Quantum to Cosmos: Fundamental Physics Research in Space
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John Marburger
Director, Office of Science and Technology Policy
Executive Office of the President

Thanks to the conference organizers for inviting me to speak this morning. One of the things I had to get used to as the President's Science Advisor is that I am never asked to speak at scientific meetings to report on recent important research results, which is probably evidence of good judgment by the program committee. At most what I can do is give some insight into areas of science relevant to government priorities, and lift the edge of the veil over the mysterious process by which those priorities get translated into programs and budgets. But I can't resist beginning with some reflections on the science. This is dangerous because I know less about the possibilities than you do, and it would be wiser for me to sit and listen and learn. Enticing science opportunities are obviously available to us now as a result of the rapid accumulation of new technologies.

Science, after all, progresses where technology and theory intersect. The technology part throughout most of the history of science has served to extend the range of empirical observation. Telescopes, microscopes, spectroscopy, means for achieving and measuring lower temperatures, higher pressures, better space and time resolution, greater energy densities – all these are ways to extend the range of our senses, and advances in each of them have revealed new structures in nature that have required advances in theory – that is, in extensions of our conceptual framework. And those extensions in turn usually produce new options for understanding the universe whose exploration requires yet further advances in technology. This stimulation of technology by discovery is a primary reason for society to support basic research.

During the closing decades of the twentieth century the technology of computing added a new dimension to this picture of discovery and invention. The implications of theory can now often be articulated so reliably and in such detail that the process of comparison with data has been transformed. Constraints on experimental design have relaxed because we can accommodate far more degrees of freedom in the systems under investigation. Laboratory experiments have become more like uncontrolled nature, and the natural phenomena we can analyze are much more complex. Physicists can extract useful data from high energy collisions that spew out enormous numbers of particles. Astronomers can trace events in the earliest stages of the universe from observations of present-day large scale structures encompassing a huge multitude of galaxies. ("Enormous" is calibrated differently for physicists and astronomers, but they are converging.) The extraction of signals deeply embedded in noise, and the management of observational parameters in real time to set "event triggers" or take advantage of serendipitous events is possible to an unprecedented degree. The information we have been able to glean from analyses of subtle properties of the cosmic microwave background radiation is astonishing. Instrumentation with the precision of the LIGO apparatus is almost incredible – 10^{-18} meters over the 4 km interferometer arms – and it is to some extent traceable to powerful information processing. It is no accident that the agencies that support investigations into the most fundamental processes of nature have made significant investments in high-end computing, and these fields have benefited greatly from investments in computing made by other agencies for other missions.

The same information technology has become indispensable for harnessing the knowledge we already have about fundamental laws. Here the issue is not how things move or what they are made of – the issue is how they are made and how structure is related to function in highly complex objects. Astrophysics has its share of complexity, but it is difficult to match the complexity of living systems and their components. So it is amazing that we are able to simulate some important features of organic systems "from scratch" in computer studies. Similar "computer experiments" use our knowledge of fundamental forces at low energy to discover and interpret the behavior of complex molecules and materials that may have technological importance. However we think of strategy in science or science policy, information technology has to be elevated to a strategic level in any discussion of work at the frontier.

As I understand it, this conference is predicated on the idea that "space" – however defined – should be regarded as part of the dynamic technology infrastructure that enables new science, and I think this idea has much merit. The possibility of placing scientific apparatus in free-fall outside Earth's atmosphere has created new opportunities for observational astronomy, high precision measurements, and materials studies. Even before Alan Guth linked particle physics with cosmology in 1979 we knew the Big Bang mechanism turns the entire universe into a high energy physics experiment. Looking out into space is equivalent to observing nature at ever higher energy densities and temperatures. The Big Bang means that telescopes – photon detectors – can perform the same function as the huge detectors at the world's great particle collider-accelerators. The universe itself is surely the grandest technology there is. At some point we are going to have to give up on Earth-based accelerators and turn to that great machinery in the sky to continue our search for the basic stuff of matter.

Meanwhile the saga of the great accelerators continues. The world physics community is grappling with the question of how to fund the next one, currently called the International Linear Collider. This is an important machine, much better suited to unraveling the symmetries likely to be involved in extensions to the Standard Model than the Large Hadron Collider currently under construction at the European accelerator center at CERN. The LHC is needed to give assurance that the current theory is on the right track, and to justify the expense of yet another huge accelerator (the ILC requires two opposing 20km superconducting linear accelerators). A Japanese study concluded the cost of such a machine would be about $5 billion (certainly a low estimate). We should keep in mind that this is the same order of magnitude as the currently estimated cost of the James Webb Telescope. I jokingly referred to the difference in the definition of "enormous" between physicists and astronomers. There is a similar difference in the perceptions of what constitutes a very expensive project. For the cost of one large space project you can build apparatus for particle physics that will occupy several generations of physicists.

I will come back to issues of expense and priorities in a moment, but let me stress here that the convergence of particle physics, astronomy and cosmology is not only important for science, but for science policy and for the organization of science within the federal government. Already the Office of Management and Budget and Congress have mandated a joint advisory committee for NSF, DOE, and NASA – the Astronomy and Astrophysics Advisory Committee (AAAC) – that will be taken seriously by OMB and OSTP, and it will need to be taken seriously by the agencies as well if they expect support for their plans at the White House level. Garth Illingworth is providing outstanding leadership of this committee and I commend its recent Annual Report to this audience.

It is not only in astronomy and particle physics that space science and space-based science are playing important roles. This conference provides an important opportunity to review the entire spectrum of space-based activities that either exploit or enhance our understanding of physical science.

Everyone here is surely aware that President Bush launched two initiatives bearing on physical science in his State of the Union address in January – the American Competitiveness Initiative (ACI), and the Advanced Energy Initiative (AEI). Since then I have been speaking about these in many different forums, and I will devote most of the rest of my time this morning to the ACI. In March I spoke to HEPAP and subsequently to NASA's annual Goddard Memorial Symposium on these initiatives, and addressed particularly the fact (which was brought distinctly to my attention) that high energy and nuclear physics did not seem to be stressed in the ACI, and NASA was not included at all among the ACI's "prioritized agencies" scheduled for significant budget increases during the next ten years.

The ACI appeared following a year of high visibility advocacy from a variety of groups, culminating in a report by a National Academy of Sciences panel chaired by former Lockheed-Martin chairman Norm Augustine. It is not correct to think of ACI as a response to the Augustine report, but the recommendations of the latter do significantly overlap the ACI and the AEI. Many other reports have appeared in recent years that make similar recommendations. They provide a policy context for understanding the significance of the Presidential initiatives. My remarks on the policy context will appear in an article in the June issue of Physics Today based on a speech I gave earlier this month on the 75th Anniversary Symposium of the American Institute of Physics. Most of the rest of my talk this morning will summarize these remarks.

The American Competitiveness Initiative differs from the recommendations of the Augustine report in a number of important respects. Its components include: Expanded federal funding for selected agencies with physical science missions; improved tax incentives for industrial investment in research; improved immigration policies favorable to high tech talent from other countries; and a cluster of education and training initiatives designed to enhance math and science education, particularly at the K-12 level. A brochure is available on the OSTP website that goes into more detail. A total of $910 million is slated for the FY07 budgets of three designated "physical science" agencies. This is a 9.3% increase for the selected agencies, and the plan is to double their collective budgets over 10 years, a cumulative cost of $50 billion. The three agencies are DOE Office of Science, NSF, and what is called the NIST "core budget," which supports research as opposed to technology transfer programs.

As this audience knows, federal physical science funding has been flat in constant dollars for more than a decade. The reasons for this are well understood, but involve multiple factors. Most dramatic was the abrupt change in Department of Defense research starting in 1991, the year historians cite as the end of the Cold War. The Department of Energy too began a re-examination of the roles of its laboratories in the post-Cold War period. Recall that there was a recession during 1990-91, and Congress was looking for a "peace dividend" following the dissolution of the Soviet Union. Congress terminated the SSC project in 1992, and House Science Committee chairman George Brown exhorted scientists to re-think their case for continued funding, especially in physical science. Toward the end of the decade a new case did emerge in a document that ought to be better known. Congressman Vern Ehlers produced a report whose short title is *"Unlocking the Future"* that clearly stated the conclusion that the rationale for funding science was to ensure future economic competitiveness. While not emphasizing physical science, the report did stress that "It is important that the federal government fund basic research in a broad spectrum of scientific disciplines, including the physical, computational, life and social sciences, as well as mathematics and engineering, and resist overemphasis in a particular area or areas relative to others."

At the turn of the twentieth century, science policy makers began to worry about a growing imbalance between support for biomedical versus physical science. Early in the new Bush Administration the President's Council of Advisors on Science and Technology (PCAST) released a report called *"Assessing the U.S. R&D Investment"* that said "All evidence points to a need to improve funding levels for physical sciences and engineering." At the time, the country was still suffering the economic consequences of the burst dotcom bubble, and was realigning budget priorities in response to the terrorist attacks of September 2001. Completing the commitment to double the NIH budget was the highest science priority, next to establishing an entirely new science and technology initiative for homeland security. Nevertheless the Administration continued to expand funding for targeted areas of physical science, including the recently introduced National Nanotechnology Initiative, and maintained funding for the Networking and Information Technology R&D program. The NSF budget continued to increase at a rate above inflation. In the first term of the Bush Administration, combined federal R&D funding soared at a rate unmatched since the early years of the Apollo program, a jump of 45% in constant dollars over four years.

The ACI improves conditions for many if not all areas of physical science, but emphasizes fields likely to produce economically important technologies in the future. These are not difficult to identify, and all developed countries recognize their importance.

Chief among them is the continued exploitation of our recent ability to image, analyze, and manipulate matter at the atomic scale. New technologies can be expected to spring from improved atomic-level understanding of materials and their functional properties in organic as well as inorganic systems. This includes much of what we would call low-energy physics, including atomic, molecular, and optical physics, and large parts of chemistry and biotechnology.

Opportunities exist in particle physics and space science and exploration as well, but these are not emphasized in the Competitiveness Initiative. Not that the U.S. is withdrawing from these fields. Some of the increased budgets in NSF and DOE will increase their vigor. The overall NASA budget is sustained in the President's FY07 budget proposal, although space science is facing flat or diminished budgets for the next few years. In my view the U.S. is devoting a very healthy budget to space science, and with 56 space science missions currently flying it would be hard to argue that our international leadership in this area is in jeopardy. The ACI priorities signal an intention to fund the machinery of science in a way that ensures continued leadership in fields likely to have the greatest impact on future technology and innovation. In particular, although ACI will relieve some budget pressure on DOE high energy and nuclear physics, its priority thrust is toward the cluster of facilities and programs within Basic Energy Sciences (BES). BES is certainly under-funded relative to its importance to society, just as biomedical research was under-funded in the 1980's relative to its rapidly growing significance for health care. In an era of extraordinary demands on the U.S. domestic discretionary budget, course corrections in federal science funding entail the setting of priorities, the rationale for which must recognize national objectives of the utmost importance.

Space science and space exploration remain priorities for the United States, and relative to other investments the federal funds devoted to them are substantial. Among science agencies, only NIH has a larger budget for science. Despite current stresses on the space science budget, I expect it will experience steady but not dramatic long term growth. Conferences like this one are important to raise awareness in the communities of science as well as among policy makers of the fact that space based science is not the same as “space science” in the usual sense, and its needs and opportunities require special attention. In particular, agencies like the Department of Defense, Department of Energy, and the Department of Homeland Security, whose missions depend on frontier technologies, need to be aware of the opportunities that space-based research and its applications hold for solving some of their problems.

From the strictly scientific point of view, the promise of space based experiments is vast and exciting. I am grateful to the organizers of this Workshop for inviting me, and I look forward to hearing and reading more about your ideas.