Sio f.8

[Institute of High Energy Physics, Serpukhov, 1976] 1 of 2

ok

Physics projections

The Serpukhov study group outlined some of the unanswered questions in high-energy physics and identified the special capabilities of each type of projected accelerator.

Among the key questions, the group said, are: "Do quarks exist and, if so, how are they confined in hadrons, and what are the forces between them? The recent results about hadron collision products which possess high transverse momentum have shown how little we understand about the internal dynamics of hadrons. Secondly, is the Weinburg-Salam gauge theory of weak interaction pointing towards the real solution or is it a wrong approach? The quantitative agreement of neutral-current data with theory is strong encouragement for gauge theories. Nevertheless, no deviations from a four-fermion structure of the weak force have yet been observed."

For weak interactions, it is expected that at about 1000 GeV (center-of-mass system) the simple four-fermion theory will break down. There might be a whole series of intermediate bosons, Higgs bosons of different kinds and a series of heavy leptons and neutrinos.

For strong interactions, there is no indication of a definite critical energy range. One would like to know whether or not further quantum numbers exist, such as charm, flavor, color and so on.

The accelerators and storage rings being discussed for the VBA each have their advantages:

- Proton-proton and proton-antiproton storage rings, which reach the highest practicable center-of-mass energies at the price of lower luminosity, appear adequate for finding the weak-interaction intermediate bosons, provided the Drell-Yan production model can be applied. In studying strong interactions, total cross sections and energy dependence of particle-production mechanisms will be probed in a significant way.

The importance of conventional proton synchrotrons is in their higher luminosity, diversity of external beams and the opportunity to use nuclear targets.

Electron-positron colliding beams allow the clean study, not only of quantum electrodynamics and electromagnetic production of hadrons, but of weak interactions as well. In addition any charged heavy leptons or other charged non-hadronic pairs (including intermediate bosons)would be produced at a measurable rate, if they exist.

Electron-proton rings permit the clean study of strong interactions at small distances. They can test the idea that the strong interactions weaken at small distances and grow at large ones (asymptotic freedom). One can study the nature of proton constituents and how (or whether) they are confined. Finally, heavy leptons might be produced (if they exist).

Projected High-Energy Physics Facilities

Maximum Energy (GeV)

Region	Facility	Status	p(l.s.)*	e (l.s.)	pp(c.m.s.)	$\bar{p}p$ (c.m.s.)	pe (c.m.s.)	e^+e^- (c.m.s.)	circumference (km)
Japan	tristan	proposed	180	17	360		110	34	~2.0
West Germany	petra	funded		19				38	2.3
CERN nations	LSR	planned	400	20	800	800	180		6.4
	LEP	planned		< 100				< 200	< 50.0
USA	PEP	funded	200	18			100	36	2.2
	Doubler	partially funded	1000						~6.0
	isabelle	proposed	200	20	400		130		3.0
	popae	proposed	1000	20	2000		280		5.5
USSR	VEPP-4	proposed		7				14	(?)
	UNK	proposed	2000	20	4000	4000	400		18.0
International	VBA (fixed target)	studies in progress	>10 000		>20 000	>20 000			30-60
	VBA e^+e^-	studies in progress		>100				>200	>50.0

* Abbreviations: p=protons, $\bar{p}$= antiprotons, e^+= positrons, e^-= electrons, l.s. = laboratory system, c.m.s. = center-of-mass system

Physics Issues and the VBA

J. D. Bjorken

These notes represent a summary of some of the homework I have been doing on VBA issues. Prime consideration was given to the scope of what machines and physics issues should be considered. I consider fair game any accelerator or rings up to 10-20 TeV for protons, 25 GeV for electrons, and 100+100 GeV for e^+e^-. I have worked up additional material—mainly "laundry lists" or miscellaneous calculations and curves—which I will only circulate on request.

Contents:

I. Futurism: does it work?

Does physics output of machines have anything to do with the prognostications futurists made before they were built? I catalogued some previous machines along with the phenomena I believe were anticipated, how they were manifested, as well as surprises. Catalogue available upon request. My tentative conclusion: in broad sense, futurism works.

Generally what was hoped or expected to be uncovered actually was. Major unanticipated surprises were expected to occur—and did. Only those who hoped for quarks, W's, monopoles, tachyons, etc., were disappointed; all of those hopes were long shots.

II. Energy landmarks and how machines fit in

Figure 1 lays out the energy level diagram for machines. I have taken ep to be ~3 times as efficient as pp in turning s = (cms energy)^2 into physics, and e^+e^- 10 times as efficient. These numbers are negotiable; e^+e^- may be a little underestimated.

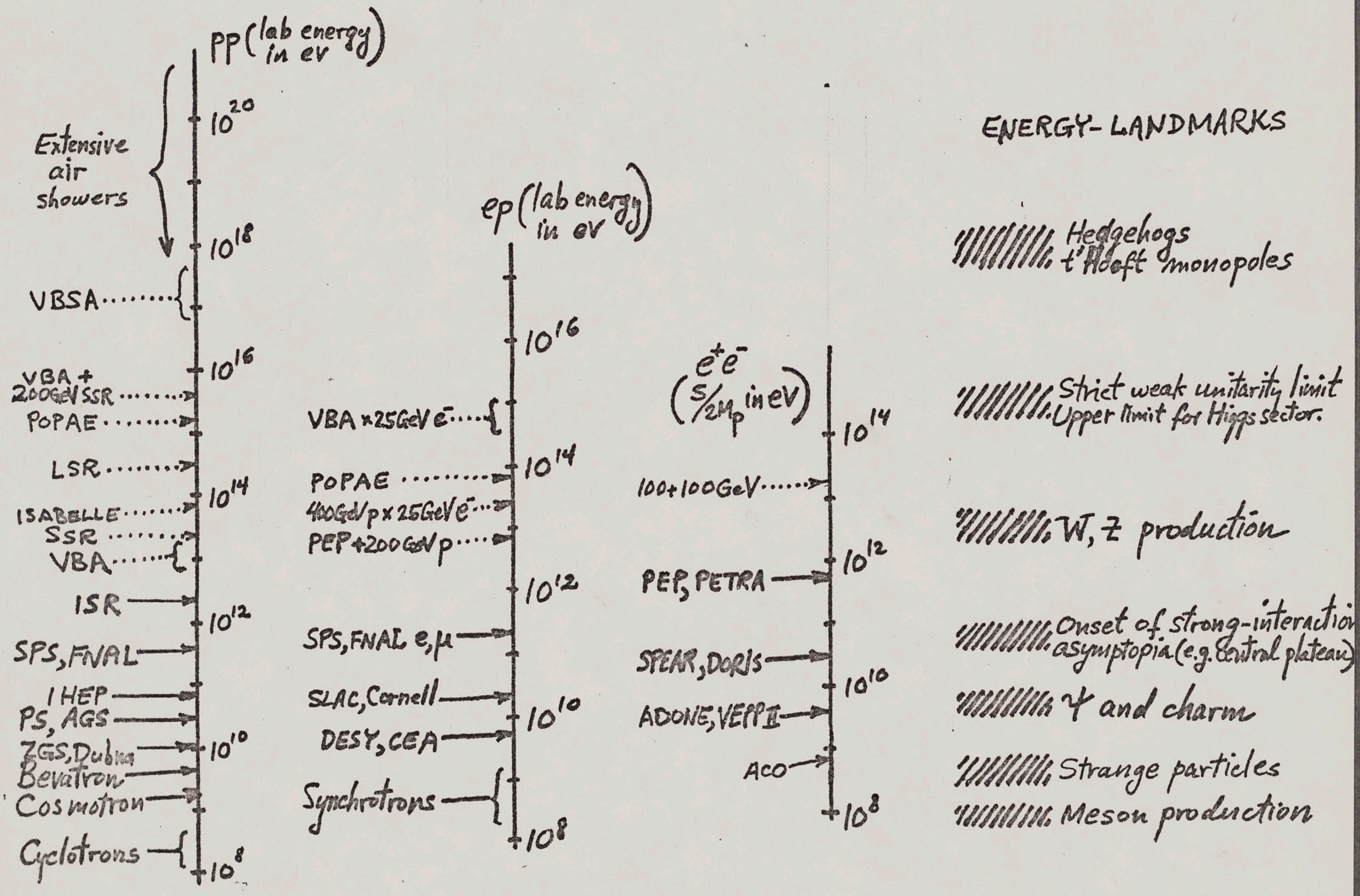
Comments:

Dynamic range between VBSA (10 TeV + 10 TeV pp rings?! ?) and ISR is same as from ISR to low energy (~40 MeV protons). Plenty of room at the top.

Energy isn't everything. Luminosity, flexibility of beams, etc., favor synchrotrons. Cleanliness of interpretation may favor e^+e^- (or ep) over pp. And so on.

Energy landmarks discussed and sharpened some later on.

FIGURE 1 — Energy Levels of Machines



III. Hints from cosmic rays

Main sources: McCusker, Phys. Reports C20, 229 (1975)
Yodh; summary of 1975 Munich Conference

The status in brief:

1. Relatively quiet below 100 TeV.
2. Hints of new phenomena above 100 TeV.

Philosophy: emphasize those observations which are very direct and difficult to understand within conventional ideology.

Examples:

Best: Niu's charm event: $E \sim 10\text{-}20$ TeV. ? But not because of charm; because of high multiplicity.
Leading fireball accompanies charm candidate
$n_{ch} \sim 25$ in $\Delta y \leq 2$
Event initiated by a neutral particle

Centauro: $E \sim 300\text{-}1000$ TeV.
Again high $\bar{n}$ and big leading "fireball"
$\bar{n} \sim 100$ at production; "fireball" mass ~ 200 GeV. Apparent absence of π^0's at production.

Schein multigamma cascade:
Energies of these relatively low. (Available even to FNAL, but nothing like them yet seen.)

Bristol event (cf. P. Fowler, CERN 61-22, p. 125 (1961)):
Two electromagnetic showers (10-50 TeV) in very high energy event which show unusual penetration (> 16 RL)

Good: Tien-Shan calorimeter measurement of hadron cascades.
(increase in mean penetration at $E \gtrsim 100$ TeV from ~ 700 to ~ 1100 gm/cm^2).

More Indirect: Evidence from several experiments for high-$p_\perp$ multiple cores (again above 100 TeV!).

I welcome additions to this list.

Messages:

1. 100 TeV is a possible landmark energy for new phenomena. If so, case for super-high energy pp rings (even with abysmal luminosity) is strengthened.
2. High multiplicity ($\bar{n} >> 100$, at least some of the time) may be a new feature of strong dynamics at these energies.
3. Centauro, Schein, Bristol events remind us that we may have shockingly different phenomena to study.
4. Lots of high-$p_\perp$ hadron cores from strong interactions can mess up searches for weak or electromagnetic phenomena (e.g., ISR experience).

IV. Fundamental issues and miscellaneous questions

1. What kind of physics will dominate our interest at multi-TeV energies?
   a) Predictable extensions of present interests. Laundry list available upon request.
   b) New directions in strong interactions; e.g., high-$\bar{n}$ events, acoplanar high-$p_\perp$ events, peculiar composition (e.g., Centauro, Schein).
   c) Dynamical structure of weak interactions
      i) Mediated by W's, Z's ??
      ii) Weak-electromagnetic synthesis??
      iii) If so, existence of Higgs sector?
      iv) If not, strong $\ell\bar{\ell}$ (or W-W) interactions?? (See comment 2a below.)
   d) Symmetry and group-structure of the sundry interactions: what is the weak group? The strong group? (How many flavors, etc.?)
   e) Study of production and decay of a zoo of new particles. Zoo exhibited in Section VI.
   f) Breakdown of QED.
   g) Breakdown of foundations
      i) Quantum mechanics nonlinear at short distances?
      ii) Lorentz or Poincare invariance goes bad?
      iii) Conservation laws deteriorate at short distances??
      iv) Causality goes bad at short distances?

2. Two comments which may bear on question 1:
   a) The options I can see for the future of weak interactions at high energy are:
      i) No W's, Z's, etc. exist. Then $\ell\ell$ (or qq) scattering strong at $\sqrt{s} \gtrsim 500$ GeV.
      ii) Z's, W's exist. No gauge theory cancellations. No renormalizability. W-W scattering strong at $\sqrt{s} \gtrsim 1$ TeV.
      iii) Renormalizable gauge theories. Then Higgs-scalars must exist. If Higgs-scalars h weakly coupled to each other (and to ordinary world), then they shouldn't be much more massive than W, Z; perhaps lighter (but heavier than a few GeV). There could very well be quite a few of them.
      iv) If Higgs' strongly coupled to each other, typical masses expected to be $\gtrsim 300$ GeV.

      Conclusion: Unless a number of "light" (< 100 GeV) elementary Higgs scalars exist, it appears difficult to avoid the existence of a new class of strong interactions at $\sqrt{s} \gtrsim 1$ TeV. It may become as much a future goal to reach this threshold as it is now to reach the gauge-theory W-threshold at $\sqrt{s} \sim 50$-100 GeV. (Notice $\sqrt{s} \gtrsim 1$ TeV => $E_{lab} \gtrsim 10^{15}$ eV, in the middle of the cosmic ray activity. Can they be related? Don't know.)
   b) Operationally, weak parton-parton cross sections get as strong as em at $\sqrt{s} \gtrsim 50$-100 GeV; also weak decay widths bigger than electromagnetic, which in turn are bigger than strong Zweig-rule violating widths when $m \gtrsim 50$ GeV (cf. Fig. 2). High-$p_\perp$ hadron production might show the same pattern: For $p_\perp > 50$ GeV, weak production > electromagnetic > strong (using CCR parametrization). (Is there a basic message in this confusing situation?? Don't know.)

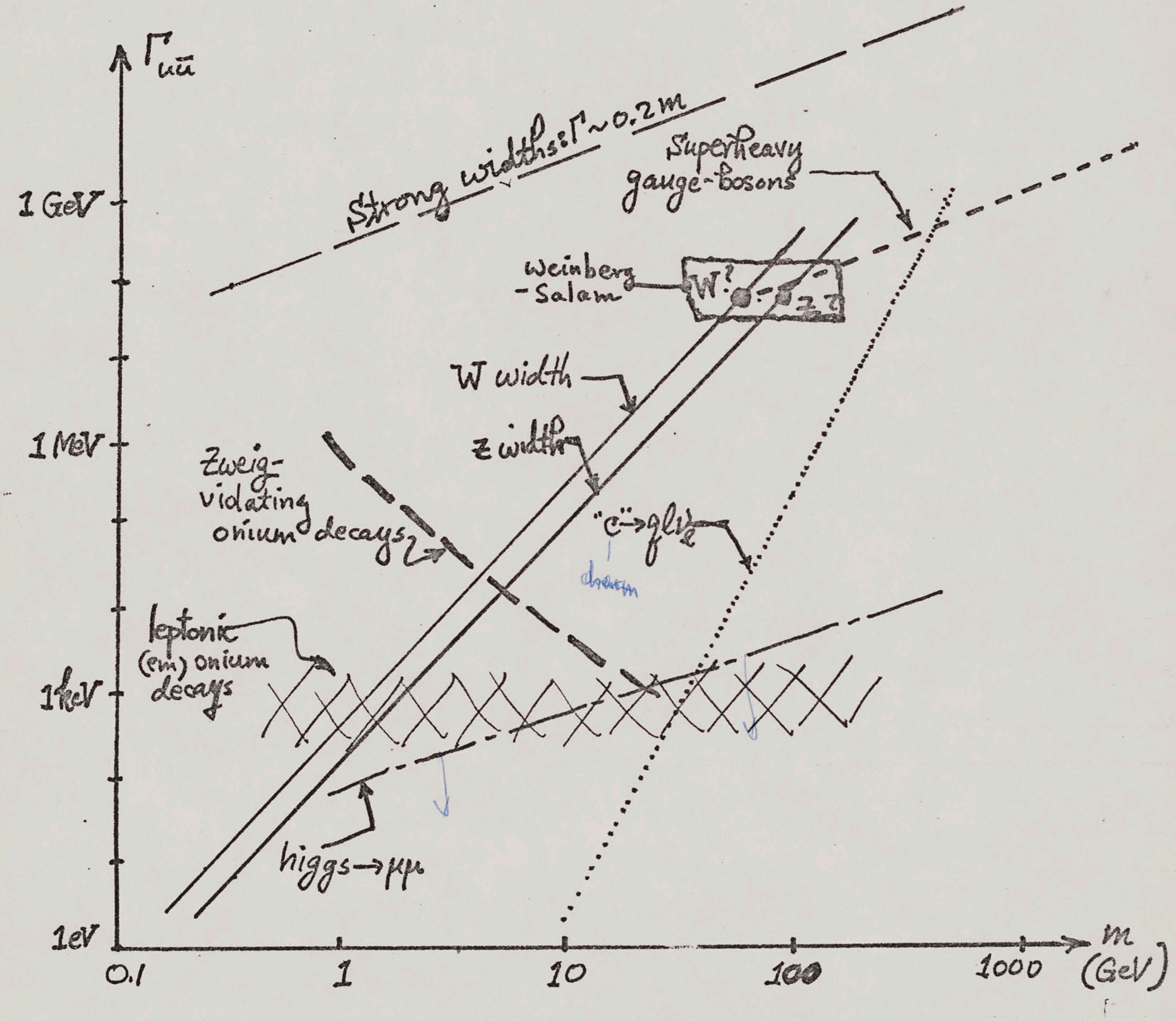


FIGURE 2 — Mass-width space, along with some of the beasts which may populate it.

3. Drell-Yan and parton concepts condition most thinking on weak and electromagnetic phenomena in pp collisions. This is potentially very dangerous:
   - i) A critique of experimental status needed. My view: data a bit high at low mass and low at high mass. But generally pretty good.
   - ii) Are there any alternatives ? ?

Despite the dangers, I'll use Drell-Yan (and scaling) uncritically in what follows.

4. Practicalities and priorities.
   - i) Exploration at high s and lower luminosity vs. development at lower s with higher flexibility and sensitivity. What are variables of overriding importance ? ?
     - Energy
     - Luminosity
     - Incident beams available
     - High (or low) γ_{CMS}: advantage or disadvantage ? ?
     - High precision
   - ii) Even if rate for new phenomena is adequate, are the new phenomena detectable ? ?
     - Acceptance
     - Backgrounds
     - Accidental rates, etc.
     - Branching ratios for new particles into easily detectable modes may be small
   - iii) What are likely detection methods ? Which get easier at higher energies ? ? Some that do:
     - Calorimetry
     - Transition radiation
     - High $\bar{n}$
     - γ, μ, e, detection in sea of π's (especially at high $p_\perp$)

     Some that don't: Flavored hadron searches using two-body hadronic decays
     - Exclusive channels of almost any type; 1c and 4c physics.

5. How do present trends in theory impact ? ?

a)	Weak-electromagnetic gauge theories	very much, obviously
b)	Parton model	very much (scaling laws, Drell-Yan)
c)	Asymptotic freedom; QCD	moderate test of scaling breakdown (ep colliding beams, etc.)
d)	Pomeron physics	moderate Argues for highest s at not all that high a luminosity. Determines length of straight sections in all colliding-beam machines. Argues for importance of nuclear target.
e)	Strings, lattice gauge theories, solitons, bags, homotopy groups, fiber bundles, etc. Quark confinement schemes.	Not yet, but might in the future.

f)	Flavor and color physics (higher strong-interaction symmetries)	moderate But extrapolation to higher masses looks like tough territory. Ideas needed.
g)	Onium spectroscopy (I think this belongs more to Schrœdinger equation than to QCD.)	very much. Could be important at higher masses as well.

V. Projections to higher energy of existing experiments and phenomena

This is mainly laundry lists (available on request for a, b, c).

a) Strong interactions
b) Electromagnetic interactions
c) Weak interactions
d) Standard exotica: tachyons, monopoles, quarks
e) CP violation:
   $p\bar{p} \rightarrow$?; search for asymmetry
   Decays of new heavy states
   Precision studies of e^+e^- processes at high energy
f) Gravitation: out of reach by only 20 orders of magnitude

VI. New phenomena and processes

This is more laundry lists, but shorter, as a consequence of (my) lack of imagination. Additions eagerly solicited:

a) Superheavy fireballs; very high multiplicity
b) Exotic composition (multi-γ, multi-μ, Centauro, etc.)
c) Highly acoplanar high-$p_\perp$ events
d) New thresholds
   Sharp rise in σ_{tot}
   Sharp change in $\bar{n}$, composition or other internal properties
e) New-particle production
   Onium
   New flavors of hadrons
   W's, Z's, etc. (including superheavy, e.g., $W^{++} \rightarrow e^+\mu^+$, etc.)
   Higgs sector
   Pseudo-goldstone bosons
   New leptons {neutral / charged}
   Super-high spin hadrons
   Colored bosons (or colored fermions)
   Leptoquarks
   Diotons
   Glueballs
   Lee-Wick boson

VII. How do specific VBA options apply? A summary.

I took the preceding material, especially the laundry lists compiled under topics 5 and 6, and tried to classify them into two groups; those topics "owned" by a given type of machine and those that are common. The latter category then may need further comparative studies.

a) Areas more or less highly preferred for a given machine.

i) Multi-TeV proton synchrotron
- Nuclear targets and cascading
- Exotic beams (Y^*, Ξ^*, charm?)
- Nuclear effects in electroproduction or ν reactions
- Electron target F_π, F_K, $(\nu W_2)_{\pi, K}$, F_Y, etc.
- Photon target (Primakoff) for exotic beams (e.g. $\gamma \Sigma^- \rightarrow Y^*$)
- 4π visual detectors (bubble, streamer chambers)
- $\nu_\mu e$, ν_e physics, ν oscillations?
- High-$p_\perp$ strong interactions (!!)

Advantages: high luminosity
diversity of beams
CMS motion good for calorimetry

(But this question needs study; pp rings may still have some advantage if very high $p_\perp$ is accessible.)

ii) pp storage rings
- Bread and butter strong interactions — very high rapidity increase is highest priority
- Highest CMS energy

iii) ep rings
- Deep inelastic scaling tests
- Photoproduction at highest energies
- Leptoquark production

iv) e^+e^- rings
- QED tests
- Exploitation of narrow resonances
- Study of sharp thresholds
- Virtual γ target
- $\gamma\gamma$ collisions

b) Major themes common to all machines but approached differently:

i) Search for narrow heavy states

ii) Direct leptons, γ, multileptons, multi γ, as signature for new physics or new particles

iii) Properties of hadron final states in deepinelastic νp, ep, μp, e^+e^-, W, Z decay

iv) Properties of hadron currents coupled to leptons
- Weak charged
  - Old
  - New
- Weak neutral
  - Old
  - New ??
- Weak jets in pp collisions
- Electromagnetic current at high Q^2
  - Spacelike (ep)
  - Timelike (e^+e^-)

v) Decays of new heavy particles as sources of other new particles

vi) New lepton production

Comments on the previous items (i. e., those under subheading VII.b)

i) Particles which decay into $q\bar{q}$ or $\ell\bar{\ell}$ can be produced resonantly in e^+e^- or via Drell-Yan. Figure 2 summarizes known classes of such resonances in Γ, m space. Figure 3 relates quark-antiquark fluxes (luminosities) to the resonance production. For $\mathscr{L} \sim 10^{32}$ (or $\sigma \geq 10^{-36}$), Figs. 4-8 show the accessible values of Γ and m for a given cms energy. For a 10 TeV synchrotron, we let $\sigma \geq 10^{-40}$ and get the region shown on Fig. 8. For e^+e^- rings, I guess the limit on $\Gamma_{\ell\bar{\ell}}$ is of the order reached at SPEAR, more or less independent of energy ($\lesssim$ 100 eV or so). pp and e^+e^- rings look best. Other resonances are perhaps best found (i) as decay products of the previous class, or (ii) singly produced by leptons (ν, μ, e) deep inelastically.

ii) The issues here are luminosity, cms energy available, and expected signal/noise. At best the considerations will be pretty diffuse: not enough experience. Perhaps it can be said that production of heavier flavored hadrons or onium (in pp collisions) does not bode well beyond the mass range 20-30 GeV (Figs. 2 and 9).

iii) The principal figure of merit here is W^2, and the next is ease of studying the hadron final state in detail. Here ep rings clearly seem to be best for deep-inelastic from a proton; e^+e^- likewise is best for jets from $q\bar{q}$ fragmentation (if that concept survives). pp more complex because low-$p_\perp$ ordinary hadrons produce confusion right in the midst of the rapidity space of interesting jets.

iv) Several questions enter here:

a) One is the available range in Q^2 for electromagnetic scattering. For ep or μp (assuming scaling)

$$\sigma(> Q^2) = \int_{Q^2}^{\alpha} dQ'^2 \left(\frac{d\sigma}{dQ'^2}\right) = \frac{4\pi Q^2}{Q^4} \cdot \left[\frac{4}{9} \int_{Q^2/s}^{1} \frac{dx}{x}\, u(x) + \ldots\right]$$

$$\cong \frac{16\pi\alpha^2}{9Q^2} L_{eu}\left(\frac{Q^2}{s}\right) \approx \frac{2.5 \times 10^{-31}}{Q^2} L_{eu}\left(\frac{Q^2}{s}\right)$$

where the lepton-quark luminosity is defined and plotted in Fig. 10, along with quark-quark integral luminosities. Assume for $\mathscr{L} \sim 10^{32}$, that $\sigma \geq 10^{-36}$ cm^2 should be accessible. Thus unless $Q^2 \geq 10^5$, it is S than limits Q^2, not rate.

For given S, $\mathscr{L}$ (and precise measurement of Q^2) become of importance. Interesting comparison:

25 × 400 ep rings
$$\begin{cases} \mathscr{L} = 10^{32} \\ S = 4 \times 10^4 \\ Q^2_{max} \sim 6 \times 10^3 \end{cases}$$

10^6 μ's/sec at 5 TeV into 20m Fe target
$$\begin{cases} \mathscr{L} = 10^{32} \\ S = 10^4 \\ Q^2_{max} \sim 4 \times 10^3\,\mathrm{GeV}^2 \end{cases}$$

Clearly 25 GeV × 10 TeV is a winner
$$\begin{cases} \mathscr{L} = 10^{32}\ ?? \\ S = 10^6\ \mathrm{GeV}^2 \\ Q^2_{max} \sim 10^5\,\mathrm{GeV}^2 \end{cases}$$

b) Weak processes

Much depends upon energy-dependence (cf. Fig. 11). Other than at resonances, elementary cross sections uncomfortably low. Study of energy dependence is

i) Good, clean in e^+e^- (at $\sqrt{s} >> 30$ GeV).
ii) Possible in ep $\rightarrow \nu$ hadrons (at $\sqrt{s} >> 100$ GeV).
iii) Dirty in pp; only easy if no gauge theory cutoff; $p_\perp > 50$ GeV ($\sqrt{s} >> 200$ GeV) needed. Note: with gauge theory, weak jets from ud $\rightarrow$ du always confused with (at least) em jets (uu $\rightarrow$ uu via γ exchange) at all s, Q^2.

Study of different types of currents, (multi leptons, etc., diffractive excitation) argue for conventional ν beams, or for e^+e^-. Cleanliness and freedom from background needed for such questions.

Weak-em interference at VBA energies are maximal (except for μp??). e^+e^- and μp look best (can anything be done with ep?? Looks hard.) But for VBA, should strive for direct weak effects, not by small perturbations.

c) Production of flavored hadrons (containing flavored quarks of mass m_f by leptons

i) Weakly: for $Q^2 > m_f^2$, signal to noise may be better than 10% for single production of a flavored quark. Diffractive production of flavored mesons carrying quantum number of flavored current is smaller but clean in ν reactions. High luminosity a must.

ii) Electromagnetically: For $Q^2 >> 4m_f^2$ not-too-bad arguments exist for ratio of flavor production to background to be as good as in e^+e^-. This argues toward ep rings. Comparison with e^+e^-, however, is needed.

v) This is vitally important especially if there is a Z. If $Z = Z_{Weinberg}$; $m_Z = 80$ GeV; $e^+e^- \rightarrow Z$ at resonance gives, for $\mathscr{L} = 10^{32}$, 30 Z/second. Rare decays of Z are sources of all kinds of wonderful things:

$Z \rightarrow W + (\text{higgs})^+$ if charged Higgs exists and is light

$Z \rightarrow (\text{higgs})^0 + \mu^+ + \mu^-$ (if higgs mass < 40 GeV, $B > 10^{-4}$!)

$Z \rightarrow$ heavy leptons

$Z \rightarrow$ flavored hadrons

$Z \rightarrow$ quark jets

$Z \rightarrow$ new, unexpected objects

vi) Charged leptons best with e^+e^- (up to $m \lesssim 0.4\sqrt{s}$); νN, μN, eN may be best for neutral heavy leptons. Up to unknown factors, $\sigma \sim \sigma_{weak}$ for $m^2_{lepton}/s \lesssim 0.1$. From Figs. 10-11, ep (certainly νp) OK up to $m_{lepton} \lesssim 0.3\sqrt{s}$.

Searches in debris of Z-decay also good.

pp seems less encouraging.

VIII. Questions needing detailed work.

1. Better cross-section estimates for many of preceding items.
2. Critique of Drell-Yan.
3. Study of high-$p_\perp$ hadron jet production; does it mess up weak-em physics at VBA energies??
4. Specifics of W and Z physics a la Weinberg model + Drell-Yan + partons.

Personal Judgments:

1. Weak interactions will be dominant theme in the next energy regime:
   i) Dynamics: how low energy structure is modified at high energy.
   ii) Symmetries: new currents
   symmetries of couplings
   What is underlying group?

2. A very powerful tool in studying symmetries would be study of decays of W's, Z's, new-flavored hadrons, etc., etc. (if they indeed exist).

3. Success of gauge theories for neutral currents encourages hope that 65 GeV W and 80 GeV Z do exist.

4. Major surprises even in strong-interaction dynamics are not out of the question, based on cosmic-ray hints. (For this, high luminosity is not so crucial, but high energy is.)

5. A push to very highest s via pp storage rings in order to rough out the territory should be very high priority. If VBA = 10-20 TeV synchrotron; should be built with SSR, electron, and ISR-type options if at all possible.

6. If W's, Z's, etc. can be found with pp storage rings, it may be most efficient to find them and then design e^+e^- system to exploit them. Z-factory is great. But should be optimized for Z-physics, if a Z or Z's indeed exist.

7. ep systems probably interpolate in physics interest between pp and e^+e^-; they may for that reason have somewhat less priority.

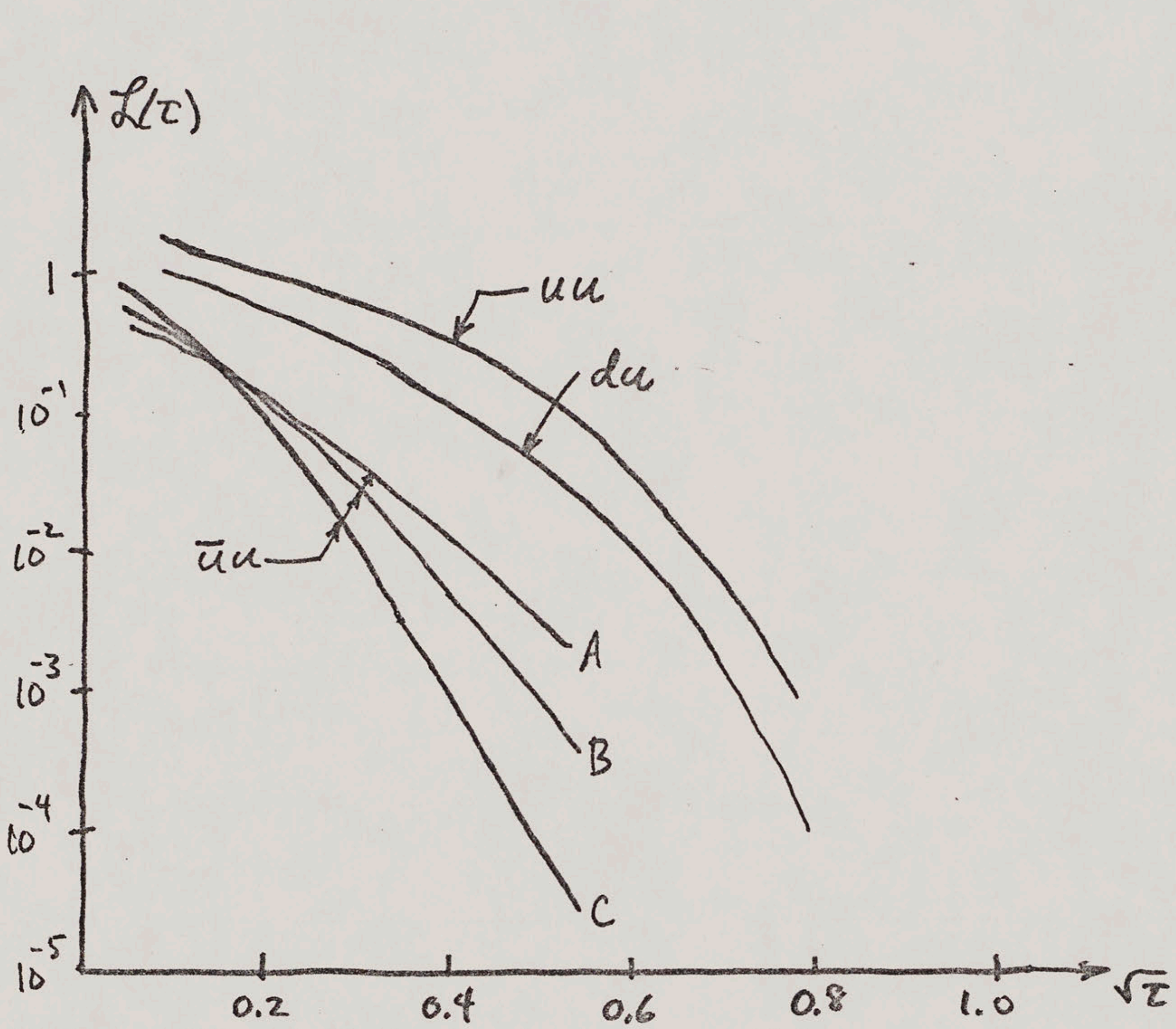


FIGURE 3 — Differential luminosity of partons in pp collisions:

$$\mathscr{L}_{u\bar{u}}(\tau) = \int \frac{dx_1}{x_1}\frac{dx_2}{x_2}\left[u(x_1)\,\bar{u}\,(x_2) + u(x_2)\,\bar{u}\,(x_1)\right]\delta\,(x_1 x_2 - \tau)$$

For resonance of mass m, $\tau = m^2/s$. No factor 3 for color included.

Instructions for use:

$$\sigma_{pp}(m^2, s)\,B_f \cong \frac{12\pi^2\Gamma}{m^3}\left(\frac{2J+1}{3}\right)B_f\left[B_{u\bar{u}}\;\mathscr{L}_{u\bar{u}}(\tau) + \;\ldots\right]$$

with J = spin of resonance, B_f = branching ratio into observable final states,

$B_{u\bar{u}}$ = branching ratio into $u\bar{u}$, and Γ = total width.

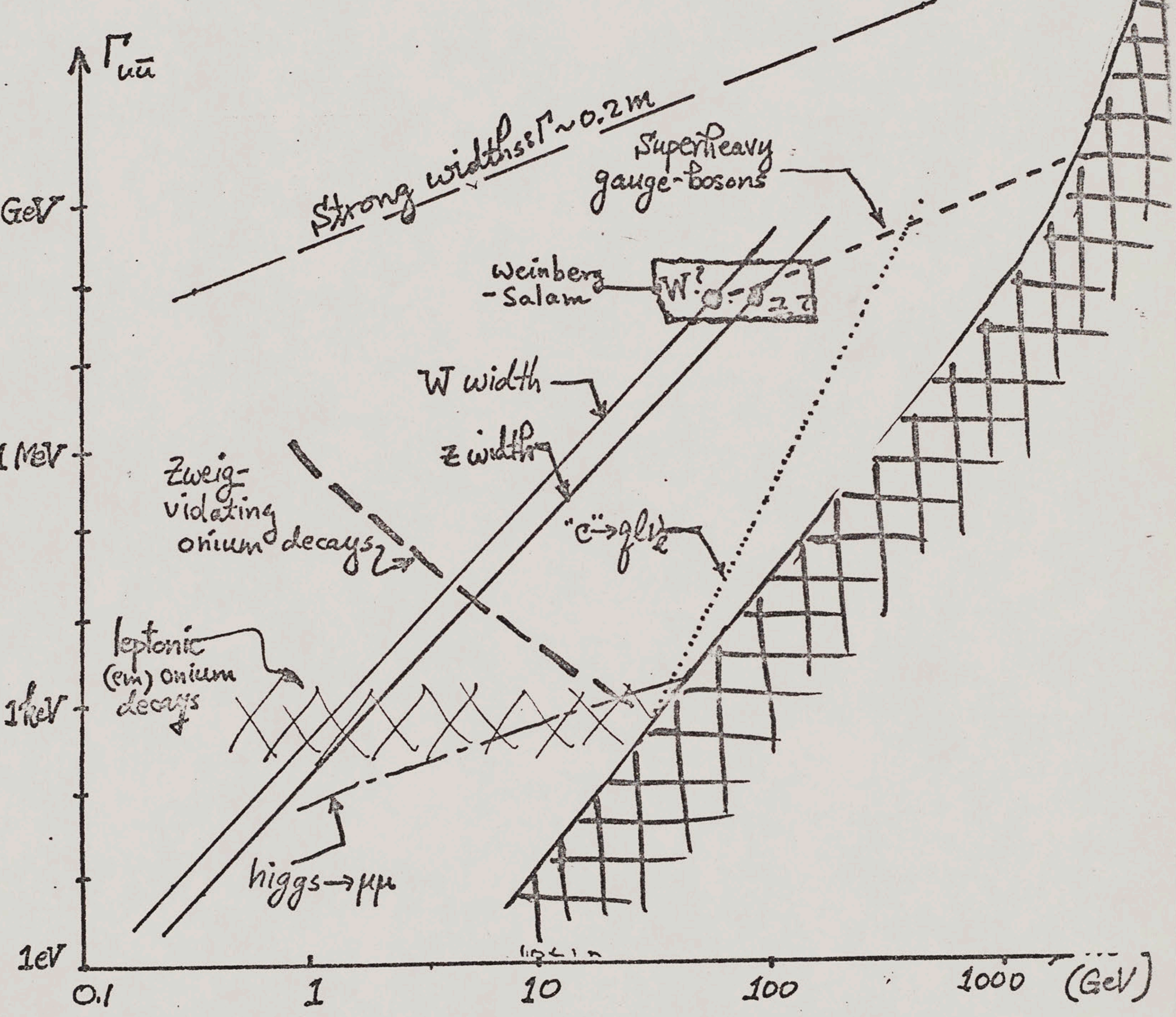


FIGURE 4 — Masses and partial widths (into $u\bar{u}$ quarks) accessible to 10 TeV + 10 TeV pp colliding beams if

Drell-Yan OK

$\sigma \geq 10^{-36}\mathrm{cm}^2$ ($\geq$ 10 events/day at $\mathscr{L} = 10^{32}\mathrm{cm}^{-2}\mathrm{sec}^{-1}$)

All final states observable

100% acceptance and detection efficiency

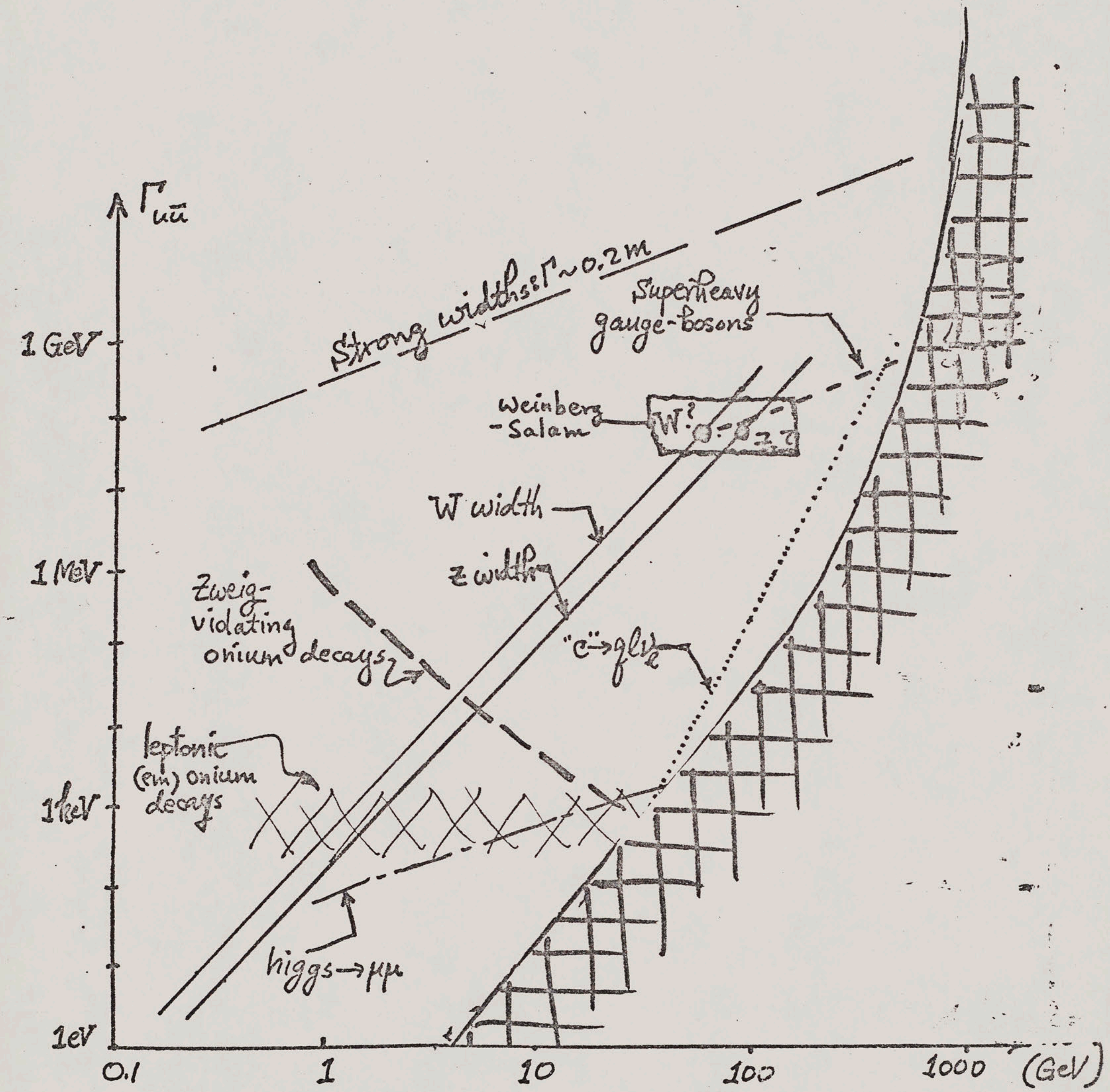


FIGURE 5 — As in Fig. 4, but for POPAE (1 TeV + 1 TeV pp).

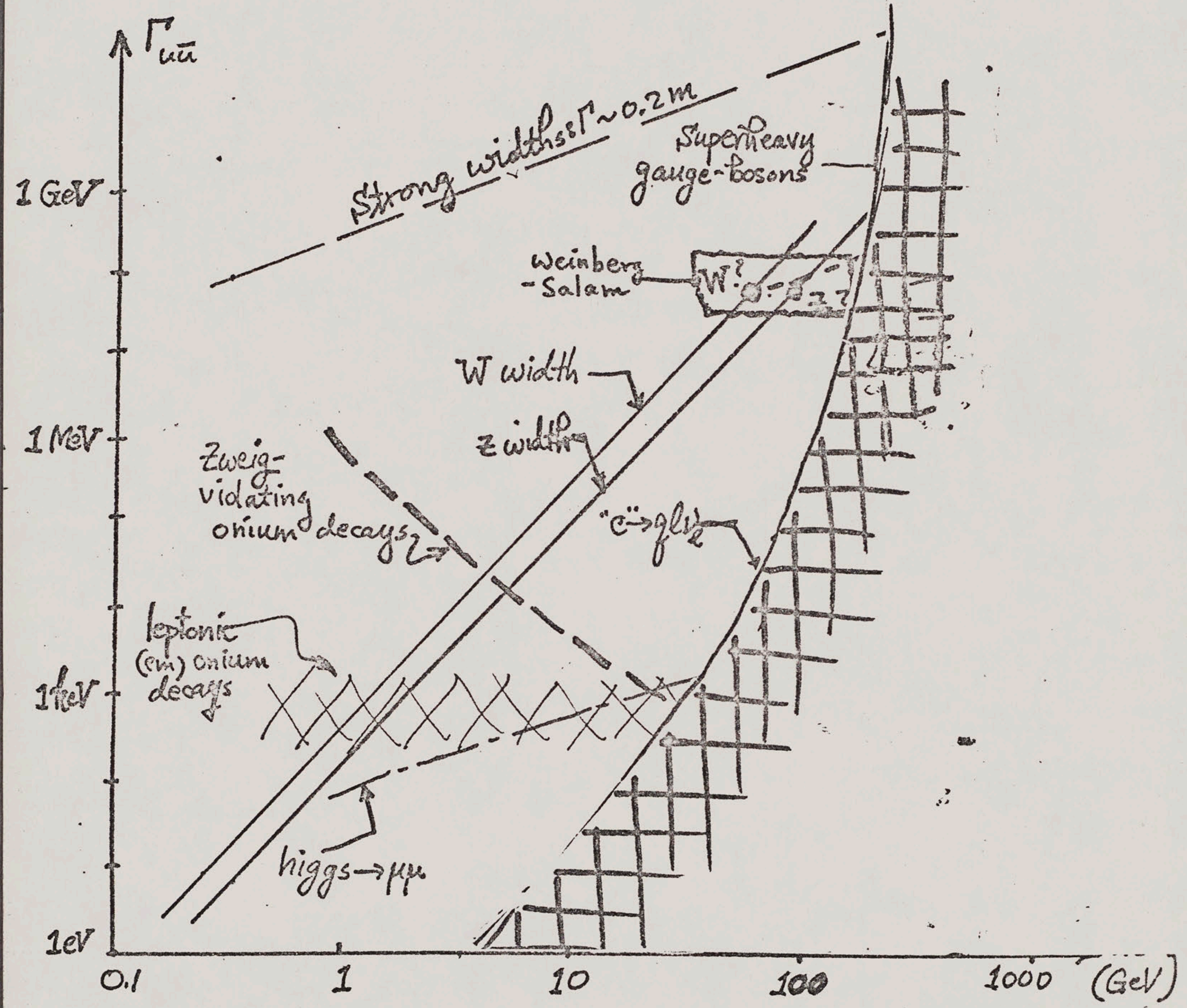


FIGURE 6 — As in Fig. 4, but for ISABELLE (200 GeV + 200 GeV pp).

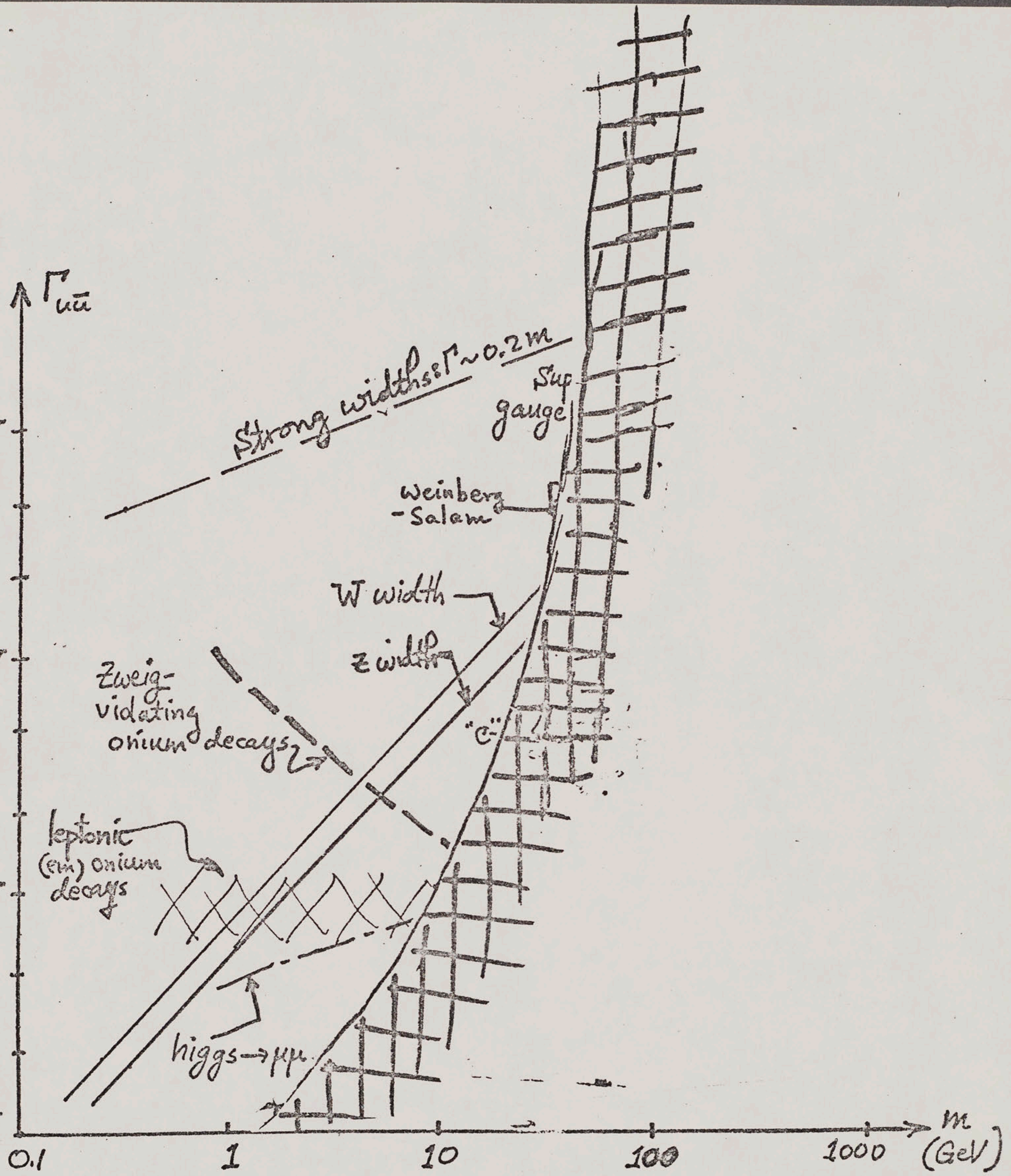


FIGURE 7 — As in Fig. 4, but for ISR ($\sigma \geq 10^{-35} cm^2$; $\mathscr{L} = 10^{31}$ assumed).

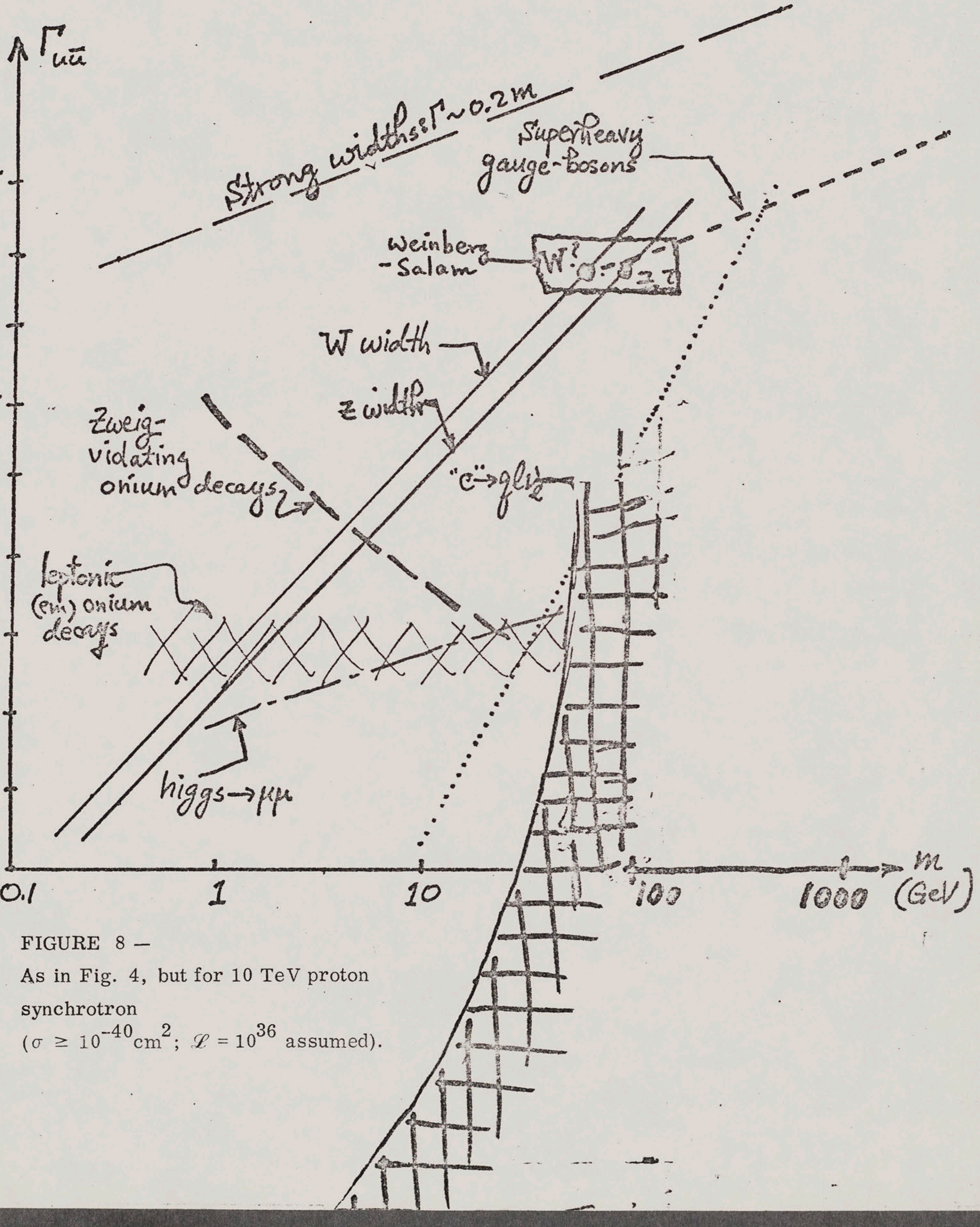


FIGURE 8 —
As in Fig. 4, but for 10 TeV proton synchrotron
($\sigma \geq 10^{-40} cm^2$; $\mathscr{L} = 10^{36}$ assumed).

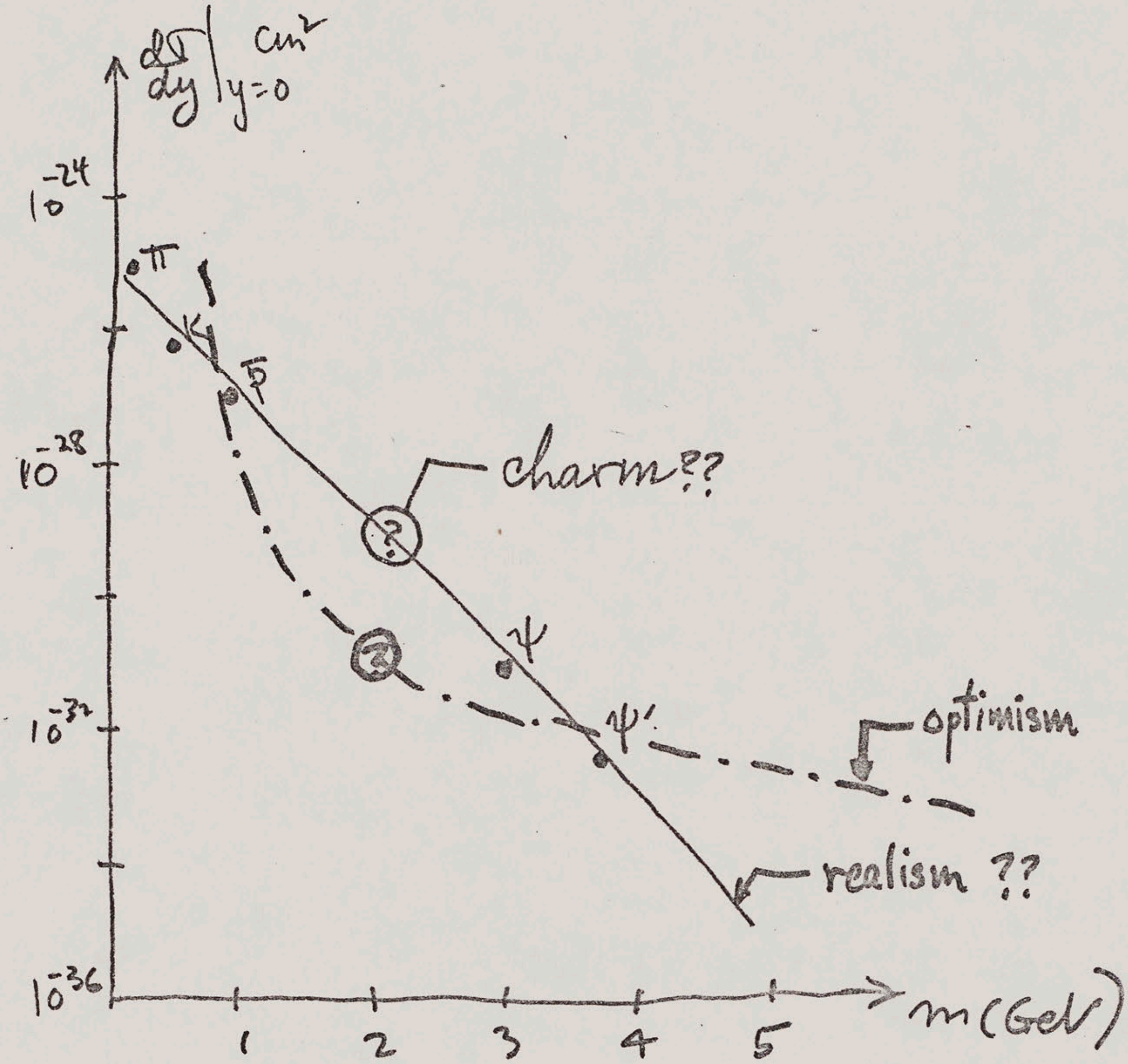


FIGURE 9 — Cross section (at highest energy) vs. mass for production of various hadrons.

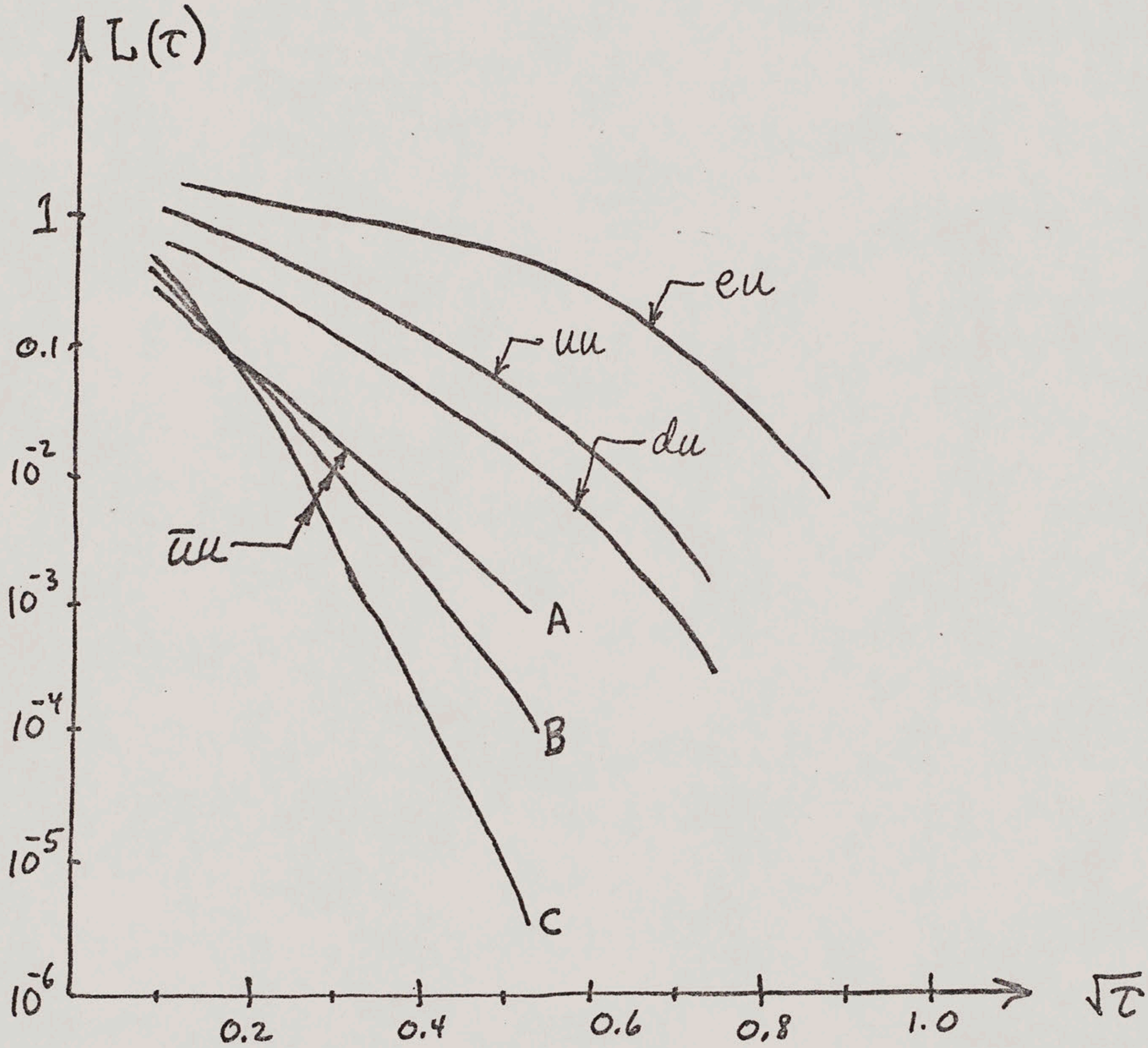


FIGURE 10 — Integral luminosity of parton-parton collisions. Instructions for use: Let $\sqrt{\tau}$ = fraction of total cms energy $\sqrt{s}$ in hard collision of partons. Let $\sigma_0(\tau s)$ be elementary cross section for this hard collision. Then to good accuracy $\sigma_{pp}(s, \tau)$ (or $\sigma_{ep}(s, \tau)$) $\equiv$ total cross section for hard collisions having subenergy $\gtrsim \sqrt{\tau s} \approx L(\tau)\,\sigma_0(\tau s)$.

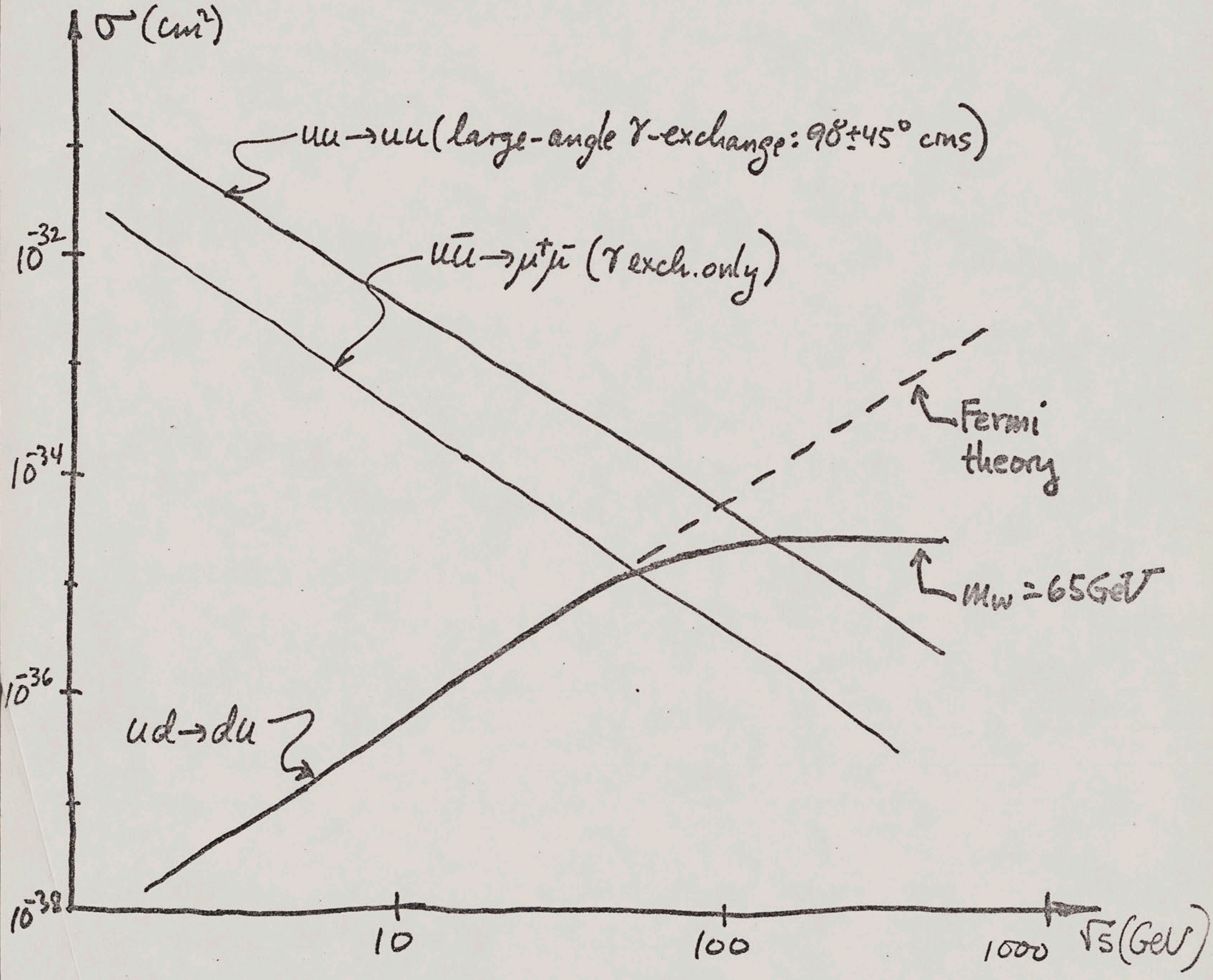


FIGURE 11 — Elementary weak and electromagnetic quark-quark cross sections. Corresponding lepton cross sections (for leptons replacing quarks) very similar.

Report by L. Lederman and V.F. Weisskopf

on the ICFA Discussions in Hamburg

29 August 1977

(Written 2 September 1977 without knowledge of what happened in the meeting of the IUPAP Committee on Particles and Fields held on August 30th)

Persons present

B. Gregory	Chairman
A. Rousset	Secretary
J. Adams	CERN
G. Von Dardel	Sweden
W. Paul	Germany
L. Lederman	USA
R.R. Wilson	"
V.F. Weisskopf	"
K. Lanius	JINR
V.P. Dzhelepov	USSR + JINR
V. Yarba	USSR
K. Myznikov	USSR
Y. Yamaguchi	Japan

First item of action was the selection of a chairman. V.F.W. proposed Gregory. It was unanimously accepted. He asked to be allowed to have A. Rousset as helper and secretary. It was accepted.

After a few tentative attempts by Gregory to formulate what we are supposed to do, it was accepted to use the recommendations of the Serpukhov meeting of May 1976 as a basis. They are :

1) Efforts should be made to co-ordinate the design and construction of new regional facilities. Consultations and exchange of experiences should be encouraged in order to optimize the diversity of facilities and to enhance the efficiency of construction and operation. The study group also recommends joint studies of new technology (e.g., superconductivity, new detectors and other experimental apparatus) and joint design and/or construction of components of regional projects.

2) Joint utilization of regional facilities by scientists of different regions should be organized on the basis of present and future arrangements or agreements. The general availability of regional installations is essential to enable scientists of different regions to take advantage of facilities with complementary research potentialities.

3) International collaboration should provide for studies leading towards the realization of a next generation of super-high energy facilities, following the regional projects referred to above. It is expected that these facilities will be so large that their realization will be possible only by pooling the resources of all regions concerned into common international projects.

Creation of a super-high energy accelerator complex (VBA) involves especially complicated scientific, technical and organizational problems. These will require several years of continuing studies and discussions. The study group recommends that these discussions begin in the near future leading to the start of the design of the VBA in about 10 years.

4) In view of the need for these extensions of international collaboration, the study group suggests to the IUPAP Division of Particles and Fields to initiate these activities in an appropriate form, for example, by appointing a sub-committee for the purpose of organizing working groups and future meetings such as the present one.

The assembled group considered itself as the sub-committee referred to in point 4).

Then the content of a Table describing planned regional facilities compiled at the May 1976 meeting was discussed and brought up to date. A copy of the new Table is enclosed.

Some discussion was devoted to a better definition of the scopes of regional projects versus projects suitable for international construction. J. Adams introduced a useful criteria by stating that the presently planned regional projects are dealing with tunnels of roughly 3 ms radius, where, e.g., larger projects, such as > 10 TeV fixed target accelerators or e^+e^- colliding beam facilities of $\gtrsim$ 100 GeV, would require tunnels of the order of 10 km. It therefore would be logical to think of an inter-regional laboratory having a multi-purpose tunnel of ~10 km radius. This consideration helped to separate the two types of projects, although it is not quite clear in what category a L.E.P. of ~70 GeV belongs.

Proposals were made to accomplish the tasks set in points 1), 2), 3). After a lengthy discussion it was proposed to set up two study groups A and B. The group A should deal with the problems 1) and 2) ; the group B with the problems of 3). One of the main problems of 1) and 2) was the extent in which the West should and could help the USSR in constructing the UNK complex. This problem and also the problems of collaboration on other planned regional facilities was discussed but no definite agreement obtained. It was proposed to ask the three main regions to deliver to Gregory within two months a possible agenda of topics to deal with by study group A and B. The two groups are different in character. It was expected that the members of the study group A should be experts nominated by the lab. directors whereas the members of group B should be more of senior character, to deal with problems of organization and principle.

Remarks

1. The ICFA committee met before it was officially approved by the IUPAP commission. Its recommendations (even its existence) could have been challenged in the subsequent meeting of the commission. In fact, it was accepted with one vote against.

2. It seems clear that the two "camps" are pursuing rather different aims. The USSR camp seems to be interested to get technical help and moral support for the UNK complex. We believe they are aware of the difficulties of reaching the aim of 3 TeV and successful colliding systems without intensive help from Western experts. They do not seem concerned about monetary support, but about design, planning and prototype production, in particular in respect to super-conducting magnets. The USSR is not much interested in the 10 km projects at this time. They are afraid of discouraging their government when telling about the next step.

The West, on the other hand, is not very interested in the success of UNK and is not enthusiastic of devoting much valuable manpower to an effective help of UNK. It is much more interested in getting on with the planning of future steps, in particular with the possibility of a large e^+e^- facility on a regional or interregional basis.

3. We suggest that a meeting should be organized this fall in order to discuss the U.S. attitude to these problems. We believe that this meeting should be attended by representatives from the office of Frank Press, by Wollenmayer and Bardon, by the lab. directors, by S. Drell and by some representatives of the A.P.S. - Division of Particles and Fields, and perhaps also by the U.S. representatives in the IUPAP Commission for Particles and Fields.

for Leon Lederman
signed by VFW

Léon Lederman

Victor F. Weisskopf

Victor F. Weisskopf

PRESENT STATUS OF MACHINES

MACHINES	PARTICLES (ENERGY)	END OF STUDIES	DECISION TIME	OPERATION TIME
TRISTAN (I)	p (70) e (17)			
LEP	e^+e^-(70-100)	78		
"SPS"	p^- (270) e^- (25)	78	77 - 78	
PETRA "PETRA"	e^+e^- (19) p (200)	75	75	78
PEP	e^+e^- (19)	75	75	79
ISA	pp(200×200) (400×400)	77 78	77 - 78	84
TEVATRON	p (1000) p^- (1000) pp(250×1000)	77 77 78	77 - 80 77 - 80 77 - 80	79 - 80 80 - 81 80 - 81
VEPP	e^+e^- (4.5→7)	74	74	79
UNK	p (70→200→3000)	79 - 80	80 - 81	

Report of the

International Study Group

on Future Accelerators and High Energy Physics

Serpukhov, May 17-25 1976

Abstract

The Seminar "Perspectives in High Energy Physics" held in New Orleans, March 1975, established a Study Group to discuss the long-range requirements for facilities in High Energy Physics. A sub-group met in CERN, October 1975, and planned an Agenda for a meeting which was held in Serpukhov, U.S.S.R. in May, 1976. In this paper a summary of the work done in Serpukhov is given.

It begins with a review of the status of our present knowledge of the fundamental structure of matter and a statement of those future problems which can be clearly identified now and which will require new facilities for their solution. This is followed by a brief description of the status of today's accelerator technology and a review of projects that are now under active study as regional facilities. The study group has noted the need for close collaboration during the selection of the range of new regional facilities to ensure coverage of the broadest possible program of research. Included in this range may be a proton fixed target accelerator of up to several TeV, colliding beam facilities with a center-of-mass energy of up to several TeV for protons against protons, up to several hundred GeV for electrons against protons, and up to about 200 GeV for electrons against positrons. The participants have emphasized the importance of joint utilization of all such facilities by scientists of different countries.

The Study Group has stressed that the further progress of High Energy Physics will require in the future the development of an accelerator complex significantly more powerful than those planned for regional facilities. This complex is likely to be of such a cost as to be beyond the capabilities of any single region. Examples include facilities such as a proton accelerator of energy higher than 10 TeV and an electron-positron colliding beam facility of more than 200 GeV in the center-of-mass. In this connection several conceptual designs of that kind were presented and discussed.

In seeking to attain the more intensive international collaboration which is a fundamental prerequisite for progress toward the stated objectives, the Study Group recommends that the International Union of Pure and Applied Physics (Particles and Fields Division) be asked to initiate appropriate activities to this end.

I. Introduction

The historical development of science has made it especially appropriate that the physicists of all countries which are active in the exploration of the deepest aspects of atomic nature should be collaborating so intensely. It is gratifying that this collaboration has resulted in so much progress in our knowledge about the particles of which the world is made and of the laws that govern their behavior. It is equally gratifying that governments have provided the necessary framework within which the collaboration could take place. The fundamental knowledge being developed will become the basis of future technology and, equally important, will provide mankind with a greater insight into the nature of the universe.

The struggle for this knowledge is difficult, and although many concepts of nature have been deepened and new concepts have emerged, nevertheless, it is anticipated that vastly more extensive investigations will be required before our knowledge of the basic particles is as firm as is our understanding, for example, of electromagnetism.

The tools for investigating matter have become more complex and more expensive as we have penetrated deeper into the inner space of the atom. For this reason organizational collaborations have developed between groups of nations to allow them to participate in this exciting and necessary development. Thus the member nations of CERN and the member nations of JINR have established organizations which have enabled them to successfully develop

research in this field. Most importantly, the close collaboration between the regional laboratories has amplified their individual efforts.

As facilities that are now being planned on a regional basis are developed, ways should be found to help in coordinating that planning. Such mutual discussion and advice would ensure the coverage of the broadest possible program of research. Joint studies of new technology and organization of wider collaborative use of present facilities should occur. Joint construction of sub-elements of regional projects should be explored.

It can already be expected that the facilities needed to explore and clarify the next level beyond that available to facilities presently being contemplated will be so large that their realization will be greatly optimized -- and may only be possible -- by the pooling of the resources of all regions in a common effort.

We underline the statement of the countries participating in the "Helsinki Agreement on Security and Cooperation in Europe", which specifically mentions high-energy physics as a field for cooperation. It says that "scientific and technological cooperation constitutes an important contribution to the strengthening of security and cooperation among (the countries) in that it assists the effective solution of problems of common interest and the improvements of the conditions of human life".

V. Conclusions

The foregoing survey leads us to the following conclusions:

A) The present status of the science of the structure of matter poses fundamental problems which require a new generation of facilities of the types listed in Table I. Such facilities are within the capabilities of the individual regions and are needed for continued progress of this field of research.

B) The success of regional and interregional collaboration in the past provides a good basis for extending and strengthening this collaboration in the new generation of regional facilities.

C) Looking beyond this new generation of regional accelerators we foresee the need for an accelerator complex (VBA) which will require international collaboration of all regions concerned.

VI. Recommendations

1) Efforts should be made to coordinate the design and construction of new regional facilities. Consultations and exchange of experiences should be encouraged in order to optimize the diversity of facilities and to enhance the efficiency of construction and operation. The Study Group also recommends joint studies of new technology (e.g. superconductivity, new detectors and other experimental apparatus) and joint design and/or construction of components of regional projects.

2) Joint utilization of regional facilities by scientists of different regions should be organized on the basis of present and future arrangements or agreements. The general availability of regional installations is essential to enable scientists of different regions to take advantage of facilities with complementary research poten-

tialities.

3) International collaboration should provide for studies leading towards the realization of a next generation of super-high energy facilities, following the regional projects referred to above (examples are given in Table II). It is expected that these facilities will be so large that their realization will be possible only by pooling the resources of all regions concerned into common international projects.

Creation of a super-high energy accelerator complex (VBA) involves especially complicated scientific, technical and organizational problems. These will require several years of continuing studies and discussions. The Study Group recommends that these discussions begin in the near future leading to the start of the design of the VBA in about 10 years.

4) In view of the need for these extensions of international collaboration, the Study Group suggests to the IUPAP Division of Particles and Fields to initiate these activities in an appropriate form, for example, by appointing a sub-committee for the purpose of organizing working groups and future meetings such as the present one.

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* The delegation from the CERN Member States was selected by the CERN Scientific Policy Committee and was under the leadership of the Chairman of the European Committee for Future Accelerators.

AGENDA

17 May Morning Session — Chairman: V. Weisskopf

Topic I: Status of national and regional facilities.

1) PETRA, PEP
   Speaker: G. Voss
2) VEPP-4
   Speaker: A. Skrinsky
3) Energy Doubler
   Speaker: R. Wilson

Afternoon Session — Chairman: G. von Dardel

Topic II: Presentation of scientific and technical aspects of big accelerators.

1) POPAE
   Speaker: R. Diebold
2) ISABELLE
   Speaker: M. Barton
3) LSR-pp
   Speaker: K. Johnsen

18 May Morning Session — Chairman: A.A. Logunov

4) LSR-ep
   Speaker: K. Johnsen
5) UNK
   Speaker: V. Yarba
6) Colliding $p\bar{p}$ - rings
   Speaker: A. Budker
7) TRISTAN
   Speaker: Y. Yamaguchi

Afternoon Session — Chairman: K. Lanius

Topic III: Presentation of general scientific and technical aspects in the construction and utilization of high-energy systems.

1) 10 TeV proton accelerator with a fixed target
   Speakers: D.B. Thomas, R. Wilson
2) 100x100 GeV electron storage ring
   Speaker: K. Johnsen

19 May Morning Session — Chairman: Y. Yamaguchi

Topic IV: Physics Projections

1) Theoretical Considerations
   Speakers: M.A. Markov
   J. Bjorken

Afternoon Session — Chairman: L. Lederman

2) Physics to 1980 - Existing Facilities
   Speakers: L. Lederman - FNAL pp
   A. Rousset - SPS, ν and μ
   U. Amaldi - ISR

3) Physics to 1985 - Next Generation of Regional Accelerators
   Speakers: U. Amaldi - LSR
   Y. Prokoshkin - UNK

20 May Morning

Visit to the IHEP Laboratories

Afternoon Session — Chairman: U. Amaldi

Continuation of previous session
   Speakers: S. Gerstein - UNK
   G. von Dardel - PETRA

Topic V: Physics Beyond 1985: VBA
   Speakers: A. Rousset - ν at 10 TeV
   G. von Dardel - hadrons at 10 TeV

Topic VI: Experimental techniques Beyond 1985
   Speaker: R. Diebold

21 May Morning Session — Chairman: L. Soloviev

Topic VII: Concluding Discussions

1) Review of situation - V. Weisskopf

2) General discussion

Afternoon Session — Chairman: V. Djelepov

3) General discussion

24/25 May

Preparation of Final Report

Appendix 3

List of papers submitted at meeting:

From CERN Member States:

VBA/CMS/1	W. Willis, "Summary of the 1976 CERN Study on the Use of a 10 TeV Proton Accelerator and of Electron-proton Colliding Beams", CERN-SD Note No. 1.
VBA/CMS/2	W. Willis, "Future Trends in Detectors for Multi-TeV Accelerators", CERN-SD Note No. 2.
VBA/CMS/3	G. Charpak, "Some Considerations on the Future of Proportional Chambers", CERN-SD Note No. 3.
VBA/CMS/4	U. Amaldi and L. Di Lella, "Physics at the CERN LSR", CERN-SD Note No. 4.
VBA/CMS/5	K. Johnsen, "Studies of New Large Storage Rings at CERN: pp, $\bar{p}p$ and ep", CERN-SD Note No. 5.
VBA/CMS/6	R. Billinge, "VBA Fixed Target Parameter List".
VBA/CMS/7	U. Amaldi and H. Lengeler, "Collinear Accelerators for High Energy e^+e^- Collisions", CERN-SD Note No. 7.
VBA/CMS/8	G. von Dardel, "Hadronic Physics at a 10 TeV Fixed Target Machine".
VBA/CMS/9	G. von Dardel, "The PETRA Physics Program".
VBA/CMS/10	D.B. Thomas, "Superconducting Magnets for a 5 to 10 TeV Proton Synchrotron".
VBA/CMS/11	"LEP Parameter List", Version 1, compiled by E. Keil.
VBA/CMS/12	"Parameters for Superconducting LSR", Version 1, edited by K. Johnsen, CERN/ISR-LTD/75-39.
VBA/CMS/13	M.G.N. Hine, "International Data Communications for European High Energy Physicists - and others".

From USA:

Mark Barton/W.B. Sampson. "Impact of A-15 Superconductors on Future Machines"

"A Proposal for Construction of a Proton-Proton Storage Accelerator Facility - ISABELLE 1976 (revised)

"A 1000 GeV on 1000 GeV Proton-Proton Colliding Beam Facility", (POPAE).

R. Wilson. "A Ten TeV World Accelerator," May 1976.

J. Bjorken. "Physics Issues and the VBA," May 1976.

<u>From USSR:</u>

"Accelerating-Storage Complex on the Basis of the IHEP Accelerator (UNK, 2-5 TeV)".

A. Budker, "Electron Cooling of Antiproton for UNK".

II. Physics Projections

The development of high energy physics in the last two decades has led to a situation where there exist many facts, synthesized by theoretical ideas. These ideas have not yet reached a fundamental character similar to theories of electromagnetism and gravitation. Nevertheless, the present knowledge makes it possible to formulate long-standing fundamental questions of physics in rather detailed form. This makes it most probable that the discoveries made by the next generation of accelerators should provide us with new fundamental knowledge, first of all about the nature of weak interactions and their possible connection with electromagnetic interactions and also about the interior structure of hadrons and the range of validity of the quark hypothesis. Some of the most important unanswered questions are these:

> Do quarks exist and, if so, how are they confined in hadrons, and what are the forces between them? The recent results about hadron collision products which possess high transverse momentum have shown how little we understand about the internal dynamics of hadrons.

Secondly,

> Is the Weinberg-Salam gauge theory of weak interaction pointing towards the real solution or is it the wrong approach? The quantitative agreement of neutral current data with theory is strong encouragement for gauge-theories. Nevertheless, no deviations from a four-fermion structure of the weak force have yet been observed.

We believe that the energies of the planned regional facilities are indeed sufficient to begin attacking these problems. In the case of weak interactions there are definite energy ranges where we expect new phenomena to occur: At about 1000 GeV (center-of-mass) the simple four-fermion theory breaks down. It is vital to reach this energy in order to fully observe the structure of the weak force in its natural domain. The gauge theories suggest that there are new phenomena, such as intermediate bosons, already at about 100 GeV. This situation is analogous to what happened in the 1930's in electrodynamics: The natural limit was the classical electron radius (10^{-13}cm) corresponding to 100 MeV whereas new phenomena (pair creation) occur already at 1 MeV.

Our present knowledge of strong interactions does not indicate yet any definite critical energy range. The higher the energy, the more information we will get. We need to know whether further quantum numbers exist, such as charm, flavor, color etc., and at what energies they will appear. Some cosmic ray observations indicate that there are unexpected phenomena occurring at about 300-500 GeV (center-of-mass) which may point to new directions in strong interaction dynamics.

Also in the weak interactions the number of entities is still unknown. There may be a whole series of intermediate bosons, there may be Higgs-bosons of different kinds and a series of heavy leptons and neutrinos. The appearance of these seemingly unlimited number of entities of a given type, even in weak interactions, is reminiscent of the discoveries of elements in the 19th century. We have a few organizing principles, analogous to Mendeleev's classification. But the need for further synthesis is clear. We have much more to discover about the behavior of matter at energies higher than those available today.

The accelerators and storage rings which have been proposed address these problems in different ways:

1. Proton-proton and proton-antiproton storage rings attain the highest practicable center-of-mass energies at the price of lower luminosity. But the luminosities appear adequate for finding the weak-interaction intermediate bosons, provided the Drell-Yan production model can be applied. Present data are of some support for this model but far from conclusive.

The high center-of-mass energy available in storage rings is also of special significance in the study of strong interactions. The nature of the increase in the total cross-sections and of the energy-dependence of particle production mechanisms will be probed in a significant way. These facilities are also very useful to study the production of hadrons at high transverse momentum.

2. Future conventional proton synchrotrons, which provide high-energy particle beams incident upon stationary targets, will most likely explore frontiers different from that of center-of-mass energy. Their importance lies in the much higher luminosity available, in the diversity of external beams available, (including $\mu, \nu, e, \gamma, \pi, K, \bar{p}, \Lambda, \Sigma, \Xi, \Omega$), and in the opportunity of using targets of various atomic nuclei in order to study the nature of the produced systems in "status nascendi". High luminosity and choice of hadron beams are properties of

special significance in studying the production of hadrons of high p_T. The lepton-beams, especially the neutrino beams, are expected to continue to play the important role that they presently do in exploration of weak and electromagnetic interactions.

3. Electron-positron colliding beams at energies beyond PEP and PETRA allow the clean study of not only quantum electrodynamics and electromagnetic production of hadrons, but of weak interactions as well. Also, <u>any</u> charged heavy leptons or other charged non-hadronic pairs (including possible intermediate bosons $W^{\pm}$) would be produced, at a measurable rate, if they exist. Such storage rings are extremely powerful tools for finding heavy resonances with an appreciable partial width into an electron-positron pair. As already exemplified by the J/ψ and ψ', the decays of such resonances provide detailed, clean information, difficult to obtain by other means. For example, the Weinberg-Salam theory predicts the production of a neutral boson $Z°$, with mass ≈ 80 GeV, (at luminosity ~10^{32} $cm^{-2}sec^{-1}$) at a rate exceeding 10 per second. Thus e^+e^- rings of such energy may be an excellent way to study weak interactions. This may be the only method (or at least the best) to find and study Higgs bosons predicted by weak-electromagnetic gauge theories. If the mass of such a particle is less than 40 GeV, the branching ratio of $Z°$ into it (plus a charged lepton pair) is estimated to exceed 10^{-4}.

4. Electron-proton rings allow the clean study of the behavior of strong-interactions at short distances. The present theoretical ideas of the weakening of strong interactions at small distances, and their growing at large ones (asymptotic freedom), as well as the ideas of point constituents of the proton, are best tested in electron-proton scattering at the energies attainable by these storage rings. The question of the nature of proton constituents, and how (or whether) they are confined may be elucidated by study of the way hadrons are emitted after such a constituent is struck by the incident electron. The e-p storage rings may be a good way to produce and study heavy leptons (especially neutral), if they exist. Finally, weak interactions of the electron with hadrons are accessible as well, and such information would be a valuable supplement to what is obtained by other means.

In summary, it is expected that the planned regional facilities will lead to the solution of many outstanding problems and to new important discoveries. For example, when the center-of-mass energy of a few hundred GeV is reached, it is most probable that the existence or non-existence of the intermediate boson will be known. We then will know much more about weak interactions and their connection with other forces. Moreover, the range of understanding of strong interactions will be considerably widened, and the internal structure of nucleons will be much better known. It is possible that free quarks or new unexpected particles may be produced. Some of the larger regional projects may even yield information regarding the region of 400-500 GeV in the center-of-mass, where there are indications from cosmic ray data of new phenomena.

In spite of the importance of the energy regions explored by the regional plans, the need of higher energies and more varied beams will remain. After all the energies necessary to get into the interesting regions are attainable only by colliding beams of protons or electrons, and their antiparticles; they need to be supplemented by beams of other particles and by beams of higher intensity. The ISR had to be supplemented by stationary target machines with comparable (though smaller) center-of-mass energy in order to experiment with particles other than protons at those energies.

We definitely expect that the regional facilities will make important discoveries in the next 15 years and that some of the problems will be solved. But it is probable that a good part will still remain unsolved. We therefore strongly believe that so-called VBA facilities will be needed such as a proton accelerator with $E > 10$ TeV and with the possibility of p-p colliding beams, and/or e^+e^- colliding beam facility of $E_{cms} >$ 200 GeV.

III. Instrumentation Projections

While the experimental exploitation of a very high energy accelerator will in general require more sophisticated techniques, many experiments can use straightforward extensions of present methods. The initial exploratory experiments may well be less complicated than those which will be in progress at the lower-energy regional laboratories.

An active and vigorous experimental program could be carried out with present techniques, but improvements may be anticipated in many areas, such as

a) electronics -- integrated circuits will drastically lower the cost of multiwire proportional chambers and drift chambers. Drift chambers are already capable of good precision, $\leq \pm 50 \mu m$, and will be very useful in the measurement of angles and momenta.

b) calorimeters -- these devices are well suited to high energies, especially for the study of multiparticle processes over a wide range of angles, as for example, for measurement of jets at large transverse momentum. Recent work using liquid argon and uranium plates has resulted in improved resolution.

c) Cerenkov counters -- techniques are being developed to achieve good velocity resolution with increased acceptance.

d) transition radiation -- this technique will take over particle identification from Cerenkov counters in the TeV range.

e) computers -- microprocessors seem destined to play a large role in control, data acquisition, and initial analysis of future experiments. In addition, significant advances can be expected from large data processors.

f) large magnets -- superconducting spectrometer magnets will provide more magnetic field at a fraction of the power cost of conventional magnets.

g) data transmission between regional and/or national facilities -- this should be implemented in the most efficient way in order to optimize analyses of experimental data. In particular, data transmission at high rates utilizing satellites should be studied.

Other techniques, not yet conceived, may well play important roles in future experiments.

The development of experimental techniques is best accomplished through the work of individuals and small groups. Close communication between groups throughout the world is very important to the timely and efficient development of these techniques.

Although many experiments will become more difficult at high energies, others will become simpler. In many cases the techniques will be changed as the energy increases, so that the required precision and the cost do not become prohibitive. Some specific experiments were considered in the report of a CERN study group*. We conclude that in general the experimental costs will not increase relative to machine costs, but may even decrease.

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* A summary appears in VBA/CMS/1.

IV. Accelerator Projections

Having analyzed the design features presented at the meeting of the international study group on superhigh energy accelerators we have drawn the following conclusions.

The status of the various facilities with center-of-mass energies above 10 GeV can be divided into three groups:

Group 1: The facilities that are now operating successfully on a productive physics program (such as the FNAL accelerator of $E_{f.t.}$ = 500 GeV and the proton-proton ISR at CERN with $E_{c.m.}$ = 2x31 GeV), as well as those in the running-in stage (such as the CERN SPS of $E_{f.t.}$ = 400 GeV).

Group 2: Accelerator and storage rings under construction (such as the three e^+e^- colliding beam facilities under construction (PETRA in FRG with $E_{c.m.}$ = 2x(5-19)GeV, PEP in USA of $E_{c.m.}$ = 2x(5-18)GeV and VEPP-4 in USSR of $E_{c.m.}$ = 2x(5-7)GeV) together with planned projects and facilities under study. If these regional projects are realized they will form the basis for a vigorous experimental program of elementary particle physics until 1990.

The projects in this second group vary widely in cost and scope, but their construction is assumed to be within the resources of a single region.

The principal parameters of this group are presented in Table I. The proton facilities on the list assume superconducting magnets, and the recent advances of this technology have made this a very realistic assumption.

Group 3: Preliminary ideas concerning very big accelerators and storage rings with average orbit radii of 5-15 km and costs in the range of 3-6 times the cost of the FNAL accelerator or the CERN-SPS. Conceptual designs of examples of such facilities were presented to the meeting, and they are listed in Table II. The presentations made might be considered as the initial stage of an accelerator complex to form the basis for the inter-regional program of experimental high-energy physics after 1990.

It is hoped that by the time such a project comes near to its realization, advantage can be taken of further progress in technology, and that, for instance, for the magnets for a fixed target accelerator superconducting materials of higher critical parameters can be used in magnet construction. For the r.f. systems for a possible large e^+e^-, it is hoped that the development of superconducting r.f. cavities can be further advanced. In both these fields, development work should be strongly encouraged.

In conclusion it is not easy to determine what ultimate limits will be imposed on new accelerator projects by technical considerations. It appears that the size and scope of projects presently envisaged will be limited by financial resources only. Technological developments over the next one or two decades may indeed result in more economical solutions being found for the construction of high energy accelerators.

It is recommended that a continuing study should be undertaken through an inter-regional collaboration to ensure that the technologies which are likely to influence future accelerator design are covered by adequate development programs with minimum needless duplication. It should be recognized however, that the potential industrial importance of such technologies adds a further dimension to the problem of international collaboration.

TABLE I

Region (Country)	Facility	p lab.s. GeV	e lab.s. GeV	pp c.m.s. GeV	$p\bar{p}$ c.m.s. GeV	pe c.m.s. GeV	e^-e^+ c.m.s. GeV	Circumference km
Japan (1982 beginning)	TRISTAN	~~180~~ 70	17	360		~110	34	~2
FRG CERN	PETRA ✓ ~~LSR~~ LEP	200 ~~400~~	19 20 < 100	800	800	180	38 < 200	2.3 6.4 < 50
USA 1979-81 Tevatron 1984-5	PEP ~~Doubler~~ ISABELLE ~~POPAE~~	~~200~~ 1000 200-400 1000	18 20 20	250-1000 80-81 400 2000	~~250-1000~~ 1000-1400 --- 300-400	~~100~~ 130 280	36 ---	2.2. ~6 ~3 5.5
USSR 78 81 construct	VEPP-4 UNK	3000	4-7 20	4000	4000	400	14	19.3

TABLE II

International	VBA fixed target	>10.000		(>20.000)	(>20.000)			30-60
	VBA e^+e^-		>100				>200	>50

CONCLUSIONS OF THE IUPAP MEETING
HELD IN TBILISSI ON THE 20TH JULY 1976

IN THE CONCLUSIONS OF THE SERPUKHOV MEETING (MAY 76) THE STUDY GROUP RECOMMEND TO THE IUPAP DIVISION OF PARTICULES AND FIELDS TO INITIATE ACTIVITIES OF INTERNATIONAL COORDINATION AND COLLABORATION ON THE FUTURE HIGH ENERGY FACILITIES INCLUDING REGIONAL AND INTERNATIONAL ACCELERATORS.IT SUGGESTED TO APPOINT A SUB COMMITTEE WITH THE PURPOSE OF ORGANIZING WORKING GROUPS AND FUTURES MEETINGS.

THE IUPAP GROUP ACCEPTS THE SERPUKHOV STUDY GROUP PROPOSAL AND SHALL CREATE THE ICFA COMMITTEE (INTERNATIONAL COMMITTEE FOR FUTURE ACCELERATORS).

THE AIMS OF THIS COMMITTEE SHOULD BE :

- TO ORGANIZE WORKSHOPS FOR THE STUDY OF PROBLEMS RELATED TO AN INTERNATIONAL SUPER HIGH ENERGY ACCELERATOR COMPLEX (V.B.A) AND TO ELABORATE THE FRAMEWORK OF ITS CONSTRUCTION AND ITS USE.
- TO ORGANISE MEETINGS FOR THE EXCHANGE OF INFORMATION ON FUTURE PLANS OF REGIONAL FACILITIES AND FOR THE FORMULATION OF ADVICES ON JOINT STUDIES AND USES.

11 MEMBERS WILL CONSTITUTE THE ICFA COMMITTEE :

- 3 MEMBERS FROM THE USA
- 3 MEMBERS FROM CERN MEMBER STATE
- 3 MEMBERS FROM USSR AND DUBNA JINR MEMBER STATES
- 1 MEMBER FROM JAPAN
- THE CHAIRMAN OF THE IUPAP DIVISION OF PARTICULES AND FIELDS AS THE REPRESENTANT OF ALL THE OTHER COUNTRIES.

THE MEMBERS OF THIS COMMITTEE SHALL BE NOMINATED BY THE RELEVANT AUTHORITIES FROM THE STATES OR THE REGIONS AND APPOINTED BY THE IUPAP COMMITTEE.

THE ICFA COMMITTEE WILL CHOOSE ITS CHAIRMAN AMONG ITS MEMBERS
THE ICFA COMMITTEE WILL REPORT ON ITS ACTIVITIES AT THE ANNUAL INTERNATIONAL CONFERENCE ON HIGH ENERGY PHYSICS.

Telegram to E. L. Goldwasser from A. Rousset	Date: October 6, 1976

Following many suggestions from IUPAP members Professor Gregory accepts to introduce changes in the first version of the Tbilisi conclusions. Is the following text acceptable to you? If yes, we shall try to negotiate it with Professor Soloviev.

Conclusions of the IUPAP Meeting Held in Tbilisi on the 20th of July 1976

In the conclusion of the Serpukhov meeting the study group recommended to the IUPAP Division of Particles and Fields to initiate activities of international coordination and collaboration on the future high energy facilities including regional and international accelerators. It suggested to appoint a subcommittee with the purpose of organizing working groups and future meetings.

The IUPAP group accepts the Serpukhov study group proposal and shall create the ICFA Committee (International Committee for Future Accelerators).

The aims of the committee should be:

- To study the justification of an international super-high-energy accelerator complex (V.B.A.) and to elaborate the framework of its construction and use

- To examine future plans of regional facilities and to give advice on joint sutdies and use

- 11 members will constitute the ICFA committee:

    3 members from the U.S.A.

    3 members from CERN member states

    3 members from USSR and Dubna JINR member states

    1 member from Japan

    the chairman of the IUPAP Division of Particles and Fields as a representant of all other countries.

The members of this committee shall be nominated by the relevant authorities from the states or the regions, and appointed by the IUPAP committee.

The ICFA committee will choose its charman among its members. The ICFA committee will report on its activities at the annual international conference on high energy physics.

The chairman of the IUPAP Division of Particles and Fields will send a letter to Professors Drell, Logunov, Nishikawa and Van Hove and ask them to take the necessary steps with their relevant authorities in order to nominate the members of the ICFA committee.

Telegram to A. Rousset from E. L. Goldwasser   Date: October 8, 1976

In response to your telex of October 6 I am in general agreement with the Gregory proposal. I have several questions, however. It is my impression that the consensus of Commission members was that the committee, itself, should not be expected to make the studies, to do the work, to give the advice. Rather it should be expected to organize appropriate groups, on an ad hoc basis, to make the various studies, to do the work and to arrange information exchanges which might lead to advice. If Professor Gregory ageees with that interpretation of responses we have received, I would suggest the following small changes in your text:

The aims of the committee should be:

- "to organize workshops for the study of problems related to an international, super-high-energy accelerator complex (V.B.A.) and to elaborate the framework of its construction and its use."

- "to organize meetings for the exchange of information on future plans of regional facilities and for the formulation of advice on joint studies and uses."

If Professor Gregory agrees that this wording most closely reflects the opinions of Commission members, I would be pleased to see my suggestion adopted. If, on the other hand, he strongly disagrees, I would with some misgivings accept the wording proposed in your telex to me.

Your new suggestion of membership for the ICFA committee came as a surprise to me, but I have no objection, whatsoever. It clearly departs from what I thought was a unanimous agreement expressed at our Tbilisi meeting, but it may be the best solution to the reservations expressed, ex post factor, by the Commission representatives from the USSR. I would leave it to Professor Gregory to decide whether or not we need to receive explicit agreement from other Commission members regarding the change in proposed membership of the ICFA committee.

As an editorial comment, I would suggest that the phrase "as the representative of other countries" be substituted for the phrase "as the representant of all other countries" as it appeared in your telex.

Finally, I would suggest that we retain the procedures which were adopted at Tbilisi and which were outlined in the last paragraph of your telex, namely that Drell, Logunov, Nishikawa and Van Hove, after consulting with their relevant authorities, should nominate candidates for the new committee. That is somewhat different from the procedure suggested in the earlier paragraph of your telex where you have indicated that the committee members will be nominated by the relevant authorities.

I apologize for splitting hairs.

I have now expressed my reactions to your proposal as clearly as I can. I encourage you and Gregory to proceed as you think best. I am confident that you will take my thoughts into consideration, but whether or not you adopt my considerations, I urge you to proceed without seeking further approval from me.

Sincerely,

E. L. Goldwasser

The following is a translation of a full page article that appeared in the Helsingin Sanomat, the leading Finnish newspaper, on Sunday, September 19th, 1976. The author is a professor of Physics at the Helsinki University and one of the leading physicists in Finland.

Professor Keijo Kajantie:

THE VERY BIG ACCELERATOR AND FINLAND

The study of the smallest building blocks of matter, elementary particles, requires a great deal of money and very large accelerators. At present, only the very biggest countries have the resources to finance by themselves the building of such large machines.

The improvement of international relations provides the opportunity for international collaboration in this field, too. In the opinion of Keijo Kajantie, professor of Physics at the University of Helsinki, it is feasible that the Very Big Accelerator, which may be built in the next few decades by an international collaboration, could be located in Finland, the host country of the Helsinki Conference on European Security and Cooperation.

Particle Physics is the branch of science which investigates the ultimate secrets of the structure of matter. Consequently, its results are quite removed from our everyday life, and are much less discussed in public than, for instance, the new achievements in space research. During the past couple of years, however, there have been several important discoveries in the field of particle physics. Perhaps the most surprising of these was the much publicized discovery of the psi particles two years ago.

Expensive but Necessary

As is well known, particle physics is a very expensive branch of basic research. This is due to the fact that it is necessary to accelerate particles to a great velocity, in order that they may penetrate each other. These very fast particles may only be steered and controlled by very large machines.

As a result, the most powerful particle accelerators are magnetic rings whose diameters may be as much as several miles. The construction and operation of such apparatus obviously requires a lot of money. No way of avoiding this difficulty has yet been devised.

Because particle physics is so costly, only the wealthiest industrial nations are able to pursue it, and even then often only through international collaboration. Indeed, the best examples of extremely succesful international cooperation are the nuclear research center of the Western-European countries, CERN, in Geneva, and the corresponding center of the socialist countries, JINR, in Dubna. Both these organizations have been in operation since the 1950's and are active and productive research centers.

At this very moment, a new big accelerator is becoming operative at CERN. This machine, which can accelerate particles up to an energy of 400 GeV (billion electron volts), will be the centerpiece of the laboratory's operations for at least the next ten years. The biggest accelerator in the world, however, will still be in the United States near Chicago. In a few years' time, it may allow one to reach energies up to 1000 GeV.

The estimated cost of the new big accelerators now being planned vary from a couple of hundred million Finnish Marks (FMk) up to a billion FMk.+) The only individual countries which may afford to invest such amounts of money in basic research are the United States, the Soviet Union, and perhaps also the Federal Republic of Germany and Japan.

However, even in these countries the support given to particle physics has declined, or at best remained constant, in the past ten years. It is also clear that, under current economical circumstances, the government of none of the above-mentioned countries is willing to increase substentially its support for basic research, at least in a field which is as removed from practical applications as particle physics is.

On the other hand, it is obvious that the extension of man's picture of the world in the direction of smaller and smaller distances will end unless the building of new and even more powerful accelerators is continued. Even the most brilliant minds cannot solve the mysteries of Nature without solid experimental facts.

Facing this clash of interests between the desire to increase human knowledge and limited financial resources, the particle physicist, while planning the development of their field in the beginning of the next century, have once more taken up an old idea: Why not put together the resources of all the countries of the world and - in the very spirit of the Helsinki Conference - build a really big accelerator, the so-called VBA (Very Big Accelerator).

Progress through Cooperation

Previously, the idea of building a world accelerator was not contemplated for two main reasons. First, since it has been possible to make good progress in physics with equipment of a smaller scale, there has been no real need for a World Collaboration. Second, and most important, the realization of such a collaboration requires much more cooperation between the socialist and capitalist worlds than has been possible in the past. However, in Western Europe, where political parties have especially wished to show their unity, cooperation has been most smooth and successful.

To many countries, such as all the Nordic countries, participation in the activities of CERN has meant very heavy economic sacrifices. CERN appropriations have been criticized, in particular, by scientists working in fields other than particle physics. However, the political desire of governments to demonstrate their membership in the community of European nations has been strong enough that they have been ready to spend a great deal of money on this branch of pure basic research, which is of no apparent practical value.

The signing of the Helsinki agreement last year removed the first political obstacles from the road that may eventually lead to the building of the World accelerator. Indeed, in the Helsinki agreement high energy (particle) physics

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+) 1 $ US = 4 FMk

is explicitely mentioned as one of the fields in which there are good opportunities to expand international cooperation. But unfortunately, "Helsinki-spirit" is not yet accepted without reservations by all the parties concerned, and the development of friendly relationships must progress much further before the political authorities are ready to seriously consider the building of a World accelerator.

Assuming that the easing of world tensions progresses, it is probable that the governments of the wealthy industrial nations will take the same stand toward the World accelerator as the nations of Western Europe have taken toward CERN: they will be ready to support it as a symbol of their togetherness. The joint Apollo-Sojuz space flight may be regarded as an early example of such an attitude. Of course it also had importance for space research, but its main value derives from the fact that it demonstrated in an impressive way the desire and ability of the Soviet Union and the United States to cooperate.

In the planning of the World accelerator, particle physicists have proceeded with more or less the following time table in their minds. To clarify the Political basis of the issue will take the next ten years. The actual technical design of the accelerator, the choosing of the building site, etc. will begin after ten years' time and the building of the machine will be carried out during the 1990's, so that the machine will begin its operation in the beginning of the next century.

As we see, this is real long-range planning. From the standpoint of particle physics the planning is easy and clear-cut. The external uncertainty factors, however, are huge: first, the direction of future political developments is incalculable and second, it is unclear what changes will occur in the next twenty or thirty years in industrial societies, whose present economy is based on burning oil.

Finland - a possible location for the VBA

From the Finnish point of view, the World accelerator project has particular interest. Just as our country had a decisive role to play in the organization of the European Conference of Security and Cooperation, Finland, as a country having good relationships both to the socialist and to the capitalist worlds, may contribute towards the realization of the World accelerator plan. In fact, it is by no means impossible that the location of the accelerator could even be in Finland. The following scenario is entirely possible:

The development of friendly relationships between the leading industrial nations continues. The cost of energy increases, but slowly enough that the foundations of commercial and industrial life are not shaken. The particle accelerators which begin their operation at by the end of the 1970's provide startling new results about the structure of matter. In the same time, however, they raise new questions which may be answered only with the help of an even bigger accelerator.

The particle physics community, represented by its international organizations, takes a unanimous stand in support of the building of the World accelerator. The political authorities are informed. The Soviet Union and the United States have developed a genuine and warm relationship, and the governments of these two countries decide to open official negotiations about the matter in 1984.

About twenty countries began to search for a location for the accelerator.

A public debate upon the matter begins in Finland, too. The general public is informed about the nature of the project. The accelerator itself is a ring with a diameter of about 10 kilometers. It is, however, located completely underground and doesn't disturb everyday life above it. The area of land needed on the ground is about a thousand hectares*, which will house the scientific, technical and administrative buildings of the laboratory.

In Finland, the choice of possible building sites is rather restricted: the site must lie within a half-hour distance by car from the Helsinki airport. The laboratory will provide work for approximately two thousand people, half of which are highly educated scientific and technical specialists from all over the world.

After a lively and exhaustive debate lasting a year, public opinion in Finland takes a firm stand in favor of the accelerator project: with an investment of 200 million FMk for land, buildings and sanitation, Finland gets an institute on her territory whose annual budget of 500 million FMk is financed mainly from abroad. Furthermore, the laboratory will provide employment for a considerable number of people, in particular if its secondary influences on Finnish economic life are taken into consideration. But the principal importance of placing the laboratory in Finland would lie in that it would be a convincing international recognition of Finland's neutrality.

Competition for the accelerator

Conservation organizations, as well as the counties surrounding the city of Helsinki, accept the project after having realized that it will put no pressure on the environment other than the appearance of a thousand foreigners in Finland and an increase of 100 Megawatts in energy consumption. Public debate upon the matter ends, and the Finnish Government decides by the end of the 1980's to make an official offer to house the World accelerator within Finland's territory.

At the same time that Finland makes her offer, many other countries, especially the neutral Central-European ones, also make their bids. They can offer many things that our country lacks: more money and experience, a more international atmosphere, warmer climate, the Alps, etc. The final decision concerning the location of the laboratory is made by the end of the 1980's on purely political grounds. It will depend mainly on the stand taken by the wealthiest participating countries, the Soviet Union and the United States.

At this place we had better stop dreaming and observe that in reality Finland's chances of obtaining the laboratory are rather small. But they are large enough that the planning of the World accelerator should be closely watched in Finland, and that Finland should be promoted as a possible location for the laboratory.

Figure caption: The biggest accelerator laboratory in the world is located in the United States, in the State of Illinois. In the planning of the laboratory, named after Enrico Fermi, particular emphasis was put on the comfort of the working spaces and the preservation of the environment.

*) 1 hectare = 2.47 acres

[Institute of High Energy Physics, Serpukhov, 1976]

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Report of the

International Study Group

on Future Accelerators and High Energy Physics

Serpukhov, May 17-25 1976

Abstract

The Seminar "Perspectives in High Energy Physics" held in New Orleans, March 1975, established a Study Group to discuss the long-range requirements for facilities in High Energy Physics. A sub-group met in CERN, October 1975, and planned an Agenda for a meeting which was held in Serpukhov, U.S.S.R. in May, 1976. In this paper a summary of the work done in Serpukhov is given.

It begins with a review of the status of our present knowledge of the fundamental structure of matter and a statement of those future problems which can be clearly identified now and which will require new facilities for their solution. This is followed by a brief description of the status of today's accelerator technology and a review of projects that are now under active study as regional facilities. The study group has noted the need for close collaboration during the selection of the range of new regional facilities to ensure coverage of the broadest possible program of research. Included in this range may be a proton fixed target accelerator of up to several TeV, colliding beam facilities with a center-of-mass energy of up to several TeV for protons against protons, up to several hundred GeV for electrons against protons, and up to about 200 GeV for electrons against positrons. The participants have emphasized the importance of joint utilization of all such facilities by scientists of different countries.

The Study Group has stressed that the further progress of High Energy Physics will require in the future the development of an accelerator complex significantly more powerful than those planned for regional facilities. This complex is likely to be of such a cost as to be beyond the capabilities of any single region. Examples include facilities such as a proton accelerator of energy higher than 10 TeV and an electron-positron colliding beam facility of more than 200 GeV in the center-of-mass. In this connection several conceptual designs of that kind were presented and discussed.

In seeking to attain the more intensive international collaboration which is a fundamental prerequisite for progress toward the stated objectives, the Study Group recommends that the International Union of Pure and Applied Physics (Particles and Fields Division) be asked to initiate appropriate activities to this end.

I. Introduction

The historical development of science has made it especially appropriate that the physicists of all countries which are active in the exploration of the deepest aspects of atomic nature should be collaborating so intensely. It is gratifying that this collaboration has resulted in so much progress in our knowledge about the particles of which the world is made and of the laws that govern their behavior. It is equally gratifying that governments have provided the necessary framework within which the collaboration could take place. The fundamental knowledge being developed will become the basis of future technology and, equally important, will provide mankind with a greater insight into the nature of the universe.

The struggle for this knowledge is difficult, and although many concepts of nature have been deepened and new concepts have emerged, nevertheless, it is anticipated that vastly more extensive investigations will be required before our knowledge of the basic particles is as firm as is our understanding, for example, of electromagnetism.

The tools for investigating matter have become more complex and more expensive as we have penetrated deeper into the inner space of the atom. For this reason organizational collaborations have developed between groups of nations to allow them to participate in this exciting and necessary development. Thus the member nations of CERN and the member nations of JINR have established organizations which have enabled them to successfully develop

research in this field. Most importantly, the close collaboration between the regional laboratories has amplified their individual efforts.

As facilities that are now being planned on a regional basis are developed, ways should be found to help in coordinating that planning. Such mutual discussion and advice would ensure the coverage of the broadest possible program of research. Joint studies of new technology and organization of wider collaborative use of present facilities should occur. Joint construction of sub-elements of regional projects should be explored.

It can already be expected that the facilities needed to explore and clarify the next level beyond that available to facilities presently being contemplated will be so large that their realization will be greatly optimized -- and may only be possible -- by the pooling of the resources of all regions in a common effort.

We underline the statement of the countries participating in the "Helsinki Agreement on Security and Cooperation in Europe", which specifically mentions high-energy physics as a field for cooperation. It says that "scientific and technological cooperation constitutes an important contribution to the strengthening of security and cooperation among (the countries) in that it assists the effective solution of problems of common interest and the improvements of the conditions of human life".

V. Conclusions

The foregoing survey leads us to the following conclusions:

A) The present status of the science of the structure of matter poses fundamental problems which require a new generation of facilities of the types listed in Table I. Such facilities are within the capabilities of the individual regions and are needed for continued progress of this field of research.

B) The success of regional and interregional collaboration in the past provides a good basis for extending and strengthening this collaboration in the new generation of regional facilities.

C) Looking beyond this new generation of regional accelerators we foresee the need for an accelerator complex (VBA) which will require international collaboration of all regions concerned.

VI. Recommendations

1) Efforts should be made to coordinate the design and construction of new regional facilities. Consultations and exchange of experiences should be encouraged in order to optimize the diversity of facilities and to enhance the efficiency of construction and operation. The Study Group also recommends joint studies of new technology (e.g. superconductivity, new detectors and other experimental apparatus) and joint design and/or construction of components of regional projects.

2) Joint utilization of regional facilities by scientists of different regions should be organized on the basis of present and future arrangements or agreements. The general availability of regional installations is essential to enable scientists of different regions to take advantage of facilities with complementary research poten-

tialities.

3) International collaboration should provide for studies leading towards the realization of a next generation of super-high energy facilities, following the regional projects referred to above (examples are given in Table II). It is expected that these facilities will be so large that their realization will be possible only by pooling the resources of all regions concerned into common international projects.

Creation of a super-high energy accelerator complex (VBA) involves especially complicated scientific, technical and organizational problems. These will require several years of continuing studies and discussions. The Study Group recommends that these discussions begin in the near future leading to the start of the design of the VBA in about 10 years.

4) In view of the need for these extensions of international collaboration, the Study Group suggests to the IUPAP Division of Particles and Fields to initiate these activities in an appropriate form, for example, by appointing a sub-committee for the purpose of organizing working groups and future meetings such as the present one.

Appendix 1

PARTICIPANTS

USSR

A.A. Logunov
A.A. Vassilyev
M.A. Markov
V.A. Glukhikh
L.D. Soloviev
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as experts:

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A. Budker
N.A. Monoszon
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V.A. Vassiliev
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JINR

K. Lanius
V.P. Djelepov

JAPAN

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USA

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L. Lederman
M. Barton
R. Diebold
J. Bjorken
D. Eulian (secretary)

* CERN Member States

G. von Dardel
U. Amaldi
D. Husmann
K. Johnsen
A. Rousset
D.B. Thomas

as expert:

G.A. Voss

* The delegation from the CERN Member States was selected by the CERN Scientific Policy Committee and was under the leadership of the Chairman of the European Committee for Future Accelerators.

MEMORANDUM REGARDING THE PRESENT V.B.A. SITUATION

V.F. Weisskopf - June 1977

One year has passed since the Serpukhov discussions. Although the IUPAP meeting in Tbilisi in August 1976 has formally accepted the recommendations of Serpukhov and asked the Chairman B. Gregory to form a Study Group, this group has not yet been established. The reasons for this delay are, partly, dissatisfaction of the Russians with the number of Sowjet members in the Group, partly a lack of enthusiasm in Europe. It is hoped that this will be remedied at the next IUPAP meeting in Hamburg in August 1977. The Americans are pressing strongly for quick formation of the Study Group and would be ready to add one Russian more to the group if necessary.

In the meantime the three regions have become more definitive in regard to their own future regional plans for the next 10 years (that is, before the envisaged start of design of the V.B.A.). The USA proposes to increase the energy of the Fermi-Lab. accelerator to 1000 GeV and to construct "ISABELLE", that is a colliding beam facility with 300 or 400 GeV in each beam. (Neither of these proposals have yet been approved by the Government). The Europeans are studying the possibility of construction of an electron-positron colliding beam facility of about 70 GeV in each beam as the next European step. No Government approval has yet been given.

The Sowjet Physicists are proposing a large increase of the Serpukhov accelerator to 2000-3000 GeV, with possible electron-proton collision facilities. It is not clear how far the Government will support these plans.

The Physicists in all three regions are still conviced of the necessity to plan for accelerators of still higher energies (V.B.A.) as the next step to be taken in 10 or 15 years from now. The only way to do this is international construction by all three regions

with participation of Japan and perhaps other industrialized regions (India, South-America).

I believe that the lack of enthusiasm and the slowness of the IUPAP procedures are caused by the fear of many physicists in all three regions, that public knowledge of the VBA-plans may discourage governments to approve the plans for the next decade. Those, who press for discussions of the VBA hold the opinion that the governments would be more inclined to support further regional developments, if they know that it will lead eventually to true international cooperation. Clearly an international laboratory would have political significance beyond the scientific values.

So far the location of the planned international laboratory was <u>not</u> discussed at anyof these meetings. Nevertheless, it is almost evident that it must be at a central location for the three regions; that means Europe. Also, it must be in a neutral country, preferably with highly developed industries. Obviously, Austria would be a most suitable candidate.

CONFIDENTIAL

May 30, 1976

Personal comments to the Report on the Serpukhav Conference

The following comments are my personal impressions of the Serpukhov discussions. I assume that the reader has read the Abstract and the Conclusions and Recommendations in the official Report.

We distinguish two time periods. Period I considers the next 10 to 15 years; Period II is what follows those years. In the first period we expect that new regional facilities will be constructed, such as the installations referred to in Table I of the Report.

There were three recommendations:
One is to coordinate the design and construction of the new regional facilities in the next 10-15 years. The second is joint utilization of regional facilities for scientists in all regions; in other words, general useability of the newly constructed machines, and also of course those that are already here. Third, international collaboration should provide studies leading toward the realization in Period II of a new generation of super-high facilities on an international basis. In other words, (1) coordination of future plans in the future, (2) better exploitation (international) of present and future facilities and (3) the planning of a world machine.

Let me first say a few words about coordination. None of the three parties desired a really close coordination, that means in exact distribution of types of machines to the three regions. Clearly each of the regions were afraid of not being able to realize their project. That goes also for the Americans, and in particular for the Russians since they are somewhat afraid that if too much coordination would be planned they may not be able to construct their 1-2 TeV machine because it is somehow a duplication of possible future plans at FERMILAB. Clearly also the FERMILAB is concerned about too strict coordination because it may not allow them to go ahead with their plans having for example a 1000 vs. 1000 proton colliding beam machine if the Russians build something like that. The same fear, of course, is held by the Russians.

The situation is in fact made rather difficult because of the previous cases of lack of coordination. First of all, it is very unfortunate that PETRA and PEP are constructed at the same time. It would have been much more in the spirit of a rational coordination if for example Germany would have built PETRA but PEP would have been the next step -- maybe 60 or 70 or even 100 GeV electrons against positrons. This would have required all resources from the American side and would have left little for ISABELLE or POPAE. On the other hand it would have allowed the Europeans to go ahead with a 400 against 400 storage ring as an addition to SPS. Another scenario would be the same the other way around, namely that PEP would be constructed the same way as planned and that PETRA should be enlarged to be not only a German but a European international collaboration of considerably higher energy, maybe 60 against 60 or more. Then the Europeans would have constructed another large facility and the Americans could have used the means and the time and the manpower to build ISABELLE or POPAE. Unfortunately the doubling of PETRA and PEP makes reasonable coordination even between Europe and America almost impossible. If America chooses as the next step the proton-proton colliding beam machine, the Europeans will not build it. Then all they could do and should do, is to build a larger electron-positron colliding facility of 60 or 70 GeV, but this is nothing but a repetition of PETRA on a much larger scale, and this feature does not look to them, (neither to me), to be a very attractive possibility. What many Europeans would like to do is get a proton-proton colliding beam facility of 400 against 400 at CERN. However, they expect correctly, that a decision for a proton-proton facility will be made in America in the next few years before 1980. It is hardly thinkable or possible that Europe would indeed approve a new big project before 1980.

So much for the coordination between America and Europe. The Russian situation is perhaps a little easier although it also represents a kind of duplication. The Russians' plan is to use the present Serpukhov machine as an injector to a 1000 GeV or perhaps 2000 GeV machine which then at a later stage might develop into a p-p storage ring. It seems that, before they develop a p-p storage ring, they would add an electron ring of about 20 GeV in order to get a e-p facility. In other words, what they are aiming at immediately is a 1000-2000 stationary target machine and an e-p facility. Somehow this program does, of course, duplicate FERMILAB efforts, in particular if the energy doubler will be realized and the energy doubler would lead to a POPAE 1000 against 1000, and also if the FERMILAB thinks of the electron-proton colliding possibility. So the whole coordination picture doesn't look too good if everybody gets more or less what they want. It will end up with proton-proton storage rings in America, perhaps even with a e-p addition, a 1-2 TeV stationary machine in Russia with e-p addition, and perhaps something in Europe which may be a 60-60 e^+e^-. Europe is worst off because they just are about to finish their last step, the SPS. Therefore, in my opinion, it would be in Europe's interest not to press for a larger regional project, but to press very hard for the realization of a world machine which after all must be in Europe, and therefore Europe would have the greatest advantage of it. However, the world machine is unsure enough and would appear to the Europeans to be too risky. If it fails, there may not be any machine at all in Europe after the SPS and PETRA.

A few words about the second point, better international exploitation of regional facilities. We know that there are difficulties with the Soviet collaboration, both in USA and in Europe. The new directorate of CERN takes a much tougher stand now. One possible bit of good news, however, is an assurance from Logunov that, starting July 1, Soviet physicists working at experiments abroad will have permanent visas, allowing them to come and go whenever they want.

Another item that was mentioned in connection with improving collaboration, was mutual help in constructing new regional facilities. It means borrowing experts from one region to the other, say from Europe to USA for the construction of ISABELLE or to USSR for UNK.

Now the world machine situation. The Russians did not like the term "world machine". They preferred "international" or "interregional" project. Their reasoning (not completely without foundation) was that not the whole world will be in; China, Africa, South America, India, etc. are probably not going to participate. There was no objection against the term VBA. The ones that pressed the most for the VBA were the Americans, and in particular Wilson and Lederman and to some extent myself. The Russians did not quite understand why we are pressing so hard for it. Indeed at one occasion I happened to be alone with Logunov in an automobile and he asked me point blank: "Why is it that your colleagues are so strongly for a world machine". I answered as follows: I said that our Government is dissatisfied that we are always asking for bigger and bigger machines and our Government would like to see scientific planning on a world scale. Therefore, if we can show them that we are working very hard for a world machine to be constructed after the next step, (ISABELLE or POPAE), we have a greater chance that our Government would approve this step. Of course, I, myself, am not sure whether that is correct reasoning. Logunov understood it but I do not think the situation in Russia is parallel to ours.

The European attitude toward the VBA is rather complicated and quite different. The CERN directorate is rather lukewarm toward the whole idea, being afraid that the governments will get scared by new expenses on big programs. And certainly the CERN directorate and those who think likewise do believe that the talking about a world machine right now will make it impossible to get funds for the next big step after the SPS, which, after all, still would be a regional step. However, at the last ECFA meeting, ECFA rather enthusiastically endorsed the VBA idea, and also nominated the European delegation to the Serpukhov conference. This delegation consisted of people who indeed are rather enthusiastic about the VBA. Von Dardel is strongly in favor and both André Rousset and Ugo Amaldi have supported the VBA idea with great vigor in all discussions. However, we should not make the mistake to think that they are representative of European opinion among high energy physicists; they might well be exceptions.

Under these conditions it was impossible to start immediately an all-out effort towards the VBA. Indeed I am surprised that we were able to get such reasonably strong statements accepted as on Page 1, paragraph 3, or Page 16, paragraph 1 and 2 of the Report. There was much urging of a "gentle" approach to the VBA questions, at least for the next five years.

In order to put all collaboration efforts into a respectable administrative framework, it was proposed to request the Division of Particles and Fields of IUPAP to appoint a subcommittee, which is supposed to organize further meetings and further work on the collaboration, including further studies towards the realization of a VBA. It will depend a lot on the mood within the Division of Particles and Fields whether or not this will be successful. I saw the Chairman, Bernard Gregory, in Paris after the meeting, and I was assured that he will support this plan whole-heartedly.

The present American members of the Division, Ned Goldwasser and Francis Low, will see to it too. Gregory intends to make A. Rousset the Secretary of the subcommittee and I hope that Lederman will be a member. I suppose that other members will be U. Amaldi, Yarba, Lanius and Yamaguchi.

Telegram to A. Rousset from E. L. Goldwasser   Date: October 8, 1976

In response to your telex of October 6 I am in general agreement with the Gregory proposal. I have several questions, however. It is my impression that the consensus of Commission members was that the committee, itself, should not be expected to make the studies, to do the work, to give the advice. Rather it should be expected to organize appropriate groups, on an ad hoc basis, to make the various studies, to do the work and to arrange information exchanges which might lead to advice. If Professor Gregory ageees with that interpretation of responses we have received, I would suggest the following small changes in your text:

The aims of the committee should be:

- "to organize workshops for the study of problems related to an international, super-high-energy accelerator complex (V.B.A.) and to elaborate the framework of its construction and its use."

- "to organize meetings for the exchange of information on future plans of regional facilities and for the formulation of advice on joint studies and uses."

If Professor Gregory agrees that this wording most closely reflects the opinions of Commission members, I would be pleased to see my suggestion adopted. If, on the other hand, he strongly disagrees, I would with some misgivings accept the wording proposed in your telex to me.

Your new suggestion of membership for the ICFA committee came as a surprise to me, but I have no objection, whatsoever. It clearly departs from what I thought was a unanimous agreement expressed at our Tbilisi meeting, but it may be the best solution to the reservations expressed, ex post factor, by the Commission representatives from the USSR. I would leave it to Professor Gregory to decide whether or not we need to receive explicit agreement from other Commission members regarding the change in proposed membership of the ICFA committee.

As an editorial comment, I would suggest that the phrase "as the representative of other countries" be substituted for the phrase "as the representant of all other countries" as it appeared in your telex.

Finally, I would suggest that we retain the procedures which were adopted at Tbilisi and which were outlined in the last paragraph of your telex, namely that Drell, Logunov, Nishikawa and Van Hove, after consulting with their relevant authorities, should nominate candidates for the new committee. That is somewhat different from the procedure suggested in the earlier paragraph of your telex where you have indicated that the committee members will be nominated by the relevant authorities.

I apologize for splitting hairs.

I have now expressed my reactions to your proposal as clearly as I can. I encourage you and Gregory to proceed as you think best. I am confident that you will take my thoughts into consideration, but whether or not you adopt my considerations, I urge you to proceed without seeking further approval from me.

Sincerely,

E. L. Goldwasser

Hauptsache: Conclusions auf S. 4-5 insbesondere Punkt 3.

Report of the International Study Group on Future Accelerators and High Energy Physics

Serpukhov, May 17-25 1976

Abstract

The Seminar "Perspectives in High Energy Physics" held in New Orleans, March 1975, established a Study Group to discuss the long-range requirements for facilities in High Energy Physics. A sub-group met in CERN, October 1975, and planned an Agenda for a meeting which was held in Serpukhov, U.S.S.R. in May, 1976. In this paper a summary of the work done in Serpukhov is given.

It begins with a review of the status of our present knowledge of the fundamental structure of matter and a statement of those future problems which can be clearly identified now and which will require new facilities for their solution. This is followed by a brief description of the status of today's accelerator technology and a review of projects that are now under active study as regional facilities. The study group has noted the need for close collaboration during the selection of the range of new regional facilities to ensure coverage of the broadest possible program of research. Included in this range may be a proton fixed target accelerator of up to several TeV, colliding beam facilities with a center-of-mass energy of up to several TeV for protons against protons, up to several hundred GeV for electrons against protons, and up to about 200 GeV for electrons against positrons. The participants have emphasized the importance of joint utilization of all such facilities by scientists of different countries.

The Study Group has stressed that the further progress of High Energy Physics will require in the future the development of an accelerator complex significantly more powerful than those planned for regional facilities. This complex is likely to be of such a cost as to be beyond the capabilities of any single region. Examples include facilities such as a proton accelerator of energy higher than 10 TeV and an electron-positron colliding beam facility of more than 200 GeV in the center-of-mass. In this connection several conceptual designs of that kind were presented and discussed.

In seeking to attain the more intensive international collaboration which is a fundamental prerequisite for progress toward the stated objectives, the Study Group recommends that the International Union of Pure and Applied Physics (Particles and Fields Division) be asked to initiate appropriate activities to this end.

I. Introduction

The historical development of science has made it especially appropriate that the physicists of all countries which are active in the exploration of the deepest aspects of atomic nature should be collaborating so intensely. It is gratifying that this collaboration has resulted in so much progress in our knowledge about the particles of which the world is made and of the laws that govern their behavior. It is equally gratifying that governments have provided the necessary framework within which the collaboration could take place. The fundamental knowledge being developed will become the basis of future technology and, equally important, will provide mankind with a greater insight into the nature of the universe.

The struggle for this knowledge is difficult, and although many concepts of nature have been deepened and new concepts have emerged, nevertheless, it is anticipated that vastly more extensive investigations will be required before our knowledge of the basic particles is as firm as is our understanding, for example, of electromagnetism.

The tools for investigating matter have become more complex and more expensive as we have penetrated deeper into the inner space of the atom. For this reason organizational collaborations have developed between groups of nations to allow them to participate in this exciting and necessary development. Thus the member nations of CERN and the member nations of JINR have established organizations which have enabled them to successfully develop

research in this field. Most importantly, the close collaboration between the regional laboratories has amplified their individual efforts.

As facilities that are now being planned on a regional basis are developed, ways should be found to help in coordinating that planning. Such mutual discussion and advice would ensure the coverage of the broadest possible program of research. Joint studies of new technology and organization of wider collaborative use of present facilities should occur. Joint construction of sub-elements of regional projects should be explored.

It can already be expected that the facilities needed to explore and clarify the next level beyond that available to facilities presently being contemplated will be so large that their realization will be greatly optimized -- and may only be possible -- by the pooling of the resources of all regions in a common effort.

We underline the statement of the countries participating in the "Helsinki Agreement on Security and Cooperation in Europe", which specifically mentions high-energy physics as a field for cooperation. It says that "scientific and technological cooperation constitutes an important contribution to the strengthening of security and cooperation among (the countries) in that it assists the effective solution of problems of common interest and the improvements of the conditions of human life".

V. Conclusions

The foregoing survey leads us to the following conclusions:

A) The present status of the science of the structure of matter poses fundamental problems which require a new generation of facilities of the types listed in Table I. Such facilities are within the capabilities of the individual regions and are needed for continued progress of this field of research.

B) The success of regional and interregional collaboration in the past provides a good basis for extending and strengthening this collaboration in the new generation of regional facilities.

C) Looking beyond this new generation of regional accelerators we foresee the need for an accelerator complex (VBA) which will require international collaboration of all regions concerned.

VI. Recommendations

1) Efforts should be made to coordinate the design and construction of new regional facilities. Consultations and exchange of experiences should be encouraged in order to optimize the diversity of facilities and to enhance the efficiency of construction and operation. The Study Group also recommends joint studies of new technology (e.g. superconductivity, new detectors and other experimental apparatus) and joint design and/or construction of components of regional projects.

2) Joint utilization of regional facilities by scientists of different regions should be organized on the basis of present and future arrangements or agreements. The general availability of regional installations is essential to enable scientists of different regions to take advantage of facilities with complementary research poten-

tialities.

3) International collaboration should provide for studies leading towards the realization of a next generation of super-high energy facilities, following the regional projects referred to above (examples are given in Table II). It is expected that these facilities will be so large that their realization will be possible only by pooling the resources of all regions concerned into common international projects.

Creation of a super-high energy accelerator complex (VBA) involves especially complicated scientific, technical and organizational problems. These will require several years of continuing studies and discussions. The Study Group recommends that these discussions begin in the near future leading to the start of the design of the VBA in about 10 years.

4) In view of the need for these extensions of international collaboration, the Study Group suggests to the IUPAP Division of Particles and Fields to initiate these activities in an appropriate form, for example, by appointing a sub-committee for the purpose of organizing working groups and future meetings such as the present one.

Appendix 1

PARTICIPANTS

USSR

A.A. Logunov
A.A. Vassilyev
M.A. Markov
V.A. Glukhikh
L.D. Soloviev
I.V. Tchuvilo
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V.P. Djelepov

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R.R. Wilson
L. Lederman
M. Barton
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D. Eulian (secretary)

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G. von Dardel
U. Amaldi
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A. Rousset
D.B. Thomas

as expert:

G.A. Voss

* The delegation from the CERN Member States was selected by the CERN Scientific Policy Committee and was under the leadership of the Chairman of the European Committee for Future Accelerators.

MEMORANDUM REGARDING THE PRESENT V.B.A. SITUATION

V.F. Weisskopf - June 1977

One year has passed since the Serpukhov discussions. Although the IUPAP meeting in Tbilisi in August 1976 has formally accepted the recommendations of Serpukhov and asked the Chairman B. Gregory to form a Study Group, this group has not yet been established. The reasons for this delay are, partly, dissatisfaction of the Russians with the number of Sowjet members in the Group, partly a lack of enthusiasm in Europe. It is hoped that this will be remedied at the next IUPAP meeting in Hamburg in August 1977. The Americans are pressing strongly for quick formation of the Study Group and would be ready to add one Russian more to the group if necessary.

In the meantime the three regions have become more definitive in regard to their own future regional plans for the next 10 years (that is, before the envisaged start of design of the V.B.A.). The USA proposes to increase the energy of the Fermi-Lab. accelerator to 1000 GeV and to construct "ISABELLE", that is a colliding beam facility with 300 or 400 GeV in each beam. (Neither of these proposals have yet been approved by the Government). The Europeans are studying the possibility of construction of an electron-positron colliding beam facility of about 70 GeV in each beam as the next European step. No Government approval has yet been given.

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The Physicists in all three regions are still conviced of the necessity to plan for accelerators of still higher energies (V.B.A.) as the next step to be taken in 10 or 15 years from now. The only way to do this is international construction by all three regions

with participation of Japan and perhaps other industrialized regions (India, South-America).

I believe that the lack of enthusiasm and the slowness of the IUPAP procedures are caused by the fear of many physicists in all three regions, that public knowledge of the VBA-plans may discourage governments to approve the plans for the next decade. Those, who press for discussions of the VBA hold the opinion that the governments would be more inclined to support further regional developments, if they know that it will lead eventually to true international cooperation. Clearly an international laboratory would have political significance beyond the scientific values.

So far the location of the planned international laboratory was not discussed at any of these meetings. Nevertheless, it is almost evident that it must be at a central location for the three regions; that means Europe. Also, it must be in a neutral country, preferably with highly developed industries. Obviously, Austria would be a most suitable candidate.

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CONFIDENTIAL

May 30, 1976

Personal comments to the Report on the Serpukhav Conference

The following comments are my personal impressions of the Serpukhov discussions. I assume that the reader has read the Abstract and the Conclusions and Recommendations in the official Report.

We distinguish two time periods. Period I considers the next 10 to 15 years; Period II is what follows those years. In the first period we expect that new regional facilities will be constructed, such as the installations referred to in Table I of the Report.

There were three recommendations:
One is to coordinate the design and construction of the new regional facilities in the next 10-15 years. The second is joint utilization of regional facilities for scientists in all regions; in other words, general useability of the newly constructed machines, and also of course those that are already here. Third, international collaboration should provide studies leading toward the realization in Period II of a new generation of super-high facilities on an international basis. In other words, (1) coordination of future plans in the future, (2) better exploitation (international) of present and future facilities and (3) the planning of a world machine.

Let me first say a few words about coordination. None of the three parties desired a really close coordination, that means in exact distribution of types of machines to the three regions. Clearly each of the regions were afraid of not being able to realize their project. That goes also for the Americans, and in particular for the Russians since they are somewhat afraid that if too much coordination would be planned they may not be able to construct their 1-2 TeV machine because it is somehow a duplication of possible future plans at FERMILAB. Clearly also the FERMILAB is concerned about too strict coordination because it may not allow them to go ahead with their plans having for example a 1000 vs. 1000 proton colliding beam machine if the Russians build something like that. The same fear, of course, is held by the Russians.

The situation is in fact made rather difficult because of the previous cases of lack of coordination. First of all, it is very unfortunate that PETRA and PEP are constructed at the same time. It would have been much more in the spirit of a rational coordination if for example Germany would have built PETRA but PEP would have been the next step -- maybe 60 or 70 or even 100 GeV electrons against positrons. This would have required all resources from the American side and would have left little for ISABELLE or POPAE. On the other hand it would have allowed the Europeans to go ahead with a 400 against 400 storage ring as an addition to SPS. Another scenario would be the same the other way around, namely that PEP would be constructed the same way as planned and that PETRA should be enlarged to be not only a German but a European international collaboration of considerably higher energy, maybe 60 against 60 or more. Then the Europeans would have constructed another large facility and the Americans could have used the means and the time and the manpower to build ISABELLE or POPAE. Unfortunately the doubling of PETRA and PEP makes reasonable coordination even between Europe and America almost impossible. If America chooses as the next step the proton-proton colliding beam machine, the Europeans will not build it. Then all they could do and should do, is to build a larger electron-positron colliding facility of 60 or 70 GeV, but this is nothing but a repetition of PETRA on a much larger scale, and this feature does not look to them, (neither to me), to be a very attractive possibility. What many Europeans would like to do is get a proton-proton colliding beam facility of 400 against 400 at CERN. However, they expect correctly, that a decision for a proton-proton facility will be made in America in the next few years before 1980. It is hardly thinkable or possible that Europe would indeed approve a new big project before 1980.

So much for the coordination between America and Europe. The Russian situation is perhaps a little easier although it also represents a kind of duplication. The Russians' plan is to use the present Serpukhov machine as an injector to a 1000 GeV or perhaps 2000 GeV machine which then at a later stage might develop into a p-p storage ring. It seems that, before they develop a p-p storage ring, they would add an electron ring of about 20 GeV in order to get a e-p facility. In other words, what they are aiming at immediately is a 1000-2000 stationary target machine and an e-p facility. Somehow this program does, of course, duplicate FERMILAB efforts, in particular if the energy doubler will be realized and the energy doubler would lead to a POPAE 1000 against 1000, and also if the FERMILAB thinks of the electron-proton colliding possibility. So the whole coordination picture doesn't look too good if everybody gets more or less what they want. It will end up with proton-proton storage rings in America, perhaps even with a e-p addition, a 1-2 TeV stationary machine in Russia with e-p addition, and perhaps something in Europe which may be a 60-60 e^+e^-. Europe is worst off because they just are about to finish their last step, the SPS. Therefore, in my opinion, it would be in Europe's interest not to press for a larger regional project, but to press very hard for the realization of a world machine which after all must be in Europe, and therefore Europe would have the greatest advantage of it. However, the world machine is unsure enough and would appear to the Europeans to be too risky. If it fails, there may not be any machine at all in Europe after the SPS and PETRA.

A few words about the second point, better international exploitation of regional facilities. We know that there are difficulties with the Soviet collaboration, both in USA and in Europe. The new directorate of CERN takes a much tougher stand now. One possible bit of good news, however, is an assurance from Logunov that, starting July 1, Soviet physicists working at experiments abroad will have permanent visas, allowing them to come and go whenever they want.

Another item that was mentioned in connection with improving collaboration, was mutual help in constructing new regional facilities. It means borrowing experts from one region to the other, say from Europe to USA for the construction of ISABELLE or to USSR for UNK.

Now the world machine situation. The Russians did not like the term "world machine". They preferred "international" or "interregional" project. Their reasoning (not completely without foundation) was that not the whole world will be in; China, Africa, South America, India, etc. are probably not going to participate. There was no objection against the term VBA. The ones that pressed the most for the VBA were the Americans, and in particular Wilson and Lederman and to some extent myself. The Russians did not quite understand why we are pressing so hard for it. Indeed at one occasion I happened to be alone with Logunov in an automobile and he asked me point blank: "Why is it that your colleagues are so strongly for a world machine". I answered as follows: I said that our Government is dissatisfied that we are always asking for bigger and bigger machines and our Government would like to see scientific planning on a world scale. Therefore, if we can show them that we are working very hard for a world machine to be constructed after the next step, (ISABELLE or POPAE), we have a greater chance that our Government would approve this step. Of course, I, myself, am not sure whether that is correct reasoning. Logunov understood it but I do not think the situation in Russia is parallel to ours.

The European attitude toward the VBA is rather complicated and quite different. The CERN directorate is rather lukewarm toward the whole idea, being afraid that the governments will get scared by new expenses on big programs. And certainly the CERN directorate and those who think likewise do believe that the talking about a world machine right now will make it impossible to get funds for the next big step after the SPS, which, after all, still would be a regional step. However, at the last ECFA meeting, ECFA rather enthusiastically endorsed the VBA idea, and also nominated the European delegation to the Serpukhov conference. This delegation consisted of people who indeed are rather enthusiastic about the VBA. Von Dardel is strongly in favor and both André Rousset and Ugo Amaldi have supported the VBA idea with great vigor in all discussions. However, we should not make the mistake to think that they are representative of European opinion among high energy physicists; they might well be exceptions.

Under these conditions it was impossible to start immediately an all-out effort towards the VBA. Indeed I am surprised that we were able to get such reasonably strong statements accepted as on Page 1, paragraph 3, or Page 16, paragraph 1 and 2 of the Report. There was much urging of a "gentle" approach to the VBA questions, at least for the next five years.

In order to put all collaboration efforts into a respectable administrative framework, it was proposed to request the Division of Particles and Fields of IUPAP to appoint a subcommittee, which is supposed to organize further meetings and further work on the collaboration, including further studies towards the realization of a VBA. It will depend a lot on the mood within the Division of Particles and Fields whether or not this will be successful. I saw the Chairman, Bernard Gregory, in Paris after the meeting, and I was assured that he will support this plan whole-heartedly.

The present American members of the Division, Ned Goldwasser and Francis Low, will see to it too. Gregory intends to make A. Rousset the Secretary of the subcommittee and I hope that Lederman will be a member. I suppose that other members will be U. Amaldi, Yarba, Lanius and Yamaguchi.

Now I would like to say a few words about my own conversations with Academy members. When I saw Markov the first time, I told him that I intended to visit Sakharov when I passed through Moscow. He said, in a friendly tone, that this is my private business and, naturally, I am free to visit anybody I wished. A few days later, Logunov told me privately that the Academy intended to elect me as foreign member at its next session which took place on May 31. A few days later I was requested by Markov and Logunov to see the Secretary of the Academy and I was asked officially whether I would accept such an election. I said I would, that it would be a great honor, and also a help in respect to collaboration between East and West. The next day, however, I asked Logunov and Markov for a confidential conversation. I said to them that I was most pleased by this election, but I would like to draw their attention upon one fact -- I am worried about the case of Sakharov. If the Academy would expel Sakharov, I would be forced to resign and I would like them to be aware of this. If this statement of mine would perhaps induce them to change their mind and decide not to offer me membership, I would by no means be offended, and I would understand the situation. They were seemingly not surprised by what I said and answered me in a rather light-handed and not at all offended way. [I did tell Markov a few days earlier that I intended to visit Sakharov when I passed through Moscow.] They said there is no intention to expel Sakharov from the Academy -- and what I do in case such things would happen is my own business and I am free to do what I want.

Later on I visited Piotr Kapitza (the old man), who already knew that they intended to elect me and read to me the summary of my activities as it was communicated to all members for a vote. I told Kapitza about my conversation with Logunov and Markov in respect to my possible resignation if Sakharov were expelled. Kapitza was glad that I did so and told me that he considers such a possibility as highly improbable, because they need a 2/3 secret vote in order to expel a member, and he doesn't think it would ever get 2/3 in the case of Sakharov.

I also wrote a letter to Logunov in which I mentioned that the feelings between CERN and DUBNA and the Soviet Union are somewhat tense and that the present directorate is not too sympathetic to collaboration with the Soviets. I said that an extended visit of Gribov to the Theoretical Division at CERN could make an enormous difference in the attitude of the CERN directorate towards collaboration and would make it much easier for me to help improving the situation. I urged him to do his utmost to make such a visit possible. I really believe that it would make a difference and I am curious whether Logunov will follow my advice or not. I also told the same thing to Yarba. Indeed I gave the letter directly to Yarba so that it doesn't go through the administrative apparatus of the Academy but will go directly into the hands of Logunov.

Coming back to Kapitza -- I asked him about his opinion as to the general situation in respect to Jewish scientists who want to emigrate. He told me the situation is by far not as bad as the Western press made it. Many people are allowed to emigrate. In fact a large number of artists, musicians, and painters have emigrated. This fact was also told to Wilson, by Alichanian. Kapitza says there are a few cases in which permission for emigration has not been granted yet, like the case of Levitch (he is convinced however that the Levitch case will be solved soon). So he thinks the West exaggerates the situation -- I hope he is right but I am not sure. Kapitza is most sympathetic towards Sakharov -- he refers to Sakharov as a saint. He said to me that, when I see him, I should tell him he should do more physics since this would make it easier for all of us to help him.

On my last day I did visit Sakharov and had a long conversation with him. It was very warm and human. There are few facts I can report except that he has difficulties with his apartment; they did not give him the right of residence in Moscow and the Academy doesn't lift a finger to help him. The children of his wife are still badly off -- nothing much has changed in that situation. His daughter is without job, his son-in-law has a job but not a very satisfactory one.

I also saw Engene Feinberg and Eugene Lifschitz when I was in Moscow. Both have given me slightly different versions of the situation. Feinberg is in general an optimist and says that things are getting somewhat better. Anti-semitism still exists, and is partly caused by the envy of other people -- envy that some Jews have the right to emigrate whereas the non-Jews cannot. There seems to be still difficulties for Jewish kids to be accepted at the university, a fact that Kapitza denied by saying that 3% of the students at Moscow University are Jews. Feinberg and Lifschitz told me, it is extremely hard for Jews to enter the Moscow University. Those two statements may not be, by the way, contradictory. It is also extremely difficult for young Jews to get the kind of job they want. Lifschitz is more pessimistic; he says the situation is deteriorating and the Jews have a more and more difficult time, and, in general, freedom and civil rights are diminishing and things get slowly worse. I don't know who is right. Lifschitz is known to me in the past as a man who always has a tendency of seeing things darker than they are. However, there is no denying that Lifschitz has now gotten the permission for travel abroad which he considers a fluctuation. But Feinberg and I consider it as a sign that things are getting slightly better. I heard three views on the Jewish question from three people who talked to me openly and frankly: Kapitza, Feinberg and Lifschitz. Their opinions range all the way from "not so bad" to "very bad".

Report of the

International Study Group

on Future Accelerators and High Energy Physics

Serpukhov, May 17-25 1976

Abstract

The Seminar "Perspectives in High Energy Physics" held in New Orleans, March 1975, established a Study Group to discuss the long-range requirements for facilities in High Energy Physics. A sub-group met in CERN, October 1975, and planned an Agenda for a meeting which was held in Serpukhov, U.S.S.R. in May, 1976. In this paper a summary of the work done in Serpukhov is given.

It begins with a review of the status of our present knowledge of the fundamental structure of matter and a statement of those future problems which can be clearly identified now and which will require new facilities for their solution. This is followed by a brief description of the status of today's accelerator technology and a review of projects that are now under active study as regional facilities. The study group has noted the need for close collaboration during the selection of the range of new regional facilities to ensure coverage of the broadest possible program of research. Included in this range may be a proton fixed target accelerator of up to several TeV, colliding beam facilities with a center-of-mass energy of up to several TeV for protons against protons, up to several hundred GeV for electrons against protons, and up to about 200 GeV for electrons against positrons. The participants have emphasized the importance of joint utilization of all such facilities by scientists of different countries.

The Study Group has stressed that the further progress of High Energy Physics will require in the future the development of an accelerator complex significantly more powerful than those planned for regional facilities. This complex is likely to be of such a cost as to be beyond the capabilities of any single region. Examples include facilities such as a proton accelerator of energy higher than 10 TeV and an electron-positron colliding beam facility of more than 200 GeV in the center-of-mass. In this connection several conceptual designs of that kind were presented and discussed.

In seeking to attain the more intensive international collaboration which is a fundamental prerequisite for progress toward the stated objectives, the Study Group recommends that the International Union of Pure and Applied Physics (Particles and Fields Division) be asked to initiate appropriate activities to this end.

I. Introduction

The historical development of science has made it especially appropriate that the physicists of all countries which are active in the exploration of the deepest aspects of atomic nature should be collaborating so intensely. It is gratifying that this collaboration has resulted in so much progress in our knowledge about the particles of which the world is made and of the laws that govern their behavior. It is equally gratifying that governments have provided the necessary framework within which the collaboration could take place. The fundamental knowledge being developed will become the basis of future technology and, equally important, will provide mankind with a greater insight into the nature of the universe.

The struggle for this knowledge is difficult, and although many concepts of nature have been deepened and new concepts have emerged, nevertheless, it is anticipated that vastly more extensive investigations will be required before our knowledge of the basic particles is as firm as is our understanding, for example, of electromagnetism.

The tools for investigating matter have become more complex and more expensive as we have penetrated deeper into the inner space of the atom. For this reason organizational collaborations have developed between groups of nations to allow them to participate in this exciting and necessary development. Thus the member nations of CERN and the member nations of JINR have established organizations which have enabled them to successfully develop

research in this field. Most importantly, the close collaboration between the regional laboratories has amplified their individual efforts.

As facilities that are now being planned on a regional basis are developed, ways should be found to help in coordinating that planning. Such mutual discussion and advice would ensure the coverage of the broadest possible program of research. Joint studies of new technology and organization of wider collaborative use of present facilities should occur. Joint construction of sub-elements of regional projects should be explored.

It can already be expected that the facilities needed to explore and clarify the next level beyond that available to facilities presently being contemplated will be so large that their realization will be greatly optimized -- and may only be possible -- by the pooling of the resources of all regions in a common effort.

We underline the statement of the countries participating in the "Helsinki Agreement on Security and Cooperation in Europe", which specifically mentions high-energy physics as a field for cooperation. It says that "scientific and technological cooperation constitutes an important contribution to the strengthening of security and cooperation among (the countries) in that it assists the effective solution of problems of common interest and the improvements of the conditions of human life".

II. Physics Projections

The development of high energy physics in the last two decades has led to a situation where there exist many facts, synthesized by theoretical ideas. These ideas have not yet reached a fundamental character similar to theories of electromagnetism and gravitation. Nevertheless, the present knowledge makes it possible to formulate long-standing fundamental questions of physics in rather detailed form. This makes it most probable that the discoveries made by the next generation of accelerators should provide us with new fundamental knowledge, first of all about the nature of weak interactions and their possible connection with electromagnetic interactions and also about the interior structure of hadrons and the range of validity of the quark hypothesis. Some of the most important unanswered questions are these:

> Do quarks exist and, if so, how are they confined in hadrons, and what are the forces between them? The recent results about hadron collision products which possess high transverse momentum have shown how little we understand about the internal dynamics of hadrons.

Secondly,

> Is the Weinberg-Salam gauge theory of weak interaction pointing towards the real solution or is it the wrong approach? The quantitative agreement of neutral current data with theory is strong encouragement for gauge-theories. Nevertheless, no deviations from a four-fermion structure of the weak force have yet been observed.

We believe that the energies of the planned regional facilities are indeed sufficient to begin attacking these problems. In the case of weak interactions there are definite energy ranges where we expect new phenomena to occur: At about 1000 GeV (center-of-mass) the simple four-fermion theory breaks down. It is vital to reach this energy in order to fully observe the structure of the weak force in its natural domain. The gauge theories suggest that there are new phenomena, such as intermediate bosons, already at about 100 GeV. This situation is analogous to what happened in the 1930's in electrodynamics: The natural limit was the classical electron radius (10^{-13}cm) corresponding to 100 MeV whereas new phenomena (pair creation) occur already at 1 MeV.

Our present knowledge of strong interactions does not indicate yet any definite critical energy range. The higher the energy, the more information we will get. We need to know whether further quantum numbers exist, such as charm, flavor, color etc., and at what energies they will appear. Some cosmic ray observations indicate that there are unexpected phenomena occurring at about 300-500 GeV (center-of-mass) which may point to new directions in strong interaction dynamics.

Also in the weak interactions the number of entities is still unknown. There may be a whole series of intermediate bosons, there may be Higgs-bosons of different kinds and a series of heavy leptons and neutrinos. The appearance of these seemingly unlimited number of entities of a given type, even in weak interactions, is reminiscent of the discoveries of elements in the 19th century. We have a few organizing principles, analogous to Mendeleev's classification. But the need for further synthesis is clear. We have much more to discover about the behavior of matter at energies higher than those available today.

The accelerators and storage rings which have been proposed address these problems in different ways:

1. Proton-proton and proton-antiproton storage rings attain the highest practicable center-of-mass energies at the price of lower luminosity. But the luminosities appear adequate for finding the weak-interaction intermediate bosons, provided the Drell-Yan production model can be applied. Present data are of some support for this model but far from conclusive.

The high center-of-mass energy available in storage rings is also of special significance in the study of strong interactions. The nature of the increase in the total cross-sections and of the energy-dependence of particle production mechanisms will be probed in a significant way. These facilities are also very useful to study the production of hadrons at high transverse momentum.

2. Future conventional proton synchrotrons, which provide high-energy particle beams incident upon stationary targets, will most likely explore frontiers different from that of center-of-mass energy. Their importance lies in the much higher luminosity available, in the diversity of external beams available, (including $\mu, \nu, e, \gamma, \pi, K, \bar{p}, \Lambda, \Sigma, \Xi, \Omega$), and in the opportunity of using targets of various atomic nuclei in order to study the nature of the produced systems in "status nascendi". High luminosity and choice of hadron beams are properties of

special significance in studying the production of hadrons of high p_T. The lepton-beams, especially the neutrino beams, are expected to continue to play the important role that they presently do in exploration of weak and electromagnetic interactions.

3. Electron-positron colliding beams at energies beyond PEP and PETRA allow the clean study of not only quantum electrodynamics and electromagnetic production of hadrons, but of weak interactions as well. Also, <u>any</u> charged heavy leptons or other charged non-hadronic pairs (including possible intermediate bosons $W^{\pm}$) would be produced, at a measurable rate, if they exist. Such storage rings are extremely powerful tools for finding heavy resonances with an appreciable partial width into an electron-positron pair. As already exemplified by the J/ψ and ψ', the decays of such resonances provide detailed, clean information, difficult to obtain by other means. For example, the Weinberg-Salam theory predicts the production of a neutral boson $Z°$, with mass $\approx$ 80 GeV, (at luminosity $\sim 10^{32} cm^{-2} sec^{-1}$) at a rate exceeding 10 per second. Thus e^+e^- rings of such energy may be an excellent way to study weak interactions. This may be the only method (or at least the best) to find and study Higgs bosons predicted by weak-electromagnetic gauge theories. If the mass of such a particle is less than 40 GeV, the branching ratio of $Z°$ into it (plus a charged lepton pair) is estimated to exceed 10^{-4}.

4. Electron-proton rings allow the clean study of the behavior of strong-interactions at short distances. The present theoretical ideas of the weakening of strong interactions at small distances, and their growing at large ones (asymptotic freedom), as well as the ideas of point constituents of the proton, are best tested in electron-proton scattering at the energies attainable by these storage rings. The question of the nature of proton constituents, and how (or whether) they are confined may be elucidated by study of the way hadrons are emitted after such a constituent is struck by the incident electron. The e-p storage rings may be a good way to produce and study heavy leptons (especially neutral), if they exist. Finally, weak interactions of the electron with hadrons are accessible as well, and such information would be a valuable supplement to what is obtained by other means.

In summary, it is expected that the planned regional facilities will lead to the solution of many outstanding problems and to new important discoveries. For example, when the center-of-mass energy of a few hundred GeV is reached, it is most probable that the existence or non-existence of the intermediate boson will be known. We then will know much more about weak interactions and their connection with other forces. Moreover, the range of understanding of strong interactions will be considerably widened, and the internal structure of nucleons will be much better known. It is possible that free quarks or new unexpected particles may be produced. Some of the larger regional projects may even yield information regarding the region of 400-500 GeV in the center-of-mass, where there are indications from cosmic ray data of new phenomena.

In spite of the importance of the energy regions explored by the regional plans, the need of higher energies and more varied beams will remain. After all the energies necessary to get into the interesting regions are attainable only by colliding beams of protons or electrons, and their antiparticles; they need to be supplemented by beams of other particles and by beams of higher intensity. The ISR had to be supplemented by stationary target machines with comparable (though smaller) center-of-mass energy in order to experiment with particles other than protons at those energies.

We definitely expect that the regional facilities will make important discoveries in the next 15 years and that some of the problems will be solved. But it is probable that a good part will still remain unsolved. We therefore strongly believe that so-called VBA facilities will be needed such as a proton accelerator with $E > 10$ TeV and with the possibility of p-p colliding beams, and/or e^+e^- colliding beam facility of $E_{cms} >$ 200 GeV.

III. Instrumentation Projections

While the experimental exploitation of a very high energy accelerator will in general require more sophisticated techniques, many experiments can use straightforward extensions of present methods. The initial exploratory experiments may well be less complicated than those which will be in progress at the lower-energy regional laboratories.

An active and vigorous experimental program could be carried out with present techniques, but improvements may be anticipated in many areas, such as

a) electronics -- integrated circuits will drastically lower the cost of multiwire proportional chambers and drift chambers. Drift chambers are already capable of good precision, $\leq \pm 50 \mu m$, and will be very useful in the measurement of angles and momenta.

b) calorimeters -- these devices are well suited to high energies, especially for the study of multiparticle processes over a wide range of angles, as for example, for measurement of jets at large transverse momentum. Recent work using liquid argon and uranium plates has resulted in improved resolution.

c) Cerenkov counters -- techniques are being developed to achieve good velocity resolution with increased acceptance.

d) transition radiation -- this technique will take over particle identification from Cerenkov counters in the TeV range.

e) computers -- microprocessors seem destined to play a large role in control, data acquisition, and initial analysis of future experiments. In addition, significant advances can be expected from large data processors.

f) large magnets -- superconducting spectrometer magnets will provide more magnetic field at a fraction of the power cost of conventional magnets.

g) data transmission between regional and/or national facilities -- this should be implemented in the most efficient way in order to optimize analyses of experimental data. In particular, data transmission at high rates utilizing satellites should be studied.

Other techniques, not yet conceived, may well play important roles in future experiments.

The development of experimental techniques is best accomplished through the work of individuals and small groups. Close communication between groups throughout the world is very important to the timely and efficient development of these techniques.

Although many experiments will become more difficult at high energies, others will become simpler. In many cases the techniques will be changed as the energy increases, so that the required precision and the cost do not become prohibitive. Some specific experiments were considered in the report of a CERN study group*. We conclude that in general the experimental costs will not increase relative to machine costs, but may even decrease.

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* A summary appears in VBA/CMS/1.

IV. Accelerator Projections

Having analyzed the design features presented at the meeting of the international study group on superhigh energy accelerators we have drawn the following conclusions.

The status of the various facilities with center-of-mass energies above 10 GeV can be divided into three groups:

Group 1: The facilities that are now operating successfully on a productive physics program (such as the FNAL accelerator of $E_{f.t.}$ = 500 GeV and the proton-proton ISR at CERN with $E_{c.m.}$ = 2x31 GeV), as well as those in the running-in stage (such as the CERN SPS of $E_{f.t.}$ = 400 GeV).

Group 2: Accelerator and storage rings under construction (such as the three e^+e^- colliding beam facilities under construction (PETRA in FRG with $E_{c.m.}$ = 2x(5-19)GeV, PEP in USA of $E_{c.m.}$ = 2x(5-18)GeV and VEPP-4 in USSR of $E_{c.m.}$ = 2x(5-7)GeV) together with planned projects and facilities under study. If these regional projects are realized they will form the basis for a vigorous experimental program of elementary particle physics until 1990.

The projects in this second group vary widely in cost and scope, but their construction is assumed to be within the resources of a single region.

The principal parameters of this group are presented in Table I. The proton facilities on the list assume superconducting magnets, and the recent advances of this technology have made this a very realistic assumption.

Group 3: Preliminary ideas concerning very big accelerators and storage rings with average orbit radii of 5-15 km and costs in the range of 3-6 times the cost of the FNAL accelerator or the CERN-SPS. Conceptual designs of examples of such facilities were presented to the meeting, and they are listed in Table II. The presentations made might be considered as the initial stage of an accelerator complex to form the basis for the inter-regional program of experimental high-energy physics after 1990.

It is hoped that by the time such a project comes near to its realization, advantage can be taken of further progress in technology, and that, for instance, for the magnets for a fixed target accelerator superconducting materials of higher critical parameters can be used in magnet construction. For the r.f. systems for a possible large e^+e^-, it is hoped that the development of superconducting r.f. cavities can be further advanced. In both these fields, development work should be strongly encouraged.

In conclusion it is not easy to determine what ultimate limits will be imposed on new accelerator projects by technical considerations. It appears that the size and scope of projects presently envisaged will be limited by financial resources only. Technological developments over the next one or two decades may indeed result in more economical solutions being found for the construction of high energy accelerators.

It is recommended that a continuing study should be undertaken through an inter-regional collaboration to ensure that the technologies which are likely to influence future accelerator design are covered by adequate development programs with minimum needless duplication. It should be recognized however, that the potential industrial importance of such technologies adds a further dimension to the problem of international collaboration.

TABLE I

Region (Country)	Facility	p lab.s. GeV	e lab.s. GeV	pp c.m.s. GeV	$p\bar{p}$ c.m.s. GeV	pe c.m.s. GeV	e^-e^+ c.m.s. GeV	Circumference km
Japan	TRISTAN	180	17	360		~110	34	~2
FRG CERN	PETRA LSR LEP	 400 	19 20 < 100	 800 	 800 	 180 	38 < 200	2.3 6.4 < 50
USA	PEP Doubler ISABELLE POPAE	200 1000 200 1000	18 20 20	 400 2000	 --- 	100 130 280	36 --- 	2.2. ~6 ~3 5.5
USSR	VEPP-4 UNK	 2000	7 20	 4000	 4000	 400	14 	 18

TABLE II

International	VBA fixed target	>10.000		(>20.000)	(>20.000)			30-60
	VBA e^+e^-		>100				>200	>50

V. Conclusions

The foregoing survey leads us to the following conclusions:

A) The present status of the science of the structure of matter poses fundamental problems which require a new generation of facilities of the types listed in Table I. Such facilities are within the capabilities of the individual regions and are needed for continued progress of this field of research.

B) The success of regional and interregional collaboration in the past provides a good basis for extending and strengthening this collaboration in the new generation of regional facilities.

C) Looking beyond this new generation of regional accelerators we foresee the need for an accelerator complex (VBA) which will require international collaboration of all regions concerned.

VI. Recommendations

1) Efforts should be made to coordinate the design and construction of new regional facilities. Consultations and exchange of experiences should be encouraged in order to optimize the diversity of facilities and to enhance the efficiency of construction and operation. The Study Group also recommends joint studies of new technology (e.g. superconductivity, new detectors and other experimental apparatus) and joint design and/or construction of components of regional projects.

2) Joint utilization of regional facilities by scientists of different regions should be organized on the basis of present and future arrangements or agreements. The general availability of regional installations is essential to enable scientists of different regions to take advantage of facilities with complementary research poten-

tialities.

3) International collaboration should provide for studies leading towards the realization of a next generation of super-high energy facilities, following the regional projects referred to above (examples are given in Table II). It is expected that these facilities will be so large that their realization will be possible only by pooling the resources of all regions concerned into common international projects.

Creation of a super-high energy accelerator complex (VBA) involves especially complicated scientific, technical and organizational problems. These will require several years of continuing studies and discussions. The Study Group recommends that these discussions begin in the near future leading to the start of the design of the VBA in about 10 years.

4) In view of the need for these extensions of international collaboration, the Study Group suggests to the IUPAP Division of Particles and Fields to initiate these activities in an appropriate form, for example, by appointing a sub-committee for the purpose of organizing working groups and future meetings such as the present one.

Appendix 1

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V.F. Weisskopf
R.R. Wilson
L. Lederman
M. Barton
R. Diebold
J. Bjorken
D. Eulian (secretary)

* CERN Member States

G. von Dardel
U. Amaldi
D. Husmann
K. Johnsen
A. Rousset
D.B. Thomas

as expert:

G.A. Voss

* The delegation from the CERN Member States was selected by the CERN Scientific Policy Committee and was under the leadership of the Chairman of the European Committee for Future Accelerators.

Appendix 2

AGENDA

17 May **Morning Session** — Chairman: V. Weisskopf

Topic I: Status of national and regional facilities.

1) PETRA, PEP
   Speaker: G. Voss
2) VEPP-4
   Speaker: A. Skrinsky
3) Energy Doubler
   Speaker: R. Wilson

Afternoon Session — Chairman: G. von Dardel

Topic II: Presentation of scientific and technical aspects of big accelerators.

1) POPAE
   Speaker: R. Diebold
2) ISABELLE
   Speaker: M. Barton
3) LSR-pp
   Speaker: K. Johnsen

18 May **Morning Session** — Chairman: A.A. Logunov

4) LSR-ep
   Speaker: K. Johnsen
5) UNK
   Speaker: V. Yarba
6) Colliding $p\bar{p}$ - rings
   Speaker: A. Budker
7) TRISTAN
   Speaker: Y. Yamaguchi

Afternoon Session — Chairman: K. Lanius

Topic III: Presentation of general scientific and technical aspects in the construction and utilization of high-energy systems.

1) 10 TeV proton accelerator with a fixed target
   Speakers: D.B. Thomas
   R. Wilson
2) 100x100 GeV electron storage ring
   Speaker: K. Johnsen

19 May Morning Session — Chairman: Y. Yamaguchi

Topic IV: Physics Projections

1) Theoretical Considerations
Speakers: M.A. Markov
J. Bjorken

Afternoon Session — Chairman: L. Lederman

2) Physics to 1980 - Existing Facilities
Speakers: L. Lederman - FNAL pp
A. Rousset - SPS, ν and μ
U. Amaldi - ISR

3) Physics to 1985 - Next Generation of Regional Accelerators
Speakers: U. Amaldi - LSR
Y. Prokoshkin - UNK

20 May Morning

Visit to the IHEP Laboratories

Afternoon Session — Chairman: U. Amaldi

Continuation of previous session
Speakers: S. Gerstein - UNK
G. von Dardel - PETRA

Topic V: Physics Beyond 1985: VBA
Speakers: A. Rousset - ν at 10 TeV
G. von Dardel - hadrons at 10 TeV

Topic VI: Experimental techniques Beyond 1985
Speaker: R. Diebold

21 May Morning Session — Chairman: L. Soloviev

Topic VII: Concluding Discussions

1) Review of situation - V. Weisskopf

2) General discussion

Afternoon Session — Chairman: V. Djelepov

3) General discussion

24/25 May

Preparation of Final Report

Appendix 3

List of papers submitted at meeting:

From CERN Member States:

VBA/CMS/1 W. Willis, "Summary of the 1976 CERN Study on the Use of a 10 TeV Proton Accelerator and of Electron-proton Colliding Beams", CERN-SD Note No. 1.

VBA/CMS/2 W. Willis, "Future Trends in Detectors for Multi-TeV Accelerators", CERN-SD Note No. 2.

VBA/CMS/3 G. Charpak, "Some Considerations on the Future of Proportional Chambers", CERN-SD Note No. 3.

VBA/CMS/4 U. Amaldi and L. Di Lella, "Physics at the CERN LSR", CERN-SD Note No. 4.

VBA/CMS/5 K. Johnsen, "Studies of New Large Storage Rings at CERN: pp, $\bar{p}p$ and ep", CERN-SD Note No. 5.

VBA/CMS/6 R. Billinge, "VBA Fixed Target Parameter List".

VBA/CMS/7 U. Amaldi and H. Lengeler, "Collinear Accelerators for High Energy e^+e^- Collisions", CERN-SD Note No. 7.

VBA/CMS/8 G. von Dardel, "Hadronic Physics at a 10 TeV Fixed Target Machine".

VBA/CMS/9 G. von Dardel, "The PETRA Physics Program".

VBA/CMS/10 D.B. Thomas, "Superconducting Magnets for a 5 to 10 TeV Proton Synchrotron".

VBA/CMS/11 "LEP Parameter List", Version 1, compiled by E. Keil.

VBA/CMS/12 "Parameters for Superconducting LSR", Version 1, edited by K. Johnsen, CERN/ISR-LTD/75-39.

VBA/CMS/13 M.G.N. Hine, "International Data Communications for European High Energy Physicists - and others".

From USA:

Mark Barton/W.B. Sampson. "Impact of A-15 Superconductors on Future Machines"

"A Proposal for Construction of a Proton-Proton Storage Accelerator Facility - ISABELLE 1976 (revised)

"A 1000 GeV on 1000 GeV Proton-Proton Colliding Beam Facility", (POPAE).

R. Wilson. "A Ten TeV World Accelerator," May 1976.

J. Bjorken. "Physics Issues and the VBA," May 1976.

From USSR:

"Accelerating-Storage Complex on the Basis of the IHEP Accelerator (UNK, 2-5 TeV)".

A. Budker, "Electron Cooling of Antiproton for UNK".