2001 Snowmass
Accelerator R&D Report

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Abstract

The purpose of this report is to provide a perspective on future accelerator projects, and to identify the R&D activities necessary to prepare for these projects. The report summarizes the conclusions of accelerator studies made during the 2001 Snowmass Summer Study on the Future of Particle Physics. In doing so, it serves as a summary of the opinions on accelerator R&D expressed by the scientific community as it looks towards the next few decades. The main technical content is provided by the Executive Summaries of each of the fifteen accelerator Working Groups. These Working Group Executive Summaries form an integral part of this report.

Part I

Executive Summaries

Executive Summaries of the Snowmass Working Groups:

Working Group		Conveners
M1	Muon-Based Systems	McDonald, Sessler
M2	Electron-Positron Circular Colliders	Oide, Seeman, Hendersen
M3	Linear Colliders	Brinkman, Toge, Raubenheimer
M4	Hadron Colliders	Peggs, Syphers
M5	Lepton-Hadron Colliders	Ben-Zvi, Hoffstaetter
M6	High Intensity Proton Sources	Chou, Wei
T1	Interaction Region	Markiewicz, Pilat
T2	Magnet Technology	Gourlay, Kashikhin
T3	RF Technology	Adolphsen, Holtkamp, Padamsee
T4	Particle Sources	Sheppard, Mokhov, Werkema
T5	Beam Dynamics	Blaskiewicz, Lee, Kim
T6	Environmental Control	Bialowons, Laughton, Seryi
T7	High Performance Computing	Ko, Ryne
T8	Advanced Acceleration Techniques	Joshi, Sprangle
T9	Diagnostics	Pasquinelli, Ross

Introduction

During the past century, particle accelerators have formed the foundation for experimental research in particle physics, the study of elementary particles. The development of accelerators has been motivated by this research, and advances in particle physics have been preceded by corresponding advances in the concepts, physics, and technology of accelerators. The particle physics community has had the foresight to prepare for ever higher energies by investing in R&D for future accelerators while at the same time exploiting existing facilities.

Table **??** shows an outline of some key accelerator concepts, and the accelerator physics and technologies that propelled the successful advance of accelerators for particle physics. The effect of these developments is illustrated by the Livingston chart shown in Figure **??**, which is discussed in more detail in the next section. The extraordinary progress to increasingly higher energies has required the development of new concepts, the study of critical accelerator physics issues, and finally the development of the required technology to achieve these new concepts. This progress has been made possible only by a sustained effort on accelerator R&D over the past several decades.

Table 1: Illustrative accelerator concepts invented during the past several decades for particle physics research, and the accelerator physics and technology developed to realize them.

Accelerator Concepts	**Accelerator Physics and Technology**
Cyclotrons	Phase focusing, Magnet technology
Synchrotrons for fixed target research	Strong focusing, Large scale vacuum
Linear accelerators for fixed target research	High power klystrons, High frequency RF systems
Colliding beam proton–proton storage rings	Nonlinear dynamics, Superconducting magnets
Colliding beam electron–positron storage rings	Nonlinear optics design, Low beta insertions
Colliding beam proton–antiproton storage rings	Phase space stacking, Stochastic cooling
High Luminosity Factories	Collective instabilities, Superconducting RF cavities
Linear Colliders	Emittance preservation, RF pulse compression, Micron scale beam diagnostics, Feedback for beam stabilization

This progress is continuing today. The next generation hadron collider, the Large Hadron Collider (LHC), is presently under construction at CERN, Switzerland. New designs of next generation linear colliders are being proposed with a center-of-mass energy reach of 0.5 to 1 TeV. New medium energy colliders at the luminosity frontier are under study to support precision physics measurements. High intensity beams of unstable muons could be captured in storage rings and explored as a source of intense neutrino beams or even brought into collision with a second counter-rotating muon beam. Exploration of the multi-TeV energy frontier continues with studies of the Very Large Hadron Collider (VLHC) and the Two-Beam Linear collider. Finally, advanced accelerator ideas such as laser acceleration and plasma acceleration are under active experimental investigation.

Snowmass 2001 marks a turning point for the field of particle physics and the development of high energy accelerators. The impressive trend of continued advances made by accelerator scientists will require, more than ever before, a sustained R&D effort. The remainder of this report will show both that the development of accelerators for particle physics is continuing and that several new avenues are opening up for the coming decades. It is vital that accelerator R&D be aggressively pursued for the proposed future projects. Fundamental research in accelerator physics and technology must be continued and even enhanced to address the long-term scientific goals of particle physics. As in the past, there is every reason to believe that other fields of science and engineering will also greatly benefit from this investment into accelerator physics and technology.

Brief history of Accelerator Developments for Particle Physics

Progress in particle physics has historically been paced by developments in the technology of high energy particle accelerators. The primary measure of the performance of a high energy accelerator is the energy of the particle beam. The enormous progress that has been made in beam energy is illustrated graphically in Figure **??**, which shows the effective beam energy plotted versus calendar year. This graph is called a "Livingston chart," after M. Stanley Livingston, the accelerator physicist who first constructed such a chart in the 1960's.

The Livingston chart shows that, since the 1930's, the energy reached in high energy accelerators has grown exponentially with time, approximately a factor of ten every seven years. This impressive rate of progress has only been possible by the continued development of new ideas and the continued exploitation of new technologies. The first accelerators to be developed in the early 1930's used direct-voltage techniques to accelerate ions to energies of a few hundred keV. This technology was sufficient to observe the first artificial nuclear disintegration in 1932. However, it was limited by voltage breakdown to energies of about 1 MeV. New ideas were needed to make further progress.

The concept of resonant acceleration led to the next advance in accelerator energy. Particles were accelerated in a series of accelerating gaps; the accelerating voltage across the gaps was generated by an electric field oscillating in resonance with the particles. Although the voltage on each gap was small, the resonance condition led to the buildup of high energies of the particles. This could be done either in a series of gaps in a straight line (linear accelerator, or linac), or with a single gap in a circular machine (cyclotron). In the 1940's, such machines reached energies of 10–100 MeV.

Cyclotrons were limited to energies of 10–25 MeV. However, another new idea, the principle of phase stability, allowed the invention of the synchrocyclotron and the synchrotron. These circular accelerators had much greater energy reach, limited only by their size and cost. In the late 1940's and early 1950's, synchrocyclotrons and synchrotrons extended the energy reach to the 1–2 GeV range.

In the early 1950's, the principle of alternating gradient focusing was invented. Use of this principle dramatically reduced the size of the magnets for large accelerators, allowing a much

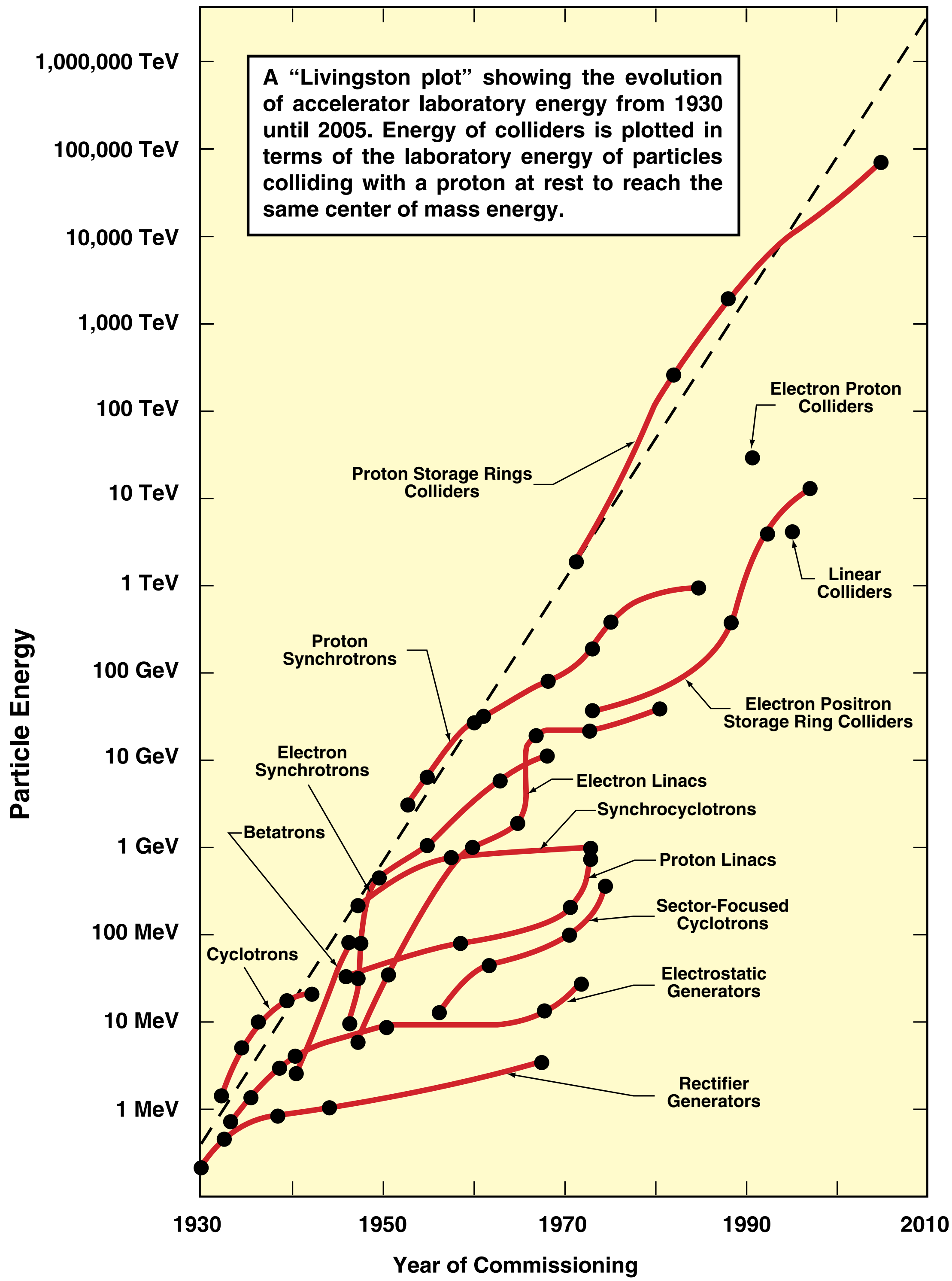

Figure 1: Time evolution of effective beam energy of particle accelerators.

larger energy to become economically achievable. Accelerators with energies ranging from 30 to 400 GeV were built in the 1950's and 1960's.

The next idea for advancing the accelerator energy frontier, introduced in the late 1960's, was the concept of colliding beams. With this idea, an enormous step was taken in effective energy in the center-of-mass for producing new particles. Colliding beam accelerators were responsible for the major discoveries in particle physics in the 1970's through the 1990's, such as the discovery of the charmed quark, the W and Z bosons, and the top quark. The charmed quark was discovered in an electron-positron colliding-beam storage ring as well as in a fixed target experiment. Key to this discovery was the further extension of the principle of alternating gradient focusing to create very tightly focused beams at the interaction region, which in turn resulted in very high colliding beam interaction rates, and great discovery potential for particle physics.

The W and Z bosons, and the top quark, were discovered in proton-antiproton colliders, which were made possible by the Noble-Prize-winning development of stochastic cooling. This development allowed the antiprotons to be cooled to dimensions and intensities comparable to the proton beams, resulting again in very high interaction rates for particle physics.

Linear colliders began as a concept to avoid the synchrotron radiation generated in circular electron-positron high energy colliders. High interaction rates were achieved using new techniques of emittance preservation and advanced final focus systems to focus beams to sub-micron dimensions.

The continued development of new ideas was not sufficient for the extraordinary progress demonstrated in the Livingston chart. As illustrated in Table **??**, it has also been necessary to exploit new technologies. The earliest accelerators made use of high voltage machines developed for the X-ray industry, and in turn stimulated that industry. The developments in radio and radar transmission during the 1940's, and in high frequency power sources such as magnetrons and klystrons, made possible the resonant accelerators such as linear accelerators and cyclotrons. As accelerators became larger, developments in vacuum engineering were crucial to allowing large-scale vacuum systems to be feasible and economical. Magnet technology took a giant step forward with the development of superconducting wire and cable technology in the 1970's. This allowed the construction of very large high energy accelerators using superconducting magnets, which could operate with much smaller power requirements than conventional magnets. The utilization of superconducting materials in RF structures also opened up a whole new range of possibilities for accelerating structures. Recent rapid progress in electronics allowed the complex control, diagnostics, and feedback systems needed in linear colliders. Highly polarized beams allowed a much greater physics reach for the first linear collider. Most recently, modern developments in laser technology in the 1990's have made it possible to consider laser-driven accelerators with unprecedented field gradients, offering the possibility of smaller, lower cost accelerators at very high energies in the future.

This remarkable progress in the development of accelerator technology would not have been possible without a broad base of accelerator R&D. By its very nature, R&D efforts do not have assured success, and at a given stage, multiple approaches on a broad front have been and will be required. For a healthy long-term future of the field, it is very important that not all accelerator R&D efforts be directed at a few specific preselected projects. Maintaining a diverse program of accelerator R&D has been crucial in the past and will be increasingly important as project lead times become longer.

Importance of Accelerator Physics and Technology to Science Research

Experiments based on accelerators have made remarkable discoveries about the basic nature of matter in the realm of particle physics. These discoveries include nuclear structure, the behavior of nuclear matter, quark dynamics, the nature of elementary particles and the fundamental forces, unified field theories, and cosmology. Future possible discoveries under active pursuit by the particle physics community include quark plasmas, the origin of the asymmetry between matter and antimatter, supersymmetric counterparts to the known existing particles, and the fundamental origin of mass.

In addition to advancing the frontiers of particle physics and our understanding of the cosmos at a fundamental level, the technical advances in accelerator physics and technology achieved for particle physics applications also have a profound impact on scientific advances in other areas of science such as nuclear physics, condensed matter physics, atomic and molecular physics, and plasma physics, to mention a few examples. Major existing applications include electron microscopy, microprobes, charged-particle-beam lithography, ion implantation, isotope production, particle beams for precision irradiation therapy, superconducting magnets and medical magnetic resonance imaging, neutral-beam heating of fusion plasmas, spallation neutron sources, synchrotron light sources, x-ray lithography, and free-electron lasers.

One outstanding example of high energy accelerators playing a critical role in other areas of science is the synchrotron radiation source. Recently, synchrotron light was used in the field of biology to determine how meters of DNA can be coiled and managed in cells, to determine the long-sought structure of bacteriorhodopsin, and to determine the largest x-ray crystal structure to date, that of the bluetongue virus made of more than 1000 separate proteins. Indeed, because of its great utility, synchrotron radiation emitted by electrons in storage rings has spawned some 50 dedicated facilities worldwide and some 26 new sources are anticipated.

Another example is the spallation neutron source. The protons produced by linacs and synchrotrons are used to produce pulsed neutron beams for material science, often complementing the studies carried out with synchrotron light sources. The principal new facility planned by the US Department of Energy is the Spallation Neutron Source. This collaborative project, involving several DOE laboratories, will make fundamental contributions to both materials research and biological science.

With accelerator physics and technology making rapid progress and the concomitant applications becoming increasingly demanding and sophisticated, accelerator R&D has become a mature scientific discipline in its own right. The synergism between accelerator physics and technology has grown substantially in recent years to become enormous in scope, encompassing such diverse areas as RF source technology, advanced magnet technology, advanced techniques in nonlinear dynamics and chaos, advanced numerical simulation techniques for terascale computing, collective processes and nonlinear dynamics of one-component nonneutral plasmas, the formation and trapping of positron and antiproton plasmas for antihydrogen production and basic atomic physics studies, the use of high energy electron beams for tunable x-ray sources, the development of stochastic cooling techniques, and the development of novel, plasma-based concepts for achieving high acceleration gradients. These developments constitute a significant advancement of the nation's science and technology base.

Possible future applications of accelerator physics and technology include intense beams for inertial fusion, the production of tritium, the production of nuclear fuels, the transmutation of nuclear waste, and high-speed and high-resolution proton radiography for nuclear stockpile stewardship. In addition, many industrial and medical applications are expected as accelerators are further miniaturized and their costs further reduced. Table-top GeV accelerators, once available, will have a substantial potential as research tools. After the 4th generation of free electron lasers, one can further imagine coherent production of x-rays using a plasma undulator. Like lasers and electronics, accelerator physics and technology is a fundamental component in the large infrastructure of the Nation's science and technology base. Training young people in accelerator physics and technology also strengthens the scientific manpower of this country in an important way. Continuation, and even enhancement of advanced accelerator R&D beyond the present level is a prerequisite to future advances in particle physics as well as a wide range of other areas of science.

Current Status of Particle Physics Research

As a result of new accelerators completed in the past few years, the field of particle physics in the US has enormous potential for discoveries in the immediate and near term. Every laboratory has been trying to maximize the particle physics output utilizing the existing facilities in most creative ways. The new PEP-II asymmetric B-Factory at SLAC, completed on schedule and within budget, has surpassed its design luminosity goals, and prospects for incisive measurements of CP-violation in the b-quark sector are extremely good. The new Main Injector at Fermilab, also completed on schedule and within budget, will allow much higher luminosities to be achieved in the Tevatron Collider, the highest energy accelerator in the world. This offers the possibility of new discoveries at the energy frontier. These discoveries could shed light on the mechanism of electroweak symmetry breaking, and perhaps provide evidence for new forms of matter, such as supersymmetric particles. Table 2 lists the envisioned plans of the next several years worldwide utilizing and maximizing the output of the existing particle physics facilities.

Outside of the US, construction of the next hadron collider, the Large Hadron Collider (LHC), is underway at CERN. The US accelerator community is a major partner in this work, involved in both the accelerator physics design and in building the challenging superconducting magnets for its interaction region. This large storage ring collider, when completed later in the decade, will extend the energy frontier to 14 TeV, seven times higher than the Tevatron Collider, and is expected to make major discoveries that may revolutionize our fundamental understanding of particle physics.

Table 2: Projects envisioned or planned at existing particle physics facilities worldwide in the immediate future (listed in alphabetical order). More discussions of near-term and future projects (such as linear colliders, neutrino/muon facilities, and very large hadron colliders) are included elsewhere in this report.

BINP, Novosibirsk

BINP just started to operate the upgraded e^+e^- collider VEPP-4M (c.o.m. energy up to 11 GeV). First stage experiments focus on hadronic cross-sections (2–4 GeV) and on precision measurement of tau mass. Soon after, the experiments at full energy would use double-arm spectrometer to study two-photon physics (mass resolution 10 MeV/c2 for two-photon mass up to 4 GeV/c2, double tag efficiency 30%, luminosity $5 \times 10^{31}\text{cm}^{-2}\text{s}^{-1}$).

In 2002, the new e^+e^- collider VEPP-2000 (c.o.m. energy up to 2 GeV, luminosity $1 \times 10^{32}\text{cm}^{-2}\text{s}^{-1}$ with "round beams") will mainly study the hadronic processes in annihilation in the range 1.4–2 GeV.

In 2002, a new injector complex should provide better e^+ and e^- beams of 500 MeV for VEPP-4M and VEPP-2000, and for the future VEPP-5 collider (Charm-Tau Factory, 3–5 GeV c.o.m.). This double-ring collider, to be constructed practically without state support, would have 3 modes of operation: (i) highest luminosity ($1 \times 10^{34}\text{cm}^{-2}\text{s}^{-1}$) using "round beams"; (ii) longitudinal polarization with arbitrary controlled helicities (luminosity $1 \times 10^{33}\text{cm}^{-2}\text{s}^{-1}$) for study of decay properties of polarized taus and charmed baryons; (iii) high monochromaticity regime (e.g., effective energy spread of 50 keV near J/Ψ, luminosity $1 \times 10^{31}\text{cm}^{-2}\text{s}^{-1}$).

BNL

RHIC will continue its present course to collide gold ions at a center-of-mass energy of 200 GeV/n and a design luminosity of $2\times10^{26}\text{cm}^{-2}\text{s}^{-1}$. Increasing this luminosity by a factor of 4 is possible with present hardware. R&D has started for electron cooling of the gold beams at full energy which will make a further luminosity increase by potentially a factor of ten. In addition the plan in 2001 is to collide protons with 100 GeV per beam with 50% longitudinal polarization. In 2003/2004, it is expected to collide protons with 250 GeV per beam with a luminosity of $2 \times 10^{32}\text{cm}^{-2}\text{s}^{-1}$. The possibility of colliding a 10 GeV electron beam with the 100 GeV/n gold beam or the 250 GeV polarized proton beam in RHIC is being studied.

During the 20 hours a day when the AGS is not used as injector to RHIC, it will accelerate intense proton beams of typically 7×10^{13} protons per pulse to 24 GeV for a fixed target program to measure rare kaon decays and search for muon-to-electron conversion. An upgrade of the AGS from the present 140kW beam power to 1–4 MW is being studied as a driver for a neutrino superbeam or for a neutrino factory.

CERN

The construction of LHC, to be commissioned in 2006, takes most of the CERN resources. The preparation of a suitable proton beam for the LHC was achieved in the PS, and is ongoing in the SPS. A neutrino beam CNGS, aimed at the Gran Sasso Laboratory, is being constructed and will be completed in 2005.

Several projects are being studied now, in addition to the CLIC R&D, and will be considered for construction from 2008 onwards: (i) proton intensity upgrades for CNGS, fixed-target physics, LHC, Isolde, using a 2.2 GeV superconducting proton linac; (ii) upgrades in LHC luminosity (possibly a factor of 10) and energy (possibly a factor of 2); (iii) upgrades to neutrino beams, leading to neutrino super-beam(s) and a neutrino factory.

Cornell After the installation this summer of new superconducting IR quadrupoles, CESR will operate for a year at 4.7–5.1 GeV, accumulating a total of $4fb^{-1}$ at the upsilon 1S, 2S, and 3S resonances.

R&D has started on a proposed CESR-c which will extend the energy range down to 1.5–2.1 GeV. This mode of operation, with an expected luminosity of $1 \times 10^{32}\mathrm{cm}^{-2}\mathrm{s}^{-1}$ at 1.5 GeV and $5 \times 10^{32}\mathrm{cm}^{-2}\mathrm{s}^{-1}$ at 2.1 GeV, allows CLEO to embark on a three-year study of tau-charm and J/Ψ physics. In three years, CLEO will accumulate $7fb^{-1}$ of data and will observe $\sim 10^9 J/\Psi$ decays.

DAΦNE

Since 1999 DAΦNE has been sharing time between MD and luminosity delivery. Peak luminosity is now $3.2 \times 10^{31}\mathrm{cm}^{-2}\mathrm{s}^{-1}$, corresponding to $> 1.5pb^{-1}$/day. KLOE has collected $100pb^{-1}$ and DEAR has completed its first experimental phase, with the first observation of kaonic Nitrogen. Present R&D aims to achieve higher tune shift after compensating nonlinearities by octupoles. Luminosity should approach $1 \times 10^{32}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. Next goals by the end of 2002 are the delivery of $500pb^{-1}$ to KLOE and DEAR completion. The IR now housing DEAR will then be ready for the FINUDA installation.

LNF Accelerator Division collaborates with CERN in CLIC Test Facility (CTF3) and is involved in the Italian FEL project (SPARX).

DESY

HERA reached its design performance in 1997 with an integrated luminosity of $37pb^{-1}$, a peak luminosity of $1.4 \times 10^{31}\mathrm{cm}^{-2}\mathrm{s}^{-1}$, and a $> 50\%$ longitudinally polarization in the lepton beams. In 2000, HERA had an accumulated luminosity of $67pb^{-1}$ and a peak luminosity of $2 \times 10^{31}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. The IR is being rebuilt in 2000 and 2001 and it is expected to provide $> 150pb^{-1}$ per year.

A proposal is being studied to collide TESLA electron beam with HERA proton beam with a luminosity of $0.5 \times 10^{31}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. Another study proposes to use the HERA electron ring as a stretcher for fixed target experiments using TESLA beams.

Two options are being considered as an alternative if TESLA is not to be built at DESY: (i) to collide the HERA lepton beam with an ion beam in the HERA proton ring, and (ii) to collide a polarized proton beam with an electron beam in HERA.

Fermilab

Tevatron has run a peak luminosity at $2 \times 10^{31}\mathrm{cm}^{-2}\mathrm{s}^{-1}$ and is currently being recommissioned for Run IIa with the new Main Injector. R&D on antiproton cooling and accumulation, and multi- bunch operations will allow Run IIb in the following few years. Run IIa aims for an integrating $2 fb^{-1}$ in 2001–2003 with a peak luminosity of $2.1 \times 10^{32}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. Run IIb will run in 2003–2007 (estimated cost of 34M$) with an integrated luminosity of $15 fb^{-1}$ and a peak luminosity increased to $5.2 \times 10^{31}\mathrm{cm}^{-2}\mathrm{s}^{-1}$.

Conceptual design study as well as some R&D are being carried out for a 1–4 MW proton driver. A prototyping effort of superconducting RF for separated kaon beams is being done.

IHEP, Beijing

The Beijing Electron Positron Collider (BEPC) and Beijing Spectrometer (BES) will continue to pursue physics in the tau-charm region. IHEP is going to make a major upgrade of BEPC/BES, called BEPC-II. BEPC-II is a two-ring machine with a design luminosity of $10^{33}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. The BES detector will also be substantially upgraded to become BES-III. The Chinese government agreed to support this project. The total cost of the upgrade is estimated to be 80M US$, including expected international contributions. The feasibility study report will be submitted by the end of July 2001. Commissioning of BEPC-II is expected to be 2006.

KEK

Continuing the present accelerator R&D is expected to raise the event rate at KEK-B steadily toward the design level of $1 \times 10^{34}\mathrm{cm}^{-2}\mathrm{s}^{-1}$ luminosity and 100 fb^{-1}/year in 2002. After 2003, more substantial changes are being considered to increase the event rate further. The present goal is to obtain $190 fb^{-1}$ in 2004 and $440 fb^{-1}$ in 2006.

SLAC

In October 2000, PEP-II reached a luminosity of $3.3 \times 10^{33}\mathrm{cm}^{-2}\mathrm{s}^{-1}$ and by July 2001 had delivered an integrated luminosity of $220 pb$ per day, and a total integrated luminosity of $35 fb^{-1}$. Continuing the present accelerator R&D on PEP-II is envisioned to lead to $100 fb^{-1}$ by July 2002 and a peak luminosity of $2 \times 10^{34}\mathrm{cm}^{-2}\mathrm{s}^{-1}$ in about 2006. Taking advantage of PEP-II experience, design R&D for a higher luminosity B-Factory with 100 times more event rate has started. The research for this new accelerator will take several years.

SLAC is also investigating the addition of a small electron storage ring called PEP-N to parasitically collide with the PEP-II e+ beam to provide precision R measurements in the energy range of 1–3 GeV.

When not used to inject into PEP-II, the SLAC linac provides beams for advanced accelerator studies and fixed target physics with polarized electron and photon beams.

Goals of Snowmass Summer Study

Major accelerator projects, just completed or now under construction, thus guarantee an exciting time in this decade for experimental particle physics. However, given the very long lead time required for new accelerator projects, it is imperative that the planning and R&D for the next generation of accelerators be undertaken vigorously now. This need is more pronounced since the cancellation of the previous flagship project of the US particle physics, the Superconducting Super Collider. One of the major goals of the Snowmass workshop is to elucidate a vision for the field in the next few decades, and to delineate the steps to be taken now to ensure that this vision has the potential to become a reality. Most, if not all, of these steps to be taken will necessarily involve accelerator R&D. Information gathered at Snowmass will serve as crucial input to the funding agencies as well as the DOE/NSF High Energy Physics Advisory Panel in their deliberations later this year.

Current theoretical expectations are that the energy range to be explored by the LHC could be very rich in terms of new fundamental discoveries. In this case, simultaneous exploration of this energy range using an electron-positron linear collider would be essential to achieve a full understanding of these phenomena. For this reason, a very large amount of R&D effort has been invested, in the US and overseas, on the physics and technology of a linear collider, operating in the 0.5–1 TeV energy range. Although many aspects of the design of this machine are quite mature, R&D is still needed in critical areas. At Snowmass, the role of this collider in the future of particle physics was discussed. The important physics and technology issues still outstanding were studied, and the required R&D efforts to support resolution of these issues were detailed.

The discovery of neutrino oscillations several years ago has generated renewed interest in neutrino physics. At the same time, a concept has been developed for a new, very powerful source of extremely well characterized neutrinos, which would allow a new generation of very-long-baseline neutrino oscillation experiments. In this concept, the neutrinos are produced by the decay of muons in a muon storage ring. The muons are produced by a powerful proton driver and cooled by a new technique, called ionization cooling. This new concept requires extensive R&D in several areas of physics and technology. Some of these developments could also be used in a multi-TeV muon collider, a much more challenging accelerator than the neutrino source, but one which would address a variety of questions at the energy frontier. At Snowmass, the role of the neutrino source and muon collider in the future of particle physics was discussed, and several of the R&D challenges facing the accelerator builders were addressed.

Hadron colliders have the potential for the greatest energy reach, and it is expected that discoveries at the LHC will motivate even larger colliders with expanded reach well beyond that of the LHC. Hadron colliders with energies from 40 to 200 TeV, and circumferences up to 230 km, have been studied in some detail. The principal issues include development of very high field superconducting magnets, and value engineering to reduce the cost of the very large systems in the accelerator. At Snowmass, the part to be played by such a very large hadron collider in the future of particle physics was discussed, and the R&D issues related to the accelerator were delineated.

The 2001 Snowmass Workshop also provided an opportunity to examine a number of generic accelerator physics and technology issues, many of which are common to all future accelerators. Such issues require sustained R&D, independent of specific accelerator projects, since they form the foundation of accelerator physics and technology. A very important goal of Snowmass was to establish a vision of the future needs and opportunities for accelerator R&D. Such a vision must be long-term by its very nature, as developments in technology can often take decades to mature and must be nourished in a sustained fashion during their gestation period. For example, radically new methods of acceleration, such as laser-plasma wakefield schemes, must receive attention now, even if they may not realize practical application in the near term.

Process of the Report

The Accelerator Organizing Committee of the 2001 Snowmass Workshop organized fifteen Working Groups. These included six Working Groups on specific “machine” configurations :

M1 Muon-Based Systems

M2 Electron-Positron Circular Colliders

M3 Linear Colliders

M4 Hadron Colliders

M5 Lepton-Hadron Colliders

M6 High Intensity Proton Sources

and nine “topical” Working Groups:

T1 Interaction Region

T2 Magnet Technology

T3 RF Technology

T4 Particle Sources

T5 Beam Dynamics

T6 Environmental Control

T7 High Performance Computing

T8 Advanced Acceleration Techniques

T9 Diagnostics

Each Working Group was organized by two or three conveners, and each received a charge from the Organizing Committee. The inputs requested in these charges concerned issues that were judged to be useful in addressing the Snowmass goals mentioned earlier. The input requested from the Working Groups generally included: an evaluation of the present status of the field or project; a comparison of various technical approaches; an enumeration of the necessary R&D efforts in a time-ordered fashion; and, where appropriate, estimates of the budgetary and manpower resources required in the R&D.

The conveners of each Working Group worked closely with members from the Organizing Committee to coordinate progress, and to assure that the Working Group summaries addressed the charges. Every effort was made to prepare Working Group reports that represented a consensus within each group. Although an attempt was made to present the technical input from all Working Groups in a coherent manner, consistency of inputs among different Working Groups, or consistency with budgetary and political realities, were not the main focus of this report. As a result, this report does not set priorities or make specific recommendations. Its function is to provide detailed technical information collected at the Snowmass Workshop so that policy decisions can be made based on this information.

A few common themes expressed throughout many of the presentations and discussions at Snowmass with regard to future particle physics facilities and accelerator R&D can be summarized as follows:

- Independent of the type of the next major facility or where is to be built, it will have to be a truly international undertaking, while keeping regional programs strong.
- Regardless of the choice of the next major facility, R&D must be continued on the remaining proto-projects.
- Beam physics research and advanced accelerator R&D must be continued to assure the near and far future of particle physics.

These views are also supported in many of the executive summaries of the working groups. It should also be pointed out that, in addition to discussions on concrete projects, Snowmass meetings in the past have always generated new ideas not in the mainstream programs. The same has occurred at the 2001 Snowmass Workshop. Indeed, some of the discussions in this workshop have surfaced to be included in written reports, but many others will undoubtedly become seeds that feed into later ideas and later programs.

Opportunities in Accelerator R&D

It is clear that a great deal of pioneering and challenging accelerator R&D tasks are needed to support immediate, and near future particle physics programs. Equally challenging advanced accelerator research will provide the backbone for the far future of particle physics. These exciting opportunities occur in parallel with an urgent need. Active participation by the particle physics community is needed across the board of accelerator research. The help needed covers a wide range in accelerator physics, engineering, computation, and beam diagnostics. As pointed out by Tigner, "The challenge is mainly intellectual." Similar to the way in which detector research and collaborations work, the cultural infrastructure must grow to accommodate effective participation of the particle physicists from laboratories and universities. In addition, long-term advanced accelerator research must be supported as pioneering research, and not as an engineering problem with predetermined milestones and timelines.

An indication of the magnitude of the problem is the fact that there are approximately 3300 particle physicists in the US, whereas only 9% of them, approximately 300, are accelerator physicists. It is clear that help will be needed to confront the overwhelming load of accelerator R&D

facing the community's future. We will need to substantially increase the support of accelerator R&D efforts. First and foremost, we need to increase the number of active participants in accelerator R&D. A plea was therefore made at Snowmass for particle physicists and young students to join in accelerator R&D activities. Interested particle physicists can contact the following volunteers to investigate possible collaborations:

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International Collaboration

Accelerators built and operated for particle physics have traditionally been undertaken primarily by one national region. Current examples are the Fermilab Tevatron and PEP-II (United States), LEP and HERA (Europe), and KEK-B (Japan). It has been clear for some time that this model will not be sustainable for future major accelerator projects. This has been reflected in the international scope of the R&D efforts for a linear collider (which are being carried out in the US, Japan, and Europe). It is also reflected in the significant international participation of the US in the construction of the LHC in Europe.

Future energy-frontier accelerators will have to be true international collaborations. These are projects of unprecedented size and scope. Within the present limits of support for particle physics, no single national region has the capital and manpower resources to carry out such a program on its own. For this reason, it is imperative that such projects be carried out by an international collaboration. This internationalization must be done and is not a matter of choice. In such a collaboration, several national regions would each contribute to the project, both in management and in technical expertise. One region would provide the project site, and would likely provide a large fraction of the resources. The other regions would nevertheless contribute in significant ways as major partners. At present, a mechanism is lacking, and needs yet to be designed.

One of the major challenges lies in solving the complex management, sociological and communication problems. In designing a management and oversight structure, much can be learned from the large particle physics experiments and modern astronomy projects which typically involve international collaborations of distant institutions.

A model for how the international partners would collaborate in the operation of such an internationally built accelerator is under active investigation. This model is called the "Global Accelerator Network." In this model, the accelerator is operated by the same international collaboration of institutions that constructed it. The expert staff from each institution remain based at home, and operate the accelerator remotely. The experts are required to be physically present only during initial commissioning of the hardware and for trouble-shooting particularly difficult problems.

Provided the accelerator design incorporates features required for remote operation, including comprehensive remote diagnostic capabilities, there appears to be no technical obstacle to such a model. Experience from existing laboratories indicates that most of the required activities are already performed "remotely," or could be with properly designed equipment. At many sites, the consoles are "remote" from the actual control computers. At SLAC, for example, consoles may be operated from office or home. Nonetheless, a dedicated high-speed network connection to the remote control room may be required to supply sufficient guaranteed bandwidth for real-time data. The rapid rate of development of communications technology should easily support the demands of accelerator operation in 5–10 years.

A number of experiments are planned to demonstrate that such a configuration can be made to work effectively. Possible pilot projects include remote operation of the Tesla Test Facility, and remote operation of the Fermilab photo-injector. A first workshop on remote operation will be held in the summer of 2002.

Ideally, the development of an international collaboration would emerge from an overall international plan for the future of particle physics. Such a plan must be developed and agreed upon by partners from all national regions. This Snowmass workshop is focused on creating a vision for the future program in particle physics. This vision will necessarily involve future energy-frontier accelerators. For the vision to become a reality, it must therefore be incorporated into an international plan.

Highlights from the working group summaries

Accelerator R&D for Linear Colliders

The next generation of linear collider is based on the foundation of experience gained with the Stanford Linear Collider (SLC) and on research performed worldwide during the past 13 years or so. This research has focused on developing the technology and accelerator physics for a 0.5–1 TeV linear collider, and has spawned several test facilities that address various aspects of a linear collider. The Final Focus Test Beam at SLAC has studied generation of small spots. The ATF at KEK is a prototype damping ring to achieve low emittance flat beams for high luminosity. The NLC Test Accelerator at SLAC is an X-band test facility is used to test technology and physics issues related to high gradient, high frequency acceleration. The Tesla Test Facility (TTF) at DESY is used to test technology and physics issues related to acceleration with superconducting systems. The CLIC Test Facilities I and II have addressed issues related to high gradient, high frequency acceleration powered by an auxiliary high current beam (two-beam acceleration). In addition to the test facilities there have been extensive experiments and calculations to understand the accelerator physics issues that are critical to obtaining high luminosity.

This extensive research has led to two proposals for a linear collider each of which would begin with an energy of 0.5 TeV and later be upgraded to an energy of about 1 TeV. The NLC/JLC proposal uses 11.4 GHz technology to accelerate the beams while the TESLA proposal uses superconducting 1.3 GHz technology . Each of these linear colliders has a design luminosity in excess of $1 \times 10^{34}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. There are prospects of upgrading the energy of NLC/JLC significantly beyond 1 TeV using two-beam technology, which is being developed by CERN as the power source for high gradient acceleration at 30 GHz for a multi TeV linear collider. Table 3 is a summary of the parameters of these linear collider options:

The executive summary of the linear collider working group is attached as appendix M3. Detailed summaries of technical issues are described the working group summaries of the T-groups. The present state of the research and development can best be described by quoting the working group:

Table 3: Summary comparison of linear collider options.

	NLC/JLC	JLC(C-band)	TESLA	CLIC
Facility	room-	room-	super-	two-beam
Technology	temperature	temperature	conducting	acceleration
Energy range (com, TeV)	0.5–1	0.5	0.5–0.8	0.42–5
Luminosity (10^{34}cm^{-2}s^{-1})	2–3.4	0.7	3.4–5.8	$\sim$ 10
Estimated cost(*)	$\sim$ 3.5B US$	–	3.14B Euro +7000 man-years	–
Proposed schedule	CDR 2003/2004 Completion 2012	–	TDR submitted Completion 2011	–

(*) Costs are compared on equal footing. They do not include escalation, contingency, pre-ops, detector.

"The NLC/JLC-X and TESLA designs and technology are sufficiently developed and either could be used to build a 500 GeV collider. The performance limitations are well understood and the measures which must be taken to achieve the design performance at a high level of confidence are precisely defined. The R&D on the *X*-band will take another 3 to 4 years, i.e. 2004, before being ready for large-scale industrial production. Similarly, TESLA will be ready in 2 to 3 years, i.e. 2003. In both cases, final engineering R&D should be performed in the framework of a funded project."

In both cases above the additional R&D described is focused on developing the acceleration technology for the energy upgrade to 1 TeV prior to the construction of the linear collider. While the final technology for 1 TeV is not presently available with either approach, the present R&D suggests that each approach should reach its high-energy goal (0.8 TeV for TESLA and 1.0 TeV for NLC/JLC) within the time period shown above.

The largest extrapolation from SLC experience is in the luminosity, which is designed to be a factor of 10,000 higher than SLC. A factor of 100 is obtained by using many bunches thus increasing the beam power, while the remaining factor of 100 is achieved by focusing very high quality (low emittance) beams to very small size.

After production at the source these trains of high quality beams are damped to a flat aspect ratio in specially designed electron storage rings (damping rings), which use radiation damping to decrease the emittance to the desired levels. The ATF damping ring at KEK has already demonstrated emittances within a factor of 2–5 of the NLC/JLC design. The TESLA damping rings are much longer (17 km) and are substantially different in design; however, it is hoped that the extensive experience with electron storage rings combined with benchmarked simulations will prove the design feasibility.

The low emittance trains of bunches must be accelerated in the linac while keeping their tiny size. Linac beam dynamics is one of the topics that has been studied most extensively. Both the TESLA and NLC/JLC designs have addressed this issue with careful tolerance studies, specially designed accelerator structures and strategies for beam- based alignment or emittance tuning in the linac.

The final focus or beam delivery systems have been studied extensively and are very similar in all designs of linear colliders. The Final Focus Test Beam has demonstrated the required demagnification of the spot size. Component stability is critical to the very small beam size at the interaction point. The tolerances on spot size stability are similar in the TESLA and NLC/JLC designs; however, they are achieved differently because of the very different repetition rates. Both designs plan a commissioning which is consistent with achieving the design luminosity in about two years.

The linear collider research effort is perhaps the first major international accelerator research effort, and if the world high-energy physics community comes together, the linear collider might also be constructed by an international collaboration. Based on this extensive research effort, the foundation has been laid for two 0.5 TeV linear colliders designs that have upgrade capability up to about 1 TeV. The research on high gradient acceleration and two beam power sources at CERN and SLAC point towards energy upgrades or new facilities with multi TeV energy. The next generation of linear colliders could provide a beginning of the precision exploration of the TeV energy scale and might point the way towards even higher energy exploration at multi TeV energy.

The International Committee on Future Accelerators (ICFA) has commissioned the International Linear Collider Technical Review Committee (ILC-TRC) to reconvene and produce a second report. The purpose of this report is to describe and assess on a common basis the four currently viable options for a linear collider (listed in Table3). The report will establish the progress made since the ILC-TRC's first report (1995) and comment on the capability of the current options to lead to a functional project with the required design and operating parameters. If any further efforts are needed to reach these goals, the report will assess them and estimate the time required to complete them. The work of the ILC-TLC will be carried out during the remainder of 2001 and the first three quarters of 2002, with the final report ready during the fall of 2002.

Accelerator R&D for Large Hadron Colliders

Proton accelerators, and more recently hadron colliders (proton-proton, proton-antiproton, and nucleus-nucleus) have historically been the means by which particle physics has expanded the energy frontier, thereby expanding our knowledge of nature at the smallest distances and creating elementary particles of the highest mass. The Tevatron collider at Fermilab currently provides the highest energy collisions available in the world, and the recently commissioned Relativistic Heavy Ion Collider (RHIC) at BNL allows exploration of nuclear matter at extremely high density. The Large Hadron Collider, currently under construction at CERN with significant collaboration by the United States and other non-European countries, will expand the energy reach for particle physics 7-fold beyond that currently available. As the energy of hadron colliders has increased, so has the luminosity required for the experiments performed with them. The Tevatron collider has reached a peak luminosity of $2 \times 10^{31}\text{cm}^{-2}\text{s}^{-1}$, and upgrades recently implemented and others planned should raise the luminosity by more than a factor of 10 to as much as $5 \times 10^{32}\text{cm}^{-2}\text{s}^{-1}$ over the next 5 years. The LHC luminosity will be still higher, reaching $1 \times 10^{34}\text{cm}^{-2}\text{s}^{-1}$ or even higher. The success of hadron colliders and the continued increase in energy and luminosity has resulted from R&D programs that developed new technologies and techniques, including high field superconducting magnets, stochastic beam cooling, and advanced beam diagnostics and feedback systems. Continued growth to even higher energies and luminosities will be required to advance particle physics research beyond what can be learned with the colliders currently in operation or under construction, and a vigorous R&D program should ensure that this progress will continue.

The hadron collider community, led by Fermilab, recently completed a Design Study for a Very Large Hadron Collider (VLHC) [Fermilab-TM-2149, June 4, 2001], which could represent the next step beyond the LHC. Many configurations of a new energy frontier hadron collider can be considered. The Design Study addressed a promising two-stage approach. In its first stage, the machine provides a facility for energy-frontier particle physics research, at an affordable cost and on a reasonable time scale. In a second-stage upgrade in the same tunnel, the VLHC offers the possibility of reaching 100 times the collision energy of the Tevatron. Both machines would occupy a common tunnel of 233 km circumference. The Stage-1 collider would be built using inexpensive magnets of an innovative design which are excited by a 100 kA superconducting transmission line. It would provide collisions at an energy of 40 TeV, three times that of the LHC, with a luminosity comparable to the LHC. The cost without escalation and contingency is estimated to be about 4B$ plus about 10000 man-years of labor. A ten-year construction period could start in 2009, depending as much or more on how the VLHC fits into the international plan for high energy physics as on the need to complete its design and development. The Stage-2 VLHC, constructed after the scientific potential of the first stage has been fully realized, reaches a collision energy of up to 200 TeV with the installation of high-field magnets in the same tunnel and at least twice the luminosity of the Stage-1 machine. It makes optimal use of the infrastructure developed for the Stage-1 machine, using the Stage-1 accelerator itself as the injector.

The Design Study showed this staged approach to reaching very high energies to be quite promising. However, it represents only one of many possible configurations. Other possible scenarios should be studied to find the most cost-effective way to provide the high energy, high luminosity collisions required for the advancement of the understanding of elementary particle physics. Increased luminosity of $10^{35}\mathrm{cm}^{-2}\mathrm{s}^{-1}$ or even higher may be required to attain the ultimate physics potential of the VLHC.

Because the VLHC is unlikely to be built for several years, there is opportunity for further cost and performance optimization through focused R&D. A long system test of the transmission line magnet is required to demonstrate its performance, and alternate designs for inexpensive, low or medium field superconducting magnets should be explored. To achieve the 10–12 T fields for the Stage-2 VLHC requires the use of superconducting materials with higher critical magnetic field than the NbTi alloy used in existing accelerators. Nb_3Sn is currently the most promising superconductor for this application, but considerable R&D, both on the material itself and on the magnets made with it is required to master the technology and reduce the cost. In special applications, such as the final focus interaction region magnets in the Stage-2 VLHC, which are subjected to large heat loads from the collision products, even higher performance material, perhaps high temperature superconductors (HTS) may be required. Since the civil construction is a major cost driver, R&D in tunneling methodologies and technologies, done in collaboration with underground construction companies, may offer opportunities for significant cost reductions.

The VLHC design is a reasonable extrapolation from designs that already work and are well understood. However, beam conditions will be more extreme in some cases than in previous hadron colliders, and further R&D should be done to ensure the highest possible performance. For example, the performance of the low-field ring is close to being limited by beam instabilities. Methods to control or avoid these instabilities have been proposed, which appear to be reasonable. Some involve high-gain distributed feedback systems, which should be tested in existing machines. Understanding other instabilities and their cures should be addressed by a combination of detailed simulations and focused beam experiments. The luminosity of the Stage-2 machine may be limited by synchrotron radiation emitted by the proton beam, if that radiation must be absorbed by the cryogenic system. A recent idea for absorbing the synchrotron radiation with room temperature "photon stops" could eliminate this limitation and allow an order of magnitude increase in luminosity. This idea requires further development. The total energy of the beam, summed over all protons in the ring, will be at least a factor of 10 higher than in the LHC, and the collision debris power emitted from the interaction points will be almost a factor of 50 higher than in the LHC. Detailed engineering studies must be done to develop systems that can deal with these large energies.

The vigorous R&D currently under way, directed at these and other problems related to very high energy hadron beams, should be continued and strengthened. A coordinated and coherent international plan for the VLHC should be developed as part of a comprehensive and global HEP program. In addition to accelerator R&D aimed at achieving 200 TeV and luminosity of $10^{35}\text{cm}^{-2}\text{s}^{-1}$, this plan should include an internationally organized physics study to understand the opportunities of both stages of the VLHC, and a study of the detector issues that outlines the necessary detector R&D program. This will ensure that the impressive advances in our understanding of the fundamental nature of matter and energy, which have been made possible by hadron colliders, will continue to be made in the future.

Accelerator R&D for Neutrino/Muon Facilities

Very intense muon beams, and neutrino beams derived from their decay, can be produced by using multi-megawatt high-energy proton beams along with novel muon collection and beam cooling techniques. Applications for such beams range from next-generation long-baseline neutrino oscillation experiments to multi-TeV Muon Colliders. Groups in the US, Europe, and Japan are engaged in a vigorous R&D program aimed at resolving the critical design issues for both a Neutrino Factory based on a muon storage ring and a Muon Collider.

In an initial phase, the high-power proton beam would impinge on a pion production target and focusing assembly to produce a neutrino beam of unprecedented intensity (“neutrino superbeam”). In a second phase, the muons from pion decays would be collected and cooled very rapidly by “ionization cooling,” a process where energy loss by ionization in matter is alternated with reacceleration in low-frequency rf cavities. The cooled muon beam could then be accelerated and injected into a storage ring where muon decays produce a very bright, well characterized neutrino beam. Such a Neutrino Factory should allow definitive studies of the parameters of neutrino mixing and CP violation.

Vigorous accelerator R&D for about 5 more years is required to establish the technical parameters and cost of a Neutrino Factory. A Conceptual Design Report could thus be initiated in 2006. The R&D program ranges from the development of targets capable of handling 1—4 MW proton beam power to production of very high-gradient rf cavities. To complete this R&D program, it is estimated that \$15M will be needed annually, for a total of about \$100M. A demonstration of ionization cooling in a realistic setting is also planned. The first step in this process, now in progress, is to form an international cooling demonstration experiment collaboration, with a goal of starting the first experiment in 2004. Assuming the prior existence of a suitable proton driver, the total cost of a Neutrino Factory is estimated by its proponents to be \$1.6B (unloaded); this cost results from a feasibility study effort, and does not yet represent a cost-optimized or fully engineered estimate.

In a last phase, the muons could be accelerated further and brought into collision with each other at energies from a few hundred GeV to multi-TeV. This last stage should be undertaken if further physics studies, technology R&D, and experimental results establish a Muon Collider as both feasible and desirable.

Accelerator R&D for e^+e^- Circular Colliders

Circular e^+e^- colliders have had a long history in the world starting in the mid-1960s with the Stanford-Princeton e^-e^- Collider in the United States and the VEP e^+e^- collider in Russia. The tradition has been carried on with many colliders, up to the present day accelerators of 2001 with BEPC in China, CESR in the US, DAFNE in Italy, PEP-II in the US, and KEKB in Japan. CESR, PEP-II and KEK-B boast the highest luminosities in the world of 1 to $4 \times 10^{33}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. All of the above mentioned colliders have been either on the energy frontier or on the luminosity frontier. The particle physics results produced with these colliders have been great and quite varied: the discovery of a new quark, a new lepton, lifetimes of many particles, CP violation, precise mass values of particles, and the number of families of quarks, just to name a few. There are a wide variety of physics measurements remaining to do with this class of accelerator and there are many physicists with strong desires to do so.

To keep these accelerators at the frontier of particle physics, the luminosity must be continually increased. History over nearly forty years has shown a factor of twenty to twenty-five increase in luminosity every decade. There are many ideas under development that should keep this trend going. For example, there are upgrades planned for CESR to extend its energy range. There are ongoing luminosity upgrades planned for PEP-II and KEKB to achieve luminosities exceeding $10^{34}\mathrm{cm}^{-2}\mathrm{s}^{-1}$ in a few years. BEPC will likely be upgraded to the $10^{33}\mathrm{cm}^{-2}\mathrm{s}^{-1}$ level. These upgrades will be adequate for the next round of particle physics in the upcoming decade. They will cost in the range of 5 to 30 M$ for the US projects.

In about ten years, significantly more luminosity will be needed in the e^+e^- B-Factory colliders to track the expected data rates in the hadron colliders. Studies are underway to determine what is needed to get PEP-II and KEKB to a luminosity approaching $10^{35}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. These upgrades, if warranted, are more substantial and will likely require expenditures on the order of 50 M$.

Another approach is to take what has been learned in the present colliders and the previous generations to design a ultra-high luminosity B-Factory with substantially higher performance, say, in the range approaching $10^{36}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. The hardware that such an accelerator will likely need is well beyond an upgrade to an existing B-Factory but could well use most of the existing infrastructure. The studies for such a collider will take several years and the costs may be on the same order as the original collider facility.

Finally, on the energy frontier, studies have started for placing an e^+e^- collider in the proposed VLHC tunnel with beam energies of about 185 GeV. Given the long history of e^+e^- colliders, a reasonable design can be made with some confidence. How such an accelerator fits in with a linear collider that has a more extendable energy range has to be decided.

All these upgrades and proposed new facilities require significant research and development to reach their goals. A few of the major research topics are multi-ampere beam currents, megawatt x-ray loading of vacuum chambers, multi-bunch beam instabilities, interaction region designs with two different beams, and increased performance from the beam-beam interaction. The results of these studies are fully shared between world laboratories but are a necessary part of each program. The accelerator field, in general, has made great advances with these research topics over the past years and the expectation for success with the next round of improvements is very good.

Accelerator R&D for lepton-hadron colliders

Lepton-hadron colliders are ideal tools for studying QCD, which contains rich physics yet to be explored. Presently the only lepton-hadron collider is HERA, which can reach x down to 10^{-4} for significant Q^2 of more than about 10GeV^2. New facilities will be needed to explore lower values of x, e.g. to analyze the unexpected rise of parton density at small x, and to measure the structure functions of hadrons in an unexplored regime of x and Q^2. This extended knowledge of the hadron structure functions will be needed to understand the results obtained at RHIC, LHC and VLHC. Colliders of polarized electrons on polarized protons will allow the measurement of spin structure functions, and electron-heavy-ion colliders will explore regions of very high gluon density.

A number of possible lepton–hadron colliders are being considered. Their main parameters are as shown in Table 4.

Table 4: Summary comparison of lepton-hadron collider proposals.

Proposal	THERA	eRHIC	EPIC	HERAe/A	eLHC	eVLHC-b
Type	linac–ring	linac–ring/ ring-ring	linac–ring/ ring-ring	ring–ring	ring–ring	ring–ring
	e–p	e–p(e–ion)	e–p(e–ion)	e–ion	e–p	e–p
E lepton (GeV)	800	10	5	27.5	60	80
E hadron (GeV)	800	250	50	450 per u	7000	3000
Lum. (10^{32}cm^{-2}s^{-1})	0.16	15	20	0.7	3	3
Estimated cost	120 M Euro without detector	300 M$ +100M$ detector	300 M$ +100M$ detector	53 M Euro with current detectors	1000 M$ without detector	unknown without detector

Most of these facilities (with the exception of EPIC) take advantage of existing (or then existing) lepton or hadron facilities. As such, they require relatively little resources. The cases of eRHIC and HERAe/A can in principle go ahead already, in which case, the construction can be completed in a short time. In particular, once the cooler R&D has been completed successfully, construction for HERAe/A will take only 2 years. EPIC is an exception, and its proposed schedule is 2 years R&D and 5 years construction. The 3TeV VLHC booster being considered here is no longer in the present VLHC proposal but it remains a possibility.

R&D needed for lepton-hadron colliders include electron cooling for bunched beams, energy-recovery linacs, and large solid angle detectors. So far bunched beam electron cooling has never been done. For high energies, electrons will have to come from a linac, which means the electron beam has to be bunched. To reach sufficiently high intensity, the electron beam from the linac can be stored in an accumulating storage ring, or an energy-recovery linac could be used. To demonstrate the feasibility of this electron-cooling scenario, resources are needed to first perform a system test.

Energy-recovery linacs require much R&D. One key issue is the loss rate, which must be kept below the level of 10^{-6}. The beam break-up instability of the electron beam in the linac is another concern. Two-energy recovery linacs have been built, one at JAERI and the other at Jefferson Lab. The one at Jefferson Lab has obtained energy recovery at 5 mA and 50 MeV, as compared with the required specifications of up to 250 mA at 10 GeV for eRHIC. Nevertheless, the prospect of reaching the goal seems reasonable.

Detectors needed to study the small x physics require a large solid angle coverage in the forward direction. Therefore they have a strong impact on accelerator IR design, and the integration of the detector into the accelerator IR design must be taken into consideration at an early stage.

To reach the proposed short bunch intervals, the hadron beam must be clear of out-of-bunch particles to a level higher than currently achieved at HERA. R&D is therefore needed to demonstrate that the required level can be routinely achieved.

0.0.1 Accelerator R&D for Intense Proton Drivers

High intensity proton accelerators have long been essential for the production of intense secondary particle beams. Recent strong interest in very intense neutrino beams and neutrino factories requires multi-GeV proton accelerators with multi-megawatt beam power. Such high power "proton driver" facilities can benefit greatly from the development of high power proton beams for spallation neutron sources.

Based on present accelerator technology and project construction experience, it is both feasible and cost-effective to construct a 1–4 MW Proton Driver. The two proton driver design studies, one at FNAL and the other at BNL are designed for 1 MW proton beam at a cost of about US$200M (excluding contingency and overhead) and upgradeable to 4 MW. After a two-year design phase, construction would take about four years.

Even though high power proton drivers are technically feasible today, a comprehensive accelerator R&D program for proton drivers, including both linacs and rings, has been proposed, which will improve and extend the performance of high intensity proton accelerators.

Fundamental Research in Accelerator Physics and Technology

Beam physics and accelerator technology have advanced in the past propelled by the push to higher center of mass energy and higher luminosity. This process has led to the substantial advances discussed in the introduction to this document, and continues through the evolution of our ideas and technology to higher energy and luminosity primarily directed towards the next generation of particle accelerators. These efforts have been the focus of many working groups at Snowmass and have mostly been discussed in the previous sections of this document.

One key to the past success of particle accelerators has been the development of the theory of dynamics of beams of particles under the influence of external and self-induced forces. Single-particle dynamics, which began its rapid progress with the theory of strong focusing, now includes complicated nonlinear effects and is better understood thanks to the use of modern map, symplectic integrator, and Lie algebraic methods. Even so, there continue to be new developments, such as the use of beam rounders and flatteners and further work will be required in the area of nonlinear effects on long-term orbit stability.

As machines of higher intensity and larger size have been developed, new multi-particle effects and instabilities have been encountered. This pattern is expected to continue with the next generation of accelerators, and a deeper understanding of multi-particle behavior will be essential. This understanding will require the extensive use of terascale (large-scale massively-parallel) computers executing new (often yet to be discovered) algorithms as well as new theoretical formulations.

While the developments just described are essential, they are far from sufficient to continue to extend the energy reach of accelerators. In the past, growth in output energy has been sustained by the development of extensions to present technology and the concurrent investigation and invention of new technologies. To make significant future impact, new ideas are needed not only to accelerate but also to generate, focus, and manipulate charged particles.

Fortunately there are many possibilities to do just that. Over the last fifteen years a small but vigorous advanced accelerator community has been engaged in finding alternatives to radio frequency acceleration methods. These researchers have proposed and demonstrated new ways of accelerating, bunching, and phasing particles. Some have demonstrated the use of laser radiation instead of microwaves to power plasma structures that can sustain accelerating gradients orders of magnitude greater than those in a RF linear accelerator. Other researchers have shown that electron and positron beams from a conventional accelerator can power plasma structures with promising results for developing new types of lenses for future machines and magnetless wigglers for next generation of light sources. This exciting new work is described briefly in the summary of the working group on Advanced Accelerator Techniques (T8) and is actively pursued by many small groups in universities or national laboratories.

The Advanced Accelerator R&D effort is poised to leap to the next stage. The initial rounds of experiments demonstrating a factor of 10-100 more accelerating gradient have been done. A new generation of tightly bunched, high quality beam sources is under active investigation. However, it is clear that this field needs scientists and resources if it is to fulfill its promise. It is time to embark on larger scale collaborations, which can leverage the intellectual contributions of the university groups and the infrastructure of the laboratories. These larger collaborations can address issues that require a significant investment both in the experimental design and execution. Large laboratories possess the infrastructure to provide high quality, stable beams that are critical for the next round of experiments. This is an outstanding research opportunity, as discussed earlier in this report, especially for physicists that expect to perform experiments at accelerator facilities in the future.

As we push the limits of acceleration to achieve high energy and the limits of beam quality to achieve high luminosity, we must carefully study fundamental limits and processes that are uncovered. The transition from metallic structures to plasma acceleration is a large jump and will necessarily involve a deeper understanding of instabilities that might appear. Higher quality beams might begin to approach fundamental limits that have to be explored. Intense beams interacting with each other push beyond our experience with strong field electrodynamics. However, the key to this progress is to build a substantial experimental foundation, which could form the basis for a new generation of particle accelerators.

Chapter 1

Snowmass Working Group M1 Muon Based Accelerators

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1.1 Introduction

A worldwide effort is under way to elucidate the unique particle physics opportunities presented by intense muon beams and the neutrino beams derived from their decay. Groups in the US, Europe, and Japan are engaged in a vigorous R&D program aimed at resolving the critical machine and beam design issues for both a Neutrino Factory based on a muon storage ring and a Muon Collider. To make progress in a time frame compatible with the needs of the physics program requires adequate R&D support; for the US program this is about \$15M per year.

1.2 Physics Motivation and Staging Scenario

Recent experimental results have confirmed neutrino oscillations as the only established example of physics beyond the standard model. The most effective way to fully explore the new physics is to construct a Neutrino Factory—–this is our goal. However, to start immediately on the physics program, and to permit progress with a lower peak funding requirement, the neutrino physics community has developed a staged construction scenario that progresses rapidly toward a better understanding of neutrino oscillations, Higgs physics, and, ultimately, multi-TeV physics. The stages are:

1. a neutrino "superbeam" from a high-intensity (1—4 MW) proton driver;
2. a low-emittance, low-energy-spread muon beam at 200 MeV/c;
3. a roughly 3 GeV muon beam;
4. a Neutrino Factory based on a 20—50 GeV muon storage ring; and finally
5. a Muon Collider operating as a Higgs Factory or at higher energy.

The US Neutrino Factory and Muon Collider Collaboration R&D activities support this program.

With a modest investment of resources, the first stage of this scenario could begin quickly (within 2—3 years) at either BNL or Fermilab. We believe that this initial stage should be included as a high priority item in the near-term plans of the community, as it will advance high-intensity meson and muon studies as well as neutrino physics. Later stages of the scenario will further advance a variety of muon studies sensitive to new physics at high mass scales. Depending on the outcome of upcoming neutrino experiments (especially MiniBooNE and KamLAND), the subsequent upgrade of the facility into a Neutrino Factory could allow definitive studies of the parameters of neutrino mixing and CP violation; the physics case for this stage should solidify within the next few years. Stage 5 should be undertaken if further physics studies, technology R&D, and experimental results establish a Muon Collider as both feasible and desirable. Our vision is that the above scenario will ultimately be adopted and carried out by a national laboratory or international collaboration.

1.3 Accelerator Physics and Technology Issues

For a Neutrino Factory, key accelerator physics and technology issues include the development of: targets capable of handling a proton beam power of 1-–4 MW; radiation-resistant solenoids or focusing horns; cost-effective longitudinal manipulation and ionization cooling techniques for reducing transverse emittance; and rapid and efficient acceleration techniques that accommodate large longitudinal and transverse beam emittances. Validating the design parameters arising from Feasibility Studies I and II (sponsored by Fermilab and BNL, respectively) involves testing of high-field, large-bore solenoids, high-gradient rf cavities (both normal-conducting, NC, and superconducting, SC), high-power LH_2 energy absorbers, induction linac units, and beam diagnostics devices. Good progress is being made in all areas. Continued development of sophisticated simulation tools to evaluate system performance and analytical theories to guide the design effort are crucial items in our work.

For a Muon Collider, the same issues are relevant, but requirements are more severe. Emittance reduction must include longitudinal cooling ("emittance exchange") and demands 6D cooling several orders of magnitude beyond that needed for a Neutrino Factory. Current Muon Collider scenarios require beams of μ^+ and μ^- with single-bunch intensities of $\approx 10^{13}$ muons, leading to potential space-charge effects. Additional technologies required for a Muon Collider may include: a ring cooler, a helical wiggler, or a bent-solenoid channel for longitudinal cooling; wedge-shaped absorbers; lithium lenses; and higher-frequency rf cavities for the later stages of cooling as well as for acceleration. The collider ring requires a low β^*, although not beyond today's achievements, a nearly isochronous lattice, and an interaction region design that minimizes backgrounds from muon decay products. Progress is being made, but significant R&D will be needed to reduce these challenges to engineering problems.

1.4 Evolution from Neutrino Factory to Muon Collider

The technically simpler Neutrino Factory is a step toward a Muon Collider. Many of the difficult technical aspects of the collider would be addressed in constructing a Neutrino Factory. Whether a Neutrino Factory can, or should, be converted to a Muon Collider is presently under study. Clearly, cost savings would result if Neutrino Factory components could be reused for the collider.

1.5 R&D Time Scale and Risks

A detailed R&D plan for the Neutrino Factory has been developed. With adequate funding ($15M per year), the technical work needed to begin a CDR requires about 5 more years; with less funding the program would take longer. We are confident that this program will be successful. The required solenoids are within today's capabilities. NC rf cavity gradient parameters are aggressive and must be demonstrated, but other pulsed rf systems have worked in a similar parameter regime. The same can be said of the LH_2 absorbers, at least at the 1-MW intensity level. The SC rf cavity parameters for the acceleration section are likewise aggressive and must be tested. However, there is every reason to expect that a suitable technical solution can be found.

The time scale for the Muon Collider R&D program is less certain; it depends on developing practical techniques for longitudinal cooling and for more transverse cooling than is needed for a Neutrino Factory. An assessment of the time scale awaits R&D activities that will happen over the next several years.

1.6 International Activities

We are in close contact with groups in Europe and Japan working on alternative approaches to intense muon beams. At the present level of understanding, all approaches look viable, and more detailed studies and cost estimates will be required to identify the best approach. Because the Japanese FFAG scheme does not lend itself easily to cooling, it may advance only some of the technologies needed for a Muon Collider. The time scale for a European Neutrino Factory is comparable to what we envision in the US. As is the case in the US, the European R&D program is resource limited. They do not expect to be ready for project approval until after the LHC financial commitment ends. The Japanese proton driver is approved and will be ready in 2007; it is not known when the beam will become available for neutrino production. A start on a Japanese Neutrino Factory is hoped for in the same time frame as for the other regions. To our knowledge, neither Europe nor Japan is currently contemplating a Muon Collider.

1.7 Cooling Experiment

Though the physics of the cooling process is well understood, and we have done detailed simulations of the process with several independent computer codes, it is prudent to demonstrate cooling, and the required component performance, in a realistic setting. We plan an international cooling demonstration experiment involving our colleagues in Europe and Japan. As presently envisioned, the initial phase would cost in the neighborhood of $10—20M, to be shared among the three regions. We hope to begin taking data in 2004 if funding is made available.

1.8 Muon Collider Performance

As discussed above, the technical and physics performance of the Muon Collider cannot yet be quantified. Continued R&D support over the next few years will permit doing so.

Chapter 2

Snowmass Working Group M2 Electron-Positron Circular Colliders

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2.1 Overview

The status, future plans, and research issues for the existing and future e^+e^- circular colliders were discussed. The operational or recently operating colliders studied were BEPC, CESR, DAFNE, KEKB, LEP, PEP-II, VEPP-2M, and VEPP-4. Upgrade plans for CESR-c, PEP-II, and KEK-B were presented. The future circular colliders studied were BEPC-II, PEP-N, Super-B-Factory (SBF), VEPP-2000, VEPP-5, and VLLC. These colliders cover a center of mass energy range of 1 to 370 GeV and a luminosity range of 10^{31} to $10^{36}\mathrm{cm}^{-2}\mathrm{s}^{-1}$.

2.2 Outlook

The working group felt that the HEP community should give strong support to the operation and proposed upgrades of these medium energy colliders as these accelerators are a very necessary and healthy component of the full landscape of high energy physics.

2.3 Present Colliders and Upgrades

The present colliders deliver data to their respective detectors at an unprecedented rate. The B-factories have reached luminosities of 3 to $4 \times 10^{33}\text{cm}^{-2}\text{s}^{-1}$ and deliver integrated luminosity at rates in excess of $4fb^{-1}$/month. The recent rapid turn-on of the two B-Factories, PEP-II and KEKB, has shown that modern accelerator physics, design and engineering can produce colliders that rapidly reach their design luminosities and deliver integrated luminosities capable of frontier particle physics discoveries.

The present colliders are planning upgrade programs to extend their data production capabilities. PEP-II and KEK-B with ongoing upgrade programs should reach luminosities of a few times $10^{34}\text{cm}^{-2}\text{s}^{-1}$ in a few years. More aggressive plans may follow allowing luminosities of order $10^{35}\text{cm}^{-2}\text{s}^{-1}$ by the end of the decade. Plans are in place at CESR to extend the energy range of the collider to $1.5\text{GeV} < E < 5.6\text{GeV}$ with the installation of damping wigglers (CESR-c). Luminosities of 2 to $4 \times 10^{32}\text{cm}^{-2}\text{s}^{-1}$ should be achievable at the lowest energies and 1 to $2 \times 10^{33}\text{cm}^{-2}\text{s}^{-1}$ at the highest. Upgrade experience at CESR over the last two decades demonstrates that steady upgrades to existing colliders are extremely cost-effective and productive.

2.4 New Colliders

The demonstrated success of e^+e^- factories over the last several years provides confidence that higher luminosities can be achieved in several energy regimes, which are now demanded by the need for precision measurements in particle physics. The proposed new colliders are designed to cover energy ranges where additional data is needed and to explore the energy frontier. Two machines, VEPP-2000 (under construction) and PEP-N (under consideration), will provide precision R measurements in the energy range $1 < E_{\text{cm}} < 3\text{GeV}$. These machines are inexpensive and complement well the ongoing programs.

With the success of the present B-Factories, ideas for a future very high luminosity B-Factory or Super-B-Factory (SBF) are under consideration. In the likelihood that B-TeV and LHC-B will be in operation by the end of the decade, a B-physics program based at an e^+e^- collider would very likely require a luminosity approaching $10^{36}\text{cm}^{-2}\text{s}^{-1}$ to be competitive. This performance level would require improvements significantly beyond planned upgrades of present facilities. Recent studies provide support that such a collider could be built.

VLLC is a proposed energy frontier collider (up to $E_{\text{cm}} = 370\text{GeV}$) to be located in the VLHC tunnel. This machine is in the early stages of consideration and many design issues remain. Several of the questions are: Are one or two rings needed? What is the injection energy and injection system? Is polarization required and achievable? What is the energy range of the main ring? Is an e–p option desired? The rational and timing for the VLLC must take into account the overall planning of an e^+e^- linear collider.

2.5 Connections to other facilities

Research and development for high luminosity e^+e^- colliders has direct applicability to other frontier accelerators including linear colliders, synchrotron light facilities, and FELs. Some of the common accelerator physics issues are wiggler-dominated rings, beam dynamics of bunch trains, multi-bunch feedback systems, and interaction region designs.

2.6 Suggested collider research needing strong community support

2.6.1 Interaction region design

The upgrades of many existing colliders and all future colliders require improvements in the design and operation of the interaction region (IR) for both the accelerator and the detector.

Nearly all future IRs require reduced beta functions forcing the interaction region quadrupoles to be moved closer to the collision point. Chromaticity, beam separation, and detector backgrounds are concerns. The recent invention of small cross section superconducting quadrupoles for a HERA upgrade has provided new possibilities for low beta interaction regions for e^+e^- colliders.

All present colliders use vertically flat beams at the interaction point. Round beams at the collision point may allow higher beam–beam tune shift limits and a higher luminosity but, perhaps, with increased backgrounds. For example, a Super-B-Factory may need round beams. CESR-C will test round beam operation in the next year.

Two beam separation issues in the IR include crossing angles, parasitic crossings, lost particle and synchrotron radiation backgrounds, low beta functions, and HOM power generation in the separation "crotches." More work is needed in this area. Research is especially needed to enable evaluation of the generated HOM power and optimization of the vacuum hardware for HOM reduction.

There is a desire to reduce the radius of the interaction region Be chamber from the present 2–2.5 cm radius towards 1 cm to improve particle tracking. Beam heating and detector backgrounds may become significantly worse and further research is needed.

2.6.2 Beam–beam interaction

The beam–beam interaction ultimately sets the luminosity limit in e^+e^- circular colliders. Many methods are used to increase the limit or reduce its effects. For example, reduced beta functions at the collision point allow more beam current with the same tune shifts.

The beam–beam issues with round beams require further experimental and theoretical work as the potential luminosity gain is substantial.

Increasing the basic beam-beam tune shift limits in e^+e^- circular colliders involves careful orbit, coupling, and dispersion control. The empirical optimization of these lattice conditions should be better understood and systematized, if at all possible.

Several new colliders will be reworked to operate at lower than the design energy and will use very strong wiggler magnets to increase radiation damping and the beam-beam tune shifts. These wigglers add strong lattice nonlinearities to the accelerator. Understanding the effects of these wigglers for colliders (as well as linear collider damping rings) is needed.

2.6.3 Very high current beams

For future high luminosity B-Factories the beam currents must be increased by up to an order of magnitude. These high currents will require many additional RF cavities resulting in higher impedance and stronger instabilities. The longitudinal and transverse feedback systems will likely need substantial improvements. The combination of the energy storage cavities and longitudinal feedback needs study.

Stress fatigue of vacuum chambers from high current temperature cycling is now an important factor in B-Factories and future studies will lead to improvements in vacuum chamber design.

2.6.4 Accelerator physics

Several new accelerator physics issues affect ring operation and need study. The electron cloud instability (ECI) can enlarge a positron beam and reduce the luminosity along the bunch train. Vacuum chamber solenoids are only a partial cure for ECI. Thus, additional cures must be investigated.

High current B-Factories will enter a regime where the Touschek effect significantly reduces the beam lifetime and, perhaps, Intra-Beam Scattering (IBS) will enlarge the transverse and longitudinal beam sizes. Measurements of IBS and calculations do not always agree, suggesting the need for theoretical improvement. In particular, calculations which take into account x–y coupling and transverse enlargement are needed.

Finally, further advances in bunch-by-bunch instrumentation are required to enable understanding of the underlying limitations to machine performance.

Chapter 3

Snowmass Working Group M3 Linear Colliders

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3.1 Introduction

The M3 Snowmass Working Group on Linear Colliders (LC) consisted of roughly 40 people who met during the three weeks of the Snowmass2001 meeting. The working group examined many of the fundamental issues regarding the design of these facilities including the rf systems necessary to attain the desired beam energy, and the luminosity performance that might be expected. In the following, the primary issues will be reviewed and then some suggestions for R&D to be completed before construction are listed. Finally, it should be noted that many of these issues were covered in more depth in the T1 (Interaction Region), T2 (Magnet design), T3 (Rf systems), T4 (Particle sources), T5 (Beam dynamics), T6 (Environmental Control and stability), and T9 (Instrumentation) working groups and further discussion can be found there.

The center-of-mass (cms) energy at a next generation LC is 10 times higher than that achieved in the Stanford Linear Collider (SLC) and the luminosity 10,000 times higher than that attained by the SLC. The working group primarily discussed the NLC/JLC X-band designs, which are based on normal conducting rf at 11.4 GHz, the TESLA design which is based on superconducting cavities operating at 1.3 GHz, and, briefly, the JLC C-band 5.7 GHz option. These designs aim for an initial energy of 500 GeV in the cms and have upgrade paths to energies of roughly 1 TeV. The group also considered many of the issues relevant to higher energy LC concepts, including the two-beam CLIC design, which is based on normal conducting 30 GHz rf and a relativistic drive beam as the rf power source.

The NLC/JLC-X and TESLA designs and technology are sufficiently developed and either could be used to build a 500 GeV collider. The performance limitations are well understood and the measures which must be taken to achieve the design performance at a high level of confidence are precisely defined. The R&D on the X-band will take another 3 to 4 years, i.e. 2004, before being ready for large scale industrial production. Similarly, TESLA will be ready in 2 to 3 years, i.e. 2003. In both cases, final engineering R&D should be performed in the framework of a funded project.

3.2 Beam Energy and Rf Systems

The rf components (modulators, klystrons, rf distribution, and accelerator cavities) of the NLC/JLC-X and the TESLA LC have been developed over the last decade. Integrated systems with prototype components have been in operation since 1997 at the NLC Test Accelerator at SLAC and the TESLA Test Facility at DESY. These test facilities have accelerated beams with loaded gradients of 40 MV/m and 23 MV/m, respectively, and with parameters (acc. gradient, beam intensity, pulse length and energy stability) sufficient for a 500 GeV linear collider. The ongoing R&D programs, described below, aim at the higher loaded gradients, 55 MV/m and 35 MV/m, required for 1 TeV/800 GeV operation, and at an optimization of rf systems with respect to cost and power efficiency.

Linear colliders with cms energy above 1 TeV are primarily envisaged using high-gradient, high-frequency acceleration with the rf energy supplied by an auxiliary drive beam (Two-Beam-Acceleration). The CLIC R&D program (30 GHz) presented designs which extend from 0.5 TeV up to 5 TeV, with the primary emphasis on the 3 TeV design. Energy upgrades to the NLC/JLC using high-gradient X-Band acceleration (1.7 TeV) and 22.8 GHz acceleration (2.5 TeV) were also presented, with both designs using by Two-Beam Acceleration.

3.3 Luminosity

All of the key topics relevant to the luminosity performance of the colliders were discussed:

1. sources and damping rings (DRs),
2. linacs,
3. beam delivery systems (BDS),
4. stabilization and ground motion issues, and
5. operational issues such as commissioning and machine protection strategies.

3.3.1 Sources and Damping rings

The particle sources for the NLC/JLC design are based on extrapolations from the SLC. The TESLA electron source is similar and the positron source uses photons produced by the high energy electron beam in a wiggler. Although the requirements for the beam quality in the damping rings (emittance, energy spread) are similar, the NLC/JLC and TESLA designs are significantly different because of the different bunch train structure. The 300-m circumference rings for the X-band LC are moderate extrapolations of the ATF ring at KEK and currently operating synchrotron radiation facilities. The TESLA damping ring is much larger (17 km) to store all of the 3000 bunches in the train.

In both cases, one of the most difficult challenges will be achieving and maintaining the very small vertical emittances. The ATF ring has demonstrated emittances within a factor of 2–5 of the DR design. Many other operating rings have achieved similar emittance ratios (0.1–0.5%) but not the same absolute emittance. Another issue that distinguishes the damping rings from presently operating rings is the large damping decrement provided by special wiggler magnets. The NLC/JLC ring has 45-m of high field wiggler, similar to ATF, while the TESLA ring has 400-m. Issues regarding the wiggler non-linearity will be addressed by simulations and by experiments at ATF and at the planned CESR-c.

Finally, the ring designs will have to address several collective effects that could be detrimental, including intra-beam scattering, space charge forces, ions, electron cloud and wakefields. It is felt that most individual collective effects can be well described in simulation. Experience with the current generation of high luminosity factories provides confidence in these beam simulations.

3.3.2 Linac Beam Dynamics

The linac beam dynamics is one of the topics that has been studied most extensively. In the normal and superconducting designs, it is important to damp the higher-order modes (HOM) of the cavities to prevent multi-bunch beam breakup instability. At this time, both the TESLA and NLC/JLC projects have demonstrated control of these HOM sufficient to prevent multi-bunch beam breakup. In the normal conducting design, four damped detuned cavities have been measured. In all cases, the transverse wakefield was decreased to the required level; however, in each case, identified construction errors prevented meeting the ideal values. In the TESLA superconducting cavities, the HOM are damped with external loads. One important mode was not adequately damped with this system and a slight modification of the HOM couplers has been proposed. Very high frequency modes must be absorbed by suitable material that will be inserted into the beam pipe between the cryo-modules to avoid additional heat load into the Helium at 2K. In both the NLC/JLC and TESLA, these solutions have been applied to prototype components and there is confidence that these methods can be implemented successfully in the final designs.

Another issue for single bunch dynamics is the component alignment. In the normal conducting designs the typical alignment is 2 to 10μm. To attain these values, beam-based alignment (BBA) techniques must be used and to this end measurement and position controllers are included on all components. These techniques and technologies have been studied and used at the SLC, the Final Focus Test Beam and the ASSET facilities at SLAC. Experiments at the FFTB demonstrated the ability to align components to within a factor of 4 of the NLC/JLC specifications. Improvements in instrumentation, optimization of the optics design for implementation of BBA, and application of other demonstrated techniques assure the needed alignment capability. In the TESLA design, the individual component alignment tolerances are between 100 and 500μm. These alignment tolerances have been obtained within 8-cavity cryo-modules. The systematic (correlated) alignment tolerance on the cryo-modules is tighter than for individual cavities, ranging between 100 and 40μm. If this tolerance is not met during installation, the increased emittance dilution can be mitigated using the techniques developed for the NLC/JLC such as emittance correction orbit bumps.

3.3.3 Beam Delivery Systems

The beam delivery systems of all the designs have very similar requirements. The discussions covered optics designs, spot size tuning, stabilization and jitter issues, beam collimation, and beam-beam effects. In general, the optics designs are far advanced and a number of recent improvements are applicable to all of the designs. The tolerances on collision stability and spot size dilution are comparable in all of the designs but are achieved differently because of the different repetition rates. In the TESLA design, the collision jitter can be effectively removed using the intra-train feedback. However, the tolerances on the spot size increase can be exceeded at a noisy site, in which case active stabilization of some of the components is also required. In the NLC/JLC design, the beam jitter must be stabilized by choosing a sufficiently quiet site and by adding additional active stabilization to magnets which do not meet the tolerances. A very high frequency intra-train feedback might also ease these jitter tolerances. Regarding spot size stabilization, the higher beam pulse rate for NLC/JLC (and CLIC) is advantageous due to the decrease of ground motion amplitude with frequency.

Two primary beam–beam issues were considered: high disruption effects and high-energy limitations. In the high disruption regime, the luminosity becomes sensitive to the single bunch kink instability. As the disruption parameter increases, there is a rapid luminosity decrease due to beam offsets and correlated emittance dilutions. With the present TESLA beam parameters, the sensitivity to a correlated emittance dilution of 1% leads to a $\sim$ 30% decrease in luminosity, about half of which is recovered by the IP feedback. In the NLC design, the disruption is half as large and preliminary calculations indicate that this reduces the sensitivity significantly. If this sensitivity to disruption is confirmed, the TESLA design parameters can be adjusted to decrease the disruption at the expense of higher beamstrahlung energy loss. At higher energy, multi-TeV designs where $Y >> 1$, a 20 mrad IP crossing angle is required.

3.3.4 Operational Issues

Three operational issues were discussed: machine protection, commissioning strategies, and the complexity of the beam-based alignment procedure. A fundamental difficulty for each design is that a single errant bunch with the nominal charge density is capable of damaging whatever it strikes. A fully integrated machine protection system is required to deal with the high potential for damage. This also imposes constraints on the speed with which full luminosity can be achieved because an extensive period of running with reduced charge densities and pulse repetition rates will be necessary. This period provides time to safely align and test the machine components, ensure that all control and protection systems are operating properly, and to validate real-time operating procedures such as the beam-based alignment. At present it is felt that these operational issues are consistent with a 2-year ramp up to full luminosity. Such a schedule assumes that initial beam operations of some subsystems can commence before the formal end of construction; this capability is specifically included in both designs.

R&D Programs The R&D required before construction can be divided into two areas: that on the rf systems and that to ensure the luminosity performance. For the X-band system at present, much attention is given to the damage in accelerator structures arising from the rf breakdown which occurred at gradients below the 1 TeV NLC/JLC design value. Recent tests with shorter structures and lower group velocities have reached the unloaded design gradient of 70MV/m but have not yet shown sufficient overhead to assure the 1 TeV specifications. Should these tests, continuing through 2001, be concluded successfully, a structure with low group velocity and sufficiently small short-range wakefields (larger iris, higher phase advance) will be tested in early 2002. Following this, it is expected that a final version of structure with full control of the short- and long-range wakefields, suitable for the NLC/JLC linac, will be available by beginning of 2003. In parallel, the rf power sources development will be completed. The NLC collaboration is aiming at a full test of the 1 TeV rf system, including the modulator, klystrons, DLDS pulse compression system, and high gradient structures, to be performed by the end of 2003.

For TESLA, the main R&D focus is on higher gradients. Gradients up to 43MV/m have been obtained in single-cell resonators and 33 MV/m has been achieved in a standard 9-cell cavity by applying an electro-polishing treatment to the Niobium surface. The reproducibility of these results must be proven in the integrated system test to ensure the upgrade capability of TESLA to the foreseen cms energy of 800 GeV at a gradient of 35MV/m. A second issue is the test of the superstructure concept where pairs of 9-cell cavities are powered by one coupler. This increases the linac packing factor by 6% and saves cost by reducing the number of rf-couplers by 50%. These R&D programs should have conclusive results by the end of 2003. In parallel, operation of the TTF linac with parameters close to those of the 500 TeV TESLA design is planned to provide additional operational experience over extended periods of time.

After first successful demonstration of the two-beam concept at CTF-2, the R&D program for CLIC will focus on the rf breakdown problem at high gradients and on the construction of a drive beam generation prototype in the framework of the CTF-3. CTF-3 is aimed at two-beam acceleration at a gradient of 150 MV/m and its construction is to be completed by 2005.

There are a number of R&D items that must be directed at the luminosity performance. First, continued studies at the KEK ATF and other existing rings are necessary to understand all of the beam dynamics issues in the damping rings. In parallel, simulation studies must be performed to address many of the collective effects that may limit the ring performance. These studies are needed for both the NLC/JLC rings as well as the less conventional TESLA damping rings. Second, continued R&D is needed to complete the ground motion and vibration studies and to accurately model the stability of the beam optics systems with all of the tuning and beam-based alignment techniques. In addition, more detailed models of the commissioning and the machine protection strategies are clearly needed. Finally, the feedback systems, the active stabilization techniques, and the diagnostic development must be continued. The LINX interaction region facility at SLAC, which will use the modified SLC final focus to perform engineering studies in the interaction region, could test these technologies and techniques.

Chapter 4

Snowmass Working Group M4 Hadron Colliders

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4.1 Luminosity and energy scaling

The VLHC Design Study examines one point in a parameter space rich in technologically possible VLHCs, in an extrapolation from designs that already work and are well understood. It studies a "2 stage" scenario in which two colliders occupy a single 233 km circumference tunnel, with beam energies of 20 TeV and 87.5 TeV, and with dipole fields of approximately 2T and 10T. Because the VLHC is unlikely to be built for several years, there is ample opportunity for further cost and performance optimization, through a focused research and development plan.

The low field ring performance is close to being limited by collective instabilities. In the high field ring the product of luminosity and energy has a maximum value, limited by the total synchrotron power that can be deposited in the cryogenic system. Beyond the Design Study, it is expected that most of the stage 2 ring synchrotron radiation can be absorbed in room temperature "photon stops." This is a very exciting development, because it breaks the nominal total synchrotron radiation power constraint, and potentially allows an order of magnitude increase in the stage 2 luminosity. With or without photon stops, 75% or 80% of the protons are "burnt off" in a typical store, so that the peak luminosity scales linearly with the number of protons stored, and therefore also linearly with the stored beam energy, and with the collision debris power at each interaction point (IP). The Design Study stage 2 luminosity of $2 \times 10^{34} \mathrm{cm}^{-2}\mathrm{s}^{-1}$ corresponds to a stored energy of 3.9 GJ and a collision debris power of 73 kW per IP, values that are well beyond current experience and LHC parameters. When photon stops are assumed, the high field ring luminosity is (softly) limited by the ability to engineer beam dumps, and interaction regions magnets. Photon stops, beam dumps, and energy deposition resistant interaction region magnets are all topics which need further research and development.

The VLHC tunnel geometry is compatible with an unpolarized Very Large Lepton Collider (VLLC). (In contrast, the tunnel is not compatible with the Very Large Muon Collider.) This enables e+e- collisions with a nominal center of mass energy of about 400 GeV, a luminosity of about $10^{34}\mathrm{cm}^{-2}\mathrm{s}^{-1}$, and good energy resolution. Electron-proton collisions are also possible. The VLLC luminosity and energy scale with circumference like $L \sim C$ and $E \sim C^{1/3}$.

VLHC design study and alternative approaches. The beam energies can be scaled around the Design Study by 50% simply by changing the tunnel circumference. Roughly 2/3 of the project costs scale linearly with the beam energy. A higher energy stage 1 may be crucial in placing its physics program sufficiently far beyond the LHC to assure a vigorous research program. While the 2 stage concept seems the most reasonable to provide a multidecade program of energy frontier physics, single stage scenarios should also continue to be studied, as parameters and costs are further optimized. Therefore, the vicinity of at least two other points in parameter space, cases A and B (below), also deserve further study.

4.1.1 Case A (2 stage): 3T, 50 TeV center of mass; 10–13T, 150–200 TeV center of mass.

The cold-iron super-ferric 3T stage 1 collider makes it possible to reduce the overall circumference while increasing the collision energy. The cold bore significantly reduces the resistive wall impedance, possibly allowing significantly higher luminosities. A large dynamic range ($\sim 30:1$) would allow a beam energy as high as 30 TeV, still using the Tevatron as injector. The stage 2 collider uses 13T Nb_3Sn dipoles to produce 200 TeV collision energies. This is a very aggressive goal; however, even a 10T field would provide a collision energy of 150 TeV.

4.1.2 Case B (1 stage): 5 TeV injector; 150–200 TeV center of mass collider.

A 5 TeV injector is built in a new 15 km circumference tunnel on the Fermilab site, using 11T fast cycling magnets. (This field and the injector energy could be somewhat lower.) The injector tunnel could also house a double ring polarized Giga-Z e^+e^- collider, supporting high luminosities at the Z pole. The case B collider is identical to the case A stage 2 ring, but with a dynamic energy range of $\sim 20:1$, not $\sim 4:1$.

HERA and the LHC face difficulties with dynamic ranges of 20 : 1 and 16 : 1, respectively, due to persistent current and snap back effects at injection and at the beginning of the energy ramp. Exciting new superconducting magnet design developments suggest that it is possible to suppress these effects, with a very positive impact on VLHC design if dynamic energy ranges greater than 20 : 1 become feasible. Research and development into persistent current and snap back suppression should be encouraged.

4.2 Superbunches.

The Superbunch concept from KEK uses induction acceleration modules with an average gradient of 25 kV/m to create very long bunches bounded by “barrier buckets,” in a high current, high luminosity scenario. This is potentially very interesting for stage 1, but may be inappropriate for stage 2, due to the very high synchrotron radiation load. The superbunch idea needs to be tested experimentally, for example in the KEK 12 GeV proton synchrotron. Although the experiments prefer a conventional bunched beam structure, superbunches are acceptable. Superbunch collective effects and the potential of stochastic cooling both need further study.

4.3 Lattice design.

If photon stops are used, the ultimate VLHC performance is limited by challenges associated with the beam dump, and with energy deposition in the interaction region magnets. Optical solutions have been found for the high field abort, and for the seamless transition from triplet (round beam) optics to doublet (flat beam) optics. More research and development is needed, integrating these optical solutions with technically feasible components, such as energy deposition resistant interaction region magnets.

4.4 Accelerator Physics.

The VLHC physical beam sizes are so small—especially in stage 2—that discussing the dynamic aperture (in units of the beam size) is less relevant. New paradigms, such as the operational aperture required during the energy ramp, need exploration and development. For example, the closed orbit must be held constant to 0.1 mm in the stage 1 resistive wall feedback pick ups, and perhaps to 1 mm accuracy near the stage 2 photon stops. Operational issues (such as beam based “single particle” feedback on closed orbits, tunes, and chromaticities) need thorough investigation, in order to relax component tolerances such as magnet field quality, and to enable rapid commissioning. Recent and continuing developments in beam instrumentation and diagnostics need to be incorporated in the VLHC design, in order to get a better machine at less total cost. Particle tracking studies, and energy deposition simulations, need to be performed.

4.5 Collective effects.

Both magnet costs and beam impedances are strong functions of the beam pipe aperture. Close attention must therefore be paid to collective effects, when optimizing cost and performance. The large circumference and small aperture of the VLHC serve to increase the transverse impedance, and to focus attention on the Transverse Mode-Coupling Instability (TMCI), Resistive Wall (RW) instabilities, and Laslett space charge tune shifts.

4.5.1 TMCI

The nominal stage 1 single bunch intensity is 50% higher than the TMCI instability threshold. This can be overcome by bunch coalescing techniques. Electron Cloud simulations indicate that neither the heat load nor the e–p instability growth rate appear to be a problem. However, simulations also suggest that the electron cloud can enhance the TMCI by a large factor. This research need to be continued.

4.5.2 RW

The skin depth of the lowest RW mode is much smaller than the stage 1 warm beam pipe thickness, resulting in an instability growth time of less than one turn. Several "trailing bunch" feedback systems are therefore required, with the potential for slow emittance growth (although calculations predict otherwise). Additional feedback simulations, and beam demonstrations, would improve the design of these novel stage 1 systems. Resistive wall effects are effectively suppressed in stage 2 by including a 0.5 mm thick copper layer on the cold beam pipe liner.

Laslett space charge tune shifts: These tune shifts are strong while beam is being accumulated in the stage 1 ring. Amelioration techniques need further study.

4.6 Beam experiments.

Well prepared beam experiments investigating both fundamental physics and also new technologies will also help in designing a less expensive and better VLHC. Since beam time is a precious resource, it is necessary for such experiments to be clearly motivated by vital VLHC issues, and for these motivations to be clearly communicated to accelerator staff and management at the hadron colliders where such time would be requested. Assuming that the community endorses beam experiments motivated by the VLHC design effort, then a 3 to 5 year program based should be formally organized.

Collaborative beam experiments provide a natural context in which to make a prototype test of the Remote Operations aspect of a Global Accelerator Network. There are 6 main beam experimental areas: 1) feedback systems to damp the resistive wall instability, 2) control of orbits, tunes, and chromaticity, 3) superbunch demonstration, 4) slow diffusion, 5) long range beam-beam compensation, and 6) beam-vacuum interactions. The first 3 topics have already been discussed, above.

Slow diffusion The operational scenario for the Design Study high field ring assumes that the beam emittances decrease by an order of magnitude or more from their injection values (typical of current colliders), with a damping time of order 2.5 hours (107 turns). Our present understanding of the catalog of slow diffusion mechanisms—including intra beam scattering, modulational diffusion, and beam-beam induced diffusion—does not guarantee that this is possible. While significant theoretical and simulational advances are possible, beam based diffusion experiments are the most promising avenue for further research.

Long range beam-beam compensation: The dynamic aperture of the VLHC at collision may be dominated by long range beam–beam interactions, which may also limit the lifetime of "Pacman" bunches et the bunch train ends. Beam studies using the Tevatron electron lens compensator would help to establish the expected performance of the present VLHC interaction region design, and will point in the direction in which more R&D is required.

Beam–vacuum interactions: Photon stops are very promising, but need to be tested "under fire" to confirm their beam impedance and vacuum characteristics. Beam experiments should also be performed to test secondary electron production rates under various conditions in a superconducting collider environment.

Chapter 5

Snowmass Working Group M5 Lepton–Hadron Colliders

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A high luminosity lepton–hadron collider can provide precise and complete data essential to the ultimate understanding of the structure of matter. Lepton–hadron colliders have a unique potential in investigating various facets of QCD: the hadron space and spin structure, the space time picture of strong interactions, confinement, and the understanding of constituent masses. Furthermore, lepton-hadron colliders are essential tools to measure structure functions in unknown parameter regimes of x and Q^2. These will be needed to understand the hadron collisions in RHIC, LHC, and VLHC.

So far HERA at DESY has been the only high-energy lepton–hadron collider. In the last year HERA has surpassed its design luminosity of $1.5 \times 10^{31}\mathrm{cm}^{-2}\mathrm{s}^{-1}$, and an upgrade should soon increase the luminosity by a factor of 4. HERA has reached Bjorken x down to 10^{-4}, but to better understand the unexpected rise of parton densities at low x, new experiments with even smaller x are needed.

During the last few years several new lepton–hadron collider possibilities have been proposed. These proposed colliders come in two varieties. One is an electron linear accelerator colliding with a proton or ion ring accelerator, the other, like HERA, an electron ring accelerator colliding with a hadron ring. While conventional linacs can only provide a comparatively low average current, yielding lower luminosity than comparable ring-ring colliders, the novel technology of energy-recovery linacs might increase the available current sufficiently to make energy-recovery linac-ring colliders the favored technology for reaching high luminosities. Some technological issues are common to all proposed lepton–hadron colliders. To achieve the desired luminosity, the intra beam scattering rates have to be compensated by cooling of the high-energy hadron beams. For high-energy proton beams this is helpful but avoidable when a moderate loss of luminosity is accepted, but for ion beams or lower energy proton beams it is mandatory. Most of the proposed lepton–hadron colliders require polarized electron or positron and polarized proton or deuteron beams. The following six projects have been discussed:

THERA is a linac-ring collider in the traditional sense, where electrons could be accelerated through one or both arms of TESLA to collide with either protons or ions in the existing 6.5km long HERA tunnel. Various combinations of electron and proton energies could be envisioned with center of mass energies of up to 1TeV. An example is a symmetric arrangement of 800 GeV electrons on 800 GeV protons. Due to the rather small electron current of around 80 microamperes, the luminosity would be $1.6 \times 10^{31}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. Assuming that TESLA has been built at DESY, then the cost of building THERA has roughly been estimated to 120MEuro without labor. This facility is very cost effective since it makes optimal use of two then existing facilities. The construction time would be roughly 3 years.

The Electron Ion Collider (EIC) initiative in the USA covers a number of alternatives. The higher energy version, called eRHIC, would use the existing RHIC as the hadron ring to collide with polarized electrons from either a linac or a ring. For e/p collisions, the center of mass energy would be 100 GeV. The linac–ring version will take advantage of the high electron currents that become available with an energy recovery linac. Two energy recovery linacs have been built so far, one at Jefferson Lab and the other at JAERI. The former has obtained energy recovery for 5mA at 50MeV. The current and the energy proposed for eRHIC are 264mA and 10GeV. The luminosity would then be approximately $10^{33}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. The total cost without scientific labor would be around 300M$, and the construction time would be around 3 years.

The ring–ring collider version is more conventional. While in the linac–ring version the electron spin can be manipulate at will, the ring-ring version requires spin rotators close to the IR to provide longitudinal polarization at the experiment. Together with the two proton beam pipes and the detectors, which can only cope with a very limited amount of synchrotron radiation, this requires a quite sophisticated interaction region. The luminosity was computed to be $1.5 \times 10^{33}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. The projected cost is also 300M$ and the construction time would be approximately 3 years.

A green-site, lower-energy version of EIC with about 32 GeV center of mass energy (named EPIC) has been proposed also in the linac–ring and ring–ring collider versions. In the linac–ring scenario, the ion ring would be 465m long and would provide protons at 50 GeV. For an energy recovery linac with 264mA at 5GeV the luminosity would be $2 \times 10^{33}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. In the ring–ring scenario, a 1390m long 7 GeV electron ring would be located on top of a 32GeV proton ring and a luminosity of $10^{33}\mathrm{cm}^{-2}\mathrm{s}^{-1}$ could be reached. MIT Bates has proposed an initial R&D phase of 3 years with a total cost of 15M$. In both cases the construction cost would be roughly 300M$ for a construction period of 5 years. A detector for the EIC facilities is estimated to cost 100M$.

The HERA proton ring and the HERA pre-accelerator chain can be upgraded to accelerate and store polarized protons, polarized deuterons, and light or heavy ions. This project is occasionally called HERAe/A. The center of mass energy for electron-proton collisions is 318 GeV. Without electron cooling, the polarized proton option has been estimated to cost about 30MEuro, a polarized deuteron option will be substantially cheaper. For heavier ions, electron cooling is mandatory and a new ion linac would be needed. This leads to an estimated cost of 53MEuro for ions in HERA. The parton luminosity could then be roughly $7 \times 10^{31}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. The construction period might be around 3 years. The existing e/p accelerator makes this project much cheaper than other lepton–hadron colliders, and additionally no new detectors would needed to be build.

An electron ring in the LHC tunnel is referred to as eLHC and would collide a 60 GeV electron beam with the 7 TeV protons. The luminosity would be $2.5 \times 10^{32}\mathrm{cm}^{-2}\mathrm{s}^{-1}$ for these collisions with 1.3TeV center of mass energy. A cost estimate has not been determined. An electron ring in the VLHC booster tunnel, called epVLHC-b, has also been proposed. The new proposal of the VLHC does not require a 3 TeV booster. But for the previous layout an 80 GeV electron on 3 TeV proton collider in the booster tunnel could have run during the construction period of the VLHC main tunnel. The luminosity would be around $2.6 \times 10^{32}\mathrm{cm}^{-2}\mathrm{s}^{-1}$. For epVLHC-b the cost has been estimated to roughly 1000M$. Construction times for these two large-scale lepton–hadron colliders have not yet been determined.

Most of the discussed facilities take advantage of existing or planned hadron storage rings and are therefore rather cost efficient. They could begin construction after the following R&D issues have been addressed:

- High-current energy-recovery linacs. These linacs would also be very interesting for high energy electron cooling and for light sources. One key issue is the loss rate that must be kept below 10^{-6}. Beam break-up is another concern. Cornell has proposed to address these issues within the next 5 years by building an 100mA, 100MeV energy recovery linac prototype.

- High-energy electron cooling. For high-energies the electron beams have to be accelerated in a linac and are therefore bunched. To reach sufficient electron intensities, the beam can be stored in an accumulator, or an energy recovery linac could be used. Various R&D issues must be investigated, including magnetized beam transport as well as electron beam brightness and matching.

- Polarized electron sources. Polarized electron guns with sufficiently high average currents have never been operated before and have to be developed.

- High-energy deuteron and proton polarization. This subject, which is being pioneered at RHIC, has to be further developed. The current of polarized proton and deuteron sources has to be increased.

- Integration of the detectors and colliders. High-energy detector requirements impact the accelerator and IP design. For example the detectors needed to study small x physics have the special requirement of covering the forward direction. Even detectors with 4π solid angle are being discussed. Their implications on the interaction region must be taken into account.

- The detectors will only be able to handle large bunch frequencies if hadron beams with a very small amount of out-of-bunch particles are being stored. To reach the proposed 7ns bunch spacing for some of the EIC versions, the out-of-bunch particle population has to be suppressed significantly below the level in HERA, where the bunches are 96ns apart.

Chapter 6

Snowmass Working Group M6 High Intensity Proton Sources

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The US high-energy physics program needs an intense proton source (a 1–4 MW Proton Driver) by the end of this decade. This machine will serve multiple purposes: (i) a stand-alone facility that will provide neutrino superbeams and other high intensity secondary beams such as kaons, muons, neutrons, and anti- protons (cf. E1 and E5 group reports); (ii) the first stage of a neutrino factory (cf. M1 group report); (iii) a high brightness source for a VLHC (cf. M4 group report).

Based on present accelerator technology and project construction experience, it is both feasible and cost- effective to construct a 1–4 MW Proton Driver. There are two PD design studies, one at FNAL and the other at the BNL. Both are designed for 1 MW proton beams at a cost of about US$200M (excluding contingency and overhead) and upgradeable to 4 MW. An international collaboration between FNAL, BNL and KEK on high intensity proton facilities addresses a number of key design issues. The sc cavity, cryogenics, and RF controls developed for the SNS can be directly adopted to save R&D efforts, cost, and schedule. PD studies are also actively pursued at Europe and Japan.

There are no showstoppers towards the construction of such a high intensity facility. Key research and development items are listed below (indicates present status). Category A indicates items that are not only needed for future machines but also useful for the improvement of existing machine performance; category B indicates items crucial for future machines and/or currently underway.

1. *H^- source:* Development goals—current 60–70 mA 35 mA, duty cycle 6—12% 6%, emittance 0.2π mm-mrad rms normalized, lifetime > 2 months 20 days. (A)

2. LEBT chopper: To achieve rise time $< 10\,\text{ns}$ 50 ns. (B)

3. Study of 4-rod RFQ at 400 MHz, 100 mA, 99% efficiency, HOM suppressed. (B)

4. MEBT chopper: To achieve rise time $< 2\,\text{ns}$ 10 ns. (B)

5. Chopped beam dump: To perform material study and engineering design for dumped beam power $> 10\,\text{kW}$. (A)

6. Funneling: To perform (i) one-leg experiment at the RAL by 2006 with goal one-leg current 57 mA; (ii) deflector cavity design for CONCERT. (all B)

7. Linac RF control: To develop (i) high performance HV modulator for long pulsed ($> 1\,\text{ms}$) and CW operation; (ii) high efficiency RF sources (IOT, multi-beam klystron). (all A)

8. Linac sc RF control: Goal—to achieve control of RF phase error $< 0.5^\circ$ and amplitude error $< 0.5\%$ presently 1°, 1% for warm linac. (i) To investigate the choice of RF source (number of cavity per RF source, use of high-power source); (A) (ii) to perform redundancy study for high reliability; (B) (iii) to develop high performance RF control (feedback and feedforward) during normal operation, tuning phases and off-normal operation (missing cavity), including piezo-electric fast feedforward. (A)

9. Space charge: (i) Comparison of simulation code ORBIT with machine data at FNAL Booster and BNL Booster; (ii) to perform 3D ring code bench marking including machine errors, impedance, and space charge (ORNL, BNL, SciDAC, PPPL). (all A)

10. Linac diagnostics: To develop (i) noninvasive (laser wire, ionization, fluorescent-based) beam profile measurement for H^-;(ii) on-line measurement of beam energy and energy spread using time-of-flight method; (iii) halo monitor especially in sc environment; (iv) longitudinal bunch shape monitor. (all A)

11. SC RF linac: (i) High gradients for intermediate beta (0.5-–0.8) cavity; (A) (ii) Spoke cavity for low beta (0.17–0.34). (B)

12. Transport lines: To develop (i) high efficiency collimation systems; (A) (ii) profile monitor and halo measurement; (A) (iii) energy stabilization by HEBT RF cavity using feedforward to compensate phase-jitter. (B)

13. Halo: (i) To continue LEDA experiment on linac halo and comparison with simulation; (ii) to start halo measurement in rings and comparison with simulation. (all B)

14. Ring lattice: To study higher order dependence of transition energy on momentum spread and tune spread, including space charge effects. (B)

15. Injection and extraction: (i) Development of improved foil (lifetime, efficiency, support); (A) (ii) experiment on the dependence of H^0 excited states lifetime on magnetic field and beam energy; (B) (iii) efficiency of slow extraction systems. (A)

16. Electron cloud: (i) Measurements and simulations of the electron cloud generation (comparison of the measurements at CERN and SLAC on the interaction of few eV electrons with accelerator surfaces, investigation of angular dependence of SEY, machine and beam parameter dependence); (A) (ii) determination of electron density in the beam by measuring the tune shift along the bunch train; (A) (iii) theory for bunched beam instability that reliably predicts instability thresholds and growth rates; (A) (iv) investigation of surface treatment and conditioning; (A) (v) study of fast, wide-band, active damping system at the frequency range of 50-–800 MHz. (B)

17. Ring beam loss, collimation, protection: (i) Code benchmarking and validation (STRUCT, K2, ORBIT); (A) (ii) engineering design of collimator and beam dump; (A) (iii) experimental study of the efficiency of beam-in-gap cleaning; (A) (iv) bent crystal collimator experiment in the RHIC; (B) (v) collimation with resonance extraction. (B)

18. Ring diagnostics: (i) Whole area of diagnosing beam parameters during multi-turn injection; (ii) circulating beam profile monitor over large dynamic range with turn-by-turn speed; (iii) fast, accurate non-invasive tune measurement. (all A)

19. Ring RF: To develop (i) low frequency ($\sim$ 5 MHz), high gradient ($\sim$ 1 MV/m) burst mode RF systems; (B) (ii) high gradient (50–100 kV/m), low frequency (several MHz) RF system with 50–60% duty cycle; (B) (iii) high-voltage ($>$ 100 kV) barrier bucket system; (B) (iv) transient beam loading compensation systems (e.g. for low-Q MA cavity). (A)

20. Ring magnets: (i) To develop stranded conductor coil; (ii) to study voltage-to-ground electrical insulation; (iii) to study dipole/quadrupole tracking error correction. (all B)

21. Ring power supplies: To develop (i) dual-harmonic resonant power supplies; (ii) cost effective programmable power supplies. (all B)

22. Kicker: (i) Development of stacked MOSFET modulator for DARHT and AHF to achieve rise/fall time $<$ 10–20 ns; (B) (ii) impedance reduction of lumped ferrite kicker for SNS. (A)

23. Instability and impedance: (i) To establish approaches for improved estimates of thresholds of fast instabilities, both transverse and longitudinal (including space charge and electron cloud effects); (ii) to place currently-used models such as the broadband resonator and distributed impedance on a firmer theoretical basis; (iii) impedance measurement based on coherent tune shifts vs. beam intensity, and instability growth rate versus chromaticity, including that for flat vacuum chambers; (iv) to develop new technology in feedback implementation. (all B)

24. FFAG: (i) 3-D modeling of magnetic fields and optimization of magnet profiles; (ii) wide-band RF systems; (iii) transient phase shift in high frequency RF structures; (iv) application of sc magnets. (all B)

25. Inductive inserts: (i) Experiments at the FNAL Booster and JHF3; (A) (ii) programmable inductive inserts; (B) (iii) development of inductive inserts which have large inductive impedance and very small resistive impedance; (B) (iv) theoretical analysis. (B)

26. Induction synchrotron: (i) Study of beam stability; (ii) development of high impedance, low loss magnetic cores. (all B)

Chapter 7

Snowmass Working Group T1 Interaction Regions

Conveners: T.W. Markiewicz (SLAC), F. Pilat (BNL)

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7.1 Introduction

The Interaction Region Working Group (T1) reviewed the issues, designs, and plans of the proposed muon collider, e-hadron colliders, the proposed e^+e^- and $\gamma\gamma$ linear colliders, and the hadron colliders. This document summarizes the IR issues, status, and R&D plans for each project.

The design and performance of IR systems at existing *hadron colliders* (Tevatron, RHIC) and the LHC have been reviewed with the goal of guiding the IR planning of the VLHC Stage 1 and Stage 2 and the necessary R&D program to validate the design choices. The key IR issues for future hadron colliders are the overall optics configuration, IR magnets, IR correction and feedback systems, energy deposition in the IR components, and the integration of machine components with the experiments. The conclusions of the working group in these areas, and the R&D program that addresses them, are discussed below.

An anti-symmetric triplet optics similar to existing colliders is the natural choice for a 20 TeV Stage 1 VLHC IR, the main challenge being the development of final focus quadrupole gradients of $\sim 300\,\mathrm{T/m}$. The 87.5 TeV Stage 2 VLHC, the first hadron collider to operate in a synchrotron light regime, opens the possibility of flat beams. Emittance and β^* ratios respectively of 0.1 are possible. The main advantage of a flat beam is the minimization of the long-range parasitic crossings and consequently of the long-range beam–beam tune shift, the main IR performance limitation. A flat beam, symmetric doublet optics, requires beam separation immediately after the IP and thus a very challenging 2-in-1 magnet design for the final doublet, with gradients in the 400–600 T/m range. In the round optics the final triplet focuses both beams, allowing for a simpler single aperture design. The main progress at the workshop has been the realization that a 4-magnet final focus solution is possible, that can provide both flat and round optics with a continuous transition from doublet to triplet optics, an operationally very attractive option.

The development of a new generation of IR magnets is critical for future hadron colliders. These require at the same time high gradients, large apertures to accommodate absorbers and crossing angles, excellent field quality to not limit the dynamic aperture, stringent alignment and mechanical stability, all that in a high radiation environment that causes high heat deposition. Furthermore, attention must be paid to quench protection and magnet powering schemes. High gradients, large apertures and high heat load means building magnets with Nb_3Sn and HTS (High Tc Superconductors). At the workshop, a plan for a vigorous IR magnet R&D has been drafted along the following lines: near term R&D (LHC upgrade and VLHC-1 single aperture Nb_3Sn) and longer term R&D (VLHC-2 double aperture with Nb_3Sn or HTS).

The near term R&D would capitalize on the experience of building magnets for the LHC upgrade (250T/m, 90mm bore) to produce single aperture Nb_3Sn IR magnets for VLHC-1 (300 T/m, 70mm bore), with the goal of a short model by FY05-08 at 10M$, and a prototype by FY08–10 at $\sim$ 20M$. Long term R&D for VLHC-2 will focus on double bore high gradient IR quadrupoles (400–600 T/m, up to 40mm bore), with the goal of a short model by FY12–16, and 12–16T separation dipoles, on the same time scale.

IR correction systems and feedback are necessary to improve the IR operational performance and to relax otherwise stringent requirements on IR magnets field quality and IR components alignment, thus achieving a more cost-effective overall design.

IR correction systems include local linear and nonlinear correctors to compensate respectively for alignment and field errors in the IR magnets, and beam-beam local compensation systems. In the VLHC, where the transverse beam dimensions are negligible with respect to the beam pipe, and vibration stabilization is an issue, ultimate performance will require orbit and IP feedback. A vigorous program of collaborative beam experiments at existing hadron facilities in the next 3–6 years is necessary to test and validate the proposed correction systems.

At the LHC, $\sim$ 900 Watts/IR side of collision debris is generated at nominal luminosity and energy. A system of absorbers and a beam tube liner are necessary to protect the IR components. The figure raises to 3 KWatts/side at VLHC-1 and 24 KWatts/side at VLHC-2. Evaluation of energy deposition and backgrounds in IR components has started for the VLHC. Modeling and design of a protection system is a high priority for VLHC-1 and 2.

The integration of machine components with the experiments is critical given the optics, background and energy deposition issues already discussed and a R&D program for integrated multi-function machine and experiment components should be planned for the next 10 years.

HERA is the benchmark to evaluate IR issues for the proposed lepton–hadron colliders, eRHIC, EPIC and THERA. The main challenge for future e-hadron collider IRs is the integration between machine and experiments, for which extended experience has been gained for the HERA luminosity upgrade. Magnets, collimators, vacuum, alignment, supports, instrumentation must be jointly developed for the accelerator and the experiments. Issues that need careful study are the optimization of the collision frequency and the energy range and tunability of the machine to match the physics processes to be studied. R&D activity will focus on the development of large aperture multi-function final focus magnets, active beam pipes, beams with small emittance and divergence, electron cooling for protons.

The primary e^+e^- Linear Collider IR issues are the production and control of backgrounds arising from both the beam–beam interaction and the operation of the accelerator and the design and support of the final quadrupole doublet. Other concerns include the design of the extraction line and instrumentation to measure beam quantities required for either the experiment or the operation of the accelerator.

At the proposed IP beam parameters for both TESLA and NLC at 500 GeV, the IP background of most concern is the incoherent production of e^+e^- pairs. The number of pairs produced is approximately proportional to luminosity and is similar for both designs. GEANT and FLUKA based simulations indicated that detector occupancies in the relevant readout time (per bunch, per train, or per drift time for gaseous trackers) are adequately low and the CCD-based vertex detector lifetime is some number of years. Elevation views of the IR layout are similar while the plan view differs only due to the crossing angle and separate extraction line in the case of the NLC. The use of tungsten shielding, instrumented masks, and low Z material to absorb low energy charged and neutral secondary backgrounds is similar. R&D plans in this area involve increasingly detailed simulations as the design of the interaction region and detectors mature. Similarly, the μe decay background that dominates the $\mu\mu$ collider IR involves the design of many absorbers whose geometry is configured appropriately for each machine energy.

TESLA uses a superconducting final quadrupole doublet as its final lens system. Incident and extracted beams are electrostatically separated 50m from the IP. There is a beam dump and collimator system for the beamstrahlung photons in line with the detector axis and a separate charged particle dump that does not point at the IP. This allows for large apertures for the passage of halo induced synchrotron radiation and flexibility for tuning the quadrupole field. Concerns of the jitter of this last lens are dealt with via an intra-train feedback system that has sufficient bandwidth and sensitivity to correct motion to the required $0.1\sigma_y$ level. Engineering studies of the SC quadrupoles are based on the similar LHC magnets while detailed simulations of the digitally controlled feedback scheme give confidence in its design.

NLC, due to its crossing angle, is considering the use of permanent SmCo magnets in it final doublet. These are transversely compact, light, stiff, and free of external connections that might couple external vibration sources. Strength variation would be accomplished via counter-rotating segments. The compact magnet design allows for a devoted extraction line that guides the spent charged beam through a chicane that allows for clean post-collision beam diagnostics to a common photon-electron beam dump. Any jitter not passively eliminated will be dealt with through a combination of active sensors, magnet movers, and correctors in either open or closed feedback loops. Additionally, an analog variation of the intra-train feedback foreseen for TESLA, operating with 40ns latency, effectively corrects any residual jitter up to $\sim 15\sigma_y$ for the trailing 80% of the 266ns bunch train. An extensive R&D program in ground motion measurement, inertial and interferometric sensor design, actuator performance, and feedback algorithm development has already begun to demonstrate proof-of-principle solutions to the magnet jitter problem. Tests on realistic, mechanical mockups of the IR are scheduled for FY2003–04. Full systems engineering tests based on the collision of 50–400nm beams at the SLC (the LINX proposal) that would validate the final engineering solution at the 1nm level have being proposed. Engineering studies of the final doublet permanent magnets, as well as compact SC magnet solutions, are planned.

The possibility of a $\gamma\gamma$ collider has been dramatically increased because of recent progress in laser development and the engineering design of the IR optics that produce the γe collisions. The Mercury laser, developed at LLNL for fusion applications, can serve as the demonstration prototype for the $\gamma\gamma$ collider laser. It will undergo full power tests by the end of FY2002. Conceptual designs to take the 100-joule, 10-Hz output of the laser and to match it to either the time structure required for the NLC or TESLA are underway. Large annular optics that permit the laser beams to be focused to the required 10-micron spots without putting any material in the path of the residual particle debris from the beam–beam collisions will be tested in FY2002. Independent laser and optics designs that profit from the longer interbunch spacing of TESLA are being evaluated. Changes to the final focus that decrease the horizontal spot size and increase the $\gamma\gamma$ luminosity are being developed.

The IR design of high current dual ring e^+e^- circular colliders with small bunch spacing, such as KEK-B and PEP-II, primarily involve the issues of synchrotron radiation and beam pipe heating from trapped higher-order-modes. SR masking, concerns about beam tail distributions, and orbit compensation due to the magnetic field of the detector are concerns shared by e^+e^- LCs. IR modifications to allow for luminosity increases to 3×10^{34} involve the replacement of permanent magnets with higher field SC magnets and the introduction of a small crossing angle. An IR design for 10^{36} luminosity is in the conceptual stage.

Chapter 8

Snowmass Working Group T2 Magnet Technology

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The T2 Working Group has reviewed and discussed the issues and challenges of a wide range of magnet technologies; superconducting magnets using NbTi, Nb_3Sn and HTS conductor with fields ranging from 2 to 15 Tesla and permanent magnets up to 4 Tesla. The development time of the various technology options varies significantly, but all are considered viable, providing an unprecedented variety of choice that can be determined by a balance of cost and application requirements.

One of the most significant advances since Snowmass 1996 is the increased development and utilization of Nb_3Sn. All of the current US magnet programs, BNL, FNAL, LBNL and Texas A&M have programs using Nb_3Sn. There are also active programs in HTS development at BNL, TAMU and LBNL. A DOE/HEP sponsored program to increase the performance and reduce the cost of Nb_3Sn is in the second year. The program has already made significant improvements. The current funding for this program is $500k/year and an increase to $2M has been proposed for FY02.

Progress in the magnetic properties of permanent magnet materials has been impressive. Materials such as Sm_2CO_{17} and new types of $Nd_2Fe_{14}B$ have a maximum energy product of 240–400 kJ/m^3. High field magnets made from these materials have applications as high gradient, adjustable quadrupoles for the NLC, injection line, correctors and Lambertsons for a VLHC and damping ring magnets and wigglers. R&D is directed towards improving the thermal and radiation stability, adjustable strength with high magnetic center stability and hybrids for improved stability and use as accelerator magnets. A combination of declining costs and improved materials has made permanent magnets competitive with conventional and superconducting magnets in many applications.

A majority of the discussion at Snowmass focused on magnets for large colliders. As one of the major accelerator components, they are significant cost drivers.

A superferric magnet for a proposed VLHC has been described in the VLHC Design Report. It has a maximum field of 2T generated by a 100 kA, superconducting transmission-line. A couple of alternative designs were discussed which offer more freedom in the choice of parameters. The Texas Accelerator Center (TAC) magnet was proposed for the SSC. Several of these long magnets were built and successfully tested. Relative to the FNAL transmission-line magnet, they have a larger bore (2.5 cm$\times$3.5 cm compared to 1.8 cm$\times$3.0 cm) and higher field, 3T. The multiple current powering scheme employed to cope with saturation effects may provide a means of extending the dynamic range, allowing consideration of a first stage VLHC with 50 TeV center-of-mass energy in a smaller ring while still retaining the Tevatron as the injector. This magnet will require a more extensive cryogenic system and beam screen at the luminosities and energies under discussion. At the time of the SSC, the multiple power supply requirement was considered a drawback, but power supply technology has progressed significantly since that time, making the TAC magnet, or some variation of it, a possible candidate for an inexpensive collider dipole. During the workshop, a couple of hybrid superconducting/permanent magnet designs were discussed. It was agreed that the next steps following the workshop would be to make a detailed cost comparison of the TAC and Transmission-line magnets and to consider a new design, combining some of the features of the proposed alternatives.

A small-bore, 5 Tesla, NbTi magnet, based on the RHIC dipole was discussed. It was agreed that magnets in this field range merit further study. Medium field magnets allow more flexibility in the choice of machine parameters and overall may lead to a less expensive accelerator.

The recent success of a 14.7 Tesla dipole built by LBNL and the 11 Tesla development program at FNAL has expanded the field range that can be considered for accelerator dipoles. The disadvantages of high field magnets and Nb_3Sn, such as synchrotron radiation loads on the cryo system, high cost and magnetization effects are being addressed. Schemes have been proposed to eliminate the required beam screens by using photon stops, which would allow the use of a smaller bore. Several schemes have been proposed to significantly reduce persistent current effects due to the large filaments and high current density of Nb_3Sn. The recent results have been promising, but high field magnet technology will need some innovative new ideas in order to meet cost reduction requirements. Success can only be achieved through an aggressive, focused magnet development program. Low-cost, high-performance magnets will eventually be required. There are no alternatives to high field magnets in an upgrade scenario. The machine energy is ultimately determined by the dipole field strength.

The greatest technical challenges are the Interaction Region quadrupoles for both linear and circular colliders. Both superconducting and permanent magnets are being considered for use in IR's for Linear Colliders. While the gradients are fairly modest, the requirements on stability are extremely challenging. IR quadrupoles for hadron colliders require large gradients (300–600 T/m), large bores and excellent field quality. Heat loads are very high; 600 W/side for the Stage-1 VLHC. These conditions, if not mitigated, will favor the use of HTS, should it become available, and/or higher performance A15's.

The US magnet R&D programs have not totally recovered from the demise of the SSC. The resources required to bring the existing magnet technology options to a point where they can be reliably costed and considered for use in a collider design, does not currently exist. In addition to increased R&D funding, there is need for a global cost framework to compare and evaluate design options. Since the RHIC dipoles are the only US example of industrial procurement, it is suggested that those costs can be used as a basis to develop a comparative cost model. The magnet programs need to work closely with accelerator physicists to push all parameters to the limit and arrive at the most cost-effective combination of magnet design, machine performance and risk. There has been informal activity in this direction, for example, at the VLHC Workshops, but there is a need to formalize this activity in a more coherent way.

Chapter 9

Snowmass Working Group T3 RF Technology

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The next-generation linear collider will require high-power microwave sources and accelerating systems vastly more challenging than its predecessor, the Stanford Linear Collider (SLC). Cost efficiency will demand high accelerating gradient to achieve beam energies five to ten times greater than in the SLC. Luminosity goals 10,000 times greater than the SLC demand efficient creation of the highest possible beam power without degradation of beam emittance.

The past decade of R&D has demonstrated the feasibility of two technical approaches for building a 500-GeV center-of-mass collider with attractive options for future upgrade. The TESLA R&D program offers the prospect of 1.3-GHz superconducting rf linacs with 23.5 MV/m gradient that can be upgraded later to 35 MV/m gradient by doubling the number of klystrons and the cryo plant, to reach 800 GeV in the center of mass. The NLC and JLC programs offer the prospect of 11.4-GHz room-temperature linacs that can later be extended to 1 TeV by doubling the number of structures and klystrons, and to 1.5 TeV by additionally increasing gradient or length. Both programs offer a 500-GeV linear collider project start within the next few years (2–3 years for TESLA, 3–4 year for NLC) based on available technology validated by experiments at numerous, complementary test facilities. Both offer their upgrades as a result of further progress in R&D that is already underway.

While both the 1.3- and 11.4-GHz approaches use klystron power sources, a longer-range design study for a two-beam accelerator, the CERN Linear Collider (CLIC) may offer a path to multi-TeV collisions after approximately six years of further R&D.

9.1 Power Sources

The push to higher gradients for the room-temperature machines has utilized higher frequencies and corresponding increases in field strength and decreases in pulse length and stored energy. The high frequencies allow the same rf-to-beam transfer efficiency to be achieved for a fixed current at a higher gradient with less rf energy per pulse. The cost-optimal unloaded gradient for the NLC is about 70 MV/m. CLIC studies for 3-TeV center-of-mass energy are based on 170-MV/m unloaded gradient.

High-power sources of the longest possible pulse-width are desirable for high efficiency and low cost. The use of superconducting accelerator structures in TESLA reduces the peak rf power requirement, permits 1.5 millisecond pulse width, and allows large interbunch spacing and high rf-to-beam power transfer efficiency.

A seven-beam klystron has been developed with industry for TESLA. It can operate with moderate high-voltage (110 kV) and high efficiency (70% goal) due to reduced space-charge forces in the vacuum tube. One of the initial tubes produced has operated at 65% efficiency at the 10-MW design power, 1.5-ms pulse length, and 5-Hz pulse rate (although 10 Hz will be required of some of the klystrons for FEL operation). This tube was used in the TELSA Test Facility at low power, and more have been produced. The full klystron output pulse in TESLA will be divided to feed 36 nine-cell superconducting cavities.

The pulse-width required for the 11.4-GHz accelerator structures of the NLC and JLC is 400 ns. Klystrons have been developed, for efficiency and cost, to generate wider pulses (3.2 microseconds for NLC, 1.6 microseconds for JLC) that get compressed in time as they are delivered to the accelerator. The klystrons developed for this purpose during the past decade produce 75-MW output, which approaches the practical limit for single beam klystrons. A major advancement was the use of periodic permanent magnet focusing of the klystron beam instead of conventional power-consuming electromagnetic solenoids. Both NLC and JLC have produced klystrons that meet the peak power and pulse-width requirements with acceptable efficiency above 50%. The pulse width in testing to date has been limited by the high-voltage pulse modulators. However, widening the pulse would produce diminishing returns because of the increased cost of the pulse compression system. The current program is to continue to develop the klystrons in association with industry to improve manufacturability and cost, and to achieve reliable operation at the design pulse repetition rate (120 Hz for NLC, 150 Hz for JLC).

While klystron technology is satisfactory for linear collider applications at 11.4-GHz, novel sources are under study for higher power and higher frequency acceleration. Multiple-beam and sheet-beam klystrons are being studied at SLAC and at Calabazas Creek Research, Inc. (CCR), in Santa Clara, California. Higher frequency sources in the 10–1000 MW peak power range are under investigation. Gyroklystrons at the University of Maryland have demonstrated 20–75 MW peak power at frequencies of 8–17 GHz. A 91-GHz gyroklystron is being built by CCR with a goal of 10 MW peak power. CPI, in Palo Alto, is designing a 50-MW gyroklystron at 30 GHz for CLIC studies (prior to the availability of drive-beam power generation). Innovative research is also underway on high-power magnicons at 11 and 34 GHz, at the U.S. Naval Research Lab (NRL), and at Omega-P, Inc., in New Haven, Connecticut.

The microwave pulse compression needed to transform the output of 11-GHz klystrons to the narrower pulse width and higher power required by NLC and JLC accelerator structures is challenging. The pulse compression and power distribution system must be efficient and inexpensive. The Delay Line Distribution System (DLDS), first proposed at KEK, was adopted as the best of available choices. Components of a two-mode version of the DLDS have been developed at SLAC to further reduce the net length of transmission line. For the NLC, this system combines the power from eight 75-MW klystrons and routes it up-beam in a sequence of eight (shorter) pulses to feed eight separated sets of accelerator structures. The DLDS for JLC is similar; the narrower klystron pulses sequentially feed only four sets of structures. DLDS components have been tested at peak power levels up to 500 MW and a test of all the critical components of a full system at the nominal (600-MW) peak power, pulse width and energy is planned in the next two years.

Although passive components have been at the center of research for pulse compression systems, active components such as switches and phase shifters can be the basis of the next generation of more elegant, efficient and low-cost pulse compression systems. Research on the topology of active systems is being conducted at SLAC and some of its basic principles and scaling laws have been established. Overmoded active components based on semiconductor devices and magnetic materials have been designed and demonstrated at power levels around 10 MW at 11 GHz. Researchers at the Institute for Applied Physics (Nizhny-Novogorod, Russia), Omega-P, and NRL have demonstrated pulse compression to 15 MW using a plasma switch. This work is in the early stages of development.

The CLIC study focuses on using low-frequency, long-pulse klystrons with high-frequency, 30-GHz room-temperature accelerator structures. In a novel form of pulse compression, the low-frequency rf is to be used to accelerate trains of bunches in 1.2-GeV "drive linac" that produces 80-MW of average beam power; the train is to be compressed in a series of chicanes and combiner rings, and routed sequentially up-beam to decelerator structures that will transform the 30-GHz harmonic power from the train (over 200 MW per decelerator structure) to the high-gradient accelerator. Tests so far have generated low power (30 MW) in short (16 ns) pulses. At least six years will be required to demonstrate the feasibility of this technology sufficiently to pursue a CLIC-type collider.

9.2 Accelerator Structures

A challenge for NLC and JLC has been to achieve the desired high gradients in prototype accelerator structures. Early tests had indicated that more than 100 MV/m should be attainable at 11.4 GHz. However, these tests were done with either standing wave or short (< 30 cm long), low group velocity (< 0.05c) structures due to the limited rf power available at the time. The structures developed later to minimize costs were longer (180 cm) and had higher group velocity (0.12c) and so required higher input power. With improvements to the high power testing capability at the NLC Test Accelerator (NLCTA) in the past two years, tests of structures at high gradient showed damage (change in phase advance per cell) at unloaded gradients above 45–50 MV/m. The pattern of damage, and microwave circuit analysis, suggested that the high group velocity in the structures was efficiently transferring the stored energy to the breakdown sites, exacerbating the damage. An aggressive R&D program has been launched during the past year to develop lower group-velocity structures and improve cleaning and handling procedures. The results have been encouraging: gradients up to 80 MV/m have been achieved without observed damage. Work is continuing to achieve a greater margin for reliable operation at the nominal 70 MV/m unloaded gradient in large-scale production. The CLIC group has also seen damage in their test structures and are also pursuing R&D in this area.

Another challenge of high-frequency rf structures is suppression of the long-range transverse wakefields that, if not reduced by about two orders of magnitude, will disrupt the multi-bunch trains proposed for NLC, JLC and CLIC. During the past decade, SLAC and KEK have jointly developed and demonstrated effective methods for damping and detuning the deflecting modes in 1.8-m long, 11.4-GHz structures. After validating the high-gradient performance of test structures in the NLCTA at SLAC, the next step will be to modify the iris size for NLC/JLC structures to produce an acceptable short-range wakefield, and to apply the well-developed long-range wakefield-suppression techniques. The CLIC study group is planning on heavily damping the deflecting modes in their own 30-GHz structures.

TESLA plans to prevent multi-bunch beam break-up in its millisecond-long, 3000-bunch train by damping the higher-order modes in the 1.3-GHz superconducting cavities using external loads. In experimental tests, all but one of the modes have been successfully damped, and a re-orientation of the output coupler has been proposed to damp the remaining mode. Very high frequency modes must be absorbed by suitable material inserted into the beam pipe between cryomodules to avoid additional heat load into the helium at 2K.

At the DESY TESLA Test Facility (TTF) site, a large number of industrially-produced nine-cell structures (1-m active length) have reliably reached gradients of 25--30 MV/m in cavity acceptance tests. To reach these gradients, high-purity niobium is used to prevent thermal breakdown of superconductivity, while high pressure rinsing and clean room assembly techniques are used to reduce field emission and voltage breakdown. In completed cryomodules of eight, nine-cell cavities for the TTF beam, one unit has reached 22 MV/m average gradient. Gradients for cryomodules have been steadily rising as final assembly techniques are improved. The maximum accelerating gradient for TESLA structures will be limited to 50—60 MV/m by the critical rf magnetic field.

An industrial base for superconducting cavity fabrication was established for LEP. Industry has acquired the generic superconducting rf technology, which includes cavity chemistry, high pressure rinsing, cryomodule fabrication, and cryomodule assembly in clean rooms. Three companies have made cavities for the TTF.

R&D is in progress to increase superconducting rf gradients. Electropolishing instead of the standard chemical polishing eliminates grain boundary steps so that gradients of 40 MV/m at Q values above 1010 are now reliably achieved in single cells at three laboratories (KEK, TTF/CERN and TJNAF). The highest gradient achieved was 42 MV/m. Preparations are underway to electropolish nine-cell cavities.

There has been substantial progress in cost reduction by increasing the number of cells per cavity to nine, and the number of cavities inside one cryomodule to twelve, and by integrating the cryogenic distribution system into the cryomodule. A superstructure based on a nine-cell pair offers more cost reduction, and will be tested in the near future. Further cost reduction efforts are forthcoming in new weld-free cavity fabrication techniques, such as spinning and hydroforming.

Superconducting rf (srf) technology recently has made substantial inroads into a variety of accelerator applications for light sources and neutron, neutrino, and muon sources. 500 MHz cavities for the Cornell Electron Storage Ring (CESR) have been adopted by two new light sources under construction. The TTF (and perhaps TESLA) will serve the FEL user community. The U.S. Spallation Neutron Source has changed its baseline to use srf cavities to accelerate beams from 200 MeV to 1 GeV. Los Alamos National Lab in the U.S. and INFN in Italy are developing srf technology for a high-intensity proton accelerator for transmutation of nuclear waste. CERN is studying the use of the LEP-II srf cavities for a high intensity proton linac for advanced neutrino beams. The RIA will use srf technology. TJNAF in the U.S. and JAERI in Japan have operated infra-red FELs producing 2-kW average power for materials processing applications. Feasibility Studies I and II in the U.S. for a Neutrino Factory are based on 200-MHz srf cavities; a prototype is under development. Subsystems of a future muon collider potentially will use srf.

Chapter 10

Snowmass Working Group T4 Particle Sources: Positron Sources, Antiproton Sources, and Secondary Beams

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Electronic copies of the talks given during T4 sessions and other supporting documents can be found at the following URL: http://cosmo.fnal.gov/Snowmass_Particle_Sources/

10.1 Positron Sources for Linear Colliders

The next generation of linear colliders require positron beams at a rate of 1×10^{14} to 4×10^{14} positrons per second which is nearly 2 orders of magnitude greater than the SLC positron system. The NLC design for positrons is a conventional system in which positrons are produced by directing 6.2 GeV electrons onto three separate thick (4 r.l.), high-Z material targets, capturing the resulting positrons, and accelerating them up to the 1.98 GeV energy of the predamping ring system. Three targets are required to handle peak shock stress in the target; a predamping ring is necessary because of the large phase space of the collected positrons. The TESLA design utilizes a 35 m long planar wiggler to generate high energy photons (in the range of 20–60 MeV). A thin (0.4 r.l), Ti target is used for photon-positron conversion. Resultant positrons are collected, accelerated, and injected into the TESLA positron damping ring at 5 GeV; a predamping ring is not required in the TESLA design. JLC has a design for conventionally produced positrons which is nearly identical to the NLC design but is based on a single target. The CLIC design is similar to both the NLC and JLC systems. JLC, NLC, and TESLA are considering polarized positron sources based on the conversion of circularly polarized, high energy photons. Peak shock stress in the targets, average power dissipation in the targets, radiation damage, and collection efficiencies are major considerations for all designs.

Peak shock stress occurs on a time scale of microseconds. This is mitigated by increasing the incident beam size on the target (on the scale of 1–2 mm, rms). For photon based production schemes, low-Z, high strength converter material can be used while high-Z materials are preferred for the conventional schemes. Average power deposition is accommodated through rapid target rotation (up to 1200 rpm for TESLA) and water cooling. Loss of material integrity due to radiation damage and thermal fatigue are active areas of research. Solenoidal magnet systems with fields in the range of 5–10T immediately after the targets are required for matching into the downstream accelerators. NLC, TESLA, and CLIC will use normal conducting L-band linac systems for the initial capture and acceleration of the positrons. L-band provides a larger aperture and hence improved acceptance over S-band designs. The JLC design uses S-band rf for positron collection and acceleration. Significant engineering development is required for the collection and initial capture systems.

None of the present linear colliders include polarized positron systems in their baseline designs. However, the JLC and TESLA groups are developing such designs as possible upgrades. The basic idea is to generate circularly polarized photons at an energy of about 60 MeV. Pair creation in thin radiators preserves the initial helicity of the photons. Proper selection and transport of the resultant positrons can produce positron beams with a longitudinal polarization of about 60%. This technique was first developed in the 1970–1980's at BINP but has not been demonstrated. The TESLA scheme for polarized positrons utilizes a helical undulator to produce polarized photons. This approach follows a relatively straightforward path of replacing the planar wiggler in their design with a short period helical undulator. TESLA positron polarization requires an undulator which is up to 150 m long and the design of a new collection/selection scheme. JLC has proposed a scheme in which circularly polarized photons are produced through Compton backscattering of circularly polarized laser beams off 6 GeV electrons. The JLC scheme eases the requirements on the electron beam used to produce photons but presents a very large demand on the laser systems ($\sim 400\,$kW of average laser power). Collection schemes for either approach have the same functional requirements with regard to polarization selection from the total positron flux, albeit the beam formats of the two designs are different. The SLAC group is presently evaluating both schemes for application to the NLC.

10.2 Antiproton Sources

At the present time there are two sources of antiprotons for the world's physics experiments—the CERN Antiproton Decelerator (AD) and the Fermilab Antiproton Source. The T4 working group restricted its focus to the technological issues limiting the rate at which antiprotons can be accumulated at the Fermilab Antiproton Source.

Presently, the Fermilab Antiproton Source collects antiprotons at a rate of $7.5 \times 10^{10}\,\bar{p}$/hour. Various improvements in the Fermilab accelerator complex over the next 3 to 5 years are expected to increase the $\bar{p}$ accumulation rate to $52 \times 10^{10}\bar{p}$/hour. Beyond that, the implementation of the Proton Driver[1] may increase the $\bar{p}$ accumulation rate by as much as a factor of 4 if the Antiproton Source can be further upgraded to accommodate the increased $\bar{p}$ flux. The two most significant technological issues that must be faced in efforts to increase the $\bar{p}$ accumulation rate are (1) maintaining the energy deposited in the $\bar{p}$ production target by a brighter incident proton beam below the point where melting occurs, and (2) increasing the gain slope of the momentum stacking system to transmit the increased $\bar{p}$ flux without unacceptable disruption of the accumulated beam. Several other issues are included in the full T4 summary report.

Presently, the peak energy deposition in the nickel $\bar{p}$ target by this beam is approximately 1000 Joules/gram.[2] This raises the temperature of the target material to very close to its melting point ($\sim 2400\,^{\circ}$K). When higher proton intensities are available, target melting will be prevented by sweeping the beam with a fast dipole kicker so that the incident pulse is distributed over a large area of the target. A proton beam sweeping system has been built but has not yet been tested. It is not yet known what reduction in target heating will brought about by the beam sweeping system. It is likely that significant target R&D will be required to design a $\bar{p}$ production target that can withstand the primary beams that will be available if the Proton Driver is built.

The bottleneck for the transmission of increased $\bar{p}$ flux is the stochastic cooling system that accomplishes the momentum stacking of the antiprotons in the Antiproton Source Accumulator Ring. Any increase in the $\bar{p}$ flux must be accompanied by a commensurate increase in the gain slope of momentum stacking system. This however, increases detrimental interactions between the momentum stacking system and the core of the accumulated $\bar{p}$ beam severely limiting the peak $\bar{p}$ intensity that can be accumulated. Consequently, any further increases in the ?p production rate will require another storage ring to which the Accumulator beam is transferred when its peak $\bar{p}$ intensity has been accumulated. The Fermilab Recycler Ring is presently being commissioned for this purpose. It is expected that the achievement of the antiproton production rates required for Collider Run II will necessitate the transfer of 20×10^{10} antiprotons approximately every 20 minutes. Significant R&D will be required to extend this scenario to accommodate $\bar{p}$ fluxes greater than the $52 \times 10^{10}\bar{p}$/hour anticipated in Collider Run II.

10.3 Secondary Beams

The secondary beams of interest to the community include neutrino, kaon, neutron and muon beams. Muon beams are also of interest as a basis for neutrino factories and muon colliders.

10.3.1 Neutrino beams.

Three types of conventional neutrino beams are considered: wide band beam, narrow band beam and quasi monochromatic off-axis beam. Current proton beams are $< 10^{13}$ ppp, future proton beams will be $> 10^{14}$ ppp. The limiting aspects for neutrino production and beam lines are target integrity and lifetime, horn performance and lifetime, accurate alignment of the beam line to point to the far detector (GPS survey < 0.01 mrad), beam control and long-term beam stability,

[1] *The Proton Driver Design Study* FERMILAB-TM-2136, December 2000

[2] This is for a 1.6μsec long pulse of 5×10^{12} 120 GeV protons with a transverse dimension (σ) of 0.19 mm incident on a nickel target.

beam monitoring (proton beam profile on target, muon beam profile at the muon pit and neutrino beam at the near detector). We can stay with "conventional" target technologies (a rod-like solid target) for proton beam power below 0.7–1 MW, and will need to switch to new ones (liquid metal jets, rotated band etc.) for higher beam powers. Unique possibilities are provided at the 2 MW Spallation Neutron Source which will produce almost 10^{15} neutrinos in 60 Hz pulses (ORLanND proposal) and a Neutrino Factory with 5×10^{20} muon decays per year in a straight section for a 4 MW proton beam. To take the next step, we need more intense proton sources, targets that withstand high-intensity beams, horns and other focusing devices which survive in very close proximity to the target, totally new ideas about focusing to get narrow band beams with high fluxes.

10.3.2 Kaon beams.

Kaon physics is alive, well and very active. The field is quite mature—many precise, fancy, even elegant beam techniques are in use and under developments at FNAL, BNL, KEK, CERN and IHEP: bent crystal channeling of machine protons to make a K_S^0 beam, "double band" beams with simultaneous K^+ and K^- beams, advanced collimation techniques to control beam tails, experiments driven by "proton blow-torches," superconducting RF separated beams, precision TOF for low energy neutral kaon beams. These new techniques in kaon beam intensity, purity and time structure are allowing a next generation of new experiments.

10.3.3 Muon beams.

Intense pulsed muon beam for the approved MECO experiment will be generated by the AGS 7.9 GeV proton beam of 4×10^{13} p/s. A very elegant production and collection system based on superconducting solenoids, will provide a 50 ± 20 MeV muon beam of 10^{11} per second with best reach to study $\mu \to e$ conversion. Stages 2 and 3 of the neutrino factory/muon collider plan, call for 0.2 and 2.5 GeV muon beams to be used directly and as a source of intense neutrino beams. They can provide up to 1.7×10^{21} decays per year with a 4 MW proton driver.

10.3.4 Targetry.

List of targetry issues includes particle production, collection and monitoring, background suppression and control, target and capture component integrity and lifetime, superconducting coil quench stability, heat loads, radiation damage and activation of materials near the beam, spent proton beam handling, and numerous shielding issues from prompt radiation to ground-water activation. All these issues are addressed in active R&D efforts: novel designs for high-performance secondary beams, shower simulation code developments and studies (MARS), thermal and stress analysis (ANSYS at FNAL and BNL), magnetohydrodynamic analysis (FronTier at BNL), instrumentation for target shocks, target experiments (E951 at BNL), particle production experiments (HARP at CERN and P-907 at FNAL).

Chapter 11

Snowmass Working Group T5 Beam Dynamics

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Though great progress has been made, instabilities remain important.For lepton machines the prevention and damping of transverse mode coupling instability (TMCI) is crucial, it has yet to manifest in hadron machines but care is needed. An effective damping scheme is needed and fully coupled calculations will help weed out ineffective methods. This is true for all stability calculations. All relevant electromagnetic processes including for example detuning wakes should be examined. With its small revolution frequency, electrodynamics of the VLHC involves new sort of quasi-static effects that deserve special attention.

Electron clouds are dangerous both from the transverse two stream instability that can result and increase heat-load in cryogenic system. Recent data suggest significant survival of low energy electrons striking the vacuum chamber. This must be studied since the estimate of both the cryoload and cloud density could increase by an order of magnitude. Studies on the electron-cloud instability are progressing. A linear response model agrees well with the existing data from positron rings, though why PEPII and KEKB see the instability in different planes is unexplained. A comparison of calculated and experimental scaling laws is warranted. A nonlinear theory appears necessary to explain the instability scaling laws in the PSR.

Space charge effects play a significant role in proton booster synchrotrons. Wide spread supercomputing allows for new level of prediction and control but better theoretical model should be sought as well. With its large radius and small emittance, space charge affects the TESLA damping ring. Beam rounders are used in the TESLA damping ring design to reduce the vertical tune shift. This is a new exciting territory.

The principles of intrabeam scattering (IBS) are well understood, it is time to develop a complete implementation. The reported discrepancies between IBS estimates (up to a factor of two) are almost certainly due to the use of approximate formulas and experimental uncertainties. Both RHIC and ATF/KEK will provide detailed verification of the IBS theory, particularly the distinctive behavior below and above transition.

All sources of beam degradation must be tightly controlled to realize high luminosity linear colliders. Techniques for controlling wakefield induced BBU have been developed both for single bunch and multibunch phenomena. Emittance degradation due to mismatch and filamentation can be reduced by precision alignment and control of all vibration sources.

Minor perturbation of the bunch tails (banana effect) could lead to significant luminosity reduction due to the complex interaction of the colliding beams. Methods to mitigate the effect are being explored. The influence of strong damping wigglers in damping rings on nonlinear beam dynamics needs to be fully understood.

Beam cooling techniques are useful to achieve high luminosity operation in many colliders. Stochastic cooling in the microwave frequency range is routinely used in antiproton accumulators. The principles of electron cooling have been extensively demonstrated for low energy hadron beams. High energy electron cooling will be implemented in the Fermilab Recycler Ring. This will be important to the high luminosity operation of the Tevatron. High energy electron cooling may also be implemented for luminosity enhancement in RHIC and PETRA using electron beams generated by a super-conducting linac with energy recovery. Optical stochastic cooling using high power laser amplifiers may provide a drastic increase in cooling rate.

To realize muon colliders with reasonable luminosities, ionization cooling by a factor of 10^6 in the 6D phase space is needed. This is being intensively studied. Neutrino factories do not require longitudinal cooling. Transverse cooling in a linear cooling channel involving liquid hydrogen absorbers, RF cavities, and a solenoidal focusing lattice looks feasible. It requires a new regime of beam dynamics due to large aperture beam transport, strong nonlinearities, and the role of angular momentum. Two major simulation codes, GEANT4 and ICOOL, have been developed and cross-checked. Efforts are underway to provide an analytic understanding of the ionization cooling starting from a linear description and systematically adding nonlinear effects. Longitudinal cooling via emittance exchange is under study. Several schemes have been proposed.

In the past, weak-strong beam–beam simulations codes have been valuable for the design and operation of high luminosity colliders, such as LEP. Strong beam–beam simulation codes have recently been developed into powerful tools in the study of beam–beam effects in high luminosity colliders. These codes have been used to compare and optimize the operation of PEPII, KEKB, and CESR. Besides the optimization of these high energy colliders, the codes can be used to study effects, such as coherent beam-beam modes on beam instabilities, the scaling law of beam–beam tune shift vs damping decrement, and correction schemes. Compensation schemes include wire correctors to compensate the long range beam–beam effects for the LHC and electron lenses to compensate beam–beam effects for antiprotons in the Tevatron. These are important experiments. Experimental studies of beam-beam effects with round beams should be carried out. The schemes of round-beam transformer, and of fully coupled betatron motion, should be further studied.

Sophisticated map methods have changed the way we design accelerators. The parallel development of pure theory and real-world applications provides a model for the study of beam dynamics. Maps can provide fast and reliable tracking and accurate modeling for nonlinear resonances. This played a key role in the design and construction of B-factories. It is expected that this design tool will be used in future high luminosity colliders. Notable improvements in the eliminating chromatic aberration at the IP of a linear collider have been achieved.

Accelerator development for high brightness requires instrumentation pushed to the sensitivity frontier. Employing model independent analysis (MIA), the sensitivity of instrumentations can be greatly enhanced. This technique, coupled with computing power, will become an indispensable tool in large accelerator complexes.

Techniques in polarization preservation have matured by using full snake, partial snake and rf dipole. Experiments in medium energy accelerators such as the AGS and RHIC will test spin dynamics at the high energy frontier. Some of these issues are the rf spin flip, snake resonances, spin chromaticity, and spin diffusion. Electron polarized sources with a high quantum efficiency will continue to play important roles in future linear colliders. A polarized positron source may be obtained from the pair production of circular polarized photons.

Chapter 12

Snowmass Working Group T6 Environmental Control

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12.1 Scope.

For the next generation of large accelerators, the civil engineering of accelerator tunnels and associated underground buildings will be a major component of the technical challenge of constructing such machines. Between a sixth and a half of the total costs for these machines must be used for the civil engineering. Because of the large physical scales of these machines the engineering will be required to be as cost-effective as possible, and because the considered beam sizes are of nanometer scale, issues such as structural and thermal stability, ground motion and artificial sources of vibration in the environment will need to be carefully studied. The working group concentrated on tunneling, ground motion, stability, alignment and environmental issues.

12.2 Ground motion.

Known information on ground motion (spectral, correlation) suggests that the considered machines (NLC, TESLA, VLHC, Muon source) are feasible. Particular concerns for each of the machine are summarized below.

In the VLHC the main effect of ground motion is emittance growth; for the high energy stage, the rms uncorrelated motion of 0.3nm above $\sim 250\,\mathrm{Hz}$ would result in doubling the emittance in ~ 2.5 hours. This is still a modest growth rate in comparison with the one for TMCI and resistive wall instabilities that would need to be cured by feedbacks. The natural ground motion in deep tunnels is much smaller than 0.3nm above $\sim 250\,\mathrm{Hz}$, the concern for VLHC is not the natural ground motion, but vibrations that may be created by equipment installed in the tunnels, the enhancement of vibrations by girders and internal mechanics of cryostats. These issues need to be addressed in design and further engineering tests.

In linear colliders the primary concern is beam offset at the IP induced by ground motion. In the TESLA and NLC designs, the tolerance for uncorrelated motion of quadrupoles is about 10nm, though the relevant frequency range roughly defined as $f > F_{\mathrm{rep}}/20$ is different ($f > 0.2\,\mathrm{Hz}$ for TESLA and f>6Hz for NLC). For the NLC case, even in modestly quiet sites, the motion is below these tolerances. For TESLA, due to low repetition rate of collisions, the motion, even in quiet sites, may reach the tolerance limit. However, due to large separation between bunches, a correction within a bunch train is possible for TESLA. An issue of concern for NLC, and to a lesser extent for TESLA, is cultural noise that may greatly increase vibration in the tunnel. In an urban area, a deep tunnel solution appears to be the best alternative. Local geologic factors (soil and rock stiffness, structure and water table) will strongly influence the in-tunnel vibration characteristics. Site-specific models of vibration propagation need to be studied in more detail. In terms of slow ground motion (minutes to months), the impact on NLC performance is more serious than on TESLA due to higher RF-frequency. Nevertheless, measured amplitudes are tolerable for NLC with a shallow site in glacial till being the most critical case. Studies are planned that would clarify this conclusion.

12.2.1 Site criteria and technical requirements.

High Energy Physics frontier accelerators are large and complex. Ideally, they should be constructed close to an existing laboratory site. The environmental impact of the project is minimized for a tunnel solution rather than a cut and cover that would involve greater surface disruption. In many respects, the tunnel design requirements for the beamline housings are not unlike the requirements for underground rail or metro tunnels. However, some key requirements, related to stability and watertightness, are more stringent than those normally associated with underground design. Meeting such criteria could be difficult to achieve in some ground units and may require design and construction mitigation measures that are not currently accounted for within in the framework of the pre-project plans. Better knowledge of key design parameters of certain ground units is necessary in order to be able to evaluate, with some confidence, the types of design mitigation measures that will be needed to meet stability and watertightness requirements.

Subsurface ground conditions. None of the projects have performed site investigations of the subsurface conditions (borings, seismic work or laboratory testing) along a specific tunnel alignment. At present, TESLA is the only project that has selected a tunnel alignment. Site- specific investigation of this alignment is scheduled to start soon. Confidence in ground conditions along the TESLA tunnel route is already fairly high given the relatively large amount of existing geologic, geotechnical and construction reference data available in the Hamburg area. Based on this data, site conditions along the alignment are projected to be similar to those encountered during the construction of HERA. There is only a limited amount of geological, geotechnical and construction data available to describe some of the ground units in which the proposed NLC and VLHC tunnels will be sited. For these ground units there is a need for additional geotechnical data to be gathered before realistic plans and costs for excavation and tunnel construction can be developed with confidence. Geotechnical data and design studies are needed in the following key areas: For the California and Illinois Tunnels sited in Expansive Shales: The impact of swelling pressures and/or displacement on the excavation, arch support and foundations of beamline housings needs to be studied. For the VLHC tunnels sited in St. Peter Sandstone: The impact of groundwater, in situ stresses and presence of abrasive minerals on the excavation and support of beamline housings needs to be studied. For California sites: geologic and geotechnical properties related to tunneling and cut and cover excavation and long term facility stability; and groundwater conditions. For the Illinois Tunnels and Halls: The impact of high horizontal in situ stresses on the excavation and support of tunnels and, in particular, any large span openings (e.g. Interaction Regions), needs to be studied further. The Muon Source facility sited at Fermilab (the only site presented) benefits from geotechnical data archived from other projects, most recently the Main Injector and NuMI. Geotechnical parameters are anticipated to be similar with those collected for other local projects.

12.2.2 Construction issues.

The layout and construction concepts being developed for TESLA will be largely consistent with those of the HERA Project. The design concepts for VLHC and NLC are still evolving. VLHC is looking at two representative sites in northern Illinois. NLC has identified a number of representative sites in California and Illinois. Cut and cover, cut and cover-tunnel combinations and various tunnel layout options are being studied. To date, none of these layouts has been subject to either "constructability" or value engineering reviews. Constructability reviews are designed to ensure that the layouts being developed to satisfy end- user requirements could actually be built cost-effectively using standard industry equipment and materials. Value Engineering reviews would enable technical and conventional designers to perform trade-off studies in the different areas of the project with the aim of identifying lower cost solutions that still respect the functional requirements of the project.

12.2.3 Conclusions and recommendations in terms of tunneling.

For the VLHC and NLC sites, it is important that a scope be developed for preliminary site investigation requirements. The Scope of the investigation of proposed sites should identify key design issues. For the VLHC and NLC sites, it is important that a process be established for reducing the number of potential sites and selecting a single site as soon as possible. A prioritized list of site selection criteria should be developed that can be used to help select specific sites. All the projects would benefit from constructability and value engineering reviews. These reviews should be undertaken with the participation of industry professionals at key moments in the design process. In the future there may be potential for the use of R&D products on one or several of the proposed projects. However, cost benefits are only likely to be achieved if bidding contractors have seen such products successfully applied underground and such products are stated to be acceptable within the construction contract. It is recommended that on-going R&D projects continue to be actively monitored and periodic assessments made to evaluate if cost savings can be achieved through the adoption of a R&D product on a given site. To date, project plans for underground work have largely been developed in-house, at individual laboratories, with indirect input from the underground industry. The formation of an underground advisory panel is recommended to improve access to tunneling expertise and help develop and coordinate plans for site investigations, designs and technical reviews for all the projects. It is recommended that the panel include international members who can relate recent underground construction experience from overseas locations, such as the Australia, Europe and the Far East.

Chapter 13

Snowmass Working Group T7 High Performance Computing

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Particle accelerators are among the largest, most complex, and most important scientific instruments in the world. They have enabled a wealth of advances in applied science and technology, many of which have huge economic consequences and many of which are greatly beneficial to society. They are also critical to research in the basic sciences (such as high energy physics, nuclear physics, materials science, chemistry, and biology). In particular, accelerators are the most versatile and powerful tools for exploring the elementary particles and fields of the universe. Experiments associated with high energy accelerators led to some of the most remarkable discoveries of the 20th century. Near-term experiments are likely to be just as exciting, if not more so, with the possible discovery of new physics beyond the Standard Model, such as supersymmetry and its associated implications for a radical new geometry of space-time, which will fundamentally change our view of the universe.

Given the great importance of particle accelerators, it is imperative that the most advanced computing technologies be used for their design, optimization, commissioning, and operation. The objective of the High Performance Computing (HPC) Working Group is to understand the modeling needs for current and future accelerator technology, identify the HPC hardware and software technologies required for such modeling, and outline a plan for the development of these technologies. The following summarizes the HPC requirements for next-generation accelerators and describes an action plan that responds to the identified needs.

13.1 HPC requirements for next-generation accelerators.

All near- and far-future accelerator designs have very challenging modeling requirements that require HPC:

High intensity proton drivers needed for conventional neutrino "Superbeams," neutrino factories, and muon colliders require precise predictions of the effects of space charge. This need is shared by currently operating proton drivers, like the FNAL Booster and the BNL AGS, which are experiencing significant losses, currently attributed to space charge effects at injection. The losses at the FNAL Booster are currently the biggest issue for the success of the near future FNAL program (RunII+neutrino program). Due to the nature of this type of problem—which involves long (high aspect ratio) bunches propagating for thousands of turns including space-charge effects and wakefield effects—a full 3D simulation is prohibitive using the current algorithms and existing multi-processor hardware. Simulations using roughly 100 processors have been estimated to require 1 year of computer time.

Next-generation linear colliders require demanding computer simulations in regard to both electromagnetic and beam dynamics modeling. For example, extremely complicated 3D electromagnetic structures for the NLC must be modeled and analyzed with greater speed, accuracy, and confidence than has previously been possible. Presently popular serial electromagnetics codes are inefficient in handling complex geometric shapes, or are limited in their ability to solve large-scale problems. However, the recent development of parallel eigenmode and time-domain codes has already increased our modeling capabilities by roughly three orders of magnitude. In addition to modeling electromagnetic components, HPC capabilities are needed to model beam dynamics in linear colliders. For example, in both the NLC and TESLA designs, the accurate treatment of space-charge effects and other collective effects is important to predicting the beam's behavior in the damping rings. In order to validate the basic operational characteristics of these machines, the linac and beam delivery systems need to be modeled including component fluctuations, tuning, and feedback systems. Such simulations are impossible on serial computers, where the execution time to run one such code with the desired accuracy has been estimated to be 1 year per processor.

Very large hadron colliders like the VLHC require HPC capabilities in areas such as long-term tracking to predict dynamic aperture, self-consistent simulations of beam–beam effects in the strong-strong regime, predicting the thresholds for instabilities (such as the electron–cloud, resistive wall, and transverse mode coupling instability), and the simulation of beam/material interactions (e.g. energy deposition from collision byproducts) that address safety and environmental issues. In addition to these "conventional" requirements, there are also "operational" ones involving the use of HPC to develop orbit correction algorithms, alignment procedures, etc., that are challenging due to the size of the machine, the large amount of diagnostic data, and the short period of time in which the analysis has to be performed. Here accelerator simulation is used in a similar way to HEP experiment simulation: accurate modeling of the machine and diagnostics are used to develop and optimize analysis algorithms, which are then used to optimize machine operation. Like a linear collider, full system simulations of the VLHC including beam dynamics and feedback systems are needed to verify operational characteristics of the proposed design.

Neutrino source/muon colliders present unique modeling challenges due to the fact that they involve ionization cooling. Ionization cooling requires accurate modeling of muon/matter interactions, especially energy loss and multiple scattering. There are a few codes that share the physics description of the above processes borrowed (or directly implemented) from HEP modeling packages. These codes are very slow, prohibiting accurate simultaneous optimization of the sub-systems of the design, although in many cases both performance and cost of these sub-systems are dependent on each other. In addition, for high intensity muon colliders space charge effects are crucial at the final stages of cooling. In both cases HPC is needed. An initial effort to embed cooling simulation capability in an HPC beam dynamics code has been successful, providing a good base for further development.

Besides the design of next-generation accelerator complexes, HPC is also needed, in concert with theory and experiment, to explore and develop novel methods of acceleration like plasma-based and laser-based acceleration techniques. Using these techniques, extremely high gradients (up to 100 GV/m) have been measured over short distances in the laboratory. The challenge is to control and stage high-gradient sections so that one can produce high quality, high energy beams in a less costly, more compact configuration that would be impossible using conventional technology. Beyond applications to HEP, such compact accelerators would have huge consequences in others areas of basic and applied science, industry, and medicine. However, modeling these complex systems requires solving the 3D coupled Maxwell/Vlasov equations. Given that the phenomena involve multiple length and time scales (a situation that is particularly challenging when the laser wavelength must be resolved), 3D simulations can only be performed using HPC resources. As an example, the simulation of a 1 GeV plasma accelerator stage using a fully explicit PIC code has been estimated to require 10,000 to 100,000 CPU hours for a single run.

13.2 Action Plan

Recognizing the challenges posed by these and other projects, a SciDAC (Scientific Discovery Through Advanced Computing) project on 21st Century Accelerator Simulation was approved in mid-2001. The primary objective of this national R&D effort is to establish a comprehensive terascale simulation capability for the US Particle Accelerator Community. The success of this effort, which is supported by both HENP and ASCR, will involve close collaboration of accelerator physicists with applied mathematicians, numerical analysts, and computer scientists to develop new theoretical formulations and new algorithms capable of high performance and scalability on massively parallel systems. In particular, the accelerator community will utilize HPC tools for mesh generation, mesh refinement, particle/mesh methods, multi-level PDE solvers, eigensolvers, performance optimization, software component integration, and visualization. Many of these tools will be developed in the SciDAC Integrated Software Infrastructure Centers. Code verification and validation will require collaboration of code developers working with researchers performing controlled, well-instrumented experiments.

As a result of the Snowmass meeting, a plan defining the necessary first steps needed to respond to the design needs of the next generation machines was formulated. This plan includes further development of HPC space charge codes for circular machines, a 3-month code comparison effort to test the accuracy and validity of the various models, and the simulation of existing proton drivers such as the FNAL Booster and the BNL AGS. The plan also includes continued code development needed to treat, on parallel computers, physical effects such as the beam-beam interaction, collisions, wakes, and coherent synchrotron radiation. In regard to electromagnetic modeling, the plan includes the development of a parallel statics solver, the treatment of lossy structures, surface effects, and the direct calculation of wakefields. In regard to laser- and plasma-based accelerators, the plan includes the development of a family of codes (fluid and particle) of varying complexity and capabilities, the most demanding of which are fully 3D parallel PIC codes, with moving windows and dynamic mesh capabilities, that have packages to include physical effects such as ionization of multiple species and the simultaneous treatment of laser and particle beams. The Working Group also addressed the issue of code integration, including the need to develop reusable software components and the need to adopt standards for exchange of data and interoperability between those components.

13.3 Conclusion

The accelerator community is well positioned to develop a comprehensive terascale capability that will utilize the latest advances in HPC technologies. Such a capability will help insure the success of future accelerators, by facilitating design decisions aimed at controlling and reducing cost, reducing risk, and optimizing performance. The use of terascale simulation, combined with theory and experiment, will provide greater understanding of the complex, nonlinear, multi-scale, and many-body phenomena encountered at the frontier of accelerator technology.

Chapter 14

Snowmass Working Group T8 Advanced Acceleration Techniques

Conveners: Phillip Sprangle (Naval Research Lab.), Chan Joshi (UCLA)

Conveners of Subgroups: A. Ting, E. Esarey, W. Leemans, D. Umstadter, T. Antonsen, W. Mori, T. Marshall, P. Chen and J. Hirshfield

There is a small but vigorous community working on advanced accelerator concepts in the United States. This effort, principally supported by the DOE, is important for the long term vitality of High Energy Physics (HEP). In addition, the program contributes essential technology and accelerator science to the benefit of all fields using accelerators in their research. Although the research is not directed at any particular project, such as the NLC, its long term focus, i.e., 10 years or more, is to advance the state-of-the-art for HEP. It addresses fundamental issues which could lead to new or improved, high-gradient accelerators, rf sources, computational techniques, beam control devices and new diagnostic tools. The advanced accelerator research provides an exciting and stimulating field of physics, which continues to attract young and talented researchers. This community is also responsible for a large number of high quality scientific publications and is invaluable as a training ground for new Ph.D. students.

Over 75 invited talks were presented within the T8: Advanced Acceleration Techniques Working Group. These talks highlighted the recent progress, developments and results in the field. The AA program is progressing along many fronts, one of which is the techniques for next generation of advanced accelerators. Experiments are being designed to produce an electron beam of well-defined energy in the multi-GeV range. The first generation laser wakefield accelerator (LWFA) has generated ?100 MeV electrons with an accelerating gradient of $\leq 100\,\mathrm{GeV/m}$ and energy spread of $\sim 100\%$. The second generation LWFA will require optical guided beams, properly controlled phased beam injection and stable wakefield generation.

To increase the acceleration length, the high intensity laser pulse must be optically guided in a plasma channel. This has been demonstrated over distances of many ten's of Rayleigh lengths (several centimeters) at several institutions, e.g., NRL, U.MD, U. Texas and LBL. A tapered plasma channel with a drive laser of ten's of TW and optical laser injection may lead to a final energy of several GeV in a distance of several ten's of cm. To have a well-defined accelerated beam energy an injected beam occupying a small phase angle is necessary. Several all-optical injection concepts are being investigated that may be capable of producing such pulses (U. Mich, NRL, LBL, UCLA).

The plasma wakefield accelerator (PWFA) mechanism utilizes a relativistic electron beam propagating in a plasma to excite a large amplitude wakefield which accelerates the tail end of the beam. A number of laboratories (UCLA, FNAL, SLAC, ANL) are presently performing experiments on this concept. One of these experiments is the E-157 project at the FFTB of SLAC. This joint effort (SLAC/UCLA/USC/LBL) involved the propagation of a 30 GeV electron beam through a 1.4m plasma column in the blowout regime of the PWFA. Simulations indicate that an accelerating gradient of $\sim$ 1 GeV/m can be achieved. The experiment has already observed, a) the betatron oscillation of the electron beam and related synchrotron radiation, b) induced transverse effects such as hosing, and c) electron beam refraction as the beam crosses the beam plasma boundary. An ongoing related experiment is the E-162 joint project (SLAC/UCLA/USC), in which a 30 GeV positron beam propagates through the plasma column. Experimental results clearly demonstrate that the plasma column acted as a focusing lens for the positron beam. Results on a related experiment, E-150, were presented in which focusing of both electrons and positrons by a factor of 2 was observed using a thin plasma lens.

The Neptune Laboratory at UCLA is being used for 2nd generation experiments on the plasma beat wave acceleration (PBWA) of electrons, plasma wake field generation and acceleration, plasma lenses, IFELs and Cherenkov wakes in magnetized plasmas.

Recently proton acceleration experiments (GA, U.Mich) have observed high energy $\sim$ 10–50 MeV protons from surface contaminants when a high intensity laser pulse is focused onto a thin solid target. The resulting proton beam can have a small energy spread and emittance ($\sim$ 1mm-mrad), but a significant bunch charge ($\sim$ 1nC). The accelerated proton pulse may find applications in basic nuclear physics studies, fast ignitor fusion, production of radionucleides, and injectors for ion accelerators.

The computational community is developing a hierarchy of new codes for AA research. Full-scale 3D modeling is presently at hand, and it is expected that the computational run time can be reduced from a month to minutes with a combination of reduced description particle models and parallelized algorithms. New rf sources are being developed either as candidate tubes for future colliders operating from 11.4 to 91 GHz, or simply to carry out high-power tests of structures and components. High frequency gyroklystrons are being developed at the U. MD (80 MW design at 17 GHz) and Calabasas Creek Research (10 MW design at 91 GHz). Magnicons are being developed at 11.4 GHz (NRL/ Omega-P, Inc.) and 34 GHz (Omega-P, Inc.)

The three largest areas of work in the non plasma area are the IFEL, dielectric wake field acceleration (DWFA), and small vacuum structures. The STELLA IFEL experiment at the ATF facility at BNL has demonstrated phasing of two IFEL stages which required the first stage to bunch the picosecond long beam into $\sim 3fs$ microbunches. These microbunches were subsequently injected into the next IFEL stage with precise phase control. The IFEL, while it cannot achieve TeV energies, can contribute to parts of a staged accelerator system, or as an injector for plasma-based accelerators. A new method of chopping ps bunches into fs pieces by the LACARA (Yale/Omega-P/Columbia) has been devised and will be tested. Tests of optical structures (Stanford University) for vacuum acceleration, are planned for the near future.

The field of wakefield accelerator research is demonstrating great progress. The upgraded ANL facility for wakefield studies was presented. A successful test has been made of their two-beam accelerator concept, and higher energy tests are planned that may soon demonstrate gradients in excess of 100MeV/m. A test at NRL of the ANL dielectric-loaded TM01 slow-wave structure using high power X-Band microwaves generated by a magnicon is planned soon. Whereas most wakefield work involves exciting a spectrum of microwave TM modes in a cylindrical dielectric wakefield device, it was pointed out (Columbia) that one might well imagine tall rectangular dielectric structures having optical-scale dimensions, that would be excited by fs bunches containing pC of charge. If the issues of stability and breakdown can be resolved, the dielectric wakefield accelerator (DWFA), which may have gradients of 100MeV/m to 1GeV/m, may play an important role in accelerator physics of the future.

In summary, plasma wakefield schemes have demonstrated jets of electrons and ions with broad energy spectra and impressive acceleration gradients, exceeding 100 MeV in a mm. Presently research is directed towards a second generation of wakefield device employing various injection and channel guiding schemes to produce relatively monoenergetic beams in the GeV range. Several facilities around the country are engaged in this research, including, NRL, ATF (BNL), Neptune (UCLA), L'OASIS (LBNL), AWL (ANL) and the planned ORION facility at SLAC. From the E-157 experiments, an idea for an energy doubler for a linear collider has emerged, called the Afterburner. The advanced accelerator community may, within 3–5 years, propose application of these ideas to the HEP community.

The schedule for the T8: Working Group on Advanced Acceleration Techniques was:

Subgroups	**No. of Talks**	**Subgroup Convenors**
Plasma Based Acceleration	38	P. Sprangle, A. Ting, E. Esarey
Plasma Based Injectors	8	W. Leemans, D. Umstadter
Computational Techniques (Joint with T7)	8	T. Antonsen, W. Mori
Non-Plasma Based Acceleration	13	T. Marshall
Plasma Based Processes	5	P. Chen
Advanced RF Sources (Joint with T3)	8	J. Hirshfield

Chapter 15

Snowmass Working Group T9 Diagnostics

Conveners: Ralph J. Pasquinelli (FNAL), Marc Ross (SLAC)

Working Group Participants:
A. Fisher , SLAC, D. Sagan, Cornell, F. Decker, SLAC, J. Frisch, SLAC, H. Sakai, KEK, J. Corlett, LBL, S. Peggs, BNL, R. Webber, FNAL, G. Bonvicini, Wayne State, A. Zholents, LBL, A. Wolksi, LBL, M. Minty, DESY, F. Pilat, BNL, P. Catravas, LBL, C. Clayton, UCLA, S. Smith, SLAC, J. Fox, SLAC, M. Wendt, DESY

15.1 Survey of Machines

The diagnostics T9 group was charged with reviewing the diagnostic requirements of the proposed accelerators for the future. The list includes the e^+e^- colliders, Muon Neutrino source, NLC, Proton Driver, TESLA, VLHC. To answer the Charge to the group we organized joint sessions with M1, M2, M3, M4, M6, T1, T4, T5, T6 and T8. In addition, due to their overwhelming importance, we held a special session on position monitor systems. For each of the joint M sessions we generated a table of required diagnostic systems, selected the highest priority items using a ranking based on need and RD effort, and pondered a RD path leading from the present state of the technology to a system satisfying the requirement. We used the joint T sessions to collect up to date RD plans and to assess the applicability of new ideas in a broad range of topics.

15.2 Common diagnostic requirements.

All of the machines of the future have parameter lists that exceed those of present operating machines by as much as one or two orders of magnitude. The beam energies involved are impressive and have the power to melt beam pipes or anything in their way with just one pulse. This makes the role of diagnostics a challenging one in that they are no longer just a means of commissioning and trouble shooting accelerator operations, but also fundamental to the protection of personnel, environment, and accelerator hardware.

Certainly the list of conventional BPMs, profile scanners, beam current monitoring, and longitudinal diagnostics exist for each of the machines. Due to the large physical size of many of the proposed future accelerators, the number of channels of such conventional diagnostics is substantially larger than current installations. As such, reliable engineering is required to sustain system performance. In applications that require protection of personnel or the environment, redundant systems will be necessary. A level of engineering reliability approaching that of a "NASA" type system may be necessary. With all the talk of a "global" accelerator network, reliability will be of paramount importance.

For most of the accelerator installations, an extensive amount of diagnostics must be located in potentially high radiation areas of the tunnel. There are plans for installing hardware in caverns excavated into the tunnel walls. Issues associated with power distribution, heat dissipation, and communication links must also be addressed. This is an area where all the machine design groups could benefit from collaboration.

The precision and resolution of the diagnostics have been defined by the machine designers, but with little input or feedback from the diagnostics designers. This lack of symbiotic approach has led to shortcomings that will be difficult to overcome once the machines are built.

Many of the proposed new diagnostics are quite complicated devices in themselves. From the operational aspect of the machine, the diagnostics cannot be an "experiment" that requires as much tender loving care as the accelerator itself.

The data collection and communication systems will need to have considerable bandwidth. Thousands of channels of BPMs or profile monitors will need to be networked with countless feedback loops. This could be a control engineer's dream or nightmare depending on the implementation.

15.3 Commissioning

Almost uniformly, each of the proposed machines has not prepared an extensive commissioning scenario. This shortfall will mean that the required diagnostics may not be available when startup commences. Historically, diagnostics have not been given the priority of other accelerator systems. Diagnostics critical to commissioning are often not necessary on a daily operational basis, hence they are de-emphasized. The diagnostics will be expected to perform with the same precision and resolution during commissioning, making strong demands on hardware dynamic range.

15.3.1 Focus

All of the proposed machines have focused on main subsystems such as RF power sources, accelerator structures, magnets, …Each of these areas are consuming most of the monies and resources. Before the conceptual design report is completed, similar attention must be given to diagnostics. The costs associated with each of the proposed accelerators are larger than anything the field has experience. These large machines will also be very expensive to operate. A strong diagnostic system will allow for the most efficient use of the funds allocated to future projects.

There is also a need to do substantial prototyping of the hardware. Once prototypes have been built, they will need to be tested in environments that simulate the future machines. This means putting hardware in radiation environments, high magnetic fields, cryogenic environments, and commensurate beam tests. Some of these tests will necessitate the testing in current operational accelerators or beam experiments. The field should invest in the future by accommodating requests for such specific tests.

15.4 Summary of highest priority diagnostic requirements—sorted by machine

15.4.1 Muon based systems—M1

Beam profile and emittance diagnostics are vital for the muon ionization cooling demonstration projects. A number of promising proposals are in progress; all of which substantial innovation and development in their own right. Perhaps tightest of all is the requirement to measure the decrease in muon emittance to an accuracy of a few percent.

15.4.2 e^+e^- storage ring factories—M2

Factories are faced with coupling/optical correction, two-stream instability and strong beam-beam effects. 1) The BPM system is the most critical diagnostic in factories, with difficult bunch-to-bunch and front end signal processing and stability requirements. Because of the complexity of the IR, absolute stability of 1um/24hrs is important near the IP, a requirement today's systems don't meet. 2) Transverse profile requirements are well below the optical synchrotron radiation diffraction limit. RD is required to improve the utility of devices such as the interference fringe monitor. The most serious instability encountered in these machines is the electron cloud instability; detailed RD is required to understand this serious limitation. 3) KEKB is operated very near the ½ integer resonance, requiring a very well understood lattice. Highly integrated precision tools for determining lattice functions are required.

15.4.3 Linear Collider (LC)—M3

The linear collider requires precision diagnostics because of its small beams and pulsed nature (SLAC PUB 8437 May 2000). Requirements for 1) BPM systems include sub-micron resolution; requirements for 2) robust, precise profile monitoring (transverse) force the use of laser-based profile monitors. The combination of small beam size and large aspect ratio make laserwire resolution $> \sigma_y/3$ unless a short wavelength laser is used. This problem is common to all LC designs. Since accurate profile monitors are the best predictor of luminosity, an evaluation of the optics and the required laser performance throughout the machine is required. X-ray interferometry may be useful down to IP sizes. 3) No good bunch length monitor is available. This is an active RD effort. An accurate, conservative design using transverse deflection cavities exists but will be expensive to apply more than 1 or 2 places. RD focuses on field probes, mm wave interferometry and synchrotron light techniques. RD is needed to evaluate parameters, determine applicability and test the deflection structure design. A special device, measuring the y–z correlation is required, for example to counter the 'banana' effect in TESLA.

15.4.4 Hadron colliders—M4

RD is needed for 1) control of fast instabilities at injection, 2) for diffusion processes in general and 3) for tune/chromaticity control during ramping (Schmickler DIPAC 2000). There is promising RD at RHIC using crystal extraction in order to analyze phase space density at large amplitudes.

15.4.5 Proton driver—M6

The most serious design failure of these machines is 1) an understanding of halo formation and matching. Instrumentation is needed to help distinguish halo generation mechanisms and thus provide information to be used in minimization. A related problem is the determination of longitudinal emittance. There is no viable bunch length monitor below 1GeV. RD on laser-based monitors will be done at SNS. 2) The mechanism for losses at injection into a downstream ring is not understood. RD is needed for injection phase space monitors.