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Preliminary Reports and Negative Results in Life Science and Humanities

Interview with Prof. Robert D. Rafal

Ten Key Questions on the Importance of Preliminary Reports and Negative Results for Scientific Progress.

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Abstract

Dr. Robert (Bob) D. Rafal (Robert Rafal - Google Scholar) is a prominent neuroscientist and neurologist known for his work in cognitive neuroscience and experimental neuropsychology, particularly in the fields of attention, visual processing, and neuropsychology. His career includes a focus on the neurological underpinnings of visual attention, examining how brain injuries and lesions in specific brain regions—like the parietal cortex and superior colliculus—affect cognitive and perceptual processes. His current research employs DTI tractography to visualize subcortical neural circuits for orienting toward threat and for integrating alocentric and egocentric reference frames in encoding memories. Bob is currently Adjunct Professor - Senior Scientist at University of Delaware.

Preliminary Reports and Negative Results in Life Science and Humanities has interviewed Bob asking him to answer 10 questions on the relevance of preliminary reports and negative studies for science advancement.

PRNR: ***In your opinion, what is the significance of negative results in neuroscience research, and how can they contribute to the advancement of the field?***

Bob: Let me start off by thanking you for your efforts in launching this journal. It's a timely effort to address an emerging consensus that the conventional peer review system is not optimized for truth-seeking in science or the humanities. The problems are both epistemological and sociological: Epistemological because, in the current environment, the data available in the literature to the research community, and which drives ongoing research, is biased to publish 'novel, new findings' - which is all well and good – and *against* publishing negative results and non-replications; sociological because scientists are human and the current environment incentivises this state of affairs. What's needed are measures to remove disincentives against rigor and transparency. For example, to discourage the temptation to bias the process of data collection by p-hacking, some journals have simply gotten away from reporting p values. So, for me, the question is how do we promulgate scientifically virtuous incentives – and what role can this journal play in remediation of the problems imposed on scientific progress by the foibles of human nature.

PRNR: ***How do you view the current publication bias towards positive results in scientific literature? What are the implications for neuroscience research?***

Bob: On the one hand, the bias is understandable. Man bites dog is news; dog bites man is not. A 'positive' finding may be new news; a negative finding provides useful information but not 'news'. Personally, I don't think that the hand-wringing over the 'replication crisis' is warranted. It's in the nature of scientists to jump on exciting new findings and explore them; and if the results can't be

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replicated, we find that out sooner or later. Furthermore, in many disciplines, like experimental psychology, 'exciting new findings' tend to get published as a series of experiments that include replications that define the boundary conditions for the phenomenon under study. Nevertheless, my sense is that these inherent safeguards protecting scientific progress are becoming degraded. With the proliferation of neuroscience journals, and an increasingly competitive academic culture, it seems to me that many more 'novel and important' experimental results are being rushed to publication. And, of course, there is the *ipsi dixit* problem that deters a replication challenge when a recognized maven in a given field reports a 'novel and important' result.

PRNR: ***How do you think preliminary reports and negative findings can influence the direction of ongoing research projects?***

Bob: Here I'd like to focus on the value of 'preliminary reports'. For certain problems there is a special place for preliminary reports. Sometimes research generates unexpected or counterintuitive findings that can change the way we think about things, not only in the investigator's own particular field, but also in other disciplines. The finding may support an interesting new hypothesis, but the investigator(s) may not have access to the methods necessary for the preliminary finding to be sufficiently convincing to reviewers to get the work published. Here's example from my own research. I use DTI tractography as a tool in analyzing brain lesions in neuropsychology case studies. Let's say that tractography visualizes potential anatomical connectivity between two structures that were not previously known to be anatomically connected. If tractography is truly demonstrating a previously unknown white matter pathway, the finding could have a profound impact on the direction of future research. However, knowing that tractography can (and often does) visualize spurious connectivity (i.e. connectivity that doesn't actually exist in the brain), reviewers are not likely to recommend publication without some kind of converging or supporting evidence. There are a number of ways of validating the veracity of the putative connections, the gold standard being a tracer study in non-human primates. But neuroanatomists who work with non-human primates are unlikely to do the necessary experiments unless some preliminary evidence is available to attract their interest to the problem. The opportunity to publish preliminary findings could prove particularly valuable in identifying interesting hypotheses that require validation with converging evidence from several laboratories with specialized investigative tools.

PRNR: ***What role do you believe methodological transparency plays in the interpretation and utilization of negative results?***

Bob: I'm not sure what you mean by methodological 'transparency'. If you mean that researchers who are reporting their work should provide sufficient methodological detail to permit replication, that has always been the scientific standard for publication. I can think of an example where lack of methodological detail was a barrier to interpretation of negative results. In the early '80s there were a series of neuropsychology papers that examined word priming effects in aphasia. Different communications reported priming deficits in some patients, while other papers reported aphasic patients with normal priming effects. A later study published in the early '90s examined word priming in groups of neurological patients with lesions in different brain regions. That study reported that patients with damage to the left hemisphere (anterior or posterior) showed no priming effect. Now, that study tested patients with chronic lesions (2-13 years post-stroke). I wondered (and still do) whether the loss of priming in that patient cohort with chronic lesions might reflect a compensatory mechanism that develops during recovery to prevent patients from making naming errors. So, I went back to the earlier papers that found normal priming effects to see if those patients

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had been tested in the acute period shortly after the stroke. None of those papers provided any information about the vintage of the patients' injuries.

PRNR: ***What strategies do you recommend for encouraging researchers to share negative results and preliminary reports more openly?***

Bob: Well, researchers are encouraged to share some negative results as posters with published abstracts. But the bias against publishing negative results militates against prioritizing submitting them for publication in a peer- reviewed journal. But a published conference abstract doesn't provide the methodological transparency that allows critical comparison with a previously published (and potentially influential) positive result – so the negative result reported as a conference abstract can easily be discounted by the field. So, a peer reviewed journal for negative results seems like one particularly good strategy.

Of course, I don't mean that the Journal is an appropriate venue for just any 'negative result'. For the Journal to have the desired impact, it's important that papers published in the Journal report negative findings that are designed to test an interesting hypothesis, to test the boundary conditions for an effect, or to test the validity of a published 'positive' result of sufficient importance that it cries out for replication; and, of course, it is critical that negative findings are reported with sufficient methodological transparency to permit replication and undergo rigorous peer review.

PRNR*: How can the scientific community better support the inclusion of negative results in academic discourse and literature?*

Bob: As far as I can tell the scientific community is open to consideration of negative results in academic discourse – when they can get published. The problem, it seems to me, is incentivizing their publication in peer-reviewed journals.

PRNR: ***Can you share an example from your own research or experience where a negative result led to unexpected insights or a change in perspective?***

Bob: Sure, here's one. Another example, that particularly concerns me are those negative results that are reported *covertly*, and perhaps erroneously, due to lack of transparency. In papers reporting fMRI findings one frequently reads that such-and-such a task activated the left (or right) areaX, implying a hemispheric functional asymmetry, while never reporting a statical test to assess whether there is actually a reliable difference in activation between the two hemispheres. This omission tends to lead to faulty inferences that carry forward into the literature: left (or right) area X, but not right (or left) area X mediates function Y. This conclusion may, or may not, be true. There are more than a few reports from the fMRI literature reporting task activations in one only hemisphere, whereas studies of neurological patients report that the task is disrupted by lesions in either hemisphere.

PRNR: ***In what ways do you think disciplines within the humanities can learn from the treatment of negative results in neuroscience?***

Bob: I guess I don't have any idea what a 'negative result' in the humanities would be.

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PRNR: ***As the landscape of scientific publishing evolves, what changes would you like to see regarding the reporting of preliminary data and negative findings in neuroscience?***

Bob: In one sense there are already incentives to publish negative results. It is said, with some truth to it, that academic promotion committees can count but they can't read. So, there is already some incentive to add to publication numbers by reporting the experiments that don't work as expected. And the adoption of some journals of publishing registered studies may be a step in the right direction. The practice of agreeing to publish results of a registered study, even if the result is negative, acknowledges that, if a proposed experiment is worthwhile doing because it tests an interesting hypothesis, then a negative result can be as valuable as a positive one. They can tell us that the hypothesis may be wrong, that a previously published positive result may be misleading, and they can help define the boundary conditions for a previously observed effect. I'd like to see more editors (and PhD supervisors) look at things this way. I also think that the emerging culture that rewards the publication of single experiment, short publications in 'high impact', fast turn-around journals (with editors requiring reviews to be submitted within 2 weeks), encourages investigators to submit as quickly as possible (to avoid being 'scooped') is a problem. The increasingly competitive environment encourages scientists to submit papers without first doing the kind of replication and extension that is necessary to confirm their findings and establish the boundary conditions for the experimental effects. Having a venue for reporting preliminary results can give authors the opportunity to plant a flag on their discoveries, while pursuing work to replicate and extend their work.

PRNR: ***What advice would you give to early-career researchers facing challenges in publishing negative results?***

Bob: Here, let me address what I think is the root of the problem, since I fear that we are looking for ways to address the symptoms of the problem (making negative results accessible) rather than its cause. Thucidides said: "That nation that makes too great a distinction between its scholars and its warriors will have it thinking done by cowards and its fighting done by fools'. I fear that the growth of highly competitive, 'big science' incentivizes the wrong rewards and engenders cowardice in our young (and older) scholars. This highly competitive environment tempts the taking of shortcuts, omitting results of analyses that 'don't fit' with the authors' conclusions and a growing problem of academic dishonesty, especially in biomedical research where the stakes are so high.

Young investigators need the right incentives that allow their curiosity to guide their science, and encourage their science to be solidified by systematic replication and transparent reporting that weighs both positive and negative findings. To answer your question, I'd advise them that they now have a venue for reporting negative results. The challenge will be to incentivize early-career researchers to report them.

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