How to Nurture Scientific Discoveries Despite Their Unpredictable Nature

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ABSTRACT

The history of science reveals that major discoveries are not predictable. Naively, one might conclude therefore that it is not possible to artificially cultivate an environment that promotes discoveries. I suggest instead that open research without a programmatic agenda establishes a fertile ground for unexpected breakthroughs. Contrary to current practice, funding agencies should allocate a small fraction of their funds to support research in centers of excellence without programmatic reins tied to specific goals.

"As for the donkeys you lost three days ago,
do not worry about them..."

1 Samuel, Chapter 9, 20

1. Seeking Lost Donkeys but Finding a Kingdom

The biblical story of Saul searching for lost donkeys and finding his kingdom by chance, has an important moral for scientists. It is essential not to define your research objectives too narrowly and open your mind to discovering something completely different and more exciting lurking at the periphery of your field of view.

Funding agencies are obligated to justify their use of taxpayers' money over a period of several years. They are naturally driven to fund low-risk research with predictable returns. Here I argue that to maximize our long-term benefits, this approach has to change. In particular, funding agencies should allocate a small fraction of their funds (say 20%) to open research in centers of excellence without programmatic reins tied to specific goals. Such a funding scheme is essential for promoting breakthroughs in the long run, since it encourages researchers to take on high-risk projects with potentially high gains but fundamentally unpredictable outcomes.

An example of an unexpected result is the discovery of the cosmic microwave background by Arno Penzias and Bob Wilson, who were attempting to reduce the noise in their state-of-the-art horn antenna in 1965. They noticed a noise floor, which turned out to be the radiation left over from the Big Bang. Interestingly, this watershed discovery that forever changed our view of the Universe, was made at Bell Labs and not at a premier research university.

It is common to think about short-term goals in funding physics, but nurturing data-driven research with no programmatic goals promotes innovation and brings unanticipated profits. The data component is essential since extended periods of time without data allow unrestrained growth of speculative theory bubbles which might have no real value in explaining nature. Data plays the important role of guiding physicists in the right direction and posing new puzzles that need to be resolved, keeping the scientific process honest and exciting. The disappointment from failures to explain puzzling data is a crucial aspect of our learning experience, as it encourages creative individuals to come up with a new way of thinking about the physical reality. Over long periods of time, decades or more, the benefits from a data-driven culture without programmatic reins are so great that even profit-oriented businesses may choose to support it.

For example, Bell Labs recognized the virtues of such a culture in the 1930s-70s. This corporation assembled a collection of creative scientists in the same corridor, gave them freedom, and harvested some of the most important discoveries in science and technology of the 20th century, including the foundation of radio astronomy in 1932, the invention of the transistor in 1947, the development of information theory in 1948, the solar cells in 1954, the laser in 1958, the first communications satellite in 1962, the charged-coupled device (CCD) in 1969, and the fiber optic network in 1976. Such long-term benefits require patience and the foresight of paying it forward. Investments in centers of excellence, hosting creative individuals without a programmatic goal-oriented agenda, establishes a fertile ground for major breakthroughs. *If the business world recognized the value of such a culture, shouldn't scientific funding agencies recognize it as well?*

Christopher Columbus was funded by the Spanish crown to find a new trade route to the East Indies by sailing westward, but he discovered the new world of America instead. The funding agency in this case clearly benefited from his unexpected discovery, as he claimed parts of America for the Spanish Empire. Sure, it is important to justify flagship scientific missions by what we expect to find, but we should fund them mainly because they might open a new window for unexpected discoveries.

2. Opening New Windows of Exploration into the Universe

In the early 1960s, a panel of "experts" was assembled by NASA to evaluate the merit of a proposal to launch an X-ray telescope into space. The panel concluded that the scientific justification for such a mission was weak, since all we could expect to observe are stars like the Sun emitting in X-rays. The proposal was therefore rejected. After a decade of delay *Uhuru*, the first X-ray astronomy satellite, was launched. Contrary to expectations from the original panel of experts, we now know that the X-ray sky is rich and contains accreting black holes, supernova remnants, galaxy clusters, and many other unexpected sources. The lesson is simple: whenever there is a technological opportunity to open a new window for exploring the Universe, we should open this window without hesitation since, like Columbus, we might discover new territories that were not anticipated.

An example for a future window is gravitational-wave astrophysics. The proposed space-based mission *eLISA/NGO* (http://www.elisa-ngo.org/) expects to find black hole binaries in galactic nuclei across cosmic time, but it is possible that we will discover instead new sources that are not being imagined at the moment and these discoveries will revolutionize physics in the century to come. Unfortunately, the funding agencies do not share this vision and *eLISA/NGO* has not currently been funded.

In contrast, resources are abundant for projects with predictable results. Funding agencies are willing to invest over a billion dollars on the specific programmatic question: *is the energy density of the vacuum constant over cosmic time to within a percent?* This programmatic goal is guaranteed to yield results. The problem is that the range of possible outcomes is defined too narrowly. Restricted by programmatic reins, large teams of astronomers are aiming to reduce vast amounts of data with limited attention to the possibility of unexpected discoveries in aspects of the data that are not related to their main business agenda. This situation is analogous to Columbus sailing away from America and ignoring any unexpected territory which is not the East Indies.

Obviously, agenda-driven projects also lead to important long-term benefits. The recent discovery of the Higgs boson in CERN culminated out of a programmatic experimental effort to confirm a theoretical idea proposed in the 1960s which lies at the foundation of the standard model of particle physics. Although anticipated, this discovery opens the door to major future advances in unforeseen directions. Recognizing the important role of goal-oriented projects, I am not advocating that funding agencies should shift their primary focus to open research but rather that they should not ignore it altogether.[1] Indeed, Bell Labs continued to operate as a profit-oriented business during its innovation period, conservatively manufacturing goods that consumers buy, so that it could afford to allocate a small fraction of its revenues towards high-risk research.

3. Progress is not Linear in Time or Invested Effort

A few years ago, one of my PhD students worked with me on an elaborate project that took a year to complete. When the student showed me the first draft of our paper, I left many comments for him on the hardcopy. One of my comments was related to the Introduction section of the paper, in which we described the existing literature on the subject of our research. My comment said: "*Please add a reference that discusses a particular possibility that we appear to ignore in our work*". The student came back to me a day later and replied: "*Sorry, but there is no paper in the literature discussing this novel possibility*". We immediately realized that this unexplored idea would be an excellent target for an exciting follow-up project. We ended up writing a short paper that was published a few months later in one of the most prestigious journals for fundamental

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[1]For example, NASA asks proposers to list the key milestones they anticipate to accomplish. This request stands in conflict with the unexpected nature of innovative research, and can only be respected by proposers who take no risks.

physics. When the student presented the research at his PhD research exam, he dedicated most of his talk to the first project and only a short amount of time at the end to the second project. In other words, he chose to organize his discussion based on the amount of time that it took to complete these two papers, rather than based on their scientific merit. After his exam, I told him: "*Forget about the long project we worked on for a year. In your next presentation at a scientific conference, just focus on the exciting unexpected idea that we came across for our second project*".

Progress is not linear in time and sometimes it is even inversely proportional to the contemporaneous level of invested effort. This is because progress rests on lengthy preparatory work which lays the foundation for a potential discovery. Therefore, it is inappropriate to measure success based on the contemporaneous level of allocated resources. Lost resources (time and money) should never be a concern in a culture that is not tied to a specific programmatic agenda, because the long-term benefits from finding something different from what you were seeking could be at an elevated level, far more valuable than these lost resources. This echoes a quote from 1 Samuel (Chapter 9, 20), concerning the biblical story of Saul seeking his lost donkeys. The advice Saul received from Samuel, the person who crowned him as a king after their chance meeting, was simple: "*As for the donkeys you lost three days ago, do not worry about them...*".

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Further Reading

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