Taking "The Road Not Taken": On the Benefits of Diversifying Your Academic Portfolio[1]

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ABSTRACT

It is common practice among young astrophysicists these days to invest research time conservatively in *mainstream* ideas that have already been explored extensively in the literature. This tendency is driven by peer pressure and job market prospects, and is occasionally encouraged by senior researchers. Although the same phenomenon existed in past decades, it is alarmingly more prevalent today because a growing fraction of observational and theoretical projects are pursued in large groups with rigid research agendas. In addition, the emergence of a "standard model" in cosmology (albeit with unknown dark components) offers secure "bonds" for a safe investment of research time. In this short essay, which summarizes a banquet lecture at a recent conference, I give examples for both safe and risky topics in astrophysics (which I split into categories of "bonds," "stocks," and "venture capital"), and argue that young researchers should always allocate a small fraction of their academic portfolio to innovative projects with risky but potentially highly profitable returns. In parallel, selection and promotion committees must find new strategies for rewarding candidates with creative thinking.

"Two roads diverged in a wood, and I –
I took the one less traveled by,
And that has made all the difference."

from the poem "The Road Not Taken" by Robert Frost.

1. Introduction

It is impossible to forecast the scientific truths that will be unraveled by future generations of astrophysicists looking at the sky, since the Universe is often more subtle than our imagination. Attempts to establish a dogmatic view about the sky have often failed; notable examples include

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[1]Write-up of a banquet lecture at the conference on "The First Galaxies, Quasars, and Gamma-Ray Bursts," Penn State University (June 2010).

the notion that the Sun moves around the Earth, that the observable Universe has existed forever, or that there is little to be found in the X-ray sky. The history of our profession teaches us modesty. We cannot pretend that we have the final answers, especially at a time when most of the content of our Universe is attributed to unknown entities (dark matter and dark energy), and most of the comoving volume of the observable Universe has not observed yet[2] (see Fig. 1). Under such circumstances, we should allow ourselves to think from time to time outside the (simulation) box. It is always prudent to allocate some limited resources to innovative ideas beyond any dogmatic "mainstream," because even if only one out of a million such ideas bears fruit, it could transform our view of reality and justify the entire effort. This lesson is surprisingly unpopular in the current culture of funding agencies like NSF or NASA, which promote research with predictable and safe goals.

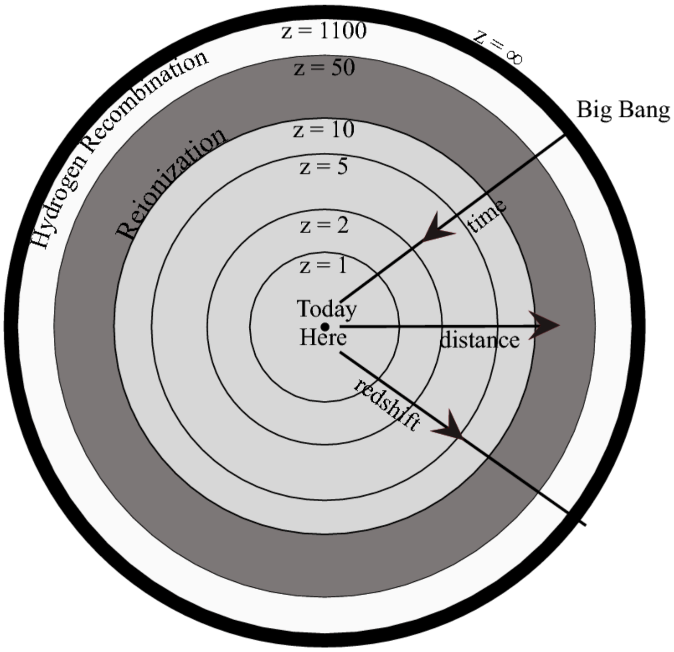

Fig. 1.— The distribution of matter through 99.9% of the comoving volume of the observable Universe has not been mapped as of yet. We should therefore avoid being dogmatic for now.

Personally, I enjoy doing science for the rare thrill of discovering something new about the Universe that has never been understood before. 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

[2] A. Loeb, "When Did the First Stars and Galaxies Form?", Princeton University Press (2010).

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. Although experience helps to cull out bad ideas without spending too much time on them, experience is also a double-edged sword since prejudice could compromise a rare opportunity for discovery.

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) 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.

Clearly, failure and waste of time are a common outcome of risky projects, just as the majority of *venture capital* investments lose money (but have the attractive feature of being more profitable than anything else if successful). The fear of losses is sure to keep most researchers away from risky projects, which will attract only those few who are willing to face the strong headwind. Risky projects are accompanied by loneliness. Even after an unrecognized truth is discovered, there is often persistent silence and lack of attention from the rest of the community 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. Martin Schwarzschild 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".

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 to an extent that suggests a need for policy change by our funding agencies. A 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 has dominated experimental particle physics for decades, it has become popular in observational and theoretical astrophysics only recently.[3] In addition, the emergence of a "standard model" in cosmology offers secure research "bonds" in which young cosmologists can invest their time with minimal risk.

The purpose of this write-up is to encourage young researchers to resist this alarming trend and pursue innovative research, and to encourage senior members sitting on selection, promotion, and grant awarding committees to find better strategies for rewarding creativity. A change in attitude is crucial for the future health of our field.

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[3]Here I define a "large group" to be one that produces research papers in which the list of authors is longer than the abstract.

2. Risk management and historical trends

Just as in monetary investments, the level of risk that a researcher chooses to adopt is a matter of both personal inclination and social factors. Figure 2 shows present-day examples of topics that belong to categories of low-risk ("bonds"), medium risk ("stocks"), or high-risk ("venture capital") investments of research time.

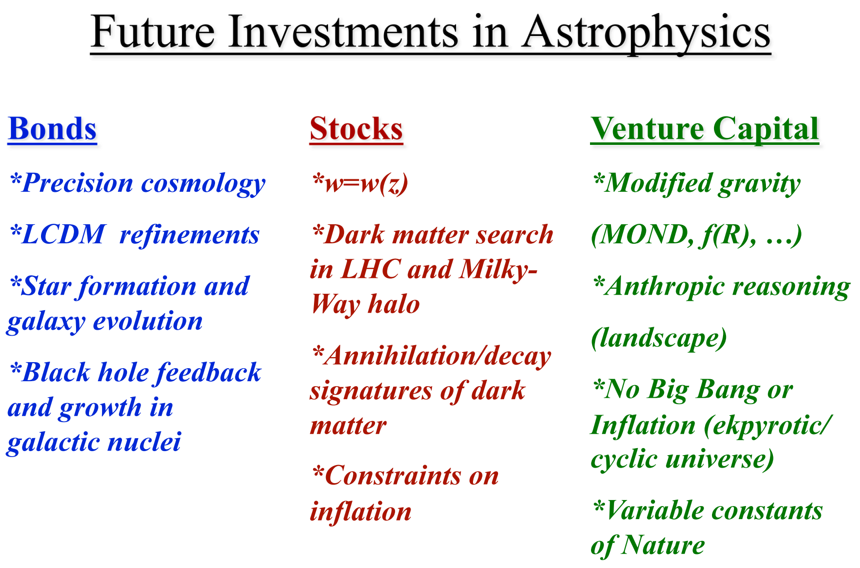

Fig. 2.— Categories of risk management in astrophysics research. Each topic includes components with different shades of risk, some of which deviate from the listed category of the topic's centroid.

Timing is crucial for making a profit. When should a young researcher be more inclined to invest in a bond, stock, or venture capital? Common sense suggests that the answer depends on the state of the field. An illustrative example is provided by 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.

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 "venture capital" ideas. Today, young cosmologists are mainly investing in bonds with the premise that they offer greater security. 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." Examples include:

- **The Big Bang**
- **The Cosmic Microwave Background (CMB)**
- **Dark Matter**
- **Inflation**

But the risks should also be recognized. In other cases, ideas that started as venture capital turned into "junk bonds." Examples include:

- **Steady-state cosmology**
- **Topological defects**
- **Dark matter is baryonic**
- **X-ray background from a hot intergalactic medium**
- **Quintessence**

The possible trajectories of innovative ideas in astrophysics are summarized in Figure 3.

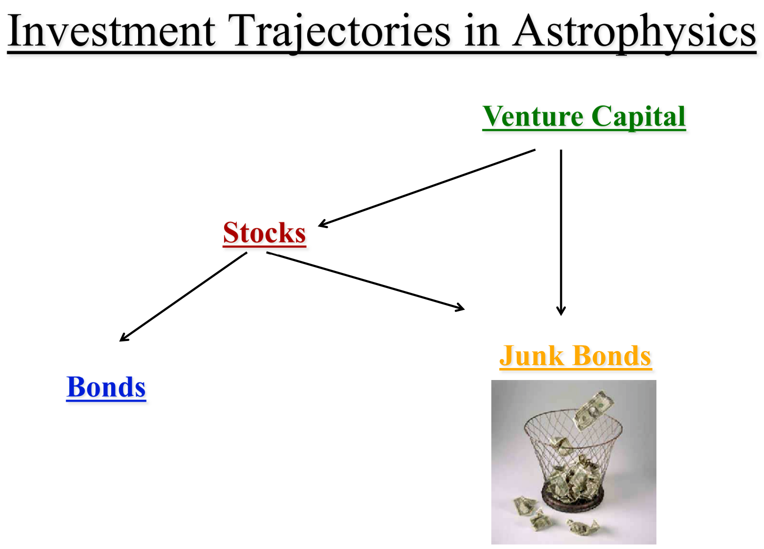

Fig. 3.— Historical trajectories of ideas in astrophysics.

3. Examples of Future Frontiers in Astrophysics

To illustrate how future frontiers in astrophysics may differ from traditional ones, let me list a few concrete examples. Given that science often brings surprises, my list is not meant to represent a forecast but rather a sample of possibilities. I am fully aware of Rabbi Yochanan's wise disclaimer reported in the Talmud: "following the destruction of the temple, prophecy became the trademark of fools and babies."

1. In the long-term future (centuries from now):

- **Biology away from the Earth:** life may be explored not only directly in our solar system (fish in Europa, enceladus), but also remotely in habitable extra solar planets. If mature civilizations exist, their signals may be detected. Astrophysicists may understand better the dark matter and dark energy based on the science done by these civilizations over the past billions of years (although this might feel like cheating in an exam). The origin of life might be explored in parallel in the laboratory.
- **Laboratory cosmology:** human-made experiments might attempt to study the inflaton or perturb the dark energy.

2. In the short-term future (decades from now):

- **Dark matter experiments:** might produce (LHC[4]) or directly detect (from the Milky Way halo) the dark matter.
- **Gravitational waves astrophysics:** Advanced LIGO[5] will be used to study neutron star-neutron star or neutron star-black hole mergers as the origin of short gamma-ray bursts. LISA[6] will map the growth history of supermassive black holes out to the first galaxies at $z \sim 20$, and test general relativity.

3. In the immediate future (years from now):

- **Imaging black holes** will be achieved with Very Large Baseline Interferometry (VLBI) at sub-millimeter wavelengths[7].

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[4] http://lhc.web.cern.ch/lhc/

[5] http://www.ligo.caltech.edu/

[6] http://lisa.nasa.gov/

[7] http://arxiv.org/abs/0906.3899

- **The 21-cm line of hydrogen in the 21st century** will be used to map the gravitational growth of perturbations throughout most of the observable volume of the Universe (LOFAR[8], MWA[9]).
- **JWST[10] and the next generation of large telescopes (EELT[11],GMT[12], and TMT[13])** will identify fainter and smaller sources of light than ever probed before.
- **Transient surveys (LSST[14] Pan-STARRS[15] and PTF[16])** will discover new types of explosions and variable sources.
- **Planet searches (e.g., with Kepler[17]):** will discover habitable planets around nearby stars.

4. Recommendations for the investment strategy of young researchers

The most common investment strategy of research time by young postdocs in astrophysics these days is:

- 80% in bonds
- 15% in stocks
- 5% in venture capital.

My recommended strategy is:

- 50% in bonds
- 30% in stocks

[8]http://www.lofar.org/

[9]http://www.MWAtelescope.org/

[10]http://www.jwst.nasa.gov/

[11]http://www.eso.org/sci/facilities/eelt/

[12]http://www.gmto.org/

[13]http://www.tmt.org/

[14]http://www.lsst.org/lsst

[15]http://pan-starrs.ifa.hawaii.edu/public/

[16]http://www.astro.caltech.edu/ptf/

[17]http://www.nasa.gov/mission_pages/kepler/main/index.html

- 20% in venture capital.

My other general suggestions are:

- **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). Examples include non-Gaussianity from cosmic strings, formation of mini-black holes as the dark matter at the end of inflation, a process that tuned the dark energy to have roughly the mean cosmic density today and to evolve roughly on the Hubble time today, and using Modified Newtonian Dynamics (MOND) + neutrino dark matter to explain the CMB anisotropies.
- **Progress is not linear in time.** 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 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.
- **Leave spare time** for the unexpected discovery that might change your research plans altogether (once per decade).

Selection and promotion committees as well as grant awarding agencies must find new ways to reward creative thinking. We will all benefit richly from the implementation of a new strategy. And for those who take "the road not taken," keep your spirits up. *Is there any point to doing science other than taking that road?*

I thank the organizers of the Penn State conference for assigning me the challenge of a banquet lecture early in my career. Special thanks go to I. Liviatan, J. Pritchard, and G. White for their helpful comments on the manuscript.