VLHC QUESTIONS AND ANSWERS
Presented by Gerry Dugan at the Fermilab Annual Users Meeting July 7, 1999

HIGH-FIELD OPTION

A.

1) The superconducting magnets are the most technical challenging and most costly component of the any hadron collider. It is, therefore, sensible to begin development of these magnets in the early part of the planning stage of such a machine. Nevertheless the requirements of the magnets must be developed within the framework of the requirements of the machine.

a) Does there exist a concept of a machine that uses high field (low field) magnets?

Machine concepts based on high and low field magnets were summarized at Snowmass '96[1] The concepts still need much work, but the basic structure of the machines, with most of the relevant parameters defined, are given there.

b) What requirements are demanded of the magnets in that concept?

Regarding the required field, the low and high field approaches are distinguished more by the magnet technologies than by the exact field. The low field approach is superferric (iron-dominated), and the highest field achievable with this approach would be chosen. Having chosen a beam energy of 50 TeV as a working parameter, then, in order to enter the regime of significant synchrotron radiation damping ($B > 10T$), the high field approach must use Nb_3Sn technology. In this case, the magnetic field that gives the lowest cost machine (which is not necessarily the highest field) would be the best choice.

General field quality requirements are known from SSC and LHC work. The specific requirements for VLHC magnets have yet to be precisely defined; in particular, the impact of synchrotron radiation damping (if any) needs to be understood quantitatively. Similarly, although the general range of the required aperture is known, work remains to be done to define it precisely.

c) The cost and quality of a magnet is very sensitive to the aperture. The change of the SSC dipole aperture from 4 to 5 cm late in the development program caused a lot damage (probably undeserved) to the SSC project. It is important in evaluating magnet technologies to be able to confidently specify the aperture and field quality as early in the magnet development program as possible. Are we confident that orbit tracking codes are reliable enough to give us this answer now?

Orbit tracking codes developed for the SSC and used for the LHC are believed to be quite reliable and should be able to provide guidance on these questions.

2) A lot of cost data was developed for the accelerator components and civil construction during the SSC days and for the LHC.

a) Has a cost model been developed that can illuminate the issue of preferred field strength for the VLHC?

The cost data from the SSC and LHC is quite specific to the technologies used for these machines. If the same magnet and tunneling technologies are used for the VLHC, the cost would be prohibitively high.

This is the major reason why R&D is required for the VLHC: to develop new technologies which deliver the required performance at much reduced cost. Until this R&D has reached a more advanced stage, reliable cost models will not be possible.

b) What are the issues other than cost that differentiate low and high field (or intermediate field) design approaches to the VLHC?

The major differences between the low and high field approaches are summarized in the table below:

	Low Field	High Field
Radiation damping	None	1-2 hr damping time
Impedance	High (resistive wall of warm bore beam tube)	Low(impedance/length typical of other machines)
Susceptibility to emittance growth	High (because of very low revolution and synchrotron frequencies)	Low(higher revolution and synchrotron frequencies, plus radiation damping)
Persistent currents at injection	None	Signifcant (with Nb_3Sn)
Beam Stored energy	15x LHC	1.5x LHC
Beam Aspect ratio	Round	Flat
Synchrotron radiation power/ring	48 kW (0.08 W/m) into warm bore	190 kW (1.6 W/m) into cold beam screen
Cryogenic system	Modest	Substantial
Wall-plug cryo power	<40 MW	72 MW

c) What are the features of an R&D program that will lead to the optimum field?

R&D must be pursued for both the low field and high field magnets, to the point of developing and testing prototypes that can be used to make reliable cost estimates for production items. The optimum field is one that minimizes the total project cost, so R&D must also be done on the major cost drivers that scale like the tunnel length, such as civil construction costs.

3) A large hadron collider will certainly follow a linear collider and possibly a muon collider. That means that the final design for the machine technologies probably do not need to be made for 15 to 20 years. Historically it has taken 10 years to develop the magnets for superconducting machines.

I would take issue with the first sentence above. A VLHC will certainly follow the LHC, but it is not obvious that it should follow a linear collider or a muon collider. Thus, the final machine design should be ready on the time scale of 2010, which is about 10 years, not 15 to 20.

a) What is a sensible development program that would optimize performance and cost of a magnet design during that period?

One of the key elements for the high field magnet is the conductor; the possible options are Nb_3Sn, Nb_3Al, or high temperature superconductor (HTS). Conductor development (in industry) must be an important part of any magnet development program. Nb_3Sn and Nb_3Al development, formerly supported by the fusion program, needs to be continued with HEP support. HTS development is already supported for applications in the electric power industry, but HTS applications for HEP magnets should be strongly encouraged.

High field magnet design and prototyping, using the best conductor available from industry, must be vigorously pursued, to the point of developing and testing prototypes that can be used to make reliable cost estimates for production items.

c) Can any technical or cost breakthroughs be expected during that time that will have a material effect on the overall cost of the VLHC?

The area of greatest potential for a breakthrough is in the use of HTS materials. The most promising material, YBCO-123, has been shown to have very high current density in the range 4-20 ^{o} K, and critical fields in excess of 30 T. To date, it has only been made in a research environment, in short lengths, and with a limited engineering current density. If its potential can be realized in large-scale production at a low cost, the impact on VLHC would be enormous.

Additional areas for possible breakthroughs are in vacuum, cooling, and cryogenics. In the vacuum area, current developments with NEG coatings of TiZrV have the potential for a mini-revolution in vacuum system design. Such a coating on the inside of the beam tube, which effectively suppresses photo- and electro-desorbtion of gas, may allow a much simpler vacuum system to be realized, both for the low-field and high-field VLHC. Although its feasibility has yet to be demonstrated, the high bandwidth provided by optical stochastic cooling offers the possibility of short cooling times, reduced emittance, and enhanced specific luminosity. The application of single-phase cryogenics, which utilizes the sensible heat of the cryofluid for cooling, may dramatically simplify the large cryogenic systems needed for the VLHC.

4) Given the experience with the SSC and the LHC, what role will industry be expected in the development of the superconducting magnets?

A good model of the proper role of industry in the production of superconducting magnets is provided by the RHIC experience. The conductor was supplied from industry. The magnet design, development and prototyping was done by BNL. The magnets were given as a build-to-print design from BNL to Northrup-Grumman. There was a tooling development / technology transfer pre-production phase, during which Northrup-Grumman learned from BNL and incorporated design changes to enhance manufacturability. The main production phase was completed with very good results, as a fixed-price contract on schedule.

5) It has long been said that the next hadron collider must be an international or at least a regional undertaking. Does the magnet development program anticipate this by involving and taking advantage of the expertise of the world's centers for developing superconducting accelerator magnets?

The development program currently includes collaborators from BNL, Fermilab, LBL, Texas A&M, and KEK. This covers all the major centers for expertise in superconducting accelerator magnets in the US and Japan. European centers of expertise such as Saclay, DESY, CERN or Protvino are not yet involved, but efforts will be made to do so.

B.

Here are a few questions. I assume this machine is designed with synchrotron radiation as a major concern; i.e. 10-12T dipoles, 10-30 MeV/turn synchrotron rad energy loss.

Actually, the synchrotron radiation energy loss per turn is about 3.7 MeV for a 12.5 T dipole.

1. General properties of the lattice? Dipole field? Phase advance per cell? Lattice quad gradient? Aperture of the magnets? Reference radius? Maximum Luminosity? Synchrotron Rad heat load? Method for dealing with synchrotron rad? Refrigeration load? Refrig cost for removal of snychrotron radiation?

A preliminary cut at specifying much of this information is provided in the Snowmass '96 proceedings[1]. The method for dealing with synchrotron radiation (a beam screen) was discussed at Snowmass[2]. The refrigeration cost and loads were also studied[3] there.

2. Interaction region design? Crossing angle and ensuing arguments? Special problems such as high grad quads and interaction rates?

These subjects were studied at Snowmass[4,5,6], and at the VLHC accelerator technology workshop this year. See also a paper on flat beams[7]

3. Magnet technology. Dipole design? Fraction of field from Fe? Force on conductors? Insulation and (Al) alloy to support SC that can handle the high field forces? Quench protection scheme? SC to be used or investigated? R&D issues?

In order to achieve the required field (>10 T), the dipoles will need to use conductors having high critical fields, such as the A-15 compounds like Nb_3Sn and Nb_3Al, or high-temperature superconductors. There are groups studying the design of such dipoles at BNL[8], Fermilab[9], Texas A&M[10], and LBL[11]. A new development in these studies is the use of the common-coil block-conductor design. This coil geometry is natural for a two-in-one magnet. The large coil bending radius makes it easier to use the brittle A-15 compounds. The block conductor design will allow the coils to be placed into a much stronger matrix, providing adequate support for the high forces arising in high-field magnets.

4. What are the major construction issues and techniques likely to be? What are the major cost drivers?

The major construction issues are likely to be production of the collider dipoles, tunneling for the collider ring, and building the collider detectors. These are also the major cost drivers.

C.

1) The key goals, in my mind, are to first demonstrate technical feasibility and then to find a way to reduce costs such that a 100 TeV VLHC, for instance, could be quickly sold to a group of governments, if and when nature presents a strong argument for such an instrument. Do you have an idea of the minimum that it would take to demonstrate the feasibility of this concept ? (money, time, manpower, facilities...) Do you percieve the existence of adequate support in the field for such an R&D program ? Can feasibility be demonstrated without building a small, complete accelerator ?

Given the existence of a high-field magnet, the technical feasibility of the machine is really not an issue: such a machine can certainly be built and operated reliably. The accelerator technology on which superconducting hadron colliders is based is relatively mature, having been explored in the Tevatron, HERA, RHIC, and (yet to come) LHC. Scale-up to 100 TeV does present some interesting challenges and opportunities, but should be relatively straightforward.

The high-field magnet, however, represents a major step beyond the present state-of-the-art, particularly in that the cost must be brought down substantially, even from that of today's intermediate-field magnets. The development of this magnet is the major goal of the R&D program, and its feasibility will certainly need to be demonstrated.

The resources required for magnet development are estimated at about 32M$ over the next 3 years, followed by perhaps another $40M over the subsequent 3-4 years. Given the broad range of applications of high field magnets in accelerators other than the VLHC, the field will benefit generally from such development work.

2) It seems to me that it is often assumed that the NLC, or equivalent, will be built next after LHC. Do you believe that we should already now be planning to build the NLC ? Is there any danger in this line of thinking ?

We don't think that the next machine after the LHC will necessarily be the NLC. However, even if this does come to pass, a VLHC has a very high likelihood of eventually being a desirable machine. Superconducting magnet development work has historically required very long lead times to come to fruition, and the problems for the VLHC are more challenging than those in the past. It is not too early to address these issues vigorously.

D.

1) This is a very long time scale with interesting challenges in the accelerator and magnet development, but the new physics potential only comes after ~20 years. Can intermediate steps be made which have some discovery potential?

The 20 year time scale for new physics is approximately correct-10 years for development, and clarification of the physics motivation (with LHC results), and 10 years to build it. As far as possible intermediate steps with physics potential, the only two that come to mind are physics with the injector, and a p-pbar option with one collider ring. In the latter case, the luminosity would be very low, because of the severe problems with providing enough antiprotons for such a large ring. In the former case, the injector energy (3 TeV) is well below the LHC beam energy, and it's hard to see how this would be competitive. (However, there are some interesting physics "niches" that deserve more discussion, such as fixed target tau-neutrino and hyperon beams, an e-p collider, forward physics, etc.) One approach would be to raise the injector energy to 10 TeV; this would make the collider easier, and could provide a (barely) competitive physics program with the injector (operating as a collider) while the main collider was being built. I suspect that this would only make sense if the LHC results showed that the interesting mass scales were very high, and that the LHC 's 7 TeV beam energy was just at the threshold for major discoveries.

2) Could you discuss a timeline for R&D and decisions point - include physics potential, input from LHC, run 2.

The principal decision point comes when LHC results are available, which will indicate the mass scale of new physics. This is expected to be about 2009-2010. (Note that information would be available earlier if the Tevatron, in Run II, discovers the Higgs or low-mass SUSY). By this time, the R&D program should have developed a cost-effective, high field magnet, and there should be a complete, validated conceptual design and cost estimate for the machine. Given that it takes probably 3 years for a complete conceptual design, the magnet development (including the fabrication and test of a substantial number of prototypes) must be completed by about 2006. This is a challenging task for the high-field magnet; the low-field magnet is in a more advanced stage of development, and could be completed by 2004. In parallel with the high-field magnet development in the period 1999-2006, work must continue on reducing the cost of the rest of the machine as much as possible, through new ideas and "value engineering" of major cost drivers such as tunneling, cryogenics, vacuum, etc.

3)What energy could you get to if you

a) picked a ring diameter based on your best guess at the most powerful magnets (Ecm = 100 GeV?)
b) filled it with magnets that could be built now (or in a few years) Ecm = ???

A guess at the most powerful magnets would be 15 T. (This is at the upper edge of magnets that are currently being studied). For a 100 TeV (CM) ring, the required circumference is (arcs only) 71 km. If we fill a 71 km ring with magnets we can now build (8 T LHC dipoles), we get 53 TeV (CM). This would be a very expensive proposition, as the LHC dipoles are not cheap.

E.

1) What is the specification for the good field region - (a) at injection, (b) at full field?

This question is still open. It is related to the lattice design for the machine. The relation between the cell length L, the good field radius r_{GF}, the injection energy γ_I, and the normalized emittance (injected) ε_n, is

$$L \propto \frac{\gamma_I}{\varepsilon_n} r_{GF}^2$$

To simplify the lattice, we'd like to have long half-cells, but that requires a larger good-field radius, and hence more expensive magnets. Evaluating the trade-off here requires knowing how the magnet costs and performance scale with length and aperture; also, what good field radius is required by the beam dynamics, with the consequences of radiation damping (at full energy) included. These are all areas for future study; we don't have good answers at present.

At full field, the aperture requirements are probably dominated by orbit control allowances, rather than the beam emittance. In collision, typically the field quality in the low-β quads, rather than the arc magnets, is the dominant consideration.

2) Are the arc quads on the same bus as the dipoles? If so, what is the tuning range afforded by trim quad circuits? If not, are the arc quads on one or two circuits. (If one, what is the tuning range...) Are there quench protection issues associated with quad buses, if they are separate from the dipole? If they are common with the dipole, how does the specification on the transfer functions of the magnets (so that they track within the tuning range of the trim quads) affect the magnet designs?

The design is not developed enough at present to be able to answer this question. For example, there is a possibility that the arc magnets may be combined function devices, in which case these issues do not arise. For separated function designs such as the SSC and LHC, similar issues were resolved, and it is expected that similar solutions would be available for the VLHC.

3) Is chromaticity control (change in injection level persistent currents) better or worse in the high-field machine? How bad is it?

Presumably this questions asks for a comparison between persistent current effects in the low and high field machines. Persistent currents will be much more serious in the high field machine; in fact, they will be essentially absent in the low field machine, with its iron-dominated magnet. In the high field machine, persistent current problems may be even more serious than in the Tevatron and LHC, because the magnets will be made using Nb_3Sn. To date, it has been difficult to make this conductor with both high J_c and small filament diameter. The larger filaments result in more persistent current sextupole: this is another motivation for raising the injection energy. It is hoped that with more effort in conductor development, this problem can be solved.

If this problem is not solved, the dynamic range of the collider would have to be reduced, to perhaps 10 or less; this would require an injector of energy 5-10 TeV.

F.

1. From recent discussions about future accelerator R&D with people in the administration (in particular, OMB and OSTP), it is clear that the people holding the purse strings would like to see:

(i) a unified national voice about the direction we would like to take for the next machine

(ii) SIGNIFICANT international participation in shared R&D efforts from the beginning.(NLC R&D funding is suffering from the perceived lack of attention to these two issues, among other things.)

How can (i) be addressed in a scenario where the HEP community would like to push THREE accelerator options (linear collider, muon collider, large hadron machine) farther along the R&D path for the next several years? To what extent is (ii) being addressed, given Europe's focus on getting the LHC underway and Germany's focus on a linear collider?

(i). Developing a unified national voice requires information about the options, in order to build an informed consensus. The R&D programs are aimed at providing the information from the technology side; the information on the physics side will come from LEP, Tevatron Run II, and LHC.

(ii). To date, there has not been much involvement in VLHC by the Europeans. This is staring to change: Eberhard Keil has done work on the high-field VLHC, and CERN's long-range planning has begun to consider machines after the LHC. More work needs to be done to encourage both DESY and CERN participation in VLHC R&D.

2. I think it is safe to say that U.S. HEP will see flat-flat funding for the overall program for the next several years. If we are to maintain a robust, growing program of advanced accelerator R&D in the near term, what parts of the present U.S. HEP program would you suggest cutting/winding-down in order to make room for more work on the future?

Even in the flat-flat funding scenario, resources become available as planned construction projects are completed. Several projects (e.g., NUMI, US-LHC) will come to completion in the next several years; the decision will then have to be made whether to start new short-term projects or to invest in the longer-term future with advanced accelerator R&D. These decisions are not easy, but it is not necessarily true that one must cut an existing operating program to provide for the future, even with flat-flat funding.

3. In contrast to the linear collider and the muon collider, VLHC R&D may suffer most from the SSC memory, which is still very real in the minds of people in Congress and the administration. How do we address this?

The problem with the SSC, from the point of view of the Congress, was primarily its (increasing) cost, not what kind of accelerator it was. This problem is the main focus of the VLHC R&D: cost reduction. Any project which tries to run the gauntlet of the Congress must be seen at the outset, and through its life, as being a real bargain, well-managed and manifestly cost-effective. In this sense, the VLHC may actually have an advantage over other projects. The SSC cost estimate, at termination, was about 11B$. If VLHC can come in much lower than this, and with 2.5 times the energy, it may be (correctly) perceived as a bargain.

4. How do we stimulate greater interest in university HEP groups to engage actively in VLHC R&D, either from the machine side or the physics-motivation and detector side? Are there particular technical accelerator issues which can be undertaken by university groups, perhaps in coordination with university engineering and computer science departments? "Cross-disciplinary" and "Interdisciplinary" are big buzzwords in the research universities these days, and the VLHC should capitalize on this, even if it is a short-term fad that university administrators like to push.

We need to encourage participation by university scientists in VLHC workshops, and provide opportunities for temporary positions for on-leave university scientists at national laboratories working on VLHC. There are a large number of issues that need to be studied, some involving calculations, but others involving design and prototyping of hardware. Many of these could be undertaken as multi-disciplinary tasks at universities, as suggested in the question.

5. If based at Fermilab, the VLHC, especially the low-field option, will have to go significantly off-site for the first time. What can be done now to enlist support/educate the state government and local communities to embrace the idea of an accelerator under their farms, schools, and businesses?

Strengthening relations with the community in the present provides the best preparation for an expanded presence in the future. Fermilab has generally had a very good reputation as a neighbor in the community. This has been enhanced even further by the recent “Open House” activities, in which the community can visit the Laboratory, see what going one, and become convinced that the scientists are not all crazy. My guess is that this is the best public relations action that can be taken to prepare for the VLHC. In addition to improving community relations, it also can serve an educational function. If the local population is supportive of the lab, the chances of getting local support for upgrades and expansion is much improved; and if the local community is supportive, the politicians (at least at the state level) will very likely follow.

6. What about the geology -- are there any hurdles or brick walls to overcome?

The local geology around Fermilab was studied in detail for the SSC; a review[12] was presented at Snowmass. The Galena-Platteville dolomite limestone bedrock, lying 120-220 m below the surface at Fermilab, is excellent material for TBM-style tunneling. TBM advance rates as high as 60 m per day may be achieved in this material; groundwater conditions are also very good in this rock. Relatively low tunneling costs may be expected with this material. The Galena-Platteville layer rises as it goes west, reaching the surface about 60 km west of Fermilab. A high-field machine could fit into this region to the west of Fermilab. The direction from Fermilab for a large low-field ring needs geology data over a wide geographic region and is not yet determined.

7. I'm interested in the latest in tunneling technology. What's up? What's foreseen?

A good review of the subject is provided in a Snowmass article[13]. Microtunneling and directional drilling do not seem to be feasible choices for the VLHC tunnel; the best approach is thought to utilize the conventional TBM (tunnel boring machine). The current cost of a hard-rock tunnel bored with a TBM is in the range of $3500-4000/m. The largest component of this cost is manpower, for the TBM and mucking operation. A recent study by the Robbins Company (TBM manufacturers) gives optimism that this cost may be reduced by up to a factor of two.

VLHC QUESTIONS AND ANSWERS
LOW-FIELD OPTION

G.

It is often said that the Galena-Platteville limestone layer under northern Illinois, including the Fermilab area, is ideal tunneling material. The longer a tunnel is, however, the more likely it is to run into difficulties with changing material. Along at least one east-west line, the Galena-Platteville ends a mere 40 miles west of Fermilab, requiring tunnels be made in water-logged sand. Considering the difficulty of constructing a tunnel that extends far to the east of Fermilab, due to population density, is it actually possible to construct a 500 km circumference tunnel safely and inexpensively in the Fermilab area?

See the answer to F6 above. The large ring needs geology data over a wide geographic region, which is not yet available, and so the question cannot be answered at this time.

H.

How do you plan to convince the public that it is ok to have an accelerator under their house?

The DESY laboratory accomplished this task for the citizens of Hamburg, in a case in which the tunnel is much closer to the surface than the proposed VLHC tunnel. So, there is an existence proof than it can be done. An absolutely necessary requirement is an excellent working relationship between Fermilab and the members of the local community (See the answer to F5 above).

I.

1) All else being equal between the low field and high field VLHC options, the non-technical problems relating to size become important.

a) Local opposition to the SSC was strong and organized. Overcoming this opposition is as important to the VLHC, more work and less fun than coming up with creative magnet designs. How do you foresee a public relations effort for siting of the VLHC at Fermilab being developed? Who does it? Who pays for it? When does it start?

The public relations effort must start with, and build upon, the relation between Fermilab and the local community (see the answer to F5 above). This is, of course, an ongoing effort, but coupling to the VLHC (or any future off-site machine) should be done now, in the context of future options for Fermilab. Future "open-house" activities at Fermilab should have explicit reference to the possibility of expansion, with clear explanations of the motivations and the benign features of such expansion. If the public comes to view Fermilab favorably, and to understand that an expanded laboratory will be an even more friendly and beneficial member of the community than the present lab, the opposition (which is inevitable) will at least have an uphill battle in gathering support.

b) It has been suggested that the machine could be located west of Fermilab (perhaps an hour's drive) but still use most of the infrastructure and other benefits. Moving to a less populated area could reduce the opposition to some extent. Would the performance and cost of such a machine be compromised?

This proposal seems a little problematic to me. For one thing, the favorable geology runs out an hour's drive to the west of Fermilab (see F6 above). Thus the tunnel costs would certainly be higher. Also, a complete injector complex would be required (or a very long injection line), which represents a very big investment.

3) A lot of cost data was developed for the accelerator components and civil construction during the SSC days and for the LHC.

a) Is this data applicable to the low field design? For example can the tunnel costs for the low field design be demonstrated to be less than the rather favorable costs for the SSC tunnel?

The current cost of a hard-rock tunnel bored with a TBM is in the range of $3500-4000 / m; this is similar to the costs for the SSC tunnel. The largest component of this cost is manpower, for the TBM and mucking operation. A recent study by the Robbins Company (TBM manufacturers) gives optimism that this cost may be reduced by up to a factor of two, but this cost reduction has yet to be demonstrated in a real project.

b) Has an complete project cost model (including life cycle costs) been developed that can demonstrate a clear advantage of the low field magnet approach?

No. Such a model is impossible at the moment, as the cost of the magnets (both low field and high field) has yet to be established with confidence. Tunnel costs, after cost reduction efforts, are also yet to be determined.

c) How much advantage do you think in cost does the low field option have to have before the disadvantages of size related issues (such as access for tunnels, public opposition, etc.) are overcome? 10%? 20%? 50%?

My guess is that a 20% cost advantage would be decisive.

d) Can you quantify the cost disadvantage of the low field approach due to the lack of radiation damping?

Alas, the full consequence of radiation damping on all the accelerator systems is not yet established. For example, damping may allow for a smaller aperture magnet and / or more relaxed field specifications, but the impact of these benefits on the cost of the magnets is not known.

J.

1) How many access points do you need to the tunnel ? How many LHe feeds ? Can people go down there, if not what is the effort to do installation and alignment.

Using the analysis made for the Nb_3Sn high-field system at Snowmass[3], the high-field ring would be divided into 16 sectors, with a feed point at the center of each sector, feeding two strings, each of length 3240 m. There are thus 16 LHe feeds from the surface (16 access points). The access point separation would be about 6.4 km.

For the low-field case, the Snowmass study envisioned 8 sectors, with 8 feed points feeding two 38-km strings. In this case, there are 8 LHe feeds, and 8 access points, separated by 76 km.

More access points may be required for life safety reasons. In both the high field and low field options, human access to the tunnel is possible.

2) For the very high energies the radius of the machine is ~ 100 km. Does the geology allow to have the access points in a "reasonable" elevation towards the surface.

The details of the geology are not know well enough at present to define the siting of the ring. Typically, there is enough freedom in defining the footprint to allow for access points in areas where the land is available for purchase. This would not be true if the ring went under Lake Michigan.

3) Does the ring follow the curvature of the earth ? If: How do you do the vertical bending ? If not: What is the pressure profile of the LHe with a difference in height from one end to the other ?

It looks like the ring will have to follow the earth's curvature to some extent; otherwise the depth will vary by almost 800 m. Presumably the vertical bending can be accomplished with small vertical bending magnets located at strategic points. The total bending angle is about 0.2 mrad.

4) Given the ATL rule and an a of about 10^-5 micrometer^2/sec/m two quadrupoles on opposite parts of the machine will misalign by ~ 0.1 mm in 5000 sec, so almost an hour. How do you control the beam dynamics ?? Is there active feedback ? Beam based feedback ??

Beam based feedback will be required to do this. BPM resolution of about 5 µm will be necessary, together with BPM's and correctors at every cell. A consequence of this is that, every time the machine is turned off for a significant period of time (e.g. a month), the beam may not circulate when turned back on, and the closed orbit will have to be re-established from scratch.

5) That question shows my ignorance but: If you build long sections without any interruption: Assume there is a ground fault somewhere: How do you localize it ??

Presumably one would have access to the bus at the interconnect points, (e.g., voltage probes) and you could thus localize the fault to one magnet using probes at the interconnects. This certainly works if the bus is warm. There may be a more clever way to do it without warming up the bus.

6) You like the 3 TeV machine at Fermi: Why should we build? If it is only an injector wouldn't you optimize the machine and the magnet very different??

Yes, if the machine were simply an injector, the magnets would probably be very different. Even the energy, 3 TeV, is not necessarily the optimum injection energy for a VLHC. The machine cannot be justified simply as a technology demonstration for the low-field magnets, as that could be accomplished with a kilometer-long string test. A substantial part of the motivation must come from the fact that the machine can explore physics "niches" (such as fixed target tau-neutrino and hyperon beams, an e-p collider, forward physics, etc.)

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