Papers Presented at a
Conference on Operational Research

Dr. H.D. Smith, President of the Nova Scotia Research Foundation, was chairman of a conference on Operational Research, which was held on June 10, 1964 in the Sir James Dunn building at Dalhousie University.

This conference was sponsored by Dalhousie University, the Nova Scotia Technical College, the Operational Research Branch of the Maritime Air Command, Stevenson and Kellogg Limited, and the Nova Scotia Research Foundation.

The purpose of the meeting was to arouse an interest in the application of the techniques of Operational Research to the industries of the Atlantic Provinces.

As part of the 50th Anniversary Celebrations for CORS/SCRO, the students of the 2007/08 IENG 3311 (Design and Modelling of Industrial Systems) at Dalhousie University transcribed the material in the conference proceedings from paper to electronic format. The objective of this assignment was to make an important historical document available to a wider audience and to provide incoming Industrial Engineering students with some knowledge about the important role that Canadians played in the early development of OR.

The conference proceedings contain papers by Harold Larnder, Cecil Law, J.R. Walter, and D.A. Grant. Dr. Omand Solandt also attended the meeting and gave a plenary talk, but the content of the talk was not recorded. Below we have reproduced the material from Harold Larnder's introductory presentation on the early history of OR. The material has been reproduced as faithfully to the original as possible, including font, page numbers, page breaks, and typographical errors.

John Blake
Halifax, NS
October 2008

THE EARLY HISTORY

OF

ORGANIZED OPERATIONAL RESEARCH

Paper presented
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Dalhousie University
June 10, 1964

By: H. Larnder

Systems Analysis Group
Defence Research Board
Department of National Defence

THE EARLY HISTORY OF ORGANIZED OPERATIONAL RESEARCH

Mr. President, Gentlemen:

It is indeed both an honour and a pleasure to have been invited to address this distinguished audience on the early history of operational research. It is also most gratifying to me that on this occasion we are assembled together at my old Alma Mater, Dalhousie University.

Modern practice of operational research, and the techniques now in use will be dealt with by subsequent speakers. However, early history is often relevant to the present since many of the lessons learned then are still applicable not only to peacetime military operational research but also to peacetime non-military operational research.

This is perhaps particularly the case when the results of experience help to establish the environment necessary for the unsuccessful carrying out of his task or tasks by the operational researcher. To establish a harmonious relationship between two sets of individuals those whose function it is to examine and advise but who have not the responsibility of decision making and executive action on the one hand, and those who do have to make decisions and then accept the responsibility for their acts, on the other and is not always too easy of achievement.

What is required is a mutual understanding of each others problems and abilities, followed by team work directed toward improving the effectiveness of the organization which they jointly serve.

It has been said that radar is the mother of operational research and this is indeed the case. Just as necessity is the mother of invention so the problem of integrating

the powerful new technique of radar successfully into the air defence system of Great Britian [sic] lead to the need for an application of the scientific method, which subsequently became known as operational research.

To tell the story of the early days of operational research, therefore, it is necessary to refer, briefly, to the early days of radar, and since I was fortunate enough to have participated in the early development of radar and to have lead the team which pioneered operational research, I fear I shall be forced into talking about many personal experiences. In the interest of brevity I must also limit this talk to the early history of that pioneer section and the experience gained in working with but one of the three fighting services.

During the early 1930's British authorities became very concerned with the vital problem of the air defence of their island in the event of a war with Germany, and in 1934 the Air Defence Committee was formed with Sir Henry Tizard as Chairman. The basic defence problem stemmed from the fact that Great Britian [sic] being an island, and a very small one at that, no part of which lies further than 70 miles from the coast, it was impossible to place observation posts of any kind at a sufficient distance beyond the periphery of our target areas to supply adequate warning of the approach of hostile aircraft. Sound-locators and search-lights were the only methods of warning then available, and neither had sufficiently great range to give warning early enough to permit us to get our fighters airborne and up to altitude in time to engage the enemy before he had penetrated to the vast majority of our targets.

Since research held out no hope of extending the range of sound locators or search lights by any worthwhile amount, the committee was forced to consider other alternatives. One alternative method of countering the threat would have been to devise some weapon whose speed-into-action and time-of-flight would be so short that even though an enemy aircraft were detected only a few miles off the coast, it could still be destroyed or its pilot killed before it could inflict any damage. The committee, therefore, turned its attention to possible weapons which might have these characteristics. Anti-aircraft artillery, though possessing some of the desired characteristics, was not considered to possess the required lethality. The only weapon the committee could envisage was a “death ray”. A number of eminent scientists were approached and asked if they could possibly suggest how a death ray might be produced. Among the answers which they received was one from a young meterorologist [sic] named Watson-Watt (now Sir Robert Watson-Watt) who said, in effect, “I do not know how to invent a death ray, but if you were to ask me to devise a system which would detect aircraft while still sufficiently far away from our coasts to permit out fighters to become airborne and to intercept in time, I could.”

This episode illustrates a most important point which should never be lost sight of when seeking advice or aid in the solution of a problem, and that is always state the problem completely and do not seek to limit the possible solution to any one method. Although the Air Defence Committee comprised some of the most eminent [sic] scientists in Great Britain, they were guilty in this respect, and only by good fortune did it happen that Watson-Watt, the man with a new idea knew the whole picture and was thus able to realize that his idea held a solution to the problem.

Watson-Watt backed up his claim by a series of calculations and one very simple experiment, and he was so convincing that the Committee has no difficulty in obtaining authority for the very considerable scientific and financial backing required for the urgent fullscale research, which ultimately led to the development of what we now call radar but which, in those days, was called RDF – Range, Direction, Fix.

To recruit his initial team, Sir Robert Watson-Watt made a personal tour of most of the universities of Great Britain culling one scientist here, another there. They were for the most part young men who had received either a very good Bachelor's degree or had completed their Master's thesis – with an occasional PhD thrown in for luck. In addition, Watson-Watt drew other scientists and engineers from various establishments throughout the country. At a later date, when many of the problems were an entirely different nature and quite unrelated to the world of theoretical or applied physics and engineering – members of the original team found no great difficulty in grasping the essentials of the new problems and in proposing solutions.

In May 1935 the team commenced work under very primitive conditions in huts on the island of Orford Ness, and such was their enthusiasm that by July of the same year ranges up to 39 miles had been obtained on known aircraft. This substantiated Watson-Watt's original calculations, though the actual equipment and its performance was somewhat unreliable. The following year, 1936, the Air Ministry acquired Bawdsey Manor near Felixstowe, Suffolk, turned it into an experimental station and all future Air Ministry and Army pre-war work on radar was carried out there.

It was not until late 1937 that we realized it would be necessary to extend our studies beyond the exclusively technical field necessary for the development of radar equipment. Prior to that time the apparent successes of our original efforts in detecting and tracking approaching aircraft up to ranges of 100 miles or more, once we had developed reasonably reliable equipment, had given us a false feeling of optimism.

Nemesis overtook us on the first practice exercise when we tried to feed our beloved RDF into the general reporting and tracking system of the RAF. The result was mortifying; we apparently contributed more confusion than help because our observations on approaching bombers not only disagreed with the information being fed into the same system by Royal Observer Corps, but also with tracks actually flown by the bombers.

In fact the aircraft detecting, reporting, and interception systems of Great Britain both pre-war and for the first 12 months of the war was, in a technical sense, rather a “hodge-podge”. Over sea enemy aircraft were tracked electronically by radar, overland acoustically or visually by sounds locators, search lights or the unaided human eye, whilst our own fighters were tracked by Ultra High Frequency Direction findings. No wonder our radar plots did not at first agree with anything else!

It was not till a year after the outbreak of war that a radar equipment suitable for both tracking the enemy and positioning our fighters under actual operational conditions was developed.

In a later exercise, when we increased our participation by feeding-in information from additional RDF stations we added still a further form of confusion to the overall picture in that the information from some of our stations was in complete disagreement with information from others. We derived a little, but only a little sardonic satisfaction from the knowledge that we were not the sole offenders in this regard, and that the Royal Observer Corps was having a similar trouble with correlating the information provided by its posts and sectors.

To set our own house in order we began, in July 1938, to expand our activities. One section, under the direction of E.C. Williams, in addition to instigating an intensive program of calibration of the radar sets themselves, also studied the amount of degradation of information which occurred when the equipment was operated on a rotational watch basis, not by trained scientist familiar with the special RDF procedures and principles, but by “lay” operators, i.e. by RAF personnel (airmen at first and later WAAF). It further studied the degradation which resulted as the “plots” were “told” down the main systems lines and finally displayed in a central filter room, after which they were correlated with information received from the Royal Observer Corps, before being finally displayed in the command operations room.

After we began to achieve some measure of success in this “non-radar” field, we were asked to extend our studies to the operations of the Royal Observer Corps system. This work, as well as a study on display of information generally, was placed under the direction of G.A. Roberts.

As further progress was made in understanding and improving the detection and reporting system as a whole, it became obvious that the problem of how to vector a squadron of fighters to successfully intercept a raid of bombers, even by daylight, required further and continuing study. This would, we realized, be even more necessary for the case of a single fighter against a single bomber during hours of darkness. Some work had previously been done for the over-land-by-daylight case by B.G. Dickens; we now decided to make a thorough review of the problem and to extend his study to include over-water conditions and into the hours of darkness. I.H. Cole was charged with this project. Truly we had by now moved well away from our original program of technical problems in the field of electronics, yet we found our staffing needs well met by the abilities for the personnel of the early team; Watson-Watt had chosen well!

In mid 1939, Sir Hugh Dowding, the Air Officer Commanding-in-Chief of the RAF Fighter Command, and Mr. A.P. Rowe the then Superintendent of the Bawdsey research station, discusses the service-science relationship with regard to RDF and agreed not only that further research and development of RDF itself must continue after the outbreak of the war which was now imminent, but also that there was a need for continued research into the whole reporting and interception system which would undoubtedly continue to grow in size and complexity. Sir Hugh therefore, officially requested that Mr. Rowe permit the combined Bawdsey System teams – of which I was the leader – to continue their work after the outbreak of war and to make his headquarters our case of operations. The request was granted in principle and became effective the day

Britain went to war. Originally it was thought that we would return to the parent establishment – after a few weeks, or months at the most – it being assumed that by that time the scientists would have exhausted their contribution, and the prosecution of the war could be left to the military.

Amusingly enough, the first of two problems confronting us shortly after our arrival at Fighter Command HQ was not a scientific or operational nature – it was the apparently mundane question of deciding on a name for our section. Since my team and I had become completely convinced of the need and value of applying the scientific approach to problems in the operational system as a whole we had no great desire to be recalled to laboratory work if we could help it, at least during wartime. It seems, therefore, that the choice of a name might deserve more than just a little casual thought. I felt that if we could associate ourselves with both Fighter Command HQ and the Air Ministry Experimental Station Aberdeen, the new location of our parent scientific establishment, in such a manner that both Sir Hugh Dowding and Mr. A.P. Rowe each felt we belonged to him, we stood a better chance of continued support and encouragement by Sir Hugh and less chance of being recalled to Aberdeen to avoid “contamination by the services”. To have called ourselves “The Fighter Command Research Section” would have pleased our RAF colleagues and our own romantic inclinations, but would certainly have provoked not-unjustified cries of “Foul Play” or “Traitors” from Aberdeen. The other obvious title of “Out station of A.M.E.S – Aberdeen” would have made us appear to the RAF as Air Ministry spies in their midst and, as much, none too

welcome. I decided to try “Stanmore Research Section” and, for the time at least, this seemed to answer our problems. Sir Hugh affectionally [sic] thought of us as his research section, based at his HW at Stanmore, while A.P. Rowe when showing the organizational Chart of his Aberdeen Research Establishment to visiting politicians and other “high brass” would proudly refer to us as “my research section at Stanmore”. In this way we were able to rely on the good will and assistance from both sides which were the sine qua non of successful work in this new co-operative venture.

The second problem confronting us was that of fitting in with the general staff structure of the Command, a similar problem to that confronting a present day Operational Research Section in either military or civilian life.

Because of our original connection with the technical development of radar, it was proposed that we should operate as part of the Chief Radar Officer’s staff; in fact, for a very short time we were known as the R.D.F. Research Section. As we were already researching into other branches of the Command’s activities we felt that this would restrict our usefulness. This view was accepted and we became responsible directly to the Senior Air Staff Officer who, under the A-O-C-in C, was responsible for all the command’s operations. This decision was not only correct but it had far reaching consequences, and at a later date, when Operational Research Sections were established throughout all commands of the RAF, the practice of placing the sections directly on the staff of the senior officer in charge of operations was always followed. The importance of this cannot be too strongly stressed,

whether the organization being served is military or civil. It provides for two essential features necessary to the successful use of operational researchers, firstly it places them in a position to become aware of the complete background of any problem needing attention, secondly an economy of effort can be obtained by detaching individuals to work as and where necessary. It subsequently became the general practice that the Senior Operational Research Officer should have a direct link to the Commander-in-Chief as his scientific advisor. However, I must not get ahead of my story.

For the first several months of the war by far the major portion of our activity was the continued study of the aircraft reporting and interception network. We were researching into a complex military operational system of very considerable magnitude and were pioneering the team approach as an integral part of the Command's staff. As with most pioneers we learned our lessons, on occasion, "the hard way". Specifically, we soon found that the use in our studies of observations made or data compiled by officers, well meaning but unpractised in precise observational methods, often lead to erroneous conclusions; as a result, our attitude towards the collection of data was to adopt the motto of the mongoose family of Kipling fame, "go and find out". Secondly we found that the use of tact paid high dividends; it was most essential that our efforts be viewed by any unit we visited as efforts to help them overcome their problems, and not to show up any of their shortcomings. So strongly did I fell on this score that we adopted the practice of frequently allowing the head of the unit visited to submit our findings as his, over his own signature. I must confess, though,

that on at last one memorable occasion I failed in the practice of tact myself. In addressing a memorandum to the Senior Air Staff Officer dealing with a German air raid on Rosyth, a Scottish port, in the second month of the war, I started off by stating that ¨owing the unnecessary presence of the Commander-in-Chief, the Air Officer Administration, the Senior Air Staff Officer, and several others in the Filter Room (where admittedly they should not have been!) the work of the filter officers was greatly handicapped and our defences were considerably confused.¨ The SASO, who happened to value the report, showed it to the A.O.C-in C and then spent three hours arguing as to why those so and so civilian scientists should not be shot out of HQ immediately. Luckily for us, the SASO won this battle. I had learned my lesson. The sequel was surprisingly pleasant – the A.O.C-in-C asked that we analyse all future raids on Great Britain, and this was done for the reminder of the war.

Another practice which we adopted was to encourage requests for our assistance, on worthwhile problems, from all levels. It seemed to me then, as it does still, that if scientific assistance in the solution of ¨high brass¨ is welcome and a good thing, then that should be true at all levels, and leaders of lower level formations should be applauded rather than criticized when they recognize a problem and request help. This is a practice which should be pursued by all Operational Research Groups which are an integral part of any organization, civilian or military.

As I have said, initially, and for several months our activities were confined to researching into the Command’s operational system.

We were not encouraged to meddle with or offer advice on actual operations as such, or the many logistic problems which confront any large organization. We were not too content with this state of affairs, but we had to posses our souls in patience and hope that a suitable opportunity would come our way – eventually it did. One Sunday morning in May 1940, Sir Hugh called me into his office. He explained that he was to appear before Churchill and the Cabinet that afternoon to consider whether or not we should reinforce the French by sending over and maintaining ten additional fighter squadrons in France in response to an appeal from the French Government.

At this time the Germans were moving about pretty freely in Europe on the ground, with very good air support of their own. We ourselves were fairly fully extended in maintaining the ten fighter squadrons which were already operating in support of British Forces in France. The Commander in Chief felt that for military reasons it was quite wrong for us to attempt to reinforce France. Partly because he was convinced that even if we did, it wouldn't stop the Germans, and partly because his whole command, his whole organization, his whole reporting system, his whole ¨everything¨ had been carefully built up over a considerable period of time and with great effort for the primary purpose of the defence of Great Britain when the Battle of Britain should occur, as it ultimately did. He had a very strong conviction that if, as a result of acceeding to this request, his force was not intact or at least reasonably strong, when the need arose to use it in its proper role, then we would be taking an unjustifiable gamble. These were his military opinions. He asked if we scientists could give him a paper which would

throw any light on the question and could he have it, please, before he left for Whitehall in two hours time!

Let me pause and advise operational researchers here and now, never poke your nose in the office on a Sunday. There will always be some senior officer there who has a problem requiring immediate solution!

In such a short time, two hours, one could only pick on one possible consequence of reinforcing our French allies, and deal with it in a simple manner. The important question seemed to be to indicate how long we could sustain this entire effort. The figures used were simple, and I remember them well.

At the time we had 900 fighters, either on our airfields or in the pipeline from the factories. Our production rate was 14 air-craft per day, and our losses (maintaining the ten squadrons already in France) was averaging 17 per day. Net loss 3 per day. If we doubled our activity by committing a further ten squadrons it seemed reasonable to assume our losses would double also to 34 per day, or a net loss at
20. You will note that doubling our daily losses pushed up the net loss by a factor of 7.

One question remained to be asked—what was the lowest level to which Sir Hugh considered it safe to let his force sink? He replied 300 fighters, but added that he considered that would be dangerously low. How right he was! *

*During the Battle of Britain, which commenced only two months later, although the operational strength of Fighter Command averaged 650-700 aircraft victory was not attained easily, and during the period August 24 - September 6 the scales actually tilted (temporarily) against Fighter Command. (Page 330—“Their Finest Hour” – Churchill)

The arithmetic was now simple. We had 900 aircraft, we could drop to 300, so we could afford a net loss of 600.

At the then rate of commitment with net losses of 3 per day that would take 200 days or say, 7 months. But if we doubled our efforts and suffered a net loss of 20 per day the 7 months shrank to 4 weeks.

In 7 months anything could happen, including an increase of production. In 4 weeks no worthwhile increase could be expected, and in fact the decreased defence of our industry might well provoke an attack which would actually decrease our production rate.

Reference to this incident is made in Air Ministry Air Publication 3368 entitled “The Origins and Development of Operational Research in the Royal Air Force.” published 1963.

I would quote, in part, “though the mathematics were of the simplest nature and the figures in loss and replacement very crude,”--that certainly cuts me down to size--“this use of the O.R.S. by the Air Officer Commanding-in-Chief when considering a serious point of high level staff policy forms a notable milestone in the progress of the idea of operational research”.

At the Cabinet Meeting, Sir Hugh felt there was one man he must convince--a man seated on the same side of the table but at some distance from him. After a few minutes he had the impression he was making little headway. Experience had taught him that he could sometimes persuade people through their eyes where he could not do so through their ears.

He got up, walked behind five or six seated figures and put his graph in fromt [sic] of the Prime Minister.

In Dowding's considered view "That did the trick". The outcome was decision not to send the ten squadrons, and a few days later all but three of the ten squadrons already in France were recalled to England.

Nowadays, of course, the presentation to decision makers of calculations designed to indicate the possible outcome of alternative courses of action is a well known practice in operational research, military or civil, and on occasions can be quite illuminating.

Starting from this, after eight months of war, we found ourselves being consulted on an ever widening scale wherever it appeared that a problem was susceptible to a scientific appreciation of analysis. There was now no likelihood of our being recalled to our parent research establishment and it was apparent that we were to be allowed to continue our efforts for "the duration". Previously we had expanded our team from its small initial start, and continued to do so. Mostly we drew our requirements from a very well organized pool, and considering that we were no longer performing any identifiable branch of science it always seemed a wonder how well our staff needs were met.

It may be of interest to touch on the question of who makes a good operational researcher. In our experience it appeared to be essential that an individual had been well trained in either science or engineering, and had thus developed the discipline of approaching all problems with an open mind, in addition he required to have the ability to observe and collect all relevant facts, plan and conduct experiments on occasions and then draw such conclusions as were justified. It was also most important that he should be able to get on well with other people and to sympathize with and understand their problems. Finally,

he must be prepared to research into any question even though it was quite outside his previous specialized knowledge. After the very early stages none of us were engaged on those problems for which we had received specialized training. We became instead practioners of the scientific approach. It became a case of “Have science – will travel”. One very good example is that of Dr. C. Gordon, a geneticist, who carried out the very excellent pioneer work on planned flying and planned maintenance, and who—during the war at least—had to discontinue his experiments with breeding flies. In his book, written after the war, in referring to the versatility of many of the operational research scientists Dr. Gordon wrote “The complete disregard for frontiers between the different subjects, and the readiness to accept any problems as within their terms of reference has been a refreshing contrast to the rigid specialization that has developed in all other branches of science. The operational research sections have recaptured the atmosphere of the period of the foundation of the Royal Society”.

Thanks to the enterprise of many universities, it is now possible to recruit men who have been trained at least in the methodology of operational research. In the early days it was a matter of beating the bushes and hoping for the best. We were really very lucky though, and I can only remember one case where the individual was a mis-fit. However since he is presently a Cabinet Minister, and often tipped as a future Prime Minister of Great Britain he must have had something on the ball somewhere.

Our last contact, as the Stanmore Research Section, with Sir Hugh Dowding was on the occasion where he turned over his command to

Sir Sholto Douglas early in 1941. On behalf of the scientists I wrote him a farewell not, wishing him well, and thanking him for the support he had given us, especially in our early day. He returned the memo, having written across its' top "Thanks. This war will be won by science thoughtfully applied to operational needs." H.D. An optimistic and encouraging prophesy made during a depressing stage of the war.

The Air Staff, both at Air Ministry and Command levels, Decided in June 1941 that the Stanmore Research Section had thoroughly proved its value, and favoured setting up further sections. This was done, and those new sections were, after some discussion called Operational Research Sections. At the insistence of Sir Sholto Douglass, the Fighter Command O.R.S was thenceforth made solely responsible to him as the A-O-C-in-C, a practice adopted by all other commanders-in-chief. Further the Senior Operational Research Officer in each section were accorded the title of Scientific Advisor to his respective Commander-in-Chief.

Thus, after some 22 months of war time experience, this new practice of science was officially recognized, legitimized and christened; to be known henceforth throughout the Commonwealth as Operational Research, and as Operations Research south of the Canadian Border.

To those of us already in the game this change of status brought satisfaction, but no change in our tasks. The work we had been doing changed in no way after we had acquired our new description from what it had been before. What did change, however, was that the environment in which we worked improved. We were now acknowledged as an integral part of a team, the scientific advisory component which

worked amicably and easily with the executive or decision making component. All petty jealousies had been overcome with the passage of time; and mutual understanding of our duties and our usefulness, as well as our limitations, vis-à-vis the other component of the staff structure had been arrived at.

One further goal reminded to be attained. Although our advice and assistance was sought over a wide range of problems, although we worked at both low and high levels, and although we were excluded from no areas of policy or action, there reminded the fact that our energies and recourses were completely absorbed by investigations instigated by other people. – the executive, the administrators, the decision makers.

We had, however, our eye on still another costumer, namely ourselves. We reasoned that we were singularly well situated, and qualified, to determine some problems that not only required solution but which were also mot susceptible to our methods of attack. In other words, we wished to be able to initiate problems ourselves, regardless of whether or not the Command felt they were worth tackling.

To obtain official agreement to reserving a portion of our resources to this end was not too easy. At first we got around this difficulty by ¨selling¨ a problem to some carefully selected officer as me which ¨he¨ would like solved. However, this wasn’t really satisfactory as a higher ranking officer could, and often did, step in and rsell our position. Nevertheless, we persevered, and in 1943, the Officer in Charge of Coastal Command O.R.S., I finally got written

into my terms of reference authority to utilize a reasonable proportion of my teams efforts on such problems as I should initiate myself and that these problems were to receive the same cooperation by his staff as if the C-in-C had personally authorized them. That this concession was effectively used by us attested by a paragraph, written by Marshall of the Royal Air Force Sir John Slessor in his book “The Central Blue”, in which he says of the Coastal Command Operational Research Section

“They proved beyond doubt that the scientifically trained analytical mind, applied to any problem, could produce invaluable results; and they frequently surprised me by telling us, not only what we did not know, but what otherwise I should never have realized was something we ought to know about an operational or administrative problem.”