

SOME THOUGHTS ON BIG SCIENCE

By

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ABSTRACT

Original insights for the most part emanate from the minds of single individuals. Consequently, institutions, no matter how well funded, cannot expect to enjoy a monopoly on creativity. However, the individual without institutional affiliation is most likely to be ignored. The situation is counterproductive to scientific progress and the world economy. This article recounts one person's experience and offers a potential solution.

When I was a young man I was shown a gravitational wave detector. The technology fascinated me from that day onwards. As the size of instrument has grown, the technical achievements have continued to command my respect. I have therefore followed the gravitational wave detection story as a layman throughout my career.

In recent years I began to wonder whether the designs of the experiments themselves were ideal. I tried to devise alternative approaches. In 2007, I made some notes regarding a technique which might be used. However, its viability depended on the existence of a particular physical effect. I was not sure whether the effect existed, and did not have the necessary theory to hand. The note soon became buried under my routine office work. I came across it again about a year later. I was able to devote some time to the theory on that occasion, and before long was able to turn up an appropriate confirmation that the required effect should exist. My original note had stood the test of time, at least in my eyes, and only required minor modifications. I also added a few new thoughts. I believed that it was well worth putting forward as an experimental proposal.

If I am correct in my rationale, my proposal could spark a fresh and improved generation of detectors. Having had my career in physics utterly disrupted many years ago, it seemed that there might be scope to repair the damage - at least partially.

I therefore wrote to a number of members of the international gravitational wave community to see what level of interest, *if any*, there would be in hearing my proposals. I am conscious that my understanding of gravitational waves is that of a modestly informed layman. It is quite possible that my little knowledge is more dangerous than useful. Should it transpire that the contents of my proposal are not particularly original, I can at least draw satisfaction from having arrived at them *ab initio*. On the other

hand, if my ideas withstood scrutiny, I wanted to know what the gravitational wave community would be prepared to offer in return.

The replies described, in what seemed to me a high handed tone, "*How Science Works*". Yes they wanted to hear my ideas. Furthermore, they seemed to believe that they had the right to hear my ideas. As far as recognition was concerned, assuming the ideas were any good, it seems I might be lucky enough to have my ideas cited.

For those inside academia, the argument seems reasonable enough. One aspect of being an academic is to come up with original thoughts. However, I am an outsider. I do not benefit from the resources of any institution. My thoughts are my own. They are the fruits of hard slog through adversity. Citations do not put food on my table.

There is another aspect. Gravitational wave detection is big business. The award of \$206M to the LIGO project is but one example. Gravitational wave detectors are funded almost entirely by central government grants. It seems reasonable that the Public funding aspect of any research should be reflected in freely available knowledge, at least within the country which paid for the research. While it is true that there is an increasing volume of scientific research results released into the public domain, much is still controlled by the publishing houses of universities and academic institutions. It seems morally incorrect and is definitely fiscally wrong that these organisations should enjoy pecuniary benefit from work funded by the taxpayer. It also seems reasonable to me that those involved in big science have at least some duty of care to ensure that their experiments have the best possible design. A terrestrial detector can well be modified to improve its performance, but when LISA is launched, the design is pretty much determined for the life of the experiment.

It also needs to be borne in mind that the existing detectors are the cathedrals of technology for a large number of scientists and technicians. Many of them have spent their entire career as acolytes to the instruments.

Despite the fact that technology has been advanced, the sum total of all the Professors, PhDs, postgraduate students and technical staff over the last 30 years have not managed to detect gravitational waves.

It is not likely to please many people, if a nobody from the wastelands of rural Ireland steps up and says “*This is how you do it!*”. It could prove very embarrassing if my proposals did indeed reveal a weakness in the experimental design, and perhaps worse still, pointed the way forward. In the event that my proposals caused other groups to become active in the gravitational wave search, there could be a shift of prominence and status amongst the research community. The present guardians of the paradigm might find their position threatened.

Then there is the problem of what to do *if* gravitational waves *are* detected. Once detected, they have been detected. The motivation for building ever larger detectors is much reduced. Many scientists and others might find themselves at a loss over what to do next. One might argue that the unstated purpose of big science is *not to discover* the thing for which you claim to be searching.

This is no idle conjecture. When a group of scientists believed they had evidence for the existence of the Higgs particle, they requested that the Cern detector be given a life extension of a few months. During that period it could run at full power. There would have been little concern over the risk of burnout, since the equipment was due for scrapping in any case. The request was denied with a flurry of arguments about detection probabilities and resulting delays to the next generation of detector. However, the Higgs particle was the principle motivation for construction of the new detector. It would have been rather unfortunate if the Higgs particle was detected by the old equipment. In the event, the argument about delays proved unfounded, in what some might consider a most poetic manner. Full commissioning of the new equipment has been significantly delayed by a design weakness in some of the superconducting magnets.

It would be utterly foolish of me, and furthermore, entirely contrary to my personal experience, to think and act as though science was devoid of power struggles, or individuals who place personal ambitions before science.

These considerations cause me to have some difficulty accepting the notion that I should engage in a costly and time consuming exercise of publishing my proposals in order that those employed full time in the sector can muse upon my ideas at their leisure.

So I appear to be in a no-win situation. I could promulgate my ideas to the limited extent that my resources will allow. If my ideas are flawed, I can look forward to generous helpings of ridicule. Conversely, if the ideas are as good as I believe them to be, then I *might* be lucky enough to get a mention in a footnote somewhere. I will then experience the questionable delight of watching countless others enjoy the furtherance of their careers while I continue making do on a very tight budget.

Arguably, I would be better advised to spend my money on lottery tickets. I will still end up in obscurity, but buying lottery tickets does not tend to attract ridicule. More seriously, science is in a no-win situation if the only ideas considered are those originating “*In-house*”. Worst of all, the Taxpayer is in a no-win situation for as long as research is so conducted.

In truth, although I am fascinated by the technology of big science, the subject matter leaves me rather cold. My principal interests have for a long time been with more mundane physics and its engineering applications. These are broadly classified by the term “*Sustainable technologies*”. The notion that I might manage to develop something in my lifetime which is worthwhile for the broader spectrum of mankind, such as my energy storage technology, is more appealing to me than the possibility of advancing fundamental understanding of the universe, even although the latter *could* ultimately have more abundant benefits. It seems to me that if my idea for detection of gravitational waves is indeed original and worth pursuing, then I should receive some form of financial recompense for its disclosure. In that way I might be able to advance the technologies which *I* have confidence can benefit humanity.

My offer to the international gravitational wave community still stands. However, if it is so outlandish for individuals like myself to seek promise of reward, conditional on the validity of their proposals, then it would seem that their ideas may as well continue to reside in their heads or amongst the pile of papers collecting dust on their desks.