**Diversifying Your Research Portfolio**

Despite the potential downside, more young scientists should dedicate a portion of their attention to high-risk research, argues Abraham Loeb.1

I have noted with dismay that today’s young astrophysicists frequently invest their time conservatively in mainstream ideas that have already been explored extensively in

the literature. There’s a better way.

This tendency to play it safe is driven by peer pressure and job market prospects – and encouraged, on occasion, by senior researchers. Past decades saw the same phenomenon, but it is alarmingly more prevalent today because a growing fraction of observational and theoretical projects are pursued in large groups with rigid research agendas. [hoping to clarify this a bit. What exactly is it about large groups and rigid research agendas that makes young scientists conservative about their research choices?] And astrophysics is not a special case – scientists in a variety of fields feel pressure to be conservative.[OK to broaden your claim some?] [Also: if we can link up large groups and the conservative choices they spur from young scientists, we might be able to briefly mention something like the big science of genomics, and how your thought is relevant there as well. OK?

No need to delve into lots of detail – astrophysics stays the focus. But a good example from another field would strengthen your argument a bit and expand the audience of the piece.]

Young researchers should resist this trend and pursue innovative research, and senior members sitting on selection, promotion, and grant awarding committees should find better strategies for rewarding creativity. A change in attitude is crucial for the future health of astrophysics and other disciplines.[OK as tweaked?]

[Are you also arguing (this MAY have gotten lost in the edit) that young scientists who take on risk stand to further their careers? In other words, is it a good idea to take on some high risk in the interest of furthering one’s career in a competitive job market? Is there a practical strand to your recommendation? As it stands now, we’re arguing mostly that taking on more risk benefits the field moreso than the individual…]

**Investment Strategies**

Astrophysics, like other fields, has both safe and risky topics – I like to think of them as “bonds,” “stocks,” and “venture capital” (VC). The best approach for the fledgling researcher is to diversify his or her academic portfolio, making sure to always devote a small fraction of it to innovative projects with risky but potentially highly profitable returns.

The most common investment strategy of research time by young postdocs in astrophysics these days is 80% in bonds, 15% in stocks, and 5% in VC. My recommended strategy is 50% in bonds, 30% in stocks, and 20% in VC.

[I like the metaphor, but what exactly do you mean by an investment strategy here? That is, are you suggesting that, say, an astrophysics graduate student take courses in “risky” topcs? That they spend a big percentage of their time reading about them? It’s a nice metaphor, but we need to flesh out exactly what these percentages represent – just ‘time spent’? ‘time in the lab’? time taking courses?]

Figure 1 [we will probably only have room for one figure. Preference? Way to combine the two w/o it being too busy?]shows present-day examples of topics that belong to categories of low-risk (bonds), medium risk (stocks), or high-risk ( VC) investments of research time. Fig. 2. details Categories of risk management in astrophysics research. Each topic includes components with different shades of risk, some of which deviate from the listed category of the topic’s centroid.

Young investigators, searching for a research focus, have much in common with young investors looking to find the best investment options for their hard-earned money. In both cases, the younger you start the better.[new transition OK?] New discoveries are naturally made in unexplored territories. Young people are more capable of exploring the “roads not taken” because they lack an unwarranted baggage of prejudice (or adopt a flat Bayesian prior [what’s this mean exactly in this context?]) on the likelihood of discovery along these roads. The window of opportunity in a scientist’s career is often short: after tenure, most senior researchers get distracted by administrative and fund-raising concerns, and prefer to maintain a conservative profile that promotes old ideas within their discipline.

But young scientists should be cognizant that taking risks can entail unpleasant consequences and major challenges. [new transition OK?] First, it can be frustrating. By its nature, creative work is unexpected and provides less security than, say, the predictable task of a “mainstream” engineer who uses existing knowledge to construct a bridge. The required day-to-day effort is also no fun: in order to find one new idea that works, I usually search through numerous other ideas that either fail or have already been examined in the literature before. As a result, the fraction of my time dedicated to innovation is far greater than the fraction of my papers which are innovative.

Clearly, failure and wasting time are a common outcome of risky projects, just as the majority of VC investments lose money. The fear of losses is sure to keep most researchers away from risky projects.

Risky projects also bring about loneliness. Even after an unrecognized truth is discovered, the rest of the community often remains silent for a while. This situation contrasts with the nurturing feedback that accompanies a project on a variation of an existing theme already accepted by a large community of colleagues who work on the same topic. Princeton astronomer Martin Schwarzschild [who is, very briefly, known for what? Need to explain to our wider non-astrophysics/astronomy audience if we’re going to retain this reference…] once told me that in the 1950s most astronomers were working on binary stars, and conferences were filled with talks that sounded just like each other on this popular topic. According to him, the sociology of astrophysics has not changed since then – only the title of the current “topic of the day”. [but he passed away, yes (Wikipedia says 1997) have things changed in the last 10 years or so? Might need to rephrase or trim down…]

Indeed, conformism is not new. But the astrophysics community is bigger now than it used to be, with stronger social pressure and more competition in the job market. These forces exaggerate the herd mentality, not only in astrophysics and in other fields,[OK? I don’t think there’s a necessity to only refer to astrophysics in this case, yes? Again, trying to broaden message a bit…] to an extent that suggests a need for policy change by our funding agencies.

Add to this the growing fraction of observational and theoretical projects are done in large groups with rigid research agendas and tight schedules. While the mode of large groups (which I define as those that produce papers with longer author lists than their abstracts)[OK?] has dominated experimental particle physics for decades, it has become popular in observational and theoretical astrophysics only recently. In cosmology, the emergence of a “standard model” – the notion that the universe began with the Big Bang and continues to expand – [OK? We’ll need to very briefly define for the non-astrophysicist – esp since other fields might have their own “standard models”] offers secure research “bonds” in which young cosmologists can invest their time with minimal risk. [why’s this exactly? Suggesting that young scientists can’t focus on some aspect of the standard model and still be avant garde?]

**Risk Management**

Just as with monetary investments, investors must understand the level of risk, which often stems from a combination of personal inclination and social factors. [OK?]

Timing is crucial for making a profit. When should a young researcher be more inclined to invest in a bond, stock, or VC? Common sense suggests that the answer depends on the state of the field. Consider the choice between dark matter and modified gravity. Once experiments push the upper limit on the cross-section of Weakly-Interacting Massive Particles (WIMPs) down well below the expectation of most reasonable models, alternative gravity models appear more appealing.[again, we’ll have to explain briefly for our more general audience. That is: What do you mean by ‘once results “push the upper limit of the cross section”? Why exactly does this make gravity models more appealing? Need to tweak this so written for an educated layperson, e.g. a biologist who knows little about astrophysics]

But the risks have potentially big rewards. [OK?]High risk research, like a VC investment, has the attractive feature of being more profitable than anything else if successful. [OK?] Even if only one out of a million non-mainstream ideas bears fruit, it could transform our view of reality and justify the entire effort.

When I was a postdoc two decades ago, there was no standard model in cosmology, and it was more socially acceptable for young cosmologists to invest in “VC” ideas. [why’s that exactly? Because there was no paradigm, i.e. it was still wide open/investigators still looking for a model?] Today, young cosmologists are mainly investing in bonds with the premise that they offer greater security.[what’s a brief example of a bond in this case?]

We should keep in mind that historically there were trajectories of ideas that started as “venture capital,” turned into “stocks,” and eventually matured into “bonds.” [so is it the *ideas themselves* that went from VC to stocks to bonds, or was it *time* spent researching them? Just want to be consistent with the analogy…] Examples include the Big Bang, the Cosmic Microwave Background (CMB), and dark matter.[how, in brief, did that happen in each of these cases?]

In some cases, though, ideas that started as VC turned into “junk bonds” because…[why exactly? What made them junk bonds?] Examples include [we should pick, say, 2 of these given the space constraints – and then explain each of the 2 briefly for our wider audience.] steady-state cosmology, which held that [?], and the notion that dark matter is baryonic, meaning [?]

[BTW, as an aside that might be relevant: How would you categorize string theory (which you mention below)?]

**Fool’s Gold**

My advice to pursue high-risk research should not be interpreted as an urging to pursue every bizarre idea that comes along. [new transition OK?] I suggest that young scientists, for example, avoid ideas that are second-order speculations (since the probability of both ideas being right is the product of the small probabilities that each of them is right). Astrophysics examples of such include non-Gaussianity from cosmic strings[more commonly known as string theory, yes?], formation of mini-black holes as the dark matter at the end of inflation[what’s wrong with this idea, again in terms broad audience could understand?]

And your research portfolio and priorities may need to be reallocated depending on what you discover. [new transition OK?] If you uncover a new unexpected finding at the end of your research project, study it carefully; if it turns out to be important, write your main

research paper about it even though it was unplanned and you invested much more time

in the original project. Notable historical examples include the unplanned discovery of the CMB in background noise tests of a radio horn antenna, or the accidental discovery of giant arcs associated with gravitational lensing in X-ray clusters, [which, in brief, was a big deal because?] Leave spare timefor the unexpected discovery that might change your research plans altogether (say, once per decade).

All of this means that selection and promotion committees as well as grant awarding agencies must find new ways to reward creative thinking. We [as in astrophysicists? Or fair to say sci community in general?]will all benefit richly from the implementation of a new strategy.[what are you referring to here, i.e. a strategy of what type exactly? And are you referring to the implementation of a new strategy by reviewers or students or both? We should specifiy… ] And for those who take “the road not taken,” keep your spirits up. After all, is there any point to doing science other than taking that road?

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1. Adapted from a banquet lecture at the conference on “The First Galaxies, Quasars, and Gamma-Ray Bursts,” Penn State University (June 2010).[OK?]