The Purpose and Principles of Research in an Electrical Manufacturing Business of Moderate Size, as Stated by J. A. Crabtree in 1930

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The study of the origins of industrial research has attracted a good deal of interest among historians of technology in recent years, especially in the United States. In the electrical industry, research in the form of experiments conducted more or less scientifically was implicit from the beginnings of the industry in the second half of the nineteenth century, but did not become explicit (for example in the form of a 'research department' or 'research laboratory') until the early years of the twentieth century — in Britain not until after the First World War.[1] Once the era of industrial research in electrical engineering in Britain had really begun in the 1920s, there were numerous papers published on how it should be organized and what its purpose was.[2] These papers were concerned with the research departments of large industrial or government organizations. Research was hardly associated with manufacturing firms of, say, 1,000 employees or less. It is therefore very refreshing to come across a down-to-earth treatment of the purpose, nature and organization of industrial research, written in 1930 by the founder and proprietor of a firm manufacturing small electrical switches and accessories with a workforce not exceeding 1,000 up to that time. The document concerned was never published and seems now to be of considerable historical importance in showing the thinking of a successful manufacturer who apparently had no ambition to be a tycoon, only to remain successful. It forms the subject of this article.

After the introductory sections, this article comprises an abridgement of two chapters in what was clearly intended to be a comprehensive textbook of business management prepared half a century ago. Its author was John Ashworth Crabtree who, starting from very small beginnings, founded in 1919 the well-known manufacturing business still known by his name and still based in Walsall, West Midlands. The firm was successful and expanded greatly both during and after Crabtree's lifetime, but he unfortunately died in 1935 at the rather early age of 49. The proposed book was drafted in 1930 and remained as an unrevised typescript,[3] probably forgotten, until recently brought to my notice by Crabtree's eldest son, Mr Jack Crabtree.

The reason why the work was never revised and published is not now known. It was a long work, setting out Crabtree's views of the principles by which a manufacturing business should be run, based largely on his own experience of running his own firm, which by 1930 had about 1,000 employees, and also partly on his observations and studies of bigger firms. It was in many respects a pioneering work, for the principles of business management, however well understood intuitively by businessmen, had not then been subject to much explicit analysis.[4]

The two chapters dealing with research are particularly interesting to historians of technology and to engineers, firstly because research was not then (or now!) a particularly noticeable feature of most small and medium-sized firms,[5] and secondly because the ideas put forward still seem as valid as ever. In this article I have made a précis of Crabtree's text, of about 40 per cent of the length of the original, using entirely Crabtree's own words and method of presentation, merely omitting what seemed to me the less valuable portions. I believe I have retained entirely the spirit of the original.

Some Comments on Crabtree's Analysis

It has for long been customary to discuss the subject of the inter-relationships between science, technology, research, industry, business and society in abstruse, tortuous and convoluted terms. Crabtree approaches his admittedly more limited analysis in a direct, penetrating and lucid manner. It was perhaps this which led one of my colleagues, a man with good experience of modern industrial research in electrical engineering, to describe Crabtree's approach as 'naive'. My own forty years' experience of applied research in electrical engineering has been in government and academic departments where the constraints and interacting factors are rather different from those in industry. I am therefore not too well qualified to express an opinion, but I would have thought Crabtree was very far from being naive. Talking of product development towards the end of these chapters on research, Crabtree says: 'The best and most reliable product is ever that which fulfils its function with the minimum of complication and the maximum of simplicity.' Is not his analysis of industrial research itself just an example of his putting his principles into practice?

It is interesting that while Crabtree sees research as having a fundamental role in progressive and competitive industry, he also implies that research is not so much a means of attaining profitability *per se* as rather a means of ensuring survival, of maintaining a market lead by offering a better product than one's competitors; it is interesting that he never mentions the idea of research leading to a *cheaper* product. (Traditionally the Crabtree firm has made only better-quality products.)[6]

Crabtree's view that perfection means stagnation — the time to get out a new product — is interesting and probably now widely accepted; and with his principle that compromise in design is essential, his definition of perfection is obviously relative rather than absolute.

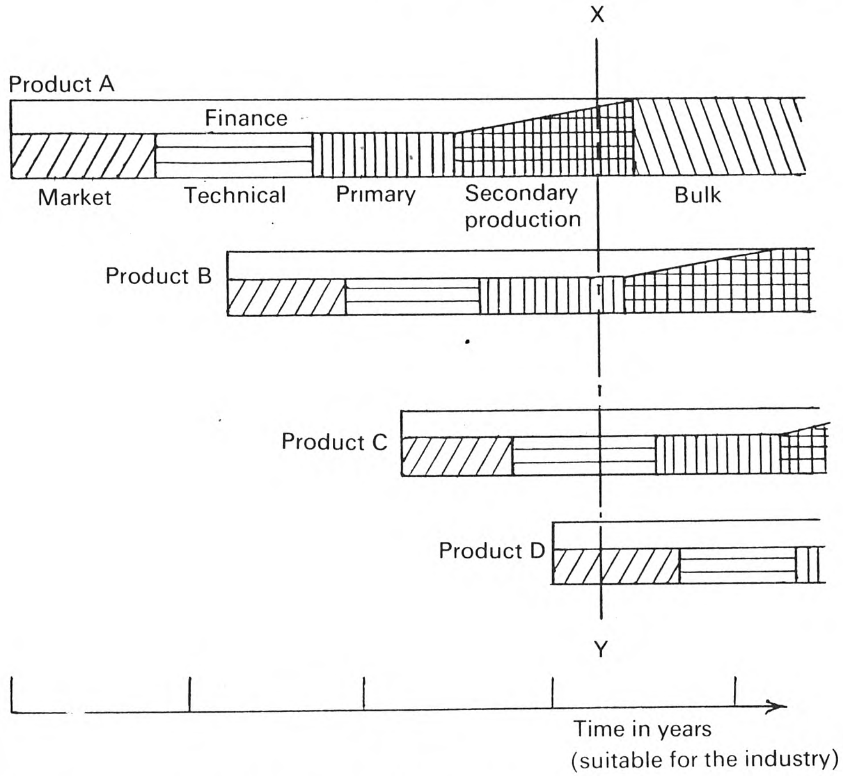

Figure 1. The ideal state of balanced research as applied to separate products, groups of products or departments. (As given by Crabtree)

It is not only technical research that he sees as important; all aspects of the business are to be appraised in a scientific fashion. The work is to be programmed as shown in Fig. 1 so that a proper balance is obtained. It is believed he put all his principles into practice.

One last point: Crabtree saw clearly the distinction between industrial and basic research: 'We must be able to distinguish between research which a business is justified in making and the more specialised research which is the work of a university or specialist laboratory.' It is interesting that many *large* industrial organizations in many countries, including Britain, began around Crabtree's time to engage in basic scientific research. What was the justification and reason for this? In her examination of the history of research in the Bell Telephone System,[7] Lillian Hoddeson poses but leaves unanswered this intriguing question. Could the answer lie in the increasing isolation of the research department in an ever-expanding large firm from the business and manufacturing sides, which permitted the realization of the natural desire of the academically-trained staff to pursue more academic research?[8] In a firm of the size of Crabtree's this could not apply and he did not see it as a permissible development. He saw research as an integrated activity over the whole field of business operations from market research to production research, from finance to science and technology. There was (and is) obviously no place for research without direct relevance in the activities of a small or medium-sized firm; yet in a huge organization like Bell, with ample resources, it could have the most profound influence on the course of technology — for example, the transistor.

Abridged Transcription of Crabtree's Views of the Principles of Research

It does not seem to be possible in these days to maintain a business on an even, undisturbed level of steady, profitable turnover. It seems as though the business must either progress, or decline, with no possibility of balance between. Any such state of balance seems so limited and precarious as to be practically non-existent, and it is preferable to ignore its remote possibility.

There is no static condition in a business: it must either progress or decline

The period of decline may be long drawn out. Indeed, I have often been surprised at the time an old-established business can linger in spite of mismanagement. Occasionally, when the whole industry is expanding, one will find that the natural growth of the market will carry the business long enough to enable new men to grow up into control, and who will then revivify the organization with new creative thought. With an intensive market, however, the decline is more rapid and, at times, spectacular.

The progress of a business in any competitive market is largely dependent upon the creative effort put into its products and management, and it is in this effort that the scientific methods of research can be so valuable.

Research plus creative thought and decision constitutes the soundest foundation for business progress

I understand by research that systematic, scientific enquiry into our problems by careful experimentation, wherever possible; a clear distinction between those things which are facts, and those things which are approximations, and finally an accurate analysis as to the significance to be attached to the various facts, the approximations and the unknowns, which make up the 'formulae' of our knowledge. I find that it is becoming increasingly difficult to know one's facts, and thereby to make correct decisions. I see no other way of meeting this difficulty save on the basis, that to every problem in commerce and industry, the systematic methods of scientific analysis, research and experiment, will indicate some solution.

Research should not lessen our creative effort, for it does not offer a 'Royal Road to Success'. But it does help to indicate the way, saving the

waste of our energy in a maze of wavering impulses — and helps us in the final decisions, by ensuring that they have a definite bias to progress.

This conception of research has the widest possible application to our business experience. It involves something much wider than the old idea, so popularized by our advertising friends, of the scientist holding up a test tube to the light, or peering into a microscope. It would be true to say that—

Systematic research will suggest a solution to every business problem

Once this principle is accepted, our mental attitude to our business becomes reorientated. We are no longer content with 'snap' decisions based upon insufficient facts, and we prefer to take action slowly, confident of the surety of our ultimate steps. There are times when, in consequence of this slower but surer attitude, we may seem to lag behind our more spectacular competitors; but the greater ultimate progress must go inevitably to the business whose fundamental research is the surer, and whose research work is untiringly persistent.

Research has its promise in every phase of our business; policy, finance, organization, management, service, product, production, and the rest. There is no field of business activity where research cannot help.

I have found that industries as a whole may be divided into two distinct groups:

(1) Those industries in which research is rare and progress has been extremely slow, with products and practice changing very little.
(2) Those industries in which research is continually operating to render products and practice obsolete.

The distinction is not necessarily inherent in an industry, but seems — so far as one can judge it from British conditions — to be due to the traditional type of mind in each specific industry. The first type of mind is indicated in agriculture and certain branches of the iron and steel trades, where it is still possible to obtain a bare and precarious living by the methods of the Victorian era, with the result that many men and methods operate much as they did fifty years ago.

A typical industry of the second class is the motor industry — an industry which has had such remarkable changes and developments during the last ten years that changes in design have almost become a fetish of the industry. Firms will bring out new models which are often no better than their predecessors, having only the virtue of being different. This fetish of 'change' has indeed become such a curse to certain firms in the motor industry, that unless it can be checked, it will eventually mean the extinction of that particular organization.

Where an industry changes little — being largely dependent upon tradition — we find that research, carefully planned and adapted, tends to produce very beneficial results, in comparison with the effort necessary. Where, however, an industry is based upon continual research as is now becoming universal, the competition in research itself tends to lessen the commercial return upon any invention or development. This comes about in two ways.

In the first place, should you initiate any new development, which may have taken years in its conception, it will at once be attacked by your competitors. They see at once how *you* have solved the problem, and they immediately receive the free benefit of much of your research. They start — not where you started — but helped considerably by what they see you have rejected, and the usual result is that their trouble and research are halved. I have found that whenever we break new ground in design, it takes the trade half the time to produce their alternative product to compete that it took us in research and experiment to produce our own. It is not long after the introduction of the competitive product that the resultant price war renders our own product unremunerative, and have come to accept as a principle that —

A product is profitable after marketing for much less than the time it took in research

The proportion must obviously vary according to the type of product, but the ratio will be well below 100 per cent. There is a reason why this should be so. Immediately your idea is known, you disclose how the problem has been solved by yourself. Much of the original work in research is in exploring how the problem should be attacked. Your solution, as exemplified by your product, shows how you have solved this, and your competitor's problem is reduced very considerably by this solution.

Some years ago we decided to re-design an electrical product having a wood case enclosing a porcelain interior to carry the mechanism. We wished to use a synthetic resin case, and decided that we must forget the traditions of design involved in the wood and porcelain product and discover new principles of design based upon the new material. It took us two years to decide these principles, and a further two years to apply them to the product. Meanwhile, our competitors merely tackled the problem by making the original wood case of synthetic resin and retaining the traditional interior of a porcelain mounted mechanism. Immediately our own product was marketed, the work of our first two years was at once apparent. Anyone could see how we had faced the problem and solved it on different principles of approach. The research problem of our competitors was accordingly halved and within two years the majority had been able to re-design and market their products.

This is a simple example of what I have seen happen very many times in my experience, and it emphasizes the need to express the result of our research with rigour, immediately it can be commercialized. 'Facile est inventis addere' is a highly effective restriction upon the profitability of research in modern industry.

A product becomes unprofitable and obsolete as it approaches perfection

I have found that as a product, a method or a service becomes so nearly perfect that it is almost impossible to improve it further, it is, in fact, obsolete and out of date. Eventually there comes a time when everything has been done which can be done within the basic limitations of design inherent within the specific product. When there is nothing left to improve — and

within its basic limitations the product is therefore nearly perfect — it is necessary to recognize that it is obsolete, and a new aspect of research must be initiated. We have now to go back to first principles, create new basic limitations to design and produce an entirely new product.

Essentials of research policy

It is not enough to limit our conception of research to the technical or chemical laboratory, but rather to view research as having the widest applicability to all our problems in business and higher control. I therefore state the essentials of research to the business as a whole, considering research as having the widest application possible. We have as our first consideration —

Essential 1: Balance

The first essential is that the whole of the research activities of the business shall be balanced — that research in any one department shall not be pursued at the expense of others.

To give an idea of the research necessary in any business, I would mention (1) Organization and administration, (2) Finance, (3) Production (Machines and Processes), (4) Technical (Product and Service), (5) Markets and (6) Human Element or Personnel. These are six main divisions in which research should be continually pursued and in which the work being done should be balanced. Figure 1 shows in conventional form (the time elements in fact being variable) what I consider to be the ideal state in our own business of balanced research directed to the production of some new product.

From the original decision to investigate the possibilities of the product to its ultimate production, there are (in our case) six stages, some of which may operate in succession or be contemporary.

(1) Market research.
(2) Laboratory and technical research.
(3) Finance (involving costs).
(4) Primary (or experimental) production or development.
(5) Secondary (or small quantity) production or development, being the first factory stage.
(6) Bulk (or standard) production being the final factory stage.

When you arrive at bulk production the product merges into the general output of your business.

I do not like to start more than one major line of research at one time. With that well started and in operation, we can then give attention to a second line of research, but not before. The same principle can apply to those working out the detailed work of research, with the result that if you consider the line X, Y (Fig. 1) cutting through the major section of research, it gives a cross section of the ideal research activities of the business at any moment.

We should have at any one moment:[9]

One line of market research.
One line of technical research.
One line of primary production research.
One line of secondary production research.
One line of bulk production research.

And finally the research essential to production as spread over the various products or departments concerned.

This diagram (Fig. 1) is only symbolical. It suggests a balance of research which is the ideal to be aimed at. Its characteristics will vary according to the specific business under consideration.

In a large business the principle of balance would still hold good, but the emphasis on individual research would require further subdivision. The whole of such research would demand a distinct executive control. If the balance is to be maintained by the man responsible for such control, he must not be unduly influenced by his own specific type of scientific training. He should be an organizer trained to think scientifically, and not a scientist trying to organize.

Essential 2: Compromise

The great distinction between research in its purely scientific form and research in a business lies in the fact that successful business research aims at compromise. To give a technical illustration: we have one mechanism to make, of which the working part goes into about one-eighth of a cubic inch. Within that volume we have to produce a mechanism which will work about a million times without re-oiling; it has to have mechanical strength, electrical insulation, has to break an electric arc and to carry an electric current. There is no way of making a mechanism ensuring all these essentials in completeness, for there is a limit to all material things. Thus we are driven to realize that our essential aim is one of compromise. If you make one feature better than another, it is definitely at the expense of some other feature, and I am amused when any of my competitors emphasize one technical point and ignore the others, for it is impossible to obtain a high degree of perfection in any single unit of development, without paying on the debit side in some other essential factor. This compromise in the application of research applies not only to materials, design, manufacture, and organization, but also in the less tangible problems of finance, market research and personnel.

Essential 3: The long view

It is essential in any research that we maintain a long view with regard to our problem. We are today reaping in commercial results what was sown in past research, and the research of today is the sowing for the business harvest in the coming years. For that reason our research requires planning, to form part of an unending process of enquiry into the needs of our business. We must, however, retain a correct perspective of the function of research in business, and recognize that there are limits to what we can afford to do.

We must, for example, be able to distinguish between research which a business is justified in making and the more specialized research which is the work of a university or specialist laboratory.

Essential 4: Honesty to face the facts

It is a basic essential that in our research we must face our problems free from bias and with open minds. If we, as the head executives of a business, controlling the research work of certain assistants, are but desirous of confirming our 'guesses' our staffs will quickly learn that we do not require the truth from them, but only some confirmation of our own ideas. They will soon colour their facts, to harmonize with what they think we want. If such an attitude becomes general among those carrying out our research work, then the time and money spent is an absolute waste and a drain in the business, for it ceases to be research.

Knowing when you have got what you require

[This stage of research] is more commercial and materialistic: to recognize when you have got what you want and apply it. Many firms waste a great deal of time in going beyond what is sufficient for the commercial needs of the moment. There is a relation between the prospective volume of profits and the cost of investigation; and that the principle of diminishing returns shows the inadvisability of working too long on one problem. It is better to spread or rotate studies, covering all matters that offer a reasonable chance of profitable improvement.

As to when we have gone far enough, I find this question helps most: 'Is the solution simple?' If it is not simple and direct in comparison with present practice and knowledge, it is not enough and we must continue our work. When we have obtained simplicity, the time is due for its introduction to practice. This is not to suggest that simplicity in fact, or form, or design is easy or accidental. The opposite is usually true. Whenever I see a product which is extremely simple, I always feel that an enormous amount of thought has gone into that product. Nothing is ever reduced to simplicity except by an enormous effort in thought and experiment. The best and most reliable product is ever that which fulfils its function with the minimum of complication and the maximum of simplicity. It is to the degree that we attain this, that we decide when to apply the results of our research in actual practice.

The conclusion of research with a view to decision and action

All business research is unremunerative unless it is given expression in some productive activity. The final stage is therefore the decision and action by the higher control, who direct the conclusion of the research worker into business activity. This stage may in fact be the greatest stage of all, for it involves the whole field of development, wherein the conclusions of research are applied to actual practice. In some cases development may have been contemporary to research. In others it will be subsequent.

Notes

1. An excellent account of the development of research in the Bell Telephone organization is given by L. Hoddeson, 'The emergence of basic research in the Bell Telephone System, 1875–1915' in *Technology and Culture*, 22, 1981, pp. 512–44. Hoddeson shows that the Bell company started employing people with a specific commitment to experimentation and invention from 1878, although the numbers were very small (single figures) until 1911, when a 'research branch' was set up which had twenty members by 1912 and expanded explosively thereafter. In Britain, the Post Office, which had rather wider operating responsibilities than the Bell system but little manufacturing activity, had shown concern with experimentation and novel design ever since the nationalization of the telegraphs in 1870, and when W. H. Preece became electrician to the Post Office in 1877 he was able to assign staff to experimental work. A great deal of fundamental engineering research and development (but not basic physics) was done by Post Office staff before the First World War, but there was no formal research organization until after that war. See early volumes of *P.O. Elect. Engrs. J.* (which started in 1907) and D. G. Tucker, 'The early development of the British underground trunk telephone network' in *Trans. Newcomen Soc.*, 49, 1977–8, pp. 57–74, and 'Sir William Preece (1834–1913)', *ibid.*, 53, 1981–2, in course of publication (read to Society on 10 March 1982; preprints available). On the heavier side of electrical engineering in Britain, the electricity supply industry also developed without the benefit of formal research organizations until after the First World War, when some of the big manufacturing firms such as Metropolitan-Vickers and British Thomson-Houston set up research departments, and the British Electrical and Allied Industries Research Association (ERA) was formed (in 1920). In the earlier years, consulting electrical engineers such as John Hopkinson and Gisbert Kapp had played a leading role in experimentation and novel design, along with the outstandingly innovative industrialists such as S. Z. de Ferranti and R. E. B. Crompton. See J. Greig, *John Hopkinson*, Science Museum, London, 1970; D. G. Tucker, *Gisbert Kapp*, Univ. of Birmingham, 1973; A. Ridding, *S. Z. de Ferranti*, Science Museum, London, 1964; B. Bowers, *R. E. B. Crompton*, Science Museum, London, 1969.

2. Two papers, in particular, are worth referring to here. The earlier one was A. P. M. Fleming, 'Planning a works research organization' in *J. Inst. Elect. Engrs.*, 57, 1919, pp. 153–70, discussion pp. 170–92. (This was later expanded into a book, A. P. M. Fleming and J.G. Pearce, *Research in Industry*, Pitman, London, 1922.) The later one was W. Wilson, 'Industrial research, with special reference to electrical engineering development' in *J.I.E.E.*, 62, 1923–4, pp. 61–82, discussion pp. 83–107. The lengthy discussions and very full bibliographies show how great an interest had been aroused in the question of industrial research at this time.

3. An original carbon copy is now in the Archives of the Institution of Electrical Engineers at Savoy Place, London, WC2R 0BL, kindly deposited by Crabtree's son, Mr Jack Crabtree, who was Chairman of the firm for many years until his recent retirement.

4. The contribution made by Crabtree's work is discussed in A. L. Minkes and D. G. Tucker, 'J. A. Crabtree: a pioneer of business management' in *Business History*, 21, 1979, pp. 198–212.

5. Some twenty years ago, Mr John Hudson of the University of Birmingham made, at my request, some inquiries into the extent of research and innovation in electrical manufacturing firms in the West Midlands of size between about 200 and 2,000 employees. In spite of letters and personal visits to senior members of staff and directors, it proved impossible to obtain really meaningful information. It was clear,

however, that research in any accepted sense was absent from most firms, although in contrast there were one or two where as much as 0.5 to 1 per cent of the employees might be engaged in investigative or innovative work which could sometimes lead to patentable inventions. One firm had a disproportionately large research effort because it had been awarded 'cost-plus' government research contracts — a form of work (so often cost-ineffective) that would have been unknown to Crabtree.

6. The role of research in increasing profits was expressed bluntly by A. P. M. Fleming (paper cited in note 2): 'It is an economic error to assume that the best method of increasing profits is, through trade combinations or other means of protection, to increase selling price. A much more logical method is to bring about the difference between manufacturing cost and selling price by reducing the cost of manufacture, and it is in this connection that the possibilities of research are unlimited.' On the other hand, in his book (see also note 2) he recognizes the survival element (p. 39): '... research is an insurance in which a suitable proportion of immediate returns is spent to secure the continuance of the returns in future.'

7. Cited in note 1.

8. As early as 1919, C. C. Paterson (who became Director of the GEC research laboratories) said: '... a great deal of latitude ought to be allowed to a research worker if he seems sometimes to deviate from the rigid industrial path. Apart from other considerations, there must be in a research laboratory a real spirit of research, and one cannot foster this by constant limitations.' (Discussion on paper by Fleming, see note 2.)

9. I have taken the liberty of correcting what must have been errors in Crabtree's typescript; he had 'major', 'experienced' and 'primary' where I have put 'market', 'primary' and 'secondary', respectively.