WHO SHOULD DECIDE HOW RESEARCH FUNDING IS SPENT?

Susan L. Svane Stipp

Financial support for universities and government research institutions comes mostly from taxpayers. Funding for research projects comes from taxpayers, private foundations and industry. The purpose of research is to find answers to questions of importance to society, to industry, or to the researcher him/herself. Who should decide which research questions are most deserving of funding?

Currently, much of the decision comes from those who provide the income. Students pay tuition, or governments provide base funding according to the number of students admitted or graduated. The students choose their direction of study and the funding goes to those departments. This controls the number of staff hired to teach them and defines the weighting of the university's academic positions, which defines the size of the various departments, which then defines the research discipline weighting within the university. Thus, the researcher expertise of a country is decided, to a greater or lesser extent depending on its student quota scheme, by the interests of the country's adolescent population.

Some universities receive base funding for research, but most rely on research grants won in open competition. Project duration is typically up to three years. A grant may provide salary, but these positions are, by nature, temporary. Contract funding does not build a stable expertise base. Rather, it promotes stars that are soon out of a job; the research centre is forced to close as the next round of centres is funded and the stars flicker and fade, or leave for permanent positions in another country. So the decision about what research questions are answered depends on the calls for proposals from grant providers.

Internationally, the trend in the past decade or two has been to focus on industry-relevant research. Funding agencies have selected some topics, called them strategic, and assigned much of the research budget to them. Much less support is then available for free research, limiting the possibility to follow up on ideas that may not necessarily lead to a saleable product but that always add to the libraries of background information. Who in the funding agencies decide which topics should be prioritised? Sometimes it is panel of experts who are selected based on their broad background and insight into where research is going. But often decisions are made by politicians with no scientific expertise or insight.

An example is the current prime minister of Denmark, Anders Fogh Rasmussen. In his first New Year's address (2002), he stated, "We do not need experts and sample tasters to decide on our behalf. There is a tendency for expert-tyranny, which risks to crush public debate. The people should not accept a lifted, pointing finger from so-called experts that think they know best. Experts can be good enough to pass on factual knowledge. But when we should make a personal choice, we are all experts. The government will remove superfluous councils, boards and institutions." In the following months, there was massive closure of government research institutions and cutbacks in research funding; a few years later, the remaining institutions were merged with universities, and massive cuts in infrastructure funding continue.

In 2007, my university department fired or retired 7 people out of about 80, and in 2008 we lost 14 more. These events predated and were unrelated to current economic instabilities. Funds for free research have been moved to specific topics of a "development" nature, and the concept "From idea to invoice" underlies all. Who decides what is strategic? The Danish Minister of Science, Technology and Development has stated that decisions on how to spend the taxpayers' money are certainly too important to leave to scientists. Are scientists not in a position to see where the leading edge is? And to suggest what developments might be possible?

> "If H. C. Ørsted, who discovered electromagnetism in 1820, had lived under the Strategic Research Council, he would have instead developed an improvement of the candle."
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> –Ben Mottelson, Danish Physicist, Nobel Prize 1975, in a recent interview

It is likely that many of you will recognise at least some aspects of these attitudes in your own country. It is fine to encourage development of products and processes that can be converted quickly from an idea to something that society wants to buy, but very often, ideas spring out of a base of general understanding that is built up from fundamental research over many years. Companies realise that it can take one or two decades to develop a product from a good idea. A recent call for proposals from the energy company BP stated, "Past experience has shown … that it can take 10 years or more for a new idea … to move from the research stage to deployment in the field." Is the best way to ensure innovation (defined as clever new products or processes) to earmark funding for specific themes? Or is it most important to gather fundamental understanding about how the world works, so it is available to serve as a source of ideas, and tools to develop them, as one would draw from a library or a bank?

The World Wide Web is a good example. It has just celebrated its 20th birthday. This invention, which sparked the information age and which has changed forever the way people interact, did

Cont'd on page 79

IAGC ELECTS FIRST TWO FELLOWS

The International Association of GeoChemistry (IAGC) has established six permanent awards: the Vernadsky Medal, the Ebelman Award, the IAGC Distinguished Service Award, the Hitchon Award, the Faure Award, and the IAGC Certificate of Recognition. In addition, in 2008, the IAGC Council decided to bestow the honor of Fellow on its most deserving members and is pleased to announce the selection of the first two IAGC Fellows: Thure Cerling and Gunter Faure.

THURE CERLING received his BS degree in geology and chemistry from Iowa State University, followed by an MS in geology from Iowa State University, and a PhD in geology from the University of California, Berkeley, in 1977. Over the course of his career, Thure has applied the tools of geochemistry, stable isotopes, and cosmogenic nuclides to study processes occurring near the Earth's surface and the geological record of ecological change. His research includes the isotope physiology and diets of modern mammals, as well as the history of diets of various mammalian lineages extending over millions of years. He is also interested in the geology of Old World paleoanthropologic sites. He applies isotope systematics to the study of modern soils, lakes, and ecosystems to understand the evolution of ecosystems, monsoons, and the atmosphere over geological time scales. Using cosmogenic nuclides, his research group studies the evolution of landscapes over the last 1 million years. He is also involved in environmental studies, including the use of tritium and helium as hydrological tracers, and investigations of contaminant migration in surface waters or groundwaters.

GUNTER FAURE received his BS degree from the University of Western Ontario (Canada) and then a PhD from the Massachusetts Institute of Technology (USA). Following postdoctoral work at MIT, Gunter accepted a position on the faculty of the Department of Geology and Mineralogy of the Ohio State University in Columbus and taught there until his retirement in 2002. Throughout his academic career, Gunter distilled the content of his courses into textbooks on the isotope geology of strontium, low-temperature aqueous geochemistry, the petrogenesis of igneous rocks, isotopes and their applications in the Earth sciences, and planetary science. He became a member of the Board of Directors of the Geochemical Society and served from 1989 to 1997 as the executive editor of *Geochimica et Cosmochimica Acta*. He joined the IAGC in 1968 and was elected as its vice-president in 1992 and, four years later, he assumed the presidency. At the end of his term in 2000, Gunter became the past president and newsletter editor until 2004, when he took over from David Long as treasurer. Above all, Gunter is at heart an Antarctic field geologist who believes that geochemists have an obligation to sound the alarm when human activities begin to threaten our well-being on the Earth.

HOWIE RECEIVES HONORARY DOCTORATE

Prof. R. A. Howie, of the Mineralogical Society of Great Britain & Ireland, recently received an honorary doctorate from Derby University, UK.

EDITORIAL *(Cont'd from page 75)*

not come about because a group of politicians set aside a pot of research money to encourage a new method of communication. It came from a good idea of Tim Berners-Lee to facilitate information management and from a pot of money for free research within CERN, the European Center for Nuclear Research. That funding has paid a big dividend to society worldwide. There are many such examples.

Support for fundamental research has been grim in the European Union and in many other countries during the past years. Aging university infrastructure and dwindling research funding have driven many academics to industry jobs – which is good for industry but not good for producing the next generation of employees. In fact, many companies are deeply concerned about the lacking pool of qualified graduates. But there is hope. In Europe, the 2002 Barcelona agreement deadline looms. By 2010, 3% of the GNP (gross national product) should be spent on R & D (research and development). And in the United States, the winds of change have refreshed political views on the value of research funding and the important contribution that scientific experts can make in advisory roles. Internationally, we look forward to renewed vigour in American research activity.

The Rector of the University of Copenhagen, Ralf Hemmingsen, recently said that leading-edge research cannot be made in dilapidated buildings with old equipment. "One cannot win this year's Grand Prix with a Model T Ford." If society is going to solve some of the major problems in environment, health and sustainability, and add to the banks of understanding, governments will need to take some bold steps: (1) to provide stable funding for buildings, infrastructure, and technical and academic staff, so teaching and research programs are not at the mercy of yearly fluctuations in student numbers and grant success rates, and (2) to establish a balanced program of research funding that allows a good proportion of free research to balance strategic or topic-focussed programs.

Development and research are fundamentally different. Development has a clear and describable goal. It is driven by the need for profit. Success is predictable. One expects an outcome before one starts, and the only unknowns are how long it will take and how much it will cost. Industry is very good at development, and that is where it belongs. Research is unpredictable. It is curiosity driven. One has a specific research question at the outset, but the results one gets are often unexpected, and it is the interpretation of unexpected results that leads to quantum breakthroughs in thought and understanding. Without a solid base of fundamental understanding, interpretation of unexpected results is hindered or prevented. Not all research projects result in quantum breakthroughs, but the more free research funding is available, the more chance there is. Research is the domain of universities. Ben R. Mottelson, who shared the Nobel Prize in Physics in 1975, concisely expressed this concept in a recent interview: "If H. C. Ørsted [a famous Danish scientist], who discovered electromagnetism in 1820, had lived under the Strategic Research Council, he would have instead developed an improvement of the candle."

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