

Building and Judging Research Ideas

EGCL Research Manual

“Good ideas are not sudden flashes of inspiration. They are born through thousands of cycles of building, breaking, and discussing.”

1. The Question Every Young Researcher Asks

Students often ask, “Professor, how do you come up with research ideas?” I asked the same question when I was a student. I watched my advisor generate new questions and experimental directions even in short conversations, and I often wondered how anyone could think that way. At the time, it seemed like a gift available only to special people.

For a long time, we are educated to find the correct answer to a predefined problem as quickly as possible. We train ourselves to learn and reproduce existing knowledge accurately, but we receive far less training in how to create a new question and design an answer that does not yet exist. That is why people can enter research with a great deal of knowledge and still have no idea what to ask.

Research ideas, however, are not produced by innate inspiration alone. People who consistently create good ideas tend to have recurring habits of thought and standards of judgment. If you observe those processes closely, generate your own ideas, study why they fail, and try again, creativity can also be trained.

Creativity is not an inexplicable talent. It is a research ability that can be built through observation, trial, correction, and accumulation.

2. Learn the Logic, Not Only the Answer

I wanted to learn not only my advisor's conclusions, but also the thinking process that led to those conclusions. So I deliberately created opportunities for conversation. I initiated discussions in the laboratory, followed along on walks, and asked questions over coffee about many different topics. The goal was not to obtain one specific idea. It was to observe how a researcher starts from an observation, turns it into a question, and decides which possibilities to discard and which to keep.

In a strong researcher's mind, knowledge is not simply stored. When they observe a phenomenon, related principles, previous literature, experimental limitations, and examples from other fields are connected rapidly. They then distinguish what is essential from what is peripheral. A student cannot learn this judgment process merely by writing down the professor's answer. You need to follow why a certain question was asked, why one possibility was eliminated before another, and why a particular experiment was judged to be decisive.

What you should learn from a mentor is not one correct answer, but the logical process used to create an answer and eliminate the wrong possibilities.

3. Bad Ideas Are the Beginning of Training

To become better at generating ideas, you first have to keep generating them. You should not expect the early ideas to be good. I also used to tell senior researchers whenever something occurred to me, and most of those ideas were rejected immediately. Some were thermodynamically impossible, some revealed gaps in basic knowledge, and some could have been shown to be wrong by reading a few relevant papers.

Those responses were not pleasant. But what mattered was not how many times an idea was rejected. What mattered was why it was rejected. Every time I heard, “That will not work,” I studied which principle I had missed and tried to understand why the idea was impossible under those conditions. Then I incorporated that lesson into the next idea so that I would not repeat the same category of error.

A person who rarely proposes ideas is rarely rejected. But that person also loses opportunities to discover where their own thinking breaks down. Early bad ideas are not failed products. They are diagnostic tools that reveal empty spaces in your knowledge and logic.

There is nothing embarrassing about having a bad idea. The problem is continuing to have bad ideas for the same reason.

4. Scientific Correctness Comes Before Originality

As this training continued, the reactions of senior researchers changed. Earlier they had said things such as, “That is impossible,” or “You need to study more.” Eventually they began saying, “The logic makes sense. Ask the professor.” I felt that I had made major progress. At least I had moved beyond ideas that were scientifically wrong.

But the professor would often say, “That study was already published five years ago.” At first this was disappointing, but I soon recognized that it represented another important stage of growth. My ideas were no longer wrong; they were simply behind the current state of the field. As I understood the literature and the movement of the field more deeply, the gap gradually shrank from five years to one year.

This stage is extremely important. Originality has to be built on scientific validity. An idea that is different from everyone else's only because it ignores basic principles is not creative; it is simply incorrect. First you need the ability to generate ideas that are scientifically possible. Then you must catch up with the current research frontier before you can arrive at questions that no one has yet asked.

The first criterion for an idea is not novelty but scientific validity. Yet validity alone does not make good research.

5. From Five Years Late to the Research Frontier

The progress of idea training could be seen in the way the professor's criticism changed. First, I was told that an idea was scientifically wrong. Then I was told that the same idea had been published five years earlier. Later, the comparison was to a paper from only one year earlier. This did not simply mean that I had become better at literature searching. It meant that I was gradually learning to move at the same speed as the questions and logic of the field.

With further repetition, I began to hear, “It seems that no one has done this yet.” But another problem soon appeared. The idea might be new, but too small, insufficiently important, or obvious enough that many people would likely think of it soon. Being scientifically correct and unpublished was not enough. Now originality and significance had to be judged.

These stages cannot be skipped in one leap. Moving from impossible ideas to possible ones, from old ideas to current ones, and from small novelty to important novelty requires hundreds and sometimes thousands of attempts and corrections. If you keep hitting the wall, the wall does not necessarily become lower. Rather, you gradually become the person capable of climbing over it.

The growth of ideas tends to follow this sequence: **wrong idea -> late idea -> small but new idea -> important and original idea.**

6. Discussion Is the External Screening System

An idea can originate in one person's mind, but it is difficult to judge its value accurately through that person's judgment alone. The person who first creates an idea is also the person most easily attracted to its strengths. They may fail to notice missing literature, false assumptions, impossible experimental conditions, or exaggerated significance. That is why ideas must collide with other people's thinking.

For me in the early stage, senior researchers and the professor functioned as a wall and as an external screening system. Senior researchers quickly identified weaknesses in basic science and literature knowledge. The professor evaluated how new and important the idea actually was in the context of the field. Rejection was not a process that destroyed ideas; it was a process that pushed them toward a higher standard.

However, it is also poor training to throw every spontaneous thought at other people without examining it first. Before discussion, you should create possible counterarguments in your own mind, check the relevant literature, and attack the core assumptions. Only after breaking the idea yourself several times and seeing whether it survives should you bring it into discussion. The quality of ideas improves rapidly when internal screening and external screening are repeated together.

Discussion is not a process in which other people create your ideas for you. It is the process of breaking weak ideas so that only stronger ones survive.

7. Creativity Is Like Building with LEGO

I compare creativity to building with LEGO. Knowledge is like LEGO blocks. The more you study, the more blocks you have, both in number and variety. If your knowledge is limited, the structures you can build are also limited, so sufficient study is a necessary condition for creativity. But simply owning many blocks does not automatically produce a new creation.

At first, you need to follow the instruction manual and reproduce existing models. In research, this is like reading papers, reproducing established synthesis and characterization methods, and following the logic of senior researchers and professors. During this stage, you learn what role each block plays and how different blocks can be connected. A person who has never followed the instructions will struggle to build a complex structure reliably.

Eventually, however, a researcher has to dismantle the completed model. The instruction manual must be put aside, and the blocks that have already been understood must be rearranged in entirely new ways. Students with excellent grades and extensive knowledge can still struggle in research not because they lack blocks, but because they have not trained enough in rearranging blocks beyond the instructions.

Knowledge is the material of creativity, but creativity is the ability to dismantle that material and rebuild it into new relationships.

8. A New Idea Is Not Necessarily a Good Idea

Once you can generate ideas, a harder problem remains: deciding whether an idea is worth pursuing. The fact that something has never been published may be a minimum condition for originality, but it is not evidence of value. The world contains many studies that no one has done simply because there is little reason to do them.

To choose good research, a researcher needs a personal standard for what good science is. Without that standard, it is easy to confuse originality with "slightly different conditions," "a slightly higher value," or "a

slightly modified material.” Such differences may be technically new while doing little to extend scientific understanding or technological possibility in a meaningful way.

At minimum, when evaluating an idea, ask the following. Does this work change an existing way of thinking? Does it ask a question that others have failed to see? Once the answer is known, could other researchers begin new research because of it? Or does it remove an important barrier in a real process or technology? If none of these questions can be answered clearly, the idea should be strengthened or discarded before experiments begin.

A new idea and a good idea are not the same thing. A researcher's maturity is revealed not only by what they decide to pursue, but also by what they decide not to do.

9. Valuable Research Often Lies at the Two Extremes

In my view, strong research often moves toward one of two extremes. At one end is conceptual advance. This includes work that asks a question no one previously considered important, explains a familiar phenomenon in a completely different way, or proposes a new principle that changes the existing interpretation. Such research does more than add one result; it changes how other researchers think.

At the other end is practical advance. This is research that solves an important problem in a real process or industry and substantially lowers a barrier that existing technology could not overcome. Here, “practical” does not simply mean mentioning possible applications. The improvement must be large enough in performance, stability, cost, scale-up, or process integration to change an actual technical choice.

Conceptual and practical research are not opposing values. A new concept can enable an important technology, and solving a real process limitation deeply can reveal new science. The key is that the advance must be sufficiently large in whichever direction the work takes. Research that makes a clear leap at one end often lasts longer than work that sits in the middle with a small performance improvement and a weak explanation.

Good research either broadens the scientific landscape through a new concept or broadens technological possibility through a major practical advance.

10. The Tree: Trunks, Thick Branches, and Twigs

Originality and advance in research can be compared to a tree. Planting a sapling where nothing existed before and growing a new trunk is like creating an entirely new research field. This is extremely difficult, but once the tree grows, many future studies can branch from it. Creating a thick new branch on an existing large tree can also be highly important because that branch can support many later studies.

By contrast, when creativity is still underdeveloped, researchers often add an even thinner twig to the end of an already thin branch. Then another twig is added to that, and another smaller one after that. Such work may create a small difference from previous papers, but it leaves little room for other researchers to build another important question on top of it.

A very thin branch can die easily when the season changes. If research interest shifts slightly or a new analytical method appears, the work may no longer be remembered. One paper may remain in the literature, but no new direction grows from it. Therefore, when evaluating an idea, do not ask only, “Is this different from the previous paper?” Ask, “Can the next research grow from this?”

Good research is not about adding one more twig. It is about creating a new trunk or a thick branch from which many future studies can grow.

11. Questions for Screening an Idea

Before selecting an idea for an actual research project, you should attack it repeatedly yourself. Rather than explaining why the idea is good, first search for reasons it could be bad. If you cannot answer the following questions, the idea is probably not yet mature enough.

1. Is this idea scientifically possible? What principles and evidence support its core assumptions?
2. Has someone already done it? Not merely with the same material, but with the same question and logic?
3. Is the difference from previous work fundamental, or is it only a small change in conditions or composition?
4. If the idea succeeds, what will we understand or make possible that was not possible before?
5. Even if it fails, can it still produce an important scientific conclusion?
6. Can another researcher build a next question on top of this result?
7. Is there a reason this research needs to be done now?

These questions are not a checklist for making an idea perfect. They are tools for finding the weakest part of the idea and deciding where further study and discussion are needed. Early in training, senior researchers and professors may make these judgments for you. To become an independent researcher, however, these questions eventually need to operate automatically inside your own mind.

12. Independence Means Internalizing the Standard

Research independence is not the same as being able to state many ideas on your own. Genuine independence is the ability to create ideas, identify their weaknesses yourself, judge their scientific validity, originality, and significance, and then choose the most valuable problem to pursue.

At first, senior researchers filter out scientific errors and the professor evaluates novelty and significance. As you grow, those external standards have to move inside you. A problem that was pointed out during discussion should be something you discover yourself the next time. An idea that the professor once called “too small” should eventually be something you discard before saying it aloud.

This process does not make you generate fewer ideas. In fact, it leads you to generate more ideas internally and destroy weak ones more quickly. The number of ideas you bring to other people may decrease, but the quality of the ideas that survive becomes higher. A good researcher is not someone with few ideas, but someone who can discard weak ideas quickly and develop strong ones all the way to maturity.

An independent researcher is not someone who avoids external criticism, but someone who has internalized the standards behind that criticism.

Closing Note

Research ideas are not completed inspirations that suddenly appear one day. They are the result of a long training process: accumulating knowledge, observing the logic of other researchers, generating thoughts continuously, learning why some are scientifically wrong, and catching up with the current frontier of the literature. In that process, criticism from senior researchers and professors is not a wall that blocks ideas. It becomes a standard that raises the level of thinking.

But the ability to generate ideas is not enough. You also need to filter out ideas that are new but small, ideas that no one has done but that are not important, and ideas that simply extend the end of an already

thin branch. You need your own standard for what constitutes good science so that you can distinguish conceptual advance from practical advance and choose problems from which future science can grow.

The researcher's goal is not to write one more paper. It is to plant a new tree or, at minimum, to create a thick branch from which other research can grow. To do that, you must repeat the cycle of building, breaking, studying, discussing, and rebuilding thousands of times. That repetition eventually creates originality and independence.

Ideas are not something you wait for. They are something you build. And a good researcher is not the person who has the largest number of ideas, but the person who can choose the ideas from which science can continue to grow.