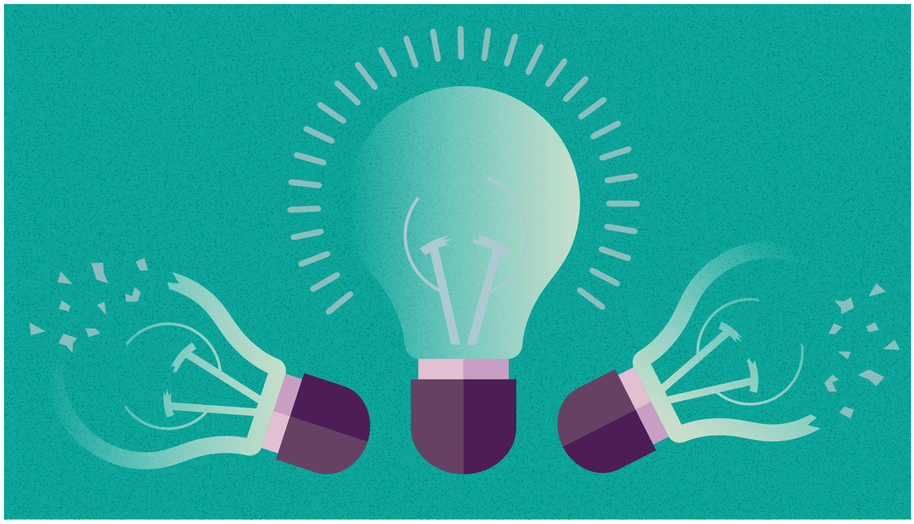
COLUMN

It's good to have lots of bad ideas

Science is all about sorting the wheat from the chaff, says **John Kirwan**.



ADAPTED FROM GETTY

Pauling's principle of electroneutrality states that each atom in a stable substance has a charge close to zero. But the physicist Linus Pauling, a two-time Nobel prizewinner, also gave us another important, if less well-known, dictum: that if you want to have good ideas, you must have lots of ideas and learn to throw away the bad ones.

This dictum (quoted by Francis Crick in his 1995 presentation, 'The Impact of Linus Pauling on Molecular Biology' at Oregon State University in Corvallis) implies at least two corollaries: that you must be willing to generate many ideas, and that some will be bad. It might be useful to see whether this applies to you. Are you generating lots of ideas? What proportion of your ideas turn out to be good ones? Do they lead to publications? Are you discarding the ones that don't work out?

I decided to see whether this particular principle applied to my own academic-research activities. I have an *h*-index of 55 (meaning that 55 of my papers have been cited at least 55 times each) and an *e*-index of 82, meaning that these 55 papers have collectively accumulated nearly 10,000 citations. Those indices suggest that I've been reasonably successful as an academic rheumatologist at the University of Bristol, UK, where I'm an emeritus faculty member. Here's what I wanted to find out:

- How many projects had I started?
- How many had born fruit in the form of a published paper?
- How much work had they involved?
- How long did they take to get into the scientific record?
- Were any of the ideas any good?

I found 185 project folders in my computer dating from 1994, each containing correspondence, data files, draft papers and other content that helped me to understand how much effort I had expended on that project. I went back through each and found the date of the first file entry. Using the content in each folder, I devised a scoring system for the amount of time and effort I had put into each project. Then I created a classification scale for what came out of each project idea, based on what actually happened (from 'thought about it' through 'moved forward' to 'produced some data' and 'several publications') and how I felt about the consequences of the project ideas that had been published ('built on' by others and 'especially good' because it led to a great deal of subsequent research).

Critics may argue that these scoring systems are entirely subjective — which of course they are. But that does not make them worthless. I calculated all the work-input scores before any of the output scores, and based the important output scores principally on publication production, a metric that is clearly verifiable.

How might you do this yourself? Thanks to publication and citation tools, you can keep an up-to-date track of outputs, maybe including your citation rates. What about your input effort? You could annotate your electronic diary with the time you spen on projects or ideas, using a code of your own devising.

In my own case, I found that I had spent about two-thirds of my effort on projects that never produced a published paper (see 'Ideas factory'). Of all the project ideas I started, only 25% ended up with one (or more) publication.

It was a relief to find that, on average, I had spent more time working on each of the ideas that turned out to be publishable than on those that were not. It became clear that once I did have a good idea, I worked on it a lot. Indeed, the correlation between the output from a project idea and the proportion of work I put into that project was high, at 0.73, which did reflect my good intentions. But it was a surprise to find how long it took for some of these papers to hit the press: for half of the projects, there was a gap of more than 4 years between the idea and the publication of the first paper. For some, the interval was 8 or 9 years.

Has my career been true to the Pauling dictum? By my own values, only 5% of my projects were built upon and only 2.7% (representing 6.7% of my work effort) were 'especially good'.

It looks like Pauling was correct. Thinking of and testing ideas that do not work out is not a waste of time and effort — it is an integral and necessary part of successful research. ■

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SOURCE: JOHN KIRWAN

IDEAS FACTORY

By far the majority of effort goes into work that never results in a publication, and only one-quarter of projects generate publishable results.