for multi-GeV particles is clearly premature; in particular, the need for diverse methods of attack remains paramount.

I hope that the foregoing discussion has shown that although some of the traditional fields of experimental and theoretical interest in high-energy physics may be less productive as the next range in energy and intensity is explored, there is very strong reason to believe that other, less explored areas will be the source of important results. Hence the time cannot be predicted when physicists will feel that the study of high-energy physics has passed its peak of fruitfulness.

STUDIES AT BERKELEY AND MURA ON FUTURE HIGH ENERGY PROTON ACCELERATORS

D. L. Judd

Lawrence Radiation Laboratory, University of California, Berkeley, Calif.

I. INTRODUCTION

The general thesis that there is a need for additional particle accelerators needs no defense at this conference, and is particularly obvious to all who are aware of the crowded experimental schedules at each of the major existing machines, the rather small number of really definitive experiments which can be conducted at any one of them each year, and the great mass of unknown facts that will be required to solve the most challenging physical problems of our time. In the sessions to follow it will be demonstrated that ample inventiveness has been applied throughout the world to produce a whole new generation of accelerator concepts and techniques, so that the new accelerators about which we speak will certainly be freed of many of the limitations affecting the productivity of the machines now operating or under construction. In the not-so-distant past it was possible for each innovator, and those associated with him, simply to convert his concepts into hardware and apply the results to the discovery of new physical facts. However, we have rapidly progressed to the stage where the feasibility of an accelerator proposal can only be determined by the detailed application of technical, engineering, and computing skills as well as of intuition and experience in the accelerator art. A more disturbing phase of our progress into the presently feasible range of particle energies is that the equipment required is so large and costly as to require difficult decisions of national and even international policy to finance its construction and use.

In this situation it becomes increasingly important to assure that all available information shall be brought to bear on the question as to which of the many possible accelerators of the future will yield maximum returns of physical understanding for the investment involved. Many high-energy physicists have given serious thought to these problems, and it is appropriate that they should arrive at a diversity of answers. I have been asked to report in this paper on some of the points which have been brought out in a series of informal meetings of a group of physicists at the Lawrence Radiation Laboratory at Berkeley during the past year, and in a more intensive, one-week study by a group of visiting workers from several laboratories, organized last June by the Midwest Universities Research Association (MURA) at Madison. This latter study followed an exploration of the feasible types of new accelerators conducted by other visitors at MURA during the preceding week. A compilation of results of the MURA studies will soon be available upon request; the Berkeley work has been summarized only in an internal memorandum until now. Both of these groups considered only the acceleration of protons, to final energies above 10 GeV, but this implies no lack of conviction that new electron machines would be of great value.

I will present this information under three headings : 1) considerations bearing on the utility of higher currents, 2) factors affecting the desirability of ultra-high laboratory energies, and 3) features felt to be desirable or essential to planning future accelerators in order to maximize their experimental utility. I will also briefly mention some studies recently started at Berkeley to explore the utility of cryogenically cooled magnet coils.

II. CONSIDERATIONS BEARING ON THE UTILITY OF HIGHER CURRENTS

As an initial step, the Berkeley group addressed itself to the question of studying the need for higher currents in the energy range 10-25 GeV. The largest effort of the visitors at Madison during the second week was devoted to the same question (although a number of other problems were also given considerable attention there). On this topic the two groups (which had a finite overlap) were in substantial agreement, and I will not attempt to separate the origins in presenting the points that were made.

Accumulated experience, particularly with the Bevatron, has shown that present intensities make it necessary for some experiments to run for long periods of time and in many cases seriously limit statistical accuracies. It is becoming clear to many high energy physicists, both theoretical and experimental, that for a long time to come there will be a need for much more detailed data on the interactions of the known strange particles. According to our present theoretical understanding, the interactions between any pair of the more than 20 strongly coupled particles are, in principle, of equal interest. In a more general way, many theorists feel that the masses of the particles are somehow a result of their interactions, and that we should not expect to understand the masses and the interactions separately. The dispersion theoretic approach to these problems is meeting with increasing success and acceptance; more precise experimental information on these interactions than has been needed heretofore, and over wider energy ranges, will be required to exploit this approach. This point of view emphasizes the importance of trying to apply the proposal of Chew and Low for conducting experiments in which particles other than nucleons may be regarded as virtually present in nucleon targets; it is clear from the start that most such experiments are impossible, or at best marginal, with present intensities.

A great deal of work remains to be done in the energy range from 6 to 12 GeV, say, much of which can eventually be carried out in beams of from 3 to 300 mμA average current; these figures bracket the best present and anticipated future performances of existing proton machines and those under construction. Perhaps the greatest handicap in planning for desirable secondary beams from a future machine is our ignorance of production cross-sections of all sorts as functions of energy and angle. It is greatly to be hoped that such information will be obtained early in the operation of the AGS machines that will soon be in use. In addition, nucleon-nucleon, pion-nucleon, and K-nucleon scattering, and perhaps some hyperon-nucleon scattering can be studied. Much of this work will proceed slowly, and could be greatly accelerated if higher currents, in the range of 3 to 30μA, were available; the greater attenuations inherent in higher purifications by separators could be tolerated, multiple target operation would enhance the rate of experimentation and make more efficient use of the time of the experimental groups, and many processes of intrinsically low probability would be opened for investigation.

An interesting table (Table I) has been prepared by Cool, Wattenberg, and Ypsilantis showing estimates of the relative feasibilities of measuring various total cross-sections σ_t, elastic differential cross-sections $\left(\frac{d\sigma}{d\Omega}\right)_{el}$, and polarizations P.

TABLE I

Currents required for studying strong interactions (Cool, Wattenberg and Ypsilantis [1])

	σ_t	$\left(\frac{d\sigma}{d\Omega}\right)_{el}$	P
πN	E	E	E
$K N$	E	E	F
$N N$	E	E	E
$Y N$	E	E	F
$\bar{N} N$	E	E	F
$\bar{Y} N$	F	F	F
πY	F	F	F
$K Y$	F	F	F
$Y Y$	$F F$	$F F$	$F F$
$Y \bar{Y}$	$F F$	$F F$	$F F$
$\pi \pi$	E	E	—
πK	F	F	—
$K K$	F	F	—

E: feasible with *existing* accelerators $I < 0.3$ μA.
F: feasible with *future* accelerators $I \sim 30$ μA.
$F F$: requires a *far future* accelerator $I \sim 3$ mA.

An experiment requiring less than 30 days of running time was arbitrarily defined to be feasible. In the table E denotes feasibility with existing accelerators, F with a future accelerator providing 30 μA, and $F F$ with a " far future " accelerator of 3 mA. The last three lines refer to the interpretation of inelastic processes with three final state particles in terms of virtual pion or K meson targets as proposed by Chew and Low; for example, the reaction

$$\pi + p \rightarrow \pi + \pi + p$$

may be related to pion-pion scattering, the target pion virtually present in the target proton. In addition, the processes $N\bar{N} \rightarrow \pi\pi$ and $N\bar{N} \rightarrow KK$, and the process $N\bar{N} \rightarrow Y\bar{Y}$, recently observed for Λ hyperons, may require currents in category F for precision measurements. The estimates used in constructing the table are necessarily crude and uncertain; in addition, the feasibility of an experiment will of course depend on many factors other than beam intensity.

It is very difficult to predict the relative future importance of extending such measurements into the 25 GeV region as compared with the importance of new, more interesting reactions that may occur at these higher energies. However, in any event the various arguments for higher intensity will certainly not have less force there; in fact, the situation may be considerably worse. The cross-section for any reaction proceeding through a particular angular momentum state is proportional, among other things, to the square of the center-of-mass de Broglie wavelength, $\bar{\lambda}^{2}_{c.m.}$, which is inversely proportional to the laboratory energy in the relativistic range. Some of the production cross-sections may turn out to be so low at 25 GeV as to complicate greatly or in some cases to prevent the design of usable separated secondary beams from the new AGS machines.

Additional possibilities opened up by higher intensities have been listed in the preceding paper by Panofsky. Rather than belabor the point that higher intensities are clearly needed for the work ahead of us, I will instead point out some of the difficulties and uncertainties in the use of such a machine. The identification of particles at energies above 5 GeV is far from easy; much additional work must be done. Our ignorance of the production cross-sections of particles needed for secondary beams has already been mentioned. (It has been estimated that usable beams of order 10^8 π/sec, 10^6 K/sec, and 10^4 $\bar{p}$/sec might be obtained from 15 μA beams of accelerated protons in the energy range mentioned above.) Better beam separators will be required. It is difficult to guess at the proper relative roles of the various known and future detecting apparatus during a period starting 5 to 10 years from now. The dissipation of heat in targets will be a matter of serious concern at the beam levels under consideration. The shielding of both experimental equipment and personnel from such an intense accelerator will present serious problems. Finally, the problem of the disposition of lost beam, and of the radioactivity it would induce, still lies ahead of us. Such an expensive device must not destroy itself by its own radiation!

III. FACTORS AFFECTING THE DESIRABILITY OF ULTRA-HIGH LABORATORY ENERGIES

About three years ago some thought was given at Berkeley to the problems of constructing an alternating gradient synchrotron in the energy range 100 to 200 GeV. Workers in other laboratories have also considered this question. Our tentative conclusion at that time was that the task was feasible but that it would be rather slow and very expensive, and that it was premature to contemplate such a large project in the absence of data from machines in the 25 to 30 GeV range. Our tentative estimates were based on a straightforward extrapolation of the Brookhaven and CERN designs, using magnets with similar field strengths, apertures, and gradients but tighter tolerances. Fortunately, under these assumptions the most difficult tolerances scale as fractional powers of the final energy, such as the one-fourth power.

During the first week of the summer study session at Madison referred to above, M. Sands of the California Institute of Technology had re-examined this matter and made independent estimates of a 100 GeV AGS similar to the Brookhaven and CERN machines, and also of a 300 GeV AGS of " more radical design " consisting of two rings tangent to each other, the beam being transferred from one to the other at an energy equal to the geometric mean of the injection energy (from a linac) into the first ring and the final energy to be reached in the second ring. This arrangement allows an exceedingly small aperture in the second, larger ring because of adiabatic damping during acceleration in the first ring. It was therefore natural for some of those present during the following week of study on the uses of new machines to examine the utility of ultra-high energies. Much of the material developed by these workers will be presented by L.W. Jones in the following paper in connection with synchroclash proposals, I shall attempt a brief report of what was discussed with respect to using protons of 300 GeV laboratory energy, which corresponds to the center-of-mass energy of two colliding proton beams of 12 GeV each.

One may orient himself by tabulating the energies, typical production angles, and laboratory decay lengths of pions, *K* mesons, and of particles of nucleonic mass for two different laboratory velocities, one corresponding to that of the center of mass in the laboratory ($\gamma \simeq 12$) and the other that of the incoming proton ($\gamma \simeq 300$) (see Table II). The tabulated angles correspond to transverse momenta of $m_\pi c$.

With respect to the energies, it was stated that those below 150 GeV would have measurable curvature (0.1 cm displacement in traversing 100 cm of 10^4 G field) in a large bubble chamber or in two chambers separated by a bending magnet; the corresponding angle that could be observed is 10^{-3} rad. Reasonable resolution in such measurements implies measuring bubbles of 10^{-2} cm diameter to a precision of a tenth of their diameter, all relative to ultra-high energy tracks in the same frame to minimize distortions. The hyperon decay lengths could be seen in large chambers, and are long enough to be separated noticeably from other forward particles.

TABLE II

Laboratory energies, typical angles, and decay lengths for various particles with $\gamma = 12$ and $\gamma = 300$ (M. Sands et al.[2])

		$\gamma = \gamma_{\mathrm{c.m.}} = 12$	$\gamma = \gamma_{\mathrm{beam}} = 300$
Energies	π	2 GeV	45 GeV
	K	6	150
	N	12	300
Angles	π	10^{-1} rad	3×10^{-3} rad
	K	2×10^{-2}	10^{-3}
	N	10^{-2}	5×10^{-4}
Decay lengths	π	72 m	1800 m
	Λ	0.36	10

To study the feasibility of identifying or separating secondary particles it was assumed that flight time differences of 10^{-11} sec through microwave separators or counter arrays would suffice for discrimination. For equal momenta, a particle of energy γmc^2 can be separated from a lighter particle if the distance is about $2\gamma^2 c\Delta t$, corresponding to 1 metre for $\gamma = 10$ and 10 metres for $\gamma = 30$, above which this method would fail even with the rather optimistic assumptions made. Some hope was expressed that higher energy particles might be identified by their characteristic interactions.

The classes of experiments suggested were (1) a search for new particles, (2) a statistical study of the production of known particles, and (3) an examination of correlations among secondary particles in the various interactions.

A few comparisons with colliding beams were made. With the expected intensities the single beam would produce up to 10^3 times as high an interaction density, without the troublesome background from the residual gas, but would have a less favourable duty cycle. The narrow angles of divergence of " one-way " interactions were felt to have some experimental advantages over colliding beams, although identification of secondaries is admittedly more difficult. The production of secondary beams seems easier than with a stationary center of mass; there are probably large yields in the range $\gamma \sim 12$ to 300, as contrasted with typical secondary laboratory energies $\gamma \sim 2$ to 12 with colliding beams. However, the report concludes that the cost of such a 300 GeV machine is much more than that of adding a colliding beam capability to a one-beam high current machine, and that there is as yet little reason to predict that basically new processes will appear at such energies to make their achievement an urgent matter. Although I have not done full justice to the report, enough has been said to indicate that this kind of experimental physics of the future will be very different, and extremely demanding, but that there is hope of obtaining some useful information with such energies if it should become necessary.

IV. DESIRABLE PROPERTIES OF FUTURE ACCELERATORS

I now return to the domain of more familiar energies to list some of the properties of future accelerators which have been recommended by the two groups mentioned above as being highly desirable to facilitate experimental programs. It is not a great achievement for experienced experimentalists to draw up specifications for " ideal " arrangements of beams, but accelerator designers may have a difficult time in meeting all such requests. This brief enumeration is given in the hope that a few of these points may not have been anticipated by everyone who has considered these problems.

The question of internal targets vs. external beams was discussed; each has special advantages. The better and more numerous the external beams that can be provided, the smaller the need for internal targets, but the converse is also true to a considerable extent, especially for high intensity accelerators. An external beam has many general advantages and is almost essential for certain experiments. The availability of large solid angles and the full angular range are vital in studying primary interactions. Lack of interference with the machine and its shielding and the possibility of stringing out many experiments provide greater flexibility. Some RF bunching schemes become simpler in external beams. Absolute measurements can be made more readily, and shorter lived particles may be studied.

It has been recommended that the energies of external beams should be variable by a factor of two if possible; it is also evident that high optical quality ($\sim 10^{-3}$ cm rad) is highly desirable. A few per cent momentum spread is thought to be usable for most applications. The beam must be highly stable in position from pulse to pulse. Many of these specifications may be difficult to meet. It has been noted that long straight sections (5 to 10 metres) may serve as effective substitutes for an external beam in many situations.

Some desirable properties of internal target arrangements will next be listed. The importance of a highly flexible arrangement of multiple target and experimental regions has been emphasized. It is felt that a versatile collection of secondary beam facilities should be designed and set up as an integral part of the accelerator, together with their extensive shielding and the separators, bending and analysing magnets that will be required. Disposing of secondary beams downstream from their targets must be given careful attention. It will be important to try to bring out secondary particles of both signs at small angles to the primary beam, perhaps typically 2° to 5°; considerable advantages in studying phenomena as a function of energy will accrue if it is possible to do this in a field-free region.

It will be important to intercept the full beam on a target of very small size. Very high luminosities may be obtained in this way by exploiting multiple traversals, particularly in machines with large momentum compaction since the inward displacement corresponding to a given energy loss is reduced. High luminosity from small targets greatly simplified the design of separating equipment. Such targets must dissipate considerable heat; a 10 microampere beam will lose about two kilowatts in traversing a nuclear mean free path, which may be approaching a practical limit even for thin, refractory targets. If the efficiency of extraction of external beams is not high, it may be desirable to be able to insert hydrogen targets into the machine. The use of internal targets may complicate the problem of avoiding radiation damage to coil insulation and other sensitive components.

The importance of flexibility of beam duty cycles has been heavily emphasized; the needs of bubble chambers and of counters and luminescent chambers are basically different. However, even within each class there is a need for flexibility, since counter control of chambers and time of flight work with electronic equipment impose special requirements. It appears at first sight that FFAG accelerators hold an important advantage in this respect, since it should be possible in principle to vary the duty cycle and repetition rate of deflecting a stored beam into a target almost continuously over a very wide range. However, the same capability can probably be obtained with a pulsed accelerator feeding a storage ring. It would be very desirable to be able to use different duty cycles on various targets during beam-sharing operation. The RF structure of an accelerated beam is important to some time-of-flight experiments; in one arrangement considered for an experiment at the Bevatron a peak-to-valley beam ratio of 10^3 was required, but measurements showed that the actual ratio was smaller. On the other hand even a moderately pronounced RF structure has an obvious bad effect on counter duty cycles, which can be avoided by slowly dribbling a coasting stacked beam onto a target. Magnet ripple may interfere with this process. All of these comments on duty cycles apply to both internal and external beams, and may at least stimulate useful discussions among those contemplating new accelerators.

V. REMARKS ON CRYOGENIC AIR-CORE MAGNETS

During the past year it has been suggested by R. F. Post at the Lawrence Radiation Laboratory that, because of various scaling laws affecting the projected performance of magnetically confined controlled thermonuclear reaction devices, one should study how to produce very intense magnetic fields (in the range 50 to 100 kG) over large volumes using cryogenically cooled coils so as to exploit the very low resistance of certain pure metals at low temperatures. The resistance of a metal in a magnetic field is due to three effects: 1) an intrinsic resistance due to impurities; 2) the theoretical temperature-dependent resistance of an ideal metal, which varies as T^5 at low temperatures; and 3) the magnetoresistance, which is approximately proportional to the field strength. Taking into account the thermodynamic efficiency of the refrigerator, one can see that for each field strength and material there exists a temperature which minimizes the total power of excitation plus cooling. For d.c. fields in the range mentioned these lie in the neighborhood of 30°K for copper and aluminium, and 10° K for sodium; sodium seems in principle to provide the greatest potential gains relative to room-temperature operation with copper in plasma confinement applications. For pulsed magnets eddy current effects must also be considered. Engineering factors other than total power will influence the determination of optimum operating temperatures. Materials of very high purity are required to realize these gains. A number of other features make this concept attractive. Recent technological advances have improved the efficiency of large refrigerating engines and have provided more efficient insulating materials. Removal of the relatively small joule heat seems possible by gas cooling in many situations, even with large packing fractions. Thermal conductivities are higher than at room temperature. The magnetic stresses are small enough to be supported without great difficulty at these field strengths; yield stresses of metals are higher at low temperatures. Post has suggested that these techniques could be applied to the design of accelerators and of auxiliary magnets if the various problems of purity, fabrication, etc., can be solved. Detailed information on cryogenic coils will be contained in a paper now being prepared by Post.

The possibility of increasing field strengths by a factor of 3 or more above those usable with iron would provide an interesting new degree of freedom in accelerator design. One should perhaps exploit this by increasing the volume of useful field somewhat, relative to a strictly scaled iron magnet, so as to ease some of the tolerances in the beam dynamics of the accelerator. At Berkeley some studies have recently been initiated to see whether such magnet structures are indeed practical for accelerators; studies of an AGS machine of this type will be described by Christofilos at a later session of this conference. Our work on d.c. magnets of this type, which may be simpler in some respects, is not as far advanced, and it seems too early to say now whether these developments will lead to the possibility of more economical construction of big accelerators, whether they will only find application for special-purpose very high field experimental magnets of large volume, or whether perhaps some unforeseen difficulties will prevent their exploitation in high energy physics.

LIST OF REFERENCES

1. Cool, R. L., Wattenberg, A. and Ypsilantis, T. J. MURA (*) 469. 1959 (?)
2. Sands, M. et al. MURA (*) 479, pt IV, 1959 (?).

DISCUSSION

Hildebrand : I would like to ask Judd a question about the relative merits of internal and external targets. With an internal target one can take advantage of multiple traversals to achieve a smaller source of secondary particles. This should be an advantage for separation of secondary beams. I would like to know how important this factor is. Does Berkeley experience lead one to expect external targets to be far inferior as sources of separated beams?

Judd : There are others here who are better qualified to discuss the experimental experience at Berkeley than I am. I do not think there will ever be any unanimity or general statement that one type of target is always better than the other for all purposes.

Lofgren : Answering only as to current experience, I would say that the efficient use of a very small target, which is possible only when the target is internal, is very important for the achievement of highly purified beams. Electric deflection of high energy beams is limited to a few centimetres, this requires that the target be a few millimetres in size. The cross-section of external beams presently in use or under design is much larger than this.

Panofsky : There is one question which I think enters into the internal versus external beam argument in combination with high intensities. Used at present, external beams have very large advantages regarding convenience of set-ups and so forth. However, as intensity increases one has to remember the problem associated with neutron sky-shine, namely the fact that at a distance of 200 or 300 metres from a target as much intensity of neutrons will come through air scattering from sky-shine as would be received directly without shield. This in turn will mean that, with an external beam, problems arise associated with high intensity. One approaches a situation where the shielding and general handling problems, associated with an external beam area, become very expensive and of comparable difficulty to the internal beam area. This I think is a very serious matter which is usually forgotten in the discussions.

Kolomenskij : I think that the proposal for dividing the high energy accelerator in two or more step by step accelerators is well known. For example, I can mention Salvini's proposal for a double chamber accelerator. Also, several years ago, Wilson considered an eight-shaped electron multi-GeV accelerator with two rings, having a common point. There were also other proposals in this direction. Are there some new original ideas, or new theoretical or designing work? Maybe one of those present has done some experimental work on the transfer from one accelerator to another, which of course is not a trivial problem.

Judd : Sands is here and I think he should answer this question.

Sands : The suggestion which I made was that one should look seriously again at a proposal which has been made by several people. It was made to me some years ago by Robert Wilson of Cornell University. One should consider using two circular synchrotrons in cascade, and in this manner hope to make use of the adiabatic damping of the oscillations before injection into a machine of large energy but very small aperture. The work, which Judd was referring to, was an attempt to do some specific calculations and try to find some optimization. First I made a simplifying assumption—I hope probably near the truth—that the work which has gone into the CERN and Brookhaven AGS machines has brought forth a shape of the pole tips which is nearly optimum. It seems difficult to make much higher gradients, at least with great convenience, so, as a simplifying assumption, I propose that all synchrotrons have the same magnet shape. Then I said: let us match the phase areas of the two synchrotrons with an optical system. Particles are injected into the first machine, are adiabatically damped, and then are transferred to the other machine which has just exactly the correct acceptance to take the damped phase area from the first machine. If one adopts these two principles: that the shape should be the same, and that the phase areas should be matched, then one can solve a simple equation for the optimum energy of transfer from one machine to the other, depending upon what one takes as the optimization criterion. The obvious criterion seems to me to minimize the stored energy of the whole complex and therefore minimize the weight and cost of the magnet. If one does this one arrives at an expression for the momentum at transfer.

$$P_{\text{transf}} = 2^{1/3} (P^4_{\text{initial}}\, P^5_{\text{final}})^{1/9}$$

which is not too far from the expression given in Judd's summary, which I had derived by some preliminary and approximate arguments.

This gives the ratio of the apertures and the ratio of the diameters of these two synchrotrons. The remaining question is how to determine the absolute scale of such machines, and I decided that one should also try to adopt a rational criterion for the choice of an aperture, rather than the (perhaps a little more conservative) attitude that has been adopted in the past. The rational criterion that occurred to me was that one should choose the aperture such that on the first assembly of the magnet the equilibrium orbit should with 50% probability lie inside the aperture during the first revolution. This seemed a little extreme and, since in the literature the expressions are usually given for the expected variations of the closed-orbit with a 98% probability, I selected that as a criterion.

Adopting these criteria, one has established completely the parameters of the magnet of this machine. Of course the idea would be that there would have to be trimming possibilities

---

(*) See note on reports, p. 696.

in the machine and probably some elaborate procedures available, first for measuring the position of the orbit and then for calculating and for correcting the magnet. (I would imagine that the magnet would be mounted on adjustable mounts in some way and these could be controlled to make a second try if one happened to be unlucky and be in the 2% probability bracket; and so that, after the first closed orbit did occur, one could tune the closed orbit to be more nearly centred in the aperture). I have looked at a number of the problems concerned with this machine; for example, the stopband widths do not seem to be too frightening, the RF problems are a little severe (it requires 7.5 MeV per turn but with the experience of M.I.T. this does not seem frightening) and so on. The general parameters come out to be the following for the usual injector of 50 MeV and a final energy of 300 GeV. The first machine becomes roughly a 10 GeV machine with an aperture in radius of 13 cm. The large 300 GeV machine has an aperture of 4 cm by 1 cm on a radius of 10^5 cm. It was a little frightening at first to think of a 1 mile diameter machine but Panofsky has removed most of those psychological disadvantages. Finally, the interesting result is that the total magnet weight of a 300 GeV machine based on these principles would be equal about to that of the AGS or the Proton Synchrotron at CERN. The cost should, therefore, also be comparable. Let's say, within a factor of two.

CHRISTOFILOS: I would like to ask if we could generalise the discussion here. What is the justification to go to very high energy machines? For example, Judd said that one justification was that all strange particles would have a much longer life, because of their relativistic energy. If Amaldi permits maybe we can hear some more scientists about what they think is the justification for going to very high energies, say 100 GeV or more.

TEUCHER: I would like to make a few remarks about the data we know from cosmic radiation and which could be useful for the construction of machines in the future. I do not want to tell you about jets, that means about energies above 10^{12} eV. I would restrict myself to energies between 10^{10} and 10^{12} eV. Machine people of course are always a little bit frightened, hearing about cosmic radiation, because the possibilities of energy measurements in cosmic radiation have been rather limited in the past. This comes from the following fact: the only possibility during all this time to determine the energy of an event was the angular distribution of the event, and it has been shown that this is rather an unreliable measurement. Now during the last years bigger and bigger stacks have been flown at higher altitudes, and we have now really a source where we can study the interactions and the mean free paths of protons of energies around 10^{11} eV. We have at the top of the atmosphere heavy nuclei, and a heavy nucleus interacting with the emulsion frequently undergoes a fragmentation into heavy fragments and α particles, but we have also protons and neutrons between the fragments of the heavy nucleus. At energies of 10^{11} eV the opening angle of the fragments is of the order of 10^{-3} to 10^{-4} rad. This makes relative scattering measurements feasible between the tracks, and the precision of the energy measurement is better than 30%. Unfortunately heavy nuclei at energies of 10^{11} eV are rather rare so the statistics are limited and one can make only a few claims. We have altogether followed 638 cm of proton tracks and observed 15 interactions, which yield a mean free path of 42 ± 11 cm. This is, of course, not very spectacular from the point of view of statistics, but I want to mention that theoreticians sometimes claim that even at energies of a few hundred GeV, the cross-section should increase considerably, by more than a factor of two, and there are theories where there is an increase by a factor of 10. This is certainly not the case. From all we know even at energies of a few hundred GeV the proton has a cross-section which is not too different from what is known at Bevatron or Dubna energies. Also, the number of hydrogen events in emulsion, that means clean events, does not increase considerably. From the Heisenberg theory you would expect about 1/2 of the events to occur in hydrogen. Among the 15 events observed here, only two are hydrogen events which is about what one should expect if the mean free path is about the same. What I will tell you now about the details of the interaction is based on 25 proton interactions at an energy of about 300 GeV. One thing of course which is rather interesting is how the multiplicity increases. These events are in emulsion and the average multiplicity is about 12. This is for all the nuclei in the emulsion. If you make an estimate of what you should expect, in rather clean collisions, that means in hydrogen-like collisions, you get that the multiplicity in such collisions should be of the order of 6. This is in agreement with the fact that the multiplicity should increase as about $E^{1/3}$ or $E^{1/4}$. One rather interesting fact is the inelasticity of those collisions. We measured all the angular distributions, and from the very well-established fact that the transverse momentum of the mesons in the secondary interactions is about 300 MeV/c you can estimate the inelasticity. If you do it this way you will end up with a figure of about 0.5. This is an upper limit because all corrections we could think of (secondary interactions in the same nucleus or such things) would make the inelasticity smaller. You can of course get a lower limit if you take the angular distribution which is measured and assume isotropy in the centre-of-mass system. Then you would get that the inelasticity must be larger than 0.2. Therefore the true value will be in the middle. This is rather interesting, because it means that even at energies of only 300 GeV only about 1/3 of the energy available in the laboratory system goes into particle production. So the collisions are on the average rather elastic. Another question which should be rather interesting, especially for colliding beam machines, is the angular distribution of such interactions in the c.m.s. One does not have very accurate data at 300 GeV, and I have to refer to the information obtained by various people all over the world, Bristol and Chicago and others, that at energies of 10^{12} eV most of the particles are found within angles of 30° in the forward and backward direction in the c.m.s.

As to the energy spectrum in the c.m.s., it is rather well-established that the transverse momentum of the shower particles in all such interactions is about 300 MeV/c. Until now it is not known accurately if this tranverse momentum holds for both π mesons and heavier particles. Investigations are in progress at the moment, but there is no exact figure available. Data by Fretter seem to indicate that even for K particles the transverse momentum is the same. In the c.m.s., the energy distribution is thus peaked towards low energies. Most of the particles are created with energies of the order of the pion rest mass or a little bit more. There are only very few particles in the tail of the distribution towards high energies in the c.m.s. and the maximum is of the order of 500 MeV. The only possible way to get information on the identity of the produced particles is to look in a jet for secondary interactions in the forward cone. If all the neutral particles were π mesons it is well known that due to the short life-time of the π^0 they would decay and not produce secondary interactions.

Therefore if we observe neutral secondary interactions in such an interaction they have to be created by particles different from π mesons. The data available at the moment refer to energies of the order of 5×10^{12} eV and all that one can say at the moment is that they are in agreement with about 30% of the particles being different from π mesons. At much higher energies, say 10^{14} eV, completely different things might happen, but I do not want to go into this. Powell mentioned this in Moscow and I think many of you know about it. We looked also into the mean free path of the charged mesons created in such interactions, and the mean free path of mesons is of the order of 41 ± 8 cm, so at the moment there is no indication that at energies of a few hundred GeV something radical happens to the mean free path. Many of the results I have mentioned here were obtained in Schein's group in Chicago, where also Lohrmann was involved. I mentioned several results obtained in Bristol and by Fretter's group, and of course, about the mean free path of protons, we have at the moment very accurate data by the Russian groups in the neighbourhood of 8.7 GeV, and there the mean free path was 37 cm

AMALDI : I think that the question raised by Christofilos has been in part answered by the remark of Teucher, but I think that he was in some way more general.

PAL : What Teucher has said may almost be used as an argument that you do not really need high energy machines, because you can do so much with cosmic rays. Of course, much important information has been obtained from cosmic rays results, but I want to point out that a large number of disagreements about these things at the moment exist which provide cosmic ray justification for high energy machines. There are many experiments in cosmic rays which do not agree as to what is the multiplicity, say in the region of 100 GeV. Further, there is the question of inelasticity, where people quote numbers from 0.1 to 0.5, all because the statistics are bad, and one does not know the energy well enough. There are of course Russian experiments which indicate in a very preliminary way that in interactions at about 300 GeV one gets asymmetry in the c.m.s.; not anisotropy but asymmetry. In some interactions they find that the π mesons go backwards and in others they go forward, there has been an interesting suggestion that you can consider these as collisions of one nucleon core with the π mesons in the other's cloud; there then is a certain probability that one core and a pion of the other's cloud will collide and there is a certain probability that the other core and a pion in the cloud of the first incoming particle will collide, and a certain probability that both cores will collide. So these may give rise to asymmetries forward, backward, or no asymmetry. Now this is by no means established and one has very few events, so one really needs machines to do this kind of thing. Then the transverse momentum is considered constant, but there are huge fluctuations. The Bristol people find a few events in which they have transverse momenta of the order of 2 GeV/c, while one believes the average is only 300 MeV/c. Further there is also the question of the mean free path, which Teucher mentioned, and there are some preliminary indications that at energies of the order of 10^{12} eV the mean free path increases very much. Some people think it becomes maybe twice as much, but this is not established. All this just points out that although with further cosmic ray experiments one will be able to obtain a lot more definite information one really needs high intensities and other features characteristic of machine work to do these things well.

STAFFORD : One small point that occurred to me when I was looking at Judd's Table I, was that it led one to believe that, a study of the nucleon-nucleon interaction could be satisfactorily done with existing machines with currents of the order of 0.3 μA. I think it is worth placing on record that when one is dealing with the nucleon one is really considering two particles, the proton with $T_3 = +\frac{1}{2}$ and the neutron with $T_3 = -\frac{1}{2}$, and it is certainly not true that even in that simple interaction one has sufficient intensity with any existing machine to study the $T_3 = -\frac{1}{2}$ interaction adequately.

PANOFSKY : I believe that the remarks made by Teucher and Pal indicate that one can find out from cosmic rays whether something very violent would happen at these very high energies, which would give gross modifications in the overall mean free path. I think that the inference to be taken from that goes in two directions; one, that one does need the high intensities of machines to get more details, but, on the other hand, that in terms of present knowledge there is no *specific* justification in this area. So it is a very two-edged situation which results from this. At this point, I am afraid, it becomes a matter of individual opinion, rather than of technical proof, whether the more standard approach, namely, extending the study of strong interactions to high energies, is going to be the main direction in which the future of high energy physics lies, or whether it is in terms of other less well-studied problems.