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Defending basic research

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Governments are demanding more value for money from scientists, and fundamental research is increasingly under threat. The benefits of basic science are easy to defend, however, and scientists should not hesitate to be more aggressive when doing so. Understanding how current funding models developed can help in ensuring effective dialogue with policymakers.

A recent editorial in this journal asked whether scientists today are still able to freely perform curiosity-driven research, or whether there is excessive emphasis on research driven by pre-determined goals [1]. Although this question may appear to be motivated by current issues of financial austerity, the relative importance of basic and applied science is a very longstanding debate [2]. Moreover, current funding models in use worldwide are based on ideas developed to effectively support both kinds of research in a way that places economic growth as a clear priority.

Many policymakers and research managers appear to be unaware of this background, however, and basic science is often viewed as a luxury that one cannot afford in times of financial downturn. Yet short-sighted cuts to the funding of basic science have potentially catastrophic consequences for long term prosperity. Of course it is essential that targeted research is performed to meet specific needs of society and industry, but history also shows that many of the most significant drivers of social and economic change have arisen unexpectedly from purely curiosity-driven objectives. It is vital to support basic research, and it is essential that scientists know how to effectively defend it. Understanding the background to this debate is more important than ever.

Models for research

Basic research can be defined as that performed to search for new fundamental laws of Nature, and applied research as that which seeks specific solutions to targeted problems by applying known fundamental results. The relative benefits of fundamental versus applied research became embedded in the political organization of science after the dramatic successes of scientists in developing military technologies during World War II. In 1944, President Roosevelt asked the then-director of the wartime Office of Scientific Research and Development, Vannevar Bush, to consider how government support for science could extend into the postwar years; Bush’s 1945 report on this subject *Science, the Endless Frontier* introduced a funding framework that has dominated thinking ever since [3,4].

Bush’s principle that “basic research is the pacemaker of all technological progress” was his starting point. He clearly recognized that research free of practical constraints was at the heart of technology and industry, and he developed a linear model of innovation as shown in Figure 1(a) to represent the foundational place of basic research in pushing technology forward.

This linear model has several problems, however. Firstly, to those who choose to interpret it superficially, it highlights not the driving nature of basic research, but only its apparent distance from industry and production. The problem with this interpretation is that, in time of crisis, basic research seems an easy target to cut at the expense of supporting activities that more immediately address industrial growth. Yet this is completely at odds with the whole point of the linear model. Bush’s own belief was that “the simplest and most effective way in which the Government can strengthen industrial research is to support basic research and to develop scientific talent.” Support for basic research is more important than ever during times of financial downturn because it provides precisely the impeteus needed to restimulate growth.

A second drawback is that the linear model implies that knowledge only flows one way, from basic research to technology and industry. But there are many counter examples: for example, the laws of thermodynamics were primarily derived from studying the operation of steam engines in the 19th century, and the science of surface chemistry developed from initial studies in industrial laboratories developing incandescent lamps [5, 6].

The simple use of Bush’s linear model has now generally been abandoned, but the change is relatively recent. It was only in 1997 that a clear alternative was presented. In *Pasteur's Quadrant – Basic Science and Technological Innovation*, former advisor to the US National Science Foundation Donald Stokes realized that mapping the aims of basic and applied research within a two-dimensional space led to a much more useful model of how research was often performed in practice [7]. Figure 1(b) shows the idea. Different types of research are represented by different quadrants in a plane defined by two axes, one oriented towards quest for fundamental understanding and the other towards consideration of practical use. Three of these quadrants are named after well-known figures. Niels Bohr gives his name to the quadrant of curiosity-driven fundamental research while Thomas Edison is associated with focused problem-solving for practical invention. The quadrant between these two is named after Louis Pasteur, whose fundamental contributions to microbiology were motivated by his desire to solve practical problems of the day such as disease. The fourth quadrant although unnamed is not empty, and may be considered perhaps to describe science at an early taxonomic stage, observing and classifying rather than trying to deeply understand or apply.

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Quadrant shortcomings

The quadrant model is an improvement on the linear model in that it shows how different styles of research co-exist and interact. Pasteur's quadrant appears especially attractive, illustrating search for fundamental knowledge, but where the questions and methods are either inspired by, or otherwise relevant to, real world problems. But the quadrant model minimizes the interface between fundamental research and industrial development, giving the misleading impression that it is research performed in Pasteur's quadrant that has the most impact on industry. And it is this that has led to the paradigm of use-inspired research that dominates current thinking.

Funding research in Pasteur's quadrant is seen to spread the risk with expectation that one cannot lose: money is spent to support research that progresses steadily towards targeted practical ends, but if there are bottlenecks which impede development, working towards solving them generates fundamental new knowledge. Many familiar features of the modern academic environment have developed with Pasteur's quadrant in mind: research projects are often only funded if there is industrial partnership, and most universities have entrepreneurial centers for technology transfer.

Ensuring that scientists are aware of the needs of society and encouraging spin-offs and entrepreneurship has many benefits, and many researchers and students anyway prefer to work on topics with clear industrial objectives. It does not follow, however, that focusing scientific ambition and funding at the academic-industry interface is actually in the best interests of creating the most revolutionary new technologies. There are many examples of discoveries of profound technological impact that have arisen from research considered obscure and of purely academic interest at the time it was carried out. Modern electronics, communications, GPS, information security, radiotherapy and the internet are obvious examples of revolutionary technologies whose origins lie in curiosity-driven studies far removed from their eventual applications. In the specific field of photonics, the development of the laser has been described by Nobel laureate Charles Townes in exactly these terms [8]: "What industrialist, looking for new cutting and welding devices, or what doctor, wanting a new surgical tool as the laser has turned out to be, would have urged the study of microwave spectroscopy? The whole field of quantum electronics is almost a textbook example of broadly applicable technology growing unexpectedly out of basic research."

So perhaps it is time to update the quadrant model altogether. Abandoning the squares and placing the three primary research sectors in a circle seems to me a much better approach. This could look like Figure 1(c) where all sectors share common boundaries. This is an important change, as it indicates that the results of fundamental research can drive industry and development directly without ever having being discovered following “use-inspired” objectives. To return to current concerns, I have also added a “funding axis” that has the benefit of highlighting that we cannot avoid the question of how much support should be given to each sector. In passing, note that the heights of the bars shown in this figure have been chosen based on funding scenarios that can be often found in current government programmes worldwide. Although basic research is not forgotten, the emphasis is on the use-inspired sector. An argument I want to push here is that these relative heights need to be revisited and interchanged.

Focus on fundamentals

Fundamental discoveries in physics and other disciplines are embedded within many of the technologies that we now take for granted, and they drive economic growth in both direct and indirect ways. Yet the commercial benefit from these discoveries has often appeared only many decades after the initial research was carried out. We must not become complacent with the tremendous advances of science in the last fifty years, but must continue to probe the knowledge boundaries of all disciplines. Existing theories need to be tested to their limits, both to provide answers to known questions, as well as to provide hints of new questions that need to be asked. History has clearly shown how fundamental science drives revolutions in technology, and we should be aggressive in stressing these benefits to policymakers. The technologies and practical benefits of science improve quality of life, and basic research is in the public good.

Arguments stressing practical applications and benefits are only part of the defence of basic science, however. Social, educational and cultural arguments can be equally persuasive [2]. It is often the case that the areas of science that attract most public interest are very far from down-to-earth technological aims. Exploring the universe with the Hubble telescope, probing the principles of quantum mechanics, and searching for new particles at the LHC are all examples of very curiosity-driven goals that have resonated tremendously with the general public.

There are excellent arguments to support different types of research and, as scientists, we need to understand them all. It is wrong to remain elitist and isolated from the needs of society, and there are many areas of applied research in areas such as healthcare and energy that need extensive effort for the benefit of both the developed and the developing world. Moreover, working with industry can provide tremendous benefit and also raise many new questions of fundamental importance.

But at the same time, we must strongly defend curiosity-driven enquiry, argue against excessive targeting towards specific goals, and argue for greater financial support for basic than for use-inspired research. Of course, supporting different kinds of research recognizes diversity in the choices of individuals, but it is important to ensure that researchers have opportunities to choose freely. Of course, this is not a problem unique to the present research environment, but success stories such the invention of the laser show how supporting scientists to follow avenues of basic enquiry can yield tremendous reward in unexpected ways.

We should remind policymakers of this history and correct the misconception that basic research is a luxury. In addition to stressing its practical benefit, we should not be afraid to defend science on the basis of its cultural and social benefit either. Scientists are most comfortable doing science of course, but we cannot afford to remain safely working in our laboratories whilst remaining silent on the very issues that allow us to do the basic research that we love. We must engage with and debate vigorously with policymakers. The arguments and examples are all there and well-known; we just need to use them.

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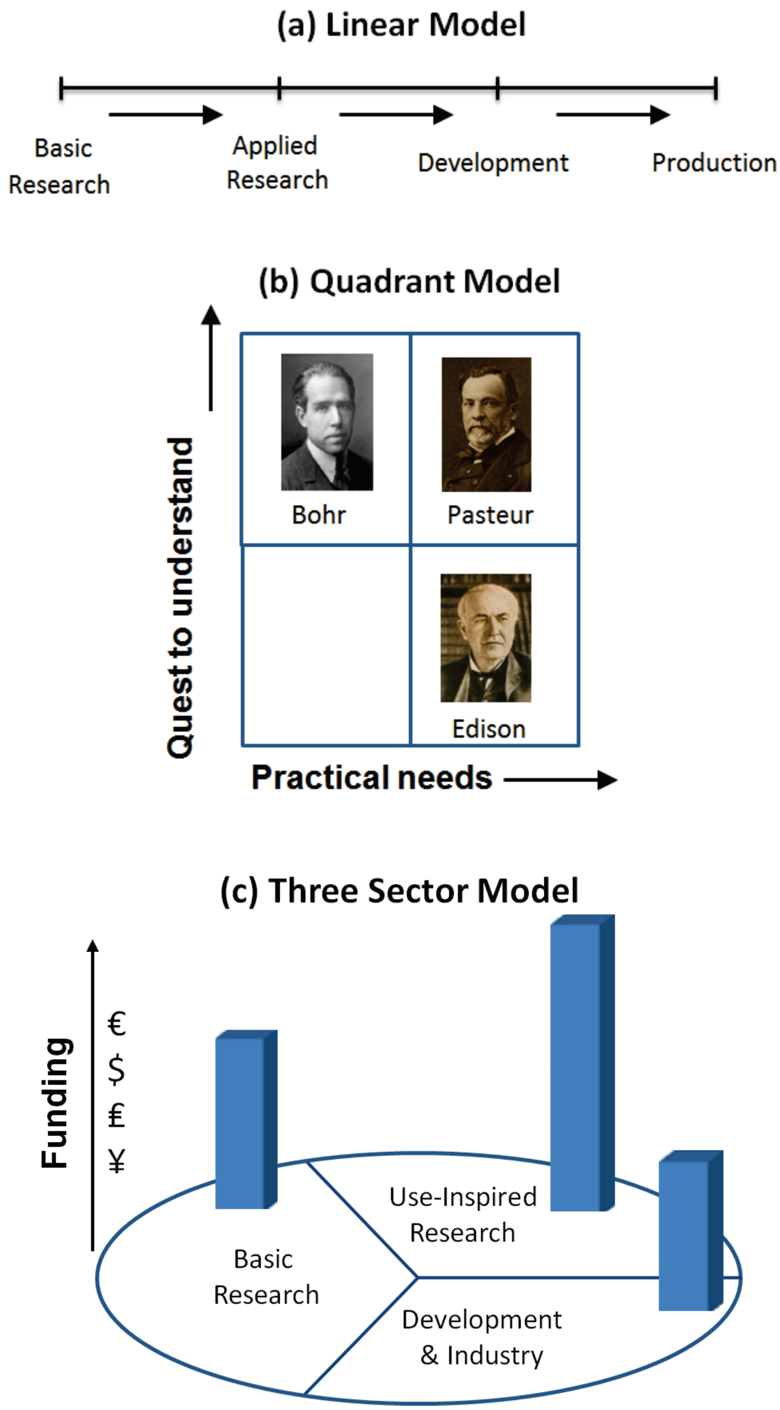

Figure 1: Three models for research: (a) Bush's linear model; (b) Stokes' s quadrant model; (c) an updated model showing three sectors with common boundaries, and funding bars.