Technological Innovation

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The Organization of Innovation—The History of an Obsession

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Will We Ever Invent Anything This Useful Again?

The Economist's mission statement, printed on the contents page in every issue, proudly proclaims that it has been leading the debate about progress since 1843, yet the January 12, 2013 issue struck an uncharacteristically pessimistic note on this core topic. The cover carried an illustration of Rodin's *The Thinker* perched on a new base—namely a toilet with a cistern. A thought bubble over *The Thinker*'s head reveals the question he is pondering: "Will we ever invent anything this useful again?" The reason for his inner turmoil is stated in a caption at the toilet's exit point, where his physical "output" is soon to be flushed down into the sewer; it is "the growing debate about dwindling innovation". This illustration is doubly apt, seeing as how the *The Economist* had not even come up with the idea for this image in the first place.[1]

What sort of debate are we speaking about here? The cover story cites high-tech entrepreneurs and academic economists in arriving at the prognosis that our society is standing on a technological plateau, with the economic way forward destined to rise no higher.[2] The authors of this piece in the *The Economist* proceed to tack on a diagnosis to this prognosis, and maintain that our degree of technological innovation does not even come close to keeping up with the previous pace, up to and including the 1960s, even though more people than ever work in research today, more money than ever flows into science, and there is more competition than ever in the marketplace of innovation. We are told that even the attainments of the digital age—from the personal computer to the internet—have not been reflected in "the productivity statistics".

Despite what the authors would have us believe, the "innovation blues" lamented in the *The Economist* have little to do with the current course of technological development. The source of the perceived problem arises instead from a sense of disappointment over the fact that their innovation theory does not hold up to its empirical promise. The theory is made up of a chain of causation that sees science as the most important driving force behind innovation, and innovation as the most important driving force for the economy, and an organizational principle maintaining that the three links in the chain function most efficiently under market-oriented competition.

The problem with this theory is that only the first part of its causal chain stands up to empirical scrutiny. There is every indication that progress in scientific knowledge leads to technical innovations, but it is highly unlikely that a higher level of innovation results in greater prosperity. Most innovations do not succeed in the marketplace, and of the few that do catch on, only a fraction get to the point of offsetting the magnitude of their "creative destruction" with their own growth impulses. In fact, there are even economic sectors that achieve high rates of increase by means of a strategic and concerted effort to *skirt* innovation. Year after year, the Swiss watch industry has expanded the market for its mechanical watches, which in comparison with quartz watches are not only technically outmoded, but also more expensive, less accurate, and more fragile. Evidently their consumers are seeking identification rather than innovation.

What ultimately emerges from the analysis in *The Economist* is the plausibility of the claim that, in spite of continuously rising investments in research and development, countries in the West are experiencing weaker and weaker attainments in the realm of innovation. With the single exception of the communications industry, the age in which we are living is marked by big promises and small advances. No matter which area we look at—energy, health, transportation, or household—the significant breakthroughs that have been prematurely hailed over decades of earnest efforts have yet to materialize. The technological foundation of our daily life is now 50–150 years old.

If we accept the above-mentioned diagnosis that innovations are moving along at a snail's pace these days, we ought to take a closer look at the era that today's observers, including the authors in the *The Economist*, regard as the golden age of technological innovation, namely the decades between 1920 and 1960. What distinguishing features did the organization of science and technology have in that era, and what do today's research structures lack in this regard? There is no definitive answer to this question, but we can take tentative steps in the right direction by looking to the formulas that leading figures in academic and industrial research in the United States at the time used to enhance the creation of fresh approaches.

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A comparison between these previously used formulas with those used today strongly suggests that our greatest stumbling block to innovation is our theory-based obsession with innovation itself. This obsession has made scientists and technical experts play by rules stipulating that they can deliver outstanding results only if they are exposed to the competitive forces of a market; and if no such market exists—as in the case of government research funding—a market has to be simulated. Before 1960, the organization of innovation had hewed to the diametrically opposite principle. At that time, the goal had been to shield academic and industrial research from the demands of the market to the greatest possible extent, and wherever there was a market—as in the case of industrial research—it had to appear to *dis*appear. At the very core of this principle was the idea of fundamental research.

The Invention of Fundamental Research

The United States emerged from World War I as the economic victor, and the technical sciences as the academic victor. Technical scientists had been treated like ragamuffins in the rarefied world of academics for quite some time by the members of the traditional university disciplines, but once they had proved their mettle in the sphere of military utility, they were able to become established once and for all when the war was over. With the rise of the technical sciences, however, a new distinction took hold at universities, namely a distinction between "applied" and "fundamental" research.

Most institutions of higher learning—including institutes of technology—tried to retain as much of their old hierarchy of values as they possibly could in making the transition to the new order. "True", "pure" science—that is to say, fundamental research—would take priority within their walls. The argument ran that these scientists not only enjoyed complete independence in their work, but were also at the very root of scientific progress. Applied scientists, by contrast, were regarded as doubly dependent; they had to utilize knowledge from scientists who had conducted the fundamental research, and to carry out tasks from parties outside the halls of academe. Although this ideological foundation is sure to strike us as paradoxical today, it enabled the ascent of the technical sciences between the wars to be accompanied by a dramatic upswing in fundamental research.

This constellation lined up with an ideal of scientists that sociologist Robert K. Merton associated with four "cardinal virtues" in World War II: universalism, communism, disinterestedness, and organized skepticism.[3] When Merton employed these terms, he did not necessarily have in mind what people might associate with them today. "Universalism" encompassed both the claim to objectivity and the irrelevance of race, class, nation, and religion. "Communism" referred not to collective ownership of the means of scientific production, but to the open availability of scientific consumer goods in the form of publications. "Disinterestedness" denoted the independence of scientific research from political and commercial objectives, and "organized skepticism" applied to the duty to give critical scrutiny to all claims to knowledge that a scientist uses as a basis for his or her own work.

Merton's "cardinal virtues" arose from two defensive reactions that were poles apart. One was directed against the totalitarian tinkering of an Aryan or proletarian science in Germany and the Soviet Union, and the other against the research industry in the United States, which was controlled by private or public funding. The anti-totalitarian argument could count on the consensus of the readership at home, whereas the antidirigist argument was politically charged at a time when American weapons research was being expanded as a consequence of the war (the Manhattan Project, of which Merton obviously knew nothing at the time, had been launched in the same year). All the developments of the era notwithstanding, Merton insisted that the research university was the sole institutional home for science.

With his ideology of the "ivory tower", Merton found kindred spirits where he had least expected them: in the executive suites of the major commercial laboratories. As Steven Shapin has shown, the leading practitioners and theorists of industrial research in America also supported an ideology based on the notion that innovations arise in an academic setting.[4] One of them was the physicist Kenneth Mees, who had spent several decades as the director of the Eastman Kodak Research Laboratories and during his tenure there had written a fundamental work on the topic of *The Organization of Industrial Scientific Research*. The first edition of this work was published in 1920, and a second, greatly expanded edition followed in 1950.

Mees introduced the second edition of his work by chronicling the huge increase in staffing in industrial research, which was dictated in part by the exigencies of the war, since the publication of the first edition. In 1920, the United States had fewer than 10000 scientists working in industry, but by 1947, propelled by the investments in the military and defense domains that had been necessitated by the war, this number had risen to more than 130000. In Mees' view, however, this enormous upswing did not give industrial scientists any triumph to lord over the universities. Instead, as he emphasized in the introduction: "The preeminence of the universities in scientific work will continue as long as the research work in university departments continues free from any external direction or organization." Compared with other public and private research institutions, he argued, universities actually ought to be at a sharp disadvantage. After all, professors spent a good deal of their time doing administrative work and teaching, and were sorely lacking in funding for research equipment; moreover, they were not given much in the way of grant subsidies, and their salaries were paltry. They were however able to compensate for all these disadvantages thanks to an invaluable benefit that a university post offered them: "They are *free*—they can explore unpromising paths and make experiments that any administrator would regard as useless, and sometimes those experiments succeed and those paths lead to new fields of knowledge."

Mees used this idea as the basis for the central argument of his book. The key to success of an industrial laboratory, he explained, lay in the ability of its directors to recreate the organizational advantages of a university in a commercial setting. Industrial scientists ought to be given the greatest possible latitude to conduct their research as they see fit, with less outside interference, flat hierarchies within the institution, and department heads who are themselves scientists. Like professors at universities, he contended, the senior scientific staff should hold permanent appointments, and all scientists ought to have the opportunity to publish their research results.

Like Merton, Mees did not believe that successful research outcomes were purely a function of the brilliance of the scientific staff. Far more crucial, in his eyes, was an academic culture that was buttressed by a university structure. Mees felt strongly that the "average man" was also capable of making a major breakthrough if placed in an inspiring setting.

For Mees, industrial research was a "gamble", and could not be conducted according to the rules of "efficiency engineering". Research, he insisted, requires a great abundance of staff members, ideas, money, and time. Anyone who is unwilling to wait ten years or more for the first results to emerge has no business setting up a laboratory in the first place. Mees established the following rule for the organization of scientific work: "The kinds of research which can be best planned are found to be those which are least fundamental." But because Mees regarded the basic sciences as the most important source of innovation, he advised research directors to try not to rein in their scientists with assignments, but instead to inspire them with questions.

Mees promoted a "linear model" of industrial research, with fundamental research as the starting point of any further technological stages of development all the way down to the finished product. The introduction to his book contains a chart that shows the most important arrows gradually shifting away from fundamental research, with none of the remaining arrows leading toward it.

The Theory and Practice of Bell Labs

A rule of communication at AT&T's Bell Labs illustrates the way in which a procedural one-way street in models of this kind was put into practice. The applied scientists had to drop in on the scientists conducting fundamental research to keep abreast of their work, while the members of the latter group were not required to take active steps to find out what their colleagues over in the applied sciences were up to. The fundamental scientists' only obligation was to keep themselves available to field questions from the other divisions, thereby enabling them to feel confident that they were at the source of all innovation.

The physicist Mervin Kelly served as the director of Bell Labs between 1934 and 1959. He occupied a similarly dominant position in his company as Mees did at Eastman Kodak, and he also promoted a linear model of research and development. In 1950, he had the honor of presenting his philosophy of research to the Fellows of the Royal Society in London. The timing of this presentation was no accident; three years earlier, William B. Shockley, John Bardeen, and Walter H. Brattain had invented the point-contact transistor at Bell Labs, and in 1956 these scientists would be awarded the Nobel Prize in Physics.

In his speech, Kelly referred to Bell Labs as an academic-sounding "Institute of Creative Technology".[5] It employed 5700 scientists, engineers, and technicians, divided among three departments: research and fundamental development, systems engineering, and specific systems and facilities development. Kelly defined the first department as a "non-scheduled area of work" in which the scientists enjoyed the same freedoms as their university counterparts: "It provides the reservoir of completely new knowledge," he explained, and extends "across all sectors of science" that might enhance the technology of communication. Kelly went on to name more than a dozen disciplines in the areas of physics, chemistry, and mathematics. In 1949 alone, he reported, this department published more than two hundred scientific articles, which made it that much easier to attract researchers from the top universities.

The only crucial distinction Kelly drew between universities and industrial laboratories was in regard to cooperation among the scientists. Fundamental research conducted in an industrial setting, he explained, is dependent on researchers in a variety of disciplines remaining in contact on a constant basis, which is the only way to ensure the integration of research that is crucial for the production of commercial goods. In order to establish the required communication in a reasonably spontaneous manner, the space would need to be structured in a suitable manner. For this reason, Kelly devoted an entire section of his speech before the Royal Society to the architecture of the laboratory complex in Murray Hill, New Jersey, which had been completed back in 1941 under his leadership.

Kelly had come up with a concept of long corridors that the researchers would have to walk down to attend to some of their less cerebral activities (such as trips to the bathroom). This is where he pictured the all-important informal exchange of ideas taking place, outside of the established work groups. If interesting ideas for research emerged in the process, the ad hoc work groups that had formed in the corridors could delve straight into implementing them. With this goal in mind, Kelly had requisitioned a system of movable walls so that the changes that took place behind the long corridors could be easily accommodated.

Statements by researchers who returned to universities after working at Bell Labs support the idea that this communications concept actually worked. The engineer John R. Pierce, who had promoted the development of the point-contact transistor and was appointed to the faculty of the California Institute of Technology in 1970, summed up the culture of Bell Labs as follows: "People cared about everything." He lamented the fact that this was decidedly not the case in his new academic home. "Now at the university, no one can tell a professor what to do, on the one hand. But in any deep sense, nobody cares about what he's doing, either."[6]

Innovation as Marketing Tool

Kelly's report about the organization of industrial working processes also shed light on the image-shaping policies of Bell Labs, which in the course of the 1950s and 1960s took on the character of an all-out campaign. The Bell Labs were no exception in this regard. Workplace communication about "creative technology" for marketing purposes in the early postwar period marked the beginning of the obsession with innovation that is so familiar to us now, but the ideology of the university setting as the proper locus for innovation was still in place. The contents of the Bell Labs campaign made for perfect examples of this state of transition.

In order to document the cooperative spirit and the benefits this spirit yielded in the laboratories at Murray Hill, scenes of the Nobel Prize winners Bardeen, Brattain, and Shockley working together were re-enacted, photographed, and published. Today they can be found in many books about the history of computers. The irony behind these group photos, orchestrated right down to the direction the scientists were facing, is that in this case the invention was the product of an exceptionally *un*cooperative collaboration among these three men. After all, Shockley, the overseer of the team, had undermined the research of the scientists subordinate to him with his own (successful) experiments.

Claude Shannon, another scientist who came to be regarded as one the founding fathers of the computer age because of his pioneering work in digital information theory, was trumpeted as the personification of academic freedom and scientific individualism in an industrial setting. Shannon was a mathematician, cryptographer, and communications theorist who joined Bell Labs during World War II. He was a scientific jack of all trades and a tinkerer par excellence. He constructed an electromagnetic maze into which he placed a metal mouse that was named Theseus and was equipped with an artificial learning device and could find its way out no matter where it had been placed. Bell Labs arranged for this high tech plaything to get a patent and produced a short film in which Shannon got to introduce his invention in person.

By the early 1950s, Shannon had become a minor celebrity within the Bell Labs, in part because he liked to ride down the long corridors on his unicycle, occasionally juggling balls in the air as he did so. Another invention that brought him fame, thanks to clever marketing, was known as The Ultimate Machine. It did not exactly look high-tech. This device consisted of a suitcaselike box with a switch. Every time the switch was flipped to the "on" setting, the lid of the box opened and a wooden arm reached out, flipped the switch back to "off", retreated back into the box, and the lid snapped shut once again. Bell Labs made several replicas of this machine and displayed them to the public.

Why had AT&T launched this sort of communications campaign for a research division? As Jon Gertner explains in his excellent history of Bell Labs,[7] observers at the time surmised that AT&T had to convince the lawmakers that it would be in everyone's best interests to continue to grant the company a monopoly in the American telecommunications market. The company had been a de facto monopoly for quite a long time as it was. This monopoly became the subject of greater and greater controversy in the course of the waves of liberalization in the postwar era, until the company was broken up in a series of regulatory and judicial interventions between 1974 and 1984.

The Bell Labs of AT&T were far from the only enterprise to combine innovative fundamental research with monopolistic market dominance. Eastman Kodak had controlled the American market for film, cameras, and other photographic materials since the 1920s, with a market share of 80 to 90%; IBM, another prime example of successful fundamental research in the postwar era, was issued a consent decree by the United States Justice Department in 1956 designed to thwart its quasi-monopoly in electronic data processing; and in 1973, IBM was stripped once and for all by the courts of its market dominance. The list of monopolistic industries with a heavy emphasis on research, which could easily be expanded, would appear to indicate that the impressive bounds in innovation made in industry between 1920 and 1960 in an array of fields were an outgrowth *not* of a functional economic competition, but rather of a monopolistic structure that enabled market leaders to invest a great deal of money, human resources, and time in fundamental research.

Because monopolies could not be justified directly, the only route the companies in question had open to them was a detour that required more and more elaborate displays of their capacity for innovation. Once again, architecture proved to be well suited for this purpose. In the late 1950s and early 1960s, several American industrial groups built new research centers. They opted to locate them in isolated surroundings in the style of a modern university campus, and favored a new architectural style that moved away from the traditional laboratory complexes based on classic industrial buildings such as the one in Murray Hill. The architect best suited to their vision turned out to be Eero Saarinen, who had made a name for himself in the United States as a modernist designer of furniture and airports. After 1955, within the space of a few short years, he was commissioned by General Motors, IBM, and AT&T, and he constructed enormously large and showy glass buildings.

The architecture critics of the time soon came up with an apt name for these buildings: "Industrial Versailles". The term was fitting because the new research centers were to industrial innovation what Versailles had been to the Sun King: complexes of representation, as the historians of technology Scott Knowles and Stuart Leslie have detailed.[8] In the new Bell Labs at Holmdel, Saarinen tried to adopt Kelly's idea of the long corridors as places for chance encounters, yet once the researchers had settled in, they quickly realized that the corridors were suited less to discussion and speculation than to the symbolic demonstration of innovation.

The opening of the opulent industrial buildings came at the very time that innovation entered the political discourse on a grand scale in Western countries. Here, too, the need for a representative gesture was a key element, but it arose from a very different outlook about the conditions under which innovation could flourish.

The Sputnik Shock

The end of the university culture of innovation was signaled not by the economy, but by the state. Kenneth Mees, the Head of Research and Development at Eastman Kodak, had already warned back in 1950 about the dire consequences that “a national directing and organizing system”, in the way it had been conducted in recent years, during World War II, would wreak on the freedom of research, in particular at universities.[9] The National Science Foundation (NSF) was established that same year and would become the first and foremost in a series of government agencies to promote research in the Western world. It began with a modest budget, but that would change in one fell swoop when the Soviets shot the first satellite into space in 1957 and plunged the United States into a collective state of shock. Fearful of forfeiting the country’s technological edge over the Soviet Union, the government quadrupled the annual budget of the National Science Foundation in 1959, from 34 million dollars to 134 million dollars. By 1968, the budget had soared to 500 million dollars. The new scientific arms race immediately drew in many western European governments as well. On both sides of the Atlantic, the goal was to step up not only the quantity of innovations, but also the tempo at which they occurred. Mees’ notion of giving free rein for ten years to researchers without a promise of a successful outcome appeared to be a luxury that could no longer be afforded, considering the speed of the Communist countries striving to catch up.

The explosion of government research funding resulted in serious quandaries about how best to allocate these funds. There were countless research institutes, and there was a need for clear rationales as to which institutions and individuals would be entitled to how many dollars that came from taxes. An attempt was made to meet this challenge by introducing an element of marketlike competition. Artificial competition for project-related subsidies, to be regulated and controlled by the funding agencies, was set in motion. Successful proposals needed to provide precise details about the scope of each project and a set time frame was assigned for the completion of a given project, which made it necessary for grant seekers to package fundamental research as though it were application-oriented. This set-up ushered in a period in which innovations were proclaimed well before the fact, and talked up as monumental breakthroughs in the quest to secure funding. Representation became integral to production.

It did not take long for this new regime to have drastic reverberations for industrial research. The flood of money that inundated the research universities heightened the incentive for industrial groups to outsource costly laboratory work to universities or public research centers. At the same time, they were inspired by the public administration’s belief in the rules of the market to pay heed in their own research divisions to the credo that the innovative impulse requires the intensity of a competitive situation. In the long run, the private sector did its part in making the new form of market-oriented project research the only accepted organizational principle.

This market-oriented structure of innovation was already firmly in place when Neoliberalism forged ahead with New Public Management in the 1980s. Needless to say, it raised the simulation of market forces to a whole new level. It blurred the lines between the public and private sectors, channeled fears of a technological lag into a frenzy of economic competition, tossed aside the linear model of research and development, and hailed applied science as the most important engine of growth. Today we have reached a point at which Western governments are beginning to openly question the value of funding scientific projects that lack a practical orientation. The Cold War has been over for a long time, but when it comes to the organization of innovation, we are still under the spell of Sputnik shock, whether we realize it or not.

Economic growth spurts may come and go, but as long as the policies governing scientific research remain beholden to the forces of the market, our relationship to innovation will remain that of a drunken man to copulation: our appetite will be great, and we will talk a big show, but our ability to perform will be limited. If we want to get out of the spot we are in, we first need to figure out how we were maneuvered into it in the first place. This Essay is an invitation to make headway in this process. However, anyone who might be inclined to infer from this discussion that the solution to the problem would be a return to the 1950s would be on the wrong track. The epoch that spanned the years 1920–1960 yields insights into what has gone wrong today, but not into how to make things better for tomorrow. There is no going back to ivory towers and industrial monopolies; the solution can only lie in the future. But the crux of the problem with innovation is bound to remain the same: we will never know exactly what prompts it.

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[1] Depictions of “Rodin’s Stinker”, which range from graffiti renderings to plaster of Paris sculptures, abound on the internet. Most of them are several years old.
[2] An essay by the American economist Robert Gordon serves as the key reference: R. Gordon, *Is U.S. Economic Growth Over? Faltering Innovation Confronts The Six Headwinds*, National Bureau of Economic Research, Working Paper 18315, August **2012**.
[3] “Science and Technology in a Democratic Order”: R. K. Merton, *J. Legal Political Sociology* **1942**, Nr. 1.
[4] S. Shapin, *The Scientific Life. A Moral History of a Late Modern Vocation*, University of Chicago Press, Chicago, **2008**.
[5] “The Bell Telephone Laboratories—An Example of an Institute of Creative Technology”: M. J. Kelly, *Proc. R. Soc. London* **1950**, *203*, 287–301.
[6] This statement comes from a 1992 interview, which is quoted in E. E. David et al., *John Robinson Pierce* in National Academy of Sciences, *Biographical Memoirs*, **2004**, Nr. 85.
[7] J. Gertner, *The Idea Factory. Bell Labs and the Great Age of American Innovation*, Penguin, New York, **2012**.
[8] “‘Industrial Versailles’: Eero Saarinen’s Corporate Campuses for GM, IBM, and AT&T”: S. G. Knowles, S. W. Leslie, *Isis* **2001**, *92*, 1–33.
[9] C. E. K. Mees, *The Organization of Industrial Scientific Research*, McGraw-Hill, New York, **1950**, p. 15.

