. If all your bombs fall in the target area, you will each get a medal. If one bomb is out, nobody gets a medal.' And it worked extremely well. It worked very well indeed, one squadron doing the work of three.

Well this all took me two weeks. I had to return via Paris, and there they had a magnificent system providing lots of very kind hostesses, French hostesses, to look after you, take you out, and so on. I went along, and was asked if I would like someone to take me out. I said, 'Of course.' 'What kind of person do you want?' I thought I would put the system to an operational research test. I said, 'I want a blonde who is interested in science and archaeology.' 'That will take some time', they said. 'We will phone you up in an hour's time.' I waited, then the phone rang. 'We are sorry,' they said, 'all we have got is a blonde who is interested in archaeology, but she is an engineering student. Will that do?' We had a very pleasant evening in the officers' mess and so on, and then I decided to try to find out more about the system. I sent a signal back to Italy, to say that I was tied up and could I have 2 weeks leave. Strangely, this was granted, but I wasted no time. I found the person in charge of the hostesses' records, and discovered that each girl, usually a student, had one star, two stars, three stars or four stars against her name. And so I got the complete list of all the four-star girls—two a day over my 2 weeks. It was very interesting! Eventually I had to return home, but I stayed friendly with a number of them after the war. One amusing thing was that after the war, in addition to getting a medal for the Italian campaign, I also got one for the French campaign. The rule was that you had to have been in France for 4 weeks at least. Now my 2 weeks with the Marauder B26 squadrons plus the 2 weeks chasing girls in Paris gave me my 4 weeks qualification!

At our Joint Air Force Headquarters in Caserta, we got on extremely well with our American counterparts. We had messes one on either side of the main road between the front at Cassino and the main naval base of Naples, and, by swapping dinners in our messes, had a very nice time together. One evening, however, the Americans came in and told us that they were very sorry but they wouldn't have any British guests in their mess that night. That night we had quite a noisy celebration in our mess, more so than that in the USAAF mess. The next morning the Americans came and asked us what we had been celebrating. We told them that while they had been celebrating Independence Day, we had been celebrating Thanksgiving! Anyway, we got on extremely well together.

There were a number of interesting ideas put forward at this time. Italy, of course, has got the Appenines down its centre, and these are a very good barrier. The 8th Army was on the right-hand side, on the east, and the 5th Army plus the Americans on the west. Each time you had a river going down to the sea, that was a front line for the Germans. You pushed them back, and they merely retreated to the next river line. And they obviously wanted to know where the next push would come from, the east or the west; they couldn't take troops from one side of the country to the other very quickly because of the Appenines. Well, I was in a camp at that time on the main road between the front at Cassino and Naples, the shipping port, as I've said. Every evening, at about 5 or 6 o'clock, large numbers of tanks came driving in from the Cassino front, past our encampment, towards Naples. At midnight, when everything was absolutely dark, they drove back again. Thus the Germans would photograph all the tanks going to Naples, apparently for transport on the east flank, but would not be able to discover that we took them back again immediately. Very ingenious! We didn't think the Army was so clever, but this was our reading of it at the time. In fact, this wasn't correct at all. The answer was simply that Naples was the only place in which there was a cinema, and tanks were the only things the troops could grab for transport!

Another idea was to cut the bridges (rather than the railway lines) so that the Germans could not supply their front-line troops. This could be done fairly readily with fighter aircraft. An attack on a bridge required a certain average number of bombs to destroy a span or two, and the Germans got very used to replacing them. So every time a report came in about an attack with 12 aircraft, say, causing one span down, we knew it would take 3 days to repair. Thus 3 days later, another aircraft squadron was sent over to resume the attack. It became a kind of game. But at one stage, we noticed that the communications across the vital bridge which had been destroyed were never repaired. We wanted to know how the Germans could remain in their front positions with a destroyed bridge behind them over which all their supplies must come. Eventually some very bright WAAF officer came up with the answer. She was a photo-reconnaissance unit officer, and noticed that a crane near the bridge had just the span that was needed to close the bridge hanging on it. Further, from one day to another she observed that the position of the jib of the crane moved ever so slightly. What was happening was that, at night, the Germans were putting the span down while they ran their supply trains across. Before it was daylight they took the span up again! And that revelation was due to this WAAF officer!

Another story describes how, when we were going to attack the South of France, all the bridges over the Rhone were bombed. Reports came in of such-and-such a bridge being down, and that photo-reconnaissance estimated it would take 3 days/4 days to repair, or whatever. One report came in about the bridge at Avignon: 'One span down, will take 4 days to repair.' In fact, the bridge at Avignon has never been finished, never been completed since the Middle Ages! That's why you dance on it!

Towards the end of the war we were using our fighter bombers to attack the enemy—like long-range guns. Although it took only 5 minutes for a plane to drop its bombs and come back again, the airfield was so congested that it had to wait as much as 45 minutes before it could land. The question put to the ORS was how many aircraft we should retain, and how many we should get rid of, 'because obviously we cannot fly them all efficiently'. So I visited the airfield and, as usual, thought, 'Let's see if there is another answer to this.' I stood watching the way things were organized. In those days, the airfield had a runway and a perimeter track all around it. Dispersal points were situated a little way off the perimeter track; each squadron had its own space allocated. I discovered that the pilots were getting so slap-happy in their procedures that the aircraft waiting to take off went and parked on the end of the runway nearest to their dispersal point. The runway thus became a sort of two-way highway. Until all the aircraft at one end of the runway had been cleared, those at the other end had to wait; you had congestion at both ends, leading to a delay of 45 minutes in landing. My suggestion was a very simple one. Just extend the runway and build a little parking lot near each end. You hold the aircraft there until the runway is clear, and then you take off, one after the other, from the parking lot. This solution was very effective. We found that within a few days, a dozen squadrons could then use the airfield instead of about four, as previously. I got mentioned in despatches as a result.

Let me tell you something about my time in Greece. The final bit of research I did there was rather amusing. We lost many of our troops in Italy fighting the Germans. We lost a lot more troops in Sicily because of malaria—the swamps. We were losing even more troops in Greece—this time because of VD and syphilis caught from the local girls. No one knew what to do about it. The local Army padre said, 'The best thing to do with the soldiers is to frighten them off. Tell them if they get any disease, there will be no treatment for them. Take them to hospital and show them how dangerous it is. Give them more footballs, and they won't bother about girls!' The Army medical officer said the very opposite. He said the thing to do was to have one place well supervised and safe from disease, and to tell the chaps they could go there 'free', without any danger. As you can imagine, this led to a terrific argument between the padre and the medical officer, one saying make it dangerous and frighten people off, the other saying make it easy and safe. They asked me to go in and decide which was correct! I had a nice time raiding all the brothels, and ended up taking all the 'registered' girls in convoy through the streets for a medical inspection at an unexpected time—me in charge of the front van, and crowds of half-dressed girls following behind in several other vans. This was in the middle of the rush-hour. I wonder what the Greeks thought of my needs! Seriously though, the answer was simple. We were trying to deal with two quite distinct approaches. If you are interested in morals, then the padre had the correct answer: you frighten them off. If you are interested in disease, you take the medical man's view. But you must not try to mix up the two. And this became the main conclusion of a report which was classified as a secret document—a very secret document, as you might expect! Of course, no action was taken on it.

After the war I got myself involved with operational research on air traffic. Those were the days when we were flying civilian aircraft all over the place and trying to guard them against collision. We have the same problems now. But then, for instance, there were only enough radars monitoring aircraft flying from London to Paris to give one radar fix for every two aircraft somewhere on the journey, which was hardly good enough! The question was 'What to do?' One suggestion was to increase the radar coverage; but with the equipment in those days, this was impossible. The answer turned out to be a very simple one. When you looked at the map, you could see aircraft routes going in several distinct directions. There was one route going to France, one going south-west, one going east, one going west to America, and so on. So I said, 'Let's make them fly in channels marked by beacons. We'll make sure they go from one beacon to another, and not just fly wherever they want (unless they are very small aircraft). At least we'll know then where they are!' This procedure, which we originated at RAF Uxbridge, is, of course, identical with that used by all aircraft nowadays.

A third portion of my operational research career began in 1948, when the Russians blockaded all the traffic going into Berlin. The roads which ran across East Germany before they got to West Berlin were 'closed for repairs'. The railway lines were also 'closed for repairs', and so we had no means of transporting supplies into the western zones of Berlin. We were determined, however, not to be stopped; so we started sending in supplies by air. I was sent out as Chief Scientist to advise on the operation.

At first we thought the operation was only going to last a few weeks, but it soon became obvious that the Russians were not going to give way—nor were we! So we had to design this operation on a long-term basis. How do you supply a city of 2 million from the air? Well the planning that had been carried out was extremely poor, in my opinion. An estimate had been made of the weight of supplies a person needs in a year. This was divided by the number of aircraft available, and by how much each aircraft could carry, and led to the conclusion that, on average, the operation was just about possible. They forgot about the fact that people in the winter months cannot be fed on the same food as in the next summer. Averages can be misleading! So I did some calculations to determine pretty exactly how much we could fly in, month by month, and also what the minimum consumption in Berlin would be, month by month. Thus we could see more exactly whether we could do it. The thing was very much touch-and-go. The Americans then decided to put in an extra 24 Skymasters, and luckily the weather was rather lenient; but even so, the people suffered pretty badly. They had only just enough food.

This experience also enlightened me about the American approach to advertising. The RAF always met the target forecast I had done (month by month, and allowing for considerable possible variations in weather) with remarkable accuracy. The Americans, on the other hand, always did better than their forecasts, and received great kudos as a result. I was reprimanded for the apparent poor showing by the RAF. In fact, all the Americans had done was to understate their targets, so they had no difficulty in exceeding them. We did a better job of prediction.

We came up with some other techniques which were quite unofficial. We needed to fly in coal to keep the people warm during the winter in West Berlin. The allowance proposed, if I remember rightly, would be 25 lb for the entire winter. I said that this was quite ridiculous; you can't get anywhere with 25 lb of coal! So we then had the idea of carrying the coal in hoppers which would open over the runway while the aircraft was flying at a few feet. The coal would cascade towards the end of the runway, be held up by a wall there, and then the Germans would come and collect it. Apart from anything else, this meant we would save a landing. But it also necessitated cutting down a large part of the forest round Berlin to give room for the aircraft to fly low. I made some enquiries, and I found that Berlin was based on sand and, as you know, if you cut down one generation of trees in such circumstances, you get a desert. So this wasn't a good idea either.

Eventually we asked whether, if we could fly in 25 lb of coal, we couldn't fly in 25 lb of something more valuable. And I suggested we flew in cigarettes, which in those days were the common currency. So, in due course, Sunderlands landed in West Berlin half full of cigarettes, and half full of currency to pay for the cigarettes—which sounds a bit ridiculous, but that's how economists work! Thus there was a mass of cigarettes floating around West Berlin! Now the Russians had put enormous stocks of coal in East Berlin in anticipation of the day when we could not supply West Berlin. Around each police station in East Berlin there was thus an enormous pile of coal. It didn't take long before an agreement was reached between the peoples in the two zones. The West Berliners travelled into East Berlin (remember this was before the Berlin Wall) and swapped their cigarettes for a few bucketfuls of coal. The piles of coal in East Berlin gradually disappeared in this way. Eventually the Russians discovered they were losing all their coal and getting a lot of unwanted cigarettes in its place; but in the meantime all the police chiefs in East Berlin were walking across to West Berlin with large suitcases full of cigarettes, selling them for money, and then flying out to West Germany. That's part of the story of West Berlin and the Berlin airlift. I don't know if it has ever been written up before—it's an amusing story.

There are lots of things I could tell you about. The Berlin airlift, for example: the pilots were complaining bitterly that they were getting overtired. They were scheduled to make two return flights, totalling 8 hours flying time, and then have 17 hours off. So they were, in effect, working a 25-hour day, and thus their working time would gradually rotate from evening to night to early morning and then to late morning, and so on. They were complaining bitterly about this change in daily schedule, so I made some enquiries and had some tests carried out. A schedule like this is apparently quite wrong. The human body is adjusted to a 24-hour cycle, and it is better to work for 8 hours and then have 16 hours off than it is to work for 8 hours and then have 17 hours off! Giving one extra hour's rest actually makes things worse! The schedule was changed as a result, and great improvements achieved.

Another concern was that we found that the reliability of the aircraft flying these very heavy loads was very, very low. Although lots of inspections were carried out, every aircraft had to be flown back to England for an overhaul after every 50 hours of flying, which is not very much. So we started looking into this. What we decided was that the unreliability fell into three types. There was that caused by things going wrong because they wear out; for these, we needed to know how long they lasted before they started to wear out and had to be replaced. Then there was that which was caused by items which don't wear out but just suddenly go wrong. You can only check whether these are working or not. And the third, most interesting types were the things which go deranged when you look at them! You know this with your car. If your car goes into a garage to have something put right, something else goes wrong. Servicing disrupts it. So what we had to do was not to find out what was the defect rate for aircraft on the average, but to separate out these different types of fault and investigate the occurrence of each from the previous inspection. In some cases we found that the time needed for a component to settle down was just the time between inspections. So this time was tripled. By doing this we were able to make an enormous difference to the maintenance required by the aircraft.

I think I shall end by saying simply that I enjoyed my period with operational research immensely, partly because there was a great challenge to meet and I thought I was doing something very useful, and partly because it enabled me to use my independence of thought, which I believe is a vital part of operational research. You imagine what you think is correct, find out if there is any truth in it, and then follow it through. You certainly have an independence of thought in defence which, in many cases, you do not have in industry. You must also have access to the highest possible authority, so that you can put your case forward to him directly. He may or may not believe it, but at least you know he has heard your views. Finally, you must make sure that the decision required is quite clear, and that you are able to present your case well.

I remember once upon a time, not all that long ago, I found that many official scientific reports seemed to be written in a standard 'form'. There was a title and the name of the author. After that, there was an introduction saying that this was 'a most important project', and so on and so forth. (It did not necessarily say what the project was—only that it was important!) Then there was an experimental section with graphs and figures, especially graphs with straight lines—lots of these—and then a discussion and conclusion. The conclusion was invariably that the theory looked as if it was probably correct and could be very important; but there had not been enough time to deal comprehensively with it, and more time was needed to continue the work. This, as you realize, is a fairly standard pattern. So I wrote this out as a proforma, leaving blank spaces for title and author. My idea was that everybody working in operational research should receive two copies of this document a year, fill it in, and go on holiday for the rest of the year! In fun, I submitted this report through the usual channels. It was accepted as a genuine report, but I was reminded that I had forgotten to fill in my name and the title of my research.

I think that's enough!

Professor R. W. Shephard:

You will recall that I mentioned previously that Professor Blackett was one of the original members of the Committee for the Scientific Study of Air Defence set up by the Air Ministry in 1934. After a spell in Anti-Aircraft Command, about which I shall say more later, he was appointed Scientific Adviser to Coastal Command at the RAF, and set up an Operational Research Section there in March 1941, with E. J. Williams as his deputy. This Operational Research Section never grew as large as the ORS in Fighter Command, which reached the size of about 60 or 65 in 1942, or the ORS in Bomber Command, which was something like 35 or 40 strong at the maximum. However, the calibre of the men involved in Coastal Command Operational Research Section was very high indeed, and of its 25 members, six subsequently became Fellows of the Royal Society. Although Blackett remained with the section only for about a year before going on to the Admiralty, he certainly left his mark on this important Operational Research Section. Tom Easterfield, who will now speak to us, joined ORS Coastal Command in 1943, which, I believe, was when Harold Larnder had just taken charge. He remained in the section until towards the end of the war, when he transferred to the Air Ministry.

Mr T. E. Easterfield:

Ladies and Gentlemen, I will begin by giving you a little bit of my background as to how I got into operational research. I should say that I was rather older when I started than most of the people coming in as juniors, because I had spent rather longer than usual at university. I had taken a science degree in New Zealand, and realized mathematics was my line. When I came to

Cambridge, I realized that I would have to take a Bachelor's degree in mathematics there before I could hope to do research. So altogether, I had 6 years there. And I drifted over to very pure mathematics.

Here I would like to say something which is perhaps not entirely relevant, but it may be. In New Zealand there was never any nonsense about the relative value, prestige and intellectual superiority of pure, as compared with applied, science, mathematics, and the like. In fact, my definition of pure science is that you do it because it's fun and you are interested; and of applied science, that you do it because you want the results. Two people could do the same piece of work, and one would be doing it 'applied' because he needed the results for something else, and the other 'pure' because he was doing it for the hell of it. And certainly my father, a university professor, oscillated very happily between advising local firms on anything that was going and doing perfectly straight classical research in organic chemistry.

By the time I had finished at Cambridge in 1940, I had a Ph.D in pure mathematics (group theory). I then got a short-term job with an optical firm (if any of you came across Aldis Lamps, they were the firm that made them, and have long since been absorbed by someone else). It was a very unsatisfactory job from my point of view because they wanted me to be a lens designer, and yet didn't seem to want any lenses designed! There was hardly anything for me to do, so I applied to the Technical and Scientific Register, which was a kind of super Job Centre for people with professional and managerial qualifications, and, in due course, I was offered a job calculating the trajectories of anti-aircraft shells, which sounded a bit soul-destroying. Luckily it fell through, so I was then told to go to Coastal Command, which I did.

My reception was a bit odd. I had not been told what I would be doing, only to report there. An elderly and rather jaundiced Camp Commandant said, when he saw me, 'Another of these scientists! What the hell can they find to do?' So, as is usual in the Civil Service, when you turn up and they don't know quite what to do with you, they gave me a pile of files to read! For some reason or another, they always induct you into a job by giving you a pile of files to read, and leave you to it. They are not always relevant files! I think they work on the principle that you must always appear to be doing something.

However, Cecil Gordon, a biologist who combined a brilliant mind, an abrasive personality and a slight tendency to empire-building, found me and asked me what I was doing. I said, 'Reading files', and he said, 'Well, I think you had better come and work with me!'—which was absolutely wonderful! I may say that Cecil was by no means everybody's cup of tea. He had, as I've said, a most abrasive personality. His mind was temporarily one-track; and whatever obsessed him, obsessed him to the exclusion of everything else for the time being. If it was his problem at work, at times it could get monotonous; and if it was his grievances, it became a bore. Indeed, I suspect he was only accepted to Coastal Command because, like a lot of rather odd types employed there, it was acknowledged that they might be mad, but they got results.

Of course, Coastal Command had been helped tremendously by a success achieved by OR—Blackett's work on the tactics of attack by dropping depth charges on U-boats (which was finished by the time I got there). Before this work, practically none of our depth charges sank any U-boats. It was a combination of very sound analysis with beginner's luck that the first attack with the new tactic sank a U-boat. The chances were, of course, by no means 100%, and it was merely a piece of marvellous luck that the first time the new tactic was used, it brought home the bacon. It undoubtedly did an awful lot for the prestige of the Operational Research Section.

Cecil Gordon had been asked to look into why Coastal Command seemed to get so few sorties per month out of their Liberator aircraft, which Bomber Command also wanted and which, being supplied by the States, were in short supply. It seemed a pity not to be able to get more out of them. So he and his team went out and made observations, both in the hangars and in the local and Command operations rooms, to try to understand what decided whether an aircraft could fly. A very interesting thing turned up.

The efficiency of aircraft squadrons was being judged by the percentage of time aircraft were 'available'. 'Available' meant flying in the air, or standing on the ground and available for flying if required. Before the war, this was almost certainly a reasonable criterion. If you expected a blitz, you wanted to be able to put as many aircraft as you could into the air at once. But in Coastal Command in particular, a good deal of the work was routine patrolling over the Bay of Biscay looking for U-boats and, at the least, forcing them to stay below the surface and run down their batteries. It was imperative to use the resources you had available and get in as much flying as you could each month. However, according to the criterion, the way you kept 'availability' high was by keeping aircraft on the ground. You see, when aircraft are flying, they have to be serviced every so often. Even if nothing goes wrong with them, there will be routine services after every so many flying hours. Thus you can keep your 'availability' 100% by keeping the aircraft not flying but merely standing on the ground. I'm not suggesting that this was actually done, but undoubtedly there was a kind of built-in tendency not to fly aircraft so that they would be 'available' tomorrow, and would swell the availability figure. And this, of course, was highly unsatisfactory.

A second thing was that, if it was clear that you knew you would be flying absolutely regularly, day in, day out, irrespective of weather and so on, you could have an arrangement in which you would have just enough men for each aircraft to go out and fly, come back, and then be serviced. No time would be spent either waiting for maintenance or awaiting flying. That, of course, is a very ideal picture. But even in routine patrolling of the Bay of Biscay, there were hazards of weather, or perhaps you had information that a cluster of U-boats would be passing, or various things; which meant you wanted to fly somewhat different patterns from day to day.

There were other jobs of Coastal Command that were just as varied. For instance, there was the escorting of convoys by sea, in which aircraft would go out and patrol around them to attack any U-boats that showed up. Undoubtedly this made a considerable contribution to the defeat of the U-boats. Now, in the case of convoy escort, of course, you would know when a convoy was coming. When it first got within range in the Western Approaches, you would have a very small proportion of the aircraft flight-time spent escorting. At the extreme, the aircraft would get to the convoy and then turn back immediately in order not to run out of fuel before it got home. The closer the convoy got, the longer the aircraft could keep up the escort. Therefore you had a predictable or fairly predictable pattern of aircraft requirements for escort duties, going up and down with time. There was, of course, a certain amount of randomness; but demand was mostly predictable and, on the whole, fairly regular (because the convoys tended to come fairly regularly). So this meant that you were bound to have time when the aircraft that were 'available' were not being flown. They would return from their sorties and, if you had a lot of men, they would be serviced quickly and then the men, as well as the aircraft, would stand idle: but if you had a lot of aircraft, then the aircraft, rather than the men, would be idle. And this is a perfectly common problem that many of you will have come across in connection with the question of how many men you should use to maintain a bank of machines in a workshop.

Another job for Coastal Command was to attack enemy convoys bringing ore down the Norwegian coast; and that had a very irregular pattern. You only had 2–3 days' notice of when a convoy would leave. Although you knew on average how many convoys there would be over time, they could come at intervals as short as a few days or as long as a few weeks. This was the same situation as before, but much more so. If you had just enough men to do all the servicing, then they would be servicing aircraft all the while. Aircraft would go out and come back. Some of them would be slightly damaged, and some of them need complete servicing when they returned. They would gradually be converted from 'unserviceable' to 'serviceable for flying' during the time until a strike came up again. If it came up pretty soon, you used what aircraft you had; if it came up late, then, presumably, all the aircraft would be ready to use.

This, of course, emphasizes the point that, if you are up against irregularities, something must be used inefficiently. If you are short of aircraft, then you must have enough men to keep your aircraft serviceable for as much time as possible. If you are short of crews, then you can only fly as much as the crews will allow, given that they need rest from the sorties, and so on. If you are short of maintenance men, then it is worth having several aircraft which can stand about so that the maintenance men can keep going as steadily as they can, working on the unserviceable aircraft in readiness for the next strike.

Nowadays, one would probably employ various forms of queueing and renewal theory to sort this out. Cecil Gordon set up a very simple model on the principle of taking out all the stochastic elements, and thus reducing the problem to something you could do with elementary algebra. Almost back-of-an-envelope stuff! The other thing he did was to observe what was happening. It soon became quite clear that, although Coastal Command might consider itself to have a limited number of Liberators, it was relatively far shorter of maintenance manpower. This was because there was a tendency partly for other commands to be allocated manpower first, and partly for men to be kept in industry making things rather than being sent to Coastal Command. So the ORS was given the opportunity to oversee two squadrons and control how they were to be used: they had a good deal to say as to when aircraft would be flown, and they had a good deal to say on the pattern by which they were to be maintained. Afterwards, some people said that this brought home the bacon, because these units were screened, as a result, against having people posted in and out—there was a conviction in the RAF that 25% of the personnel were always away from the station on courses or on posting to somewhere else.

If I may digress for a moment, there was a period when I had some time to test this. I went to a station and got all the records from the guardroom and looked at them. It wasn't 25%; it was about 2%. But, of course, it was still particularly noticeable if you got irregularity due to bad record-keeping by the people who did the posting. In one extreme case, I found an extraordinarily jaundiced pilot who had been retrained on five different types of aircraft, and had never flown any of them operationally because each of them had been taken out of service just as he was ready to do so! Another extreme was at a camp, miles from anywhere in the wilds of East Anglia, where I got talking to a Medical Officer, who said, 'Of course, the trouble with RAF is that they don't post people often enough!' Well, I pricked up my ears at this, which was, in the least, a minority view. He said, 'Yes. Most of the people who come into my surgery have been stuck here for 4 years, and it's getting them down!' So one could see that some of the human factors which people could well have started looking into when asking these questions included the whole system of why those people were posted and when.

To revert to my main theme. The fact that the two squadrons under ORS control got very much more flying out of their resources than had previously been the case led to the then Commander-in-Chief, Joubert (who left at just about the time I arrived), to think that, although some of the people in the ORS may be odd, they did at least produce results. One elderly officer said to me, 'Marvellously efficient chap, but he was no gentleman!' (That was of Cecil Gordon.) But they were accepted as 'licensed jesters' and, like all licensed jesters, their behaviour was odd.

So I arrived in the Section in what was, undoubtedly, a rather booming situation. Larnder had just joined us, and he had a marvellous gift for hob-nobbing with senior officers, picking up what was in the air, and spotting the problems before they were formulated by the officers themselves. Simply marvellous! But we were there too, and we were in civvies; and that was marvellous too because we could talk without too much constraint with anyone of up to quite senior rank, and we could also go around and talk to the NCOs and to the erks themselves. There wasn't any feeling that we were there as 'superiors'; we might be odd in some ways, but all the same we were there as people. It made a tremendous difference to the ease with which you could pick things up.

There was some discussion here earlier on about how, if you had people who knew too much about a subject, they had lost the habit of looking for new ideas; if they knew too little, their new ideas were no good. This was typical, for example, of some of the work which came out of a mathematical section called the Air Warfare Analysis Section, which was out of contact with the forces. We, on the other hand, were actually meeting the people concerned. We lived at the HQ of the Coastal Command, ate in the same mess, and drank in the same bar. For some people (not me), the latter provided a very big part of the contact. I know one man said that '90% of operational research is beer!'. Consequently, if you had some work in hand that was beginning to suggest improvements to practice, you could easily go and talk to the people involved. Among other things, this allowed you to show them the logic of what you were suggesting, before it got to someone more senior who might use it as a way of telling that they had boobed. In fact, quite often you would be putting them in a position of being able to say, 'We thought this so important that we are already doing it', thereby showing initiative, and, far from getting a rocket, they would get credit for it. On the other hand, if your discussions showed that your idea was going to be impracticable, you could put at the beginning of your report that, for reasons outside the scope of the report, it was not being acted on.

There was some work I did comparing Lancasters and Halifaxes, in terms of their relative efficiency, from the point of view of flying hours per man-hour and resources used, including, in this case, the man-hours for building the aircraft. It turned out the Lancasters were in fact very poor. But the view was taken, which was probably right, that since we were getting well on through the war by this time, it would take a lot of effort retraining pilots who flew the Lancasters to fly something else, and it would be bad for morale, because a lot of people thought you really can't beat the good old Lancaster! The Command should stick to the Lancasters even though, if we had started from scratch, we would never have touched them. That seemed to me to be a very sensible approach. I have always felt that, with expert advice, one has to take the view expressed in the epigraph to a nice little booklet, which some of you may know, called *The Awful Handyman's Book*, which begins with the statement 'Expert advice is usually sound but frequently impossible to apply.'

Well, we were in this very close contact with our customers, and could try out our ideas on them, the people who would be affected. They would frequently tell you either that you had obviously got it wrong somewhere, or that you had got it right but it wasn't applicable, or that you had got it right and they would do something about it. It also meant that you could sometimes do some quite startling things about something fairly obvious. For example, one of the things that got me interested in information flow was that a colleague of mine looked at the daily reports that were produced on the availability of aircraft, and the state they were in. He found there were three different reports that never agreed. There were things he wanted to know that weren't in any of them, and, on occasions, what purported to be the same figures were different in two or even three of them. He managed to reduce all three to one signal, rather shorter than any of the originals but containing all the information wanted and, to a large extent, self-checking. I thought this was rather wonderful. I may say that he saw the thing into operation and was subsequently expected to run it. This can be a danger in operational research. You develop something and are asked to run it for the start. Running at the start is good experience, because one of the things we noticed in Coastal Command was that we could often check the models we had used; these were often most primitive in form (back-of-the-envelope stuff), and sometimes completely non-mathematical. There is, however, a danger that you may find yourself responsible for the system when it is well established, when you ought to have moved on to something else.

An example of a non-mathematical study is, for instance, a wonderful piece of work in which all the various signal forms—Forms Green, Forms Orange, and so on—were used as the basis for a piece of historical research into what had happened in a real cock-up over the Bay of Biscay. The report was called 'A Day in the Bay' and was about a combination of the rules for radio silence, ineptitude in various places in analysing things, and a piece of sheer hard luck. A U-boat had been reported in a certain place. In the course of transmission, two of the co-ordinates had been interchanged. A naval ship which was handy was sent to the wrong place, but nevertheless found a U-boat there and reported it! Well, the situation, which had been confused before, was now confused almost beyond redemption—which was why the investigation was set up in the first place. The outcome had quite an effect on procedures for imposing radio silences and the like.

The work that Cecil Gordon was doing on Planned Flying, Planned Maintenance in Coastal Command used a very mixed bag of people. I was the only pure mathematician there; there were at least two chemists and one physicist, and one or two people who I think were statisticians as such. There was one chap who was a social scientist with a strong sideline interest in natural history. If any of you know the book, a bit old now, called *London Natural History* by R. S. R. Fitter, you will know it is a wonderful account of the wildlife you find in London and its development over the ages. He had been with Mass Observation, and was awfully good at picking things up in an unselected but not uncritical way. He would come back from reporting on something, and would give you a bit of a dogs' breakfast of stuff. But if you went through it, there would be a hell of a lot of good stuff in it. He was a very useful member of the team. I think we had one lawyer (although I am not certain that I am not mixing him up with a chap I met in another unit later) and lots of biologists, who showed up particularly well because they were so extraordinarily versatile. They usually were good at statistics, often very good at statistics. But equally they were perfectly prepared to crawl around in the mud looking for leeches or whatever.

It became very clear to me that this combination of observation with being prepared to apply such theory as you need—anything from back-of-the-envelope stuff up to what nowadays you would do with a first-class mathematician and a computer—is of the essence to good OR. You use maths if you want it; you don't feel that it is there as a stamping-ground for people doing what I have heard described as 'inapplicable applied mathematics'. Biologists know what to do with data, and are prepared to go out and do the 'natural history'. To my mind, the really key thing in OR is what you might call the 'general practice' work or the natural history: trying to find out what the problem is, what people are trying to do and what stops them—forming a model of sorts, and then trying to get information about it; and usually finding either that the information isn't there, or that what purports to be there is not worth the paper it's written on. If, afterwards, you want to do your big calculations and what not, fine! I'm all for it! But in many cases, by the time you know what the problem is and you have it formulated, and have some decent data, or have persuaded some people to record it, this approach is not necessary. I know one chap who went to look at stock-keeping in a furniture shop. It turned out that the only records they kept were of what they sold, so they knew, for example, that they had sold 25 settees. They also knew a little bit more about what they had bought, because they kept invoices and so would know they had bought five of the settees from G Plan, although they were not always terribly clear when. So this man asked me what to do. I said, give them a list of the records they ought to keep, and tell them you will come back in 3 years and analyse them. He didn't think he could do that, so that, I am afraid, was the end of it.

Now I will just say a bit more about Planned Flying because here is an example of how studies grow. You obviously needed very good records of how long things lasted, what it was that was stopping the aircraft flying, and so on; and the reports were very poor. Often they were kept by people who worked on the basis of the fitter who, having found a defect in a particular part, was asked why he did not fill in the appropriate form. He replied, 'Oh you don't need to do that. Everybody knows they are breaking down all the time!' But of course the people who received the form didn't know this, because they were never sent any forms about the thing.

I had to do some work in which, in order to determine the likely numbers of replacement engines we would need if the war spread to the Far East, I needed figures on how aircraft engines deteriorated in use—whether in fact they would tend to fly out their 500 hours until they were taken in for overhaul, and probably replaced, or whether they didn't last so long. But there were no records. So I had to do an actuarial job on records of their ages at the time they were withdrawn because of defects or failures. You had to assume a fairly steady population, but I was by no means sure that the population was as steady as all that. It was one of these cases where, if you can't get good information, then you use the best you have got. And it was quite clear, first of all, that the engines deteriorated much faster than anyone thought; and secondly, when they came back after overhaul, although the RAF swore they were as good as new, they weren't. An aircraft brought back reconditioned was only as good, from the point of view of liability to break down, as a new engine which had been running for 100 hours. Rolls-Royce swore it couldn't be so, but conducted tests which substantiated my figures. They then said that if the engines were maintained 'the way we maintained them before the war', they would have been as good.

It was at this point I stopped working with the RAF, so I don't know what happened after that. They were, in fact, very shirty about what we found out, although I would have thought they would have wanted the information themselves and been glad we got it. My particular work was very largely 'filling-in work' on the actual picture of the planning—doing odd bits concerned with economics and, in particular, what you did about what was short.

So I will end on one thing which is a kind of epilogue. Shortly before the end of the war, I wrote a paper called 'The economy of the RAF', which investigated the way in which all the operational research, the basic economics of manpower, planes and so on, and cash, intermeshed. And I did say in this paper that Planned Flying, Planned Maintenance is based on manpower being the item in short supply. However, there would probably come a time after the war when it was something else that would be in short supply—cash (because you are limited by a budget) or, if the war continued in the Far East, transport capacity, or manufacturing capacity for equipment, or whatever. You would then have to replan the indices on which you were working in order to cope with these particular circumstances. About 1955, someone who had read this paper sent me a photocopy of a minute saying 'It appears that Planned Flying, Planned Maintenance is based on a shortage of manpower. As we are not short of manpower, it appears no longer to be a useful system'!

Professor R. W. Shephard:

So far we have concentrated on the Air Force: now it's the Army's turn.

In 1940, the General Officer Commanding Anti-Aircraft Command in the Army, General Pile, asked Professor Blackett to join him as Scientific Adviser to help him combat the air attacks that were expected to occur by night. Incidentally, it should be explained that anti-aircraft gunnery was an Army assignment at the beginning of the war, although Anti-Aircraft Command was under the operational control of Fighter Command of the RAF for the air defence of Great Britain.

Up until 1940 anti-aircraft engagements required the targets to be visible from the ground, but it was clear that, even with the aid of searchlights, there would be many occasions on which the attacking aircraft would be invisible. Although radar equipment had been introduced for gun laying, it gave only the bearing and slant range to the aircraft. Reliance was placed upon sound locating for the other required parameter for gunnery, namely the angle of elevation to the target. Now, sound location, as you know, is not the most accurate of locating devices. The whole situation was far from satisfactory. A man who was a gunner at the time wrote to me a couple of years ago and said, 'Morale was only sustained by the occasional enemy aircraft which somehow managed to get into the way of our shells and was shot down!'

Blackett made two main recommendations to General Pile. One was that scientists should be attached to, and reside on, the gun sites to keep the RDF equipment calibrated and in good running order. The second was to establish a school at Petersham under J. A. Ratcliffe in November 1940 to train the 60 or so scientists that were needed quickly.

Our last speaker, Ted Treadwell, was recalled from active service in February 1941 to join the training staff at this school. While there, he worked with members of the AA Command Research Group, often referred to affectionately as 'Blackett's Circus', and then transferred with them to the Ministry of Supply in 1941 to join the newly formed Air Defence Research and Development Establishment (ADRDE) Operational Research Group. The work of this group was originally concerned only with anti-aircraft and coast defence radar, but gradually its interest spread to other Army activities and, by 1943, it was composed of eight to ten sections covering tank and anti-tank warfare, artillery, signals and munition lethality, to name but a few. It was then that the change in name to Army Operational Research Group (AORG) took place. The Group left its ADRDE parent to become an establishment in its own right. Ted remained in operational research throughout these changes, and has subsequently spent most of his life in one or other of the various Army OR organizations.

Mr E. E. Treadwell:

I am not sure whether it's better to come first or last. If you come first, you have got a wide open field: if you come last, you have heard what everybody else has had to say! Anyway, I will try and give you some idea of my personal involvement in OR.

It's over 40 years since I first went into the OR game, and my memory of events is obviously related primarily to my own experiences and my own views. You must keep in mind, of course, that I was very junior at the time I started in 1941. I was concerned for many years with purely Army problems—a great variety of them. But this changed in later years when, after the formation of DOAE at the end of 1964/65 as a tri-service establishment, my operational research experience covered all three services. I then became mainly concerned with Army and Air Force problems and the interface between them—air defence, close air attack, things like that.

But let me begin by telling you a little about how I got into the game, because it is quite interesting. My introduction to OR was not by choice. I had little idea of what it was about. Incidentally, I was delighted to hear the last speaker praise the biologists and physiologists, because I had been an assistant in the bio-physics laboratory of A. V. Hill for a number of years—until, in fact, I was called for military service in 1939. We thought at that time (the war had not yet started) that this was only going to be a 6-month job of military training, and that I would soon be back. But of course things changed a bit, and I was soon on active service as a gunner.

But a little diversion here. You know that, these days, we pay enormous attention to OR studies on the selection of personnel. When I was inducted into the Army, I went with a background of about 5 years in quite a high-powered laboratory. I knew a little bit about electronics, but I was basically trained in physiology and biophysics. I had also done quite a lot of work with the St John's Ambulance Brigade, so I was a fairly good trained nurse. When I went for an interview with the Army, I thought they would be sure to want to make use of this experience, so I explained my background to them at great length. The Major who was interviewing me said, 'Well, what was your father in World War I?' I said he was in the Royal Artillery, and that's literally why I became a gunner! On active service, I went to France, and then came back to England in June 1940. We were redeployed defending Poole Harbour when I heard on the grapevine that, because of my previous experience, I was being posted to a mysterious hush-hush unit. It was said in the posting room that I was being sent to a most top-secret unit from which I would have no leave. I was told to report forthwith. This caused the CO to ask to see me. He wanted to know how many people of influence I knew!

I arrived at Petersham in February 1941, and I was interviewed by J. A. Ratcliffe from the Cavendish Laboratory, who was setting up the AA Command School in conjunction with Blackett and various other people. I remember that there was another chap there named Shire, who I believe also came from the Cavendish. After a chat with Ratcliffe, in which he said, 'I believe you know a little bit about electronics', and I replied, 'I have done a bit, because in the bio-physics work I was doing we converted from the old string galvanometers to electronic amplifiers, and things like that', he said, 'Fine!' He gave me a sheaf of papers and said, 'Well, you know we teach radar here. In a fortnight's time you are going to be instructing in elementary electronics and radar!'

So I had a fortnight to look at these papers and do something about it! I had had a bit of experience because I was an anti-aircraft gunner, and I knew some of the problems. Indeed, I have still got my gunner's slide-rule with two cursors on it!

When I said earlier that I didn't go into operational research by choice, it wasn't quite by chance. Some time previously, in the late summer of 1940, Henry Tizzard and A. V. Hill had suggested to General Tim Pile, who commanded Anti-Aircraft Command, that he needed a Scientific Adviser and a scientific set-up. At that time, the major universities used to send an MP to Parliament, and A. V. Hill was the MP for Cambridge. He was also chairman of the Parliamentary Scientific Committee. Luckily, Tim Pile accepted these ideas. (A little aside here. We are celebrating the 50th Anniversary of Operational Research, so you might be interested to learn that A. V. Hill had been involved in the First World War in anti-aircraft gunnery research, and had done some work which was essentially operational research. In fact, he was one of the people who wrote the gunnery instruction manual after the First World War.)

In the Summer of 1941, 'Blackett's Circus' spent part of its time at Petersham—the AA School had all the equipment it needed for training, believe it or not, in the deconsecrated church hall at Petersham church.

We used to teach electronics during the day, and then, in the evening, part of Blackett's Circus used to come and use the equipment. There were four of us who were in the Army, all Lance Corporals or Lance Bombadiers doing some teaching on subjects associated with radar. However, the chap who taught aerials was a Major, and there were also half a dozen civilians. But Ratcliffe never recognized rank, and to him a Major was the same as a Lance Corporal, which didn't really please various people! The Army personnel of the AA School were co-opted in the evening to help the Blackett group, who were looking at the use of radar and anti-aircraft predictors.

With the foundation of the Operational Research Group in July 1941, I found myself put on the war technical reserve, and became a pseudo-civilian, although I was still in the Army. I was delighted at this because it was very nice to get out of uniform, but I wasn't very happy about the pay, because I found that the Army had put me in a very expensive billet and, on civilian pay, this probably wasn't a good financial transfer.

The Group was first set up in the vicarage of Petersham church. I think one of the interesting things about the Group was that it was the people who made it. The contribution they made was enormous and established OR in the Army as an important contributor to defence.

The nucleus of the new Group were the original members of Blackett's Circus, and covered an enormous mixture of disciplines. There was an astronomer, about four physiologists or biologists and some mathematicians. One of the mathematicians was Sir Neville Mott. There were also some physics people and a couple of engineers. It was quite interesting that, out of the original 30 members of the staff of this Army group, at least eight became Fellows of the Royal Society, and one became the President of the Royal Society and also won the Nobel prize.

So there was an extraordinary amount of talent. It was an incredible team, mostly young people, all very versatile. I had come from a laboratory which was a sort of 'DIY' laboratory: you did everything yourself. If you wanted a piece of kit, you had to make it yourself, and this applied to everyone from the boss downwards. All the staff of the Group were accomplished theoretically; Huxley, in particular, was an incredible person because, not only did he cope with all the higher theory and stuff like that, but, if there was something to be made, he used to go into the little workshop that we had and make it himself. It was absolutely incredible. I think they were really without equal.

This versatility was most important because the early work for the Army was very experimental and practical in nature, and involved an enormous amount of field-work. One advantage we had was that in those days Richmond Park was closed, and we used it as a trials and experimental area; most of the radar work could take place there. One of the earliest studies we had was on the effectiveness of the AA gun predictors. There were two types of predictor: there was a Sperry predictor and a Vickers predictor, both very similar. The idea of a predictor was that you fed into it data on the target aircraft's co-ordinates, its output was multiplied by the time of flight of the shell, and that indicated to the AA guns the future position at which to fire. This was fine in theory, but the settings were derived from a variable rate mechanism fed by manual inputs, which depended on the speed and the movement of the target aircraft. These inputs were not very steady. Sometimes they lagged, and sometimes they led: anti-aircraft gunnery really wasn't very good in those days.

Another thing, of course, was engagement at night, and this is where the radars were coming into being. I was first put into a team led by L. E. Baylis, a physiologist. Andrew Huxley and D. K. Hill (the son of A. V. Hill) were in the team and were also physiologists. So we were a group of physiologists studying anti-aircraft gunnery, which I suppose you could consider a bit odd! Experimental data used to be fed from the predictors second by second, by stop-watch. It was very elementary. Every second, a bit more data would be fed in and the future position of the aircraft recorded. But there was a limit on how well you could do this. So they designed what they called a 'top hat' machine. It is interesting to speculate on how people's backgrounds really colour the way they do things. In physiological labs, when you recorded data, it was all very crude. You used to record it on a big drum with smoked paper round it—a thing called a kymograph. Well, they developed this thing called the top-hat machine to feed three-dimensional data into a predictor. It consisted, in its early stages, of three rotating drums with graphs on them, a handle and a knotted cotton which fed onto a magslip. A magslip was an electrical transmitter which transmitted rotational data down a cable. One drum was for bearing, one drum for range, and the other for elevation. This meant that you could feed in data continuously instead of bit by bit, and thus plot a continuous course. Although this was a very crude piece of apparatus, it really did the job. We could feed in any sort of course that we wanted; we could try fast-moving courses, changes in heights, and all that sort of thing. We used to have a number of young ladies operate this machine, and I think the young people here ought to be conscious of the influence of this sort of thing because, 3 years later, I married one of them!

There was another quite interesting little study that was done. In the Thames estuary there were a number of forts; I think they are still there. The Stuka dive-bombers used to come over and knock hell out of these forts, and there was no way in which either the Sperry or the Vickers predictor could cope with this because they needed the aircraft to fly on a nice straight and level course. There was a lot of thought given to how to combat this threat. The rate mechanism in the predictor was controlled by a cam. After lots of thought and lots of headache, Andrew Huxley took the side of the predictor off and cut a new cam out of plastic, shaped to be capable of dealing with a very high rate of change in aircraft height, and this actually solved the problem!

The work in Richmond Park was mostly concerned with the early gun-laying radars—the really early ones, the GL Mark I and Mark II. The GL Mark I only gave range and bearing to the target. It wasn't until later that angle of elevation was also given. I don't know if any of you remember the GL Mark I. It was nothing like the modern radar, because it was in a great big cabin, and it had two big dipole arrays on it. It used a null method for bearing, and the reflection time for range. You used to control it by rotating the cabin using a sort of bicycle-wheel handle—none of the modern things we have these days. And one of the problems was how accurate were these radars? When I was at Poole, they had one of these radars sitting on the coast and, oddly enough, they were run by the RAF at that time. The only thing I knew about them then was the rumour going around that the poor chaps who operated them were getting sterile! Just the sort of story that used to go around in wartime!

To study the accuracy of the GL Mark I, we used to spend hours (literally from dawn to dusk) following an aircraft with the radar, checking its position with a kinetheodolite and then comparing the two. You can imagine the hours of work that went into this. We used to use a Lysander for this work. Richmond Park at that time was criss-crossed with telephone wires and power cables, and we had one pilot who would come in at first light every morning—we would hear him on the radio quite a long way off, 'Brandy one-two approaching'—but his signal that he had arrived was to come down and whistle under the telephone wires.

This radar work went on for several years, probably to late 1943. As the radars developed, they became more sophisticated. The radar Mark I had a system of two dipoles added to it, the signals from which were mixed in a thing called a goniometer. Thus, the GL Mark II at last gave us the angle of elevation to the target. But the greatest progress came in about late 1942 with a new radar, the Canadian Mark C3. This was a marvellous piece of equipment because it was like a big caravan with a dish on the top, so you hadn't got to pedal the whole thing round. It was whilst doing this work on radar and looking at the sky that J. S. Hey, in about 1943, noticed that there were some spurious signals appearing from space. And it was this observation that really formed the basis of his work on radio telescopes and helped in the institution of the Jodrell Bank telescope. Hey later won the Appleton Medal for this research.

I'd better continue by saying a few words on how the group changed. We started off purely on anti-aircraft problems, because that was the link between the original Blackett group and the AA Command School. But gradually other things were creeping in. For instance, Omund Solandt had formed a unit at Lulworth doing operational research on tanks. So, in about 1943, the group moved to a larger house at the other side of Richmond Park, and started extending the whole scope of its work. Radar, for instance, had become far more sophisticated. Originally it worked at about 5 meters wave lengths (I think about 60 megacycles), but centimetric radar had started to come in and was much more accurate; things like wave guides were starting to be used. Tank and anti-tank warfare studies were commenced in the establishment. We were doing research on mines. We were also doing field research on soil mechanics because, in the First World War, in 1916, when the tanks went in, an officer had to go ahead and prod the ground to check they would not bog down. To this day the Royal Tank Corps have as their stick an ash stave. There was a lot of work to be done on airborne systems. One of the problems was that the chaps used to jump out with a damn great load, a hundredweight or so, strapped to their legs. You can imagine landing by parachute with this on your legs. So they started developing systems in which the equipment bag was suspended from a sort of friction device, which slowed it down when it was dropped on leaving the aircraft. The bag hit the ground first, and then the parachuter came down.

I did a lot of work for the invasion. One particular thing I worked on was a special-purpose equipment to guide tanks in at night. This was one of these bleepers that gives a high-pitch reading one side and a low-pitch one the other, and you null them to obtain the correct direction to drive in.

Earlier there had been much work done on the air defence of London. There were thousands and thousands of forms (ZZ forms) that were filled in for every shot that was fired at an aircraft. Initially I believe that for every bird hit, 30,000 rounds were expended. It came down after a while to about 2,000 rounds per bird. This was an example of really applying operational analysis to every gun site in London, and the analysis was being done at the time the events were taking place. Indeed, it turned out that there was much more danger from falling shrapnell in those days in London than from being hit by a bomb!

And now to conclude. As I have said, OR started off very much as field-work. I sometimes wonder whether nowadays, with so many computers about, we tend to get a bit out of touch with what reality is. None of us in the early days was trained in OR, or indeed intended to go into OR.

I personally had hoped very much to go back to University College after the war, but I didn't because they only offered me a third of the salary that the Civil Service offered me.

I think OR is an attitude of mind. You have to be inquisitive; you have to keep in mind what you are trying to look for; and, certainly in military OR nowadays, you must not forget the customer. And after all, it is the customer that counts and who wants help.

Big simulations are alright as long as we do not forget the man in the system. I think I was very lucky in having to serve a lot of my time as a Scientific Adviser in the field. It changed my views completely. When I first went to 1 BR Corps, I was full of ideas about war-gaming and how you should do it, and how the battle should be fought, and so on. When you get onto the ground and into the mud, you change your ideas completely!

I think that, with modern electronic equipment available to the OR worker, we can now study problems in depths that were not possible in the early days. However, it is important to remember that, in doing so, the final value of any OR study rests on its validation in the field.