

# Future Accelerators(?)<sup>1</sup>

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**Abstract.** I describe the future accelerator facilities that are currently foreseen for electroweak scale physics, neutrino physics, and nuclear structure. I will explore the physics justification for these machines, and suggest how the case for future accelerators can be made.

In asking me to give one of the closing presentations at this meeting, I imagine the conference organizers may expect me to impart some inspiration as well as information. The inspirational part will explore why I have added a question mark to the title. The informational part will describe future accelerators aimed at understanding electroweak symmetry breaking (TeV scale physics), neutrino physics, and nuclear physics.

## WHY THE ?

The “?” indicates that the existence of future accelerators is far from assured. In fact, the climate is arguably rather hostile. In recent years we seem to have done a poor job of making the case for future machines, at least where particle physics is concerned. Here are two examples of statements from representatives of the administration that show how far the case is from being made:

- Michael Holland of the White House Office of Management and Budget, at Snowmass 2001: *“How much importance do scientists outside your immediate community attach to your fervent quest for the Higgs boson? How else would you expect us to evaluate your priorities? What would you do if the government refused to fund any big accelerator?”* [1]
- Dr. John Marburger, Director of the Office of Science and Technology Policy, at SLAC, October 2002: *“At some point we will simply have to stop building accelerators. [...] we must start thinking about what fundamental physics will be like when it happens. [...] experimental physics at the frontier will no longer be able to produce direct excitations of increasingly massive parts of nature’s spectrum [...] There*

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*are two alternatives. The first is to use the existing accelerators to measure parameters of the standard model with ever-increasing accuracy so as to capture the indirect effects of higher energy features of the theory [...] The second is to turn to the laboratory of the cosmos, as physics did in the cosmic ray era before accelerators became available more than fifty years ago.” [2]*

With all due respect, I have to assert that Dr. Marburger is wrong on both counts. At some point, yes, any given accelerator technology becomes too expensive to pursue. That does not mean that we have to stop building accelerators; it means that we need to develop new accelerator technologies. Secondly, the very richness of the “laboratory of the cosmos” is exactly the reason why we need to keep building accelerators. There is a universe full of weird stuff out there — the more we look, the more weird stuff we find. Do we really think we can understand it all without making these new quanta in the laboratory and studying their properties under controlled conditions?

How might we then start to better make the case? I have a couple of suggestions.

## **1. Emphasize the Unknown**

As Shakespeare had Hamlet point out, “there are more things in Heaven and Earth than are dreamt of in our philosophy.” In justifying and describing the potential of new facilities, I believe that we have tended much too far in the direction of ‘one last piece of the puzzle’ or ‘we know what we’re doing and we know what we’ll find.’ This reinforces the mistaken idea that we are close to ‘the end of science’ and is rather hard to justify given that 95% of the universe is not made of quarks and leptons. In fact, exploring the unknown has a lot more resonance with the public. We have to search for new phenomena in ways that are not constrained by our preconceptions of what may be ‘out there.’ The Tevatron collider experiments have done just that. The DØ collaboration has published [3] a model-independent search for deviations from the standard model in the 1992-95 data. Only two channels had any hint of disagreement and overall the confidence level for the standard model—in this small dataset—was 89%. CDF has also pursued signature-based searches. Such approaches are good science but also good tools for publicity and outreach.

## **2. It’s all about the Cosmos**

The composition of the universe is a powerful unifying theme for particle and nuclear physics. Mass shapes the universe through gravity, the only force that is important over astronomical distances. The masses of stars and planets arise largely through QCD (binding energies of protons and neutrons), but it has long been known that there is substantial invisible (dark) matter and that (from primordial D/He abundances) that this matter is not baryons. Recent measurements of the multipole moments of the cosmic microwave background such as that from WMAP[4] have allowed the dark matter density to be extracted quite precisely. There seems to be about six to seven times more mass ( $27 \pm 4\%$ ) than baryons ( $4.4 \pm 0.4\%$ ). The most likely explanation is that the dark matter is a new kind of

particle: weakly interacting, massive relics from the early universe. There are two complementary experimental approaches that should be pursued: to search for dark matter particles impinging on Earth, and to try to create such particles in our accelerators.

Supersymmetry (SUSY) is an attractive idea theoretically; it can unify couplings, cancel divergences in the Higgs mass, and provides a path to the incorporation of gravity and string theory. It also predicts a particle, the lightest neutralino, which is a good explanation for cosmic dark matter and which could be discovered at the Tevatron or LHC, and studied in detail at a linear collider. In fact the search for dark matter is underway now, in Run II at the Tevatron collider. Neutralinos would be produced in cascade decays of squarks or gluinos and could be detected through their escape from the detector, as missing transverse energy.

The same cosmic microwave background data, together with supernova measurements of the velocity of distant galaxies, suggest that two-thirds of the energy density of the universe is in the form of dark energy—some kind of field that expands along with the universe. Again, there are two complementary approaches to learn more. We should refine our cosmologically-based understanding of the properties of dark energy in bulk (its ‘equation of state’) through new projects such as SNAP. We should also understand what we can do under controlled conditions in the laboratory. Ultimately I am sure we will want to make dark energy quanta in accelerators. For now, we should explore the only other example of a ‘mysterious field that fills the universe,’ namely the Higgs field. The Standard Model Higgs field would produce something like 54 orders of magnitude too much dark energy compared with the cosmological observations, but surely it cannot be totally unrelated.

We know that photons and  $W$  and  $Z$  bosons couple to particles with the same strength—this is electroweak unification. Yet while the whole universe is filled with photons, the  $W$ ’s and  $Z$ ’s only mediate a weak force that occurs inside nuclei in radioactive beta decay. This is because the  $W$  and  $Z$  are massive particles, and the unification is thus broken. This mass (the electroweak symmetry breaking) appears to arise because the universe is filled with an energy field, called the Higgs field, with which the  $W$  and  $Z$  interact (and in fact mix). We want to excite the quanta of this field and measure their properties. The field need not result from a single, elementary scalar boson: there can be more than one particle (as is the case in supersymmetry), or composite particles can play the role of the Higgs (e.g. in technicolor or topcolor models). We do know that electroweak symmetry breaking occurs, so there is something out there coupling to the  $W$  and  $Z$ . Precision electroweak measurements imply that this thing looks very much like a standard model Higgs (though its couplings to fermions are less constrained). We also know that  $WW$  cross sections would violate unitarity at  $\sim 1$  TeV without it, and this is a real process that will be seen at the LHC. For all of these reasons, electroweak symmetry breaking remains a focus of the experimental high energy physics program.

This naturally leads me to the second part of my presentation, where I shall review future accelerator initiatives, starting with those aimed at the electroweak scale.

## FUTURE ACCELERATORS FOR ELECTROWEAK SCALE PHYSICS

The flagship future facility for TeV-scale physics will be the **Large Hadron Collider** (LHC) at CERN. The LHC is a 14 TeV proton-proton collider. It will serve two large general purpose detectors, ATLAS and CMS, together with a heavy-ion and  $B$ -physics program. Underground construction is well advanced and the detectors are making good progress. Accelerator dipole magnet production is the overall pacing item; if all goes well, first beam will be circulated in 2007.

The LHC will be able to discover a standard model Higgs over the entire range of allowed masses (115 GeV – 1 TeV). Beyond discovery, we will need to verify that the observed state actually provides both vector bosons and fermions with their masses. The LHC will be able to start this job by measuring various ratios of Higgs couplings and branching fractions (at the 25% level) by comparing rates in different Higgs production and decay channels.

The more complex Higgs sector in supersymmetric models can also be quite thoroughly explored. Tau decay modes are very important over a large region of parameter space at moderate to large  $\tan\beta$ . At least one Higgs state is visible no matter what; the most problematic region of parameter space is where one light state  $h$  is discoverable, but looks very much like the standard model  $H$ .

To elucidate this case, and of course in general too, one would use the LHC to search for supersymmetry through sparticle production. The mass range covered for squarks and gluinos is huge (up to  $\sim 2.5$  TeV) and a signal to background ratio as high as ten can be achieved even with simple cuts. Exclusive mass reconstruction of SUSY cascade decays has been demonstrated for several benchmark points. New Higgs signals also appear in such decays.

The combination of high energy (14 TeV) and luminosity ( $100 \text{ fb}^{-1}$ ) means that the LHC will have the potential to observe almost any other new physics associated with the TeV scale. Extra dimensions of space-time and/or TeV-scale gravity could have subtle, indirect effects—or direct, spectacular signatures like the production of black holes. The LHC would also be sensitive to compositeness, excited quarks, leptoquarks, technicolor, strong  $WW$  interactions, new gauge bosons, and heavy neutrinos.

In summary, by the year 201 $x$ , if all goes well, we should have observed at least one and maybe several Higgs bosons, and will have tested their properties at the 25% level. We will not always have been able to distinguish a Standard Model from a SUSY Higgs, but we almost always expect to have discovered SUSY in other ways. If we don't see a Higgs, we will have observed some other signal of electroweak symmetry breaking (technicolor, or strong  $WW$  scattering, for example). In addition, we will have learned a great deal more about the physics landscape at the TeV scale: is there supersymmetry? Are there extra dimensions?

There is an international consensus[5] that the highest priority facility to follow the LHC should be an **Electron-positron Linear Collider** (LC). This would collide  $e^+e^-$  beams at a center-of-mass energy between 500 GeV and 1 TeV and deliver a few hundred inverse femtobarns per year. The cost is perhaps \$5–7B and will require an international effort to build; it could be in operation by 2015–20.

The physics of the Linear Collider is no longer about discovery, it is about precision. (In this sense, it plays a similar role to the one that LEP did, after the  $W$  and  $Z$  had been discovered at the SPS collider). The LC program aims to exploit aggressive detector technology such as displaced vertex charm-tagging and energy-flow calorimetry, and also make use of highly polarized beams to reduce backgrounds.

Higgs production at a LC occurs through both  $e^+e^- \rightarrow HZ$  and  $e^+e^- \rightarrow \nu\bar{\nu}H$  processes. The  $HZ$  process can be used to reconstruct the Higgs (actually whatever the  $Z$  recoils against) even if it decays invisibly, and permits the  $g_{HZZ}$  coupling to be determined to a few percent. This in turn provides a simple test of whether the observed particle is actually the only Higgs: namely, does it account for all the mass of the  $Z$ ? For example, in minimal SUSY the  $h$  couples  $g_{hZZ} \sim g_Z M_Z \sin(\beta - \alpha)$  and the  $H$  couples  $g_{HZZ} \sim g_Z M_Z \cos(\beta - \alpha)$  and together they create the full  $M_Z$  that we observe. The  $\nu\bar{\nu}H$  process, with  $H \rightarrow b\bar{b}$ , allows the  $g_{HWW}$  coupling to be extracted with a precision of a few percent.

The couplings of the Higgs to fermions determine whether the Higgs field is indeed responsible for fermion masses as well as for electroweak symmetry breaking. With  $500 \text{ fb}^{-1}$  at  $\sqrt{s} = 500 \text{ GeV}$ , the Yukawa couplings of a 120 GeV Higgs could be determined at the level of  $\Delta g_{Hbb} = 4\%$ ,  $\Delta g_{Hcc} = 7\%$ ,  $\Delta g_{H\tau\tau} = 7\%$ , and  $\Delta g_{H\mu\mu} = 30\%$ . At  $\sqrt{s} = 800 \text{ GeV}$ , it would also be possible to measure  $g_{Htt}$ , through  $t\bar{t}H$  production, at the 10% level. We could thus determine whether the top quark's unexpectedly large mass arises from the Higgs or from some other mechanism.

The quantum numbers of the Higgs itself can be explored. The angular dependence of  $e^+e^- \rightarrow ZH$  and of the  $Z \rightarrow f\bar{f}$  decay products can cleanly separate  $CP$ -even and odd Higgs states ( $H$  and  $A$  in minimal supersymmetry). One would be sensitive to a 3% admixture of  $CP$ -odd  $A$  in the “ $H$ ” signal. This could be a window to  $CP$  violation in the Higgs sector. With sufficient luminosity, the Higgs self-coupling can be probed through  $ZHH$  production (six jets in the final state). The cross section is tiny, about 0.2 fb, so of order 1 ab (1000 fb) is required for a 20-30% measurement of  $g_{HHH}$ . Such a measurement would constrain the Higgs potential and, compared with the expectation from the Higgs mass, would give a self-consistency test for the Higgs.

There are very clean signals for light superpartner production at a LC. For example, chargino pair production occurs through  $s$ -channel annihilation or through  $t$ -channel sneutrino exchange. One can select the mixture of processes by polarizing the electron beam: since a right-handed electron has no coupling to the sneutrino, one suppresses the  $t$ -channel process. In this way the “Wino” and “Higgsino” parts of the chargino can be separated. The Wino coupling to  $e\tilde{\nu}$  can then be compared to the  $W$  coupling to  $e\nu$  — if it is truly supersymmetry, they must be equal. The chargino decays to neutralinos, and at the LC all the masses can be measured. This would enable the expected dark matter abundance and properties to be calculated.

In summary, we are planning a relay race at the electroweak scale. The Tevatron will discover new TeV-scale physics if we are lucky. The LHC is “guaranteed” discovery and will start to measure and constrain. The Linear Collider will measure, measure, measure — and build the physics case for the next accelerator to follow.

## FUTURE ACCELERATORS FOR NEUTRINO PHYSICS

We now have three distinct signals for neutrino oscillation:

- **Solar neutrinos:** missing  $\nu_e$ , as observed by Homestake, GALLEX, SAGE, Kamiokande, SuperK, SNO and KamLAND.
- **Atmospheric neutrinos:** missing  $\nu_\mu$ , as observed by Kamiokande, SuperK and K2K.
- **LSND signal:** a  $\nu_\mu \leftrightarrow \nu_e$  oscillation, as seen by the LSND experiment at Los Alamos.

Parenthetically, we may note (and point out to Dr. Marburger) that while the “laboratory of the solar system” gave us the first two signals, it required terrestrial beams (at KamLAND and K2K) to really understand and have confidence in what we were seeing.

The solar and atmospheric signals form a consistent picture in which three neutrino mass eigenstates each contain admixtures of the flavor states. The  $\nu_1$  and  $\nu_2$  states are separated by  $\Delta m^2 \sim 5 \times 10^{-5}$  eV (the solar oscillation signal) while  $\nu_3$  is split from these two states by  $\Delta m^2 \sim 3 \times 10^{-3}$  eV (the atmospheric oscillation signal). The overall mass scale and ordering in mass is not known. Unlike quarks, there is a lot of mixing; the mass eigenstates do not correspond “mostly” to any single flavor. If the LSND result is confirmed, it would require drastic extensions to this picture: either additional neutrino states, or new physics (*CPT* violation, for example).

There are a significant number of neutrino experiments now running. At Fermilab, miniBooNE is seeking to confirm LSND’s signal for  $\nu_\mu \rightarrow \nu_e$  (and also  $\bar{\nu}_\mu \rightarrow \bar{\nu}_e$ ). In Japan, K2K is pursuing the “atmospheric” oscillation using an accelerator neutrino beam, and KamLAND is exploring the “solar” signal using reactor neutrinos. SNO continues to detect solar neutrinos with flavor selection. It will soon be joined by Borexino, a solar neutrino detector with a very low energy threshold. Two new long-baseline projects are also under construction: MINOS, with a beam from Fermilab to Soudan to measure the atmospheric oscillation and search for  $\nu_\mu \rightarrow \nu_e$ ; and the CERN Neutrinos to Gran Sasso project which will focus on  $\nu_\mu \rightarrow \nu_\tau$  using the OPERA (emulsion) and ICANOE (liquid argon) detectors.

These experiments will tell us whether the LSND result is correct (if yes, confirming that there is new physics). They will better pin down the mass-squared splittings and mixing angles in the solar and atmospheric oscillations. Most importantly, they will give some information on the critical parameter  $\theta_{13}$ , which describes how much electron-neutrino there is in the  $\nu_3$  eigenstate. It is  $\theta_{13}$  which governs the size of possible *CP* violation in the neutrino sector, which is of great interest in understanding the baryon asymmetry of the universe. Currently,  $\theta_{13}$  is known to be less than about 0.10. If it is large enough (where large enough means greater than 0.05 or so), a rich program of next generation experiments opens up. The goal would be to search for electron neutrinos in the “atmospheric” distance/energy regime, to observe matter effects (to resolve the mass ordering) and ultimately *CP* violation. This would require any or all of the following:

- Bigger detectors, 20-100 kt compared with MINOS’s 3 kt fiducial mass;
- Better instrumentation (for example, calorimetry);

- Higher intensity neutrino beams (“superbeams”).

There are a number of concepts that exploit new beams to existing detectors, or new detectors in existing beams, or entirely new projects: Fermilab to Minnesota or Canada, Brookhaven to Homestake or WIPP, and JPARC to Kamioka. One could also access the physics through  $\nu_e$  disappearance using a very high precision reactor experiment.

If  $\theta_{13}$  is small, things become much more challenging. Baselines of thousands of kilometers become optimal, and low rates require new technology for neutrino beams. In this scenario, a muon storage ring neutrino factory may be essential.

No matter what we learn in the next few years, it is clear that we will need major new accelerator and detector facilities for neutrino physics. There is no complete consensus—yet—on just what those facilities should be, but there are lots of good ideas, and lots more data are coming.

## FUTURE ACCELERATORS FOR NUCLEAR PHYSICS

The nuclear physics community has developed a long range plan for the next decade[6], and recently the Facilities Subcommittee of the Nuclear Science Advisory Committee reported on the importance of the science and readiness for construction of new facilities[7]. The following three projects were the highest ranked in the two categories:

- The Rare Isotope Accelerator (RIA)
- A new gamma-ray detector array GRETA (instrumentation for RIA)
- CEBAF energy upgrade (from 6 to 12 GeV).

(RHIC upgrades and an underground detector were also highly ranked but not judged to be immediately ready for construction.)

RIA is a facility to produce rare isotopes. It is driven by a linac (400 MeV/ $u$  U, 900 MeV  $p$ ) which feeds production targets followed by online isotope separation, possible re-acceleration, trapping or isotope recovery. So why do we need such a major (~\$900M) new facility for nuclear physics now? The science case is based on:

- Nuclear structure;
- Astrophysics — the origin of elements heavier than iron. Creation of such elements in supernovae is believed to occur through a complex series of reactions involving unstable, neutron-rich nuclei that could be explored at RIA;
- Low energy tests of standard model symmetries.

As well as the science, RIA would offer “collateral benefits” through the production of medical isotopes and the understanding of processes relevant to nuclear stockpile stewardship.

In preparing this talk I discussed the RIA science case with several of my high energy physics colleagues. Their initial skepticism generally turned to interest once they heard the astrophysics aspects (and, implicitly, how much had been glossed over in the undergraduate astronomy classes they had taken). This observed resonance is a good lesson for

all of us in how to explain the relevance and interest of future facilities to those outside our immediate field.

## CONCLUSIONS

Accelerators are the key to understanding this weird and wonderful universe that we inhabit. Only accelerators can provide the controlled conditions, known particle species, high rates and high energies that we need to make sense of cosmological observations. Recent progress in astroparticle physics and cosmology does not weaken the case for new accelerators, it strengthens it; and there is no shame in exploiting public interest in these discoveries. The major problems are political. As Joe Lykken stated at the Lepton-Photon Symposium at Stanford in 1999, “It is much more likely that we will fail to build new accelerators than that these accelerators will fail to find interesting physics.” It will take a concerted effort to overcome the political obstacles, but if we work together we can do it.

## ACKNOWLEDGMENTS

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