Research in Industry*

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Is there really something to understand about industrial research? Is it really so different from academic research with which we all have a comfortable degree of understanding and familiarity? It is certainly true that most industrial researchers are originally trained as academic researchers. Isn't industrial research basically academic research in a new setting?

Certainly, at a very basic level, there are similarities. Both academic and industrial research require very good people and are enormously sensitive to the degree of skill and insight and technical excellence of the people involved. There is the characteristic, not true of many occupations, that one outstanding person can do more than many who are merely good. Although there are many important and basic similarities like these, there are also major differences and it is these I want to discuss.

Before addressing the unique characteristics of industrial research, there are two points to be made: first, I will discuss only research work carried out by an industrial firm in order to benefit from the work in some fairly direct sense. It is not work carried out to gain prestige, improve the firm's image, do better recruiting, et cetera. This kind of motivation, although sometimes important, can be only a subsidiary one for any research organization that expects to have a long life in a competitive industry. Second, I need to fit industrial research into the industrial setting and explain what I mean by the term "Industrial Research," which is certainly not completely self-explanatory.

We know that manufacturing is actually building the product, a computer, for example. Behind this is product development, the process of designing, prototyping (i.e., building an example) the next computer, testing the prototype, debugging it, correcting its flaws, and finally redesigning it or otherwise modifying so that it is manufacturable—preferably in large quantities.

Industrial Research is doing technical work that lays a foundation of knowledge which is usable later in product development or manufacturing. In the computer setting it could be a study of the design of computers to understand the bottlenecks of the present approach and suggest modifications that would make a computer run faster, or perhaps work on an entirely new design that would run faster still.

The point is that, unlike product development, one is creating a knowledge base to be used later. This knowledge could also be a new process that improves transistor performance or even fundamental new information about semiconductor surfaces and interfaces that will ultimately lead to better transistors.

Now to some differences from academic research. First, in industrial research it is possible to be too early or too far ahead. Normally in research one worries about being behind, having someone else discover first and publish first; but, although this worry surely persists in the industrial setting,

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there is also the real problem of being too early. For example, and I take one that has a certain inherent simplicity, some years ago transistors were being worked on in manufacturing at the 4 micron level, and in development at the 3 micron level. We, in Research, leaped far ahead using electron beam fabrication techniques, studied and published on many aspects of one micron transistors. It was not, in fact, possible to create a complete one micron manufacturing process.

Our work was good, and it looked very advanced to the technical community. But, many years were to pass before one micron was a practical manufacturing technology. In these years a lot happened: (a) the knowledge we obtained was generally diffused, (b) most of what was needed was developed gradually by the practitioners in their gradual advance from 4 microns to 3 microns to 2½ microns, etc., including what we had learned, and (c) some of the surrounding circumstances changed, that is, electron-beam was deemed unnecessary, optical extension was thought to do the job. So some of the work and insights became irrelevant.

The learning of the advanced content through evolutionary advance, the changing circumstances over many years that make some of the work beside the point, and the impossibility of keeping secrets over such a span of years, these are general phenomena.

A related but distinct observation is that one's technological lead in an industrial setting is not the time it takes you to do something, or even how much in advance you start or finish your research or your technology, but it is never greater than the time it takes to copy. Many good ideas are easily copied and although the copy time can sometimes be extended through artificial (or legal) means, that is, patents or copyright, there are many domains and circumstances under which these extenders don't apply. Such an advance will likely be a contribution to the world, but not a competitive advantage.

On the more positive side, in this competitive picture, is the observation that it pays to learn to do things that are genuinely difficult to do. A storage device based on a non-obvious understanding of the structure of a silicon oxide layer may be difficult to duplicate even after the fact of its existence is known. A fabrication process for a thin film head may call for an advanced plating technique, which makes its replication difficult. Knowledge of these techniques then, has a double payoff: it enables one to see the possibility of fabrication, thus obtaining an initial lead, while difficulty of the technique and the necessity for relatively esoteric expertise lengthen the copy time.

One way to make competitively advantageous progress, then, is to do hard things that relate to key aspects of technology. This is helped by a detailed knowledge of one's own product. Although I have used hardware rather than software or design and architecture examples, the analogous software or systems examples do exist. So one comes to the conclusion that one way to make progress is to do hard technical things and drive the technology ahead. A basic research capability in appropriate subjects may be precisely the sort of capability that fits into this scenario.

In addition, a second fit for this capability may be through instrumentation rather than through subject matter. A surface physicist may not deal with the surface of magnetic tape as subject matter, but the delicacy of his or her instrumentation technique gives the physicist an unparalleled virtuosity in dealing with such a surface in times of technical crisis. I have seen this happen repeatedly.

Another, very nontrivial aspect of dealing with basic research in industry relates to the complexity gap between the simplicity of what we can deal with scientifically and the complexity of what we would like to understand in technology. We can deal with individual atoms alighting on a nickel surface, but we need to understand a silicon surface exposed to a gas plasma and interacting with it chemically. Choosing areas of work that are scientifically alive, but in which the complexity gap between scientific understanding and the technical problem is not too great, is part of the task of getting the great power of basic research to function effectively in an industrial setting. It is a specifically industrial problem.

Much of what I have said appears to relate to a world of micro-innovation in which the strategy is to solve tough technical problems rather than introduce totally new and simple ideas or new technologies. This sounds upside down, but it is, in fact, the usual, rather than the exceptional situation. It seems upside down because in general, the cumulative effort of steady technical progress is usually underestimated relative to that of the more spectacular "breakthrough."

The transistor was a breakthrough and it defeated all attempts to incrementally advance CRT tubes or magnetic cores, but since its introduction, incremental advances from 1 transistor/chip to 1 million have in turn warded off every attempt to introduce bold new technologies such as optical logic or cryogenic logic. In technology the most novel technology usually is not always the best, in fact, if the in-place technology is moving rapidly a novel technology has a hard time. So we realize then in technology the most innovative is not always the best. This carries over into products. It is not always true that the most innovative product is the best one. Often it is defeated by incremental improvements in the old.

Perhaps I have said enough to convince you that industrial research, affected as it is by a need to produce competitive products, has certain characteristics of its own. I have also omitted much. For example, I have not discussed the problem of dealing with or trying to foster truly radical innovative events. The subject is truly complex, and it is interesting to note that like many complex subjects, it is being paid the compliment of myth. John Kennedy said, "The great enemy of the truth is very often not the lie—deliberate, contrived, and dishonest—but the myth—persistent, persuasive and unrealistic."[1]

Although we don't usually associate myth with science and technology, it is there. One of the heroic and appealing aspects of Christopher Columbus is that he championed the present scientific belief in a round earth and triumphed over his opponents who believed he would sail west only to topple off the edge of a flat world.

This is pleasing stuff. The "good guy" has the modern and scientific view and the "baddies" are dead wrong. However, the reality was different and not nearly as wish fulfilling. All parties to the dispute about Columbus's voyage knew the earth was round; the dispute was about how large the earth was and hence how far it was to China and Japan. Columbus argued for a small earth with Japan, therefore, not far to the west. In fact, he argued that only 2,400 nautical miles separated the Canary Islands from the coast of Japan. Ten thousand is right. The court advisers had a much more correctly sized earth and said it was too far—he'd never get there. They were right. In the end it didn't matter, the myth has stepped in to improve the story and to make it simpler and more to our liking.

James Watt and the steam engine is another example of a myth many are familiar with. The charming story says that Watt saw the kettle boiling on his mother's stove, realized the power of steam, and when he grew up invented the steam engine. This myth is particularly interesting to me because Watt was, in fact, a significant contributor to the 140-year evolution of the steam engine from a toy to a practical engine of revolutionary impact. But, myth has replaced that evolution by a single invention by Watt, even though in his childhood, the Newcomen steam engine was already hard at work in British coal mines.

Similarly, there are today simple statements relating to technical development. For example, the famous economist, Joseph Schumpeter, in *Capitalism, Socialism, and Democracy,* asserts that large monopolistic firms are ideally suited for introducing technological innovations that benefit society. "What we have got to accept is that [the large-scale establishment or unit of control] has come to be the most powerful engine of [economic] progress. . . ."[2] This is a simple sweeping statement, not popular today, so it is easy to say that it is untrue.

On the other hand, it is almost impossible to say "small company" without saying "small innovative company," and many claim that studies and economists have shown that small companies are more innovative than large. We often hear, "Small firms and independent inventors play a prominent and perhaps even disproportionate role in generating the new ideas and concepts upon which technological advances rest."[3] I have studied these assertions and find little substance there either. I think it is more probably wish fulfillment of one form of the American dream.

Both these statements are attempts to capture, too simply, a subject that has many aspects and complexities. The reality is that the conditions for innovation and effective research are not captured by the simple statement of large and small companies. They are not captured either by the wish-fulfilling thought that the Japanese cannot innovate because they use or do not use the left half of their brains.

The factors relating to innovation and to the role of research in industry are complex and not well understood. They are important to future progress. Today we are dealing largely with oversimplified pictures and sometimes myth. The realization that this is the case is, I hope, part of the process of making progress.

Notes

1. J. F. Kennedy. 1962. Commencement address, Yale University.
2. J. A. Schumpeter. 1950. *Capitalism, Socialism, and Democracy,* 3rd ed. Harper. 106, quoted in F. M. Scherer. 1980. *Industrial Market Structure and Economic Performance.* Houghton Mifflin. 408.
3. Scherer. *Industrial Market Structure.* 417.