Astro2010 Town Hall meeting in Boulder (University of Colorado and HAO/NCAR)

University of Colorado JILA auditorium

Thursday **22 January 2009**, 4:00-5:30 pm

A diverse audience of about 60 persons was in attendance. There were postdocs, students, faculty, and a variety of wavelength regimes represented, including solar and most other modes of astronomy. Juri Toomre and Mike Shull represented Astro2010. Jim Green coordinated the meeting, and Jeremy Darling and Sarah Gibson were the scribes who briefly summarized here the issues raised.

Juri Toomre began the meeting with a general presentation of the structure, timelines, membership, and procedures of the Decadal Survey.

There followed general discussion of the procedures, priorities, community concerns, previous pitfalls, and science cases for the Survey, outlined below.

DISCUSSION TOPICS:

<u>Decadal Survey priorities</u>

Need for clear prioritization

Green: The last decadal survey presented a smorgasbord of priorities without clear prioritization and beyond what resources ($) could support. There was subsequently a diffusion of the community's will. Everyone wanted his or her own piece of the pie. The new survey needs to be clear about what is most important, what needs to be done next, and how to get the community behind the survey priorities. Everyone can't get a piece of the pie. We need to make the hard decisions and be clear about them.

Green: This is our once-per-decade opportunity to identify our priority. NASA wouldn't have gone to the moon if they knew the cost. We need to have the idea we're building something no matter what. The community needs to crystallize its will. If everyone says they want everything, the survey won't work.

Gilman: The AURA board has been faced with two groups each wanting to build a 30-meter class telescope. The community needs to make a decision.

Need for balance of assets

Burns: Broad-capability telescopes are expensive: e.g., JWST costs 4.5 billion. There is a trade space. Earth sciences went too large in their planning, and only a few were funded. Lots of things fell through the cracks. Now they are moving toward smaller, targeted

projects. The ideal is a mix of large and small. At most we should plan for one large flagship in space and one large ground telescope per decade.

Bally: However, we need to carefully study the advantages/disadvantages of a big telescope able to do multiple tasks vs. lots of little targeted ones. Before we commit to small, dedicated missions, we should consider the possibility of one dedicated aperture that can be multiplexed.

Cash: How will Astro2010 decide what is the "best science"? That decision is crucial, particularly if funding can only go in one direction. Scientists might decide dark energy, but people on street might choose the search for life (and they are the ones paying).

Toomre: Astro2010 probably won't choose only one monolithic idea – it is more likely that a few will be promoted, and balance argued to be essential. The question is how much money there is, and is anyone listening to arguments for balance.

Hamilton: We need a fractal distribution of sizes, down to the smallest things. If all the emphasis is on handful of big projects, these overshadow smaller things. Is Astro2010 only doing top things, or will it worry about the rest of it?

Toomre: The committee agrees that the concept of a mixed fleet needs to be addressed, as do operating costs. A mixture including theory, laboratory analysis, etc. is also a keen concern. This time round there is no theory panel. Last time there was, but it was largely ignored. The idea this time is that each Science Frontiers Panel will ask what needs to be done theoretically, in the laboratory, and in technology to accomplish the science they are considering. The question is, do the agencies pay any attention to this, or will they just look at the matrix showing rankings and focus on the top two space and ground telescopes (and ignore all discussion etc. about mixed fleet/balance)? There is sensitivity to this issue in the panel, but uncertainty on how best to be effective.

Shull: Of the 5 panel heads, 4 are theorists. We envision one flagship/large ground-based project per decade, medium-sized ones perhaps every 3 years, smaller every 2 years. So, something like the product of frequency times size to be equal on scale and rough cost (i.e. fractal).

Need for realistically-planned support of assets

Burns: (discussing McCray report - Radio Futures) That report looked at the full landscape. ALMA will revolutionize star formation, accretion disks, cosmology, and the search for exoplanets. Supporting it over the next decade is a real concern because new facilities will operate in full service mode. There will be maps, for example, which represent a new feature, with new costs. The McCray committee recommended that funding be allocated along with telescope time. The NSF is reluctant to move in this direction. The message we need to send is: take care of your big new instrument, and support it well. Also, there is a need to synergize between wavelengths. This theme should be echoed throughout the 2010 report.

Need for clear and consistent guidelines from agencies

Knoelker: Agencies tend to change accounting rules on projects. Operating costs are generally not clearly planned for. Rules may be different for optical/IR vs. radio. Agencies change how they deal with risk. Projects balloon because of changes in accounting rules. What will be the rules be under which agencies want to see projects prioritized/planned for?

How previously ranked projects will be dealt with

Gilman: The failing of the last DS was that few of projects proposed got very far. This was true for ground-based projects in particular. What is the guidance to committees about re-reviewing old ones versus new ones?

Toomre: The agencies funding Astro2010 (NSF NASA DOE) say: anything that isn't in construction should be re-reviewed. The Program Prioritization Panels will look at what has happened to all the old projects and costing models. DOE encourages caps to be put on total amounts for those that are likely to be built. Three levels of budget should be planned for: enthusiastic/enthralled; moderate; and tight.

Need for technology development

Froning: For technology development, ground-based is not as well supported as space-based. There is currently no good funding mechanism for technology development.

How U.S. science connects to European

Toomre: Europeans want an EELT 42-meter -- Keck technology but larger. 42 is bigger than 30 (and also significant a la Hitchhiker's Guide). We used to think 8 and 10 meter were so big that there could only be one, but there are now many in existence! Maybe there could be more than one 30+ cases, but we need sugar daddies. Also operating costs need to be taken into account, which are difficult with private funding.

Shull: There is likely to be cross-pollination with Europeans, both in facilities and in priority-setting.

Knoelker: An issue that came up in the last DS, but was not dealt with, was ground vs. space based astronomy. For NSF what we say we can only do once per decade is in the NASA cost envelope of a Discovery class mission. There is a mismatch in scale. What didn't happen was going out to the Hill to sell this problem. Europe (by comparison) has a well-oiled machine with operational costs generously provided for.

Exciting science

Dark energy

General concurrence that this was a central research theme in the coming decade.

Comparative stellar/planetary systems

Knoelker: In addition to fundamental cosmology/dark matter/dark energy, to be pursued with JWST, ALMA, etc., a big change in the last decade is that we now know there are 335 planets orbiting 284 stars other than the Sun. Our detection limit is down to Neptune size (ish), and there are scenarios being considered to go even smaller. How should we catch the imagination of the general population? Astronomy is in general good at that. Extra-solar planets are an opportunity to do so: people are fascinated by the chance to see terrestrial planets. The concept of the development of habitable zones in systems comparable to ours is a whole new frontier. We need to bring together what we know about star formation, planetary formation, and stellar evolution in that context.

Gilman: So, comparative stellar-planetary systems -- how does this fit with the current Astro2010 science panel structure, and will Astro2010 address it?

Toomre: Kepler will force the issue. Also panelists are smart and inclusive.

Time-domain astronomy

Metcalfe: Time-domain astronomy is a next frontier, with movies of the sky from facilities like Panstars, LSST to be available. Smaller projects can specialize in areas of phase space, smaller time scales, and more complete coverage. Asteroseismology is important. New technologies like optical interferometry are important. We need to learn how stars are built.

Darling : Commensal observing, whether radio, optical, etc, is the future, and time-domain astronomy is one of the beneficiaries. Some aspects of this are already underway at radio facilities.

Interstellar dust measured in situ

Horanyi: Interstellar dust exists in the Solar System. We can capture it, bring it back to the lab, and do isotopic work and chemistry. We can do lots of astrophysics with it. We need to do something like STARDUST, which did very well for cometary dust, but not interstellar dust. How can we interest astronomers in this?

Computational facilitation of astrophysics

Brown: Theoretical astrophysics is greatly facilitated by computation. We can use simulations to probe the physics inside stars, and magnetic turbulence throughout astrophysics. Also, stellar physics, stellar structure other than sun, dynamos and magnetic field generation are hot topics. There will be a lot of growth in the next decade.

Toomre: Also simulations can probe the large-scale structure of the universe. There are many threads.

Vasil: Supercomputers are going to be built – we should make some we can use. The paradigm has been to string Dells together with inadequate communication. There are other ways that need to be explored and developed.

Knoelker: This is a grand challenge – numerical simulations can guide observations, theory, and data mining in general.

Solar coronal magnetic field observations

Gibson: The storage and release of magnetic energy is observed at the sun but has broad and fundamental relevance to dynamic processes throughout astrophysics. Solar coronal magnetic fields will be observed in the next decade for the first time.