Minutes of Astro2010 Town Hall Meeting
Held at Ohio State University, April 9, 2009, 1-3 pm

Lynn Hillenbrand representing Astro2010 committee.
Marc Pinsonneault moderating.
David Weinberg recording.

Attended by: Ohio State faculty, postdocs, and students;
Joe Shields, Tom Statler, and Markus Boettcher from Ohio University.

Prefatory remarks from the recorder: The recorded comments are far from verbatim; they are a re-creation and not direct quotes. A preliminary version was circulated to the attendees, so this version includes some corrections.

Introduction by Hillenbrand:
History of astronomy decadal surveys.
Structure and Charge of Astro2010 survey.

Weinberg: A good baseline for thinking about allocation of funding is equal amounts spent in equal logarithmic bins -- e.g., equal amounts in the dollar bins 0.1-1M, 1-10M, 10-100M, 100M-1B (and maybe a 1B-10B bin for space). This allocation probably should be considered separately for ground and space (or NSF and NASA). This rule shouldn't be rigidly applied, of course, but if there are large departures from it the committee should be sure that there is a strong rationale for the departure (e.g., maybe more is needed in the lowest bin because that is what supports training of students and postdocs and the greatest diversity of science, or maybe more is needed in the highest bin for space because otherwise one can't afford anything ambitious). But if the highest bin has substantially more funding than the next highest, one should really be sure that each item in the highest bin really has more science return than ten items in the next highest bin, and so on down the line.

My anecdotal impression is that the 1-10M bin is undersupported, perhaps because the items in it are too small to be individually ranked by the decadal committee but it also isn't the individual grants program whose importance is obvious to all.
The committee might consider making a specific recommendation on the amount of funding to go (on a competitive peer-reviewed basis) to projects in this category.

Also, in the last decade the most expensive NASA missions seem to have squeezed out the Explorers, though perhaps that balance is improving at last.

Hillenbrand: This is one way of thinking about the distribution of funding, but there is a wide range of opinions.

Jennifer Johnson: I served as a member of the ReSTAR (Renewing Small Telescopes) committee and gave input to the ALTAIR (Access to Large Telescopes) committee. The blueprints produced by these committees were based on lots of community input, and I urge the decadal committee to pay attention to them. In some cases the recommendations were quite contrary to what NOAO had been planning. Most notably, the community puts a high priority on maintaining access to a broad spectrum of workhorse instruments on telescopes of various apertures to support a broad range of science. If substantial fractions of time on major telescopes are allocated to surveys or to observations with specialized instruments, then we need to replace the lost access to workhorse capabilities.

David Nataf: Where do TMT/GMT fit into the discussion and priorities?

Hillenbrand: This is clearly one of the big issues facing the committee.

Weinberg: I'd like to comment specifically on GSMT (generic stand-in for 20-30m telescope or telescopes). I am very skeptical that this should be a top priority for national investment in the next decade. First, I think it will put so much of the funding into the highest bin that it risks killing off everything else -- and maybe itself as well, as it could become astronomy's Superconducting SuperCollider. Second, I think most major advances in astronomy come from programs that involve lots of observing time (as well as new technological capabilities, but lots of observing time is an even more consistent ingredient). With one big telescope, the number of people who will be able to get enough observing time to do breakthrough programs is small. There will be some great science that comes out, but it will be from a small number of people or groups at the institutions that have large private shares in the telescopes. The rest of the community will only have enough time to do incremental science.

Gemini is an instructive example. It has had relatively low impact because it is essentially one telescope shared by the whole U.S. community. Conversely, when the Europeans went into VLT, they built four telescopes, enough to enable large observing programs, and they instrumented them well.

At the very least, the threshold for making GSMT investment a high priority should be that taking half that amount of money and spending it on improving the performance (or increasing the number) of 6-10m telescopes would not yield more great science.

Tom Statler: The top goal in each decade doesn't have to be "we're going to build the biggest telescope, again." The top priority could be to develop strong instrumentation on existing telescopes.

Also, in assessing a particular facility, the users community is not the only useful source of input. You also want to know the opinions of the "not quite users" -- people who would take advantage of the facility if it had better instruments or were more straightforward to learn how to use.

Connected to instrument development, I think the astronomical community has undervalued training in instrumentation. We may not be training the people we need to build the instruments that actually make our telescopes worthwhile.

Kris Stanek: I think it's clear that the biggest telescope should NOT be the top priority. The biggest impact in the current decade has come from projects like the Sloan Digital Sky Survey or 2MASS that have produced powerful PUBLIC data sets.

Rubab Khan: Going back to instrumentation, I think there is a perception among students that instrumentation isn't viable as a primary training path. If you're trained primarily in instrumentation, you won't be able to get a job.

Jason Eastman: As a student working in instrumentation, I partly disagree with this statement. It's true that instrumentation students are evaluated largely on the basis of the science they do with their instruments, but that science IS a critical part of the training -- you can't build good instruments unless you know what science should guide them.

Weinberg: What is the committee's thinking on adaptive optics, on the ways that it might be supported and the importance of doing so?

Hillenbrand: I can't tell you what the committee's thinking is. However, we did get numerous white papers on AO technology (which are now public). There has been lots of private and public investment in those AO systems.

Weinberg: A threshold for investing national resources in GSMT should be that the science return would outperform the return of a comparable investment in AO systems and instrumentation for existing telescopes. There are huge gains to be made if one can get each 6-10m telescope to the point of wide-field diffraction limited imaging on a routine basis and equipped with powerful instruments. It seems crazy to move forward to a hugely expensive and challenging large telescope when we have not exploited the capabilities of our existing telescopes, which offer much lower hanging fruit.

Chris Kochanek: The point is stronger than this, because the success of a 30-meter telescope is predicated on wide-field AO working essentially perfectly -- otherwise the instruments are too expensive and the science gain is limited. We haven't shown that we can make AO work well enough to support a 30-m telescope. There's an attitude that "we'll work it out because we have to," but we should be resolving this issue on 8-m telescopes before we go on to building bigger telescopes.

Rick Pogge: The previous decadal survey underestimated the costs of instrumentation and operations. This was a critical failing. Gemini, in particular, lost out to other telescopes scientifically because it had insufficient instrumentation funding, so we couldn't equip it to compete with (say) the VLT.

Hillenbrand: Roger Blandford, the decadal committee chair, is strongly committed to full lifecycle costing for facilities. We have people on the committee whose specific expertise is cost estimation, so we will not just be taking the projects' own words on how much they will cost. There is also an independent contractor who will be participating in a detailed costing verification exercise for selected projects.

Kochanek: Will there be review of the relationship with the funding agencies? There is a steadily increasing burden of inane paperwork. The review process is getting slower and slower, and the unpredictability of timing is a problem in itself. The emphasis placed on different criteria (intellectual merit vs. broader impact, and what counts as broader impact) seems to shift randomly from one year to the next.

[If there was a response to this question, I missed it.]

Weinberg: Since I've been making mostly negative comments so far, I'll say that I am enthusiastic about LSST. I'm not convinced that a single big telescope is really better than a global network of 3-4m telescopes, but it looks like LSST is the one that has the best chance to happen. It will support a broad range of science as well as having a big impact in some specific identifiable areas like dark energy and the outer solar system. It will produce public data sets that will be widely used and spark a lot of activity.

I also think we need a wide-field spectroscopic capability to match the wide-field imaging capability of LSST and other facilities, something like WFMOS and/or wide-field IR spectroscopic instruments. The conventional wisdom is that if you do imaging with one telescope you need a bigger telescope to do the spectroscopic follow-up of the things you find, which would argue that an 8-m imaging telescope needs to be accompanied by a 30-m spectroscopic telescope. But the SDSS shows that you can also get a lot of science from massive, systematic spectroscopy of the sources that are significantly brighter than the photometric limit. In the case of SDSS, the spectroscopy was done from the same 2.5-m telescope. Wide-field, highly multiplexed spectroscopic capabilities on 8-m telescopes would be a powerful complement to LSST-like imaging surveys.

Paul Martini: The likely cost of a very wide field, highly multiplexed spectrograph like the WFMOS concept is somewhere in the range of $10-50M. In general, state of the art instrumentation for 8-m class telescopes, comparable to what the Europeans have built and continue to build for

VLT,
will cost on order $10M each. At present there does not seem to be a good mechanism in place to request funds for instruments of this scale. For example, this is roughly an order of magnitude above the typical MRI proposal to NSF. It would be useful to have a program to fund projects in this range, even if only one were funded for every 10 $1M instrument proposals. This also presents a good illustration of how the principle of equal funding across logarithmic intervals, discussed at the beginning of the town hall, could have a substantial positive impact.

Weinberg: I think the TSIP program has been one of the big successes of the last decadal report. In addition to the instruments themselves, it has been a good model of getting time on large telescopes for the community in exchange for financial support. One could also add to this model the option of buying time directly from private observatories -- since many universities are financially strapped, I think it would be quite possible to do this.

Joe Shields: As someone who comes from a university without its own large telescope, I strongly endorse this comment. The time allocated to the community through TSIP has been enormously valuable to us. I think the TSIP program does include the possibility of directly purchasing time but that this option has not been used.

[I made some comments here about JDEM and LISA, which I am omitting from the record to avoid any conflict with my role on the Cosmology and Fundamental Physics Science Frontier Panel.]

Smita Mathur: I think that Con-X/IXO should be given higher priority than JDEM because (a) it is a general purpose facility rather than a specialized facility, (b) X-ray astronomy can only be done from space, and (c) the questions that JDEM is supposed to answer, e.g., whether dark energy is a cosmological constant, may already be answered by ground-based efforts (like the Dark Energy Survey or SDSS-III) by the time the mission is ready to fly. Maybe the optical/infrared imaging is easier to do from space than from the ground, but if you don't go to space there is no X-ray astronomy at all. General purpose missions have more capability of discovering things we have not thought of, compared to missions that are dedicated to investigating a specific question.

Statler: There is a wide range of interesting science questions and possible discoveries. We need to balance mission-specific facilities (like WMAP, or JDEM) with general purpose facilities (like HST, or Con-X).

Shifting subjects, I'd like to ask about ALMA. It's a huge investment. Is it going to serve a broad community?

Pogge: Chandra opened up X-ray astronomy to a wide community. Before Chandra, you usually had to be an X-ray specialist to use X-ray data, but with Chandra if you have a good science idea you can propose to investigate it, and you get data that you can do something with.

Hillenbrand: The ALMA people say that they will do this, and that ALMA will be accessible to a broad community. But this does come at a cost; it is more expensive to make a facility that can be used by non-specialists, and it is expensive to provide a good data archive.

Pogge: These are big force multipliers, well worth the investment.

[Discussion of electronic publishing, in which I did not capture many of the comments. Several comments that the NASA ADS abstract is a HUGE benefit to the community, a real boost to scientific productivity, and that the decadal survey should endorse it explicitly.]

Statler: Education and Public Outreach is highly segregated from research. There are big barriers in both directions. If you're at a research university, then engaging in E/PO doesn't get you much credit. It is also hard to do something useful in E/PO with a small amount of time, so there is a barrier to researchers spending enough time to make progress on E/PO. Therefore the E/PO is done by specialists, often at different institutions, and the results of their research often doesn't get communicated back to the research astronomers.

Weinberg: We desperately need better ways of multiplying E/PO efforts. In the case of astronomy research, a useful contribution has to be novel, something that someone hasn't been done before. That isn't at all the case with E/PO -- if something good has been developed, there is no reason not to to reuse it many times. We don't have good mechanisms for re-using what's been done, or even for becoming aware of it. If we had these mechanisms, they would provide leverage so that research-focused astronomers could have valuable E/PO impact in the time they have available.

Martini: My impression is that the selection of E/PO proposals is too focused on new and novel approaches, while the emphasis should be on maximizing public benefit. This impression stems from NSF's CAREER proposal process in particular, but also from E/PO proposals related to various NASA missions and, to some extent, NSF's broader impact criterion for all proposals. Unlike in astronomy research, where the new and novel is a virtue, it should be perfectly acceptable to request E/PO funds to simply copy proven approaches. In addition, E/PO experts are generally better equipped to develop effective approaches to E/PO than astronomy experts. The evaluation of E/PO proposals should place greater emphasis on reaching larger segments of the public in general, or targeted efforts to reach groups that have been traditionally underrepresented in math and science. This would provide more cost-effective use of E/PO funds.