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RESEARCH IMPACT QUANTIFICATION

R. JOHNSTON

Australian Centre for Innovation and International Competitiveness, University of Sydney, Sydney (Australia)

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The development of methods for the quantification of research impact has taken a variety of forms: the impact of research outputs on other research, through various forms of citation analysis; the impact of research and technology, through patent-derived data; the economic impact of research projects and programs, through a variety of cost-benefit analyses; the impact of research on company performance, where there is no relationship with profit, but a strong positive correlation with sales growth has been established; and calculations of the rates of social return on the investment in research.

However, each of these approaches, which have had varying degrees of success, are being challenged by substantial revision in the understanding of the ways in which research interacts, and contributes to, other human activities. First, advances in the sociology of scientific knowledge have revealed the complex negotiation processes involved in the establishment of research outcomes and their meanings. In this process, citation is little more than a peripheral formalisation. Second, the demonstration of the limitations of neo-classical economics in explaining the role of knowledge in the generation of wealth, and the importance of learning processes, and interaction, in innovation within organisations, has finally overturned the linear model on which so many research impact assessments have been based. A wider examination of the political economy of research evaluation itself reveals the growth of a strong movement towards managerialism, with the application of a variety of mechanisms – foresight, priority setting, research evaluation, research planning – to improve the efficiency of this component of economic activity. However, there are grounds for questioning whether the resulting improved efficiencies have, indeed, improved overall performances. A variety of mechanisms are currently being experimented with in a number of countries which provide both the desired accountability and direction for research, but which rely less on the precision of measures and more on promoting a research environment that is conducive to interaction, invention, and connection.

Progress on research impact quantification

There has been extensive research effort in developing a wide range of approaches to the quantification of the impact of research. Some of these are recognised as falling within the mainstream of work on the development and testing

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of performance indicators and, more generally, research evaluation. Others have their origins in quite different concerns, and academic disciplines.[1]

This work can be broadly divided into five distinct categories:

i) *Impact of research outputs on other research* – a wide range of science indicators based on the manipulation of citation data[2] have been produced. For example, relative comparisons of citation rates have been used to compare the performance of research groups, university departments, and universities as a whole in Australia.[3] Essentially, the frequency of citation is used as a proxy for the 'importance', or 'impact', of a publication. While most analysts are careful to deny this can be taken as a direct measure of quality, in practice citation-based measures are accepted as a valid measure of relative importance, and hence of research impact.

ii) *Patent-based measures of the impact of research and technology* – patents constitute an alternative type of reference or citation, with an emphasis on industrial commercialisation, or at least, potential interest. Extensive analyses[4] have identified a variety of patterns of linkage between the literature-based outputs of the research system, and its application in seeking to establish ownership of the intellectual property underlying an industrial application. However, it is recognised that the patenting process is marked by great variation dependent on both the industry of application and the knowledge discipline, and subject to a range of other idiosyncrasies, which represent limitations to the validity and comprehensiveness of these as quantified impact indicators.

iii) *Economic impact of research projects and programs by cost-benefit analyses* – an array of cost-benefit analyses techniques focussed on R & D projects have been developed. Some of these have been embodied in software programs, and are routinely applied in project evaluation and project selection.[5] These approaches rest on the establishment in some way of the economic return, discounted in some appropriate fashion, from a project or program. When set against the cost, a calculation of the return on the investment can be made. Typically, returns of 1.5 to 15 times the investment have been calculated.[6] However, while this approach does appear to be able to be used to calculate the economic benefits of a particular research project after it is completed, it is not able to provide a general measure of the impact of an area of research.

iv) *Impact of research on company performance* – analyses of the historical records of company performance, and of R & D investment, particularly in the US,[7] has revealed a strong positive correlation between R & D investment and sales growth, with a predominant lag of 2–3 years. However, no such relationship with profit has been established. In other words, the quantitative impact of research on the growth of future sales can be calculated, at least for the US. Not surprisingly, the rate of return varies between different industries.

v) *The rates of social return on the investment in research* – an extensive research program by economists such as *Mansfield* and *Griliches*[8] have carried out analyses of contribution to production function and social rates of return. For the latter, rates between 100% and 250% have commonly been reported.[9] However there have been many criticisms of the comprehensiveness and reliability of the data, and of the many assumptions upon which such calculations rest.

However, each of these approaches, which have had varying degrees of success, are being challenged by substantial revision in the understanding of the ways in which research interacts, and contributes to, other human activities. First, advances in the sociology of scientific knowledge have revealed the complex negotiation processes involved in the establishment of research outcomes and their meanings. In this process, citation is little more than a peripheral formalisation.

Second, the demonstration of the limitations of neo-classical economics in explaining the role of knowledge in the generation of wealth, and the importance of learning processes, and interaction, in innovation within organisations, has finally overturned the linear model on which so many research impact assessments have been based.

A new political economy of research

The basis of the current paradigm

The current paradigm in science and technology policy is essentially composed of two distinct elements: the economists' view of scientific knowledge as a public good, and the scientists' view of research as an endless source of knowledge upon which new economic activity can be built.

The economist's view is firmly established in the academic literature, policy analysis, and in informed public discussion. Scientific knowledge is regarded as having a range of intrinsic characteristics that make it impossible for it to be treated, or managed, as a commodity; as a consequence there is an imperfect market for this knowledge, which causes business to under-invest; this market failure provides a justification, indeed a powerful reason, for government's to act to supplement and stimulate this investment.

The second component of the current paradigm in science and technology policy has its origins in the political views which the scientific community managed to establish in the 1950s as the basis of the social contract between science and the community, and their representatives, government.

The exemplary author of the post-War contract between science, governments and the community was Vannevar Bush, in what must be one of the most famous and oft-quoted government reports, *Science, the Endless Frontier.*

The essential Bush arguments are captured in two canons. The first of these is:

basic research is performed without thought of practical ends..

The underlying argument is that creativity is constrained by any thought of application. Indeed, the commonly accepted interpretation is that there is an inherent tension between the goals of understanding and use and, by extension, a radical separation between the derived categories of basic and applied research.

The second canon provides the delivery end of the social contract between science and government:

basic research is the pacemaker of technological development.

> If basic science is not short-circuited by premature thought of practical use, it will prove to be a remote but powerful dynamo of the technological advances that will meet the full range of our society's need, as the advances of basic science are converted into technological innovations by the processes of technology transfer.[10]

The power of these ideas has been quite remarkable. They spawned not only the modern form and extraordinary growth of science, setting its path irreconcilably from intellectual pursuit to industrialised manufacture. But they also very significantly contributed to the shaping of the modern industrial, and indeed, post-industrial, society. Its roots have penetrated so deeply that the majority of scientists, and much of the thinking public, consider it to be a statement of the obvious.

The extent of the synergy between the economists' view of scientific knowledge as a public good, and the scientists' view of research as the endless source of knowledge and consequent economic application becomes apparent when the two perspective's are laid side by side, though there has been curiously little explicit comment or analysis.

The public good/market failure view of scientific knowledge, in placing the emphasis of the economic process on the generation of knowledge, is implicitly based upon, and provides strong conceptual and rhetorical support for, the linear model of innovation.

The most powerful element of the linear model has been that innovation is based on discovery processes within science. As a consequence, the emphasis of policy in this area has, until recently, been almost exclusively upon research.

There have been many demonstrations of the conceptual and empirical inadequacies, indeed distortions, of the linear model. However, these apparently well-argued cases have had relatively limited impact on the fundamental basis and practice of science policy. The linear model has resonated too strongly with deeply held views, and the nexus between the economists' public good and the scientists' view of the primacy of their knowledge, to allow simple factual inadequacies to count for much.

It has become apparent that the overthrow of the linear model will require a much deeper, and concerted challenge to the set of ideas which constitute the dominant 'world view' of science policy.

Some fundamental challenges to the current paradigm

There appear to be at least three major challenges to the current paradigm, and the views of research and innovation upon which they are based.

The *first* of these is concerned with the *non-rivalry*, and to some extent, the *appropriability* of scientific knowledge. The emphasis of many representations of this perspective is on the extent of control of new knowledge by the producer.[11] However, the fact that the producer of knowledge may have limited control over access to their product does not provide a sound basis for arguing that such knowledge is freely available.

This argument can be countered from two perspective's. Firstly, the sociology of scientific knowledge has demonstrated that an isolated 'piece of knowledge', statement, or theory, is quite literally useless, indeed has no meaning, unless it is embedded in a supporting context of well developed theory, evidence, and argument. Making use of any piece of knowledge requires a considerable investment in establishing the necessary interpretive context of theory, concepts, data and tacit experience.

In addition, it is now established that a scientific or technical resource has no intrinsic value or use. It is only when the necessary "complementary assets"[12] of technological support systems, production capacities, and distribution networks are appropriately assembled that knowledge can be converted to profitable use.

On this basis, it appears reasonable to conclude that the extent of the public or private nature of scientific knowledge is highly variable, and context-dependent, rather than an intrinsic property of the knowledge itself:

> Degrees of appropriability and of rivalry are the outcome of the strategic configurations of the relevant actors, of the investments that they have already made or are thinking of making.[13]

The *second* major challenge is provided by the increasingly documented and accepted phenomenon variously referred to as *irreversibility*, *increasing returns*, or *path dependency*.[14]

In order to limit the potentially infinite number of goods that could be offered on the market place, to offer the consumer the possibility of 'ordered, informed choice', and to ensure the possibility of a return on the investment in new technology and new products, "that strange conspiracy between technology and the marketplace"[15] occurs to develop a common techno-economic trajectory. Furthermore, it is the first decisions in technology and product that commonly provide the powerful and self-reinforcing determinants of that trajectory.

Under these conditions, what is commonly called public good science, might be seen as a source of variety in knowledge, outside the confines of the accepted trajectories. It provides the supply of mutations which are assessed via the selection criteria of the existing evolutionary systems, and which occasionally challenge successfully, and transform, the dominant paradigm.

The *third* major challenge is presented by advances in the theoretical, and practical understanding of *the innovation process*. The past decade has seen major changes in the understanding of the nature and characteristics of innovation processes, and their economic effects.[16]

The common threads running through these works, with their emphasis on evolutionary systems of innovation, organisational learning, and a range of non-market activities such as cooperation and collaboration, are a rejection of the assumptions underpinning the neo-classical model of economic activity, and an emphasis on interaction:

> the overall innovation performance of an economy depends not so much on how specific formal institutions (firms, research institutes, universities, etc) perform, but on how they interact with each other as elements of a collective system of knowledge creation and use, and on their interplay with social institutions (such as values, norms, and legal frameworks).[17]

This evolutionary view of technological knowledge and innovation provides the basis for a quite different consideration of the justification, and appropriate form of, government intervention. Whereas current neo-classical-based policies emphasise support for knowledge creation, because of the externalities of the knowledge commodity, the evolutionary model places an emphasis on coordination across all components of the system. The most appropriate place for intervention, and public support, may be in providing mechanisms to assist in knowledge identification, location, and distribution.

As early as 1980, OECD analysts were arguing that the variable economic performance between nations seemed to be less a matter of levels of investment in R & D, than of the strength of mechanisms for learning, knowledge-sharing, and cooperation.[18]

> Knowledge of how to develop new knowledge, how to locate and acquire rapidly knowledge generated elsewhere, how to diffuse knowledge throughout an organisation, how to recognise possible inter-connections between two distinct pieces of knowledge, how to embody knowledge in products and services, how to obtain access to the learning experiences of customers – all of these are the challenge for the modern manager, and for those who would make science policy.

The political economy of research evaluation

A range of management mechanisms, including priority setting, planning, accountability, selectivity, and evaluation have been widely introduced into research policy in recent years. These have been interpreted as a response to resource limitations for research,[19] and more broadly, to the emergence of industrialised science, resulting from the increasingly valuable contribution of research to economic growth.[20]

There can be little argument against the need for improving the accountability and transparency of government research agencies' investment decisions, or for raising the efficiency of research. However the ways in which this accountability and efficiency are achieved, and their contribution to the overall effectiveness of the research effort certainly deserve scrutiny.

Significant effort has been devoted in the past decade to improving the methods and management of priority setting and evaluation for R & D. There is general reason to believe, though little detailed evidence to support, the view that at least some of this improved management has led to a higher return on the national investment in research.

However there is also a growing body of evidence, most of it still at the research manager experience level, that excessive attention to the new requirements of research management can, on occasion, lead to a reduction in the quality of the research activity itself. A great deal more empirical evidence is necessary to substantiate this connection.

Ziman has strongly challenged the effectiveness of some of the elements of the new research management. Thus,

> The various practices through which the essential functions for 'good science' were traditionally performed have been melded with or replaced by a whole host of new practices and procedures... In many cases, it is not even clear whether these functions are being carried out at all, even though they are vital to the long-term health of the enterprise.[21]

He has also argued that the requirement for accountability lays stress on the narrowly instrumental aspects of science, at the expense of its exploratory, speculative aspects.

It is also worth reflecting on the increasing imposition of bureaucratic decision-making and reporting requirements on researchers at the very time when command-control management systems are being rejected in general management as cumbersome and ineffective, and replaced by an emphasis on reduced levels of management, decentralised decision-making, and work group autonomy.

A particular weakness of the research evaluation and selectivity methods is their implicit assumption of a commodity view of research. Much analysis, including the underlying treatment of knowledge as a public good which generates spillovers, relies on the assumption of knowledge as a commodity, with the characteristics of other commodities, apart from acknowledged special differences.

While regarding knowledge as a commodity has enabled its transactions to be encompassed within the domain of economics, there is an emerging view that this seriously misrepresents the way in which knowledge is actually recognised, used, transferred, and valued. On the basis of the arguments outlined above, a piece of knowledge carries no intrinsic use value. It is only when it is assembled in the context of a wide range of other pieces of knowledge that its possible use value may be perceived.

However, in all but a limited range of cases, it is not possible to define the transactions of research in these market terms.

> The conflict between the corporate interests of academic institutions and the personal interests of their academic staff... arises from their involvement in different but overlapping market systems, whose diverse trading arrangements have not been systematically harmonized.[22]

Leaving aside the issue of the competence of government departments to identify their science needs in principle, the underlying model of the 'purchase' of research outputs is of a totally articulated problem, for which the best knowledge to achieve resolution can be sought.

In reality, knowledge is needed to articulate the problem, and the clarification of the precise nature of the problem commonly emerges through the research process, in interaction with the 'purchaser'. In the same way, the 'resolution' of the problem is likely to require a continuing knowledge input, as the purchaser's view of what counts as adequate resolution is changed by the availability of knowledge. The purchase model is based on the totally inaccurate and counter-productive separation of use and understanding.

The interactive nature of understanding and use, of knowledge and action, places important issues such as the need for clear articulation of government research needs, the major role for contestability, and the separation of government roles of policy-maker, purchaser, and performer, in a very different light.

For the first, the consequence is not that government departments should somehow identify their knowledge needs, or more appropriately, the needs of *their* customers. Rather, there is a need for much higher levels of interaction between government officials and those, wherever they are located, who may possess knowledge which can assist in illuminating, identifying, characterising and resolving potential problems. Closeness, rather than arms-length relationships is what is called for.

For the second, contestability in theory should ensure all the virtues of competition. However, if in practice it prevents the development of long-term capabilities, and long term relationships, such competition may only ensure a continuing waste of research resource investment, as all the energy goes to knocking off the competitors. In this sort of environment, the competition for excellence may be a far more effective driver than the only lever available to the economist – the competition for money.

For the third, the separation of roles is undoubtedly very important to ensure that monopoly conditions are not established by a research performer. But with the highly pluralist science and technology systems common today, with a wide range of both 'purchasers' and 'sellers', the danger seems less one of monopoly, than of the policy-makers, 'purchasers' and performers all operating in isolation of one another. Promotion of more effective dialogue between the players, and significantly improved coordination mechanisms, seem to be the more pressing requirement.

One might conclude, at this stage, that the benefits of the introduction of a range of research management tools are twofold. Firstly, they have provided a bureaucratic accountability which was previously absent, and which is widely regarded as being the appropriate way to guard the public purse. Secondly, in an era of strong competition for resources, they have provided what, while not universally accepted, has nevertheless been regarded as a neutral rationing device.

Conclusions

I will conclude by providing just two illustrations of new approaches to the organisation and management of research which appear to be based, explicitly or otherwise, on an understanding of the negative consequences of the application of an excessive managerialism to research. The examples are drawn from Australian experience, with which I am most closely familiar. However, I am aware that there are some comparable schemes being developed in nations within the European Community.

The first scheme was launched as a typically managerialist government intervention. Based on concern over the limited level of interaction of government research agencies with industry, a requirement was introduced that these agencies attract 30% of their revenue from external sources.

Initial responses were fairly predictable, with a shift in emphasis towards short term and applied research, pursuit of contracts to provide testing and instrumental services, and a general rush to sell whatever might seem valuable. This led to a short term decline in basic research, in publications, and on the available evidence, in morale.

However, over a period of 2–3 years, supported by some reasonably sensitive long term management on the part of the government research agencies, there has been a marked shift in the mode of operation of this management mechanism. The was focus is upon the establishment of supportive relationships and the performance of negotiations which allow industrial firms and the researchers to jointly identify areas of strategic research which both offer significant potential in application and draw appropriately on the skills and capabilities of the research agency.

Under these arrangements, accountability is maintained not so much by the requirement for external funding, as by the development of relationships which are effectively steering research programs without damage. As a consequence the level of publications and extent of fundamental research has again risen and in addition linkages with industry become much stronger.

The second example of new approaches to investment in research are provided by the Australian Cooperative Research Centres Scheme. Under this scheme, some 61 Cooperative Research Centres (CRC) have been established. The CRC mechanism requires research groups within universities, government agencies and appropriate industrial and business organisations to jointly develop a long-term research program to serve their identified interests.

The process was one of open competition with no explicit identification of any preferred areas of activity. It has lead to more than 300 proposals being developed, representing very wide sectors of the Australian economy from mining to information technology and bio-technology, from welding to new materials, from horticulture to Antarctic fishing. The successful CRC's are funded on a roughly 1 to 1 basis by the government to augment the funding in cash and in kind provided by the members of the CRC.

The scheme has now been in operation for just over 3 years and hence it is a little early to speculate on the performance in detail. However, preliminary reviews have shown the development of intense of interaction between the members of the CRC, significant growth of a research culture in the industrial members, and an improved understanding of the way in which research and new technology may be used to support their business objectives. The quality of the research in general is first class and certainly shows no sign of dramatic deterioration from that performed previously or under other schemes. Prime accountability is to the Boards of Management of the CRCs with additional reviews and management support visits from the CRC Secretariat.

By this mechanisms, significant steering of the Australian research capability has been achieved, and high levels of visibility, transparency, and accountability established Importantly, they have been established without resort to traditional managerialism or high levels of directive intervention either in terms of influencing the content of the research program or in meeting very strict output requirements.

These two examples provide, I believe, an illustration of alternative approaches to achieving desired goals of effectiveness, accountability and directiveness, without making the mistake of viewing knowledge as a commodity and its production as a process which can be most effectively managed by simple industrialised command and control processes. But much more is yet to be done. In particular there is a need for improved understanding of the ways in which the productive business economy and the knowledge economy can interact with each other to the greatest mutual benefit.

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