[EDITORIAL]

# The Challenge **of Communication**

**The coming years are full of challenges for physics and physicists. Researchers are under more and more pressure to provide value for money to governments, and funding models are evolving towards supporting specific technical goals of relevance to society and industry.**

Curiosity-driven research into the fundamentals is perceived as increasingly unaffordable. The need to focus on applied research and industrial concerns is understandable to address problems in areas such as telecommunications, climate change, sustainable energy, healthcare, agriculture and so on. Ensuring that there is a critical mass of technical effort in applied research fields is of course important.

Yet it is clear from history that many of the most pervasive technologies that we now benefit from have not arisen from target-driven research at all, but have developed from curiosity-driven directions with no link to their ultimate application. Using an example from my own field of optical physics, laser pioneer Charles Townes in his wonderful book *How the Laser Happened: Adventures of a Scientist* (Oxford, 2002) illustrated this beautifully by asking: "What research planner, wanting a more intense light, would have started by studying molecules with microwaves?" The laser is a clear example of the unpredictability of technology development from fundamental science. Indeed, whilst some of the applications of lasers such as industrial machining or perhaps even surgery might have been expected as a practical use of a bright light source, who would possibly have anticipated the use of lasers as a critical component of audio products?

There are many similar success stories that show how basic research in science has led to dramatic and unexpected benefits to society. As scientists, we recognize these achievements, but we are also motivated in our research by the belief that the creation of new knowledge provides intrinsically valuable insight into the physical world. We need no further convincing.

Yet the recent pressure being placed on fundamental research support suggests that we are clearly not effectively explaining its benefits. So we must try harder to ensure that the importance of physics and its central place in education and research is clearly communicated to policymakers.

But this is not easy. Our training as scientists does not necessarily prepare us for the environment of vigorous debate that is needed to interact in a political context. We often prefer to remain in our familiar research environments rather than spend the necessary days and weeks in committees and on boards.

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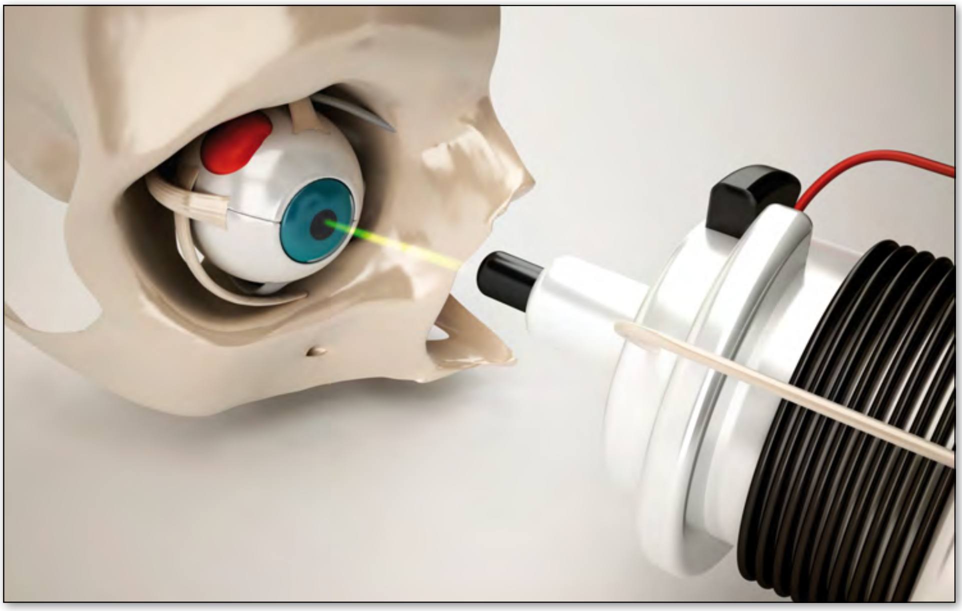
Yet it is essential that we leave the comfort of our laboratories and argue effectively for our research. This aspect of communication with decision-makers now forms an essential part of our educational mission.

Pressure on basic research is strongly felt worldwide, and is not just a problem for physics, but for all of science. It is here that EPS can play a central role in coordinating efforts between different national societies, and by acting together with professional societies in different fields.

The problem is of concern for all scientists. Participating in the activities of professional and learned societies on a national and international level is more important than ever as we work together to solve it. ■

**John Dudley**
*President of the EPS*



▼ Eye surgery, an example of laser applications ©iStockPhoto

[EDITORIAL]

# Surviving in Science

**Like most scientists, I have greatly benefited from interactions with colleagues from many different countries. I have also been lucky to participate in several travelling lecturer programmes set up to support seminars for students and early-career researchers. This has allowed me to meet many young researchers working in very different environments: from small groups operating on a shoestring in developing countries, to students working with Nobel Prize Winners in major national labs.**

Irrespective of geography or environment, however, it is immediately apparent that the same two questions preoccupy the vast majority of young physicists. Firstly, how do I obtain a permanent job? Secondly, having got one, how do I survive as a scientist in the long term?

Of course, it has never been easy to obtain a permanent position in physics, and research and teaching are extremely demanding. But everything does seem to be harder for the current generation of young scientists: there is a real scarcity of openings relative to the number of applicants, and there is increasing pressure for young scientists to take on administrative responsibilities early in their careers. Students are keen observers, and as they watch their supervisors work in the modern research environment, it is clear that they will naturally ask serious questions about the best way to navigate their own future careers.

These concerns are extremely important, but it is not often that they are addressed head on. However, as scientists we are also educators, and so we should not hesitate to actively provide advice on careers as well as on physics! Obtaining a PhD is of course an important and significant achievement, but it is really only the start! A successful career in research requires many other skills: from an appreciation of the politics of science, to writing and communication, to management and leadership.

When starting out, the breadth of this required expertise can seem daunting, but it is very easy to identify topics where simple and practical advice can help young researchers to build and enjoy a long-term career in physics. There is no shortage of helpful resources available, and assembling this material into a half day format of seminars and talks and exchanges is not only straightforward but is in fact a great deal of fun! I have been organizer, speaker and a member of the audience at many events of this kind, and I have seen at first hand the tremendous benefit that they provide.

There are many important points to make during such an event, but my own favourite is the importance of developing a collaborative spirit. Physicists work in an environment where we constantly challenge and test each other's ideas, and this places great demands on collaborations. I like to stress the need to successfully maintain working relationships with colleagues who are at times co-workers, at times competitors, and at times employers! Learning how to manage long-term collaborations is in my view one of the most important skills that we can discuss with young scientists. Learning how to effectively share ideas is essential to stimulate new discoveries. And this, after all, is what we are all aiming at. ■

**Learning how to manage long-term collaborations is in my view one of the most important skills**

**John Dudley**
*President of the EPS*

▼ A successful career in science involves maintaining long term collaborations

[EDITORIAL]

# A Brussels voice for physics

**The European Physical Society recently held its 2014 Council at the International Centre for Theoretical Physics (ICTP) in Trieste. Councils are always busy times with many reports of the preceding year to look over and endorse, but the 2014 Council had a very forward looking agenda, as we focussed on a number of future developments: plans for the International Year of Light in 2015; the approval of an ambitious project to establish an EPS Supplementary Secretariat in Brussels; and the election of a new President-Elect!**

Let me begin by offering my heartiest and sincere congratulations to Christophe Rossel who was elected to serve as the next EPS President from April 2015. Christophe has made pioneering contributions to the fields of experimental superconductivity and magnetism, and his career as a scientist working at IBM in Zurich will bring a unique perspective to how to better engage EPS with industry. Chris has worked for and supported EPS in many ways over many years, and his experience will be invaluable as we move forward to address many new challenges in the years to come.

One particular challenge is of course how to improve the broader visibility of physics, since this is crucial for the future of our discipline, and is vital in order that we continue to attract the best young minds to study physics at schools and universities. Of course this problem is well-known, and has been discussed within EPS for many years. In 2010 when the EPS Strategy Plan 2010+ was crafted, an important element of a solution to this problem was identified as an effective presence in Brussels to engage with decision-makers and other political stakeholders. Now, after detailed planning over the last twelve months, Council approved the project and business plans that will allow EPS to open a Supplementary secretariat to carry out this essential mission.

Providing a clear voice for physics to the European Commission will of course be the primary aim of the Brussels secretariat. By linking with other professional and scientific bodies in Brussels, as well as key players from the Commission including research, innovation, industry, and entrepreneurship, EPS will promote the need for continued investment in physics as a discipline, and recognition of the central role of physics in underpinning many areas of technology. A presence in Brussels will also ensure more efficient communication of calls and deadlines to all EPS members, and advance notice of policy initiatives on the European level that may have influence on national trends as well.

**Providing a clear voice for physics to the European Commission will of course be the primary aim of the Brussels secretariat.**

The next 18 months are really a unique chance for physics, with the extra and unique visibility that physics will have naturally during the International Year of Light, the first phases of Horizon 2020, and the natural momentum from the opening of the Brussels secretariat. EPS staff in Brussels will aim to be at the service of all EPS member societies and members in many ways such as supporting meeting organisations, arranging introductions, and facilitating partner searches for projects. But an effective voice of EPS in Brussels, however, must be the voice of all EPS members. As with all of EPS's activities, it is up to you to get involved! ■

■ **John Dudley**
*President of the EPS*

[EDITORIAL]

# Lessons from a Giant

**A quotation well-known to physicists is Newton's acknowledgement of the debt he owed to others in his work: 'if I have seen further it is by standing on the shoulders of giants'. We in the global physics community have lost our own contemporary giant on January 27 with the death aged 99 of Charles Townes, whose work on the maser and laser essentially opened up the fields of modern optical and atomic physics and their many associated applications.**

The story of Townes's life and his discovery of the principles of maser operation is well-known. He shared the Nobel Prize in Physics in 1964 for his work on maser and laser devices and from 1966 until 1970, he chaired NASA's Science Advisory Committee for the Apollo Lunar Landing programme. He also was involved in many other committees and organisations, served as University Provost and was also past-president of the American Physical Society. His commitment to and enthusiasm for science was lifelong, and he continued to carry out research actively after his 'retirement', in fact publishing as first author a paper describing stellar dust distributions at the age of 96! His writings and works have had very broad impact in many other areas of science, technology and policy.

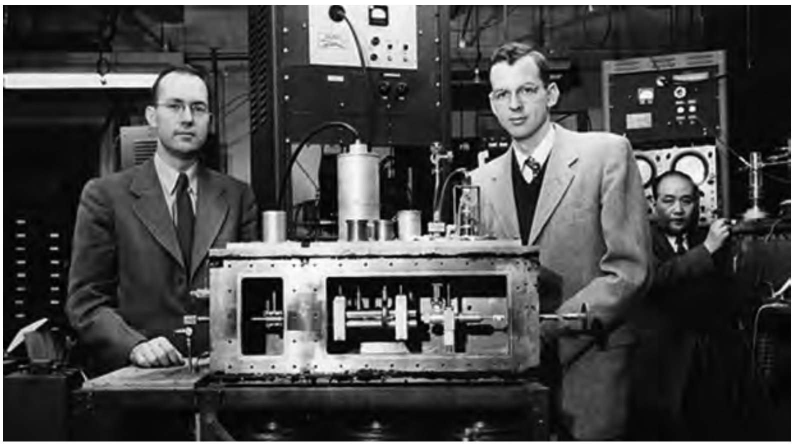
What prompts me especially to write about Charles Townes in this first editorial of 2015 is the fact that his example is of so much relevance to us all as EPS both celebrates the International Year of Light and begins a strategic programme towards influencing science policy in Europe. Charles Townes certainly saw the many reinforcing and positive links between basic science and engineering, but he was a very strong advocate for improving understanding amongst the public and politicians of the economic impact of long term basic research. In an interview appearing in a 2013 production for South Carolina ETV, he says clearly: 'Politicians can't support science so strongly because it isn't going to pay off immediately. It pays off many years later. The laser is now billions of dollars of business, but it's been about 50 years…" This is a message that EPS along with many other partners has been struggling with little success to have heard by decision-makers for years now, but we must be persistent and keep trying.

**Politicians can't support science so strongly because it isn't going to pay off immediately. It pays off many years later.**

I am approaching now the end of my mandate as EPS President, and although I have been very strongly supported by many committed scientists who volunteer their time to EPS and other societies, or who work on policy and other government committees, I am convinced more than ever that the number of physicists who take on the task to spread this message is far too small. Many more of us need to get involved, and this was something I tried to convey to a broader audience when I had the honour to speak at UNESCO Headquarters on the 19 January at the Opening Ceremony of the International Year of Light. Given the particular theme of light science being celebrated, I did not hesitate to again quote Charles Townes who wrote in his book *How the Laser Happened: Adventures of a Scientist* that he '...always felt that scientists should provide public service from time to time.'

It is of great concern that at the same time that science becomes more and more essential to the running of modern society, it is understood by the public less and less. Having in 2015 an International Year on a science theme provides a wonderful lever for us to promote the importance of physics and science in general, but it really is only the start. In my view, one of the most important outcomes of the next year must be to identify more of us within the community of physicists who will accept the lesson of Charles Townes, and who will take up the challenges ahead. ■

**John Dudley**
*President of the EPS*



▸ Charles Townes and James Gordon with their second MASER device, 1955. In the background T.C. Wang stands beside the first maser. Via www.aip.org.

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.

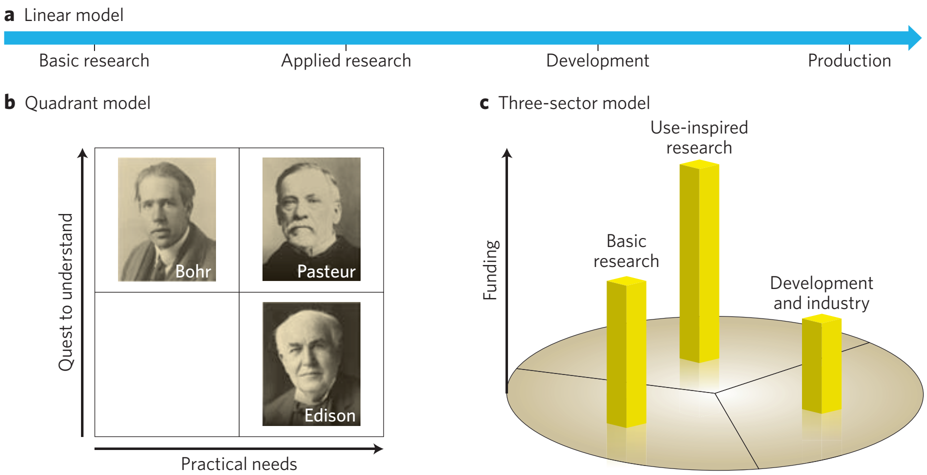



**Figure 1 |** Three models of research. **a**, Bush's linear model. **b**, Stokes's quadrant model. **c**, An updated model showing three sectors with common boundaries and funding bars. Photos from Niels Bohr Archive, AIP Emilio Segre Visual Archives (Bohr), AIP Emilio Segre Visual Archives (Pasteur)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

## 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].

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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