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Forum

How To Choose a Good Scientific Problem

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Choosing good problems is essential for being a good scientist. But what is a good problem, and how do you choose one? The subject is not usually discussed explicitly within our profession. Scientists are expected to be smart enough to figure it out on their own and through the observation of their teachers. This lack of explicit discussion leaves a vacuum that can lead to approaches such as choosing problems that can give results that merit publication in valued journals, resulting in a job and tenure.

The premise of this essay is that a fuller discussion of our topic, including its subjective and emotional aspects, can enrich our science, and our well-being. A good choice means that you can competently discover new knowledge that you find fascinating and that allows self-expression.

We will discuss simple principles of choosing scientific problems that have helped me, my students, and many fellow scientists. These principles might form a basis for teaching this subject generally to scientists.

Starting Point: Choosing a Problem Is an Act of Nurturing

What is the goal of starting a lab? It is sometimes easy to pick up a default value, common in current culture, such as "The goal of my lab is to publish the maximum number of papers of the highest quality."

However, in this essay, we will frame the goal differently: "A lab is a nurturing environment that aims to maximize the potential of students as scientists and as human beings."

Choices such as these are crucial. From values—even if they are not consciously stated—flow all of the decisions made in the lab, big and small: how the lab looks, when students can take a vacation, and (as we will now discuss) what problems to choose. Within the nurturing lab, we aim to choose a problem for our students (and for ourselves) in order to foster growth and self-motivated research.

The Two Dimensions of Problem Choice

To choose a scientific problem, let us begin with a simple graph, as a starting point for discussion (Figure 1). We will compare problems by imagining two axes. The first is *feasibility*—that is, whether a problem is hard or easy, in units such as the expected time to complete the project. This axis is a function of the skills of the researchers and of the technology in the lab. It is important to remember that problems that are easy on paper are often hard in reality, and that problems that are hard on paper are nearly impossible in reality.

The second axis is *interest*: the increase in knowledge expected from the project. We generally value science that ventures deep into unknown waters. Problems can be ranked in terms of the distance from the known shores, by the amount in which they increase verifiable knowledge. We will call this the interest of the problem.

In a forthcoming section, we will discuss the subjective nature of the interest axis. But first, let us first consider aspects of problem choice using our diagram.

Looking at the range of problems in this two-dimensional space, one sees that many projects in current research are of the easy-but-not-too-interesting variety, also known as "low-hanging fruit." Many other projects in science today are unfortunately both difficult and have low interest, partially stemming from a view that hard equals good. A few problems are grand challenges: tough problems with the potential to considerably advance understanding. But most often we would like problems in the top-right quadrant, both feasible and with high interest, likely to extend our knowledge significantly.

The diagram suggests a way to choose between problems, using the Pareto front principle of optimization theory. If problem A is better on both axes than problem B, one can erase B from the diagram. Applying this criterion to all problems, one is left only with problems for which there are no problems clearly better in both feasibility and interest. These remaining problems are on the Pareto front.

To decide which problem to select along the front depends on how we weigh the two axes. For example, a beginning graduate student needs a problem that is easy; positive feedback can thus be rapidly provided, bolstering confidence. These problems are on the bottom right of the Pareto front. The second problem in graduate school can move up the interest axis. Postdocs need projects in the top-right quadrant, since time is limited. Beginning PIs, who need to select a field on which to spend many years and with which to train students, may seek a grand challenge that can be divided into many good, smaller projects. Thus, the optimal problems move along the Pareto front as a function of the life stages of the scientist.

Take Your Time

A common mistake made in choosing problems is taking the first problem that comes to mind. Since a typical project takes years even it if seems doable in months, rapid choice leads to much frustration and bitterness in our profession. It takes time to find a good problem, and every week spent in choosing one can save months or years later on.

In my lab, we have a rule for new students and postdocs: *Do not commit to a problem before 3 months have elapsed*. In these 3 months the new

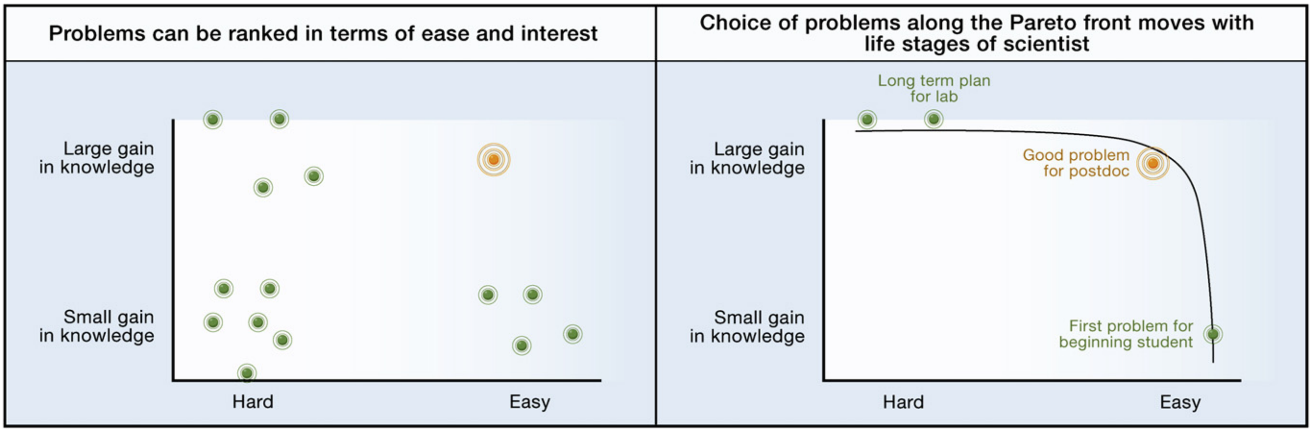

Figure 1. The Feasibility-Interest Diagram for Choosing a Project
Two axes for choosing scientific problems: feasibility and interest.

student or postdoc reads, discusses, and plans. The state of mind is focused on being rather than doing. The temptation to start working arises, but a rule is a rule. After 3 months (or more), a celebration marks the beginning of the research phase—with a well-planned project.

Taking time is not always easy. One must be supported to resist the urge: "Oh, we must produce—let's not waste time, and start working." I am under no illusion that everyone is free to choose their own problems, or has the time needed for an extended search. Taking time can be especially difficult when funding is insufficient and grant deadlines approach. In such difficult situations, nurturing is not enough, and you need to find support and do all you can to get into a better situation. Even so, for many of us dealing with the difficulties of running a lab, taking time to choose problems can make a huge difference.

The Subjectivity of the Interest Axis

Let us now look in more detail on the axis of problem interest. Who decides how to rank the interest of problems? One of the fundamental aspects of science is that the interest of a problem is subjective and personal. This subjectivity, however, makes things confusing. The confusion is due to the mixing of two voices—one is a loud voice of the interests of those around us, in conferences, in our department, etc. The other is a faint voice in our breast, that says, "This is interesting to me." Ranking problems with consideration to the inner voice makes you more likely to choose problems that will satisfy you in the long term.

The inner voice can be strengthened and guided if one is lucky enough to have caring mentors. A scientist often needs a supportive environment to begin to listen to this voice. One way to help listening to the inner voice is to ask: "If I was the only person on earth, which of these problems would I work on?" An honest answer can help minimize compromises.

Another good sign of the inner voice are ideas and questions that come back again and again to your mind for months or years. These are likely to be the basis of good projects, more so than ideas that have occurred to you in recent days. Another good test: When asked to describe our research to an acquaintance, how does it feel to describe each project?

It is remarkable that listening to our own idiosyncratic voice leads to better science. It makes research self-motivated and the routine of research more rewarding. In science, the more you interest yourself, the larger the probability that you will interest your audience.

Self-Expression

What is the essence of the inner voice? The projects that a particular researcher finds interesting are an expression of a personal filter, a way of perceiving the world. This filter is associated with a set of values: the beliefs of what is good, beautiful, and true versus what is bad, ugly, and false. Our unique filter is what we bring to the table as scientists. A multiplicity in styles and questions, based on the uniqueness of scientists, is the basis of a viable and creative science.

To choose a good problem, therefore, we need to reflect on our own world view. And, as mentors, we can help students in the late phases of their PhD or in the postdoc stage to strengthen their inner voice. A mentor can help by listening to a student describe what they like in science, in life outside of science, what moment made them decide to become scientists, and what scientific work they admire. We sometimes begin to see patterns in what the student is talking about. There emerges a map of values, in the way that deep rocks in an ocean are discernable by the waves made on the surface. Is this student motivated by visual aesthetics or by abstract ideas? By supporting the dogma or by undermining commonly held truths? Likes techniques or logical proofs? Basic understanding or applied work? And so on. This can help the mentor select a project in which the student has the potential for self-expression. As mentioned above, when one can achieve self-expression in science, work becomes revitalizing, self-driven, and laden with personal meaning. It may also have a better chance of discovering something profound.

The Schema of Research

What happens after we choose a problem? Before we end, I'd like to discuss the mental picture or schema we hold of what research will look like (Figure 2). A common schema is expressed in the way papers are written: one starts

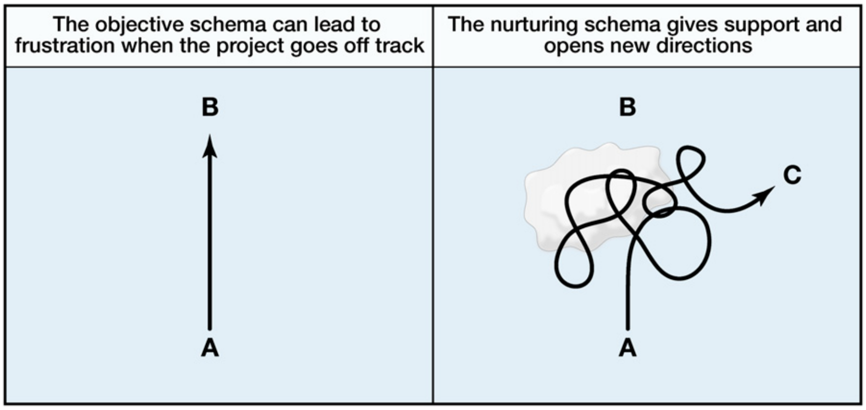

Figure 2. The Objective and Nurturing Schemas of Research
The nurturing schema includes ''the cloud''—a period of time in which basic assumptions break down.

at point A, which is the question, and proceeds by the shortest path to point B, the answer. There is a danger, if one accepts this schema, to regard students as a means to an end (an arrow to B). Furthermore, for those that hold this schema, any deviation from the path (experiments that don't work, students that become depressed, etc.) is intolerable. Deviation causes stress because of the cognitive dissonance between reality and the mental schema.

However, one can adopt a second schema, one that resembles more the course of most projects. As before, one starts at point A and moves toward the goal at point B. Soon enough, things move off course, and the path meanders and loops back. Experiments stop working, all assumptions seem wrong, and nothing makes sense. The researcher has entered a phase linked with negative emotions that may be called ''the cloud.''

Then, in the midst of confusion, one senses a new problem in the materials at hand. Let's call this new problem C. If C is more interesting and feasible than B, one can choose to go toward it. After a few more detours, C is reached. The researchers can pause to celebrate before taking time to think about the next problem.

In this second schema, the meandering of research is seen as an integral part of our craft, rather than a nuisance. The mentors' task is to support students through the cloud that seems to guard the entry into the unknown. And, with this schema, we have more space to see that problem C exists and may be more worthwhile than continuing to plod toward B.

In the nurturing schema, we celebrate the courage and openness of scientists. Sailing into the unknown again and again takes courage; seeing there something different from expectations, and usually more rich and strange, requires uncommon openness.

In summary, take your time (recall the 3 Month Rule) to find among the problems available the one that is most feasible and most interesting to you rather than to others. A good project draws upon your skills to achieve self-expression.

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