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Michael G. Albrow

Fermi National Accelerator Laboratory
P.O. Box 500, Batavia, Illinois 60510

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THE VERY LARGE HADRON COLLIDER

Michael G. Albrow
FermiLab, USA
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I present some of the current ideas about a Very Large Hadron Collider [1] which could eventually extend the high energy frontier beyond that of the Large Hadron Collider (LHC) or any other machine seriously conceived at this time.

I. INTRODUCTION

After the termination of the Superconducting SuperCollider (SSC) in 1993, the *long term* future for High Energy Physics in the US looked bleak. It would hopefully be possible to become active participants in the European Large Hadron Collider (LHC) at CERN, Geneva, and this has indeed happened. However the LHC, colliding head-on 7 TeV proton beams, would not have the "physics reach" of the SSC with its 20 TeV beams[1]. Without the SSC, would "LHC" come to stand for "Last Hadron Collider"!? In 1994 at the Indiana Accelerator Workshop [2] and again, with growing enthusiasm, at the Snowmass 1996 Workshop [3] this issue was addressed and creative ideas were developed on how it might be possible to make a major step "beyond the LHC". Beyond in energy, thinking in terms of an order of magnitude higher energy, with the implication that we should try to reduce the *cost per TeV* by a similar factor. And beyond in time. The LHC and its two major detectors, each involving about 1500 physicists, is now under construction and scheduled to begin operation in 2005. Whatever the profile of discoveries and improvements to the complex, it seems likely that a period of diminishing returns will arrive after about 15 years. It will get progressively harder to make discoveries there than by making an energy jump to a new machine. For many years now ideas about an extremely high energy proton collider called the *Eloisatron* have been promoted in particular by A. Zichichi [4]. The machine as conceived at Snowmass is called the Very Large Hadron Collider (VLHC) [1] with 50 TeV colliding proton beams, a factor $\times 7$ above the LHC. There is nothing magic about this factor except that it is the same as the factor between the Tevatron (0.9 TeV beams so far, 1.0 TeV in Run II starting in 2000) and the LHC. Still higher energies would give higher physics reach; the issues will be probably be economics and politics rather than physics or technical. The luminosity (rate of interactions per unit cross section) of the VLHC is nominally L $= 10^{34} cm^{-2} s^{-1}$ initially but should be able to become higher if the experiments can take it. The LHC may reach this luminosity; the large extension of the physics reach of the VLHC comes from the beam energy. Many of these points were discussed in a VLHC Physics and Detector Workshop, March 13-15 1997, which attracted 150 participants and resulted in a summary book [5] (copies of transparencies).

How is it possible to take seriously at this time a collider such as the VLHC when the LHC is stretching the resources presently available for high energy physics over more than a decade? Several factors have to be made to work together.

Number one is probably to find big economies, so that the machine will cost less per TeV by a factor of "several" than the LHC. The LHC was constrained to fit in an existing tunnel at CERN, which forced the magnets to be the strongest technically feasible at the time. The VLHC tunnel is not built, so one possibility is to make it very large (hundreds of kilometers in circumference) with relatively weak, cheaper magnets. Tunneling technology is getting cheaper. I shall briefly describe some innovative approaches which could conceivably bring about major cost reductions, and which merit an intense Research and Development program over the next several years.

A second factor is to ensure that the project is really *International*. High energy physicists in Europe collaborated internationally and created CERN in the 1950s in order to share expensive accelerators. The result has been a huge success by any measure, and not least by demonstrating that countries that were at war a decade earlier can work well together, an example that spawned many other international organizations. Even though it was not possible to maintain research accelerators at home (with the exception of Germany), physicists in the member countries found ways of participating fully in the research through frequent travel, periodic stays, computer links[2] and now

[1] When both machines were under consideration, the LHC proponents argued that some of this shortfall could be compensated for by more intense beams. This argument has decreasing validity as $M/\sqrt{s}$ increases. For multi-TeV states the difference becomes enormous.

[2] Leading to the World Wide Web, invented at CERN as a communications aid for high energy physicists and leading who knows where!

videoconferencing. I hope that the high energy physicists of the world will unify themselves to the extent of not trying to get built *two* VLHC-like machines, but collaborate on one, wherever it is. By sharing the costs, this should make it more affordable to all participating nations, as happened among the CERN Member States. One of the reasons for the eventual collapse of the SSC was, I believe, a misguided political desire to make it a national project. Other countries should be involved as early as possible (already now, in the conception and R&D stages).

The question *where?* must also be addressed, although not necessarily right now. The European initiative to build the SPS nearly died because all member states could bid for their own green field sites. Much pain could have been avoided by considering seriously only the obvious existing CERN site, with its proton injector (the PS) and infrastructure. Similarly allowing the SSC to be on a green field site, with the resulting political decision to put it in Texas, made the whole project more expensive and contributed to its downfall. My personal (provocative) conclusion is that there are only three sites in the world that would make logical sense for a VLHC-like machine,namely those that already have proton machines on the TeV scale and a strong infrastructure. These are Fermilab, CERN and DESY (Hamburg). Being at Fermilab, it is self-evident to me that this is where it should be! Being surrounded by a vast plain with excellent tunneling rock and nearly horizontal strata is a practical point. Another lesson from the SSC can be learnt here. When an Illinois-based SSC site was on the table, its position was specified from the beginning which was an unreasonable (political) constraint. For a Fermilab-based VLHC we still have time to choose its orientation (one parameter θ_{FC}, the compass bearing from Fermilab to the center of the ring) and within some range of radius R. A factor in this optimization, perhaps more important than geology, is the minimization of potential difficulties in convincing the general public that this is a good project and should be supported. Perhaps *we* know that this tunnel 100 m. below your home will have *no* negative consequences (I speak from personal experience; the LEP tunnel was built exactly underneath my house while I was there and that was less deep!) but we will have to persuade many people.

The alternation between the USA and Europe for pp ($p\bar{p}$) colliders at the energy frontier (ISR and $Sp\bar{p}S$ at CERN, Tevatron at Fermilab, LHC at CERN, VLHC at Fermilab ...) could be mutually beneficial.

II. A BRIEF HISTORY OF HADRON COLLIDERS

Before 1971 there were no hadron colliders. We had a model that hadrons might be composed of quark constituents, perhaps with masses of a few GeV which could be liberated in sufficiently energetic collisions. The hypothetical charged carriers of the weak interaction, $W^{\pm}$, might also have a mass in the GeV range. The CERN Intersecting Storage Rings (ISR) started in 1971 and free quark and W searches found none. But at the same time deep inelastic ep scattering experiments at the Stanford Linear Accelerator Center (SLAC) found evidence for point-like charged scattering centers within the proton, which we now know to be quarks confined within hadrons. The ISR had an energy (in the collision center of mass) of up to 63 GeV, nearly a factor of 10 over the pre-existing possibilities at the CERN PS or Brookhaven AGS and 5 over the Serphukov 70 GeV accelerator. During the 12 years of the ISR's operation Quantum Chromodynamics (QCD) became the accepted theory of strong interactions at the quark/gluon level. The fourth and fifth quarks, charm and beauty, were discovered in the USA (perhaps they might have been discovered at the ISR with better instrumentation; they were being produced with detectable rates). Hard scattering between quarks and gluons together with their confinement was predicted to lead to high E_T jets which were earnestly sought, and found most convincingly[3] in 1982 the year before the ISR was closed.

In 1981 the CERN $Sp\bar{p}S$ Collider with 10 times the energy of the ISR started ($\sqrt{s}$ = 540 GeV then 630 GeV). High E_T jets showed up easily as collimated bundles of high p_T particles in events with "hard" triggers. Both $c-$ and $b-$quarks were produced prolifically, and most important the W and Z bosons, mediators of the weak interaction, were discovered.

The $Sp\bar{p}S$ Collider was turned off in 1989, in its turn eclipsed by the Fermilab Tevatron which had started in 1985 with a factor about $\times 3$ in energy at $\sqrt{s}$ = 1800 GeV, to be increased to 2000 GeV. The extra energy and luminosity combined to make W and Z production prolific, with about 100,000 W's detected in $W \rightarrow e\nu$ alone. We learnt how to take advantage of the intense b-fluxes to be very competitive with other machines such as LEP, or even ahead as in discovering the B_c meson. Most important, the top quark was finally discovered at around 175 GeV, more than a factor ten heavier than typical guesses after the b was found (remember the sequence for the s,c,b,t onium states 1,3,9,27,...?). The Tevatron still has at least 6 years of *operation* before the LHC will be producing competitive physics, with machines and detectors much upgraded to give a factor of perhaps 200 in sensitivity to new discoveries.

[3] A controversial statement which I am ready to defend!

A Higgs boson up to $M_H \approx 130$ GeV is discoverable, which covers the expected region for the single H° of the Standard Model or for at least the lightest Higgs of the Minimal SUSY Models. The reach into parameter space of Supersymmetry will be very significant, and SUSY might be discovered, but if so it will presumably be just a first glimpse, with handfuls (not thousands) of exciting events.

In 2006 the Large Hadron Collider (LHC) at CERN will be starting to produce physics with $\sqrt{s}$ = 14 TeV, a factor 7 above the Tevatron. Now $t\bar{t}$ production will be *relatively* prolific, although their environment will be harsher (even in single events background from QCD jet production will be increased, but at higher luminosity with a dozen superimposed events it will be that much worse). We are all expecting that LHC will produce a bonanza of new knowledge. Perhaps there will be a whole spectrum of SUSY partners and Higgses to measure everything we can about: masses, decay modes, production rates, spins etc. Perhaps we will be concentrating on studies that today we have no idea about, through some spectacular unexpected discovery.

Whatever will be the situation some 15-20 years from now, the probability that we will have written the final chapter in the story of particle physics is vanishingly small. There will still be a frontier and still questions about what lies beyond, and we will still want to explore and answer those questions. At present the only way we can conceive of producing particles with masses in the 10 TeV range is with a hadron collider of 100 TeV or more, another factor $\times 7$ beyond the LHC. Even for particles as "light" as 1 TeV such a VLHC has a major advantage over the LHC. Although we are very much in the dark about physics beyond the LHC, we can make many quantitative statements, such as calculating production rates and discovery limits for hypothetical particles.

III. PHYSICS AT THE VLHC

A large amount of work has already been done towards preparing the quantitative physics case for a 100 TeV collider. Pre-eminent is the EHLQ "Bible" [6] prepared by Eichten, Hinchliffe, Lane and Quigg in preparation for the SSC. Most of the (250 or so!) graphs include predictions for $\sqrt{s}$= 100 TeV. Secondly, we can justifiably include many of the studies that have been done for the 14 TeV LHC which now has some 3000 physicists involved. The Snowmass '96 VLHC Study Groups' reports are available [3]. In March 1997 the VLHC Physics and Detector Workshop had 150 participants organized into the following working groups: New Strong Dynamics, Supersymmetry, Exotics, Full Rapidity Physics, Precision Measurements of Heavy Objects, Issues related to Multiple Interactions, Particle Identification, Tracking, Calorimetry and Muon Detection. Summaries of these groups were produced [5]. There was also a VLHC Study Group through the summer of 1997 co-ordinated by E.Malamud with four sections: Accelerator Physics (Mishra), Accelerator Systems (Foster), Construction/Installation (Lach) and Physics and Detectors (Denisov and Keller). Material from these studies is available [7] [1]. Here I can only give a few comments; please consult the above references for more details.

A. Differential parton-parton luminosities

A key concept for hadron colliders is that of differential parton luminosities. The protons are each broad-band beams of quarks and gluons (in the naive parton model scaling violations are ignored, while in QCD the beam composition varies with Q^2). Partons collide with c.m. energy squared $\hat{s}$ out of a total pp collision energy squared s; let $\tau = \frac{\hat{s}}{s}$. We can then define a *differential luminosity* $(\tau/\hat{s})(\mathrm{d}\mathcal{L}/\mathrm{d}\tau)$ for each type of parton-parton collision *vs* $\sqrt{\hat{s}}$ for different machines. This is shown for gg collisions in Fig. 1. If we take the example of $\sqrt{\hat{s}} = 6$ TeV we have 1 fb at the LHC(14) and about 100 pb at VLHC(100), a factor 10^5. To go from these numbers to actual rates [6] one has to integrate over an appropriate τ region, multiply by dimensionless factors like $(\alpha_s/\pi)^2$ depending on the specific process, and then multiply by the luminosity $(cm^{-2}.s^{-1})$ of the collider. With a VLHC at $10^{34} cm^{-2}.s^{-1}$ this gives of order 50/year gg interactions at 6 TeV ($\pm$ 5%) but a factor 10^5 fewer, i.e. none, at the LHC.

B. SUSY

Supersymmetry (SUSY) has many beautiful features as a theory beyond today's Standard Model, and if true it will open up a whole new world for exploration and hopefully lead to an understanding of many mysteries, including why a Universe containing *forces* must also contain *matter*. SUSY of course is something of a misnomer because the fundamental symmetry is very badly broken in the real world ... the mass of the electron m_e is very much less than the mass of the selectron $m_{\tilde{e}}$ etc. We expect the masses of most SUSY partners to be in the range 100 GeV (present experimental limits) to 1000 GeV (required by "naturalness" in the theory). This makes for great interest in searching for SUSY at the Tevatron (with significantly better sensitivity in 2000-2006 running than up to now) where it might be discoverable. If so, LHC will certainly have a physics bonanza in studying all the new particles in detail: masses, decays, production rates, classifications etc. An analogy might be B-physics, a sector which could be only glimpsed at the ISR but where the Tevatron is having a "field day". However perhaps our best efforts will not find SUSY at the Tevatron, and then either the LHC will discover it or theorists may have to abandon it, hopefully finding some even more attractive theory in the process. If LHC finds SUSY, we may still have a situation where a higher energy machine such as the VLHC is necessary to cover the new ground properly. Firstly, production rates of such massive particles will be a lot higher (see Fig 1) the factor depending on the mass. Second, some SUSY particles may well be much more massive than others. It is also possible that SUSY breaking occurs by gauge interactions involving so-called "messenger" fields [8], with corresponding particles in the 10's of TeV region, beyond the reach of the LHC (perhaps also beyond the VLHC).

C. $W_L W_L$ Scattering

Although we do not yet have experimental handles on the cross-section for scattering of two longitudinal W's, W_L, LHC will be able to begin to probe this physics. The cross-section should rise with energy, and in the absence of any Higgs-particle(s) would continue to rise eventually violating Unitarity ... unless we have something like "strong electro-weak symmetry breaking (!)" which could give a rich structure akin to resonances in $\sigma_T(WW)$. The analogies with hadron interactions (e.g. $\pi\pi$-scattering) could be realized, including multi-W production interactions, for which the phase space at VLHC will be very much greater than at the LHC. Consider $W_L W_L$-scattering at $\sqrt{\hat{s}} = 4$ TeV; the ratio of differential luminosity is a factor 10^3 with rates of about 100/year at VLHC compared to 0.1/year at LHC. While mentioning multiple-W production, there is a fascinating possibility that QCD- or ElectroWeak *instantons* [9] could be made manifest at 10's of TeV. Instantons are actually part of the Standard Model, discussed by t'Hooft and others, and result from tunneling of the vacuum between different states. A consequence could be reactions like $q+q \rightarrow 7\bar{q}+3\bar{l}$. Cross sections are expected to be miniscule but might [10] be enhanced by multi-gauge boson emission (many Higgses and W/Z's). This would be very exciting and deep physics, but I think one would have to be very optimistic to expect to see such events.

D. Exotics

Of course, beyond the Standard Model we have no idea what we will see! Theorists and experimenters have plenty of imagination, and it is the essence of scientific progress that our theories *must* be tested against reality i.e. experiment. For a hypothetical particle with guessed properties we can calculate the potential of future machines, and the following gives a few examples. Scalar leptoquarks could be discovered or ruled out with mass up to about 300 GeV at the Tevatron in Run II, 1.5 TeV at LHC and about 7 TeV at VLHC. Heavier intermediate vector bosons, W' and Z', are now ruled out by CDF and DØ to about 700 GeV but VLHC could see them up to about 25 TeV. Excited quarks q^* might be found up to 30 TeV, and the compositeness scale Λ_c probed to about 100 TeV (10^{-19} cm). "The search potential for these new states is truly enormous [5]".

I would like to quote a conclusion from the March 1997 VLHC Workshop [5]:

"The VLHC should be designed to probe the TeV scale in detail, since the physics associated with electroweak symmetry breaking will be there. If the LHC has discovered this physics, the VLHC will be able to explore it in depth. If this physics lies just beyond the reach of the LHC, we will nonetheless know it must exist, and the VLHC will catch it. ... While a compelling case for studying the TeV scale exists, far less is known about what might lie at higher scales. A challenge for theorists is to identify the possibilities for 10 TeV-scale physics."

E. Diffractive Physics

Especially because this talk was given at the LISHEP98 Workshop on Diffractive Physics it is appropriate to say a few words of speculation about diffractive physics at $\sqrt{s} = 100$ TeV. By that time will we have a satisfactory understanding of that field? Will the pomeron still be part of our vocabulary? How could one do an experiment to measure σ_T? What about ρ, the ratio of real to imaginary parts of the forward elastic scattering amplitude? There is a feature of the low-field (2T) VLHC magnet design that might make such experiments possible, namely a room temperature vacuum pipe with an exposed side. One can imagine insertions of very small strip or pixel detectors far downstream, after hundreds of meters of magnet. None of this has been thought out yet, but an intersection region with perhaps low luminosity but very long straight sections is possible (unlike the constrained situation of the LHC in the existing LEP tunnel).

Whether or not we will still be using pomeron language, the reaction we now call DPE or Double Pomeron Exchange should be very interesting. With both incoming protons losing less than 5% of their momentum, we can have a central system (globally neutral, vacuum quantum numbers) up to mass $\approx$ 5 TeV! With larger rapidity gaps we would have purer DPE and could still have masses of order 1 TeV. When one realizes that this is comfortably above the threshold for $t\bar{t}$ production, $W/Z/H$ production, probably also $\tilde{g}\tilde{g}$ and other SUSY pair production it should be clear that this could be a rich hunting ground for new physics. Even in a model where hard DPE is essentially $gg \rightarrow X$ followed by a soft color neutralization, these will be clean events, to be studied at low luminosity (single interactions) in a 4π detector. Gluinos and squarks will couple to pomerons like gluons and quarks, presumably. DPE $\rightarrow H$ may only happen via heavy loops (e.g. top), but who knows whether something surprising may happen (after all the pomeron and the Higgs both have vacuum quantum numbers!)

IV. DETECTORS

An important realization is that even at a luminosity of $10^{34}cm^{-2}s^{-1}$, which is the design luminosity of the LHC, the VLHC has much greater physics reach. Therefore we can take as a starting point the vast amount of R & D which has been done for SSC and LHC detectors. Occupancy and radiation levels grow roughly logarithmically with $\sqrt{s}$. A reasonable extrapolation of progress, provided that further detector R & D is funded at the appropriate time (probably continuing the LHC work), leads one to expect that appropriate detectors can be built at least for $10^{34}cm^{-2}s^{-1}$. We could then expect that later developments may push both the machine and the detectors up to about $10^{35}cm^{-2}s^{-1}$; some accelerator physicists already consider this possible.

Probably the VLHC would have one ... perhaps two ... very large detectors in the CDF-DØ or ATLAS-CMS tradition, measuring jets, missing E_T, electrons and muons, photons, and tagged b-jets. Note that a 5 TeV B-meson often travels tens of cm before decaying so it can leave a track, not just a displaced vertex! Microvertex detectors will surely be standard (using diamond pixels?) for charm and τ as well as b-tagging. One will want to be able to measure 10 TeV muons well; this would today be expensive, and is therefore an area where development could pay off. New ways of thinking for calorimetry (probably aimed, for electromagnetic calorimetry, at high precision and granularity without a commensurate cost increase) are to be encouraged. A promising avenue for hadron calorimetry, because it is cheap and very radiation hard, may be stacks of high pressure gas tube ionization "pipes" [11]. However in this environment, at $10^{34}cm^{-2}s^{-1}$, W-bosons of generic P_T and jets of low E_T (below about 50 GeV?) will be difficult to detect/measure.

A case was made for the above "Generic High E_T detector" to be complemented by another detector covering the full ± 12 units of rapidity (compared to about $\pm$ 4 or 5 for the above detector). I know that at this Workshop (on Diffractive Physics) this idea will receive strong support. The central part of this detector could look like an upgraded (with newer technology) CDF or DØ detector, together with forward tracking, magnets and calorimetry for hundreds of meters downstream. The luminosity would be lower to have typically one event per crossing; still $\approx 5.10^{32}cm^{-2}s^{-1}$. This would enable study of processes with rapidity gaps from color singlet exchange; not only pomeron but also W-exchange (e.g. $WW \rightarrow X$). It would also allow a much cleaner study of processes such as top-quark production and multi-W production.

As far as detector techniques are concerned, there do not seem to be show stoppers if tracking and calorimetry are developed to the stage of being able to operate at $\approx 10^{34}cm^{-2}s^{-1}$ at LHC. In read-out electronics and computing we can expect major advances to continue between now and then. Tracking will probably rely on development of fast and radiation hard (diamond?) strip and pixel detectors, and for outer muon tracking cheaper ways of covering very large areas with precision measurements. Hadron calorimeters will also be larger than at LHC so novel ways of making them cheaper (per m^3) should be developed. For electromagnetic calorimetry, precision with high granularity (together with speed and radiation hardness) will be at a premium. Basically we need R & D for detectors in the LHC environment to continue in the era when the "first round" of LHC experiments are running, both for LHC upgrades and for VLHC.

V. THE ACCELERATOR

The idea that an accelerator with 5 - 10 times the energy of the LHC[4] is thinkable is largely based on the idea that we may be able to find ways to reduce the cost per TeV by a similar factor. This will certainly require innovative ideas. Two general directions are being pursued: a high field option and a low field option. The former is to try to push magnets, perhaps of the SSC/LHC type, to higher fields; maybe 10-12 T would be optimum[5]. There are several challenges: critical currents in the superconductor, magnetic forces acting on the coils, high cooling capacity needed, strength of steel (if steel) collars, etc. It is hard for me to imagine a cost reduction factor of "several" with this approach. An alternative being actively pursued by Bill Foster and colleagues at Fermilab is a low field (2T) magnet. Bob Wilson, Fermilab's Founding Director, was thinking along these lines at a Snowmass 1982 meeting [12] when he said:

"Whether the next large proton accelerator (20 TeV?) is built on a national basis or as an international effort, to be affordable, innovations in construction must be made. The design of a superferric magnet ring buried in a pipe in the ground is explored here to see what reductions in cost might result."

"...superferric magnets (an old idea) have the advantage of simplicity, of being more sparing in the use of superconductor, less sensitive to the position of the superconductor, easier to construct, and perhaps more reliable to use."

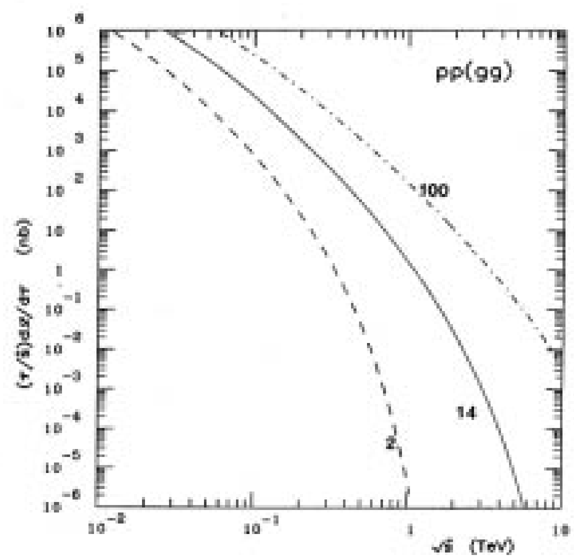

FIG. 1. Differential luminosity (proportional to interaction rate) for gg-collisions vs $\sqrt{\hat{s}}$ for the Tevatron (2 TeV), LHC (14 TeV) and VLHC (100 TeV) [6].

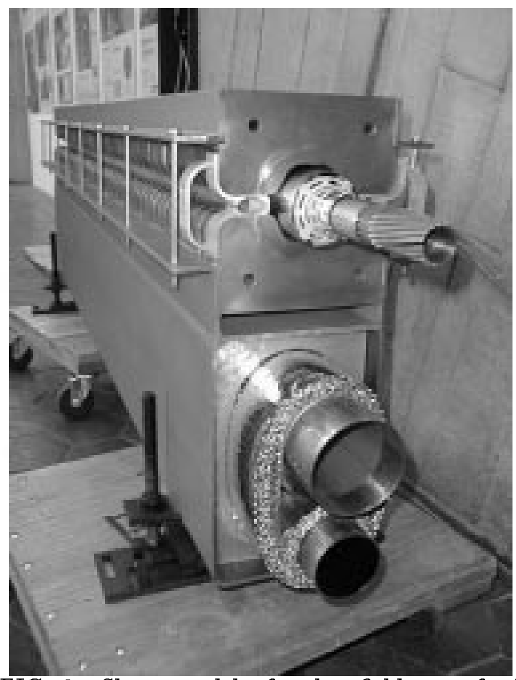

FIG. 2. Short model of a low-field superferric VLHC magnet. The central top pipe carries liquid He and is wound with spiralling superconductor and wrapped in insulation. One of the two elliptical beam pipes is visible at the left, between the warm iron poles. Underneath are return lines for the current and Helium.

The basic idea is a single superconducting (75 kA) transmission line, with superconducting cable helically wrapped on invar cryopipe. This is wrapped in insulation; the field which circulates around the conductor is concentrated in gaps to the left and right by warm iron poles above and below, as shown in Fig. 2. The vacuum pipes in the gaps carry counter-circulating proton beams. There are many nice features, of which perhaps the over-riding one is simplicity compared to the SSC/LHC cos θ coil magnets. There are no magnetic forces on the superconductor, which

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[4] $\sqrt{s} = 100$ TeV is an option, selected now to have a focus, but perhaps it could be higher.

[5] The Tevatron magnets are 4.3 T and the LHC are to be 8.6 T.

sees less field than the beams by a factor of at least 2. The cold mass is small which allows a rapid cool-down. These magnets might be produceable in long (500 m?) sections to minimize the cost of end effects, with field correctors under stepping motor control. Several years of R & D are needed in developing such magnets and the accelerator physics that goes with them before a real costed VLHC design could be produced.

There are rapid developments now in "warm" (liquid nitrogen cooled) superconductors, driven by the power transmission industry, which can invest far more into this R & D than we can. We should keep a close eye on this progress.

One aspect of the low-field concept is a very large tunnel, perhaps about 550 km in circumference! This would be bored and there have been very rapid developments in this field during the last 5 years. Today the cheapest tunnels per unit length have a diameter of about 10-15 feet; the cost depends on the geology. A goal for about a decade from now would be: optimal diameter 5-10 feet (very suitable) and cost about $300 per foot[6]. It is likely that robots will perform 90% of the operations. Note that a very small "pipe" seems to be *not* the cheapest solution, and a 10' tunnel leaves room for other machines, a strategy that worked well both at Fermilab and CERN. (In fact one advantage of the low-field-big-tunnel option is that it prepares the way for a higher field machine in the really long term.) Even though a 550 km tunnel seems spectacular, it will not be the most expensive item. Fermilab is keeping a close eye on developments, being a member of the North American Society for Trenchless Technology and the American Underground Construction Association. Among the many issues under study are ground motions and survey/alignment, access times (how many surface shafts?), property issues, beam abort issues and, as I mentioned in the introduction, minimization of environmental impact and optimization of our public relations.

VI. OTHER USES FOR A BIG RING

If we were to build a 550 km circumference tunnel for the pp VLHC, an e^+e^- collider with $\sqrt{s} \approx 600$ GeV might be possible [13], well above the $t\bar{t}$ threshold around 350 GeV where it could have a luminosity around $10^{33} cm^{-2} s^{-1}$. (Note that the dipoles for this machine would only have about 120 Gauss!) The electron ring would allow ep collisions [14] at 300 GeV + 50 TeV compared with about 30 GeV + 0.9 TeV for HERA, a factor of 25 in $\sqrt{s}$. Nobody (as far as I know) has put much thought into fixed target physics from 50 TeV proton beams but perhaps the neutrino sector would have exciting possibilities. Other fixed target thoughts: (a) $K_L^o \to K_S^o$ regeneration at the highest possible energies should go to zero; this tests the Pomeranchuk theorem and the possible presence of Odderons (b) testing Lorentz Invariance: are K^o lifetimes, mass differences, CP-violating parameters etc. independent of the γ-factor in this very sensitive system? (c) studying high mass hard diffraction (pomeron structure) with pion, kaon and many other beams. Just as LHC plans some operation with colliding beams of gold nuclei to search for QCD phase transitions to quark-gluon plasma (*quagma*) so could VLHC (note: this physics has to do with the properties of the vacuum, chiral symmetry breaking/restoration, astrophysics both in the early Universe and in exotic "stars" and a field of QCD which might become very exciting). Realizing all these possibilities led Gerard t'Hooft [15] to suggest the name "*Omnitron*" for the complex of possible machines. I like that.

VII. ACKNOWLEDGEMENTS

I thank Ernie Malamud, coordinator of the VLHC study group at Fermilab for discussions and reading this paper, Bill Foster for his inspirational ideas, and Dmitri Denisov for lending me many transparencies for this talk. I thank the U.S. Department of Energy for funding my attendance at this workshop.

[1] The web page http://www-ap.fnal.gov/VLHC is a good place to start browsing, and has links to many articles.
[2] Indiana Accelerator Workshop 1994: Proceedings of the Workshop on Future Hadron Facilities in the U.S., Fermilab-TH-1907 (1994).

[6] i.e. about $0.6B for a 550 km circumference.

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[7] VLHC "Tourquoise Book", January 1998, Compiled by S. Mishra; copies available from E. Malamud at Fermilab. The articles can be found on the web [1].
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