**Rating Risk**

Too many young physicists jump into fields and projects not knowing their intellectual risks. There’s a better way, argues Avi Loeb.

In physics, the value of a theory is measured by its agreement with experimental data. But how should the physics community gauge the value of an emerging theory that has not yet been tested experimentally? With no reality check, a hypothesis like string theory may linger for a while before physicists will know its actual value in describing nature.

This sort of uncertainty not only has real implications for the knowledge-gathering of the scientific enterprise, but for fledgling physicists . The investment of research time in strong intellectual assets is of critical importance for beginning graduate students who wish to establish their careers on a good foundation. Yet young researchers are unaware of the full menu of research areas and the history accompanying each of them. In the current research landscape, students often have to rely on a word of mouth from their PhD advisor or colleagues.

I advocate crafting a website operated by graduate students that will use various measures of publicly available data (such as the growth rate of newly funded experiments, research grants, publications, and faculty jobs) to gauge the future dividends of various research frontiers. The analysis can benefit from past experience (e.g. in research areas that suffered from limited experimental data over long periods of time) and aim to alert the community of the risk from future theory bubbles.

**Theory Bubbles**

The study of the Cosmic Microwave Background (CMB) as the Rosetta Stone inscribed with the initial conditions of our Universe, provides an example of how theory and data can generate exciting opportunities for young scientists. The field started with theoretical work in the 1960s and exhibited gradual progress in experimental capabilities over a period of several decades until experiments reached the sensitivity threshold for a detection. As soon as the *COBE* satellite reported conclusive evidence for the temperature fluctuation CMB across the sky in 1992, subsequent experimental work generated huge dividends for young researchers (theorists and observers alike) who joined this field at that time.

But then consider a hypothesis like string theory which attempts to unify quantum mechanics with Einstein’s theory of gravity, but so far lacked direct contact with experimental data over a long period of time comparable to a physicist’s career. The appearance of such an unusual branch within the tree of physical sciences followed the cancellation of the superconducting super-collider, a decision that sealed particle physics from contact with fresh experimental data on collisions of particles at higher energies than ever achieved before. A theory-based research program with only a loose connection to data resulted, in which most of the low-hanging fruit was picked up already and the elevated fruit takes longer and longer to reach.

When there are limited experimental facts to test the validity of an emerging field’s underlying theoretical ideas, the physics community should be able to call on a credit rating agency, not unlike Standard & Poor’s, Moody's Investor Service or Fitch Ratings in the financial world. This “agency” would evaluate the future promise of the field.

At first blush, senior scientists would appear best suited to rate the future promise of research frontiers. But too many of these physicists are already invested in speculative frontiers whose promise needs to be evaluated. This leads to a conflict of interests and wishful thinking – something like the AAA rating that financial rating agencies gave to the very securities from which they benefited (as the agencies were paid by the companies who issued these securities). This unseemly situation contributed to the recession that started in 2007. In the physics world, a long-lived bias of this type could lead to similarly devastating consequences – an extended period of intellectual stagnation and a large community of talented physicists unwisely investing their time in research ventures unlikely to elucidate nature – i.e. a “theory bubble” to borrow a term from the financial world.

**A Credit Rating Website**

The proposed website would use quantitative measures of publicly available data to gauge the promise and future dividends of different research frontiers. The evaluation metrics should factor in, with proper weights, all the ingredients that ultimately makes scientific research successful.

In the case of physics for example, this might include the existence of an underlying self-contained theory from first principles, the potential for experimental tests of this theory, and the track record of related research programs. The evaluation metric has to be pre-determined and anchored in numbers based on archival data. Of course, factors like intellectual excitement cannot be quantified, but as long as funding agencies are doing their job and the integrity of the researchers can be trusted, the data about the growth of a field should echo this excitement factor (albeit with a time delay). The website should be operated by graduate students, since they would be the main consumers of its recommendations and they also possess the lowest level of unwarranted bias or prejudice.

The data required to calibrate the free parameters of the ranking algorithms can be gathered through automated searches for keywords in electronic data archives (such as arXiv.org or NSF.org. Aside from automated searches, practitioners from fields that are being evaluated can submit supplementary data that will be incorporated into the analysis.

The relevant data includes the level of funding allocated to experiments and research grants, as well as the number of publications and faculty jobs within the particular field of research. The quality of the underlying theory can be measured on a scale where the maximum value represents a unique self-contained theory, derived from first principles, and the lowest value represents pure phenomenology with no theoretical understanding. The simplest model relates the rate of change in these parameters to a linear combination of their values. For example, the publication rate is expected to scale as a linear combination of the number of experiments, faculty jobs, and the available research funds. With the proper mix of theory, experimental work and grant support, a healthy research frontier would show exponential growth. Obviously, the next step in advancing this initiative would be to use historical data in calibrating a simple algorithm of this type to best match the evolution of particular research frontiers in the history of physics.

The evaluation algorithms can benefit particularly from past experience in research areas that suffered from limited experimental data over extended periods of time. The main purpose of the website is to extrapolate existing trends in research to the future and alert the community to the risk of potential theory bubbles. Since surprises are inherent to scientific exploration and innovative ideas are often under-funded, any predictive algorithm would occasionally fail in a particular field, but that should not take away the value of this endeavor in offering a global perspective on the current state of many other fields.

A balanced assessment of the level of risk and potential dividends for investing research time in emerging research frontiers can increase the efficiency of the work force, leading to stronger growth. In other words, the existence of a balanced rating algorithm can aid funding agencies (such as NSF, DOE, or NASA) in optimizing their allocation of funds to promote progress in physics research. In fact, it would be in the interest of these funding agencies to support the proposed website and provide incentives for talented students (e.g. through special grants or fellowships) to take part in this high quality statistical analysis, provided that no bias is attached to this funding.

The proposed website would be most effective if it convinces senior researchers to shift their focus to new research areas, perhaps as a result of funding agencies being influenced by the rating procedure. A strong feedback could lead to an exponential growth of successful disciplines. It is extremely important, however, that the funding agencies maintain balance and diversity among sub-fields, take some risks, and avoid funneling most of the resources to a small number of successful but conservative programs. (This point was explained in more detail in Loeb, A., Nature **467**, 358 (2010); *http://arxiv.org/abs/1008.1586*.}

After posting an extended version of this article on *http://arxiv.org/abs/1108.5282*, I received the following e-mail from the eminent physicist Freeman Dyson:

***“Dear Avi,***

***I agree with you a hundred percent and hope your idea will bear fruit. But the action has to be taken and the organization run by students, not by old people like you.***

***I was lucky to grow up at a time when students had no respect for elder statesmen. The elder statesmen at that time, Heisenberg and Dirac and Born and Schrodinger and Yukawa and Einstein, were all pursuing fantasies that were obviously going nowhere. So we ignored the elder***

***statesmen and went ahead using our own judgment. The students today should be doing that too.***

***Yours ever,***

***Freeman.”***

Needless to say more; I rest my case.

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