December 2010

Office of Health Economics

seminar briefing no7

SCIENCE POLICY RESEARCH – HAVING AN IMPACT ON POLICY?

Professor Ben R. Martin, SPRU – Science and Technology Policy Research, University of Sussex

CONTENTS

The seminar led by Professor Martin on which this briefing is based took place on 6th September 2010 at the Office of Health Economics, 12 Whitehall, London SW1A 2DY

Introduction

My subject is the interaction between policy research and policy making. I come specifically from the area of science policy research, but I hope that some of what I have to say will be of relevance to policy research more generally, including medical sciences and health policy research.

The field of science policy research is about 50 years old and I have identified about 20 major advances in understanding during that period. I will go through some of these rather quickly, others in a bit more detail, and then ask which of them have actually had an impact on policy making. The answer is a distressingly small number, which then raises the question: what are the factors affecting the impact of policy research on policy making? To look at that, I will take you through four or five case studies based on areas I have been personally involved in over the last 30 years, and from that I try to put together the elements of a very preliminary model of the interaction between policy research and policy making. Lastly, I will offer some comments about where we perhaps need to go next in our field.

The scope of "science policy research"

The field I am discussing comprises economic, policy, management and organisational studies of science, technology and innovation, with a view to providing useful inputs for decision makers concerned with policies for, and the management of, science, technology and innovation. So it applies to companies spending money on R&D as much as to government policy makers.

The primary focus is on specific policy and management issues rather than theory. We are driven by problems, not by theory, although perhaps our field is changing with respect to this as it becomes more mature.

Science policy research began in the late 1950s, when there were perhaps half a dozen people in the entire world interested in what we would now call "science policy and innovation studies." Now there are probably several thousand researchers around the world.

The terminology for the field has changed over time. When it got going in the 1960s, it was generally called "science policy research" or "research policy" or even "R&D management". In the 1970s and 1980s technology was often inserted into the terminology. Now we more often talk about innovation studies or even evolutionary economics. My preferred terminology is "science policy and innovation studies".

This field is intrinsically multi- or inter-disciplinary and draws on a range of social science disciplines: economics, management, organisational studies, sociology, history, geography and psychology.

Why is it important? Science, technology and innovation are a major source of progress, although there are sometimes adverse consequences. They comprise a major contributor to prosperity, delivering not only more goods and services but also new ones, enabling us to do things we have never done before and changing the quality of life and the environment, generally for the better, though sometimes for worse. As we have faced globalisation and growing competition over the last 20 years, so the premium on knowledge, technology and innovation has grown. These are expensive to pursue and they can bring risks or social costs, so it is important that we have effective policies for them.

Twenty major advances in understanding

How did I set about assessing the impact of science policy and innovation studies? There have been numerous previous reviews in books and articles, but most were based on a subjective assessment by the author and have been rather more limited in scope, for example, just looking at technology management or some other sub-component of the broader area I am interested in. I have tried to adopt a rather more rigorous approach to identifying the main contributions covering the entire field of science policy and innovation studies, and I have ended up with a list of 20.

To do that, I searched for high impact publications. Ideally I would have liked to have had some measure of impact on decision makers, but there is none. So instead I started with highly cited papers, i.e. papers highly cited by other academics. The assumption here is that the most highly cited works are generally the most influential. I began with a list of leading authors in the area, surveyed the 80 journals where most of the work gets published using a key word search, and out of that identified about 200 publications with at least 250 citations each (Martin, 2008). Having got that list I assessed which ones I felt had had most impact on policy or management practice. From those, I synthesised 20 major advances, as follows (see Box 1).

Box 1: Twenty advances in science policy

1. From individual entrepreneur to corporate innovators
2. From laissez faire to government intervention
3. From two factors of production to three
4. From single division to multidivisional effects
5. From technology adoption to innovation diffusion
6. From science push to demand pull?
7. From single factor to multi-factor explanations of innovation
8. From a static to a dynamic model of innovation
9. From the linear model to an interactive 'chain-link' model
10. From one innovation process to several sector-specific types
11. From neoclassical to evolutionary economics
12. From neoclassical to new growth theory
13. From the optimising firm to the resource-based view of the firm
14. From individual actors to systems of innovation
15. From market failure to system failure
16. From one to two 'faces' of R&D
17. From 'Mode 1' to 'Mode 2'
18. From single technology to multi-technology firms
19. From national to multi-level systems of innovation
20. From closed to open innovation

1. From individual entrepreneur to corporate innovators

The first of the 20 advances actually pre-dates our field of science policy and innovation studies. Schumpeter was one of the few economists in the first half of the twentieth century to be interested in innovation and technology. He distinguished between invention and innovation; the latter being the first successful introduction into the market or into widespread social use of an invention. He also pointed out that over time we seemed to be moving from individual entrepreneurs as we had often had in the 19th century, people like Brunel, to collective innovative activities in large firms and elsewhere, as we saw in the mid-20th century (Schumpeter, 1934, 1939 and 1942).

2. From laissez faire to government intervention

Prior to 1939, government was not much involved in research and development (R&D), with the exception of agriculture and health, but all that changed as a result of the Second World War and its direct support of research on weapons (including the Manhattan Project), radar, cryptography and so on. After the War there were major government R&D programmes in nuclear energy, space, health and so on; all of them based on belief in a linear model of innovation, which Vannevar Bush

(1945), the US presidential science advisor, had put forward in 1945. In this model the start point is basic research, which generates applied research and then technical development, and then eventually out the other end of the chain comes innovation. It had the merit of being a simple clear model and also a very politically convenient one for scientists. The implication for government is: put money into the beginning of the process and out the other end of the chain eventually will come contributions to wealth, health and national security.

We had various policies in the 1950s and 1960s based on this linear model. The economists, in particular Richard Nelson (1959) and Ken Arrow (1962), came up with the so called 'market failure rationale' for why governments should get involved in science, technology and innovation. The nature of science and technology is such that firms will tend to underinvest because the knowledge they produce would have the character of a 'public good', i.e. it is 'non-rival' (my use of the knowledge does not diminish your ability to use it) and 'non-excludable' (once the knowledge exists, anyone can acquire it and hence make use of it). Thus investors in R&D cannot appropriate all of the benefits that result from that investment and hence will underinvest. Therefore governments need to come in to expand the pool of useful knowledge to the socially optimal level.

3. From two factors of production to three

Prior to 1957 economists tried to explain economic growth in terms of two factors of production: labour and capital. Then in 1957 Solow highlighted a large residual growth that cannot be explained by growth just in those two factors, and he attributed this to technical change (Solow, 1957). Many other authors subsequently expanded on and developed this insight.

4. From single division to multidivisional effects

"The Management of Innovation" by Burns and Stalker (1961) was one of the first books with that sort of title. Burns and Stalker emphasised that technical innovation is influenced by the form of the organisation and communication patterns within the company. They identified the key requirement for successful innovation as being the integration of R&D with other things, in particular knowledge of the market, something which is often hindered in companies by internal organisational divisions.

5. From technology adoption to innovation diffusion

Adoption of technology is not just a single point event but a gradual process of diffusion. Rogers has written the most highly cited book in the field: "Diffusion of Innovations" – originally published in 1962 and with four subsequent editions and a total of 15,000-20,000 citations, a huge number. In it, Rogers pointed out that often the diffusion of an innovation follows an S-shaped curve: initially slow diffusion, then rapid growth leading to eventual saturation and slowdown. You also have different categories of innovators: the early adopters, the majority and then the laggards (Rogers, 1962).

6. From science push to demand pull?

The science push model (Bush, 1945) held sway for 20 or so years after the Second World War, but then some economists, including Schmookler (1966), came along and said: "No, it's not like that at all. It all starts with the market and changing market demand for innovation." So things start in this model with changing market demand, which then calls forth applied research and technical development and hence innovation.

Because this demand pull model had very different policy implications than the science push model, a number of empirical studies were then carried out in the 1960s and 1970s to try to work out which was more accurate. Project Hindsight by the US Department of Defense claimed to find that market demand was the key thing, and the US National Science Foundation immediately responded with Project TRACES, which found the opposite: claiming that science push was all important. The battle went on for about 10 years and was finally settled by Mowery and Rosenberg (1979) with their review, which showed that generally both demand and supply sides are crucial.

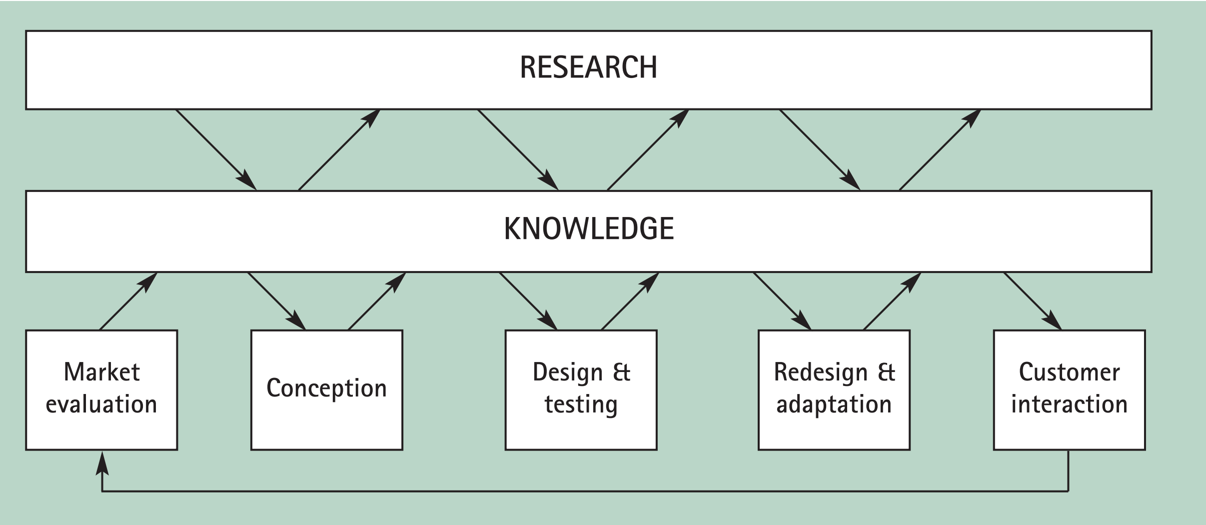
7. From single factor to multi-factor explanations of innovation

Early studies of innovation had just looked at successful innovations. Project SAPPHO looked at 43 matched pairs of successful and unsuccessful innovations and tried to identify the factors that account for success and failure (Rothwell et al., 1974). The project revealed that a variety of factors play a part: understanding user needs, attention to marketing, having the support of a senior product champion (I will come back to that bit later on), the size of the project team, the co-ordination of R&D with production and marketing, and good communication with the extended scientific community. Equally, the study found that success was not greatly affected by how R&D was organised, the academic qualifications of the staff, or the size or rate of growth of the firm.

8. From a static to a dynamic model of innovation

Utterback and Abernathy (1975) introduced a dynamic model of innovation, in which an initial radical product innovation attracts new entrant firms and gives rise to several competing designs. Eventually one design becomes dominant and then process innovations become more important. There are many historical examples of this: typewriters and the QWERTY keyboard, the Model T Ford cars, the Hoover, Boeing 747 jet airliners, IBM personal computers and so on.

Figure 1: The interactive 'chain-link' model



Adapted from Kline and Rosenberg (1986)

9. From the linear model to an interactive 'chain-link' model

Neither the science-push nor the demand-pull models in themselves were sufficient, so in 1986 Kline and Rosenberg put forward the interactive chain-link model, illustrated in Figure 1. This is a much better representation of a rather complex reality, but has the problem that if you start trying to explain this to policy makers their eyes start glazing over!

10. From one innovation process to several sector-specific types

By the 1980s we had dozens, if not hundreds, of innovation studies, all coming up with their own models of the innovation process. Keith Pavitt (1984) showed that if you classify firms into a number of sectors, then you could begin to make sense of the rather baffling picture that was beginning to emerge. Pavitt defined four different sectors, each with a different innovation process:

- Supplier-dominated
- Scale-intensive
- Specialised equipment suppliers
- Science based

11. From neoclassical to evolutionary economics

Nelson and Winter in the 1970s realised that mainstream neoclassical economics was pretty poor at explaining technological progress and innovation. So they went looking for a useful theory of innovation (Nelson and Winter, 1977) and in 1982 came up with the evolutionary theory of economic change (Nelson and Winter, 1982). There are a number of key components to it. First of all, technological change and innovation were central, generating variation in the form of new products and services. Firms then compete with these products or services, with the market providing a selection mechanism. The third part of their argument was that those new products and services are strongly influenced by routines within the companies which helped them produce successful innovations. So these provide a self-replication mechanism. If you take those three things – variation, selection and self-replication – you have an analogy with biological evolution, which was at the heart of this evolutionary theory of economic change. Nelson and Winter (1982) is one of the most highly cited works in the whole of social science. The interesting thing is that it is heavily cited by virtually all social scientists apart from economists, who do not like it!

12. From neoclassical to new growth theory

Neoclassical economists then responded to evolutionary theory. In particular Romer (1990) came up with 'new growth theory'. This provided an explanation of growth in terms of human capital, education, R&D and spillovers from it, and incentives to innovate, particularly patents. Investment in 'intangibles' was emphasised in addition to the previous neoclassical economics focus on 'tangibles' such as capital goods.

13. From the optimising firm to the resource-based view of the firm

Neoclassical economists presented the firm as an optimising organisation with perfect information and perfect rationality. But in the last 25 or so years this has gradually been replaced by the resource-based view of the firm; the firm being seen as a collection of resources such as brand names, technical knowledge, equipment, skilled personnel, trade contacts, efficient procedures and capital (Wernerfelt, 1984; Grant, 1991 and 1996). Subsequent work has developed the notions of core competences (Prahalad and Hamel, 1990), dynamic capabilities (Teece et al., 1997; Eisenhardt and Martin, 2000) and the capacity of organisations to learn (Senge, 1990).

14. From individual actors to systems of innovation

This is probably one of the most important concepts to come out of the field of science policy and innovation studies in the last 25 years. Chris Freeman, the founder of SPRU, was analysing the success of Japan in the 1980s and concluded that its success in hi-tech industry was heavily dependent on what he called the "wider national system of innovation" (Freeman, 1987). That idea was taken up by others such as Lundvall (1992) and Nelson (1993) and extended to other countries. The key point is that how effectively a national system of innovation operates depends not just on the strength of the individual actors – firms, Governments, labs, universities – but also on the strength of the links between them. You can imagine two scenarios. In Scenario A you have a system with strong actors and weak links, and in Scenario B a system with not particularly strong actors but strong links between them, and the second may work more effectively than the first. In crude terms, in the 1980s, Scenario A was Britain and Scenario B was Japan. Things have changed since then.

15. From market failure to system failure

The new focus on systems of innovation led in turn to the notion of system failure: that the rationale for government intervention was not just to fix market failures but also to address systems failures where there was a lack of links between actors or the links that did exist were not sufficiently strong.[1] Thus government policy could be aimed at building up those links to encourage networks of collaboration, alliances and indeed Technology Foresight, which can in part be a mechanism for 'wiring up' national systems of innovation by getting the players to talk to each other more than they had done in the past.[2]

16. From one to two 'faces' of R&D

Why do companies do R&D? The conventional reason is to generate new knowledge within the company and hence new products. Cohen and Levinthal (1989 and 1990) identified a second reason: companies also do R&D to identify potentially useful research being done elsewhere around the world that might be relevant to them, so that they are then in a better position to absorb it and to then quickly and efficiently turn it into innovations. Thus they came up with a notion of 'absorptive capacity' to exploit spillovers from the research of others.

17. From 'Mode 1' to 'Mode 2'

Michael Gibbons, a past Director of SPRU, wrote with a number of co-authors: "The New Production of Knowledge" (1994), which argued there had been a shift from 'Mode 1' to 'Mode 2' research. 'Mode 1' research was discipline-based and largely done in universities, primarily for the purpose of furthering knowledge, and largely subject to internal, not external, scrutiny. 'Mode 2' research is trans-disciplinary, carried out across a variety of institutions, pursuing knowledge "in the context of application" – which is the key phrase in relation to Mode 2 – and subject to external accountability.

18. From single technology to multi-technology firms

Many major innovations involve 'technology fusion', i.e. bringing together previously separate streams of technology. Granstrand and colleagues (1997) showed that technological diversity was of growing importance to innovation.

19. From national to multi-level systems of innovation

I have already mentioned national systems of innovation. A variety of authors have extended this concept to other dimensions, in particular regional, sectoral and technological systems of innovation. The notion of the regional system of innovation builds on work on the 'spillovers' from R&D (Jaffe et al., 1993) and other regional advantages (Saxenian, 1994). Also important here are cultural factors; Florida (2002) has done a lot of well-known work on this, linking innovation to cities that have a more Bohemian lifestyle.

20. From closed to open innovation

The knowledge required for innovation is arguably becoming more organisationally dispersed and increasingly being coproduced with partners. This puts greater emphasis on the desirability of openness to external agents (e.g. Chesbrough, 2003; von Hippel, 2005).

Those are the most important 20 advances I have identified. But which of them have had an impact on the management or policies for science, technology and innovation?

There are several which I could probably make a convincing case have had a significant impact on technology and innovation management in industry, namely:

1. From individual entrepreneur to corporate innovators
3. From two factors of production to three

[1] There is no single very highly cited publication on this, but the notion of systemic failure can be found, for example, in Smith (2000), who distinguishes four different types.

[2] See Martin and Johnston (1999).

4. From single division to multi-divisional efforts
7. From a single factor to multi-factor explanations of innovation
13. From the optimising firm to the resource-based view of the firm
16. From one to two 'faces' of R&D
18. From single technology to multi-technology firms
20. From closed to open innovation

Which of the 20 have had a major impact on science, technology and innovation policy? Here the list is rather shorter:

2. From laissez faire to government intervention
3. From two factors of production to three
14. From individual actors to systems of innovation
15. From market failure to system failure

A fifth advance from my list of 20 looked as though it was about to take off in the UK when Gordon Brown, then the Chancellor of the Exchequer, started talking about post-neoclassical indigenous growth theory (advance 12), but whether there was any subsequent impact on UK policy is doubtful.

Factors affecting impact on policy

This is a rather distressingly short list of advances having a significant impact. So, what are the factors that have affected impact on policy? Ideally I should do some rigorous research on this but I have not had time to do that, so instead I have taken five examples of areas in which I have been involved over the last 30 years to try and learn some lessons about what makes for successful impact on policy making.

1. Research assessment

The first example is the work I was hired to do at SPRU back in 1978; the so called "Big Science" project. At that stage the Science Research Council was the largest research council, and two thirds of its budget went on just six 'big science' labs. We wanted to find out what were the benefits to the UK from this heavy investment and set about devising a methodology for assessing the scientific, technological and educational benefits.

The assessment of scientific benefits was based on a combination of bibliometric indicators and extensive peer review (Martin and Irvine, 1983). We showed that peer review, which up to then had been the main method for making funding allocation decisions, broke down in the case of 'big science' where there are no neutral peers. To take one example: Cambridge University and Jodrell Bank were the only two groups doing radio-astronomy. So if one came up with a project, the other one was going to be asked to assess it. Cambridge and Jodrell Bank realised it made sense to join forces so that each time they supported the other's proposals. That arrangement only broke down when Jodrell Bank asked for a big new Mark 5 telescope, which was more than the Treasury could afford.

Our study received a rather hostile reaction from scientists. We social scientists were seen as threatening their autonomy and the monopoly of peer review. Directors of two of the big science labs – I won't say which – got rather annoyed. One of them threatened us with libel for saying that his lab had not done very well. We got a sceptical, rather than hostile, reaction from most policy-makers.

What lessons did we draw from this experience? At that stage, around 1980, the demand pull from policy makers was evidently not very great for this sort of evaluation. Also, innovative policy research is likely to provoke some opposition because you are seen as criticising decisions that have been made by senior people. We learned that policy research needs to find a 'product champion' – we were fortunate that Brian Oakley, then Secretary of the Science Research Council, was quite open to our work and encouraged us. Lastly, we learned the need for perseverance: you need to keep 'marketing' your results; you cannot just sit there saying you have invented a better mouse-trap, you actually have to go out and sell it, for years, if you want to have an impact.

2. Science indicators

The second example of factors affecting impact on policy, concerns science indicators. Prior to the 1980s there had been a lot of anecdotal evidence and claims that British science was in decline, from British scientists wanting more money to offset this decline, but little hard evidence. I and my colleagues at SPRU came along in 1985 and did a bibliometric study which showed that Britain's share of publications and citations was indeed declining (Irvine et al., 1985). This time we had a very significant impact straightaway. The House of Lords picked it up; the BBC covered it in one of their 'Horizon' television programmes; and this time we were welcomed by scientists. We were giving a message that they actually liked, so here there was much stronger demand for our study's findings, and that was one reason why it had a quick impact.

Even so, it took several subsequent studies to diffuse the message more widely and it only finally took off in 1995. That year we invited Bob (later Lord) May, the new UK Government Chief Scientist, to SPRU for an informal off-the-record meeting. He quite liked all the indicators and he went away and spread word of what he had learnt at SPRU. In 1997 he wrote a paper in the journal Science called "The Scientific Wealth of Nations", which showed that Britain came in at number two behind the US in terms of publications and citations, well ahead of France and Germany. He went on to say that this was evidence surely that Britain was pursuing the right policies, like getting most of its basic research done in universities rather than in independent institutes as they did in France and Germany. But this conclusion ignored all the caveats in our earlier reports about the problems with publication and citation data, including the English-language bias in the bibliometric databases used in such studies.

3. International comparisons of government funding

Comparison of government funding was the third major area of work. In the mid-1980s the Advisory Board for the Research Councils (ABRC) sought better data on government funding of academic and related research to see where Britain stood compared with its nearest competitors. We actually knew the answer before we started: we spent less. But we had to do better than the existing OECD data, which we quickly found beneath the surface suffered from a number of quite major problems. We set about producing better data. Along the way we were subjected to very significant political pressures. When we came up with different data from France, I was summoned into a room with a senior French official and told: "You cannot use those data; they are not the official ones". We nevertheless stuck by our independent data.

We produced our answer in 1986: that Britain was spending significantly less on scientific research as a proportion of GDP or per capita than France, Germany, the Netherlands and the US, among other countries (Irvine and Martin, 1986). Initially we thought we had had little impact but then in 1987 we were asked to update and extend the study to eight countries and to adopt a more rigorous methodology. Before we had completed this second study, my colleague John Irvine was called in to meet the minister responsible for science, Robert Jackson. A few weeks earlier, the minister had been asked to present to the Cabinet some information on public funding of the science base, or the 'science vote' as it is called. Robert Jackson had shown the Cabinet the final table from our 1986 report. As a result, Mrs Thatcher, the Prime Minister, had authorised an extra £100 million to be added to the 'science vote'. John Irvine and I felt very pleased to have had this impact!

4. Technology 'Foresight'

In 1983, the UK Cabinet Office and the Advisory Council on Applied Research and Development (ACARD) commissioned SPRU to carry out a study of how other countries identify exploitable areas of science. We needed to come up with a title. As I noted earlier, an earlier project called "Hindsight" had taken prominent innovations and looked back to see what advances in science and technology had made them possible. What we were being asked to do in this study was the opposite: to look forward from current science and technology to try and think what innovations they might lead to in 10 or 20 years' time. So somewhat whimsically we decided to call our study "Project Foresight" (Martin, 2010).

We focused on four countries: France, Germany, the US and Japan; and we concluded that Britain should learn from these other companies, particularly Japan, and that we should perhaps launch a small pilot foresight exercise. We transformed our report into a book called "Foresight in Science" but we were not quite sure that people would know what foresight in science was all about so we had to give it a subtitle. We called the book "Foresight in Science: Picking the Winners" (Irvine and Martin, 1984). This turned out to be precisely the wrong subtitle to use! The Prime Minister, Mrs Thatcher, apparently took a strong view that it was not the job of government to pick winners; that was the job of the market. Also there was a general view that, if a policy worked in Japan, then it would not work in the UK because the two countries were so different. That report, had virtually no impact on the UK Government. The political circumstances were not right. The Government was trying to reduce the role of the public sector and here we were suggesting a new role for it, so there was no policy demand whatsoever.

We subsequently, in 1987-1989, did a similar study for the Dutch government which had a major impact. There was a lot of demand there. They set up not one but two foresight programmes, in rival ministries competing with each other.

In 1992 I received an unexpected telephone call from the Cabinet Office expressing interest in having a study done on foresight, and asking whether SPRU would be interested in doing it. It was a very small project. We could only look at a couple of countries, so this time rather than focusing on Japan, where there was still most to learn from, I proposed that the focus be on Germany and the United States – i.e. two countries with right-of-centre governments, like the UK, and a decentralised approach, unlike in Japan. I submitted my report setting out a range of options, and was then invited to brief the Cabinet Minister, William Waldegrave. With a lot of effort I summarised the 60-page report into a two-page brief and went along and presented that. The Minister liked the idea of technology foresight so much that a few months later he gave pride of place in his White Paper (OST, 1993) to the establishment of a UK Technology Foresight programme following the 'big bang' Japanese approach, which I had described at the end of my report merely for the sake of completeness, never thinking that it would be the favoured choice.

The establishment of Foresight did not mean that government alone should identify priorities, but that government in co-operation with industry and academia and others has a role in determining national level policies and priorities. I do not think we can just rely on bottom-up initiatives to steer science research, whether it's from academics or from industry. There are certain issues which are just not amenable to that sort of market type of approach – global climate change may well be one of them, health another and agriculture a third – where government needs somehow or other to arrive at priorities, and that is what this first version of the Foresight programme was intended to do. Indeed, I think that it did that quite well. Given that this was new to the UK and was very ambitious, the Foresight exercise from 1993 to 1995 did, I believe, achieve a number of the goals it set out and ended up with priorities, some of which were unsurprising, others of which were quite new. One of the latter was the identification of a growing problem with electronic fraud, security, terrorism, etc., with the realisation that this was also an area where Britain had some comparative advantages: the English language, software, the defence sector and so on.

The UK did start a second Foresight Programme in 1999 and it rather ran into the sands in 2000; it was never properly completed. The 15 or 16 panels all produced reports, but there was no overall outcome. The new Chief Scientist David King replaced it with a micro-level form of Foresight focusing on chosen areas, which has a number of advantages. It is the model that the Australians and the Dutch had followed in earlier years with some success, but it does not give you the panoramic overview of the whole of science and technology and hence a chance to identify a number of priorities for the nation as a whole.

However, I think the first Foresight Programme in 1993-1995 **was** quite successful, and did have a significant impact. There is a need today for an overview type of Foresight to give us this handle on priorities, particularly given the current, very tight, funding situation.

The lessons from the "foresight" experience are to:

- be attuned to the political context;
- go in at the right level. Talk to the middle-ranking civil servants who actually write the briefs for the ministers and senior civil servants;
- find a product champion for your policy research;
- prepare, so as to be able to capitalise on opportunities to influence that arise;
- be prepared to take on other roles. In the case of Foresight, I switched from being a researcher to a practitioner and even on occasions a 'missionary'! In the pre-Foresight phase, we had "Focus on Foresight" seminars around the country, where we met with local industry and local government and told them about Foresight and why it was important and worth them giving up some of their time to take part.

5. Estimating the benefits of publicly funded research

In 1996 the Treasury, which for many years had been rather sceptical about the benefits coming from publicly funded research, invited SPRU to tender for a study, though the available funding for the study was considerably less than the actual cost of doing the research.

We then had a dilemma. We could give the Treasury a relatively easy answer based on estimates of the rate of return. This is what Edward Mansfield, a prominent US economist, had done. He had contacted the managers of R&D in large companies and asked them to identify what proportion of their output was accounted for by new products and processes, and for those how much depended on the last 10 years' of research in universities and publicly funded research labs. Out of all that, and with a lot of heroic assumptions, he estimated a rate of return of 28 per cent (Mansfield, 1991); a very impressive rate, but perhaps implausibly impressive. Nevertheless, Mansfield's findings were politically very convenient. They had a lot of mileage in Washington. This partly explains why during the 1990s the US Congress and the President were very generous towards research.

But we considered Mansfield's work methodologically somewhat dubious and too simplistic. So my colleagues and I went for the more complicated message that there are at least six different channels through which benefits flow from publicly funded research to the economy and society, all leading to substantial benefits, but that in many cases they cannot be easily quantified. Consequently, we concluded that, while scientific research was indeed very beneficial, the Treasury could not be presented with a simple rate-of-return figure. By then, William Waldegrave had become Chief Secretary to the Treasury, and we like to think that our findings (Martin et al., 1996) had some influence on subsequent government comprehensive spending reviews, in which scientific research did quite well in terms of increased funding.

The lessons I take from this example are:

- To be opportunistic and take one's chances to exert an influence, even if one has to do the work as a "loss leader";
- To strike a balance between simplicity and complexity with regard to the message one tries to convey.

We also learned that government has a short collective memory. Two or three years after this small project for the Treasury, we received another invitation, from a different Government department, to answer the question: "What are the benefits from publicly funded research?" The people at the Treasury we had spent several months educating about all of this had long since moved on. Over the next six or eight years SPRU has updated this study three or four times.

Towards a model of the interaction between science policy research and policy making

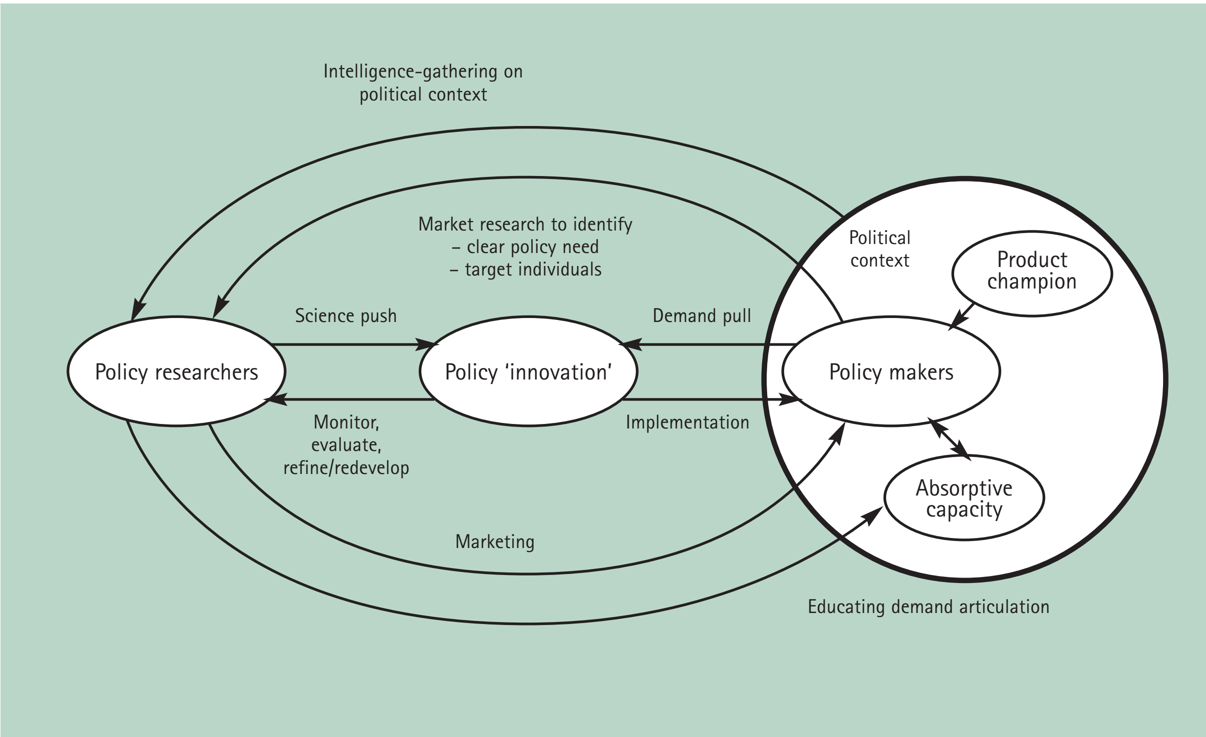
Can we draw some conclusions about the model of interaction between policy research and policy making? As we have seen, there have been a significant number of major advances from science policy and innovation studies over the last 50 years. Some have had an impact on policy, others have not. Why is this? It is because this is not just a simple linear, science push process. This should come as no surprise. The science policy and innovation studies field has spent the last 30 years showing that the science push model does not hold for innovation more generally. Neither does it hold for science policy 'innovations'.

Where do we find people still believing in the science push model? It is we, policy researchers, thinking we have good ideas and that if only there were people in governments who would lap them up, then all the problems of the world would be solved! But the world is not like that. You need to have a significant demand pull, which in turn means doing a certain amount of market research to find out what the demands actually are and helping policy makers to articulate that demand. It will certainly involve quite a bit of 'marketing', which many British academics feel uncomfortable with. It means finding a 'product champion'; you cannot do it all on your own. It means trying to identify organisations where you might find 'absorptive capacity': people who know enough about what you are trying to say to make sense of it. There are analogies here you can see with the model we now have of the innovation process more broadly.

There are other things that researchers need in order to have impact. They must identify clear policy needs and understand the wider political context. If you get that wrong, even if you have a good message to deliver, it will not be picked up. You need to go into government at the right level, which is not necessarily at the top. You need to seize opportunities and to deliver on time. You might like to spend two years doing a project properly but if government wants an answer in six months, you have to do it in six months and cut corners accordingly.

Figure 2 illustrates schematically my first attempt at a model to capture all of this. At the heart of it is the mixture of science push and demand pull. As you try to produce some policy 'innovation', you have also got to be doing a number of other things: market research to identify clear policy needs, which are evolving over time, and to identify which individuals amongst the policy makers you might work with; intelligence gathering on the wider political context is necessary if you want to make sure the message you come up with is likely to be accepted; marketing and demand articulation; and monitoring, evaluating, refining and redeveloping your work as this will not be a one-off event, you will have to continuously refine what you are trying to do. The key role for a 'product champion' for your research is included, and so too is the need for a level of 'absorptive capacity' among policy makers in order to make sense of your message.

Figure 2: A model of the interaction between policy research and policy making



To achieve all of this will necessitate deployment by the researcher of three important skills:

- To compromise between engagement with policy makers' wishes and pursuing academic objectives;
- To simplify to achieve a balance between accessibility and sophistication. You cannot give senior policy makers too complicated a model because they are busy people and will not have time to engage with it. On the other hand, you must not give them too simplistic a model;
- To persevere as achieving impact is more often a question of knowledge gradually 'creeping' (Weiss, 1980) and taking hold rather than of achieving an immediate 'direct hit'.

A vital dimension that I was not able to capture in this simple diagram is time. Just as in trying to look at the benefits from scientific research for innovation, you need to adopt a time scale of 10 or 20 years to study the impact of policy research. If you just focus on the short-term and then draw lessons only from that, I think you will end up with very misguided policies. That is part of my concern about research more generally in the UK at present. Under pressure from the Treasury and Government and others for answers to such questions as: "What are the benefits?"; "Can we have accountability?"; "Can we be assured of value for money?"; we are being driven increasingly down a rather short-term and tactical route, away from the more important research which can take 10 to 20 years to materialise. Our first Foresight study, as noted earlier, had no impact, at least in the UK, for nine years, and it was only at the second attempt that it finally had an effect. If you had assessed it after four or five years, you would have seen almost zero impact. It is vital to adopt a long timescale.

Where next?

Is the science policy and innovation studies community keeping up with our changing world, or are we, like generals, always 'fighting the last war' or like politicians in the thrall to the ideas of some long dead economist (to paraphrase Keynes)?

I edit "Research Policy", which is arguably the leading journal in the science policy and innovation studies field. So I went through the empirical studies among the articles published in it over the last 10 years or so and found that a large proportion of them focus on manufacturing and particularly on hi-tech manufacturing. But manufacturing in the UK is now only about 15 per cent of GDP; in other words, there is 85 per cent of other stuff going on, which is largely ignored by my peers, my colleagues and myself. Also, most of the studies still focus on innovation for wealth creation, competitiveness and productivity. There is distressingly little on sustainability and global change. Most of it is also focused on wealth creation rather than on enhancing well-being or our quality of life.

My answer to the question "where next?" is, therefore, to strike out from the well-researched areas and to study innovation in other areas – such as financial services where there have been lots of innovations in the last 10 years, some of them with rather disastrous results – rather than just looking at IT, mobile phones, cars and so on, which is where we have focused until now.

References

KJ Arrow (1962), 'Economic welfare and the allocation of resources for invention', in R Nelson (ed.), The Rate and Direction of Inventive Activities (Princeton University Press, Princeton), pp. 609–625.

T Burns, GM Stalker (1961), The Management of Innovation (Tavistock, London).

V Bush (1945), Science the Endless Frontier (US Government Printing Office, Washington).

HW Chesbrough (2003), Open Innovation (Harvard Business School Press, Boston, Mass.).

WM Cohen, DA Levinthal (1989), 'Innovation and learning: the two faces of R&D', Economic Journal 99, 569-596.

WM Cohen, DA Levinthal (1990), 'Absorptive capacity: a new perspective on learning and innovation', Administrative Science Quarterly 35, 128-152.

KM Eisenhardt, JA Martin (2000), 'Dynamic capabilities: what are they?', Strategic Management Journal 21, 1105-1121.

R Florida (2002), The Rise of the Creative Class (Basic Books, New York).

C Freeman (1987), Technology Policy and Economic Performance: Lessons from Japan (Pinter Publishers, London).

M Gibbons, C Limoges, H Nowotny, S Schwartzman, P Scott, M Trow (1994), The New Production of Knowledge (Sage, London).

O Granstrand, P Patel, K Pavitt (1997), 'Multi-technology corporations: why they have "distributed" rather than "distinctive core" competencies', California Management Review 39, 8-25.

RM Grant (1991), 'The resource-based theory of competitive advantage', California Management Review 33, 114-135.

RM Grant (1996), 'Toward a knowledge-based theory of the firm', Strategic Management Journal 17, 109-122.

J Irvine, BR Martin (1984), Foresight in Science: Picking the Winners (Pinter Publishers, London).

J Irvine, BR Martin (1986), 'Is Britain spending enough on science?', Nature, 323, 591-594.

J Irvine, BR Martin, T Peacock, R Turner (1985), 'Charting the decline in British science', Nature, 316, 587-590.

AB Jaffe, M Trajtenberg, R Henderson (1993), 'Geographic localization of knowledge spillovers as evidenced by patent citations', Quarterly Journal of Economics, 108[3], 577-598.

SJ Kline, N Rosenberg (1986), 'An overview of innovation', in R Landau & N Rosenberg (eds) The Positive Sum Strategy: Harnessing Technology for Economic Growth (National Academy of Sciences, Washington DC), pp.275-305.

BÅ Lundvall (ed.) (1992), National Systems of Innovation: Towards a Theory of Innovation and Interactive Learning (Pinter, London).

E Mansfield (1991), 'Academic research and industrial innovation', Research Policy 20, 1–12.

BR Martin (2008), 'The evolution of science policy and innovation studies', TIK Working Paper Series on Innovation Studies No. 20080828, University of Oslo.

BR Martin (2010), 'Foresight in Science and Technology: Some Terminological Reflections', Technological Forecasting and Social Change 77, 1438-1447.

BR Martin, J Irvine (1983), 'Assessing basic research: some partial indicators of scientific progress in radio astronomy', Research Policy, 12, 61-90.

BR Martin, R Johnston (1999), 'Technology foresight for wiring up the national innovation system: experiences in Britain, Australia and New Zealand', Technological Forecasting and Social Change, 60, 37–54

B Martin, A Salter, D Hicks, K Pavitt, J Senker, M Sharp, N Von Tunzelmann (1996), The Relationship Between Publicly Funded Basic Research and Economic Performance: A SPRU Review. HM Treasury, London.

RM May (1997), 'The scientific wealth of nations', Science 275 (5301), 793-796.

DC Mowery, N Rosenberg (1979), 'The influence of market demand upon innovation: a critical review of some recent empirical studies', Research Policy 8, 102-153.

RR Nelson (1959), 'The simple economics of basic research', Journal of Political Economy 67, 297-306.

RR Nelson (ed.) (1993), National Innovation Systems: a Comparative Analysis (Oxford University Press, Oxford).

RR Nelson, SG Winter (1977), 'In search of useful theory of innovation', Research Policy 6, 36-76.

RR Nelson, SG Winter (1982), An Evolutionary Theory of Economic Change (Harvard University Press, Cambridge, Mass.).

OST (1993) Realising our Potential: A Strategy for Science, Engineering and Technology, Office of Science and Technology, HMSO, London, UK, 1993.

K Pavitt (1984), 'Sectoral patterns of technical change: towards a taxonomy and a theory', Research Policy 13, 343-373.

CK Prahalad, G Hamel (1990), 'The Core Competence of the Corporation', Harvard Business Review 68 (3), 79-91.

EM Rogers (1962), Diffusion of Innovations (Free Press, Glencoe).

PM Romer (1990), 'Endogenous technological change', Journal of Political Economy 98 (5/2), S71-102.

R Rothwell, C Freeman, A Horsley, VTP Jervis, AB Robertson, J Townsend (1974), 'SAPPHO updated: Project SAPPHO Phase II', Research Policy 3, 258-291.

AL Saxenian (1994), Regional Advantage: Culture and Competition in Silicon Valley and Route 128, (Harvard University Press, Cambridge, Mass.)

J Schmookler (1966), Invention and Economic Growth (Harvard University Press, Cambridge, Mass.).

JA Schumpeter (1934), Theory of Economic Development (Harvard University Press, Cambridge, Mass.).

JA Schumpeter (1939), Business Cycles: A Theoretical, Historical and Statistical Analysis (McGraw-Hill, New York).

JA Schumpeter (1942), Capitalism, Socialism and Democracy (Harper, New York).

PM Senge (1990), The Fifth Discipline: The Art and Practice of the Learning Organization (Doubleday, New York).

K Smith (2000), 'Innovation as a systemic phenomenon: rethinking the role of policy', Enterprise and Innovation Management Studies 1, 73-102.

RM Solow (1957), 'Technical change and the aggregate production function', Review of Economics and Statistics 39, 312-320.

DJ Teece, G Pisano, A Shuen (1997), 'Dynamic capabilities and strategic management', Strategic Management Journal 18, 509-533.

JM Utterback, WJ Abernathy (1975), 'A dynamic model of process and product innovation', Omega 3, 639-656.

E Von Hippel (2005) Democratizing Innovation, (The MIT Press, Boston, Mass.)

CH Weiss (1980), 'Knowledge creep and decision accretion', Knowledge. Creation, Diffusion, Utilization 1, 381-404.

B Wernerfelt (1984), 'A resource-based view of the firm', Strategic Management Review 5, 171-180.

About the Office of Health Economics

Founded in 1962, the OHE's terms of reference are to:

- commission and undertake research on the economics of health and health care;
- collect and analyse health and health care data for the UK and other countries;
- disseminate the results of this work and stimulate discussion of them and their policy implications.

The OHE's work is supported by research grants and consultancy revenues from a wide range of UK and international sources.

The research and editorial independence of the OHE is ensured by its Policy Board and Editorial Board.

Further information about the OHE can be found at www.ohe.org

Office of Health Economics
12 Whitehall London SW1A 2DY
Telephone: +44 (0)20 7747 8850
Facsimile: +44 (0)20 7747 8851
www.ohe.org