

analysis

Low risk, high reward

An international group of scientists wants to establish a funding stream that will allow researchers to follow their whims. **Don Braben** outlines his plans for a Planck Club for the 21st century that could shorten the odds on high risk research.

Most funding organisations include a small proportion of “high-risk” or “speculative” research in their portfolios. They do so, apparently, because they believe academic research has become too predictable, too safe.

They are right. However, from a scientist’s point of view, reputation is slowly earned and quickly damaged and the length of a productive career can be all too short. This gives rise to a simple question about undertaking high-risk research: why should academics embark on major quests if their funding agency expects most of them to fail?

Funding agencies should recognise that perceptions of risk are relative. One person’s risk might be another’s challenge. Some people might see skydiving as dangerous, for example, because they believe that the probability of a nasty outcome is high. Skydivers themselves, while acknowledging and even enjoying the risks, expect their next jump to be successful. They would not jump otherwise. They manage the risks very carefully, taking every reasonable step to reduce, eliminate or control them. As a result, they are confident that their jump will be exhilarating and enjoyable, and that they will land in one piece and, more or less, where they expected. On the other hand, if the spectators were responsible for the risks skydivers take, it is most unlikely that any jumps would take place. Consensus opinion would opt for safety first, as it usually does, and yet another expression of *joie de vivre* would vanish. Responsibility shared is responsibility declined.

In research, adoption of best-value-for-money policies means that funding agencies will generally give the highest priorities to projects with the lowest risk of

failure. Researchers must manage the residual risks, but as long as they do not stray too far from the beaten track they can take comfort from the fact that consensus deems these risks to be acceptable and small. Should it be surprising, therefore, that research has become mainly predictable and sometimes even dull?

Imagine if we could plot all this, as estimates of a project’s success (in terms of objectives met) against its potential impact (*see graph, opposite*). ‘Mainstream research’

seems to accord well with experience. We expect most projects to succeed but to have a potentially low impact. However, that does not mean that the research was not worth doing. Indeed, mainstream research plays a vital role in consolidating existing knowledge. The long tail indicates that a small proportion of researchers might achieve interesting and exciting results, even though they started out conventionally, as say Penzias and Wilson (who discovered the cosmic background radiation), or Fleming (who discovered penicillin) once did. But those pioneers had the freedom to persevere for as long as it took. The graph of ‘high-risk/ high-reward research’ illustrates the funding agencies’ recent excursions from their traditional approach.

These two schematics should cover every type of research the funding agencies currently support. It seems reasonable to ask, therefore, where the work of the 20th century’s great scientists would have fitted in—those such as Planck, Einstein, Avery, Townes, Crick and Watson, McClintock, Perutz, Kendrew and perhaps 300 others of similar calibre whose work dominated the century. I call this glittering assembly the Planck Club.

Their work undoubtedly had high impact but was it high risk when they began? For them, all decisions on risk were their prerogative. So the question becomes rhetorical: how successful did they think they might be, and in particular, how risky did they think their work was? I cannot conceive that they thought of risk. My conceptual abilities are irrelevant, of course. However, my analysis is based on what they did and, as far as we know, how they went about it. My thesis is that although they could not know precisely where they were going, they were confident that their work would prove significant—hence their enormous dedication. They had no timetables, except their own, and no one was specifying their goals or urging them on. They would fail only if they gave up, which would be most unlikely as they clearly believed that if anyone could succeed they would. As long as they were determined, it would only be a matter of time.

Research has become too predictable mainly because the rules changed in the 1970s. Scientists were required to have their proposals vetted by their peers. This works well enough for mainstream research, but fails at the margins where great discoveries are made. Hence, we have the supposedly remedial high-risk research initiatives. But they promote the idea that science is risky.

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It is not. It is difficult, but scientists are fully aware of that when they begin their careers. Code breaking, too, can be difficult but it is not risky; it is what code breakers do. They might fail or succeed in a given time, but if the code is important they will continue until they crack it. Scientists are no different. In addition, risk is a word usually closely associated with danger. Why should the funding agencies associate danger with difficult problems? If the funding agencies would allow researchers to express their individuality, some would choose to tackle the difficult problems because they would see them as personal challenges—as they did in the past.

In 1980, BP invited me to create and run an enterprise, called Venture Research, to attempt to identify future great discoveries whatever the field. While no one knows the minds of Planck *et al*, we modelled our ambitious initiative, which ran throughout the 1980s, on what we imagined their motivations might be. Most of the 26 groups we eventually supported went on to make potentially 'high-reward' discoveries (as I discuss in my book, see below). All involved the most basic research, and almost all had been rejected by the usual agencies. Costs were remarkably low. The high proportion of successes indicates that the research we supported was not high risk. The graph of 'venture research' illustrates our experience.

Venture Research offered 'low-risk, high-reward' opportunities, although we did not use such fashionable clichés at the time. We set out from the beginning to recreate the environment of total freedom that members of the Planck Club took for granted. Thus, we expected every scientist we supported to succeed in their ambitions, but we set neither timetables nor goals.

If our institutions do not create a 21st century Planck Club, the prospects for civilisation will be dire, and they will have failed in their duty as custodians of creativity. We must restore the freedom that allows scientists to do great works. However, there are millions of scientists, and no one knows which haystack hides the needle.

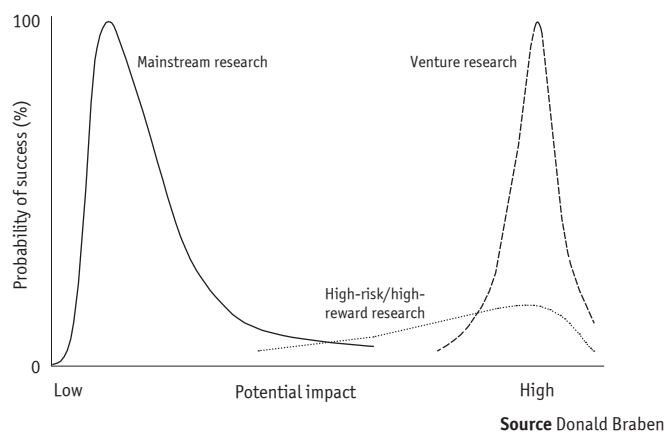
Can we identify the potentially great ones? Yes, but everything depends on the quality of the environment we create. If you get it right, the potentially great scientists will select themselves, as indeed we found. However, it is not easy. At Venture Research, it took us several years before we were confident that we could do it.

It involves respect—respect for every aspect of the sciences, respect for those who seek to deepen and extend understanding independently of tangible benefit, and respect for the courage needed to tackle the difficult problems. Above all, it involves the development of

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Calculated gambles

Most mainstream research meets objectives and consolidates knowledge; most high-risk/high-reward research is expected to fail; venture research offers the best of both worlds



mutual trust so applicants feel secure in sharing their real intentions. Einstein said that Planck was inspired by a "hunger of the soul". Such scientists might not be overly interested in the power, glory or other transient rewards major discoveries might bring.

I do not know of an agency anywhere in the world that, as currently constituted, could respond to these challenges. That is why an international group of 27 senior scientists, of which I am a member, is trying to create one. Some agencies have come to recognise the need to attract adventurous proposals, but they seem reluctant to provide the freedom to enable that need to be satisfied. Instead, they consult committees of the best scientists for recommendations on the big problems that may be solvable. The agencies then seek the researchers who might solve them. High risk indeed. But no member of the Planck Club worked on anyone else's agenda. They chose their own problems, and many were told by their colleagues that they were wasting their time. But it was their unpredicted discoveries that changed the world.

In recent years, politicians have increasingly imposed such objectives as the promotion of national health, prosperity, welfare, competitiveness and the optimisation of efficiency on public funding agencies. Remarkably, private agencies have adopted similar policies. These actions might be understandable for mainstream research, but they make it difficult or impossible for the agencies to fund the mavericks whose work is motivated only by a need to understand—the very people who would constitute a future Planck Club! Our hope, therefore, is that these challenges might appeal to private investors. For relatively modest outlay—some \$20-30 million (£10-15m) a year for a global scheme—they could indeed change the world.

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