Charges to the Snowmass 2001 Working Groups

The Snowmass 2001 Organizing Committee*

1. Accelerator Working Groups

M1: Muon-Based Systems

Intense muon sources have been discussed as a starting point for very-high-energy colliders and even more in recent years as a source of very intense and well-collimated neutrino beams. This working group should identify, but clearly distinguish, the main accelerator physics aspects of both the Muon Collider and the Neutrino Source. Even more, it is crucial to understand for the high energy physics community, how much a Neutrino Source represents a first step to a muon collider and what are the additional burdens. Given the variety of technologies that require R&D makes it necessary to have the group present a risk assessment of the various subcomponents, their R&D goals and the time scale on which the R&D could be realized. The more recent refocus of the collaboration towards Neutrino Sources should reflect in the main topics of the discussion. The different approaches: CERN, KEK-JAERI, and the Muon Collaboration (including the Fermilab and Brookhaven locations) should be compared in performance, risk, and (if possible) schedule. A discussion on whether a Muon Cooling experiment is necessary and/or viable is absolutely required and should be presented by the group. For the Muon Colliders, the technical performance, especially for a low-energy (Higgs collider) machine should be addressed. Technical performance (power consumption, risk assessment, luminosity, etc.) should be compared to linear colliders in the same energy range. Input here will be required from the high-energy physicists to define the measure of performance for these two concepts (MC, LC). For the long-term R&D the advantages compared to electron-positron accelerators should be worked out and quantified as much as possible.

M2: Electron-Positron Circular Colliders

Perform a survey of the present status as well as the vision of the future promises of the various electron-positron circular colliders. The colliders to be covered include those currently in operation, currently under construction, or envisioned as a possibility of the future, and in the US and abroad. Special emphasis should be placed on the clear identification of the beam physics limits and accelerator technology limits, and an examination of the extent that they have been addressed by past research or need to be addressed by futher research. Identify new and promising ideas even though they may need additional work. These issues should be addressed for all of the leading technical realizations of the circular electron-positron colliders. Finally, the group should summarize in a brief report (a few pages) the highest priority research topics for different techological realizations of circular electron-positron systems and give an approximate timetable for key R&D development. The group is also asked to provide comprehensive presentations to high-energy and accelerator physicists in plenary sessions during the Snowmass workshop.

M3: Linear Colliders

The linear collider group should give a vision of the potential of linear colliders both in the near and far term. Special emphasis should be placed on the clear identification of the beam physics limits and accelerator technology limits and an examination of the extent that they have been addressed by past

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research or need to be addressed by future research. These issues should be addressed for all the leading possible technical realizations of a Linear Collider. Finally, the Linear Collider group should summarize the highest priority research topics for different technological realizations of both the near term proposals and longer term concepts and give a rough time scale for key calculations, experiments or technology developments. In particular, we would like the linear collider group to pay special attention to the NLC/JLC design and the TESLA design for possible near term projects. For the longer term, the group should examine the upgradability of each design with extensions of the proposed technology. The group should also examine two-beam ideas as either an upgrade option or a stand-alone technology for higher energy linear colliders.

M4: Hadron Colliders

A long-term goal of the US high energy physics program is to regain the energy frontier after the start of LHC operation. A very high energy, high luminosity hadron collider is the only sure way to accomplish this goal. The working group on hadron colliders should develop a vision and a long-term plan for the US hadron collider program. In particular, it should examine the physics and technology issues central to the design of very high energy, high luminosity hadron colliders and specify the most critical accelerator physics and engineering issues that determine the performance of the machine; identify the technology developments and accelerator physics experiments needed to prove the machine feasible, and evaluate and estimate the technological and physics limitations on ultimate energy and luminosity in hadron colliders.

The results of the recently completed VLHC study of a staged collider in a large-circumference tunnel should be evaluated and compared with other potential approaches to building and operating very large hadron colliders.

Finally, an R&D plan that will accomplish the goals set out above should be developed. This plan should prioritize the areas of technology R&D that will provide maximal benefit to a future VLHC in terms of performance and cost-effectiveness, and should include an estimated cost and schedule for the R&D.

M5: Lepton-Hadron Colliders

Perform a survey of the present status as well as the vision of the future promise of the various lepton-hadron colliders. The colliders to be covered include those currently in operation, currently under construction, or envisioned as a possibility for the future, and in the US and abroad. Special emphasis should be placed on the clear identification of the beam physics limits and accelerator technology limits and an examination of the extent that they have been addressed by past research or need to be addressed by further research. Identify new and promising ideas even though they may need additional work. These issues should be addressed for all of the leading technical realizations of the lepton-hadron colliders. Finally, the group should summarize in a brief report (a few pages) the highest priority research topics for different technological realizations of lepton-hadron systems and provide an approximate schedule for key R&D developments. The group is also asked to provide comprehensive presentations to high-energy and accelerator physicists in plenary sessions during the Snowmass workshop.

M6: High-Intensity Proton Sources

Several present and future high energy physics facilities are based on high intensity secondary particle beams produced by high intensity proton beams. The group is to perform a survey of the beam parameters of existing and planned multi-GeV high-intensity proton sources and compare with the requirements of high energy physics users of secondary beams. The group should then identify areas of accelerator R&D needed to achieve the required performance. This should include simulations, engineering and possibly beam experiments. the level of effort and time scale should also be considered.

2. Accelerator Physics / Technology Working Groups

T1: Interaction Regions

Perform a survey of the interaction region designs of recently completed colliders and those of proposed colliders both under construction and in future planning. The interaction region issues for both the accelerator and the interface between the detector and accelerator should be covered. Special emphasis should be placed on identifying the needed beam physics, technology limits, and detector requirements and reviewing the extent that they have been addressed in past research. Identify new and promising ideas even if they are in early stages. The group should summarize in a brief report the highest priority research topics and give an approximate time scale for key R&D developments.

T2: Magnet Technology

(i) Superconducting magnets and associated cryogenic and vacuum systems. Review the forefront technological issues in the development of superconducting magnets, together with their associated cryogenic and vacuum systems, for the next generation of high-energy particle accelerators. Examine in detail the most important and challenging aspects of these technologies, both from the point of view of performance and cost-effectiveness. These aspects should include the development and use of superconducting materials (including high temperature superconductors), magnet design for high field quality, magnet fabrication, cryogenic systems and their integration with the magnets, and cold beam vacuum issues. Identify practical and “fundamental” limitations on magnet performance and cost. Prioritize the R&D efforts, in terms of the potential to provide maximal performance and/or cost-effectiveness; determine the major cost drivers for the magnet, cryogenic, and vacuum systems; and establish a technology-limited time line, and the resource requirements, for the R&D efforts.

(ii) Permanent magnets. Review the leading issues in the development of high-performance, low cost permanent magnet systems for the next generation of high-energy particle accelerators. Both high performance magnets for specialized applications and lower cost technologies for large scale applications should be addressed. Specify the principal R&D activities required to address the most challenging issues, prioritize these activities, and establish a technology-limited time line for accomplishing the R&D.

(iii) Magnet power supplies. Examine the principal technical challenges that must be met for magnet power supply systems needed for the next generation of high-energy particle accelerators. Define the principal R&D activities required to meet these challenges, and specify a technology-limited time line for accomplishing the R&D.

T3: RF Technology

Any of the next generation accelerators will need high power rf sources and rf accelerating systems that transfer ac power to beam power efficiently. The challenges though span a wide range of technologies and rf wavelength. From very low frequency cavities used in Muon Colliders (70 MHz) to very high frequency cavities in multi-TeV linear colliders (30 GHz and more), many of the designs are based on experience and where experience is missing, scaling laws are used. How does Breakdown scale with electric field stength, pulse length and frequency? What limits peak power and efficiency in modern power sources?

The experts in this field should generally try to answer these questions and therefore give guidance to the accelerator designers. Limits on fields, peak powers and efficiencies should therefore be an outcome of the working group. Given the experience in the ongoing R&D programs for normal and superconducting cavities the performance achieved today should be described, as well as the limitations and possible cures. The time scale for establishing these cures should be summarized as well. For both the normal conducting and the superconducting case, the subsystems (Modulators, Klystrons, Pulse Compression systems) and cavities should be addressed independently with a description of present status and of the progress being made over the last five years to allow some extrapolation. For the power source itself, a very active field only partially driven by accelerator builders, future trends and new directions of improvements should be described.

This group should also describe the likely spinoffs of these different technologies into other (and which) fields, coming out of the techncial developments being done in the HEP research environment.

T4: Particle Sources

(i) Positron and antiproton sources. High-performance positron sources will be required for the next generation of linear colliders. Antiproton sources are a source of antimatter for proton-antiproton colliders and can provide copious numbers of low energy antiprotons for fundamental research. Review the forefront technological issues in the development of the next generation of positron and antiproton sources. Examine in detail the most important and challenging aspects of these technologies, both from the point of view of performance and cost effectiveness. What are the new ideas and avenues for sources? Prioritize the R&D efforts, in terms of the potential to provide maximal performance and/or cost effectiveness; establish a technology-limited time line, and the resource requirements, for the R&D efforts.

(ii) Secondary beams. Although collider experiments dominate the current high-energy physics landscape, high-intensity secondary beams of particles still form the basic tools for some important experiments. Review the leading issues and limiting technologies for the development of high-performance secondary beams potentially available from the next generation of high-energy particle accelerators. Identify the secondary beams of interest to the community. Identify the most important R&D efforts that could lead to significant advances in the performance of such secondary beams.

T5: Beam Dynamics

Perform a survey of our present understanding of the beam dynamics problems facing the high energy accelerators and colliders, linear or circular, which are currently in operation, currently under construction, or envisioned as a possiblility of the future. The specific beam dynamics areas to be covered are:

- Collective effects
- Beam lifetime
- Nonlinear effects
- Beam-Beam interaction
- Beam polarization
- Beam cooling

It is the job of the group to identify the key beam dynamics issues of each of the areas above. Be specific in pointing out which types of accelerators or colliders each identified beam dynamics issue will impact, and give an evaluation of the magnitude of the impact. Identify the R&D activities in theoretical, experimental, as well as by numerical simulation, to be carried out to resolve or at least to improve the understanding of these effects. An estimate of the required effort level and/or time scale would be very useful. A brief summary report (a few pages) is expected at the end of the Snowmass workshop of the conclusions by the group.

To carry out the work in a timely fashion, it will be necessary to start the organization work prior to Snowmass. It may be more efficient to form subgroups, each for one of the subtopics listed above. In that case, each subgroup would have its own set of coordinators during the three-week period.

T6: Environmental Control

For the next generation of large accelerators, the civil engineering of accelerator tunnels and associated underground enclosures will be a major component of the technical challenge of building such machines. Because of the large scale involved, the engineering will be required to be as cost effective as possible, and issues such as ground motion and artificial sources of vibration in the environment will need to be carefully considered. Installation and alignment of the machine components will be tasks of unprecedented scope, and will require unprecedented precision. Examine in detail the most important and most difficult aspects of these challenges, both from the point of view of performance and cost effectiveness. In particular, identify what the site requirements are for the different machines under discussion (NLC, TESLA, VLHC, Muon source), and describe how tunneling methods are affected by them. Identify, for the different types of accelerators, the different length scales that are involved in defining the alignment tolerances, and what are the tolerances over that length scale. Specify the R&D efforts needed to define the scope of the most critical challenges, and prioritize the efforts, in terms of the potential to provide maximal performance and/or cost effectiveness. Establish a technology-limited time line, and the resource requirements, for the most important of these efforts.

T7: High-Performance Computing

Computers have played a larger and larger role in the theory, design and development of accelerators and the associated technologies. Some examples are calculations of beam optics, simulation of instabilities, electromagnetic field calculations, simulation of space-charge dominated beams and halo formation, beam-beam simulations, start-to-end simulations of systems, real-time modeling of accelerators, and simulations of new accelerator ideas such as those involving lasers and plasmas. This group should explore the impact that advanced computational techniques using the most powerful computers would have on research and development in particle beams and accelerator technology. The group should document past success and look at the immediate and long-term future of high-performance computing as applied to particle beams and accelerator technology. In particular the group should outline a program of proposed research which will bring the world's most powerful computers, and the hardware and software technologies associated with them, to bear on the most challenging and important problems in our field.

T8: Advanced Acceleration Techniques

This group is formed to explore new beam physics and new accelerator technology that are at the forefront of advanced accelerator research and identify those concepts which might open new opportunities for advancement of the energy and luminosity frontier for high-energy physics. The group should explore laser-plasma devices, beam-plasma devices, high-frequency RF techniques, laser-driven accelerators, laser-driven particle sources and any other new ideas appropriate to the charge. Finally, the group should identify general research directions that might be especially promising for high-energy physics applications and explore the research necessary to fulfill this promise.

T9: Diagnostics

Perform a survey of diagnostic systems for high-energy particle accelerators and test accelerators for future machines. This group should discuss with other groups to find new and needed diagnostic systems for future accelerators. Special emphasis should be placed on identifying the needed beam physics and technology limits and reviewing the extent that they have been addressed in past research. Identify new and promising ideas even if they are in early stages. The group should summarize in a brief report the highest priority research topics and give an approximate time scale for key R&D developments.

3. Experimental Approaches Working Groups

E1: Neutrino Factories and Muon Colliders

If muon colliders can be realized, they offer a path to high-energy lepton collisions for the study of electroweak symmetry breaking and new phenomena. The large Yukawa coupling of the muon to Higgs bosons and similar objects, coupled with the naturally small beam-momentum spread, raises intriguing possibilities for formation experiments. The idea that a millimole of muons might be collected over the course of a year opens new opportunities for neutrino factories based on muon storage rings.

Consider the physics potential of accelerator complexes that produce very intense muon and neutrino beams. Any such facility will require a very-high-intensity proton source, and could also include conventional neutrino beams, a muon-storage-ring neutrino source or a muon collider. Many of the scientific goals to be explored here overlap with the interests of P2: Flavor Physics, P5: QCD, E3: Linear Colliders, E4: Hadron Colliders, E5: Fixed-Target Experiments, and the underground laboratory aspects of E6: Astro/Cosmo/Particle Experiments. This group will work in close collaboration with working groups M1: Muon Storage Rings and Colliders, and M6: Intense Proton Sources.

A general goal is to describe a roadmap for the development and productive exploitation of intense muon and neutrino sources over the next 20-30 years. Are there synergies between underground neutrino detectors, high-intensity neutrino sources based on pion "superbeams," muon storage rings, and muon colliders? Does a staged construction program for a multi-TeV muon collider promise a strong physics program at each stage? What is the scientific imperative for a lepton collider at tens or hundreds of TeV per beam?

For the different aspects of the physics program:

1. Neutrino oscillations.
    (a) How rich is the program of neutrino oscillation measurements that a neutrino factory would make possible? Will the questions still be interesting when a neutrino factory operates?
    (b) What is the optimal machine energy, baseline and flux for measurements of neutrino mixing angles, matter effects, and CP violation? Is more than one baseline necessary?
    (c) What results from ongoing experiments might materially change the targets for oscillation studies at a neutrino factory?
    (d) What is the role of very-low-energy experiments that might be done using a neutrino beam created at the Spallation Neutron Source?
    (e) How many of the pressing questions could be answered using super-intense muon-neutrino and muon-antineutrino beams generated by pion beams?
    (f) Are mixed beams of muon neutrinos and electron antineutrinos (or vice versa) a benefit or a curse? How important is muon polarization?
    (g) Can one envisage a practical, large-volume detector that will identify electrons, muons, and taus, and measure their charges? If such a detector could be constructed, what advantages would it bring to experiments?

2. Nonoscillation neutrino physics. Consider the scientific potential of neutrino beams from muon storage rings and pion superbeams.
    (a) What is the potential for conventional neutrino measurements at short baselines at a low-energy neutrino factory or at a high-energy muon storage ring? Specifically, what are the prospects for (i) extracting parton distributions from hydrogen targets? (ii) measuring neutrino cross sections? (iii) determining the weak mixing parameter and the strong coupling constant?
    (b) Can a neutrino program based on intense pion beams share detector facilities with a neutrino factory?
    (c) What novel experiments could be carried out using polarized targets or silicon targets? What is their scientific importance? Are there other ways to obtain equivalent information?

3. Intense muon sources. Consider the scientific program that could be developed using intense sources of low-energy muons. How do the requirements on beam properties differ from those appropriate to neutrino factories or muon colliders?
    (a) How can intense muon sources advance the study of lepton flavor violation? What are the relative capabilities of muon decay or muon conversion experiments and the study of rare kaon decays?
    (b) How could the availability of intense muon sources improve the precision of measurements of fundamental static properties of the muon, including the anomalous magnetic moment and a permanent electric dipole moment?
    (c) Are there important applications of copious supplies of low-energy muons beyond particle physics?

4. Muon colliders. Consider the physics potential of muon colliders as Higgs factories, and at center of mass energies of 500 GeV, 1 TeV, and several TeV.
    (a) For a Higgs factory, what is the program of measurements a muon collider could accomplish? What luminosity and energy spread are required for incisive studies of the Higgs boson's properties. Other than precise determinations of mass and width, what measurements would establish the nature of the Higgs boson?
    (b) For a modest-energy muon collider, can the luminosity be competitive with a linear collider? How great a disadvantage is the less-flexible polarization of the muon beams?
    (c) Define a program of experimentation for a 4-TeV muon collider. What are the principal scientific goals? What luminosity is required to carry them out?
    (d) Consider the novel experimental environment at a muon collider: evaluate the effects of the different background environment at a muon collider relative to electron and hadron colliders.

   (e) For all energies, what compromises must be made for a detector to operate gracefully in the environment? Will detectors at a muon collider need new or different technologies than those at electron or proton machines?
   (f) What are the eventual limitations on beam energy and luminosity?

5. How can full international collaboration on a muon storage ring or muon collider project be realized? Is it feasible to assign full responsibility for design, construction, commissioning, test and operation of major subsystems to different portions of the world community while maintaining effective overall project management?

E2: Electron-positron Colliders below the Z

Asymmetric electron-positron colliders have become highly productive b factories, and it is natural to consider luminosity improvement programs or new machines with greatly enhanced luminosity. Phi factories offer special conditions for the study of kaon decays, quantum entanglements, and other issues. Tau / charm factories hold promise for detailed measurements at unprecedented sensitivities. The task of this working group is to consider the future of low-energy electron-positron colliders and to evaluate developments needed to make incisive instruments practical.

Electron-positron colliders at energies below the Z mass should be evaluated in terms of the physics potential that they offer and the characteristics required to realize that potential.

1. This group should coordinate with the physics issues working groups to help compile the scientific cases for electron-positron colliders as b factories (including "giga-Z" machines as b factories), charm factories, phi factories, and tau factories. What are the outstanding questions that heavy-flavor factories could address? For each physics topic, consider the contributions from existing and potential experiments using other instruments, and give a critical assessment of the competitive advantages and disadvantages of electron-positron colliders. Working group P2: Flavor Physics is a natural forum for laying out the comparisons.
2. What is a reasonable goal for the desired luminosity at the various cm energies? For each kind of instrument, outline a comprehensive experimental program and estimate the integrated luminosity required to carry it out.
3. Quantify the physics gained at each machine as a function of the beam polarization.
4. Are there additional energy scales that could yield important results?
5. What special detector capabilities are required to achieve the scientific goals of each of the machines? Do any detector R&D issues arise?

This group should interact regularly with working group M2: Electron-Positron Circular Colliders, to exchange information on the desirable machine properties and help define accelerator R&D issues for each type of machine.

E3: Linear Colliders

Electron-positron linear colliders (and options using $\gamma\gamma$, $e\gamma$, e^-e^-) should be evaluated in terms of the physics potential that they offer, and in terms of the accelerator issues that will guide their evolution. This group should work in close collaboration with its counterpart, M3: Linear Colliders, and should examine the impact of a very large circular electron-positron collider discussed in M2: Electron-Positron Circular Colliders.

1. Coordinate with the physics groups to help compile and critically examine the case for an initial phase of the e^+e^- collider at a cm energy of up to about 500 GeV, depending on the results from prior experiments at the Tevatron and LHC. For some representative physics scenarios, what is a reasonable goal for integrated luminosity at various cm energies, beam polarizations and beam particles? is there a compelling initial physics program at a luminosity of a few $\times 10^{33}$ cm^{-2} s^{-1}? Are there particular advantages or challenges to experimentation raised by the different running conditions in the TESLA and NLC/JLC designs?

2. Review the case for and feasibility of special options for LC operations:
   (a) Catalogue the physics needs that may require positron polarization, $\gamma\gamma$ collisions or e^-e^- collisions. Compare the capabilities of an electron-positron collider and a $\gamma\gamma$ collider for making detailed measurements of the properties of Higgs bosons, and for discovering Higgs bosons. What are the R&D issues remaining for each option? What are the requirements on the initial design to allow any of these to be added after the initial phase?
   (b) Examine the case for high-luminosity operation at the Z pole. What are the benefits and drawbacks from the design of a special beam delivery system for low-energy collisions? Should there be a special detector devoted to operating below 500 GeV?
3. What special requirements are imposed if a free electron laser program is added to the high-energy physics facility? What should the HEP community do to facilitate the potential for a FEL program?
4. Evaluate the scientific case for an initial-phase “Higgs factory” at an energy of about 300 GeV.
5. What new physics landmarks come into view as the energy of a linear collider is raised to 1 TeV; to 1.5 TeV; to 2 TeV; to 5 TeV? What luminosity and other performance characteristics would be required to maximize the scientific output?
6. Are there particular issues that detector R&D must address to guarantee the productivity of a linear collider?
7. What are the beam physics limits and accelerator limits imposed on LC performance, and what are the primary outstanding R&D issues that are critical to study in the next several years?
8. The eeLC group should assume that a technical review panel will likely be established within the next year to evaluate the superconducting L-band and warm rf X- or C-band accelerator proposals. That review, conducted under the auspices of some worldwide body, would examine the performance parameters of the machines, the technical risks, needed R&D, comparative costs and upgradability. Without undertaking the work that such a panel would do, the eeLC group should work to sharpen the questions that this review panel should examine, and consider the way in which the panel should operate.
9. What are the paths for upgrade of an initial LC, both in energy and in luminosity? What extensions in energy using the original TESLA or X-band LC designs are feasible? What R&D issues should be given priority? What is the possibility of upgrading either TESLA or X-band LC using two-beam drive power sources? What are the critical R&D issues? What constraints on the initial phase would ultimate conversion to two-beam drive impose?
10. How can full international collaboration on a LC project be realized? Is it feasible to assign full responsibility for design, construction, commissioning, test and operation of major subsystems to different portions of the world community while maintaining effective overall project management?

E4: Hadron and Lepton-Hadron Colliders

With the Tevatron and the LHC we anticipate an exciting decade of discovery physics with hadron colliders ($\bar{p}p$ and pp) exploring the energy frontier. The task of this working group is to look beyond these immediate prospects to develop a clear vision of future physics at hadron and lepton-hadron colliders. This group should work in close collaboration with its counterparts, M4: Hadron Colliders, and M5: Lepton-Hadron Colliders.

1. What is the physics potential for a pp or $\bar{p}p$ collider operating at center-of-mass energies of 100 – 200 TeV? What about 30 – 40 TeV? Elaborate on possible physics discoveries in this decade which would point the way to specific physics opportunities at a future Very Large Hadron Collider (VLHC). This task should be closely coordinated with working groups P3: Scales Beyond 1 TeV and P1: Electroweak Symmetry Breaking.
2. Examine the importance of luminosity for a VLHC, both for more generic physics measurements, and for specific physics opportunities. Compare pp and $\bar{p}p$ physics potential taking into account the likely differential in luminosity.

3. Identify the main challenges of building and operating detectors for a VLHC. Identify particular issues that detector R&D must address to guarantee the productivity of a VLHC.
4. Examine the physics potential of possible energy and luminosity upgrades to the LHC (Super LHC). This should include both discovery potential and the role of higher energy in a mature program of detailed measurements of physics at the TeV scale.
5. Explore the ultimate reach of the mature Tevatron and LHC colliders for B physics. Will we need upgrades or extensions to the BTeV and LHCb experiments in order to maximize their ability to make essential measurements?
6. What is the physics potential for THERA, eRHIC, and other possible next generation lepton-hadron colliders? What is the physics driving a 1 – 1.5 TeV center-of-mass ep collider? What is the physics driving a lower energy e-nucleus collider, or an ep collider with polarization in both beams?
7. Examine the importance of flexibility in the design and staging of future hadron collider projects, including a moderate-energy electron-positron collider in the big ring. Identify the critical R&D issues whose vigorous pursuit will optimize our ability to respond to a variety of possible new physics discoveries. Are there particular issues that detector R&D must address to guarantee the productivity of a very large hadron collider?
8. How can full international collaboration on a hadron collider project be realized? Is it feasible to assign full responsibility for design, construction, commissioning, test and operation of major subsystems to different portions of the world community while maintaining effective overall project management?

E5: Fixed-Target Experiments

Fixed-target experiments offer a great diversity of beams for use in experiments of high sensitivity over a wide range of topics in particle physics. Decay, formation, and scattering experiments (including scattering on polarized or nuclear targets) all hold important potential. The study of subtle effects and rare processes offers a virtual window on very high energy scales. New kinds of experiments may reveal the structure of hadrons at an unprecedented level of detail. The task of this working group is to consider the future of fixed-target experimentation (other than experimentation using muon and neutrino beams, which are the province of working group E1: Neutrino Factories and Muon Colliders). The group should work closely with working groups M6: Intense Proton Sources, and M3: Linear Colliders, and with P2: Flavor Physics, and P4: QCD and Strong Interactions, as well as P1: Electroweak Symmetry Breaking and P3: Scales beyond 1 TeV.

Consider the scientific opportunities for fixed-target studies in light of the capabilities of existing and planned accelerator complexes. The group should also call attention to the case for beams of novel character that might require research and development to be realized.

1. Provide an inventory of existing and planned accelerators and the fixed-target beams they supply.
2. What are the scientific imperatives for the study of rare kaon decays, and what requirements do they place on detectors and accelerators?
3. What are the needs for the study of charmed particles? Give a critical assessment of the comparative advantages and disadvantages of fixed-target experiments. Working group P2: Flavor Physics is a natural forum for laying out the comparisons with electron-positron colliders discussed in working group E2: Electron-positron Colliders below the Z.
4. What are the most important issues in hadron spectroscopy, and how can they best be addressed?
5. What questions drive new experiments using pion, kaon, and proton beams? What beam characteristics do the physics issues demand?
6. What questions drive new experiments using hyperon beams? What beam characteristics do the physics issues demand?
7. What questions drive new experiments using electron beams and photon beams such as derived from backscattering lasers from electron beams? What beam characteristics do the physics issues demand?

8. What opportunities arise from copious sources of antiprotons? What characteristics of the antiproton beams are required for the key applications? What would be the goals and attributes of an “antimatter factory?”
9. What are the prospects for new studies of neutron properties, including the persistent electric dipole moment, using accelerator sources?
10. Consider the case of fixed-target beams of higher energy than will be available over the next decade. What are the most compelling scientific goals, and what would it take to address them?
11. What would be the utility of fixed-target beams derived from a linear collider? What beam characteristics do the physics opportunities demand?
12. In cooperation with working groups E1 – E4, survey the needs for test beams over the next decade and beyond and compare those needs with the available inventory.

E6: Astro/Cosmo/Particle Experiments

Experimental research in Astro/Cosmo/Particle Physics is being carried out by detectors underground, undersea, in ice, on the surface of the Earth, and in the sky. In the last decade, remarkable discoveries have been made by instruments of each flavor, including

▷ the evidence for neutrino oscillations from underground atmospheric and solar neutrino experiments,

▷ the detection of cosmic-ray particles above the GZK cutoff by ground-based air shower experiments,

▷ the evidence for the acceleration of the expansion of the universe from supernovae observations made by ground-based telescopes,

▷ the spatial and temporal mapping of the distribution of gamma-ray bursts by satellite instruments, and

▷ the detection of anisotropy in the cosmic microwave background radiation from satellite, balloon, and ground-based detectors

Such results have galvanized interest in astro/cosmo/particle physics, and there has been an explosion of experimental activity. New instruments are now coming on line (including SNO, Fly’s Eye, AMS, Chandra, XMM, LIGO, etc.) or are currently under construction (including CDMS-II, Axion, Auger, ICECUBE, ANTARES, NESTOR, INTEGRAL, VERITAS, HESS, MAP, PLANCK, GLAST, LIGO-II, etc.). Even larger and more ambitious experiments are being seriously considered (including UNO, SNAP, DEEP, OWL, LISA, etc.).

The next decade and beyond promise to be very exciting for experimentalists working in these areas. Not only can we expect new and exciting results from experiments that are an order of magnitude (or several orders of magnitude) more sensitive than earlier ones, but the use of new technology and instrumentation in all areas will lead to important breakthroughs in capability. Experiments in astro/cosmo/particle physics have a strong intellectual and technological overlap with those in accelerator-based particle physics and astronomy, and thus it will be important to carry out cohesive development of instrumentation across the various fields. Experimentalists in this area have a broad base of backgrounds including particle physics, nuclear physics, cosmology/astrophysics, and astronomy.

This working group will be most closely connected with working groups P2: Flavor Physics and P4: Astro/Cosmo/Particle Physics. In the area of neutrino oscillations, the group should coordinate its work with working group E1: Neutrino Factories and Muon Colliders. The main goals for this working group are to sketch, in broad terms, the experimental program in astro/cosmo/particle physics, to examine the rationale for future large-scale experiments, and to understand how experimentation in this field will benefit from developments in other fields, and vice-versa. We also need to identify the unique opportunities for experimentation required to move beyond the current generation of detectors.

Along with the new detectors come challenges that also need to be considered by this working group. Among other things, we should consider the issues of coordination between experiments, the need for permanent infrastructure in the field, the mechanisms of project review and funding, and the way in which collected data will be made available to the community at large.

1. Review the current status of experimentation in the following areas:
    (a) nonaccelerator particle physics carried out underground, including solar neutrino, atmospheric neutrino, proton decay, double beta decay, and supernovae neutrino experiments,

  (b) high-energy particle (gamma-ray, cosmic rays, and neutrino) detectors,
  (c) dark energy and dark matter experiments,
  (d) cosmology experiments, and
  (e) detectors of gravitational waves.

The emphasis here should be on existing experiments, or those already under construction.

2. In the various areas of experimentation, examine the generic detector capabilities needed to carry out the desired scientific program during the next decade and beyond. For example, among other things, how will we be able to:

  (a) detect the polarization of the CMBR?
  (b) carry out photometric and spectroscopic measurements of many supernovae out to redshifts of 1.5?
  (c) detect pp solar neutrinos in real time?
  (d) detect WIMPs or axions, or rule them out as interesting dark matter candidates?
  (e) make very sensitive wide-field observations of the high energy gamma-ray and neutrino skies?
  (f) search for proton decay at sensitivities exceeding 10^{35} years?
  (g) search for cosmic ray particles with energies exceeding 10^{21} eV?

   The future scientific program will reflect strong input from working groups P2 and P4. In conjunction with these generic capabilities, determine what new advances in technology/ instrumentation will be critically needed, and outline the prospects for developing these technologies.

3. Identify the major new facilities that are being considered. Enumerate their quoted performance (sensitivity, resolutions, etc.) and examine their technical feasibility. What new technologies will be required?

4. An important infrastructure issue concerns a national underground laboratory. Taking as a starting point the Underground Science (Bahcall) report, consider the scientific programs that motivate an underground laboratory and enumerate the desirable characteristics of an underground laboratory. Are the requirements of different experiments compatible with a single site? Taking into account underground laboratory space around the world, consider whether the U.S. needs such a facility. What advantages and challenge would the creation of a new national underground laboratory entail?

5. The development of experiments in this area has proceeded in a somewhat random fashion. Instrumentation development has not been supported vigorously enough, there has been insufficient communication between the experimental groups, and duplication of experiments with similar scientific goals. How can some of these issues be better handled in the next decade? What mechanisms can be used to coordinate worldwide experimental effort in this field?

6. Experiments in astro/cosmo/particle physics are being built by people with a variety of backgrounds (nuclear physics, particle physics, astrophysics/astronomy, etc.). Funding comes from a variety of agencies (including NSF, DOE, NASA, Smithsonian, and private sources). Often, cultural differences lead to problems that should be avoidable in obtaining funding or developing efficiently organized collaborations. For example, the astronomical and particle physics communities have different outlooks on the public availability of gathered data. Another example is the project review and funding procedures. Different agencies have different procedures, and this often leads to a lengthy funding process. Examine some of these cultural differences, seeking community and agency input. What mechanisms are working and what are not? How can the community and the agencies work together more effectively?

E7: Particle Physics and Technology

Particle physics has often been the driver of progress in technologies that are the key to advances in other scientific fields, in industry and eventually commerce. Examples of past decades range from cryogenic vacuum systems and superconducting wire and magnet technology to the invention of the World Wide Web. At other times, although not directly the generator of new technologies, our field has sparked progress by pushing new technologies to meet the needs of our next-generation experiments or numerically intensive theoretical investigations. Recent examples include high-precision radiation-tolerant particle detectors like silicon pixels that are now finding applications in the field of medical imaging as fast, low-exposure alternatives to x-ray films; compact high-speed electronics capable of acquiring and processing vast floods of data; high-gradient linear accelerators for electron-positron colliders that may form the basis for the future development of x-ray free electron lasers of super-high instantaneous brilliance; petabyte-scale analysis challenges of current and next generation collider experiments and the plans to meet these needs through the development of "Data Grids." Astrophysics has joined particle physics in this role through new programs such as large-scale sky surveys, precise measurements of cosmological parameters, and simulation of astrophysical processes. The scale, complexity, and duration of ongoing and future programs have forced new approaches to the development of software by large and distributed collaborations, and have benefitted from the application of new statistical and algorithmic approaches from applied mathematics. These changes have resulted in the adoption of new programming models and tools and have led to a major role by computing professionals (software engineers) in experiments and advanced computation.

This working group should review leading-edge technologies recently developed (or in need of development) for new experiments and theoretical or computational investigations. Developments related to particle detectors, accelerators, online data acquisition systems, offline data analysis systems and networked "Grid" systems, or other areas, should be examined for their impact on experimental, theoretical, and computational investigations in particle physics. Technologies developed, or to be developed, within particle physics should be examined for their potential impact on society. The group should also review the demands for computer science expertise and software engineering in the current and future programs. It should comment on:

1. The technology advances developed or enhanced by research in particle physics over the past 20 – 30 years.
2. Key problems and areas in particle detection technology, accelerator technology, information technology, and advanced algorithms for further developments that are vital for progress in our field.
3. Areas of opportunity in the above fields where particle physics may play a principal role in fostering progress in key technologies important to scientific research and/or society at large.
4. Areas where new developments in other fields may directly benefit particle physics over the next several years.
5. Promising areas for common developments among experiments, and between particle physics and other fields, that could be of great mutual benefit.
6. The changing role of computing professionals in our field and the need of physicists to enhance their knowledge of modern computing approaches and tools.

The working group should aim at formulating a plan for further development and improved exploitation of such technologies, to the mutual benefit of our field and society at large. It should estimate the scope, structure, manpower and other resources that will be required to make such a plan effective.

4. Physics Issues Working Groups

P1: Electroweak Symmetry Breaking

This group has within its purview three fundamental issues to be tackled by high-energy physics during this decade and the next:

A. What is the mechanism of electroweak symmetry breaking?

B. What is the relation between electroweak symmetry breaking and the origin of quark and lepton masses? How many problems of mass are there?

C. What scale of new physics is associated with electroweak symmetry breaking?

Physics scenarios that address these questions can be roughly divided as:

i. standard-model Higgs mechanism

ii. composite Higgs boson from new strong dynamics

iii. the “no Higgs” scenario of strong WW scattering

iv. MSSM Higgs mechanism in various representative regions of the MSSM parameter space

v. extended Higgs sectors with extra pseudoscalars, singlets, radions (or other bulk scalars), Kaluza-Klein modes, etc.

vi. other supersymmetric models with extended Higgs sectors

1. Tree of questions. Produce a tree of more narrowly posed questions, each of which can be decided by a well-defined experimental measurement or series of measurements, and which, taken as a whole, will serve to answer (A) – (C) above with a high degree of confidence and insight. The number of branchings of this tree will obviously be limited by practical considerations, but at the first level of fine-graining it should include the ability to discriminate among the physics scenarios outlined above.

2. Experiments to illuminate the questions. Identify, for each question in this tree, what kinds of experiments at what kinds of machines could plausibly answer them, and with what accuracy and confidence level. Critical machine parameters or detector capabilities should be identified where appropriate.

3. Comparison and plan. Integrate the information laid out for the first two tasks into a coherent plan. Where multiple experimental strategies at different machines address the same questions, compare and contrast them. Address the challenges of integrating information from different sorts of experiments.

A major task in responding to these charges will involve the broad topic of precision measurements. The group should clearly delineate the constraints on models of electroweak symmetry breaking coming from current data, particularly electroweak precision data. The group should examine how future precision measurements, not just, e.g., of the Higgs sector, but also, e.g., of the masses and couplings in the gauge-boson, top-quark, and SUSY partner sectors, will specifically address the question-tree developed for the first charge. Does the top quark, because of its great mass, provide a special window on electroweak symmetry breaking?

For all the topics within the province of the group, describe what calculations will be needed and propose a plan for assuring that they are done.

The final product of this group will be a comprehensive, coordinated, and aggressive plan for discovery and understanding of the physics related to electroweak symmetry breaking and the generation of fermion masses, based upon our best current knowledge.

The tree of questions should be developed, to the extent possible, before the beginning of Snowmass 2001. This activity should be coordinated with the convenors of the instrument-oriented E groups. During Snowmass, many of the specific experimental questions can be addressed in the instrument-oriented E sessions, reserving the EWSB sessions for other issues, comparisons, synthesis, and discussion. Coordination with the other physics working groups, in particular P2: Flavor Physics and P3: Scales beyond 1 TeV, will be important.

P2: Flavor Physics

The Flavor Physics Working Group encompasses both quark and lepton flavor physics. At an operational level, the two major areas of interest are quark flavor physics and the CKM matrix, and neutrino flavor physics and the analogous MNS matrix for neutrino mixing. The CKM (quark-mixing) matrix has been studied for several decades and is entering an era of increasingly precise measurements in which the CP-violating phase will be determined and unitarity can be tested. Experimental evidence for the presence of non-diagonal elements in the MNS (neutrino-mixing) matrix, on the other hand, is rather recent and we cannot even be certain about which neutrinos mix or whether mixing is limited to the three known neutrino species. We need to learn whether there are sterile neutrinos. It is also an open question whether CP is violated in the neutrino sector and whether it may be experimentally observable.

In an important sense, all the fermion masses and mixing angles–today’s primary concerns of flavor physics–have their origin in physics beyond the standard model. Accordingly, behind the description of the properties of and behavior of fundamental fermions lie important questions of principle, including

- The riddle of identity: what makes an electron an electron, and a top quark a top quark?
- The flavor scale(s): at what energy scales are the properties of the fundamental fermions determined? (Are they the same for neutrinos as for quarks and charged leptons?)
- The origin of CP violation: How does CP violation arise? What is it telling us?
- The nature of neutrinos: Is a neutrino its own antiparticle?

1. Current knowledge. Adopt a convention to describe each matrix in terms of a convenient set of parameters, and summarize our present knowledge. What are the dominant sources of error–theoretical or experimental, statistical or systematic? In the case of neutrinos, comment also on how additional sterile neutrinos enter and what limits are available.
2. The decade ahead. Extrapolate the expected improvement in our knowledge of the flavor parameters over the next 10 years. Catalogue the existing and projected experiments that will study the issues of quark (prominently strange and bottom, but also charm and top) charged-lepton (largely muon and tau), and neutrino flavor. Are any important opportunities being missed?
3. Toward a coherent picture.
    - (a) What are the opportunities and needs for improving our knowledge of the flavor sector for all of the quarks (up, down, charm, strange, top, bottom) and charged leptons (e, μ, τ)?
    - (b) What theoretical developments are required to make sense of forthcoming measurements? What are the strengths and limitations of our current tools, including heavy-quark theory, chiral perturbation theory, and lattice gauge theory? What kind and level of theoretical effort is demanded by current and planned experiments? What measurements are needed to test and inform calculations of hadronic matrix elements? This group should coordinate with the working group P5: QCD and Strong Interactions.
    - (c) Give examples of how multiple measurements of the CKM parameters over-constrain the standard model, and how they can, in the presence of new physics, lead to conflicting results or conflict with unitarity. Examine how new physics would show itself in the flavor sector.
    - (d) What future experiments or facilities would be required to establish and further explore new physics that shows up in the quark flavor sector?
    - (e) For neutrino mixing, what additional information would be provided by future experiments with conventional neutrino beams, a muon storage ring as an intense source of electron and muon neutrinos, reactor experiments, and solar or atmospheric neutrinos? In particular,
        - i. Compare the capabilities of a neutrino factory based on a muon storage ring with those of a neutrino beam generated by an intense pion "super beam."
        - ii. What additional information should be provided by nonaccelerator experiments in a new deep underground facility, e.g., new large-scale double-beta decay or solar neutrino / supernova experiments?
        - iii. How will searches for lepton flavor violation, precision $g-2$ measurements, and searches for permanent electric dipole moments add to our knowledge?
4. The origin of flavor. What do theories of flavor suggest as crucial questions for experiment? How can the accumulating knowledge of the flavor sector—for the quarks, charged leptons, and neutrinos—guide the development of a theory of flavor, and the identification of one or more flavor scales? What significant clues are provided by the structure of the fermion mixing matrices?
5. Why the Universe is made of matter. Confront the measurements of CP violation with the level of CP violation required to explain the observed baryon asymmetry of the Universe. What further program of measurements (including cosmological measurements) or theoretical developments will be most useful in completing our understanding of baryogenesis? What are the likely cosmological consequences of CP violation in the neutrino sector? These discussions should be coordinated with working groups P4: Astro/Cosmo/Particle Physics and P3: Scales beyond 1 TeV.

The background information for Points 1 and 2 should be developed, to the extent possible, before the beginning of Snowmass 2001. This activity should be coordinated with the convenors of the instrument-oriented E groups. During Snowmass, many of the specific experimental questions can be addressed in the instrument-oriented E sessions, reserving the Flavor sessions for other issues, comparisons, synthesis, and discussion. Coordination with the other physics working groups, in particular Electroweak Symmetry Breaking, Scales beyond 1 TeV, and QCD and Strong Interactions, will be important.

P3: Scales beyond 1 TeV

In the past decade, we have established the standard model gauge interactions by precision measurements. We are now entering a new decade with a strong emphasis on the physics of electroweak symmetry breaking and the origin of fermion masses and mixings. At the same time, experimentation in this decade could well bring new information beyond the physics of electroweak symmetry breaking and genuine surprises.

There are at least two big reasons why the standard model is incomplete: (i) The hierarchy problem, or why the electroweak scale is so much smaller than the Planck scale. (ii) Gravity is absent from the standard model. Therefore at least two approaches may be fruitful. In the bottom-up approach, we study possible solutions to the hierarchy problem and work out their observable consequences. In the top-down approach, we begin with certain theories of quantum gravity (e.g., string theory) and work out their consequences for low-energy experiments. The experiments may include rare decay studies, b, c, or τ factories, electroweak precision measurements, experiments at the energy frontier, searches for proton decay and for dark matter, gravitational-wave detectors, high-energy astrophysics experiments, and experiments yet unknown. The implications of new physics at scales beyond 1 TeV touch all the other physics working groups, so we encourage joint sessions to explore areas of common interest.

Among the usual candidates for new physics beyond the standard model are the collider signatures of supersymmetry, new strong interactions, or extra dimensions, but this group should also consider new frontiers, such as gravity measurements below 0.1 mm. Brainstorming sessions might be useful.

1. Survey theoretical scenarios that stabilize the electroweak scale far below the Planck scale, including supersymmetry, new strong interactions, large extra dimensions, small extra dimensions, and their combinations. Also examine scenarios motivated by reasons other than the hierarchy problem (e.g., axions, new gauge interactions, etc.), and consider where we might look for surprises. Review the current experimental situation and consider prospects for future improvements.

2. Study how TeV-scale measurements could give trustworthy information on much higher energy scales, and evaluate what set of measurements (of what quality) would be needed to draw definite conclusions. Example: How could knowledge of the superparticle spectrum discriminate among different mechanisms of supersymmetry breaking or different unification schemes?

   Within the framework of supersymmetry,

   (a) What does SUSY tell us about the mechanism of electroweak symmetry breaking?
   (b) How do we determine the mechanism of SUSY breaking, the messenger mechanism, and the scales associated with this new physics?
   (c) How do we use measurements of SUSY at the TeV scale as a window on the physics of strings, extra dimensions, and unification?
   (d) How do we decipher the role of SUSY in flavor physics and in CP violation?
   (e) When do you give up SUSY?

3. Evaluate the current status of unified theories of the strong, weak, and electromagnetic interactions, and survey the important targets for experiment, including proton decay, neutron-antineutron conversion, neutrino properties, and lepton flavor violation. Consider the role of various sorts of precision measurements in testing models of unification.

4. For a representative set of scenarios:

   (a) Work out experimental signatures and study how they might best be observed. To cite a few examples: (i) How could we establish new strong interactions at hadron or a lepton colliders? (ii) How would we observe quantum decoherence due to Planckian physics in the neutral kaon system? (iii) How could we be detect mini-black hole formation at TeV-scale colliders? (iv) What are the prospects for micron-scale gravity measurements?
   (b) Study how models of fermion masses might have consequences for rare processes. To cite a few examples: (i) What do fermion-mass models based on “fat” branes imply for rare decays? (ii) What are the properties of leptoquarks that arise in fermionic string constructions? (iii) How would supersymmetry manifest itself in muon-electron conversion? (iv) What is the connection between lepton flavor violation and neutrino oscillations?

(c) Study the implications of new physics beyond the 1-TeV scale for astrophysics and cosmology. Examples: (i) How would violations of Lorentz invariance influence ultrahigh-energy cosmic rays? (ii) What are the cosmological consequences of modifying the gravitational force law? (iii) Catalogue the plausible dark-matter candidates. How would different kinds of dark matter show themselves, and what are their implications for structure formation?

(d) Consider a representative sample of new phenomena, such as new neutral weak bosons, signals for quark and lepton compositeness, magnetic monopoles, fractionally charged particles, etc. Review thoroughly the current limits and the assumptions that underlie them, and discuss the discovery limits that might be reached in the future.

5. The first decisive evidence for new phenomena may admit competing interpretations. Explore several scenarios in which the first collider signatures might fit more than one picture (e.g., technicolor and supersymmetry), and devise strategies to unambiguously determine the nature of the new physics.
6. For the exotic signatures considered, summarize the control over standard-model processes that must be achieved in order to establish and study the "new physics." Identify areas in which major progress is required to make new-physics searches effective and reliable.
7. For the universe of models investigated, consider how speculations about new physics beyond 1 TeV should inform the discussion of future accelerators and other experimental initiatives.
8. Starting from theories of quantum gravity, develop scenarios for low-energy experimental consequences, including proton decay, stochastic gravitational waves, violations of Lorentz invariance or CPT symmetry, and black hole physics.

Background information should be developed, to the extent possible, before the beginning of Snowmass 2001. This activity should be coordinated with the convenors of the instrument-oriented E groups. During Snowmass, many of the specific experimental questions can be addressed in the instrument-oriented E sessions, reserving the Scales beyond 1 TeV sessions for other issues, comparisons, synthesis, and discussion. Coordination with the other physics working groups, in particular P1: Electroweak Symmetry Breaking and P2: Flavor Physics, will be important.

P4: Astro/Cosmo/Particle Physics

The Astro/Cosmo/Particle Physics Working Group encompasses a broad range of scientific topics that border on particle physics, cosmology, and astronomy. This area of research has been delineated more by historical accident than by calculated design. One of the goals of this group will be to explore what constitutes astro/cosmo/particle physics. For the purposes of this working group, we will consider research done in the following areas as at least being pertinent:

- Cosmology and the early Universe
- Dark matter and dark energy
- High-energy particle astronomy (using gamma-rays, cosmic rays, and neutrinos)
- Gravitational waves
- The search for nucleon instability and the problem of why the Universe is made of matter

Regardless of definitions, during the last ten to fifteen years astro/cosmo/particle physics has enjoyed an explosion of exciting results, along with greatly increased interest. These discoveries have answered some questions, but a number of exciting questions remain, including:

- The detection of fluctuations in the microwave background revealed the seeds of structure formation in the early Universe. What information will the next generation of precision cosmology measurements provide, and what self-consistency checks among different measurements will be possible with this new body of data?
- From the study of type I supernovae, we have evidence of a new dark energy that acts as a negative pressure to accelerate the expansion of the universe. What is the nature of this dark energy and how does it relate to particle physics?

- Individual cosmic ray particles have been detected with energies exceeding 100 EeV. What are these particles and how are they produced?
- We have detected high-energy (MeV-TeV) gamma rays from a variety of powerful astrophysical objects, including gamma-ray bursts and active galactic nuclei. How are these objects powered, how do they channel such a large fraction of their power into gamma rays, and do they play a significant role in the origin of the cosmic rays?
- New experiments to detect dark matter, high-energy astrophysical neutrinos, and gravitational waves are being comissioned or considered. What are the prospects for this new generation of experiments, which have substantial increases in sensitivity over the previous generation?

One of the most important aspects of this field is its increased connection to particle physics. Historically, astro/cosmo/particle physics has derived both scientific impetus and experimental methodology from high-energy physics, but more recently, it has become clear that astrophysical research will very likely have a profound impact on particle physics. It is largely in this context why it is so essential to have a vigorous working group in astro/cosmo/particle physics at Snowmass.

The basic charge for this working group is to broadly define and review astro/cosmo/particle physics, to examine its connections to, and ramifications for, particle physics, and to consider a vision for future research in the field.

In somewhat more detail, it will be essential to:

1. Review the field of astro/cosmo/particle physics and summarize the current status of research in the field (theory, phenomenology, and experiments—both operational and under construction).
   (a) Try to come up with the defining elements of astro/cosmo/particle physics and how the field relates to particle physics and astronomy. How many people are working in astro/cosmo/particle physics?
   (b) Delineate the major sub-areas of the field (e.g., UHECRs, gamma-rays, neutrinos, gravitational waves, dark matter, cosmology, etc.)

   This educational exercise will be important for providing a baseline and common language, as well as for improving understanding of the field in the larger communities of particle physics and astronomy.
2. Outline a vision for astro/cosmo/particle physics for the next decade and beyond. Among other things, consider:
   (a) What are the broad scientific goals, and what are the key measurements to be made? What new experiments are required? What advances in technology are required?
   (b) Where is there important overlap (in theory, experimental techniques, instrumentation) between astro/cosmo/particle physics and accelerator-based particle physics? What can astro/cosmo/particle physics learn from high energy physics, and vice-versa?
   (c) How can ideas and measurements in astro/cosmo/particle physics (e.g., precision cosmology) help identify new energy scales of interest to particle physics?
   (d) What are the prospects for astrophysical techniques to detect new fundamental particles or to provide evidence for new interactions?
   (e) What information from particle physics is needed to interpret the results of astro/cosmo/particle experiments?
3. Seek community and agency input on the current mechanisms for project funding and review, and examine the funding matrix for astro/cosmo/particle physics. Which mechanisms are working and which are not? What steps can be taken to improve the situation?

Background information should be developed, to the extent possible, before the beginning of Snowmass 2001. This activity should be coordinated with the convenors of the instrument-oriented E groups. During Snowmass, many of the specific experimental questions can be addressed in the instrument-oriented E sessions, reserving the Astro/cosmo/particle sessions for other issues, comparisons, synthesis, and discussion. Coordination with the other physics working groups, in particular P3: Scales beyond 1 TeV, and P2: Flavor Physics, will be important.

P5: QCD and Strong Interactions

This group should consider the full range of topics associated with the strong interactions, including critical tests of Quantum Chromodynamics, the developing area of hadronic physics including our understanding of hadron (particularly nucleon) structure, the fundamental parameters of QCD including the strong coupling constant and the quark masses, the ramifications of the richness of QCD under unusual conditions, and QCD as a tool for calculations and measurements of cross sections and decay rates.

An important responsibility of this group is to interact with the other working groups on common problems. QCD has a strong influence on almost all measurements in particle physics, via the scattering cross sections and backgrounds at hadron colliders, fragmentation in electron-positron colliders and weak or strong matrix elements in hadron decays. A solid understanding of the QCD issues underlies many measurements (and discoveries) in HEP.

1. Status and prospects. Provide a compact summary of the current status of QCD, catalogue the new information on QCD that may become available in the next decade, either through experimental measurement or improved theoretical techniques, and—after consultation with the other physics working groups—report on the interrelation of QCD with other topics in particle physics. Assess the current state of our knowledge of the quark masses and the strong coupling constant. What issues surround the precise definition and meaning of quark masses? What are the limitations to current knowledge, and how might they be overcome? How do uncertainties in the QCD parameters propagate into predictions for observables? Survey the range of experimental studies of QCD and ask which important experiments are not yet being undertaken, and what kinds of instruments will be needed to make them happen.

2. The technology of perturbative QCD. Survey the current state of the art in making reliable perturbative calculations in QCD—not just at very high energies, but in all the domains in which QCD is applied. What are the points at which current methods encounter unresolved issues? What are the prospects for major advances over the coming decade? What calculations will be required by the coming generation of experiments? How can we ensure that the needed theoretical work is done?

3. Nonperturbative methods. Survey the current state of the art in making reliable nonperturbative calculations in QCD—by lattice gauge theory, sum rules, and other approaches. What are the points at which current methods encounter unresolved issues? What are the prospects for major advances over the coming decade? What calculations will be required by the coming generation of experiments? How can we ensure that the needed theoretical work is done?

4. Confinement and the hadron spectrum. How close have we come to a quantitative understanding of the hadron spectrum through lattice QCD? What are the prospects for a complete solution (including dynamical fermions) over the next decade, and what developments are required to make that happen? What insights into the mechanism of confinement come from developments in string theory and supersymmetric gauge theories, and what do they suggest for investigations (on the lattice, or by other methods) of theories other than four-dimensional QCD that might yield important lessons?

5. Hadron structure.

   (a) Static properties. Briefly summarize what is known about the static properties of the nucleon and other hadrons, and discuss the areas in which improvements are needed. In consultation with the experimental working groups, consider the kinds of measurements (by improvements in traditional methods, using intense neutrino beams, etc.) that could yield the desired information.

   (b) Parton distribution functions. Make a critical assessment of the current crop of parton distribution functions, with attention to how well they reproduce the data from which they are extracted, how precisely they respect important theoretical constraints, and how well they serve the needs of their users. Evaluate the newly available parton distributions with uncertainties, and characterize what would be an ideal set of parton distribution functions. What are the current theoretical and experimental limitations on the reliability of parton distribution functions? On a related topic, consider what is currently known about fragmentation functions, and what needs to be known for applications that will be important over the coming decade.

   (c) Partons and the structure of hadrons. What progress can we expect in relating the parton degrees of freedom in the infinite momentum frame to the structure of hadrons in the rest frame? What are the prospects for developing quantitative tools and physical pictures to make this link?

6. Hadronic physics. The study of strongly interacting matter is an area of fruitful interaction between nuclear and particle physics, and many important questions involve experimental results and theoretical tools from both disciplines. The QCD working group should report on the state of hadronic physics, considering a few key issues (such as chiral symmetry breaking and the development of sound models and approximations, particularly those based on effective field theory, to QCD) to give form to the discussion. What role can high-energy experiments play in advancing our understanding of hadronic physics? The group should coordinate with working group P2: Flavor Physics.
7. Spin. What is the value of spin observables, and of polarized beams and targets, in probing the implications of QCD and in looking for new phenomena?
8. Diffraction. What are the important issues in diffractive physics that must be addressed by theory and experiment? Are there special situations in which diffractive phenomena can be an effective tool in the search for new physics?
9. Compositeness. The idealization that quarks and leptons are elementary is one of the foundations of the standard model. What are the prospects for finding, or setting limits on, a compositeness scale over the next decade and beyond, in all the instruments we might contemplate? Examine theoretical scenarios for composite quarks and leptons (in consultation with working group P3: Scales beyond 1 TeV). What special considerations might present themselves for the top quark, or for the third generation?
10. The richness of QCD. Explore the novel phase structure of QCD under unusual conditions, including the prospects for observing and understanding the quark-gluon plasma and the consequences of phenomena such as color superconductivity. What are the implications of heavy-ion experiments for our understanding of QCD? What other experimental approaches might yield similar, or complementary, information? What lessons can we expect for the quark-hadron phase transition and other phenomena in the early universe?

Background information should be developed, to the extent possible, before the beginning of Snowmass 2001. This activity should be coordinated with the convenors of the instrument-oriented E groups. During Snowmass, many of the specific experimental questions can be addressed in the instrument-oriented E sessions, reserving the QCD and Strong Interactions sessions for other issues, comparisons, synthesis, and discussion.

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