

Pyramid of ideas: the art of generating novel research questions



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Pioneering research ideas are critical to advancing science and the ability to generate them is a hallmark of scientific leaders. Another professor once remarked to my undergraduate advisor, “Please, no new ideas before lunch today”. Wow! How does one generate so many new ideas that colleagues are bothered by them? As students, formulating innovative and fundable questions is a challenging task, yet we receive little training on how to do this.

I asked a few dozen successful scientists – mostly academic and federal-agency ecologists, atmospheric scientists, and biogeochemists (eg DELS 2006) – how they develop original ideas. I found, as in any such endeavor, that while there is no standard recipe, there are common ingredients. Many respondents invoked the adage, “There is nothing new under the sun”, emphasizing that the generation of novel research questions requires adjusting personal expectations. Finally, I found that, as students, we hold onto “our” ideas too tightly.

In developing a thesis topic, we often hear similar advice: review the literature, attend seminars and conferences, note what is funded, consult peers, and spend time in the field or laboratory. Less often articulated in my survey, but equally vital, was the warning that we are wasting time if we do not formalize our critical thinking during these activities. Asking yourself a list of set questions (eg What are the implications of this work? Do the data support other conclusions? Did this paper reinforce ideas or change my mind about something?) is an excellent heuristic technique to regiment analytical thinking. Chatting, free-writing, listing, and mapping with Venn diagrams were also suggested as ways to generate creative ideas.

Nearly every person I spoke with kept a research journal. Those who did not said they knew that they should. Why? Because writing permits the mind to forget, move on, revisit, and remember. Robert Frost (1931) claimed, “To learn to write is to learn to have ideas”. It seems, then, that leaders of original research would do well to greet their new students or employees with a blank notebook, with a list of questions to aid critical thinking pasted to the inside cover. A plant biotechnology professor shrewdly pointed out that keeping a journal can also establish idea ownership, should the need arise.

While proprietary disputes are probably less frequent in ecology, this does raise an important consideration:

should students expect, or be expected to generate entirely new ideas? Professors told me that new graduate students often assume that they will develop a completely new idea, a prospect so unrealistic that it can be restricting. One professor of aquatic ecology described a doctoral student who had produced sufficient ideas for his dissertation work, yet kept searching for something revolutionary. The ground-breaking idea never materialized, but the student did eventually graduate – after 8 years!

Recognizing that a *mostly* new idea is ample to proceed is important (Figure 1). On this point, the only Nobel Prize-winning ecologists had careers marked by comprehensive, incremental studies, not monumental leaps. A 1973 Nobel Prize was awarded to Karl von Frisch, Konrad Lorenz, and Nikolaas Tinbergen. Best known for their research on the language of bees, fixed-action patterns such as imprinting, and responses to supranormal stimuli, respectively, they advanced behavioral biology by building on earlier ideas. Success is not guaranteed with entirely new ideas, nor is it prohibited by mostly new ideas (Malakoff 2001).

Related to the propensity for weak critical analysis and expectations of a revelation is the beginner’s predilection to treat his/her ideas as offspring. I admit that I am substantially more motivated by my own ideas than by those of others and that this precludes the detachment I need to examine their quality. One geneticist advised that students should first attempt to logically kill their ideas on paper. If you are able to “murder your darlings”, then you are more likely to be able to judge their validity.

The problems caused by “parental affection” for personal ideas have long been recognized and the method of multiple working hypotheses has been offered as a way of minimizing this interference (Chamberlin 1890). Briefly, develop several opposing hypotheses about the idea you



Figure 1. Few scientific advancements are as revolutionary as Thomas Edison’s invention of the incandescent light bulb. Still, incremental improvements upon existing ideas, such as compact fluorescent light bulbs, can represent scientific progress.

wish to explore. Because some of the hypotheses will be contradictory, some (maybe all) will be false. This approach reduces our partiality for a particular explanation and allows us to weigh the merits of the idea.

While research veterans have the experience to appreciate the rarity of entirely novel ideas and the tenacity with which we hold on to our own, this does not typify the novice (Ford 2000; Karban and Huntzinger 2006). So what should we expect as students?

Perhaps Charles S Elton's pyramid of numbers, an early ecological concept, can serve, metaphorically, to describe a realistic perspective (Elton 1927). Elton's pyramid describes energy flow in ecosystems as a horizontal bar chart, describing the number of individuals at each trophic level. The number of organisms declines as the level of the pyramid – and hence, trophic level – increases. This is due to the large amount of energy lost (about 90%) during transfer between trophic levels. In the context of generating novel research questions, expect a tenth of your ideas to be worth serious development, a tenth of your worthwhile ideas to be feasible, and a tenth of your feasible and worthwhile ideas to be fairly original (ie untested and unpublished). Hence, critical

brainstorming has to be a nearly constant activity and generating multiple working hypotheses a creative reflex.

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Faculty response



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Collectively, scholars and researchers have various strategies for developing and inspiring new research ideas. Joseph Bump offers a wonderful summary of these strategies. To his catalogue of approaches, I can add one more.

As Bump mentioned, few ideas are genuinely new, and many seemingly novel ideas have been recycled from, or inspired by, other sources. Thus, I challenge myself to employ a certain reading strategy as a source of new ideas. The strategy is to spend roughly half of my reading time on scholarly articles and books that are squarely within one of the areas to which I devote my research, then to spend about a quarter of my reading time on scholarly pieces within the scope of life or environmental sciences, but clearly outside my area of research expertise, and finally, to spend the remaining quarter of my time on scholarly pieces from distantly related branches of knowledge, especially, but not limited to, disciplines within the humanities.

I have found this strategy to be both more important and more difficult than its simplicity may suggest. First, we are exposed to the anxiety-building pressure of trying to “keep up on the literature” within our field of expertise.

While you have a responsibility to be an expert within your chosen field, you have to decide what portion of the literature you need to read to achieve that expertise. Trying to read it all may be unwise and/or impossible (though I have successful colleagues who disagree).

Second, it is not enough merely to read outside your discipline. You need to find connections between seemingly unrelated ideas and your own research. Although finding such connections is challenging, the mind of an ecologist is well-suited for the task. Much ecology is about discovering and understanding less-than-obvious (ecological) connections.

Third, this strategy, taken seriously, will raise questions in your mind about other disciplines and branches of knowledge. These questions ought to cause you to engage in dialogue with scholars from those other disciplines. Such discussions provide the greatest prospect for developing ideas that transcend disciplinary boundaries – transdisciplinary ideas. As social processes continue to fragment and isolate the diversity of our collective knowledge, transdisciplinary ideas have the great potential for enriching and otherwise benefiting society.

Be warned – reading outside your discipline is difficult. Patience is required to learn the concepts and jargon, but the endeavor is worthwhile. For me, this approach has led to collaborations focused on the relationship between ethics and science. Finally, an aphorism to encapsulate the entire idea: are you more likely to develop a novel idea by reading literature that is very similar to what you already know, or by reading literature with which you and your colleagues are unfamiliar?