

## Directors' Perspective

### Big Problems, Big Time

A colleague recently sat in my office and explained how he had put in two proposals for HST time, one for 70 orbits and another for 50 orbits, specifically to avoid the dreaded “triple-digit” request. It turns out he was misinformed. The chances of getting an HST program approved in Cycle 10 were actually better (statistically) for 100+ orbit proposals than for proposals between 50 and 99 orbits, thanks in large part to Meg Urry and her innovative approach to structuring the HST peer review. No matter, he got the time anyway (sometimes you beat the odds), but it pointed out a problem we astronomers have with big programs.

A similar concern for size arose in a conversation about measuring the acceleration of the universe using distant supernovae as standard candles in a classic Hubble diagram. HST is capable of finding supernovae at redshifts as large as 2, as Ron Gilliland, Adam Riess, and their colleagues recently demonstrated to wide acclaim (Gilliland *et al.* 1999, *Ap.J.*, 521, 30; Riess *et al.* 2001, *Ap. J.*, in press, astro-ph/1014455). Supernovae at redshifts between 1 and 2 record the universe's expansion history in that important epoch and can help us decide if they really are standard candles. If we built up a sample of a few tens of such supernovae, we might begin to tease out that history sufficiently well to distinguish between various forms of quintessence and determine whether the dark energy was Einstein's cosmological constant or something else that now dominates the cosmos. HST could conceivably prove that the universe was quintessential—oh, what a press release that would make! It would cost a few hundred orbits of HST observing time, about 10% of a year's allocation, about the amount that the Director has at his discretion. But since less than 10% of the fields observed are likely to have supernovae at any time, the low yield would make it difficult to get the time through peer review.

Our colleagues in particle physics would wonder what the problem is. They are used to directing enormous enterprises aimed at getting one physical constant or one new particle critical to testing a prevailing theory. There is little doubt that a clear proof of dark energy and a demonstration that it is the cosmological constant, say, and not some other form would be a profound intellectual achievement for astrophysics. But in our present system of time allocation, a few hundred orbits all at once are difficult to come by even with the new TAC procedures. We tend to divide up our resources so as to satisfy the largest number of people. Yet keeping our science vital by demonstrating the capability and will to resolve big problems is also a matter of interest to us all.

Programs above some threshold can sometimes solve several scientific problems at once, although they are rarely proposed. A workshop at the Institute in March brought together astronomers with many different interests to examine how a large survey with the new Advanced Camera for Surveys (ACS) might address different fields of astronomy. Groups interested in the evolution of galaxies, active galactic nuclei, the measurement of cosmological parameters, mapping dark matter, and measuring Galactic structure met separately to propose surveys that would address the major questions in their respective

areas. Remarkably, almost every group came up with separate surveys that, except for minor details, looked the same. An epiphany occurred on the final day when we all realized that a single large survey might address simultaneously the burning questions in at least four fields of extra-galactic astronomy. I suspect similar synergisms exist between other fields not present at that workshop.

It is apparent that there is important science requiring hundreds of orbits of HST. Much of this science will need to be done rather soon after the new instruments are installed during future servicing missions. The charge transfer efficiency of CCD detectors deteriorates on orbit over a few years as a result of radiation exposure. Programs requiring the greatest sensitivity should be done at the outset. Of course, almost all programs push our instruments to their limits. Yet it would be hard to argue that a program to measure the cosmological constant, say, or to pick out the faintest galaxies in the deepest HST field, is of only passing interest and should wait until everyone has had a chance to look at his or her favorite targets. We need to do some things now because they have far-reaching implications for astronomy or physics.

Largely for this reason, we introduce the Hubble Treasury Program in the Call for Proposals for Cycle 11. The Treasury Program is patterned after the SIRTf Legacy program, for which the goal is to gather large data sets of lasting importance to astronomy and which need an up-front investment of a lot of telescope time. Because these programs are a public resource, there will not be proprietary rights to the data. The successful teams will have to produce data products in short order for all scientists to share. Through the Treasury Program, we hope to tackle some of the big problems as a community.

Discovering supernovae at redshifts beyond 1 is a tricky problem. The low yield of a dedicated search program would mean that more than 90% of the data would be useless for the proposed science. We can tackle this problem by making our programs multipurpose, a bit like the survey described above. We need to search for supernovae in deep images of the sky which we take for other purposes: to study distant galaxies, to measure gravitational lenses, or to follow gamma-ray bursters, for example. If we split the observations of each field into two sessions separated by about a month, we could find supernovae that erupt in those fields while also getting useful data for an entirely different program. No observations would be wasted, and we would discover some supernovae well-suited to measuring the geometry of the universe.

Because it is important to develop this methodology, the Institute is starting a pilot program to leverage approved observations calling for deep I band images of high latitude fields. We will observe these fields twice and look for supernovae in distant galaxies. It will take some experiments to figure out the best methods for rejecting false positive signals – from transient hot pixels, for example – and to refine our knowledge of the yield per observation. By the time ACS and NICMOS are available next year, we will be well placed to measure the magnitudes and colors of distant supernovae throughout their light curves, using them as standard candles to map out the geometry of spacetime.

As you prepare your observing proposals for Cycle 11 this fall, consider dual-use science. You may find that dual-purpose observations will have an irresistible appeal to the TAC, especially in cases where the primary science was is below the cutoff for approval.

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