Status and Outlook for International Collaboration on Future Accelerators

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Last July, I received an invitation from the organizers to deliver the terminal paper at this Conference. This is not the first time I have been asked to give the last talk and to address the issue of International Collaboration. If memory serves me, I did this at Stanford[1] in 1974 and in Chicago[2] in 1977. I assumed that the results of my last efforts were not terminal, but that it did take about ten years to forget. Little did I realize when I accepted the invitation that a topic as close to motherhood as is international collaboration would become such a hot potato. Actually, the talk in 1977 was entitled VBA and discussed the formation of the International Committee on Future Accelerators (ICFA). This was a romantic idea to begin the planning for a World Laboratory which would house the Very Big Accelerator, a machine by definition so expensive that no single nation or region could afford it.

In my 1977 paper, I designed the VBA as an accelerator complex that would have 40 TeV pp collisions (!!!), have a 20 TeV fixed-target facility and provide 200 GeV e^+e^- collisions via a circular machine concentric with the pp machine. I proposed using Manhattan Island as the site in order to avoid site searches and because New York City was about to go into fiscal default and therefore make available a vacated island complete with tunnels, a high rise and UN complex.

With all this excessive prescience, what happened to ICFA and the World Laboratory? The short answer is that ICFA was in fact formally organized in 1976 with membership drawn from US (3), Western Europe (3), USSR (3), Japan (1), and Dubna member states (1). It had two missions: (1) to provide good communications, sponsor workshops, assure open access to facilities and reduce duplication. (2) Organize the World Laboratory to build VBA.

ICFA did sponsor a resolution which was supported by all the existing laboratories (and by implication, the sponsoring agencies) to the effect that criteria for acceptance of proposals should be based only on scientific merit and ability of the proposers to carry out the research. It is doubtful whether this has any legal standing and, to my knowledge, no rejected experimental team has yet appealed to IUPAP to overrule the Director.

ICFA did sponsor three technical workshops, in 1978, 1979 and 1981. These were excellent meetings and in fact were influential in the eventual proposals for LEP, SLC and the SSC. It is surely an oversimplification to say that not too much was accomplished outside of these activities. The role of fairly intimate communication was certainly useful but for a variety of sociopolitical reasons, the definition of VBA kept changing and region after region took turns in preventing any substantive advances towards the World Laboratory idea for fear of delaying their own plans.

In 1983, the 8th meeting of ICFA took place at Fermilab shortly after the announcement of HEPAP's recommendation that the U.S. build the SSC. The meeting was unusually tense with both the Europeans and Japanese delegates claiming that the U.S. had finally and completely preempted the VBA concept.

It was then decided to hold a series of workshops aimed at the future of ICFA. The first of these was held in Tokyo in 1984. In the language reminiscent of State Department pronouncements, full and frank discussions took place. It was my personal impression that some CERN-based Europeans attacked SSC because it provided a threat to the future of CERN, some Japanese attacked SSC because of their idealistic belief in VBA and the Soviets held coats. In any case, there were two conclusions:

(1) ICFA should facilitate construction of accelerators i.e. promote international collaboration in construction and use of new facilities.

(2) ICFA would convene panels on accelerator technology and seminars for review of progress in this field.

The Economic Summit

In 1983, in a meeting largely ignored by high energy physicists and accelerator scientists, the Presidents and Prime Ministers of the seven industrial countries met in Paris in what was to become an annual event. People of the calibre of Thatcher, Reagan, Mitterand, etc. discussed, among other things, of course, scientific and technological collaboration. For example:

"Fundamental Scientific Research is one source of technological progress in industry and should be given support by governments."

Also: "Science and Technology are a source of national and international strength and can provide immense opportunity, for neutralization and growth of the world economy."

The Heads of State resolved to continue to include detailed agenda of collaboration at future meetings.

And ... high energy physics was specifically included as an appropriate subject for collaboration. In subsequent negotiations the U.S. was assigned the lead role in the Working Group on HEP. The Chairman was the Assistant Secretary for Energy Research of the DOE, Dr. A. Trivelpiece. The first working group meeting on HEP took place in Brussels in July of 1984. The entities represented were: US, Canada, European Community, France, Federal Republic of Germany, Italy, Japan and the U.K.

What was interesting here was that whereas ICFA was a grass roots movement to discuss collaboration and served to (in principle) pressure governments, the Summit was a summons from on-high for the scientists to discuss collaboration so as to decrease the costs of expensive research by minimizing competition.

The Brussels Working Group meeting came at a dramatic time, when U.S. scientists in a summer workshop in Snowmass, were whipping up enthusiasm for the SSC.

A long evening discussion in Snowmass led to a fairly strong consensus that the U.S. should do everything it can to secure collaboration on SSC from Japan, Canada, Mexico etc. as well as from Europe. This consensus was obtained in spite of the possible negotiating delays that would ensue from attempting to internationalize the SSC.

The warning from our leaders came in the London Summit of 1985:

"Effective cost sharing is becoming a more important element in the construction of major new facilities. Collaborative projects would benefit if coherent long-term plans for the construction and sharing of facilities were to be developed."

We are thus effectively cautioned that we need a world plan. The issue facing the HEP community is: Can we in fact agree on a plan that provides for a viable future for all the major players while coming, ultimately, to realize the dream of the early ICFA visionaries: a world laboratory?

Table I reviews the world inventory of facilities that are scheduled to operate in 1987-1993 interval. This is the "base" from which to examine the world future. We see a very powerful base indeed but with perhaps too much emphasis on the energy domain of 100 GeV and only modest excursions beyond. (TEVATRON and LEP II).

We now note that the U.S. has under very active consideration the SSC, approved by President Reagan in January, 1987 and now being considered by the U.S. Congress. The SSC has had the benefit of over three years of R&D at about $25 million/year and a very extensive design and cost estimate as well as many thousands of pages of workshop reports on physics potential and detector design.

The SSC, described in M. Tigner's talk in this Conference, is a 40 TeV proton-proton collider, estimated to cost about $4 billion (1988) and to be ready in 1996.

In Europe, the SSC has stimulated a much more active survey of European options for the 1990's. It was quickly recalled that, in the discussions before approval of LEP, the possibility of using the LEP tunnel (27 km) for hadrons had already been anticipated. This possibility was given a name: Large Hadron Collider (LHC) and the CERN establishment began their own studies of cost, feasibility and physics potential. More formally, a committee under Carlo Rubbia was convened to study LHC as well as an e^+e^- linear collider, with acronym CLIC, to operate near 1 TeV of CM energy.

LHC is also a pp collider, the energy being constrained by the tunnel and current magnet technology. The total CM energy is between 10 and 16 TeV, depending on assumptions as to the state of magnet technology. G. Brianti's talk in this Conference gives more details on the European plans.

A recent interim report by Chairman Rubbia suggested that if SSC goes ahead, CLIC would be a likely alternative, whereas if SSC is delayed, LHC has a viable physics opportunity and is "cost effective" because of the existence of the LEP tunnel.

The world HEP community now faces a unique and delicate challenge. Very few scientists (but not zero) believe that both SSC <u>and</u> LHC make sense. Even fewer funding officials would be overly enthusiastic. Since LHC is considerably behind SSC in both technical design and in the political process, it is unlikely (but not impossible) to have such a facility ready <u>much</u> before 1996. This is especially true when one takes into account the four large e^+e^- colliding beam detectors that over 1000 physicists are building to be ready in late 1989.

The major arguments of each side (insofar as a highly biased observer can state them objectively) go somewhat as follows:

<u>US HEP</u>: SSC is a fantastic scientific tool. It is <u>designed</u> to address the most crucial problems facing HEP, problems that seem to go to the heart of our ignorance of the structure of fundamental particles. In any case theory asserts that new physics must show up in the 40 TeV collisions. SSC, after three years of hard work is on the verge of approval and deserves the support of our European colleagues - even the commitment, however hedged, to collaborate in construction of the machine and its detectors after LEP II and HERA are operational. Europe can then, with U.S. help, launch into the post-SSC machine, presumably a linear collider in the ~ 5-10 TeV CM range. An alternative, post-SSC machine is one proposed by A. Zichichi called ELOISATRON. It is something like a 100 TeV pp collider.

<u>European HEP</u>: SSC is too big a step. It is not cost effective. The U.S. should help Europe build LHC which could, with U.S. help, appear in 1994 or so and begin to address the problems of the 1 TeV mass scale. The next step would be a linear collider in the U.S., based upon the pioneering work of SLAC. Or, alternatively, the U.S., with Europe's help, would build a Super SSC say 60 TeV x 60 TeV starting say in 1993-1995. LHC would, incidentally, also provide e-p collisions well beyond the HERA reach.

<u>Soviet HEP</u>: Нам все равно, что делают эти капиталистические эксплуататоры.

The U.S. response is: LHC may not solve the Higgs Problem. Certainly LHC is a powerful machine to advance Physics well beyond LEP and TEVATRON. However, the scientific and engineering man-years of investment in <u>any</u> multi-TeV machine with its array of detectors is enormous and almost independent of the energy. The U.S. proponents then argue, why not insist that this effort will be <u>sure</u> to address these profound issues such as the origin of mass or the mechanisms for symmetry breaking that have all been swept under the Higgs rug of our ignorance? Furthermore, if the U.S. is to look to LHC for a significant part of its scientific future, we must recognize that LHC is constrained by a tunnel filled with 4 major e^+e^- detectors which will only begin to take data in late 1989. Once SSC is halted, the momentum to pick up again will be difficult to find.

Crucial to this debate is the uncertainty in both regions about the feasibility of a linear collider without first constructing a prototype of modest energy - say 2-300 GeV. Until the SLC has several years of experience, even this machine is difficult to cost or design. If SSC is the "last machine," then Europe's future in HEP is shaky after SSC.

Some change in this "technological pessimism" was generated by the explosive development of high temperature superconductors but it is too early to judge how this will ultimately influence the Gordian Knot facing our world community.

I would like to conclude with the Question: Is the vision of the VBA in a world laboratory hopeless? What are the imperatives for such a project?

The continued viability of regional laboratories is an essential condition although these need not be frontier machines, especially if we ever get to the world laboratory phase. In fact, in the optimistic view we all share, an increasing number of nations will create accelerator-based infrastructures. Examples are PRC with the e^+e^- collider nearing completion in Beijing, Brazil with ambitious plans for a synchrotron light source. India, Taiwan, Mexico, Korea, Argentina and Israel are countries with respectable nuclear accelerators, synchrotron light sources or advanced plans. In the time scale of VBA, all of these nations are likely to join US, Western Europe, Japan, Canada and the Eastern Bloc in the grand adventure of a world laboratory.

As accelerators and accelerator-derived technology continues to percolate into industry, medicine and as general research tools, more and more nations will develop experts and the infrastructure base from which one can draw resources towards the world laboratory. This laboratory, dominated by VBA, could well assume other functions contiguous to its main mission, including development of accelerator science, fusion research, research in arms control technology i.e. topics suitable for a world technology and scientific center.

What kind of machine and what parameters would be relevant to VBA? Clearly we have been too modest in the past. Also, we have no theoretical guide beyond the 1 TeV mass scale except for the necessarily vague statement that if a Higgs-like object shows up near 1 TeV, it may be the tent under the camel's nose. That is, there may exist a "Higgs sector" analogous to the pion sector. Arguing from analogy this would indicate that one would want another factor of 10 in the CM. Let's round it to 1000 TeV. Such a machine was designed pedagogically by J. D. Bjorken about five years ago.[3] A mere 500 TeV against 500 TeV. Whereas the technology of high gradient lasers may well be the technology of choice by the start of construction (2001!) the frenzy of high T_c superconductors makes it tempting to extrapolate this technology and see how a circular machine might appear. It could have 50T magnets and a circumference of 120 km. Using the same magnets, the ~ 20 TeV injector would have a radius of a mere 1 km. Lest this machine may appear to be "too easy" for accelerator designers, a little thought will reveal problems of exquisite complexity everywhere one looks!

If the linear costs are scaled from SSC, i.e. if the cost per meter for 50T is the same as for 6T, the cost would be about 2x the SSC, say \$8 billion in 1988 dollars. This cost rule is almost a definition of a successful R&D program. Add \$2 billion for the international complexities and we have an affordable machine, shared between all the nations on some GNP type of formula.

This is obviously a project worthy of closing the twentieth century! I hope it is a vision worthy of closing this accelerator conference.

Table I

ACCELERATOR INVENTORY

I.	"Old Timer" Machines (But still operating for good physics)	
	KEK, Japan	12 GeV protons - fixed target
	AGS, Brookhaven	30 GeV proton - fixed target (plus heavy ions)
	SpS, CERN	450 GeV proton - fixed target
	CESR, Cornell	8 x 8 GeV e^+e^-
	PEP, Stanford	15 x 15 GeV e^+e^-
	VEPP IV, USSR	6 x 6 GeV e^+e^-
II.	Brand New or Nearly Working Machines	
	TEVATRON II, Fermilab	800 GeV protons - fixed target
	Sp$\bar{p}$S, CERN	315 x 315 GeV $p\bar{p}$
	TEVATRON, Fermilab	900 x 900 GeV $p\bar{p}$
	TRISTAN, Japan	30 x 30 GeV e^+e^-
	SLC, Stanford	50 x 50 GeV e^+e^-
III.	Under Construction	
	LEP I, CERN	50 x 50 GeV e^+e^-
	HERA, W. Germany	30 GeV e^- x 1000 GeV p
	UNK, USSR I	3 TeV protons - fixed target
	UNK, USSR II	3 x 3 TeV $p\bar{p}$
	BEPC, PRC	3 x 3 GeV e^+e^-
IV.	Proposed, Designed or Dreamed	
	LEP II, CERN	100 x 100 GeV e^+e^-
	LHC, CERN	6-8 x 6-8 TeV pp
	SSC, US	20 x 20 TeV pp
	ELOISATRON, Italy	100 x 100 TeV pp

References

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2. L. M. Lederman, VBA, Transactions on Nuclear Science (IEEE) June, 1977, p. 1903

3. J. D. Bjorken NATO Advanced Study Institute on Techniques and Concepts in High Energy Physics, 1982 Lake George, N.Y.