Discover how and why Assured Science came to be

3: Overview of the traditional system

It is a paradox that the traditional system aims to capture groundbreaking researchers and projects that rarely depend on it but poorly serves the vast majority of them that critically depend on it.

If you are associated with scientific research in any capacity you can glance over this essay, but do not skip it, as it provides the framework for the subsequent essays. Also, please bear with me, as I will be as general and non-technical as possible so that everyone, including the general public, can understand what I am talking about.

The prevalent system for research, which I will refer to as the traditional system, has remained essentially unchanged for more than four hundred years. Before the 20th Century, scientists were generally appointed to positions in an institution of learning, or a research laboratory, and provided with funds to conduct research as part of their responsibilities. In addition to research responsibilities, scientists were often required to teach, mentor, and carry out administrative work.

Since the early part of the 20th Century, scientists have also been appointed to positions with the expectation that they will obtain funds from an extramural agency (individuals, groups, foundations, commercial companies, private institutions, or governmental agencies). In both cases, scientists have pursued research projects that interest them, which also meet the objectives of their sponsors. Extramural funding is the principal source driving scientific research today (outside of in-house research conducted by commercial companies and governmental agencies). Researchers awarded extramural funds (grants) are called principal investigators and head research groups that range in size from small to very large, depending on the size and number of grants they are awarded.

Obtaining an extramural grant is an elaborate process. It requires developing a research plan, obtaining preliminary data, writing the grant application to standardized guidelines, and submitting the application to a funding agency following strict bureaucratic procedures designed to ensure faith and integrity in the process. At each step, a number of scientists, supporting staff, and administrators spend a considerable amount of time, resources, and money, further ensuring the integrity of the process. Sizable units/departments are established in institutions to facilitate the grant application as well as to administer funds awarded to successful applications.

Major funding agencies receive hundreds or thousands of grant applications every year. Panels of peers review applications, generally one to three times each year. The panels use a given set of criteria to assess the science, its significance, and innovation of the proposed research, and rank the applications from the most fundable to the least fundable. Depending on funds available and the current focus of the funding agency (often developed in consultation with scientists who are considered leaders in the field), a small number are selected for funding from among the top ranked applications. The funds cover direct research costs (personnel salaries, supplies, and services) and indirect costs to the institutions at which the research will be conducted. Indirect costs cover the costs of providing the necessary infrastructure, facilities, and resources for the research. Scientists go through the same application process each time they wish to obtain funds to continue a research project or start a new one.

Governmental funding agencies have established vast bureaucracies and departments, or institutions, to conduct peer review of the grants and regulate the funding process. Many private funding agencies also have similar, albeit smaller, establishments for this purpose. These administrative costs to the funding agencies are considerable even when only those items that can be calculated or tracked are taken into account.

There is an additional administrative cost that is generally ignored: the time and expertise of the scientists who review the grants. I am talking about the 'value of grant evaluation,' not per diem allowances or costs of travel to peer review meetings. Deemed 'service' to one's field, this cost is probably incalculable. A large number of scientists review numerous grants each cycle, at the expense of their own research, fully aware that most grants would be rejected, even the ones they support. I think that exercises in futility of any kind, let alone highly skilled and technical ones, are not just wastes but are also a cost because resources are diverted from accomplishing something else that is more useful and has a better return.

One of the first questions I pondered when I started thinking about research systems was: 'What does the traditional system of research hope to achieve?' I was interested in understanding what the system strives to achieve ideally rather than what it can achieve in reality. I believe that 'striving' best represents the purpose or the function of a system. To investigate this striving, I considered the general or common criteria used by funding agencies for awarding grants and stretched them to the extreme, on the positive side. From this perspective it appears that the traditional system is designed with the hopes of identifying and supporting the likes of Newton, Lavoisier, Darwin, Mendel, Faraday, and Einstein, people who make landmark scientific discoveries. Please keep in mind that this is an extreme exercise for the purpose of illustrating the point made below.

The discoveries of the people named above are well known to be landmarks in science, but the circumstances and contexts of these discoveries have very little in common. I doubt that any system can capture scientists like them. In fact, I think such scientists, and even the many that are of more modest brilliance or achievement, are a minority, even rarities, who do not depend on any system. They and their discoveries will emerge and will garner the attention of both scientists and the general public. Sometimes it takes time, as was the case with Mendel's discovery of the mechanism of inheritance that was re-discovered independently by three scientists more than 30 years later (and more than 15 years after his death).

A couple of intriguing thoughts about the system emerge if one considers the hypothetical situation of what would have happened to Einstein's career if he had worked on quantum mechanics first or to Darwin's career if he had worked on the theory of inheritance first. It is a wildly imagined scenario, for sure, but how do you think their theories of relativity and evolution, respectively, would have been received had they been proposed subsequently? It is a sobering thought because the traditional system is generally harsh on people who fail, particularly those who fail tackling big ideas. Lamarck comes to my mind — a great zoologist at the time, but one who missed on the big question and suffered. The second sobering thought is that even such brilliant people as Einstein and Darwin can be right only 50% of the time and when they are wrong they can be quite wrong. Consider also how rare it is to find scientists with repeat landmark discoveries who can join the league including the likes of Marie Curie and Francis Crick.

How Darwin and Einstein labored on in the last decades of their lives is the life of the majority of scientists. In fact it would be much worse because these regular scientists would have to contend with lesser abilities and support, which is often true even for those who ultimately end up making landmark discoveries. These scientists, and their projects, critically depend on the research system for finding new things or making the connection that leads to new things or discovering something that others can take further. Much of this scientific process is slow, unpredictable drudgery (Edison's proverbial 98% "perspiration" part of science), that almost guarantees a

DOA (Dead on Arrival) status at the funding agencies. Also DOA are projects exploring results that are unexpected, un-understood, or merely intriguing despite the fact that the history of science is littered with instances of how such results have led to breakthroughs often years or decades later.

It is a curious paradox to me that the traditional research system, while aiming to capture the "ground-breaking" researchers and projects that in reality rarely depend on it (as they would garner attention and funds somehow), poorly serves the vast majority of them that critically depend on it (including the latent or unexpected breakthrough scientists and projects). I call this paradox the "Faraday-Mendel" paradox, in honor of people with non-traditional backgrounds or little direct support from the system, who still have made remarkable contributions to science.

The crystallization of the Faraday-Mendel paradox in my mind was very much like observing data that come across the bench once in a while and provide both a solid basis and a clear path to pursue. I knew then what goal to achieve for the research system and how to go about achieving that goal. I simply sought to identify problems in the traditional system that need to be addressed in order to resolve this paradox.

Scientific research is not only one of the biggest human enterprises but also one of the oldest. So, I expect you to be skeptical. I was too, until I resolved to find a way, fearing that bench scientists and small laboratories might go extinct and with them the pleasures and value of small science for the generations to come. If you also believe in the integrity of this purpose, please join and bring all your skepticism so that we can together create enough ingenuity for the system to succeed and flourish.

The problems I have identified in the traditional system fall within three main categories: funding problems (i.e., problems with the input to the system, in other words, with the fuel), mechanism problems (i.e., problems with the way the system works, in other words, with the engine), and data problems (i.e., problems with the output of the system, in other words, with the work done). I will talk about these problems in my next three essays. In the seventh essay, I will talk about fixes to the traditional system proposed by others as well additional criteria or guidelines I have used for developing the new system for research.

Please read all of the following essays, in sequence, because eliminating, mitigating, or avoiding the problems plaguing the traditional system, and making the research mechanism very inclusive and robust, were both critical factors used in designing the new system. Essays 4 to 7 are longer pieces because I want to present to you in detail the thoughts, experiences, and arguments that influenced me, as well as the core values, principles, and purposes I envision for the new system. For this important communication, I prefer clarity and coverage rather than brevity.

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