PANEL DISCUSSION ON THE CHALLENGE OF ULTRA-HIGH ENERGIES*)

U. Amaldi (Moderator)
J. Lawson
D. Keefe
B. Richter
M. Tigner
G. Voss
W. Willis

U. AMALDI

During this panel discussion we want to review the main topics presented during these days and to compare the points of view of those participating in the meeting in an open debate. We do not aim to reach firm conclusions, and we do not think that the arguments which will be brought forward will modify the intention of some of us to continue to pursue their lines of research. But we hope to arouse some interest in other people and give at least a framework for the further activities of ECFA and, in general, of our community.

I would like to remind you that the purpose of this meeting is to look for accelerators beyond the ones discussed in the two ICFA Workshops[1,2]: electron-positron colliders of 2×0.35 TeV and proton-antiproton colliders and fixed-target accelerators of 20 TeV per beam. If I can extrapolate from previous experience and say that at least a factor of 3 is needed for a new generation, the machines we have been trying to contemplate in our challenge of these days certainly have energies greater than 2 TeV for electrons and greater than 60 TeV for protons. The typical point-like cross-sections for these two machines are very small. One can argue about what is interesting as far as hadron physics is concerned; but if one takes (as was done in the ICFA Workshops) the point-like cross-section multiplied by the ratio of the strong to the electromagnetic coupling constant, it turns out that for both types of accelerators the interesting cross-sections are of the order of 10^{-38} cm^2. This implies 100 events per year with a luminosity of 10^{33} cm^{-2} s^{-1}, and sets the framework for any realistic discussion. Of course, one can argue that there are special processes having much larger cross-sections: for hadron accelerators the total cross-section is of the order of 10^{-26} cm^2, and in proton-antiproton colliders the cross-section for producing W's and Higgs' bosons increases with energy. In my opinion these arguments do not modify the conclusion that luminosities definitely larger than 10^{33} cm^{-2} s^{-1} are needed, but justify the division of our discussion into two parts: we want to review first the acceleration methods which have been presented during the meeting and hear opinions on those methods which are likely to survive; and then, if time allows, we would like to see how they could be fitted into an accelerator complex by considering luminosity, staging problems, space requirements, and so on.

Table 1

Particle accelerator classification according to J. Lawson[3].
The accelerators in italics were presented during the meeting.

	Accelerated particles in free space		Accelerated particles in a medium
	No free charges in the accelerator	Free charges in the accelerator	
Harmonic field	Synchrotrons *Linacs* (M. Tigner) *Laser and grating* (R. Palmer) *IFEL* (C. Pellegrini)		*Beam-wave acc.* (J. Nation) *Beat-wave acc.* (T. Tajima + C. Joshi + D. Sullivan) *Inverse Cherenkov* (C. Pellegrini)
Field not harmonic	Induction linacs Multigap accelerator	*Wake-field acc.* (T. Weiland)	*Beam-front acc.* (M. Reiser) *ERA* (U. Schumacher) *Superaccelerator* (F. Winterberg)

---

*) Text edited by U. Amaldi.

As a starting point, I will use a somewhat modified version of the table shown by John Lawson in his lecture (see Table 1). The acceleration methods in italics have been discussed during the meeting[3)]. In parenthesis I have written the names of the speakers. In the first part of the panel discussion Denis Keefe and John Lawson will give us their points of view on the collective accelerators appearing in the last column of the table, whilst Burton Richter and Maury Tigner will speak about the accelerators mentioned in the first two columns. I hope we shall have a lively discussion after each intervention of the panel members.

D. KEEFE

In judging how well collective acceleration may be expected to contribute in the ultra-relativistic domain, it is important to bear in mind that past work on collective methods has almost exclusively been concentrated on the problem of accelerating ions at low velocities. This in itself is a very difficult problem; some schemes work quite well at picking up the ions at near-zero velocity and accelerating them to $v \simeq 0.1$ c but run into difficulties beyond that, whilst others are better suited to acceleration at higher velocities but are ineffective near $v \simeq 0$. In my view, any such accelerator system to bring ions up from $v \simeq 0$ towards $v \simeq c$ must be a multistaged device, with the early and late stages perhaps employing different principles of collective acceleration. The electron ring accelerator stands unique in this regard: first, in its having been conceived of from the beginning as a multistage device and, secondly, in its having been demonstrated to work, in this mode, in the laboratory[4)]. (Note, however, that success with electron ring acceleration has not come easily; it has taken 20 years and a large amount of research at several laboratories to obtain the results now reported from Dubna.)

The application of collective acceleration to ultra-relativistic energies ($\gamma \gg 1$) therefore needs a change in attitude. Advances in the technology of high-energy accelerators have now set an ante which is very high ($\gamma \sim 1000$ for protons, $\gamma \sim 100{,}000$ for electrons) for any new player to face. Nonetheless, there are several features inherent in collective systems that could have exciting applications in the future. First, there is the possibility of using the strong electric field of an intense electron cloud to provide focusing and bending for positive ions (protons), a system first suggested by Budker[5)] and now under study by Rostoker[6)]. For hadron colliders the enemy is the guide field, which in strength is approaching an ultimate limit of 10 T or so; collective focusing has the potential for exceeding this considerably. (That such a scheme is inapplicable to antiprotons should be considered a detail at this point.) Secondly, experiments have already demonstrated very high collective fields (~ 100–200 MV/m), albeit at low "phase-velocity" and over only short distances. Thirdly, certain collective methods exploit a "negative-energy" feature in coupling to the accelerated beam; in this case the larger the beam loading the higher are the gradient and the efficiency.

In short, it seems that hadron colliders could benefit only from the collective focusing/guiding schemes, whereas e^+e^- linear colliders stand to gain from higher accelerating gradients and better efficiencies. There is little to say at this time about the first application (focusing/guiding) and we must await experimental results. We can, however, discuss certain features of a collectively driven linac for high γ. It is clear that the collective devices must be made modular — say, each a few metres in length — and stacked in series with suitable phasing to form the high-energy linac. In a stacked sequence of collective accelerating modules such as this, one has the choice of extracting the spent driving electron beam at the end of each module and recovering its energy, or of reprocessing the beam and inserting it into a succeeding module. Operating parameters for the drive-beams would be presumably in the few megaelectronvolt, multi-kiloampere range.

With electric acceleration in mind and looking back at the catalogue of collective schemes discussed by Sessler and by Nation[3)], I conclude the following:

i) *Localized space-charge* or controlled moving electrostatic wells have no application for large γ.

ii) *Slow waves* on beams (cyclotron or space-charge) — and electron-rings — could be useful in principle, since v_{ph} can come close to c, but probably are impracticable. Since one has $v_{ph} < v_e < v_b < c$, where the subscripts denote the phase velocity, drive-beam velocity, and accelerated beam velocity respectively, the accelerated particles will tend to slip out of phase with the wave. This need not be a problem if the module length is chosen correctly and successive modules are suitably phased. Much more serious, and probably fatal, is that the accelerating electric field of the wave varies as $(\gamma_e - \gamma_{ph})$ and hence tends to zero at high phase velocity. [As usual, γ is $(1-\beta^2)^{-1/2}$.]

iii) *Electron beam beat-wave devices,* in which two slow waves can be beaten together to generate a fast wave (i.e. $v_{ph} \cong c$), are clearly applicable and worthy of much more study than they have received. The systems explored by Friedman and Velikhov have been described by Nation[3)]; both employ an intensity-modulated electron beam which undergoes further (spatial) modulation by a zero-frequency wave — a periodic solenoid array in one case, a rippled wave-guide in the other. The result is a slow ($v_{ph} < c$) forward wave and a fast ($v_{ph} \cong c$) backward wave, and the latter is clearly the one of interest for the high-γ domain.

iv) *Laser beat-wave systems* such as that described by Tajima, Joshi and Sullivan[3)] rely on beating two laser waves (ω_1 and ω_2, chosen such that $\omega_1 - \omega_2 = \omega_p$) in an underdense plasma ($\omega_1, \omega_2 > \omega_p$). Although each wave has a phase velocity greater than c in underdense plasma, the beat action can produce a plasma wave that propagates at a phase velocity slightly less than c. Computer simulation of the non-linear optical mixing indicates a very high degree of charge modulation in the wave and consequently enormous accelerating gradients, perhaps in the range 1–100 GV/m. Although there are many serious issues that remain to be addressed for the practical exploitation of this phenomenon (e.g. coherence length, phasing, staging), research that could lead to such high accelerating gradients seems well worth pursuing.

Finally, it should be emphasized that both collective methods and far-field ("real-photon") methods share an advantage, in principle, over conventional methods, in that the electric field at the location of the accelerated beam can be very high, whilst the field at the walls of the system remains within manageable limits. Ultimate collective accelerating gradients need not then be bounded by the breakdown gradient limits of material surfaces.

J. LAWSON

I was asked to comment on the actual physics of collective field accelerators, particularly bearing in mind the possibilities of going to extremely high field gradients — hopefully to introduce some air of controversy into the proceedings. Nobody enjoys controversy more than I do, but on the whole I must say that I very much agree with the picture as presented by Denis Keefe. I might just make two general points.

First, are there any radically new possibilities that we have not thought of yet? Collective accelerators have been thought about for a long time: there is a book on the subject by Craig Olson and Uwe Schumacher. Olson lists 34 possibilities, some of which he rejects rather quickly. So if you take up a new subject it may have already been booked.

Secondly, I believe we can learn something from the story of the electron ring accelerator, which was studied in great detail. This study gave rise to some interesting new physics, and even if this did not lead to the building of a large machine the subject was nicely covered, as can be seen from Dr. Schumacher's very elegant presentation of the subject. So even if things do not show immediate promise, they should be studied through to a neat conclusion. By the way, the knowledge is now there to be used again; such a ring is obviously very relevant to the new idea of the wake field accelerator — which we have also heard about at this meeting.

U. AMALDI

Let us now discuss these two opening contributions on collective accelerators.

G. VOSS

I would like to ask if the beat-wave accelerator is really a machine for extreme high energies. Is it possible to make the phase velocity equal to the velocity of light?

C. JOSHI

The answer is that it is possible. If one wants to make the group velocity of the waves exactly equal to the phase velocity of the plasma wave, then the phase velocity becomes roughly 0.9999 c. But if one varies the plasma density, the plasma wave propagates towards a slightly higher density region. It becomes non-synchronous with the electromagnetic wave but, since the density decreases, the wave number of the plasma wave must go down and the phase velocity very quickly exceeds the speed of light. So there are ways of making them exactly equal.

W. WILLIS

I want to comment on a statement by Denis Keefe. He said that a single wave moving through a plasma cannot accelerate particles. It is interesting to see that in principle a single wave packet moving through even a very thin plasma will accelerate particles. This is because, as can be seen from the sequence of movie pictures we saw, the wave packet picks up electrons; eventually it leaves them behind, but there is an excess of electrons generates a wave packet and this excess of electrons is the collective field which can accelerate particles. This is efficient only if the individual electrons become relativistic, but this was in fact the case that we saw simulated. To me this illustrates the ability of such strong laser fields to imprint their image on the plasma in a way which is not so sensitive to plasma dynamics.

J. NATION

I would like to clarify one comment made by Denis Keefe on the space-charge wave that I think may be a little misleading. This is a slow wave that will not reach the speed of light and is thus subject to the same limitations as the beam front accelerator unless multi-stage accelerator modules can be developed. I think, however, that it has a number of applications in fields that are different from high-energy physics. On the other hand, I do think that the beat-wave system, with its almost infinite variety of variants, has already been demonstrated in high-power microwave generation experiments, and this clearly has some potential for very high energy physics.

M. REISER

I want first to comment on the difference between beam-wave and beam-front accelerators. In the first case, as the name implies, one generates waves on a relativistic electron beam with a phase velocity less than the speed of light; there are many little buckets and, since it is a wave on the beam, generally one cannot get very high gradients. On the other hand, in the beam-front accelerator one has a sort of snowplough system: there is a discontinuity at the beam front with a very high charge density, and in principle one can get very high field gradients but there is only one bucket in which to carry the ions. Thus we have a trade-off in the sense that a shorter accelerator can be built but higher peak powers are needed. The other comment is that in both systems the upper limit for the energy is in fact due to the energy of the electrons. With the development at Livermore of 50 MeV electron beams we would have an energy factor of $\gamma = 100$ for the electrons, and this could give us 100 GeV protons. I suggest that even in the scenario for high-energy accelerators we do have a problem of getting up to 100 GeV in a short distance and in an inexpensive way, and that collective accelerators could be used to reach relativistic velocities; then some other scheme could take over, for instance the laser accelerators.

Finally, I want to say a few words on what we have to do to reach ultra-high energies: we have to use the relativistic electron beam as a medium to generate accelerating fields that travel faster than the electrons. We would have to produce fast waves rather than slow waves as pointed out by Denis Keefe. In the beam-front accelerator we have to generate a disruption, a snowplough, which starts at the rear and sweeps through the beam to the front. Nobody has given any thought to it but, for the reasons I have just stated, I would not agree with Denis Keefe's conclusion that collective accelerators with localized space charge wells are not interesting for high-energy physics.

U. AMALDI

Before closing the discussion on collective accelerators I think it could be interesting to hear some comments on the possibility of staging them to reach ultra-high energies.

M. REISER

In all the schemes based on relativistic electron beams, we have to stage. At present the pulse lengths are of the order of 10–30 ns; so, once we have accelerated over an equivalent distance, the ions have to get into the next stage. One could think of making a longer pulse length, maybe 100 or 200 ns, but this does not change the problem. We do not have specific schemes at this point, but I do not see any fundamental reason why staging could not be done.

J. NATION

The only comment I can make is that staging concepts have not been addressed in any serious fashion and that there was even less work done on electron accelerators. Nonetheless a collective accelerator study was carried out in the United States last year, or the year before, in which collective accelerators were viewed as possible candidates for injectors into high-energy machines[7)].

There are certain accelerator mechanisms for ions, such as cyclotron waves, for which I would find it very difficult to visualize any mechanism for staging. The beam-front accelerator and the space-charge wave accelerator give beams which are well defined, and the problem of staging does not seem to me to be insurmountable, but it is one that has not been properly addressed. In particular with the wave accelerator it does not seem to be a terribly complicated problem; however, no attention has been paid to it at this stage — it is too early for it.

F. WINTERBERG

If one wants to accelerate an appreciable number of particles to the large energies we are speaking about, one needs a 1000 TW accelerator. I express my doubts that most of the concepts proposed could qualify for such an accelerator. Let me now remark that in the superaccelerator the energy source is inductive energy and a large coil is also an inductive device. A coil about the size of this room could store such an energy with a field of 20 kG, which shows that the required energy is not terribly large. In a superaccelerator the electron cloud is pushed by a travelling magnetic wave and the discontinuity can move with a velocity as close as one wants to the velocity of light. (There is also a bunch of electrons moving behind, maybe at smaller velocities.) So, as far as I can see, this is a scheme with which one can achieve the required accelerations.

M. TIGNER

I would just like to make a little promotional speech for the plasma beat-wave accelerator. I do not know if it works, but I am very excited about the idea. There are a number of questions which we have just skated over here and which will have to be dealt with. I think that those of us in this community who are concerned about the problem of where we are going from here, ought to try to understand this concept in our own terms. If we can be brought to the point where the enthusiasts are now, then I think we have to push it experimentally.

F.J. KELLY

There may be some opportunities for doing work on the laser beat-wave accelerator at radio frequencies using the ionosphere as a plasma at a place like Arecibo, where they have very good diagnostics on the ionosphere and also ionosphere heaters at high frequency.

U. AMALDI

Let us now leave collective accelerators and consider the accelerators which do not contain electric charges, and thus appear in the second and third columns of Lawson's table.

B. RICHTER

Ugo Amaldi has asked me to make some prejudiced comments on the accelerator systems that have been discussed — and I am always willing to make prejudiced comments.

We have heard of two kinds of lasers: i) far-field lasers from Pellegrini, and ii) near-field lasers from Palmer. We have also heard about a class of devices which can be called *transformer accelerators:* iii) wake compression (Weiland), iv) trapped particle wake (Balakin), v) undulator powered, vi) klystron powered. They all use copper structures, they all transform energy from one beam into another; but, while some do it with wake fields, other do it with trapped particles.

i) Pellegrini's system is an inverse free-electron laser. An electron beam is made to wiggle in a magnetic field, and by the relativistic transformations the electron wiggles are in synchronism with the laser beam and the laser feeds energy into the beam. At least in calculation the system promises acceleration of the order of hundreds of megavolts per metre, but it is limited by synchrotron radiation. It saturates at some energy, and at what particular energy depends on how clever one is in tailoring the magnetic field. The example given by Claudio Pellegrini is limited to about 300 GeV. It is a remarkable example, because a year ago nobody believed an inverse free-electron laser would work at all — however, high-energy physics is extremely greedy. We want more, and so, in the spirit that I am supposed to pose contentious questions, I ask: Is this a dead end?

ii) Palmer discussed a near-field accelerator in which the particles are accelerated by the evanescent wave created close to a grating by bringing in laser beams from the appropriate angles. One talks about gradients of 10 GeV/m with lasers that are beyond the state of the art. Apart from the power of the lasers there are several problems, and the one that bothers me most is the blow-off plasma, which is due to the fact that this system heats the surface to enormous temperatures. Palmer discussed what happens to the atoms, which do not move very far; the distances are small compared to the wavelength. However, my calculations show that the electrons blown off move to 5 or 10 wavelengths from the grating, and this is very worrying because of the interaction of the laser beams with this electron cloud. In spite of this remark, this possible accelerator is extremely interesting even with gradients lower than 10 GeV/m. It is interesting just because it promises big accelerations, but it has a long list of nasty problems, such as laser power, alignment, energy spread in the beam, etc. In particular the energy spread is important for the final focusing of colliding beams, whilst probably it does not matter for fixed-target experiments.

The second class of accelerators uses copper structures. Maury Tigner talked about thermal limits and ended up with numbers of the order of GeV/m as limitations from thermal gradients, but there is another limit which is fundamental and certainly applies to all structures. The conduction bands inside a metal are about 1 eV below the surface potential, and the surface thicknesses are of the order of a few angstroms. By dividing 1 eV by 10 Å one gets a gradient of 1 GeV/m, which may be the limit for any kind of pulse longer than a few picoseconds. Such a gradient would open the conduction band out into the vacuum; if it would last for 10 ps or so, the extracted electrons would propagate for distances of the order of centimetres and suck all the energy out of the system. This may be the fundamental limit at frequencies up to 50 or 100 GHz but it certainly does not apply to optical frequencies because at optical frequencies the electrons are pushed back into the surface very rapidly.

iii) Let me now consider the wake field accelerator. Here a ring of electrons is accelerated and injected in an axially symmetric structure. A wake field is generated which propagates down towards the centre hole of this structure, where the second beam is accelerated. Gradients of 200 MeV/m appear to be reasonable with ring beams having a peak current of tens of kiloamperes at energies of tens of MeV. The problems are in the transverse stability of these rings during both the acceleration and the deceleration phase.

Of course the other problem is breakdown. Here again the question is: What is the upper limit? The breakdown problem is more serious for positron than for electron beams, since for accelerating positrons the first "bounce" of the wake field is used. If one gets any breakdown the first time that the wake field reaches the central hole, the reflections are greatly distorted and there may be problems with the "bounced" field. In spite of these remarks I think that this is a very promising concept which should be pursued vigorously.

v) Another accelerator which was not mentioned during the meeting is the undulator drive, discussed by Sessler on other occasions. Schematically one has an undulator system, in which a low-energy high-current beam flows. The undulator is arranged to give wavelengths which fill a resonant structure, which is coupled all along to the accelerating structure. Topologically one can also put these two structures together. In principle at least, the accelerator is made by having an undulator and an accelerator combination and then an induction module which reaccelerates the high-current low-energy beam before re-using it in the next undulator.

vi) A final example of this class of accelerators is the perfectly conventional one based on klystrons, which are put alongside the machine. In fact the only difference between this and the undulator is one of topology: in the first case the beam rides parallel to the accelerated beam, while in the second case the source beam rides perpendicular to the accelerated beam.

All the transformer accelerators need gigawatt peak power sources, and such sources are made either by wake fields, or by undulators, or by klystrons. They all have to worry about surface breakdown, and in this connection the only thing we know is that lower bounds to the field that copper can hold are > 160 MV/m according to some ancient experiments at SLAC and > 100 MV/m in a recent experiment at Novosibirsk. Both these limits are merely lower bound, because neither laboratory had large enough power sources to drive the cavities to breakdown and actually see what the limits were. All these transformer accelerators will have to worry about the beam stability of the low-energy beams. They all will accelerate protons as well as electrons, but the injection energies in the case of protons have to be of the order of 10 GeV. All these systems require beam emittances of the order of what we are hoping for in the SLC, if they have to work as colliders.

My personal summary is that the near-field laser and the copper systems are the most promising accelerators. All need much work, and clearly the laser system needs the most because it has, in addition, all the plasma stability problems. Superconducting cavities seem to be out of the question for very high energy. It is interesting to note that in this meeting no one has mentioned superconductivity for very high energy machines. The reason, I think, is that people have come to agree in general that a very high gradient is really necessary to bring the cost of these big machines within control. Also, synchrotrons are obsolete because they cannot reach the hundreds of MeV/m to tens of GeV/m of which we are now speaking. For example, the machine in the desert, so much talked about recently, has only 16 MeV/m, which is really small.

M. TIGNER

I want to start by stating that on the general lines I agree with the conclusion of Burton Richter. As far as our future activity is concerned I think we ought to pursue at least two of the near-field schemes which work in the microwave wavelengths: the wake field (or broad-band) concept, and the RF pulse-driven structure (or narrow-band accelerator) of the type discussed by Sessler. We must find out what the limitations are, in particular the surface field limitations. Richter's remark about what the electric field limit on the surface might be is a little optimistic and is certainly an upper limit. How close we will get to that I do not know, but I think that there is evidence that the limit is somewhat below one GeV/m. I also think we should work hard on at least one of the near-field schemes that uses (or abuses) a structure driven at light frequencies by lasers. Finally, as I already said, we should pursue the idea of at least one of the accelerators in a medium, and at the moment the plasma beat-wave accelerator looks best.

As far as sources go, it seems to me that we ought to be working hard on at least one far-field source, and the only one that looks interesting at the moment is the free-electron laser. We certainly need to pay more attention to the relativistic electron beam devices because of their potential for much higher efficiency, which we badly need. Finally, I am happy to know that the very short pulse power sources which will be needed for the wake field geometry will be worked on.

With regard to superconductivity I would like to say two things. We have been implicitly assuming that we can squeeze down the beams to dimensions smaller than one micron so as to get the luminosity we need with respectable powers. If this will not be possible, I think we should not so easily drop the idea of energy recovery, which allows the use of high-current beams so that one does not need the enormous densities that we are talking about. It is certainly true that superconducting linacs have not performed up to the theoretical limits which are predicted by the fundamental theory of superconductivity. However, I should mention that small devices have worked up to that limit, so there is not any real basic physics problem. There are certainly many applied physics problems, and there is certainly a motivation for continuing. One could object that if Nb_3Sn works, the maximum gradients are of the order of 100 MeV/m and not more, so that the linac will be too long and will cost too much. To this I would add that even in my short lifetime I have heard at least three times that the electron synchrotron was dead because the cost of the RF system was too large. But history has been different because of new sophisticated technology and cost-cutting beyond anything we were able to imagine, even 10 or 15 years ago. The same may happen for superconducting cavities.

B. RICHTER

I have a completely different opinion because the Q-values of superconducting cavities are too low, so that the power dissipated in the cold structure is too great. For a 500 + 500 GeV collider the refrigerator power, at 100 MV/m, is 4 GW. If superconductivity is to be appropriate for very high energy machines, it seems to me that it is not only the gradient that has to become larger but that there have to be some dramatic improvements in the Q-value of the cavities.

M. TIGNER

As I explained in a series of lectures at SLAC last summer, those Q figures are wrong. They are the Q-values achieved today in rather large devices. However, they are far smaller than the ones calculated from first principles and, in fact, than the ones that have been seen in small devices. One of the advantages of Nb_3Sn, in addition to its being able to withstand a much higher magnetic field on the surface, is that it has a much larger energy gap. This means that the transition temperature is much higher and, by virtue of this, the surface resistivity is very low: the theoretical Q-values for such cavities are of the order of 10^{14}–10^{15}. Thus there is no fundamental physical law that prevents reducing the powers that you mentioned by about a factor of 100.

D. KEEFE

I intend to comment on power sources and the approach towards collective acceleration in linacs. Previous discussions have emphasized the need to strive for higher efficiency and higher gradient in a linac. Someday a collective method, in which the driving and the driven beam are superimposed, may provide the answer to these two needs and, in addition, allow a gradient higher than that set by the breakdown strength. There is much room for improvement with present systems, however, before one actually needs the last feature offered by collective methods. SLAC, for instance, when upgraded to 50 GeV for the SLC, will have a gradient of 17 MeV/m, still a factor of 5 or so below the electrical breakdown limit. Breakdown limits as high as 100 MeV/m have been reported at Novosibirsk. If one traces the direction of people's thoughts on improving linacs, it is towards geometrical and topological arrangements that get closer and closer to the appearance of a collective device. Figure 1 shows a simple sketch of a section of the S-band linac ($\lambda = 10$ cm) at SLAC. Radio-frequency power for a 12 m length of the structure is derived from a single driving electron beam in a klystron less than 2 m long, and fed through long wave-guides that penetrate the shielding. The resonant structure acts as a voltage step-up transformer, and 120 MeV can be delivered to the accelerated beam in the 12 m section.

For future resonant RF devices the lines of reasoning or speculation have followed several steps, which I shall illustrate here by just a few examples.

Step 1: Use shorter wavelengths. The discussion of scaling by Tigner$^{3)}$ shows the advantages. As the skin depth becomes smaller, wave-guide losses become serious and the RF plumbing must be minimized. Sessler$^{8)}$ has suggested using millimetre waves from a multistage free-electron laser (FEL) — the driving electron beam being rejuvenated in a sequence of induction cavities. Whilst details of the RF coupling are not mentioned, it is clear that the concept calls for the FEL and the millimetre RF accelerating structure to be run side by side in very close proximity.

Step 2: Incorporate the driving beam(s), the accelerator structure, and the accelerated beam in the same vacuum envelope. This arrangement simultaneously minimizes the RF transmission difficulty and eliminates nasty insulator problems. The proton klystron described by Skrinsky falls into this category. Another example grew from the suggestion by Maschke$^{9)}$ that klystrons with higher power might be made, not by going to higher voltage but, better, by using many beams of small cross-section and higher total current. When Faltens and I examined this idea a few years ago we concluded that the best way to use such a device was not as a power source for a separate accelerating structure, but to arrange the many small drive-beams in an annular array around the structure and accelerate the high-energy beam on axis.

Step 3: Incorporate the driving beam(s) and the accelerated beam in the same envelope but without any structure intervening between them. The beat-wave accelerator schemes worked on by Friedman and by Velikhov [see Nation$^{3)}$] fall exactly in this category. The structures providing the zero-frequency wave lie entirely outside both beam components.

In his summary (Table 1), Lawson$^{3)}$ noted the paucity of harmonic field acceleration schemes that utilized free charges in the accelerating system. Devices mentioned under Step 2 above probably fall under that heading, and those under Step 3 certainly do.

Step 4, in which the driving and accelerated beams overlap in location, is not yet to hand.

An analogous progress in thought can be identified for *non-resonant,* i.e. pulsed, systems. The wake-field accelerator, described by Voss and Weiland$^{3)}$, incorporates an annular driving beam, a radial line, and the accelerated beam in the same envelope. The inward increase in impedance of the parallel-plate radial lines provides voltage amplification of the 30 ps pulse on axis. [A related experiment with much longer pulses (1 ns) and with discrete voltage pulses arranged on a circle has demonstrated the axial voltage amplification feature$^{10)}$.]

Finally, one should note an unusual concept, the Autoaccelerator$^{11)}$, in which the driving and the accelerated electron beams overlap in location but not in time (see Fig. 2). The energy in a large number of electrons in the first part of the beam is used, via the electromagnetic energy transferred to a coaxial cavity, to accelerate a smaller number towards the rear of the beam. The linear current rise (Fig. 2) charges the cavity, meanwhile

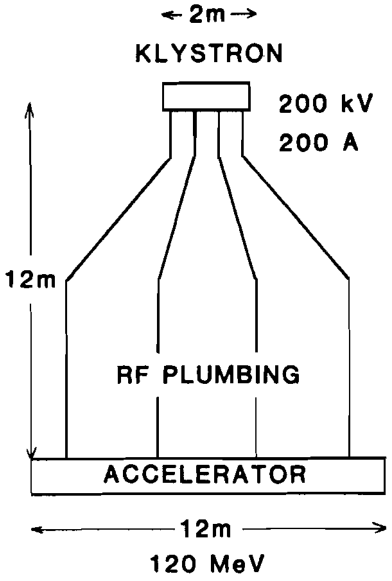

Fig. 1 The schematic shows the disparity in length between the driving electron beam (klystron) and the accelerated beam in a section of the SLAC

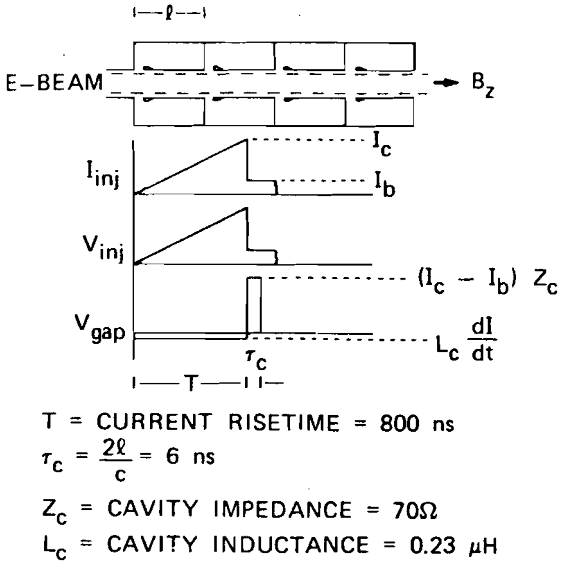

Fig. 2 Schematic of four-cavity autoaccelerator with current and voltage profiles (Ref. 10)

producing a small decelerating voltage at the gap. When the current rise is halted, and the current suddenly and drastically reduced, a very high voltage appears on the gap for a time equal to the double transit time of the cavity, and accelerates the tail of the pulse. A sequence of cavities, such as shown, would be self-synchronous and eliminate the need for high-voltage insulators. Friedman has demonstrated success with a single cavity but has had trouble operating a sequence of cavities.

U. AMALDI

In discussing the challenge of ultra-high energies, Burton Richter has dismissed the inverse free-electron laser because of its energy limitation and has cast some doubts on the concept of the near-field laser accelerator. Perhaps somebody wants to comment on these two points.

A. SESSLER

First of all I would remind you that when Phil Sprangle suggested this idea in March 1981 at the Accelerator Conference, a number of us felt that he must have made a mistake. We subsequently decided that he had not, and six months ago (in February 1982) at Los Alamos, after a great deal of effort, people concluded that it would work perhaps up to a few GeV, but certainly not more than that. Now we believe that it would work up to 300 GeV, but will that be a limit? I think it would be very wrong to conclude so. First of all, in the example that was given by Pellegrini[3)] the wavelength of the laser was about 10 μm, and one can certainly go to a higher frequency. Secondly, we can also stage this device. One can, for instance, have a device that goes to 300 GeV with a 10 μm laser and then goes beyond that with some higher frequency (1 μm is certainly possible in a glass laser, and one can presumably go higher if one can drive it with a free-electron laser). We know very little about the operation of free-electron lasers at this point, but I am not aware of any reason why, in principle, we could not use a free-electron laser as a driver, and we do not really understand what the limits are in a free-electron laser in terms of frequency and wavelength. In fact this is a very interesting experiment to do, and so we should really put a lot of effort into free-electron lasers and the inverse free-electron laser accelerator before we could dismiss the idea.

R. PALMER

On the near-field laser accelerator I fundamentally agree with what Burton Richter said: an enormous amount of work is needed. We have really very little idea how fast the plasma goes off the surface, but the number that I gave was for the electrons and not for the ions. It came from a calculation of Paul Channel at the Los Alamos Workshop, using a particular model for the growth of the plasma. However, one does not know, and one has to find out experimentally.

C. JOSHI

I can clear up this point almost right away. The 1000 Å that Bob Palmer was talking about is the ablation depth of the material that would be evaporated during the laser pulse. But the point is that the surface temperature would rise to a few eV, and this means that, although it is true that only 1000 Å would come off during the laser pulse, after the laser pulse is over there would be hundreds of microns blasted off. In this sense a part of the grating would be permanently destroyed. I have been doing laser experiments for years, and that is what would happen.

J. LAWSON

I would like to make one small point. Much of the thinking about the laser accelerators has been done by people in the accelerator community, and I think there is a lot of scope for getting together very seriously with those who have practical laser experience and understand the constraints of what is theoretically or technologically possible, how much it costs, and so on.

S. TAZZARI

I have been convinced at the Los Alamos Workshop that, from rather general arguments, one can conclude that a near-field accelerator of the type proposed by Palmer could hope, at maximum, for 10^3 to 10^4 particles per pulse. If this is so — and I would like to hear different opinions — how can we expect to reach the high luminosities that we are aiming at with such a device?

U. AMALDI

Since there is no answer and time is short, we will go to the last part of the panel during which we want to discuss how the accelerator concepts discussed until now can become an accelerator complex. We have thus to pay due attention to luminosity, staging, cost, space, and so on. Of course, given the preliminary stage of many of our considerations, the subject is particularly difficult and it may bring us bad news. I have asked Bill Willis to introduce it by telling his views.

W. WILLIS

I have been given an unpleasant job because it is my task to bring bad news, and then some worse news, and you know what happens to messengers bearing bad news. Fortunately I have also some good news.

I think we have not discussed the bad news very much because we know that it is a mistake to be too unimaginative. On the other hand, at the first talk in the Conference, Abdus Salam[3)] gave us a program that particle physicists would like to carry out, and we have to take that very seriously because it has been a long time since the community of physicists agreed with an overall picture of physics. The ideas that Professor Salam described were largely his in generation, but they are accepted by many very prudent and conservative people. It is reminiscent of the time when we were confronted by electrodynamics that predicted results to many decimal points, and after 30 years of tests the number of decimal points has reached eight. We ought to address ourselves to what it takes to carry out his program.

For reasons that will become clear in a minute, I assume that we start out with electron beams or, maybe better, muon beams, and then let them annihilate electromagnetically at a centre-of-mass energy = $\sqrt{s}$, creating a pair of particles with charge q. Now we cover all unknown physics by putting an f(s) in the formula for the cross-section:

$$\sigma = (4/3)(\pi\alpha^2/s)\, q^2 f(s) = (87 \times 10^{-27}/s_{\mathrm{Gev}^2})\, q^2 f(s)\ \mathrm{cm}^2 .$$

There was a time in the 1940's, 50's, and part of the 60's when it was thought that the f(s) for any particles that were not electrons or muons was a small number because the other particles were known to be fuzzy, not point-like. As far as I know, the first hint that someone thought that this was not true was in the talk given by Feynman in 1960 at the Rochester Conference, who said that the fuzzy object was made of several point-like particles. It was an incredible intuition; but experimentally they did it at SLAC in 1967 when it was found that inside the proton there were point-like particles. So the first good news appeared in 1967, i.e. the claim that f(s) is equal to one or thereabouts. Let us suppose that the good news goes on, so that the cross-section continues to be 90 mb/s, since the unknown structures have f(s) $\simeq$ 1. If for very excellent reasons we want $\sqrt{s}$ to be 10^5 GeV, in the region of unknown structures, the cross-section is about 10^{-41} cm^2. That is what Professor Salam referred to as copious production, quite fairly, since that is a huge cross-section for such a big mass. If we like to have one such event per day, we need a luminosity of 10^{36} cm^{-2} s^{-1}. Why not? The future colliders have to violate some of Maury Tigner's scaling laws, but that is a technical detail. Still, if the beam spots are as big as angstroms, the average beam power has to be many gigawatts.

Since I am supposed to be controversial I will say: I do not think that this is going to happen. If many of our ideas work extremely well, after a lot of hard work and a lot of money we will get 10^{32}, maybe 10^{33} cm^{-2}s^{-1}, but not 10^{36}. Then what do we do? We can gamble on resonances since sometimes this has worked. If the new particles have some kind of interaction they can be formed into a system that whirls around many, many times and has a cross-section as big as one likes. It was not actually in the character of Professor Salam's program to have the particles he was looking for resonating together, but never mind. However, I suppose we still have to obey quantum mechanics, so that if the resonance is big it is also narrow. Since the machines that we are going to make will not have extremely small energy spreads, we must take in the formula for the cross-section the effective value of f(s) convoluted with the energy spread. It would, I think, be most difficult to assume that the effective f(s) will be greater than 100, so that we can raise the questions about how we are going to scan to find the resonance. If you disagree with this analysis, please tell me what luminosity you *would* specify for an e^+e^- collider at $\sqrt{s} = 10^5$ GeV, and then explain why the new physics could not be done with a $p\bar{p}$ collider. This is the end of the bad news.

Now I will go on to the worse news or really better news because I am not a pessimist. I assume that somehow we are going to carry out Professor Salam's program because there is a God and She would not make the particles without providing us with some method of investigating them. And the hint is there. You may have noticed a certain twinkle in Professor Salam's eyes during the last words of his talk: some of the new particles may be stable. Better, some of them are predicted to be stable. If so we must go and find one, and then we must find two, and then, by that process, we can find any number. And probably half of them are going to be the antiparticles of the others so that we can annihilate them. That is good, because if they are indeed at the energy of the unification scale we will get masses of the order of 10^{16} GeV.

Figure 3 is a redrawing of the Livingston plot. John Lawson reminded us that the history of accelerators did not begin with the construction of Cockcroft and Walton but that there were accelerators long before that. They were used to study the passage of cathode rays through matter, canal rays, and so on, but they were not relevant for nuclear physics, and this for two reasons. The more important one is that people had better beams available from uranium, thorium, and radium sources. (For this reason I have drawn in the figure the beam energies that came from radioactive sources; the curve crosses the Livingston line at the point at which nuclear physicists became interested in accelerators.) There was a technical problem too: it was not realized that protons would have been useful at a lower energy than alphas because the tunnelling effect was missed. Moral: Don't be pessimistic about theoretical cross-sections.

I have put in the figure a bunch of other discoveries made with natural sources: the electron, the alpha, the nuclear structure, beta decay, nuclear disintegration, the neutron, nuclear fission, the compound nuclear model. All of these were done without the assistance of accelerators. The dotted line shows the cosmic rays available at any given time, which gave us the muon, the pion, the kaon, the lambda, the sigma, high-p_T phenomena, charm, the rising total cross-section, and so on. At the far right of the plot I have drawn the hypothetical relict

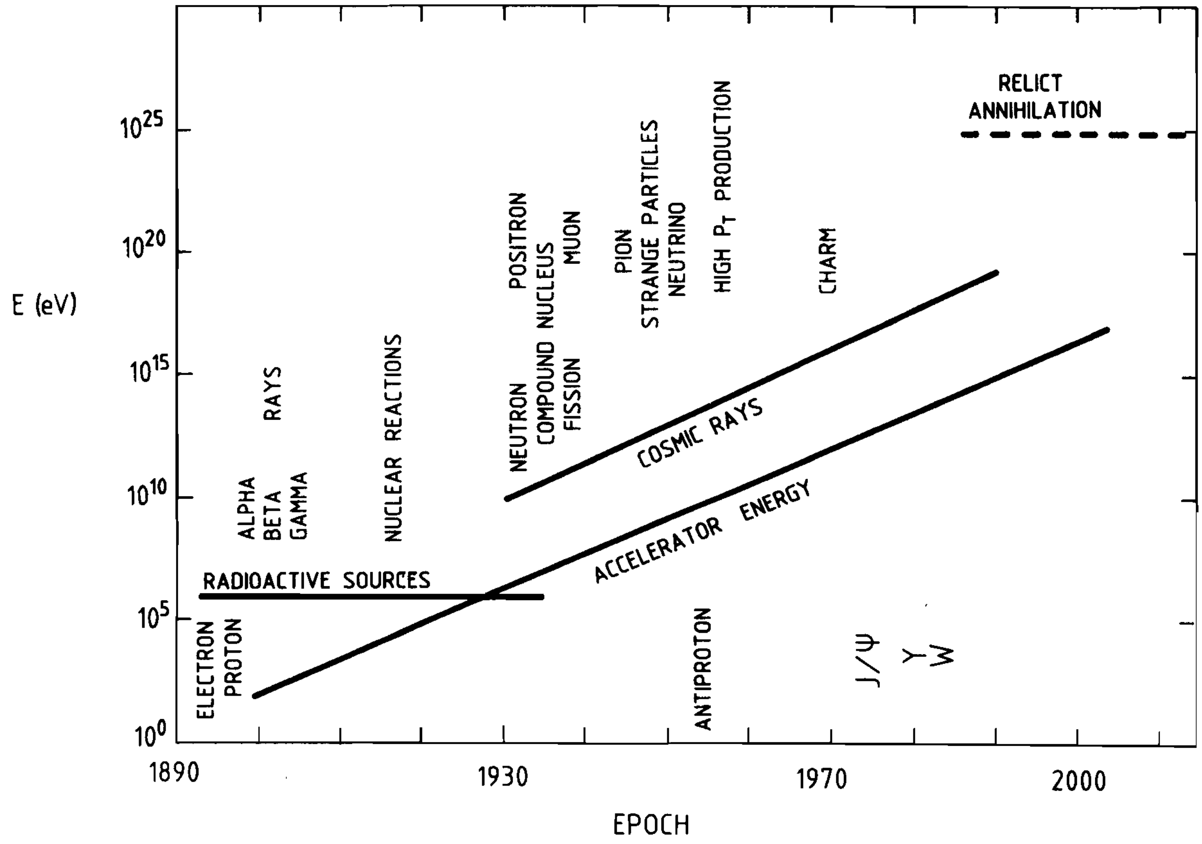

Fig. 3 Livingston plot where the discoveries made with sources different from accelerators are displayed.

annihilation, another kind of radioactive source, sitting at 10^{25} eV (I have taken the case of monopoles, which is perhaps the most likely relict). This scenario seems to me of comparable probability to the proposition that we are going to make accelerators with luminosities of 10^{36} cm^{-2} s^{-1} at energies of 100 TeV in the next 50 years. That is what I call the worse news, not for the particle physicists' but from the accelerator point of view.

Now, finally, the good news. It is very hard to make predictions about what is going to happen next year, much less 30 years from now. Powerful tools in science find something, but usually something *different.* There are various ways out. Maybe the number of new particles at some threshold such as 100 TeV is so large that one channel has a small cross-section — but there are hundreds of them. Maybe there are many resonances, thick on the ground, so that when we shall start scanning we shall find them one after the other. Maybe quantum mechanics breaks down and my basic formula, which scales as 1/s, is wrong. Maybe — and this I would say is quite likely — besides the branch of physics that looks always at more and more point-like objects, there is *another* branch of physics that has not yet been invented, where we look for big blobby things of great mass. Then quantum mechanics will allow us to condense these objects, whatever they are, out of hadronic systems with enough energy.

The conclusion is that we ought to make new accelerators that seem to have a good set of parameters, and then see what physics can be done with them. In particular, I would look for accelerator designs that do not demand very extreme conditions on beam quality and average power at the same time as asking for super high energies, as linear colliders do. If we speak of machines with fixed targets, even very low intensity is interesting once the machine has the highest energy. We must ask whether that means the highest lab energy, or whether the energy must be compared with the equivalent lab energy of some existing colliding beam machine. I think it will be agreed that the answer to this question depends on the type of particle being accelerated. If it is an electron, probably we think that its interactions are of such a simple character that the shift to a lab frame does not buy enough to make it worth while. With a beam of protons, or other hadrons, this is by no means the case, for several reasons. Some are just experimental; with a fixed target machine we can make measurements within a micron, perhaps even less, of the interaction point, and thereby discover phenomena unaccessible to a colliding beam machine. We can use nuclei as targets, which may sometimes be important (we might also use them in beams). Probably more important, the cross-sections for hadrons to produce other hadrons are large, so that we can obtain secondary beams, even with *very* low intensity accelerated beams, and thereby do experiments forever denied to colliding beams: we have only to mention neutrino beams, which have proven so important for particle physics. At super high energies, neutrino beams can be made, with quite low intensity primary beams, because of the rising neutrino interaction cross-sections, and the possibility of very massive detectors. Also, beams of short-lived and very short-lived particles become possible. It seems to me fairly clear then, that our first goal is probably a fixed target proton accelerator.

J. LAWSON

This just reminds me about the story of the man who was out walking one evening and saw a small boy looking around the lamp-post for something. He asked, "What are you looking for?" And the boy said: "I have lost a penny". When asked did he lose it near the lamp-post, he said: "I don't know, but it's the only place I can see". However, the most interesting part of the story is that he went on looking and he found a shilling.

L. HAND

I think that your speculation is very dangerous, but even if we might try it I do not agree at all with the conclusions. We already have seen an enhancement in the predictions of the J/ψ production which is of a factor of several thousand and not of a hundred. Of course Bill Willis covered that by saying that such a large factor is eaten by the beam spread. But let us imagine, for example, that there are bound monopole-antimonopole pairs contained in the particles that we now have. Their coupling constant would be much larger than the one of a charge of 1, and the cross-section would be very large. These pairs would not be visible to us now, but they might become visible in an e^+e^- collider of sufficient energy. So I do not agree with the conclusion that the future machines have to be fixed-target proton accelerators.

A. HOFFMAN

We heard that the point-like cross-section will be very small. But how large is the background due, for instance, to two-photon exchange?

W. WILLIS

By looking at the ratio of the cross-sections, one finds that it is a disaster, even if the angular distributions are very different. For this reason I said that if you are serious about 100 TeV colliding beams, they will not be electrons, they will be muons. And I suppose that the cost of producing muons will somehow disappear in the overall cost of the accelerator.

U. AMALDI

I also do not agree with Bill Willis' conclusions. I think we should still aim at electron-positron or proton-antiproton colliders, but we have to reach very large luminosities. And here I would like to remind you that there is a formula we have always been writing when discussing linear colliders: the luminosity is proportional to the power in the beams multiplied by this famous disruption parameter, which we assume to be of the order of 1. The luminosity also contains in the denominator the length of the bunch, so that shorter bunches are to be favoured. To give an idea of the constant appearing in this formula, I would remind you that the 350 + 350 GeV collider of the second ICFA Workshop[2] needs about 200 MW in the beams, with a bunch length of 5 mm, to get 10^{33} cm^{-2} s^{-1}. If one wants to reach 10^{36} or even 10^{35} cm^{-2} s^{-1}, many gigawatts of power may be needed in the beams. My conclusion is that we must recuperate the energy which is in the beams after they cross, and this will apply to whatever system we are thinking of: plasma plus grating, beat-wave accelerator, copper structures, and so on.

B. RICHTER

I think your conclusion is wrong because if you are going to recover the energy in the beams, you have to do something with it. Now, if you can recover it in the form of electricity, already converted into what you want to use, you will get something out of it; but if you recover it in the form of heat, then you are going to get nothing out of it. Nobody can hope to get more than 30% energy conversion efficiency, and if that is all you can do it is really not worth that enormous amount of emphasis. In fact it is certainly not worth making it a requirement for future accelerator systems.

U. AMALDI

What would you do, then, with the gigawatts you put in the beam? Throw them away? In a storage ring accelerator this is the circulating power in the beams and does not figure in the thermodynamic efficiency. In other colliders one would like to recover it with an efficiency close to 1, as is possible with superconducting linacs. And this is in general a desirable property of the ideal ultra-high energy collider if it has to reach very large luminosities.

J. ALLABY

I want to support Burton Richter's statement. In all these accelerators we are dreaming of converting power from a wall plug into an accelerator with, perhaps, 15–20% efficiency. So how can you possibly worry about trying to recover the power in the beam, which is at most 15–20% of the power you have taken out of the wall plug. The emphasis has got to be on higher efficiency of converting power from the wall plug to the beam.

M. TIGNER

It is true that we have implicitly focused on accelerators designed to throw away the beam after a single pass. There, AC to beam power conversion efficiency is essential. It is possible, however, to envision a system in which the beams, or their kinetic energy, are preserved from collision to collision and there the Q-value (or energy recovery) is of importance, e.g. a storage ring or superconducting linacs.

D. KEEFE[*]

The recent ICFA studies[1,2] and the US Summer Study at Snowmass[11] led to the conclusion that a plausible next generation of accelerators would have parameters in the range of 20 TeV for a $p\bar{p}$ accelerator-collider, and 350 GeV for an e^+e^- linear collider. Such parameters do not rely for justification on any unique theoretical prediction such as a new mass-threshold. Instead, they represent a scaling of about one order of magnitude above machines now under construction (Tevatron, UNK, SLC, LEP) and, therefore, lead to "imaginable" accelerator designs. The purpose of the present meeting is to look ahead to still the "next step beyond", to recognize where the limits may lie, and to identify the most promising research in accelerator physics which could provide us with tools to push back those limits.

Some implicit assumptions made below include the following:

a) The single-pass linear collider experiment at SLAC will verify that e^+e^- linear colliders work as advertised.
b) Beyond ~ 200–300 GeV the unfavourable scaling of cost and size ($\sim B^2\gamma^2$) of circular e^+e^- colliders will have driven them out of the competition with linear colliders.
c) The highest energy in the centre of mass will provide the most exciting physics and is best obtained in a $p\bar{p}$ (or pp) accelerator-collider[**].

---

[*] Because of time limitations, this intervention could not be presented during the panel. However, in the course of the discussion, some of the points were mentioned by the author.

[**] The attractive feature of being able to use just one ring for a $p\bar{p}$ collider may not survive in the future. If a second (intersecting) ring is needed, the distinction between a $p\bar{p}$ and a pp collider disappears for the purpose of this discussion.

Assumption (c) may be wrong. The extraordinary Centauro events observed occasionally in cosmic-ray experiments may suggest possible new physics for nucleus-nucleus (e.g. iron-air) collisions at an energy $\gtrsim$ 1 TeV/amu in the centre of mass. If total c.m. energy, for some reason, is more important than γ_{cm}, then a $\mathrm{p\bar{p}}$ or pp collider would be a poorer investment than a heavy-ion collider to study such physics. Worse, if the new physics shows itself for high-energy multi-nucleon matter only, it might never appear at $\mathrm{p\bar{p}}$ or pp or e^+e^- colliders, no matter how high the energy. A design for a future hadron collider ought to include the capability of its being converted — if ever needed — to a heavy ion collider.

The discussion below includes some views on ultimate limits to achieving high energy, and on which aspects of technology for ring- and linear-accelerators may pay off in the future. For the first, the problem is the guide-field, and for the second, the impediment is the abysmally low electrical efficiency of RF acceleration methods. A merging between the thinking that has gone into certain (but not all) collective methods and the more traditional accelerator concepts could have encouraging consequences.

Several practical factors conspire to become, at the same time, almost insuperable for the "step beyond":

i) *Capital cost:* Lawson[3] reminds us that whilst the cost per MeV has diminished with time, upward progress along the "Livingston curve" has involved monotonically increased cost so that future machine costs will be measured in billions of US dollars. Not unconnected is the question of:

ii) *Constituency:* In the ultra-high energy era, the number of particle physicists involved will probably diminish as the lower energy machines are turned off, and the number of affordable interaction regions shrinks towards one. At the same time, the partial cross-sections for a given channel of interest may well follow the same diminishing trend. Measured in "dollars per event-physicist", the degree of difficulty in getting support for further new accelerator construction will unfortunately become greater as the energy goes up.

iii) *Electricity:* To maintain a reasonable event rate from a collider, the beam power must increase with energy, because lowering the accelerated beam current is not an option if luminosity is to be kept high. An average power consumption of (a few × 100 MW) is probably the upper limit set by both the size of the annual power bill and the ability or willingness of the utilities to supply such a large single-user load. A power level of 1 GW could require a dedicated on-site power station; the additional capital cost of 3 G$ (US) would be intolerable.

iv) *Site:* The energy gradients in MeV/m of structure for linacs and rings differ today, and will continue to do so in the immediate future, by an order of magnitude or slightly more (see Table 2). This factor of 10–20 discrepancy is reflected in the vertical displacement between the two Livingston curves for protons and electrons. A point emphasized by many speakers, and also below, is the need to push up the gradient for single-pass linac systems to 100 MeV/m or more, so that in the path to ultra-high c.m. energies, electrons can begin to enter into competition with protons. With present, or slightly scaled, technology one can see that the end of the line for hadron accelerators will be independently set by a site size comparable to national or state boundaries. For example, a 1000 TeV $\mathrm{p\bar{p}}$ ring with 10 T magnets has a diameter of 1000 km — a nation-sized device that represents not too great a factor beyond the envisioned "next step" of 20 TeV ("not too great", that is, if we contemplate the immensity of the "desert").

Table 2

Example energy gradients, dT/dz (MeV/m of structure)

Electrons		Protons	
Accelerator	dT/dz *) (MeV/m)	Accelerator	dT/dz (MeV/m)
SLAC (original)	7	FNAL original	80
SLAC (SLED I)	11	FNAL (Tevatron, B = 5 T)	170
SLAC (SLED II, future)	18	FNAL (B = 10 T, future)	340

*) These values are set by limitations in microwave power sources and are much less than the breakdown limit.

In practice, sites for large circular tunnels are more difficult to find than sites for linear colliders. For the latter there are the many possibilities of using existing long linear rights-of-way such as railroads, highways, utility power-runs, etc. A circular tunnel can, however, offer an interesting way of housing colliding linacs if we allow the "linacs" to have a gentle curvature to conform to the tunnel layout. (Synchrotron radiation is negligible in a slightly bent, single-pass, high-gradient structure.) Two colliding linacs, each stretched once around the circumference of the LEP tunnel, could provide 2 × 1 TeV if operated with a gradient of 33 MV/m, which is well below the sparking limit of an S-band structure. The energy could be increased by recirculation.

v) *Accelerator physics:* In contemplating the societal problems that can limit kinetic energy, we tend to forget that there are also several issues concerning beam physics and stability (see, for example, refs. 1 and 2). New features of concern for future p$\bar{p}$ colliders include the huge amount of energy stored in the beams (which can be the equivalent of a ton or more of TNT), and a significant amount of energy loss by synchrotron radiation.

The above remarks are intended, not as an exercise in gloom, but as a reminder of the directions in which solutions need to be developed. What we cannot guess today are the new inventions, technology revolutions, and material developments that are sure to turn up in the coming decades and cannot but be to our advantage. (Imagine, for example, how the discovery of an inexpensive room-temperature superconductor would change the picture.) Nonetheless, there are many obvious ideas to be explored which might alleviate the somewhat different problems of p$\bar{p}$ colliders and linear colliders.

p$\bar{p}$ collider-ring: If one considers an accelerator-collider system that takes, say, five minutes to accelerate and five days to circulate the full energy beam, it is clear that the time-averaged power to the beam is negligible. There is therefore nothing much to be gained from striving for higher gradient and greater efficiency in the RF system. Likewise a beam-energy recovery system is pointless. The guide-field magnets represent the dominant electric power load. For superconducting magnets the energy is mainly consumed by the refrigeration system.

Recently, there has been some debate about the most suitable magnetic field to choose for the superconducting guide-field magnets[12)]. Where the site issue is controlling, e.g. if one wishes to use an existing tunnel, the highest practicable field (~ 10 T) is preferred. If the site issue is not controlling, however, Wilson has pointed out advantages in using iron-dominated superconducting magnets at 2 T ("superferric" design)[12,13)]. If analysis shows that there is merit in the superferric approach, namely that a guide field of 2 T is acceptable, then it seems to me that serious attention should be given to an all-mechanical design based upon the newly available unit-permeability permanent magnet materials and using iron to shape the fields.

The mechanical manipulation of magnets — either permanent magnets such as Alnico, or gradient electromagnets — to supply a pulsed guide field during acceleration has been the subject of speculation by several people in the past. There are two major reasons for raising the subject again, however. First, the field-superposition property of the $\mu \simeq 1$ permanent magnets gives a new degree of freedom. Secondly, the long acceleration time and the very long flat-top time peculiar to an accelerator/storage-ring device are especially well matched, in the one case to a slow mechanical system and in the other to a zero-power permanent magnet system.

Several materials are known that have $\mu \simeq 1$ in the second B versus H quadrant (Fig. 4), and if blocks of material are arranged in a chosen configuration the resultant field distribution is a direct superposition of the

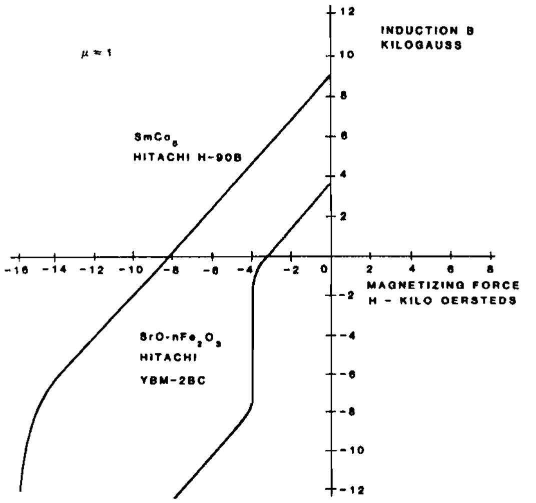

Fig. 4 Typical magnetization curves for rare earth cobalt and strontium ferrite.

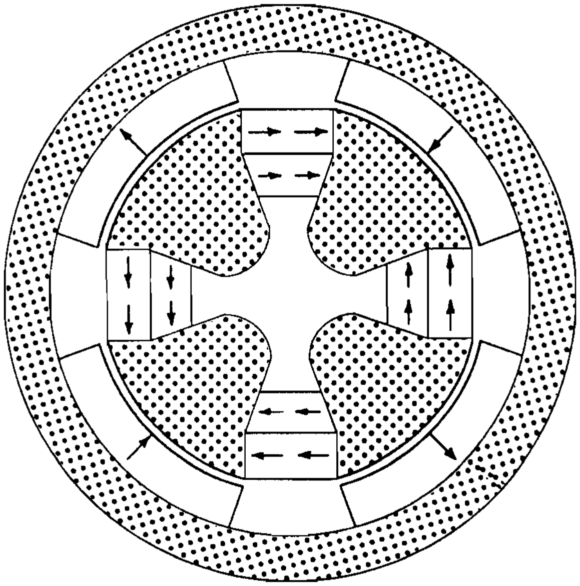

Fig. 5 A quadrupole design using $SmCo_5$ (open areas with magnetization arrows) and iron (shaded) with a pole-tip field tunable from 0 to 1.2 T by rotating outer ring (Ref. 14). The concept is applicable to dipole or other multipole magnets.

independent fields, i.e. each block behaves like an air-core current-sheet loop[14]. One of the best-known materials is the rare earth samarium-cobalt, which has the highest peak BH product, and has found application for undulators and for linac quadrupole lenses. Unfortunately it is expensive, and for large-scale application to a storage ring one would choose other materials, such as barium or strontium-ferrite which have a peak BH product one-quarter that of $SmCo_5$ but are only one-hundredth the unit cost. Enough material must be used to drive sufficient flux into the iron pole-tips to provide 2 T in the median plane; even so, the cost per metre of bending magnet turns out to be less than that for a superferric magnet and ancillary refrigeration.

Whilst the cost of electricity for a permanent magnet ring running for days in the collider mode is virtually nil (in practice some trimming and correcting electromagnetic elements would be needed), energy must occasionally be supplied for a period of minutes in order to depress the field to the injection value and allow it to ramp back up during acceleration. This could be done electrically at the expense of adding bucking coils and a power supply, but a recent development due to Halbach[15] suggests a more elegant, purely mechanical method. Figure 5 shows a samarium-cobalt quadrupole design that can be tuned from $B \simeq 0$ to $B \simeq 1.2$ T at the pole tip by a mechanical rotation by 90° of the outer iron cylinder, on which are mounted some of the $SmCo_5$ blocks. In the one case, the outer blocks drive flux in the iron to cancel the contribution from the pole-to-pole blocks; in the other case, to aid it. The design is readily extendible to dipoles (180° rotation needed). The energy required for the mechanical rotations during injection and acceleration could be stored either electrically or mechanically.

e^+e^- linear colliders: The issues here are almost the opposite of those for $p\bar{p}$ rings—the guide field presents no difficulties, and the two main concerns are the electrical efficiency from the power line to the beam η and the accelerating gradient E.

Tigner[3] has given an excellent analysis of the situation, and it only remains for me to concur with most of his remarks. His proposed goal is $\eta \simeq 10\%$, $E \gtrsim 100$ MV/m. Part of the difficulties stem from the fact that electron linacs today operate at very low efficiency (a few percent) where certain scaling laws are unfavourable. For example, increasing the gradient in a given structure by increasing the voltage V leads to structure losses that rise as V^2 and hence an efficiency that drops as $1/V$; matters would not be at all as bad if one were already operating with a high microwave-to-beam efficiency, say 50% or more. Nonetheless, in many regards the search for high gradient and the search for high efficiency tend to be in opposition. The situation can get better, however, as the microwave frequency is increased (but not indefinitely, as some of Tigner's examples show).

As Table 2 shows, linacs lag behind rings by a large factor in energy gradient. The desire for higher gradient is mainly driven by the need to reduce capital cost and not so much, at this time, by the limitations of site. The push for high efficiency is crucial for avoiding the electricity limit.

If a high beam efficiency, such as 30% or more, can be achieved, then an efficient beam-energy recovery system will become important. Fortunately, the geometrical arrangement of linear colliders seems ideal for such a system.

U. AMALDI

Unfortunately the allocated time is over and we must bring this panel to an end. As foreseen, whilst we had a detailed enough comparison of points of view on the various acceleration concepts presented during the meeting, the available time and the difficulty of the subject did not allow a comprehensive discussion of the problems connected with transforming these concepts into instruments for physics. We heard the opinion of Bill Willis, and certainly not all of us agree with his conclusions. However, I think we all agree with him when he says that it is a mistake to be too pessimistic and too unimaginative. This is the main message we have to carry home with us. In conclusion let me thank the panel members and the participants for their contributions and all of you for your attention.

REFERENCES

1) *Proc. Workshop on Possibilities and Limitations of Accelerators and Detectors,* Batavia (Ill.), 1978, ed. D. Edwards (FNAL, Batavia, 1979).
2) *Proc. ICFA Workshop on Possibilities and Limitations of Accelerators and Detectors,* Les Diablerets (Switzerland), 1979, ed. U. Amaldi (CERN Report RD/450-1500, Geneva, 1980).
3) See the contribution appearing elsewhere in these Proceedings.
4) V.P. Sarantsev and I.N. Ivanov, *in Proc. 4th Conf. on High-Power Beams,* Palaiseau, 1981, eds. H.J. Doucet and J.M. Buzzi (École Polytechnique, Palaiseau, 1982), p. 691.
5) G.I. Budker, *Proc. CERN Symposium on High-Energy Accelerators and Pion Physics,* Geneva, 1956 (CERN **56-25**, Geneva, 1956), **Vol. 1**, p. 68.
6) A. Fisher, P. Gilad, R. Goldin and N. Rostoker, *IEEE Trans. Nucl. Sci.* **NS-28,** 3383 (1981).
7) *Collective Accelerator, a Study Carried out for the DOE,* Batavia (Ill.), ed. F. Cole, Fermilab Report FN-355 (1981).
8) A.M. Sessler, *Proc. Seminar on New Trends in Particle Acceleration Techniques,* Capri, 1982 (Lab. Naz. CNEN, Frascati, 1982), p. 243.
9) A.M. Maschke, Brookhaven National Laboratory Report BNL-51094 (1979).
10) E.C. Hartwig, *Proc. Symp. on Electron Ring Accelerators,* Berkeley (Calif.), 1968 (Lawrence Berkeley Laboratory Report, UCRL-18103, 1968), p. 44.
11) T.R. Lockner and M. Friedman, *IEEE Trans. Nucl. Sci.* **NS-26,** 4237 (1979). This paper contains references to the Russian literature on the same topic.
12) Proc. Summer Study on Elementary Particle Physics and Future Facilities, Snowmass (Colo.), 1982, to be published by the APS Division of Particles and Fields.
13) "News and Views", *Particle Accelerators* **12,** 177 (1982).
14) K. Halbach, *IEEE Trans. Nucl. Sci.* **NS-26,** 3882 (1979).
15) K. Halbach, Lawrence Berkeley Laboratory Report, LBL-14934 (1982).