Introduction to the UCL Provost's Venture Research Fellowship scheme

This website is most unusual. It contains an invitation to anyone employed by University College London to submit original proposals in fundamental research, which if approved would be supported from the university's own resources. Such proposals would almost certainly not be funded by the usual sources because they raise issues that would not automatically command the full support of colleagues. Thus, novel approaches on, say, the nature of gravity, or consciousness, or any other topic whose importance has yet to be generally recognised might qualify for support. Under today's rules the researcher may choose to stick to safer subjects.

That was not always the case. In the 20th century, few of the ~ 500 scientists who made Nobel Prize winning discoveries, which select gathering I've called the Planck Club after Max Planck who discovered quantisation in 1900, had to write proposals. They became necessary only after about 1970 when the rapidly rising number of academics progressively led to them becoming mandatory. Before that time, appointed academics and indeed many others could simply pursue their own ideas without consultation. The discoveries of Planck Club scientists subsequently transformed our lives, but their modern successors might have serious difficulty getting funded.

The purpose of this site is to invite proposals from scientists whose research might turn out to be of similar importance to those of the 20th century Planck Club. Approval will therefore be rare, but the importance of their research could be vital to humanity. A one-page proposal is needed to make an initial application. It would outline what it is you have in mind and why your proposal might not usually be supported. If approved, the proposed research would become the researcher's main interest. Written proposals are not strictly necessary and bearing in mind the low probability of funding, the applicant might choose to discuss their ideas with Don Braben in the first instance as to their suitability.

Other universities will be invited to participate in this unusual scheme.

The attached papers give some background information.

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20 May 2019

Braben Venture Research Index (BRAVERI)

The term Blue Skies Research as widely used today has become meaningless. For us, Blue Skies Research is useful shorthand for the most innovative and creative research that might lead to outcomes unimagined. This means that initially the research should have no specific or directed objectives.: the "skies" should initially be clear and full of promise. Any imagined outcome should be possible. However, these sentiments run counter to current practice in the Blue Skies Research game, due, of course, to the dead hand of peer review. Since the conventional agencies cannot normally release funds unless they have the endorsement of this sacred cow, disciplinary constraints must be imposed. How else can peer review work? But in our view, to speak of Blue Skies Research in the field of, say cancer, or any specified objective as is usually the case, is a contradiction: such initiatives merely play lip service to the idea of fostering the full range of human creativity.

As should be much better known, quality cannot be quantified. There are national schemes claiming to support Blue Skies Research that do not even come close to deserving that lovely accolade, although they may sponsor excellent research. Venture Research, that is the route to our version of Blue Skies Research, is exclusively dedicated to uncovering the unknown, and seeding the Blue Skies. It is vital, therefore, that we differentiate what we are trying to do from the multitude of mission-orientated endeavours now extant.

It may be instructive, therefore, to propose a novel way of measuring the skies' blueness in a research proposal. The Braben Venture Research Index - BRAVERI* - offered here is intended to be a rough rule of thumb. *It is, of course, not to be taken too seriously.*

*The Index was presented at the 60th birthday meeting in Messina Sicily, to honour Gene Stanley, one of our American Venture Researchers. An earlier version was published in Physica A 314 (2002) 768.

The Index is based on ten features that might be used to describe a research proposal. They are not independent. Each feature yields a score of 0, 1, or 2. The BRAVERI level is found by adding together the scores in each category, and expressing the total as a percentage--that is the purest Blue Skies Research would score 20/20, or a BRAVERI of 100%. The features are: -

1. The proposed research: -
- Is within the purview of a national funding committee, priority, or initiative. If so, score 0.
- Straddles two national funding committees, priorities, or initiatives. If so, score 1.
- Extends over several national funding committees, priorities, or initiatives; or none are relevant. If so, score 2.
2. The objectives of the research: -
- Are detailed and specific. If so, score 0.
- Are general. If so, score 1.
- Have not been externally imposed. The researchers are completely free to tackle difficult problems in a general area in any way they think fit. If so, score 2.
3. Assessment of a proposal by peer review: -
- The reviewers are fellow experts. Their opinions are pertinent. If so, score 0.

- Involves several experts from different fields. It may be difficult to find a consensus. If so, score 1.
- Is problematic in principle, because the researchers are radically challenging accepted thinking or proposing something totally new. Genuine peers may be difficult to find. If so, score 2.

4. Funding requirements: -

- The probability of success would increase as the funding level increases. If so, score 0.
- The probability of success is not sensitive to funding level. If so, score 1.
- Success is not easy to define. Expensive equipment may not be required. Thinking time is essential. Only one or two research assistants can be efficiently employed. If so, score 2.

5. Implications of the expected results: -

- The most likely outcome is a useful addition to knowledge. If so, score 0.
- They could lead to a breakthrough in the development of a field. If so, score 1.
- They could be revolutionary, and radically change accepted thinking. They could lead to the development of new fields. If so, score 2.

6. Timescale: -

- The research is expected to meet its targets in the time allowed. If so, score 0.
- The research is part of a long-term programme. The ultimate goal is unlikely to be achieved in the time allowed for a specific phase. If so, score 1.
- Is indeterminate. Successes, whatever they are, could be achieved at any time, or never. If so, score 2.
- 7. Competitors: - The researchers are striving to be first to a specific goal. The competition is fierce. Cards should be played close to chests. If so, score 0.
- The researchers are striving to be first, but the field is wide open and direct competition is unlikely. If so, score 1.
- The researchers are striving to understand. The research is probably unique. There is therefore little or no competition. If so, score 2.

8. Publication: -

- The expected results will probably be published by a mainstream journal. If so, score 0.
- The expected results might be published by a prestigious journal such as *Nature* or *Science*. If so, score 1.
- It is not clear at the outset what the results might be. When they come, it might initially be difficult to get them published. If so, score 2.

9. Prizes: -

- The researchers do not really expect to win a prize. If so, score 0.
- It is conceivable that the researchers might win an award from a learned society. If so, score 1.
- It is conceivable that the researchers might be recommended for a Nobel Prize. If so, score 2.

10. Collaborators and commitment: -

- The leading proposers have many irons in the fire. If so, score 0.
- The proposed research involves a new collaboration, but each collaborator has other interests. If so, score 1.
- New collaboration may or may not be involved, but the proposed research is a major interest for everyone involved. If so, score 2.

If research would have a low BRAVERI, it is very unlikely to deserve a Blue Skies appellation. However, I do not imply that research with a low BRAVERI score cannot be innovative or adventurous. In the past, what might have been called mainstream research was the source of all points of departure. All the major discoveries stemmed from it. But scientists working in the mainstreams today are rarely free to follow the unexpected leads they may come across. In any case, mainstream research is readily funded by the conventional agencies. All our Venture Research programmes (supported under either BP's or UCL's auspices) would have scored more than 50%, although the Index was not used.

Sadly, the higher the BRAVERI, the less likely it is that the research would be funded by a national agency. *Almost all research funded today would have a BRAVERI close to zero.* That is a remarkable fact in itself. Our Venture Research initiative is searching for funds to support research that would have a high BRAVERI. Agencies claiming to support Blue Skies Research initiatives, and the governments and other investors who sponsor them, might care to apply our tongue-in-cheek BRAVERI assessment. They might be surprised at the outcome.

Don Braben
December 2018

Don Braben: Brief CV.

Summary. After graduating in physics from the University of Liverpool in 1957, I spent some sixteen-years in nuclear structure and high-energy physics research. That was followed by positions at the Cabinet Office in Whitehall, the Science Research Council in London, and the Bank of England. From 1980-90 I was Head of Venture Research, British Petroleum, a successful initiative to back people anywhere in Europe and North America working in any subject who might radically change the way we think about something important. From 2003 to the present I have held honorary positions at University College London and in 2008 helped set up the UCL Provost's Venture Research Fellowship.

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 meeting 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 20th 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 quantisation, relativity theory, atomic and nuclear structure, quantum mechanics and molecular biology to give some impressions of the sciences that would shortly come. With mounting excitement, he outlined some of the magical technologies that would shortly 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 he had finished and questions were invited, he was 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 is vital that you understand how it works. Creativity is the essence of the human spirit and flowers best when it is unconstrained. If you try to control it for your own ends you must learn that you get only what you ask for. The unexpected will not arise. You are not wizards". The last words were uttered in an intense growl. The 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 or so when Francis Bacon and a few others began to appreciate the value of scientific research. Before that, the world had been more or less stagnant for a long time, so there was nothing new to herald. His lecture on the 21st century was clearly long overdue so 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 od wonder. As you know, my foresight is perfect, but I am not allowed to reveal anything of the major discoveries that await you. The sole purpose of my centennial lectures has been to inspire, to conjure up 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 will get nowhere unless you listen to what I'm trying to tell you, and back those rare individuals whose vision transcends need".

*Based on an article that first appeared in *The Scientist,* 27 September 2004, p 8.