

# EPP 2010: Elementary Particle Physics in the 21st Century

Fermi Lab  
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# Committee

- Harold Tafler Shapiro, Chair
- Sally Dawson, Vice Chair
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- David J. Gross
- Norbert Holtkamp
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- Sandra M. Faber
- Sidney R. Nagel
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- Nigel Lockyer
- Homer A. Neal
- Stuart J. Freedman
- Takaaki Kajita
- Edward Witten
- Paul Steinhardt
- Norman R. Augustine
- Neal F. Lane

## **The charge to EPP2010:**

At the dawn of the 21<sup>st</sup> century, elementary particle physics is poised to address some of the most basic questions in science. Obtaining the answers to these questions will require a global effort of great scale and complexity. The committee is charged to construct a plan for U.S. participation in this effort. In particular, the committee will

1. Identify, articulate, and prioritize the scientific questions and opportunities that define elementary-particle physics.
2. Recommend a 15-year implementation plan with realistic, ordered priorities to realize these opportunities.

# Meetings

- Meeting 1 Washington
- Meeting 2 SLAC
- Meeting 3 Fermi Lab
- Meeting 4 Cornell (August)

# Budget and Strategic Issues

# What is the Current Budgetary Context for EPP 2010?

1. Current budget outlook for physical sciences is very different from historical experience.
  - Pre-2000 real growth (2.3% annual average over 40 years) has been replaced by post-2000 “Flat-Flat”
  - Is this a temporary excursion or a permanent reality?
2. Large uncertainties further complicate the future outlook.
  - Lack of a clear institutional policy (initiatives more likely to be driven by events, personalities and polls)
  - Lack of funding certainty for multi-year programs
3. The outlook for traditional EPP Partners is also constrained.
  - Large demands on E.U. and Japan funding for current projects
  - The current bidding war over ITER siting could adversely affect international appetite for new mega-projects

# DOE Office of Science Planning Profile



\$127M (~3.7%) decrease over 5 years

# DOE Office of Science FY 2006 Congressional Budget Request

- FY 2006 Request is 3.9% below FY 2005 Appropriation
- No new starts in FY 2006
- FY 2006 House Bill

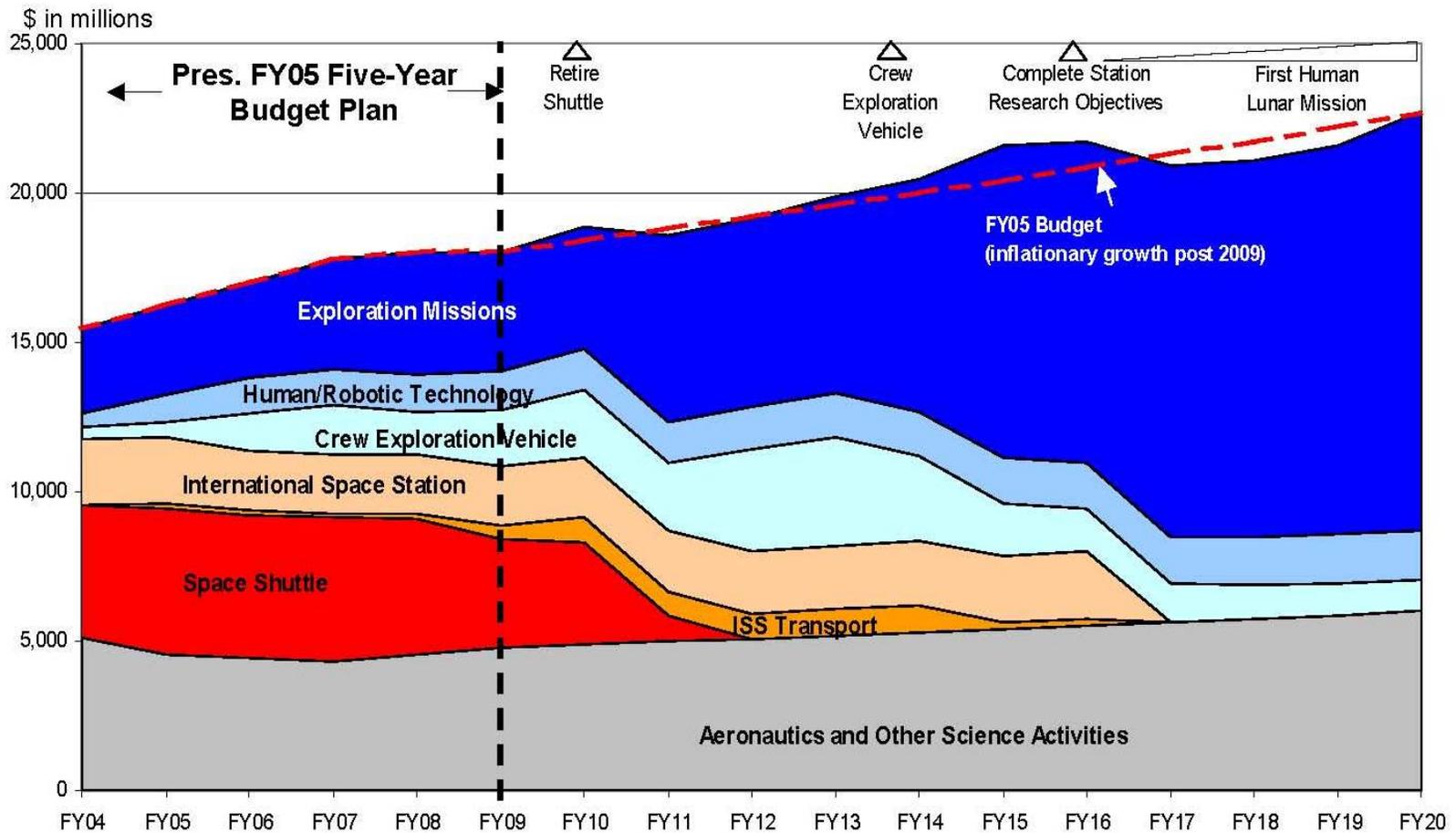
	FY 2004 Comparable Approp.	FY 2005 Comparable Approp.	FY 2006 President's Request	FY 2006 House Appropriation (\$M)
Basic Energy Sciences	991	1,105	1,146	
Advanced Scientific Computing Res.	197	232	207	
Biological & Environmental Research	624	582	456	
High Energy Physics	716	736	714	
Nuclear Physics	380	405	371	
Fusion Energy Sciences	256	274	291	
Other	384	270	279	
Total, Science	3,548	3,605	3,463	

# What are the Implications for the EPP 2010 Plan?

1. “Flat-Flat” budget outlook changes strategic decision making
  - New initiatives have to come from re-direction
  - Pursuit of new world class scientific initiatives requires sacrificing base programs
  - Recent Administration examples: NASA Space Exploration, DOE Magnetic Fusion
  
2. Zero-sum planning entails new risks
  - Proposed re-directions can become reductions
  - “Losers” can generate greater political intensity than “winners”
  - Failure to achieve consensus for change could undermine support for the entire enterprise



# Strategy Based on Long-Term Affordability



NOTE: Exploration missions – Robotic and eventual human missions to Moon, Mars, and beyond  
Human/Robotic Technology – Technologies to enable development of exploration space systems  
Crew Exploration Vehicle – Transportation vehicle for human explorers  
ISS Transport – US and foreign launch systems to support Space Station needs especially after Shuttle retirement

# Questions?

1. Why even have such an NRC study?
2. Can a mixture of experts and non experts come up with meaningful recommendations?
3. Setting scientific priorities is a difficult task. Large projects, small projects, accelerators, neutrinos, dark matter, energy.
4. Can we make the case that a dollar spent on particle physics creates a comparable return to investments elsewhere such as biology, nanoscience, etc. ? Can we turn around the disinvestment that is taking place in Particle Physics?
5. The LHC is the last big investment, the ILC may be the next big big investment, what will be the impact on the field?

## Questions cont.?

6. When can the most persuasive case be made for the ILC. Should we wait for LHC results? If there is a delay in making the decision what should we be doing now?
7. Given the current budget situation, we are going to have to shut things down to get head room for new things. Does the field have the discipline to do this? In my view budget priorities are not in line with scientific priorities. Employment and make work arguments are a real danger in crowding out new ideas.
8. Are international collaborations the only way to make something really large? Why are there really no good examples out there? What set of principles will form the basis for establishing successful collaborations? What does the I in ILC really mean?

## Questions cont.?

9. What is the future of Fermi Lab? Should it be the home of an ILC?  
Is there a compelling scientific program at Fermi without an ILC?
10. What important questions are we not asking?

# Sociological Questions

- How can we capture the imagination of students and attract them to careers in science or engineering?
- How can we convey the excitement of discovery to the public?
- What are the elements of national leadership?
- How can we most effectively organize a collaboration of thousands? How can we capture the creativity of each individual?
- How can we harness the talent in scientifically under-represented parts of the world?
- How can the world carry out large international projects?