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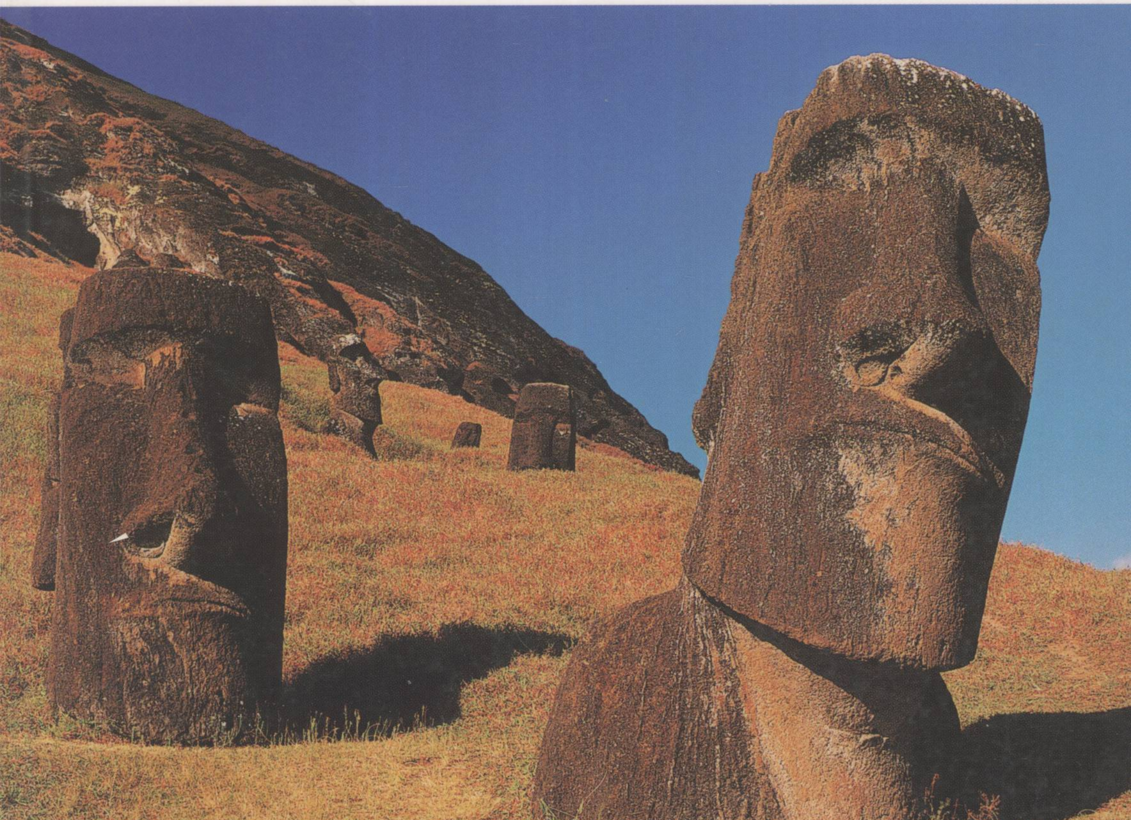

The Elixir of Civilization

DONALD W. BRABEN

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To Thomas Edward and Christopher Jack

Preface

The genesis of this book began in December 2005 when a U.S. National Science Board Task Force was meeting in Santa Fe, New Mexico to discuss transformative research. Nina Fedoroff, the Task Force chairperson, suggested over lunch that I might like to consider writing an essay on how an organization might go about setting up a transformative research initiative. I have somewhat extended that remit, but I hope that the book also goes some way to meeting Nina's original specification. I am also immensely grateful to her for the invitation to join the Task Force and fully to participate in its extensive deliberations. Other Task Force members, Michael Crosby, Douglas Randall, and Jerry Pollack, were also especially helpful.

I am most grateful to Claudio Vita-Finzi, fellow founder-member of the "never say die" club, and an enthusiastic supporter over the years. John Allen, Peter Cotgreave, Irene Engle, Ross Gayler, Nigel Keen, Iain Steel, Ken Seddon, and Isa Zalaman have been unfailing sources of advice and much appreciated encouragement. Nina Fedoroff, Claudio Vita-Finzi, and Isa Zalaman also made many helpful comments on the early drafts for which I am grateful.

I would also like to express my gratitude to David Price, Duncan Wingham, and my many other friends in the Earth Sciences Department at University College, London, where I am a visiting professor. My weekly visits to the department's research seminars are invariably stimulating, and for a few hours each week they make it possible for me to pretend that I am a normal academic.

As ever, I am grateful to my wife Shirley, and to David Braben, Peter and Lisa Braben, and Jenny and David Lightfoot, who have provided invaluable feedback in addition to their usual love and affection.

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July 2007

By the same author:

To Be a Scientist: The Spirit of Adventure in Science and Technology
Pioneering Research: A Risk Worth Taking

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Introduction

> New products, new industries, and more jobs require continuous additions to knowledge of the laws of nature, and the application of that knowledge to practical purposes. . . . This essential, new knowledge can be obtained only through basic scientific research. Science can be effective in the national welfare only as a member of a team, whether the conditions be peace or war. But without scientific progress no amount of achievement in other directions can insure our health, prosperity, and security as a nation in the modern world.
>
> —Vannevar Bush, *Science—The Endless Frontier*, Report to the US President, 1945, p. 5.

Thinking has always been humanity's greatest strength. That abstract ability separates us from the rest of the animal kingdom and has brought us to our present dominance. Like skipping in children, it is innate; it does not have to be taught. Civilizations have prospered or failed as our thirst for knowledge has been tolerated or suppressed. Remarkably, however, until the European Renaissance humanity's progress had been glacial, and centuries might pass before a detached observer might have noticed significant changes to global population or ways of life. The Renaissance then stirred the human spirit, and later created the conditions that launched the Industrial Revolution, first in Britain, and then rapidly elsewhere. Slowly at first we began to harvest our understanding of Nature and to use it for the greater good—material and intellectual. The result was that by the end of the twentieth century the average productivity of every man and woman had increased more than a 100-fold in

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real terms since the Renaissance. Global population also increased rapidly, but material wealth in the industrialized nations more than kept pace.

This prodigious progress came from our growing ability to harvest the fruits of humanity's intellectual prowess—scientific endeavor, as it is usually called. Material wealth continued to accelerate through most of the last century despite financial crashes and global wars. But then gradually, around about 1970, signs of major change began to emerge. Science's very success had unsurprisingly led to a steady expansion in scientists' numbers. That could not continue indefinitely, of course, and the inevitable crunch came when there were more than could adequately be funded. This was not only a numbers problem—the unit costs of research were also increasing. The funding agencies should have seen this coming, but they did not. Indeed, as I shall explain, many today do not accept this version of events, and are thereby contributing to one of the greatest tragedies of modern times. This perhaps surprising statement arises because the agencies' virtually universal response to the crisis was to restrict the types of research they would fund. Thus, to use a truly horrible word, they would *prioritize*, and focus funding on the most attractive objectives—that is, objectives the agencies *perceived* to be the most attractive. Thus, for the first time since the Renaissance, the limits of thinking began to be systematically curtailed.

The new policies* would seem to have been phenomenally successful. Modern life is enriched by vast and expanding ranges of astounding technologies. Communications, entertainment, food production, leisure, travel, and many other aspects of modern life have been transformed. Closer inspection would reveal, however, that most of this bounty stems from generic scientific discoveries made decades ago, a source that seems to have dried up in recent years. Consequently, our intellectual account is becoming overdrawn at a time when the demands on it are increasing. There is no shortage of initiatives aiming to deal with such problems as global warming, population growth, and terrorism, but one vital factor is usually overlooked.

Imagine for a moment that you are invited to list humanity's basic material needs. It would probably contain the obvious things such as food, water, heat, light, health, and security, but it might fail to mention the air we breathe simply because that vital ingredient can usually be taken for granted. Scientific freedom could indeed be placed in that latter category before 1970 or so because research policies then were usually based on laissez-faire. However, we have moved on, as they say. Nowadays, interference is the norm. However, as few funding agencies seem to have noticed, if current policies had applied at the beginning of the twentieth century, say, the world would now be a much harsher place. Today, far from being an inconvenience confined to featherbedded academics, their consequences are approaching "the operation was a success but the patient died" category, and will affect the very foundations of our civilization.

*An analysis was presented in my *Pioneering Research: A Risk Worth Taking* (2004).

Such abstract qualities as freedom are difficult or impossible to define. Freedom's loss may be easier to recognize, but it does not necessarily lead to chains. Increasingly nowadays, freedom is a *managed* commodity, but the consequences are subtle and varied. Indeed, at least for the time being, it is possible for almost everyone to live happily and productively within the current bounds. That is also generally the case in the sciences except for one essential factor. Those exceptionally rare scientists whose revolutionary work can open new horizons can do so only if they have total freedom. The routes to new types of knowledge can be deceptively disguised, and may appear to ordinary mortals as unimportant byways leading nowhere. There must be no filters whatsoever on what they do, therefore, however well intended. Furthermore, their work is vital to future prosperity. In an increasingly complex and populous world, any attempt to limit it will lead us down the path to stagnation and pain.

Scientific progress comes in a vast number of ways, ranging from the apparently spontaneous comprehension of a new facet of Nature's behavior* as typified by Albert Einstein's research on relativity, say, to the prolonged and often agonizing study of a perplexing phenomenon as typified by Max Planck's work that led to the discovery of energy quantization. But if discoveries are to become part of the scientific lexicon, they must be endorsed by the scientific community, and that can often be problematic. However, leaving social problems aside for the moment, research for most scientists is indeed 99% perspiration with maybe 1% flashes of inspiration that hopefully culminate in the complex pieces coming together to form a coherent picture, at least in the investigator's mind.

Max Planck was one of the most influential scientists of the twentieth century, and therefore an appropriate role model for the story recounted here. In 1933, he wrote a typically succinct comment on the problems facing researchers who perceive serious flaws in accepted wisdom and know what to do about them:

> No doctrinal system in physical science, or indeed perhaps in any science, will alter its content of its own accord. Here we will always need the pressure of outer circumstances. Indeed the more intelligible and comprehensive a theoretical system is the more obstinately it will resist all attempts at reconstruction or expansion. And this is because in a synthesis of thought where there is an all-round logical coherence any alteration in one part of the structure is bound to upset other parts also. For instance, the main difficulty about the acceptance of

*I have long used the word "Nature" as if to describe a being. My use has no religious or mystical significance, but is shorthand for the universe and every aspect of everything in it. We have made some progress, but our understanding of that infinitely faceted system is still in its infancy. We are not casual observers, of course, as Nature embraces the entirety of our very existence. Nature is, therefore, my affectionate and respectful anthropomorphism for a system that is the constant preoccupation of every scientist.

> the relativity theory was not merely a question of its objective merits but rather the question of how far it would upset the Newtonian structure of theoretical dynamics. The fact is that no alteration in a well-built synthesis of thought can be effected unless strong pressure is brought to bear from outside. This strong pressure must come from a well-constructed body of theory which has been firmly consolidated by the test of experimental research. It is only thus that we can bring about the surrender of theoretical dogmas hitherto universally accepted as correct.
>
> —Max Planck, *Where is Science Going?* Ox Bow Press, 1933, pp. 44–45.

These words are as valid today as when Planck wrote them a lifetime ago, and reveal something of the dilemma he faced when he began his remarkable career some 50 years earlier. But the issues he identifies are still relevant. Indeed, they seem invariant and timeless.

One would hope, therefore, that research-funding organizations would have Planck's thinking in mind whenever they were contemplating new policies. Until about 1970, that effectively would seem to have been the case.* However, one must always be careful with generalizations. Not surprisingly, there is a voluminous literature, but it is concerned mostly with the qualities of freedom enjoyed by the profession as a whole. It deals with rights to freedom and academics' responsibilities to their various sponsors—society, government, industry, charities, and philanthropists—but the extent to which *individuals* can acquire or lose freedom is rarely discussed. Change in the academic world is often slow, so that my cited date is the peak of a broad distribution. Before 1970 or so, tenured academics with an individual turn of mind could usually dig out modest sources of funding to tackle any problem that interested them without first having to commit themselves in writing. Afterward, unconditional sources of funds would become increasingly difficult to find. Today, they are virtually nonexistent.

The way forward for ambitious young researchers was once clear, therefore. All they had to do was to acquire the necessary qualifications, and then to find a tenured appointment. To say the least, that was not easy, but not substantially more difficult than it would be today. However, having served their apprenticeship, they were free. They may have had to overcome the inevitable peer pressure if their plans were controversial, but *their peers did not have power of veto*—see Poster 1. Written applications were necessary if expensive equipment or large teams were required, but tenured researchers with modest needs would meet few obstacles. One's dedication and talent would usually be sufficient to silence the critics if the problem chosen were controversial, or if progress appeared to be lacking.

*Leon M. Lederman, when he was president-elect of the American Association for the Advancement of Science said that funding for academic research was adequate in 1968, which year he described as the peak of a "golden age" of American science (Lederman 1991).

Poster 1: Charles Townes and the Laser

Charles Townes was awarded his PhD in physics at the California Institute of Technology in 1939, and went on to join the Bell Laboratories, then located in Greenwich Village on Manhattan Island. Soon after, the Bell management directed him to help develop radar-guided bomb-aiming systems as part of the US war effort. This intense work on radar and microwaves, as he describes it, led to his career's work on molecular spectroscopy. Similar war work had been done at the nearby Columbia University in New York, so when at the end of the war Bell suggested, as they say, that he should focus his work on subjects of interest to the company, he decided in 1948 that he would pursue his own interests, and accepted an appointment as associate professor of physics at Columbia.

For some time, he had been trying to make intense beams of submillimeter radiation, rather than the centimeter or more wavelengths that he had been working with. Eventually, he conceived a possible method to generate photon "avalanches" using excited ammonium molecules. But he could not get it to work. As he relates in his book (Townes 1999):

> [After] we had been at it for two years, Rabi and Kusch, the former and current chairman of the department—both of them Nobel laureates for work with atomic and molecular beams, and both with a lot of weight behind their opinions—came into my office and sat down. They were worried. Their research depended on support from the same source as did mine.* "Look," they said, "you should stop the work you are doing. It isn't going to work. You know it's not going to work. We know it's not going to work. You're wasting money. Just stop!"

But Townes had come to Columbia on tenure, so he knew he couldn't be fired for incompetence or ordered around. Nevertheless, the awesome weight of Rabi's reputation in particular—a one-time senior member of the Massachusetts Institute of Technology's legendary Radiation Laboratory set up by Vannevar Bush to develop wartime radar—must have been daunting. Such top brass cannot be defied lightly, and showing extraordinary courage, this junior faculty member stood his ground, and respectfully told his exalted colleagues that he would continue. Two months later (in April 1954), his experiment worked, and the maser (microwave amplification by stimulated emission of radiation) was born. Three years after that Arthur Schawlow, Townes' postdoc at Columbia, had moved to the Bell Laboratories, and their collaboration led to the optical version of the maser—the laser. Townes was awarded the Nobel Prize in Physics in 1964 for these discoveries [shared with Aleksander Prokhorov and Nikolai Basov (USSR), who developed the maser and laser independently]. Schawlow was awarded the Nobel Prize for Physics in 1981 for his work on laser spectroscopy.

*Their support came from a Joint Services contract managed by the Army Signal Corps.

As Planck says, researchers who claim that an accepted dogma is seriously flawed have a duty to persuade the scientific community that their alternative is better. Planck was indeed his own severest critic in this respect as serious researchers often are. As he would have been the first to point out, the status quo should not be changed lightly. He was, in his own words, a reluctant revolutionary. In Planck's time, academic research was essentially unmanaged, whether by objectives or otherwise. There was no central direction or coordination. The issues involved were largely scientific. That is no longer the case. Now that the new policies are firmly established, for the first time in science's long history researchers must submit their proposals in writing to an appropriate agency. Spontaneity has been lost. The funding agencies subject these proposals to an arcane set of tests (peer review) designed to flag what they perceive as the best, expecting thereby that the rest will probably be lost.

These well-intentioned changes have created lumbering bureaucracies to ensure compliance. They have also inhibited exploration outside the mainstreams and challenges to convention. This is most unfortunate because the great discoveries that transformed the twentieth century came out of the blue. *There was no demand for them.* One might suggest that the Manhattan Project and the Human Genome Project are among examples to the contrary, but the unexpected discoveries on which they were based had come much earlier. Their successes are monuments to organised creativity and depended on exquisite fine-tuning of existing knowledge and orchestration of resources on unprecedented scales rather than on new science per se. However, scientists today with radical turns of mind—the successors to Planck et al. (see Poster 2)—are unlikely to get funded because their ideas are unlikely to impress a committee *before* they have been confirmed. Consequently, there has been a dearth of major scientific discoveries in recent decades. We are living off the seedcorn.

Poster 2: The Twentieth-Century Planck Club

The twentieth century was strongly influenced by the work of a relatively small number of scientists. A short list might include Planck, Einstein, Rutherford, Dirac, Pauli, Schrödinger, Heisenberg, Fleming, Avery, Fermi, Perutz, Crick and Watson, Bardeen, Brattain, and Shockley, Gabor, Townes, McClintock, Black, and Brenner (see Table 1). However, I give this list only to indicate something of the richness of twentieth-century science. I wrote it in a few minutes, and it obviously has many important omissions. Other scientists would doubtless have their own. If the criteria for inclusion were based on success in creating radically new sciences, or of stimulating new and generic technologies, a fuller list could easily run to a couple of hundred.

I will refer to the extended list as the *Planck Club* or alternatively as *Planck et al.* for the remainder of the book.

However, issues much wider than research are at stake. In everyday life we rely, usually unquestioningly, on humanity's basic needs being available at affordable prices so that we can get on with our lives at our own pace. For those of us living in the industrialized countries—say, the 20 or so richest states of the Organization for Economic Cooperation and Development (OECD)—we can usually take them for granted. But global population is steadily increasing. In addition, many millions in China and India, for example, deemed hitherto not to have such needs are now beginning to assert their rights to them. Demand for some vital resources—oil, copper,* and water—is increasing rapidly. Many doomsayers take the Malthusian line. Earth's resources are strictly limited, they say, and we are all going to hell in a handcart.

They are wrong, because they reckon without humanity's apparently boundless command of the intellectual dimension. Thanks to that precious gift, and despite the havoc of world wars, financial crashes, and a threefold rise in population, per capita economic growth† soared in the twentieth century, reaching a peak, coincidentally perhaps, around about 1970. It then began a steady decline. Unfortunately, however, that decline is now being masked by unprecedented rises in residential and other property values dubbed by *The Economist* as perhaps the biggest bubble in history (see Chapter 3). Bubbles usually collapse more quickly than they inflate, and who knows what the effects will be. Humanity is indeed blessed with the ingenuity necessary for survival, but much of this priceless asset will be wasted if we smother it with consensus. See Poster 3.

This book seeks to extend the debate to anyone who takes a serious interest in global affairs—industrialists, academics, legislators, consumers. Sadly, however, even though the fruits of research are essential for modern life, the media seem to confine their interest to specific *discoveries*; research as an *enterprise* is generally ignored, and *research policies* have even less appeal apparently, if that is possible. This indifference would seem to have a simple explanation. Since the Renaissance, the policy on academic research has generally been *not* to have a policy. Patronage and sponsorship have always been important, but their agendas were diverse and they often backed creative talent for its own sake. Society's reward was a remarkably steady trickle of astonishing miracles. But times have changed; funding agencies' mission statements (or other expressions of purpose) are now de rigueur, and often cast in stone. This situation has long been the case for researchers in the industrial sector, but that is to be expected. Industrialists must know where they are going. Now, academics, too, are almost invariably subject to externally imposed constraints. Unfortunately, these dramatic changes seem to have passed the media by, so would-be reformers are starved of the oxygen of publicity.

**The Economist's* metal index published on March 22, 2007—which includes aluminum, copper, lead, nickel, tin, and zinc—was at its highest level for 16 years.

†During the twentieth century, the average real-terms productivity of every person on the planet increased 4.3-fold despite a 3-fold increase in population.

Poster 3: The Wizard's Warning*

Once upon a time, I dreamed about a meeting about a hundred years ago at which a wizard addressed a large gathering of industrial and scientific leaders. He announced that he would use his 20/20 foresight to describe the powerful discoveries that would enrich the coming twentieth century. "However, your language does not yet contain the words I need to describe the future," he said, so he put a spell on the audience that conjured visions of energy quantization, relativity theory, atomic and nuclear structure, quantum mechanics, and molecular biology to give some impressions of the sciences that might shortly come. With mounting excitement, he outlined some of the magical technologies that might stem from them: magnetic resonance imaging and a wealth of other medical diagnostics, lasers, nuclear power, computers, telecommunications, and genetic manipulation. The audience was quite literally spellbound. They had expected him to talk about developments in electrical and steam power, hydraulics, and oil and coal technologies. What they heard was totally unexpected. When the wizard had finished, someone asked what we had to do to make these fantastical things happen. His reply was equally stunning: "Nothing," he said. His voice began to quiver with emotion: "Humanity has been given the priceless gift of creativity, but it's vital that you understand how it works. Creativity is the essence of the human spirit, and flowers best when it's unconstrained. If you try to control it for your own ends you must learn that you can get only what you ask for. The unexpected will not arise. You are not wizards." These last words came in an intense growl. Then he disappeared, and I woke up.

I dreamed about the wizard again recently. He told me that he had given similarly prescient lectures every hundred years since 1600 when Francis Bacon and a few others began to appreciate the value of organized scientific research. Before that, the scientific world had been more or less stagnant for a long time, so there was nothing new to herald. His lecture on the twenty-first century was clearly long overdue, and I asked him when next he would be speaking to us. His answer was depressing:

> I will give no more lectures until humanity regains its sense of wonder. As you know, my foresight is perfect, but I'm not allowed to reveal anything about the major discoveries that await you. The sole purpose of my centennial addresses has been to inspire, to conjure for the best of you subconscious images of what your creative talents are currently capable of. The rest is up to you. Unfortunately, your leaders have now decided that wonder is inefficient because it cannot be controlled, quantified, or targeted. You must consolidate what you think you know, of course, but nowadays that is all you are doing. Humanity's powers of foresight have always been puny, so you'll get nowhere unless you listen to what I am trying to tell you and back those rare individuals whose vision transcends need.

*Based on an editorial by the author published in *The Scientist* on September 27, 2004.