

# 24 The Once and Future Industrial Research

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Industrial research is closer to its original aim of the application of science and technology to the advancement of industrial goals than it has been at any time since Sputnik orbited the Earth in 1957. Industrial labs have become better at targeting research on areas of corporate strategic interests, both long-term and short-term. And they are much more adept at working with customers inside and outside the company. In short, they are better at being the engines of tomorrow, which is vital to their long-term survival.

But there are some troubling caveats that surround a lab's existence. One is that, even though research is seen as the starting point of ideas and products, it is the last thing created at a big company. It comes after sales and marketing, development, manufacturing, and all the rest. This makes it harder to expropriate lab ideas for company profits. It also embodies a deeper problem: the belief that, for the short and medium terms anyway, research is not necessary.

In writing *Engines of Tomorrow*<sup>1</sup>, I focused on the great research bloodbath of the early 1990s, which I saw as not just a leveling out but actually a decrease in research and development (R&D) spending. Big companies, like Bell Labs, IBM Research, and DuPont, had to cut back. They trimmed their research staff and they slashed basic science. The general impression was that they were selling their seed corn—that they had sacrificed their long-term future in order to concentrate on short-term improvements to existing products. *The New York Times* said that the resulting shortfall of new ideas could “shackle the economy.”<sup>2</sup>

But research labs, like any company (like any organism, human or not), have to go through periodic upheaval in order to stay vibrant and

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healthy. So I launched a major study of industrial research and evolution to try to understand what had happened.

Some background is needed first. Industrial research was borne out of the German dye industry of the 1870s. It basically brought scientists and engineers into companies in order to change the prevailing hit-and-miss existence. Finding new colors was necessary for survival. But initially this was very much a trial-and-error process. By institutionalizing the hunt for these new colors, they were able to ensure their future stability to a much greater degree.

It turned out that the same intermediate products and processes used to find dye colors could be used to make heavy chemicals, pharmaceuticals, and photographic film. This launched a huge industry. The companies that pioneered this research (Bayer, Hoechst, and BASF) are still around today, 130 years later.

Not long after that U.S. firms started central research labs, and in many ways, the United States set the tone for what industrial research should be. It was companies like DuPont, AT&T, and General Electric (GE) especially that set the tone. The first great lab was General Electric's. The idea came from Charles Steinmetz, their chief engineer and a renowned mathematician. He convinced the corporation to fund the research lab in order to protect its business of lighting. The lab was born in December 1900, on the banks of the Erie Canal, in a carriage barn behind Steinmetz' rooming house.

The lab went through the same traumas that we saw in the 1990s. It was disconnected from the rest of the company, trying to do cutting-edge research in a vacuum. And it was almost eliminated because the rest of the company noticed that it was not contributing to the bottom line.

So the lab instituted programs to get closer to the business divisions and learn more about these businesses. From that springboard, while also proving their worth on relatively small-scale things, they were able to explode into the future with tremendous advances in radio, medical imaging, and other areas that still form GE businesses today.

The lab became so famous that by the 1930s it was known as the "House of Magic." The House of Magic show went on the road all over the country. Up to 12 million people saw it over the next decade or two, and it set the tone for how we came to view industrial research.

This tone carried on past World War II. The war had started another era in research. Scientists emerged from the war as national heroes and keepers of the future. As the Cold War heated up, research at universities and in industry received unprecedented federal funding. A lot

of this funding was for basic research, enabling labs to grow even faster. Scientists enjoyed a unique freedom to pursue ideas.

People sometimes have this perception that researchers just tossed ideas over the wall to development without any concern for how these ideas would work in the real world because that was for development to work out. This perception is somewhat true. But it is just as true that the people on the other side, the developers, did not pay any attention to what came their way, because they were very busy with their existing products. For a time it really did not matter because American companies were enjoying a unique hegemony coming out of World War II. But by the mid-1960s, something was wrong, and it was something that science was not going to be able to fix, alone.

Over the next two decades, as U.S. firms in autos, steel, consumer electronics, and computer memory chips found themselves out-innovated by a host of Japanese and European companies, as well as start-ups like Apple, Cisco, and Microsoft, the need to transform research became apparent. A new view of industrial research very slowly took root. Researchers and developers, or researchers and the rest of the company, had very little practice working together in a way that was compatible with this new world. It was very difficult to make timely improvements to existing products because of this change in pace.

Doing something new was almost impossible. The name of the game became speed and everyone was caught unprepared. For example, IBM invented reduced instruction set computing (RISC) in the early 1980s. The perception is that they just tossed this over the wall, and the company did not do anything with it. Consequently Hewlett-Packard and other companies brought it to market first. But in truth, IBM very quickly recognized the importance of this new invention. They adopted it fairly quickly as a product plan in the business division that made mid-range computers. But introducing RISC to the market meant cannibalizing an existing line of computers. IBM could not do that organizationally. When they finally did bring it to market in 1990, they did so only after they had created an entirely new business division devoted to advanced work stations to bring it to market.

This kind of organizational structure and changes in organization kept occurring as this crisis accelerated in the first half of the 1990s. We saw a blaze of across-the-board cutbacks and layoffs. Basic science was slashed and general turmoil prevailed. The pundits all proclaimed that we were focusing so much on the D side of R&D, that we were forsak-

ing the more fundamental R work that creates the breakthroughs needed to spawn new industries.

When I started my book, I was the contrarian. My research showed that those cuts were actually obscuring a vitally needed realignment that was good for the economy. Bill Brinkman, who is now the head of research at Bell Labs, and was then the head of the physical sciences lab that is still the bastion of the most fundamental work at Bell Labs, said the managers at AT&T asked the researchers in the lab what they were doing for the company. The answer, Brinkman says, was that researchers were too “researchy.” So what happened was that research got a little “de-researchified.” One of the first things to go was the old notion that more R&D is always better R&D. It meant focusing on areas of strategic importance, bringing down barriers between research and development and the rest of the company, and vanquishing the old linear model that said ideas go from the lab to development to manufacturing to market. We needed a much more dynamic system with constant interaction all over this chain.

This was a very painful time for companies. How they reacted depended a lot on their corporate culture. One of the big changes, almost across the board, was a change in the way research was funded. Instead of a central funding mechanism, many labs went to contracts with business divisions for big pieces of their budget, if not all of their budget. That meant that they did not get money for research unless the business divisions funded it. And they were going to fund it only if the labs were doing something much more relevant to the business divisions.

This plan comes with its own set of dangers. But the plan does have a lot of payoffs. One is that it forces researchers to think more about market needs and realities and to understand how business and the real world works to a much better degree.

I studied many interesting programs. I want to describe one more IBM example. In the early 1990s, the head of research at IBM was Jim McGroddy. The company was hemorrhaging billions of dollars every year. He went to visit Citibank’s chief technology officer, Colin Crook, who was a physicist. Crook began talking about the information technology value chain. Atoms and basic math were at the bottom; storage, displays, and chips were in the middle; and customer solutions were on the top. He told McGroddy that it was one thing to have great stuff at the bottom and middle, but what was really important was at the top, with customer solutions, where Citibank could really differentiate itself from its competitors.

Stunned into looking at IBM Research in a very candid way, McGroddy realized that all of its activities were heavily skewed to the middle and bottom layers in the value chain. As he recalls, the model was to be famous for science and technology and vital to IBM. Were the labs really vital to IBM if they were doing almost nothing in the layers of the value chain that mattered most to their customers? The answer, says McGroddy, was obvious.

This realization engendered a dramatic reorganization at IBM Research that saw the creation of a whole new strategic area called services, applications, and solutions. This corresponded to nothing IBM had ever before put in an organizational chart. A big reason behind IBM's successful turnaround is that it was able to drive the amount of research that fit into that category from basically nonexistent in 1990, to more than 25 percent in 2001. This dovetailed perfectly with the new chairman Lou Gerstner's global services initiative, which has become the fastest area of growth for IBM.

The alternative was the demise of IBM Research. And that was very much in the cards. If it had not gotten more connected, and if it had not trimmed back some of its more cutting-edge work and readjusted to the needs of the current day, it would not be here today.

This happened in many places. It is a much more realistic view of industrial research. We now see less basic research going on. IBM does not chase magnetic monopoles anymore, but should it have in the first place? You can make an argument that it set a climate of discovery that attracted people to the company. But people coming to IBM can still work in areas like quantum computing, which is very much alive at IBM, Bell Labs, and AT&T labs. These areas, where work is going on, have potential relevance to what the company is doing.

The current plateau almost assuredly will level off in the next two years, probably in FY 2002 budgets. It may decrease if the economy does not improve. But the name of the game is all about being much better aligned to the world.

The best labs always maintain a small core of basic research. A general rule of thumb in the physical sciences is that research is about ten percent of the overall R&D budget. The basic, fundamental work is about ten percent of that ten percent. So one percent of what is going on in R&D is basic, fundamental research. Companies protect this core for many reasons.

One reason is to make fundamental discoveries, but that is really the ancillary goal. The real reason is to create a climate of discovery that

attracts people to the lab. Another reason is to gain a very deep understanding of the processes involved. If you are making chips and something goes wrong with the chip, it may be that someone with fundamental knowledge in surface state chemistry can help solve the problem.

Another reason is to create a bridge to the rest of the world. Today, you have to partner much more dynamically with the rest of the world. You have to have a core inside your company that can understand, relate, and adapt discoveries from outside the company and apply them to your business needs.

I hear talk that industrial labs are dead. We recently posted on our Web site ([technologyreview.com](http://technologyreview.com)) a column by Hank Chesbrough, a Harvard Business School professor, who says that central research is dead.<sup>3</sup> Of course it is not. (I think he is trying to be provocative.) It is my belief that in this dynamic world, the fusion of big technological areas (e.g., information technology with communications or with biology) is just a piece of what is going on. All kinds of specialties, major technologies, and scientific disciplines are fusing together. You need a place where people can step back from the fray and integrate, assemble, and adjust to all this massive technology that is coming together in important ways.

Central labs are such places, so in many ways they are actually becoming more important for big companies as times become more confusing. It was not basic science that made places like Bell Labs great. Innovation and great discoveries almost always come from crosscurrents of interactions between people of different disciplines and ideas, of theorists with experimentalists, of biologists with chemists, and of computer scientists and circuit people with wave guide people.

It was from this tremendous mix that the great discoveries of Bell Labs and IBM sprang. And we are expanding this mix on a massive scale. We are talking about research mixing with the rest of the company and getting ideas from marketers, developers, and manufacturers. We are talking about this company interacting with other companies, and this nation interacting with firms from other nations. We are going to explode with innovation and continue this at an ever-faster pace over time. Although I know there will be blips on this path, I am very optimistic for the future.

## Endnotes

1. Buder, Robert. *Engines of Tomorrow: How the World's Best Companies Are Using Their Research Labs to Win the Future*, Simon & Schuster, New York, 2000.
2. Uchitelle, Louis. "Basic Research is Losing Out as Companies Stress Results." *The New York Times*, October 8, 1996, A1.
3. Chesbrough, Hank. "Rethinking Corporate Research: Is the Central R&D Lab Obsolete?" *Technology Review*, April 24, 2001. ([www.technologyreview.com/web/chesbrough/chesbrough042401.asp](http://www.technologyreview.com/web/chesbrough/chesbrough042401.asp))