

ACADEMIA

Unorthodox Ideas

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I often get ideas about science that, on the face of it, seem unlikely to command popular attention but nonetheless make sense. Publishing negative research results is one example. Obviously, not every research project is successful. On the contrary, most experiments fail. Some, with a bit of luck and obstinacy, turn out to be successful. One scientist aptly compared this to the night sky. Positive results are the shining stars. Negative ones are the dark matter: invisible, and far more abundant. Others have compared science to an iceberg – for every successful experiment, sticking out like an icy tip, there are many more failed ones hidden in the water. However, results are results, and they should be publishable so long as they're accurate and reliable. The trouble is, negative results are not a hot sell. Publishers are leery to accept them, and they don't get cited much. Because having numerous publications and citations is key to a successful career in science, negative results offer no payoff. Journals willing accept them run into financial difficulties, and a vicious circle results.

However, the obsession with success in science comes at a price. The highest price is the fear of taking risks. To ensure positive results, researchers home in on topics that offer the highest chances of success and publication. But predictable results are by definition unoriginal. In science it is the pioneering project, the unexpected outcome, that is valued the most. Also, the failure to publish negative results comes at a price in a quite literal sense: resources are squandered when somebody else tries something similar and fails. The absence of a positive results is itself a valuable thing to know, pointing to research methods or approaches which are best avoided. In this respect a negative result is not a failure (as it tends to be regarded), but an integral part of science.

The question arises, can we do anything about it? With the many data platforms available in our information age there must be a cost-effective way to ensure the publication of negative results in online journals. Negative findings may not bring the coveted glory or tenure. But they show

honest effort, provide much needed information, and, importantly, save research work from going to waste. All we need to do is acknowledge that negative results are not failures.

Another unorthodox idea is that we might reverse our grant procedures so that funding is given to teams *after* the scientific work is done, not beforehand. Under communism, when no grants were available, we would officially “plan” the previous year's results for the coming year, confident we could deliver. There is a downside to awarding grants before the research is done, namely researchers are no longer in a position to pursue new ideas. More than once have I shared an interesting research idea with a colleague, only to find out that he was tied up in a three-year research grant, and was unwilling to take on any new projects. Obviously, funding is essential in experimental work, and PhD students need scholarships to survive. But some of that remuneration could be disbursed after a project is complete, not when it's still planned. This way scientists would have more flexibility, and the funding bodies would know exactly what they're paying for.

Alternatively, researchers with a successful track record (over, say, three years) could get extra funding for the next three years on top of their usual salaries, and account for the spending afterwards. The idea is that we should have faith in the creativity and intuition of researchers without limiting the range of their ideas. I recently attended a lecture by Serge Haroche, a French physicist and Nobel Prize winner in physics in 2012 for his research on photons in confined spaces. Haroche talked about how lucky he was to have been working in France, where his team got funding for a ten-year period without having to publish anything. Anywhere else, this would simply not have been possible. Clearly, that is not to say that every ten-year dry streak will reliably end in a Nobel Prize. But Haroche is a good illustration of the fact that one should trust the experienced scientists. Because nobody knows better than an expert in a given field what to do and how to do it. ■