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Getting Good Ideas and Making the Most of Them

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Abstract

Good research ideas and hypotheses do not just magically exist, begging to be tested; they must be discovered and nurtured. Systematic methods can help. Drawing on relevant scholarly literatures (e.g., research on creativity) and on the published personal reflections of successful scientists, this chapter provides an overview of strategies that can help researchers to (1) gather research ideas in the first place, (2) figure out whether an idea is worth working on, and (3) transform a promising idea into a rigorous scientific hypothesis. In doing so, it provides pragmatic advice about how to get good ideas and make the most of them.

KEYWORDS: Ideas, hypotheses, creativity, research methods

Scientific progress occurs through a kind of evolutionary process: Scientists identify innovative new ideas and hypotheses about what might be true, and they use empirical methods to test them (i.e., to eliminate those that fail to meet accepted standards of evidence and to selectively retain those that do; Campbell, 1974; Hull, 1988; Popper, 1963). Both parts of this process are equally essential to scientific progress, but they receive unequal attention within scientific education. Scientists receive enormous amounts of formal training in methods to use and best practices to employ when testing ideas and hypotheses against empirical data. That's good. In contrast, scientists typically receive very little formal training in methods and practices that might help them to identify new ideas and develop new hypotheses in the first place. That's too bad.

Scientific ideas and hypotheses don't just magically exist, begging to be tested. They must be discovered and developed by scientists themselves and communicated coherently to other people in the scientific community. Just as the empirical testing part of the scientific process benefits from strategy and methodological skill, so too does this innovation part of the process. Systematic strategies can be used to increase the likelihood of being inspired with innovative ideas and to determine whether those ideas are worth pursuing or not. It takes both strategy and skill to transform an informal idea into a precise, logically coherent scientific hypothesis.

That is why this handbook includes this chapter. We've designed it to provide systematic methodological guidance—and pragmatic advice—about how to get good ideas and make the most of them.

Strategies for Gathering Ideas and Lots of Them

There is a lovely line in the novel *Of Love and Other Demons* (García Márquez, 1995, p. 56): "Ideas do not belong to anyone . . . They fly around up there like the angels." What you want is for some of those ideas to fly from the sky and grace your brain with inspiration. It's not merely luck; scientists can do things to make it happen, again and again and again.

A first rule of thumb: *Don't worry about whether those ideas are good ones or not*. This might seem counterintuitive because scientific training emphasizes methods to diagnose the rightness or wrongness of ideas. However, that diagnostic work comes later, and it cannot happen until *after* inspiration has occurred. A self-critical mindset is useful when designing studies, when analyzing data, and when drawing conclusions from those data, but it's counter-productive to creativity (Lam & Chiu, 2002.)

To invite inspiration that might be right, savvy scientists allow themselves to be wrong. At this earliest stage of scientific discovery, it is helpful to cultivate a mindset that is open to anything, including good ideas, mediocre ones, and even mistakes. (This rule of thumb is collected with ten more in Table 1.)

This open-minded perspective is encouraged by many philosophers of science. Paul Feyerabend (1975, p. 17) wrote: "Science is an essentially anarchic enterprise: theoretical anarchism is more humanitarian and more likely to encourage progress than its law-and-order alternatives . . . The only principle that does not inhibit progress is: *anything goes*." One reason why it is okay to adopt an "anything goes" approach to inspiration is because of the communal and self-correcting nature of the scientific process. David Hull (1988, p. 7) reminds us that "Science is a conversation with nature, but it is also a conversation with other scientists." We are allowed, even encouraged, to introduce ideas of any kind into that conversation because the conversation—the scientific process which follows any act of inspiration—judges those ideas rigorously and can refute those that fail to meet strict standards of evidence. The refutation of flawed conjectures is *essential* to scientific progress (Popper, 1963). As long as you commit in good faith to that rigorous process, there's little harm to making mistaken conjectures; they are common, inevitable, and can even be useful in unexpected ways. Again, we defer to a philosopher of science, Ilkka Niiniluoto (2019): "scientific theories are hypothetical and always corrigible in principle. But even when theories are false, they can be cognitively valuable." Any idea—whether "right" or "wrong"—has the potential to help point scientists in the right direction. And if an idea is downright dumb? No problem. Scientists are pragmatic; if an idea is unproductive it won't be pursued for long. Successful science is littered with wrong ideas. From Archimedes to Ahmed Zewail, from Ainsworth to Zajonc, every serious scientist has had them. They also had ideas that turned out to be right, and one reason they did was because they were willing to be wrong.

A second rule of thumb: *Really good ideas usually do not start out as really good ideas*. They often start out as vague thoughts, niggling questions, half-baked observations. One of us once started with nothing more than a catchy title. It eventually turned into an extensive, rigorous, multi-study research project (Bahns et al., 2017). The supposedly "catchy" title was never used, as it turned out to be less good than the idea it turned into. Another personal example: A random bit of laughably amateurish musing about infectious diseases blossomed—after conversations and collaborations with many people—into a multi-pronged program of research on the "behavioral immune system" (e.g., Murray & Schaller, 2016), within which dozens of new hypotheses have been generated and tested, with wide-ranging implications for human cognition, human behavior, and human culture.

Simply start with inspiration—even laughably amateurish ones. A promising idea will surely be improved, truly unpromising ones will be discarded, and the scientific conversation will help you sort out which is which.

Table 1
Eleven useful rules of thumb for getting good ideas and making the most of them.

Getting Good Ideas

1. At the early stages, do not worry whether your ideas are good ones or not.
2. Really good ideas do not often start out as really good ideas.
3. Expose yourself to diversity; new experiences promote creativity.
4. Do things that you actually want to do. Intrinsic motivation helps.
5. Inspiration is idiosyncratic. Try many things.

Making the Most of Them

6. Interact with other people. Talk, share, disagree, discuss, and agree.
7. Ideas with real-life relevance tend to find more people who are interested in them.
8. If an idea is too obviously true, people might not find it interesting.
9. Define carefully and precisely an idea's conceptual components and state their relations to each other.
10. Ideas do not belong to anyone; avoid identifying with "your" hypotheses.
11. Specify your assumptions explicitly.

Cultivating a Receptive Mind

Some people are more creative than others (Feist, 1998), but *everyone* has the capacity for inspiration, and *anyone* can discover useful hunches and hypotheses. To do so, one must be receptive. Research on creativity suggests some strategies that can help you cultivate a receptive mind.

A third rule of thumb: *Expose yourself to diversity*. Young scholars are sometimes advised to narrow their interests, or to focus their reading on the restricted range of academic literature that is most directly pertinent to their particular academic discipline. That advice may be well-intentioned, and perhaps even pragmatic in a short-sighted way, but it can inhibit inspiration and cramp creativity. The most creative people in the sciences tend to have interests and skills that transcend disciplinary boundaries (Root-Bernstein & Root-Bernstein, 2004; see also Chapter 32 in this volume). Successful scientists often find inspiration in their non-scientific interests, and their non-academic activities often nourish and serve their academic aspirations. Creativity is fueled by exposure to diverse people, places, activities, and perspectives.

Exposure to diverse cultures fortifies the cognitive foundations of creative thought and enhances innovation (Leung et al., 2008). You may not have to sojourn to a far-away land to benefit (but it can help; Maddux & Galinsky, 2009); cultural enclaves can often be found much closer to home. More generally, creative ideas may be stimulated if you strategically seek out cognitively challenging experiences. Try to learn a new language; spend time regularly with people whose norms and values and life experiences differ substantially from your own; visit with religious or political groups that are new to you. It can pay off.

A fourth rule of thumb: *Do things that you actually want to do*. This key to creativity is summed up nicely by Csikszentmihalyi (1997, p. S8): "Creative persons differ from one another in a variety of ways, but in one respect they are unanimous: They all love what they do." People are more creative when they do things that they find fun or enjoyable to do, and that they chose to do because of their personal interests or passions (Amabile, 1998). Best of all, people are more creative when they are happy (Baas et al., 2008). You are not just doing yourself a favor but may also be serving the broader goal of scientific innovation when you do things that you want to do. If you favor experiments, plan them. If you prefer applied work, apply yourself. If you prefer complex multivariate non-experimental analyses, disentangle away.

There will be times when you are unexcited, unhappy, and uninspired. Frustration, rejection, and bouts of burnout are common and normal, and there are good resources that provide advice on handling it (e.g., Jeremka et al., 2020). And you will sometimes be compelled to do things that other people think you *should* do rather than what you really *want* to do. Still, you can cultivate a creative mindset more effectively if you deliberately devote *some* of your time to activities that you are intrinsically motivated to do and that make you happy. After all, ideas are everywhere—in great books and trashy novels, films and movies, the lyrics of your favorite songs—and inspiration can strike not only when you're pouring over scientific papers but also when you're surfing the internet or walking in the woods or dancing with your friends. Some of these enjoyable activities might even be research projects. Designing a scientific study can be a fun. Designing a scientific study with your friends can be *really* fun. If you seek out projects that excite you, collaborators that you enjoy, and working environments that make you happy, you're more likely to be inspired with more ideas.

Idea-Generating Heuristics

Even if your mind is open, inspiration can be elusive. There are systematic strategies that scientists can use to develop worthwhile research ideas. McGuire (1997) provides a kind of catalog of strategies, identifying 49 "heuristics" that can be taught, learned, and used for the purpose of generating ideas. Many of these idea-generating heuristics involve reading the scientific literature and thinking systematically about what is and isn't known. McGuire (1997) applied different labels to these different heuristics (e.g., "Reversing the Plausible Direction of Causality"; "Conjecturing Interaction Variables that Qualify a Relation"; "Generating Multiple Explanations for a Given Relation"). Fancy labels aside, these heuristics generally represent different ways of reacting thoughtfully to research results that seem to be not-quite-completely-true—different ways of saying "Yes, but …": Yes, research shows that X influences Y, but maybe Y influences Variable X too? Research shows that X influences Y, but what if it sometimes doesn't (i.e., the effect occurs only under some conditions or is limited to specific populations)? Research shows that X influences Y, but why? (What is the underlying process? Is the proffered explanation the only plausible one?) Research ideas can be generated by addressing such questions thoughtfully.

McGuire (1997) also identifies idea-generating heuristics that do *not* require reading scholarly literature, but instead involve attention to everyday life (e.g., "Recognizing and Accounting for the Oddity of Occurrences," "Introspective Self-Analysis," and "Sustained, Deliberate Observation"). There is an important principle underlying these heuristics: The goal of social and behavioral science research is to learn about the full scope of human behavior, and the scholarly literature is inevitably more narrow than that. Within the psychological sciences, for example, Berscheid (1992) describes how the important topic of close relationships was mostly ignored when most psychological scientists were men. Regardless of why these omissions exist, *they do*. It is limiting to look for ideas only within the scholarly literature. As a psychologist Nisbett (1990, p. 1078) wrote "All of life is a source of psychological ideas"—but it's an important principle that applies to all the social and behavioral sciences.

The important implication is that you can discover many fruitful ideas by raising your gaze from your scientific studies and casting it upon the real world instead. Cialdini (1980, p. 22) describes what happened when he took a break from puzzling over a frustratingly small effect observed on a rating scale, and went to a football game:

> The crowd was suddenly up and shouting, and yelling encouragement to their favorites below. Arcs of tissue paper crossed overhead. The university fight song was being sung. A large group of fans repeatedly roared "We're number one!" while thrusting index fingers upward. I recall quite clearly looking up from thoughts of that additional half unit of movement on a 7-point scale and realizing the power of the tumult around me. "Cialdini," I said to myself, "I think you're studying the *wrong* thing."

The "*wrong* thing" was whatever that 7-point scale was failing to find. The *right* thing—the idea inspired by his fortuitous foray into the football stadium—turned into a productive multi-year program of research on group identification, self-esteem, and "basking in reflected glory." Cialdini also made additional, more strategic observational journeys beyond the narrow halls of academe, such as the sabbatical he spent learning the tactics used by car dealers and pyramid scammers and other people whose real-life livelihoods depend on successful persuasion (Cialdini, 2006). These observations led to many new research projects and seminal contributions to the social and behavioral sciences.

A fifth rule of thumb. *Inspiration is idiosyncratic*. Some heuristics might work better for some people, and others for others. Try everything and anything, and remember Feyerabend's (1975): "*anything goes*."

Other People Are an Essential Source of Inspiration

There is a theme lurking in this chapter, and it merits being made explicit. Scientific research is a highly collaborative process, and most successful scientists operate within social networks of fellow scientists from whom they receive—and to whom they provide—social support (Perry-Smith & Mannucci, 2015). Other people are not only an asset when carrying out research projects, they are a great source of inspiration and ideas.

The sixth rule of thumb is perhaps the most important: *Interact with other people*. All of life is a source of ideas, and its corollary is that the more interesting the people you spend time with, the more interesting ideas you are likely to encounter (Nisbett, 1990). Close connections with other people serve as a catalyst for the generation of creative ideas, especially when those other people have diverse arrays of knowledge (Sosa, 2011). If you can forge those relationships within the context of the research that you do, it can make the research that you do feel less like work and a lot more fun. Rather than racking your brain in isolation in search of lonely inspiration, it might be more fun—and productive—to brainstorm research ideas with collaborators. The "catchy title" project started out mediocre, but conversations in the lab made the idea mature, catch fire, and become worthwhile.

Not your cup of tea to try McGuire's (1997) heuristics on your own? Try it over a cup of tea with a couple of friends. From modest beginnings, good ideas can grow. Science is a conversation; seek out opportunities to join it. Ask questions. Attend conferences. Talk with the people around you—students, teachers, friends, lovers, and maybe even strangers. If you can, find ways to ensure that the people around you have diverse interests, diverse attitudes, and diverse backgrounds. If you want to be graced by good ideas about how people feel, think, and live their lives (and by ideas about how to make their lives better), it helps to be actively engaged in people's lives. It helps to be a truly *social* scientist.

Strategies for Figuring out Whether an Idea is Worth Working on

You have an idea. Now what? A few pages ago we justified an "anything goes" attitude toward getting ideas with the observation that there would be time later to assess whether those ideas are any good or not. That time has come.

This kind of assessment is important. Research projects require a substantial investment of time—almost always more than you anticipate. The "catchy title" project began with one simple study and blossomed into a dozen more, some of which took months to complete—and that was a successful project that produced publishable results. Many research projects are less successful, but they still consume researchers' time and effort before they are abandoned. It is best to think carefully about whether an idea is worth pursuing before you do so.

How do you know which idea to pursue? That exact question was posed to some very productive psychological scientists some years ago ("Which Scientific Problem to Pursue," 2002). Their responses suggest that a wise decision about whether to pursue an idea (or not) is informed by answers to three important questions: (1) Is it interesting to you? (2) Is it interesting to other people? (3) Can you get it done?

Is it Interesting to You?

If you decide to pursue a research idea with an actual research project, you will devote a lot of your time and effort to that project. You will immerse yourself deeply in a scientific literature written with jargon and complexity. You will do the painstaking labor of designing a methodology and collecting data and analyzing those data; ideally you will also do the painstaking labor of writing up the results in a manuscript and shepherding that manuscript into publication. Rarely does it all proceed as straightforwardly as you hope it will. Manipulations and measures may need to be pilot-tested, even multiple times. Before a manuscript is published, it may be rejected, often multiple times. Unless you have a will of steel and a disdain for reinforcement, your project is unlikely to succeed unless you are intrinsically motivated to see it through. There may be rewards along the way as well (e.g., new insights, new inspirations and ideas, the joys of surmounting a methodological challenge or learning a new data analytic technique or making a novel scientific contribution), and these rewards too are more likely to accrue if you are truly passionate about the project. For all these reasons, this is a good place to repeat—and repurpose—one of the rules of thumb identified above: *Do things that you actually want to do*.

Successful scientists typically prioritize ideas that excite them personally. In that compendium of psychologists' responses to the question "How do you know which idea to pursue?" Brenda Major replied, "Does the idea grab me? Is it interesting? Can I get enthusiastic about it?"; and Elliot Aronson said, "I try to follow my own curiosity…to ask a researchable question that I am passionately interested in finding answers to" ("Which Scientific Problem to Pursue," 2002, p. 12). Some of these scientists advocated strategies to help assess whether initial interest might actually endure. Yoshihisa Kashima likes to imagine a future in which the initial idea has panned out perfectly—"hypotheses (or hunches) are supported, and everything is beautiful"—and then asks himself "Am I excited?" (p. 13). Anthony Greenwald offered the following pragmatic advice: "When you have a new research idea, try writing the title and abstract of the article that will report it. If (a) you can't write them or (b) you can write them but don't find them compelling, then abandon before you start" (p. 12). This kind of exercise can help you think about an idea more deeply—to consider it from multiple angles, to identify connections to existing lines of research, and perhaps even to generate additional ideas too. Interesting ideas often become even more interesting as you think about them more and more. If this doesn't happen for you, then perhaps it is not the idea for you.

Individuals' interests are idiosyncratic (we can expand that fifth rule of thumb: *both inspiration and interest are idiosyncratic*) and there are many reasons why you might be passionate about an idea. It doesn't matter why an idea excites you; what matters is that it does.

Is it Interesting to Other People?

It is a promising sign if an idea excites you, but that's only the beginning. It's important to ask whether an idea is interesting to other scientists and to people in general. There are both philosophical and practical reasons to ask this question.

Scientists don't do science in isolation. Philosophers define science not simply as an intellectual endeavor but as a fundamentally *social* activity involving a large number of people who, collectively, engage in inspection, criticism, disagreement and discussion—that ultimately leads to progress (Grene, 1985; Longino, 1990; Thagard, 1978). Individual research projects are actually community projects; even a small research project is typically conducted by multiple people working in collaboration, using methodological strategies developed and refined by many other people, with the direct support of broader research communities (e.g., universities, funding agencies) and the indirect support of even larger communities (e.g., taxpayers, people who pay tuition). Scientists who draw upon those community resources have a responsibility to consider more than their own personal curiosity—they must also consider the interests of everyone else.

This philosophical perspective is complemented by purely a pragmatic consideration. Regardless of results, and regardless of your personal interest, your research project is unlikely to be published (or to make any kind of meaningful contribution) if that research is of interest only to you. The underlying ideas must interest other people too.

Some topics are more generally interesting than others. Topics such as altruism, depression, language acquisition, religious belief, and social status have been of broad and enduring interest; whereas other topics may be more faddish or of interest only to niche audiences. To some extent, these differences reflect differences in conceptual scope and range of applicability (van Lange, 2013). Scientists' interests also reflect real-world relevance. Although some social and behavioral scientists—especially psychologists—use contrived methods in controlled laboratory environments, the phenomena under inquiry are expected to reflect the real world. The more this connection is evident, the more other scientists (and non-scientists) are likely to find an idea interesting. Some research ideas have transparent implications for useful real-life applications, including applications that might help to solve social problems, promote health and well-being, or to otherwise improve humans' lives. People are likely to find these kinds of ideas important and, therefore, interesting. Cialdini observed "if there is evidence that the effect occurs regularly and powerfully in multiple environments, it is simply more worthy of examination". Similarly, Aronson said "From time to time, as a researcher, I ask myself: "Is this research ever going to do anyone any good?" ("Which Scientific Problem to Pursue," 2002, p. 13). These observations lead us to a seventh rule of thumb: *If an idea has more real-life relevance, people are more likely to be interested in it*.

And before you can catch your breath, we offer an eighth rule of thumb: *If an idea is too obviously true, people might not find it interesting*. Because scientists value veracity, it might be tempting to think that the more obviously true some hunch or hypothesis is, the more obviously interesting it will be; that's not the case. Davis (1971) argued that the subjective experience of surprise is a critical component of subjective interest value and that people are more likely to consider a scientific proposition to be interesting if it challenges some presumption that they have previously taken for granted. According to Davis (1971, p. 313), the essential formula for an interesting idea can be expressed semi-algebraically: "What seems to be X is in reality non-X" or "What is accepted as X is actually non-X." A good example of this is the discovery that partial reinforcement leads to more durable performance than continuous reinforcement—less *is* more (Skinner, 1938).

This principle helps to explain scientists' attraction to counter-intuitive ideas (Gray & Wegner, 2013). In fact, researchers in some social and behavioral science fields have been criticized for being a little *too* fond of counter-intuitive phenomena—and for not attending closely enough to the real possibility that results that violate conventional presumptions of truth might actually be false (Yong, 2012). But the most interesting and useful ideas are not merely counter-intuitive; they provide a way to resolve the apparent conflict between an existing presumption (X) and a challenging new proposition (non-X). Galen Bodenhausen observed "Interesting ideas often have elements that are surprising and, at least at the first pass, difficult to reconcile with one's most immediately relevant knowledge structures, but in bringing other knowledge to bear in a novel way, the inconsistencies are resolved in a way that can have an intellectually satisfying elegance…that marks an idea as interesting and worthy of pursuit" ("Which Scientific Problem to Pursue," 2002, pp. 12-13).

Ideas do not need to be counter-intuitive to fit Davis' (1971) formula. For instance, the results of replication studies are rarely considered to be counter-intuitive, but the ideas underlying replication research can still fit that formula. A phenomenon presumed to be robust and replicable may not be so robust or easy to replicate after all. A phenomenon presumed to be of questionable replicability may be revealed to be replicable after all (e.g., Noah et al., 2018). There are many ways in which ideas may challenge people's preconceptions. Savvy scientists think carefully about what those preconceptions are and about whether and how an idea might challenge them.

Some ideas may be so unconventional that they might seem implausible or even incomprehensible, and that too is a barrier to attracting others' interest. The most successful ideas are often those that occupy the sweet spot between the extremes of obvious and outlandish. Marilynn Brewer characterized this sweet spot as a kind of *optimal distinctiveness*: "does the idea seem grounded in current research (i.e., have a degree of familiarity) and yet hasn't already been introduced in the recent literature (i.e., have a degree of novelty)" ("Which Scientific Problem to Pursue," 2002, p. 14). Daniel Gilbert also highlights this sweet spot, while also neatly summarizing a handful of other characteristics that make ideas interesting to other people ("Which Scientific Problem to Pursue," 2002, p. 14):

> A good idea is original, tractable, economical, synthetic, generative, and grand. By that I mean it is not well-explored (original), it is explorable with scientific methods (tractable), it provides an elegant and simple solution to a complex set of problems (economical), it brings together phenomena that initially seemed to have nothing in common (synthetic), it generates many more interesting questions than it answers (generative), and it speaks about some fundamental truth (grand). Good ideas are almost never outlandish: When someone tells you a really good idea, you almost always have the sense that you were just about to think of it yourself except that...well, you didn't.

Any idea can be scrutinized for interest, and this process benefits from familiarity with relevant scholarly literatures. A thorough reading of those literatures? Daunting. One must do the deep dive eventually (if you actually do pursue the idea), but it is rarely the best place to begin. A more efficient way to begin is to bounce the idea off other people. Science is a conversation, and a potentially promising idea is a great conversation-starter. Talk about the idea with experts; even established scholars are usually happy to discuss ideas, especially if you are well-prepared and succinct. Talk about the idea with people who *aren't* experts. Their perspectives—along with their questions, criticisms and occasional confusions—will help focus the idea, sharpen it, and clarify exactly what it is and why it matters. Nisbett (1990, p. 1082) made this plain: "The necessity of explaining one's concerns to others, and of putting them into a broader context, together with the effort to demonstrate why certain topics are interesting, all have the most direct benefits for thinking about research." The benefits are many. If an idea withstands public scrutiny and remains interesting, it may be worth pursuing.

These conversations can help refine the idea, reveal non-obvious nuances that make it more interesting, or identify important real-life applications that might make it even more worthwhile to pursue. If you can excite other intelligent people with an idea—and maybe recruit them as research collaborators—the resulting research project is likely to be more fun *and* successful.

Can You Get it Done?

You have a research idea that excites you and others. You are confident that the research—if done rigorously and well—will make a worthwhile scientific contribution. Someone should do it. Should that someone be *you*?

Before starting any research project, it is sensible to think about it from a purely pragmatic perspective—to consider not only the rewards it might bring to you (e.g., pleasure, publications) but also the resources required to pull it off. Some research projects are cheap to do. Others are not and may require extraordinary resources—special personnel, expensive equipment, dedicated laboratory space, access to exceptional populations, that sort of thing. Can you realistically acquire these resources? Do you have colleagues with connections? Can you write a grant application with a reasonably high probability of success? Can you do so in a timely way?

Time is a cost that you would be wise to consider carefully (and not just because people who place a high value on time are happier than those who don't; Whillans et al., 2016). The time spent on any research project is time that cannot be spent on anything else that might matter to you, including other potentially rewarding research projects. Regardless of the number of hours you personally spend on a project, some projects take [much] longer to complete than others. This can be an important consideration, perhaps especially important depending on your circumstances. Tenured professors may have the luxury of pursuing a project that might take years to pay off; untenured faculty and graduate students might not. When Chris Crandall was in graduate school, he chose—perhaps optimistically—to pursue a longitudinal field study for his dissertation. It took three years to complete and delayed (by a year) the completion of his Ph.D. It paid off, but plenty of equally time-consuming projects don't.

You would be wise to consider these kinds of costs carefully and to consult with other people about them. If after doing so, you are convinced that you are the right person to pursue a research idea, go ahead and do it. If not, you might want to pursue a less costly project instead. That doesn't mean that you should just abandon entirely the costlier idea. Perhaps you will have the opportunity to return to it sometime in the future when you can more readily afford the costs. Some good ideas can wait, but don't trust your memory (*write it down*).

Science is a community project, but individual human beings are the vessels through which scientific ideas and empirical results must travel. Any decision about whether an idea is worth working on (or not) is a personal decision that will be informed by your own idiosyncratic interests, constraints, and aspirations. With that in mind, we give the last word here to the editors who solicited, and compiled, successful psychologists' thoughts about these decisions ("Which Scientific Problem to Pursue," 2002, p. 15):

> Which idea to pursue must depend upon your own goals.... If you want to publish a large number of articles in a reasonable amount of time, then one might pursue moderately novel ideas. If you want to have a lot of impact, then pursue innovative and contrarian ideas in a currently hot topic. If you want a grant, then focus on ideas that will pay off in a straightforward way in a reasonable amount of time (and money). If you want to enjoy your work, then follow your heart. These are not necessarily mutually exclusive.

Strategies for Transforming an Idea into Something Scientific

You've got an idea and you're excited to pursue it. The idea is taking shape not only in the form of an interesting research question, but maybe also a speculative answer—your hunch about how the world works or your personal prediction about some relation between some set of variables. You might even be talking about your "hypothesis." Not so fast! There is work to be done. No matter how compelling your idea, no matter how convinced you are that your hunch or personal prediction might be right, it may not yet rise to the level of rigor that characterizes good science.

Scientific inquiry is characterized by methodological rigor—by methods that are systematic and precise and that are designed to minimize the impact that scientists' biases, blind spots, and subjective beliefs might have on scientific knowledge. People are accustomed to applying these principles to the *empirical* part of the scientific process, during which scientists collect and analyze data to test scientific conjectures. Less obviously, the same principles can be applied to the *conceptual* part of the process—the part in which scientists develop and articulate those conjectures in the first place. Among the many elements that characterize scientific rigor (e.g., Casadevall & Fang, 2016), there are two elements that you might be especially mindful of when developing a research idea into something that meets the high standards of science: *precision* and *impartiality*. These can transform a vague idea into a good idea.

Precision

"To ask a scientific question about individual or social behavior, we must specify the parts of a system and the relationships between them… The precise specification of parts and relationships is what defines a scientific question and separates it from wishy-washy pseudotheory" (Smaldino, 2017, pp. 314-315). Precise specification is a nontrivial challenge in the social and behavioral sciences because the "parts" of conceptual interest—*constructs* such as resilience, social status, or moral reasoning—are broad in scope and abstract in principle. They tend to be understood intuitively but imprecisely. For example, one person's intuitive understanding of "resilience" may only approximately match someone else's understanding of it. Unless these constructs are defined transparently and precisely, problems may arise in the form of mismatches between the empirical methods people use and the constructs of actual interest. To test an idea about social status, you might sensibly use a measure that someone else had used to measure "social status" without realizing that it measures something different from the sort of social status that what you had in mind. Two people may attempt to test the same hypothesis about social status but have different intuitive understandings of "social status" and consequently use different measures that produce different results—creating the superficial appearance of inconsistent support for a conceptual hypothesis when, in reality, that hypothesis might actually have only been meaningfully tested by one (or none) of the studies (Oberauer & Lewandowsky, 2019). Your initial ideas and hunches are unlikely to be characterized by the level of conceptual precision required to avoid these problems.

The goals of transparency and precision lead us to a ninth rule of thumb: *Before pursuing any idea seriously, define precisely its conceptual "parts."* You—and anyone who reads or listens to you—should be able to articulate clearly what each relevant construct is and is not.

Formal modeling methods can help with this task (Smaldino, 2017). Also helpful are systematic methods of construct validation (Clark & Watson, 2019; Grahek et al., 2021). It is tempting to think that the proper time to consider construct validity is only after an idea has been formulated and a scientist has begun designing an empirical study. *This is wrong*. A precise conceptual definition of a construct is necessary right from the get-go. Clark and Watson (2019, p. 1413) wrote "an essential early step is to crystallize one's conceptual model by writing a precise, reasonably detailed description of the target construct"; and they provide useful guidance. Try asking a few simple questions about every construct you work with. What exactly is it? What isn't it? In what specific ways does this construct overlap with and differ from other similar constructs? Is this construct truly a single coherent thing or are there different varieties that deserve their own distinct conceptual definitions (and empirical operationalizations)? This kind of systematic conceptual work takes careful thought and effort, but, as Grahek et al. (2021) observe, "the effort can pay off in the form of more precise conceptual definitions of constructs (and, consequently, better measures of those constructs), more carefully articulated theories about those constructs, and more nuanced hypotheses that make accurate predictions."

Impartiality

People sometimes think that a scientific hypothesis is much the same thing as a scientist's own personal prediction. Philosophers of science beg to differ. Karl Popper (1959/2005, p. 22) made a sharp distinction between a truly scientific conjecture (e.g., an objective statement stipulating some logically plausible relation between constructs) and scientists' subjective beliefs about whether that conjecture is true or not. A hunch or personal prediction is indistinguishable from a subjective belief, and simply calling it a "hypothesis" does not make it so. It typically takes careful logical analysis to transform an informal idea or personal prediction into a rigorously objective scientific hypothesis.

In addition to high standards of scientific rigor, there is also a purely pragmatic reason to engage in this kind of systematic logical analysis. It can help you make well-informed decisions when designing studies to test hypotheses—increasing the likelihood that these studies will produce useful data, replicable results, accurate inferences, and publishable papers.

When people perceive something to be their own personal creation or personal possession, they over-estimate its value (e.g., Morewedge et al., 2009). The implication is that when people personalize a hypothesis ("my hypothesis"), they are more likely to believe that it's true even if it's not. In addition, if the hypothesis is true, they are more likely to overestimate the size of the effect and the extent to which it generalizes across different circumstances or populations. These kinds of overestimates can lead researchers to make problematic decisions when designing studies and analyzing data:

> "When researchers overestimate the veracity of hypothesized effects, they are less likely to make the kind of decisions (in data analytic strategies and subsequent reporting of empirical results) that guard against the documentation of false-positive inference. When researchers overestimate the size of hypothesized effects, they are more likely to employ underpowered research designs—increasing the likelihood that, whenever effects are detected, they are likely to be erroneously big. And when researchers overestimate the generalizability of hypothesized effects, they are less likely to empirically test its context-specificity or to otherwise draw attention to its potential fragility" (Schaller, 2016, p. 109).

To avoid falling prey to these problems, it helps to adopt an impartial attitude toward ideas, predictions, and hypotheses. Can you be impartial even when doing research on topics of great personal interest to you? Yes! You can be personally interested in a research *question* while still cultivating an impartial attitude regarding the accuracy of hypothetical *answers* to that question. As a scientist, passionate interest in an idea need not—and should not—supersede your passion for honesty, accuracy, and truth. If you cannot accept reliable findings, you'll need to examine your commitments.

Let us revisit that lovely line from García Márquez (1995, p. 56) and reframe it as the tenth rule of thumb: *Ideas do not belong to anyone*. You may be a more effective steward of ideas and hypotheses—and make wiser decisions when testing them—if you adopt the mindset that you are steward (and not owner) of those ideas. You may have your informal hunches and subjective beliefs, but they are distinguishable from scientific hypotheses. To be scientific hypotheses, conjectures must be stated impartially. To be *compelling* hypotheses, they require careful and coherent justification.

A useful pathway to transforming an informal idea into an impartially stated, carefully justified scientific hypothesis leads us to one last rule of thumb: *Specify your assumptions explicitly*. Try to identify all the assumptions underlying a personal prediction, and then derive a clearly-stated and testable hypothesis from these assumptions, using a sequence of "if–then" statements (Schaller, 2016, offers examples). If you cannot get a hypothesis to follow logically from the assumptions, it might be a clue that your hunch is wrong or perhaps you haven't yet specified precisely *why* it might be right. Have you failed to specify a key assumption? Is there is a necessary logical step that you intuitively appreciate but haven't yet articulated? Connecting those logical dots makes a more convincing case that the hypothesis is not merely an idiosyncratic hunch but is a plausible scientific hypothesis.

This explicit identification and systematic inspection of underlying assumptions and derivations help forecast the plausibility, size, and generalizability of hypothesized effects. This leads one to make better choices for empirical research (e.g., sample sizes, measurement strategies, power of manipulations). Is every assumption and logical derivation completely convincing? If not, this is a reminder to maintain skepticism (a key scientific value) toward the hypothesis you're developing and guard against confirmation bias. Does each assumption and if-then statement apply equally to *all* people under *all* circumstances? If not, the overall hypothesis may accurately describe some people but not others or may be true under some circumstances but not others. This information too can inform methodological decision-making (e.g., decisions about specific populations to sample or about specific moderating variables to manipulate or measure) and may lead you to new ideas and new, more nuanced (i.e., better) scientific hypotheses. That's been your goal all along.

Does every scientist subject their ideas to this kind of systematic logical analysis? Alas, no. Compared to our scholarly cousins in the physical, biological, and cognitive sciences, many social and behavioral scientists have tended to be looser and lazier about articulating hypotheses with precision and rigor, *but that's changing*. Scientists are increasingly aware of the problems that arise from informal conceptual analyses and the benefits that accrue from the extra work required to transform inspiration and intuition into precise, carefully-articulated, and logically-transparent statements that meet high standards of scientific rigor (e.g., Fiedler, 2107; Gervais, 2021; Grahek et al., 2021; Gray, 2017; Klein, 2014; Muthukrishna & Henrich, 2019; Oberauer & Lewandowsky, 2019; Schaller, 2016; Smaldino, 2017, 2020). It's an important part of the ongoing effort to do science better.

You might reasonably ask: Isn't all this extra effort time-consuming (and sometimes tedious) to do? Yes—and that's a clue that it can be good to do. Compared to less rigorous means of inquiry, more rigorous methodological practices are, inevitably, more time-consuming (and sometimes tedious). That's science.

But you don't have to do it all by yourself, and it's best if you don't. The kind of painstaking conceptual work that we have described here (i.e., precise definitions of abstract constructs, detailed logical dissections of hypotheses) is likely to be more productive—and more fun—if you do it in collaboration with other people. It's a good way to do good science.

Envoi

Scientists love new research ideas, and so it is ironic that scientists receive so little formal education about how to find new ideas and develop them rigorously. To the extent that scientists get this guidance, it is haphazard and idiosyncratic ("the apprenticeship model"), consisting of informal discussions with mentors and peers, feedback (sometimes fulsome and constructive, often not) from reviewers, brevity-is-the-soul-of-wit editors, committees, and a lot of reading-between-the-lines. A few books and articles provide useful guidance of one sort or another (e.g., Beveridge, 1957; McGuire, 1997; Nisbett, 1990), and young scholars can learn a lot from anecdotes and personal reflections that are sometimes compiled in out-of-the-way places (e.g., "Which Scientific Problem to Pursue," 2002).

We have drawn upon these and other sources (such as the psychological research on creativity and philosophy of science) to identify strategies—and guiding principles—that might be helpful. Getting ideas and making the most of them takes more than idle inspiration—they benefit from strategy, skill, and labor. Science—as practice, as a profession, as a cultural product—does not usually come easily. Still, most people are well-equipped to meet those challenges. Curiosity is natural. Opportunities for inspiration are everywhere. The skillset required to transform informal ideas into useful scientific products is attainable. Most of these challenges can be more readily surmounted by using one simple trick: *talk to other people*. Time, training, practice, and talk make the "idea" part of science easier, and you get better at it.

We close with a snippet of conversation from two people who are very good at it: Shelley Taylor and Susan Fiske (Taylor & Fiske, 2019, p. 8):

> **Susan Fiske**: Do you have any suggestions for people starting out in the field about how to have a good idea, and how to implement it?
>
> **Shelley Taylor**: I have always thought that you look around you and if you're psychologically minded, you notice things, and you think, *Well, what does that mean?* You keep trying to step it up a level, which will ultimately lead you to theory. I would say trusting your own ideas is a very important way of coming up with a research program that is novel and exciting and that ultimately wins people over.
>
> **Susan Fiske**: I think that's a great place to end.

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