Research Policy

The roots of innovation

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"The writings on invention seem to be of extraordinarily mixed quality. There seems to be no subject in which traditional and uncritical stories, casual rumours, sweeping generalisations, myths and conflicting records more widely abound, in which every man seems to be interested and in which, perhaps because miracles seem to be in the natural order, scepticism is at a discount."

JOHN JEWKES, DAVID SAWERS, RICHARD STILLERMAN, 1969¹

Much mythology surrounds the source of important discoveries. Every child knows that Newton had an apple fall on his head, that Fleming discovered penicillin because a spore drifted in through his open laboratory window, and that Kekulé dreamt the structure of the benzene ring after falling asleep in front of the fire. The reality is much more complex (and less romantic) and largely unelucidated. The years since the war have, however, seen studies of how innovations arise; and the studies have led to intense debates because they have obvious implications on how to spend limited research funds.

Two models of innovation

Science policy researchers (a mix of social scientists, historians, scientists, political scholars, and economists) have proposed two main models of how innovations arise. The first is the linear model of development in which basic research leads to applied research leads to experimental development leads to an innovation (fig 1).

The linear model

Curiosity driven→Applied research→Experimental development→Innovation

The market pull model

Market need→Applied research→Experimental development→Innovation

FIG 1—Two models of innovation.

This model has also been called the "science push" model—discoveries in basic science provide the impetus that eventually leads to the innovation. This is the model to which many scientists, particularly basic scientists, adhere (even if it's an unarticulated adherence). The other main model is the "market (or technology) pull model"—the urgent need for a particular innovation leads to the scientific work being done to develop it (fig 1).

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The atomic bomb is the best example of an innovation that was developed in the linear way. Towards the end of the 19th century scientists first began to question the indestructibility of the atom, and in 1917 Rutherford first split it. Chadwick discovered the neutron in 1932, and later Fermi used neutrons to split uranium into what Hahn and Strassman identified in 1938 to be barium and krypton. Hahn calculated that these two atoms would repel each other with a far greater force than previously described in any atomic event and predicted the chain reaction. Joliot-Curie produced a chain reaction and published their results in *Nature* in 1939. Within six years the United States had spent \$2 billion and constructed an atom bomb. As Ronayne says, this innovation "resulted entirely from the scientific discoveries of men and women working in universities, whose motivation was the advancement of knowledge, not practical utility. It is a classic example of the linear model of innovation."²

Ronayne illustrates the market pull model with the example of the Interscan aircraft landing system.² It was in 1968 that the International Civil Aviation Organisation decided that a new system was needed because of increasing air traffic and the deficiencies of the old system. It asked for submissions, and by coincidence a threatened Australian department of radiophysics looking for a way to keep itself solvent discovered the organisation's need. The department used published reports on radioastronomy to produce the system that was eventually adopted.

Here the market need was clear, and no new basic research had to be done to lead to the important innovation. The concept was followed sequentially by strategic research, applied research, and experimental development. Mrs Thatcher will also be interested that economic threats to the government division caused it to come up with an important and economically valuable innovation. If more money had been available more might have been discovered about remote stars but other ways would have had to be found to bring down the world's aircraft safely in the 1990s.

Contributions to innovation

Many innovations, however, probably do not result from either the science push or the market pull models. Several retrospective studies have now been done that have attempted to investigate in more detail and quantify the factors that are important in innovation. These studies are all retrospective and so suffer from the familiar defects of such studies, and they also concentrate on innovations and inventions rather than on conceptual developments and theoretical insights. This is thus to load the dice towards technologists and to undervalue the contribution of people like Einstein.

One of the first of these studies was done by Jewkes, Sawers, and Stillerman and published in 1958³; they looked at the development of 61 important scientific inventions, including streptomycin, insulin, and the electron microscope. They concluded that 33 of the inventions were the result of individual research and that 21 resulted from industrial or mixed government and industrial research (seven

were unclassifiable). The routes to an invention were many and various and could not be easily planned or predicted: "the path of innovation is always thorny . . . there are no short cuts to success, no infallible formulae."

Jewkes, Sawers, and Stillerman also concluded that "the theory that technical innovation arises directly out of and only out of advance in pure science [the linear model] does not provide a full and faithful story of modern innovation." In a later edition of their work that considered another seven case studies they extended these conclusions; science and technology, they wrote, do not always march together; science does not always carry with it an economic pay off; and supremacy in science does not guarantee maximum economic growth.

In terms of policy Jewkes, Sawers, and Stillerman came down strongly on the side of eclecticism: "the conditions under which inventions have arisen up to the present day are so diverse that safety would seem to lie in numbers and in variety of attack . . . there is much truth in the belief that 'the only thing men of power can do for men of genius is to leave them alone' . . . it is all to the good that there are very many different types of research institution . . . because the invention can easily be discouraged by an unfavourable environment and may often be frustrated in a large research organisation."

This study was highly influential when it was first published, and the lessons on eclecticism might well be reconsidered by the government and the Advisory Board for the Research Councils before they go further down the path of concentrating the research effort in Britain. Freeman has, however, criticised the study; the innovations studied were too important and so not representative of the many smaller and less spectacular but still important innovations of the 20th century; the list was biased in several ways towards independent inventors; and bias was detectable in one case study—that of the catalytic cracking of crude oil—because although an independent innovator made the original discovery his technique was quickly superseded by a much superior one developed in the laboratories of the big corporations.

A still more important criticism of the study viewed from today is that it is just too old. Since the war governments, particularly the United States government, have poured money into funding research in a way that they didn't do before, the pace of research has increased enormously, and scientists have been brought together into larger and larger groups. Few of the researchers that I spoke to had any sympathy with the romantic concept of an isolated researcher—perhaps a clinician—making an important discovery in medical research. They just don't think that it happens like that anymore, and they talk a great deal of "critical mass"—the idea that you have enough talented researchers from different disciplines around you to spark ideas and give you ways to follow them through. Some evidence is available to support these ideas—it comes from a project described below that looks at the sources of five major innovations, including the contraceptive pill.⁵

Project Hindsight

The next important retrospective study of the source of innovations after Jewkes, Sawers, and Stillerman was undertaken by the American defence department in the mid 1960s.⁶ It had at this time spent \$10 billion on research since 1946 and wanted to study which factors were important in leading to successful research—that is, effective new weapon systems. The aim was to improve resource allocation and the management of research programmes—aims with which any body funding research would empathise. The study, which needed more than 50 person years of effort (explaining why such studies are not common), was interested particularly in the optimum balance between basic and applied research and in whether government, industrial, or university laboratories were more productive.

Twenty weapon systems (including the Polaris missile and the Minuteman intercontinental ballistic missile) were considered by groups of five to 10 scientists and engineers, who identified "research or exploratory development events" that had been important in developing the systems. A research or exploratory

development event was defined as scientific or engineering activity during a relatively brief period of time that included the conception of an idea and the initial demonstration of its feasibility. The scientists and engineers identified 686 such events and concluded that only 9% were the result of scientific research—0.3% to pure research and 8.7% to applied research. The other important events were all in technological development. Furthermore, 39% of the events happened in defence department laboratories, 49% in industrial laboratories, and only 9% in universities.

The conclusions were thus that basic scientific research was relatively unimportant and so were universities. The project gave strong support to the market push model of innovation. At the time the defence department was spending \$100 m a year on basic research, and yet Project Hindsight concluded:

"Science and technology funds deliberately invested and managed for defence purposes have been about one order of magnitude more efficient in producing useful events than the same amount of funds invested without specific concern for defence needs."⁶

These conclusions were very disturbing to pure scientists working in universities—not only because the defence department was a rich source of funds (and might now restrict them) but also because those funding research unrelated to defence might also be influenced by the results. Perhaps, for instance, important developments in health care were also not much to do with basic research. There were two immediate criticisms of Project Hindsight: firstly, it went back only 20 years and therefore was inevitably biased against basic research, which was likely to precede technological development; and, secondly, the systems to be investigated had been chosen by defence department staff, who were likely to be biased towards systems that they thought had been developed inhouse. Keilkamp has called the study inept and biased.⁷

The Traces study

The main response of basic academic scientists to Project Hindsight was to mount their own study. Soon after the publication of the interim report of Project Hindsight the National Science Foundation invited proposals for a "study to investigate the manner in which non-mission-related research has contributed over a number of years to practical innovations of economic or social importance." A team from the Illinois Institute of Technology was commissioned to do the study, and it chose to use a similar approach to Project Hindsight only to look at five major technological developments—magnetic ferrites, the video tape recorder, the contraceptive pill, the electron microscope, and matrix isolation.⁸ Importantly the Illinois team looked back 50 rather than 20 years and classified the events into non-mission research (or, to use alternative jargon, "curiosity driven"), mission oriented research, and development and application work.

The results were published in a report called *Technology in Retrospect and Critical Events in Science*, and so the study is widely known as the TRACES study. All the innovations were found to have their origins in basic research, and of the 340 events identified 70% were classified as non-mission research, 20% as mission oriented research, and 10% as development and application work. Furthermore, universities had been responsible for three quarters of the non-mission research and a third of the mission oriented research.

These results were thus almost the opposite of those from Project Hindsight, and the National Science Foundation got what it wanted. The science push model of innovation was supported. The difference in the results is explained almost entirely by the different time cut offs—because 30 years before the five innovations investigated by the Illinois team almost half of the non-mission research had been completed, and the number of non-mission research events peaked about 20 to 30 years before the innovation. Another source of the different results lay in the definition of what was basic research or non-mission: the government scientists conducting Project Hindsight might well see an event as mission oriented, whereas academic scientists might see the same event as curiosity driven. Layton has suggested that scholarly judgment on both

FIG 2—The research origins of the contraceptive pill (Illinois Institute of Technology, National Science Foundation).

Project Hindsight and TRACES should be suspended because "there is more than a suspicion that the interests of the sponsoring societies influenced the results."⁸

Irvine and Martin, when discussing the results of the TRACES study, suggest that some of the other conclusions drawn may ultimately be of more interest than those that seem to refute the results of Project Hindsight⁹:

"The diversity of knowledge, and therefore of research required to achieve innovation...[is] an important factor... Another important factor inherent in several of the tracings was that of communication between scientific disciplines and/or highly effective personal communication... [Consequently,] organisations which support and guide research must increase their emphasis on communication particularly among disciplines and between non-mission and mission oriented research... The continued involvement of a variety of institutions would appear to be a worthwhile objective to help meet the need for diversity of research."

Certainly the contraceptive pill resulted from the flowing together of work done on hormones, steroid chemistry, and the physiology of reproduction (fig 2).

Balancing the Illinois conclusions

The TRACES study was itself much criticised, mainly on the grounds that the five innovations chosen were far from representative. The National Science Foundation responded by commissioning a more sophisticated study from Batelle Laboratories.¹⁰ It looked at eight innovations, including the heart pacemaker and three from the Illinois study, and tried to pinpoint the few events that were decisive—in other words, the innovation would not have occurred without them. Of the decisive events 15% were classified as non-mission research, 45% as mission oriented research, and 39% as development and application work. These results were thus much closer to those of Project Hindsight, but when all events (not just decisive ones) were considered the results were in between those of Project Hindsight and those from the Illinois team: 34% were classified as non-mission research, 38% as mission oriented research, and 26% as development and application work.

The Batelle study also tried to identify factors that were important in influencing the rate and direction of innovation. In 87% of decisive events recognition of a technical opportunity was important (supporting science push), while in 69% recognition of a need was important (supporting market pull). Also important in 56% of decisive events was the existence of a "technical entrepreneur," which led *Science* to comment that "If any suggestion were to be made as to what should be done to promote innovation, it would be to find—if one can—technical entrepreneurs."¹¹ Batelle needed Dhiagalev, and the Medical Research Council would undoubtedly benefit from a research Dhiagalev. One of the previous secretaries, Sir Harold Himsworth, might have fitted such a description, but there hasn't been one since.

From research to profit

Other British studies of how innovations come about have been done and have concentrated on innovations that have been commercially successful, reflecting perhaps anxieties in Britain about its economic future and about its inability to make money out of its many basic science discoveries.¹²⁻¹⁴ These studies have tended to show that the contribution of basic science and of universities is small, but the findings have been disputed.¹⁵ Gibbons and Johnston, for instance, looked at the sources of information needed to solve the technical problems that arose in producing 30 British innovations drawn from pages of trade journals.¹⁶ Of the 887 sources identified only 107 were classified as "scientific" and academic scientists or academic journals accounted for only 60 of these inputs.

The conclusion of the study by Gibbons and Johnston could go for most of these studies, both British and American:

"... it is apparent that the relationship between science and industrial

technology is more complex than previously assumed by either scientists or economists; there exists a wide variety of potential forms of interaction. While this settles the issue of whether science contributes to technological innovation, and provides a justification for maintaining an effective research capability, the very complexity of the relationship precludes simple calculations of the optimum size or distribution of the science budget."

Conclusions and implications

The sources of innovation are thus numerous, varied, and scattered, and the relation between them is complex. Both the "science push" and the "market pull" models of innovation are far too simple, but both help to explain a small part of eventual innovation. The complexity and the varied sources of innovation mean that governments and organisations that fund research must put their eggs into many baskets. Attempts to force more "relevant" research may backfire.

Another general lesson that has been drawn out of these studies by Irvine and Martin, who are primarily interested in predicting innovation and using the information to decide where to devote resources, is that synthesis is important—the coming together of different lines of research and of scientists from different disciplines seems to be important in seeding innovations.⁷ The authors of the Battelle study concluded:

"The occurrence of an unplanned confluence of technology was characteristic of six of the innovations. Interestingly enough, confluence of technology occurred for the four other innovations as well, but as a result of deliberate planning... [because] confluence of technology... is essential to innovation... it presents an opportunity for management, by promoting interdisciplinary R and D teams, to accelerate the innovative process."¹⁰

It is this kind of thinking that has led the Advisory Board for the Research Councils to advocate interdisciplinary research centres,¹⁷ although the policy seems to be based less on evidence and more on intuition (or even fashion). The Medical Research Council is also following this thinking: the Clinical Research Centre was started with the idea that clinicians, clinical scientists, and basic scientists could work together, and now the MRC wants to bring in the Royal Postgraduate Medical School—and ultimately the National Institute for Medical Research at Mill Hill. The National Science Foundation in the United States is also pouring billions of dollars into centres for science and technology.

We must hope that these policies of concentration and of bringing together different disciplines will prove fruitful. Hard evidence that the policies will work is lacking.

The next article will consider the one substantial study that has looked specifically at the sources of medical discoveries.

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