Commentary: it is a good idea to have bad ideas in science.

Abstract

There are no truly bad ideas in authentic science. We need to embrace science as a process-driven human endeavour to better understand the world around us. Products are important, but through better transparency, we can leverage ideas, good and bad, ours and others, to do better science. In a brief commentary here inspired by a recent discussion of the topic, it is proposed that whilst it is a good idea to track ideas and all the processes that generate outcomes such as publications, there is inherent merit in all scientific ideas. That said, organizing and framing our ideas into the networks that we already use to explore and examine hypotheses and questions in science and considering the linkages between ideas, data, and projects we produce as scientists, we can enhance and diversify the value of the work we do individually and collectively.

Preamble

A recent editorial suggests that science is all about sorting the wheat from the chaff (Kirwan 2017). To be clear and fair, the author conceptualizes bad ideas using suggestions from eminent scientists on the value and necessity of bad ideas to the advancement of good science. The bad-idea science paradigm proposed is a simple dichotomy defining good ideas as those that generate publications and bad as those that do not. This is a functional taxonomy for the purposes of a self-assessment of work done (i.e. effort allocated) and project ideas by the author, and to the defence of this analysis, every effort was made to qualitatively include ideas that were 'built on' for subsequent positive outcomes, i.e. publications. This working definition is an absolutely necessary and convenient short-cut, but it is not the end of the story. The accountability of ideas to the progress of a discipline such as ecology is not a new idea (Aarssen 1997) nor without debate (Weiner 1999). Common ground suggests that we should individually evaluate the merit of our ideas if for no other reason then to better prepare our work for the review process of others. We can even conceptualize some of the less productive ideas as stepping stones to more useful ones or as a counterpoint to frame and anchor the relative merit of those that succeed in whatever capacity we elect to define the positive outcomes. Creation, divine or otherwise, is not a perfect system. Evolution needs mutations. That ideas are not necessarily bad if they do not either directly or indirectly generate publications is perhaps too limiting. New ideas are novel, and new and useful ideas are creative (Runco and Jaeger 2012). The former can become the subtrate for the latter. Useless today can become indispensible tomorrow. Introspection of our workflows and the relationship between ideation and implementation can streamline the mapping of ideas and hypotheses to effective testing through evidence. However, we also need to ensure that ideas do not get lost through self-criticism, lack of data, availability of a mechanism to test today, or the extensive volume of information we process. In the spirit of replication science (Brandt et al. 2014; Mulkay and Gilbert 1986), I examined my relative idea management and outcomes to explore whether this more generous interpretation of merit and my workflow kept most of the wheat and some of the chaff.

Self-study of idea successes

A simplified, direct replication of the concept of idea merit self-assessement from the editorial that inspired this commentary was done. A check of idea efficacy scrapes the files and structures (hosted locally, but see below on how this landscape is changing) used to support a scientific workflow. The author in the former self-study examined all projects completed in career, recorded initiation date, scored effort for each, and developed an outcome classification scale that included direct and indirect publications from each project (Kirwan 2017). A project was defined as a 'project folder' that likely represented parent directories organized around each independent research thread. The author concluded that 25% of projects generated a publishable result. The assumption of this workflow is that every scientific task is assigned to/nested within a project folder. The folder dimension of my scientific workflows is similar but not a perfect match (and it is fundamentally evolving). I use project folders to organize protocols and ideas, dataset folders for data and some other forms of scientific evidence such as camera trap pictures, an 'idea archive' parent folder to store all ideas, and a

'papers-in-progress' parent folder with subfolders to store ideas that have some development in written form. However, this workflow and structure for organzing the processes that support scientific inquiry is dramatically changing thanks to GitHub and RStudio where I couple code, annotation, and written interepretations with the associated datasets. Scoring effort and time allocated, pre-Github and without effective, time-stamped version control was not viable in terms of reproducibility with my former workflow and its files. Consequently, I restricted analyses primarily to counts of both folders (parent and child) and files. Hence, I use a more topological approach to model the merit of ideas herein.

Literature cited

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