FERMILAB AND THE FUTURE OF HEP

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I. General Comments

I assumed that this DPF assembly was designed in large measure to address the issue of U.S. HEP in the "late 80's," where our last Woods Hole panel identified a need for new and exciting facilities. My initial comments are made as a citizen-physicist. Later I will put on my director's hat and discuss Fermilab's options. The scale is set by Europe where by the late 80's, they will surely have LEP, and have had six to eight years of $\bar{p}p$, and may well have HERA. By the early 1990's there will be a European capability to pave the LEP tunnel with superconducting magnets to make 1 TeV/tesla of proton acceleration, which, at 6 tesla is a 6 TeV ring. By 1990 or so, UNK (USSR) is scheduled to come on at 3 TeV for fixed target physics with collider application some years later.

These are formidable challenges and, at the same time, especially in the case of LEP, a very daring and imaginative thrust towards definitive tests of our current understanding. Considering the U.S. posture, I began to have nightmares. Dare we be any less imaginative? Are we settling into a comfortable, secondary role in what used to be an American preserve?

And what are the scientific imperatives? In my opinion, theoretical physics beyond the standard model has been treading water for several years.*

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* "By the year 1985, the Fermilab Collider should operate at 2 TeV. It is now abundantly clear that these energies are not adequate to reveal nature's secrets at high energy. ... We need a 20 TeV hadron-hadron collider."

S. Glashow, Rome Workshop, October, 1981

"Do not ask theorists at which energy to aim for the next generation of high energy accelerators. Aim at the highest possible."

A. Salam, Paris Conference, 1982

"The outstanding problems in today's theory of particles are such that none of the projections beyond the standard model can be considered with any confidence. What we need is experimental guidance: exposure to the no man's land of lepton-lepton or quark-quark collisions up to the mass range of 1 TeV and beyond."

M. Veltmann, SLAC Accelerator Summer School, 1982.

In contemplating the late 80s, where will the breakout occur? Who will lead us to the green, intellectual pastures? In the U.S., the problem is that we have, over the past two decades, been reduced to four aging laboratories. Each of these laboratories properly does accelerator R&D in order to maximize the physics that can be realized on its site. Our history and traditions do not extend back far enough to prove that this may not be best for HEP, even for U.S. HEP. But I believe it is a dangerous situation. I happen to believe in the lessons of history (standard model or no standard model) and, therefore, in the urgency of proceeding to the next energy step, as soon as possible. This belief will and should be debated hotly. (There were theorists in the 60's that preferred a high intensity 10 GeV machine to a 200 GeV accelerator.) But just suppose I'm right and 20-40 TeV in the CM turns out to be decisive for higgs or constituent quark models or whatever. In my nightmare, I noticed that none of the four labs has a large enough site for this energy range without a great advance into the > 10 tesla supermagnet technology. This may well explain why there has not been a proposal for the great leap forward.

As proposals for the late 80's, all four laboratories have been pressing on projects which may not, in my opinion, provide "sufficiently bold thrusts into the unknown" and, in this sense, do not seem to me to promise to provide the excitement which draws the best and brightest. In particular, I fear that these proposals do not promise to dramatically enlarge the domain of observations when we consider the world's activities. Specifically, I believe it is important to at least examine the possibility that the machine for the late '80s be, in fact, a very bold advance. We need to ask ourselves hard, introspective questions: are we, as a community, growing old and conservative, and is there a danger of quenching the traditional dynamism we have surely enjoyed in the past three decades?

All of this led me to consider the problem: how can we break out of the aging lab and inadequate lab site constraints -- how can we creatively leapfrog the world and get to the multi TeV domain soon? The possibility of near-term (less than ~4 years) technological breakthroughs seems very remote. Our experience with SAVER magnets and the complexities of 10 tesla magnets indicates that here, again, we face a long R&D program, with no assurance that we will break through on costs (see below). We were then led to consider old technology: iron magnets with radical innovations in fabrication, mass production, installation, etc, so as to bring the costs per meter down substantially more than the ratio of magnetic fields. Since the operating costs are also relevant, the iron would have to be energized by superconductors; i.e., we are talking about an old idea, superferric magnets. Since we are now dealing with state-of-the-art systems, it seemed plausible that a 1-2 year R&D program could yield a very good assessment of the possibilities. Now, with 2-3 tesla magnets, we are talking about a very large site -- clearly a new laboratory which would become the U.S. High Energy Lab. It would have to contain a ring of ~15-30 Km radius, and if shallow trenching (instead of conventional tunnels) is the mode, then the site must be very flat, sparsely populated, yet near a good, international airport. Hence the accolade, "Machine-in-the-desert."

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Later in this meeting, you will hear from Bob Wilson, Paul Mantsch, Dick Lundy, Russ Huson, and others who have begun to consider this problem of the Multi TeV-in-the-Desert scheme. If we are encouraged by the results of discussion here, we should convince at least one of the labs to devote some modest R&D time to the problem of supercheap accelerators located in unlimited (and free!) real estate.

II. Costs

Let me address the cost problem in a simple-minded way. We know rather precisely what the SAVER costs. The 1979-1982 total project costs (R&D, equipment and plant) is \$95M. To this, we must add the Helium Liquefier capital costs, magnet assembly facility tooling costs, and funds which bought NbTi ore at low cost. We should also add some labor costs which were applied to SAVER by other parts of the lab but not charged accurately. Of course, we should subtract the costs of mistakes and of that part of the R&D that need never be repeated. When these corrections are made, we find that \$100M (1980-1981 dollars) is a generous estimate of the SAVER costs. Since these are very largely linear, we arrive at \$16K/meter as the cost, without tunnel, of a 4.2T ring with all its services, refrigeration, straight sections, controls, rf, aborts, etc. We now bring this to 1982 costs in a tunnel (\$5K/meter) and arrive at \$23K/meter.

III. The New Laboratory: SLERMIHAVEN II

Now we come to the superferric machine in the desert. (Actually the Santa Fe-Albuquerque area, for example, is not unpleasant). What is crucial is the idea that a good quality iron magnet energized with simple superconducting bars can be made by imaginative mass production techniques in hundreds of foot lengths, assembled in a pipe which is buried underground. Using costs of materials, site development, rf, refrigeration, pipeline data, we have a preliminary estimate of \$3K/meter for the main ring. We stress that we want reliability and exposure to physics at the earliest times. We must clearly invent all kinds of gadgets to solve the problem of such a geographic machine. There are interesting problems as to the requirements on field quality, lattice design, etc. Any or all of many as yet inadequately considered problems can in principle sink our cost estimates. But let us suppose the number we "wished" holds up. Then, using an optimistic 2.5T and desiring $\sqrt{s}$ = 20 TeV gives us a ring of R = 16Km at a cost of \$300M. To this we must add an injector. Injectors should not cost more than 20% of the cost of the main ring. Here I may be way off, but let me take \$100M. I add \$50M for site preparation, roads, an initial, primitive complement of buildings and workman's cafeteria. I add \$50M for 6 interaction regions, \$50M for a $\bar{p}$ source, and \$200M for 6 years worth of incremental salaries and contingency. So I say, "For \$750M, we can begin serious observations of collisions at $\sqrt{s}$ = 20 TeV!" And we have begun to develop a new high energy laboratory. My wild optimism says we start construction in 1986 and finish in 1992, using an average of \$120M per year out of a \$400M (1982) annual budget. By 1993, we begin to phase out other labs, and we can afford to operate and still continue to invest \$100M per year in improvements: now an ep ring, now a high rise, fixed target areas, then another p-ring etc. I have finessed an important point. It will require some debate, some inspired intuition. If we stretch out the time by ~three years (still starting in '86), we can go for $\sqrt{s}$ = 40 TeV by doubling the ring radius. Alternatively, we can invest more of our (assumed fixed) budget per year to go to this higher energy.

It is fully anticipated that these cost "guestimates" will be greeted with skepticism by the community. Since the economics are crucial to the argument, I plead that criticism be specific and not based upon experience which may be irrelevant.

There are of course serious drawbacks -- the sociology gets worse and worse. All our eggs are in one laboratory. Initially, we can only support ~400 users or so, although this number should grow rapidly. My feeling is that the greatest drawback is the resistance engendered by conservatism. But any program we choose must be compared to LEP which, in 1990, will be doing ~150 e^+e^- physics which is equivalent to ~750 GeV hadron collisions as well as HERA and UNK with a 3 TeV proton accelerator. In order to discuss the competing options, I put on my Fermilab Director's hat and discuss what we have to offer for the late '80s.

IV. Fermilab Options

One of the Fermilab options, submitted by request to the Trilling Committee, is to build a dedicated $\bar{p}$xp collider at ~2 TeV in the CM. This would have four interaction regions. It would provide an order of magnitude more collider physics than TeV I and allow the fixed target program to run 100% of the time. Since we now inject at ~1 TeV, rf, refrigeration and aperture savings are estimated to bring the cost down to less than \$18K/m or \$110M for the ring. Adding beam transfer (\$10M), interaction regions (2 at \$8M, 2 at \$4M) we have a total cost of \$145M. An electron (10 GeV) ring to provide ep option would be a very natural addition and would add \$30M (1982 dollars). Alternatively, a pp ring for 10^{32} luminosity would bring the cost to \$225M without ep. These are acceptable costs and will produce excellent physics. We believe in fact that within the constraints of \$250M we may well achieve a modest increase in energy to $\sqrt{s}$ = 3 TeV in this program.

Now suppose we try to estimate the cost of a site filler ring with 10T magnets. We don't know how to make them, but let's assume that a brilliant R&D program gives a cost per ring of \$25K/m without tunnel. This is very optimistic since the 4.2T magnets came to \$16K/m and the problems must go at least like the ratio of fields. So a site filler at R = 2.5 Km has a ring cost of \$30K/m × 2πR = \$470M. Adding interaction areas and beam transfers requires over \$500M, and this is for $\bar{p}$xp. It gives us somewhat more than 5 × 5 TeV collider, but if fixed targets are desired, the requirement of extraction inside the ring limits the energy to 4 TeV. Here, again, a pp option would raise the cost to \$900M. I am not at all sure that this is a scientific bargain. We should stress an important point here: The issue of cost vs luminosity is a very important one. It may be that very clever inventions can make pp cheaper than 2 × a single ring e.g. the Palmer 2 in 1 scheme. We don't know this, and so we may be overestimating the cost of the extra luminosity. Nevertheless, the $\bar{p}$ source will exist, and it is our opinion that a luminosity close to $10^{31}cm^{-2}sec^{-1}$ is ultimately achievable. In any of the scenarios for high energy colliders, there will be an issue as to how much to pay for a factor of 10 (or even 50) in luminosity. See the appendix. The scientific issue will be (in the case of a new site) the trade-off of energy and luminosity.

I have summarized the Fermilab options and the desert machine comparison in Table I. Alternatively we can wait patiently for the great accelerator breakthrough. Clearly we should work much harder toward this goal. At ~one TeV per meter, each university can have its own accelerator again and this would be the best of all worlds. However, if there is a realistic possibility that we can reach ≥20 TeV in CM energy within the time and budgets cited, we must very seriously examine the counter arguments, each of us weighing with our infinite collective wisdom the greater good to our science.

To summarize, if this workshop is to address the future of HEP "in the late '80's," my fervent plea is that one includes a new laboratory option as a possibility. The key point at issue, I believe, is whether the economics is more or less correct. If it is correct, then I believe this is the best possible move for U.S. HEP and indeed world HEP. I also feel that the perceived political difficulties of establishing a new site would evaporate under the glow of wide community enthusiasm. In the past thirty years we have acquired fantastic traditions and universal respect in high places. These are derived from past successes. Let us not grow old, querulous and over-cautious in facing our future.

Acknowledgments

These ideas derive very largely from the prescient writings of R. R. Wilson and Bill Willis. The early work of Gordon Danby at BNL on superferric magnets must also be mentioned. We are additionally encouraged by the superconducting power transmission studies of Eric Forsyth's group at BNL.

Table I

Fermilab Collider Options

$\sqrt{s}$ (TeV)	Magnet	Tunnel Km	Cost* $M		Earliest
≥ 2	Modified SAVER	R ≅ 1	$\bar{p}p$ $ 145	(a)	1988
			pp 225		1989
11	10 tesla	R ≅ 2.5	$\bar{p}p$ $ 500	(b)	1990
			pp 900		1994+
20	2.5 tesla	^{o}R = 16(trench)	$\bar{p}p$ 750	(c)	1992
	"	"	pp 1100		1995+
40	"	^{o}R = 32 "	$\bar{p}p$ 1100	(c)	1995+
3	"	R = 2.5 "	$\bar{p}p$ 85	(c)	1988
		"	pp 130		1990

* 1982 Dollars. Does not include detectors
** Assumes a 1986 start
\+ Funding limited
o New "desert" site
(a) Confidence level 95%
(b) Confidence level 20%
(c) Confidence level 40%