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Can Basic Science Be Valued?

John Mulvey

Introduction

There was a time when governments in the UK were content to leave science to the scientists. An OECD report of 1978 explained: ‘In the UK, objectives for science and technology are not centrally defined, ... it is considered that priorities in fundamental research are best determined by the scientists themselves, ...’

In times of war, of course, the scientists and engineers had been brought out of their laboratories and mobilised to apply their knowledge and expertise to winning battles on the technological front line, feeding intensively as they did so on the ‘seed-corn’ of earlier basic research.

Over the last twenty or so years the possibilities for discovery and the development of new technologies have expanded enormously, and in many areas the time gap between new scientific understanding and its application has shrunk. In most advanced countries the expenditure on research and development, both publicly and privately financed, has increased substantially to take advantage of these opportunities.

The potential economic value of basic research in science and technology has become better appreciated by our politicians. And in an economic context of intensifying global competition combined with a political imperative to contain, and reduce, public expenditures, it was inevitable that governments would also wish to seek ‘better value for money’ in their funding of basic/strategic/long-term research; or more visible economic benefit for less money.

In the United States a new act of Congress became law in 1993. It requires every federal agency including those responsible for funding
research, like the National Science Foundation (NSF), to do strategic planning, to report annually in terms of performance indicators, and to use programme evaluation. ‘... US research in the 1990s must be prepared to demonstrate its benefits to the nation if it is to survive let alone grow’ says Susan Cozzens, Director of the NSF’s Office of Policy Support, adding: ‘Rather than protesting, “trust me”, researchers must answer the challenge “show me”.’ Cozzens puts her finger on a new problem facing the academic research community: the implicit ‘trust’ – that left to themselves scientists would advance science and technology in the most effective and beneficial way – has been withdrawn.

But – although they show little sign of realising it – the challenge for governments, responsible for delivering to the tax-payers ‘better value for their money’ is now very much harder if a value is to be assessed before the money is spent: the potential value of much basic research is not known, not even knowable, at the time it is done, and often not sensibly quantifiable afterwards; so what trust can be placed on prior evaluations of that which is as yet undone?

To get a measure of the task we consider a few examples of past basic research, defined more in respect of its style or aim than whether it was done in a university or elsewhere.

Example 1
In 1959 at the Cavendish (physics) Laboratory in Cambridge, Perutz and Kendrew made the first identifications of protein structure, the molecules myoglobin and haemoglobin. Several years later Sir John Kendrew told an interviewer that while the work was in progress experts said they were doomed to fail, and they worked together for more than ten years with no results. The interviewer thought to correct him: “You mean no useful results?” “No” said Sir John, “no results at all”, adding that Max Perutz had started work on the problem 12 years before he had joined him.

Under the current regime of ‘assessment’ and ‘auditing’ would Perutz and Kendrew have been turned off, for not delivering value for money, long before accomplishing the discovery that won a Noble Prize and started the magnificent tradition continued today in Cambridge’s MRC Laboratory for Molecular Biology? Sir John, in recent conversation, said he believed such a programme of research showing no sign of getting anywhere for so many years would not survive today.

Future generations will still be counting the value of their work as ‘fathers’ of molecular biology.