

DISCOVERY & DEPTH

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1. BACKGROUND

In the United States, the National Science Foundation (NSF) has commissioned a review of NSF-funded astronomy assets with the goal of determining how to best spend money for this decade.¹ The review is motivated by the perceived flat funding for Astronomy for the rest of the decade against the backdrop of paying for the costs to maintain current facilities, the anticipated great cost of paying for operation costs of new facilities (ALMA), future facilities (LSST, GSMT) and the desire to maintain a healthy funding for individual researchers and groups. The financial issues and costs are reasonably well determined. Thus the primary purview for the Portfolio Review committee is analysis and prioritizing of the astronomical returns derived from existing facilities, new facilities and the importance of funding researchers (individual or groups).

Here, accepting the boundary conditions posed above, I have focused on fields centered on optical astronomy which offers the best opportunity for progress in this decade.

2. ASTRONOMY & PHYSICS

Astronomy, like Biology, is primarily a phenomenologically driven subject. In contrast, Physics is a reductionist subject. A single profound realization (e.g. Newton's formulation of gravity) can keep a large army of physicists busy with figuring out the ramifications of this one realization. Thus in physics *Deep* understanding is what drives the field and the main activity (between periods of great understanding) is consumed on the ramifications. In contrast, in Astronomy both *Discovery* and *Depth* are equally important and neither is rare. Discovery is important in astronomy because we are not sufficiently imaginative to construct the Universe and its constituents from first principles. Discoveries are needed to guide us to the choices made by the Universe. Understanding requires deep observations (usually spectroscopy or detailed time series) and theoretical (and increasingly numerical) analysis.

Take, for instance, the case of dark matter. The expectation of dark matter did not come from theoretical considerations. Astronomers making some of the most basic measurements, in this instance the mass of galaxies and clusters via rotation curves, were forced to consider non-luminous matter or dark matter. Supernovae of the type Ia were studied by astronomers who were curious to know more about (the then) brightest explosions and

¹The introduction is to give some background for those readers of this article who are not familiar with the US astronomy scene.

entirely innocent of the future importance of these explosions for cosmography. We still do not know what makes Ia supernovae explode but their use as standard candles led astronomers to a model of an accelerating Universe. Neither of these great advances were a result of a well laid-out physics experiment but resulted from astronomers doing routine (and curiosity driven) research.

In astronomy, unlike physics, rarely a single discovery captivates the community and keeps a substantial fraction of the community busy for a long time. It is because, unlike in Physics, the inference we make in astronomy is limited by not merely the precision of measurements but also by our ability to “marginalize out” unrelated phenomenon. The case of solar neutrinos is an informative and illustrative example. The discrepancy between the measured flux of solar neutrinos and that expected from solar models was initially attributed to an incomplete understanding of the working of the Sun. Gradually, ever increasing and penetrating helio-seismological observations showed that the solar neutrino puzzle must have a different explanation. Thanks to concerted efforts – detailed observations of neutrinos from: the Sun, atmospheric neutrinos and accelerator neutrinos – the case for neutrino oscillations was established.

Another classic example is the truly amazing advances in our understanding of the Early Universe via detailed measurements of the Cosmic Microwave Background (CMB). Unlike the two examples given above the basic physics of the CMB intensity is very well understood (involving photons and electrons) and once astronomers obtained 1 part per million the ensuing progress was extra-ordinary. In contrast, it is most unlikely that we will see rapid progress in our understanding of dark energy. Unlike the solar neutrino problem we do not have supporting laboratory measurements and unlike CMB the theoretical foundation is murky. Finally the accuracy (inference) of astronomical measures (of all proposed diagnostics save that of CMB) for cosmography are subject to a host of systematics.

3. WHAT DOES THIS DECADE HOLD?

Astronomy has enjoyed a golden period and all indicators are that this golden phase will continue into this decade and beyond. One could reasonably argue that almost all sub-fields of Astronomy are enjoying a boom. Nonetheless, given the financial situation (§1) it is important to identify those sub-fields where the greatest progress is expected and ensure that adequate support is provided for such fields.

In my opinion there are three fields that will most certainly enjoy great growth during this period. This is not an exhaustive list and I will leave it to other proponents to argue for additional fields. The fields I have in mind are Extra-solar planets (§4), Astrometry (§5) and Transients (§6). Owing to my much greater familiarity with the field the case for transients is more developed.

4. EXTRA-SOLAR PLANETS

Extra-solar planets as a field did not exist until 1992. This field is now sizzling and has an assured future in this decade. A rich panoply of methodologies (occultation, precision RV, microlensing, high contrast imaging & spectroscopy) are now being pressed by astronomers

to study extra-solar planets. The costs for ground-based activities are modest: new high precision spectrographs for large telescopes, a dedicated network of telescopes for microlensing, radial velocity and occultation (in most instances this could be re-use of existing telescopes or a collection of inexpensive small telescopes) and exploiting extreme AO systems that will shortly come on-line on large telescopes (e.g. GPI on Gemini; P3K+P1640 on Palomar). The space-ground synergy of Kepler+Keck can be expected to yield a steady stream of spectacular results. The end of the decade should see a clear understanding of planetary architectures and this will constitute a fundamental and important advance in astronomy.

5. THE ERA OF ASTROMETRY & THE IMPORTANCE OF HIGHLY MULTIPLEXED SPECTROSCOPY

Astrometry is perhaps the oldest of astronomical methodologies. Over time, photometry, spectroscopy and adaptive optics flourished but astrometry remained behind. Along with many astronomers I see *Gaia* in making astrometry as powerful as other methodologies. The combination of *Gaia*, VLBA and AO-assisted narrow field astrometry will usher in an era of (ten) microarcsecond astrometry. In tandem ground based surveys (PanSTARRS-1 and SkyMapper) will extend (ten) milliarcsecond astrometry to fainter magnitudes. Separately, these surveys will complete the multi-band digital imaging revolution started by SDSS. I expect great progress in this field simply because of the incredible abundance of new data compared to what exists now.

As dramatically illustrated by SDSS photometry with spectroscopy has a great multiplicative effect.² For this reason, the anticipated revolution from *Gaia*, PS-1 and SkyMapper will only be completed with abundant availability of spectrographs with massive multiplexing (many thousands of channels).

The gains will be primarily in stellar and Galactic astronomy (including “near field” cosmology). The primary costs are in exploiting the trove of data that we expect from these missions and projects. Highly efficient single object spectrographs on existing medium and large telescopes will help astronomers study unique objects whereas BigBOSS & PFS (also on existing telescopes) will result in a comprehensive (elemental abundance, dynamics, mass distribution) study of our Galaxy.

6. TRANSIENTS

The third area which I expect to see spectacular advances is transients. In some ways this is an old field. After all, a century ago, the study of variable stars was a major focus of the biggest astronomical observatories. Thanks to Moore’s law operating not just for computing but also for transmission of data, for storage *and* for optical sensors, astronomers are now able to build, at relatively low cost, large field-of-view cameras and undertake analysis and rapidly transmit their results for follow-up observations. Radio astronomy is on the verge of undergoing a similar revolution (for similar reasons).

²The opposite is equally true. Large photometric surveys without appropriate spectroscopic and related follow up will have limited impact.

Elsewhere³ I have noted the great value of highly focused synoptic surveys. The Catalina Sky Survey is currently the most efficient Near Earth Asteroids (NEOs) discovery machine. The heads-up and rather precise localization of the entry point of the NEO 2008TC3 by Catalina [B] is perhaps the most cost effective sample return mission ever funded by any agency.

Another cost-effective and focused project and also based on two aging 1-m class telescopes is the Palomar Transient Factory (PTF). In only two years of operation, PTF has classified more than 1500 supernovae (and detected probably three times more candidates). A new class of luminous supernovae (and ascribed to the deaths of the most massive stars), the de-lination of sub-classes of supernovae (some linked to the coalescence of white dwarfs) and the rapid (within 11 hours of the explosion) of a nearby Ia supernovae are some of the highlights emerging from this project.

The first experimental detection of a coalescing neutron star binary will truly constitute a great advance in both physics and astronomy. In recognition of the importance of this area NSF has invested large sums of money (pre-LIGO, LIGO, eLIGO and aLIGO). Subsequent progress in this field would *require* electro-magnetic localization (if only to set the physical scale based on the redshift of the host galaxy). To do so requires not merely the detection of the electromagnetic counterpart but elimination of a vast fog of fore- and back-ground transients [C,D,F]. By 2018 (aLIGO era) we will have sufficient assets (PS-1, PTF-2, SkyMapper, DES, ODI, EVLA and rapid spectroscopy on large telescopes) to realistically pursue EM counterparts of nearby events ($\lesssim 200$ Mpc).

Separately, the current suite of synoptic surveys are very well suited to explore the phase space of transient searches (for which follow-up spectroscopy is essential; the current stable of 4-m and 8-m telescopes is adequate for this task). Going fainter with future facilities will be only useful for those transients which are either intrinsically faint or apparently faint (because they are located at cosmological distances). Synoptic surveys done on larger telescopes will provide better photometric precision and so subtle phenomenology in variable stars are better detected with large aperture studies (e.g. LSST).

So convinced am I of the promises of this field that along with colleagues I am proposing an integrated and dedicated facility consisting of outfitting three existing telescopes and equipped with a 40 square degree imager, a low resolution classification spectrometer and robotic AO photometric machine.⁴ This Zwicky Transient *Facility*¹ is designed to explore the phase space of transients in the range of minutes to a day.

7. AN OPTIMAL BALANCE

Here, I argue that progress in astronomy rests on making discoveries and obtaining the necessary depth of understanding. Discoveries usually arise from large surveys (e.g. the Hulse-Taylor pulsar, a landmark object; high redshift quasars from SDSS; an entirely new spectroscopic class (coolest brown dwarfs) from the WISE all-sky survey) or from dedicated

³The article, originally written for IAU Symposium 285 on Time Domain Astronomy, can found at <http://www.astro.caltech.edu/~srk/PTF0xford.pdf>.

⁴See white papers by N. Konidaris and C. Baranec and submitted to the Portfolio Review Committee.

programs almost always on small telescopes (e.g. the first brown dwarf and the first planet around a normal star) or from development of new techniques or methodology (e.g. cosmic X-ray astronomy).

In contrast, depth requires detailed study and almost always comes from large telescopes (e.g. the timing of the Hulse Taylor pulsar; the onset of the Gunn-Peterson effect from sensitive spectroscopic observations of SDSS quasars undertaken at Keck Observatory) followed by or preceded by modeling and theoretical studies (which, in many cases, requires laboratory studies).

If we accept the thesis of equal importance of Discovery and Depth in Astronomy then our resource allocation should be sensibly partitioned. However, in practice we start large projects based purely on aspirations (“bigger is better”, “more is more”) and with virtually no cost benefit analysis. Furthermore, one of the primary reasons for the Portfolio reasons is the increasing costs of operating large facilities. One could argue that the Lick 120-inch and the Palomar 200-inch were very expensive telescopes at the time they were built – either in absolute cost (appropriately inflation indexed) or a percentage of GDP. However, what is not arguable is that the annual cost of operations for modern facilities has increased dramatically and is roughly 10% of the capital cost⁵. It is useful to note that the annual ops cost for the Hale 200-inch is 5% [0.5%] of the initial capital outlay scaled by CPI-Urban index [scaled by GDP; ref. E].

Thus large facilities place a burden not only on the generation which funds the facility but also place a lien on future generation of astronomers. Thus the *Opportunity Cost*⁶[A] of large telescopes extends over several decades.

In times of easy finance one does not have think hard about undertaking cost-benefit analysis. Astronomy in the US (and to some degree in Europe as well) literally enjoyed a bubble with a rapid increase of funding over the past three decades. This bubble of funding coincided with a genuine bubble of ideas. The financial bubble has now deflated and we are suffering withdrawal symptoms, first in space⁷- and now in ground-based astronomy.

Rebalancing between large and “flagship” facilities and innovative approaches (which can be undertaken with new instrumentation on existing telescopes, re-purposing of old facilities) and taking advantages of synergies from existing missions and missions that will take place within this decade will allow US astronomers to continue to contribute mightily to astronomy – even in this financially constrained decade.

[A] Bastiat, F. 1848, “What is seen and what is not seen”

[B] Boattini, A. et al. 2009, DPS 41, #9.02

[C] Bloom, J. et al. 2009, arXiv 0902.1527

[D] Kulkarni & Kasliwal 2009, 2009astro2010S.166K

[E] MacDonald, A. 2010, pers. comm (to W. Sargent)

[F] Phinney, E. S. 2009, arXiv 0903.0098

⁵I include new instruments in the annual operation cost, appropriately spread out over the construction period.

⁶Well known to businesses who have to balance their financial books on timescales much shorter than that for governments.

⁷All points raised here are already applicable to NASA investments in Astronomy.