On choosing meaningful research projects in the natural sciences

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October 8, 2018

Abstract

Over the last few decades, the nature of scientific research has changed in response to external influences. Firstly, powerful networked computers have become a standard tool. Secondly, society presses ever harder for research to deliver something “useful” back to society, both through the kinds of funding opportunities that are made available, and through a critical public eye. Many funding agencies now demand “deliverables” that seem to select research of a particular kind. Lastly, teamwork, often within very large projects, has become commonplace.

Here, we step back and ask how prospective research scientists might select productive research projects in this evolving environment. We hope that our suggestions might also help to improve public understanding and thereby restore flagging faith in science.

1 Motivations

Researchers in the natural sciences usually feel privileged. Excited about our subjects, thrilled by the prospect of stepping into the unknown, we count ourselves lucky when we can support our families by doing something we enjoy – research. Motivated by our belief that research in natural science is a noble and productive pursuit, something worth preserving for future generations, this informal article aims to help those considering science careers to decide if research is for them, and if so, what kind of research.

We are also motivated to try to understand what it is that makes a good research project. Increasing numbers of young researchers seem to struggle to answer the question, “why should one care about your research?” The question is not meant unpleasantly, it is asked to provoke thought about the motivations behind the research project in relation to the more abstract desire to advance knowledge of nature.

2 Science, technology, research

We must first specify our terminology. In modern culture, science and technology go hand-in-hand. For example, NASA is known, rightly, as both a scientific and technological institution. High impact publications such as Science and Nature report both scientific and technological advances. But what do we mean by research in technology and research in science?

Most people might agree that research in technology is the pursuit of advancing tools. The
The goal of research in technology is improvement of technology itself, not a better understanding of nature. Technological research is not scientific research *per se*, because the essence of scientific research is to perform experiments to challenge our understanding of nature.

The necessity of experimental arbitration in science is traditionally associated with Galileo Galilei. Four centuries later, Einstein became famous for “thought experiments”, mental scenarios rooted in simplicity and symmetry, again meant to discriminate between acceptable and unacceptable theories of how nature works. Accepting both real and thought experiments as the arbiters for new ideas gives us a working definition of modern research into natural science.

Karl Popper (e.g., Popper [1972]) suggested further that scientific research must also be falsifiable or refutable by experiment. Any advancement must also determine its new domain of applicability which must also be open to refutation through experimentation. Not everyone agrees with Popper’s viewpoint. But if one embarks on research where falsification seems remote, one should be prepared to defend the work in other ways. Generations of string theorists may have to live with this reality, unless practically testable predictions can be uncovered. This possibility is, to the authors’ knowledge, non-zero. Research on any theory that might unify physics surely remains of prime importance to natural science, no matter if it can currently be refuted.

Research into natural science has a unique goal. There is nothing to be “sold” or “delivered” except a better understanding of nature. Curiosity about the interaction of atoms with light led to the discovery of the laser, the interaction of magnetized atomic nuclei with low frequency radiation led to MRI machines. Neither development could have taken place without the research into the natural science beforehand. Therefore

*curiosity-driven research without foreseeable outcomes remains important.*

Most modern research will fall between curiosity-driven and what we will call deliverable-driven research. Prospective students should be able to judge where in the spectrum a given research project may lie. Those focusing on tangible deliverables necessarily limit the scope of the research, for

*if one already knows the outcome (i.e., promises deliverables), the research must usually be of a technical, not natural scientific nature.*

These points are not meant to be judgmental, after all, these are all human activities and they are necessarily imperfect. But prospective scientists might ask themselves:

*Am I really interested in science or technology, or both?*

To some, an answer might help in making an important decision, to others it might not matter. Indeed many great scientists did or do both (for example, Sadi Carnot, Michael Faraday, Kristian Birkeland), but they are rarer nowadays owing to specialization. Such people gain much respect from their peers, or they deserve to!

We turn to the meaning of research, although the reader will have a good idea of what it entails. It is worth a look at some definitions. The Oxford English Dictionary states:

*The systematic investigation into and study of materials and sources in order to establish facts and reach new conclusions.*
From Webster’s Dictionary, we have

1. Diligent inquiry or examination in seeking facts or principles; laborious or continued search after truth... – Macaulay.
2. Systematic observation of phenomena for the purpose of learning new facts or testing the application of theories to known facts; also called scientific research.

Implicit in Webster’s point 1 is that something is research if it is new, new to an individual or to society. It is in this second sense and in point 2 that professional research, the subject of this article, must be limited.

But what makes scientific research? It would help if there were a prescription, an established “scientific method” identified and acknowledged universally. But that taught in schools is a myth generally dispelled before graduate school. In reality, there is no universally recognized method (e.g. Newton-Smith [1981]). It can vary from “anything goes” through “trial and error” to strict hypothesis testing.

History again is useful to consider. One view presented by Thomas Kuhn in his “The structure of scientific revolutions” [Kuhn 1970] argues that advances tend to move in a “science as usual” fashion with modest changes to existing ideas (this is often dubbed incremental science), until a discovery is made which, eventually, will overthrow a previous paradigm (quantum mechanics rejecting determinism, for example).

We believe that prospective scientific researchers owe it to themselves to try to do research that might just change paradigms. This is a controversial issue, since few research organizations can afford to fund, and usually they do not fund, research that plainly states this as a goal. For their “stakeholders”, this is just too risky.

How then is a young researcher to decide on their first research project? Are they simply to accept the words of wisdom of a potential advisor? Most do precisely that! But make no mistake, your first or second research project will usually determine your future career which will hopefully last a lifetime of productive research in science. So there is a lot at stake.

3 Risk

To discover something new, risk is necessary. How is one to tell if a research project has a genuine element of risk and potential for discovery? One might simply ask:

What in the project is genuinely new?

Any good scientific advisor should happily answer this obvious question without feeling offended. But we believe that there there is also a less obvious form of non-newness that has evolved in response to external forces. Scientific research is sometimes so well planned as to admit little room for real discovery. Some research groups thrive by repeating an established process, but with a new twist. Under pressure from outside to deliver, increasingly “risk-averse” funding agencies often welcome proposals along these lines. Funding agencies also gain credit from development of new tools. Thus there is a trend towards “tool driven research”, in which the choice of a research project is driven entirely by new or older facilities and associated funding opportunities. Risk-averse work can also be easier to publish, genuinely new ideas often receiving greater scrutiny by referees. Members of such groups graduate “on time”, with several publications to their name, and a track record of
laboratory success, measured by these and other such “metrics”. But were originality and innovation inadvertently filtered away by a system that has become risk-averse? Therefore we encourage prospective researchers to ask the additional question,

**Does the research allow for truly unexpected outcomes?**

Good advisors will be delighted to hear this question from a potential student and will energetically discuss exciting possible new discoveries. In other words, we warn students about advisors who seem to treat graduate school as a revolving door, focused on quantity of results, publications, and other such “metrics”. For what kind of “metrics” could one apply to Einstein’s research, for example?

4 Experimentation

Computers are now far more widely used in scientific research than any other tool, with the exception of the human brain. Even the notion of an “experiment” is undergoing revision to include “numerical experiments”, in which “data” (Latin for “things that are given”) are generated by a computer program. This begs the question, if falsification through experimentation is a main-stay of modern scientific research, then what is the role of such “numerical data”? This issue is a tricky one that prospective researchers in natural science should be aware of, because of other recent trends:

- Many researchers are given a numerical model – a “code” – as a primary research tool.
- Others are given data from a remote machine or instrument wholly developed by others with data delivered by a computer.

- It is increasingly common to pit numerical data directly against instrumental data, without necessarily recognizing associated practical and philosophical problems.

The last point can be straightforward or tricky. Consider the claim that a non-trivial numerical calculation agrees with experiment. Nothing is refuted, the theory survives another day. But several important questions arise. What is the “information content” of the data, in other words, how strong is the connection between the essence of the model and the data? Could data have been acquired with a stronger connection to the model? Are there other models compatible with the same data? Have the authors inadvertently “cherry picked” from computed or experimental data, or both? Are there areas of disagreement, however minor, and if so, how is this handled? More generally, how are we to compare data from a virtual calculation with data from nature?

The common use of “numerical experiments” is relatively recent, and the current situation marks a departure from what was considered as standard as recently as three or four decades ago. Fascinating new phenomena have been discovered of direct relevance to science and nature, for example, in the field of non-linear systems. Entirely new subjects including complexity and chaos have emerged, sparked by the pioneering work of Fermi, Pasta & Ulam (1955, see Dauxois 2008). These new research areas are directly relevant to natural science. But there remain problems that are inaccessible to computers and that will remain so for decades to come. A well-known example is the coupling across enormous spatial and temporal scales needed to address critical problems in weather forecasting, atmospheric science, biology, astrophysics, and many
other fields.

We must return to our goal here to offer advice, not to delve into these difficult but important issues. Indeed we might risk the wrath of numerical experimenters merely by pointing these issues, so much of modern research is done using such tools. But numerical data are not on the same footing as actual data, if only for the simple reason that current computers always return a deterministic solution to a given problem. There is no genuinely “random” number generator for a computer, yet nature does just this. Thus a computer can in principle never simulate reality. Instead, researchers mimic randomness using techniques such as ensembles, multiple realizations of dynamical systems started from different initial states.

In a more mundane but no less worrying development, some new researchers have to treat a computer code as a “black box” that is assumed to represent something in nature itself. It is as if the computer has become a real, actual “experiment”. We are not qualified to assess further if computer data can be treated as if they were data from nature, so we restrict our advice to the following. We suggest that a prospective numerical scientist ask

**what is incomplete and/or missing from the numerical model, and how will the calculations connect with reality?**

If no satisfactory answer is forthcoming, then the prospective numerical scientist will shoulder much of the burden of making genuine breakthroughs, and of convincing the scientific community how their work connects with reality.

We have not been entirely fair in singling out “numerical experimentation”. If a computer and/or code steps into regimes demonstrably new owing to a technological breakthrough, the chances of doing excellent and meaningful research is significant. We are also limited by our brief human lifetimes – we do not live long enough to witness a galaxy merger, or the full life of a star. In such cases, analytic theory and numerical calculations can bring us closer to understanding phenomena than we ever could have hoped for otherwise. Perhaps surprisingly, “real” experimental data also suffer from problems in common with numerical data. Every measurement requires a finite time to make, we only ever measure “averages over time”. The averages are also usually over space, or they radically undersample the space of interest. Most stars with their associated complex phenomena are completely unresolved! In the same way that a numerical code for the dynamics of fluids is limited by the grids upon which they are based (or some equivalent parameters), observations are limited by their resolution or sampling rates in time, space, wavelength, among other parameters.

It must also be remembered that Nature does not always permit us to observe what we, as a society, might really like to know. Generally speaking, experimental data carry with them a certain – limited – “information content” noted above. We measure photons, electrons, other particles, bubbles traced by particles. In the area of solar physics, for example, we need to know how magnetic free energy is stored and released in solar plasma to produce damaging solar flares. But this free energy is not observable, instead we observe integrated signatures that are related weakly to it.

Fortunately, these potential worries, of importance philosophically, are often pragmatically allayed by asking again the deceptively simple questions:
What is new, what is the potential for genuine discovery?

The prospective researcher should be prepared for quite complex answers, for much work in science is also an art.

5 Scientific freedom

Seen from the outside, scientific research can appear to be an ongoing advancement, planned years before, perhaps like the slow development of a new town. But many exciting discoveries are made while making all kinds of mistakes and blunders. The mistakes often offer increased understanding. A good research environment will implicitly give permission to fail, at least some of the time. This is perhaps the most freeing part of any job. So we offer up the following suggestion:

Give yourself permission to make mistakes and even to fail.

By doing so, you will be able to take on risk. Research without risk is not in fact research, because something “new” must be discovered which, by definition, is unknown! Many historical instances testify that mistakes and failures are an important part of scientific discovery. The discovery of penicillin by Fleming is an obvious example. A case can be made that overly-planned, limited-risk research can lead primarily to incremental scientific discoveries.

There is an old cartoon (see the accompanying figure) showing a gentleman looking for a quarter under a street light. By using the tools he knows (the street light), he will never discover something important (the quarter) unless he takes a risk and tries something new (use a flashlight or metal detector, perhaps). The gentleman may find something, but he is “risk-averse”, he chooses to use only the tools at hand. Science advances when we advance through the dark, not when we remain under the streetlight.

As noted, funding agencies often demand to know the likely outcome of the research even before it has been done! “We ask to use this nice radio telescope to find something no-one else has found” is a proposition unlikely to succeed during any time in the modern era of astrophysics. But such “fishing expeditions” are an important part of discovery (see below). As a result, many researchers have learned to play a game, that of guessing outcomes, in order to succeed in gaining funding. Some actually know the outcomes before proposing, although no-one will admit this, because the research has actually been done. But risk is perhaps the most important ingredient needed to do truly meaningful research.

6 Research in large projects

A glance at the numbers of authors on publications reveals another interesting development. The days in which an individual scientist can make significant advances by working alone are getting rarer. It is easy to think of many examples of huge projects (LHC, Hubble Space Telescope, the Human Genome Project, ITER,...), as well as many smaller projects involving many individuals. In this state of affairs, prospective scientists should try to get a clear statement of how their original work will fit into a large project, and how the advisor plans to protect the student’s interests.

Good advisors of course understand this very clearly, they get funding promising one thing and
then, with a very open mind, perhaps discover something truly new. So, in addition,

*try to find advisors who welcome latitude in research,*

even if they are part of a much larger project.

7 Some final thoughts

*Get some experience as a summer intern.*

There are many opportunities! This will enable prospective researchers to get a feel for the research environment, and to think on the issue of science vs. technology, or both.

*Choose a research area that you find compelling.*

This may seem obvious, but there are many bright people who did not succeed because they simply did not have that dogged determination to solve problems that really interested them.

Tenacity is a great virtue in research. For most good researchers, their research is never simply a “job”.

*Find an advisor who does not insist on being on every paper that their students publish.*

A young scientist who has a single author paper is a rarity these days. But these are the only publications where the real mettle of the person can be judged by an outsider, unless the person is well-known in the community through presentations, meetings, contacts. Imagine reviewing yourself for a tenure track job in say 5-10 years time. How will you stack up if you have twenty-odd papers as a subordinate author, compared with a few first or a couple of single author papers?

*Find an advisor who loves their subject.*

The enthusiasm will be shared and will make the journey easier.
Find an advisor who at least knows about “Bayesian” methods.

This might sound strange in an article of general interest. But when comparing hypotheses (usually encaptured these days in “models” on a computer) with experimental data, this question will reveal the seriousness of the issue of comparison of theory with experiment. This question lies at the heart of methodologies in natural science. Bayesian techniques can help one avoid doing research of the kind that is “not even wrong”, by forcing you to make some kind of hypothesis and assess quantitatively how well a given set of observations are compatible with it. Bayesian methods are in fact not used as much as one would think, neither are they necessary. The alternate statistical description (taught first in colleges) – “frequentist” – draws a conclusion based upon the frequency of the results that lead to this certain conclusion. But Bayesian methods require one to make an hypothesis, up front, to be tested. In this sense they automatically satisfy and quantify the “falsifiability” test advocated by Popper.

Bayesian methods contrast with another perfectly valid kind of scientific research unkindly called a “fishing expedition”, for obvious reasons. Because there is no single accepted scientific methodology, “fishing expeditions” are sometimes exactly what is needed. Of many successful expeditions throughout history, Darwin’s voyage on the Beagle is a good example. This falls into the “anything goes” philosophy of science. Provided that some earlier barrier has been removed, these studies are essential.

In most subjects these barriers move slowly but surely. In solar physics, the last “big breakthrough” was arguably around 1989 when the internal rotation of the Sun was first brought to light using the technique called helioseismology. Since then, we have seen newer, better instruments, gradually stretching out those barriers in resolution, time-span, energy, measurement precision, something that may ultimately lead to a new and genuine break-through. But for now, we are in more of a phase of “business as usual” than “paradigm changing” solar research. We hope to be astonished by the findings of young people in the future, in which classes of models might be rejected. This “rejection” automatically satisfies Pauli’s desire to do research that is definitely not, “not even wrong”.

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Acknowledgments. We are grateful to Erica Lastufka, for helpful comments on the manuscript.

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