On the usefulness of useless knowledge

Helmut Schwarz[1,2]

Basic research may or may not lead to new technologies. Our safest bet in this gamble is to give our brightest minds the funding and freedom to dream big.

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Most breakthroughs in research are not and could not be planned. Rather, they appear, like Puck, in entirely unexpected corners. Because it is the passion of individuals that sparks major discoveries or inventions, choosing outstanding people and providing intellectual freedom and generous funding are key to the success of academic institutions.

Abraham Flexner, the founding Secretary General of the Institute for Advanced Study in Princeton, penned in November 1939 a most readable essay on fundamental research[1]. *The Usefulness of Useless Knowledge* described, in Flexner's fluid prose, how apparently random experimentation eventually leads to the most important discoveries. He argued vehemently against the need for utility in the promotion of research and the allocation of funding. Instead, Flexner delivered a rousing plea for the "freeing of the human spirit". His article is an eloquent discourse on the benefits and virtues of freedom in fundamental research. Flexner's words are music to the ears of scientists who pursue science because they are curious and, in the venerable words of Friedrich Schiller, do not live off science but, above all, for science. Although Flexner's essay appeared more than 75 years ago, it is still one of the most compelling pieces on the vital role of fundamental research — extolling not only its cultural value, but also its benefit to mankind in general.

The role of individuals

Fundamental research itself thrives on the desire of scientists to explore new territory — *terra incognita* — to discover the truly unknown, and to investigate and explain and, perhaps, to apply it. This is usually a protracted process fraught with setbacks. It turns out, time and again, that breakthroughs in research cannot be planned and that their value and usefulness are initially unknown or difficult to assess. Although science, like a mosaic, is made of the contributions of many, crucial breakthroughs are nearly always based on the achievements and passion of individuals, contingent on a combination of creativity, intelligence, curiosity, perseverance and serendipity. These individuals need space, freedom and trust to be able to develop to their full potential. Only then will they have the courage to take the risks that make discoveries possible. To foster the development of researchers, they should not be restricted by shortsighted timelines and narrowly defined objectives, but should instead be provided with funding continuity. This is indeed the demand at the heart of Trust Researchers, a manifesto signed by over 13,000 scientists and submitted to European institutions and parliaments.

Fundamental research

New knowledge is generated when researchers are driven by the desire to understand, a desire that can only be acted on when intellectual pursuit is liberated from the constraints of concrete applications and tightly set targets. "Knowledge ought to precede application," stated Max Planck[2] amidst the depressing atmosphere hovering over Europe in the wake of the First World War, and it has remained the precept on which the Max-Planck-Gesellschaft (MPG) operates to this very day. Along with generous public funding, their focus on basic research, the uncompromising pursuit of excellence and hiring the very best, has put the MPG on par with the best and most prestigious research institutions in the world. No fewer than eighteen Nobel Laureates have emerged from the ranks of its scientific members since 1948. Similar success stories include the Laboratory of Molecular Biology in Cambridge, UK, as well as the AT&T Bell Laboratories and US National Institutes of Health, the latter two having blossomed in the mid-1960s. The common thread is that all of these institutions were based on the principle of excellence.

Although curiosity-driven research is the engine that eventually generates innovation, at the outset such research entails travelling a long and winding road, swimming against the current and overcoming hurdles with no shortcuts in sight. Basic research is time consuming and expensive. It is rarely easy for scientists to explain this to the general public, legislators or ministerial decision makers. Actually, in many countries, basic research has come under pressure to justify itself: it is endangered and has become vulnerable.

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Today's prevailing policy is to measure the value of research solely on whether it is 'useful', whether the research objective addresses a social problem or whether it is possible to deliver a marketable product in the foreseeable future, preferably within a few years. All of this has prompted, for example, many leading academics in Britain to establish a Council for the Defense of British Universities in the hope of reinstating an ethos and an intellectual atmosphere that in the past were key to the success of institutions of higher learning[3]. Clearly, if one cuts out the node of basic research from a network also comprising applied research and business, and if one then assumes that in this network the path from one node to another may be anything but linear, the network as a whole is imperilled. Thus, fundamental research is not only absolutely necessary but also very much the most cost-effective public good[4].

From basic research to innovation

As amply evidenced, we all know how infinitely less secure and comfortable our lives would be without things that society takes for granted and which originated from basic research. For example, although Albert Einstein's esoteric theory of general relativity is seemingly irrelevant to any practical application, there would be no GPS devices without it. It was the apparently random 'trials' that led to Röntgen's discovery of X-rays, and a curiosity-driven exercise par excellence that allowed Watson and Crick to elucidate the structure of the DNA double helix — a finding that has revolutionized the life sciences as a whole. Remarkably, the first lasers were described as a 'solution in search of a problem'. In a similar vein, Paul Dirac's 1927 prediction of antimatter (such as the positron) was regarded at the time as an entirely useless oddity with little, if any, practical significance. Now, decades later, almost every major hospital uses positron emission tomography for the early diagnosis of cancer. Consider Michael Faraday's ground-breaking work on the riddle of electricity and magnetism. Without his scientific interest in electromagnetism, which was seen as an amusing but largely useless diversion in the first half of the 19th century, we would all still be in the dark! When asked by William Gladstone, the British Prime Minister of the day, whether his publicly funded research on electricity would ever be of any use, Faraday coolly replied, "One day, Sir, you may tax it". Although Gladstone himself did not live to see the rise of the electrical industry or benefit from the tax revenue it generated, the salient point is that returns on research investment are long-term and can be massive: up to 60% per year according to recent estimates[5]. In another example, approximately 20% of the global economy is related in some way or another to chemical catalysis: that is, to the initially purely academic question of how bonds in molecules are made and broken. Also, does Schrödinger's famous equation from 1926 (equation 1), which was formulated to describe the wave nature of electrons and represents basic theoretical physics research in its purest form, serve any use? According to estimates that about 20% of gross domestic product is based on applications of quantum mechanics, the answer would be a resounding 'yes' (REF. 5). Indeed, the list is endless. So let us be in no doubt: basic research that, sooner or later, enables further discoveries or inventions and spawns applications that benefit society is a public good. As far as funding is concerned, it must remain an essentially public matter; it must not be subjected to economic rules of quick profit maximization or politically opportunistic considerations.

$$i\hbar \frac{\partial}{\partial t}\Psi(\boldsymbol{r},t) = \hat{H}\Psi(\boldsymbol{r},t) \quad (1)$$

The role of funding organizations

With the words "Innovation im Konsens ist Nonsens" ("consensus innovation is nonsense"), the German innovator Erich Staudt recognized that novelty arises only when well-trodden paths are eschewed in favour of daring to examine new possibilities. Staudt claimed that projects solely motivated by technological outcomes are less likely to succeed. In fact, he advocated a strategy not focused on projects at all, but rather on people and their expertise. It is always individuals or minorities, Staudt noticed, who spark true innovation by taking risks and spearheading change. Many such daring ideas fall flat, but some will succeed.

In the experience of the Alexander von Humboldt Stiftung, generously sponsored sustainable support for individuals is the best method to provide scope for risks and changes. This Foundation has its own, rather special profile: its sponsorship principles have remained unchanged ever since its establishment more than 60 years ago. These principles have withstood the test of time and are quite simply to support people, not projects; to look for the very best; to not believe in quotas — not for countries, disciplines, gender or age; and to not get involved in short-term project funding dictated by the fashion of the day. We promote people — individuals — and have very good evidence to suggest that this is indeed the best policy to support research.

With our various programs we try hard to offer scientists, in particular the young ones, a chance to realize their dreams. Ultimately, society's dreams and aspirations are dependent on how their young people develop, and what space and creative opportunities they are afforded. Are they able to develop freely, unhindered in their implementation of ideas and able to live up to their potential? In the context of science this is equivalent to asking: how are young researchers faring? Young scientists and scholars need space to pursue their quest for knowledge and to test themselves and their ideas — too much control and too many restrictions are fatal. After all, young people are particularly full of enthusiasm, a passion that can blossom into a genuine love of research, not least basic research. Only in this way will their abilities fully unfold. These young talents need to be nurtured in creative environments; judging them and their potential by bibliometric data or the amount of research money they have managed to raise is mindless, because such numerical indicators do not capture the creativity and originality of a person's work. We have to ask ourselves whether presently applied criteria for hiring academics and granting them tenure would be conducive to attracting a young Maxwell, Fischer, Pauling,

> "the enthusiasm of young people is the most secure currency we have"

Crick or Heisenberg to pursue a career in academia. Would a Fred Sanger survive today's world of science with an overload of continuous reporting and a scientific record that consists of only a handful of publications?

Guiding and supporting, creating space, respecting and strengthening personalities, and sharing personal networks — these are all essential, interconnected elements of a serious approach to promoting young researchers. Such support should never be considered a luxury that senior academics might furnish provided they have the time outside of their demanding university routines. An attitude like this, if prevalent, would be truly fatal because it would endanger not only the future of science, but also our concept of universities as places in which we can and must consider issues that may only prove their relevance decades from now. Fundamental research gives young people the opportunity to head for new shores. In order to master the future together, the enthusiasm of young people is the most secure currency we have.

Basic research — like writing an opera — is first of all a cultural undertaking, not a luxury, and certainly essential. Moreover, the promotion of research requires patience and perseverance. The principle of trusting people rather than trusting a monitoring system based on distrust has proved its worth, and the principle of funding people rather than projects has withstood the test of time. All of these ideas have been summarized in a most convincing way by Vannevar Bush, the scientific adviser to President Franklin D. Roosevelt, who was behind the creation of the US National Science Foundation. As Bush simply put, "Scientific progress on a broad front results from the free play of free intellects working on subjects of their own choice, in the manner dictated by their curiosity for exploration of the unknown" (REF. 6).

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Competing interests statement
The author declares no competing interests.

FURTHER INFORMATION
Alexander von Humboldt Foundation: https://humboldt-foundation.de/web/home.html
Community Research and Development Information Service (CORDIS): http://cordis.europa.eu/news/rcn/31775_en.html
Council for the Defence of British Universities: http://cdbu.org.uk
National Science Foundation: https://nsf.gov

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Web summary
Fundamental research is critical to the advancement of society. Such research is promoted by generously supporting talented inviduals to undertake ambitious work. While technology may spawn from these endeavours, it should never be the sole motivation for blue sky research.

Subject categories
policy, funding