

Session 2

Science and Sensibility: When Pasteur's Quadrant Meets the Real World of Innovation

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This paper presents some observations about innovation as it relates to the nature of research and technology, with some attendant policy implications. My aim is to illustrate that the application of familiar typologies to classifying types of R&D can be complicated by the complexity of innovation in real world environments, and the constraints of implementation. These observations are based on my research findings from Singapore as well as the broader academic literature on innovation.

I will begin by reviewing the use of Stokes's typology, which has influenced current US science policy thinking perhaps to a greater extent than most. In some ways, the heavy state-led funding of biotech and health-related fields in Asia is also driven by a similar philosophy, but it is one, I will argue, that has connections to different commercial realities than Stokes might have imagined. Hence, the straightforward application of Stokes's or Stokes-like typologies to forming policy needs to be more carefully examined.

It is useful to begin by recalling that Stokes (1997) sought to displace Vannevar Bush's 1950s notions that, firstly, curiosity-driven basic research was paramount in a national R&D scheme, and secondly, that innovation necessarily had to come from following a linear path (in which basic research results flow through succeeding stages in a process of increasing productization). In addressing the first notion, Stokes noted that while much of the research work conducted in the past had been curiosity-driven basic research (Bohr's quadrant) and use-driven applied research (Edison's quadrant), increasingly, use-inspired scientific research (Pasteur's Quadrant) could have a greater impact on economic development.

However, I would argue that the notion of "use-driven" may be too general to be of help, be it in applied or basic research, when we consider how innovations actually occur nowadays, or various other notions of innovation. Used too liberally, it may even drive out other forms of innovation. While most of my observations relate to applied (technological) research, some of the same points might be partly applicable to basic (or pure science) research. My concerns stem from four observations of how innovation takes place, or how research and innovation are related:

1. Problems from assuming that application is rational and straightforward: the crooked path (and adaptive business behavior) to "serendipitous" outcomes?

The actual application (of a technology) is not always determined beforehand, and serendipity may play a role in the final application of a technology. Classic examples are 3M's Post-it pad; and Edison's belief in his phonograph's most likely uses — all of which were wrong.

In our example from Singapore, one particular program that we recently investigated was that of spinoffs from one of the public research institutes focused on applied (engineering) research (with a limited sample of cases). (Kumaraswamy and Tschang, 2006; Amsden and Tschang, 2003). These were initially developed under the Singaporean government's "technopreneurship" program of 1997. Our starting point was the investigation of the process by which applied research done in public research institutes was translated into successful (or unsuccessful) spinoffs. Public funding of research has been a well-known mechanism for handling market failures in innovation, but the process of innovation in public research institutes has been less studied on a project and process level. One preliminary observation emerging from our study is that: while researchers accumulate general knowledge about how specific technologies can be used, e.g. by working on contracted projects (from external clients), the projects themselves were generally not of commercial value until the team members left the labs as part of spinoffs. In our study, spinning off led to a process by which technologies were differentiated (from the market) and applied (in product development) by way of combining these technologies with other technologies (i.e., combinative innovation) and adaptation (to the market needs and other conditions). Thus, while the lab technology may remain as the core of a product, it is the combinative engineering and business model activities and business behaviors outside the lab that truly helps to adapt it to the marketplace.¹

This suggests that while the research stage may actually be quite rational, serendipitous acts may be as or even more common when the technology is moved into the marketplace. This may be a consequence of the inundation of a host of other factors rather than the pure technological trajectory.

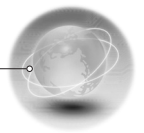
2. Problems in understanding just how malleable modern technology really is: Is the rise of business models and other "problem-setting" actions the real driver of innovation?

The above behavior of adaptive behavior towards technological innovation can be considered to be a specific instance of a more general issue: that technological innovation is increasingly combinative in nature (as well recognized by scholars of innovation such as Fumio Kodama with his concept of technology fusion). In current day industries, successful innovations can be driven as much by the combination of appropriate business models with technologies and applications, as by technology or technology and application alone. At one extreme, it has been recognized alike by businesspeople such as Internet business model pioneer Jay Walker that business models and the problems they are founded on may be the true innovation drivers. In this model of reality, the combining or blending of technologies can easily be done. It may be that the ability to mix and match technologies to form combinations of technologies is increasing as the world becomes increasingly software-driven and as technology becomes componentized.

3. Problems in understanding "use-driven": Which user are we talking about anyway?

There are a number of types of "users" out there (at least three by my count), and which "user" actually helps to create an innovation might even be considered a serendipitous affair.

¹ While the licensing model can be made to work, it may be rarer with combinative technologies, as larger companies which might like these technologies would rather deal with an entity that can do further technology development or productization.



In one of our spinoff cases, the first type of user – a specialized user who works with the innovator – was found to be invaluable in refining the technology, as per conventional studies of user-led innovation. However, in benchmarking our study with other startups and cases, we also came across instances where the head of the organization (often times a businessman) became a second type of “user” who could help the invention by “setting the problem” or vision at the outset. Finally, in studying other industries, we have also discerned a third type of user – “the developer as user”. For instance, in videogames, the developers often think and design for themselves as users/players.

In general the likelihood that a wider variety of users might impact on a technology’s uses might be particularly true for the modular or general “platform” technologies that can be reapplied or recombined, e.g. flash memory and other storage devices. This creates a hope for further differentiation by a wider host of actors, including firms and product developers (e.g. engineers) in the “applications” market, but may also dictate that new approaches to industrial development strategies are needed.

4. Problems in capturing value: Whose capitalization are we talking about?

Who actually capitalizes on a technology to make an innovation can be quite different from who does the innovative research. This has strong policy implications for countries where the innovation systems are not as complete as needed to be able to take advantage of basic research results.

Knowledge of modern industries, particularly in the US context, suggests that large multinational corporations are more robust than was once suggested. Harrison’s final work suggested that large corporations have two strengths: larger resources and their global reach. On top of this, there is the important evidence from the US biotech industry that smaller, supposedly more innovative firms, have eventually been acquired by the so-called “big pharma”, or in the case of the agro-biotech, the “big agros”.

The high cost of commercialization is particularly true in areas of biosciences, where products can only be productized with great effort and resources (in product development, testing, marketing, etc.). That there is still a “short, but linear link” between science and product development cannot be ignored. In this way, the typical vehicle for productization must almost always be: (1) either a larger company with significant R&D absorptive and transformative capacity or; (2) a smaller company that directly starts-up from that particular lineage of scientific work, and which can access capital and work with the large companies as intermediaries to a more productized stage, at which point the acquisition by a larger entity takes place. While this collaborative arrangement can be mutually rewarding as it can share risks of development, for countries as Singapore which do not have these native large companies, but which are forming these native science bases and entrepreneurial environments for smaller firms, it becomes even more important to examine possible paths by which the ultimate commercial value is realized, and whether some paths lead to the nation’s value being curtailed by the acquisitions process.

References

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