Promoting the Planck Club

How defiant youth, irreverent researchers and liberated universities can foster prosperity indefinitely

Donald W. Braben

WILEY

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To Bill, Margie, and Ken, and to the memory of Jean

Major breakthroughs in science invariably involve the amalgamation of a kaleidoscope of disparate research studies making the development of any rational strategies a futile exercise. There are as many ways to do outstanding science as there are outstanding scientists. Research often starts off in a specific direction but, as results, unfold new avenues open up. Discoveries that appear to arrive from "left field" litter the history of the sciences and serve as ubiquitously unheeded warnings to those who think they know how research should be carried out and what science is important. In this crucially important book, Don Braben has assembled an overwhelming case based on a plethora of historically significant scientific breakthroughs. He shows how foolhardy and, in fact, dangerous for the economy are the present research funding strategies, which focus primarily on "impact" when it is blatantly obvious that, as far as fundamental science is concerned, "impact" is impossible to assess before a fundamental advance has been made.

I only hope that the people who presently control research funding are prepared to read this book, think carefully, and heed the advice.

Harry Kroto, The Florida State University, Nobel Laureate

Don Braben's sobering book is right on the mark regarding the current disastrous path of funding of scientific research. Funding agencies are increasingly making decisions based on the proposed research's perceived impact and benefit for society. As Braben documents so well, the emphasis on short-term performance cannot lead to scientific revolutions such as Rutherford's discovery of the nucleus and Townes' invention of the laser. Scientists now eschew risky proposals, knowing that someone on a review panel will say the work is "impossible." Even when scientists are able to secure funding, much of their time is sapped by the increased paperwork, such as frequent reports on how "benchmarks" are being achieved. If a scientist dares to spend a few years developing a novel idea, his or her funding will be lost because of the "lack of productivity." Braben proposes an approach to turn the tide of preoccupation on short-term performance: each funding agency could set aside a small portion of its budget to fund non-peer-reviewed proposals. Braben illustrates how this could work using as a model the Venture Research Program he directed in the 1980s. One can hope that Braben's model will be widely adopted—it could change the landscape of science in future decades.

Harry L. Swinney, University of Texas at Austin,
Member of the US National Academy of Sciences

Funding agencies and policy-makers should emulate Don Braben's clear thinking, straight talking, wise values, broad learning, and acuity of insight. They might then liberate science, embolden innovation, and inspire academics in a more rational, prosperous, and interesting world.

Felipe Fernández-Armesto, University of Notre Dame, Indiana

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Foreword

In this provocative book, Donald Braben presents compelling data, cogent analysis, and vivid historical episodes tracing the immense economic and social impact of frontier scientific research. He focuses on revolutionary discoveries that emerged from decidedly unorthodox "outlier" work of a relatively few scientists. Those pioneers he designates as the "Planck Club." The name is apt: Max Planck, when early in the twentieth century, confronted with experimental results inexplicable by well-established physics, reluctantly advanced an iconoclastic idea. After gestation for more than two decades, his idea gave birth to quantum mechanics, which profoundly transformed understanding of the nature of light and matter and produced a myriad of technologies.

As in two sibling studies published by Wiley (Braben 2004 and 2008), Braben himself has emulated Planck. Armed with strong evidence, Braben has forthrightly challenged the now well-established and pervasive procedures for assessing and granting support for scientific research. These policies, based on "peer review" (actually, "preview" as Braben emphasizes) have evolved over decades. Well-intended, but in many respects deeply flawed, the procedures imposed have increasingly dire consequences.

Many scientists share Braben's deep concern that prospects for support of future work of Planck Club caliber are becoming severely limited. This case was made starkly by the late Luis Alvarez, assuredly a Planck Club member. In his autobiography (*Adventures of a Physicist*, 1987), he wrote:

> In my considered opinion, the peer review system, in which proposals rather than proposers are reviewed, is the greatest disaster to be visited upon the scientific community in this century. . . . I believe that U.S. science could recover from the

> stultifying effects of decades of misguided peer reviewing if we returned to the tried-and-true method of evaluating researchers rather than research proposals. Many people will say that my ideas are elitist, and I certainly agree. The alternative is the egalitarianism that we now practice and that I've seen nearly kill basic science in the USSR and in the People's Republic of China.

Alvarez would be still more dismayed by how US science has become further burdened by current funding policies. At top-flight research universities, many professors must seek funding from several agencies in order to maintain their research groups. That requires them to devote inordinate time to writing proposals and reports, to the detriment of their teaching, mentoring, and own creative efforts. Thereby, graduate education has been degraded. The vital need to generate grant proposals causes faculty to avoid teaching small, advanced classes and also to discourage their graduate students from taking courses not directly relevant to their research project. Serving as hired hands on a project is also a major factor in stretching out the time to obtain a PhD, since veteran students are most useful in obtaining results to justify a grant renewal. Once usually about 4 years, the median time to obtain a PhD is now 6 or 7 in most fields of science. For postdoctoral fellows, terms have likewise become prolonged. Overall, the funding system has tended to narrow the training of our young scientists, prolong apprenticeship, and inhibit changing fields.

Braben acknowledges that peer previewing of proposals will likely remain prevalent. Then it is all the more important to address problems and advocate feasible reforms. Here I want to augment his suggestions by commenting on two aspects. First, the previewing process, as now implemented, is needlessly capricious. Typically, National Science Foundation and other agencies accept grant proposals only during a "window" that is a month or so wide each year. The applicant usually is not informed of the fate of the proposal for a full year or more and is not provided with the assessments of the five or so anonymous previewers until a few weeks later. That deprives the applicant of objecting if one or more of the assessments is egregiously in error, or even resubmitting a revised proposal until the next window, another year hence.

Such a system is misnamed "peer review." For papers submitted to scientific journals, the author can respond to objections of anonymous reviewers, so has a fair chance to persuade the editor that the paper merits publication. I suggest that funding agencies try out a similar approach. The grant applicant could be given the option to post the proposal on a web site to which only viewers registered with the funding agency are given access. The agency would post the assessment from each anonymous previewer as soon as it has been received. Then the applicant could respond to criticism and actually be a "peer" in, say, two or three exchanges with the previewers. Also, the applicant and perhaps the agency, could designate a few other scientists, not anonymous, to have access to the web site and post comments on both the proposal and the anonymous assessments.

Second, funding of university research is largely to support graduate students and postdoctoral fellows, an essential investment in producing our scientific workforce. That investment is weakened by inflation of the time to obtain a doctorate, which makes pursuit of a scientific career less attractive to many students, especially women. In my generation, young scientists usually launched their independent research careers before reaching 30; now that is rare. For scientists receiving their first grant from the National Institute of Health, the median age has reached 42. That alarming situation has led the current director of NIH to initiate an "Early Independence Program," for exceptional students, providing funds to enable them to bypass usual postdoctoral work and pursue their own ideas.

I hope more such programs appear but urge that a much wider, radical approach is needed, which I'm convinced would markedly shorten the apprentice time and enhance its quality. Stipends in support of graduate students (and eventually postdoctoral fellows also) should be uncoupled from project grants to individual professors. The same money could be put into expanding greatly fellowships students could win for themselves, as well as into block training grants to university science departments. Winning a fellowship or obtaining a training grant profoundly influences a student's outlook and approach to research; they are certified as national resources rather than as hired hands. Also important is the freedom to choose, without concern for funding, which research group to join. That would especially benefit young faculty. In applying for the student support (as done now for more limited NIH training grants), science departments would need to shape more coherent graduate programs, designed to produce doctorates who have broader backgrounds and perspectives and who are better equipped to be architects of science rather than narrow technicians.

Donald Braben deserves gratitude from everyone concerned about wisely managing our investments in science, particularly in developing our future scientists. May a "Braben Club" arise to amplify his clarion calls!

Dudley Herschbach
Professor of Chemistry and Nobel Laureate
University of Harvard

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Don Braben
January 2014

Introduction

The sciences play almost as vital a role in everyday life as the air we breathe. The water from our taps, the food we eat, our jobs, communications, travel, leisure activities, health, and unprecedented longevity all owe huge debts to science. However, such simple factual statements give no hints about the mountains of complexity that had to be overcome before any of these gains could be realized. The most important lesson to be learned is that science does not necessarily progress with the march of time. There is nothing inevitable about it; centuries may pass without any progression, and prolonged stagnation has been the usual result. Although science has led to the generally high living standards that most of the industrialized world enjoys today, the astounding discoveries underpinning them were made by a tiny number of courageous, out-of-step, visionary, determined, and passionate scientists working to their own agenda and radically challenging the status quo. Indeed, twentieth-century life was dominated by the unpredicted, revolutionary discoveries of about 500 of these pioneers. I call this seminal fellowship the "Planck Club" in honor of its first member (so to speak), Max Planck, who in Berlin on December 14, 1900, somewhat reluctantly announced that he had discovered an important new property of the universe. As I explain later, his work inspired a revolution, and nothing in science thereafter would ever be the same.

The Planck Club's uninhibited explorations eventually transformed our lives, yet many had to wait for years before the scientific community finally accepted them. Not surprisingly, it needed time to adjust to the radically new

Promoting the Planck Club: How defiant youth, irreverent researchers and liberated universities can foster prosperity indefinitely, First Edition. Donald W. Braben.

mental pictures and ways of thinking that the discoveries required even after their authenticity had been conclusively demonstrated. Old habits die hard. However, after about 1970, when most Planck-Club campaigns had either come to fruition or were within range of doing so, the considerable expansion of the academic sector and its demand for funds led to the progressive introduction of new policies for dealing with the huge funding shortfall. Astonishingly, considering that academics are noted for their individuality, the policies adopted turned out to be virtually the same everywhere. Common themes have been that research selection processes should be as free from favoritism and discrimination as possible and should aim to support the researchers who will make the most efficient use of requested resources. Such fairness-based policies have been easy to sell to the public and academics generally as they can be presented as being above suspicion and as being the best ways of allocating scarce resources. Everyone with a good idea should have the same chance of getting funded, of course, but fairness is a social concept. It can be achieved only by collective decisions. For research selection, adoption of the now ubiquitous new policies means that freedom to explore without restrictions or control has been replaced by Byzantine procedures in which funding agencies seek endorsement from a selection of an applicant's peers before they will consider their proposals—peers who, of course, are drawn from the notoriously conservative scientific community. To make matters worse, peers are usually allowed to express their opinions anonymously. My implied criticism here might be surprising, as anonymity surely means that peers can express their opinions without fear of the consequences, which of course is a laudable aim. However, scientists are also people; and, when asked to comment on the ideas of a close rival (or would-be rival), we should expect that some scientists might be unable to resist an opportunity for putting the boot in if they can get away with it. Indeed, as I argue, these well-intentioned but misguided policies are having disastrous consequences and are, in effect, unprecedented, global-scale gambles with future prosperity.

The overwhelming majority of members of the Planck Club were academics, a section of the community often renowned for their supposedly other worldly detachment and indifference to the problems of real life. Nevertheless, their work inspired the creation of such down-to-earth technologies as the laser and myriad of their spin-offs, countless components of the electronic and telecommunications revolutions, nuclear power, biotechnology, and medical diagnostics galore, all of which are now indispensable parts of everyday life. They also gave huge boosts to economic growth throughout the century. Some $100 trillion in today's currency would probably be a conservative estimate of their centennial global value, but economics is not a precise science, and who can put a value on the intangible benefits they brought to quality of life?

Fine, you might say, that's what academics do; but get your tenses right. That is indeed what they did, but bureaucracy has now intervened. For most of the twentieth century and indeed for most academic research—large projects and major national initiatives excepted—government policies were, in effect, to not have specific policies. Funds were always tight, but appointed academics were usually free to tackle any problems that interested them as long as the necessary funds were modest. However, by about 1970 the scale of academic research had become too large to leave unmanaged. Most academics are publicly funded, and today researchers are not allowed to lift the proverbial test tube without having to convince their peers that the effort would be the best use of the required resources. Proposals usually take months to prepare, and most fail at this compulsory hurdle; sadly, many agencies either do not allow resubmissions or strictly control them. These policies lead, therefore, to frustration and colossal wastes of time and energy. Had they applied throughout the twentieth century, it is unlikely that the work leading to the most radical discoveries would have been funded simply because the researchers who made them were necessarily out of step with their colleagues, and life today would be unrecognizable.

Academia is not the only source of scientific discovery, of course. Indeed, major industrial companies such as Bell Labs, BP, GE, and IBM were once altruistic and visionary in the research they would support, and spawned many Planck Club–type discoveries. Such large philanthropic organizations as those run by Andrew Carnegie, Howard Hughes, and John D. Rockefeller not only had similarly enlightened policies in the past but also have generally continued with them. Nowadays however, companies keep their scientists on tight leashes and firmly focused on short-term company benefit; and many other philanthropists now seek to target their giving and to increase the efficiency of its use—decisions that inevitably mean that they also concentrate on the "fashionable" fields.

It is ironic that post ~1970 there have been huge increases in the numbers of bodies devoted to science policy and such questions of how nations, companies, and organizations in general might improve their prospects by basing decision-making on the most robust advice available. One might think therefore that selection policies based on the opinions of an applicant's closest competitors—or "peer review," to give it its anodyne and widely accepted name—would have been evaluated *ad nauseam* long ago. As I have long argued, this arcane process by which future research is assessed should more accurately be called "peer preview," which is the term I use henceforth. On the contrary, such consideration is conspicuous by its absence. Indeed, in the science-policy world, advice proudly presented as being "robust" simply implies that it has been thoroughly and properly assessed by peer preview. Thus, the ubiquitous funding bureaucracies have created their own set of catch-22 rules for ensuring that all criticism can be dismissed out of hand; comments on peer preview must, if they are not to be rejected, have peer preview approval. Thus, the received wisdom among scientific organizations everywhere today seems to be that any policy, advice, or research proposal that does not enjoy peer preview's full blessing must be considered suspect or worthless.

The question therefore arises: Will the twenty-first century produce a Planck Club as spectacularly successful as that of the last century? No one can answer with certainty, of course. The potential of science as a source of major new opportunities for humanity in general is as great as ever; but since the begetters of new ideas must now run unscathed through mandatory gauntlets of their fellow experts who do not even need to publicly reveal their identities, the probability does not seem high. As I hope is explained more fully in Chapter 2, these issues are not merely the usual abstract affairs beloved by academics and that have no serious interest for ordinary people. Indeed, they could hardly be more important. Unless funding agencies can answer this crucial question with a resounding and convincing "Yes!" we should all be very worried about prospects for growth and a stable society. Unfortunately, there are signs that it is not even being discussed. It seems to have been tacitly assumed by governments and funding agencies that creativity will not be adversely affected by the radical policy changes and that we can continue to rely on academics to come up with steady streams of priceless new ideas as they always have done. In any event, public funding has always been subject to severe pressure, now made much worse by the current economic crises, of course; and the consensus among governments seems to be that academic research should not be exempted from the rigorous controls with which others must cope.

By the end of 1941, it was beginning to be clear that the Allies would not lose the war. Economists and politicians began to think about how the postwar world should be changed and, in particular, how we might avoid the mistakes of the last war; mistakes, many asserted, that had led to the current conflagration. To say the least, views were diverse. Joseph Schumpeter (1883–1950) called for "incessant innovation," and the "perennial gale of creative destruction." John Maynard Keynes (1883–1946) believed that encouraging spending and discouraging savings could cure depressions. F. A. Hayek (1899–1992), an economic superstar following the publication of his *The Road to Serfdom* in 1944, believed in free markets, free trade, and sound money and was very influential in guiding the miraculous post-war German recovery—the *Wirtschafstwunder*—and won the Nobel Prize in 1974 for his "penetrating analysis of the interdependence of economic, social, and institutional phenomena." They all wanted change and they all wanted growth, but there was little general agreement on precisely how that growth should be achieved.

In July 1944, only a few weeks after the Allies' D-day landings had begun, and with millions locked in mortal combat, the U.S. President invited 700 delegates from 40 countries to thrash out their differences on monetary policy at Bretton Woods. It did little to help the research enterprise. Money was desperately short, and an obvious priority was to get national economies moving again as quickly as possible. The Second World War had, of course, halted impartial scientific inquiry for a time, but it soon began to flourish again thanks to the passionate and sustained efforts of Vannevar Bush (1890–1974) in the US, and Henry Dale (1875–1968) in the UK, two countries that were then the world's most scientifically influential. In 1945, economic problems were severe even by today's standards. The US had 11 million personnel under arms, the UK some 3 million, all due to return home shortly and in need of jobs. Many millions of Europeans were homeless and hungry. Factories were heavily geared toward munitions, and the huge transition to profitable commercial operations and "normality" had to be accomplished as soon as possible. Against these stark imperatives, it would have been understandable had the authorities continued with the successful wartime policy of directing scientific research toward immediate national goals. Indeed, a powerful lobby led by US Senator Harley Kilgore wanted to set up an agency—an embryonic National Science Foundation (NSF)—that would indeed be under political control. In 1944, following a request from US President Franklin Delano Roosevelt, Bush prepared what became one of the most famous and inspirational reports ever written on scientific policy, *Science: The Endless Frontier* (Bush 1945), to take the initiative away from Kilgore. Its uncompromising recommendation to the President was for sustained federal commitment "*to basic scientific research of no recognisable usefulness*" (author's emphasis). The dispute raged for some 5 years but was eventually resolved in Bush's favor, leading to the creation of a largely independent NSF in 1950. On August 7, 1945, in the UK, the Nobel Prize–winning Henry Dale and President of the Royal Society wrote a lengthy and impassioned letter to the *The Times* in London making similar arguments (Braben 2008, p. 60). It concluded:

> The true spirit of science working in freedom, seeking the truth only and fearing only falsehood and concealment, offers its lofty and austere contribution to man's moral equipment, which the world cannot afford to lose or diminish.

The clear vision of Bush and Dale has been confirmed again and again. Similar vision is required today. Unfortunately, we scientists have failed miserably in convincing politicians and the public that new sciences are like vitamins: unless they ensure by whatever means adequate sources of fresh supplies, every one of us will suffer severely, and the global consequences of failure could also be grave. However, I should stress that I am not making a plea for more funding. Current levels are adequate, even in the UK, which to say the least has never been a world leader in the funding stakes. But total freedom is an essential but missing ingredient nowadays. My task, therefore, is to describe the barely credible stories of some of the Planck Club's precious few, to show what a vital role freedom has played in them and how their experiences justify my apparently extravagant remarks.

Perhaps our biggest problem is that funding agencies have lost sight of the fact that radically new discoveries have almost always stemmed from a single person becoming aware of a new and potentially important question or making an observation that exposes current ignorance. Science has many examples, but my favorite was made by the German physician-turned-astronomer

Heinrich Olbers (1758–1840), who in the 1820s famously publicized the question of why the night sky should be dark, though he was not the first to pose it. The stars we can see as bright specks are merely the ones closest to us. But if the universe is infinitely large—and there was no reason at that time to think it was not—there should be an infinite number of them. Our eyes might not be able to resolve them all, of course, but the light from an infinite number of stars, however individually feeble, should reach us anyway; and the night sky should therefore be as bright as day. A full answer to his paradoxical question is complex and had to wait for over a hundred years; it does not concern us here.* My point is that this profound question no doubt inspired countless scientists to grapple with it and reminded them of how little we truly understand, a lesson that indeed we should never forget.

Without an awareness of ignorance, we are unlikely to have dissent, and without dissent, there can be no progress. It would seem, therefore, that humanity's salvation surprisingly depends on a capacity to recognize ignorance. As soon as someone becomes aware of it and does something about it, humanity can hope to advance. Unfortunately, pioneers pointing to generally unrecognized areas of communal blindness are unlikely to be welcomed by senior apparatchiks and others delighting in the quality of the Emperor's New Clothes and their control of the purse strings. Their implicit responses are that we should avoid territories in which operations cannot be efficiently managed and controlled: safer and more rewarding options can be found by intensifying the exploration of productive, predictable, well-chartered fields, as it can be argued that they offer the highest returns on investments. These policies might be defendable for industrial companies as their short-tem survival is clearly imperative, but for academic research they create serious limitations on the types of problems researchers are allowed to tackle. Indeed, they have changed the scientific landscape. Hitherto, its wild and unexplored terrain had always been a magnet for courageous and ambitious researchers; nowadays, however, those who choose the most accessible, obvious, and attractive objectives are given priority.

Thus, current policies undermine the very spirit of research and exploration as they virtually ensure unsurprising outcomes. Indeed, it would seem that the future is now predictable. If we want to restore credibility, we must therefore find ways of restoring a faith in humanity's unrestrained creativity that not so long ago was taken for granted; a faith that tolerated uninhibited pioneers, mavericks, iconoclasts, eccentrics, characters, rebellious youth and the awkward brigade in general, and which has now been abandoned. Although funding agencies and others have followed understandable routes to reach the present pass, we are unlikely to make progress until they recognize, tacitly or otherwise,

*There is a vast amount of literature. See, for example, Harrison (1987).