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RESEARCH—ACADEMIC AND INDUSTRIAL

By Sir HARRY MELVILLE, K.C.B., F.R.S.*

SYNOPSIS

This paper discusses the factors that encourage productivity and profitability in Research and Development.

The Reasons for Research

The topic of this paper deals with one of the most difficult problems in research and development. It is a problem which everyone who has to do with R & D considers very carefully for, in the end, the reason for doing R & D has to be justified to authorities, private and public, who provide the necessary financial means for doing it. At one extreme there is the theory of blind faith—without R & D, an enterprise will simply not prosper so it must be done—at the other extreme is the narrow approach where the profitability of a project must clearly be seen before expenditure is authorised. As usual the real position lies between the two; it seems to depend on the industry concerned, and it can vary a great deal depending upon profitability in industry, government expenditure and many other factors which determine the way in which the money can be found.

However, before considering the matters mentioned above, we must remember that the first requirement for high productivity in research, the production of new ideas, novel techniques, new materials and processes and the exploitation of these, is to ensure that manpower of top quality is selected and trained in suitable ways. This is especially important in the United Kingdom where the supply of manpower of the requisite quality is strictly limited—not always by lack of money and facilities, but by sheer ability. Under the present system the universities and colleges train to first-degree standard, financial support being provided from the University Grants Committee, the Ministry of Education, and local education authorities. After that the responsibility is a divided one for the good reason that the financial requirements for carrying out research and for training research students are quite different and much more highly selective than those for undergraduate teaching.

In encouraging and selecting those graduates who are suitable for postgraduate study several factors have to be considered. The first is ability; if the candidate has not got the necessary intellectual ability and sense of pioneering it is a sheer waste of money to contemplate training him for research. The next question is whether all such people should do research at universities. Should they not go into industry or research institutions straightaway and learn the job by doing it, instead of the more gradual transition through studies in the universities? The fact is that quite a number of students do decide to do this, and their choice naturally is completely free.

The Atmosphere of Research

In discussing this matter different viewpoints arise with regard to pure and applied science. In pure science there is no doubt that the student should stay at the university for research. There are many reasons for this course. The first is that such a student really needs further close supervision and instruction in the ways of setting about original investigation, and this can best be done by the close personal contact with his immediate supervisor. This would in general be less likely to happen in the course of normal employment where, naturally, the man is employed to attempt to solve problems which are currently besetting the industry concerned. What is more, in this country, with only three, or sometimes four, years for undergraduate instruction there is not sufficient time to impart the knowledge that must be acquired for doing effective research. Thus the tendency should be, and actually is, to extend instruction into the postgraduate phase of a student's career. Properly done this means that the student's training is broader, and that therefore he should be more adaptable in later life when he has to tackle problems on his own and maybe on a wider basis than he studied in university. The other important reason is that advances do, in fact, occur in the inter-disciplinary fields, not by accident, of course, and it is necessary to receive considerable instruction in the neighbouring field if effective research is to be done in the regions between well-established disciplines. This cannot be done in the undergraduate training period for, apart from lack of time, the student does not then know into which field he will be going to do research. This country has been at a disadvantage compared with the United States of America and Continental Europe in this connection, but the matter is gradually being put right. These changes will certainly meet some of the criticism levelled at the nature of training of people who have the Ph.D. degree.

A further reason for maintaining so many people doing research in universities (DSIR in fact supports about half the British students receiving postgraduate research training in the country) is that universities in general do the fundamental research. It might be argued that it would be better if the universities set up research institutes staffed with people whose sole job was to do such research. In consequence it might be done more expertly and maybe more profitably, simply because the constant flow of new research students through a department means that the rate of progress cannot be fast when they are just beginning to learn the ways of doing the job. In these days of complicated apparatus and teams of workers it might be thought that the research institute is the right way. Somehow, however, this does not seem to work. In the atmosphere of the research institute the work tends to be done competently but without the right kind of inspiration that comes from the constant contact of young research students with the more mature and experienced academic scientsts. It may be that this work is not quite so well done or even so complete in character, but with fundamental work what is most important is that it shall be original. There may be gaps and even a lack of precision,

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but these deficiencies can be rectified by others once the initial line of approach has been clearly defined. Productivity in this kind of research is not easily measurable—it is not a matter of a number of pages in reputable scientific journals.

The Provision of Equipment

But there may be barriers to the realisation of the full potentialities of academic research. These can arise in the lack of sufficient apparatus and technical assistance, always assuming that buildings, libraries and the like are already provided through the usual university system. In the physical sciences the DSIR has made tremendous efforts in recent years to provide the necessary additional and expensive apparatus needed for research. This has supplemented the very substantial sums of money spent by the University Grants Committee in equipping the many new laboratories being built in the universities. Though no one will ever be satisfied in this field, my own feeling as a result of visiting university, government, and industrial laboratories in this country is that the universities are now reasonably well supplied with such equipment. Even by United States standards we have nothing to be ashamed of. It is not now a matter of having one electron microscope, or one nuclear magnetic resonance machine to a department, but of sometimes having several. There are very many departments in British universities which are supplied with a range of such equipment through the DSIR grants scheme. There is no doubt too that such apparatus, as it gets more complicated, will continue to be supplied by DSIR and other bodies so that this country can be kept in the forefront of progress of this kind. So from that standpoint productivity ought to be reasonably high.

Technical Assistance to Research Workers

In the matter of technical assistance the situation merits examination. Here the universities have certainly been at a disadvantage compared with government and industrial laboratories. While the full-time academic staff ought to be supported by adequate assistance of this kind so that they do not waste their time on purely technical jobs, the question is whether the research student should be similarly supported. This would, in my opinion, be wrong. It is right that such workshop services as mechanical equipment, glass blowing, and electronic workshops should be supplied but it is essential that the student should receive some training in these skills. This will teach him to appreciate what is done for him and to be able to talk to technical workshop staff in an intelligent manner so that the required job is done as a result of collaboration between the scientist and the technician in the workshop. If the student is pampered and supplied automatically with too much of this type of assistance at this stage his initiative and determination to do a research job may well be weakened; the result will be that the work will not be of such high quality, nor will he be given a real incentive to get over any difficulties himself.

Research in Applied Science

The remarks so far apply to pure science. The problems in applied science, particularly in civil, mechanical, and electrical engineering, are rather different. In such subjects as metallurgy and chemical engineering the pattern is to some extent between the pure and the applied sciences, but in general, in this country, the problems in regard to chemical engineering are rather similar to those in pure science. However, it is convenient to take applied science together and here the problems are much more serious.

There has been a good deal of discussion recently on research and development in technology, and there is plenty of evidence to show that it is urgent for this country to improve its technology in all respects, of which research is only one. Without an adequate effort here, technology will simply not achieve the high standing that is necessary in a country such as the United Kingdom. In applied science it is argued, quite properly, that experience can only be gained in manufacturing units or in big development establishments close to actual production and that further research in universities cannot give the right training for applied scientists. But what technology needs is innovation and far-sightedness, coupled with a full realisation of the difficulties of putting new ideas into production in a profitable way. It might be thought that those trained in pure science who go into industry would soon adapt themselves and be able to talk to the practical engineers and so achieve the desired result. This is partly true but is not the whole solution. What we need, in addition, is more able and better trained applied scientists. By training they should be in close touch with advances in pure science and yet be able to see how to apply these results in actual practice. Traditional methods of approach in applied science will not do. There is consequently an urgent need to induce more boys to take up applied science—some of the mathematicians, physicists, and even chemists might be induced to change their line of instruction. This done, such students should be given the opportunity to do postgraduate research in the universitites to a greater extent. It may well be that such work could be very close to production in real cases but the function of the postgraduate training is to give the student advanced knowledge which can help him in later life to accept and carry out innovation as a matter of course. We shall have to make the most strenuous efforts to do this from all sides. If it is not done then one of the most important factors in increased productivity will be missing, and we shall not be able to afford the luxury of spending money in the universities on the scale which is necessary for all aspects of our national well-being. Part of the solution of this problem is in much closer association of industry and government establishments with universities themselves so that the practical knowledge can be brought into the educational system in a suitable way and at an appropriate stage. In this way the practical problems can be kept in mind without actually lowering the standards of fundamental training which are vital at the undergraduate stage. There is need, too, to break down the barriers between traditional departments where the recent research needs a wider field of knowledge and the integration of an inter-disciplinary approach to the solutions and the problems. Some universities have realised this and DSIR has made substantial grants to encourage just this kind of development.

A Career in Research

Now we come to the next stage. What happens to the student when he gets his Ph.D.? At present there are opportunities to continue his personal research by means of fellowships, in this country and abroad (particularly in the United States of America) for two or more years. And there is too, of course, the attraction of academic posts, and there is outside work of industry and research establishments of various kinds. The first choice is naturally the most comfortable and pleasant; the holder of the fellowship does his self-chosen research to add to his personal prestige. He produces results of quality and of an advanced character but the question is whether this adds anything more than the results to the sum total of scientific knowledge. But support for all this comes eventually from the profits of manufacturing industry through the taxation system. So if a high level and quantity of academic research is to be supported because it is a long term investment for the future, it is absolutely essential that technology should be well supported, and that a sizeable proportion of the best scientific brains should be employed in industrial research and development, and in production too. There is an opinion in some quarters that all that is necessary is an opportunity for doing pure research, and no other kind, in the universities. If these claims grow, as they do, and if they were to be met without taking a broader view of productivity in research, then we can envisage that in some fields the postdoctorate output will be wholly absorbed in doing still the same pure fundamental research. The result is no supply of people for university teaching and none for industry. Thus the total support and the employment of manpower at this high level has to be watched with great care, subject by subject. On the one hand some young men feel that this country owes them a living in this kind of way. On the other hand it is up to industry and the various kinds of research institutions to see to it that their programmes and conditions of employment are attractive to able and keen young people so as to give them the utmost scope for the exercise of their ability.

Criteria for Productivity in Research

Having provided the manpower, how can we then choose the topics and determine the strategy that will give the optimum result? There is no secret formula for this or ready-made set of rules to guide us here. In a government department like DSIR, which is obliged to provide services for other departments and for the community as a whole, it is even more difficult. In the main, the customer does not even pay directly for the research, so it is not a matter of a bilateral arrangement between customer and service department. For example, how are we to determine what sort of effort should be devoted to the field covered by the Standards Division of the National Physical Laboratory? The NPL must strive for the utmost accuracy by whatever techniques are available for the job. It may be the precision reached is in fact many orders of magnitude greater than that needed in manufacturing industry. In fact, is the precision needed at all? Switzerland, notable for its very fine engineering products, gets along without an NPL altogether. But the feeling here is that in a big industrial country we must have such a service and a team of men who are accustomed to this kind of work. But its absolute magnitude cannot easily be determined except maybe by comparison with other countries faced with similar sorts of problems.

Similarly consider the Geological Survey. In its narrowest sense it is a purely academic institution concerned simply with the geology of the country. But it has its important economic aspects too. While the United Kingdom is not well endowed with minerals, those it has must be effectively utilised and therefore the Geological Survey and Museum has the most important function to guide the exploiter in the initial phases of the development of new mineral deposits. It may be thought that all is discovered. Not at all! The discovery of large salt deposits in north-west England and the more recent discovery of rocks containing a high proportion of potassium in north-west Scotland show the surprises that can occur. But to estimate the value in monetary forms of this activity is extremely difficult. Thus this effort costing about half a million pounds a year is to some extent an article of faith, long experience having shown that the annual cost probably represents a reasonable limit.

Many more examples of similar services might be quoted. The National Chemical Laboratory service in thermodynamic measurements on pure substances is an instance of particular interest to chemical engineers. So far the effort has been small and, unlike the few physical standards of NPL, the National Chemical Laboratory could actually encompass thousands of such substances. Nevertheless, it is the proper function of a national laboratory to create and maintain the collection of data so that in planning chemical processes the chemists and engineers can, at any rate, explore rapidly the possibilities of processes without necessarily having to go through the whole laborious procedure of trying them out in the laboratory and maybe even a pilot plant beforehand. This I would particularly draw your attention to because in this case it is not just a question of the NCL providing the data. Industry must collaborate with the National Chemical Laboratory in order that the knowledge may be built up in a realistic sort of way, and the necessary compounds examined which are of interest to British industry.

The Profitability of R & D

But, apart from these services, it is possible to try and estimate the profitability of research and development in certain cases. One of the most precise concerns the work of the Hydraulics Research Station. This is a matter of designing harbours, breakwaters, dams, etc., and the problem is to get designs which will be effective but yet cost the minimum amount of money. Since this is a developing science, and since the individual conditions vary so much from job to job, it is not easy for the civil engineers straight away to design schemes that will necessarily be economical. Model work is ideal in this connection for it is easy to experiment, try out, and modify empirically so as to get the optimum performance. Experience is that very large sums of money indeed can be saved by experimental work of this kind. Including the basic work of the station, which is needed to build up a more complete knowledge of this kind of science, the savings identified amount to many times the total cost of building and running the station since it was created. On individual jobs the savings, of course, can be very many times the actual cost of work of carrying out the job.

It is, in fact, in the civil engineering field that, in the case case of DSIR, research and development can be made to pay handsomely. In fact the accumulated savings so achieved can be shown to have paid for the total cost of DSIR since its inception. This estimate, of course, depends on detailed information about the real cost of carrying out experimental work for a defined project. This is not usually difficult to calculate. Where the real difficulty arises is that the creation of the research teams, and the accumulated experience which has been built up over a long period of years, is an element behind these calculations. These costs can be very considerable. Moreover the basic cost of keeping the teams together, so that they can go into action when necessary, is an additional charge. Thus the difficult problem is to try and keep the experience of general knowledge running alongside the solution of practical problems and to maintain a reasonable balance. This is sometimes particularly difficult if the basic knowledge is lacking itself. In some cases the basic knowledge can be obtained from the literature and from current university work. But in many cases it is completely lacking. What is more, universities are often reluctant to accept extra-mural contracts for specifically designed problems when it does not happen to suit their own programmes. The consequence is that, for example, in DSIR stations at least, much of the basic work must be done. However, the economies that can come from the solution of practical problems when properly chosen can often pay for all this rather expensive basic work and make the profitability of the whole exercise when considered over a period of several years rather than over one or two years, to be a practical proposition.

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PRODUCTIVITY OF INDUSTRIAL RESEARCH WITH PARTICULAR REFERENCE TO RESEARCH IN CHEMICAL INDUSTRY

By Sir RONALD HOLROYD, Ph.D., D.Sc., F.R.S. (MEMBER)*

SYNOPSIS

In industrial research it is necessary for every industry to ensure that its efforts are producing profitable applications, although it is essential to remember that there is an inevitable time-lag before research can pay off. Some attempts which have been made to obtain a quantitative measure of the productivity of different types of research work in ICI are described, the studies having been made over a period long enough to allow to some extent for the time-lag.

Research projects are classified into four general types:

(1) Background research in fields in which more basic knowledge is needed in connection with company interests.

(2) Research directed towards improvements of existing products and processes usually obtainable without appreciable capital expenditure.

(3) Exploratory or speculative research aimed at obtaining a lead to entirely novel products or processes.

(4) Research to follow up and develop leads to new products and processes, whether arising from inside or outside the Company.

Assumptions are made upon which the financial " yield " from successful research projects can be calculated, and the yields from all projects concluded over a determined period are set against the research costs.

It appears that the ICI research effort as a whole certainly justifies itself economically (as well as producing various intangible benefits) but the return is relatively modest, and the study was also designed to indicate some of the points at which performance could be improved. Projects which failed were examined, to determine the various reasons for failure; the origins of both successful and unsuccessful porjects were examined; and the relative profitability of different classes of project were assessed. Examples of the lessons learned are:

(*a*) continuing programmes without finite term, devoted to improving particular processes or finding new outlets for some major manufacture, appear decidedly less profitable than specific programmes directed to definite objectives;

(*b*) the most profitable programmes are those in which the discoveries of the Company's own speculative research are exploited; and

(*c*) a much higher proportion of projects originating from discoveries external to the Company fail than of projects originating from within.

To improve the productivity of industrial research generally, it is essential that it should not be an isolated activity. There must be constant interchange of ideas with production and marketing, and continual (although sensible) planning and vetting by people aware of these other aspects of business. Research staff themselves must be trained to be commercially and technologically versatile.

Introduction

It is by no means universally accepted that research in general is an activity which can or should be subjected to productivity investigation. It is argued that most new discoveries result from the work of men with inspiration working on a line which appeals to them personally, rather than selected because of their preconceived importance. We are also often reminded that it is seldom possible to assess the ultimate value of any new knowledge arising from research.

There is, of course, something in these arguments and I for one would certainly agree that purely academic research should be free to follow its own line irrespective of immediate " productivity " in the shape of worthwhile applications. At the same time, I believe that academic research like any other activity comprises work which is outstandingly brilliant, some which is only average and some which is downright poor and, as more and more money and effort goes into this field, there is a growing need to find some form of quality or " productivity " measurement which is more effective than simply counting the number of publications.

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This paper, however, is concerned not with academic but with industrial research and particularly with research inside industrial organisations. Here there is no doubt that productivity is vitally important.

In industry it is quite common to speak both of " Research " and of " Development ", but it is usually very hard in practice to distinguish where one ends and the other begins, with the result that most industrial statistics lump the two together. They are, for example, regarded as a single category in the Federation of British Industries' survey of industrial research. On average, large industrial concerns in most countries spend about 3% of their sales turnover on research and development. In my own industry, the chemical industry, the proportion of turnover devoted to research and development is a good deal higher than the average, since this industry depends more than most on the results of research: the average percentage for large chemical industry is about 4½%. My own company (leaving its subsidiaries out of account) spent some £18 million on research and development in 1962, (of which we classified £10 million as research and £8 million as development) on a sales turnover of £353 million and a total profit after taxation of £32·8 million. To see this research and development effort in perspective it is also worth noting that the cost of research and development was 21% of the company's wages and salaries bill, 58% of its total plant maintenance costs and 84% of its total marketing expenses.

The reason for all this effort is that we believe that it is essential in the interests of the continuing, and I emphasise continuing, welfare and progress of the company. It is not done for the advancement of science or for prestige and, except that the welfare of any large industrial organisation goes hand in hand with the welfare of the community, it is not done for the social good. Industry has therefore to examine the productivity of its research just as much as that of its production, maintenance and sales operations, although always bearing in mind that the time lag in pay-off for today's research can be up to 10 years or more.

The detailed methods for ensuring high and improving productivity in research must obviously vary from industry to industry and indeed from company to company. Nevertheless, there are many common basic features and I feel that the best way I can contribute in this paper is to describe the "research into research" which we have carried out in ICI and the conclusions we have reached about research productivity after studying 20 years of research results in various ways, in the hope that this might be helpful to others.

I propose to divide the paper into three sections. The first dealing with the attempts we have made to measure more or less quantitatively the effectiveness or productivity of the company's research, the second devoted to a summary of the results of this investigation and the third outlining what I consider, on the basis of these investigations and more general observations, to be some of the most important factors in securing high and continuing productivity.

Measurement of Research Productivity

In its attempts to devise methods for measuring quantitatively the effectiveness of its research activities, ICI has had the considerable advantage that it has been conducting research on an extensive scale for the past 30 years and has kept fairly detailed records of research expenditure and of the actions taken on the basis of research results. This, to some extent although not completely, avoids difficulties associated with the time lag between the carrying out of research and the implementation of successful investigations. Nevertheless there are many other difficulties and ICI cannot claim to have done more than make a partial and empirical start.

Research categories

The company's research has for many years been classified under four main headings:

(1) Background research in fields in which more basic knowledge is needed in connection with company interests.

(2) Research directed towards improvements of existing products and processes usually obtainable without appreciable capital expenditure.

(3) Exploratory or speculative research aimed at obtaining a lead to entirely novel products or processes.

(4) Research to follow up and develop leads to new products and processes arising either from the company's own exploratory work as in (3) above and from external sources.

In categories (1), (2) and (3) research programmes have necessarily been of two kinds, the first covering what we call omnibus programmes (*e.g.* "Research to find new outlets for chlorine") which can go on indefinitely and secondly specific programmes with more definite objectives which can be terminated either because they are successful or deemed to be, for one reason or another, no longer worth while.

So far in our attempts to measure productivity we have had to leave out the background research which amounts to about 20% of our total research effort and treat this as an overhead expenditure spread over the other research categories. We have also had to concentrate on specific programmes in the remaining categories. All we have been able to do with omnibus research programmes which, while they do not figure largely in category (4) researches, continue to be a high proportion in categories (2) and (3), is to estimate each year the value of actions taken on the basis of their results over the past five years and to compare this with the expenditure on such programmes over the same period. Assuming that we are in a more or less steady state, the comparison gives some idea of the productivity of this type of programme.

Analysis of research effort

Analysis of specific research effort is carried out in the following way. At the end of each year a complete analysis is made of all specific programmes terminated in that year. Each closed programme is classified as a success if it is judged to have led to some definite beneficial action on the company's part, or a failure if it has not.

Wherever possible the financial value of any beneficial action resulting from a successful research is assessed in terms of:—

(*a*) Annual savings in raw materials or other operating costs obtainable without appreciable capital expenditure.

(*b*) Capital expenditure (on new plants or on modification of plants) which has been considered worth while to implement the research discoveries.

(*c*) Any receipts from royalties or know-how payments resulting from the research.

The "research yield" for the year, both for the whole of the company's research and for researches in individual categories, is calculated as the sum of the saving under (*a*), the additional receipts under (*c*) and an assessment of the equivalent annual income value of the ability to invest the amount of capital under (*b*). This equivalent annual income value may be calculated in various ways. One conservative method is to assume that the successful research has avoided the payment of an annual royalty of, say, 3% on the estimated annual turnover of the project concerned.

These "research yields" are then compared with the cumulative costs of the resolved researches, expenditure on background research being allocated to specific researches as an overhead.

Admittedly the procedure as so far described goes only a small way. It gives rise to a minimum quantitative figure for the productivity of specific researches and in consequence is conclusive only if this provides a positive assurance that the research activities of the company are paying off either as a whole or in particular categories of research. Even if it gives this assurance it does not take us very far in showing whether productivity could be improved and, if so, how.

Productivity assessments

In order to delve more deeply into this question the analysis we carry out is taken a good deal further in a more qualitative or semi-quantitative way and I would like to give a few examples.

A watch is kept on the volume of research in each of the various categories (expressed in terms of the expenditure which has been involved in it), which remains unresolved (*i.e.* not terminated either as a success or failure) at the end of each year. This unresolved research is categorised according to the reason for the programme not being terminated. For example in some cases the research itself is incomplete, in others a decision on implementation is awaiting commercial or other policy decision. Any abnormal increase in unresolved research in the pipeline calls for appropriate investigation.

In many ways the actual productivity data for a particular year are less important than trends observed over a number of years. For example, it is useful to compare the productivity data for category (4) researches with the relative extent to which these are based on leads from the company's own category (3) research and those obtained from external sources. This comparison gives a valuable guide to the appropriate effort which should be put into exploratory and speculative research programmes.

Particular attention is also paid in the annual analysis to " failed " programmes. The proportion of such programmes in any particular category is recorded as also is the proportion of expenditure in any particular category which has been involved in programmes finally designated as failures. The reasons for a programme being classed as a failure are also examined, *i.e.* technical, economic, commercial, or political reasons. Logically, if proper control is being exercised the proportion of expenditure on failures to the total expenditure in a category should be less than the numerical proportion of failure programmes. Also the proportion of failures due to scientific and, to a considerable extent, commercial reasons should be less in category (4) than in category (3).

Resolved programmes resulting in either success or failure are also analysed according to the source of the basic idea leading to the research. This idea can arise purely from scientific considerations in the research department itself, from production departments because of observed deficiencies in existing processes or products, from commercial departments who can see a market demand for products of specific properties, from economic considerations such as the desirability of switching from one class of raw materials to another, just to give only a few illustrations. Since there always tend to be more ideas for research than people and facilities to investigate them, this historical information on the relative pay-off potentialities of these different kinds of ideas is obviously most useful.

In addition to showing up the relative productivity of the various categories of researches, the analysis also reveals any marked differences in the profitability of research in the various fields of the company's activities, *e.g.* fibres, fertilizers, explosives. It also helps research management in many ways, for example by providing a check on whether company policy, say to increase exploratory research and to decrease expenditure on omnibus programmes, is being implemented and at what speed, and gives over the years an indication of the effects of such changes. It is also possible to observe whether the time taken for completing researches of different types is increasing or decreasing over the years or tending to vary in different parts of the company.

Conclusions from Research Productivity Measurements

These research productivity investigations inside ICI have at least provided positive assurance that research conducted on the present scale is paying off and that even with its present productivity there is a case for expansion rather than contraction of research effort. At the same time the return from research is not by any means fantstically high as is sometimes imagined; the pay-off is of the same order as that of other worthwhile industrial activities. For every £1 million spent on research of all kinds, background research plus specific programmes plus omnibus programmes, the assessable yield on the basis outlined earlier in this paper averages around £200 000 a year for, say, ten years.

However, proof that the research is overall being worth while is not proof that it is as efficient as it should be and we are quite sure that it is not. Approximately only 10% of the assessable research yield comes from omnibus programmes; although these still constitute a very considerably larger proportion of the company's research expenditure. The yield from omnibus research, although not analysable in very great detail, is quite clearly unsatisfactory. The yield from specific researches to improve existing products and processes, although worth while, is lower than that from investigations devoted to entirely new products and processes and the highest yield of all appears to come from new products and process research based on exploratory and speculative work carried out by the company itself.

The proportion of the research programmes adjudged to have been successful in that they have led to some positive action by the company, as would be expected, varies considerably with the type of research. In the case of researches to improve existing processes and products the proportion of successes is high (about 70%). For researches concerned with with specific new products and processes it is only slightly lower but only about 40% of the exploratory research to provide leads for novel processes and products leads finally to a successful result. In all categories the proportion of the research expenditure associated with successful programmes is appreciably higher than the above figures which indicate that critical review of the prospects of researches has been reasonably effective.

Analysis of successful programmes shows that on average less than 5% have stemmed from ideas generated from the company's own background research, whereas over 80% are based on ideas emanating from other sources inside the company such as general research, development, technical, production and commercial departments. Only about 15% of successful researches have been based on ideas from external sources. An interesting observation has been that this 15% of successful programmes based on external leads has been responsible for over 30% of the total expenditure, whereas the 80% stemming from internal ideas has involved less than 60% of the expenditure. Presumably this is because, in the latter case, much more supporting information is already available.

Of the " failure " researches rather less than 10% have had as their basis ideas originating from the company's background research, about 70% have been based on leads arising elsewhere in the company and roughly 25% on external leads. Rather more than half the " failure " researches directed to improvement of existing activities were dropped for scientific or technical reasons, the remainder being terminated for economic, commercial or policy considerations. In the case of exploratory researches looking for new leads 70% of the " failures " were due to inability to surmount scientific and technical hurdles. On the other hand, only 30% of the researches on defined projects were due to technical causes and the main reason for their termination was the conclusion, as more detailed information on costs of the potential project were revealed, that they were unlikely to be economic. These figures and their relationship to each other again suggest that control is reasonably good.

The average duration of individual specific research programmes has been much the same in all categories, namely one and a half to two years.

Several positive actions have been taken in the light of these results. An obvious one has been to cut out omnibus programmes as far as possible, although in practice this presents very considerable difficulty. Greater emphasis has been placed on exploratory work to provide new leads to novel products and processes including the setting up of a new central research organisation for this type of work. Special attention has been paid to see that exploratory research is spread over the various fields of company interest in reasonable accordance with their economic and commercial growth prospects. This is particularly important in a company in which research is largely decentralised and carried out by separate Divisions. In these circumstances there tends to be more concentration on exploratory work in Divisions whose existing activities have relatively poor growth prospects and which are, therefore, looking for diversification, than in those with more obvious and more immediate development possibilities. To correct this we have in recent years instituted a system for deciding centrally on the priority of exploratory work in the various fields.

General Observations on Industrial Research Productivity

To my mind the basic requirement if industrial research is to be productive is that it should not be treated as an isolated activity. To begin with, ideas and suggestions for worthwhile subjects of research arise only in part from people concerned with research itself. In fact the greater number come from management and production staff who are most aware of the technical and economic shortcomings of existing operations and from development and commercial departments which are constantly analysing long term market requirements.

When a research has been started a great deal of guidance, even in the early exploratory stages, can be given to the researcher concerning preferred raw materials, undesirable by-products, etc., which can cut out unrewarding work. At a later stage, when a lead or leads have been obtained, research inevitably arrives at a cross-roads when decisions have to be taken as to which of the various possible routes should be followed. Such decisions can rarely be taken on purely technical and still less on purely scientific grounds. Maximum yield of the ultimate product or a high speed of reaction, always appealing to the scientist, can for example be often less important than simplicity and minimum capital cost of the production unit or avoidance of toxic hazards or excessively difficult manual operations. All researches have therefore to be vetted periodically by a range of people providing between them not only the scientific skills but also intimate knowledge of the relevant technical, economic and commercial factors.

Obviously this planning and vetting of research has to be done sensibly and not carried to such an extent as to miss the benefits of the unpredictable side issues which so often arise from research of all kinds. Organised co-operation between the various industrial groups is therefore not enough unless the people in these groups really understand and appreciate each others outlook. For this reason I feel it is extremely important that a considerable proportion of scientists entering industry should acquire experience in at least two spheres of activity.

The majority of scientists joining a company such as ICI do so as researchers from the universities and some of them should properly remain continuously in that work for their working lives. In the main, however, such people should be restricted to exceptional specialists and those temperamentally uninterested in wider activities. The majority, irrespective of whether their main contribution later on is likely to be in research, should early in their careers move into a production, technical planning, development or commercial job for a year or two in order that the men themselves and the management can decide where they are likely to be most usefully and happily employed. In ICI it is not unfrequent for a man to leave and re-enter research more than once during his career and many of the most effective research directors have been produced in this way. This policy is sometimes criticised as involving a " waste " of scientific manpower but it is just as important to make economic use of scientific knowledge as to acquire it and it is our experience that this movement of staff ensures a maximum of collaboration between research and the other industrial functions and a practical outlook amongst the majority of our research workers. It has the further advantage that it facilitates the transfer from research of older men who have lost their inventive flair to other jobs in which they can make a full contribution. In saying this I should add that it is not our experience in ICI that research people necessarily become less imaginative in the early 40's; this depends very much on the individual and many people continue to do most inspired research work right up to retirement. A further and most important advantage of giving industrial scientists as wide an experience as possible is that it opens the way to the promotion of men with a thorough understanding of science to top management positions.

This integration of research with other industrial functions which I have argued is essential for economic productivity is clearly most easily achievable when research is carried out inside a particular company. In the main this is possible only with reasonably large industrial organisations and smaller ones have to rely for research on sponsored research companies, research associations or on government-operated research establishments. Productivity of this sort of external industrial research will depend very largely on the degree of co-operation with the industrial groups concerned and one of the things which would improve this co-operation would be to encourage more of the staffs of these research establishments to move into industrial jobs in the companies they have been serving.

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NEW TECHNIQUES FOR INCREASED RESEARCH PRODUCTIVITY

By FRANK C. CROXTON, B.A., M.A., Ph.D. (MEMBER)*

SYNOPSIS

When the productivity of research is important, the motives for undertaking the studies in the first place have strong economic implications. This is in contrast to basic research founded on pure curiosity or conducted as part of the training of new scientists. The impetus behind research for profit is influenced by the output to be expected much more than by the input.

New techniques and new apparatus have been made available to the scientists and engineers, enabling them to accomplish well-known tasks more rapidly and more precisely and enabling them to accomplish heretofore impossible tasks as well. New techniques such as measurements of nuclear magnetic resonance, electron-spin resonance, and characteristic X-radiation with the aid of electron microprobes have required inter-disciplinary co-operation which, in itself, is a recently well-perfected method of research. The independent research institutes have expanded rapidly and have provided especially good facilities for group research, inter-disciplinary research, and technical information centres.

The Measurement of Productivity in Research

Productivity in research, as in manufacturing, may be thought of as a sort of efficiency relating output and input. James Brian Quinn[1] has identified productivity as the amount of technological output (regardless of its economic value) per unit of scientific effort expended. The economic value of the company's research is considered separately. Now input is fairly easy to define in pounds or dollars, on the one hand, or in terms of man-hours on the other. It must be the definition of output, then, which is so difficult.

Perhaps it should be stated in the beginning that productivity in research is of concern to us mainly when we are doing research for profit. Even a not-for-profit institute conducts investigations for profit albeit not its own but that of its sponsors or clients. In fact, except for those few cases of altruistic support for research of a very basic sort, there is no other reason than profit in the mind of a firm when it engages someone else to do research.

Accordingly, it is important to mankind, as we all depend so much on the contributions of research to our future, to elucidate the factors contributing to investigative productivity, ignoring any real or fancied distinction between basic and applied research.

First of all, let us agree to speak of research input in terms of men and time (man-hours, man-years, or whatever) because this may be thought of as a real expenditure year after year. Furthermore, it furnishes a similarity of basis, at least among the more developed countries.

As for research output, there can be many measures. One concerns the number of published papers. It is widely used in American universities as one criterion in appraising members of the teaching staff for promotion. The productivity of individual investigators, as evidenced by the number of their publications, has been a subject of study for many years.

Another measure of research output is number of patents. Since this is likely to apply largely to those engaged in industrial research, there may also be an attempt to estimate the income to the firm from one or more of the patents. Increasingly, it is found that investigators, intuitively graded as quite superior, are found to have few or no publications to their credit. Instead, they are authors or co-authors of many reports, all too often of limited distribution and thus not giving an index of their stature among their peers. Similarly, in industry, the greatest variation exists in the opportunities to invent patentable processes or products.

Another measure of research output is the capital that its results require for their implementation. Obviously, comparisons on this basis must be made with the greatest care. In fact, G. W. James and his co-workers at Battelle believe that research and development on the one hand, and investment in new plant are alternative uses for corporate funds. Further, the editor of *The Financial Times*[2] said that it is the newer British industries which have been able to develop the highest level of productive efficiency. These industries, he adds, require both heavy capital expenditure and continuous research and adaptation.

Related to this measure, however, is the rate of growth of firms which have demonstrated themselves to be successful innovators. Edwin Mansfield[3] of The University of Pennsylvania, as part of an extensive study of technological change and economic growth, has found that these successful innovators have grown more than twice as rapidly as other firms during the five to ten year period following the appearance of their innovations.

Yet there is conflicting evidence that research has increased its own productivity. Figs 1, 2, and 3 show how number of patents, volume of scientific literature, and capital investment have changed in relation to research expenditures in the United States during the period 1953 to 1962. No attempt has been made to state research expenditures in constant dollars since the best deflator available so far yields an index ranging only from 88 to 107, which is probably within the range of reliability of the other data used in these figures.

It is probable, however, that each successfully completed research programme furnishes an improved basis for the conduct of subsequent research. Boolean algebra lay unused in practice for more than a century. The theory of relativity had been a part of the published literature for 40 years before *m* became *e* explosively for the first time on this planet. There had been tremendous input of research before Salk and Sabin polio vaccines became realities. But because of the volume of information resulting, a successful measles

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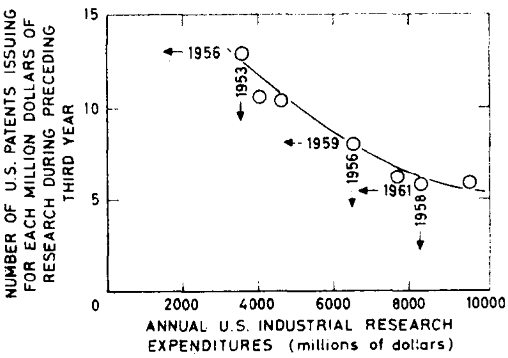
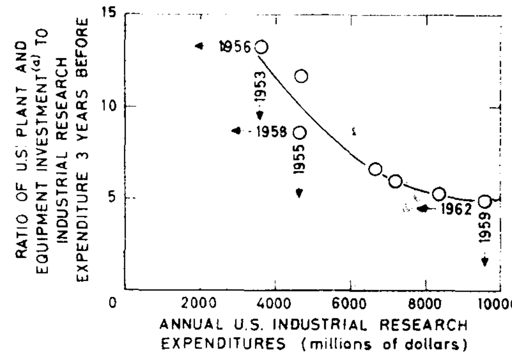


Industrial Research Expenditure		US Patents Issued		
Year	Millions of Dollars	Year	Number	Number per Million Dollars of Research[a]
1953	3630	1956	46 800	12·9
1954	4070	1957	42 800	10·5
1955	4640	1958	48 400	10·4
1956	6590	1959	52 500	8·0
1957	7720	1960	47 200	6·1
1958	8350	1961	48 400	4·8
1959	9610	1962	55 700	5·8

[a] During third year preceding.

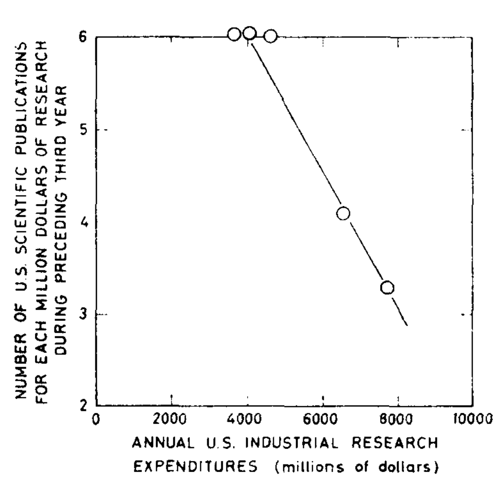
Fig. 1.—*The relationships of US patent output to research expenditure*



Industrial Research Expenditure		Plant and Equipment Investment		
Year	Millions of Dollars	Year	Millions of Dollars[d]	Ratio[e]
1953	3630	1956	47 300	13·1
1954	4070	1957	47 300	11·6
1955	4640	1958	39 400	8·5
1956	6590	1959	42 100	6.5
1957	7720	1960	45 300	5·9
1958	8350	1961	43 500	5·2
1959	9610	1962	46 100	4·8

[d] In constant 1957 dollars.
[e] To research expenditure three years before.

Fig. 3.—*Relationship of plant and equipment investment to research expenditure—US conditions*



Industrial Research Expenditure		US Scientific Publications		
Year	Millions of Dollars	Year	Number	Number per Million Dollars of Research[c]
1953	3630	1956	22 000	6·1
1954	4070	1957	25 000	6·1
1955	4640	1958	28 000	6·0
1956	6590	1959	28 000	4·2
1957	7720	1960	28 000	3·7

[b] Abstracts of US Papers appearing in *Chemical Abstracts.*
[c] During third year preceding.

Fig. 2.—*The relationship of US scientific publications[b] to research expenditure*

vaccine required a much less elaborate programme of research and development. Thus it may be pictured that research aids future research and the subject matter of the latter is like the sculpture of Michelangelo who felt that the statue had always been in the block of marble and it was his responsibility to remove the stone which had enveloped it and concealed it for ages.

One finally moves toward the conclusion that there is really no exact measure of research output and therefore no measure of its productivity. The only scientific measure of the output of one research group is to compare its successes in problem solving and creativity with those of another group. Direct comparisons are almost impossible but appraisals by those having long experience in research can furnish valuable information.

The Reasons for Industrial Research

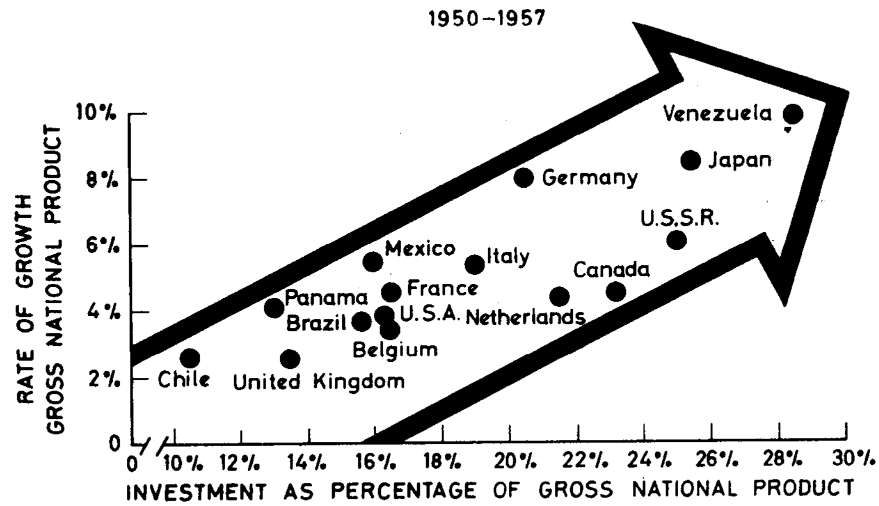
At this point we should give some attention to the reasons why industry does research and how it selects the level of funding. There seem to be two general reasons for conducting research. The first has to do with profits and competitive position encompassing such questions as increased sales, decreased cost of manufacture, new products, new processes, maintaining or improving a competitive position, or staying in business at all. The second reason, fortunately quite minor with respect to the first, involves such considerations as prestige or corporate image.

In attempting to achieve the results included in the first set of reasons, we must consider money for research as an investment. Capital investments from country to country have been shown to be roughly related to rate of growth of gross national product. Fig. 4, based on data published by Chase Manhattan Bank,[4] illustrates this. Although it is not to be assumed that a similar relationship exists for investments in research, Yale Brozen[5] has found that company profits are higher in many cases where the research effort is greater.

With further reference to that large fraction of research which is being done for reasons other than prestige and appearance, it must be made clear that the hoped for return is many times, perhaps many hundreds of times, the investment. It becomes abundantly clear then that a certain latitude in research input is permissible. The primary consideration is the output, *i.e.*, its quality.

Since a major fraction of research cost is labour, it is especially important to increase productivity in this field. During recent years a number of techniques have appeared for its accomplishment. The purpose of this paper is to describe some of them and to attempt to appraise their importance.

founded and in 1929 began to do research, largely at that time in metallurgy and related subjects. Battelle is a typical example of the research institutes which have found an important place in the United States of America. There are now considered to be about a dozen of them with similar characteristics. They are essentially independent, not being owned by a company or even closely associated with a university. They undertake research by contract and convey the results of each project to the sponsor whether industry, government, a group, an association, or an individual. The patentable results are similarly conveyed to the sponsor with some exceptions, as in the case of the government contracts. They operate on a not-for-profit basis, which means that they have no shareholders and any slight excess of income over expense is returned to the institute for the benefit of present and future research. With the exception of Mellon Institute, they operate with permanent staffs.



Source: The Chase Manhattan Bank Economic Research Department, *Business in Brief*, No. 51 (March–April, 1960), based on data from the United Nations and the Pan-American Union.

Fig. 4.—*Relationship of investment to economic growth*

It is proposed to consider the following techniques, new to different degrees, but all contributing greatly in recent years to increased productivity: (1) the research institute, (2) team research, (3) group research, (4) modern research management, (5) stimulation of individual creativity, (6) information centres, and (7) research tools.

The Research Institute

In 1913, referring to the student searching for a mission, it was said:

> " If he feels that there remain no more worlds to conquer, let him begin by making lubricating oils that will not carbonise, or by saving the enormous amount of waste of heat in the manufacture of cement, or by finding new uses for cobalt from the enormous cobalt residues from the far North, or for arsenic and sulphur which today are or could be produced in enormous quantites, or for stale bread; or let him find a really valid method of extracting copper from low-grade copper ores or tailings; let him make good soap from petroleum or alcohol from natural gas; and when all these are accomplished, there are still a million more ".

The speaker was Robert Kennedy Duncan, who is generally considered to have had the research institute idea and whose enthusiasm led to the establishment of Mellon Institute, in Pittsburgh. Ten years later, Battelle Memorial Institute was

The independence of these institutes makes them attractive to many companies since their survival depends on assurance that no secret information will be passed on. It has been found, however, that opportunities arise for bringing two or more companies together for their mutual good. Typical would be the development of a product for one company at about the time that another needed a material of the same sort. An interesting true example in which two companies joined to sponsor research is that of a bible publisher and a company making playing cards; both were interested in edge-gilding.

If it can be assumed that the research institute is well equipped with laboratories and with apparatus, the only thing it has to offer potential sponsors is an outstanding staff of scientists under first-class management. It might be noted in passing that the matter of managing research and development is a subject in itself.

Reasons for contract research

There has been a concept of research institutes involving the idea of critical size in somewhat the sense of nuclear fission. A small institute can be an effective research unit. It is not until the organization has reached a certain size and diversity of abilities, however, that it really becomes an especially valuable adjunct to the research departments of industry. To illustrate this, we might review for a moment the reasons why a firm uses contract research. These reasons may be that:

(1) A firm needs additional technical manpower or additional laboratory space.

(2) It needs research abilities not existing in its own laboratories.

(3) It needs a fresh approach to the solution of a problem which so far has not been solved in the company's own research department.

One scarcely needs to elaborate on the first reason. It is the least flattering to those involved in research institute operations, but it is realistic. During a period of unusual activity in a firm's research department, it is a relatively simple matter to contract for certain research rather than employing more scientists and setting up additional laboratories. This is especially true, of course, if it can be foreseen that the need is temporary. A characteristic of a scientist in a research institute is an adaptability and a willingness to transfer from one investigation to another.

An example of the second reason, which occurred at Battelle several years ago, was that of a petroleum refining company having by-products of potential usefulness in the paint industry. This company had no paint laboratory. Accordingly, it called on Battelle to do the research necessary to evaluate the by-products and then define the conditions under which they could be commercially useable in paint formulations. The work was so successful that the markets became large and the company decided, quite properly, to man and equip its own paint laboratory, after which the project at Battelle was closed.

There could be numerous examples of the value of a fresh point of view in research. Fleming found what Tyndall and Pasteur had failed to find even though all three had encountered the same phenomenon in their laboratory cultures. The paper industry has as one of its many problems the removal of bark from wood. Their representatives discussed the question with Battelle, not only with the mechanical engineering department or even with chemical engineering, but also with the ore-beneficiation group which had been separating materials for many years. They developed the Vac-Sink process which amounts to chipping the logs, subjecting the mixture of wood and bark particles to waterlogging conditions and floating off the wood. The bark then sinks. The process is now commercial.

It will be seen then that the research institutes are putting at the disposal of industry, needed services not otherwise available to them. This inevitably leads to an increased productivity. Aside from direct research services, the institutes perform a number of other functions which are of unusual assistance to the nation's economy. They hold technical conferences, often international in scope. Battelle Institute, in co-operation with a nearby university, has just given a course in research management and research methodology. It has recently been host to a group of 70 for a conference on automation and technological change. The institutes publish research results as freely as their sponsors will permit.

The efficiency of research in an institute

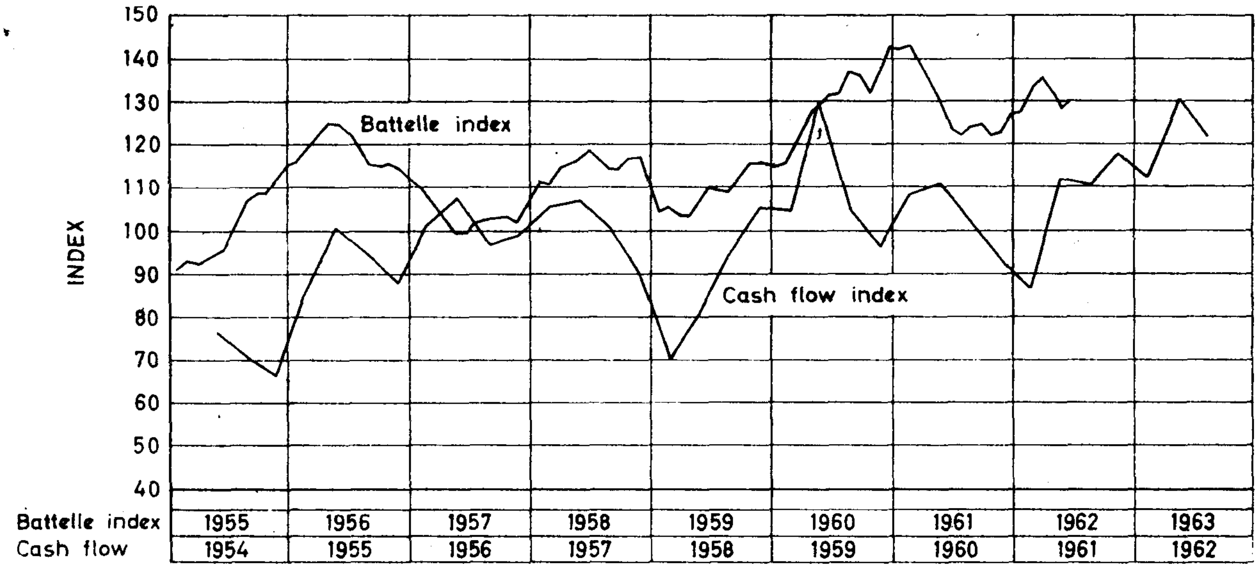
A feature of the research institute which makes for high productivity is that of total utilisation of staff and apparatus. It is the practice of the research institutes to charge each sponsor only for the actual time that such an instrument is used on its project. Thus, an economy is realised in the conduct of that project.

The cost of having research done at an institute is probably about that of doing it in one's own laboratory. As discussed above, however, it is the output which is really important and which is so hard to define. It is believed very strongly that research done in an institute of adequate size and quality is often more productive than that done in a company's own laboratory. There are several reasons for this. First, the business of the institute is solely research. It does not digress from time to time for trouble shooting. In fact, its distance from the company, the company's own laboratory, and its manufacturing facilities, all help in this respect. It might be thought that this distance would make communications so difficult that the research might suffer. Such is not the case, however, because the subject matter is generally chosen so as to avoid that problem. In addition, face-to-face meetings at monthly or bi-monthly intervals are encouraged. Written reports are of considerable assistance to both organisations. Because a relatively minor error could do untold damage to the reputation of a research institute, a proposed research study is reviewed very carefully with the sponsoring organization before there is agreement to proceed. As might be expected, many proposed research projects are not undertaken because they appear to be inadvisable. It is not that the long-shot is avoided, but rather that the problem which is completely unsuited to the abilities of the institute or the problem which is not really research, is turned down. The growth of the research institute principle in the United States is rather good evidence that it is valuable to industry, not instead of its own laboratories, but in addition to them.

Although the institutes have research as their only activity, it cannot be denied that they undertake many projects of short duration. At Battelle, during the five-year period 1955–59, there were 1491 non-governmental sponsoring entities. Four invested more than one million dollars in research; nine invested more than $500 000. It is interesting to note, however, that 850 companies spent less than $10 000 each during those five years and 255 among them spent less than $1000. It is a characteristic of the research institutes which makes this possible without loss of efficiency. Most such short-term projects are either preliminary investigations or economic studies. The former group would be handled by the sponsoring company if not by a research institute but perhaps not with so much expertise. The bulk of the smaller projects, especially those greater than $1000 but less than $10 000, are economic studies. The Economics Department is geared to such work in a way that a laboratory-oriented section cannot be. Battelle's economists keep a running collection of data useful in diversification studies, area development investigations, and questions of technical feasibility. Like an expensive piece of laboratory apparatus with a high-use factor, this body of data is economically obtained and maintained—economically to Battelle and therefore economical to many sponsors.

The experience of years has shown that the companies knowing research best are the ones which recognize the benefits to be derived from contract research. In 1961, 42% of Battelle's industrially sponsored research came from companies in Fortune magazine's list of top 500 companies. Eight per cent were in the second-500 list. Both of these percentages could be taken as either number of projects or dollars. Companies having more than $1 million annual turnover but not in the top-thousand group sponsored a quarter of the projects but only 14% of the dollar volume.

G. W. James and his socio-econonomic research group, mentioned earlier, have studied the cyclical fluctuations in the volume of research at Battelle. They have found a rather remarkable correlation with national cash flow figures with a delay of almost exactly one year. Fig. 5 illustrates this. The caption describes the method of constructing the two curves. The business aspects of a research institute can be handled with more foresight when information of this type has been developed. The result is still greater productivity.



The Battelle Index is a 12-month average (centered) of new and renewal industrial contract acceptances. The Cash Flow Index is a measure of quarterly totals of net profit, after taxes, retained in business plus depreciation and depletion for manufacturing corporations. (Note that a structural change may be taking place reducing the time lag between cash flow and contract acceptances from four quarters to three),

(f) Represents the sharp increase in profits as a result of heightened economic activity to build inventories in anticipation of the steel strike.

Fig. 5.—*Battelle industrial contracts and cash flow of manufacturing corporations*

Team Research

The creative individual is the key to most of the really important scientific discoveries. Increasingly, however, the small groups or teams of research workers have assumed added importance in the production of technical change. Centres of unusual scientific achievement such as Rutherford's Laboratory, the Berlin-Dahlem community, and others of this type are extremely interesting phenomena, which lead one to speculate about their causes and their achievements. The creative climate was undoubtedly very important. A loosely-knit team existed, however, in most of such places. This made possible prompt, frequent, and easy exchanges of ideas and especially of questions.

As the years have passed, the teams have become more formalised. They have been one method of introducing the inter-disciplinary approach to research. It cannot be said with certainty that research has become more complicated, because at each stage it has been as sophisticated as current knowledge and ability would permit. It is true, however, that the more extensive foundation on which present-day research is based not only requires but permits the abilities of several disciplines to combine to solve problems more quickly.

It is only to be expected that I should point out here that the research institutes make inter-disciplinary research available to industry which could not otherwise afford it. An institute larger than a certain critical size is in a position to form teams consisting of scientists and engineers from various disciplines. As an example, problems in radiation chemistry can be investigated with far greater productivity if a team can be formed consisting of organic chemists, chemical engineers, and nuclear physicists. Of course, as with almost any research problems in which productivity is of concern, technical economists become a part of the group as early as possible.

It will be seen that the co-operative effort of several disciplines is very likely to result in greater achievement than the mere combination would suggest. Exchange of knowledge among the participating scientists and the resulting atmosphere conducive to creative achievement, lead to a gratifyingly high apparent productivity of that type of research.

Group Research

Although research for competitive advantage has the highest requirement for increased productivity, there is a certain area of research in which a joint attack has been found to be highly advantageous. This area of research has to do with technological change, of interest to a variety of companies, often for a variety of reasons. At Battelle a group of 45 companies is sponsoring research on fuel cells, not to develop a better fuel cell, but to advance the knowledge of principles of the operation of fuel cells with the expectation that each participating company can benefit in its own way. The types of companies involved include manufacturers of electrical equipment, makers of lead-acid storage batteries, natural gas producers and distributors, and petroleum companies. Universities, and especially the research institutes, provide excellent facilities for research like this, in the United States of America. In the United Kingdom, the Research Association laboratories perform this function admirably.

The concept of group research, in itself, is most highly generalised since it makes it possible for each of several companies to obtain the total ouptut of a research project while contributing only its individual share of financial support. From the standpoint of the firm, this results in an enhanced productivity which is advantageous to it.

Modern Research Management

It must be insisted that proper management carries an entirely different connotation when referring to research than it does in any other connection. In this sense, management involves no rigid controls, no disciplinary restrictions. Rather it is a means of supporting research, smoothing the way, establishing the right climate, and furthering the implementation of results.

Research management has progressed rapidly since the end of the war; it still has far to go. Its contribution to the productivity of research can be tremendous. Good management respects research's need for freedom.

It has been too frequently true, however, during the last quarter century, that firms have noted the glamour of research and have said to themselves, " We should have some ". It is scarcely necessary to say that this is quite the wrong approach. Rather, there should be a demonstrated need for research before undertaking it either within one's own company or through contract elsewhere. It is not a question of the number of research scientists to be employed any more than it is a question of the number of surveyors. A manufacturer of pharmaceuticals will probably have as little use for surveyors as a constructor of steel bridges will have for research personnel. It is good, of course to introduce the research atmosphere into an organisation which can benefit from it and which has been without it heretofore. But the important point is to be prepared to use the results of research before proceeding too far or even proceeding at all with research.

A sympathetic but not uncritical attitude toward research can be management's greatest contribution to its increased productivity. Good management stimulates creativity. It retains its faith in the ultimate return on wise research investment even though the delay may be great. Its faith is unaffected even though the cost of research itself is a contributor to the " profit squeeze " as shown in Fig. 6.[6]

An enlightened management will treat research as a powerful tool, a means toward substantial cost savings and, at times, the key to survival itself.

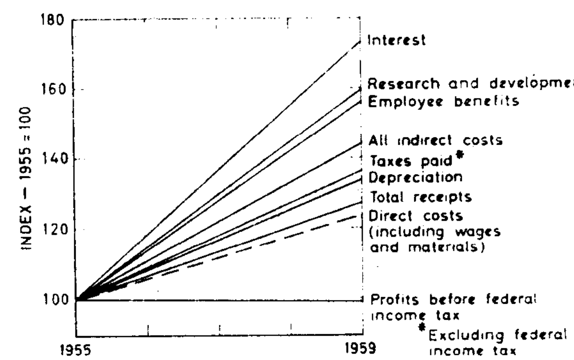


Fig.6.—Factors contributing to the profit squeeze

Stimulation of Individual Creativity

Although team research has become important among the many means of producing technological change, the creativity of the individual and his freedom separately or within the team, remains perhaps still more important to the giant steps in innovation. As Thoreau[7] put it, he who is out of step with his companions may be hearing a different drummer.

W. D. Hitt[8] in his studies at Battelle, has pictured research investigators as being placeable in a quadrant of a diagram such as that of Fig. 7. Those in Quadrant I show a high degree of general ability in both original accomplishment and problem solving. It might be said that they are the most valuable type, all things considered, for a research organisation. They can not only innovate, but they can see the ultimate in applications and economics. They are creative; they are energetic; they are highly self-confident. Those in Quadrant II are especially skilled in originality. They are the ones who, we may say, are rather impractical but they do have ideas and may furnish the bases for more practical applications by others. These people exhibit divergent thinking in the sense that they generate many ideas from a single fact or new concept. The research investigators in Quadrant III very probably should not be in research to begin with, since they are relatively low in originality and in problem-solving capabilities. They are the plodders who show little self-confidence and vigour. They are unusually cautious. Those who are placed in Quadrant IV, however, are the problem solvers who, given a description of the situation and the need for a solution to the problem, can frequently produce a valuable answer. They are not adept at developing entirely new concepts or in producing a break-through of the sort that had scarcely been thought of before. Type IV are responsible, systematic, and skilled in logical reasoning. They have convergent thinking processes, and so are able to boil down many facts, observations and ideas to yield an integrated picture or a logical course of action. Perhaps there are more of these in the engineering profession while there are more of those falling in Quadrant II involved in investigations in the more fundamental sciences.

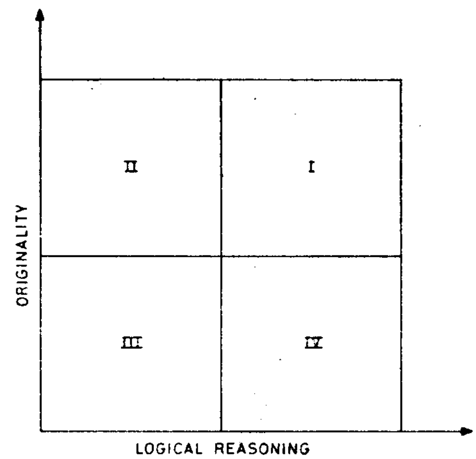


Fig. 7.—Creativity diagram

The extreme importance of creativity has fortunately been observed and taken account of by management and even by fellow scientists, especially in recent years. Educators are concerned with the various influences which contribute to enhanced creativity even at a very early age. It is easily possible that the educational process before age ten can stifle creativity for the remainder of one's life or, on the other hand, can stimulate it for the near future. We have a rare asset, therefore, when we find a research investigator with innovative talents, and an especially rare one when we find such a person with practical abilities in application and economics.

Industry's awareness of the impact of creativity on technological change is made clear by the steps taken to improve the climate for it. Many examples of courses, seminars and the like could be given. It must be remembered, however, that, far beyond teaching creativity, research management must provide an atmosphere conducive to innovational thoughts and acts. The difficulties which have preceded the present approach to better training, better atmosphere, and better inter-personnel relations involving creative scientists have been many. There is much hope now and in the future. The many more creative studies, and their subsequent realisa-