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"As far as I am concerned . . . there is a research gap at the present time. We aren't putting enough money into basic research to keep ahead of the Soviet Union. We are going to close that gap — and then create one where they are behind us all around the world".

Republican Presidential Contender Richard M. Nixon to a group of Southern delegates to the nominating convention, (Today, August 7, 1968, Cocoa, Fla.)

UNITED STATES
ENERGY RESEARCH AND DEVELOPMENT ADMINISTRATION
WASHINGTON, D.C. 20545

September 7, 1976

HIGH ENERGY PHYSICS ADVISORY PANEL

J. Ballam	H.J. Frisch
M.Q. Barton	F.E. Low
D.A. Bromley	B. McDaniel
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REPORT OF THE 1976 ZGS REVIEW PANEL

Enclosed is a copy of the Report of the 1976 ZGS Review Panel. Professor Robert L. Walker, Chairman of the ZGS Panel has been requested to post the Report to you in order to minimize the total time required for the Report to reach you. We shall discuss the Report in considerable detail during the September 27-28 meeting of HEPAP. I look forward to seeing you in Conference Room 200 at the ERDA Headquarters offices on 400 First Street, N.W. in a few weeks.

E. Coleman

Ernest Coleman, Ph.D.
Executive Secretary
High Energy Physics
Advisory Panel

Enclosure
As stated

AMERICAN REVOLUTION BICENTENNIAL 1776-1976 ®

REPORT

OF

THE 1976 ZERO GRADIENT SYNCHROTRON REVIEW PANEL

August 31, 1976

Robert L. Walker, Chairman	Calif. Inst. of Technology
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Contents

1. Introduction

Two and a half years ago, in its review of the FY 1975 Budget, the Office of Management and Budget requested that the AEC and the Science Advisor develop "a plan for shutting down the ZGS accelerator at the earliest reasonable time." Discussions of this request indicated that the intent was to provide a plan for "orderly and reasonable approach to shutdown."

A ZGS Study Committee and two Subpanels were formed to carry out this task and their findings and recommendations were presented in a Report dated September 9, 1974. The Physics Subpanel was the predecessor of the present review panel and most members of the present panel were associated with the previous study.

The following key recommendations were made by the ZGS Study Committee in its 1974 report:

(1.) In view of the important contributions of the ZGS experiments to the overall national High Energy Physics Program, operation of the ZGS should be continued, possibly at a more intensive level, through FY 1976, FY 1977, and FY 1978. Mid-to-late FY 1979 is projected as the earliest reasonable time to shut down the ZGS. The review proposed below would consider whether shutdown of the ZGS at the end of the decade would be in best interests of the national High Energy Physics Program.

(2.) A policy of advance notice should be established and announced relative to accelerator shutdown. In the case of the ZGS, the appropriate lead time appears to be two-to-three years. The nature of the apparatus and procedures for high energy physics experiments requires such a period for orderly completion of work underway when the shutdown is announced.

(3.) If funding for high energy physics remains approximately level in constant value dollars, it appears unlikely that all existing high energy physics facilities can continue to operate effectively when new facilities begin to require operating funds. In such circumstances an entire facility or some large segment of a facility will need to be phased out. The ZGS must be considered a leading, but by no means unique, candidate for such elimination. The programs at the ZGS, the Alternating Gradient Synchrotron (AGS), the Stanford Linear Accelerator Center (SLAC), and the Cornell Electron Synchrotron should be reviewed within two years to identify which segment of the Program might be eliminated, if necessary, by the middle or end of FY 1979 with least adverse impact. The timing of this review has been chosen to permit consideration of the latest scientific and programmatic information prior to giving the recommended advance notice prior to a shutdown at the middle or end of FY 1979.

Subsequently, in 1975 and early 1976, plans for High Energy Physics formulated within ERDA and other agencies have included provision of the PEP Electron-Positron Ring and other new facilities, and a letter to the Laboratory on March 5, 1976 indicated the expectation that the ZGS will cease operation by the end of 1978.

Because of these plans and the previous Study Committee recommendations, the present Review Panel has been constituted to carry out a scientific review of the ZGS program. The following specific charge for the Panel is contained in a letter to its chairman from James S. Kane, Director of the Division of Physical Research, USERDA, dated June 28, 1976:

> You are requested to review the status and role of the ZGS in the U.S. High Energy Physics Program and to update the 1974 report of the Physics Subpanel of the AEC-NSF ZGS Study. We are especially interested in obtaining your advice on what approach should be used prior to shutdown to gain the optimum scientific benefit from the ZGS and its special and unique facilities during its remaining lifetime. Present ERDA plans contemplate that the ZGS will be shut down by the end of calendar year 1978.
>
> We would appreciate receiving your report by September 1, 1976.

The Panel has benefited greatly from other recent studies of the present and future program of the ZGS. One of these is a Report of the Argonne Universities Association (AUA) Special Committee on High Energy Physics, F. J. Loeffler, Chairman, entitled Long Range Planning Study of the High Energy Physics Program at ANL, June 15, 1976. Another is a proposal prepared by the Argonne National Laboratory (ANL) staff in June, 1976 for Future Operation of the Zero Gradient Synchrotron as a Dedicated Polarized Proton Facility. In addition, the Panel attended a very informative presentation at ANL on June 30 and July 1, 1976.

In the report which follows, the Panel reviews the present and planned program of the ZGS in the context of the overall activity in high energy physics, and suggests priorities for different parts of a program which might be carried out during the remaining lifetime of the accelerator. In addition it evaluates the proposal for operation of the ZGS as a dedicated polarized proton facility after shutdown of the other parts of the program.

2. Recent Developments in High Energy Physics

In the past two years a series of astonishing discoveries have been made that foretell very major progress in our fundamental understanding of the nature of particles and their interactions. The most striking of these was the discovery of the J/ψ particle of mass 3.1 GeV in two independent experiments at the AGS and at SPEAR, announced in November, 1974. Subsequent work at SPEAR and DORIS has shown that the J/ψ is but one member of a family of extremely narrow (long-lived) quantum states above 3 GeV in mass. These states, formed via a virtual photon in electron-positron annihilation, or from the cascade decay of a state so formed, were almost totally unexpected, at least as far as their extreme relative stability is concerned. Immediately upon discovery, numerous theoretical interpretations were given, the most plausible of which was that they were a consequence of a new hadronic degree of freedom called charm. Charm is an additive quantum number of the strong interactions akin to isospin and strangeness. It can be incorporated into the mathematical description of hadronic states by enlarging the symmetry group of interactions from SU(3) to SU(4). The J/ψ and its partners are not "charmed" particles in the sense of carrying non-zero values of that quantum number, but reflect its presence in the same way that the ϕ meson of mass 1.02 GeV reflects strangeness in SU(3).

A necessary consequence of the hypothesis of charm and SU(4) symmetry is the presence of truly "charmed" particles, carrying non-zero values of the charm quantum number. The masses of such objects are estimated to lie in the 2-3 GeV range and indications are that this may indeed be the case. There is a suggestive rise in the total cross section in the e^+e^- annihilation into hadrons at 4 GeV, where such charm particles would be expected to be pair produced. Neutrino interactions have produced events with clear charm signatures, - single strange baryon production with a mass of 2430 MeV and lepton pairs with associated strange particles. More recently evidence has been presented from SPEAR for the discovery of a neutral charmed particle of mass 1.86 GeV, decaying (presumably weakly) into $K\pi$ and $K\pi\pi\pi$, and also a charged counterpart. These observations make it almost incontrovertible that the J/ψ, and its partners, and now these new states are manifestations of charm.

Another area where important new observations have been made is in neutrino interactions. In the report on the future role of the ZGS two years ago, the then recent discovery of neutral weak currents was discussed. The existence of such currents has now been firmly established. In fact, they have recently been observed in their most pure form; namely in elastic neutrino scattering where they occur with a rate that is 10-20% of the ordinary charged current induced two-body quasi-elastic rate (i.e., $\nu p \rightarrow \nu p$ = 10-20% $\nu n \rightarrow \mu^- p$). Large scale experiments at Fermilab in the past two years have studied both charged and neutral weak current interactions using high energy neutrino beams. The extreme feebleness of the interaction of neutrinos with ordinary matter places severe limitations on the precision and detail with which one can study neutrino-induced processes. Nevertheless, in a number of experiments with counters and now with the 15-foot bubble chamber, many properties of the interactions have been explored. These include a study of scaling in the x and y variables with comparison to charged-lepton interactions, first indications of the detailed Lorentz (spacetime) structure of the couplings, di-muon production and other apparent manifestations of the production of new (possibly charmed) hadrons in weak processes at the highest energies ($E_\nu > 50$ GeV). These latter data, though impressive, are still fragmentary and much work remains; present evidence indicates consistency with the hypothesis of charm.

Two years ago the committee report stressed the idea that high energy physics is a multi-frontiered field where discoveries in one area may impact significantly on some other area. The story of charm is a beautiful example. The possibility of a fundamental symmetry between the leptons and hadrons has excited the minds of physicists over the years. In 1964, a number of theorists remarked that a symmetry between the four spin 1/2 leptons (ν_e, e^-, ν_μ, μ^-) and hadrons would exist if the three quarks (p, n, λ) of SU(3) were augmented by a fourth spin 1/2 particle. One version of this scheme, with four quarks on an equal footing within SU(4) and having charm as an additional quantum number, was suggested by Bjorken and Glashow. New particles, possessing charm, and extending the SU(3) octets to SU(4) 15-plets, etc., were predicted. In the absence of their discovery, it was necessary to assume that the new quark (p') was much heavier than the other three, forcing the mass of charmed hadrons above 1.5 to 2.0 GeV, into a relatively unexplored mass region.

Several years later, the experimental absence of strangeness-changing weak neutral currents was a striking and puzzling fact in weak decays. Processes like $K_L^o \rightarrow \mu^+\mu^-$ were sought with high precision and were established to be less than 10^{-7} of all K_L^o decays. This remarkable absence of $\Delta S \neq 0$, $\Delta Q = 0$ transitions cannot be understood in terms of conventional (Cabibbo) theory, but can be explained elegantly with a four-quark model of weak currents, as was pointed out by Glashow, Illiopoulos and Maiani in 1970. It was most natural and economical to identify the four-quark model with the previously proposed SU(4) scheme possessing charmed hadrons.

By 1970 there were thus two independent arguments for charmed particles, one based on a largely aesthetic desire for symmetry among the leptons and quarks and the other on a singular property of the weak interactions. There is an additional, rather technical, field-theoretic argument (the avoidance of the so-called Adler anomalies) that requires the sum of the quark charges (times three, for the color dimension) to be equal and opposite to the sum of the lepton charges.

Prior to the experiments at SPEAR and BNL in the fall of 1974, however, there was no hint of the charm degree of freedom in hadronic physics. Mesonic and baryonic states seemed to fit into the patterns of SU(3). The theoretical arguments were none the less rather compelling. Theorists began to realize that if the charmed states were sufficiently massive they might effectively decouple from the lighter ordinary hadrons. This would make the states metastable on the hadronic time scale of 10^{-23} second --too narrow to be detected easily. A theoretical development called "asymptotic freedom" in which certain types of theories exhibit weak coupling at high energies gave hope that semi-quantitative calculations could be made for bound quark-antiquark systems, provided the quarks were massive enough. The idea of hadronic states of "charmonium," in analogy with positronium, thus emerged in the summer and fall of 1974, before the discovery of J/ψ. While details are wrong in the early charmonium predictions of Appelquist and Politzer, the qualitative features are just what was subsequently found in e^+e^- annihilation -- a few narrow "bound" states, with a continuum beginning at higher energies, the threshold for pair production of charmed particles!

This sketch of the history of charm, the new degree of hadronic freedom, serves to illustrate the interplay of theory and experiment, of concepts in strong and weak interactions, and also an idea whose time has come.

The reader may think that a complete description of the interaction of fundamental particles is at hand with the discoveries of the past two years. Far from it! The discovery of charm is indeed a triumph. But Nature divulges her secrets one at a time, sometimes teasing with glimpses of possible sweeping generalizations, often with confusing facts that spoil an otherwise pleasing pattern. One example is presented by the μ-e events observed at SPEAR. It is found in e^+e^- annihilation at energies somewhat above the J/ψ, near the threshold for charmed particle pair production, that occasionally the annihilation results in production of a muon and an electron, with no other accompanying charged particles. It is possible that these events come from the leptonic decays of charmed particles, but that seems unlikely. Most probable is the pair production of charged heavy leptons that subsequently decay into ordinary leptons (e.g., $L^+ \rightarrow \mu^+ \nu\bar{\nu}$). It is apparently just by chance that the thresholds for charm and for heavy lepton production occur at roughly the same energy.

If we recall the theoretical arguments on lepton-quark symmetry and/or the requirements for avoiding Adler anomalies, we see that SU(4) and charm cannot be the end. More leptons means more quarks, probably at least six. Theorists thus discuss a world with at least two more quarks, presumably still heavier than the charmed quark, and manifest in the spectrum of hadronic states only at still higher energies. There are other arguments, from weak interactions, that require more quarks, possibly with right-handed instead of left-handed weak couplings. We thus see that the discovery of charm takes us a big step along the path of probing Nature's secrets, but leaves us with new unsolved puzzles and possibilities.

How does the program of the ZGS fit into these very significant developments of the past two years? How does the ZGS program compare and relate to the programs at other ERDA laboratories?

First of all, it should be stated clearly, as it was in the 1974 report, that experiments at the highest energies are more likely to produce fundamental new discoveries than those at lower energies where the exploratory work has already been done. Granted the premier position of facilities at the highest energies, the ZGS has, nevertheless, impressive, even unique, capabilities that make its program important in an absolute sense. It is not a question of competition as much as complementarity. For example, in the area of neutrino physics the ZGS effort is not unique; Brookhaven has comparable programs with higher energy neutrinos than at Argonne (although "low energy" in comparison with Fermilab). However, neutrino physics is so difficult, so low in statistical accuracy, and so fraught with potential for systematic errors, that redundancy of experiments is essential in determining the truth. Only when several more or less similar but independent experiments yield similar results is the physics community as a whole persuaded of their validity. Low energy neutrino experiments are important because of the relative cleanness and simplicity of the final states. The ZGS program involving the 12-foot bubble chamber and the low energy neutrino beam is providing important data concerning the nature of weak currents, both charged and neutral. The quasi-elastic reaction $\nu n \rightarrow \mu^- p$ and the reactions involving single pion production do not require high energy and are being explored at Argonne in a definitive way.

In hadronic spectroscopy, excellent work is being done at the ZGS. The discovery of new $J = 0^+$ states in the mesonic spectrum with the streamer chamber and the effective mass spectrometer and the detailed study of neutral particle decay modes of mesons in the charged-neutral spectrometer are examples. The charmed particle spectroscopy being uncovered at SPEAR makes the spectroscopy at lower energies more important, not less. The charmed or hidden-charmed states seem to be just those expected from a realistic, nonrelativistic quark model, whereas the mesonic states at low energies, although qualitatively in agreement with such a model, show peculiarities, such as states that are apparently missing. The differences may be due to asymptotic freedom -- applicable at 4 GeV, but not at 1-2 GeV. Whatever the reasons, it is more important than ever to study the spectroscopy of hadronic states in as complete detail as possible.

In the area of polarized proton beams the ZGS is truly unique. For technical reasons (weak focussing in the ZGS, strong focussing in all other accelerators) no other accelerator has at present, or is expected to have in the foreseeable future, beams of polarized protons at high energy. Although it is one of the oldest of the established quantum numbers, the role of nucleon spin in the dynamics of hadronic processes is very poorly understood. Until recently, the paucity of data on the spin dependence of high energy nucleon-nucleon processes has discouraged detailed theoretical investigations. Several theoretical ideas suggest, however, that spin plays a fundamental role which must be better understood.

Together with polarized targets, the polarized beams at the ZGS provide the capability for a "complete" experiment that determines all possible <u>amplitudes</u> for proton-proton and proton-neutron elastic scattering. Good data at one energy (e.g., 6 GeV) and a reasonable range of momentum transfer will teach us much about the spin dependence of scattering. Data at two energies will permit study of the energy dependence of the dynamical mechanisms responsible for the scattering and its spin dependences. At large transverse momentum where quark-quark scattering appears important, these experiments may be able to observe effects due to the quark spin.

Recent experiments have completed part of the program on elastic scattering and have studied polarization phenomena at large angles in elastic scattering and in a variety of inelastic processes, exclusive and inclusive, at 6 and 12 GeV/c. The observed effects seem surprisingly large and call into question long held beliefs on the importance of spin in high energy hadronic interactions. Nowhere else is polarization work of this sort being done.

In brief, the past two years have seen momentous and rapid progress in high energy physics, with e^+e^- annihilation into hadrons providing a major share of the new discoveries, while neutrino physics has been close behind. With some notable exceptions, the important discoveries were not made at the accelerators of modest energy, the AGS and the ZGS. Nonetheless, significant and often unique work continues to be accomplished at these machines because, as we have stressed, the field of high energy physics is multi-frontiered and energy is only one frontier. Some noteworthy aspects of the ZGS program and their relationships to the overall effort have been mentioned above. A more complete description of the present and planned physics program of the ZGS is given in Section 4 of this report.

3. ZGS Facilities

A. Accelerator and Beams

The facilities at the ZGS are built around a 12.5-GeV proton synchrotron which delivers an unpolarized beam of approximately 4.5×10^{12} protons per pulse at a typical repetition rate of 13 per minute. The intensity has nearly doubled over the past two years. A 700 ms flat-top permits a 20% duty factor for counter-spark chamber experiments. The accelerator is reliable with an operating efficiency better than 90%, in spite of the tight budgetary restrictions on manpower available for maintenance and operation.

The beam delivery system to experiments is based on two external proton beams (EPB's) which operate simultaneously. Operation at different energies by means of a "front porch," as well as simultaneous operation of short and long spills have been routine for years. Fast resonant extraction for the 12-foot bubble chamber has also been in use for several years. Slow resonant extraction into both EPB's has recently been significantly imporved, both as to extraction efficiency and spill structure.

At present, the two ZGS EPB's feed five external targets which, in turn, supply particles to ten secondary beams. In addition there are four beams used exclusively for transporting polarized protons to experiments. These beams provide an ample selection of the secondary particles which can be produced by the ZGS. Often five or six beams are in simultaneous use, either for data taking or test purposes, which permits a highly cost-effective program. For example, in FY 1976, while the accelerator operated for 4,000 hours for particle research, a total of 11,000 research secondary beam hours was utilized.

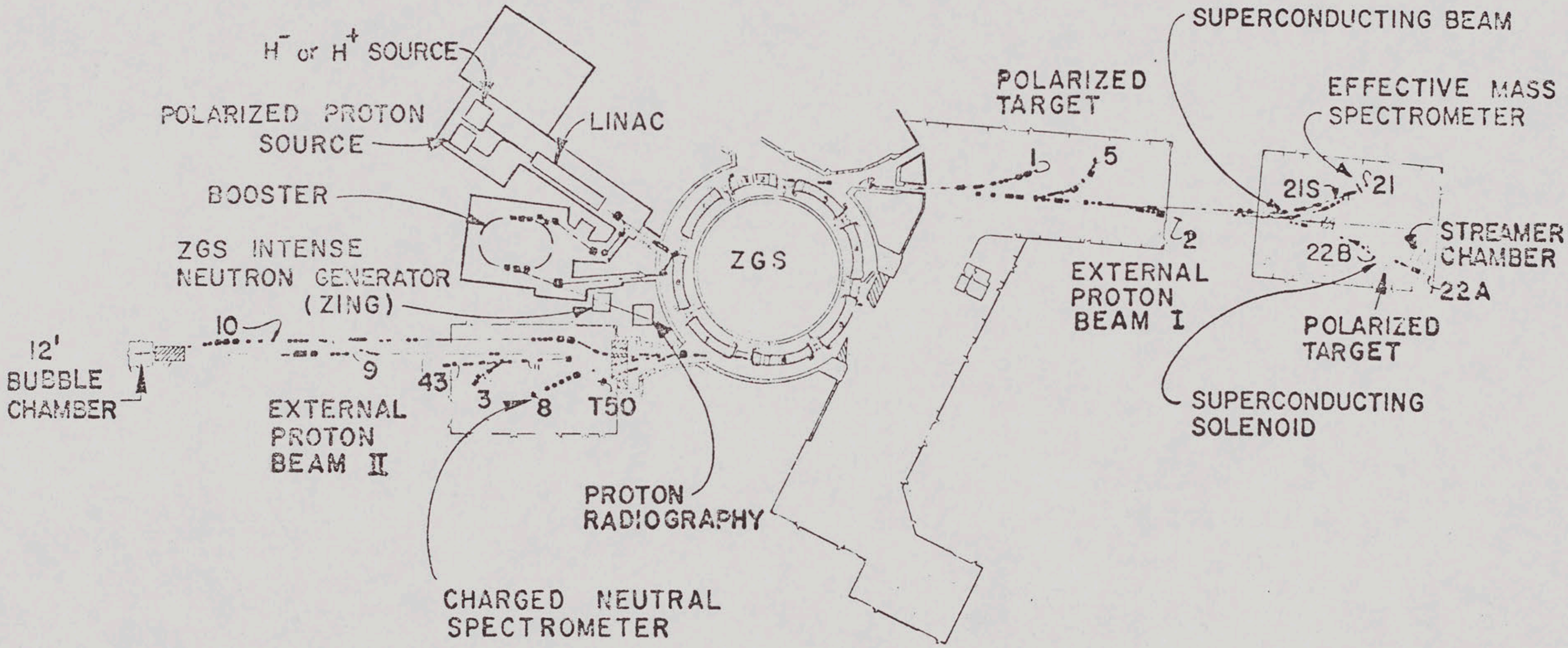
A summary of the characteristics of beams currently in use is given in the accompanying table, and the layout of these beams is shown in the accompanying figure. Several new beams have been installed since the 1974 ZGS study, including a superconducting beam line to transport 12 GeV/c polarized protons to the Effective Mass Spectrometer (EMS). With ten dipoles and two quadrupoles, this beam is the only fully superconducting beam line in the world and it saves several megawatts of power.

Acceleration of a beam of polarized protons in the ZGS was achieved in July 1973; since then the polarized proton beam has become a reliable ZGS facility. Two years ago, at the time of the 1974 ZGS study, an intensity of 4×10^{8} protons per pulse had been achieved and a factor of four improvement was anticipated. During the most recent month of running, an average beam of 1.0×10^{10} protons per pulse with a period of 2.4 sec, about 70% polarization and 6 GeV energy was shared by three concurrent experiments. Even this intensity was surpassed during an 8-hour run in June when an average of 1.5×10^{10} per pulse was achieved, a factor of ten over that anticipated two years ago! The reliability of the polarized beam has also improved; during the most recent month of running, the overall operating efficiency was maintained at an enviable 94%. Another important improvement has been the ability to reverse the proton spin each pulse without otherwise affecting the beam characteristics; this feature is essential for high precision experiments.

In 1975 a polarized beam was accelerated to 12 GeV. The best polarization achieved thus far at this energy is in the neighborhood of 60%. The difficulties encountered in obtaining this value (still somewhat less than the 70 to 75% at lower energies), as well as a series of ZGS accelerator physics experiments carried out in collaboration with CERN scientists, indicate that it will be very difficult to accelerate polarized protons in a strong focusing machine, especially above 12 GeV. This means that not only is the ZGS polarized beam unique at the present time, but it is very unlikely that this capability will be duplicated in the forseeable future at any other machine in the world.

ZGS Beams

Beam No.	Characteristics
1 & 2	Both of these are primary proton beams. By using a front porch, lower energy protons can be transported. They are currently being used for polarized proton experiments.
3	Enriched 500 MeV/c kaon beam - 1950 $K^-/10^{11}$ protons, 13500 $K^+/10^{11}$ protons.
5	High momentum (1-9 GeV/c) large momentum acceptance beam ($\pm$ 5% $\Delta P/P$ with $\pm$ 0.25% $\Delta P/P$ resolution). At 4 GeV/c, flux is 10^6 $\pi^-/10^{11}$ protons. Has run with 2.5 x 10^6/pulse at 8.6 GeV/c. By 11/76, it will be able to compatibly transport polarized protons up to 12 GeV/c.
21	Intermediate momentum π beam. The momentum range is ~ 1-6 GeV/c. Typical fluxes for 10^{11} protons for momenta 3-6 GeV/c are π^+ 240K to 96K, π^- 185K to 65K, K^+ ~ 8K, K^- 1.8 to 0.5K.
21S	For polarized protons up to 12 GeV/c; this superconducting beam ends in the same location as 21.
22A	High momentum π beam 1-9 GeV/c. Typical fluxes for 10^{11} protons: π^- 130K to 25K. Currently being used for the streamer chamber facility. Can transport polarized protons up to 9 GeV/c; is being upgraded to 12 GeV/c.
22B	Intermediate π beam 1-6 GeV/c. Typical fluxes for 10^{11} protons: 130K to 80K. Also used to transport 6 GeV/c polarized protons.
8	High momentum π beam 1-9 GeV/c. Maximum flux per 10^{11} protons at 5 GeV/c: 160K π^-, 480K π^+.
43	High intensity K^o_L beam. Estimated flux 1.2 x 10^7 $K_L/5 \times 10^{11}$ protons, 10^8 n/10^{11} protons. Could also be used to transport polarized protons.
12' BC beams	Several beams are available to this facility: (a) Separated $K^{\pm}$ beams at 4.6, 5.1, 6.5 and 8.0 GeV/c. $\bar{p}$ beams at 2.9, 3.3, 4.1, 5.8 GeV/c, and stopping to 1.5 GeV/c. - pions and protons 1-11 GeV/c. (b) Polarized protons up to maximum energy.


H⁻ or H⁺ SOURCE
POLARIZED PROTON SOURCE
LINAC
BOOSTER
ZGS INTENSE NEUTRON GENERATOR (ZING)
10
12' BUBBLE CHAMBER
9
43
3
8
T50
EXTERNAL PROTON BEAM II
PROTON RADIOGRAPHY
CHARGED NEUTRAL SPECTROMETER
ZGS
POLARIZED TARGET
1
5
2
EXTERNAL PROTON BEAM I
SUPERCONDUCTING BEAM
EFFECTIVE MASS SPECTROMETER
21S
21
22B
STREAMER CHAMBER
22A
POLARIZED TARGET
SUPERCONDUCTING SOLENOID

B. Experimental Facilities

Experimental setups utilizing counter-spark chamber techniques are generally built by the research teams to meet the needs of specific experiments. In addition, several more permanent facilities have been set up at the ZGS. The Effective Mass Spectrometer (EMS) is a multiparticle spectrometer of wide aperture which, since its construction in 1971, has been used by three ANL and five University teams in some sixteen experiments. It has yielded excellent new data of high precision on a variety of multiparticle processes at intermediate energies. A similar spectrometer, but with the detection of γ rays as well as charged particles, has been constructed and operated by a Carleton, McGill, Michigan State, Ohio State and Toronto collaboration. It has already performed several outstanding experiments on both the production and radiative decay of various mesons. Another ZGS facility, the streamer chamber, built in collaboration with physicists from the University of Illinois, has initiated detailed studies of baryon exchange processes in multibody final states. The energies available at the ZGS are particularly well suited for studies of this kind. More recently the streamer chamber has been used in a very successful experiment which gave evidence for a new S-wave enhancement in $K_S K_S$ near 1300 MeV. Also, a sophisticated K_L^o-decay spectrometer has been used to obtain data on the very important branching ratio for $K_L \rightarrow \mu^+\mu^-$.

The 12-foot bubble chamber has been by far the most reliable big bubble chamber in the world, having taken over four million pictures, more than one million in the last cooldown. It has a superconducting magnet which generates an 18-kG field over a visible volume of 16 m^3. The tracks are of very good quality for a large chamber and ionization information is useful up to about 800 MeV/c.

The large volume is especially useful for neutrino experiments and hadronic experiments using a track sensitive target (TST). It has also been used to good advantage to observe the decays of strange particles which would have escaped from a smaller chamber, to trap low momentum particles (allowing good particle identification), and to observe secondary interactions of $\bar{n}$'s produced from $\bar{p}$ charge exchange. The large volume is doubly important for the TST work: first, space is required for the liquid-hydrogen filled target, and secondly, a distance is required for γ rays to convert in the hydrogen-neon mixture surrounding the target. At present, the 12-foot chamber is the only bubble chamber in the world with an operational TST capability and is thus in a unique position to study reactions with more than one missing neutral.

The 12-foot chamber is also unique in that it is at present the only large bubble chamber with hadron beams of $\leq$ 10 GeV. It has an extensive beam switching arrangement which provides a wide variety of separated beams, ranging from stopping antiprotons (electrostatic separation) to beams of $K^{\pm}$ at 8 GeV/c (rf separation). These capabilities could be duplicated at other laboratories, but only at considerable expense.

The most important contributions of the 12-foot chamber have been in the field of neutrino reactions below 1 GeV. At these energies, most of the events can be completely reconstructed and this work complements that of other laboratories at higher energies where reconstruction is usually impossible due to missing neutrals and resolution problems. The recent increase of intensity, as well as that expected from the booster, will considerably improve the neutrino studies.

Another outstanding capability at the ZGS is provided by a number of excellent polarized proton targets. Two targets have been in use with spins aligned in the vertical, normal to the scattering plane. These targets have each been equipped with a He^3 refrigerator, allowing polarization of up to 90%. A new capability had its very successful inauguration during the May 1976 run - an "A and R" target with superconducting magnet to allow the target spin to lie in the scattering plane, either along the beam direction or at right angles. This target is the only one of its kind in the United States.

Another recent addition to the capability of handling spin orientation is a superconducting solenoid capable of rotating the proton beam spin direction from vertical to horizontal at momenta up to 6 GeV/c. A second solenoid will extend this capability to 12 GeV/c. Vertical bending magnets are being used to rotate the proton beam spin into the longitudinal direction, along the beam, for an experiment looking for parity-violation effects in the total cross section. Longitudinal beam polarization will also be used in the study of inelastic reactions with the EMS.

The construction of a polarized deuteron target is presently planned. This target would allow further comparison of the pp and pn systems, to follow up a recent ZGS experiment which showed a surprising difference in the energy behavior in the polarization asymmetry of the two systems.

C. Improvements

The ZGS experimental program began in 1964, several years after the CERN PS and Brookhaven AGS. Since then there have been a number of major improvements to the ZGS. The experimental area for EPB II and the 12-foot bubble chamber were funded in FY 1965 at a cost of $18.6 M. A new titanium vacuum chamber with pole face windings was installed in the summer of 1972. The second preaccelerator and the polarized beam first operated in 1973. There has also been a continual upgrading of the accelerator and the associated facilities.

The accelerator still has a great potential for further development. A higher energy injector should yield a large improvement in the beam current. For the past several years a development program has been pursued toward the use of a rapid cycling 200-500 MeV synchrotron as a booster accelerator. The booster program got underway by using H^- injection with subsequent electron stripping into a small-aperture rapid-cycling synchrotron (Booster I) constructed from the old Cornell 2 GeV machine. In addition to demonstrating accelerator techniques such as H^- injection, this machine provided beam for experiments in proton radiography and in materials research using neutron scattering. In 1974 work began on Booster II, a 500 MeV machine designed to substantially increase the ZGS intensity. Although final assembly was originally scheduled for summer 1976, problems with magnet fabrication have delayed this schedule. These problems have been resolved recently, and assembly is expected during November 1976. A minimum ZGS intensity of 10^{13} protons per pulse is expected by summer 1977. This increase in intensity of more than a factor of two, coupled with better neutron shielding, will substantially improve the neutrino program. The booster will also be used directly to provide beams for neutron scattering and perhaps for muon experiments. It will also be an important tool for accelerator research, since it incorporates several major new advances in accelerator technology.

The polarized-beam intensity has improved exponentially by a factor of 100 over the first run three years ago. While this situation cannot continue indefinitely, the next factor of four can already be anticipated, just as it was two years ago. The increase of intensity is especially important for studies of spin dependences at large momentum transfers. Recent and future improvements in the various beam lines, including the ability to rotate the proton spin and to provide beams of different energies to different experiments operating at the same time, are adding considerable flexibility to the polarized proton program.

4. Present and Planned ZGS Program

In this Section we review the present physics programs being carried out at the ZGS and we consider the question of how these programs should be continued, phased out, or extended during the time remaining for ZGS operation. It is convenient to group the experiments into three general categories based upon their compatibility for running simultaneously.

The first category we shall call the "conventional program," - a misleading name by which we simply mean all experiments which do not require either the polarized proton beam or the neutrino beam. The second category is the neutrino program using the 12-foot bubble chamber. It requires maximum intensity, maximum energy, and a fast spill so that when this experiment is running, the ZGS is fully dedicated to it.

In the third category are the experiments which make use of the unique capability of the ZGS to accelerate polarized protons.

A. The "Conventional Program"

That part of the ZGS program which is carried out with the unpolarized proton beam, or with secondary beams produced by it, deals with unresolved problems in the energy range below 12 GeV. Among current puzzles perhaps the most significant is that of the missing states of the quark-antiquark boson model; aspects of reaction dynamics and some specific questions in weak and electromagnetic decays are also of considerable importance.

Inevitably, experiments of this character could, at least in principle, be carried out at other accelerators, in particular at the AGS. Nevertheless, as will be discussed below, the ZGS program in this area is of very high quality and is continuing to add valuable insights. Also, the conventional program allows a high degree of University participation; about thirty University groups are or have recently been engaged in such experiments.

The conventional program has three major parts. The first part depends on three functioning spectrometer facilities, the EMS (effective mass spectrometer), the charged-neutral spectrometer and the streamer chamber. The 12-foot bubble chamber with its track sensitive target (TST) serves as focus for the second. Finally, the third part involves individual setups designed to investigate specific topics.

(i) Magnetic Spectrometers:

Some of the interesting work of the EMS with polarized beams will be described later. The EMS continues to be a reliable and versatile facility which yields data of previously unachieved precision. The most recent work involves isolation of the I = 0 and I = 1 interference effects in the reactions $\pi^- p \rightarrow K^+ K^- n$ and $\pi^+ n \rightarrow K^+ K^- p$. 110K examples of the former and 50K of the latter reaction were obtained, which permitted a precise moment analysis of the difference of these cross sections. Such an analysis can reveal very delicate effects. Indeed, the data show interference between the I = 0 S* and the I = 1 $\delta(970)$, a $J^{PC} = 0^{++}$ boson state. In addition the data show evidence for the existence of another S* at 1300 MeV.

This same state was observed in a recent streamer chamber experiment in a study of $\pi^- p \rightarrow K_s K_s n$. Here the S*(1300) appears both in the cross section and as a S and D wave interference in the Y_2^0 moment. The data have less statistical precision than those of the EMS but the effect is quite convincing.

The new charged-neutral spectrometer, the product of a collaboration of three Canadian and two US university groups, has been put into operation recently to investigate multibody final states and it is beginning to yield data of very great interest. It is a large aperture spectrometer which permits the measurement of both charged and neutral secondaries. As a result it can yield high statistics results on boson decays which involve π^0's and γ's. This is a poorly known field and the Panel was much impressed by preliminary results which showed clear evidence for the D meson and its decay into π and δ, for the rare electromagnetic decays $\omega \rightarrow \eta\gamma$ and $\eta' \rightarrow \omega\gamma$, and for the B meson. Evidently the charged-neutral spectrometer is just beginning to tap a most interesting field; it seems most unlikely that this area can be adequately explored before the scheduled ZGS shutdown.

As may be seen from the foregoing, the main thrust of experiments using the three spectrometers is what might be termed SU(3) physics. The quark version of SU(3) is by now supported by an impressive body of experimental evidence, but important puzzles remain. One of the most persistent difficulties is the continuing confusion in the classification of the predicted L = 1 quark-antiquark ($q\bar{q}$) boson states. This may indicate that an important ingredient of the picture is still missing. While SPEAR and Fermilab discover new quarks, the ZGS spectrometer experiments address an older but still central problem, the spectrum of $q\bar{q}$ states.

(ii) 12-Foot Bubble Chamber:

The main apparatus improvement since the 1974 report was the first successful operation, in December 1975, of the 12-foot bubble chamber with a track sensitive target (TST). In this mode of operation the central section of the chamber where interactions occur is pure hydrogen, while the outer parts of the chamber contain a 35% Ne-H mixture which enhances the materialization of photons. In the 12 GeV pp interactions studied in the test run, the fraction of events exhibiting three or more γ rays was 30% as compared to 0.7% in the same chamber without TST. In future experiments the neon concentration will be increased to 75%, thus further increasing the sensitivity to γ rays. It appears that the TST technology will make hitherto intractable multiphoton events (the majority of events at high energies) accessible to measurement and interpretation.

Given the variety of beams of various hadrons up to ~ 9 GeV/c available at the ZGS, the study of multiphoton events could, in principle, be pursued for a number of years. At present, five exploratory exposures, of 50K to 160K pictures each, with incident protons, $\bar{p}$'s and π^+'s are approved. It remains to be seen how fruitful this technique will turn out to be. Past attempts, involving smaller chambers, to introduce photon materialization regions into bubble chambers have not been highly successful. However, the size of the 12-foot chamber may change this situation.

In view of the newness of the TST, the decision to concentrate initially on a number of exploratory exposures was an appropriate one to permit many experimenters to become familiar with the analysis of the complex events expected in this field. However, this decision needs reevaluation in the context of the projected ZGS shutdown. There is now less need to acquaint experimenters with this research tool but more need for a few experiments of high statistical significance.

During March, 1976, the 12-foot chamber, operating in its normal (non-TST) mode, obtained in excess of 700K photographs of K^-p interactions at 6.5 GeV/c, thereby completing a 1000K picture, 75 events/μb exposure. This run again demonstrated the great reliability of this complex research tool and emphasizes the importance of a decision concerning the fate of the chamber after the ZGS is shut down.

(iii) Special Setups:

Specialized setups designed to yield data on specific topics are a relatively small component of the overall ZGS program. Furthermore, because of their transitory nature, they have less influence on the recommendations which this Panel must make. Two experiments of this nature have recently been completed. One, on Ericson fluctuations, did not confirm earlier indications of a rather large effect reported by CERN. The other, on the very important and rare $K_L \rightarrow 2\mu$ decay, is still being analyzed but a preliminary analysis of a subsample of the data has already isolated more events than the previous world sample. A major experiment on the beta decay of polarized Σ's is still being debugged. This is a very difficult experiment which will require a substantial effort to complete during the very limited running time remaining for conventional experiments before the ZGS shutdown.

No other major experiments of this general character are on the currently approved list of ZGS experiments. This is at least partly a result of the uncertainty concerning the future of the ZGS.

B. The Neutrino Program

Recent discoveries of new phenomena in neutrino induced weak interactions reaffirm the belief that the study of these interactions is of basic interest and fundamental importance to the understanding of elementary particle physics. Neutrino reactions together with electromagnetic interactions provide the principal means of probing the structure of hadrons and of studying the "currents" of particle physics.

The study of neutrino reactions at low energy is of importance, not only in providing detailed quantitative data for specific reactions, but also in providing a "base line" to which high energy data can be compared. Understanding the threshold behavior as production of new particles or new currents becomes energetically allowed is crucial to the establishment of these phenomena.

The ZGS neutrino program, carried out by the ANL-Purdue-Carnegie Mellon collaboration, has made a beginning toward the goals described above. The first experiment (E234) consists of 1.3 million stereo photographs of the 12-foot bubble chamber filled with hydrogen and deuterium. From an analysis of these pictures about 1450 events have been obtained, distributed among the different final states as indicated in Table I, page 24.

From the analysis of these data, the group has published results on the following subjects:

(i) The quasi-elastic reaction $\nu n \rightarrow \mu^- p$.
A comparison of the data obtained in this experiment was made to the conventional current-current weak interaction theory which requires three real form factors. Using the electron-nucleon scattering results to fix the vector mass, the experiment is in good agreement with an axial vector mass $M_A = 0.95 \pm 0.12$ GeV.

(ii) Single pion production by neutrinos.
In an analysis of the three reactions

$$\nu p \rightarrow \mu^- p \pi^+$$
$$\nu n \rightarrow \mu^- n \pi^+$$
$$\nu n \rightarrow \mu^- p \pi^o$$

in the first exposure, the experimenters found a large isospin 1/2 amplitude as well as a resonant isospin 3/2 amplitude. The large I = 1/2 amplitude was predicted by Adler in 1968.

(iii) Strange Particle Production.
Among the approximately 1450 events found in the exposure were seven events in which strange particles were produced. Four were examples of associated production by the usual charged current and one event was interpreted as the first observation of associated production by the neutral weak current. Two events were examples of $\Delta S = 1$ charged current interactions.

(iv) Single pion production by the neutral weak current.
At these low energies the experimenters isolated the reactions

$$\nu p \rightarrow \nu n \pi^+$$
and
$$\nu p \rightarrow \nu p \pi^o \quad .$$

The result of their analysis suggests isospin 3/2 dominance, which would rule out the suggestion of Sakurai that the neutral weak current is isoscalar.

In addition, a number of other results have been presented at conferences. These included the observation of a dilepton event, a comparison of charge symmetric reactions showing that the neutral current has both isoscalar and isovector components, and total cross section measurements showing a linear rise in the threshold region, with the ratio $\sigma(\nu n)/\sigma(\nu p)$ close to two as expected by the naive quark model.

The data obtained so far on all these interesting subjects is quite limited. Thus one of the important goals to be accomplished before the ZGS is shut down is the collection of significantly improved data on low energy neutrino interactions. A second generation neutrino experiment involving 1 to 1.5 million pictures with deuterium in the bubble chamber is planned. Some of the ways in which this proposed experiment differs from the first exposure are:

(i) Reduction of hadron induced background in the new experiment by better than an order of magnitude. This is extremely important for neutral current investigations.

(ii) The average intensity of accelerated protons has been increased from ~ 2.0×10^{12} to 4.3×10^{12} per pulse.

(iii) An important part of the proposed program is the completion and successful operation of the booster which is expected to increase the circulating intensity to at least 10^{13} protons per pulse and perhaps to even higher intensitites. Booster operation for physics is anticipated during the summer 1977.

(iv) The insertion of tantalum plates in the bubble chamber will improve the detection of γ-rays to 15% (30% for π^o) averaged over their spectrum. This will improve the separation of reactions $\nu n \to \mu^- p$; $\nu p \to \nu p$, and $\nu p \to \mu^- p \pi^+$ from the same reactions with additional π^o. Although the use of plates in this chamber is new, experience at other laboratories and with the 30° chamber at ANL would indicate little difficulty in accomplishing this improvement.

The improvements embodied in the proposed experiment will yield results which will place those physics areas of E234 that are still qualitative on a sound quantitative basis. The experimenters plan to operate the ZGS and bubble chamber in one month periods during each of which they expect to get 300-350 thousand pictures. The first such neutrino operating period is scheduled for October 1976; for the remaining exposure the booster is expected to be in operation. In Table I event rates for the proposed experiment are compared to the total numbers of events obtained in the old experiment. From this table one may easily see the improvement in statistical accuracy expected from the new experiment under two possible scenarios; one that the booster is successfully completed on schedule and only 300,000 pictures are taken under the current operating conditions, the remainder with the booster; and two, all pictures are taken without the use of the booster and within the demonstrated capability of the ZGS. Irrespective of the booster the additional one to one and a half million picture exposure can provide an impressive and definitive picture of low energy neutrino interactions.

As emphasized in Section 2, it is very important to obtain overlapping neutrino data from independent experiments. It is hoped that data will be obtained from experiments at both the ZGS and AGS for this purpose. These data will be complementary to a considerable extent because the spectra of the two neutrino beams are different. A comparison of various characteristics of the ZGS and AGS bubble chamber neutrino experiments is presented in Table II.

Table 1

Event Rates in ZGS Neutrino Experiments

Reaction	Total Events in E-234	Rates per Million Pictures: Proposed Experiment w/o Booster	Rates per Million Pictures: Proposed Experiment w Booster
$\nu n \rightarrow \mu p$	795	2500	5000
$\nu p \rightarrow \mu^- \pi^+ p$	368	900	1800
$\nu n \rightarrow \mu^- \pi^\circ p$ (E > 1.5)	68	250	500
$\mu^- \pi^+ n$ (E > 1.5)	82	300	600
$\nu n \rightarrow \mu^- p \pi^+ \pi^-$	25	75	150
$\nu p \rightarrow \mu^- p \pi^+ (m\pi^\circ)$, $\mu^- n \pi^+ \pi^+ (m\pi^\circ)$	73	225	450
$\geq 3\pi$ production	31	100	200
Strange Particle	9	25	50
Total Charged Current Events	1450	4400	∿9000
Neutral Currents*			
$\nu p \rightarrow \nu p \pi^\circ$	8	35	70
$\nu n \pi^+$	3	25	50
$\nu n \rightarrow \nu p \pi^-$	16	60	120
$\nu n \rightarrow \nu K \Lambda$	1	∿3	∿5

*The number of events depends critically on cuts needed to reduce background.

Table II. Comparison of Facilities for Bubble Chamber Neutrino Experiments at ANL and BNL

	ANL	BNL
Proton Energy	12 GeV	30 GeV
Number of protons per pulse delivered to target	2.6×10^{12} (6×10^{12})*	1×10^{13}
Repetition period	~ 3 sec	1.2 sec
ν Flux per pulse into chamber	$\sim 0.7 \times 10^{11}$ $(\sim 1.6 \times 10^{11})$*	1.8×10^{11}
Average Energy of Neutrino Events	0.8 GeV	2.5 GeV
Chamber Fiducial Volume	10 m^3	4 m^3
Rate of ν interactions in the fiducial volume per pulse	0.46×10^{-2} (1×10^{-2})*	$\sim 0.5 \times 10^{-2}$
Neutrino pictures taken as of July 1976	1.3×10^{6}	0.75×10^{6}

*Assuming Booster operation - 1×10^{13} protons per pulse accelerated.

C. Polarized Proton Beam Experiments

For the forseeable future, the ZGS will be the only accelerator capable of producing high energy beams of polarized protons. Therefore the plan to terminate operation of the ZGS places special importance on those experiments which require this unique capability.

The general objective of most of the polarized beam experiments is the study of dynamics, the reaction mechanisms of high energy interactions. (The exceptions are a few experiments designed to investigate basic symmetries such as parity and time reversal invariance.) The description of reactions involving particles with spin is complicated by the spin dependence which introduces a number of independent amplitudes, each with its own dependence on the kinematic variables s and t (or energy and angle). Any real understanding of such reactions depends on our understanding these different spin-amplitudes and this, in turn, requires experiments with polarized beams and targets.

Consider, for example, the relatively simple and basic reaction, pp elastic scattering. Even with constraints imposed by parity and time-reversal invariance, there are five independent (complex) amplitudes needed to describe this reaction. Thus there are nine real numbers to be determined at any given angle and energy by a "complete set" of experiments. An overcomplete set of eleven measurements will, in fact, be needed to resolve discrete ambiguities inherent in extracting amplitudes from bilinear ovservables. Some of these experiments are very difficult as they involve simultaneously measuring two or three spins in all three possible orientations. They do appear possible at the ZGS; they are totally impractical without a polarized beam. Initial experiments at the ZGS have shown that spin effects are large (and in some cases even increasing) up to the maximum energy studied, 12 GeV. Some recent results, largely unexpected, to come from the ZGS polarized program are the following:

(1) The spin dependence of $np \rightarrow np$ is very different from $pp \rightarrow pp$.

(2) Double spin flip cross sections in $p + p \rightarrow p + p$ are substantial, even in the diffractive peak.

(3) At large p_T^2 in $p + p \rightarrow p + p$, C_{NN} (see notes on Table III, p. 28) seems to grow with incident energy.

(4) Inclusive cross sections have a large and complicated spin dependence.

(5) There is a large spin dependence in σ_{Tot} near 2 GeV/c.

It is clear that spin dependence is important and non-trivial.

The ZGS polarized beam program is relatively new and much work remains to be done. Critical questions raised by the proposed termination of ZGS operation are: what might be considered a minimal but essential program of polarized beam experiments which should be carried out before the polarized beam is turned off, and how long a period of operation will be required to complete this essential program. There is, of course, no unique objective answer to these questions, but the Panel believes the program should include the following:

(1) A complete set of experiments to determine the pp elastic amplitudes at one energy, 6 GeV, in the diffraction peak.

(2) Sufficient data on pp elastic at 12 GeV to see how the amplitudes vary with energy.

(3) Data on pn elastic amplitudes at 6 GeV in the diffraction peak using a polarized neutron target and the polarized beam.

(4) Some data on np charge exchange.

(5) Some data on the spin dependence of pp elastic in the medium t range.

(6) Measurements of C_{NN} and 3-spin correlations for pp elastic at high t, including the energy dependence at 90°.

(7) Tests of P and T symmetries by pp elastic 3-spin correlations at medium t.

(8) A survey of spin dependent effects in inclusive reactions such as

$$pp \rightarrow \pi^{\pm}X$$
$$pp \rightarrow K^{\pm}X$$
$$pp \rightarrow pX$$

(9) A survey of spin effects in a number of exclusive inelastic reactions which can be studied in the EMS or the streamer chamber.

A more detailed discussion of this essential program and the time-schedule for carrying it out will be presented later. As background for this discussion, and as part of our review of the ongoing ZGS program, we now attempt to summarize the present status of the polarized beam program and the progress expected during the next two and a half years. This summary is presented below in tabular form. The notation is explained in the following notes:

Notes on Table III: "Summary of Polarized Beam Program"

(1) Polarization direction is denoted by: (↑) = normal to scattering plane, (→) = longitudinal, (*) = information for each of the three directions.

(2) The parameter C denotes both beam and target are polarized; D denotes target and (slow) recoil nucleon are polarized or equivalently beam and (fast) scattered particle; K denotes beam and (slow) recoil polarized.

(3) Indices to C, D, K are N, L, S, where N = normal to scattering plane, S = normal to particle direction but in scattering plane, and L = along particle direction.

(4) The setup used is indicated as follows:

Y = beam 22a
K = beam 1
EMS = effective mass spectrometer
SC = streamer chamber
CNS = charged-neutral spectrometer

Otherwise a numbered experiment is used: these are all in beam 5 except E403 which used beam 2.

(5) Low energy polarized beam running in August 1976 is included in "done already" column.

Table III: Summary of Polarized Beam Program

Physics Area	Finished Already	Program which would be finished by the end of CY 1978 if the Experiments of column 5 are not mounted. (This is not a satisfactory situation.)	Experiments which could be finished by the end of CY 1978 if new equipment requiring special funding is constructed.	
			Not yet Proposed	Extensions of Existing Programs
Small t $NN \rightarrow NN$ Elastic and Charge Exchange Scattering	p(↑) p(↑): Energy dependence of P and spin dependent σ_{Tot} p(↑) n elastic 2 to 6 GeV/c P p(*) p(*)elastic 6 GeV/c Amplitude determination approximately half done. (Y, K, EMS).	p(*) p(*)elastic: 6,12 GeV/c Amplitude determination approximately complete. (Y) p(↑)n elastic 12 GeV/c P. (EMS)	p(*)n elastic and CEX K_{NN},D_{NN}. (Does not need polarized neutron target.)	p(*)n(*) elastic C type measurements (Y) (Needs polarized neutron target.)
Large t elastic NN Scattering	p(↑) p(↑) elastic 6, 12 GeV/c : C_{NN}. (K)	p(↑) p(↑) elastic 12 GeV/c C_{NN},D_{NN},K_{NN}. (K) p(↑)p,n elastic at large angles. (E418)		(?)p(*)p K_{LS},K_{SS}.(K) (?)p(↑)p(↑) energy dependence of P,C_{NN}. (K)
2-body exchange dynamics $pp \rightarrow \Delta^{++}n$	p(↑) p → $\Delta^{++}n$: 3-6 GeV/c (EMS)	p(→) p → $\Delta^{++}n$: 6 GeV/c p(↑) p → $\Delta^{++}n$: 12 GeV/c (EMS)		p(↑)p(↑)→ $\Delta^{++}n$: 6 GeV/c (EMS)

Table III (Continued)

Physics Area	Finished Already.	Program which would be finished by the end of Ci 1978 if the Experiments of column 5 are not mounted. (This is not a satisfactory situation.)	Experiments which could be finished by the end of CY 1978 if new equipment requiring special funding is constructed. Not yet Proposed	Extensions of Existing Programs
Inelastic diffraction (2 body decays)		$p(\uparrow)p \to (\Lambda(*)K^+)p$: 6,12 GeV/c (EMS) $p(\uparrow)p \to (\Lambda,\Sigma(*)K)p$: 12 GeV/c (SC, Proposed)		$p(\uparrow)p \to (n\pi^+)p$ (EMS) $p(\uparrow)p \to (p\pi^0)p$ (CNS)
Inelastic Diffraction (3 body decays)	$p(\uparrow)p,n \to (p\pi^+\pi^-)p,n$ 6 GeV/c (EMS)	$p(\uparrow)\ He_4 \to (p\pi^+\pi^-)\ He_4$ (SC) $p(*)p \to (p\pi^+\pi^-)p$: 12 GeV/c (EMS) $p(\uparrow)p \to (pK^+K^-)p$: 12 GeV/c (EMS)		
Inclusive Reactions	$p(\uparrow)p \to p(\uparrow)\ N^*$ P, D_{NN} 6 GeV/c (E407) $p(\uparrow)p \to (p,\pi^\pm)\ X$ (E393,408) 6,12 GeV/c $p(\uparrow)p \to \Lambda(*)\ X$ 6 GeV/c (EMS)	Second generation $p(\uparrow)p \to (p,\bar{p},\pi^\pm,K^\pm)\ X$ (E399)	(?) $p(\uparrow)p(\uparrow) \to (p,\bar{p},\pi^\pm,K^\pm)\ X$	
Parity Violation	$\sigma_{tot}\ p(\overset{\leftarrow}{\rightarrow})p$ (E403)	(?) Improved systematics		
Others: Low energy 1 → 3 GeV/c	σ_{tot}: p(*)p(*) (Y,K) C_{LL}: p(*)p(*) → pp (Y) (in a very limited range of angles) P : $p(\uparrow)He_4 \to pHe_4$ (E414)			
Bubble Chamber	$p(\uparrow)$ p → .... (E367)			

For more details about the program summarized in Table III we refer to the ANL Proposal: "Future Operation of the Zero Gradient Synchrotron as a Dedicated Polarized Proton Facility," June 1976.

As is made clearer on pages 34 and 35, the experiments in column 3 of Table III (which do not need new equipment) cannot be finished by the end of CY 1978 if those in column 5 (which need new polarized targets) are run. The latter are particularly important and should take precedence if the CY 1978 shutdown is adhered to. A shutdown at that time will leave the essential polarized beam program outlined on page 27 incomplete. Thus, the Panel has tried to estimate a minimum time required to complete this essential program. The scheduling is complicated by the need to consider which experiments can run in parallel and which cannot, and by questions of the availability of apparatus such as a polarized neutron target. We conclude that a shutdown at the end of CY 1978 will stop the polarized beam program at a quite unreasonable time and we recommend that the ZGS continue operation for this program alone for an additional year beyond that date. The estimates of times required for different experiments, scheduling questions, and other considerations on which this conclusion is based will be presented in the following section.

5. A Plan for Optimal Utilization of the ZGS During its Remaining Lifetime Including Consideration of the ANL Proposal to Operate the ZGS as a Polarized Proton Facility after 1978.

We now consider the question of how the remaining operating time of the ZGS should be allocated among the different programs in order to gain maximum scientific benefit from its special and unique facilities. This objective places prime emphasis on the polarized beam program because of its unique status. However, we also assign high priority to the second generation neutrino experiment using the 12-foot bubble chamber, and believe this major experiment should be included in the schedule. Decreasing emphasis may be placed on the "conventional program" because, although these experiments are very interesting and have been particularly successful, they can, in principle, be carried out at other accelerators so that this physics need not be lost due to the ZGS shut-down. However, in view of existing committments to users and substantial investments in specialized apparatus, we agree that the ongoing experiments in this category should be carried to completion which will require some operating time during FY 1977.

The general points of view expressed above agree reasonably well with those of the laboratory management and the Program Advisory Committee and they are reflected in the present scheduling plans.

A. Plan for Optimal Utilization Assuming Shutdown at the End of CY 1978

We now try to estimate more specifically how the remaining operating time should be allocated in order to achieve the above goals insofar as possible. We first consider this question within a boundary condition that the ZGS should cease operating at the end of CY 1978. Under this condition, we propose the following general (and approximate) allocation for the period October 1, 1976 to Dec. 30, 1978:

(1) Three months for "conventional," or unpolarized beam program for experiments with the charged-neutral spectrometer, the 12-foot bubble chamber with TST, the streamer chamber, and the β-decay of polarized Σ^-.

(2) Five months for a major improved experiment on neutrino interactions in the 12-foot bubble chamber.

(3) Twelve months for the polarized beam program, details of which will be discussed below.

A possible general schedule showing how this proposed allocation of time might fit into the period covered is indicated in the chart below:

Illustrative Approximate Schedule

	CY 1976	CY 1977	CY 1978
Jan. 1		2 mo. Polarized 2 mo. Conventional	2 mo. Neutrinos 5 mo. Polarized
July 1	1 mo. Polarized p (Low Energy)	Install Booster Injection 2 mo. Neutrinos	
Oct. 1	1 mo. Neutrinos 1 mo. Polarized 1 mo. Conventional including TST	2 mo. Polarized	2 mo. Polarized
Dec. 31			

Some comments on the proposed allocation and scheduling of time are the following:

(1) The polarized beam program receives the major allocation of time because of the importance associated with its uniqueness. At the same time, we do not feel that the conventional and neutrino programs should be reduced below the approximate levels indicated.

(2) The proposed schedule calls for an early conclusion of the conventional program.

(3) The combination of alternate neutrino and polarized proton running provides optimal scheduling flexibility to meet equipment failures and maintain efficient operation of the accelerator.

(4) It must be anticipated that the actual ZGS program will deviate from that outlined above, in response to unforseen physics developments and the continuing detailed examination of experimental proposals by the Program Advisory Committee.

We note that the schedule outlined above calls for nine months of total operating time in both FY 1977 and FY 1978, whereas the present (FY 1976) funding level for the ZGS permits only six or seven months operation per year. Therefore our proposed schedule will require supplemental funding in FY 1977 and increased funding in FY 1978 over present FY 1976 levels. We feel that the importance of the physics results to be obtained before shutdown justifies the additional funding. (The laboratory has estimated in the recent Form 189 that the additional funding required to allow nine months operation per year is close to $1 M.)

Although the polarized beam program receives the major allocation of time in the above proposal, the time and equipment available are not sufficient to exploit the polarized beam capability in a satisfactory manner. A number of choices will have to be made concerning what things to measure if the beam is to be shut down at the end of CY 1978.

We now summarize some of the measurements to be carried out in the polarized beam program assuming it will stop at the end of CY 1978. In some cases where the availability of equipment such as polarized targets is uncertain, different options are indicated.

Polarized Beam Program: (See notes to Table III for the polarization notation.)

Breakdown by Beam Area - Proposed allocation of twelve months polarized beam running starting in November 1976.

Beam 22a, PPT III (called Y)

Option 1 (No Polarized Neutron Target)

Twelve months on a reasonably complete set of amplitude measurements for $pp \to pp$ at 6 and 12 GeV/c.

Option 2 (Polarized Neutron Target available at start of CY 1978)

Seven months on a complete set of measurements for $pp \to pp$ at 6 GeV/c and partial results at 12 GeV/c.

Five months on partial set of measurements on pn elastic.

Either of these options precludes any further polarized beam to the streamer chamber.

Beam 1, PPT V (called K)

Two months : P, C_{NN} for $pp \to pp$ out to 90° at 12 GeV/c.

Six months : D_{NN}. K_{NN} for $pp \to pp$ at 6, 12 GeV/c out to $p_T^2 = 2.5$ $(GeV/c)^2$

Four months: Fixed angle 90° energy dependence of C_{NN} and K_{NN} or D_{NN}, or measurement of K_{LS}, K_{SS}.

Beam 21S, Effective Mass Spectrometer (EMS)

Option 1 (No Polarized Proton Target available)

Reaction	Energy	Time
$p(\uparrow)p \rightarrow p\pi^+ n$	12	1 month
$p(\uparrow)p \rightarrow p\pi^+\pi^- p$	12	1 "
$p(\uparrow)p \rightarrow \Lambda(*)K^+ p$	6	1 "
$p(\uparrow)p \rightarrow \Lambda(*)K^+ p$	12	2 "
$p(\uparrow)p \rightarrow pK^+K^- p$	12	2 "
$p(\rightarrow)p \rightarrow p\pi^+ n$	6	1 "
$p(\rightarrow)p \rightarrow p\pi^+\pi^- p$	12	1 "
$p(\uparrow)p \rightarrow n\pi^+ p$	12	2 "
$p(\uparrow)p \rightarrow n\pi^+ p$	6	1 "

Option 2 (Polarized Proton Target Available)

Select ≈ 7 out of 12 months listed under Option 1. Add

$p(\uparrow)p(\uparrow) \rightarrow p\pi^+ n$	6 GeV/c	3 months
$p(\uparrow)p(\uparrow) \rightarrow p\pi^+\pi^- p$	12 GeV/c	2 months.

Currently Proposed in Beam 5 (No Polarized Target)

E399 $p(\uparrow)p \rightarrow (p,\bar{p},\pi^{\pm},K^{\pm})X$

E418 $p(\uparrow)p,n$ elastic at large angles.

Streamer Chamber

E351 $p(\uparrow)\ He_4 \rightarrow p\pi^+\pi^-(\pi^0)\ He_4$

There seems a paucity of polarized beam experiments for this device.

Possible in Charged Neutral Spectrometer - (CNS)

$p(\uparrow)p \rightarrow (p\pi^\circ)p$

$p(\uparrow)n \rightarrow (p\pi^-\pi^\circ)p$

Other Possibilities not yet proposed

$p(*)n \rightarrow np(*),\ p(*)n$: i.e., pn elastic and CEX measurements without polarized neutron target.

$p(\uparrow)p(\uparrow) \rightarrow p,\pi,K$ inclusive.

B. Proposal to Continue Operation of the ZGS as a Polarized Proton Facility after 1978

Because of the unique nature of the ZGS polarized beam and the interest in experiments which can be carried out with it, the ANL staff, following the recommendation of a special AUA study committee, has proposed that the ZGS continue operating for three years beyond CY 1978 as a polarized proton facility. This proposal is contained in a document (June 1976) entitled "Future Operation of the Zero Gradient Synchrotron as a Dedicated Polarized Proton Facility." In the proposal it is estimated that this restricted program could be carried out with operating funds of $5.5 M per year, compared to the present ZGS operation at $10.5 M. (These figures do not include the physics research and machine and facility R + D efforts at the ZGS which are expected to continue.) A full description of the facilities to be used and the physics programs to be carried out is contained in the proposal.

It is clear that a large program of polarized beam experiments which could easily occupy the ZGS for the proposed three year extension can already be foreseen. However, the Panel is not now prepared to endorse this commitment of rather large operating expenditures to this specialized (though unique) program for such a long period extending over the next five and one half years.

On the other hand, the Panel feels strongly that what is described in Section 4 as a minimum but essential program of measurements should be carried out with the polarized beam before this beam is turned off. This program cannot be completed by the end of CY 1978.

The time required to carry out this program is determined by that needed for two long series of measurements of nucleon-nucleon elastic scattering to be run in parallel in beam lines 22a (called Y above) and 1 (called K). Except for the streamer chamber, other experiments discussed in Section 4C can run concurrently in the other beam lines and have less critical needs for time. Estimates for the two schedule-determining programs are the following:

Beam line 22a (Y)

(1)	pp amplitude measurements (complete) at 6 GeV in the diffraction peak	5 - 9 months
(2)	Same at 12 GeV (less complete)	5 - 7 "
(3)	pn measurements at 6 GeV in diffraction peak with polarized neutron target. (Needs target construction.)	5 "

(4) pp elastic data at medium t at 6 or 12 GeV — 5 - 8 months

(5) pn charge exchange data (first look)

Beam line 1 (K)

(1)	pp C_{NN} at high t, 12 GeV	2 months
(2)	pp 3-spin correlations at medium t, 6 + 12 GeV	3 + 3 "
(3)	pp C_{NN} at 90°, 3 - 12 GeV	3 "
(4)	pp 3-spin correlations at 90°, 3 - 12 GeV	8 "
(5)	pp 3-spin correlations at medium t to test P, T invariance to ~ 2%	6 "

These programs require a minimum of 20 - 25 months of polarized beam operation beginning in October 1976, and this estimate does not allow extra time to follow up on new discoveries or to cope with unexpected problems. Nevertheless, the Panel believes (or hopes) that a reasonably good set of measurements can be obtained in something like 21 operating months after October 1976. Since our proposed schedule with augmented operations in CY 1977 and CY 1978 includes only 12 months of polarized beam running before the end of CY 1978, we strongly recommend that operation of the polarized beam continue for approximately one year thereafter to provide an additional 9 operating months. This implies that sufficient funds be provided to allow nine months of beam time during this final year.

6. Conclusions and Recommendations

One of the conclusions in the 1974 report of the Physics Subpanel was:

> The present physics program at the ZGS is of high quality and it is very productive of results important to high energy physics. There is no indication that this program is approaching a natural end.

This situation remains true today. The productivity of the ZGS program and the scientific importance of the research which can be carried out there would clearly justify, in absolute terms, continued operation beyond CY1978. However, given the apparently existing view that a major high energy accelerator must be closed down in order to permit construction of new facilities, we cannot argue that ERDA should close down another accelerator rather than the ZGS.

Much of the "conventional" program at ANL is of very high quality, highly competitive with that done at other laboratories. Recent examples are the experiments carried out with the "charged and neutral spectrometer." Although these experiments contribute greatly to the overall competitive stature of the ANL program, they could, in principle, be carried out elsewhere (AGS, CERN, SLAC, or Tokyo). Therefore, we concur with the laboratory planning under which this program is being phased out, with only three or four months in FY 1977 devoted to its completion.

The 12-foot bubble chamber is the most effective chamber in the world in use for the study of low energy neutrino physics. This study is, therefore, a very important part of the present program. The objectives of the present neutrino program are finite and reasonably well defined. It is believed that they can be completed in approximately five months of running prior to the end of CY 1978.

The most serious impact of the contemplated ZGS shutdown, from the point of view of loss to physics, is the resulting early curtailment of polarized beam experiments. At present the ZGS has the only polarized high energy proton beam, and studies during the past two years have shown that acceleration of such a beam in an alternating gradient machine is more difficult than previously hoped. Therefore, it now appears unlikely that the ZGS polarized beam will be duplicated anywhere else in the world; it must be regarded as a unique facility.

This situation makes the timing of the ZGS shutdown very unfortunate from the point of view of the physics dependent on polarized beams. A program of important measurements extending three years beyond 1978 can already be outlined. Thus, the ANL proposal to continue operation of the ZGS as a Dedicated Polarized Proton Facility during this three-year period has clear scientific merit. The problem is that the cost ($5.5 M per year) is high and the Panel considers it premature to recommend committing these funds for such a length of time this far in advance. We recommend a retention of flexibility, but recognize that this may be very difficult in view of the existing pressures for a ZGS shutdown.

In any case the Panel believes that sufficient time for polarized proton experiments should be provided to allow completion of an essential program of polarization studies. The most important and time-consuming part of this program consists of a resolution, at one energy, of nucleon-nucleon scattering into its spin and isospin amplitudes along with an initial survey of the energy dependence of these amplitudes. We estimate that this program will require at least twenty months of polarized beam operation for completion, and emphasize that this estimate allows no extra time either to exploit new discoveries or to cope with unexpected delays.

Since only approximately twelve months are available for polarized proton running in the period ending Dec. 31 1978, (even with the supplemental funding recommended below) it is clear that this essential program cannot be completed until about the end of CY 1979. Therefore the Panel recommends most strongly that polarized beam operation be continued for approximately one year beyond the time of shutting down the remainder of the ZGS program at the end of CY 1978, in order to provide nine months of polarized beam operation after that time.

In order to accomplish the above program on the proposed time scale, it will be necessary to operate the ZGS for nine months per year beginning with FY 1977, and continuing through CY 1979. The current budget of the ZGS supports six or seven months of accelerator running per year. It is recommended that sufficient funds be provided to permit nine months per year operation during the above period. The laboratory management has estimated that the costs involved for nine months operation of the full facility are approximately $1 M per year above the current level.

Besides sufficient running time, some of the polarized beam experiments need equipment or facilities not now available. These include a "front porch" variable energy capability in EPB I, a polarized neutron target, and possibly another polarized proton target. Supplemental equipment funds of approximately $300 K will be required in order to provide these capabilities without unduly compromising the other equipment needs of the laboratory. These funds are needed in FY 1977 to assure that these facilities will be ready for the majority of the remaining ZGS operation.

Summary of Principal Recommendations

(1) Prime emphasis during the next two and one half years should be on the unique program utilizing the ZGS polarized proton beams.

(2) The second generation neutrino experiment using the 12-foot bubble chamber should be carried out, using approximately five months of operating time.

(3) The "conventional program" should be phased out after fulfilling existing committments with approximately three months devoted to it during FY 1977.

(4) A total time of approximately twenty operating months should be devoted to the polarized beam program after October 1976. This will require:

   (a) Continued operation of the ZGS for the polarized proton program for approximately one year beyond CY 1978, and

   (b) A level of funding which permits nine months of operation per year in FY 1977, FY 1978, and extending through CY 1979.

(5) Sufficient equipment funds should be provided in FY 1977 to construct new apparatus and facilities essential to the polarized beam program.

April 1, 1971

Dr. William A. Wallenmeyer
Assistant Director for High
Energy Physics Program
Division of Research
United States Atomic Energy Commission
Washington, D. C. 20545

Wallenmeyer

Dear Bill:

Thanks for your letter of March 22 in which you answer my emergency appeal in respect to our Theoretical Group. No, it is not help to define our priorities which I wanted from you. I wanted to use the dire situation in our theory group as an example of the ~~tropic~~ tragic consequences of the recent 9% cut in the LNS funds. I consider this cut unwarranted in view of the fact that the total budget was trimmed by 4.3% and that most of the university groups were cut by this amount only.

This is why I am delighted to learn that Bernie and Al are coming for review of the program, and that there will be an external technical review later on. The outcome of these reviews can only be one of these two alternatives: Either you will be convinced that our program is at least as good per dollar as most of your other programs and therefore the discriminatory cut was not justified; or I will be convinced that LNS is not doing as well as most other groups. I believe that the first result is much more likely.

With best wishes.

Yours sincerely,

Victor F. Weisskopf
Head, Department of Physics

VFW/mlu

Demos
Hulsizer

UNITED STATES
ATOMIC ENERGY COMMISSION
WASHINGTON, D.C. 20545

March 22, 1971

Professor Victor F. Weisskopf
Head, Department of Physics
Massachusetts Institute of
Technology
Cambridge, Massachusetts 02139

Dear Viki:

Thank you for your letter of March 16, 1971, which deals with AEC funding of theoretical studies at the MIT Center of Theoretical Studies. We certainly do realize the excellence of the individuals participating in the particle theory effort and recognize that a nine percent reduction in funding from FY 1971 to FY 1972, coming on top of the series of stable or declining budgets, in recent years, is very detrimental to the research effort. You, of course, are well aware of the overall FY 1972 funding limitations for the High Energy Physics Program.

With regard to providing a breakdown of the funding between various parts of the total AEC funding for high energy physics at LNS, we would be pleased to provide such guidance if that is desired.

Bernie Hildebrand and Al Abashian are planning a trip to MIT on May 6, 1971 for a review of the high energy physics program. It would be useful to have an advanced copy of the Form 189 before that time, if possible. This visit would be a good time to discuss in detail the effects of the FY 1972 budget reduction. We are also planning, somewhat later, an external technical review of the MIT High Energy Physics Program. This external (to the AEC) review, you will recall, we try to do for each contract once every three to four years.

With best regards and best wishes.

Sincerely,

Bill

William A. Wallenmeyer
Assistant Director for High
Energy Physics Program
Division of Research

NATIONAL RESEARCH COUNCIL
NATIONAL ACADEMY OF SCIENCES NATIONAL ACADEMY OF ENGINEERING
2101 CONSTITUTION AVENUE WASHINGTON, D.C. 20418

DIVISION OF PHYSICAL SCIENCES
PHYSICS SURVEY COMMITTEE

July 14, 1970

MEMORANDUM

TO: Members of the Physics Survey Committee

FROM: George W. Wood, Staff Officer

A lecture delivered earlier this year by Sir Brian Flowers was mentioned during the discussions at Woods Hole. Allan Bromley thought members might like to see the lecture in its entirety. A copy is enclosed.

GWW:b

Enclosures

Copy of lecture "Science in Universities" - Sir Brian Flowers

SCIENCE AND SCIENTISTS: OBLIGATIONS AND OPPORTUNITIES

Philip Handler
President, National Academy of Sciences

Sigma Xi Lecture
University of Houston
Houston, Texas
October 21, 1970

Some years ago, Lady Barbara Ward said that, "The forces of change unleashed on the world in the last century can be used for good or evil. For good to create a dynamic society of free citizens working together, or for evil to set in motion the destroying juggernaut of the totalitarian state. But one thing cannot be done with the spirit of the age, and that is to ignore it, to repress it, or pretend it isn't there." When viewed from the standpoint of science -- or of scientists -- what is the spirit of our age?

In the early 19th Century, when world population was approximately one billion, with the invention of the steam engine, the industrial revolution began. Exponential rather than the earlier linear growth of the human population was already well under way. Increasingly, the environment in which he dwelt became the product of man's own creation. Only in our time has it become evident that these processes are not limitless; that population growth and exploitation of the resources of the planet can become threats to the very survival of the species.

The application of scientific understanding to human affairs was paralleled -- at least here and in Europe, albeit in considerable degree elsewhere across the globe -- by continuing improvement in the condition of man generally. From his brute condition he first developed societies where leadership was held by force of arms and terror. Very slowly, he learned to establish leadership by common consent, accompanied by a continuing equalization of privilege, i.e., diffusion of privilege to those previously without it by curtailment of special privilege to those who had previously enjoyed such. In our own country this has been evident in the Bill of Rights, in most subsequent amendments to the Constitution, the anti-trust acts, and the spate of post-World War II civil rights legislation.

Due to the application of scientific understanding, the lot of the average man improved steadily. Science-based technology surrounded every aspect of human endeavor; its benefits were joyfully welcomed, massive new industries arose here and abroad. Economic expansion -- like population growth -- burgeoned exponentially, indeed, trebling in the United States in the last two decades, while population increased by only one-third. No change in our life style has been more dramatic than the fact that, as our population grew from 100-200 million, the agricultural labor force declined from almost 14 million to less than 4 million, thereby depopulating the countryside, providing the labor force for productive and service industries while generating urban ghettos. Per capita income increased so remarkably that the poverty line in the United States has been drawn at $4,500 per year for a family of four -- a level vastly above that known to the great majority of the peoples of many other nations.

Yet not all was well. The spectre of nuclear war could not be dispelled, the disparity between the quality of life of those in the more developed nations and those in what is euphemistically called the developing nations grew ever larger. International tension grew and a war in a far distant country cast a continuing pall over every aspect of our society. Minority groups began to demand translation of well-intentioned legislation into the reality of their daily lives. Suddenly, we witnessed widespread repugnance to the diverse penalties of our insufficiently regulated technology. The family lost its position as the stabilizing unit of society, universities found themselves subject to pressures they could not satisfy, and unrest became the order of the day. Although the great bulk of Americans go about their daily tasks, much as before, the pace of change, which like economic and population growth, had long been held as yardsticks of American accomplishment, suddenly became frightening to many. Led by its youth, a nation no longer able to draw spirituality from religious faith or from continuing contact with undefiled nature, embarked upon a frenetic, sometimes destructive search for new values, for a clear sense of direction, a perception of new national purpose.

One senses a deepening national despair and, truly, the times are dangerous to national survival. Yet what we are witnessing is really the culmination of a great historic success. It is our historic national concern for the individual which underlies many of our problems, combined with our unwillingness to resort to force or repression. Hence, the vacillation of our international posture; hence, the accelerated growth of university student bodies; hence, growing marital discord and rising divorce rate as we retreat from the authoritarian relationship between man and wife; hence, the permissive attitudes of parents but uncertainty of their children; hence, the tolerance for diverse militant, even revolutionary groups; hence, the increasing share of national wealth available to most of us. It is the ever accelerating pace of change, the headlong rush into an uncertain future which is alarming. As a nation we thirst for leadership, stability and assurance that the problems of the day are understood and manageable.

Slowly, government is responding. For the first time since World War II, federal expenditures for domestic programs in the current year will exceed those for military purposes. To be sure, that is a fragile arrangement; events in Southeast Asia, the Middle East, Cuba, or breakdown of the Strategic Arms Limitation Talks could easily reverse that trend. Meanwhile, new federal agencies have been brought into being, new social action programs formulated and projections of future budgets are increasingly directed toward social benefit.

In this milieu, what of science? Support from the federal government, principal patron of science for the last two decades, has declined, in constant dollars, by 20 - 25% in the last four years and the scientific enterprise may be imperiled. Most commentators agree that this trend reflects principally growing public disenchantment with science and the technology it makes possible. It is not occasioned by the costs of the Vietnam war. Concerned that continuing economic growth may be as much a threat to the ultimate national well-being as it is a virtue, some ask a moratorium while we develop a national science and technology policy for the future. Loud voices among the biological community decry the rate of environmental degradation; placing a brake on that process is probably the most popular single cause in the nation. Equating science with technology, a large fraction of our youth find both immoral, holding them guilty of both weapons production and deterioration of the quality of life. This, in turn, is reflected in declining relative undergraduate enrollments in the hard sciences, giving grave concern for the future of the scientific endeavor. The crumbling of the scientific enterprise, the slow-down of the economy which no longer readily absorbs the products of our graduate schools and the disappearing blind faith in the utility of science by a public which never did appreciate the beauty of the intellectual structure of science -- all combine to generate a rising sense of apprehension in the scientific community.

Yet in truth, our scientific capabilities were never greater; our scientific productivity remains the marvel of the world, although leadership in some areas of science is slowly moving abroad. In view of the international character of science, that cannot be seriously objectionable, of itself. If employment has become difficult it is because of the pause in the economy and reduction in federal applied R&D expenditures. If there is cause for anguish, it is for lack of funds for new scientific starts, reduced funding of educational programs, and the mission-oriented narrowing of vision of federal agencies. Meanwhile, relatively few academic laboratories known to me have yet been really seriously injured. Only a few major national facilities have been closed and I presume these to have been only marginally productive.

Under these circumstances, then, in this moment of historical transition as we struggle to learn how to make major decisions in the public sector rather than simply permit them to occur in a free market economy as in the past, as we grope to establish acceptable goals and to develop an appropriate posture vis-a-vis the other nations of the world, what should be the attitudes of scientists?

The most important action a scientist can take is to transfer to his public posture the honesty and integrity which, presumably, necessarily characterize his work in the laboratory. The burden upon the scientist is not to engage in hortatory declamation but to document his statements while being certain that he does not live in a vulnerable glass house. If the scientific community will not constitute the voice of reason in national affairs, whence shall we seek it? Let us consider some specific problems, several of which may yet try your conscience.

Continuing Education of the Public. Overridingly, it is incumbent on each of us engaged in scientific research or science education to generate opportunities to make known to our fellow citizens our sense of the value of science in the world of tomorrow. If you believe -- as I do -- that science remains the most powerful tool the mind of man has yet conceived to alleviate the condition of his fellows -- please say so. If you believe that the pursuit of science is not merely the expensive hobby of scientists but both the leading edge of our culture and the only rational basis for a better way of life tomorrow -- please say so. If you sympathize with our youth as they grope for new insights, for new relations among men, but you also understand that it is the tremendous productivity of the very science-based technology our youth decry that now provides their opportunity to seek new directions and, hence, must not be rejected out of hand -- please say so.

Scientists believe that free, untrammeled investigation is the surest path to those discoveries which will illuminate the nature of the universe, or of man himself, of those insights which will be translatable into practical public benefit. It remains incumbent upon us to demonstrate the truth of that statement, but not by recourse to now ancient examples, e.g., that electricity, hertzian waves, and nuclear power were not and could not have been discovered as a consequence of research directed toward new means of communication, or the generation of power. We need illustrations from the current scene. The origins of the laser, of the transistor, of microminiature circuitry, of the basis for video tape and instant playback, are all well described in non-technical language, but have not achieved public recognition. A recent illustration I enjoy is the perhaps surprising demonstration that even astronomy can be practical. As smog became more frequent, optical and radio astronomers were forced to learn to identify and quantify the signatures of all common air contaminants. Indeed, such data provide a 15 year history of the composition of the atmosphere in some regions. Accordingly, all the research has already been done which is required for construction of relatively small, radar-like machines which could conduct a continual monitoring of the atmosphere in any locality. It is most unlikely that any environmental engineer would have invented this possibility; research in astronomy, as in high energy physics or space, does have societal spin-off benefit.

In the biological realm, I direct your attention to the long history which necessarily antedated development of "The Pill", to the history of cytosine arabinoside, currently the most successful anti-leukemic agent under investigation. This compound was found in sponges, tested for effects on nucleic acid metabolism in tissue culture and synthesized by a procedure which rests on observations made by an organic chemist seeking to reconstruct the chemical events which preceded the origin of life. Consider the history of the prostaglandins -- for fifteen years merely a curious set of lipid-soluble compounds originally observed in mammalian prostate glands and now the most likely candidate for the next generation of widely used chemical contraceptives. Or choose your own examples, but as you "sell science", understand that support of unrestricted research is provided grudgingly by our society and largely in the hope that the results can usefully be applied to human affairs. The point must be that free research continues to offer surprise and totally unexpected, unpredictable bounties -- whereas closely goal-oriented,directed research can offer little promise of success until the time is right and the scientific stage has been set. If you agree -- please find opportunity to say so.

I agree that science is beautiful, I believe it to be the noblest expression of our culture. But if it is to seek support on that ground alone, then it must compete with our bankrupt orchestras, museums and community theaters for public support. But science has a special worthy place because it is not only a great esthetic experience, it is powerfully practical and relevant to the public purpose.

The Morality of Science. I am among those who decried the spectacular departure from science of the team at Harvard which successfully isolated a gene. Certainly if the very doing of science is potentially harmful to our society, the public cannot be expected to pay the bill. But I deeply believe that they were wrong. With Jefferson, I believe that, "There is not a truth on earth that I fear to be known." But not all, particularly not all of our youth, agree. Where do you stand? There is the other side of the coin. It was, perhaps, too easy to place all the onus for the existence of vastly destructive weapons on those in position to pull the trigger, while suggesting that those who had contributed the requisite scientific understanding were merely pursuing the path of science, a totally amoral activity. Can we continue to hold such a posture? But what choice have we? I find it hard to believe that mankind would be better off if no one knew that $e = mc^2$. Clearly not -- and so, man is doomed to satisfy his groping for understanding and then live with the resultant knowledge -- which can never be exorcised away.

Closer at hand, for some of us perhaps, is the growth of euphenic medicine -- techniques which permit the survival and reproduction of the genetically defective, thereby increasing the incidence of such genes in our population. Witness the increasing incidence of diabetics. If ever we can achieve useful transduction of good genes into the somatic cells of such individuals this problem will only be exacerbated. Such practices are surely true to the classical traditions of medicine, but are they in the public interest? If not, what <u>should</u> we do?

There are perhaps a dozen centers in the United States now engaged in the examination of the young fetus for hereditary defects, usually by cytological examination of the chromosomal karyotype. This can soon be extended both to the search for numerous genetic biochemical defects and to every large hospital in the country. Have we had adequate national debate concerning such procedures? Patently, no such analysis should be undertaken unless there has been prior determination to abort defective fetuses when these are detected. This practice, which I commend, now expands in the hands of scientific practitioners of medicine, whereas ultimate decision is a matter of the public morality. The decisions to be made are not scientific, yet they can only be made in full awareness of the potential of science in this regard. Such education is your responsibility. Do you work at it?

Some Public Issues. If you believe, with me, that degradation of the environment is already serious and all too genuine, but that it has frequently been exaggerated by our own colleagues, that with few exceptions such deterioration is reversible but that, to do so will require yet more, rather than less, technology, please do say so. Environmental problems are usually presented as a series of unchallenged horror stories rather than as a collection of hard verifiable data. Our world has not yet arrived at the state described by an ancient poet as:

> "Even now, in many places over the earth
> Walls stand wind beaten
> Heavy with hoar frost, ruined habitations...
> The maker of men has so marred this dwelling
> That human laughter is not heard
> And idle stand these old giant works."

Perhaps that process is in train, but the day is not yet. We are frequently told that Lake Erie is dead -- when the total fish catch is rising, not falling. Lake Washington was on its way to becoming as dead as Erie, so it was said, and its reversal is viewed as a great triumph. And it surely was a triumph of political organization. But the polluted lake was great for salmon fishing, and now the water is so pure it has difficulty supporting the salmon. Air pollution is always unpleasant and a lengthy inversion can undoubtedly be dangerous -- but automotive emissions are controllable and I cannot help but wonder whether this problem is trivial compared to the carnage we suffer on the highways to which we seem to have become inured with intoxicated youth the most frequent offenders. Bumper stickers decry DDT across the land demanding its total ban rather than regulated usage -- but the next generation of pesticides has killed or blinded dozens of people this year -- and no American is known to have been injured by DDT. What puzzles me is the apathy of college students toward our vast arsenal of nuclear weapons -- the infrequency of demands for a massive systematic approach to the attainment of a stable, enduring, just peace. Or why our students do not become angry about the desperate state of our rural poor?

Please understand. I have no wish to minimize the seriousness of our environmental problems. They are huge and in need of large-scale, urgent attention. But, is it not time to stop frightening the American people and instead demand the very large systematic program necessary to acquire the data which would permit quantitative evaluation of the risks versus the benefits in all those areas where man's intervention has already or yet may degrade the environment? We need to know far more about pesticide usage, food additives, drugs, industrial practices, radiation hazards, atmospheric phenomena and the alleged fragility of ecosystems than we do today if we are to make sound judgments and establish public policy.

Everyone is aware that population growth, resource utilization and environmental degradation are aspects of a single problem. But I am not sure that it is understood. The problems differ sharply in the developed and developing nations. Although population growth is faster in the latter, the ultimate penalty for such growth is far greater in the former. As compared to a native of the Amazon jungle, an Indian village, or a Nigerian town, we make enormously greater impact on the environment and our resources. Each American is entitled, by birth as it were, to a school desk, a dormitory bed, a hospital bed, perhaps 400 sq.ft.

of personal dwelling space, 16 ft. of steel on the highway and all the gasoline he can burn, as well as perhaps 40 grams of animal protein per day. To add to such a consuming population is horrendous to contemplate. The difficulty in merely sustaining the life of a Brazilian native may loom even larger -- but it has little impact on the reserves of coal, oil, iron ore, copper ore, or phosphate rock and contributes little to the despoliation of the environment. Current projections suggest that by the year 2000, as compared with 1950, oil consumption in the U.S. will rise by 500%, automobile production by 700%, residential construction 1000%, chemicals and chemical products 1200%, air passenger miles 2600%, highway construction 2000%, electric power consumption by 1800%, and disposable per capita income, in constant dollars, by 250%! If unchecked, we could undoubtedly create the industrial plant and technical capability to realize those projections. But there are grave doubts that our resources or environment could tolerate the result; "America the Beautiful" may be desecrated beyond the limits of tolerance of her custodians.

Hence, the growing sense of urgency that, in advanced nations, total populations be stabilized or even reduced, that economic growth be slowed while we develop a coherent quantitative model of our national life, establish what order of resource utilization and recycling, of land use, etc. is compatible with a harmonious steady state with our resourses and environment, while we develop a commensurate national population policy, and implement social action programs that can, in time, assure that all Americans enjoy the full advantages of citizenship. We have made small starts in this direction, but only that. Meanwhile, well intentioned legislation concerned only with automotive emissions or nonreusable beer containers, important as that may be, is much like prescribing aspirin for a brain tumor.

We may, one day soon, be forced to resolve to reduce our standard of living in a variety of ways, if only for lack of electric power. But I am unwilling voluntarily to return to a more primitive era until the necessity for so doing has been clearly demonstrated -- not merely hysterically demanded.

Meanwhile, I prefer to work at social and political mechanisms which will spread the benefits of our civilization to all fellow citizens and to the peoples of the underdeveloped third world. For the latter, a brighter future depends upon accelerated -- not decelerated -- use of their exportable raw materials and expanded agricultural productivity by extension of the "green revolution", accompanied by a decreased rate of population growth. In developing nations, it is not the absolute size of populations but their growth rates which are the threat to their success. Malthusian cataclysm seems decreasingly likely, but rapid population growth precludes accumulation of capital resources and denies to the ever younger populations of such lands the opportunities we know. To prevent this, one requires massive educational campaigns and a cheap, reliable, reversible contraceptive procedure. In short, the future of the peoples of the underdeveloped nations requires more science, more technology -- not less. If such programs succeed, then one day the problems of such nations will resemble ours -- hence measures to reduce population growth rates now and thus also minimizing populations later are in both the short-term and long-term interests of those nations and of humanity the world over. If you agree -- please say so.

Withal, it must be clear that environmental quality, in the physical and biological rather than social sense, is a white upper-middle-class problem, soluble in the rather early future if we are but willing to pay the costs. Such matters offer little trouble to laborers who equate smoking industrial chimneys with jobs, or to Blacks and Chicanos who have much more compelling concerns. But compare our outrage with environmental deterioration with the fact that, at the White House Conference on Nutrition last winter, speaker after speaker alleged that the health of 30 million Americans is adversely affected by malnutrition. Again, I suspect they exaggerate and we need hard data. But even if they exaggerate by two orders of magnitude, such circumstances are intolerable in this affluent society which already produces all the food necessary to feed all of us well. This is even more painful now that we know that nutritional deprivation in early life results in a deficit in neurons in the central nervous system, a deficit which can never be overcome or compensated later. In my view, this problem is more immediate, crucial and severe than is any aspect of the environment. Yet one hears little of it, on campus or off. Should you not be demanding that society rectify this unnecessary tragedy?

In general, the public is continually informed with respect to the nature and rewards of medical research. Yet here, too, misunderstanding and incomprehension is the norm. Publicity is given to what are termed "breakthroughs" when these are but the visible tips of the research iceberg. The public, Congressmen, and too many medical practitioners demand that medical schools devote themselves to the production of "ordinary practicing doctors" in great numbers, at the expense of research activity, somehow forgetting that virtually every useful procedure available to modern medicine is the product of the research effort of the last few decades. Further, they demand that this diminished research effort be more directly addressed to the problems of disease rather than exploring the nature of life, and that already limited federal resources be diverted from support of research to the delivery of health services. They could not be more wrong.

As Ivan Bennett noted, what is really meant by "medical care" is the mobilization of resources of manpower and facilities to bring to bear inadequate "half-way technologies." When research provides a basis for truly definitive prevention or therapy, invariably the resultant control of a disease is enormously simpler and cheaper than the palliative half-way technologies which were utilized before. Moreover, each time this sort of advance has occurred, it has been the consequence of fundamental insight into underlying disease mechanisms provided by basic research. Consider if you will a partial list of diseases, each of which was, at one time, a major drain upon the then extant health care system but is now of little consequence in this sense: infectious diseases such as tuberculosis, typhoid fever, infantile diarrhea, epidemic meningitis, typhus, trachoma, scarlet fever, poliomyelitis, cholera, yellow fever, bacterial endocarditis, syphilis, gonorrhea, lobar penumonia, measles, rubella, whooping cough, diphtheria, smallpox, tetanus, or peurperal sepsis - nutritional diseases such as pellagra, rickets, scurvy, iron deficiency anemia, and pernicious anemia, or Addison's disease, hyperthyroidism, goiter, juvenile diabetes, glaucoma, erythroblastosis fetalis, and Parkinsonism. In every case, today, their control or prevention is relatively simple and cheap. It is not these diseases, now under control,that pose the great problems of logistics, manpower, and costs for the current health care system.

In contrast, stand those only partially understood diseases which can be somewhat mitigated only by major efforts -- but for which we lack definitive cures or preventive measures. It is these which now demand the most complex technologies research has yet made available to the modern hospital, which, nevertheless, constitute only palliative or physiologically corrective measures. These disorders engender large human and financial cost and frustrate the health care system not because of shortage of professional manpower or of hospital facilities, but primarily because there is so little truly effective medical technology available even in the very best of circumstances. This is true for most forms of cancer, stroke, coronary thrombosis, myocardial infarction, hepatic cirrhosis, glomerulonephritis, pyelonephritis, rheumatoid arthritis, osteoarthritis, acute rheumatic fever, disseminated lupus, bronchial asthma, multiple sclerosis, the senile psychoses, schizophrenia, mental retardation, emphysema, most genetic disorders of metabolism, muscular dystrophy, cystic fibrosis, and virtually all the virus disorders which are not preventable by early immunization.

There are promising avenues of research with respect to practically each of these disorders. None are regarded as hopeless problems by those engaged in their study; an atmosphere of confidence is shared by the research community in almost each instance, in large measure the consequence of the rapidly developing understanding of normal structure, physiology and metabolism in molecular terms, permitting rational, penetrating questions concerning etiology and pathogenesis. Elimination or effective therapy of the major lethal and incapacitating diseases which now afflict mankind is not a hopeless dream but a rational projection into the future based on the capabilities of the present.

Whereas, if this nation foreswears research progress, it must plan for at least 50% more hospitals, more doctors, more nurses, more sanitaria, and more suffering by the turn of the century -- scarcely a brave sight. Have you found a platform to say so?

Education. Articulate students demand that undergraduate education be relevant and state that science is not so. Without knowing it, they echo a statement by Karl Marx to the effect that, "Philosophers seek to study and understand the world, the point is to change it." Is it not our responsibility to make it abundantly clear that, while change is necessary and desirable, if it is to be beneficial, then most assuredly one must first understand that world and that such understanding is in no small measure to be achieved in the language of science? And yet numbers of our colleagues have abandoned their own values, their former belief in the contributions of science and technology and, in a burst of emotion, have joined their dissident junior fellows. Have we naught to say to them?

Out of the same concerns, fused to complaint from industry that the products of our graduate schools are overspecialized, there is a growing demand for a revamping of both undergraduate and graduate education. This takes several forms. Some request an abridged, multidisciplinary experience in science without the extensive experience in independent research normal to the Ph.D., but adequate for addressing one or another of the ills of our time. I find myself sympathetic. Probably wisely, inertia in changing curricula is characteristic of the academic world. If you too believe that such programs are desirable as a means to provide trained people who really never are going to do significant independent research but who could be extremely useful, the burden is upon you to take such initiative. A second form is the demand that universities extensively restructure themselves, replacing the classical disciplinary structure by creating multidisciplinary units engaged in research and education, at all levels, oriented about current societal problems. I find myself quite out of sympathy with this suggestion.

I was Chairman of the National Science Board and Leland Haworth was Director of the National Science Foundation when we invented and decided to fund a program entitled "Interdisciplinary Research on Problems of Our Society." This has found enthusiastic support and ready funding. But I view it as an experiment and believe that it should be employed only in special circumstances.

Universities can boast few significant multidisciplinary accomplishments. Multidisciplinary teams have been strikingly successful in government and industry, but not on campus. I do not mean to deter the natural alliances which spring up among groups of faculty with temporary common interests. This has occurred in the past and should in the future. But wholesale reorganization of the university, as some propose, so as to focus upon multidisciplinary attack on problems of the environment, population control, drug abuse, urban redevelopment or ethnic problems, etc., important as these are, seems inimical to the central life of the university, the only guardian of scholarly disciplines and should be considered only with great caution. The frontiers of the disciplines are the frontiers of our civilization and, disciplinary specialization has accelerated their progress. Patently, there is too much which is archaic, irrelevant or, worse still, uninteresting in conventional college curricula. But, while undertaking their reformation, it should be clear that the university must continue to be dedicated primarily to the "life of the mind", through transmittal of classical values, through preservation, presentation and expansion of the essential core of the natural and social sciences and the humanities. If you agree, your voices are seldom heard.

Multidisciplinary research can be encouraged on campus -- but in separate structures, separate administrative units deliberately created to such ends, each with a core multidisciplinary staff, each concerned with one major aspect of society such as crime and justice, urban life, domestic housing, educational reform, violence, the drug culture, transportation, the search for a lasting, just peace, or some aspect of the environment. Every university should consider creation of one or two such. If members of the disciplinary faculties, undergraduates, graduate students and postdoctoral fellows were free to flow through such units on a temporary basis, they could contribute to the success of such organizations while retaining their own disciplinary identifications and pursuits which will have been enriched by such experience. If you agree, please make yourself known.

Largely due to the fall-off in federal funding, the prospect of unemployment of trained scientists gives one pause for the first time since World War II. For my part, I deeply believe that this is a temporary transient -- that if society takes that course best calculated to provide a richer life for all, tomorrow -- we will soon find trained scientists once again in short supply. Meanwhile, however, some in government who make policy in these regards already take deliberate steps to reduce graduate enrollments. They wish to reduce the numbers of available fellowships and traineeships, and perhaps abolish them totally, suggesting that these might be replaced by loan programs. But higher education has been among the principal means of upward social mobility in our society, and loan programs simply cannot serve in equivalent fashion. To be sure, it may legitimately be asked why the government singles out graduate students in science for support, largely ignoring other groups. And I fully agree. But instead of abandoning such programs entirely, I suggest that the time has come for American society to underwrite the education of all students, graduate and professional, who undertake education beyond the baccalaureate. The costs would be quite substantial. But, so would the benefit to the nation.

As a means to this end, I have proposed a National Youth Service Program, which would offer stipend and tuition support to all students in good standing engaged in advanced education beyond the baccalaureate, regardless of field, be it the natural or social sciences, medicine, law, engineering, etc. In exchange, upon completion of such education, all would then be committed to two or three years of national service, but under regional, state or local control. Although away from the university for a year and a half now, I remain convinced that no program would find a warmer welcome among the highly motivated young people of our time.

Social scientists and humanists might be apprenticed to federal agencies or undertake teaching assignments in junior colleges, or high schools, particularly in disadvantaged areas. Young lawyers could serve in legal aid clinics or in local government; nothing could so upgrade local and state government as an annual wave of bright young lawyers and social scientists. New physicians could serve in a modernized public health service, assigned to clinics across the country or to experimental health teams assessing new mechanisms for delivery of health care. Natural scientists and engineers could teach or serve in federal laboratories or in the multidisciplinary institutes on campus of which I spoke earlier. The impact of this flow of motivated, highly trained young men and women throughout the diverse elements of our national life would be profound and exhilarating, a "Domestic Peace Corps," if you will, but of individuals thoroughly trained for their jobs.

This is not as drastic a proposal as it may seem. Long ago the government accepted full responsibility for the education of those embarking upon uniformed service -- police and fire-fighting academies, the academies of all of the armed forces and of the Coast Guard. Millions of young male bodies have been processed through the military, and then later educated under the GI Bill. But why not educate them first, and have society reap the full benefit, whether in military or civilian service? Almost all male physicians are now obligated to military service under the doctor draft. Why not extend this benefit to the civilian sector? And, if we are to continue to have an overseas Peace Corps, why not send fully trained, competent young men and women. Why utilize the well-intentioned but uneducated?

I know that this has been incompletely thought through, that it would be a major change in our national life, as I appreciate the unlikelihood of the necessary legislation in the near future. But if we open such discussions today, we can shorten the time until this becomes "an idea whose time has come", the next extension of the historic process which began with publicly funded universal primary school education. If you agree, please help me say so.

Research Funding. For some years, I have been concerned with mechanisms by which federal funds appropriated in the name of research have been utilized in the support of academic science. Such funds have not only enriched our graduate and professional schools and created the world's greatest scientific capability, but have also been utilized to ensure the very operation of the university itself; they have been utilized to pay professorial salaries and graduate student stipends, build buildings and, through so-called "indirect cost payments" contribute to the salaries of university presidents, deans, purchasing agents, and janitors. Little more than a fourth of all such funds are utilized for the classic purposes of a grant-in-aid -- the consumable supplies and equipment, immediately related travel and publication costs, as well as the salaries of those engaged full-time for the conduct of the research project itself. The other three-fourths of the funds assure that the university will be there so that the first fourth can be effectively utilized. But as we have witnessed in these last few months, as such funds decline, as research grant or contract awards are significantly reduced from the amount requested, this arrangement engenders serious difficulty. If, in sum, one were to reduce expenditures for the research proper, one could not justify the utilization of the other three-fourths of the money. And if the latter were reduced, the institution would not be in position to conduct the research. Although this arrangement was generated with the full knowledge of all concerned, it now returns to haunt us. It becomes impossible to manage research expenditures without seriously affecting the fiscal solvency of the university itself.

It was with such unhappy prospects in mind that, some years ago, I began to campaign for large-scale, institutional funding, not of the immediate costs of research, but to return to institutional control those funds which need not be utilized at the discretion of the faculty member. The academic community, then enjoying great entrepreneurial success, almost unanimously objected. Fearing that not only the basic supporting funds but also the research funds might find their way into the hands of deans by this process, they resisted change. And the penalty is evident on many a campus today.

Early in 1969, the National Science Board delivered through the President to the Congress a report entitled "Toward a National Policy for Graduate Education in the Sciences." The essence of this report was that federal funds should be provided for the explicit purposes for which they are required: the universities require block funds to sustain them, so that all payments for what we now term indirect costs and for professorial salaries would go directly to the central university administration. We proposed a program of training grants, patterned after those of the National Institutes of Health, to sustain departmental activities while research grants were still to be awarded to individual investigators after appropriate peer review and judgment but limited to those funds which the investigator quite properly must control himself. Although 5,000 copies were distributed, the academic community was silent. Investigators still chose to live in their dream worlds, university presidents remained concerned with the imbalance between the natural sciences and the humanities on their campuses -- but were overtaken by concern with the brutal problem of simple survival. Congress and the White House listened, but heard few voices and the opportunity passed. But the problem worsens. Only a set of solutions which will make honest men of us can be expected to return stability to the academic research enterprise so that it can continue to contribute to the national welfare. The audience which was fashioned last year may no longer be quite so receptive, but it could be generated again if you would do so. If you do not, research funding will continue to be affected by the capricious winds of political change. The federal government will not accept responsibility for the continuing welfare of institutions of higher education unless sufficient pressure is brought to bear. If you are silent, the pressure on agencies to demand immediately applicable research results will be strengthened, but the nation impoverished thereby. And, as has happened in history before, those who knew better but were silent will have been most at fault.

On the Washington scene, the Mansfield Amendment to the Defense Procurement Authorization Act has had its teeth drawn, and the Defense Department is again free to determine what areas of science are relevant to its mission. But the episode will not die. The administrators of all mission agencies henceforth will enjoy far less latitude in the support of frontier research. And again, your voices should be heard. If you believe it necessary for mission agencies, including Defense, to have continuing contact with the best of science -- please say so. If you think such agencies should support fundamental as well as applied research but that the Science Foundation should become the major agency for support of most frontier research, please so indicate. If you feel that a closer seemingly mission-oriented approach to research funding at NIH could be disastrous, tell them so. You have not been heard.

May I end with a few words concerning the operation of the project grant system as we have known it. Many of you have been both on the receiving and the judging end of this process. Lay fellow citizens now pose some hard questions which demand reply: How much research has been only pseudo-research? Has an unacceptably large fraction of the research endeavor been ineffectual and mediocre? Are some supported investigators in fact unimaginative and even incompetent?

The public, particularly the Congress, has long pressed the scientific community to provide some expression of priorities. While the total scientific budget grew exponentially, we could avoid meeting this question. But as budgets harden -- and they will remain restrictive for some years to come -- can we continue to avoid that question, and if not, what answer would you give? Is one necessarily a troglodyte if he asks that the wasteful, the incompetent, the nonessential aspects of research can be eliminated during a period of restricted financing such as this? Or, is it reactionary to inquire whether America really can aspire to 200 first-line universities with truly significant research enterprises -- the path down which we began a few years ago?

Questions such as these torture the academic community. But they cannot long be avoided nor need we fear them. Honest responses will clear the air, and, I am confident, reveal only that our research endeavor should be a source of great national pride. If there was "fat" in the system, these past four years have wrung most of it out. If we can give honest, wholehearted answers to the diverse, broader questions I have raised, we can help in our way to restore public confidence in a vital aspect of our society.

Science <ins>is</ins> capable of fulfillment of the American dream. Biological and physical research <ins>can</ins> permit us to refashion ourselves and our world. If the dream fails, it will be because of the limitations of man the social creature. There is really no question whether man can live with his technology; the question is whether man can learn to live with himself. Just as ecology is too immature to cope with our vast environmental problems, the social sciences are too young to cope with our most pressing national and international problems: terminating the war in Southeast Asia, establishing a stable permanent peace, learning to deal with political terrorism and the challenge to the legitimacy of government, achieving a successful progressive modus vivendi in our racial problems, coping with violence and crime, reconstruction and management of large cities, curbing the drug culture, developing an adequate system for the delivery of health care, abolishing poverty, illiteracy and ignorance the world over. Nor have the natural sciences a great deal to offer in these regards, I regret to say.

It is not obvious that we have the understanding, or the social and political institutions to deal with these furious challenges -- but seek them we must. In the midst of our despair, the long upward struggle of man from his brute animal origins affords cause for hope. Meanwhile, never was there more need to heed Whitehead's dictum that "the art of progress is to preserve order amid change, and to preserve change amid order."

Thank you.

PRIVILEGED INFORMATION 6/16/77

PRELIMINARY AGENDA

HIGH ENERGY PHYSICS ADVISORY PANEL

20 Massachusetts Avenue
Washington, D. C.

Fourth Floor - Room 4222C
June 27-28, 1977

Monday, June 27, 1977

9:00 AM	Administrative - Discussion of Agenda - Approval of Minutes	OPEN
10:00 AM	Manpower Subpanel (J. Sullivan)	OPEN
10:45 AM	FY 1978 Budget - NSF (M. Bardon) - ERDA (W. Wallenmeyer) - New Budget Procedures	OPEN
11:30 AM	NSF Physics Panel (K. Strauch)	OPEN
12:15 PM	International Affairs - JCC-FPM (J. Kane) - Foreign Conferences	OPEN
1:15 PM	Lunch	
2:00 PM	New Facilities Subpanel* (J. Sandweiss)	OPEN
5:30 PM	Adjourn	

*New Facilities discussions may continue into the evening.

Tuesday, June 28, 1977

9:00 AM	Politics of Energy (H. Cantus)	OPEN
10:00 AM	Computer Subpanel (J. Ballam)	OPEN
10:45 AM	Equipment for Major Experiments (J. Peoples/J. Ballam)	OPEN
11:30 AM	EXECUTIVE SESSION - ZGS Status - Long Range Planning and Projections - New Facilities Subpanel	CLOSED

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Tuesday, June 28, 1977, cont'd

11:30 AM	EXECUTIVE SESSION - ZGS Status - Long Range Planning and Projections - New Facilities Subpanel	CLOSED
1:15 PM	Lunch	
4:30 PM	Adjourn	

PRIVILEGED INFORMATION

UNITED STATES
ENERGY RESEARCH AND DEVELOPMENT ADMINISTRATION
WASHINGTON, D.C. 20545

June 17, 1977

HIGH ENERGY PHYSICS ADVISORY PANEL

J. Ballam
D. A. Bromley
D. O. Caldwell
R. E. Diebold
S. D. Drell
V. Fitch
H. J. Frisch
T. D. Lee
J. E. Leiss
B. McDaniel
J. H. Peoples
N. P. Samios
J. Sullivan
G. H. Trilling
V. F. Weisskopf

HEPAP MEETING--JUNE 27-28, 1977--ERDA HEADQUARTERS, 20 MASSACHUSETTS AVE., NW; WASHINGTON, D. C., ROOM 4222C

Enclosed for your information is the agenda for the HEPAP meeting to be held June 27-28. Also enclosed is a copy of the final minutes for the November 1976 meeting held at SLAC.

The following accommodation reservations have been made for you at the Quality Inn Capitol Hill, 415 New Jersey Avenue, N.W.: . Late arrival is guaranteed for those arriving Sunday evening. Liz Burdette will confirm reservations for Monday arrival as appropriate. If you have any changes concerning your reservations, please call Liz (301-353-3367).

E. Coleman

Ernest Coleman, Ph. D.
Executive Secretary
High Energy Physics
Advisory Panel

P. S. The draft minutes of the February HEPAP Meeting are also enclosed.

ORGANISATION EUROPÉENNE POUR LA RECHERCHE NUCLÉAIRE
CERN EUROPEAN ORGANIZATION FOR NUCLEAR RESEARCH

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1211 GENÈVE 23

Genève, 17 July 1974

Professor Norman Ramsey
Physics Department
University of Oxford
12 Parks Road
Oxford OX1 3PQ

Dear Norman,

I thank you very much for your letter regarding our subpanel report. I understand very well your criticisms and I am aware that some of the formulations could have been more precise.

Let me make a few remarks to our recommendation II. First, we have received a very large number of letters from members of the community in which there was a strong emphasis upon the necessity of three centers for high energy physics in America. Thus there seems to exist a strong desire to continue the development of Brookhaven.

Second, there is no doubt that the work in Brookhaven on superconductive magnets has been very successful. The continued success of this work will depend more and more upon the probability of having it applied to a real project.

Third, after careful studies, the technical group in our subpanel found that the necessity of accelerating the protons from 30 to 200 GeV does not represent a major difficulty.

Fourth, it was generally felt that NAL will have a lot of work to do in the next five years in improving the laboratory and in working towards the energy doubler.

All these reasons have finally convinced the subpanel to formulate recommendation II in the way it was done. I should add that the recommendation does not exclude the construction of 200 GeV proton-proton storage rings at NAL if the time scale of development will be much longer than anticipated. This may happen, either because the Government will not come forward with the necessary appropriations, or because of a lack of leadership in the Brookhaven administration.

I agree with your second point in which you mention that the 200 GeV colliding beam facility should be constructed in such a fashion that it be effective in testing designs for the next steps in energy. Surely, we could have mentioned that in our recommendation but it seems to me that it goes without saying within the community of accelerator builders.

I believe that we agree as to the interpretation of recommendation III. It is certain that there will be a need for constructing a multi-TeV facility within a period of 10 to 15 years. I cannot see how one could interpret our formulation in any different way than the one in which you and I interpret it. I am sure that one could have formulated it better, but I see no danger of misinterpretation.

Thank you again for your criticisms. Our work was difficult but it was a pleasure to work intensively with such an excellent group of highly intelligent people, and there was not much disagreement among us about the main points of the recommendations.

With best regards.

Sincerely yours,

Viki

V.F. Weisskopf

PS. : I am at CERN until the end of September.

TO: HIGH ENERGY PHYSICS ADVISORY PANEL

J. Ballam	J. E. Leiss
D. A. Bromley	B. McDaniel
D. O. Caldwell	J. H. Peoples
R. E. Diebold	N. P. Samios
V. Fitch	J. Sullivan
H. J. Frisch	G. H. Trilling
T. D. Lee	V. F. Weisskopf

FROM: S. Drell

SUBJECT: HEPAP letter on Energy D/S Support

I was very sorry not to have been able to attend the HEPAP meeting on June 27-28, but a very nasty flu bug did me, as well as my whole family, in. I was glad to be able to have the conference call and especially I want to thank you for your careful attention to the Sandweiss report and the thoughtful draft of a transmittal letter which I have communicated with only minor changes as you can see on the enclosure. Substantively, the only change I made was in the last paragraph in which I broke the causal connection contained in the draft between the great strides in high energy physics and the improvement of funding support, and I mentioned that PEP has started since my last letter.

Enclosed is a draft for your comment of the second letter to Jim Kane about funding support for the completion of the Doubler Saver. I will wait to hear from you on this one before transmitting it.

Best wishes,

Sid

SDD:rp
encl.

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

Mail Address
SLAC, P. O. Box 4349
Stanford, California 94305

DRAFT

July 11, 1977

Dr. James S. Kane
Deputy Assistant Administrator
for Physical Research
Energy Research and Development Administration
Washington, D. C. 20545

Dear Jim:

I am writing to inform you of the concern of HEPAP for the appropriate support of the research and development of the Energy Doubler/Saver at Fermilab.

The impressive progress in the development of the superconducting technology needed for the Energy Doubler/Saver may make possible the realization of fixed target 1000 GeV physics as early as 1980. This development is essential in order to maintain the unique position of Fermilab in view of the initiation of research operations at the CERN-SPS. The possibility of achieving colliding p-p and $\bar{p}$-p collisions of very high energy at an early date is a further exciting and important component of the Tevatron program. The Panel is also sensitive to the increasing power costs and the important savings to be achieved by operation in the saver mode.

In its report the 1975 (Low) New Facilities Panel specifically recommended that the Energy Doubler/Saver program be supported as an accelerator research and development effort. The 1977 (Sandweiss) New Facilities Panel has recognized the progress made at Fermilab in the intervening time and has recommended that funds be provided for the conversion of the ED/S to a Tevatron facility at the highest energy for fixed target physics. The R&D effort must be nearly complete before this conversion can be made.

The magnitude of accelerator R&D funds required for the completion of the Energy Doubler/Saver is large compared to programs conventionally funded in this category. Therefore, special effort is required, in FY 1978 and FY 1979, to provide funds for the completion of this very important effort. HEPAP urges that the total operating funds for the FY 1978 and FY 1979 High Energy Physics program adequately reflect this special need.

With best personal wishes,

Sidney D. Drell
Chairman, HEPAP

STANFORD UNIVERSITY

STANFORD LINEAR ACCELERATOR CENTER

Mail Address
SLAC, P. O. Box 4349
Stanford, California 94305

July 6, 1977

Dr. James S. Kane
Deputy Assistant Administrator
for Physical Research
Energy Research and Development Administration
Washington, D. C. 20545

Dear Jim:

I am forwarding to you the report of the 1977 HEPAP Subpanel on New Facilities which was formed in response to the charge given to it by you at the HEPAP meeting of February 18-19, 1977 in Washington, D. C.

This report was discussed by HEPAP at its June 27-28 meeting in Washington; and the recommendations contained therein were unanimously endorsed. We urge you to take all possible steps to effect their realization.

The discoveries made in the last two years and the technical advances in the accelerator art have reenforced our view that the fundamental strategy of the approach to higher energy via development of electron-positron and proton-proton colliding beams and high energy fixed target facilities is essential to successful investigation of the fundamental structure of matter. ISABELLE is now the critical feature in this program and its construction should begin as soon as possible.

The ISABELLE proton-proton colliding beam facility provides an increase of a factor of more than ten in the center-of-mass energy over that available in the highest energy collisions that are now possible at Fermilab and at CERN.

With this advance of the high energy frontier we expect, on the basis of present theoretical ideas, to cross the threshold for producing the massive fundamental particles that are believed to be the quanta, or carriers, of the weak forces of β radioactivity. The observation of these hypothetical particles will be a major triumph for our current concepts; and the opportunity to explore their properties will be of fundamental importance. On the other hand, failure to confirm their existence in experiments at the very high energies of ISABELLE would also have a very major impact on our understanding of elementary particle interactions. The actual energy recommended by the Subpanel and endorsed by HEPAP, namely a maximum of about 400 GeV for each beam, is higher by a factor of two than that originally proposed for the ISABELLE project. This higher energy is justified by the greatly enhanced physics potential of the facility even though an increase in cost is required. The high luminosity expected coupled with the flexible design of the interaction regions will make possible a broad and rich program of experimentation at ultra high energy. As already emphasized in previous Facilities Subpanel reports, such a facility must be a major part of our future thrust in High Energy Physics.

Since the report of the 1975 Subpanel on New Facilities was issued there have been great strides in high energy physics, highlighted by the discovery of particles exhibiting the new quantum number "charm." We are very appreciative of the considerable efforts by ERDA and the Congress which reversed previously decreasing budgets and have provided a measure of increased support to the field of high energy research. We hope that this trend will continue in the future so that, within the given budgetary guidelines, we can maintain a balanced program utilizing existing facilities and taking advantage of the technical and scientific opportunities for new exploration. With the positron-electron project (PEP) now under construction we have made a start on the three-pronged program which is essential for exploring the fundamental structure of matter. Construction of ISABELLE is now the critical next step to be taken in implementing this strategy.

With best personal wishes.

Sincerely yours,

Sidney D. Drell
Chairman, High Energy Physics
Advisory Panel

sj

cc: Herb Kinney
W. Wallenmeyer

UNITED STATES
ENERGY RESEARCH AND DEVELOPMENT ADMINISTRATION
WASHINGTON, D.C. 20545

JUN 9 1976

Professor Francis E. Low
Professor V. F. Weisskopf
Department of Physics
Massachusetts Institute of Technology
Cambridge, Massachusetts 02139

Dear Professors Low and Weisskopf:

Thank you for your joint letter of April 21, 1976, expressing concern for the limitation on attendance at the forthcoming XVIII International Conference on High Energy Physics to be held July 15-21, 1976, at Tbilisi in the USSR.

We appreciate your concern and are well aware of the value of international conferences as a valuable means of communications in High Energy Physics, both from the programmatic standpoint and as an important factor in international relations. The value of international collaboration and of visits to foreign laboratories and participation in international conferences has been well demonstrated. We in ERDA expect to continue to support US participation in these important international activities.

Although the US quota of invitations to the Conference at Tbilisi has been set by the International Union of Pure and Applied Physics at 150 (including 110 invitees who are working in ERDA supported activities), it has been necessary to limit the ERDA dollar support to the equivalent of full travel expenses for 20 attendees, with no more than 40 attendees receiving full or partial conference travel support. It is believed that the latter ceiling, on the basis of cost sharing with contractors, would optimize the use of the limited ERDA funding. This limitation on the ERDA supported participation in this Conference included consideration of the requirement by the Office of Management and Budget to make "drastic reductions" in travel to conferences in foreign countries. In addition, the sharply increased international

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activities of the many new ERDA programs have placed a substantial demand on the overall ERDA foreign travel ceiling, thus forcing reductions in other established programs and activities. We regret that we are unable to approve travel for a larger number of ERDA attendees at the Tbilisi Conference.

Sincerely,

Original signed by
Robert L. Hirsch

Robert L. Hirsch
Assistant Administrator for
Solar, Geothermal, and
Advanced Energy Systems