FERMILAB-SLIDES-19-017-T

In Leon's company, it seemed that anything might be possible

Chris Quigg · *Honoring Leon Lederman* · APS April Meeting, Denver 2019

This manuscript has been authored by Fermi Research Alliance, LLC under Contract No. DE-AC02-07CH11359 with the U.S. Department of Energy, Office of Science, Office of High Energy Physics.

Observation of Long-Lived Neutral *V* Particles*

K. Lande, E. T. Booth, J. Impeduglia, and L. M. Lederman,
Columbia University, New York, New York

and

W. Chinowsky, *Brookhaven National Laboratory, Upton, New York*
(Received July 30, 1956)

OBSERVATION OF HIGH-ENERGY NEUTRINO REACTIONS AND THE EXISTENCE OF TWO KINDS OF NEUTRINOS*

G. Danby, J-M. Gaillard, K. Goulianos, L. M. Lederman, N. Mistry, M. Schwartz,† and J. Steinberger†

Columbia University, New York, New York and Brookhaven National Laboratory, Upton, New York
(Received June 15, 1962)

OBSERVATION OF ANTIDEUTERONS*

D. E. Dorfan, J. Eades, L. M. Lederman, W. Lee, and C. C. Ting

Columbia University, New York, New York
(Received 4 May 1965)

Observations of the Failure of Conservation of Parity and Charge Conjugation in Meson Decays: the Magnetic Moment of the Free Muon*

Richard L. Garwin,† Leon M. Lederman, and Marcel Weinrich

Physics Department, Nevis Cyclotron Laboratories, Columbia University, Irvington-on-Hudson, New York, New York
(Received January 15, 1957)

PRODUCTION OF ANTIPROTONS VIA NUCLEAR MOTION*

D. E. Dorfan, J. Eades, L. M. Lederman, W. Lee, and C. C. Ting

Columbia University, New York, New York

and

P. Piroué and Stuart Smith

Princeton University, Princeton, New Jersey

and

J. L. Brown, J. A. Kadyk, and G. H. Trilling

Physics Department and Lawrence Radiation Laboratory, University of California, Berkeley, California
(Received 4 May 1965)

Magnetic Moment of the Free Muon*†

T. Coffin, R. L. Garwin,‡ S. Penman, L. M. Lederman, and A. M. Sachs
Columbia University,§ New York, New York
(Received October 1, 1957)

MUON MASS AND CHARGE BY CRITICAL ABSORPTION OF MESONIC X RAYS*

S. Devons,† G. Gidal,‡ L. M. Lederman, and G. Shapiro
Columbia University, New York, New York
(Received September 6, 1960)

Observation of Massive Muon Pairs in Hadron Collisions*

J. H. Christenson, G. S. Hicks, L. M. Lederman, P. J. Limon, and B. G. Pope
Columbia University, New York, New York 10027, and Brookhaven National Laboratory, Upton, New York 11973

and

E. Zavattini
CERN Laboratory, Geneva, Switzerland
(Received 8 September 1970)

A Cloud Chamber Determination of the Lifetime of the Negative Pi Meson and the Mass of the Negative Mu Meson

Leon M. Lederman

A Dissertation

Submitted in Partial Fulfillment of the requirements for the degree of Doctor of Philosophy, Faculty of Pure Science, Columbia University

April,1951

Observation of Direct Production of Leptons in *p*-Be Collisions at 300 GeV

J. A. Appel, M. H. Bourquin, I. Gaines, D. C. Hom, L. M. Lederman, H. P. Paar, J.-P. Repellin,* D. H. Saxon,† H. D. Snyder, J. M. Weiss, and J. K. Yoh
Columbia University, New York, New York 10027‡

and

B. C. Brown, J.-M. Gaillard,* and T. Yamanouchi
Fermi National Accelerator Laboratory, Batavia, Illinois 60510§
(Received 15 July 1974)

OBSERVATION OF π^0 MESONS WITH LARGE TRANSVERSE MOMENTUM IN HIGH-ENERGY PROTON-PROTON COLLISIONS

F.W. BÜSSER*1, L. CAMILLERI, L. Di LELLA, G. GLADDING*2, A. PLACCI, B.G. POPE, A.M. SMITH, J.K. YOH*3 and E. ZAVATTINI
CERN, Geneva, Switzerland

B.J. BLUMENFELD*4 and L.M. LEDERMAN*5
*Columbia University*6, N.Y., USA*

and

R.L. COOL*5, L. LITT and S.L. SEGLER
*Rockefeller University*7, N.Y., USA*

Received 10 August 1973

Observation of a Dimuon Resonance at 9.5 GeV in 400-GeV Proton-Nucleus Collisions

S. W. Herb, D. C. Hom, L. M. Lederman, J. C. Sens,[a] H. D. Snyder, and J. K. Yoh
Columbia University, New York, New York 10027

and

J. A. Appel, B. C. Brown, C. N. Brown, W. R. Innes, K. Ueno, and T. Yamanouchi
Fermi National Accelerator Laboratory, Batavia, Illinois 60510

and

A. S. Ito, H. Jöstlein, D. M. Kaplan, and R. D. Kephart
State University of New York at Stony Brook, Stony Brook, New York 11974
(Received 1 July 1977)


June 25, 1963

Part II

THE TRULY NATIONAL LABORATORY (TNL)

L. M. Lederman
Nevis Laboratories, Columbia University

A. Introduction

We are facing, as a result of all the feverish activity of the sort we are having here, the onset of two or three new super-large facilities for high energy physics. The question of organization of these new laboratories is obviously of very great importance. We have examples (not in high energy physics!) of large laboratories containing unique facilities which, through poor organization, are generally considered to be flops. Another exceedingly important question relates to the role of the university in the era of the super-large laboratory, with the super-expensive hourly running cost, surrounded by the necessary highly professional on-site groups. Finally, there is the ever present competition between institutions and regions for the presumably finite number of authorizations for accelerators costing more than 100 million dollars.


STOP

RRW: "Money and effort that would go into an overly conservative design might better be used elsewhere… A major component that works reliably right off the bat is, in one sense, a failure—it is over-designed."

"Being a professor at a university is the best invention of Western civilization. There's where you have power, you have freedom, you can do anything you want. … Who wants to be a director where you are not free to do anything, everyone is watching you? God help you if you fall asleep, which you often do at seminars, everyone notices and puts it down."

Securing a future for the laboratory

Fantasies of future Fermilab facilities

R. R. Wilson

Fermi National Accelerator Laboratory, Batavia, Illinois 60510

The author presents a perspective on possible future projects at Fermilab.

CONTENTS

Oh, fancie that might be, oh, facts that are!

(Browning, 1889)

I. FOREWORD

Fancies can be fantasized for fabricating future facilities at Fermilab, but fulfillment will depend on the unfolding of physics, on finding funds, on the focus of other laboratories, on forceful personalities and fierce fights; but most of all it will depend on new facts, new findings, new fancies. Thus Fermilab physicists might find it futile to feel their way to 5 TeV, might find it more fun to fill in facts about physics at 50 GeV, or they might find more felicitous the flowering of photon physics at 500 GeV. In the following phantasmata, let me first figure on the most fruited fulfillment, let me flounder in a veritable fantasia of physics facilities; for realistic factors finally "little by little will subtract faith and fallacy from fact."

II. INTRODUCTION

The Fermi National Accelerator Laboratory was established in 1967 after the dramatic selection of a 7000 acre site located near Chicago, Illinois from the many sites presented throughout the nation. Figure 1 shows the site as it now appears; it is very flat and roughly rectangular, 5 km on a side. The proton synchrotron shown in Fig. 1 was brought into operation at 200 GeV in March 1972. It has supplied protons to the four experimental areas, also shown, which have successively been brought into operation. The synchrotron was designed to accelerate 5×10^{13} protons per pulse (ppp) to 500 GeV. Although the accelerator did reach an energy of 500 GeV, it regularly operates at 400 GeV and at intensities of about 2×10^{13} ppp, the maximum so far being 2.6×10^{13} ppp at a cycle time of about 10 seconds.

The characteristics of the accelerator and the experimental areas have been described in detail in a review article by J. R. Sanford (1976). As of July 1978 some 250 experiments had been completed of the 300 proposals for experiments which had then been approved. The results of those experiments have been published in about 225 articles, (Half of the articles about experimental particle physics appearing in Physical Review Letters during 1977 were about work done at Fermilab).

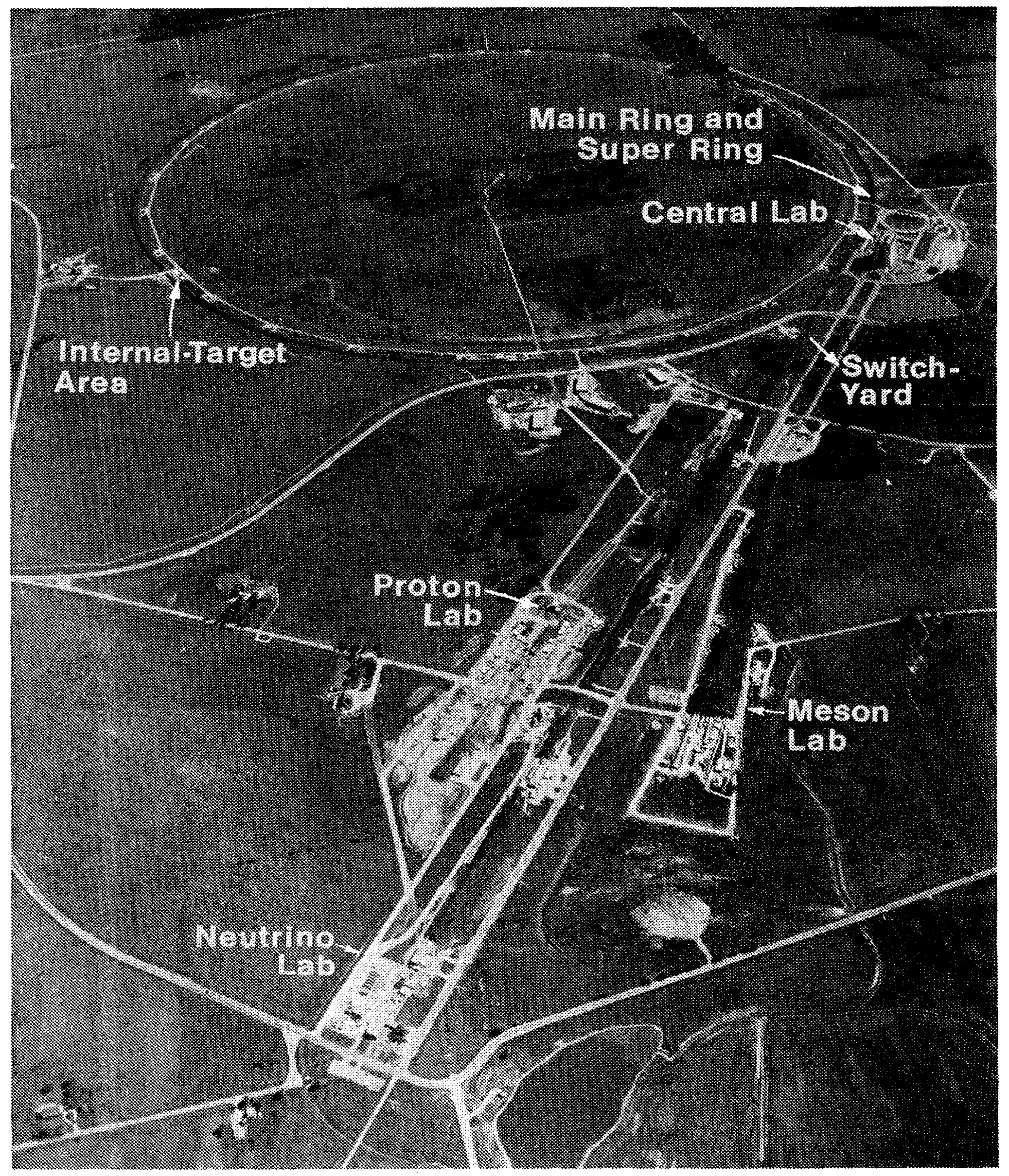
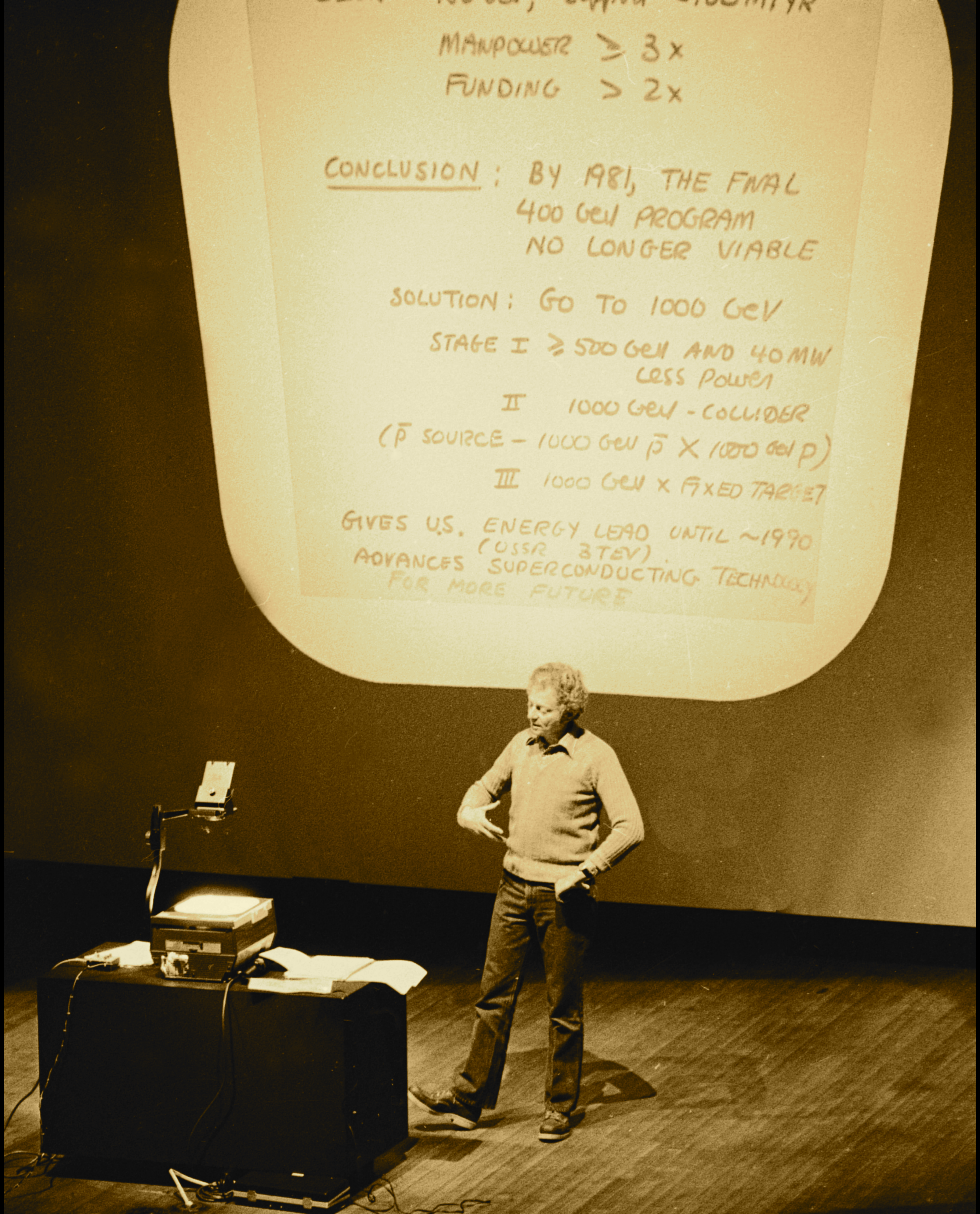
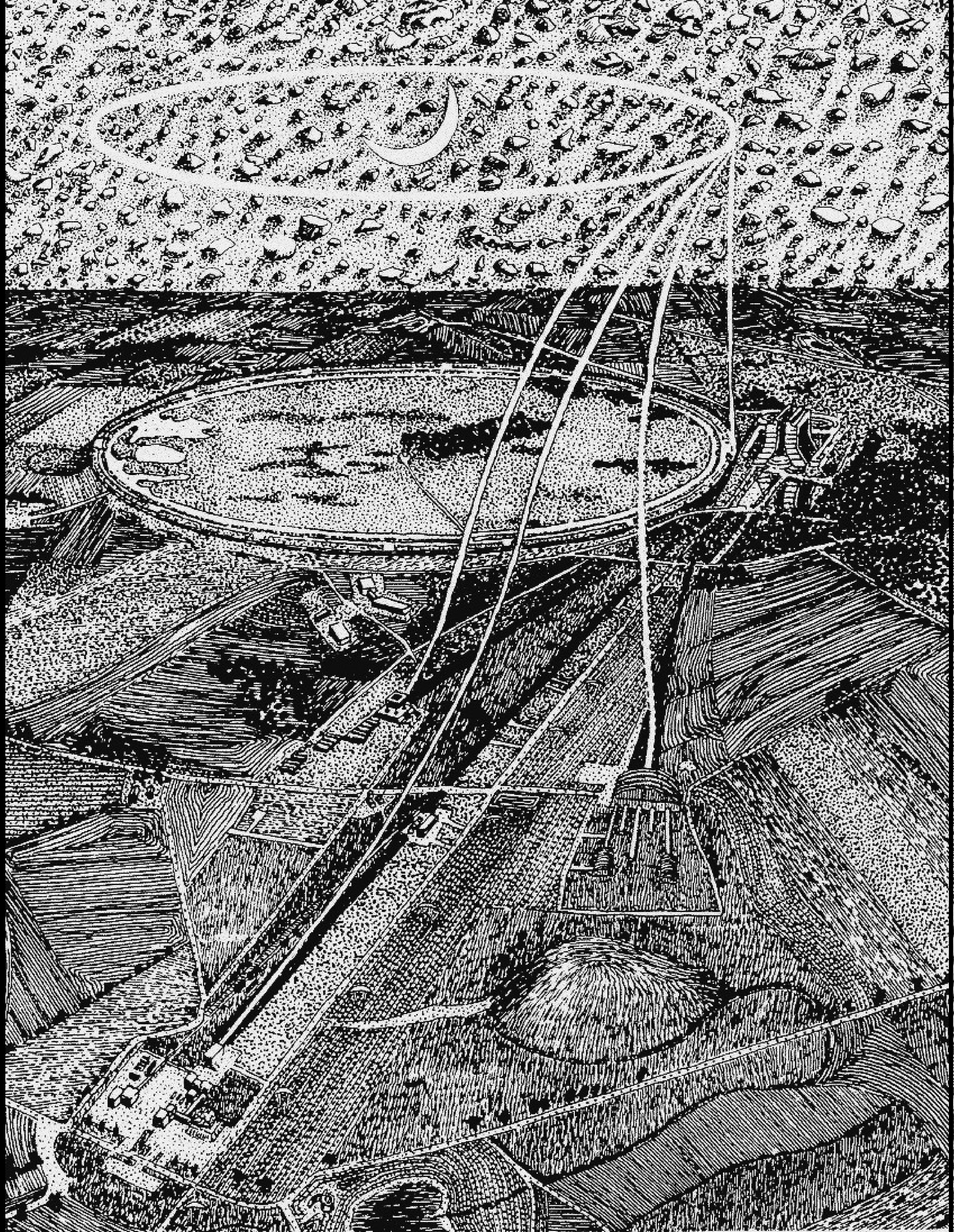
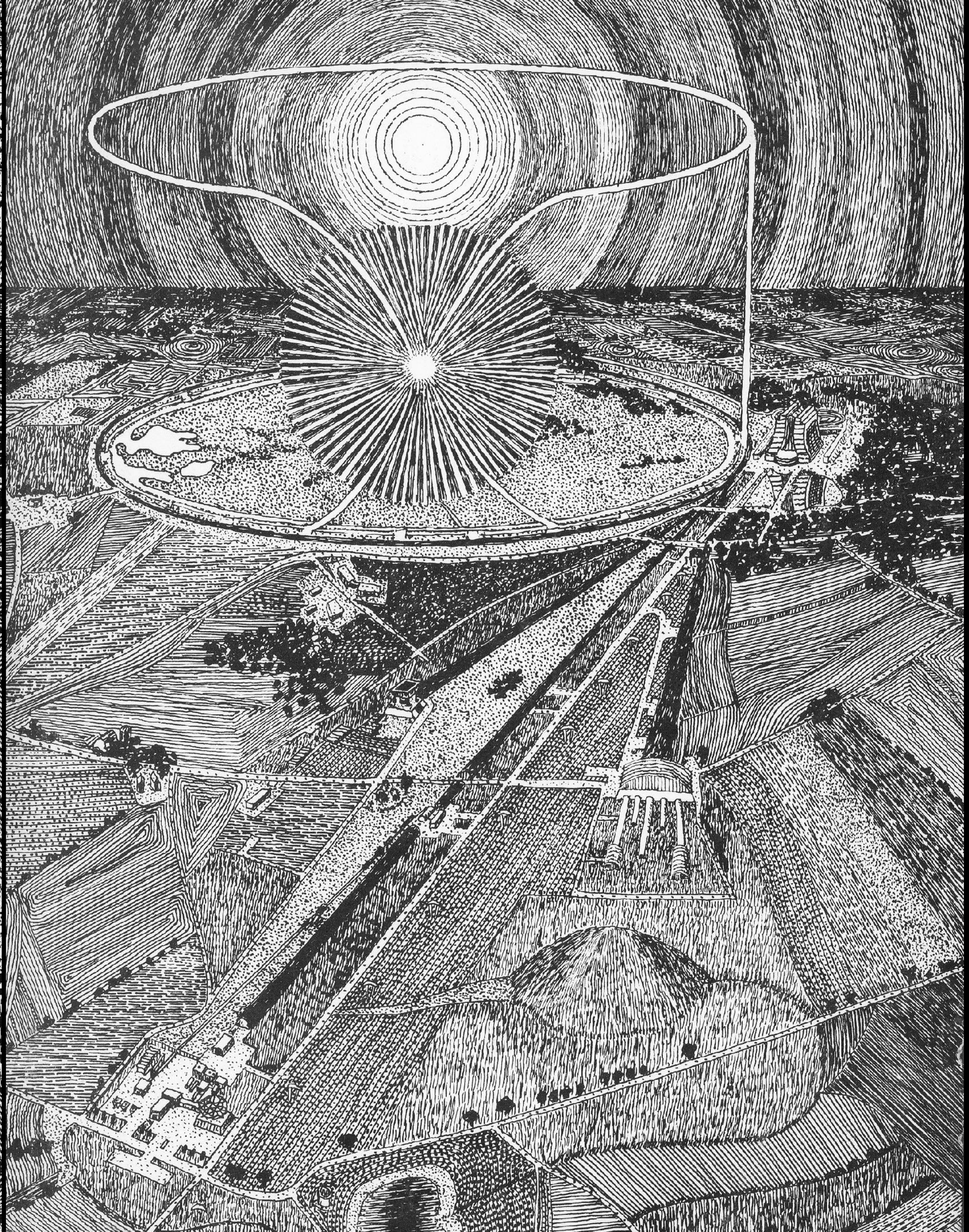


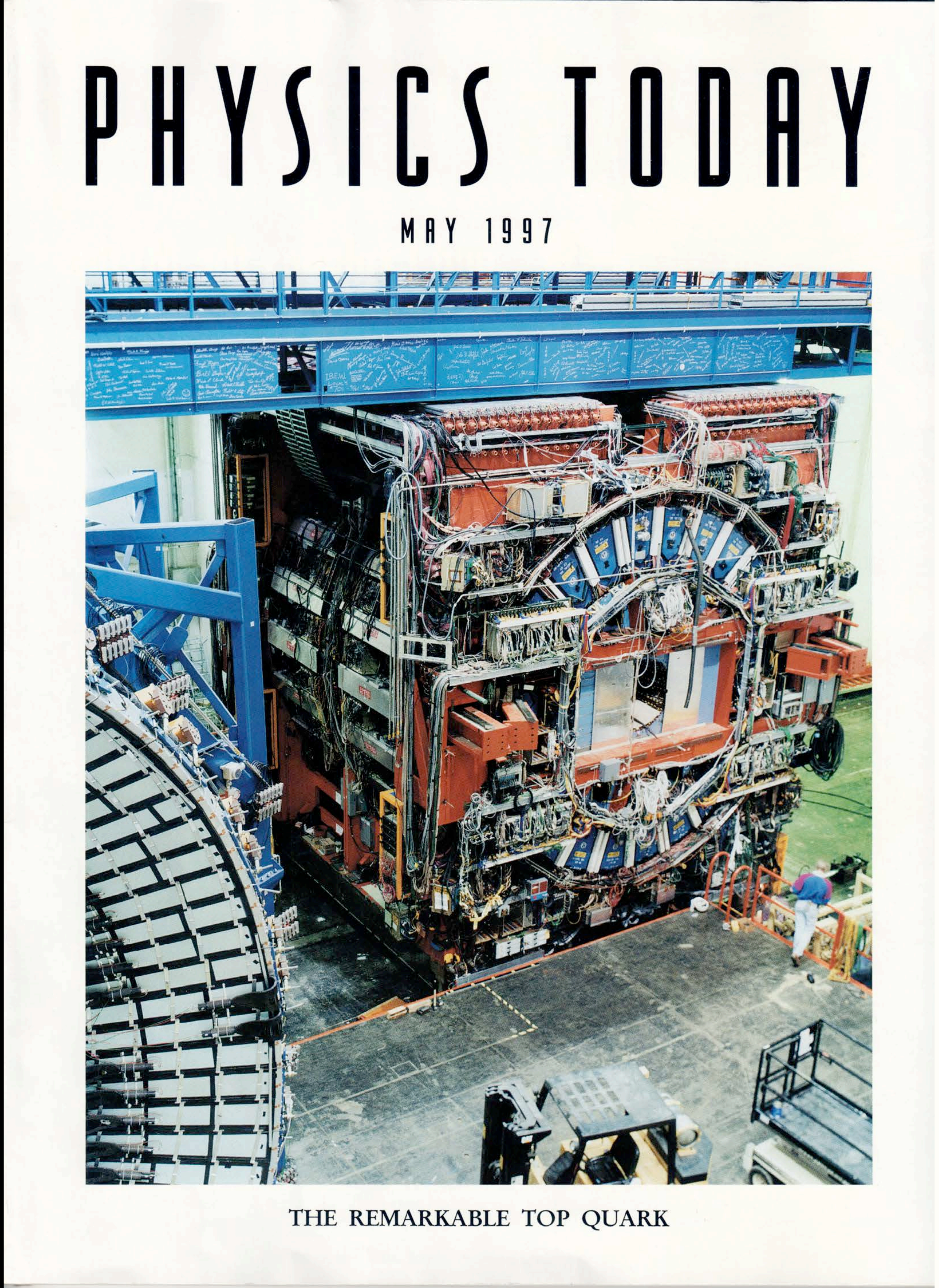
FIG. 1. Aerial view of the accelerator and experimental areas at Fermilab. Some improvements to the experimental areas have already been started to accomodate the extracted 1 TeV beam when available.

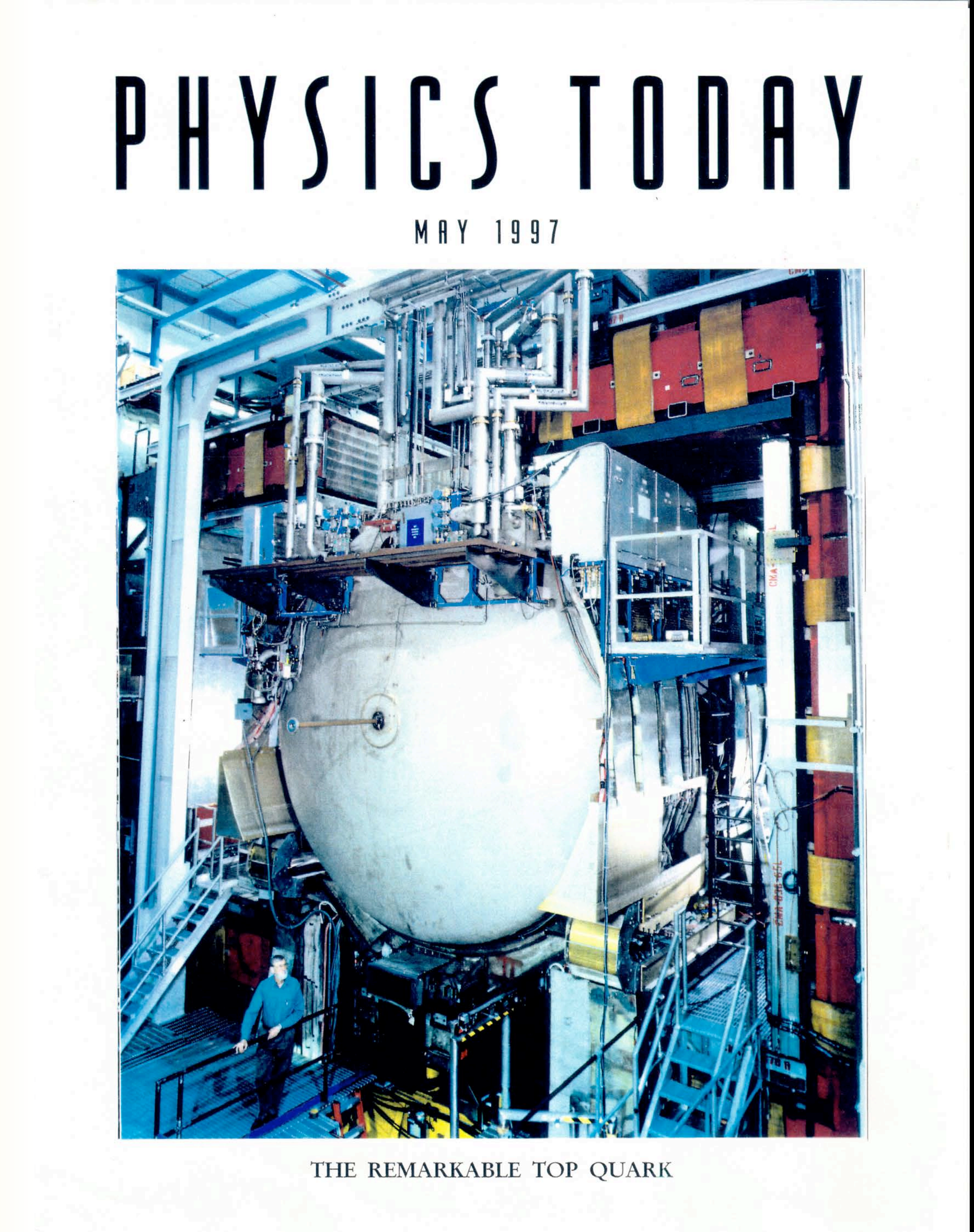

MANPOWER ≥ 3x
FUNDING > 2x
CONCLUSION: BY 1981, THE FINAL
400 GeV PROGRAM
NO LONGER VIABLE
SOLUTION: GO TO 1000 GeV
STAGE I ≥ 500 GeV AND 40 MW
LESS POWER
II 1000 GeV - COLLIDER
(p̄ SOURCE – 1000 GeV p̄ X 1000 GeV p)
III 1000 GeV x FIXED TARGET
GIVES U.S. ENERGY LEAD UNTIL ~1990
(USSR 3 TEV)
ADVANCES SUPERCONDUCTING TECHNOLOGY
FOR MORE FUTURE

Angela Gonzales


DIPOLE
QUADRUPOLE


PHYSICS TODAY
MAY 1997
THE REMARKABLE TOP QUARK


PHYSICS TODAY
MAY 1997
THE REMARKABLE TOP QUARK

Bringing dignity to the office

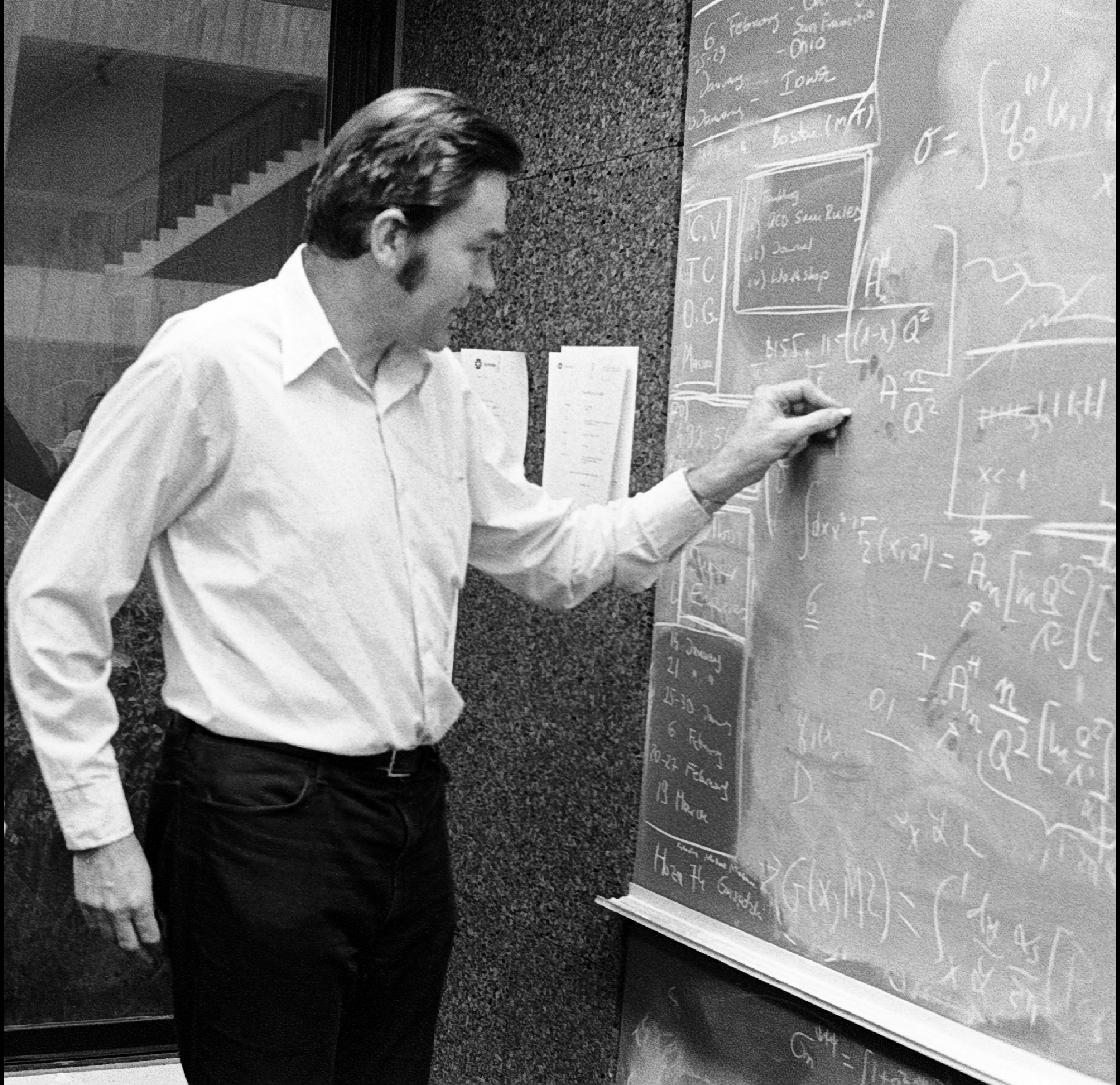

WHITEWATER


WHITEWATER

Enhancing the Quality of Life


Happy
Birthday

Enriching the scientific environment


Ohio
IOWA
QCD Sum Rules
Workshop

SEMINARS TO COVER PARTICLES AND COSMOLOGY

Seven seminars will be given at Fermilab covering the general theme of particles and cosmology.

About the middle of this decade, Fermilab expects to be able to slam 1 TeV protons into 1 TeV antiprotons for center-of-mass collisions of 2 TeV, the most powerful ever created by scientists. These energy levels are so high that for the moment they remain the playground of theoreticians.

In order to gain some hint of what may happen when particles of those energies collide, Fermilab has organized this series of seminars devoted to the connections between particle physics, cosmology and cosmic rays.

Two of the seminars already have been given. They were Prof. James Peebles of Princeton University, who spoke on Oct. 29 about "Cosmology, New Physics and Old," and Prof. William Fowler of the California Institute of Technology, who spoke on Nov. 5 about "Nucleosynthesis in Supernovae."

1980–81

The remaining five lectures are: Prof. Tom Gaisser, Bartol Research Foundation, University of Delaware, "Particle Collisions Above 10 TeV as Seen in Cosmic Rays," December,-(he will give four talks);

Prof. David Schramm, University of Chicago, "Neutrinos and the Big Bang," Jan. 14, 1981;

Prof. Malvin Ruderman, Columbia University, "Elementary Particles and Superdense Matter," Feb. 12;

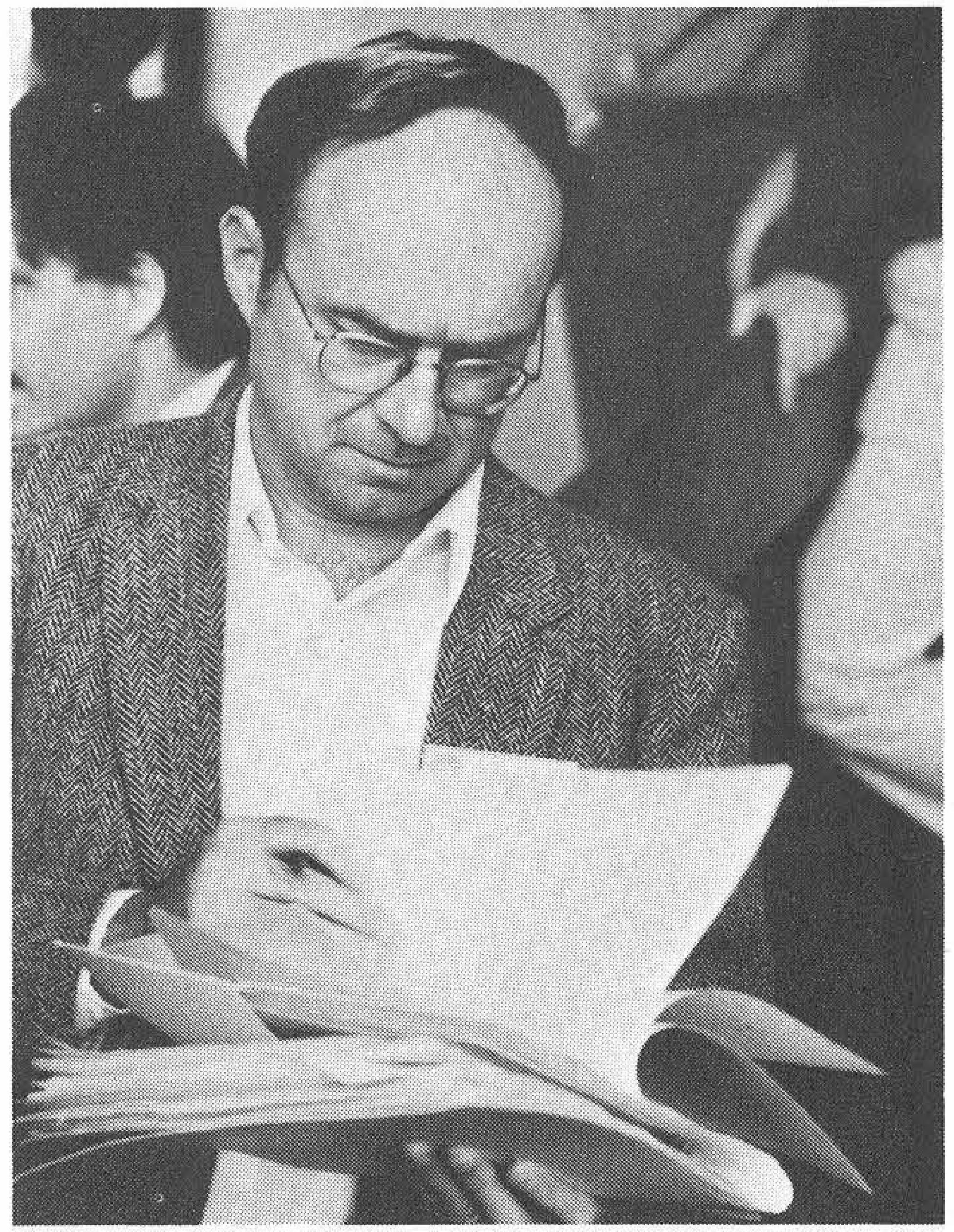
Prof. Gordon Baym, University of Illinois, "How Can We Learn About Particles From Neutron Stars," March 11;

Prof. Steven Weinberg, "The Very Early Universe," sometime in April.

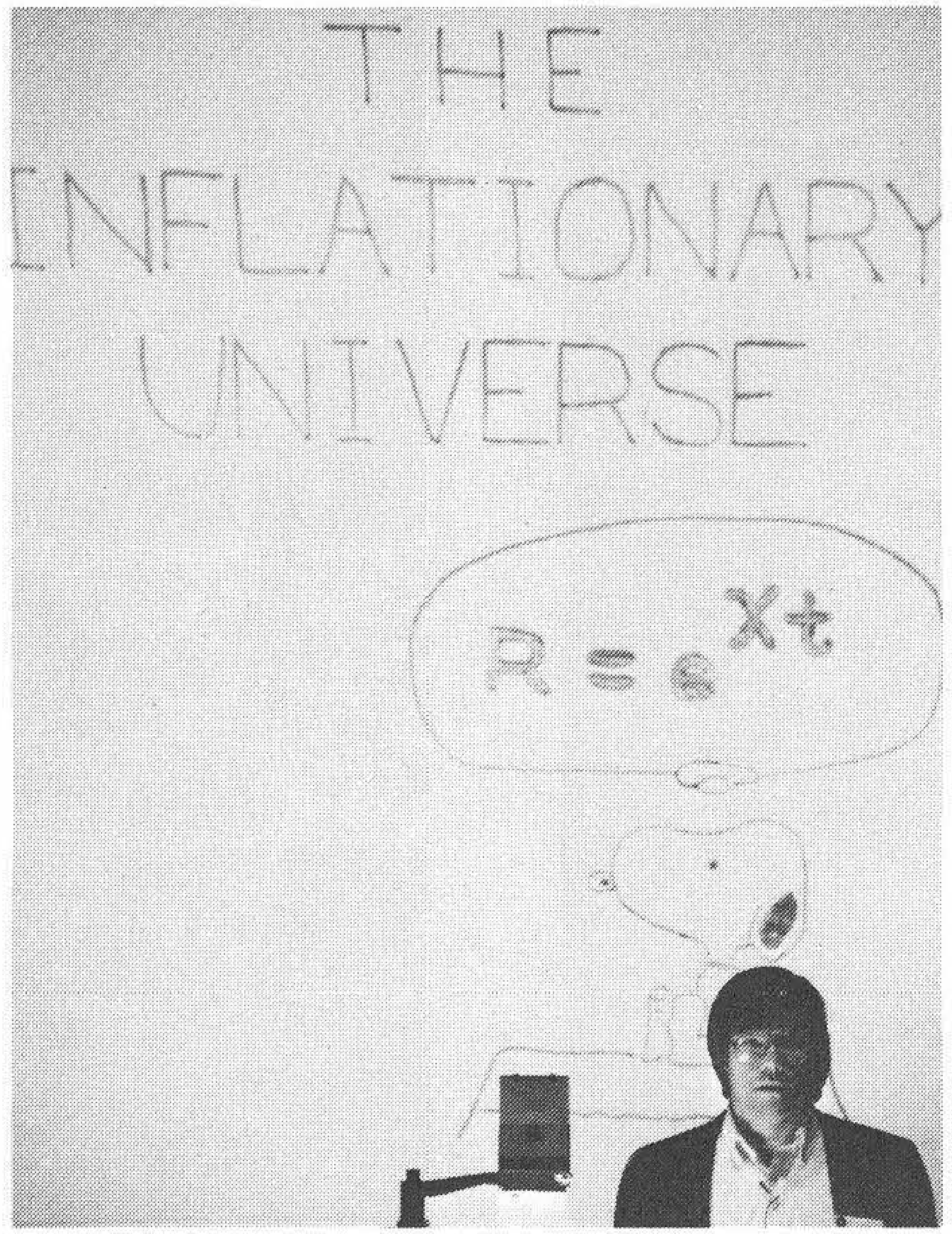
May 31, 1984
FERMI NATIONAL ACCELERATOR LABORATORY

FermiNews

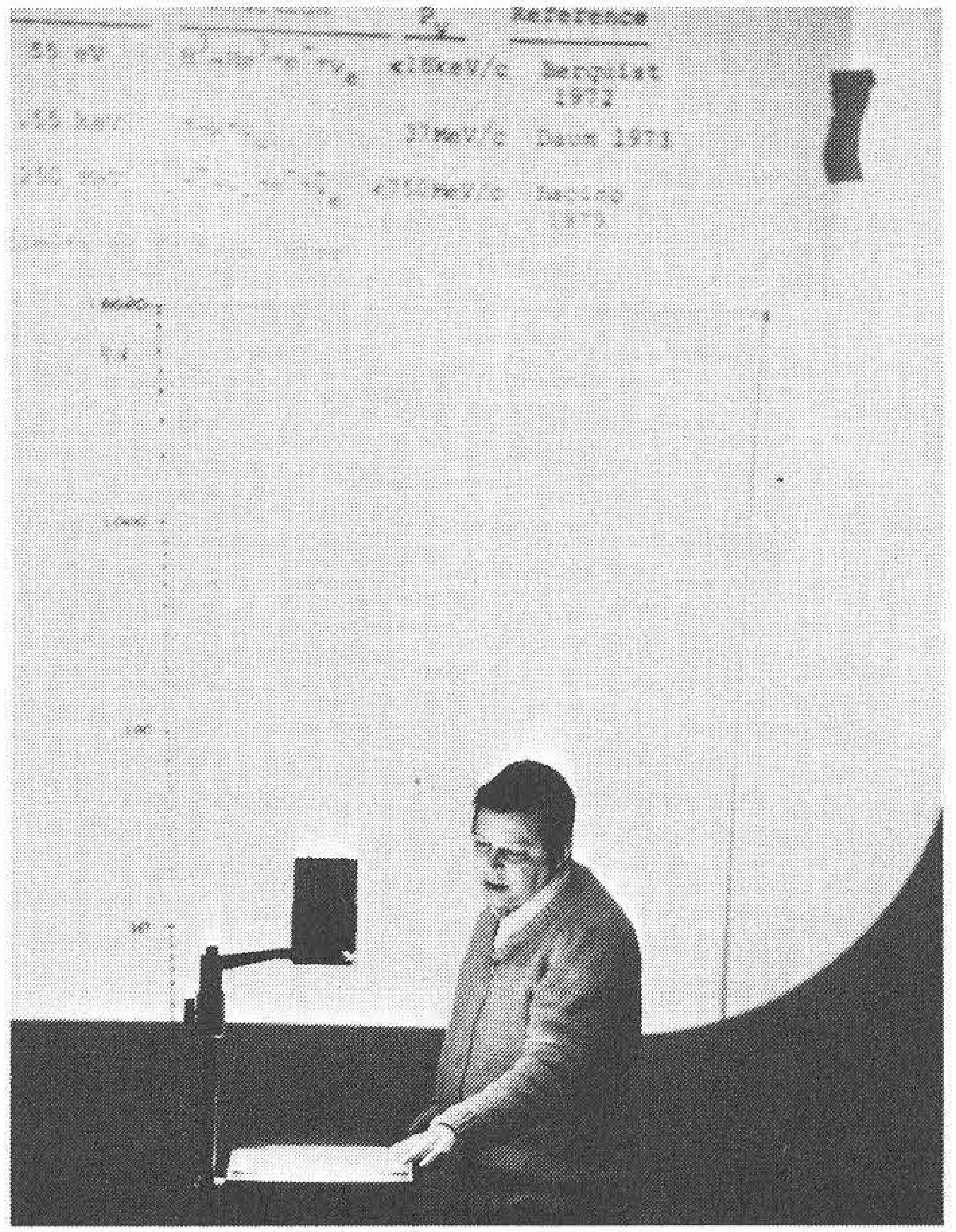
ASTROPHYSICISTS HOST SPACEY CONFERENCE



Jim "Maxwell" Bardeen, brother of theorist "Buffalo" Bill Bardeen, looks for dark matter in his conference packet.



Snoopy, with the help of Alan Guth, particle theorist/cosmologist from MIT, ponders the Inflationary Universe.



Particle Experimentalist, Frank Sciulli, from Columbia University, gave a review on Neutrino Mass/Oscillation Experiments.

by Rocky Kolb and Michael Turner

During the first week of May, the theoretical astrophysics group at Fermilab hosted an international conference on science at the interface of particle phys-

the possibility that most of the mass in the Universe resides in a yet-to-be-detected sea of elementary particles which are relics of the earliest moments of the Universe. Marc Davis (UC Berkeley) gave an observer's view of the large scale struc-

Scientific Advisory Group & Junior SAG

Director's Coffee Break

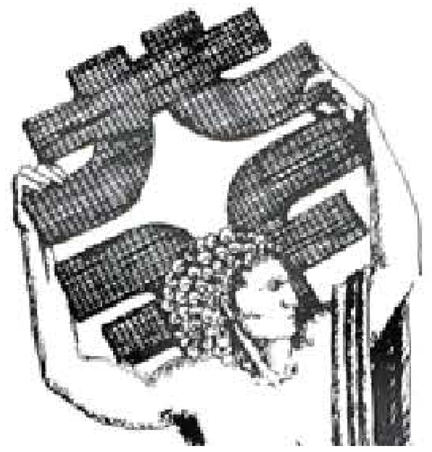
Visits to experiments, Main Control Room

Hyper-CP

"High-Energy Experiments" @ Les Houches 1981

1985–: Joint University–Fermilab Doctoral Program in Accelerator Physics and Technology

Director's Special Colloquium



Bruno Zumino
University of California
Berkeley, California

Supersymmetry,
Gravity, and Unification

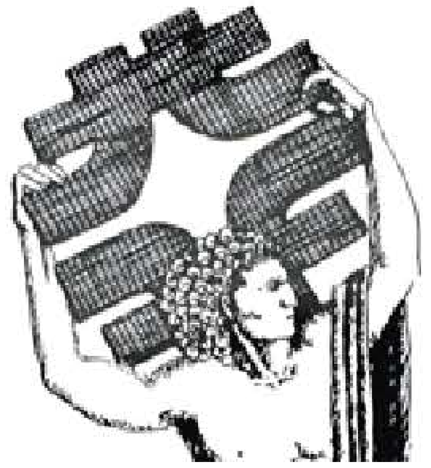
Frank Wilczek
Institute of Theoretical Physics
Santa Barbara, California

The Ongoing Search for
Dark Matter Candidates

John Schwartz

Toward a Unified Theory
Of All Interactions

Director's Special Colloquium



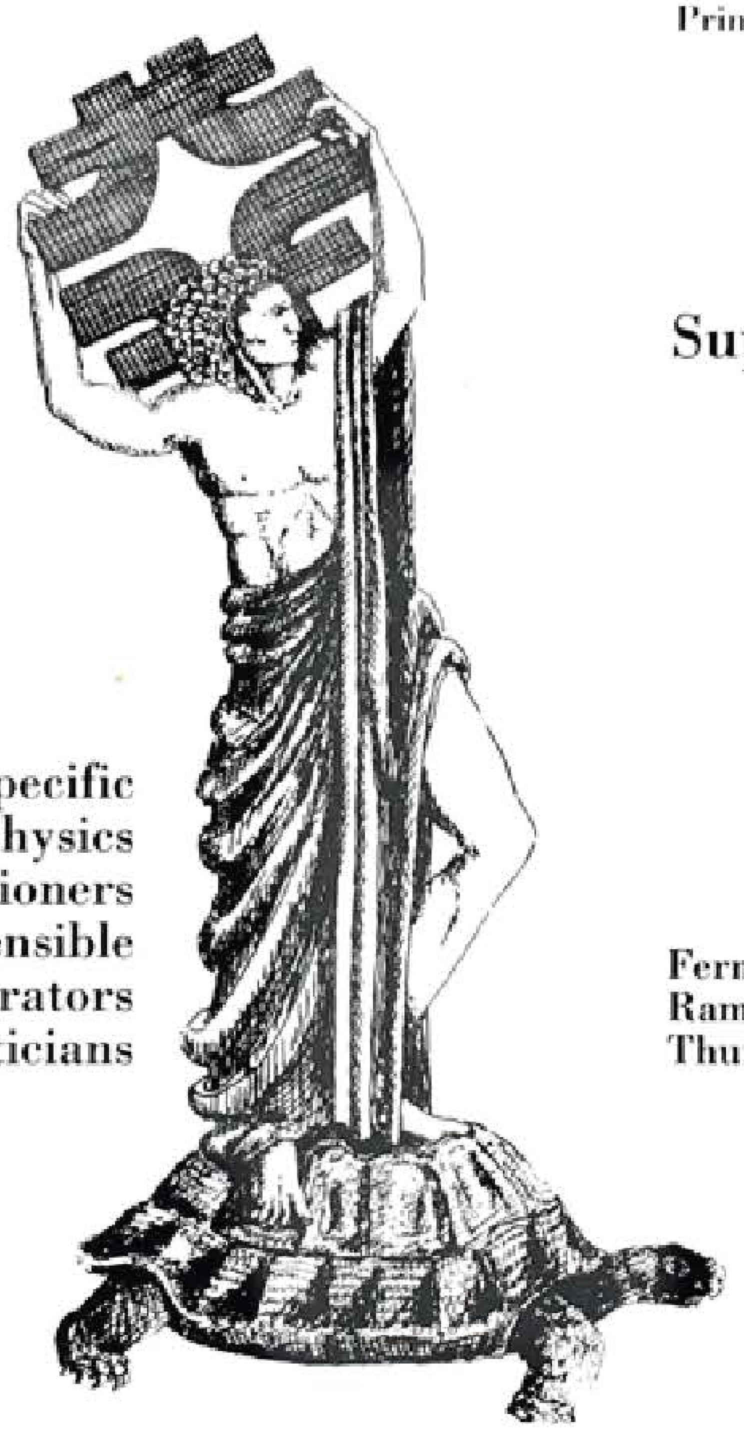
Norman Christ
Columbia University

A Supercomputer
for Lattice Gauge Theory:
Results and Prospects

Jim Cronin
University of Chicago

CP, Past, Present
and Future

Director's Special Colloquium



Being a series of colloquia on specific
topics in high energy physics
given by outstanding practitioners
and designed to be comprehensible
to graduate students, administrators
and abstract string theoreticians

Edward Witten
Princeton University

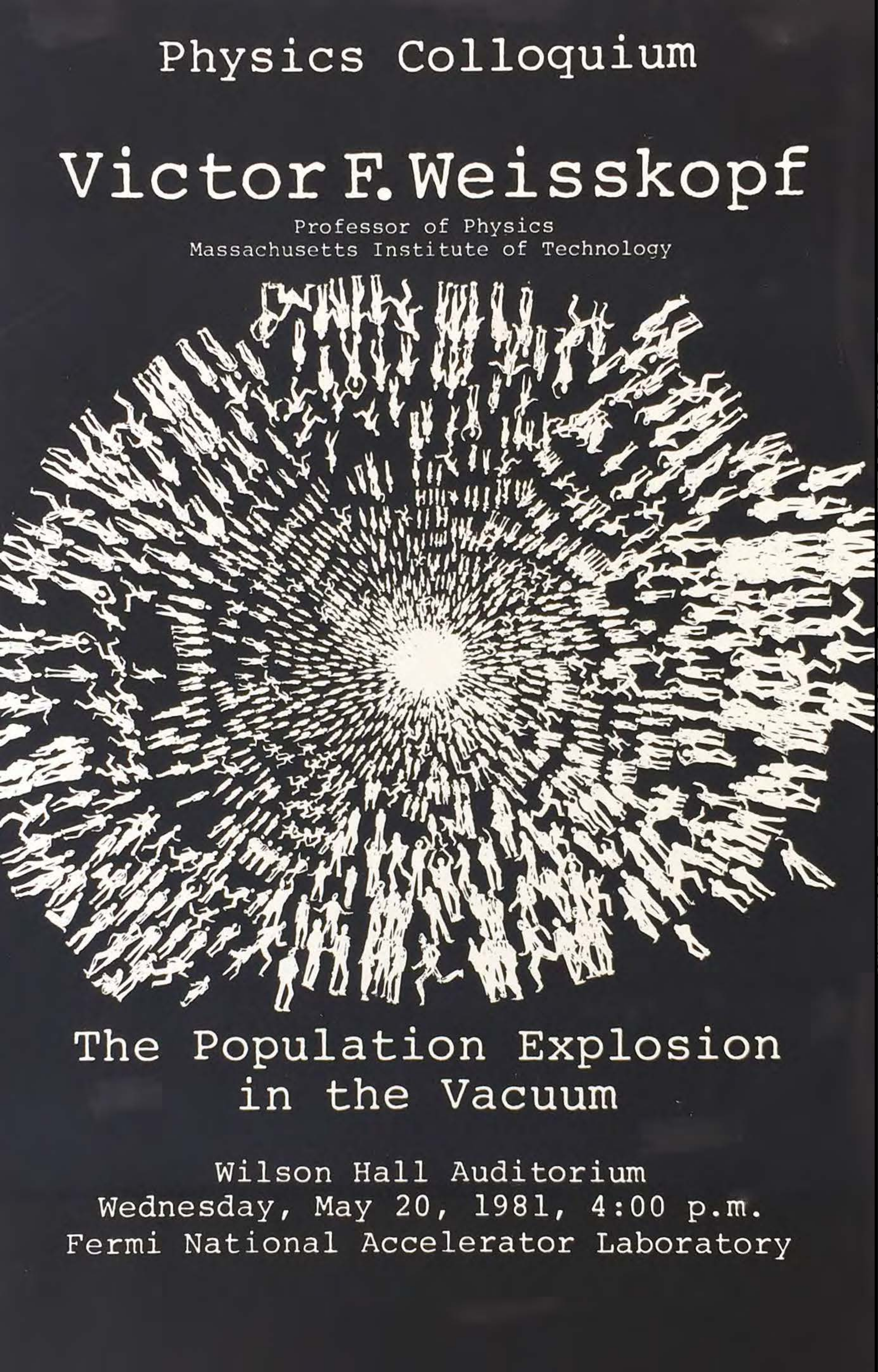
Superstring Theory

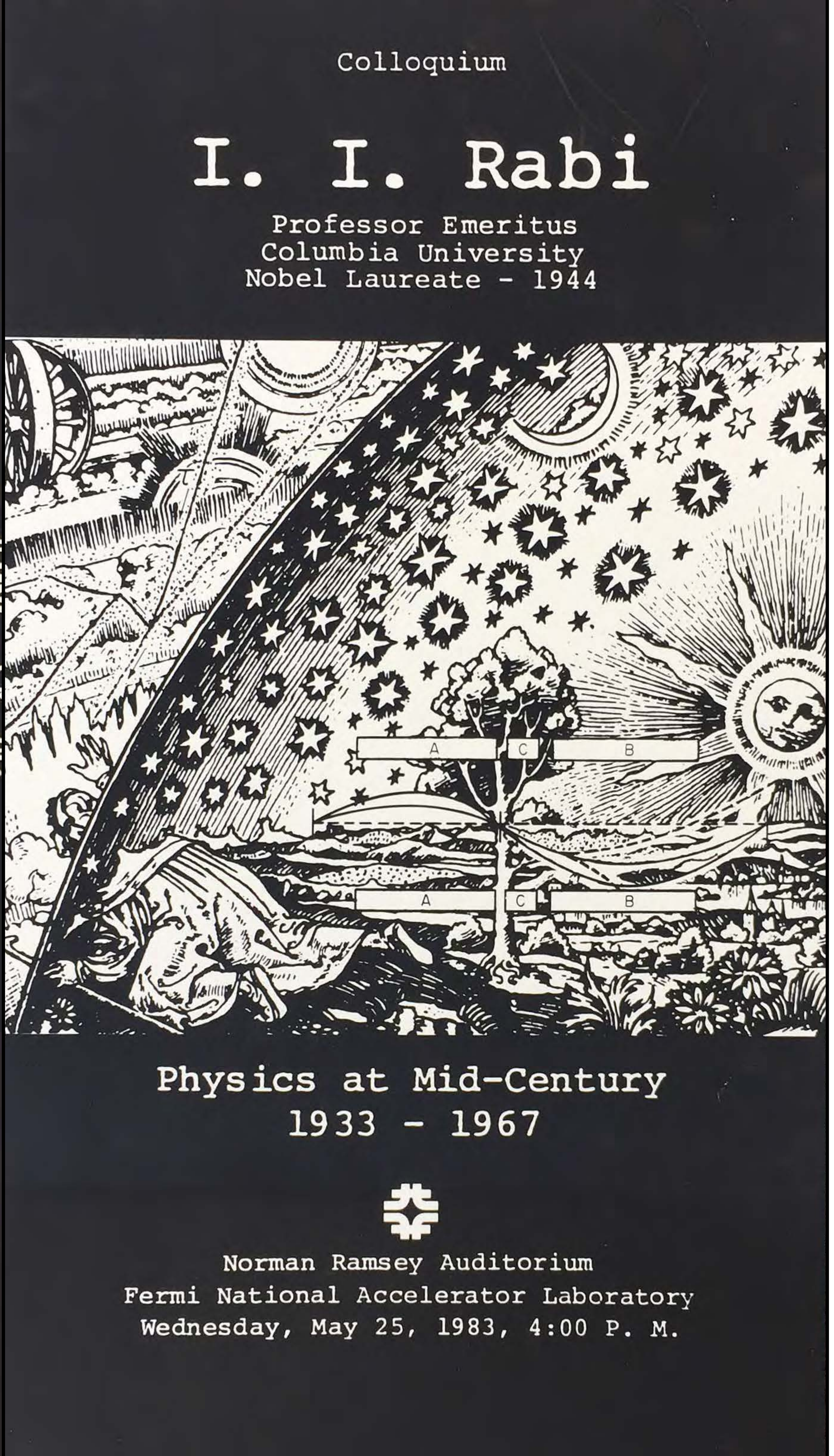
Fermi National Accelerator Laboratory
Ramsey Auditorium
Thursday, January 23, 1986, 3:00 p.m.

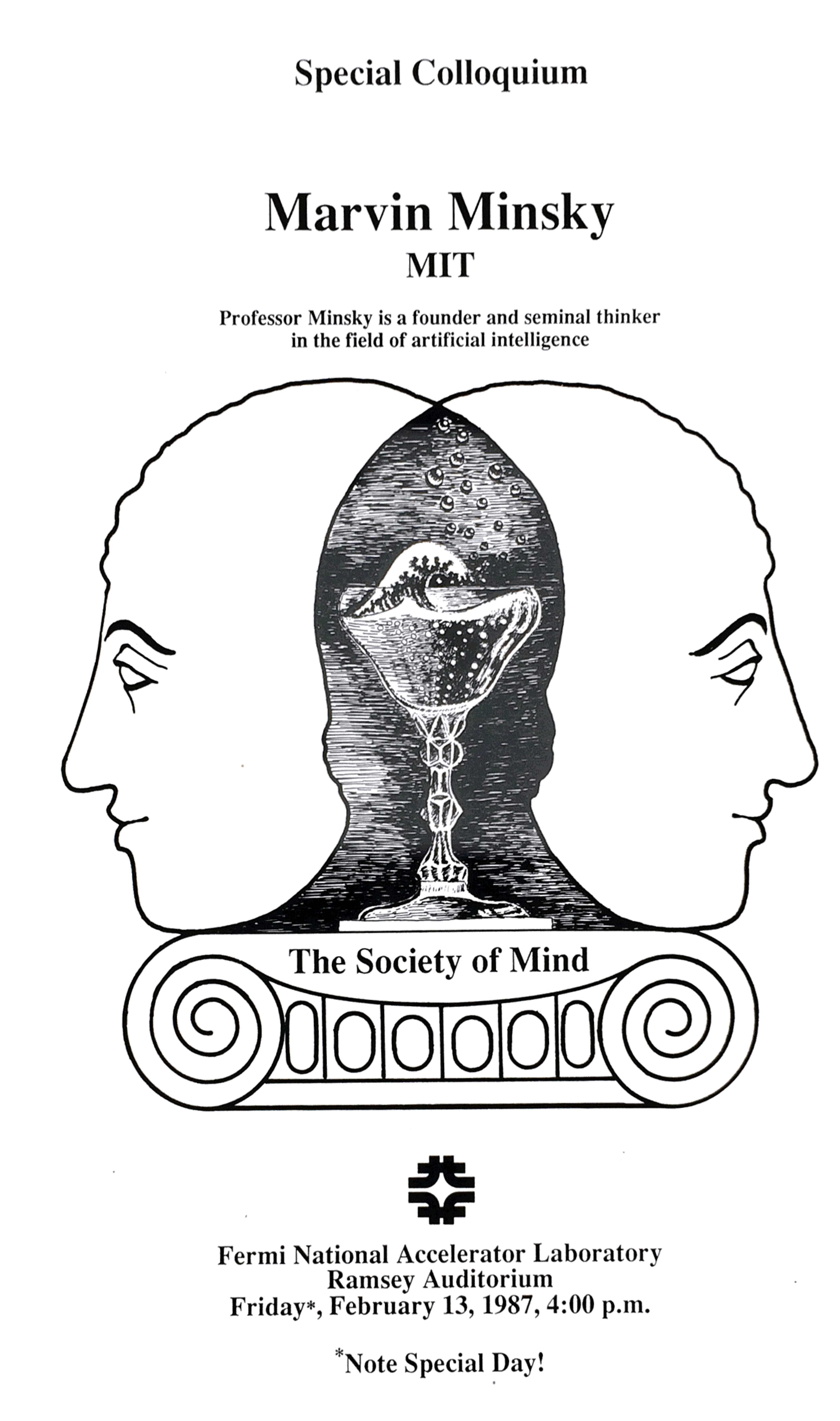
Val Fitch
Princeton University

Strange Matter

Fermi National Accelerator Laboratory
Ramsey Auditorium
Friday, February 14, 1986, 4:00 p.m.


Physics Colloquium
Victor F. Weisskopf
Professor of Physics
Massachusetts Institute of Technology
The Population Explosion
in the Vacuum
Wilson Hall Auditorium
Wednesday, May 20, 1981, 4:00 p.m.
Fermi National Accelerator Laboratory


Colloquium
I. I. Rabi
Professor Emeritus
Columbia University
Nobel Laureate - 1944
A C B
A C B
Physics at Mid-Century
1933 - 1967
Norman Ramsey Auditorium
Fermi National Accelerator Laboratory
Wednesday, May 25, 1983, 4:00 P. M.


Special Colloquium
Marvin Minsky
MIT
Professor Minsky is a founder and seminal thinker
in the field of artificial intelligence
The Society of Mind
Fermi National Accelerator Laboratory
Ramsey Auditorium
Friday*, February 13, 1987, 4:00 p.m.
*Note Special Day!

Creating the Pan-American Connection

Creating the Pan-American Connection with colleagues from South of the Border

First Pan-American Symposium on Elementary Particles and Technology
Cocoyoc (Morelos) Mexico, January 1982

Leon + J. D. Bjorken, G. Charpak, R. Feynman, S. Glashow, R. Marshak, M. Moravcsik, B. Richter, A. Tollestrup, N. Samios, W. Panofsky, R. E. Taylor and R. R. Wilson.

LML, "Fermilab and Latin America"

Creating the Pan-American Connection with colleagues from South of the Border

First Pan-American Symposium on Elementary Particles and Technology
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Leon + J. D. Bjorken, G. Charpak, R. Feynman, S. Glashow, R. Marshak, M. Moravcsik, B. Richter, A. Tollestrup, N. Samios, W. Panofsky, R. E. Taylor and R. R. Wilson.

LML, "Fermilab and Latin America"

1984: "The U.S. should offer our Latin American neighbors a massive graduate fellowship program in science and engineering."

Looking over the horizon:
From the Desertron to the SSC

1982

FERMILAB AND THE FUTURE OF HEP

L. M. Lederman
Fermi National Accelerator Laboratory*
Batavia, Illinois 60510

I. General Comments

I assumed that this DPF assembly was designed in large measure to address the issue of U.S. HEP in the "late 80's," where our last Woods Hole panel identified a need for new and exciting facilities. My initial comments are made as a citizen-physicist. Later I will put on my director's hat and discuss Fermilab's options. The scale is set by Europe where by the late 80's, they will surely have LEP, and have had six to eight years of $\bar{p}p$, and may well have HERA. By the early 1990's there will be a European capability to pave the LEP tunnel with superconducting magnets to make 1 TeV/tesla of proton acceleration, which, at 6 tesla is a 6 TeV ring. By 1990 or so, UNK (USSR) is scheduled to come on at 3 TeV for fixed target physics with collider application some years later.

These are formidable challenges and, at the same time, especially in the case of LEP, a very daring and imaginative thrust towards definitive tests of our current understanding. Considering the U.S. posture, I began to have nightmares. Dare we be any less imaginative? Are we settling into a comfortable, secondary role in what used to be an American preserve?

And what are the scientific imperatives? In my opinion, theoretical physics beyond the standard model has been treading water for several years.*

---

* "By the year 1985, the Fermilab Collider should operate at 2 TeV. It is now abundantly clear that these energies are not adequate to reveal nature's secrets at high energy. ... We need a 20 TeV hadron-hadron collider."

S. Glashow, Rome Workshop, October, 1981

"Do not ask theorists at which energy to aim for the next generation of high energy accelerators. Aim at the highest possible."

A. Salam, Paris Conference, 1982

"The outstanding problems in today's theory of particles are such that none of the projections beyond the standard model can be considered with any confidence. What we need is experimental guidance: exposure to the no man's land of lepton-lepton or quark-quark collisions up to the mass range of 1 TeV and beyond."

M. Veltmann, SLAC Accelerator Summer School, 1982.

In contemplating the late 80s, where will the breakout occur? Who will lead us to the green, intellectual pastures? In the U.S., the problem is that we have, over the past two decades, been reduced to four aging laboratories. Each of these laboratories properly does accelerator R&D in order to maximize the physics that can be realized on its site. Our history and traditions do not extend back far enough to prove that this may not be best for HEP, even for U.S. HEP. But I believe it is a dangerous situation. I happen to believe in the lessons of history (standard model or no standard model) and, therefore, in the urgency of proceeding to the next energy step, as soon as possible. This belief will and should be debated hotly. (There were theorists in the 60's that preferred a high intensity 10 GeV machine to a 200 GeV accelerator.) But just suppose I'm right and 20-40 TeV in the CM turns out to be decisive for higgs or constituent quark models or whatever. In my nightmare, I noticed that none of the four labs has a large enough site for this energy range without a great advance into the > 10 tesla supermagnet technology. This may well explain why there has not been a proposal for the great leap forward.

*Operated by Universities Research Association, Inc. under contract with the U.S. Department of Energy.

As proposals for the late 80's, all four laboratories have been pressing on projects which may not, in my opinion, provide "sufficiently bold thrusts into the unknown" and, in this sense, do not seem to me to promise to provide the excitement which draws the best and brightest. In particular, I fear that these proposals do not promise to dramatically enlarge the domain of observations when we consider the world's activities. Specifically, I believe it is important to at least examine the possibility that the machine for the late '80s be, in fact, a very bold advance. We need to ask ourselves hard, introspective questions: are we, as a community, growing old and conservative, and is there a danger of quenching the traditional dynamism we have surely enjoyed in the past three decades?

All of this led me to consider the problem: how can we break out of the aging lab and inadequate lab site constraints -- how can we creatively leapfrog the world and get to the multi TeV domain soon? The possibility of near-term (less than ~4 years) technological breakthroughs seems very remote. Our experience with SAVER magnets and the complexities of 10 tesla magnets indicates that here, again, we face a long R&D program, with no assurance that we will break through on costs (see below). We were then led to consider old technology: iron magnets with radical innovations in fabrication, mass production, installation, etc, so as to bring the costs per meter down substantially more than the ratio of magnetic fields. Since the operating costs are also relevant, the iron would have to be energized by superconductors; i.e., we are talking about an old idea, superferric magnets. Since we are now dealing with state-of-the-art systems, it seemed plausible that a 1-2 year R&D program could yield a very good assessment of the possibilities. Now, with 2-3 tesla magnets, we are talking about a very large site -- clearly a new laboratory which would become the U.S. High Energy Lab. It would have to contain a ring of ~15-30 Km radius, and if shallow trenching (instead of conventional tunnels) is the mode, then the site must be very flat, sparsely populated, yet near a good, international airport. Hence the accolade, "Machine-in-the-desert."

1982

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M. Veltmann, SLAC Accelerator Summer School, 1982.

In contemplating the late 80s, where will the breakout occur? Who will lead us to the green, intellectual pastures? In the U.S., the problem is that we have, over the past two decades, been reduced to four aging laboratories. Each of these laboratories properly does accelerator R&D in order to maximize the physics that can be realized on its site. Our history and traditions do not extend back far enough to prove that this may not be best for HEP, even for U.S. HEP. But I believe it is a dangerous situation. I happen to believe in the lessons of history (standard model or no standard model) and, therefore, in the urgency of proceeding to the next energy step, as soon as possible. This belief will and should be debated hotly. (There were theorists in the 60's that preferred a high intensity 10 GeV machine to a 200 GeV accelerator.) But just suppose I'm right and 20-40 TeV in the CM turns out to be decisive for higgs or constituent quark models or whatever. In my nightmare, I noticed that none of the four labs has a large enough site for this energy range without a great advance into the > 10 tesla supermagnet technology. This may well explain why there has not been a proposal for the great leap forward.

As proposals for the late 80's, all four laboratories have been focusing on projects which may not, in my opinion, provide "sufficiently bold thrusts into the unknown" and, in this sense, do not seem to me to promise to provide the excitement which draws the best and brightest. In particular, I fear that these proposals do not promise to dramatically enlarge the domain of observations when we consider the world's activities. Specifically, I believe it is important to at least examine the possibility that the machine for the late '80s be, in fact, a very bold advance. We need to ask ourselves hard, introspective questions: are we, as a community, growing old and conservative, and is there a danger of quenching the traditional dynamism we have surely enjoyed in the past three decades?

All of this led me to consider the problem: how can we break out of the aging lab and inadequate lab site constraints -- how can we creatively leapfrog the world and get to the multi TeV domain soon? The possibility of near-term (less than ~4 years) technological breakthroughs seems very remote. Our experience with SAVER magnets and the complexities of 10 tesla magnets indicates that here, again, we face a long R&D program, with no assurance that we will break through on costs (see below). We were then led to consider old technology: iron magnets with radical innovations in fabrication, mass production, installation, etc, so as to bring the costs per meter down substantially more than the ratio of magnetic fields. Since the operating costs are also relevant, the iron would have to be energized by superconductors; i.e., we are talking about an old idea, superferric magnets. Since we are now dealing with state-of-the-art systems, it seemed plausible that a 1-2 year R&D program could yield a very good assessment of the possibilities. Now, with 2-3 tesla magnets, we are talking about a very large site -- clearly a new laboratory which would become the U.S. High Energy Lab. It would have to contain a ring of ~15-30 Km radius, and if shallow trenching (instead of conventional tunnels) is the mode, then the site must be very flat, sparsely populated, yet near a good, international airport. Hence the accolade, "Machine-in-the-desert."

*Operated by Universities Research Association, Inc. under contract with the U.S. Department of Energy.

A PRIMER ON DETECTORS IN HIGH LUMINOSITY ENVIRONMENT

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I. History

The following remarks are relevant to the problem of balancing luminosity versus energy in new HEP construction.

In a 1973 Isabelle Summer study,[1] it was stated that the only experiment that would succeed at a luminosity of 10^{33}cm^{-2}sec^{-1} was one in which the apparatus was shielded from the collision region by massive quantity of steel. In 1981, this opinion was confirmed by an authority no less than S.C.C. Ting.[2] It may be instructive to review the progress of collider detectors over the past decade. In 1973, the time resolution or, better, the integrating time of tracking detectors was ~100 ns. In 1982, this time has remained the same since PWC's are still the fastest tracking devices available. The fundamental limit is the saturated drift velocity of electrons in gases. Better resolution and three dimensional properties have led to the choice of drift chambers and TPC's which have considerably longer integration times. A new characteristic of 1982 detectors is the increasing pervasiveness of calorimeters which have become indispensable devices for measurement of electromagnetic and hadronic energy, especially at momenta where magnetic measurements become imprecise. Calorimeters, because of their innate geometric dimensions set by the nuclear mean free path and their distance from the interaction point have integration times of ~200-1000 ns. Of course this is the present state of the art which depends on the properties of BBQ, gas chambers, liquid argon, lead glass, etc.

The conclusion is that things have only gotten worse since 1973.

II. Integration Time - Tracking

What are the implications of long integration times? We are facing collision energies so high that the charged and neutral multiplicities, $\bar{M}$ average about 60 particles near 1 TeV. These typical multiplicities have surprisingly large fluctuations, such that Gaussian or Poisson statistics do not apply.[3] For example, the probability of having 2 $\bar{M}$ particles is one quarter that of having $\bar{M}$ particles. A track detector that integrates over, say, N events (with its integrating time of ≥100ns) must add N times the average multiplicity to the number of particles in the triggering event. If this is a typical hard collision it may well have a track multiplicity many times higher than the average multiplicity.[3] At 10^{33}cm^{-2}sec^{-1}, ±100ns integrates over an average of 10 events. If each event generates an average of 30 charged[3] particles (and ~30 neutral particles) one must add an average of 300 particles to the trigger induced event. Not all of these will conveniently stay in the beam pipe. (See typical events attached.) According to UA1[3] an average of 50 particles enter the central calorimeter at $\sqrt{s}$ = 540 GeV in minimum bias events. Many others will strike flanges, supports, pole pieces, etc. and shower with very high multiplicities, the end products of which give rise to noise or albedo, i.e., single hits in detectors or random tracks. This has severe implications for tracking efficiency; there is in fact a fair likelihood that these high multiplicities will render any of the tracking devices, as we now understand them, inoperable. PWC's have operated at ambient singles rates of 10 Mcps with fairly simple track configurations. However, experience with 20-30 tracks, e.g., at the ISR's Split Field Magnet or at various multiparticle spectrometers suggest a CDC 7600 CPU analysis time per event of hundreds of milliseconds up to ~5 sec! To contemplate the functioning of a track chamber with several hundreds of tracks, many of low and "curling" energies (even given scintillation tagging) clearly requires a major advance. As a dramatic example, look at Fig. 1 and imagine superposing 2, 3 or 5 such events in a single trigger.

We should note that before one can reject tracks for pointing incorrectly one must be able to do the pattern recognition. A more quantitative tabulation of the influence of finite integrating time is presented in Tables I and II.

III. Calorimetry

To this tale of woe we must add the problem of the calorimeters. Now we have ~30 charged and 30 neutral particles incident upon the calorimeter which has an optimistic integrating time of ±200ns. This is at ~1 TeV. Multiplicities will about double at 10 TeV. It is true that a typical event may add negligibly to a (say) 100 GeV/c transverse momentum trigger. Some fraction of good events would be confused by the integration, but it is also clear that a large enough number of random accumulations of 10 or 20 minimum bias events can generate fake physics. These may provide a background for a large fraction of the anticipated physics signatures. During the interval between real 100 GeV/c jets say (at the rate of 10 per day) there would be ~5x10^{11} accumulations of twenty random events! If each charged particle generates a transverse energy of 500 Mev[3] and each photon 250 Mev, a minimum bias event produces an average of ~20 GeV of E_t. Twenty events yields 400 GeV!! Gating may reduce this to ~200 GeV. A patient Monte Carloist can decide how often these will fluctuate and cluster so as to fake a PT = 100 GeV/c event. However, this intrepid soul must be sure he is using the correct distribution function for fluctuations and the "typical" minimum bias trigger. This does assume either a breakthrough in tracking or, more likely, ability to see jets without tracks.

IV. Current State of the Art

There is ample data from 1982 experiments that support this pessimism. Charm was discovered in 1975. In spite of eight years and three generations of experiments at Fermilab, ISR, SPS and AGS the total number of clear charm events observed in hadron collisions is about one hundred! Nevertheless, literally millions of charmed particles were produced in the targets of the dozens of experiments looking for charm. It is obviously even worse for bottom mesons. Why? The primary problem is that the hadronic production cross section is less than 0.1% of the total cross section. Then, high (5-10 tracks) multiplicities, combinatorials, backgrounds, i.e., the

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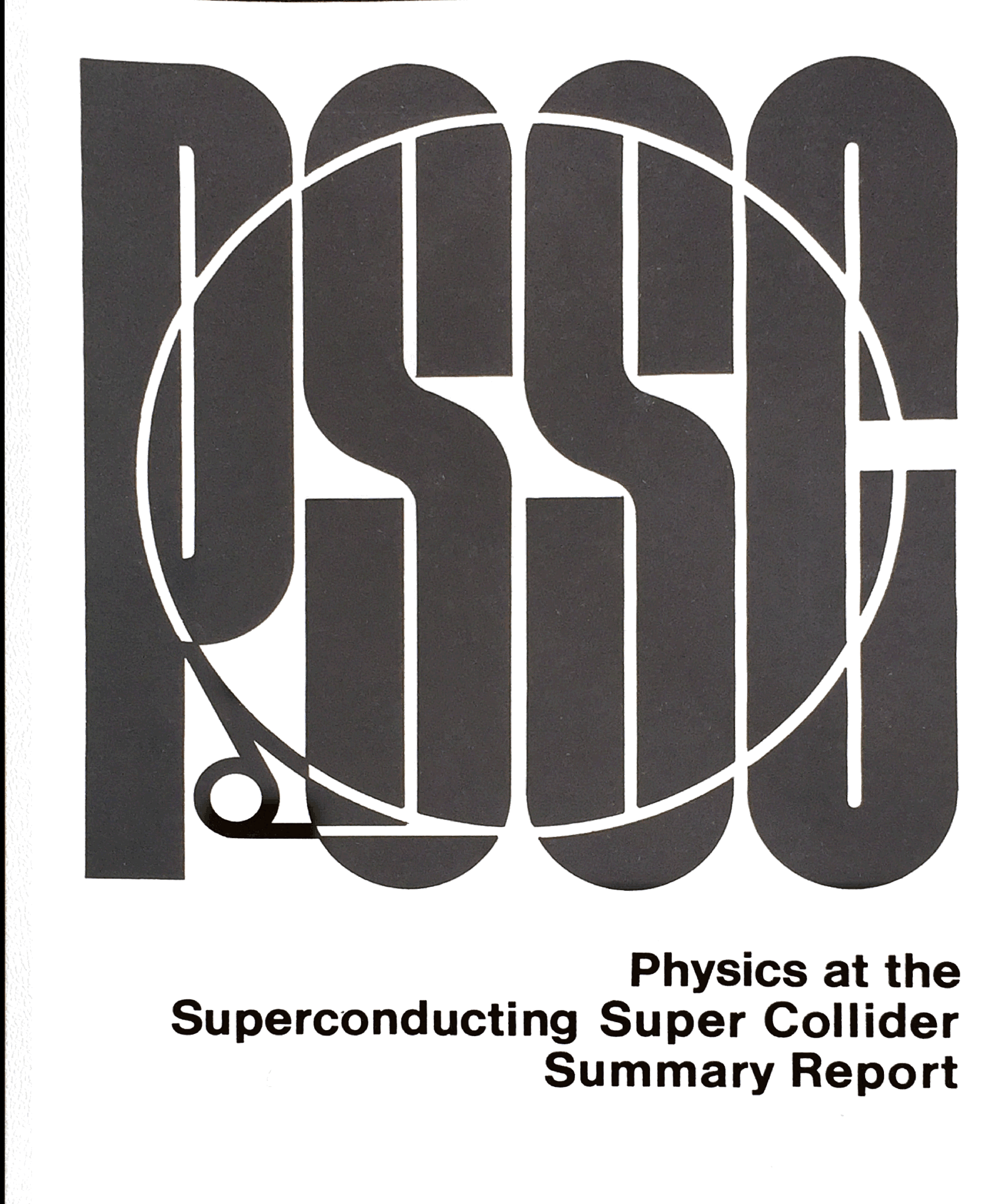
PSSC
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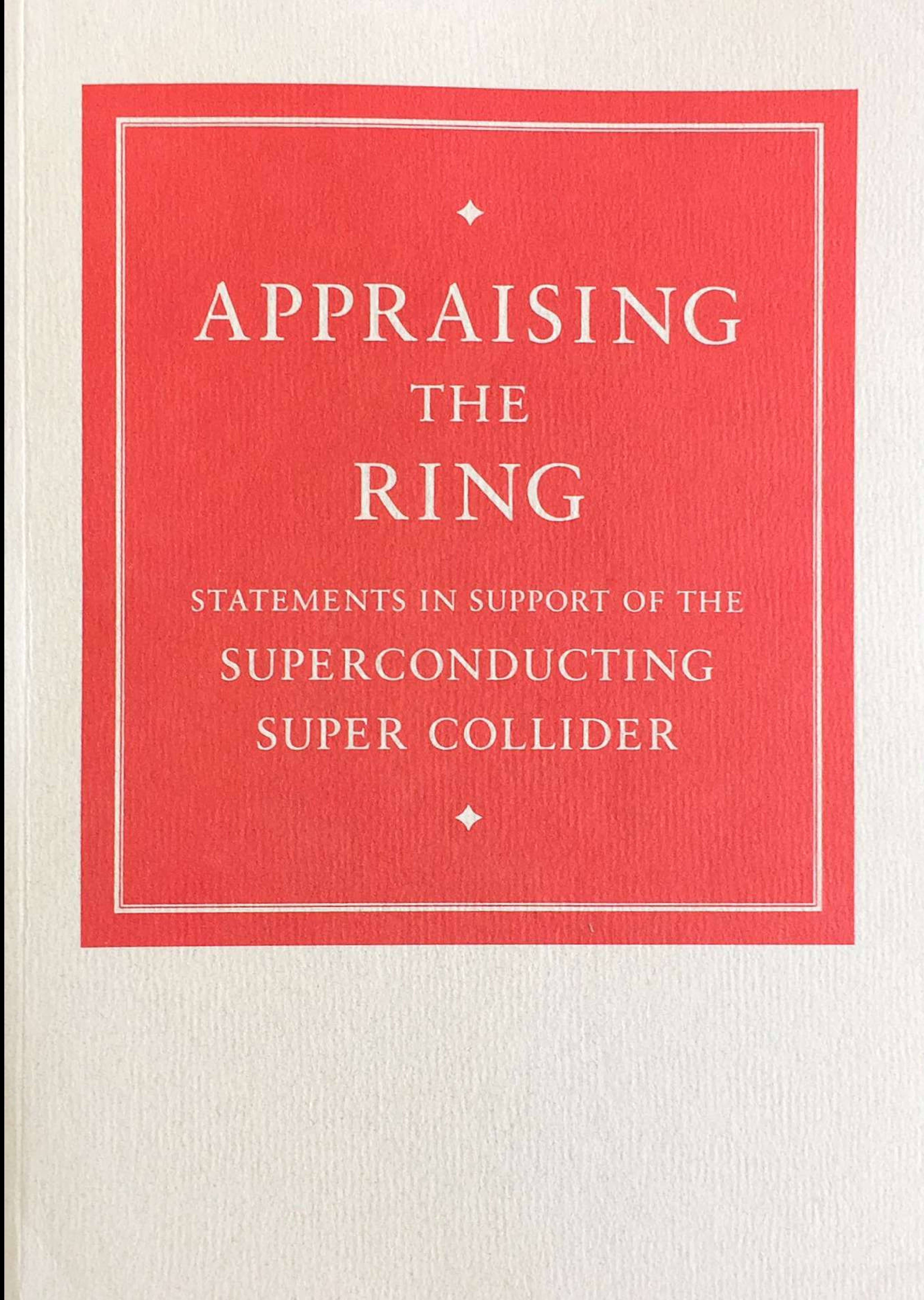
Superconducting Super Collider
Summary Report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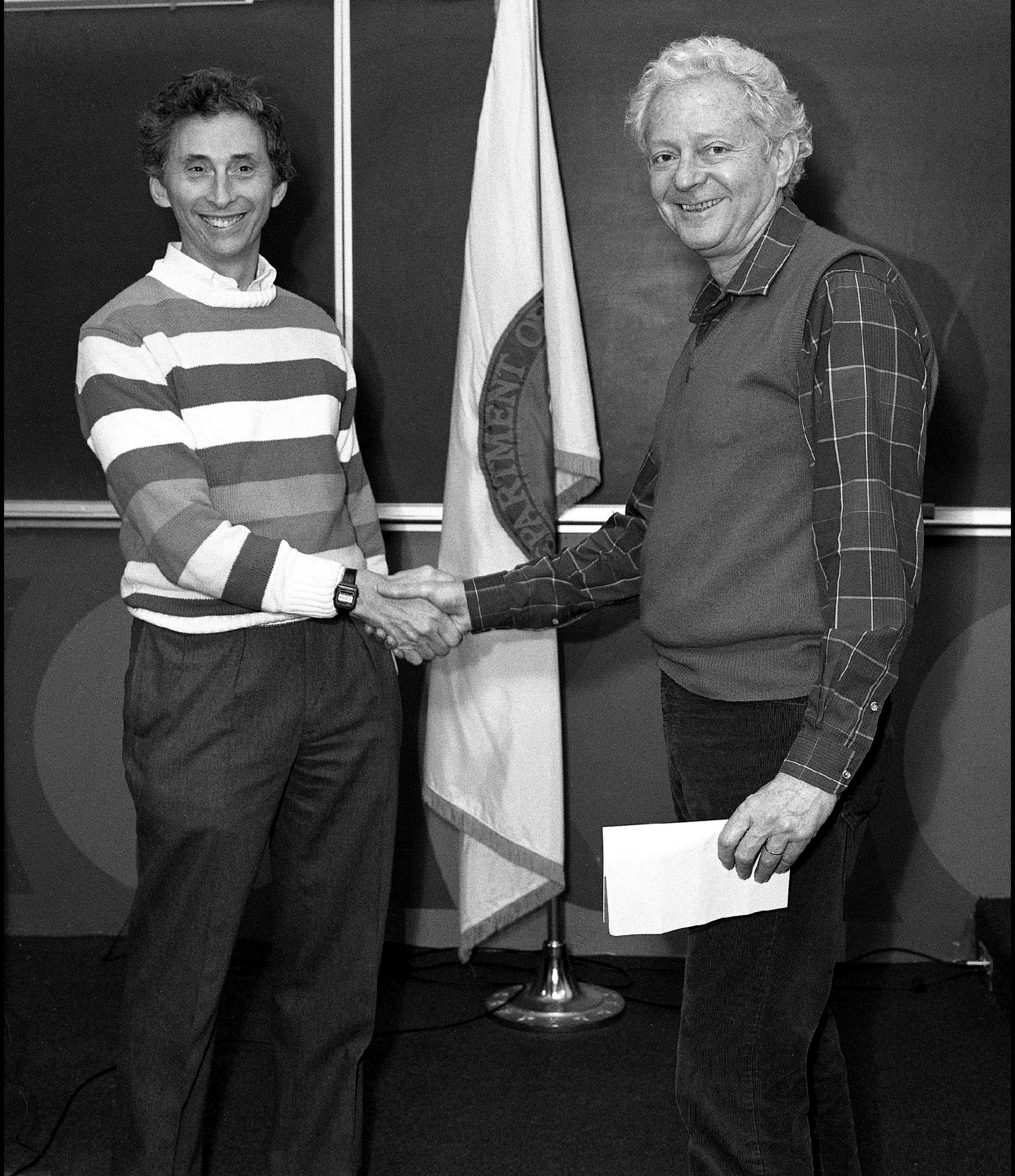
Hats Off to Waxahachie

Passing the baton

"Ten years is a good round number."



Savoring the afterlife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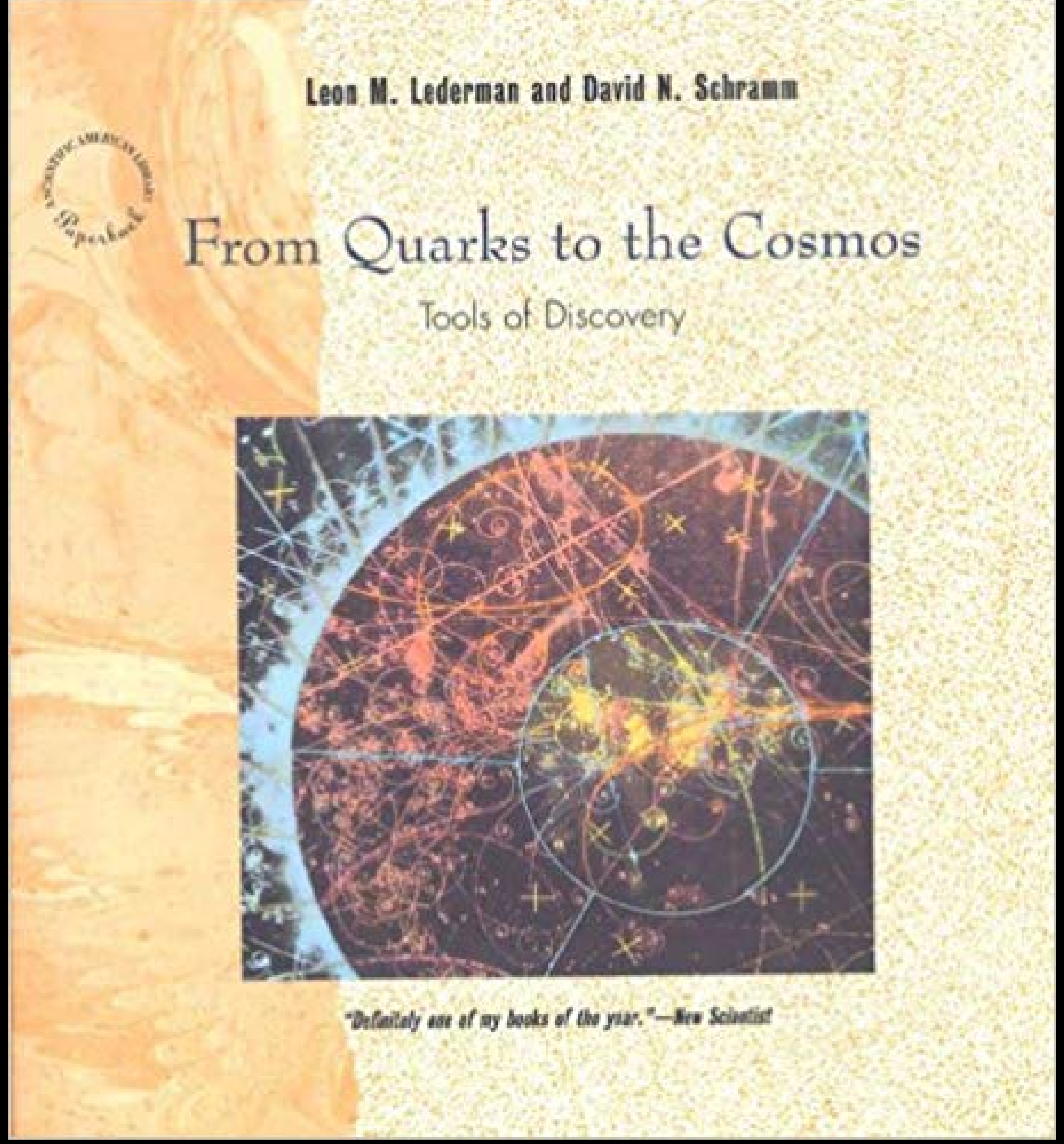
+ IMSA, University of Chicago, IIT, AAAS, …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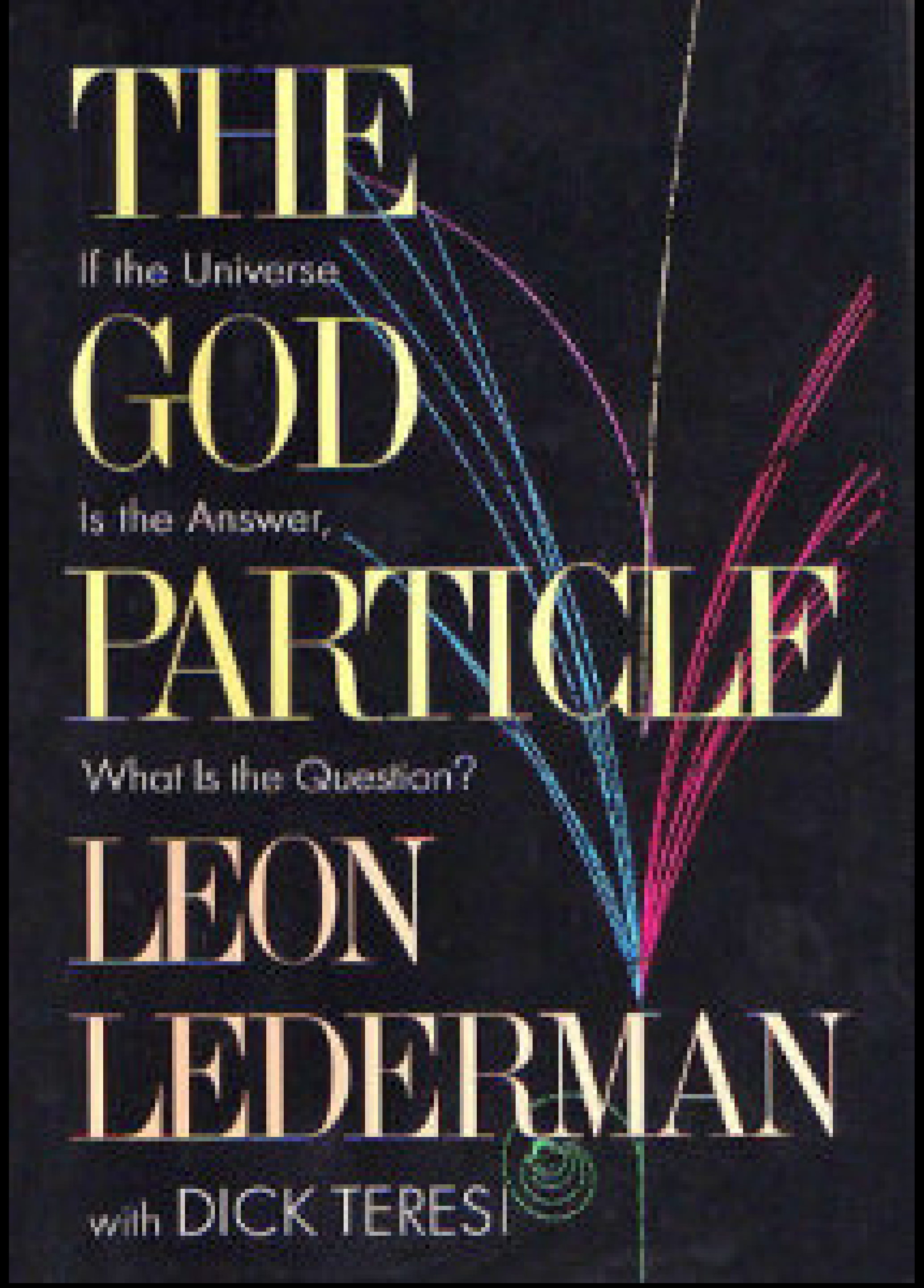
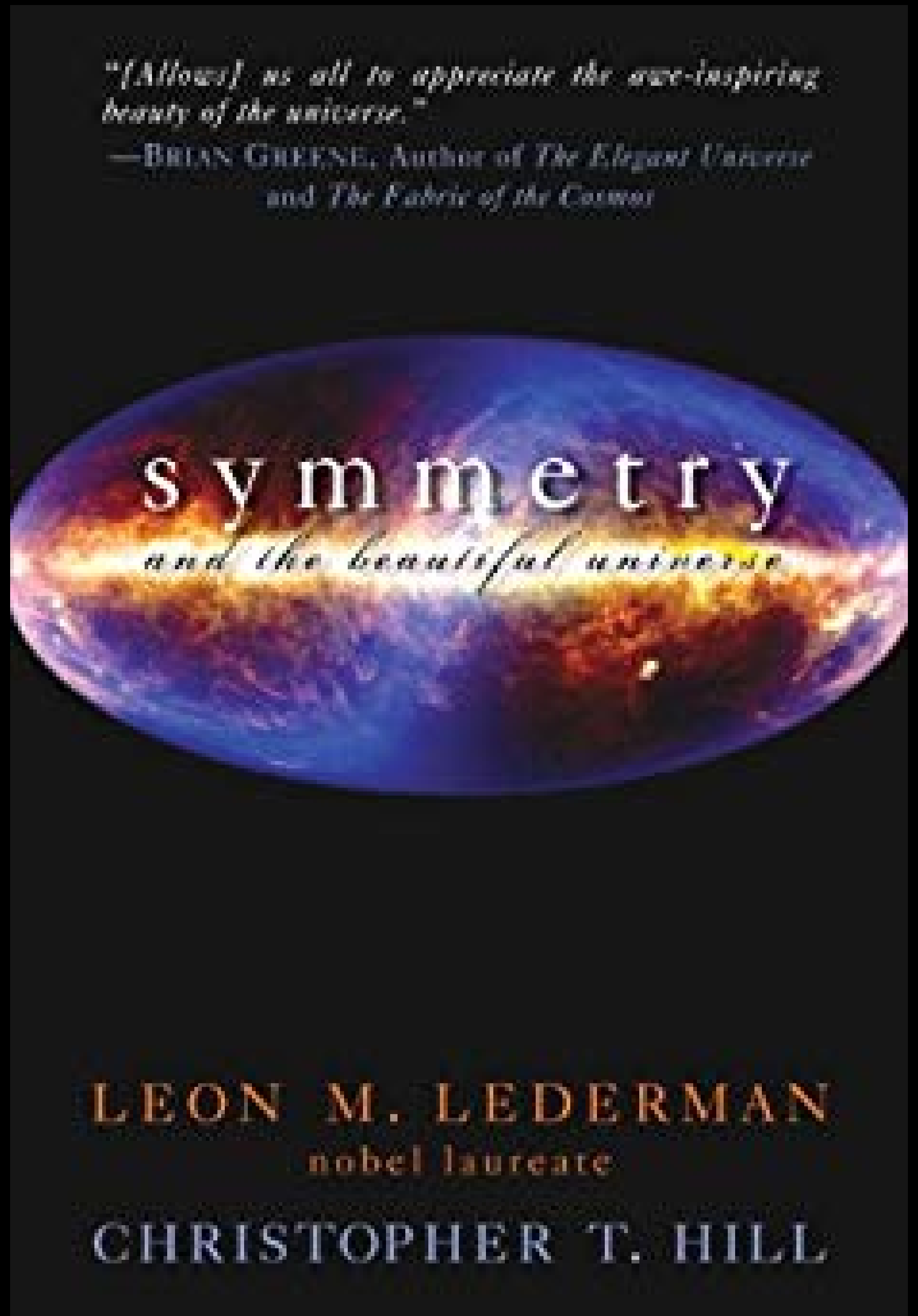
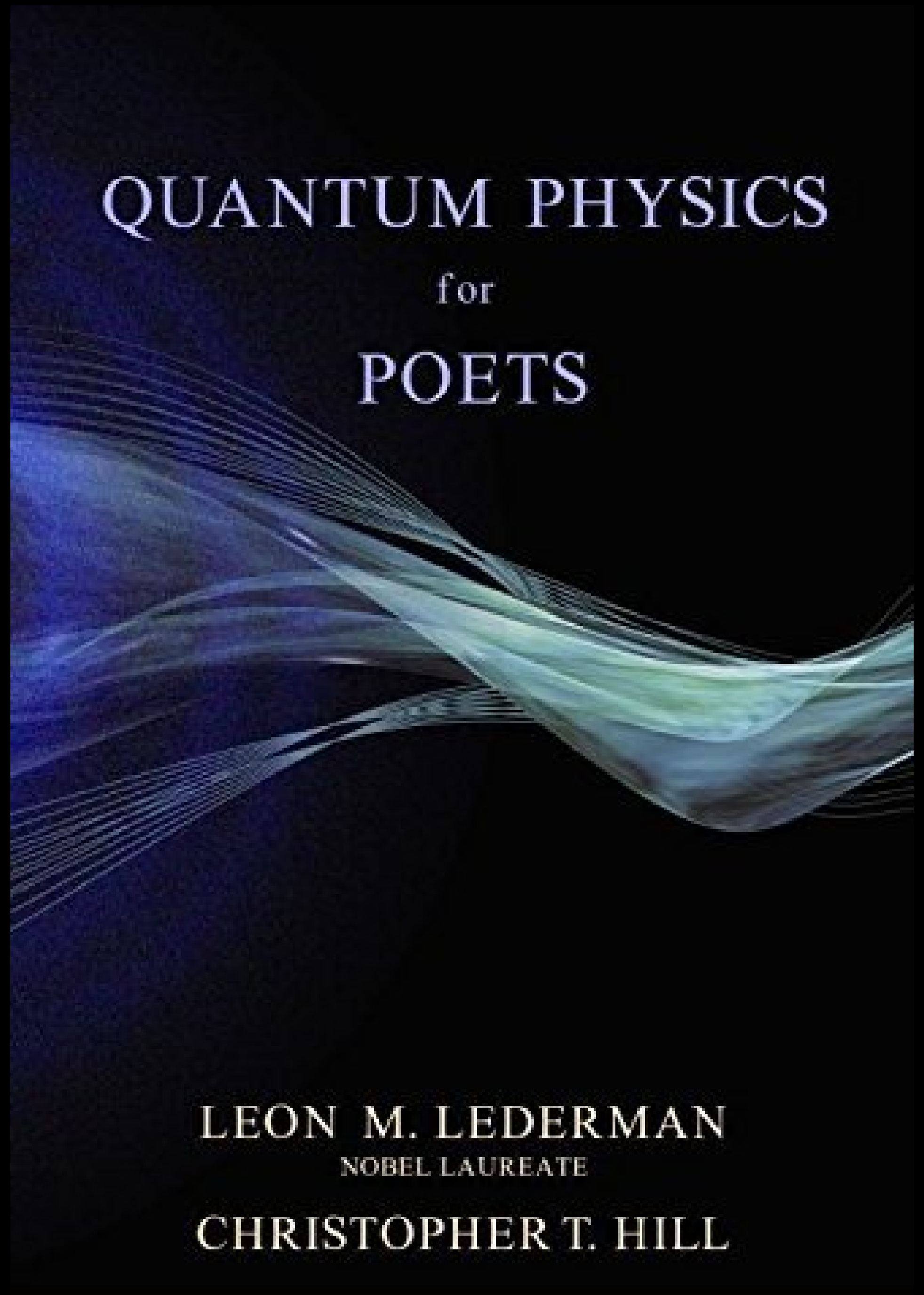

THE
If the Universe
GOD
Is the Answer,
PARTICLE
What Is the Question?
LEON
LEDERMAN
with DICK TERESI


"[Allows] us all to appreciate the awe-inspiring beauty of the universe."
—BRIAN GREENE, Author of The Elegant Universe and The Fabric of the Cosmos
symmetry
and the beautiful universe
LEON M. LEDERMAN
nobel laureate
CHRISTOPHER T. HILL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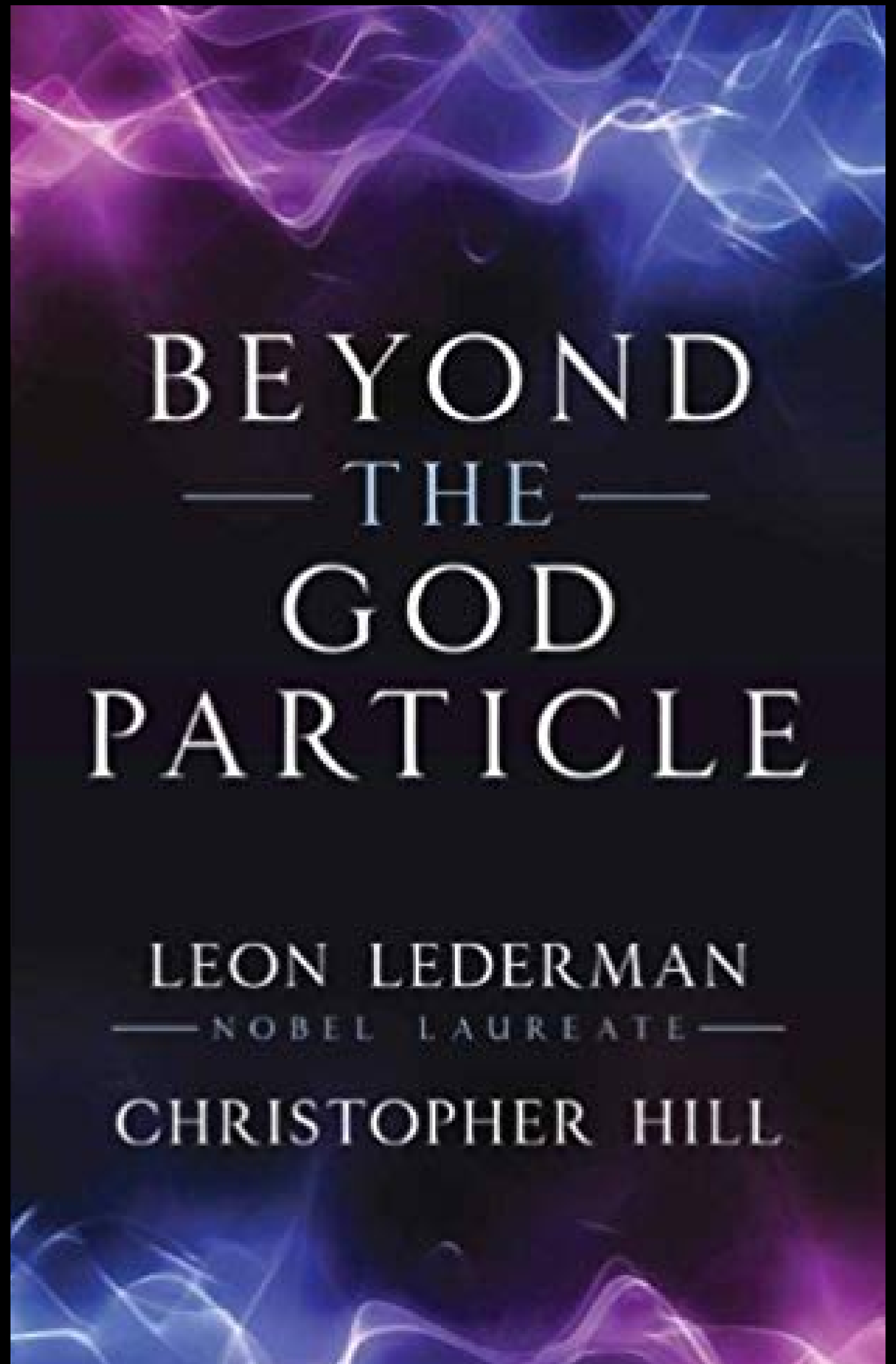
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Delighting in talent


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Energy
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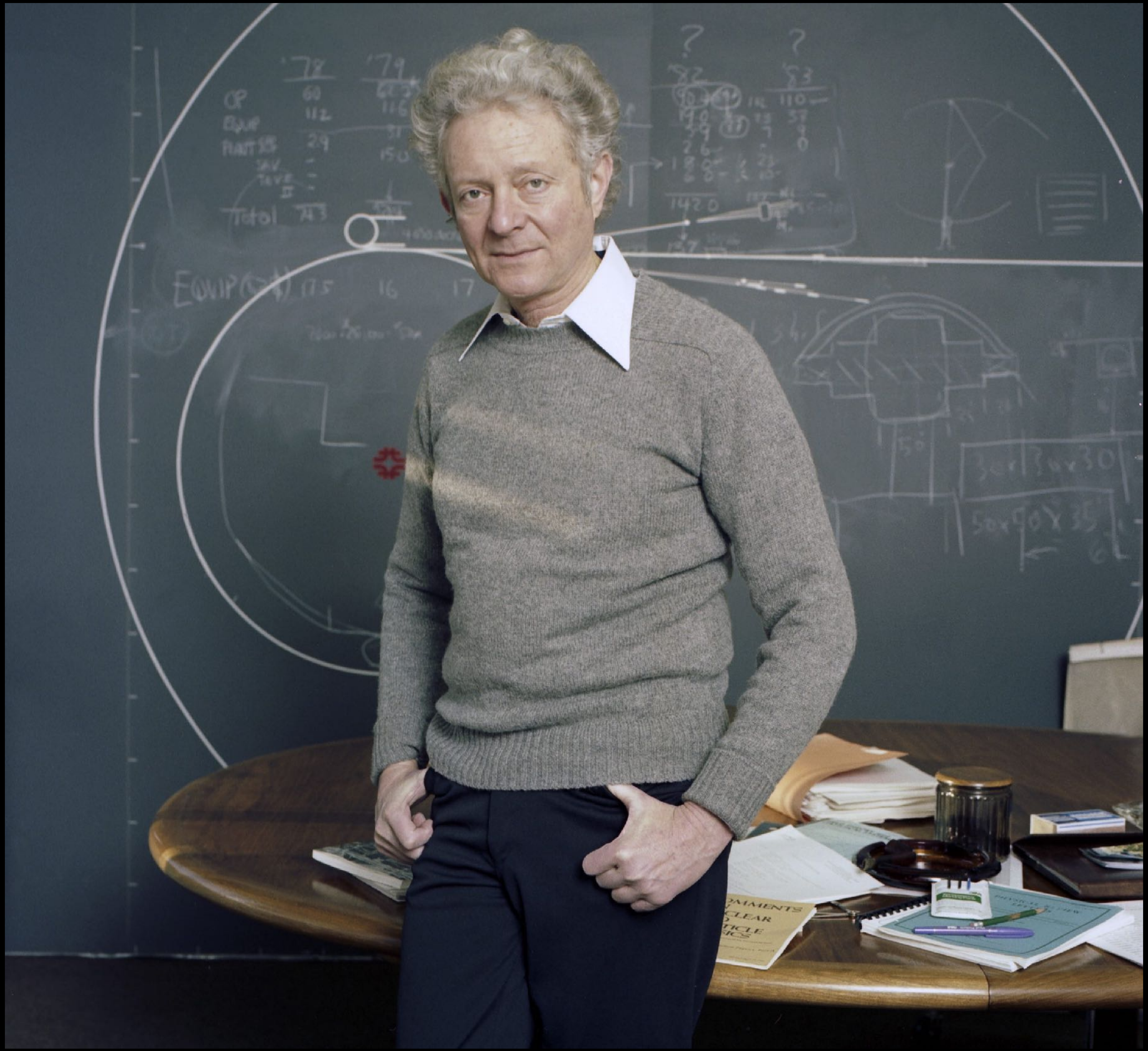
for next week
Time for the Stars.
Field
T = 29 days

50th Saturday Morning Physics Graduation, 1996

In Leon's company, it seemed that anything might be possible.



Thank you, Leon!

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