The American Astronomical Society
Opportunities and Challenges

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On the occasion of a 100th anniversary, it is important to take time to look forward as well as backward. We have been the beneficiaries of the astronomical legacy provided to us by the astronomers who established the AAS, developed the journals we rely on, and built the magnificent observational tools that we use. But what will we leave as our legacy? What changes do we see during the next century—or more realistically in the first decade of the next century—in the kinds of questions we ask about the universe? And what changes will be required in the methods we use to answer them?

The Astronomy and Astrophysics Survey Committee's report that recommended priorities for the decade we are just now completing was subtitled, ''The Decade of Discovery.'' The pace of discovery has been even more astounding than the authors of that report could have imagined. Astronomers have now found more planets outside our own solar system than within it; obtained observations of dynamics in the central regions of active galaxies that can best—and perhaps only—be explained by the existence of massive black holes; proved that at least some galaxies evolve over redshifts that are directly accessible to observers; detected fluctuations in the microwave background; and observed galaxies that formed when the universe was little more than a billion years old. New discoveries appear almost weekly, not only in the professional literature but in the popular press.

The goal for the next decade and beyond will be to explore and characterize the physical processes that have shaped the evolution of the strange and wonderful objects we have discovered. It is no longer sufficient to discover planets around other stars. Now we want to know the systematics of those other solar systems. Not one of the systems discovered to date is anything like our own. As our techniques improve, will we find many systems like the one we inhabit, or is this one, with the hospitable environment that it offers for the development of life on one of its planets, very unusual or even unique? And what factors determine the outcome of the planet formation process?

The evidence for large quantities of dark matter in the universe is compelling, but just how much is there and where is it located? Large scale studies of microlensing can provide the answer. We now know that galaxies form and grow at redshifts $1<z<3$, but what causes the changes in structure, star formation rates, etc.? A laboratory science would answer this question through controlled experiments; in astronomy, we cannot set the parameters of nature's experiment, but we can sample the range of environmental conditions through time to determine what factors are important.

What these and many other important open questions in astrophysics have in common is that they can be answered only through large data sets that provide a fair sample of the class of objects under study. New techniques and facilities will be required to obtain those fair samples.

Fortunately, new technologies combine with our growing understanding of astrophysics to make it possible to ask and answer questions that we could not have contemplated even ten years ago. New approaches to multiplexing observations—today's mosaic imagers in both the optical and infrared and fiber spectrographs coupled to the new generation of large telescopes—give only a hint of what will become possible.

The new techniques allow us to make qualitatively new types of observations at all distance scales. For example, for the first time ever, it is now possible to observe the interior of the Sun via helioseismology and link interior changes to external manifestations of activity. Recent discoveries show that it is possible to detect large activity regions while they are still on the far side of the Sun, and thus provide some advance warning concerning times when large flares or coronal mass ejections may occur. With higher throughput on nighttime telescopes, we can begin to put the Sun in context by studying activity levels in other stars of similar age and composition. Evidence is starting to support the idea that the Sun is in an unusually benign state, with a very low level of variability, when compared with other stars. Can the Sun revert to a more normal state? On what time scale? And what would such a change mean for the Earth and its climate?

For the first time ever, it is now possible to design, build, and—most challenging—manage the data flow from a telescope that would scan the whole sky every week or so. A design for a 6.5- to 8-m telescope with a 3-degree field of view has been developed by Roger Angel and collaborators. Such a telescope, operated in a mode where a typical exposure is on the order of a minute or less, could scan the entire visible sky every few nights. Such a telescope would be able to delineate the distribution of mass on scales from 10-6 to 6×109 parsecs. It could, for example, in less than 10 years time detect about 90 percent of all the near-Earth asteroids with diameters greater than 300 m, and provide a sample of objects with diameters as low as 100 m. This size limit is important. If an asteroid with a diameter of only 300 m were to impact the ocean, the resulting tsunamis would devastate coastal regions. This survey telescope could also characterize the objects in the Kuiper belt, and thereby study the fossil record of the formation of the solar system.

The addition of repeated scans of the sky would produce very deep images that could be used to identify high redshift galaxies. At 29th magnitude, there will be on average one distant blue galaxy every few arcsecs. Observations of these faint, distant galaxies can be used to map all of the matter, both bright and dark, in a cone to $z=1$, thereby constraining certain cosmological parameters. Up until now, luminosity has been used as a surrogate for mapping mass on large scales. The combination of weak lensing plus photometric redshifts makes it possible to detect the presence of both bright and dark matter directly and thereby construct a 3-dimensional map of the distribution of total mass as a function of redshift back to a time when the universe was only half its current age.

One product of a very deep wide-area survey would be maps that could be used to constrain models of structure formation in the universe. N-body simulations by Simon White and others show that the standard cold dark matter model cannot simultaneously reproduce the large scale structure that we see today and the fluctuations in the microwave background radiation detected by COBE. There are variants on the standard model that are more successful—models that assume the presence of a relativistic neutrino background with higher density than the standard model or that assume that the cosmological constant makes a significant contribution to the present day energy density. One way to break the degeneracy of these models is to observe the evolution of large scale structure with redshift, a project that will require both imaging and spectroscopy of thousands of objects with a high density of sampling on the plane of the sky.

The whole domain of time variable objects could be explored systematically for the first time with rapid all-sky surveys. We would expect to find, for example, 100–400 supernovae of type Ia per square degree/year. Observations of supernovae with redshifts greater than one are key to distinguishing a non-zero cosmological constant from the effects of reddening and metallicity differences. Supernovae at redshifts as large as 2 would be detectable with the proposed telescope. Other transient events and optical bursts could be detected and studied systematically.

Managing, distributing, and analyzing the petabytes of data that would result from this experiment and the many other datasets that will become available over the next decade present an enormous challenge. The creation of a National Virtual Observatory (NVO) that would meet this challenge has been advocated by Alex Szalay, Tom Prince, and others. The ability to access multi-wavelength databases and submit complex queries concerning image properties all from one's office will be as effective a tool for discovery as a real observatory.

The growing number of databases of ever fainter images, both from the ground and from HST and NGST will create a demand for spectroscopy. In order to understand the evolution of galaxies, we must quantify such factors as morpohology, environment, metallicity, mass, star formation history, and interactions as a function of redshift. Because of the large number of variables, observations of tens of thousands of galaxies will be required. In order to obtain such data in a reasonable amount of time it will be necessary to devise instruments that can obtain spectra of hundreds to thousands of objects simultaneously. Such a project on galaxy evolution is within the reach of a 10-m class telescope optimized for wide-field spectroscopy.

Even though the 8- and 10-m class telescopes are just now coming on line, it is not too early to start the development process that will lead to still larger telescopes. It is already the case that only about 10 percent of the objects detected in the Hubble Deep Field can be observed spectroscopically even with the world's largest telescopes. NGST imaging will reach even deeper. Objects that are detected in x-rays or gamma rays from space may be faint and difficult to study in the optical and infrared regions of the spectrum, but these are the regions richest in spectral diagnostics.

Spectroscopy of the faintest and the most distant objects will require the largest feasible aperture, and at least for the next decade or two such a facility is best built on the ground. Various groups are undertaking technology studies that would lead to the construction of telescopes with apertures ranging from 30 to 100 meters. A 30- to 50-m telescope based on the approach used by the Hobby Eberly telescope could be built with today's technology with confidence concerning performance and cost. If we scale the costs of 8- to 10-m telescopes to 30 m using the normal scaling laws, we find that a fully steerable 30-m telescope would cost 1.5–2 billion dollars. Obviously, we need to find ways to break the cost curve before undertaking construction. The most challenging technical problems will be to develop adaptive optics systems that will produce diffraction-limited imaging and to devise structures that can maintain image quality in the face of wind buffeting and other disturbances.

However, the facilities described above are all feasible and would lead to major breakthroughs in our understanding of the physical processes that transformed the universe from a nearly featureless soup of elementary particles and energy into the fascinating world we observe today. Taken together, the all-sky survey telescope, the wide-field spectroscopic capability, the National Virtual Observatory, and a telescope with an aperture of at least 30 m constitute a coherent plan for groundbased astronomy for the next two decades: first, the survey telescope would provide deep images of the sky; the NVO would make it possible to compare the images obtained through multiple scans of the sky with each other to discover variable objects, and with other space and ground-based datasets to select fair samples of classes of objects; the spectroscopic survey capability would enable follow-up of objects at intermediate brightnesses and at intermediate redshifts; and the very large telescope would make it possible to study the faintest and most distant objects spectroscopically to determine their compositions, stellar content, ages, and other physical properties.

All of these are ambitious projects, and there are equally ambitious plans for the development of new space observatories. What do projects of this scale mean for astrophysics? And, more specifically, what do they mean for the evolution of the AAS during its second hundred years? The only thing we can be sure of is that we don't know the answers to these questions.

The one thing that the AAS must do is look out for the health of the astronomical community as we confront the major changes that will characterize the field through the early part of the new century. One strategy that the AAS can follow is to provide a forum for debating issues and priorities. For example, the flagship facilities that the astrophysics community will propose over the next century will be increasingly costly and will require a major commitment of public funds. Yet astronomy is a phenomenon-rich science. No single facility can answer all of the questions that we wish to ask. Astronomy is not like physics, where once a new energy threshold is crossed, the earlier generation of accelerators is no longer scientifically interesting.

As we go forward with flagship projects of the kind described above, we will want to maintain access to all of the supporting facilities needed to answer broad scientific questions. We need imaging and spectroscopic telescopes and different modes of observing (queue, service, remote, targets of opportunity, as well as conventional observer present runs); diffraction-limited imaging and spectroscopy as well as wide-field survey capabilities; space and groundbased telescopes. Just as we need a diversity of facilities, we need a diversity of scientific approaches and ideas; it is crucial to ensure that we have open, competitive access to all classes of observing facilities. What is the right balance between investment in new facilities and the continued operation of existing facilities? How many telescopes of what types do we need? What is the right pace of development for the field? What is the right balance between public and private investment? All of these issues will be debated in Congress, in the universities, and in the press. The AAS can provide a forum in which to develop community consensus before we engage with these powerful external forces.

Concerning other issues, there are only questions about what the AAS might do. For example, major projects of the kind outlined here will require a very broad mix of skills. Currently, practicing astronomers are for the most part either professors or students with training specifically in the field of astronomy. In the future, we will be increasingly reliant on skilled instrumentalists, engineers of all types, data reduction specialists, and software developers in order to complete these complex projects. We will need to take advantage of technical developments in fields other than astronomy. How does the AAS best interact with and serve this more diverse community?

Many of the science projects that will make use of the new capabilities for all sky surveys, etc. will require large teams of people and many years to complete. Long term funding will be required, a pattern that has not characterized grant funding in the US. What role can the AAS play in advocating changes in funding approaches?

Publishing is being profoundly changed by the revolution in information technology. What then is the future of the AAS journals? How do we balance the speed of publication against the need for quality control? Some other disciplines have proposed that papers be graded according to the quality and rigor of the refereeing process, allowing some papers to appear very quickly but with limited prior review. Is this a good strategy for the AAS publications? And what should be distributed via the AAS? Now the distribution is limited primarily to journal articles. What about data sets? Or community software codes?

What role should the AAS play in education? Education is a major activity of many of the AAS members. What kind of support should the Society provide? Should there be an astronomical equivalent of the American Association of Physics Teachers? Could the AAS help with the assessment of education strategies and innovations and their dissemination?

One thing that has not changed in the last 100 years, and is not likely to change in the next 100, is the fascination that astronomy holds for professional and layperson alike. In the volume describing the First Hundred Years of the AAS, Don Osterbrock quotes from a sermon given by Lloyd Jones on the occasion of the dedication of Yerkes Observatory: ‘‘Astronomical research should inspire us with a new zeal for the quest, for such study releases us from the trammels of matter and carries us into the fellowship of the spirit . . . The shackles of superstition fall off and the soul, unfettered, revels in the boundless universe of truth, beauty, and love.’’