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ZGS HIGHLIGHTS AND SPECULATIONS

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ZGS HIGHLIGHTS AND SPECULATIONS*

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ABSTRACT

Several examples of unfinished business of the ZGS program are described. These examples cover physics, apparatus, and institutional subjects. Speculations are given about the evolution of these subjects during the 1980's.

INTRODUCTION

I would like to begin with a few general remarks about the talks which we have heard during the past two days. I think that these talks have summed up the history of the ZGS in fascinating and incisive ways and that they add up to a rather complete picture. Previously, I had harbored some strong doubts as to whether a retrospective symposium like this is a good way to bring forth or to record history, since the customary groundrules for public speaking are not necessarily compatible with telling the whole story. It seems to me that the speakers have met this challenge and have set forth fascinating unvarnished descriptions of the history of the ZGS program from many different points of view.

Since these speakers have done their jobs so well, I shall not attempt here to further summarize the history of the ZGS. Such additional condensation might easily lead to an over-simplification of what scientific research is and how and why it gets done. That is, the history of a large scale scientific research enterprise such as the ZGS can't be accurately described by a few breakthroughs or a few equations. That is the way one usually describes physics results, but it's not the way physics research actually happens. Real physics research, as we all know, involves many long periods of all kinds of work: planning, constructing, testing, running, and analyzing data; doing some of these things very well, some of them not so well; backing up for a second try, getting new kinds of insights which are occasionally breakthroughs but which more often are not. There are also many organizational tasks and fund-raising and other practical activities which are essential for achieving continuity in a research enterprise. The previous speakers have covered these diverse kinds of activities from many different points of view, and I believe that it all adds up to history and reality. If one should desire to search for the lessons of this history, one should take a broad look at all of the history and reality described at this symposium.

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Rather than further summarizing the ZGS history, then, I shall take the main goal of this talk to be the description of some important kinds of unfinished business of the ZGS program. I hope that it will be interesting and useful to consider some aspects of high energy physics research in whose development the work at the ZGS has played a prominent role, but whose further evolution or impact upon the field remains to be seen. That is, the history of the particular subjects which I have chosen is far from complete, so that foreseeing their future evolution involves some speculation. Of course, it is an important responsibility for future work in our field to further illuminate these subjects and thereby to replace speculation with new knowledge, insights, and capabilities.

Most of these areas of unfinished business have already been described in their respective historical contexts at this symposium, but my goal here is quite different - to emphasize some still-unanswered questions, particularly ones which have been closely associated with work at ZGS and which also may have a large impact on the future evolution of high energy physics research.

Before describing these subjects, I should like to digress in the next section by giving two significant examples of finished business which have not apparently been covered in previous talks and should be briefly mentioned here for completeness' sake.

TWO EXAMPLES OF FINISHED ZGS BUSINESS

These are examples of ZGS work whose consequences have already been well incorporated into the present day mainstream of high energy physics.

The first example is the choice of the window frame dipoles for the bending magnets of the ZGS. This type of magnet design allowed the first use of a 20 kilogauss guide field in a synchrotron, and hence yielded the maximum practical energy for a given bending radius. The choice, a decade later, of window frame dipoles for the Fermilab separated function lattice allowed the Fermilab accelerator to reach 500 GeV in a tunnel of radius 1 kilometer. Fig. 1 gives the energy per unit radius for various proton synchrotrons, and shows that 0.5 GeV/meter is still the present limit. This limit will be exceeded only when synchrotrons using superconducting magnets come into operation - (more about this later).

A second example of finished ZGS business concerns the first search for direct muons from hadron collisions. This method of searching for the intermediate vector boson was invented, at least at the ZGS, by M. L. Good, then at the University of Wisconsin, and led to the first published report of such an experiment.[1] By now we know that much higher proton energies will be required to produce the intermediate vector boson, but in the meantime the generalization of this method by L. Lederman and his coworkers at the AGS to the study of direct production of muon pairs has led to the opening of whole new areas of particle physics.

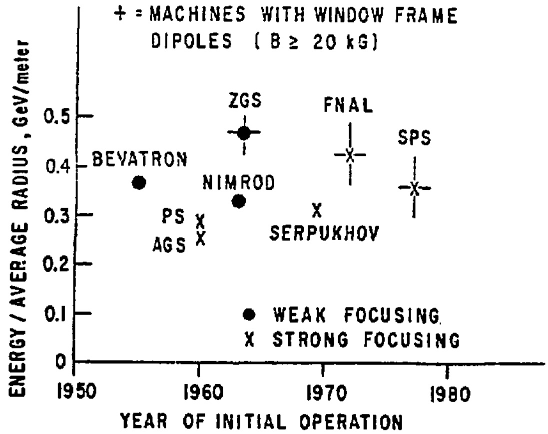

Fig. 1 Energy divided by mean radius for various proton synchrotrons.

SUBJECTS FOR SPECULATION - UNFINISHED BUSINESS

Now I come to the main part of this talk. I shall describe nine examples of important subject areas in which the ZGS program has made basic contributions, but where there is still much room for speculation, for new ideas, and for new approaches. The first four examples are in the area of high energy physics itself, the next three concern apparatus for high energy research, and the last two are institutional matters.

1. SCALAR MESONS

Notable early work at the ZGS involved the first observation of the $\delta_2(980) \rightarrow \pi\pi$ in K^-p collisions by the Northwestern - Argonne groups[2] and the search for the $\varepsilon(700) \rightarrow \pi^+\pi^-$ under the ρ° peak by the Wisconsin - Toronto groups[3]. Important recent work at the ZGS on scalar mesons decaying to $\bar{K}K$ has been carried out by the Notre Dame - Argonne streamer chamber group[4] and the Argonne Effective Mass Spectrometer group.[5]

Two kinds of challenging questions concern the 0^+ mesons. The first kind consists of theoretical questions which center on the contrast between, on the one hand, the simple nonet structure of the 0^-, 1^- mesons which seem to be $\bar{q}q$ systems in a relative S wave, as well as the corresonding simple nonet pattern of the 2^+ mesons, and, on the other hand, the observed complexities of the 0^+ mesons. Perhaps the 0^+ mesons contain a substantial admixture of $\overline{qq}qq$ states or even of glueballs (a gluon-gluon bound state).[6] The second kind concerns avenues for making further experimental progress. What types of next-generation spectrometer experiments should be carried out to better understand the 0^+ mesons? Will there be sufficient priority and funding to permit experimental progress in this kind of meson spectroscopy in the 1980's?

2. HADRON DYNAMICS AT SMALL TRANSVERSE MOMENTUM

As already described at this symposium, several kinds of ZGS experiments have made important contributions to the phenomenological understanding of hadron dynamics at small p_T. Some pioneering examples are elastic scattering at small and medium values of momentum transfer, two-body inelastic hadron reactions, elastic scattering at 180° as a function of beam energy, inclusive production experiments, and polarization measurements of various hadron reactions.

Here the present-day unsolved theoretical questions mainly concern the problem of connecting these low p_T phenomena with the behavior of quarks and gluons, the fundamental strongly interacting quanta of quantum chromodynamics (QCD). (As a related historical note, we recall that an early quark model for the dynamics of small p_T reactions[7] found important support in experimental data from the 30 inch bubble chamber.[8]) It seems compelling to try to understand low p_T data in terms of QCD ideas even though these data involve the quark confinement regime where quantitative QCD calculations can not yet be made. Perhaps low p_T reactions may eventually yield new insights into QCD phenomena comparable to those which are now being obtained from high p_T phenomena. Of course, a similar challenge exists for hadron spectroscopy. One can reverse the emphasis by observing that quark confinement effects surely need to be studied in depth, both experimentally and theoretically, for both their fundamental interest and their practical importance.

3. DIBARYON STATES

As described earlier in this symposium, experiments at the ZGS have shown that there is considerable structure in the energy dependence of the polarized beam and target total pp cross section differences $\Delta\sigma_L$ and $\Delta\sigma_T$, as shown in Fig. 2. This structure has been interpreted as evidence for dibaryon resonances.[9] More phase shift analyses and polarized beam polarized target experiments will be necessary to fully determine the properties of these structures - just as was necessary during the past two decades to sort out the baryon resonances. Some of these experiments can be carried out at medium energy acelerators such as LAMPF but others will require higher energy proton polarized beams.

In any case, it is clear that these previously unexpected dibaryon phenomena have now become experimentally accessible by using polarized beams and targets. Further work is needed to determine whether these phenomena can be interpreted as six- quark bound states, and if so, what the implications of this will be for "quark chemistry". A very important related question concerns the role of multiquark structures within nuclei. Again, confinement effects in QCD are what we are speculating about.

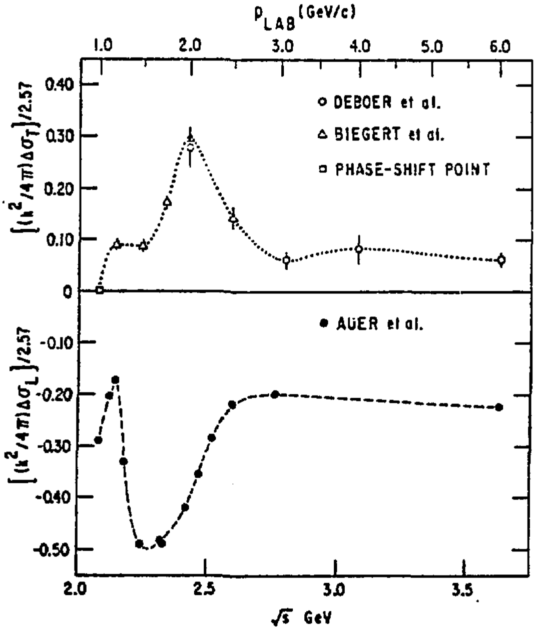

Fig. 2 Proton-proton total cross section differences using polarized beam and polarized target.

Many of the speculations about the physics of multiquark bound states are like y to be clarified by the totally new level of precision in $\bar{p}p$ studies which will be made possible in 1982 by the CERN Low Energy Antiproton Ring (LEAR). If narrow baryonium $(\overline{qq}qq)$ states exist, this will be a powerful method for their study. Will comparable progress in methods for observing new properties of dibaryon systems take place during the next few years?

4. SPIN EFFECTS AT LARGE P_T

Fig. 3 shows the very large spin-spin effects which have been observed by the Michigan-AUA-Argonne group for pp elastic scattering at 11.75 GeV/c. Of course, it is very tempting to try to interpret these effects as manifestations of a spin-spin dependence of the scattering of fundamental constituents of the proton: quarks of spin $\frac{1}{2}$ and gluons of spin 1. If this is indeed the case, then this type of experiment will yield a direct measure of the spin dependence of the basic interactions in QCD.

To test these ideas quantitatively requires a better understanding of QCD and its application to elastic hadron scattering at

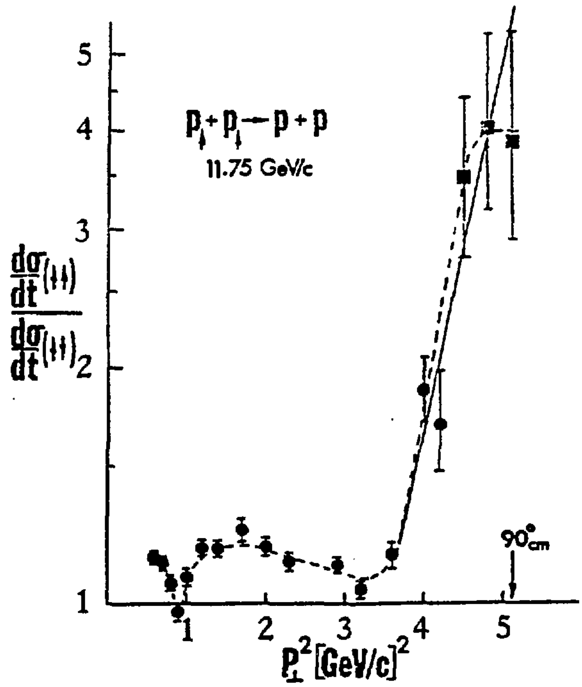

Fig. 3 Spin-spin effect in elastic pp scattering as measured by the Michigan-AUA-Argonne collaboration.

large P_T than has yet been achieved. Perhaps achieving this understanding will be expedited by measuring single and double spin effects (i.e. spin-orbit and spin-spin forces) at larger values of energy and transverse momentum. Perhaps a better phenomenological understanding of $\bar{q}q$ and qqq potentials for describing the masses of known hadrons can provide a related quantitative measure of the large spin-spin interaction between quarks.

There are also unsolved experimental challenges in finding ways to carry out these large transverse momentum polarization measurements at Fermilab and SPS, and someday at very high energy $\bar{p}p$ and pp colliders.

A general observation about the above four physics topics is that they illustrate the progress of the quark concept during the years of the ZGS program. At the time of the first ZGS experiments in 1964, the quark had just been invented and served mainly as a mathematical convenience for remembering SU_3 multiplets. Although quark models have come a long way since then, quantitative tests of QCD, the theory of quarks and gluons, are still few. Of course, there is also a continuing, tantalizing, ultimate question of whether free quarks can exist.

5. APPLICATION OF SUPERCONDUCTING MAGNETS TO HIGH ENERGY PHYSICS

Since the first use of a superconducting magnet in a high energy physics experiment occurred at the ZGS some fourteen years ago, and since the giant superconducting magnet for the 12 ft bubble chamber was first operated 10 years ago, it is reasonable to wonder why superconducting magnet technology could still be described as unfinished business. The reasons for this are well known to some of you--the exceptional technical challenges involved in the design and fabrication of accelerator-quality superconducting dipoles, the lack of funding for appropriate development and demonstration projects and the underestimation of the overall technical obstacles involved in new superconducting accelerator projects. The net effect is that today no superconducting accelerator ring has been built, nor is one close to completion. So we have very little real data on such basic matters as beam heating effects or the operational reliability of large rings of superconducting magnets and their associated cryogenic systems.

The question for the future is not only obvious but also is a key to the future of much of the U.S. high energy physics program: Will the technology of large systems of superconducting accelerator magnets now begin to progress at the hoped-for rate so that Isabelle and the Fermilab Energy Doubler/Collider will achieve their design goals with a reasonable degree of operational reliability and overall practicality? There is also an important present question - should the U.S. research and development program on superconducting accelerator magnets be strengthened?

6. BEAM BRIGHTENING

Over the years, work at the ZGS has made some important contributions to the development of new methods for brightening the proton beam in an accelerator. The use of H^- stripping injection, in which one can achieve large increases in beam brightness within a synchrotron by injection (and stripping) of a continuous H^- beam through a thin foil, an idea which originated at Novosibirsk, was first achieved in a synchrotron at the ZGS in 1969. Since then, several existing and planned accelerators, including the Fermilab machine, have adopted H^- injection. Important open questions include: whether this will become the standard injection method for proton synchrotrons, whether an intense H^- polarized proton source can be developed, and whether there are major new applications for beam brightening by charge-changing processes in addition to those already in use for proton synchrotrons and for Tokomak-type devices.

The most exciting presentday questions about beam brightening in high energy accelerators concern the application of stochastic cooling (invented at CERN) and electron cooling (invented at Novosibirsk) to achieve $\bar{p}p$ colliding beams in the CERN SPS and at Fermilab. (In fact, a significant early step along the path toward the concept of stochastic cooling was taken in the mid 1960's with the development by MURA and Argonne of feedback systems for damping the coherent transverse instabilities of the ZGS circulating proton beam.) Clearly, the course of high energy physics during the 1980's and beyond will be crucially affected by the overall amount of brightening which can be achieved for antiproton beams. The CERN LEAR project, mentioned above, also depends on such antiproton beam brightening.

7. ACCELERATION OF POLARIZED PROTON BEAMS TO HIGH ENERGY

The pioneering contributions of work at the ZGS in this area are described in the talk of Everett Parker. Here I wish to remark mainly on the possible impact of this ZGS work upon higher energy proton accelerators. (Of course, there are also important physics opportunities connected with achieving polarized beams in e^+e^- machines such as PETRA, PEP, and LEP, as well as with the invention of the Siberian Snake, but I shall not describe these here.)

Two kinds of future uses for accelerated polarized proton beams are clear. One kind is an extension of the large p_T studies which have been begun here at the ZGS to energies where jet production and other "simple" QCD processes can be clearly identified and their spin dependence studied. Although first-generation experiments of this kind may be performed using a secondary polarized proton beam, the high level of precision which seems appropriate for detailed measurements of the spin structure of ba~ic quark-gluon interactions will require intense and thus directly-accelerated beams of polarized protons. Achieving such acceleration with alternating gradient accelerators will be the next challenge, one which may be addressed using the AGS at Brookhaven.

The second kind of possible use for high energy polarized proton beams will be for use in a pp collider (Isabelle) or perhaps in $\bar{p}p$ colliders. Such an achievement would offer the chance to measure strong interaction spin effects in a totally new energy range, where large momentum transfer QCD effects are expected to be essentially background-free and unambiguous. In addition, the polarized colliding beams could offer a practical means for detecting weak interaction effects by their parity violation.

8. END OF THE ERA OF REGIONAL ACCELERATORS

With the shutdown of the ZGS, we are entering a period where each of the three DOE-supported U.S. high energy physics accelerators is designed for completely different kinds of experiments. Thus each one should be expected to serve the entire U.S. high energy physics program. The increasing scale of new accelerators as well as the difficult budget situation (described below) seem to leave no practical alternative to such an arrangement. But a host of deep issues are raised, one of which is a managerial and institutional question-what form of organizational arrangement will allow an accelerator laboratory to best serve the interests of the entire U.S. high energy physics community? Note that the three remaining DOE-HEP accelerator centers each have different contractor arrangements: a single university, a regional consortium of universities, and a national consortium of universities. Perhaps the safest speculation about organization is that historical precedent will prevail and that these three different arrangements will continue beyond the ending of the era of the regional high energy accelerator.

Related problems that may become increasingly evident as the work is confined to fewer accelerator centers are a loss of diversity in styles and directions of research, decreasing leadership opportunities for younger scientists, and a narrowing of opportunities for the development of new ideas in accelerator science and technology. Moreover, at some later time, the painful issues involved in further decreasing the number of national high energy accelerator centers will no doubt have to be addressed.

9. THE ERA OF SINGLE PURPOSE HIGH ENERGY PHYSICS LABORATORIES

The ZGS has been the only U.S. high energy physics accelerator to be imbedded within a very large multipurpose energy research laboratory. The new era of mainly single purpose high energy accelerator laboratories appears to be inevitable because of the large cost and physical size of new machines and, for international laboratories, to avoid complications concerning commercial and other aspects of international technology development. The decreasing role of high energy physics in multipurpose laboratories may impede our ability to exchange personnel and advances in technology with other areas of R&D work. Such mutual exchange might become a more important goal now that energy problems and their technological challenges and controversies have become critical issues for industrialized nations. This kind of issue may be of particular importance in the U.S. since most federal support for high energy physics research is budgeted through the Department of Energy.

REMARKS

The above examples of issues for further work and thought show clearly that the field of high energy physics offers many fundamental challenges in physics, instrumentation, and organization. Additional examples from other areas of high energy physics could easily be given. We are now entering a decade in which new theoretical ideas, new accelerators, and new experimental techniques offer the real possibility of achieving a very fundamental level of understanding of the "elementary particles" and their interactions. There is also the possibility of finding a total surprise which does not fit into present schemes for the quarks and leptons. The extent to which these possibilities are realized will depend upon the quality of the answers to questions of the types I have outlined, as well as upon the level of funding for the work.

In fact, the budget problems which have led to many difficulties in carrying out the ZGS physics program are still casting a shadow on the future of U.S. high physics research. My calculations indicate that for the entire decade of the 1970's, the annual rates of change of various budgets (measured in constant value dollars) were fairly steady, with the following average values:

U.S. Gross National Product	+ 2% per year
U.S.Federal Budget	+ 4% per year
U.S. High Energy Physics Budget	- 3% per year
ZGS Budget	- 9% per year

In such a situation, it is not surprising that the ZGS program was unable to fully utilize its new capabilities during the 1970's. These new capabilities included the 12 ft. bubble chamber and its neutrino research program, the polarized proton beam (particularly for 12 GeV running), the polarized deuteron beam, and the development of superconducting accelerator magnets. The Argonne proposal in 1971 for the construction of a Superconducting Stretcher Ring for the ZGS, a demonstration project which could have made major contributions to the development of practical superconducting magnet accelerators, was not seriously considered by the funding agency in view of the overall ZGS budget outlook. Another important opportunity to develop practical superconducting dipole magnets was lost in 1976 when the POPAE high energy colliding beam design project was not supported.

Contraction of support for the U.S. high energy physics program during the 1970's has created several kinds of problems. First, as illustrated by the above specific ZGS examples, some important opportunities have been lost forever. Some of those losses are likely to lead to delays and difficulties in the projects of the 1980's. Second, even after the shutdown of the ZGS, present budget levels will not permit full utilization of U.S. accelerators during the 1980's.

ACKNOWLEDGEMENT

As an ex-administrator, I recall that one of the duties of an administrator is to try to keep things coordinated. But, as shown in Homer's Neal's talk, the question of who is coordinating whom is a complicated one in high energy physics. There are complex webs of relationships among the user research groups, the people who actually make the accelerator and detectors work,the laboratory management, and the funding agency people who have found support for the work even in very difficult times. These relationships involve more than a thousand people who have contributed to the ZGS program over a period of some two decades. To end on a coordinating note, I should like to take the liberty of speaking on behalf of all of the ZGS administrations and offer thanks to all of these people who have worked so well together on the ZGS program and have accomplished so much.

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