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OCCASIONAL
PAPER

In Search of the Holy Grail

Understanding Research Success

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RAND EUROPE

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Introduction

A major and recurring challenge facing those involved in science and science policy is how best to spend research money. This is even more pressing in fields with great diversity of science and opinion, such as mental health. An earlier version of this paper was used to stimulate thinking prior to a workshop hosted by the Graham Boeckh Foundation in Montreal on 21 and 22 April 2009 to discuss the evidence base for mental health research funding, with a special interest in funding for schizophrenia research. The primary purpose of the workshop was to engage other research funders (both government and philanthropic), research practitioners and policy researchers in a discussion on mental health research funding and how to facilitate the translation of research into patient benefit. The workshop examined whether other funders are interested in policy research into the effectiveness of research funding in the field, and would derive value from it. The outcome of the workshop was the establishment of *SoS Mental Health*[1] – a network that will convene funders of mental health research in Canada, US, UK and elsewhere, along with mental health scientists and practitioners, and policy researchers interested in the science of science.

The network will identify a 'living' portfolio of policy research that will lead to improvements in the effectiveness and efficiency of research funding. This portfolio will be delivered through a series of 'science of science' projects. The first project to be funded through the network is Project Retrosight, a multinational study which will evaluate the translation and payback from basic or early clinical mental health research into clinical application and community practice. Building on successful methodologies used to evaluate diabetes, arthritis and cardiovascular research funding, described later in this paper, this project will provide a long term view of the factors involved in successful research.

The Graham Boeckh Foundation was created by Tony Boeckh and his family. Tony is a Canadian philanthropist with an interest in mental health, in honour of his late son, who suffered from schizophrenia. The private family foundation supports research and programmes in the area of mental health and has previously funded a Chair in Schizophrenia Studies, based at the Douglas Hospital Research Centre of McGill University. Tony and his family are interested in improving the lives of patients, supporting families, and ultimately, finding a cure for schizophrenia. In other words, how to improve the translation of discovery to patient benefit, and whether it is possible to identify some key factors to accelerate the process.

Science policy addresses four broad questions: which areas of science to invest in? How best to make that investment? What are the returns from those investments? And are there different roles for different types of funder? Various meetings and discussions are aimed at determining which scientific areas to fund. This is not the focus of this paper. Our focus is on the evidence base for how funding actually works and whether it can inform subsequent decisions. That is, we are interested in measuring the return, or payback, from research funding with the objective of identifying what works. We want to identify bottlenecks that are hindering research translation and use funding to stimulate improving the process.

This short paper is divided into three main parts. The first provides a brief overview of the mental health and schizophrenia research field, the challenge of translating advances in neuroscience into patient benefit, and the current state of clinical and community practice. The purpose of

[1] In this context SoS abbreviates 'science of science'

this section is to provide a primer for non-mental health research specialists.

The second part focuses on what we know about research policy. It is divided into two subsections. The first reviews studies that have contributed to an evidence base on how best to make research investments; the second, on whether those investments are worth it. As with part one, the purpose is to provide a primer of relevant science policy research. The final part of this paper draws together these themes by identifying some of the key questions that the *SoS Mental Health* network wishes to address.

This paper does not claim to be a systematic or unbiased review of the relevant literature – it inevitably draws on research in which we have been involved. Rather, it provides a selective background, drawing on our own work to supply a common starting point for discussion and to highlight key issues for debate.

Part 1: The mental health research translation gap

The last two decades have seen tremendous advances in basic sciences – the human genome has been decoded, molecular biology is unravelling the basic structure of how cells function, new imaging technologies are unveiling the intricate functions of the brain. But, how does basic research translate into clinical application?

There is a general concern that the very significant investments in basic biomedical research made over the last 20 years are not providing a dividend in terms of improvements in healthcare. This seems to come into specific focus in the field of mental health research where, despite significant advances in the biomedical understanding of mental health and brain function, little of this has directly impacted on day-to-day clinical practice. There are still no diagnostic blood tests, imaging is still not clinically valuable or routine, treatments are still chosen largely on a trial-and-error basis, and there are no objective biochemical markers to follow. Few truly innovative treatments have been developed, and several promising new treatments (for example, second generation antipsychotics) have had disappointing results when disseminated into practice. At the same time the global burden of disease resulting from neurological disorders looks set to increase, which entails significant social and economic costs both in developed and developing economies. Two broad directions lie ahead of the field. At one level, basic neuroscience discovery has to continue because one cannot second-guess advances that are just around the corner. At another, a special effort has to be made to translate existing and new discoveries into clinically meaningful applications, recognising that this takes time and effort (1).

This is especially the case for schizophrenia. Schizophrenia is a chronic, severe and disabling brain disorder. It is characterised by symptoms like hallucinations, delusions, disordered thinking, movement disorders, social withdrawal and cognitive deficits. Today, schizophrenia affects approximately one per cent of the population worldwide. There is substantial burden associated with the disease, which usually has its onset in early adulthood (15–35 years) and, despite the best available treatments, approximately two-thirds of affected individuals have persistent but fluctuating symptoms.

The causes of schizophrenia are still unknown, so current treatment focuses on eliminating the symptoms of the disease using antipsychotic medications and psychosocial interventions (2). There were no efficacious treatments for schizophrenia until the beneficial effects of chlorpromazine were discovered in the early 1950s. These treatments enabled patients to leave hospitals and function moderately well in society at large (3). However, this first generation of typical antipsychotic medications could cause side effects, such as rigidity, persistent muscle spasm, tremors and restlessness. In the early 1990s, a second generation of atypical antipsychotics with fewer side effects were developed, although trials, such as the CATIE study[2], have questioned their clinical superiority, and there is now widespread concern about the adverse effects of these medications.

A number of psychosocial interventions are also used in the management of schizophrenia. These include supported employment, family psycho-

[2] The Clinical Antipsychotic Trials of Intervention Effectiveness (CATIE) Study, funded by the National Institute of Mental Health (NIMH) from December 2000–December 2004, is a US nationwide public health-focused trial comparing the effectiveness of older and new antipsychotic medications used to treat schizophrenia. It is the largest, longest and most comprehensive trial ever carried out to examine existing therapies for this disease. http://www.mentalhealthamerica.net/go/research/catie (as of 19 March 2009)

education, cognitive behaviour therapy, assertive community treatment, peer support programmes, and skills training, to name only a few (4, 5). Over the past few years, the nature of research on psychosocial interventions has been changing, including expectations that studies should resemble randomised controlled drug trials and that a more standardised process of care should be developed that assesses constituency in the implementation of the intervention.

In other words, despite substantial investments in both basic and applied mental health research, improvements in patient outcomes have been modest.[3] At the same time, the burden of disease continues to increase. It is thus hardly surprising that the condition weighs heavily on the research funding agendas of governments, research councils, charities, philanthropists and industry.

A key challenge for funders, therefore, is to improve the efficiency and impact of their investments in this important area of mental health. The first step towards tackling such a challenge is to understand what the key enablers and impediments are to knowledge advancement and to translating schizophrenia-related research into effective prevention, diagnosis and treatment strategies. An evidence base is needed to answer these policy relevant questions, and to inform future funding decisions. What kind of research support, given when and to whom, delivers the best translation? What are the barriers to translation? Why did some exciting scientific discoveries not translate? In a nutshell, what are the factors that lead to success or failure in efforts to promote research translation from bench to bedside?

This is not just an academic question. Funding agencies worldwide are trying to evaluate the impact of health research in order to inform their research strategies, justify public expenditure on research, and engage stakeholder communities in illuminating the research process as well as the research itself (6–9).

[3] For example, the total amount of grants awarded from the National Institute of Mental Health, the primary funding agency for mental health research in the US has increased from around US$ 714 million in 1999 to over US$ 1.1 billion in 2008.

Part 2: Science of science

Research on science policy can be grouped into three types of studies. The first type aims to inform the formulation of R&D strategies and includes field reviews and future studies such as foresight. The second type aims to support and reduce uncertainty in today's decision making through rigorous policy analysis. Examples would include studies into peer review processes, scientific careers and infrastructure needs. The third type of study is research evaluations, and here we make a distinction between evaluation for learning and evaluation for accountability.

The primary objective of the April 2009 workshop was to understand what works in research funding, so our focus, and the weight of this section, is on evaluation for learning. However, as this taxonomy is not mutually exclusive, we first briefly review selected studies that have informed strategy and policy analysis, as well as different types of research evaluations.

Research strategy

During the 1980s, research on schizophrenia, patient care and support for relatives of those suffering from schizophrenia began to gain momentum. The US led the way in these efforts with the emergence of the National Alliance of Mental Illness (NAMI) in 1979 as an advocacy and support group for families affected by schizophrenia; the initiation of the Winter Workshop on Schizophrenia Research in 1982; the International Congress of Schizophrenia Research (ICOSR) in 1987; and a *National Plan for Schizophrenia Research* initiated by the US National Institute of Mental Health (NIMH) in 1988 (10).

The *National Plan for Schizophrenia Research* dealt with the full spectrum of recommendations for research on schizophrenia. Since then, NIMH has produced multiple reports and plans for research related to schizophrenia in neuroscience, health services, and numerous other areas. In 1999, the first US Surgeon General's report on the topic of mental health was published. This report addressed many issues related to mental health, including research (11). Although the report did not specifically focus on schizophrenia research, it did include it. The report also incorporated "a vision for the future" of mental health, which entailed continuing to build the science base; overcoming stigma; improving public awareness of effective treatment; ensuring the supply of mental health services and providers; ensuring the delivery of state-of-the-art treatments; tailoring treatment to age, gender, race and culture; facilitating entry into treatment; and reducing the financial barriers to treatment.

Similarly, in 1999 in the UK, the Wellcome Trust published a field review (or audit, as it was termed) of neuroscience research (12). The study examined the incidence and economic burden of diseases of the nervous system, the funding of neuroscience research internationally, the published outputs, and the development of new therapeutic products. Opinions were also sought on the barriers limiting research activity, on measures to tackle those obstacles, and on strategic objectives for future research. The review came to four major conclusions: there was a need for multidisciplinary research; future scientific opportunities depended on further understanding of organisational complexity within specific experiment systems (for example, molecular neuroscientists foresaw the study of intercellular signalling focusing on the single cell); that the newly developed genetic technologies needed to be applied to neuroscience; and that there was a need for high-quality infrastructure support, including imaging facilities.

Finally, it is worth noting a long history of foresight studies, originally emanating from Japan in

the 1970s, which aimed to understand future scientific developments, including critical points in the process and their wider impacts on society. Such studies have evolved since the 1970s, with the UK government's foresight programme working to a broader, less technological, focus of strengthening "strategic policy making in government by embedding a futures approach" (13). Of relevance to the current discussion is a recent foresight study published by the UK's Government Office for Science, which aimed to advise the government on how to achieve the best possible mental development and mental well-being for everyone in the UK. One of the areas examined was mental (ill-) health, and the report provided a comprehensive assessment of the scientific state of the art, concluding with an assessment of the interventions needed to meet the future challenges in mental health (13). The report identified a number of areas that need further research – but did not address *how* that research agenda should be delivered. This is the role of policy analysis.

Policy analysis

In contrast to research strategy, policy analysis focuses on the nuts and bolts of supporting science. Two areas that have attracted particular attention are the peer review process and attempts to integrate other forms of evidence, such as bibliometrics, into selection.

Peer review is the dominant method for allocating research funding and has attracted the attention of policy researchers because of its importance and expense. A number of studies in the UK, Canada and the US have examined the peer review process. In 2002, the Canadian Institutes of Health Research commissioned a study to examine how group discussions and external reviews affected adjudication committee members' assessments of the merit of applications, and to determine if differences in adjudication processes and outcomes could be found between committees (14). The results revealed that disagreements between the internal reviewers were common and were unrelated to the number or length of external reviews. More recently, Research Councils UK (RCUK; – the umbrella organisation for the six research councils in Britain) undertook a study on the efficiency and effectiveness of the peer review process (15, 16). The overall costs of peer review were estimated to be £196 million, or 15 per cent of the total research council budget. Seventy-five per cent of the cost of peer review is associated with the preparation and submissions of grant applications, 20 per cent with the cost of refereeing, investigator response and panel membership, and five per cent with internal research council costs.

To give one recent example, the use of bibliometrics to support the procurement of Biomedical Research Centres (BRCs) by the National Institute for Health Research (NIHR) was examined by van Leeuwen et al. (17). It was envisaged that the BRCs should be located within "leading National Health Service/Universities" partnerships that could demonstrate international excellence across a broad range of clinical areas. The first stage of the procurement process included the publication of a bibliometric analysis of biomedical and health research in England, identifying those centres leading in terms of scientific publications (as measured through citation counts) and the network of partnerships between the health and research system. By publishing the bibliometric analysis the NIHR was, in effect, encouraging the community to review the analysis and, where appropriate, self select out of an open procurement process, thereby reducing transaction costs.

Research evaluation for learning

Interest in understanding the contributions of research to society blossomed in the 1960s and 1970s. A series of large, early studies aimed to demonstrate the importance of research by looking at the genesis of individual innovations. A major theme was the comparison of basic and applied research contributions (18–24). Some studies also considered the impact of different types of funding and research settings on innovation outputs, as well as the organisational conditions that influence innovation success.

Interest gradually shifted to smaller studies focused on better understanding the *process* through which research contributes to innovation, i.e., research translation pathways and variables (25–29), the diverse types of *outputs* from research and how they could be *measured*, including bibliometrics (30–38).

Case-study approaches have dominated evaluation research, and these have evolved over time.

Bozeman et al. (39) explain how previous studies have moved through four incremental phases: 1) *historical descriptions* – tracing innovations back to their fundamental supporting inventions; 2) "*research event" based case-studies* – building a family tree of research events that led to an innovation; 3) *matched comparisons* – taking matched successful and unsuccessful innovations, tracing and comparing their development; and 4) *conventional case studies* – using action research, interviews, surveys, narrative descriptions – *complemented with economic and bibliometric techniques* in an attempt to increase methodological rigour and objectivity.

Qualitative and quantitative approaches have been pursued simultaneously. Some scholars have used very loosely structured narratives to try and develop an improved historical understanding of the research process and to achieve descriptive depth. An example would be the Wellcome Trust witness seminars, which bring together a group of experts involved in a particular advance to recollect and discuss, with the resulting debate being transcribed and published to produce a socially constructed history (40). But bibliometric and economic analyses have also permeated the literature (30, 34). Others have developed methods to quantify various aspects of case studies (for example, by scoring data), to facilitate more systematic analysis and hypothesis testing (37–39).

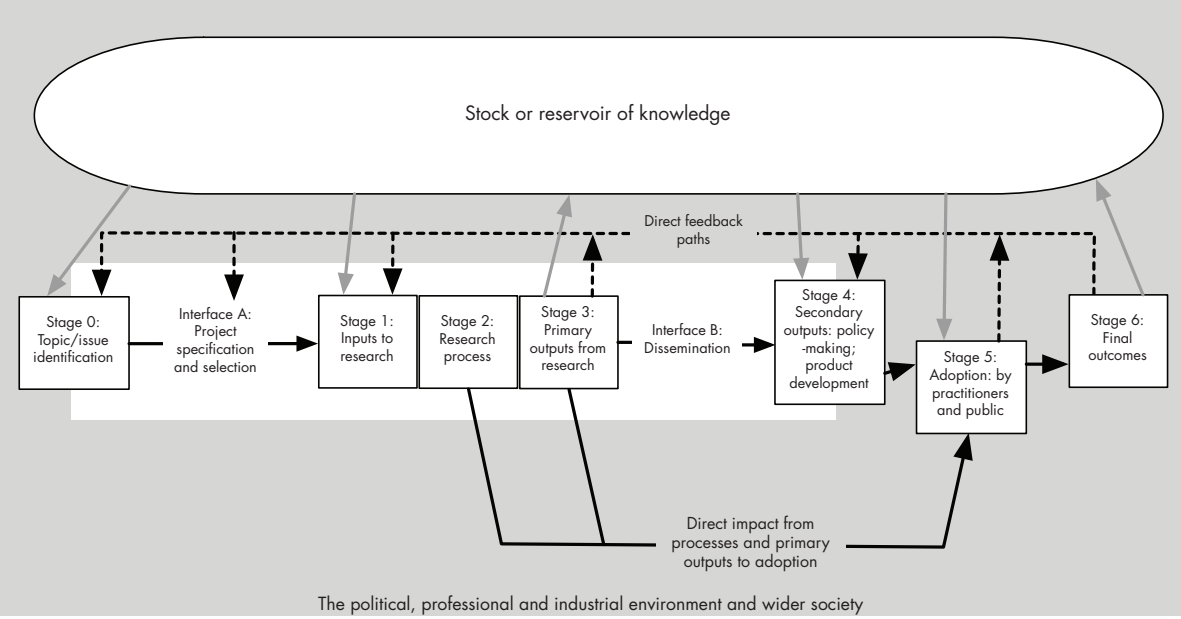
Both qualitative and quantitative research methodologies provide their own insights, but also have their own weaknesses and trade-offs between depth and breadth. For example, surveys and case study approaches allow for more detailed tracing of research translation processes than econometric models, but are often difficult to generalise from (41). On the other hand, econometric assessments suffer from difficulties related to integrating detail into calculations, using assumptions of speculative reliability and validity, and oversimplifying the dynamics of research translation processes (42).

There is widespread agreement that criteria for evaluating research need to integrate both qualitative and quantitative information bases, and that single indicators can be misleading (39). There is also little to dispute the argument that flawed research methodology leads to flawed and ambiguous findings and implications. The most powerful and rigorous methodologies would be those that combine a number of techniques and make use of triangulation – the idea that integrating a number of sources of evidence, a number of methods of analysis, and even a number of investigators, increases the validity and reliability of conclusions (39, 43). Triangulation needs to be based on compatible assumptions about the nature of innovation processes, and integrative conceptual and methodological frameworks. "In the absence of such a framework, the result is a disparate array of indicators and measures. Each set reflects the influence of the underlying discipline, so preference for indicators and measures is guided by the variables of concern in the discipline" (44).

Towards this end, conceptual frameworks for research evaluation have been developed. They attempt to provide a unifying template for conducting research evaluation studies and for organising enquiries, and seek to facilitate the use of multiple methodologies and methods of analysis, as well as cross-case and cross-study comparisons (45). One of the most comprehensive and widely adopted frameworks is the Buxton and Hanney payback model developed by the Health Economics Research Group (HERG) at Brunel University (33), and subsequently used in a number of collaborative studies with RAND Europe (37–38, 46). The framework consists of a multi-dimensional categorisation of benefits and a model of how to assess them. It is a tool for evaluating a comprehensive range of potential outputs from research, and (unlike most other frameworks) also provides a way of conceptualising the process through which outputs are created (that is to say a logic model). Figure 1 summarises the process stages of the payback framework.

The framework has been applied to a number of different contexts and different countries. As an example, the payback model has been recently adopted and modified by the Canadian Institutes of Health Research in Canada, and fine-tuned by the Canadian Academy of Health Sciences (47). The model has also informed analysis of health research systems on behalf of the World Health Organization (48). It has most recently been used in assessments of the payback from Health Technology Assessment programmes in the UK (49) and the Netherlands (50); to explore the payback on arthritis research funded by the Arthritis Research Council (37–38); the research of the Irish Health Research Board (46); a social science research programme of the Economic and Social

Figure 1: The research process in the payback framework (37)



Research Council (52); and payback on cardiovascular disease research in a multinational study (53). Other payback studies are cited in a Spanish paper by Buxton and Hanney (54).

One of the potential and increasingly realisable advantages of using a common and unifying conceptual framework is that a library of comparable case studies could be built up over time. Looking to the future, this could make it possible to start inferring common factors associated with successful research translation along the lines of those identified in Box A for arthritis research. However, at this time, the collective body of payback studies is usually located within a specific disease area (for example, arthritis), research type (for example, health technology assessment) and country (for example, the United Kingdom). In other words, we do not know the impact of the health research system, disease area or confounding variables on potential correlates of research success.

Research evaluation for accountability

In trying to understand what works, it is necessary to have an outcome or dependent variable; this could be, for example, a catalogue of research paybacks (as in the ARC study described above), an analysis of published papers or a monetarised estimate of economic return. Such evaluation measures are widely used to justify research expenditure and to demonstrate that research provides value for money.

Since the turn of the century, a number of studies have attempted to quantify the financial return from biomedical and health research. The primary purpose of such studies is to demonstrate (and at times advocate) the value of investing finite public or philanthropic resources in research. In the language of economics, all resources – including research funding – have opportunity costs; that is, research funding could be put to other uses of benefit to society. To make an informed judgement on whether to allocate expenditure to research or, say, healthcare, budget holders need to understand the likely rate of return from such investments.

One of the first studies that attempted to measure the economic returns from medical research was a US initiative sponsored by the Lasker Foundation called *Exceptional Returns: The Economic Value of America's Investment in Medical Research*. This work focused on the economic value of the reduction in deaths from cardiovascular disease (CVD) that occurred in the United States between 1970 and 1990. The top-line findings of the study were dramatic. Increases in life expectancy of US citizens from 1970 to 1990 were worth around US$2.8 trillion per year; the reduction in deaths from CVD alone was worth roughly US$1.5 tril-

Box A: The returns from arthritis research (37, 38)

To mark its 65th anniversary in 2002, the UK Arthritis Research Campaign (ARC) undertook a review of research activities and impacts. The review found a gap between the aspirations of people affected by arthritis and the ability of science and academia to meet those aspirations. To address this gap, ARC commissioned a study on the long-term outcomes of research grants it awarded in the early 1990s, with the aim of informing future strategy and identifying success factors in translating research into practice.

The research was led by RAND Europe in collaboration with the Health Economics Research Group at Brunel University. The team conducted case studies of 16 research grants awarded in the early 1990s. The research was structured around the Buxton-Hanney payback framework. Cross-case analysis based on individual qualitative assessment and consensus-scoring for each case study led to the following six observations:

Individuals translate research

Human interaction, rather than funding mode or publication impact, is the key factor in driving effective translation of research into practice. Translating findings from the bench to the bedside is due to the conviction, effort and personal networks of the principal investigator. Research funders concerned with improvements at the front line of patient care have an interest in investing in networking activities as well as scientific research itself.

Short, focused project grants seem to provide value for money

There is a view in science policy that long-term stable funding is preferential. However, smaller project grants could offer similar payback to that identified for larger programme grants. The value for money of these projects illustrates that funders should continue to include smaller, focused grants as part of its wider research funding portfolio.

There is a diversity of research payback

The broad definition of research benefits used by the payback model revealed that each of the research projects examined yielded a much richer range of outputs and outcomes than expected. Relevant benefits generated included research training, advances in methods for clinical testing, as well as improved health outcomes and wider economic returns.

Intended or unintended flexibility in funding is used advantageously

Flexibility in the scientific and administrative management of grants, for example, allowing use of funds for exploration of new research avenues, has an often positive, and at worst a neutral, effect on the outputs and outcomes of the research sponsored. Therefore, a case can be made for building a degree of flexibility into scientific funding.

Referees' contributions to the peer-review process are of variable benefit

The opinions of referees on proposed research projects are an established and central element in grant allocation. However, only in a minority of the case studies examined did referees' comments have any influence on the design of the work subsequently undertaken.

The payback framework could be operationalised

The payback framework proved to be effective in capturing the diverse range of research outputs and outcomes, and in identifying the cases where the research had been translated to benefit people with arthritis. Given the appropriate management information, there is good evidence, therefore, that the payback framework could be operationalised to prospectively monitor the returns from arthritis research.

lion per year; and, assuming that only a third of the reduction in CVD deaths could be attributed to medical research, spending on medical research yielded an annual 20-fold rate of return (55).

These results were published in 2000 and stimulated renewed interest in how the impacts of medical research could be evaluated. In 2003, the Australian Society for Medical Research used a similar methodology to show that returns on Australian research were also "exceptional": the total return on the investment in CVD research in the year 1998–99 was estimated to be as high as 788 per cent. For "nervous systems and mental", the rate of return was 23 per cent (42). The Australian team updated their analysis and methodology last year and produced a revised estimate of 117 per cent for health R&D overall, but did not break down their estimate by disease areas (56).

Box B: Estimating the economic benefits from medical research in the UK (57)

A recent study carried out by a consortium involving Brunel University, the Office of Health Economics and RAND Europe focused on measuring the returns from investment into research in cardiovascular disease and mental health over 17 years between 1975 and 1992. Cardiovascular disease was chosen because much is known about how therapies and diagnostics affect health and lifespan; and mental health was selected because, conversely, there is less understanding of such effects.

Developing a methodology to work out the health and gross domestic product gains from investing in these two areas, the researchers aimed to address a raft of questions, including, what proportion of global cardiovascular disease/mental health research can be attributed to the UK? What is the time lag between research expenditure and its impact on health? What were the key treatments and interventions over this period and how many people used them?

Data were gathered from UK research funders, including the Medical Research Council, the Department of Health, and the Wellcome Trust, to work out total investment in the two chosen disease areas. Evidence-based clinical guidelines were used to estimate the UK's research contribution to interventions in these fields, including those from the National Institute for Health and Clinical Excellence (NICE). Evidence on 46 different combinations of cardiovascular diseases and interventions to treat or prevent them was analysed, for example aspirin, beta blockers and smoking cessation, while the study for mental health used evidence on six such combinations. Quality Adjusted Life Years, estimated by NICE to have an opportunity cost value in the NHS of £25,000 each, were used to measure the quantity and quality of life gained from a health intervention.

The researchers estimated that the health and gross domestic product gains from UK public and charitable investments in cardiovascular disease research over the study period were equivalent to an annual rate of return of around 39 per cent for cardiovascular disease, and 37 per cent for mental health research. Thirty percent of the gains reflected a common estimate of the economic benefits to the UK economy, and the remaining nine per cent and seven per cent represented the opportunity cost value of the gains from new treatments or preventative measures.

The findings emphasised that public and charitable funding of medical research encouraged greater investment from the pharmaceutical industry, a spill-over effect. One example of this is that public investment in universities generates skilled graduates, new ideas, networking opportunities and high-quality libraries. The report points out that it is no coincidence that high-tech firms choose to base themselves near top-quality universities. Each £1 of extra public/charitable investment in UK medical research was shown to yield £2.20 – £5.10 of extra pharmaceutical company investment, which taken together earned an extra £1.10 to £2.50 GDP per year for the UK economy.

The study also estimated a time lag between research expenditure and eventual health benefits of around 17 years. This raises further questions, such as whether the measured returns on investment are specific to the time frame studied. Do returns differ depending on the area of research funded? These uncertainties also apply to the time lag between investment and benefit in different disease areas. More research is clearly needed to answer these questions and expand upon the insights gained from the study.

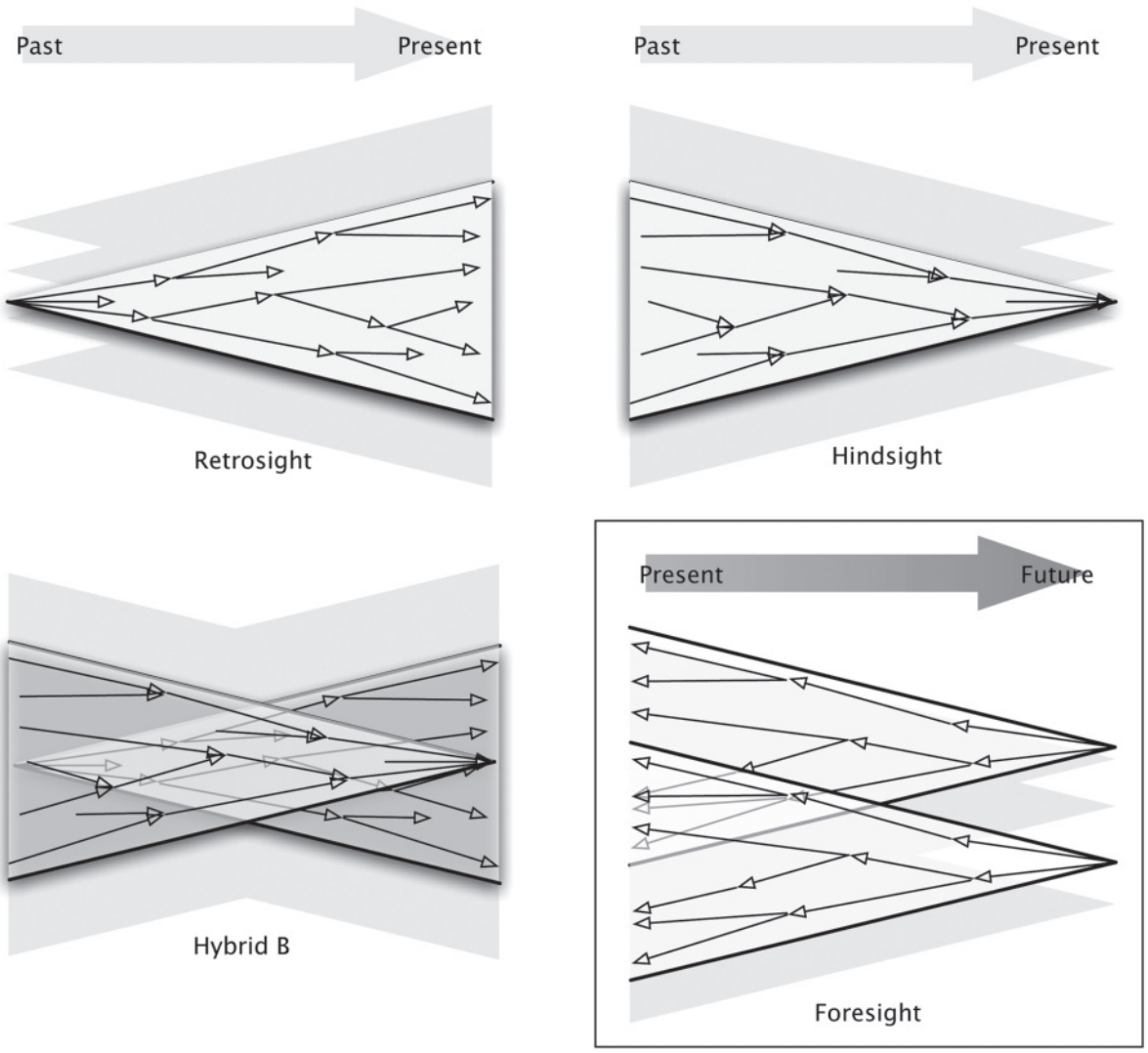
The researchers point out that the study was not intended to be viewed as a one-off exercise, but rather as an opening into a new research field that will lead to even more robust studies in future. However, the results do provide the first real quantitative estimates of the economic benefits of UK public and charitable investment in medical research.

Both the US and Australian studies had important limitations (57, 58), which encouraged the UK's Wellcome Trust, Academy of Medical Sciences and Medical Research Council to commission a one-year study to compare the economic benefits accruing from UK public and foundation medical research with the costs of that research. The research was conducted by a consortium led by the Health Economics Research Group (HERG), and including the Office of Health Economics (OHE) and RAND Europe. A key objective of the study was to address some of the limitations and assumptions of the US and Australian studies and to consider how the question of calculating economic returns could be tackled in the UK context. Using a very different – and thus non-comparable approach – from the US and Australian studies (see Box B), the team's best estimate for total health and economic return from public and foundation mental health research for the period 1975–92 was around 37 per cent. The equivalent figure for cardiovascular research was 39 per cent.

Part 3: Moving from propaganda to performance

Although a nascent area of investigation, understanding what works in research funding, is at a point where the primary objective should not be solely to make the case for research funding, but to improve the effectiveness and value for money of such funding. Or as John Marburger, the former Science Advisor to President George W. Bush, put it in an editorial in *Science*: "A new 'science of science policy' is emerging … But [this] demands the attention of a specialist scholarly community. As

Figure 2: Examples of viewpoints



more economists and social scientists turn to these issues, the effectiveness of science policy will grow, and of science advocacy too" (59).

This requires a better understanding of research performance and, more importantly, the drivers of improved performance. At a conceptual level, we need to understand what factors lead to research success. For example, what kind of science, what kind of scientists, what kind of setting and what kind of funding mechanism are most successful in promoting the translation of research from bench to bedside? We also need to develop innovative approaches and methodologies for answering these questions. So a key question for debate is whether the notion that an evidence base is needed to answer some of these important science policy questions, and whether they would derive value from such an evidence base.

To be grounded, these approaches will inevitably need to learn lessons from the past. This raises an inherent question about whether it is possible to apply historical analyses to contemporary science policy. Some would argue that the science of the past is so fundamentally different that lessons cannot be learned, while others would counter that our focus is on the administration of science rather than the science itself and thus such lessons can be valid. That said, it is also acknowledged that such an approach may restrict the opportunity for innovation because, by evaluating past activity, one inevitably draws lessons from existing practice and may narrow the options available to a research funder. This raises a second key question: whether it is helpful to look at the past to learn for the future, and specifically whether this is likely to be the case for mental health research.

So how do you look to the past to learn for the future? One approach is to work back from a clinical advance and look at its research antecedents and how that research was supported – this is what we term the hindsight approach, coined from the US Department of Defense's Project Hindsight, which examined factors that lead to successful technological change. An alternative, which we term retrosight is to take a point in history – for example, where there was a research investment (such as a project grant, programme grant or fellowship) – and then follow this through to the present day to see what happened to the ideas it produced and the people it supported. This is the approach adopted for the ARC study described in Box A, and currently being implemented for a multinational study in the field of cardiovascular and mental health research. It is possible to combine the two approaches to give a hybrid methodology which works backwards from clinical advances and forwards from research investments. Finally, a fourth approach is to look to the future, without necessarily learning from the past, as in foresight exercises. Such studies generally use a variant of a consensus surveying methodology, whereby experts are asked to identify and then rank research priorities with the aim of identifying and focusing on a few key areas of scientific promise. All four approaches use subtly different research designs, the fundamental difference being the starting points for the analysis (Figure 2).

Concluding comments

Researchers, including those in mental health, seek to secure more resources by identifying new scientific opportunities to fund. We believe that there should be a parallel undertaking to understand better the mechanisms for spending those funds, based on developing evidence about how funding actually works and how that evidence should inform decisions in the future. The examples we have described here are steps towards this end but are not sufficient. In order to identify correlates of research success, it is necessary to build a stock of evidence-based research that can be derived from or applied to mental health, and specifically schizophrenia, research funding. In one sense this is like chasing the Holy Grail – a never-ending quest for an illusory formula that will predict research outcomes. In another sense, it is a statement of the obvious; just as science is the effort to discover and increase human understanding of how physical reality works, science policy should be about understanding how science works.

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