commentary

Defending basic research

John M. Dudley

Governments are demanding more value for money from scientists, which is putting fundamental research under increasing pressure. Scientists should know how to champion it more effectively.

A recent editorial[1] in *Nature Photonics* asked whether scientists are still able to perform curiosity-driven research freely, or if there is an excessive emphasis on research driven by predetermined goals. Although this question may seem to be motivated by the current climate of financial austerity, the relative importance of basic and applied science is a very long-standing debate[2]. Moreover, current funding models used worldwide are based on ideas developed to support both kinds of research while also prioritizing economic growth.

However, many policymakers and research managers seem unaware of this background and hence basic science is often viewed as an unaffordable luxury in times of financial downturn. Yet short-sighted cuts to the funding of basic science can potentially have catastrophic consequences for long-term prosperity. Of course, it is essential that targeted research be performed to meet the specific needs of society and industry, but history shows that many of the most significant drivers of social and economic changes arose unexpectedly from purely curiosity-driven objectives. It is vital to support basic research, and it is essential that scientists know how to defend it effectively. Understanding the background to this debate is more important than ever.

Linear model

Basic research can be defined as that performed to search for new fundamental laws of nature, whereas applied research is that which seeks specific solutions to targeted problems by applying known fundamental results. The relative benefits of fundamental versus applied research have dominated discussions of science policy since the dramatic successes of scientists in developing military technologies during the Second World War. In 1944, President Roosevelt asked the then Director of the Office of Scientific Research and Development, Vannevar Bush, to consider how the government should support science after the war. Bush's 1945 report on this subject introduced a funding framework that has dominated thinking ever since[3,4].

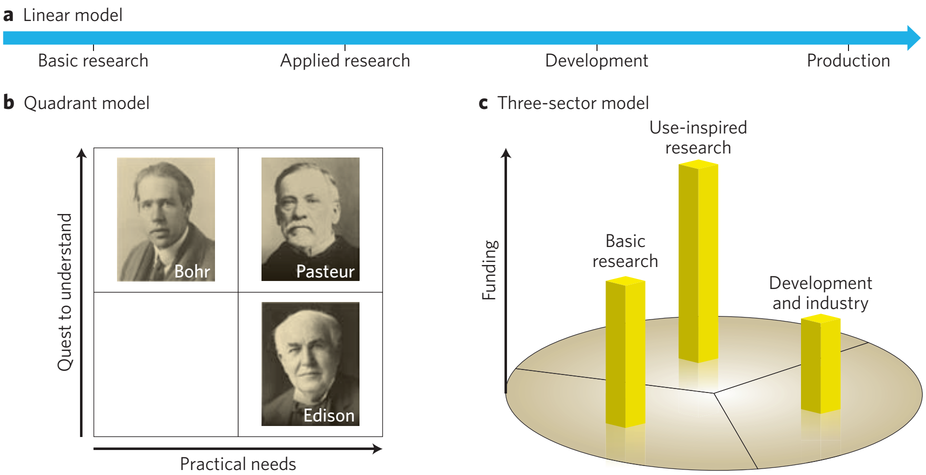

Figure 1 | Three models of research. **a**, Bush's linear model. **b**, Stokes's quadrant model. **c**, An updated model showing three sectors with common boundaries and funding bars. Photos from Niels Bohr Archive, AIP Emilio Segre Visual Archives (Bohr), AIP Emilio Segre Visual Archives (Pasteur) and Library of Congress by Bachrach (Edison).

Bush's starting point was the principle that "basic research is the pacemaker of all technological progress". 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 (Fig. 1a) to represent the foundational place of basic research in advancing technology.

This linear model has several problems, however. First, to those who interpret it superficially, it highlights not the driving impetus of basic research, but rather its apparent distance from industry and production. The problem with this interpretation is that it promotes the redirection of funding from basic research to activities that more immediately address industrial growth in times of crisis. 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 financial downturns because it provides precisely the impetus needed to restimulate growth. A second drawback of the linear model is that it implies that knowledge flows in only one direction: from basic research to technology and industry. However, there are many counter examples. For instance, the laws of thermodynamics were primarily derived from studying the operation of steam engines in the nineteenth century, and the science of surface chemistry emerged from initial studies in industrial laboratories developing incandescent lamps[5,6].

Quadrant model

The simple application of Bush's linear model has now generally been abandoned, but this change occurred relatively recently. It was only in 1997 that a clear alternative was presented. Donald Stokes, former advisor to the US National Science Foundation, realized that mapping the aims of basic and applied research in a two-dimensional space provides a much more useful model of how research is often performed in practice[7] (Fig. 1b). Different types of research are represented by different quadrants in a plane defined by two axes: one representing the quest for fundamental understanding and the other indicating the development of practical applications. Stokes named three of these quadrants after well-known scientists: the quadrant of curiosity-driven fundamental research is named after Niels Bohr, whereas Thomas Edison is associated with focused problem-solving for practical invention; the upper quadrant adjacent to these two is named after Louis Pasteur, whose fundamental contributions to microbiology were motivated by his desire to solve the practical concerns of the day, such as the treatment of disease. The unnamed fourth quadrant is not necessarily empty, but for simplicity we do not consider it further here.

The quadrant model is an improvement on the linear model as it indicates how different styles of research co-exist and interact. Pasteur's quadrant seems especially attractive: it represents the search for fundamental knowledge, but where the approach is either inspired by or is applicable to real-world problems. But the quadrant model minimizes the interface between fundamental research and industrial development, giving the misleading impression that research performed in Pasteur's quadrant has the greatest impact on industry. This erroneous impression has given rise to the paradigm of use-inspired research that dominates current thinking. Funding research in Pasteur's quadrant also seems to spread the risk with the expectation that one cannot lose: money is spent to support research that progresses steadily towards specific practical goals, but if there are bottlenecks that impede development, working towards solving them will generate new fundamental knowledge. Many familiar features of the modern academic environment have been developed based on Pasteur's quadrant: research projects are often funded only if there is industrial partnership, and most universities have entrepreneurial centres to promote technology transfer.

Ensuring that scientists are aware of the needs of society, and encouraging spin-offs and entrepreneurship have numerous benefits. Furthermore, many researchers and students prefer to work on topics with clear industrial objectives. It does not follow, however, that focusing scientific ambition and funding on the academic–industry interface best serves the creation of 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, the Global Positioning System (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 field of photonics, Nobel Laureate Charles Townes has described the development of the laser in the following manner[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."

Three-sector model

So perhaps it is time to update the quadrant model. Abandoning the squares and placing the three primary research sectors in a circle seems a much better approach. This could look like Fig. 1c, in which all three sectors share common boundaries. This is an important change, as it indicates that the results of fundamental research can directly drive industry and development. A typical 'funding axis' has also been included to reflect current economic concerns; this indicates that the question of how much support should be assigned to each sector is unavoidable. Although basic research has not been completely neglected, the current emphasis is on the use-inspired sector. The above arguments suggest that the relative heights of these two sectors need to be reconsidered.

Defending fundamentals

Fundamental discoveries in physics and other disciplines are incorporated in many of the technologies that we now take for granted, and they drive economic growth both directly and indirectly. Yet the commercial benefits of these discoveries often appear only many decades after the initial research. As scientists, we must not become complacent about the tremendous scientific advances of the past 50 years; rather, we should 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 and to suggest new questions that need to be asked. History clearly shows how fundamental science drives revolutions in technology, and we should aggressively stress these benefits to policymakers. Because the technologies and practical benefits generated by science improve the quality of life, basic research promotes the public good.

However, arguments stressing practical applications and benefits represent only one component of the defence of basic science. Social, educational and cultural arguments can be equally persuasive[2]. Many areas of science that excite the 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 using the Large Hadron Collider are all examples of very curiosity-driven goals that resonate with the general public.

There are excellent arguments to support the different types of research and, as scientists, we need to understand them all. It is not right to remain elitist and isolated from the needs of society. Undeniably, there are many areas of applied research in areas such as healthcare and energy that require extensive effort before they can benefit both the developed and the developing world. Moreover, working with industry can provide tremendous benefits and generate many new questions of fundamental importance. At the same time, we must strongly defend curiosity-driven research and argue against excessive targeting of specific goals. Of course, supporting different kinds of research recognizes the diversity in the choices of individuals, but it is important to ensure that researchers have opportunities to choose freely.

We must vigorously debate with policymakers, reminding them of history and correcting their misconception that basic research is a luxury. In addition to stressing its practical benefits, we should defend pure science based on its cultural and social benefits. Naturally, we are most comfortable doing science, but we cannot afford to remain safely working in our laboratories while remaining silent about the very issues that allow us to conduct the basic research that we love. The arguments and examples are all well known; we just need to use them. ❐

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