Expect the Unexpected

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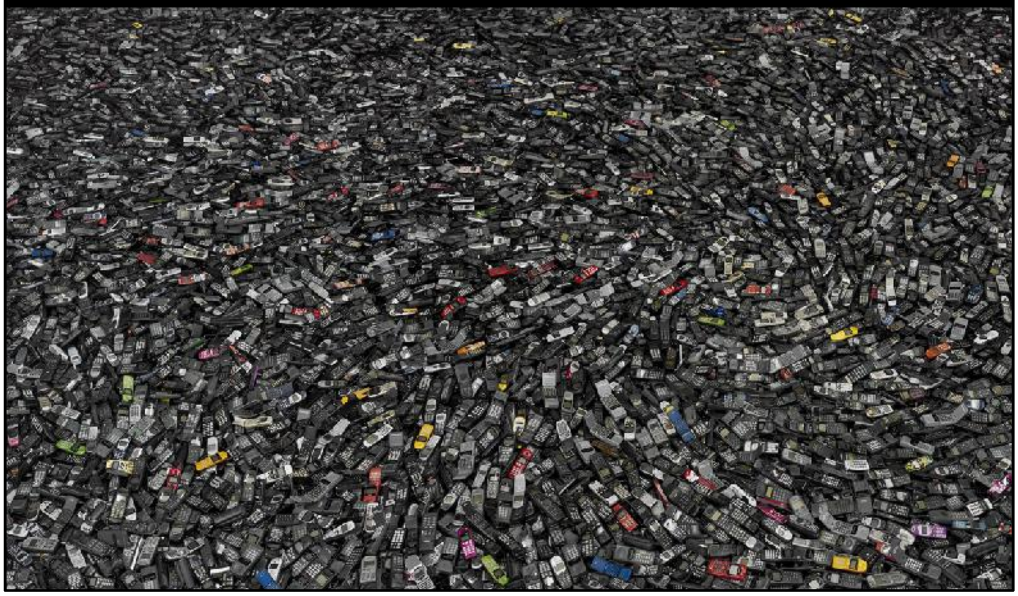
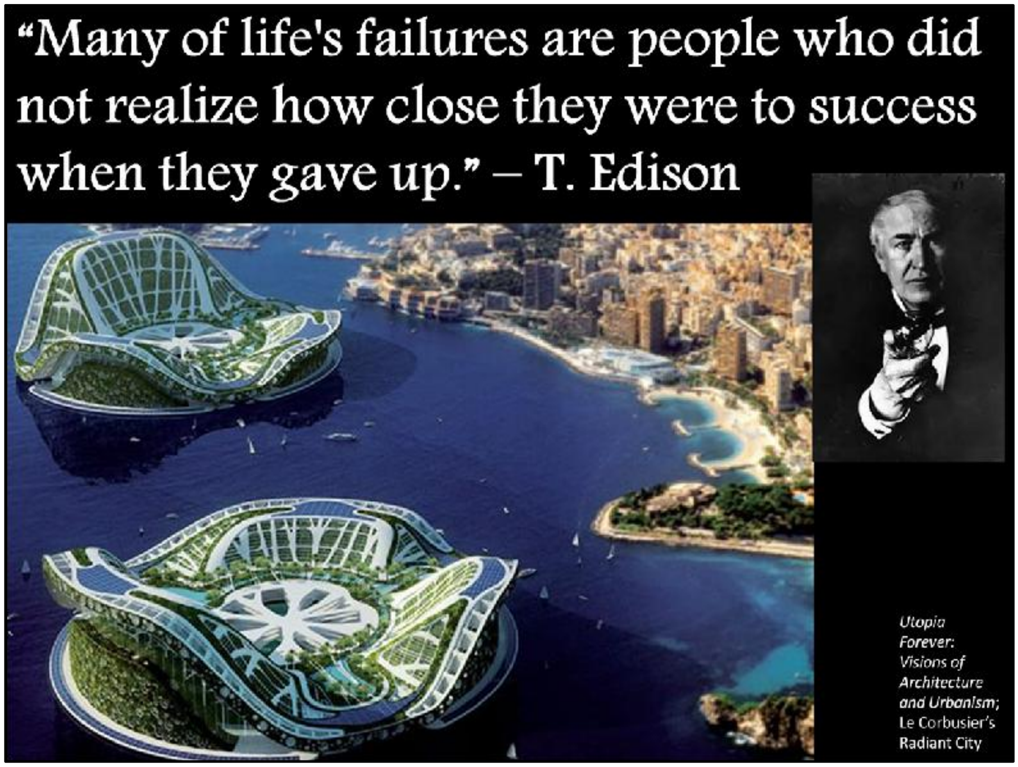
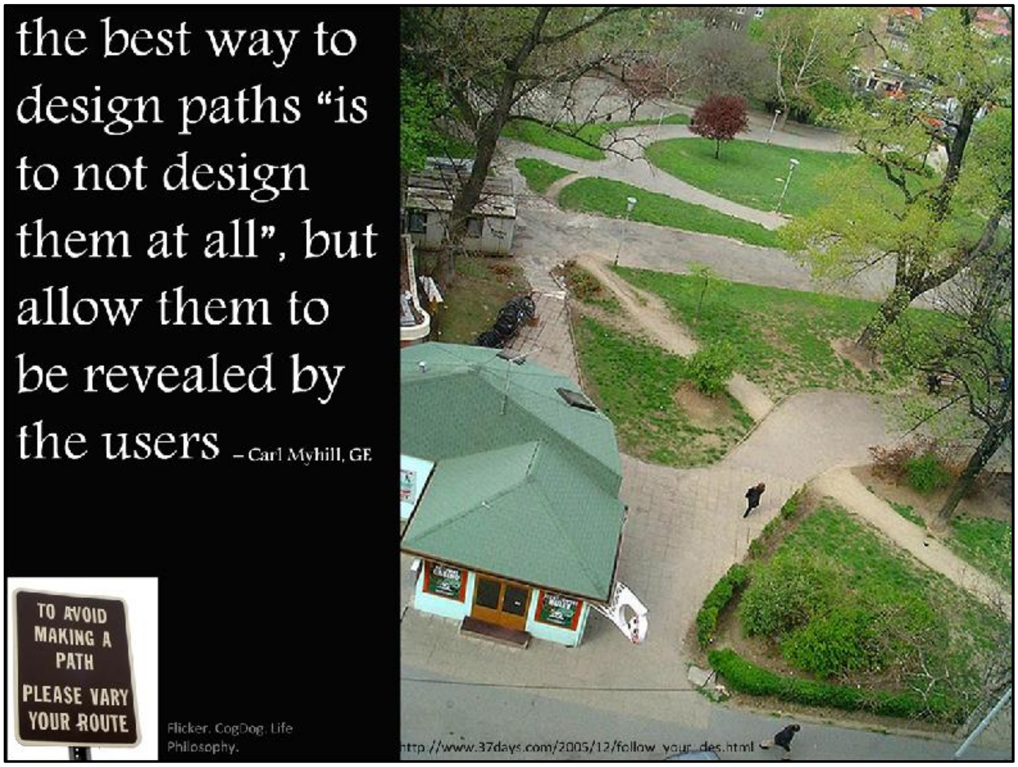
Environmentally conscious design can seem impossible



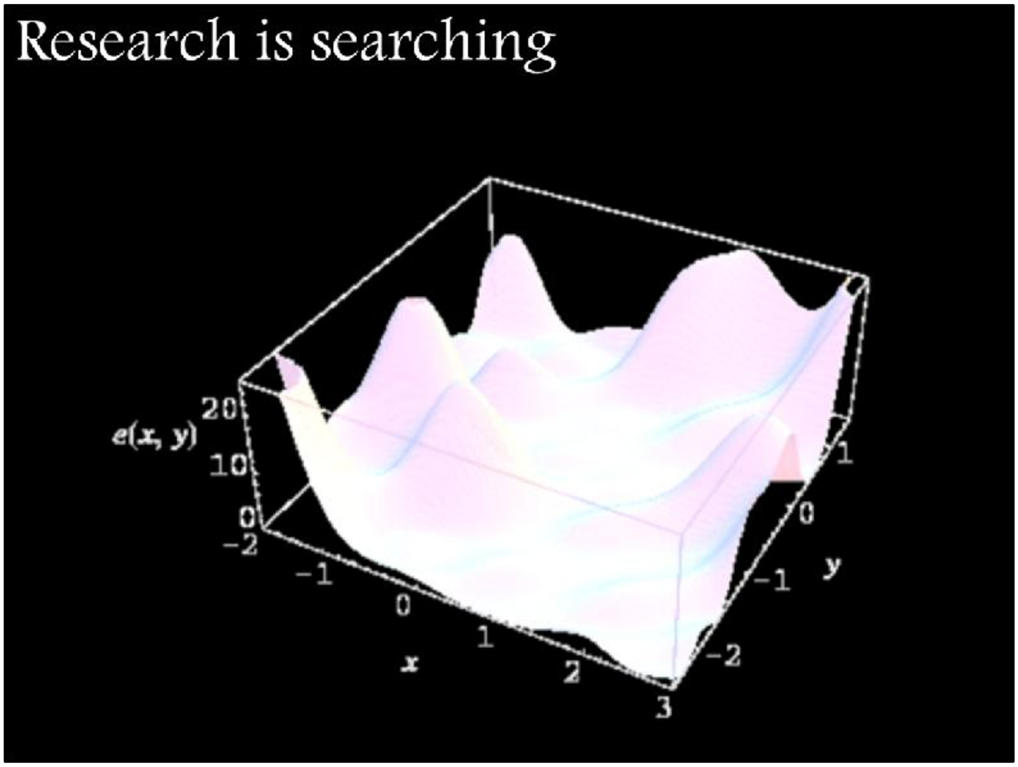
This past semester, I was privy to a conversation between a TA of an Eco-materials and manufacturing class during which the student noted that “nothing is green” the more she learned about environmental analysis the more impossible sustainability seemed. The TA then agreed with her, noting that he was leaving the field to pursue another field of analysis.



I should have chimed in and quoted Thomas Edison who found 10,000 ways that light bulbs wouldn't work. "Many of life's failures are people who did not realize how close they were to success when they gave up." Sustainability is a long-term goal. Some ideas are terrible, sure, but many negative results and backfired designs are helpful in the long run. So I would like to extend this mantra to the greater area of design research and propose that we work harder to expect and embrace unexpected or undesired results.

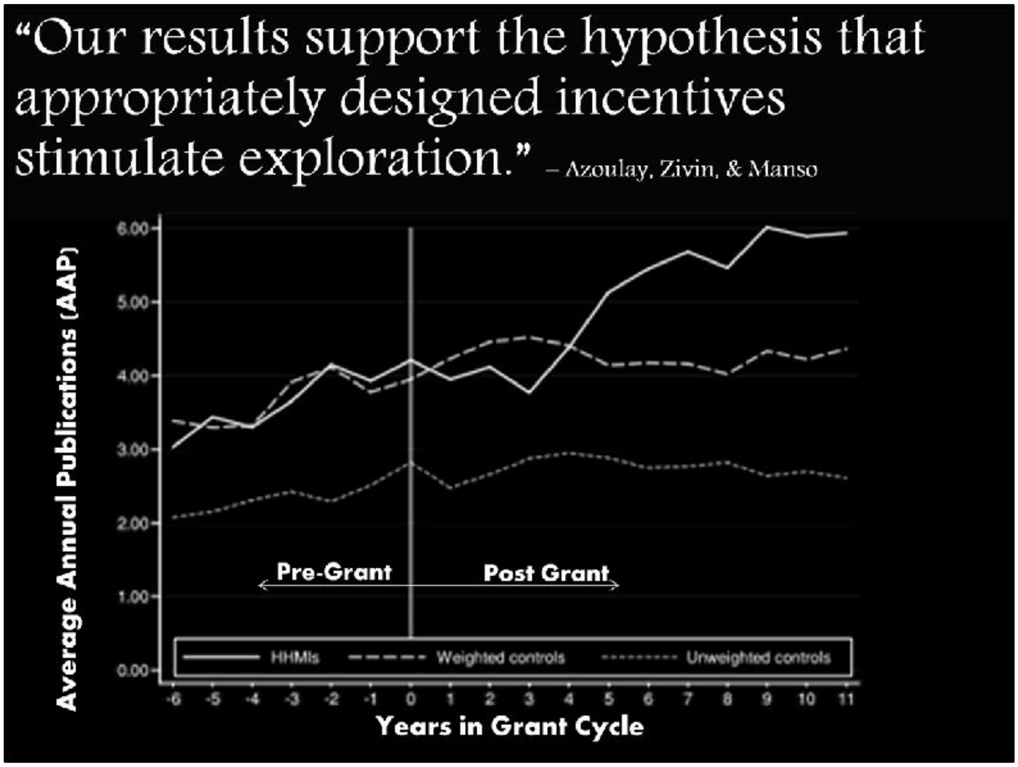


In Urban Planning, the best way to design paths is not to design them at all, but allow them to be revealed by the users. This photo shows desire lines, paths created by frequent traffic where no planned path previously existed. In examples of university campuses, only the major pathways were constructed and the alternate routes were revealed by eroded grass. In this case, failures of urban planning revealed the best routes for development.

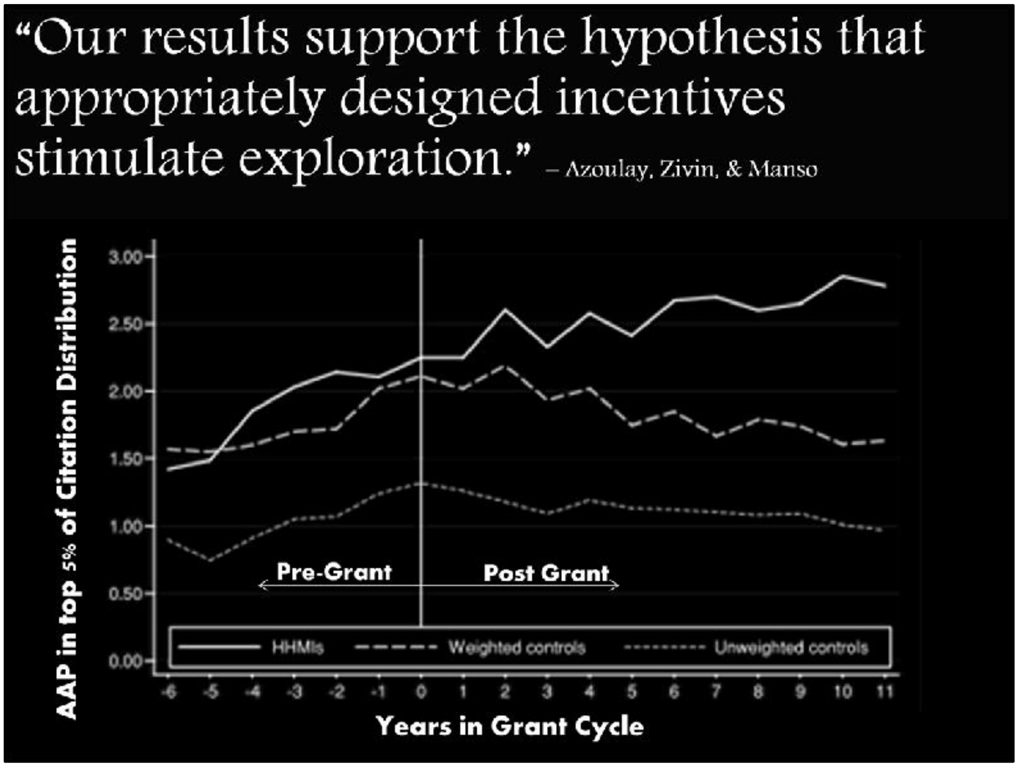


Now you may say, “Well, that’s all very well and good, Cassandra, but I already allow my research to develop on its own. I already talk to users and help my students develop their own research pathways.” To which I can only say, “Of course we do! We have to; but are we doing it enough?”

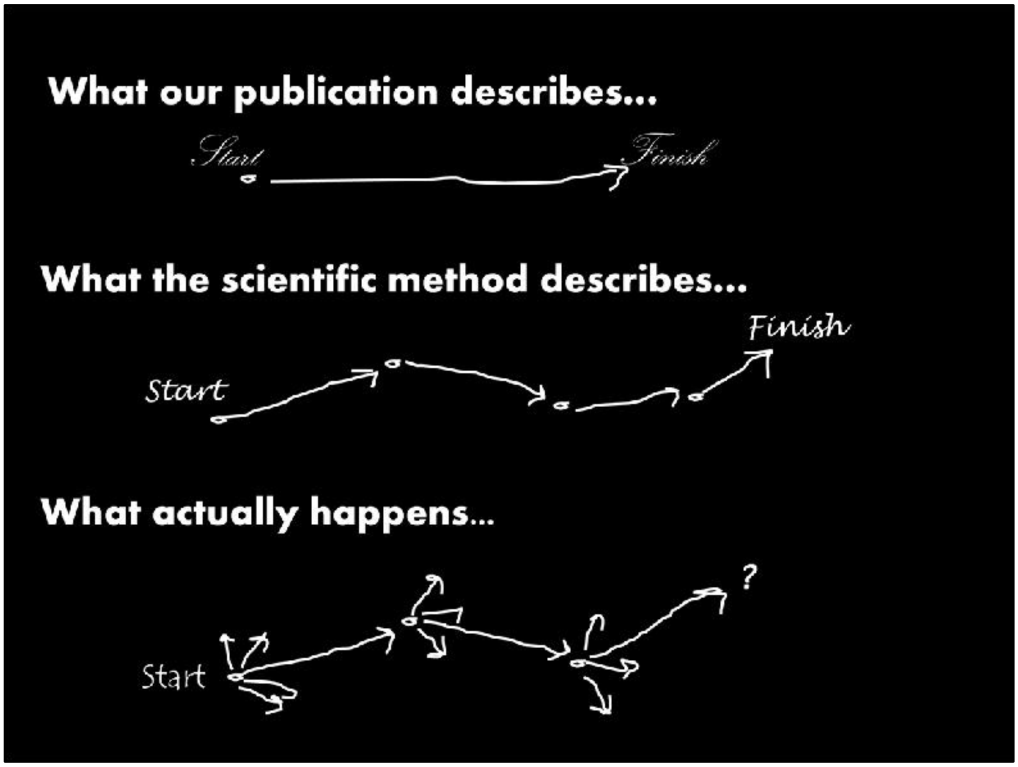
If we were to make an analogy with optimization, do we spend too much time exploiting current ideas for incremental gains and not enough time exploring and risking failures, failures that could provide equal or greater gains?



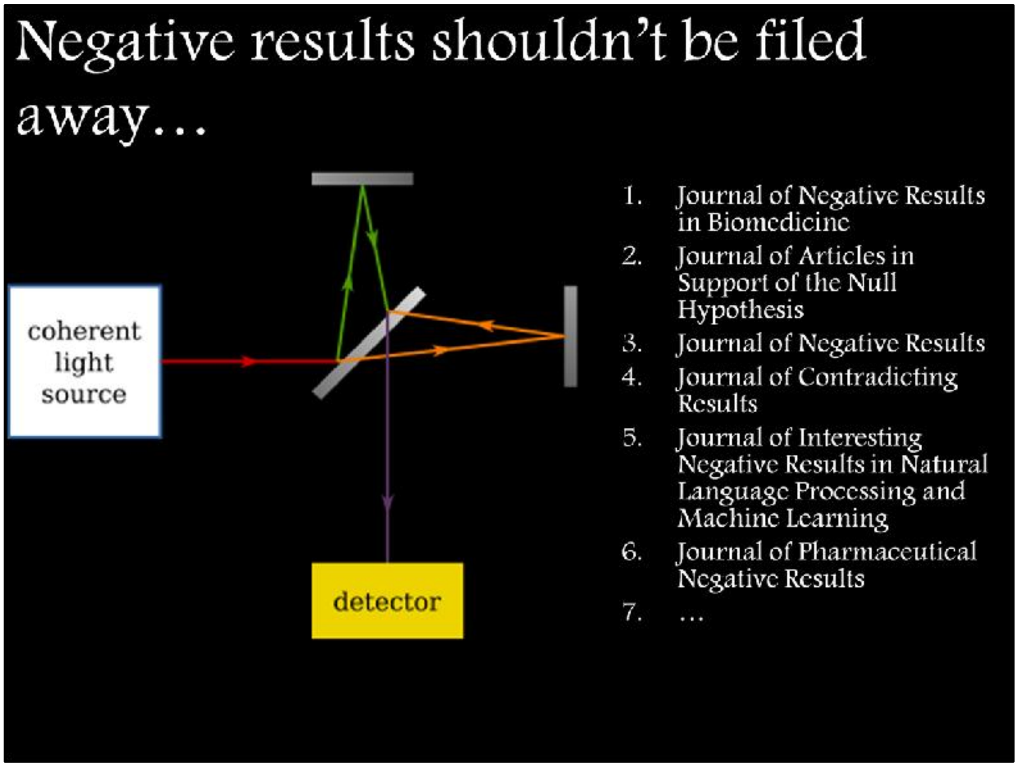
That is the hypothesis in some of the natural sciences. In a study of HHMI and NIH investigators, researchers at UCSD and MIT found that the HHMI funding program which allows time for failure and experimentation early in the grant cycle, yielded more higher impact papers than similarly accomplished NIH and young career award winners. Here we see the average number of yearly publications before and after the grant cycles.



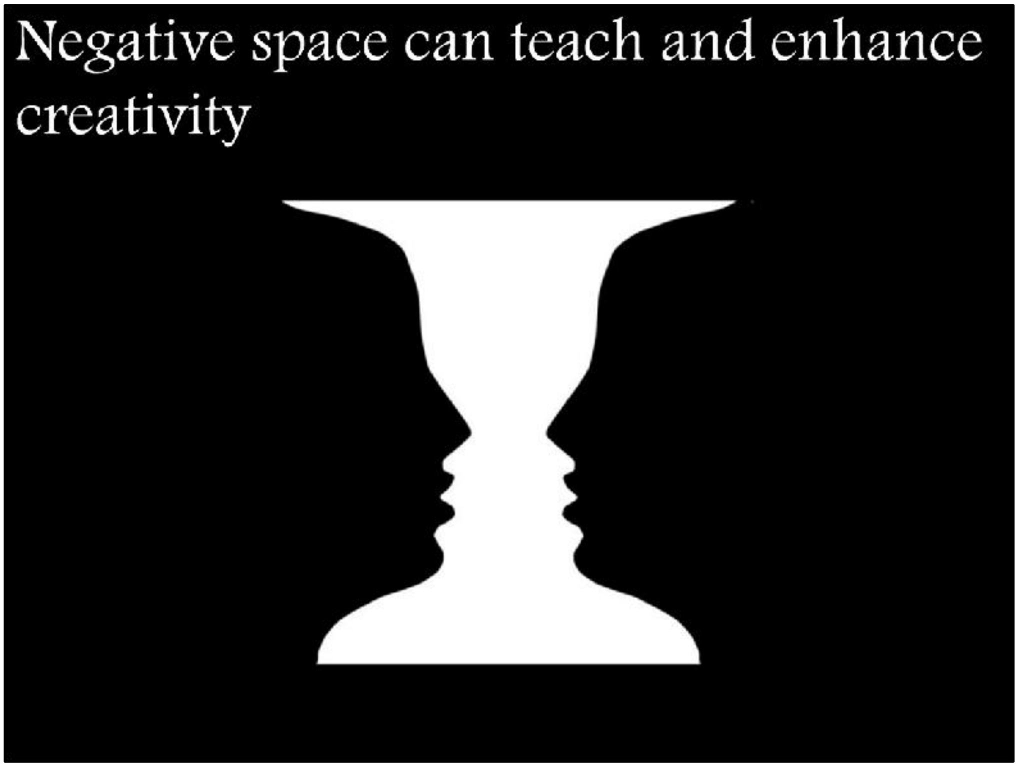
And here we see the average number of yearly publications in the top 5% of the citation distribution, before and after the funding cycle. Now, of course, not only did the HHMI grants have a much high rate of success, but they also had a higher rate of failure. In this study, the HHMI grantees published 96% more high-impact papers, and 35% more papers that failed to surpass the impact of pre-grant publications.



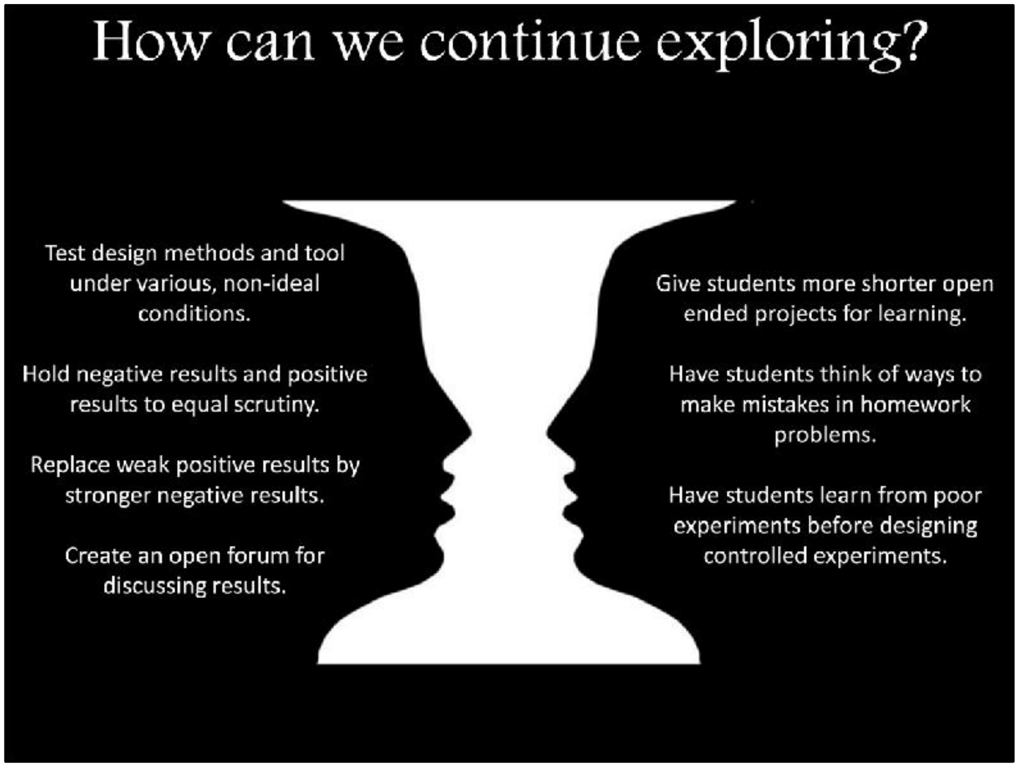
These results make intuitive sense. How often have you had your first idea be a success? As Linus Pauling said, “The Best Way To Have A Good Idea is to Have a Lot of Them.” But within academia, the way we report results, the way we are evaluated, expects our method of development to follow this straight line. Or, at best, a series of straight lines between points of interest. In reality, we experience a lot of false starts, individual failures that we either file away or keep to ourselves. Maybe we generalize from these lessons and share them by word of mouth, but we don’t publish failures or seek them out.



I think our field has entered the age where we should start seeking negative results in order to advance. Michelson and Morely were convinced that there was ether; they designed a model experiment and got negative results that changed physics. But we shouldn't have to be Michelson and Morely, to explore null hypotheses. In the last decade journals that publish interesting negative results, what one might consider failure have gained traction in the life sciences, psychology and language processing and machine learning.



Thinking about this negative space can not only progress our research, but also aid in the classroom. Jack Matson is a vocal proponent of the idea of “Intelligent Fast Failure.” A creativity enhancing pedagogical and research technique that requires us to think about breaking down a big idea into a number of concurrently testable parts. The alternative is “Slow Stupid Failure”, where ideas are tested sequentially, not reducing the impacts of failure, or worse, even recognizing failure. His class on innovation graded the students on how many failures they experienced, how well they tested their ideas, and the creativity of their risks. To bring it back to the optimization analogy, Jack Matson graded on the quality of the search method.



I think that as we develop more of the research space in design, as the world of research problems gets bigger and more complex, quality research will yield more compelling negative results. And quality teaching will give students more, fun opportunities to learn from failure in the classroom.