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BERKELEY NATIONAL LABORATORY

Future Options Working Group

LBNL Physics Division

February 1999

Chapter 1

Introduction

The Working Group on Futures Options was established by Physics Division Director Jim Siegrist in March 1998, with the following charge:

> The Future Options Working Group will report back to the Division by June 1 with its evaluations of opportunities for research over the next ten years or so. Its assessments will consider
>
> - The physics potential of various existing and proposed experiments, both in the U.S. and abroad, in light of the recommendations of the Gilman HEPAP Subpanel. Among the options to be explored are hadron and electron-positron collider experiments, fixed-target experiments, neutrino experiments, and astrophysics experiments.
> - The probable availability of Division scientists in light of currently anticipated schedules at SLAC, Fermilab, and CERN, and the particular skills and interests of those scientists most likely to be free to pursue new initiatives.
> - The special technical capabilities available in the Engineering Division as well as in the Physics Division.
> - Opportunities to collaborate with other LBNL divisions, including Nuclear Science and Computing Sciences.
>
> The Working Group should assume that the Division will have constant purchasing power over the period under consideration.

The composition of the Working Group was

David Brown
Bob Cahn (chair)
Stuart Freedman (Nuclear Science Division)
Gil Gilchriese
Young-Kee Kim
Michael Levi
Kam-Biu Luk
Hitoshi Murayama
Saul Perlmutter
Mark Strovink
Bill Turner (AFRD)

Meetings were generally held on a weekly basis, though with less frequency during the summer when many of the members of the Working Group were out of town.

We met as a group or singly with an number of physicists from both inside and outside LBNL to discuss the Groups work. These included

- Debbie Harris, to discuss neutrino physics with a muon-collider source
- Pierre Sokolsky, to discuss Hi-Res/Fly's Eye
- Steve Geer, to discuss muon storage ring sources
- Willi Chinowsky, to discuss AMANDA and other water/ice detectors
- Michael Ronan, to discuss NLC detectors
- George Smoot, to discuss cosmic microwave background

1.1 Current Program in the Physics Division

At present, the Physics Division is involved in four major accelerator experiments, ATLAS, BABAR, CDF, and D0. These provide a comprehensive program in hadron-collider physics and a strong effort in e^+e^- physics. In addition, the Division leads Fermilab experiment E-871, a search for CP violation in $\Xi \to \Lambda\pi \to p\pi\pi$. The ATLAS program is certainly long range, with the first data coming in 2005 at the earliest. BABAR is being installed at PEP-II. Its physics program will extend for many years. CDF and D0 are finishing upgrades that should lead to new running with the Main Injector beginning in 2000. The effective operation of LHC is expected to reduce interest in much of the Tevatron's program.

While it seems likely that all four of the large programs will still be in place in 2005, say, some of the staff on these experiments will seek new opportunities long before then. Indeed, as hardware tasks on BABAR and CDF are completed, there may already be some looking for new projects.

The program in particle astrophysics includes the Supernova Cosmology Project, cosmic microwave background measurements, and work on AMANDA and prospective large water/ice neutrino detectors. The SCP and CMB work has had impressive results, but the questions of Ω and Λ are not likely to be settled soon.

1.2 Other Reports

Two reports addressing the future direction of high energy physics in the U.S. were issued in 1998: the Gilman Subpanel of HEPAP and the National Academy of Sciences study led by Bruce Winstein. In addition, an NSF Special Emphasis Panel issued a narrower study dealing only with B physics.

1.2.1 Gilman Subpanel Report

The HEPAP subpanel issued its report in February 1998. It made a series of recommendations, setting as its highest priority the optimum utilization of the Main Injector, PEP-II, and CESR. It next endorsed U.S. participation in the LHC accelerator and detectors.

It recommended that there be a new facility at the energy frontier. In particular it is urged that SLAC be authorized to produce a Conceptual Design Report, in collaboration with KEK, for a linear collider with $\sqrt{s} = 1$ TeV, extendable to 1.5 TeV. The decision whether to proceed with construction, it said, should be based on a future subpanel. It urged that R&D be extended, as well, on both muon colliders and a VLHC (Very Large Hadron Collider).

The subpanel called for upgrading the Tevatron Collider so that an integrated luminosity 20 fb^{-1} could be achieved before LHC turn-on. It said that only one of the two collider detectors should be upgraded to match the increased luminosity. It also urged that there R&D be directed towards increasing the luminosity at CESR and PEP-II.

The subpanel called for joint evaluation of proposals to either Fermilab or Brookhaven for the use of intense K or μ beams.

The subpanel urged that "Fermilab carefully evaluate the configuration of the NUMI/MINOS facility in light of results becoming available from experiments elsewhere." It endorsed, in a general way, non-accelerator experiments and recommended continuation of SAGENAP.

1.2.2 NRC Report

The National Research Council panel chaired by Bruce Winstein also reported in early 1998. Its recommendations were quite similar to those of the Gilman panel. Briefly, they stated:

- The United States should capitalize on the potential of the Fermilab Collider Facility while it has unique capabilities for investigations of high mass scale physics.
- The committee enthusiastically endorses U.S. participation in the Large Hadron Collider project as a vital and essential component of the U.S. experimental particle physics program.
- The committee recommends support of an international effort leading toward a complete design and cost estimate of an electron linear collider that would be able ultimately to reach a center-of-mass energy of 1.5 TeV and a luminosity of $10^{34}\mathrm{cm}^{-2}\mathrm{s}^{-1}$.
- R&D targeted at developing the technologies for muon and very large hadron colliders should be vigorously pursued.
- The committee recommends strong support for a well-targeted program to study the fundamental particle physics that can best be explored with experiments below the TeV scale.

1.2.3 NSF Special Emphasis Panel Report

The NSF Special Emphasis Panel Report was made final on June 1, 1998, though its major points were reported at a HEPAP meeting in February. The Executive Summary follows:

> The Special Emphasis Panel met to study the opportunities for the Elementary Particle Physics program of NSF in the burgeoning field of B physics. Representatives of the CDF, D0, HERA-B, BaBar, CLEO, BTeV, and LHCb collaborations presented summaries of their programs and plans.

B physics tests a critical element in the Standard Model of particle interactions, which predicts that weak decays of hadrons containing b quarks are governed by the Cabibbo-Maskawa-Kobayashi matrix. The four physical parameters of the CKM should account for all b-decay phenomena, including the anticipated CP violation. To determine whether this is so requires a very broad range of measurements.

Both e^+e^-colliders and hadron machines will participate in the next phase of the B-physics program, starting in 1999-2000. In some areas, the two approaches compete directly, in others one approach or the other has a distinct advantage. None of the experiments beginning at that time will be able to answer all the fundamental questions of B physics. A subsequent phase with higher luminosity e^+e^-machines and detectors specifically designed for B physics at hadron colliders will be required to give a final answer to the adequacy of the CKM matrix explanation of weak interactions and CP violation.

The NSF program in particle physics has already made enormous contributions to B physics, especially at Cornell, where much has been learned about the magnitudes of various elements of the CKM matrix. The Panel believes that B physics should continue to be a major focus of the EPP program.

The Panel recommends for the B-physics program that:

- *Full exploitation of the CESR and CLEO upgrades be the highest priority in the immediate future.*

- *NSF should support efforts in both hadron-collider and* e^+e^- *experiments to explore B physics in the period extending to 2005 and beyond. In particular*
  - Investments should be made in research at CESR to evaluate the prospects for a very high luminosity upgrade, to $\mathcal{L} = 3 \times 10^{34}\mathrm{cm}^{-2}\mathrm{s}^{-1}$.
  - Investments should be made in BTeV to enhance the research and development of the challenging technical components required for this very ambitious but promising program.

- *Around 2001 any proposal from CESR for a luminosity upgrade should be evaluated in the context of the experience of the asymmetric B-factories. If the CESR proposal provides better opportunities than those that would be provided by other* e^+e^- *machines, NSF should strongly pursue the upgrade.*

- *Around 2001, a full technical and scientific evaluation of the BTeV experiment should be conducted. If Fermilab proceeds with the BTeV project and if it is competitive with international alternatives, NSF should give it strong support.*

- *NSF's support of current B-physics research programs (BaBar, CDF, D0, HERA-B) is fully warranted. Arguments for increasing NSF support for the HERA-B program were not persuasive.*

Chapter 2

Physics Issues and Experimental Status

The research program of the Physics Division must address the critical issues in our understanding of fundamental particles and their interactions. The Standard Model of particle interactions is at once remarkably successful and frustratingly incomplete. We know that the weak and electromagnetic interactions are unified but we don't know how the underlying symmetry that joins them is broken. We don't understand the pattern of masses of the quarks and charged leptons. We aren't even certain whether neutrinos have mass, though there is increasing evidence that they do.

The Standard Model describes the particles that were present at early moments of the Universe, but puzzles still remain. There is circumstantial, but not direct, evidence for dark matter. The inflationary scenario is an extremely appealing way of sweeping away embarrasing problems, but its simplest form is not supported by data from the Supernova Cosmology Project (or its competitor) unless there is a cosmological constant or something else exotic. The basic cosmological parameters, Ω and Λ, can be measured both by the supernova survey and by observations of the cosmic microwave background.

The fusion of particle physics and cosmology is part of a larger trend to use particle physics to investigate the cosmos. Neutrinos are a particularly attractive way to explore because they come straight to us, unperturbed by intervening fields or matter.

These issues are sketched in greater detail below to provide context for the later discussion of research opportunities.

2.1 Electroweak Symmetry Breaking

Physics of electroweak symmetry breaking is clearly *the* most outstanding question in particle physics. Since Fermi first wrote down his theory, the weak interaction phenomena have been described by a dimensionful constant: G_F. In the standard model, the Fermi constant is replaced by the expectation value of the Higgs field $v = (\sqrt{2}G_F)^{-1/2} = 246$ GeV within the framework of non-abelian gauge theory. There must be a condensate in the vacuum which spontaneously breaks the $SU(2)_L \times U(1)_Y$ gauge invariance but we do not know the nature of this condensate yet. There is a mounting evidence that this picture is correct from the precision electroweak measurements such as m_W, m_t from Tevatron, in which LBNL Physics Division has been playing a major role. However the fundamental questions remain: Why is this particular energy scale chosen? How does the condensate generate fermion masses? What is the condensate?

Fortunately, we know the energy scale of the problem, and hence we know the center-of-mass energy and the luminosity for the collider experiments needed to address the question. The LHC was designed to discover/exclude the standard model Higgs boson over its entire mass range allowed theoretically. The LBNL Physics Division is actively involved in the ATLAS experiment at the LHC.

It may be possible to increase the luminosity the of Tevatron and thus extend the current search for the standard model Higgs boson. If it is not found, limits may be set at 120 GeV with 2 fb^{-1} and CDF/D0 combined, and possibly up to 190 GeV with 10 fb^{-1} at 95% CL. A 5-σ discovery would require much higher luminosity, such as 30 fb^{-1} to cover 100–120 GeV and 160–180 GeV ranges.

Since the Standard Model is likely to be just a low-energy approximation ($E \lesssim 100$ GeV) of the true theory of electroweak symmetry breaking, model dependences of the signal are of particular interest. Many studies have concentrated on the Minimal Supersymmetric Standard Model. With 300 fb^{-1} luminosity and ATLAS/CMS combined, the LHC experiments will cover the entire MSSM parameter space for Higgs masses, assuming the Higgses don't themselves decay into superparticles. The Tevatron can have improved reach for the MSSM if $\tan\beta$ is large, using the production $b\bar{b}h^0$, $b\bar{b}H^0$, $b\bar{b}A^0$.

An e^+e^- linear collider would see the standard model Higgs boson up to within 10 GeV or so of the kinematic limit for $e^+e^- \to ZH$, independent of details of the models.

2.2 Supersymmetry

If the electroweak symmetry breaking is due to the condensate of an elementary Higgs boson, the electroweak theory runs into a serious inconsistency. The mass-squared of the Higgs boson has no reason to be small. Indeed the quantum corrections to it diverge quadratically. Thus m_H^2 should get a shift of $g^2\Lambda^2$, where Λ is a cut-off at which new physics enters, quite possibly the grand unification or Planck scale, and g is some coupling constant associated with the quantum correction. If the Higgs boson is to play its role in electroweak symmetry breaking, its mass must be very roughly on the scale of 1 TeV. This can only happen if there is an extremely fortuitous cancellation between the initial mass-squared and the gigantic quantum correction.

In a supersymmetric theory, there is an automatic cancellation of the quadratic divergence (which leaves an innocuous logarithmic divergence). Above the supersymmetry-breaking scale, M_{SUSY}, there is no problem, so the quantum correction to the mass-squared is just $g^2M_{SUSY}^2$ (ignoring the logarthmic correction). In sum, the non-supersymmetric theory requires a magical cancellation, while the supersymmetric theory does not, as long as the supersymmetry-breaking scale is on the order of a TeV or so.

LEP-II is still improving the limit on superparticles, mostly sleptons, charginos and neutralinos. Even though there is only a small window left for a possible discovery at LEP-II, there is a particular interest in this window because of the above argument. Tevatron Run 2 will also probe a similar region of the parameter space, mostly searching for squarks, gluinos, gaugino-like charginos and neutralinos. Their sensitivities are comparable and complementary.

LHC is believed to cover the entire range of supersymmetry parameter space motivated by the above fine-tuning argument. Extensive studies have been devoted the efficacy of searches for supersymmetry at an NLC and also to the precision with which the supersymmetric spectrum could be studied. The search will be mainly for squarks, gluinos, and charginos and neutralinos, and also sleptons if they appear in decay products of the above list.

An e^+e^- linear collider would discover/exclude superparticles up to kinematic limit for fermions and up to the beam energy less about 40 GeV for scalars. The search will be fairly independent of model assumptions. Mass and mixing parameters would be measured at a few percent-level accuracy in most scenarios.

If supersymmetry is indeed found, the primary goal of the experiments will change to measuring all parameters precisely and testing models of grand unification, supersymmetry breaking mechanisms, and origins of fermion masses. The combinations of a hadron collider and together with an NLC would provide comprehensive studies of supersymmetry.

2.3 Neutrino Masses and Oscillations

2.3.1 Goals

While neutrino experiments are notoriously susceptible to error, there is increasing evidence that there are indeed neutrino oscillations and thus neutrino masses. The primary goals for neutrino experiments over the next decade will be the convincing demonstration of neutrino oscillations, for both ν_μ and ν_e, and the determination of the mass differences and mixing angles between the neutrino weak eigenstates. Immediate questions include determining whether the oscillations evident in the Super Kamiokande data connect ν_μ to ν_τ or to a sterile neutrino (accepting current evidence that the oscillations are not to ν_e) and resolving the solar neutrino problem.

2.3.2 Overview of Neutrino Oscillations

The fundamental equation describing oscillations between two neutrino species is

$$P = \sin^2 2\theta \ \sin^2 \left[1.27 \frac{\Delta m^2 L}{E}\right] \tag{2.1}$$

where Δm^2 is measured in eV^2, L is measured in km, and E is measured in GeV. This simple equation determines the appropriateness of various kinds of experiments to explore different regions of the $\sin^2 2\theta - \Delta m^2$ plane. When Δm^2 is large, the second factor oscillates rapidly and averages to 1/2. When Δm^2 is sufficiently small, we can use the small angle approximation for the second factor. Then

$$\ln \sin^2 2\theta \ + 2 \ln \left[1.27 \frac{\Delta m^2 L}{E}\right] = \ln P \tag{2.2}$$

so that the boundary of the region covered is a straight line on a log-log plot. The transition between the two region occurs when Δm^2 increases sufficiently that there are no more oscillations. See Fig. 2.1.

The single-event appearance limit is roughly $P_1 = 1/N$, where N is the total number of neutrino events. More precisely, an appearance experiment

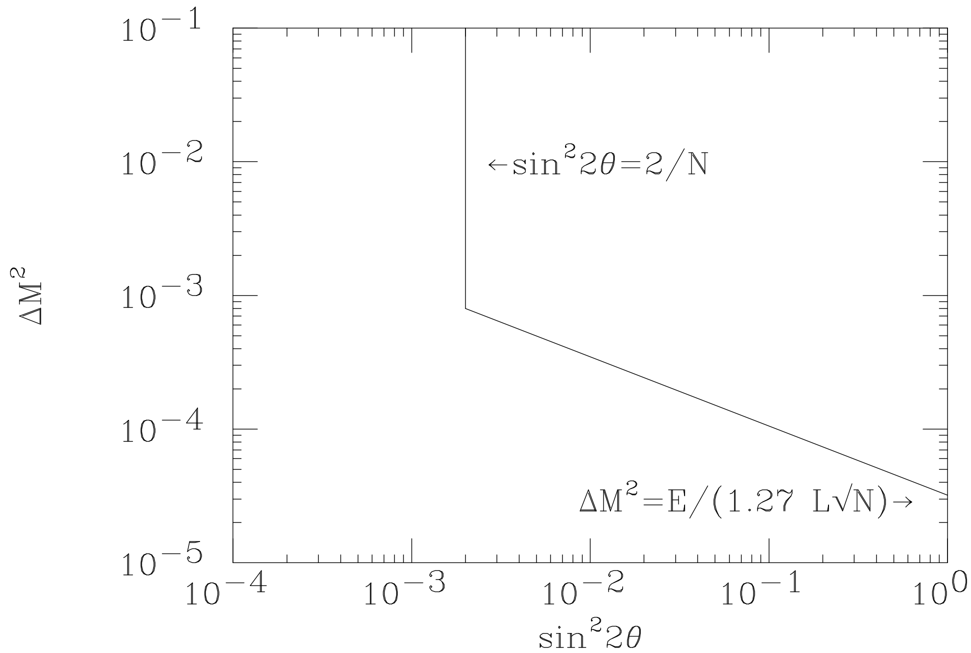

Figure 2.1: The region accessible in an appearance experiment with N observed regular events and no events of the desired type. The value $1/N$ should be replaced by the appropriate limit taking into account Poisson statistics. More properly, N should be replaced by $N\eta$, where η is the reconstruction efficiency for the appearance events.

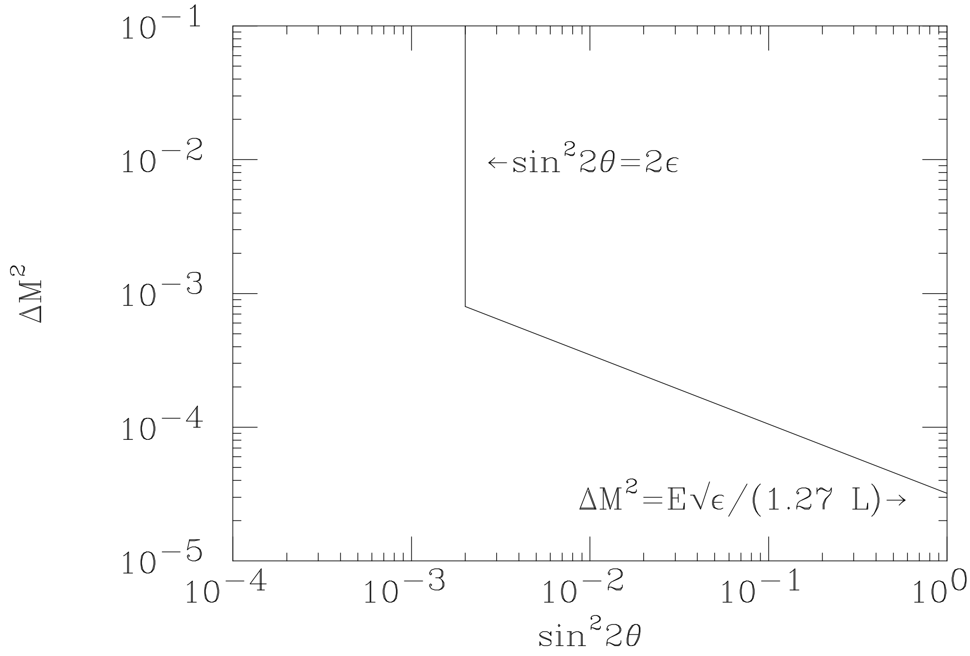

Figure 2.2: The region accessible in a disappearance experiment with the limit ϵ on disappearance.

sets a limit on the probability of oscillation, averaged over the energy spectrum of the incident neutrinos. This limit must take into account threshold suppression if the produced lepton is a τ.

Now consider the lowest Δm^2 measurable with $\sin^2 2\theta = 1$. This is given by

$$\Delta m^2 = \frac{E}{1.27L}\sqrt{P_1} \tag{2.3}$$

In a disappearance experiment (using nuclear reactors, for example), a limit ϵ is placed on the deviation of the event rate from what is anticipated in the absence of oscillations. There are contributions to ϵ from systematics and possibly from statistics as well. At complete mixing, $\sin^2 2\theta = 1$, the limit on Δm^2 is

$$\Delta m^2 < \frac{E\sqrt{\epsilon}}{1.27L} \tag{2.4}$$

From these simple observations, it is clear that for appearance experiments we want to maximize the number of appearance events:

$$N_{\text{appearance}} \propto \sigma\Phi \left(\frac{\Delta m^2 L}{E}\right)^2 \tag{2.5}$$

where σ is the appearance production cross section (e.g. for τ from ν_τ) and Φ is the neutrino flux. We assume that Δm^2 is small. Far above threshold σ grows linearly with E. For geometrical reasons, we expect $\Phi \propto E^2/L^2$. Altogether then, the number of appearance events is roughly independent of L, but grows linearly with E: You can do an appearance experiment anywhere, but you want a high energy beam.

For disappearance experiments, the limiting factor can be the precision with which the expected rate can be determined. In the case of accelerator experiments, this argues for a near detector to supplement the primary distant detector. For reactor experiments the state of the art for relating the neutrino flux to the reactor power is of the order of 3%. As long as the experiment is not statistics-limited, the figure of merit is simply L/E.

In Fig. 2.3, the parameters N=total number of charged current events per year, and L/E are displayed for a number of current and future experiments.

A glance at the figure shows the basic situation. It is hard to compete with Super Kamiokande for ν_μ oscillations. Similarly, KamLAND is well situated for finding a small Δm^2 in ν_e, as is suggested by the solar neutrino situation.

For measuring the parameters $\sin^2 2\theta$ and Δm^2, a muon storage ring is not a lot better than MINOS. However, MINOS is challenged by K2K, which will run earlier. The issue for K2K is whether it can accumulate enough statistics to reach the limits its fine value of L/E would reach.

For appearance experiments like NOMAD and CHORUS, it is essential to use high enough energy to surmount the threshold. For τ production, the suppression is still about 50% at $s = 100$ GeV^2.

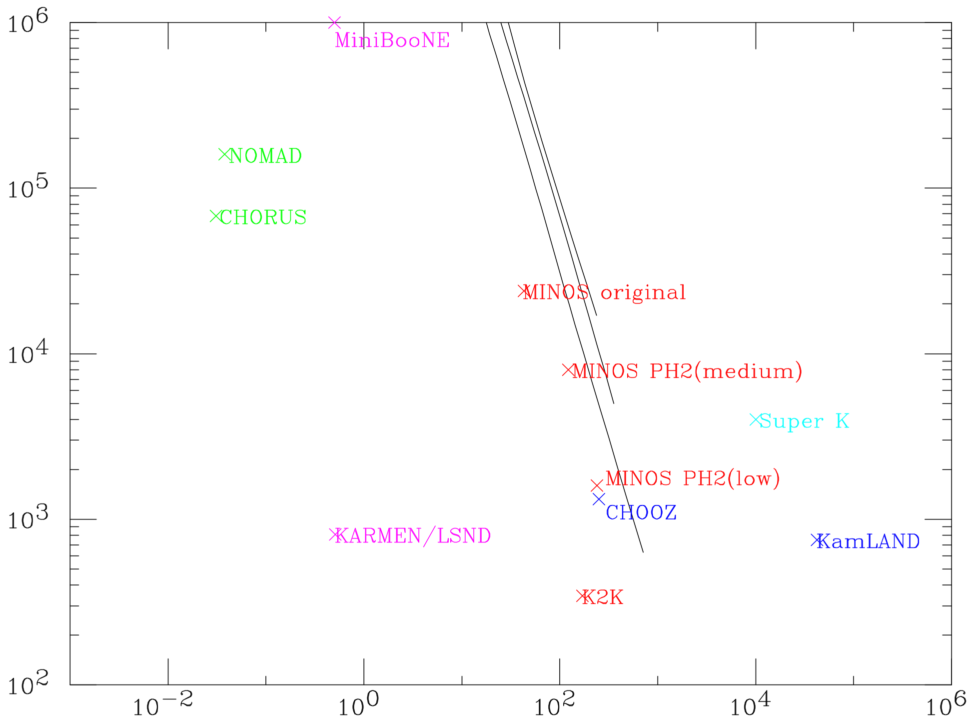

Figure 2.3: Parameters for a variety of neutrino oscillation experiments. The vertical axis shows N, roughly the number of charged current events per year. The horizontal axis shows L/E is in km/GeV. The three straight lines correspond to a muon storage ring source with a beam energy of 20, 40, or 60 GeV. The numbers for the muon storage ring are derived from S. Geer, *Phys. Rev.* **57**, 6989 (1997). For KARMEN, LSND and Super K (the atmospheric neutrino experiment is shown), the values for the number of events and L/E cannot be specified precisely, so the position on the graph is intended to give a general reflection of their power.

2.3.3 Current Status of Neutrino Oscillation Experiments

To establish the context we sketch briefly what is currently known about neutrino oscillations. We consider oscillations from ν_e and $\overline{\nu}_e$, following with oscillations from ν_μ and $\overline{\nu}_\mu$.

Oscillations from ν_e and $\overline{\nu}_e$

Since ν_e and $\overline{\nu}_e$ beams are not easily produced from accelerators, much of our knowledge about potential ν_e and $\overline{\nu}_e$ oscillations comes from elsewhere. Indeed, the very same technique that produced the first direct observation of neutrinos – reactor production of $\overline{\nu}_e$ followed by the detection of $\overline{\nu}_e p \to e^+ n$ through the annihilation of the positron and absorption of the neutron leading to γ emission – can be used to look for neutrino oscillations. The understanding of the processes inside power reactors is good enough to give the $\overline{\nu}_e$ spectrum to a few percent.

The sensitivity of a disappearance experiment using a reactor depends on controlling systematics to a few percent, as well, and on the distance, L, from the reactor to the detector. Naturally, the more powerful the reactor, the larger L can be while still obtaining a sufficiently strong signal. If the limit on disappearance ϵ can be reduced to 0.06, say, and if we take as the mean energy of an interacting $\overline{\nu}_e$ a value 4 MeV, then the rough limit on Δm^2 at $\sin^2 2\theta = 1$ is $4 \times 10^{-3}\sqrt{0.06}/1.27L = (7.7 \times 10^{-4}/L)$ eV^2, where L is in km.

A particularly effective experiment was carried out (hep-ex/9711002) by observing reactor neutrinos at Chooz in the Ardennes. The detector was located about 1 km from the 8.3 GW reactor. The actual result is given as $R_{measured}/R_{expected} = 0.98 \pm 0.04 \pm 0.04$. This is based on 1320 measured neutrino events accumulated in 2718 hours of running with a 5-ton target of 0.09% Gd-loaded scintillator. In this way CHOOZ set a limit of $\Delta m^2 < 0.9 \times 10^{-3}$ eV^2 at $\sin^2 2\theta = 1$ and $\sin^2 2\theta < 0.18$ at large Δm^2. The Palo Verde experiment, which is running now, is expected to have slightly lower sensitivity, $\Delta m^2 = 1.5 \times 10^{-3}$eV^2 at maximal mixing.

The earliest indications of neutrino oscillations came in solar neutrino experiments. The initial step in the fusion cycle that powers the Sun is the weak process $pp \to de^+\nu_e$. Because the total rate of energy production is proportional to the rate at which this reaction occurs, there is little uncertainty about the neutrino flux it generates. This turns out to be about 6×10^{10} cm^{-2}s^{-1}. These neutrinos have energies below 0.5 MeV and are thus below threshold for charged current interactions with most nuclides, though they of course can scatter elastically off electrons. The next most copious source of solar neutrinos is electron capture on Be^7: $\mathrm{Be}^7 e^- \to \mathrm{Li}^7 \nu_e$, with discrete neutrino energies near 0.4 and 0.9 MeV. The third significant source of solar neutrinos is the decay $\mathrm{B}^8 \to \mathrm{Be}^{8*} e^+ \nu_e$, which produces a continuum of neutrino energies up to nearly 18 MeV. Even though the flux of these neutrinos is about 10^{-4} of those from the pp reaction their high energy makes them very important in solar neutrino experiments.

The solar neutrinos can be detected if they are captured by isotopes like Cl^{37} and Ga^{71}, which become radioactive and whose subsequent decays can then be observed. The threshold for the former capture is 814 keV, while for the latter it is 233 keV. As a result, chlorine experiments are blind to the pp reaction, while gallium experiments can detect it. The chlorine experiments are dominated by neutrinos from B^8 and Be^7. The chlorine experiments were pioneered by R. Davis at the Homestake Mine in South Dakota, starting back in the 1960's. Gallium experiments are pursued by the GALLEX collaboration at the Gran Sasso in Italy and by the SAGE collaboration at Baksan, in Russia.

An alternative detection scheme relies on Cerenkov light. Because an enormous target is required to obtain sufficient rate, the natural medium is water. Kamiokande and its successor, Super-Kamiokande have been the leading experiments using this approach. The threshold for observability is several MeV and these experiments are thus dominated by neutrinos from B^8 decay.

The solar abundances of elements like beryllium and boron must be deduced from models and this adds some doubt to the predictions for these contributions to the solar neutrino flux. However, there is good agreement between the various calculations that have been done to estimate these abundances. This makes it hard to dismiss the results from the Cerenkov and chlorine experiments. Moreover, fully half of the reaction rate expected in the gallium experiments is due to the pp reaction, about whose rate there can be little doubt.

The discrepancy between the expected and observed rates for solar neutrino experiments is consistent and persistent. Attempts to blame the problem on solar models are weakened by the GALLEX and SAGE results. What remains suggests strongly that there are neutrino oscillations involving electron neutrinos.

The 1998 RPP review of solar neutrino experiments, written by Nakamura, lists the following results (SNU=solar neutrino unit):

Experiment	Technique	Result	Expectation
Homestake	$^{37}\text{Cl} \rightarrow ^{37}\text{Ar}$	2.33 ± 0.25 SNU	≈ 8 SNU
GALLEX	$^{71}\text{Ga} \rightarrow ^{71}\text{Ge}$	$69.7 \pm 6.7^{+3.9}_{-4.5}$ SNU	≈ 130 SNU
SAGEX	$^{71}\text{Ga} \rightarrow ^{71}\text{Ge}$	$73^{+18}_{-16}{}^{+5}_{-7}$ SNU	≈ 130 SNU
Kamiokande	water Cerenkov	$2.80 \pm 0.19 \pm 0.33$ (some units)	≈ 6(same units)

To explain this with ordinary (vacuum) neutrino oscillations would require $\Delta m^2 \times 1.5 \times 10^8$km/few MeV ≈ 1, or $\Delta m^2 \approx 10^{-11}\text{eV}^2$. Moreover this would have to work over a range of neutrino energies to account for the suppression in the variety of experiments with different thresholds. A solution of this sort needs to be very delicately tuned.

The ordinary solutions to the solar neutrino problem rely on the Mikheyev-Smirnov-Wolfenstein mechanism. The passage of neutrinos through matter can be analyzed as if it were described by an index of refraction. The index of refraction is determined by the forward elastic scattering amplitude. This amplitude is due to neutral current scattering alone for ν_μ and ν_τ. However $\nu_e - e$ elastic scattering also has a contribution from charged currents. This leads to additional mixing over and above the mixing that would be present in vacuum. Using MSW, solutions are found in the regions $\Delta m^2 = 5 \times 10^{-6}\text{eV}^2$, $\sin^2 2\theta = 8 \times 10^{-3}$ (small mixing angle solution), and $\Delta m^2 = 1.6 \times 10^{-5}\text{eV}^2$, $\sin^2 2\theta = 0.6$ (large mixing angle solution). Some analyses identify a third region, at lower Δm^2 and large $\sin^2 2\theta$, $\Delta m^2 \approx 10^{-7} - 10^{-8}$ eV^2, the so called LOW solution as an alternative.

Oscillations from ν_μ and $\overline{\nu}_\mu$

The most convincing evidence for neutrino oscillation experiments comes from Super Kamiokande. Super K uses a 50-kt water Cerenkov detector with 11k PMTs. They report on 4 k events in a 33-kt y exposure restricted to the 22.5-kt fiducial volume. The primary comparison is between ν_μ and ν_e from cosmic rays. These are generated as a result of collisions of primary cosmic rays about 15 km above the Earth. The expected ratio is very nearly two ν_μ for every ν_e as a result of $\pi^+ \rightarrow \mu^+\nu_\mu$, $\mu^+ \rightarrow e^+\nu_e\overline{\nu}_\mu$. To minimize uncertainties they form the ratio of ratios, $R = \frac{(\mu/e)_{obs}}{(\mu/e)_{MC}}$ and find R near 0.65. Moreover, the μ signal shows a strong directional dependence. The neutrinos that were created further from the detector are more depleted. If this is ascribed to oscillations, then the oscillation length is of the order of the diameter of the Earth or less. Since ν_es do not seem increased, the preferred explanation is oscillation of ν_μ to ν_τ or to a sterile neutrino. Super Kamiokande's published 90% CL region is $5\times10^{-4}\mathrm{eV}^2 < \Delta m^2 < 6\times10^{-3}\mathrm{eV}^2$ and $\sin^2 2\theta > 0.82$. Its more recent data point to a region centered at $\Delta m^2 = 3\times10^{-3}\mathrm{eV}^2$.

The earlier results from Kamiokande also show evidence for ν_μ oscillations, but with a somewhat larger value of Δm^2 that is consistent with the Super K result.

Attempts have been made to demonstrate oscillations from ν_μ by observing τ appearance. CHORUS, an experiment at CERN, uses 770 kg of nuclear emulsion with auxillary electronic tracking to search for τ appearance. The mean neutrino energy is 27 GeV and the distance from the target to the emulsion is about 850 m. An analysis of 68 k charged current events and 7 k events without a visible muon led to no τ candidates. From this, CHORUS derived a limit $P(\nu_\mu \to \nu_\tau) < 6.0\times10^{-4}$. Using these numbers, the high Δm^2 limit is $\sin^2 2\theta < 0.13\times10^{-3}$ and the $\sin^2 2\theta = 1$ limit is $\Delta m^2 < 0.6$ eV^2.

NOMAD, another CERN experiment, has published limits on $\nu_\mu \to \nu_\tau$ based on kinematical identification of τ decays. The NOMAD detector also has a small mass (2.7 t) and is located a similar distance (625 m) from the target. The result is a large sample of charged current events (162 k events). The signature for a τ is the missing momentum carried away by the ν_τ in $\tau \to \nu_\tau X$, where X can be e, μ, or hadrons. The active target is the drift chamber itself.

Very roughly the numbers work out as follows. The typical efficiency for each mode is about 3%. The total branching ratio for the modes searched is 70%. The cross section for τ production is suppressed by about 0.5 from threshold effects. We can estimate the total number of τs that would be produced if the transition probability P were unity as (162k events $\times\, 0.03 \times 0.7 \times 0.5 = 1700$. The more detailed calculation in the NOMAD paper gets 1582. They end up taking a 90% CL limit of 3.3 events (they found one τ^- against a background of $2.7^{+3.0}_{-0.8}$ and 27 τ^+ with a background of 28 ± 6. In the "high Δm^2" domain $P = 3.3/1582$ gives $\sin^2 2\theta < 4.2\times10^{-3}$ while for $\sin^2 2\theta = 1$, it gives $\Delta m^2 < 1.4$ eV^2.

It is also possible to demonstrate oscillations from ν_μ and $\overline{\nu}_\mu$ by electron appearance. This has been the path followed by LSND. LSND has reported evidence for $\overline{\nu}_\mu \to \overline{\nu}_e$ using μ^+ decay at rest and for $\nu_\mu \to \nu_e$ from π^+ decay in flight. In the latter, 40 events with electron appearance are found with a background of 21.9 ± 2.1. The ν_μ flux is in the 60 - 200 MeV range. The muons are generated at a target 30 m upstream from the detector and also by two thin carbon targets further upstream. The oscillation probability is claimed to be $(2.6 \pm 1.0 \pm 0.5) \times 10^{-3}$. Using $L = 30$ m, $E = 150$ MeV, we would deduce $\Delta m^2 = 0.2$ eV^2, in good agreement with the LSND figure.

By contrast, KARMEN, using a similar approach, has not seen oscillations. KARMEN uses decay at rest of π^+ and μ^+, giving equal numbers of $\nu_\mu, \overline{\nu}_\mu$, and ν_e. The goal is to look for oscillations into $\overline{\nu}_e$. For full mixing, the expected number of events would have been 811. Instead, none was observed, though the background was calculated to be 2.88 ± 0.13. They set a limit of $N = 1.07$ events at 90% CL. Taking $L = 17.7$ m and $\langle E \rangle = 40$ MeV, we find $\Delta m^2 < (40/(17.7 \times 1.27))(1/\sqrt{811}) = 6 \times 10^{-2}$eV^2, in some disagreement with LSND.

2.3.4 Future Neutrino Oscillation Experiments

MiniBooNE is designed to follow up on the reports from LSND. MiniBooNE would use neutrinos produced in the interactions of 8 GeV protons from the Fermilab booster and a 445-t (fiducial) target of mineral oil located 500 m away. This experiment would produce enormous numbers of events, on the order of 10^6. If the LSND results are correct, there would be of order 1000 electron appearance events in MiniBooNE.

Possibilities Under Consideration at CERN

Picchi and Pietropaolo [hep=ph/9812222] report on a CERN-LNGS (Gran Sasso National Laboratory) study of neutrino oscillation options. It proposes both a large (20-kt) calorimetric detector for atmospheric neutrinos and a long-baseline accelerator program. The Gran Sasso neutrino beam would produce about 4000 ν CC events per kt-year with a mean ν_μ energy of about 30 GeV. This is about 10% of the rate anticipated for the muon storage ring discussed in Chapter 4. The τ production rate is correspondingly reduced. For $\Delta m^2 = 1 \times 10^{-3}$eV^2 there are about 4 events per kt-y. This grows as $(\Delta m^2)^2$.

A variety of experiments would benefit from the CERN to Gran Sasso neutrino beam.

- ICARUS, a liquid-argon TPC, would look for τ appearance. Clearly the mass of the detector is a serious issue. Each ICARUS module has a mass of 0.6 kt, with one module due for installation in 2000 and three more in 2003. The ICARUS collaboration has proposed a Super

ICARUS with 30 kt, placed outside the LNGS. The τ events have missing transverse momentum. The best channel is $\tau \to e\nu\nu$, where the efficiency is supposed to be about 50%.

- OPERA will have 0.75 kt of lead with emulsion as the detection medium. The lead sheets are 1 mm thick. The emulsion is applied in 50-micron layers on both sides of a plastic base layer. Electronic detectors will locate the events within the $3.5 \times 3.5 \times 40\text{m}^3$ structure. The best events are produced in the lead plates. Taus leave a track in one layer of emulsion, decay in a 3-mm drift space and then are detected again in the next layer of emulsion. The estimated efficiency is 35%.

- AQUA-RICH would be a water Cerenkov imaging device, dwarfing even SuperK with a mass of 125 kt. Quasi-elastic events $\nu_\tau n \to \tau p$ would be visible in the $\tau \to \mu\nu\nu$ mode.

- NOE (Neutrino Oscillation Experiment) would emphasize electron identification obtained with transition radiation detectors. The TRDs would alternate with fine-grained calorimetry. The total mass envisioned is 8 kt.

- NICE, in contrast to many other new proposals, is a disappearance experiment and thus would need a lower energy beam to investigate the SuperK domain of oscillations. It might have a 10-kt iron-scintillator calorimeter.

The reach of the appearance experiments can be estimated from our approximate reach relation at $\sin^2 2\theta = 1$:

$$\Delta m^2 = \frac{E}{1.27L} \frac{1}{\sqrt{N}} \tag{2.6}$$

where here N must include a factor for τ detection efficiency. With $E = 30$ GeV, $L = 730$ km, $N = 4000/\text{y}^{-1} \times 4\text{y} \times 25\% = 4000$, we find $\Delta m^2 \approx 0.5 \times 10^{-3}\text{eV}^2$ at maximal mixing. To get a 90% CL we need to take $\sqrt{2.3}$ times this. This is in good agreement with Table 2.1 reproduced from Picchi and Pietropaolo.

Detector	Mass(kt)	Background	Signal ($\Delta m^2 = 0.005\text{eV}^2$)	Min $\Delta m^2(\text{eV}^2)$ Exclusion 90%CL	at full mixing Discovery $S > 4$
ICARUS	2.4	2.5	84	1.1×10^{-3}	1.6×10^{-3}
Super-ICARUS	30	3.7	421	0.3×10^{-3}	0.8×10^{-3}
OPERA	0.75	0.45	37	1.2×10^{-3}	1.8×10^{-3}
AQUA-RICH	125	< 1	63	1.4×10^{-3}	2.3×10^{-3}
NOE	2.4	4.6	15	2.0×10^{-3}	3.9×10^{-3}

Table 2.1: Anticipated capabilities of CERN-proposed neutrino oscillation experiments using the Gran Sasso neutrino beam line. The limits here are for τ appearance. (Picchi and Pietropaolo [hep=ph/9812222])

2.3.5 Future Developments at SuperKamiokande on Atmospheric Neutrino Oscillations

The Super Kamiokande experiment dominates the current scene. Its capabilities establish a baseline for future work so it is essential to consider how far Super K may go in answering the fundamental questions of neutrino oscillations.

R

The double ratio

$$R = \frac{(\nu_\mu/\nu_e)_{\text{data}}}{(\nu_\mu/\nu_e)_{\text{MC}}}, \tag{2.7}$$

has been the most important signature of neutrino oscillation, is already systematics limited: $R = 0.63 \pm 0.03 \pm 0.05$ for sub-GeV and $R = 0.65 \pm 0.05 \pm 0.08$ for multi-GeV data. The dominant systematic uncertainties, such as that in the calculated flux ratio, the uncertainty from single-ring multi-ring separation, and the uncertainty in the nuclear cross section, are unlikely to be improved. There will be no dramatic improvements in this measurement.

Zenith Angle Dependence

The distortion in the zenith angle dependence is another important signal of neutrino oscillation. This is still statistics limited. For instance, the up/down ratio in the multi-GeV μ-like data is $0.54^{+0.06}_{-0.05} \pm 0.01$ compared to the Monte Carlo expectation $0.98 \pm 0.03 \pm 0.02$. The statistical uncertainty in the MC can obviously be reduced, but the systematic uncertainty in the MC, mainly the effect of 1 km of rock above Kamiokande on the calculated ν_μ flux and the possible contamination of the cosmic ray background, cannot be substantially improved. Still, overall determination of up/down ratio at the 3% level appears feasible after 8 years.

Δm^2, $\sin^2 2\theta$

Even with the current data, Super K can try to combine information from the contained data, which give the best signal, the zenith-angle distribution of the up-going muon data, and the ratio of stopping to through-going up-going muon data. However, the combined fit probably does not improve Δm^2 determination much, judging from the plot of the allowed regions from each analysis. Especially the up-going muon sample has a history of bad systematics and background issues (recall the by-now-defunct IMB exclusion plot). The stopping/through-going ratio is already nearly limited by systematics: $0.22 \pm 0.023 \pm 0.014$ (data) compared to 0.39 ± 0.05 (MC). The sensitivity is limited by the theoretical uncertainty 0.05 in the Monte Carlo calculation.

Improving the statistics of the contained data would probably result in a better study of the distortion in the zenith-angle dependence. The main problem here is that SuperK does not measure the direction of the neutrino, but rather measures the direction of the lepton, which has a correlation only of order 15–20° even for multi-GeV data. Because the preferred Δm^2 range happens to correspond to the near-horizontal bin in the zenith angle, a small error in the angle results in a large uncertainty in the oscillation length, and hence a large uncertainty in Δm^2. Improving it drastically using the contained data sample is unlikely. On the other hand, the $\sin^2 \theta$ determination is improved purely by statistics from the up-down ratio in multi-GeV μ-like sample.

Further Tests

Super-Kamiokande can test the oscillation hypothesis in various ways in the next few years. One idea is to use up/down asymmetry in the inclusive multi-ring event sample. This would confirm the depletion of the ν_μ in a statistically independent sample without relying on particle ID. This can be done at the 3-sigma level even with the current statistics. Increased statistics would discriminate $\nu_\mu \to \nu_\tau$ and $\nu_\mu \to \nu_s$ oscillations. Another idea is to use neutral-current π^0 production to measure the neutral current rate. The analysis is currently limited by the lack of precise knowledge on the π^0 production cross section, which will be measured soon with the K2K near detector. One can also study the up/down asymmetry of the produced π^0 which has a reasonable correlation with the incident neutrino direction once appropriate momentum cut is made (with consequent loss in efficiency).

τ appearance

If the oscillation is $\nu_\mu \to \nu_\tau$, Super Kamiokande must already have 10–20 τ events. The best hope to see them is in the $\tau \to \nu_\tau \rho$ mode in quasi-elastic production of τ. The rate is down to 1 event per year. Even with 50% efficiency and low background (whose estimate is quite uncertain), the most one can hope to obtain is something like five ρ's going up and one ρ going down (background) after 10 years. Not impossible, but obviously hard.

Conclusion

There will be many more cross checks of the oscillation hypothesis: inclusive multi-ring, statistically improved sample of zenith angle dependence, etc. The "evidence" may be elevated to "observation" in a year or two. Combining different techniques, the distinction between ν_τ and ν_s case is probably possible after 5 years of running. But the result may not be too convincing.

The objective of competing experiments had better not be the observation of oscillation itself, but rather the precise measurement of oscillation parameters or detection of τ appearance.

2.4 Particle Astrophysics

2.4.1 Cosmic Ray Physics

The origins of particle physics lie in the experiments done with cosmic rays. It was there that the positron, the muon, the pion, and the strange particles were discovered. Even more recently, the first signs of charmed particles may have appeared in a cosmic ray experiment. Now cosmic rays are studied more for what they might reveal about their astrophysical sources. What mechnisms are responsible for the generation of ultra-high energy cosmic rays and gamma rays? What is responsible for the sudden burst of gamma rays? Among the important new experiments are:

- GLAST, a satellite experiment to measure gamma rays in the 10 MeV to 100 GeV range and beyond. The gammas are pair-converted in thin foils, which are interleaved between silicon strip detectors. The tracking system is followed by a colorimeter. GLAST follows the earlier work by EGRET and CGRO (Compton Gamma Ray Observatory). These have seen powerful gamma-ray sources with rapid time variation.

- MILAGRO, located near Los Alamos, which uses a large pool of water (60m x 80m x 8m) as cerenkov detectors for gammas in the TeV to 100 TeV range. Timing the signals from the PMTs gives a good determination of the direction of the shower.

- Pierre Auger Project, which will use both water cerenkov detection and atmospheric fluorescence. Each cerenkov unit will contain 12 tons of water. Ultimately, there will be 1700 such units, spaced 1.5 km apart, covering 3000 sq. km. It should detect thousands of air showers with energies above 10^{19} eV and nearly 100 above 10^{20} eV.

2.4.2 Neutrino Astrophysics

Neutrino detection offers the prospect of extending astronomical observations to the highest energies now known or expected, to "see" into the cores of the most energetic objects in the cosmos. Neutrinos, interacting only weakly with matter, are unique probes of the very highest energy processes, better than are photons, which are absorbed electromagnetically both in the source material and the intervening cosmic microwave and infrared background. Furthermore, neutrino telescopes are sensitive to incoming particles day and night, viewing half the sky.

At present the only astronomical sources from which neutrinos have been detected are our sun and supernova 1987A. A general argument that high energy neutrinos should be observed follows from the energy distribution of cosmic rays that reach surface detectors on earth - air-shower arrays and fluorescence detectors. The spectrum extends to at least 10^{20} eV. The mechanism that produces these ultra-high energy particles is a mystery but, if it is hadronic, then charged as well as neutral pions must appear, yielding neutrinos with energies comparable to those of the photons.

Neutrinos provide a new means for the study of Active Galactic Nuclei (AGN) and Gamma Ray Bursters (GRB), extremely bright yet localized objects. Models have been made that predict neutrino fluxes from these objects, but results vary widely, from perhaps 100 to 100,000 events in a year for a square kilometer area.

Neutrinos of somewhat lower energy but likely greater flux should be produced in the known Galactic photon emitters, and in supernovae. More speculative is the possible observation of neutrinos from WIMP annihilation in the core of the sun or the earth. There will also be a flux from Galactic WIMPs (the same weakly-interacting massive particles that get trapped in the Sun and Earth). Neutrino detectors are complementary to accelerator-based and direct detection schemes for dark matter in that the neutrino detector only becomes sensitive to masses above those that can be searched by accelerator-based and direct detection techniques.

Flux estimates and neutrino interaction rates set the relevant incident-neutrino energy range to be approximately 1 TeV to 1000 TeV. At the low energy end, experiments are limited by background from neutrinos from cosmic ray showers. At the high energy end the predicted flux of neutrinos is too low to provide useful counting rates. These same considerations call for these neutrino detectors to be of kilometer scale in collecting area and volume.

2.4.3 Experimental Cosmology

LBNL has been a leading contributor to the transformation of experimental cosmology over the past decade or so. Even earlier, Muller and Smoot measured the dipole anisotropy of the cosmic microwave background. The COBE results on the non-dipole anisotropy began the new era, and the Supernova Cosmology Project's first measurements opened an entire new field of research. The goal is nothing less than to understand the beginnings of the Universe. The measurement of two crucial parameters are the heart of this work: Ω_{mass} and Ω_Λ, the energy densities due to matter and the cosmological constant. Together they determine the fate of the Universe. The very attractive inflationary scenario insists that their sum is unity. Very general arguments (or what some might call prejudice) indicate that the cosmological constant ought to be zero. Current results from SCP (and its competition) indicate these cannot both be so. Together, measurements of supernovae and the cosmic microwave background can pin down Ω_{mass} and Ω_Λ. The CMB measurements can, in principle, address other questions, like the nature of the presumed dark matter.

Cosmic Microwave Background

The COBE satellite and subsequent balloon-based observations of the Cosmic Microwave Background have begun to give us a detailed picture of the Universe at its earliest moments; the Hubble Space Telescope and the Keck Telescope have found galaxies at distances corresponding to the Universe at one-tenth of its present age; the CfA and APM large-scale redshift surveys are mapping the present structure of the nearby Universe and new surveys such as the Sloan Digital Sky Survey, DEEP, and 2DF are underway to extend and deepen these surveys. Supernova surveys and weak gravitational lensing observations are providing the tools to probe the large scale structure and properties of space-time. Thanks to the extraordinarily large investments by NSF, NASA, DOE, and ESA, data on all of these fronts will increase by orders of magnitude in the coming decade, enabling us to determine the history of the Universe (including its expansion rate, dark matter content, deceleration, and baryon content) with unprecedented precision.

Some CMB experiments are already completed or currently underway:

MAX (Millimeter-wave Anisotropy eXperiment) was the second-generation balloon-borne anisotropy detector developed here at Berkeley. The MAX receivers and gondola flew five times and provided six data points on the CMB anisotropy power spectrum. Its flight days and data processing are complete.

MAXIMA (MAX IMaging Array) is very similar to MAX in concept but physically larger. The most important step forward was to change the receiver from a single four-frequency pixel to an array of 16 pixels with more sensitive bolometer detectors and much finer (10 arcmin) angular resolution. MAXIMA had its most recent flight in August of 1998 and the data are currently being processed. The gondola (now in Bldg. 60 at LBL) is being prepared for another flight (or two) this summer. The future of MAXIMA is not yet clear due to some key personnel obtaining faculty positions at other universities and the usual funding cycle issues.

BOOMERANG, a balloon experiment, had a test flight from North America in September 1998 and those data are through processing and in the middle of analysis. From 29 December 1998 to 8 January 1999

BOOMERANG had a 10.5-day flight from McMurdo around Antarctica and back to within 33 miles of McMurdo. Preliminary indications from the data are very promising.

Supernova Cosmology Project

As of the end of 1998, the Supernova Cosmology Project had discovered and studied approximately 80 supernovae, primarily in the redshift range between $z = 0.4$ and 0.6 but with a secondary peak in the redshift range around $z = 0.8$. With 42 of these supernovae analyzed, it has been possible to obtain a confidence region in the plane of mass density vs. cosmological constant: the linear combination $0.8\Omega_{\rm mass} - 0.6\Omega_\Lambda$ is approximately -0.2 ± 0.1, while the orthogonal constraint is much wider, essentially between 0 and 1.5 for $\Omega_{\rm mass}$. These results indicate that the universe is accelerating and that the cosmological constant, i.e. the vacuum energy density, is positive.

There are two reasons to be concerned about a positive cosmological constant. One is that the cosmological constant is so small. We might expect that the cosmological constant should be set by the supersymmetry breaking scale. This would give a number that is approximately 51 orders of magnitude too large. The second problem posed by nonzero cosmological constant is sometimes called the "Why now?" problem. While the universe has been expanding the matter density has dropped by factors proportional to the scale of the universe cubed, while the vacuum energy density has stayed constant. The puzzle is why the mass density, which has ranged through many orders of magnitude, is now within a factor of three of the fixed, cosmological-constant vacuum energy density.

If the vacuum energy density is zero, it is still possible that other sources are responsible for the accelerating universe. Several theories have been proposed using decaying dynamical scalar fields, and it has been suggested that at least some of them may naturally allow the energy density of this field to approach asymptotically the mass energy density. These alternative fields can be characterized by the equation of state, i.e. the ratio of the pressure to the density for these energies. The cosmological constant equation of state is simply $p/\rho = -1$. The SCP supernova data allow for p/ρ between -1 and approximately -0.6, but prefer values closer to -1 for values of the mass density approaching 0.3.

SCP has worked closely with Steve Holland and Nick Palaio in developing the LBNL CCD. Based on work done by Holland for SSC detector R&D, silicon can be processed in such a way that it retains high-resistivity while

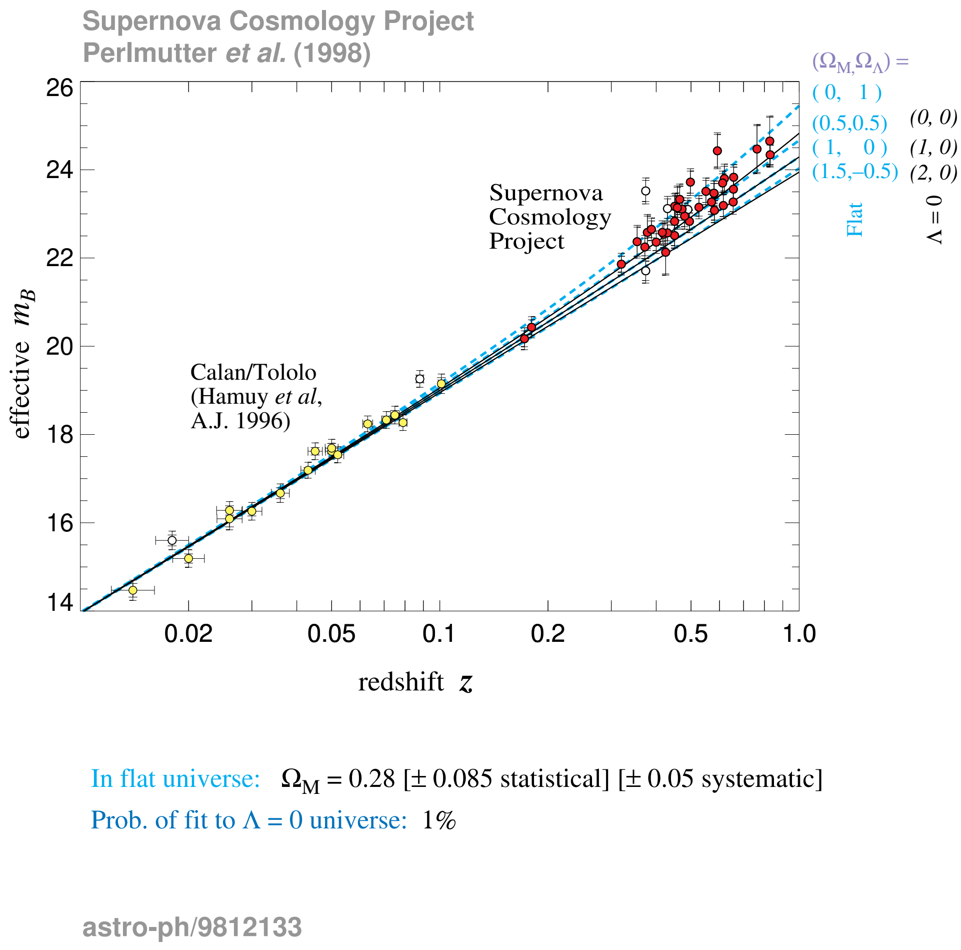

Figure 2.4: The Hubble plot of analyzed supernovae from the Supernova Cosmology Project.

the CCD is produced. As a result, the full 300 microns can be depleted and the CCD has high quantum efficiency into the far infrared. See Fig. 2.5.

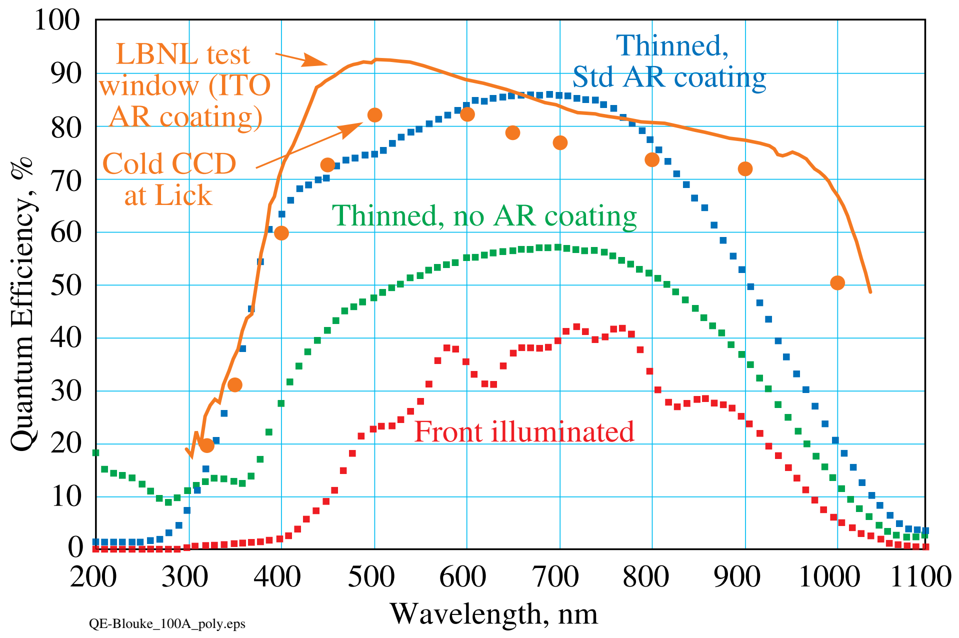

Figure 2.5: Quantum efficiency (QE) of the LBL/Lick CCD as compared with standard front-illuminated, thinned back-illuminated, and thinned back-illuminated antireflective coated CCDs. (The latter are from M. Blouke and M. Nelson, SPIE 1900, 228-240 (1993). The solid curve is the LBL/Lick QE as measured on an equivalent photodiode using a monochromater at LBL. The dots are nominally the same measurement, taken with a prototype CCD using narrow bandpass filters at Lick. The difference is not understood. The thickness of the single AR coating was chosen for maximal transmission at 400 nm.

Chapter 3

Opportunities at NLC

The Gilman HEPAP Subpanel recommended that the US community, in collaboration with Japan, produce a Conceptual Design Report for an e^+e^- linear collider of center-of-mass energy of 1 TeV, extendable to 1.5 TeV. In parallel with this activity, the US community has recently organized itself to produce a Conceptual Design Report exploring in considerable detail NLC detector concepts within approximately the next three years. Similar detector studies are underway in Japan and Europe, and there is collaboration among these efforts.

LBNL is currently involved in the NLC accelerator design, studies of the performance of a TPC-based detector concept, and detector R&D for the application of a new CCD technology developed at LBNL to be used in the NLC vertex detector. Each of these is briefly described below.

3.1 AFRD Program on NLC

The conceptual design phase of an x-band microwave based Next Linear Collider (NLC) is to take place over the next three years culminating with submission of a final CDR in FY01. SLAC is the lead laboratory for the NLC Collaboration of which LBNL is a member. The NLC Collaboration is international in scope and membership is expected to include many laboratories and universities from around the world. During the conceptual design phase, AFRD will work on the Damping Rings and the Interaction Region (IR). Both of these areas are technically demanding and capitalize on areas of particular LBNL expertise. LBNL will carry major responsibilities in the engineering and design of the damping rings and the positron pre-damping ring. LBNL will work on the 714 MHz RF systems, magnet and vacuum systems (especially those for the wigglers), injection and extraction systems, and perhaps other systems as well. These efforts include procurement, fabrication, and testing of prototype components, as well as design and execution of experimental studies at the existing ring facilities such as the ALS at LBNL, the PEPII rings at SLAC, and the ATF at KEK.

Work in the IR is directed to two areas: superconducting magnets and simulation of the beam-beam interaction. The Superconducting Magnet Group within AFRD has taken responsibility for developing a concept for using superconducting quadrupoles for the low-beta insertion in the Interaction Region of NLC. In the Center for Beam Physics, Monte-Carlo simulations and analysis are being carried out to study QED and hadronic backgrounds due to beam-beam interaction at the IP, to evaluate the effects of these backgrounds on detector performance and to explore possibilities for luminosity optimization.

3.2 NLC Detector Studies

Design studies for future electron-positron linear colliders have been ongoing for the last several years in the U.S., Japan and Europe. As a precision instrument for measuring high energy processes, the linear collider naturally complements first measurements and searches for new particles that are expected for the Large Hadron Collider (LHC).

Two solenoidal detector designs are being considered for the linear collider: one based on a large Time Projection Chamber (TPC), and the other centered on a relatively small silicon tracking system. The large, highly-segmented, TPC in a 3-T magnetic field offers a robust tracking system with many high resolution (140 micron) measurements, excellent pattern recognition and dE/dx particle identification. The impressive performances of ALEPH and DELPHI demonstrated the effectiveness of a TPC at a high energy e^+e^- machine. The small silicon detector relies heavily on excellent single-hit resolution (10 microns) and a higher (6-T) magnetic field. With this choice one hopes to improve flavor tagging by employing a smaller, 2-cm radius, CCD vertex detector as opposed to the 3-cm CCD detector that would be used in the large detector because of beam-beam and electron-positron pair backgrounds.

3.3 LBNL CCD's for NLC

The NLC physics will depend critically on the ability to distinguish and tag heavy flavor jets and provide a clean separation between b, c, and light quark jets. Key benchmark measurements, such as the branching fraction of Higgs decay, require clean identification of heavy quark jets. Examples like $Z^0H^0 \to q\bar{q}f\bar{f}$ – a key test of a standard model Higgs, $HA \to t\bar{t}t\bar{t} \to WbW\bar{b}WbW\bar{b}$ – a 12-jet final state, or associated production of $H^+ \to c\bar{s}$, show that any planned detector must identify complex event topologies. Although the machine luminosity and center-of-mass energy have not been established, event rates no greater than 1000's/year can be expected. Thus it essential that efficiencies for reconstructing such events be high.

Simulations have shown that a low-mass high-precision vertex detector can provide the requisite efficiency and purity. A good example of the performance possible with a high precision vertex detector can be found in the SLD experiment. There, high efficiency c-jet and b-jet tagging has been demonstrated with high purity.

Fortunately, the linear collider machines lend themselves well to the use of powerful and accurate vertex detector technologies. The SLAC design for the NLC permits the use of a very small radius beam pipe allowing the vertex detector to be placed very close to the interaction point. Furthermore, the long spacing between collider acceleration pulses permits the use of vertex detector technologies that have slow response or readout times. In particular, charged-coupled devices (CCD's) have been used very successfully in the SLD vertex detector to exploit their high precision, in spite of their characteristic long readout times. CCD's with 15-micron square pixels can achieve unprecedented accuracy in two dimensions while consuming minimal power.

LBNL is fortunate to possess a unique and innovative CCD technology developed by Stephen Holland in the Microsystems Laboratory. This technology should achieve *sub-micron* 2-D point resolution in each layer. The use of a thinned silicon substrate (100 μm) will keep the material below $0.1\%X_0$. Signal-to-noise values can be as high as 2000:1 on these thinned substrates. In fact, technologies exist to make much thinner self-supporting films of CCD's.

The LBNL technology is made on high-purity n-type high-resistivity silicon substrates. The techology for making detectors on this substrate material was originally developed for the SSC. The CCD's made at LBNL have been shown to be more radiation resistant than commercial CCD's and to meet the radiation requirements of the NLC. Successful devices as large as 2048 $\times$ 2048 pixels have been already made in the Microsystems Laboratory and this technology is now being transferred to a commercial vendor.

The use of CCD's in the SLD vertex detector has been highly successful, with impact parameter resolutions as small as 9 μm (ignoring multiple scattering). This development for SLD was spearheaded by Chris Damerell (Rutherford Appleton Laboratory) and again he has taken the lead in proposing this technology for the NLC detector. Already a small collaborative effort has formed in the U.K. working on the mechanical support design for the vertex detector (wiht U.K. funding). This group is currently in partnership with EEV Ltd. the original vendor of CCD's for the SLD vertex detector.

As a developer of CCD's, LBNL finds itself in a unique position in the H.E.P. and NLC community and could easily become a U.S. center for the development of the NLC vertex detector. As compared to the commercial CCD's available, the LBNL technology is more radiation resistant and offers significantly better point resolution, higher signal-to-noise, and lower power. The technology is therefore very well suited to NLC and LBNL could use this as a means to gain leadership for the development of this key component of an NLC detector. Furthermore, in one of the design concepts for an NLC detector, an all-silicon outer tracker is proposed. It is likely that the CCD technology would be ideally suited to this application as well.

Physics Potential:

The physics potential of the NLC depends critically on the threshold for significant production of supersymmetric particles (or any other new particles). In the very simplest terms, the NLC would provide an exciting physics opportunity if its center-of-mass energy is above this threshold for new particle production. The NLC center-of-mass energy needs to be $1-1.5$ TeV so that its "physics reach" for supersymmetric particle production is comparable to the LHC. While the NLC will study supersymmetry only if it is already discovered earlier at another accelerator, it will have unparalleled capability to study the spectrum of new particles in detail. The details of that spectrum would have enormous significance, as it would be the primary means for distinguishing between alternative supersymmetric models.

Risks:

The risks related to involvement in the NLC are clear. First, it may be deemed too expensive. Second, funding limitations may cause the NLC to be built with an initial center-of- mass energy below the (unknown) threshold for new particle production. Third, the threshold for interesting physics may exceed even the most optimistic capabilities of the accelerator builders. Fourth, it might take so long to align the political and funding sources to build an NLC that other collider concepts become more attractive. Fifth, major involvement of the Physics Division in an NLC detector in the next five years would inevitably detract from the ongoing efforts (of course this applies to any new major involvement, not just the NLC).

We note that there are also risks in *not* being involved in a major way in the NLC. The NLC has (so far) been judged in the United States (and elsewhere) to be the leading candidate for the next new facility beyond the LHC. For the last 25 years LBNL has always been a leader at the forefront of collider experiments. It would appear to relinquish this role, at least partly, unless it contributes soon to NLC detector studies.

Suitability for LBNL:

Many technical challenges presented by an NLC detector are very well matched to strong LBNL capabilities. It is generally agreed that a CCD vertex detector/tracker is the preferred choice for an inner NLC detector. The existing work on CCD development (described above) is an excellent starting point for major LBNL involvement in this aspect of an NLC detector. LHC-scale silicon tracking is also under serious investigation for an NLC detector, and the CDF, D0, BaBar and ATLAS experience is directly applicable. The major alternative to silicon tracking is the use of a TPC (albeit with greatly increased channel count compared to existing devices) and again LBNL has unique (at least in the US) capabilities in this area, especially if the recent work by the Nuclear Science Division on STAR is included. It is also clear that LBNL could contribute in many other technical areas (calorimetry, data acquisition and trigger). Finally, we note that there has already been strong involvement in NLC physics studies.

The level of activity must increase if there is to be a major LBNL involvement in the detector design studies to be completed in the next years for a Conceptual Design Report. Ideally this would include both detector research and development and simulation studies to guide the overall detector design. The potential LBNL capabilities are sufficiently broad that it would seem appropriate to determine first for ourselves the best detector concept as well as continued involvement in particular R&D and simulation studies. A study group with some degree of organization might be an appropriate next step. The opportunity for collaboration with members of the Nuclear Science Division (e.g. in TPC development) should be explored.

Chapter 4

Opportunities in Neutrino Physics

We provide here an evaluation of the strengths and weaknesses of some neutrino oscillation experiments both on general grounds and from the perspective of the LBNL Physics Division.

4.1 KamLAND

Outline of the Experiment:

KamLAND will outfit the vacant Kamiokande underground cavern at Kamioka with a 1,000-ton liquid-scintillator detector. As currently conceived, 1300 newly developed fast 17-inch PMT's view a scintillator-filled plastic bag that is suspended inside a buffer-fluid-filled spherical steel tank. The central tank is surrounded with a PMT-instrumented water tank acting as a veto for external radiation. KamLAND is primarily conceived as a long-baseline reactor neutrino experiment using Japanese power reactors located between 140 and 200 km away. The enormous L/E is sufficient to extend the existing limits in Δm^2 by two orders of magnitude. Electron anti-neutrinos are detected by inverse beta decay on the proton using the traditional delayed coincidence method. A prompt-positron signal is identified by requiring a delayed 2.2-MeV gamma ray from the thermalized neutron capture on the hydrogen in the scintillator fluid.

Radiopurity is a major concern for KamLAND. However, if radioactive backgrounds (coming primarily from ubiquitous uranium, thorium, potassium and ^{14}C) are low enough then KamLAND is also a sensitive solar neutrino experiment capable of detecting neutrinos from electron capture decay of ^{7}Be in the Sun. In this case the signal is simply scintillation light from electron-neutrino scattering events. The detection energy threshold must be low, about 250 keV. With this capability KamLAND would be superior to Borexino, a five times smaller liquid scintillator detector being constructed at Gran Sasso. However, the Gran Sasso underground laboratory is deeper than the Kamioka mine so the background from muon-generated radioactivity is expected to be higher for KamLAND.

The project was approved for funding in Japan about two years ago with a five-year project grant. The Japanese group comes mostly from Tohoku University, led by Prof. Atsuto Suzuki. The condition for continued funding by the Japanese funding agency is that the detector begin operation during 2001, putting the project on an extremely fast track. Prof. Giorgio Gratta from Stanford organized a U.S. collaboration involving several university groups. The collaboration, which includes Caltech, New Mexico, Tennessee, Alabama, North Carolina State, Duke, and Oak Ridge, submitted a proposal to DOE HEP. The proposal was turned down in December, but the proponents were encouraged to submit a new proposal with LBNL as a lead collaborator. A new proposal is being prepared by an LBNL group for submission in early March, contingent on approval by the Physics and Nuclear Science Divisions. LBNL engineers are generating a cost estimate for the possible US contribution to KamLAND. These contributions include: increasing the number of PMTs by 50%; providing the electronics and data acquisition system; engineering for the scintillator-containing plastic bag; the veto shield; low-background radioassay of critical materials; and providing the expertise and engineering equipment for making the KamLAND underground site into a clean low background area.

Physics Potential:

The powerful Japanese reactors (up to 25 GW) provide a significant event rate (750 events/Kton-y) despite the large distance. The L/E of KamLAND is much larger than any long baseline experiment now being planned; sensitivity to Δm^2 is below $10^{-5}\mathrm{eV}^2$. KamLAND could directly confirm one of the possible MSW solutions (the large mixing angle solution, LMAS) with a terrestrial experiment. KamLAND would also be a sensitive detector for terrestrial anti-neutrinos generated by uranium and thorium in the earth's crust and possible electron anti-neutrinos from supernova explosions. The sensitivity of the KamLAND reactor neutrino experiment is displayed in

Fig. 4.1.

If the backgrounds can be reduced to the levels required by Borexino, KamLAND is a much more sensitive ^{7}Be solar neutrino experiment. Because of the larger size KamLAND can detect the ^{7}Be signal from its seasonal variation, mitigating the need for a detailed understanding of the background underlying the ^{7}Be signal. With a 250-keV threshold to avoid ^{14}C background KamLAND is insensitive to neutrinos from *pp* solar fusion. The rate for ^{7}Be neutrino events in the Standard Solar Model is significant, about 800 events/kton-day. The experiment would be capable of detecting the much smaller signal rate expected from the MSW solution to the solar neutrino problem.

While a two-order-of-magnitude improvement in sensitivity to reactor neutrinos is certainly adequate justification for KamLAND, a physics program that could include ^{7}Be solar neutrinos is an extremely attractive prospect. With the ability to detect seasonal variation of ^{7}Be solar neutrinos, KamLAND tells us something about all possible solutions to the solar neutrino problem: both the large and small mixing angle solutions, vacuum oscillations, and even the so-called LOW solution (named by Bahcall for its low probability of being correct). Combining together the results that can be expected from SNO and KamLAND, one can anticipate conclusively establishing the neutrino-parameter root cause of the solar neutrino problem.

Risks:

DOE funding for KamLAND is far from certain. The first request by the initial collaboration was turned down. The time frame established by funding constraints on the Japanese group increases the difficulty of the US making meaningful contributions. Indeed, decision making based on a 2001 completion date will likely adversely effect the ultimate performance of the detector. Moreover, the Japanese are forced to cover all detector aspects with their own resources until there is a firm DOE commitment to fund a US effort. The inevitable compromises increase risk.

Neutrino experiments at this scale are obviously difficult. There are a number of LBNL scientists interested in the physics of KamLAND and, while a sizeable group of active participants already exists, nearly everyone has another commitment to manage, at least for the near term. Meaningful participation in KamLAND will likely require extra resources from Physics and Nuclear Science Divisions. On the other hand KamLAND could become a model for collaboration between Physics and Nuclear Science and this would likely be a benefit to both divisions. It could also become the model

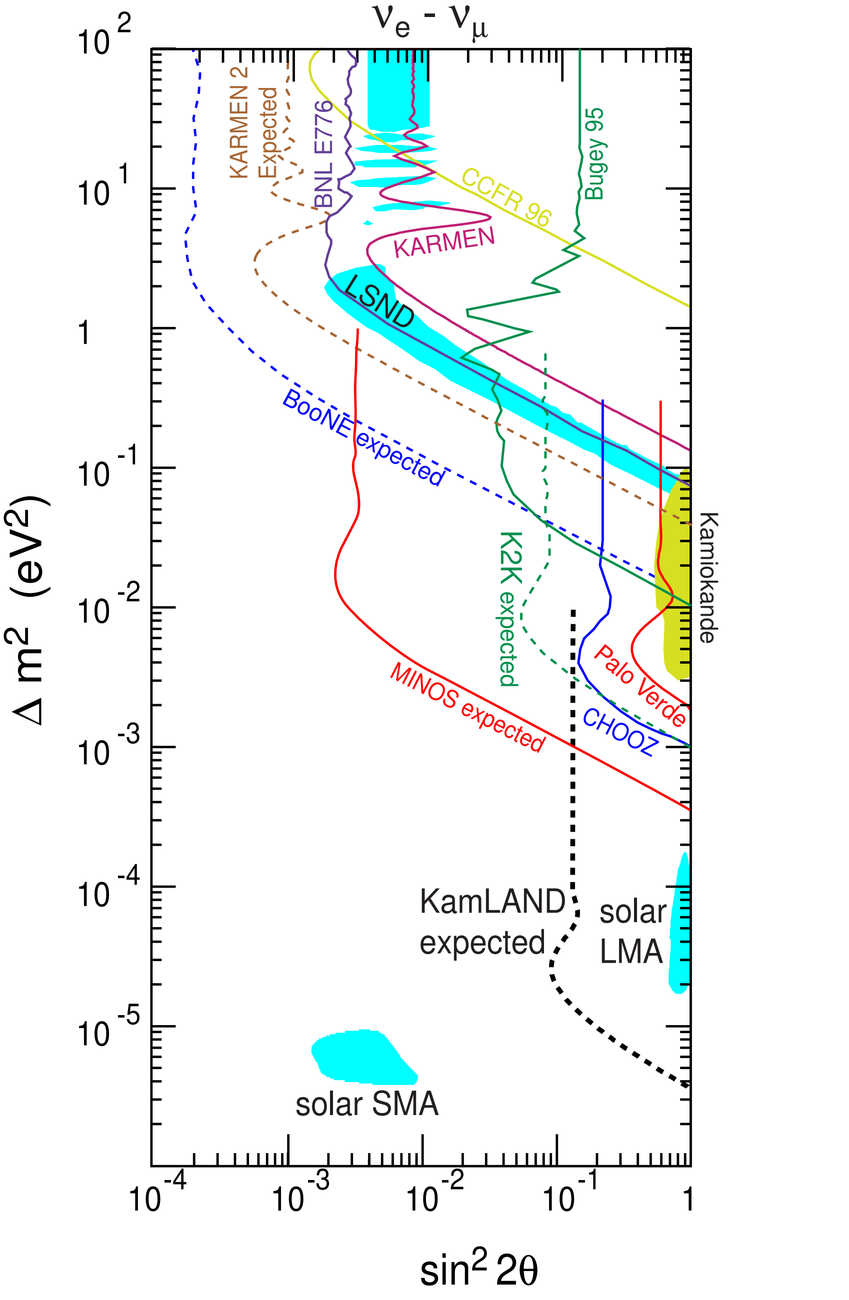

Figure 4.1: Allowed and excluded regions in the $\Delta m^2 - \sin^2 2\theta$ plane for $\nu_e - \nu_\mu$ oscillations. The existing limits are compared with current and future experiments and the region obtained by interpreting the solar neutrino anomaly as due to oscillations. The MSW mechanism is used in deriving the solar neutrino regions. Limits are at 90% CL. Figure taken from the U.S. KamLAND proposal.

for university - national laboratory cooperation in large non-accelerator experiments.

Suitability for LBNL:

The Nuclear Science Division is currently working on SNO, an important neutrino-oscillation experiment at Sudbury, Ontario. Much of the experience gained from SNO will be directly applicable to KamLAND. Both are large-scale liquid detectors read-out with PMTs. Both experiments require extremely low backgrounds, not only from cosmic rays, but also from contamination in construction materials. As was the case for SNO, LBNL's low background counting facility at Oroville would be an asset for KamLAND.

The Physics Division has developed electronics for AMANDA and other large-scale underwater/ice neutrino detectors. These use the ATWR (asynchronous transient waveform recorder) invented by Stuart Kleinfelder. This appears to be very suitable for the KamLAND electronics and would be a major improvement in the experiment, although there is a competing design from Oak Ridge. There are many other technical problems in KamLAND that LBNL physicists would be expected to tackle, including the challenge of calibrating the largest scintillator ever constructed and protecting against radon contamination of the active volume.

KamLAND is anxious to have the LBNL Nuclear Science Division or Physics Division provide the project manager for the US effort. LBNL would be the "lead laboratory" for the US part of the project with responsibility for technical management of the project. In addition one U.S. KamLAND co-spokesperson would come from LBNL.

KamLAND would provide the Physics Division an opportunity to contribute to an important non-accelerator neutrino physics experiment, in an area in which there has been, except for some collaboration with AMANDA, little previous LBNL activity. KamLAND has an excellent potential for making important scientific discoveries.

While some members of the Physics Division have indicated an interest in KamLAND participation, most are already participating in other ongoing projects. Maintaining LBNL's current commitments while undertaking KamLAND would put a strain Divisional resources, at least in the near future.

4.2 MINOS

Outline of Experiment:

MINOS will use the 120-GeV beam from the Fermilab Main Injector and an 8-kt detector in the Soudan Mine, 730 km away. Of the 8.1 kt, 5.6 kt are inside the fiducial volume. If only two of the three supermodules are built, the fiducial volume is 3.3 kt. The original design called for a wide-band beam "H66," but the current baseline design uses a lower energy configuration, either PH2(medium), with a mean energy of about 7 GeV or PH2(low) with a mean energy of about 3 GeV. While the H66 beam would have given about 3000 event/kt/y, the lower energy beams will give more like 1000 events/kt/y (medium) or 200 events/kt/y (low). An upper limit of 10% on ν_μ disappearance would give an upper limit on Δm^2 of $1 - 2 \times 10^{-3}\mathrm{eV}^2$.

Physics Potential:

MINOS would measure several independent quantities: the energy spectrum for "CC" (charged current events of $\overline{\nu}_\mu$); the ratio of "NC" (not just neutral current events, but events without muons) to "CC"; the energy spectrum for "NC" events; and also the number of electron appearance events. For $\overline{\nu}_\mu \to \overline{\nu}_\tau$ oscillations, the most powerful statistic is the ratio NC/CC. For $\overline{\nu}_\mu \to \overline{\nu}_e$ oscillations, the most powerful measurement is actual appearance of charged current $\overline{\nu}_e$ events, recognized by the shower pattern in the calorimeter. The limitation is said to be the fluctuations in such events originating from initial contamination of the beam by $\overline{\nu}_e$. At $\sin^2 2\theta = 1$, an upper limit of $\Delta m^2 = 3 \times 10^{-4}\mathrm{eV}^2$ for $\nu_\mu - \nu_e$ oscillations is said to be attainable in 16 kt-y of running.

Risks:

MINOS has redesigned its neutrino beam to gain sensitivity to the Δm^2 region indicated by Super K. With this lower energy, the event sample will be significantly reduced. The success of MINOS will depend critically on Δm^2: if it is too low, MINOS will have trouble. At the same time, MINOS will have to compete with K2K, which will have run earlier. K2K has a similar L/E, but is limited by its low event rate. See Fig. 2.3. Since MINOS will have a relatively low event rate, it is not well suited to a τ-appearance measurement.

Suitability for LBNL:

MINOS has been under development for many years. It is not clear that LBNL could have a substantial effect on the experiment at this point, nor is there any particular aspect of the experiment that appears to call for a unique LBNL capability.

4.3 Muon Storage Ring

Outline of Experiment:

The possibility of using a muon storage ring as a source for a neutrino beam has been explored by S. Geer in *Phys. Rev.* **57**, 6989 (1997). This would provide an intense neutrino beam, which could be aimed at a distant detector at a location like Soudan, or even Berkeley, KEK, or Beijing. The beam would have (for a μ^+ source) both $\overline{\nu}_\mu$ and ν_e components. The $\overline{\nu}_\mu$ component would be harder, but the ν_e cross section is greater. While the beams would be far from monochromatic, their energy spectrum would be known exceedingly well.

Physics Potential:

In Table 4.1 we record the rates for a one-year exposure of a 10-kt detector for both ordinary charged current events and τ production via $\overline{\nu}_\mu \to \overline{\nu}_\tau$ oscillations, for a variety of distances, energies, and mass differences, using the parameters of Geer's storage ring, assuming $\sin^2 2\theta = 1.$. We see that the τ production rate is enormously dependent on Δm^2, and that you don't want to put the detector too far away.

If it is seen at all, the sign of the observed τ will signal whether it was produced by oscillation from $\overline{\nu}_\mu$ (giving τ^+) or from ν_e (giving τ^-). While the ν_e spectrum is a bit softer than that of $\overline{\nu}_\mu$, the ν cross sections are about twice those of $\overline{\nu}$. The rates for τ^- production from the ν_e beam are given in Table 4.2.

In general, the rates are about twice those for production from the $\overline{\nu}_\mu$ beam component alone. This is so because the charged current neutrino cross section on a quark ($Q = -1/3$) is three times that of an antineutrino on a quark ($Q = 2/3$). If Δm^2 is only 1.0×10^{-3} eV^2, the rates for τ production are not large and high energy is desirable. The Tables show that in this case the rate is essentially independent of L.

Risks:

The construction of a muon storage ring as a neutrino source would be a major undertaking, comparable to PEP-II, or perhaps more. It is thus not something we shall see soon. As a result, much of the physics may have already been done elsewhere. This is particularly so for the ν_μ beam. The ν_e beam, however, provides a special capability not easily duplicated elsewhere. As was suggested by Steve Geer, a ν_e experiment establishing τ appearance would be an ideal use for the muon storage ring. While Geer's proposal is for a storage ring designed specifically to do neutrino physics, it might be possible to use a storage ring built in conjunction with a muon

$E_\mu = 20$ GeV					
Δm^2 (10^{-3}eV^2)	1	3	5	10	$\nu_\mu \to \mu$
$L = 0.2 \times 10^3$ km	8.6×10^1	7.8×10^2	2.1×10^3	8.5×10^3	1.6×10^6
$L = 0.7 \times 10^3$ km	8.6×10^1	7.1×10^2	2.1×10^3	7.4×10^3	1.3×10^5
$L = 2.0 \times 10^3$ km	8.5×10^1	6.9×10^2	1.6×10^3	2.8×10^3	1.6×10^4
$L = 10. \times 10^3$ km	6.4×10^1	6.8×10^1	4.7×10^1	5.9×10^1	6.3×10^2
$E_\mu = 40$ GeV					
Δm^2 (10^{-3}eV^2)	1	3	5	10	$\nu_\mu \to \mu$
$L = 0.2 \times 10^3$ km	3.6×10^2	3.3×10^3	9.1×10^3	3.6×10^4	1.3×10^7
$L = 0.7 \times 10^3$ km	3.6×10^2	3.3×10^3	9.0×10^3	3.5×10^4	1.0×10^6
$L = 2.0 \times 10^3$ km	3.6×10^2	3.2×10^3	8.3×10^3	2.5×10^4	1.3×10^5
$L = 10. \times 10^3$ km	3.3×10^2	1.6×10^3	1.4×10^3	7.6×10^2	5.0×10^3
$E_\mu = 60$ GeV					
Δm^2 (10^{-3}eV^2)	1	3	5	10	$\nu_\mu \to \mu$
$L = 0.2 \times 10^3$ km	7.3×10^2	6.6×10^3	1.8×10^4	7.3×10^4	4.3×10^7
$L = 0.7 \times 10^3$ km	7.3×10^2	6.6×10^3	1.8×10^4	7.2×10^4	3.5×10^6
$L = 2.0 \times 10^3$ km	7.3×10^2	6.5×10^3	1.7×10^4	6.1×10^4	4.3×10^5
$L = 10. \times 10^3$ km	7.0×10^2	4.5×10^3	7.1×10^3	3.2×10^3	1.7×10^4

Table 4.1: Rates for τ production via $\overline{\nu}_\mu \to \overline{\nu}_\tau$ oscillations and ordinary charged current interactions for a 10-kt detector for one year assuming $\sin^2 2\theta = 1$. The flux is taken from S. Geer, *Phys. Rev.* **57**, 6989 (1997)

$E_\mu = 20$ GeV					
Δm^2 (10^{-3}eV^2)	1	3	5	10	$\nu_e \to e$
$L = 0.2 \times 10^3$ km	0.15×10^3	0.13×10^4	0.38×10^4	0.15×10^5	0.26×10^7
$L = 0.7 \times 10^3$ km	0.15×10^3	0.13×10^4	0.36×10^4	0.12×10^5	0.21×10^6
$L = 2.0 \times 10^3$ km	0.15×10^3	0.12×10^4	0.26×10^4	0.37×10^4	0.26×10^5
$L = 10. \times 10^3$ km	0.10×10^3	0.71×10^2	0.92×10^2	0.97×10^2	0.10×10^4
$E_\mu = 40$ GeV					
Δm^2 (10^{-3}eV^2)	1	3	5	10	$\nu_\mu \to \mu$
$L = 0.2 \times 10^3$ km	0.72×10^3	0.65×10^4	0.18×10^5	0.72×10^5	0.21×10^8
$L = 0.7 \times 10^3$ km	0.72×10^3	0.64×10^4	0.17×10^5	0.67×10^5	0.17×10^7
$L = 2.0 \times 10^3$ km	0.72×10^3	0.62×10^4	0.15×10^5	0.45×10^5	0.21×10^6
$L = 10. \times 10^3$ km	0.63×10^3	0.24×10^4	0.17×10^4	0.15×10^4	0.84×10^4
$E_\mu = 60$ GeV					
Δm^2 (10^{-3}eV^2)	1	3	5	10	$\nu_\mu \to \mu$
$L = 0.2 \times 10^3$ km	0.15×10^4	0.13×10^5	0.37×10^5	0.15×10^6	0.71×10^8
$L = 0.7 \times 10^3$ km	0.15×10^4	0.13×10^5	0.37×10^5	0.14×10^6	0.58×10^7
$L = 2.0 \times 10^3$ km	0.15×10^4	0.13×10^5	0.35×10^5	0.11×10^6	0.71×10^6
$L = 10. \times 10^3$ km	0.14×10^4	0.81×10^4	0.11×10^5	0.40×10^4	0.28×10^5

Table 4.2: Rates for τ production from $\nu_e \to \nu_\tau$ oscillations for a 10-kt detector running one year, assuming $\sin^2 2\theta = 1$. The flux is taken from S. Geer, *Phys. Rev.* **57**, 6989 (1997)

collider. This, too, must be regarded as being far in the future.

Suitability for LBNL

There is the opportunity to get in early on such a project. Certainly there is a big challenge in trying to design a detector capable of identifying individual τ leptons and having an active mass of 10 kt.

Chapter 5

Opportunities in Particle Astrophysics

5.1 Cosmic Rays

5.1.1 HiRes

Outline of Experiment:

HiRes is the second generation of the highly successful high energy cosmic-ray experiment, Fly's Eye, in Utah. The unique feature of this experiment is the detection of fluorescent light generated by the cosmic-ray airshower.

At this moment, HiRes is made up of two detectors located on top of two mountains with a separation of 13 km. Currently there are 22 mirrors in each detector. The fluorescent light is focused onto a cluster of 256 photomultiplier tubes by spherical mirrors. The mirrors point to different fixed direction, thus providing a multifaceted view of the sky. The total coverage of the detector is more than 6000 km^{2}sr.

With two or more detectors, stereo observation of an airshower will provide better resolution of the direction and energy of the priminary particle. When the second detector is completed in 1999, it will have a total of 43 mirrors. The plan is to increase the number of detectors to twelve.

Physics Potential:

Eighty years after the discovery, the origin of cosmic rays is still poorly known. It is conceivable that cosmic-rays with energies up to 10^{15} eV could be charged particles accelerated by supernova shock wave. Between 10^{15} eV and a few times 10^{19} eV, cosmic rays might originate from active galactic nuclei. Because protons with energies above 10^{19}eV should lose energy through pion production off the cosmic microwave background, cosmic rays above this energy are presumed not to have traversed long distances. Recent observations of several cosmic-ray events with energies greater than this Greisen-Zatsepin-Kuz'min cutoff seem to indicate that there are active sources at distances less than 50 Mpc from Earth. HiRes is in an excellent position to explore the nature of these ultra high energy cosmic rays.

Risks: Even with two detectors, the statistics on events with energies greater than a few times 10^{19} eV collected by HiRes are still quite small. It is not clear that HiRes can shed enough light on the origin of these spectacular events. HiRes has been under construction for a few years. Another risk is that HiRes is addressing an astrophysical problem which may not have any bearing on fundamental physics.

Suitability for LBNL:

The Physics Division is already involved in AMANDA which focuses on the detection of high energy cosmic neutrino interactions. By detecting the charged cosmic-ray particles, HiRes can provide complementary information about the sources. It is unlikely that LBNL could play a leading role in the short term. Should HiRes continue to expand and exploit new intrumentation, LBNL might find an appropriate role.

5.1.2 GLAST

Outline of Experiment:

GLAST is a gamma-ray detector that will be launched in a satellite in 2005. NASA is now soliciting proposals and SLAC, UCSC, together with several other institutions are submitting a proposal based on 25 towers of silicon strip detectors, followed by CsI calorimetry. GLAST would be sensitive to gamma rays in the 10 MeV to 100+ GeV range.

Physics Potential:

There is increasing interest in high energy gamma rays as evidence for cataclysmic astronomical events. Gamma-ray bursters are sudden events, lasting seconds to minutes. GLAST is a successor to EGRET, offering improvements in coverage and precision of order 10. Active Galactic Nuclei are enormous sources of energy and EGRET saw about 50 AGNs. GLAST might be expected to find 5000. Pulsars and supernova remnants are other targets for GLAST.

Risks:

GLAST would likely address issues more of interest in astronomy or astrophysics than in particle physics and thus would represent a new direction for research altogether.

Suitability for LBNL:

The SLAC-UCSC design is based on silicon strip technology and thus plays to LBNL's strong suit. SLAC and UCSC are long-time partners of LBNL and a collaboration might work well. Discoveries made with gamma rays could connect to our telescope-based work. For example, gamma-ray bursters found with GLAST could be viewed in the optical range rapidly using automated telescopes of the sort LBNL has constructed or operated.

5.2 Large Scale Water/Ice Neutrino Detectors

Outline of Experiment and Physics Potential

Natural volumes of clear water or ice, instrumented with a grid of photomultiplier tubes are a cost-effective way to detect Cerenkov light produced by muon or electron neutrino-interaction products. Each PMT can be protected from ambient pressure by a thick glass sphere, which, together with associated electrical components, comprises the optical module. Arrival times of Cerenkov-photon PMT signals are measures of the distances from their origins on tracks.

First-generation sensor arrays installed in Lake Baikal water and in Antarctic ice at depths as great as 2.5 km have been operating for almost a decade. Both the efficacy of the extremely difficult deployment methods and the ability to reconstruct events have been demonstrated. Theoretical considerations indicate that a kilometer-scale detector volume is needed, instrumented with an arrangement of thousands of optical-module photon detectors. Building especially on the experience of the AMANDA telescope in South Polar ice, the technological and cost challenges are being addressed now, preparatory to proceeding to construction of kilometer-scale neutrino telescopes.

AMANDA, the most advanced of the high-energy neutrino telescopes, consists of vertical "strings" of widely spaced optical modules, nearly all pointing down, sunk into deep hot-water holes drilled in the South Polar ice cap. The most recently deployed strings have 42 PMTs equally spaced from 2.4 km to 1.5 km depth.

A critical issue is the data acquisition: local signal processing and transfer from deep remote sensors to a surface storage and analysis facility. The electronic circuitry must be cost-effective, robust and reliable. It should provide high quality data from which complex events can be characterized with accurately reconstructed tracks. These requirements lead to a specific digital system architecture. At LBNL, R&D on digital, intelligent optical modules has advanced to the degree that large-scale tests of digital system concepts are now being prepared.

Several data-transmission approaches have been evaluated in situ by AMANDA. Analog signals from PMTs are sent on both electrical and fiber-optic links to the surface to be processed. However, the long analog electrical path introduces serious limitations. Signal quality is severely degraded by dispersion and attenuation, requiring extremely high PMT gain, limiting dynamic range to less than 10, and degrading beyond recovery the waveform information. Analog signal transmission by optical fiber preserves waveform information, but is very costly; in addition, experience indicates that a significant fraction of the fibers are damaged during deployment/freeze-in, for reasons that are not well-understood. Digital signal transmission, on the other hand, superbly maintains waveform information and its implementation costs less than analog optical. A digital system avoids the difficulties and high costs of an analog system. For some years LBNL has promoted the use of digital electronics whose basic component is a custom integrated circuit developed at LBNL that captures and digitizes waveforms from a photomultiplier tube at each module. In a collaboration between LBNL and the Jet Propulsion Laboratory, two prototype Digital Optical Modules (DOMs) were constructed and deployed in the ice during the 96-97 deployment season. These two were equipped with first-generation custom waveform-capture ASICs which returned, for the first time in any water- or ice-based experiment, true waveform information from real events in a neutrino telescope. These DOMs demonstrated the basic capabilities sought for PMT signal capture. See Fig. 5.1

The path to a km-scale detector array may have four stages: The first is an R&D stage, which has two components 1) Physics and Detector Simulations; 2) Detector Electronic Systems. The second stage is a Prototype Demonstration Stage, which will consist of a single string of optical modules installed at the AMANDA site and a deployed two-floor prototype NESTOR tower, each instrumented with digital electronics. It is not yet clear whether the successive stages will be made in collaboration with AMANDA, or NESTOR, or both; an independent effort is unlikely. If AMANDA, the next stage foreseen is an extension of that with six additional strings. Stage III spans about 10% of a square kilometer, and would begin a productive

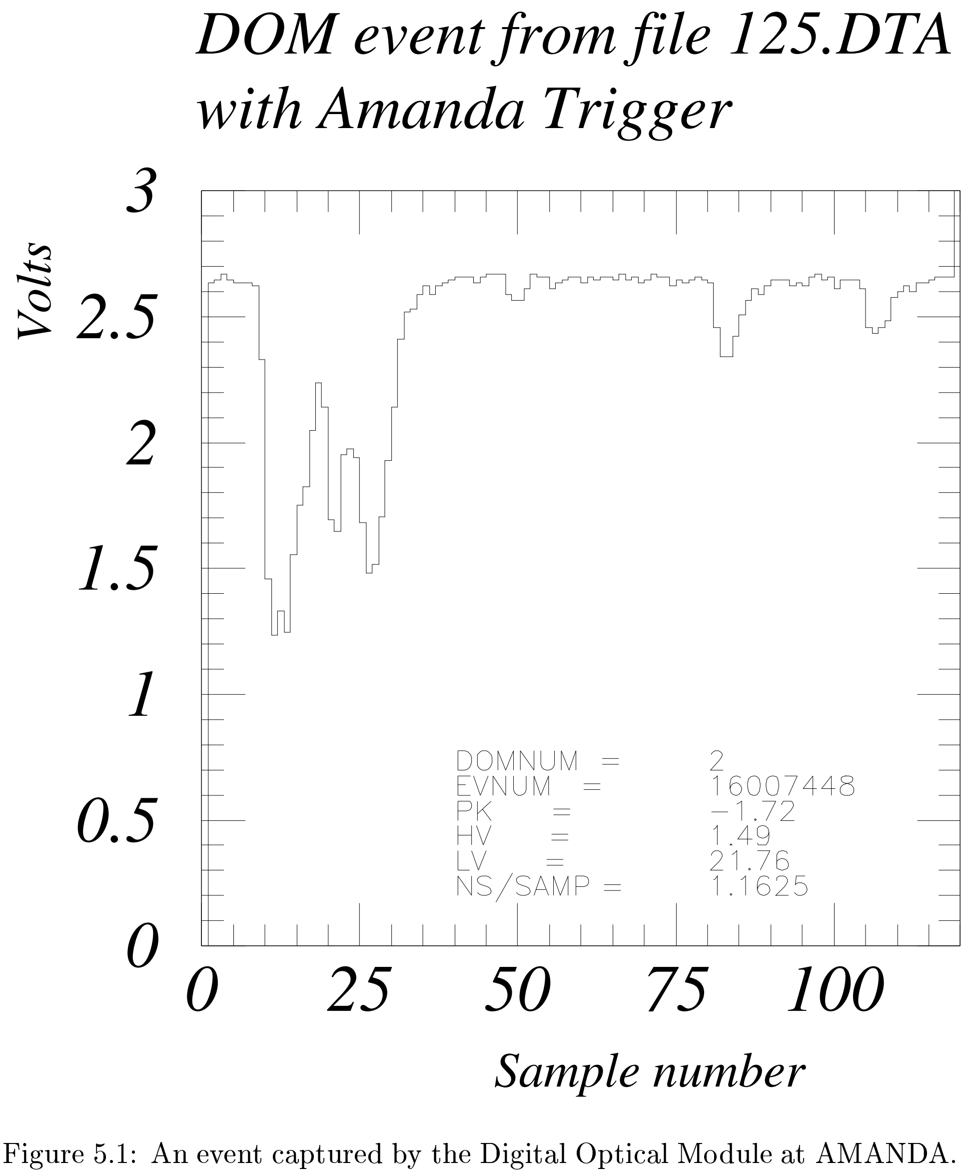

Figure 5.1: An event captured by the Digital Optical Module at AMANDA. Each sample is 1.1625 ns. Only six of eight bits were transmitted due to bandwidth limitations in the communications link.

scientific era. The last stage, the most costly, would expand Stage III to a full km-scale complex of optical modules. Stage I is underway; Stage II expected to take three years, FY99, FY00, and FY01. Stage III would proceed with a series of deployments starting in FY02.

To allow use of the AMANDA deployment and operation infrastructure at its Antarctic site, and to have access to all data made, the km3 group has joined the AMANDA collaboration. LBNL has joined with physicists from DESY, Univ. of Wisconsin, Univ. of Pennsylvania, and UC Irvine, in an MOU that sets out responsibilities of the participating institutions for the construction of the test string. LBNL will provide sub-surface electronics and data transmission network.

The working relation established with the AMANDA group has made available Monte Carlo code to simulate the performance of a km-scale detector. The NERSC facility is ideal for running MC code and, indeed, for data reduction. Raw data from AMANDA, a fraction received in real time via a satellite link, are stored at the NERSC mass storage facility and subjected to a first-pass filter as the initial step in event selection and reconstruction. Study of these data is critical to understanding detector capability and thus to optimizing the km-scale configuration design.

Risks

Inherent in this sort of exploratory work is the uncertainty that a signal exists to observe or that it is intense enough to be seen with an affordable detector. While there are plausibility arguments for the one-kilometer-scale of the detector, these can be verified only a posteriori.

A limitation, rather than a risk perhaps, is the possibility that while a signal will be observed, what will be learned will be of interest to astrophysics rather than fundamental particle physics.

A second risk is that there will not be sufficient funding to build a detector of adequate size.

Work directed toward a km-scale neutrino telescope has progressed at LBNL for some years, supported by LDRD funds, as well as Physics and Nuclear Science divisions' salary support for key personnel. A proposal was submitted to NSF Office of Polar Programs to fund procurement of components of the digitally-instrumented string of optical modules to be deployed during the Austral summer (calendar) 2000/2001. A total of $365K was requested. Favorable action is expected.

Funding has not been secured for the next stage in the progression to a km-scale array. A possible source is the proposed U.C. Berkeley Science and Technology Center (STC), under consideration by NSF. The U.C.B. proponents have requested funds to equip and deploy six additional strings to evaluate technology to be used in the ultimate stage, the km3 instrumented volume. NSF will make its decisions on STCs late in FY99.

No proposal has yet been made anywhere for construction of a km-scale array. The most advanced planning has been within the AMANDA collaboration; the most likely route to a full detector and likely earliest to appear is an extension of AMANDA, already with a name: IceCube. About 100 strings with 50 optical modules each are being considered. A proposal for IceCube may be developed under the auspices of that STC, or by an unaffiliated international consortium, to be submitted to funding agencies perhaps in FY01. Cost estimates are in the \$50M - \$100M range. A healthy fraction of that, even as much as half, may be expected to be provided by foreign sources, Germany and Sweden especially. The completion date depends on drilling ability. Eight strings per season appear close to certain; 16 may be possible with improved capacity, but this has yet to be realized. Thus, construction would continue for at least six years, or, optimism aside, a dozen years, starting in FY02.

Suitability for LBNL

LBNL is well suited to taking a major role in design, management and construction of a km-scale neutrino telescope in ice or water. LBNL personnel have led in the development of digital instrumentation, likely to be the technology of choice. LBNL engineering and management skills and experience directing large projects put it in a strong position to be the lead, rather than a contributing institution, in design, construction, and operation. Expert computing staff and major facilities, NERSC among them, make LBNL a prime candidate to lead in data handling and analysis. It remains to be demonstrated that DOE will make the substantial cultural shift to fund substantially a project in concert with NSF, which, because of its stewardship of Antarctic science, must have a dominant role in IceCube. Other options include making NSF funds available to LBNL, or an alternate effort at an ocean-based apparatus.

5.3 Experimental Cosmology

5.3.1 Cosmic Microwave Background

Outline of Experiments:

The LBNL CMB group is involved in two experiments under development:

MAP (Microwave Anisotropy Probe) is a NASA MidEX mission scheduled for launch around early 2001. It is expected to provide essentially full-sky coverage at five frequencies (30, 40, 60, and 90 GHz) with reasonably high sensitivity and with angular resolution corresponding to about 10^6 pixels. This is quite a large data set; the current MAP data analysis plan depends upon the noise properties of the data being close to white and on azimuthal symmetry in the data. A failure to achieve either of these is likely to cause a serious degradation in the results. This is one reason to have an effort here to develop more general processing and analysis techniques that could be accomplished in a reasonable time.

The Max Planck Surveyor is the ESA M3 mission and launch is anticipated in 2007. Planck has nine active frequencies covering the range from 30 to 850 GHz. Planck's sensitivity and angular resolution are significantly better than MAP's. The Planck CMB anisotropy maps are anticipated to have approximately 10^7 pixels. A simple analysis shows that the Planck data should be capable of deterimining many cosomlogical parameters to roughly the 1% level. The full analysis of the Planck data will require new techniques and capabilities as well as a deeper understanding of the foregrounds.

A major effort here in collaboration with NERSC is the development of new techniques for map generation and likelihood fitting of the data to cosmological models and parameters. Straightforward alogrithms require memory and data storage that goes as the number of pixels squared and computational requirements that go as the number of pixels cubed. We have identified and are testing approximations that reduce the number of operations to the number of pixels to the 2.5 power.

However at the present time, the full T3E 1024 processors are required for several hours to process the BOOMERANG-NA and MAXIMA-1 data sets with 25,000 and 30,000 pixels respectively. We anticipate that the BOOMERANG-LDB (long duration balloon-flight around Antarctica) data set will have more than 200,000 pixels. Even at somewhat reduced resolution, its analysis once will take our full T3E allocation (roughly a week of the full T3E).

Physics Potential:

Observations of the CMB provide not only support for the hot Big Bang models but also strong constraints on its development and basic parameters. Ultimately, observations of the CMB have the potential for precision tests of models and to measure these fundamental parameters to the per cent level.

Predictions of theories for CMB anisotropy are statistical in nature. For many theories, the complete description is given by the angular power spectrum. The angular power spectrum of the CMB fluctuations depends on virtually all classical cosmological parameters: the Hubble expansion rate h, the density parameter Ω, the cosmological constant Λ, the constituents of the universe, and so on.

Current cosmic microwave background observations already permit some constraints to be placed on certain cosmological parameters, such as the density parameter, Ω and the Hubble expansion rate, $H_0 \equiv h$ (100 km/s/Mpc). One of the more ambitious analyses to date (Tegmark 1998 astro-ph/9809201) made a nine-parameter fit using just the CMB data and the CMB and Supernova 1a data.

Figure 5.2 shows the currently observed power spectrum and some fits made by choosing different values of the cosmological parameters.

Risks

The CMB experiments are of two basic sorts: balloon observations and observations from space. The balloon observations are relatively inexpensive, but are subject to the vagaries of weather and geography. Satellite observations are tremendously expensive and require many years of planning. DOE has no history of supporting this field. Funding is always a problem. Proponents of balloon experiments have predicted that new measurements will have sufficient precision to answer many of the important questions by determining the power spectrum as a function of angular size, but their claims are yet to be demonstrated.

Suitability for LBNL

LBNL has a very strong tradition in this field, both in satellite and balloon experiments. This efforts is an excellent complement to the Supernova Cosmology Project, which seeks to learn many of the same numbers, but with entirely different techniques. LBNL collaborates with the UCB campus on balloon flights. As data sets grow, the value of our collaboration with NERSC will increase.

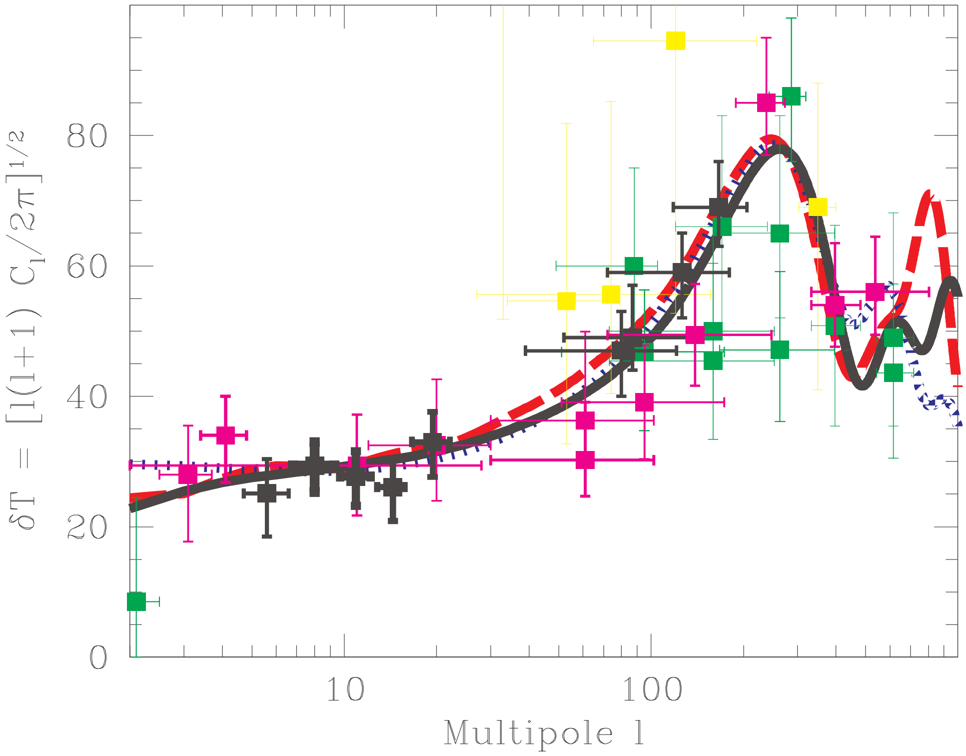

Figure 5.2: The cosmic microwave background power spectrum observed in various experiments. The three curves show fits based on various choices of cosmological parameters including Ω_Λ and Ω_M.

5.3.2 Supernova Cosmology Project

Outline of Experiment and Physics Potential:

The Supernova Cosmology Project created the field of distant supernova study. The analysis of the 42 of the 80 some supernovae they have discovered has provided the best determination of the fundamental cosmological parameters Ω_M and Ω_Λ, the mass density and cosmological constant, relative to the density that would just close the Universe. The current constraints in the Ω_M- Ω_Λ plane are shown in Fig. 5.3.

The Supernova Cosmology Project at LBNL is currently working on two new directions this year. One direction is a pilot study to discover supernovae at twice the typical redshifts of previous searches. SCP completed this pilot study in the fall and were able find a supernova at redshift of $z = 1.2$ and to obtain a spectrum and lightcurve for this supernova. The technique used was essentially the same as the technique that discovered the 80 supernovae at lower redshifts, but the search was performed on a rather narrow-field camera that is available on the Keck telescope.

To follow the pilot study of very distant supernovae with more extensive data, it will be necessary to find or build a wider-field camera on a large telescope or use much more time on a next-to-largest telescope.

This spring there will be a large multigroup and multi-telescope campaign intended to greatly improve the data set for low-redshift supernovae. This is SCP's first attempt at working with supernova at redshifts $\sim$0.1. This redshift range is the most suitable for studying the appearance of supernovae across a wide range of host-galaxy environments (as would occur in high redshift studies). Depending on the success of this spring run, the data set may be strong enough to serve for several years in this redshift range. Alternatively, a long-term search-and-follow-up strategy at this redshift range may be required.

SCP has identified three goals for the next few years. The timescales are probably on the order of two through nine years.

Goal 1. Continue the study of systematics, extending the Hubble diagram (the plot of supernova apparent magnitudes vs. redshift) to still higher redshifts. With upcoming technology it should be possible to work at redshifts up to approximately $z = 1.5$. Beyond a redshift of 1.2, some of the cosmologies predict supernova magnitudes that are brighter than for the zero-cosmological-constant universe, as opposed to the lower redshift range, where our best-fit cosmology is fainter than the zero-cosmological-constant universe. Thus it is possible to test the grey-dust hypothesis since dust can

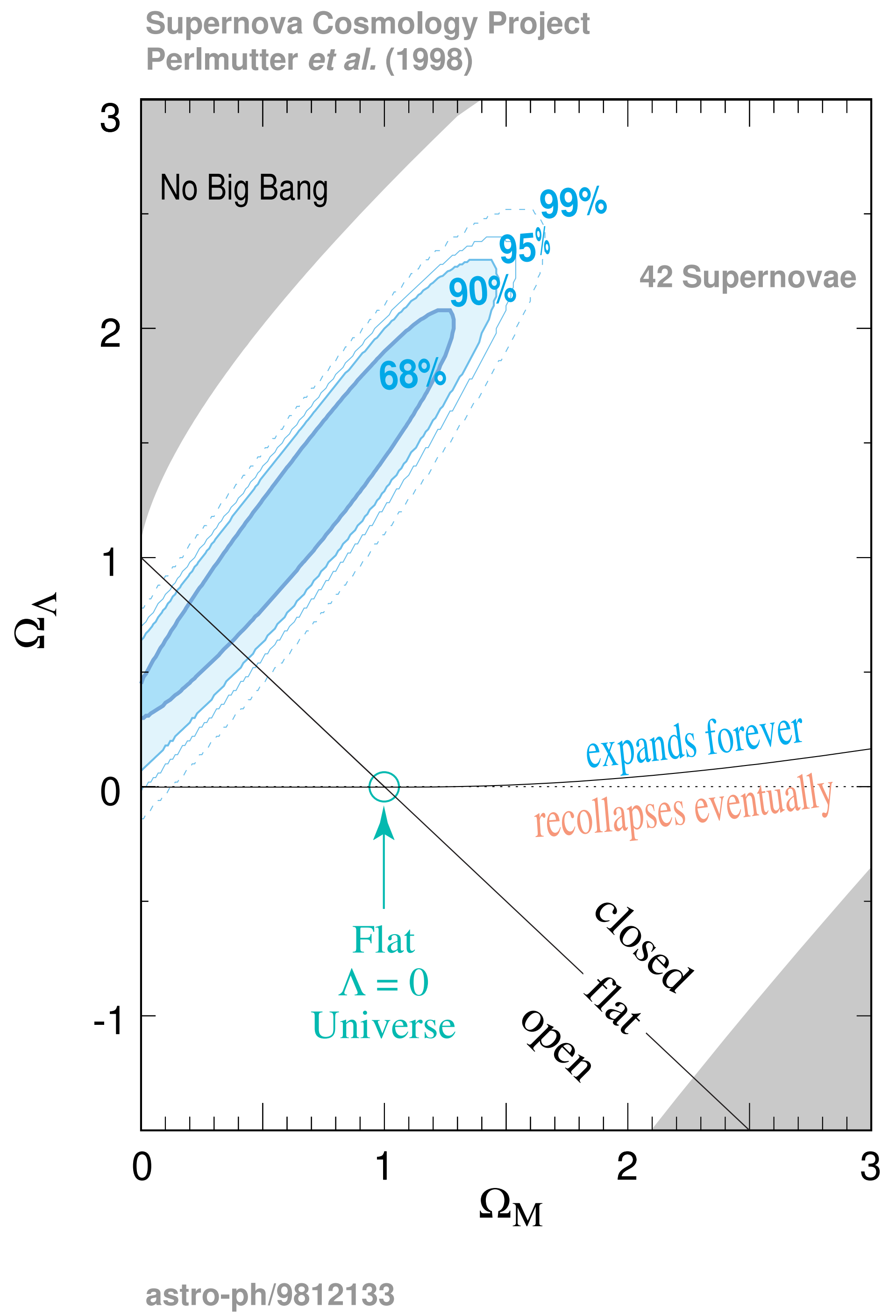

Figure 5.3: Constraints in the Ω_M- Ω_Λ determined by the Supernova Cosmology Project from the first 42 of their supernovae.

only make supernovae fainter rather than brighter. Studying a much larger sample of low redshift supernovae could reveal indications of evolution in different environments.

Goal 2. It is important to separate measurements of the mass density $\Omega_{\rm mass}$ and the cosmological constant Λ using supernovae by themselves. So far results are primarily sensitive to the difference between mass density and cosmological constant, but more supernova at the highest redshifts would constrain the two densities independently. Cosmic microwave background measurements can tell us about the linear combination $\Omega_{\rm mass} + \Omega_\Lambda$ (i.e. the spatial curvature of the universe), but for numbers as important as the cosmological parameters it is important to have more than one independent route to the measurement.

Goal 3. Finally, if data show that there is actually a dark energy that is causing the universe to accelerate, its investigation will be of utmost importance. So far the only route to identifying this dark energy that has been suggested is through its equation of state. Some of the alternative dynamical scalar field theories suggest an equation of state that changes as a function of time, i.e. as a function of redshift. A detailed study of about ten type Ia supernovae at each redshift interval between redshift of 0.2 and redshift 1.2 in steps of 0.03 could answer the question. This is clearly a very ambitious undertaking.

The exploitation of new instrumentation being developed at LBNL could be the key to achieving these goals. Among the possibilities are:

1. A wide-field camera for the Keck telescope/Subaru telescope. By tiling the focal plane of the Keck telescope with over a hundred 2K-by-2K CCD chips (presumably LBNL CCD chips, to make this affordable) it would be possible to discover and study large numbers of supernovae at redshifts out to $z = 1.5$. This would be a relatively ambitious instrumentation undertaking, since no astronomical detector of this scale has ever been built. This scale of detector does not appear to be beyond the range of designs used for particle physics detectors. It is not clear whether it will be possible obtain an agreement with the Keck telescope that would allow SCP use this detector enough of the time, but it would certainly be worth pursuing if SCP decides that this is the science that is most important.

2. Trade CCDs for time on an existing camera/telescope. A less ambitious undertaking might be to fabricate LBNL CCD chips to the specifications of an existing camera on a telescope. This option might allow us a faster access to science that can be obtained with an existing camera. It is possible to pursue this simultaneously with one of the other instrumentation

options.

3. Buy time or trade for time on a smaller 2.5-meter telescope to study supernovae at redshifts $z = 0.2$ to 0.3.

4. Most ambitious of all, a Space Station or Small Explorer satellite mission. This can be thought of as an update of a proposal that we submitted to NASA five or six years ago for a Small Explorer satellite mission. The concept is to build a wide field camera, ideally one degree by one degree, with an approximately one-meter aperture mirror, and observe a small set of fields of sky over and over again for one to two years. An objective prism (or equivalent) could be put into the optical path so that every supernova discovered would also have a low resolution spectrum obtained. This would allow lightcurves to be synthesized in several different filter bands that would match low redshift bands, redshifts to be determined, and supernova type to be determined. Ideally the telescope could collect data for a year or two and all this data could be analyzed after the fact to pull out all of the supernova lightcurves and spectra. Preliminary estimates suggest that this could easily obtain hundreds of supernovae in the redshift ranges between $z = 0.5$ and 1.5. The question remains as to what would be the most useful data set to obtain.

Risks:

The primary doubts that remain concerning the supernova cosmology measurements include the possibility that there is a new previously unobserved form of dust in the universe – dust that does not redden light as it absorbs it. (Presumably this would mean dust grains that are substantially larger than 0.1 microns.) There are several studies underway to check for such a "grey" dust, and the SCP distant supernovae might be useful here. Another potential concern is the possibility that type Ia supernovae were different at earlier times. However, these supernovae have already been studied in the various environments in which they occur in the earlier Universe and no such effect is seen. The statistics for this conclusion are small and need expansion.

Suitability for LBNL

The Supernova Cosmology Project is one of the most important achievements of the Laboratory in the past ten years. It is an outgrowth of a small but continuing program in astrophysics, begun by Luis Alvarez and continued by Rich Muller and his collaborators. Under the leadership of Saul Perlmutter it has gained broad recognition for its pathbreaking work. The Division has a very strong interest in its continuation.

Chapter 6

Conclusion

6.1 Current Program

The LBNL Physics Division has an extensive experimental program in hadron-collider physics, both at the Fermilab Tevatron Collider and at CERN's Large Hadron Collider. Our involvement with the Fermilab program will likely continue beyond 2005, about the time that running will begin at the LHC. In this report we have taken these efforts as givens and have not examined them further.

Similarly, the BaBar program at SLAC's PEP-II will surely run beyond 2005. The Division is heavily involved in this CP-violation experiment and we have taken this program, too, as a given.

The Division's effort in experimental cosmology has received wide notice. Because this work is less familiar to many physicists in the Division and because it is not so traditionally part of the work of the High Energy Physics program of DOE, we have reviewed this program much as we have reviewed possible new directions for the Division. For similar reasons, the work on large-scale astrophysical neutrino detectors has been reviewed.

6.1.1 Computing

Within the existing program there is increasing activity in computing. Bob Jacobsen in BaBar, Ian Hinchliffe in ATLAS, and Marjorie Shapiro in CDF, all have major leadership roles in areas like reconstruction and/or Monte Carlos. Just as the Physics Division's work on these experiments has depended in the past on the expertise available in IC design from the Engineering Division, it is now becoming dependent on expertise available from

Computing Sciences. LBNL was the leading U.S. institution in silicon detectors and their read-out electronics in the 1990s. It may be increasingly viewed as a center for analysis excellence in the coming decade.

6.2 Potential New Commitments or Expansions of Current Efforts

6.2.1 NLC

NLC is a major priority of the national HEP program, as stated, for example, in the Gilman HEP Subpanel report. Our traditional partner, SLAC, is leading the NLC effort and LBNL's AFRD is heavily involved. LBNL has the talent and experience to make major contributions to an NLC detector design. The leading candidates for tracking in an NLC detector are an all-silicon system and a TPC. In both these technologies, LBNL is a leader. In the latter instance, the Nuclear Science Division has valuable recent experience that could be brought on board. For the former, not only does LBNL has unparalleled experience in silicon strip and pixel development, but the LBNL CCD may be the detector of choice for the innermost layers.

While NLC is a national priority for HEP, obtaining funding for such a large project will be difficult indeed. There are potential risks, too, in the physics. If there are no new particle thresholds accessible the NLC, the physics opportunities will be limited.

6.2.2 Neutrino Physics

The impressive results of the Super Kamiokande experiment have provided the most convincing evidence for neutrino oscillations and thus for neutrino masses. This is an area in which the Physics Division has had next to no participation. Many new experiments are being launched. It is not possible to know with certainty which ones will turn out to be most successful. If the Super Kamiokande results turn out to be correct and the proper interpretation is that they have observed $\nu_\mu \to \nu_\tau$, they will have already obtained reasonably good measurements of the mixing angle and Δm^2. MINOS and K2K will also be able to measure these quantities. In the longer term, a muon storage ring might be built to provide an intense source of both $\overline{\nu}_\mu$ and ν_e. There will be intense competition between these experiments and others being launched at CERN in connection with Gran Sasso.

Of course, this will leave unanswered questions involving ν_e and solar neutrinos. The KamLAND experiment, which uses a large liquid scintillator detector to observe reactor neutrinos in Japan, provides a two-orders-of-magnitude step in this area and would test one of the MSW solar neutrino solutions. It might even allow direct measurement of solar neutrinos and a test of the other solar neutrino solutions. KamLAND has an awkward schedule, dictated by Japanese funding procedures. Moreover, significant U.S. participation depends on multi-million dollar funding by DOE, which is far from assured. An LBNL joint Physics Division - Nuclear Science Division team is leading an effort to resubmit KamLAND to DOE (contingent on approval from the Division Directors).

6.2.3 Large-Scale Neutrino Detectors

LBNL has already made important contributions in large-scale astrophysical neutrino detectors, especially with its work on digital optical modules. This effort is carried out in concert with the Nuclear Science Division. Because the funding of this program is necessarily not of the traditional DOE sort, it is harder to predict how it will develop. Despite its small size, the LBNL group has had an important impact on the international communities working both in water and in ice. Funding has often been a problem for this work, which tends to fall in between cracks. If successful, the U.C.B. proposal to NSF for a Science and Technology Center would be a big step forward.

6.2.4 Experimental Cosmology

The cosmic microwave background experiments and the Supernova Cosmology Project at LBNL have received enormous recognition and publicity for good reason. They are now poised to attack in complementary ways some of the most fundamental questions ever addressed by science. Already, SCP results point to values for the mass density of the Universe and the cosmological constant that were not anticipated. Nailing down the precise values is a compelling challenge for both SCP and the CMB work.

Both the SCP and CMB efforts will see enormously increased data sets in coming years. Part of the response to this will necessarily come from improved computer resources and in particular from NERSC. However, Additional personnel may be needed simply to carry the stated goals of these programs.

6.3 Detector Development and Instrumentation

While the Working Group did not address directly issues of detector development and instrumentation, consideration of future directions inevitably revealed the importance of investing in these areas. Some of the most promising direction for future experimental work have been opened by advances in detectors and instrumentation here at LBNL. Much of the work on large-scale neutrino detectors derives from the ATWR chip developed by Stuart Kleinfelder. This same chip would be an excellent match to the needs of KamLAND. The CCD's developed by Steve Holland seem poised to take over imaging instruments in astronomy, benefiting directly the Supernova Cosmology Project, which has collaborated in their development. The very same CCD's appear ideally suited to a vertex detector for NLC, a fitting result since they grew out of SSC detector development.

The lesson is that continued investment in detector and instrumentation development is essential to providing LBNL physicists unique capabilities for future work. Beyond that, these successes demonstrate how science, technology, and industry can interact. The CCD's, which grew out of a combined science and technology effort, may well have important commercial significance.

6.4 Collaboration with other Divisions

Collaboration with the Engineering Division is inherent in all the experimental work of the Division. This may be most apparent when Engineering provides the very innovations that make experiments possible, ASICs, CCDs, etc., but it is true more generally. The health of the Engineering Division is essential to our success.

There is a long tradition, as well, of collaboration with AFRD. This has continued with PEP-II. AFRD is active on all fronts of high energy physics accelerator development: NLC, muon-collider, VLHC. Direct interaction between the Physics Division and those in AFRD working in this area is obviously imperative.

What is new are the increasing opportunities for collaboration with the Nuclear Science Division in projects like AMANDA, KamLAND, and NLC. For these three projects, the Nuclear Science Division can contribute important expertise. In AMANDA and KamLAND the very subject matter straddles the domains of the two divisions. Working collaboratively on these projects will give the Physics Division opportunities it would not have on it own.

NERSC provides the LBNL Physics Division a very special resource, one that fits well the needs of the developing program. Both the hadron and e^+e^- collider programs will generate vast data sets and NERSC personnel are active in developing the techniques required to cope with this wealth of information. NERSC will be essential, too, for the analysis of the very large data sets generated by the experimental cosmology program.

It is certain that the Physics Division, which has always been dependent on the Engineering Division and on AFRD, will be working closely with the Nuclear Science Division and with NERSC in the coming years. It will be essential to find effective ways of collaborating with these divisions, despite the complications caused by their different sources of funds within DOE.

Chapter 7

Views from the Division

7.1 Questions sent to the staff

At the conclusion of the Working Group's study a draft of this report was made available to the Division's scientific staff and a public presentation was made. Afterwards, the staff was queried by e-mail, asking for their responses to a series of possible conclusions. The statements were chosen deliberately to be mutually incompatible in order to stimulate the exchange.

1. Participation in the NLC detector design effort is essential for the Division. We should establish an ongoing working group, led by someone making a real commitment of time to the effort.

2. It is important for the Division to become involved in some work on neutrinos, especially an investigation of neutrino oscillations.

3. LBNL should seek a significant role in MINOS as the major U.S. high energy physics program to investigate neutrino oscillations.

4. While neutrino oscillations would be a good complement to the current program in the Division, MINOS does not offer a sufficiently attractive opportunity. We should focus on a later effort in the U.S.

5. While neutrino oscillations would be a good complement to the current program in the Division, MINOS does not offer a sufficiently attractive opportunity. We should focus on an accelerator-based effort either at CERN (probably using Gran Sasso) or in Japan (K2K or the Japanese Hadron Facility, which will send a beam to Super K).

6. Neutrino oscillations would be a good complement to the current program in the Division and the KamLAND reactor-neutrino experiment offers the most attractive opportunity at this time and should be pursued by the Division.

7. The LBNL experimental cosmology program studying the cosmic microwave background and the expansion of the Universe through supernovae is so successful that we should make an effort to shift more of our resources in that direction.

8. Opportunities in astrophysics like GLAST (a satellite detector to look at gamma rays) and MILAGRO (water cerenkov detection of very high energy gamma rays) represent an important new direction that LBNL should join.

9. While the Division's program in B physics is already strong, both at SLAC and at Fermilab, these programs will not answer all the outstanding questions and LBNL ought to join a project like BTeV, which should complement the SLAC program and surpass CDF and D0.

10. The LBNL CCD developed by Steve Holland and co-workers represents a major contribution to astrophysics, particle physics, and possibly other fields as well. LBNL should devote more resources to exploiting this new advance.

11. The Division spends less and less on detector development that is not connected to a specific experiment. This trend has to be reversed if we are to maintain our leadership in this area.

12. The increasing role of the Division in major software projects should be further augmented though this may restrict other new efforts.

13. The AMANDA/km-cubed work already has significant Physics Division (and Nuclear Science Division) activity and this should strengthened rather than putting resources in a new astrophysics program like GLAST or MILAGRO.

14. The Cold Dark Matter Search has received important contributions from LBNL. It is running at Stanford and will run in an improved version at Soudan. LBNL should increase its participation in this project which is in fact a search for an elementary particle.

7.2 Excerpts from the responses

The purpose of the Working Group's Report is to generate an ongoing discussion on the future directions for the Division and to prompt individual scientists within the Division to identify most promising opportunities. The e-mail responses to the above statements are a beginning of that process. Naturally, the replies showed some variation in opinion.

- General Remarks
  - Our highest priority over the next decade is to fully support our current accelerator-based experiments at the Tevatron, PEPII and the LHC to maximize the physics return on the enormous investment we have made, and will make, in these experiments.
  - Nothing on the immediate horizon compares with the direct exploration of the TeV region at LHC, which promises to break open many of the major questions in high energy physics.
- NLC
  - I believe that LBL should not only be a sideline booster for the NLC, but also contribute genuine resources to making the NLC a success.
  - Participation in the nascent NLC detector design effort is essential to the Division in the near future. LBNL has unique technical strengths to bring to the design and construction of NLC detectors and we plan to form an ongoing working group to provide a forum for our contributions in this early design phase.
  - Although I am not working at the energy frontier, I always believe the best approach in making discoveries is to go to the highest energy and the cleanest environment if the rate is there. In addition, the challenge of reaching the highest energy and doing experiments in such a condition will provide us strong motivation to explore new ideas. Hence I support establishing an ongoing working group.
  - I believe that the issue here is not whether we have a clever widget (like a CCD) that we can contribute, but whether the NLC is scientifically the right next capital investment for the international high energy physics community. That case will have to be made, and we need to be involved in that decision process.

- Neutrinos
  - We're a big program; it would be a mistake to focus too narrowly and miss neutrinos altogether.
  - As for neutrinos, I am completely unconvinced that now is the right time for a major re-direction of effort. ...I would like to see a real oscillation, preferably in an appearance experiment.
  - The study of neutrino oscillations is of fundamental interest and would complement the current program in the Division.
  - No doubt neutrino oscillations is getting very interesting. It is to the point that it will be a mistake for us not to participate in this area. The key is to identify a definitive experiment.
  - Even if they are successful the neutrino experiments will probably not lead quickly to a precise understanding of what lies beyond the standard model.
  - a major new initiative in neutrino physics should have a critical mass within the LBNL Physics Division so that we are not just marginal players yet it must also not compromise our ability to reap the profits of the investment we are making in the LHC.
- MINOS
  - Would make sense to me if a nucleus of interested LBL people wanted to participate in MINOS.
  - I think it is important for LBNL to be involved in neutrino oscillations, but at the moment I don't see a meaningful role in MINOS which exploits our uniqueness.
  - Aside from BooNE, which should produce a null result, I'm unaware of what else besides MINOS offers opportunity for neutrino physics in the U.S.
  - The goal of MINOS is attractive. However, I can't identify an area that we can contribute significantly. Partly it is because we don't know how to detect tau appearance at such a low energy and at a reasonable low cost.
- KamLAND
  - if we can pursue it without being devastated by the quite possible lack of funds for U.S. KamLAND, I am supportive.

  - The study of neutrino oscillations is of fundamental interest and would complement the current program in the Division. A modest investment by the Division, in strong partnership with the Nuclear Science Division, in the KamLAND reactor-neutrino experiments offers the most attractive opportunity immediately for participation in this area.
  - I am not as yet convinced about the technological feasibility of KamLAND. If feasible, then it certainly provides an opportunity to exploit our role on the national scene.

- Experimental Cosmology
  - The LBNL experimental program in cosmology through the observation of the cosmic microwave background and the expansion of the universe through the detection of supernovae has had a profound effect on our picture of the universe. These programs have exciting prospects over the next decade and increased support and participation by the Division would be highly beneficial.
  - By all means we must maintain our leadership in this program. My concern is whether DOE is willing to provide additional support or allow us to shift more of our resources to this program.
  - Astronomy/Cosmology should be expanded since it is a growing field with a guaranteed futures and LBNL is recognized as a world leader.

- Astrophysics
  - I'd go slow on astrophysics that doesn't appear to have high import for cosmology or particle physics. On the other hand, high-energy neutrino astronomy does seem to me to have high such import, and to represent an important new direction for LBL/U.C. Berkeley, particularly if Price's NSF proposal flies.
  - There is already significant involvement by the Division, in collaboration with the Nuclear Science Division, in neutrino astronomy by AMANDA and a clear vision for a major future program utilizing LBNL strengths in instrumentation and management of large projects.

- CCD Development

- This should be pushed without a doubt.
- The CCD effort must be strongly supported and augmented significantly.
- We should vigorously pursue the detailed R&D on CCDs in order to clarify their possible impact. It's still early to throw money at big CCD systems, or to base our geopolitical plans on them.