AESTHETICS AND SCIENCE

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PROCEEDINGS OF THE
INTERNATIONAL SYMPOSIUM
IN HONOR OF ROBERT R. WILSON
APRIL 27, 1979

FERMI NATIONAL ACCELERATOR LABORATORY

BATAVIA, ILLINOIS

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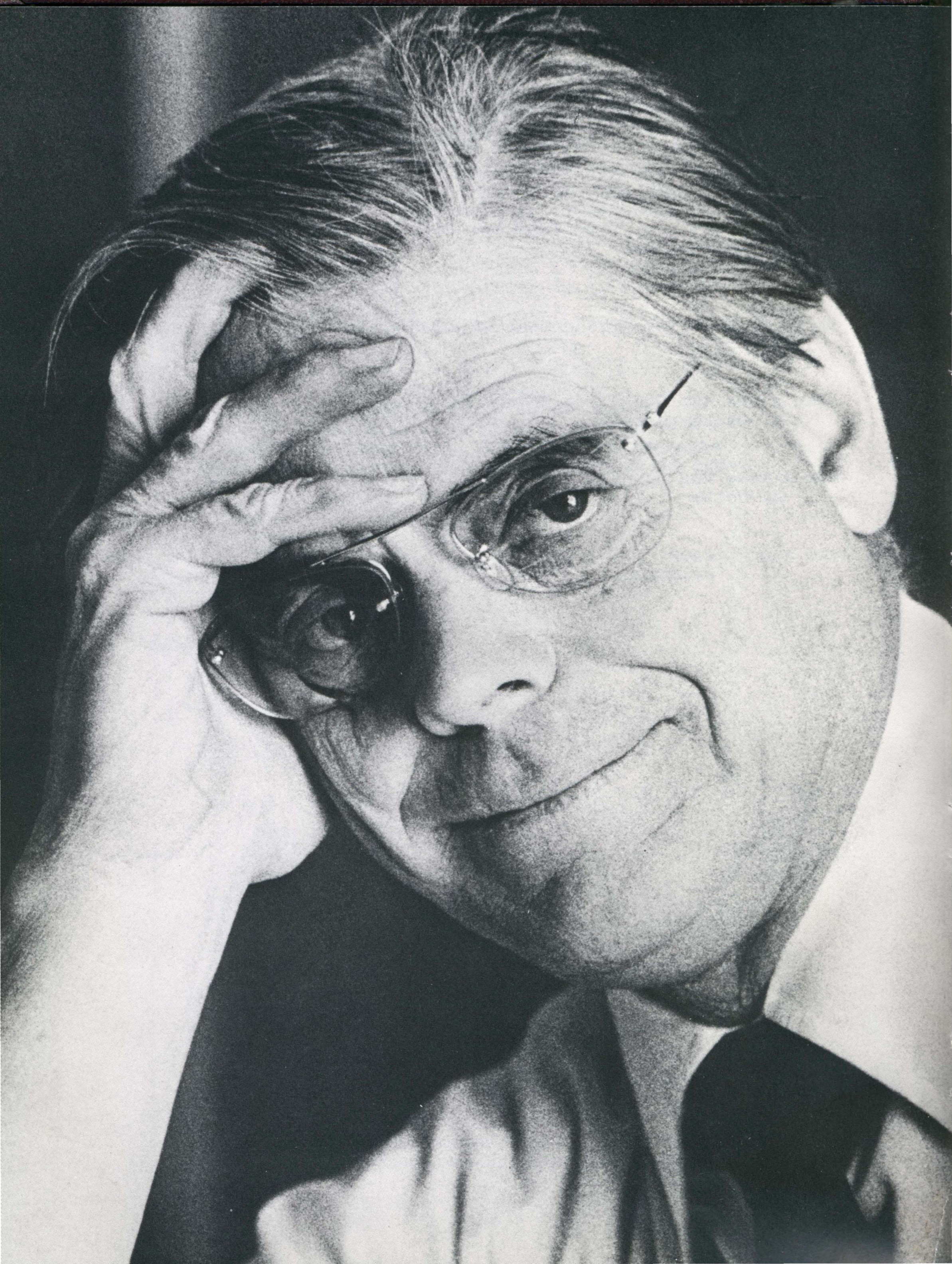


Preface

On April 29, 1979, 450 friends from many parts of the world gathered to honor Robert R. Wilson at a symposium held at Fermilab. The symposium was arranged following a spontaneous desire by many groups and individuals to give recognition to the achievements of Robert Wilson. This volume contains the lectures presented at the symposium. The subjects of the lectures are diverse. Their unity can be found in the diversity of interests and accomplishments of the man they honor.

The dedication of Angela Gonzales and Rene Donaldson in the preparation of this volume is gratefully acknowledged.

James W. Cronin
for the Symposium Committee

Wilson And Fermilab

L. M. Lederman

I'm terribly sorry that I was introduced by Phil Livdahl. I was expecting an introduction by Norman Ramsey. In that case you know I would have another half hour to prepare my talk. I always find in this place I'm running out of time. Symposia in honor of some great scientist, great heroes, festschrifts, retirement parties, etc., are always nerve wracking enterprises. Speakers are in a delicate situation. They have to praise the hero, they have to list his great accomplishments, his honors, the overwhelming difficulties he surpassed. They have to tell hysterically funny anecdotes about things he did and after a certain amount of this there is always the danger some small kid in the audience will say, "Mom, where's the casket?" and a nervous response, "Shhhh, he's still moving." I want to assure you that this symposium is going to be different. You may hear from the speakers coming after me (I'm not responsible for what they say) some praise for Robert Wilson but you won't hear any praise from me, no sir! If Wilson can't do any better in the next years than he's done in the past he's in plenty of trouble. Now I have to confess to a certain prejudice because for the past twelve years or so he's been my constant nemesis. As a fellow trustee of URA in its formative years he kept making trouble. He was always criticizing, complaining, making outrageous statements about what an accelerator should look like, how long it should take to build, and all sorts of stuff like that. He just never gave us any peace. Somehow he got to be Director in spite of all that, and then he made life miserable for all us trustees, and for me in particular as a user of the Laboratory. He had his priorities all twisted. He seemed more concerned about trees than about having neat, straight beam lines. He seemed to care more about human rights and real affirmative action than about efficiency. He seemed to care more about architectural elegance, style, aesthetics (and leaking roofs) than about pert charts. And to complete this irritating injury to us, the damn crooked beams worked, the affirmative action turned out to be efficient and the pert charts were wrong anyway.

Now some of you know, that a funny thing happened to me on the way to the highrise last summer. I became designated. Since then I've been studying the problem of why I can't trust Wilson. Recently I found a book. It's a Design Report dated 1967. It has all the promises he made to everybody about what a great laboratory this was going to be and I just thought I'd analyze this book very carefully. I had some help from the staff, and I'd like to share with you the promises and the reality. I became thoroughly suspicious about Wilson because there are all sorts of incidents (this is the category of hysterically funny anecdotes) in the archives. There is a story that during the days of considerations of what the accelerator should look like, Wilson happened to be in Paris. When he goes to Paris he has this entree (I don't know how he gets it) to an art studio where he can sketch live models, Parisian models, probably not very well dressed. And there he was, I can see him now, looking at this beautiful Parisian model, you see, there is the model and he looked down at his sketch pad and he saw ..... magnets! Would you trust a man like that? Would you buy a used accelerator from him? So now let's look at this 1967 report. (See Fig. 1.) Promises about the machine, what he promised, and what really happened. I know that there are representatives of the government here and I know it's a very dangerous thing to show all these things. However, I think I should; some of these things are already known.

He promised 200 GeV and what do we get - 400 or 500! There is in fact in this book an estimate of the cost of going to 400 GeV, and the cost estimate based on escalated dollars was \$70 million and he didn't spend anything for that; he went to 400 without \$70 million. (I don't know where the \$70 million is, I'd like to find it.) Now look at the intensity. He promised 5×10^{13}; he only got 3×10^{13}. He failed to meet the cost estimates. He was okay on the groundbreaking but that's easy; all it takes is a shovel. Completion of construction, you see, messed up everything. And then his beam date was supposed to be June '72 and he just eked it out to March for 200 GeV and December for 400 GeV. There was a 42-month interval between groundbreaking and the beam date promised in the proposal. He didn't deliver. He promised 2000 physicists, no, I'm sorry, 2000 people working in the Laboratory, and we only have 1400. He promised an annual budget - well... That's not all, I'm just getting warmed up here.

Talk about the experimental program. (See Fig. 2.) This was called a reduced-scope accelerator. He promised four target stations and in 1974 when the machine really began operating there were eight instead of four, leading to lots of confusion. There were supposed to be ten particle beams; there turned out to be fourteen. There were supposed to be twelve experiments set up, there turned out to be twenty-seven. We were supposed to finish twenty experiments a year; the number was more like thirty. Promises! Then to make matters worse, what about the staff? Well, he expected ninety experimental physicists doing research and there were only fifty. Here he did pretty well: he promised fifteen theorists and we only got eight. Perfect!

There were expected to be 300 non-resident experimenters in this book I'm talking about, and there things went wild; we counted over 600. Now let me talk a little bit about those 600. Wilson apparently let anybody into the Laboratory. They came from something like thirty states and eighty-two

PROMISES ABOUT MACHINE PARAMETERS

	1967 DESIGN	WHAT REALLY HAPPENED
ENERGY	200 BEV*	400/500 GeV*
INTENSITY	5×10^{13} ppm	3×10^{13} ppm
COST	\$250 M	\$243.5
GND BREAKING	Dec '68	Dec '68
CONSTR. COMPLETE	JUNE '72	JUNE '71
BEAM DATE	JUNE '72	March '72 200 Dec 72 400
INTERVAL: G.B→ BEAM DATE	42 Mo.	39 mo.
POPULATION	2000 (1975)	1400 (1979)
ANNUAL BUDGET IN 1975	\$130 M (1979 \$)	\$74 M (1979)

* COST TO GO TO 400 Estimate \$70M → 0 (1979)

Figure 1

1967 PROMISE: EXPERIMENTAL PROGRAM

		1974
TARGET STATIONS:	4	8
PARTICLE BEAMS:	10	14
EXPTS SET UP	12	27
EXPERIMENTS/YR	20	~30

1967 PROMISE: POPULATION

RESIDENT EXP'TL PHYSICISTS IN RESEARCH	90	50
THEORISTS	15	8
NON-RESIDENT EXPERIMENTERS	300	600*

* See Details

Figure II

institutions and since there are lots of people here today and they all want to see their own institution I'll give you a list (Chart I a and b). There are places like Hawaii, and Tufts, and Lehigh, and all sorts of places, even Harvard! These are people with approved or recently completed experiments. This is all just to prove that this is indeed a truly national accelerator laboratory. Having done that, what does Wilson do? He let in foreigners! And so there are 101 foreign institutions involved presently at Fermilab from Australia, Canada, China, England, France, etc. (See Chart II.) Each of these institutions is involved in one or more Fermilab experiments.

Now Wilson is very active in the World Laboratory. It's typical of him. He goes around forming committees to make a world laboratory, doesn't even know that he has one right here with more world-wide participation than any other laboratory. Okay, now, in order for these people to do something he promises them experimental areas. Figure 3 is the reduced scope picture out of this book - this book full of promises. What happened in 1973 when the areas were finished? Here is Fig. 4. You see all of these beams and in fact just enormous amount of activity that confused everybody. Some people, a tired user perhaps, would stumble into the wrong portakamp take the wrong data, and go home with it all mixed up.

Okay, now, this is very interesting. I studied this book, the Fermilab Proposal. I know, and everyone knows, that Wilson is a very strong director and nothing gets into this book that he didn't approve. **Everyone** knows that. In the very beginning it addresses the question of why we should build this accelerator. High-energy physics has problems and this accelerator is designed to solve these problems. The Book lists these problems, circa 1967. (See Fig. 5a.). And so now I would like to confront you with the question of how well one did in these twelve years in responding to these questions. I had some theoretical assistance. We took the questions (some of them were awkwardly phrased because the language was changed a bit since those times) rephrased them in more modern language and I will quickly review what happened in the last twelve years between the questions and the facts.

One set of questions is: "Which of the known particles are elementary? What new particles can be made at higher energy? (This was going to be 200 BeV.) Are they particles associated with a weak source? And are there building blocks more fundamental than the protons and neutrons?

This was 1967 and what are the 1979 answers? Well, the first thing that was done at the Lab was a lot of particle searches which didn't find anything. The W boson? We didn't find it. We didn't find free quarks, magnetic monopoles, and stable new particles. Some particles were seen here somehow and a lot of activity went into studying them. The psi was seen and in fact its hadronic character was established by the

CHART I (a)
FERMI NATIONAL ACCELERATOR LABORATORY

Number of Institutions by State with Approved or Completed Physics Experiments - October, 1978

Arizona	1
California	12
Colorado	1
Connecticut	1
District of Columbia	1
Florida	1
Georgia	1
Hawaii	1
Illinois	9
Indiana	3
Iowa	1
Kansas	1
Louisiana	1
Maryland	2
Massachusetts	9
Michigan	2
Minnesota	1
Mississippi	1
New Jersey	4
New Mexico	1
New York	10
North Carolina	2
Ohio	2
Pennsylvania	4
Rhode Island	1
Tennessee	3
Texas	2
Virginia	2
Washington	1
Wisconsin	1
TOTAL	82
States	30

CHART I (b)
INSTITUTIONS WITH APPROVED OR COMPLETED
PHYSICS EXPERIMENTS - OCTOBER, 1978

Arizona

Arizona, Univ. of

California

Cal. Inst. of Tech.
Cal., Univ. of Berkeley
Cal., Univ. of Davis
Univ. of Los Angeles
Cal., Univ. of Riverside
Cal., Univ. of San Diego
Cal., Univ. of Santa Barbara
Cal., Univ. of Santa Cruz
Harvey Mudd College
Lawrence Berkeley Laboratory
Stanford Linear Accel. Center
Stanford Univ.

Colorado

Colorado, Univ. of

Connecticut

Yale Univ.

District of Columbia

National Science Foundation

Florida

Florida State Univ.

Georgia

Georgia Inst. of Tech.

Hawaii

Hawaii, Univ. of

Illinois

Argonne National Laboratory
Chicago, Univ. of
Fermilab
Ill. Inst. of Tech.
Ill., Univ of

Illinois (continued)

Ill., Univ. of, Chicago Circle Campus
North Central College
Northern Illinois Univ.
Northwestern Univ.

Indiana

Indiana Univ.
Notre Dame, Univ. of
Purdue Univ.

Iowa

Iowa State Univ.

Kansas

Kansas, Univ. of

Louisiana

Louisiana State Univ.

Maryland

Johns Hopkins Univ.
Maryland, Univ. of

Massachusetts

AF Cambridge Research Laboratory (CRFC)
Emmanuel College
Harvard Univ.
Mass. Inst. of Tech.
Mass., Univ. of
Northeastern Univ.
Space Physics Div., AF Geophysics Lab., Hanscom Air Base
Suffolk Univ.
Tufts Univ.

Michigan

Michigan State Univ.
Michigan, Univ. of

Minnesota

Minnesota, Univ. of

Mississippi

Mississippi State Univ.

New Jersey

Princeton Univ.
Rutgers Univ.
Stevens Inst. of Tech.
Upsala College

New Mexico

Los Alamos Scientific Lab.

New York

Brookhaven National Lab.
Columbia, Univ. of
Cornell Univ.
General Elec. Co. R & D Center
New York Univ.
State Univ. of Albany
State Univ. of Buffalo
State Univ. of Stony Brook
Rochester, Univ. of
Rockefeller Univ.

North Carolina

Duke Univ.
North Carolina, Univ. of

Ohio

Case Western Reserve Univ.
Ohio State Univ.

Pennsylvania

Carnegie-Mellon Univ.
Lehigh Univ.
Pennsylvania, Univ. of
Pittsburgh, Univ. of

Rhode Island

Brown Univ.

Tennessee

Oak Ridge National Lab.
Tennessee, Univ. of
Vanderbilt Univ.

Texas

Houston, Univ. of
Johnson Space Center, NASA

Virginia

Virg. Polytechnic Inst. & State U.
William and Mary College of

Washington

Washington, Univ. of

Wisconsin

Wisconsin, Univ. of

CHART II
FOREIGN INSTITUTIONS - 101 TOTAL

Australia

Australian National University, Canberra
Melbourne, University of, Parkville
Sydney, University of, Sydney
Tasmania, University of, Hobart

Belgium

Brussels, University of
Universite de L'Etat, Monz

Canada

Canadian Institute of Particle Physics, Montreal
Carleton University
McGill University
Montreal Universite de
Ottawa, Universite de
Quebec, Universite du Cresala, Montreal
Toronto, University of
Western University, London

China

Institute of High Energy Physics, Academia, Sinica, Peking

England

Cavendish Laboratory, Cambridge
Imperial College, London
Liverpool, University of, Liverpool
Open University, the, Bletchley
Oxford University of
Rutherford High Energy Laboratory
University College, London

France

Centre de Recherches Nucleaires de Saclay
Centre de Recherches Nucleaires, Strasbourg
Lab. du Rayonnement Cosmique, Lyon
Laboratoire de L'Accelerateur Lineaire, Orsay
Lyon, Universite de
Nancy, Universite de, Nancy
Paris Vi, U. de., Lab. Physique Generale
Rene Bernas Laboratoire, Orsay
Strasbourg, University of

Germany

Christian-Albrechts Universitat, Kiel
Kiel Universitaet, Inst. Reine Ange. Kernphysik
Max Planck Institute, Munich

Greece

University of Athens

Hungary

Central Research Institute, Budapest

India

Delhi University, Delhi
Jammu University, Jammu-Tawi
Punjab University, Chandiaarh
Ralasthan University, Jaibur
Tata Institute of Fundamental Research, Bombay

Ireland

University College Dublin

Israel

Israel Inst. of Technology, Technion City, Haifa
Tel-Aviv University of, Tel-Aviv
Weizmann Institute of Science, Rehovot

Italy

Bari, Universita di
Bologna, Universita di
Firenze, Universita di
Padova, Universita di
Pavia, Universita di
Rome, Universita di
Torino, Universita di
Trieste, Universitat Degli Sudi Di

Japan

Aichi University of Education, Kariya
Ashikaga Institute of Technology, Ashikaga
Hirosaki University, Hirosaki
Isas, Tokyo University
Kanagawa University, Yokohama
Kinki University, Kobe
Kobe University, Kobe
Konan University, Kobe
Kwansei Gatuin University, Nishinomiya
Nagoya University, Nagoya
Okayama University, Okayama

Osaka University
Saitama University, Urawa
Science Education Insitute of Osaka Prefecture
Shinshu University
Tohoku University
Tokyo, University of, Cosmic Ray Laboratory
Tokyo, University of, INS
Utsunomiya University, Utsunomiya
Wakayama Medical College
Waseda University, Tokyo
Yokohama National University, Yokohama

Korea

Korea University, Seoul

Netherlands

Nijmegen University, Nijmegen

New Zealand

Auckland, University of, Auckland

Poland

High Energy Physics Lab, Warsaw
Institute of Nuclear Physics, Cracow
Institute of Nuclear Research, Warsaw
Warsaw University, INS

Singapore

Singapore, University of

Spain

Barcelona, Universidad Autonoma de
Instituto de Fisica Corpuscular, Valencia
Santander, Universidad de, Santander
Valencia, Universidad de

Sweden

Lund, University of, Lund
Stockholm, University of, Stockholm

Switzerland

CERN
LHE, ETH Honggerberg, Zurich

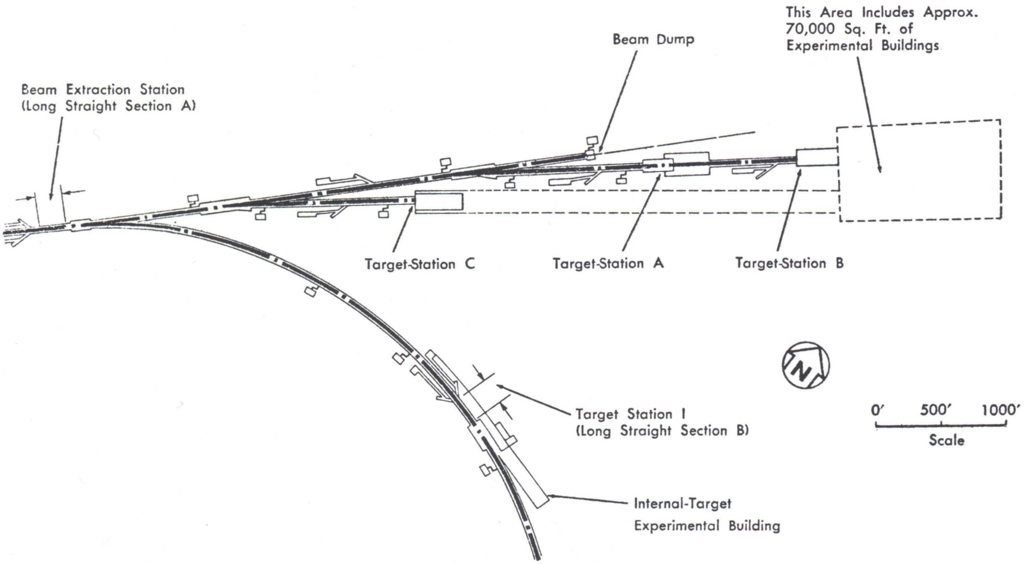
USSR

IHEP, Academy of Sciences of the Kazakh, Alma-Ata
Institute of Theoretical and Experimental Physics, Moscow

Institute of High Energy Physics, Serpukhov
Institute of Nuclear Physics, Novosibirsk
Joint Institute for Nuclear Research, Dubna
Kharkov Physical Technical Institute
Lebedev Physical Institute, Moscow
Leningrad Institute of Nuclear Physics
Moscow University, Moscow
Physical Technical Institute, Tashkent
Tomsk Polytechnic Institite

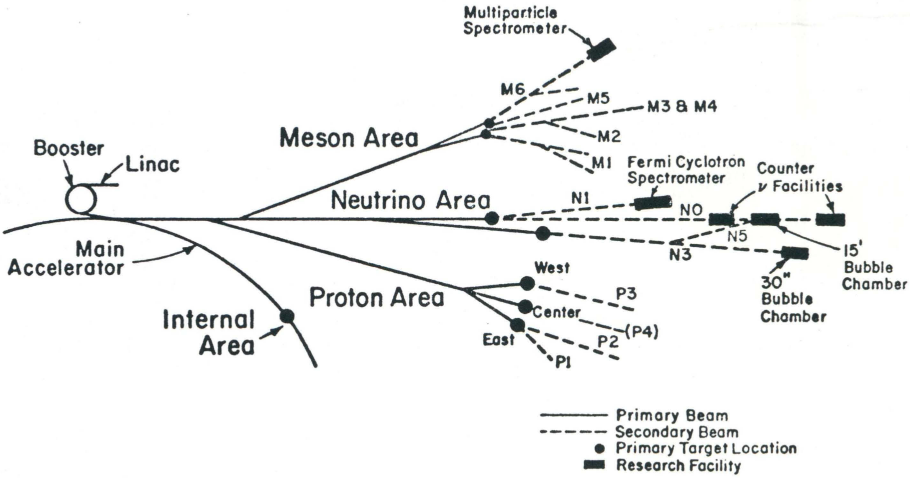
Yugoslavia

Belgrade, University of, Belgrade



EXPERIMENTAL AREAS IN
1967 PROPOSAL "REDUCED SCOPE"

Figure 3



EXPERIMENTAL AREAS FNAL
1973/4 →

Figure 4

PROBLEMS OF HIGH ENERGY PHYSICS
(FROM 1967 "NAL DESIGN REPORT")

QUESTIONS

- WHICH OF THE KNOWN PARTICLES ARE ELEMENTARY?
- WHAT NEW PARTICLES CAN BE MADE AT HIGHER ENERGY?
- ARE THERE PARTICLES ASSOCIATED WITH THE WEAK SOURCE?
- ARE THERE BUILDING BLOCKS MORE FUNDAMENTAL THAN PROTONS + NEUTRONS

ANSWERS

(1) PARTICLE SEARCHES WHICH FAILED

i) W-BOSON
ii) FREE QUARKS
iii) MAGNETIC MONOPOLES
iv) "STABLE" NEW PARTICLES

(2) PARTICLES SEEN AT FNAL

i) PSi (PHOTOPRODUCTION PROOF OF ITS HADRONIC NATURE)
ii) CHARMED MESONS
iii) CHARMED BARYONS
iv) UPSILON FAMILY

Figure 5a

protoproduction process. Charmed mesons were seen, also charmed baryons and something called the upsilon family. And then, to continue answering the question, the quark structure of hadrons was illuminated. There were experiments on the deeply inelastic scattering of muons and of neutrinos. These established structure functions and discovery of scaling violations took place in one of those beam lines. These scaling violations (as we'll see) are also evidence for a field theory of strong interactions; it is called QCD and contains new objects called gluons. (See Fig. 5b.) Measurements of muon pairs established a model in which quarks and antiquarks can annihilate and at the same time gave confidence in the quark structure of hadrons. Jets were seen. These are essentially outgoing quarks arranging themselves. Large transverse momentum experiments gave further evidence for point-like structures.

Then another series of questions: "What symmetry exists at higher energy? Is the Pomeranchuck theorem true?" (It's hard to believe what people were worried about in 1967!) Do total cross sections become constant at higher energies? And Fermilab evidence showed a rise of cross sections from the Serpukhov energies very clearly, and many precise experiments indicated strong support for Regge pole ideas in both inclusive and exclusive reactions. And then, in fact, these high-mass diffractive scattering experiments established the first, as some like to call them, Pomeron beams at Fermilab (Fig. 5c). And another question, "Are short-lived particles different when produced at high energies?" A Fermilab experiment studied the space-time structure of hadrons by looking at the A dependence in hadron nuclear reactions.

"What are the form factors?" Now we talk about quark structure functions. The quantitative photographs of quark structure of nucleons and pions have been taken here in great detail. In another series of questions, "Is there any connection between electromagnetic and strong forces? Is quantum electrodynamics valid? Is there a strong/weak connection? Do weak forces get strong? And the answers in the last twelve years include Fermilab experiments which confirmed and extended the properties of neutral currents. Weak and electromagnetic probes observed one of the more subtle attributes of the quarks, the sea distributions. They turned out to be the same when viewed by the weak and by the electromagnetic probes. There was in fact evidence, not that the weak interactions became strong but that the strong interactions got weak at short distances (Fig. 5d).

Is there a law which predicts the existence and nature of old particles? A nice modest question! Are local field theories valid? What are the relevant fields? Experiments established strong evidence for electro-weak interactions as in the Weinberg-Salam theory but don't forget there is no W or Z^o yet. (One of those promises that are broken.) QCD support in many experiments suggest the fundamental theory of strong forces and so on.

(3) QUARK STRUCTURE OF HADRONS

i) Deep Inelastic μp and νp structure functions: Discovery of scaling violations as evidence for QCD (i.e. GLUONS)

ii) Measurement of muon pairs as evidence for quark-antiquark annihilation

iii) JETS

iv) Large $P_\perp$ Experiments

QUESTION

- What symmetries Exist at higher Energy?
- Is the Pomeranchuk theorem true? Do total cross-sections become constant at higher energies?

Answers: i) FNAL measurements show rise of cross sections from Serpukhov energies

ii) Many precise measurements ⇒ strong support for regge pole ideas in both inclusive and exclusive processes.

Figure 5b

iii) high mass diffractive Scattering
⇒ first "pomeron beams" at FNAL

QUESTION : ARE SHORT LIVED PARTICLES DIFFERENT WHEN PRODUCED AT HIGH ENERGY?

ANSWER THE SPACE-TIME STRUCTURE OF HADRONS IS STUDIED AT FNAL VIA THE A-DEPENDENCE IN HADRON-NUCLEAR REACTIONS

QUESTION : WHAT ARE FORM FACTORS?

ANSWER : WE NOW TALK OF QUARK STRUCTURE FUNCTIONS - THE QUANTITATIVE "PHOTOGRAPHS" OF QUARK STRUCTURE OF NUCLEON AND PION ARE TAKEN HERE.

QUESTIONS :
- IS THERE ANY CONNECTION BETWEEN ELECTROMAGNETIC AND STRONG FORCES?
- IS QED VALID?
- IS THERE A WEAK-STRONG CONNECTION?
- DO WEAK FORCES GET STRONG?

Figure 5c

ANSWERS : i) FNAL EXPTS CONFIRM + EXTEND STUDY OF NEUTRAL CURRENTS

ii) WEAK (ν) and Electromagnetic Probes (μ and $\to\mu\mu$) observe quark, sea, distributions - they are the same. Ditto quark quantum numbers

iii) Evidence here, that STRONG interactions get WEAK at short Distances (asymptotic freedom)

QUESTIONS : • Is there a law which Predicts the Existence and Nature of all particles?
• Is local field theory Valid?
• what are the relevant fields?

Answers : i. Strong Evidence for "Electro-weak" Weinberg-Salam model (But no W, Z^0 yet!)
ii) QCD support in many Expt's suggests a fundamental theory of STRONG FORCES

Figure 5d

Another standard cliché in these books that propose new accelerators is, of course, that we don't really know what the real questions are now, they will come out in the future. So another way to summarize discoveries and developments over the last twelve years, not all of which was uniquely Fermilab (See Fig. 6.) We found two new quarks, new particles, the tau lepton and heavy quark bound states. We found neutral currents, as a new process, were able to clarify the structure of weak interactions via a whole new set of reactions, the decay of charmed quarks and hopefully some B quarks. There is a great success of Regge and Regge-Mueller theories for inclusive and exclusive reactions. We already mentioned the success of the quark structure of hadrons and of the gauge theories. Unified weak and electromagnetic theory of Weinberg-Salam, the quark model of strong interactions which we call QCD, and the intense study nowadays of grand unification.

The scope of the effort in this Lab is a little mind boggling and just for fun, out of a standard FNAL publication I've picked out, a set of research results.* You can get this book if you're interested; it's good reading before you go to sleep. These are not the same pages flashed over you. They are different. That's what happens when your budget isn't high enough and you have all these beam lines. This gives you a clue as to what the problems are here. To bring you up-to-date and point to the future just a little bit, what is the basic problem? The basic problem, as perceived here, is that we have a 400-GeV accelerator and we have people who do the same physics in other places and it is a very difficult thing to ask a group of people to spend three years of their life working late at night, going through all the hassles of doing experiments to find out later that someone else can get the same results sooner and with more detail because they have more powerful facilities. And that's a very fundamental question. We find that in Europe, for example, the total expenditures in high-energy physics are about a factor of two more than in the United States and it's a factor of two almost anyway you do it, either by looking at the rate of exchange of the various currencies, or by dividing by the gross national product, or dividing by populations, you always get a number like a factor of two. And if you look in detail at the support 400-GeV physics at Fermilab and CERN, the factor is much worse.

Now Wilson had a solution and the solution was a five-point solution. (See Fig. 7.) It said build an Energy Doubler. Use it to make a Tevatron (which is 1000-GeV protons on fixed target). It's not something that is totally obvious but 1000 GeV is incredibly more powerful than 400. To give an example, there are at least three or four large European experiments that have already expressed some interest in moving their massive and

*A. F. Greene and T. Yamanouchi, *Fermilab Research Program Workbook*, May 1978.

DISCOVERIES AND DEVELOPMENTS

1) NEW PARTICLES

two quarks: c, b

τ lepton

heavy Quark bound states

2) New Processes

neutral currents

3) Clarification of structure of Weak Int

decay of charmed quark

4) Success of Regge + Regge-Mueller theories for exclusive + inclusive hadron reactions

5) Success of Quark structure of Hadrons

6) Success of Gauge Theories

- unified weak + em - W-S
- quark theory of STRONG - QCD
- intense study of GRAND UNIFICATION of all interactions

Figure 6

PRESENT AND FUTURE

THE BASIC PROBLEM :

EUROPEAN EXPENDITURES IN H.E.P.
(BY LEGAL RATE OF EXCHANGE) $650
U.S. EQUIVALENT $330

RATIO : EUROPE/US : 2.0

(BY GNP) RATIO : 2.2

FERMILAB/CERN FACTOR IS WORSE.

WILSON SOLUTION :

1. BUILD AN ENERGY DOUBLER
2. USE IT TO MAKE A TEVATRON
1000 GeV PROTONS ON FIXED T
3. USE IT TO MAKE COLLIDER :
1000 GeV P × 1000 GeV $\bar{P}$
4. COMPLAIN ABOUT FUNDING
5. QUIT!
6. WORK ON S.C. magnets

→ HE IS STILL MOVING! ←

Figure 7

elaborate detectors to Fermilab to get a crack at 1000 GeV; so that turned out to be a very powerful idea: Wilson called it a leap frog. So his plan: i) Build an Energy Doubler, ii) use it to make a Tevatron, iii) use it to make a collider so that one can have 1000-GeV protons against 1000-GeV antiprotons. Once you've got that underway, iv) complain about the funding and v) quit. So what we did is this: we had a little piece of paper he signed as a condition of my taking the job; it was a five-year warranty on the accelerator or 1000 miles whichever comes first. And he's got to work on the superconducting magnets. And he has several other ideas here. So the answer to the little boy is "He's still moving." Thank you.

Early Days In The Development Of Accelerators

W. Paul

Ladies and Gentleman!

The symposium today is dedicated to Robert R. Wilson. The subjects of the lectures cover a wide span from accelerator physics to beauty and science. Some people may be surprised by this but only those who are not familiar with the life of Bob Wilson, physicist and artist. As all his scientific life has been strongly coupled to particle accelerators, he himself would be the most appropriate speaker for the talk I am going to submit. But today, listening is his role.

Before starting, I have to apologize for concentrating on the part of the field that I am most familiar with, accelerators for fundamental physics. I restrict myself to the basic ideas and I will mention only names who played a major role in discovering the principles, leaving out my colleagues who brought the relevant devices to perfection, so that they are really useful for modern research.

What is an accelerator? If one consults a dictionary, accelerate means to increase the velocity of something. But dictionary editors normally are not familiar with the theory of relativity. Acceleration, as already formulated by Newton, means an increase of the momentum, the product of velocity and mass.

Having this in mind, W. W. Hansen (Ha 48), the late inventor of the electron linac, proposed to use the name mass aggrandizer, or -- after consulting the department of classical philology at Stanford -- ponderator, but we are still talking about accelerators.

Well, what are accelerators good for? All progress in the knowledge of matter comes by studying the interaction of its components with accelerated particles. This is done by three basic methods:

1. Scattering gives information about the structure

2. Particle spectroscopy or excitation of resonances elucidates the binding forces and

3. Energy transfer causes the production of secondary or new particles.

The study of the atom, the nucleus and the nucleon started immediately after the experimental technique of accelerating particles was developed for the relevant energy region. But these developments were strongly correlated with other techniques such as producing high voltages, generating radio frequencies and especially producing the necessary vacuum. Faraday, in studying the passage of electric currents through gases, was handicapped by the leather piston pump.

In 1860, Plücker and Hittorf in Bonn with the help of their glassblower Geissler reached 10^{-3} Torr and immediately detected the cathode rays. Around 1900 a vacumm of 10^{-5} to 10^{-6} was achieved, good enough for scattering of canal and electron rays. The diffusion-pump gave the necessary pumping speed for accelerators and without the modern ultra-high vacuum technique no storage ring would work and therefore no theoretician would publish papers on psis and upsilons.

The pioneering experiments for all the three tasks I mentioned were all performed around 1900. The first one was the generation of x rays by Röntgen in 1894, as an example for secondary particles.

After Heinrich Hertz (He 86) in 1886 had found that cathode rays are able to penetrate thin aluminum foils, his assistant Philip Lenard let the electrons out of the vacuum tube and observed that they had a defined range in air. In 1895, Lenard studied the effect of electron scattering in various gases as a function of pressure and accelerating voltage. He introduced the gas-kinetic conception of a cross section to particle physics, and found that the cross section decreases rapidly with the particle velocity. From this he concluded that the atoms contain constituents, which he called at first Dynamides. A few years later he identified them with electrons and calculated an upper limit for their diameter of 10^{-11} cm. He showed, in agreement with the Thompson model, that the atom is in the main empty.

His was the first experiment in which such measurements were performed with electrons artificially accelerated up to a few 100 thousand volts. For this experiment he was awarded the Nobel Prize in 1905.

Lenard was followed by Rutherford, who performed his famous scattering experiments, with alpha particles with even higher energies from radioactive decay, looking for the angular distribution of the scattered particles. He showed that the positive constituents of the atoms are agglomerated in a nucleus in the center of the atom. These two men, together with, Niels Bohr, are the fathers of the picture of the atom that we consider now as the reality.

A few years later, in 1913, James Franck and Gustav Hertz succeeded in exciting the atomic shell by bombardment with accelerated electrons, resulting in the consecutive emission of photons. The voltage generators at that time were electrostatic machines designed by Toepler and Wimshurst, namely the spark inductor with rotating rectifiers or Tesla coils.

All in all, it was an instrumentation well suited to the study of the structure of the atomic shell, but mostly these instruments did not exceed the 100-KeV region.

It is worthwhile to repeat that all the experimental methods which are still in use in particle physics: scattering of charged particles, energy excitation, production of secondary radiation, and the use of the secondary particles to the same purposes were already developed in a few years around the turn of the century (Figs. 1, 2).

In 1919 the great breakthrough in physics occurred. Rutherford demonstrated that the nitrogen nucleus could be disintegrated by natural alpha particles from radium. A new era was opened in physics. But the energy of some million eV of the alphas exceeded by far the energies available in the laboratory. These were limited not only by the voltage supplies but also by the discharge tubes with their corona discharges and insulation breakdowns. It looked hopeless at that time to build devices for sufficiently high voltages in order to study this new nuclear phenomena in greater detail with higher particle intensities. Still, in 1927 Rutherford expressed in a letter to his hope that artificial accelerators with adequate energy could be built discussing the various methods of generating high voltages (Ru 27). In the same paper he pleaded for the use of very high magnetic fields in nuclear physics, referring to experiments of Kapitza and Cotton, who reached 320 kilogauss at that time.

In 1928 Gamow showed that due to the laws of wave mechanics, a penetration of the nuclear potential barrier by charged particles seemed to be possible at energies of 500 KeV or even less. Several laboratories started immediately to develop the necessary accelerators. The first to succeed were Cockcroft and Walton in the Cavendish laboratory (Co 32). In 1932 they reported the successful disintegration of lithium by protons of about 400 KeV, using the voltage multiplying circuit invented in 1921 by Greinacher in Bern (Gr 21).

In that period this extremely successful collaboration of experimental and also theoretical physicists with engineers started. In the course of developing step by step more and more powerful accelerators, they achieved not only higher energies but also higher intensities, greater stability, and energy precision as well. As Stan Livingston, one of the great accelerator pioneers, wrote: "Physicists converted to engineers and vice versa forming this powerful club of machine builders," talking nowadays only in terms of giga or tera volts and storage rings with currents of many amperes. Let me start now a review of this development:

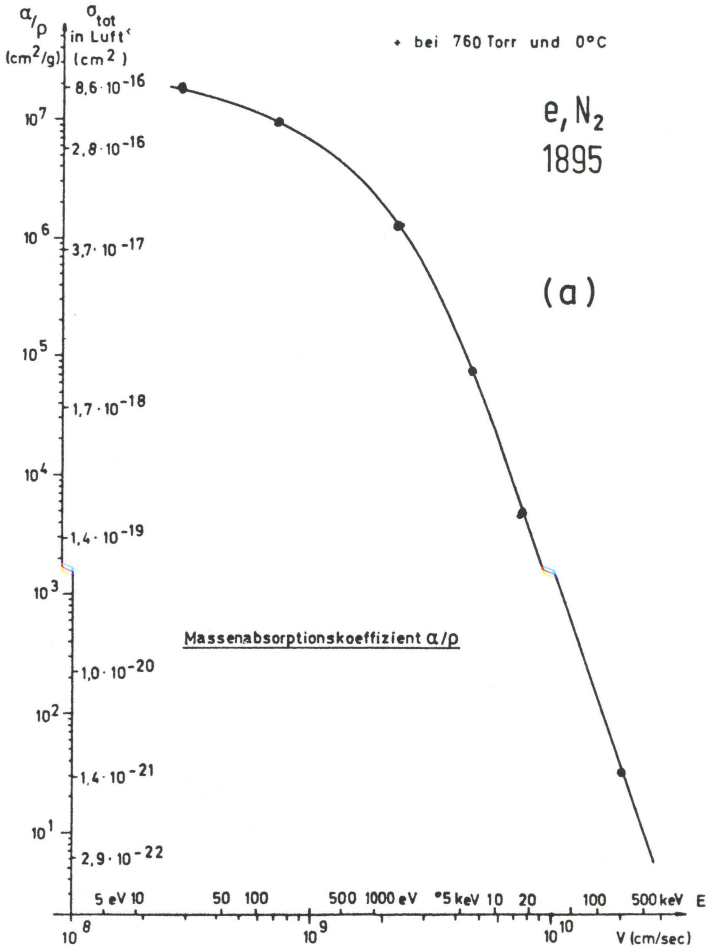
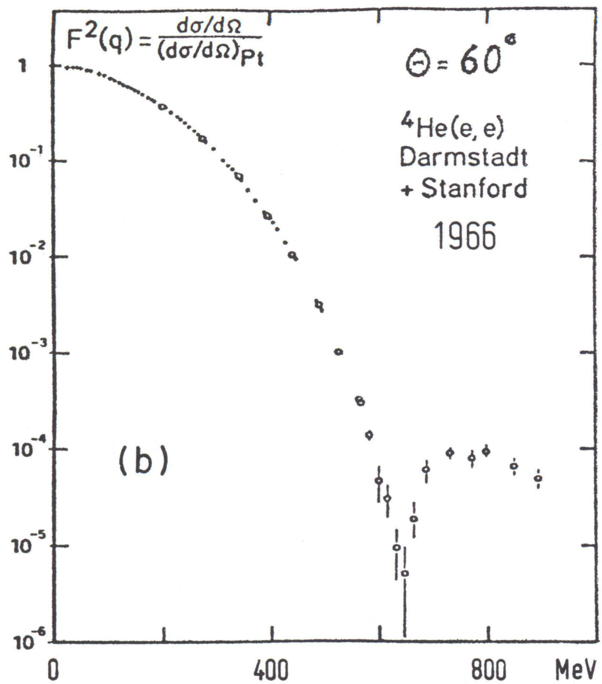
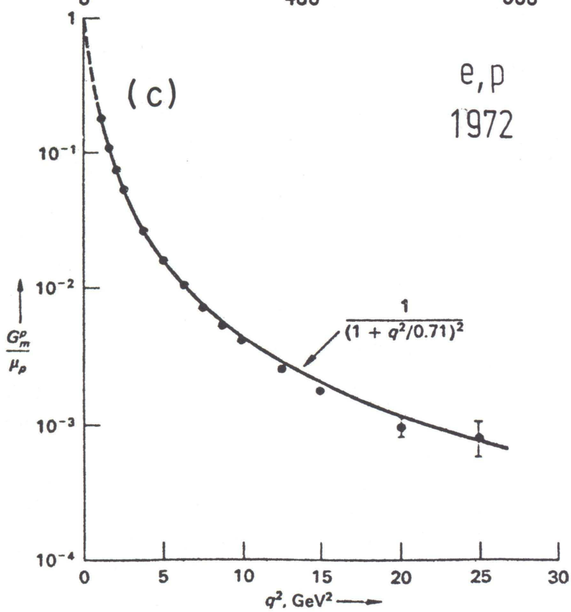


Fig. 1. Energy dependence of the cross section for elastic electron scattering. (a) atom: charge distribution in nitrogen (Lenard 1895); (b) nucleus: electric form factor (charge distribution); (c) proton: magnetic form factor (magnetic moment distribution).



$F^2(q) = \frac{d\sigma/d\Omega}{(d\sigma/d\Omega)_{Pt}}$
$\Theta = 60°$
$^4He(e, e)$
Darmstadt
+ Stanford
1966
(b)
1
10^{-1}
10^{-2}
10^{-3}
10^{-4}
10^{-5}
10^{-6}
0
400
800
MeV



e, p
1972
(c)
$\frac{1}{(1 + q^2/0.71)^2}$
$\frac{G_m^p}{\mu_p}$
1
10^{-1}
10^{-2}
10^{-3}
10^{-4}
0
5
10
15
20
25
30
q^2, GeV^2

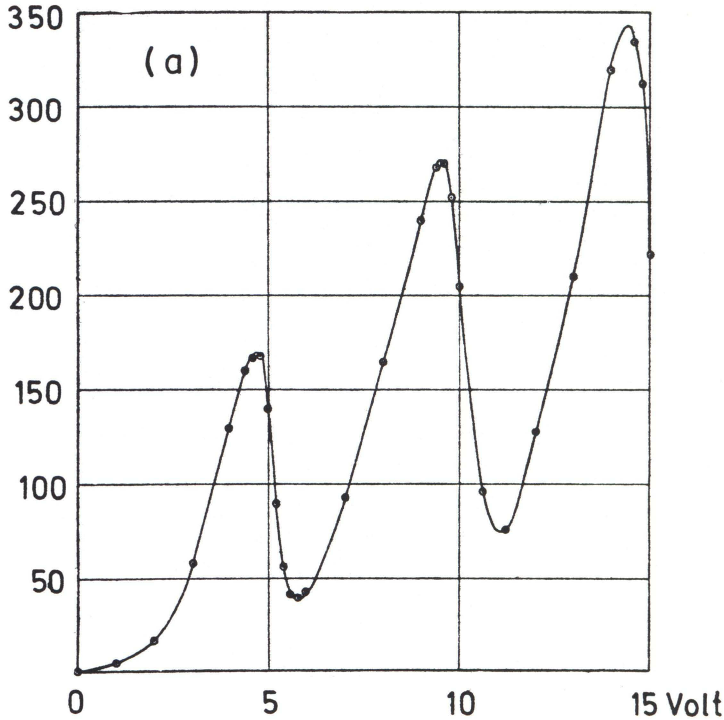
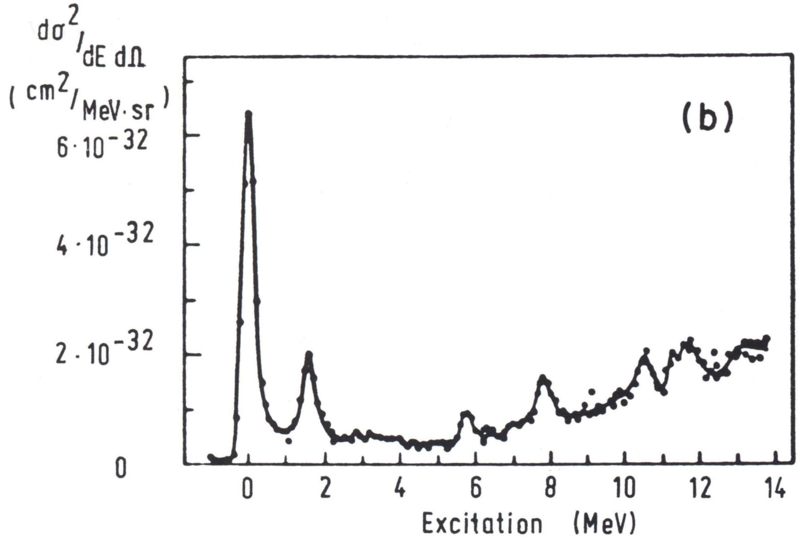


Fig. 2. Excitation of energy levels by electron impact. (a) Hg atom 1913; (b) nucleus 1962; (c) proton 1972.



Spectrum of electrons scattered at 180° from ^{25}Mg, with E_1 = 55.9 MeV.

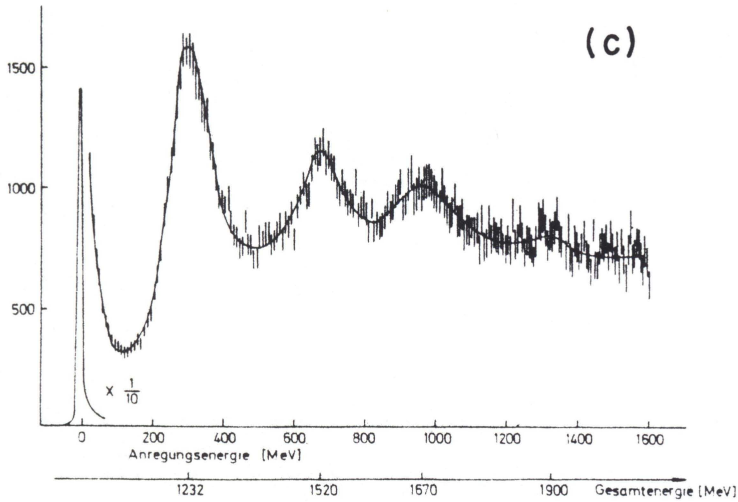


Fig. 3. The greenhouse of a university. H.A. Bethe and B. McDaniel in the accelerator tunnel at Cornell University.

It would take too long a time to go into all the details. Therefore I would like to concentrate on some of the basic ideas of the various types of accelerators which are listed in the table below. They were developed in many relatively small university laboratories, often sponsored by industry and often stimulated by the lack of the sums of money or technical resources, which seem to be indispensable for us. Today accelerator physics and engineering is concentrating more and more in very few large laboratories in the world, with their own standards of precision and perfection. A new device has to work at the first attempt. Due to the large sums of money involved, only well-founded techniques are used; at least the risk must be calculable. But discounting very rare cases, new ideas and new techniques need their time for ripening. They grow best, not in the framework of a national project, but in the greenhouse of a university. A typical example of this is shown in Fig. 3. The growth of energy of accelerators is shown in Fig. 4.

Types of Accelerators

I.	Direct Voltage Acceleration	
	Electrostatic Generators	1670
	Voltage multipliers	1932
II.	Induction Accelerator	
	The Betatron	1921-1940
III.	Resonance Acceleration	
	In electric high frequency fields	1924
	Linear Accelerators	1928
	Cyclotron	1930
IV.	Synchrotron Acceleration	
	Synchrotron	1945
	Synchro-Cyclotron	1946
	Isochron-Cyclotron	1955
	Strong-focusing optics	1950-1952
V.	Storage rings	
	Intersecting beams	1943,1956
	Cooling	1966-1968
VI.	Coherent Acceleration	
	Collective effects	Budker 1956
	Plasma accelerators	Veksler 1956

I. Direct Voltage Acceleration

In this type of accelerator, electrons or ions (in the old diction: cathode and canal rays) are accelerated in a discharge tube by an applied high voltage. They are limited in energy and current by the voltage generator and the electrical insulation strength of the tube as well.

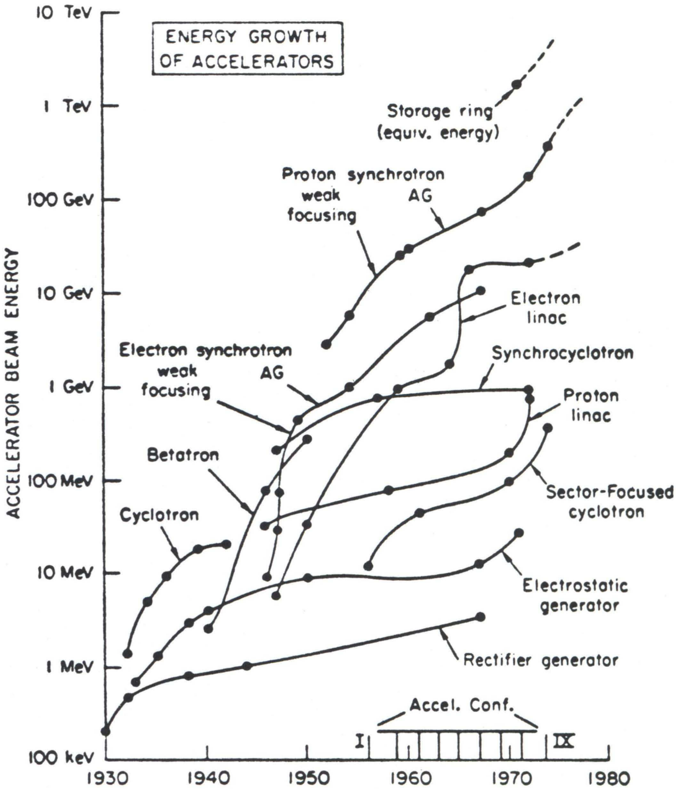


Fig. 4. Energy growth of accelerators.

The Electrostatic Generator

All electrostatic generators have their origin 300 years ago, when O. von Guericke in 1671 built the first friction electricity machine. In the following decades many improvements were made; a generator of special curiosity was the charge water drop or water vapor generator, constructed by Armstrong in 1845 (Ar 45) (Fig. 5). It was based on the observation of Faraday (Fa 43) that vapor emitted from a nozzle is electrically charged by friction. The water droplets were discharged on a metal plate, leaving the insulated steam vessel charged in the opposite sign.

Very common machines used in many laboratories until the 1920's were the induction generators, introduced by Toepler (Toe 65) and brought to perfection by Holtz, Wimshurst and Villard (Vi 11). They were quite powerful and in a multiplate version driven with an electromoter gave voltages up to 250 kilovolts at a current of 1 milliampere. Such a generator is shown in Fig. 6.

The most successful device is the belt generator developed by R. J. Van de Graaff (Gr 31). As a Rhodes scholar in Oxford (1928) he became aware of the necessity of high-voltage supplies for nuclear physics. Back in Princeton he succeeded in 1931 in reaching 1.5 megavolt with his relatively simple and cheap machine. In this generator an endless insulating belt transports charges to a spherical conductor. Looking through old books I found its prototype already commercially built in 1893 by R. Busch (Bu 93) (Fig. 7). In order to be independent from the room humidity, the driving cylinders were heated from inside and thereby also the belt. Even a very modern belt (the laddertron) was anticipated (1876) by A. Righi in Bologna. In order to avoid discharges along the belt, he constructed it from metal and insulating strips alternately linked together.

Van de Graaff's success immediately brought him the support of K. T. Compton, president of MIT, and a project was started, gigantic for that time. Two belt generators for 7.5 MeV each, but of opposite charge, were planned with a discharge tube between and an observation laboratory inside one of the terminals (Fig. 8a). The apparatus was installed in a huge hangar but never reached the full voltage. Figure 8b shows one of the impressive discharges between the voltage terminals and the metallic walls of the hangar. Another group--Tuve, Hafstad, and O. Dahl (Tu 35)--at the Carnegie Institution of Washington was more modest. They produced in 1933 a proton beam of 600 KeV and performed the first nuclear physics experiment with a Van de Graaff generator. In 1935 they reached 1.3 MeV with a beam current of 750 μA (Fig. 9). Van de Graaff's group at MIT followed soon with a revised version of their former ambitious project, reliably achieving 2.75 MeV. It was successfully used for research for many years.

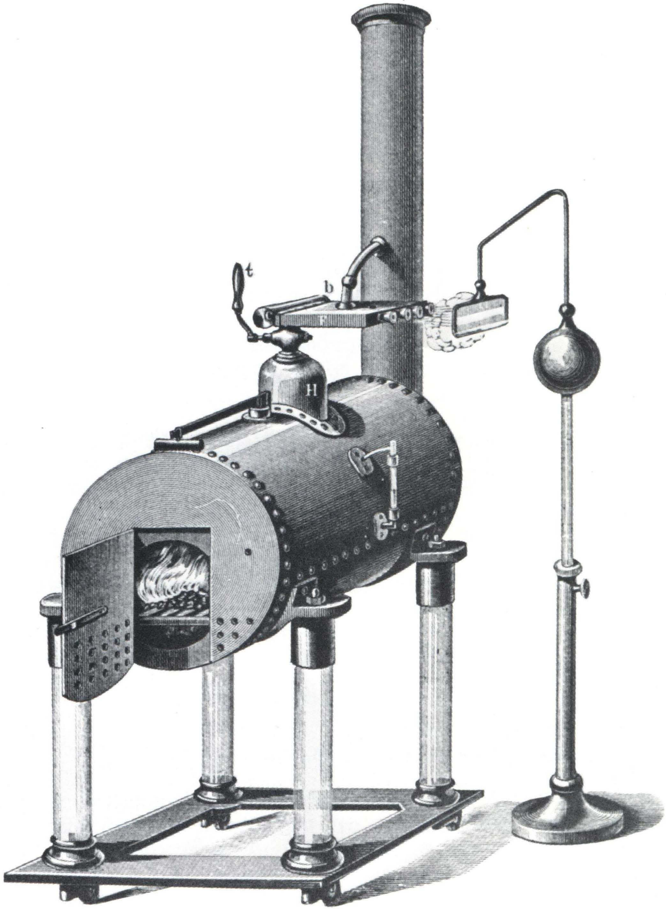


Fig. 5. High-voltage generator using charged water droplets (Armstrong 1843).

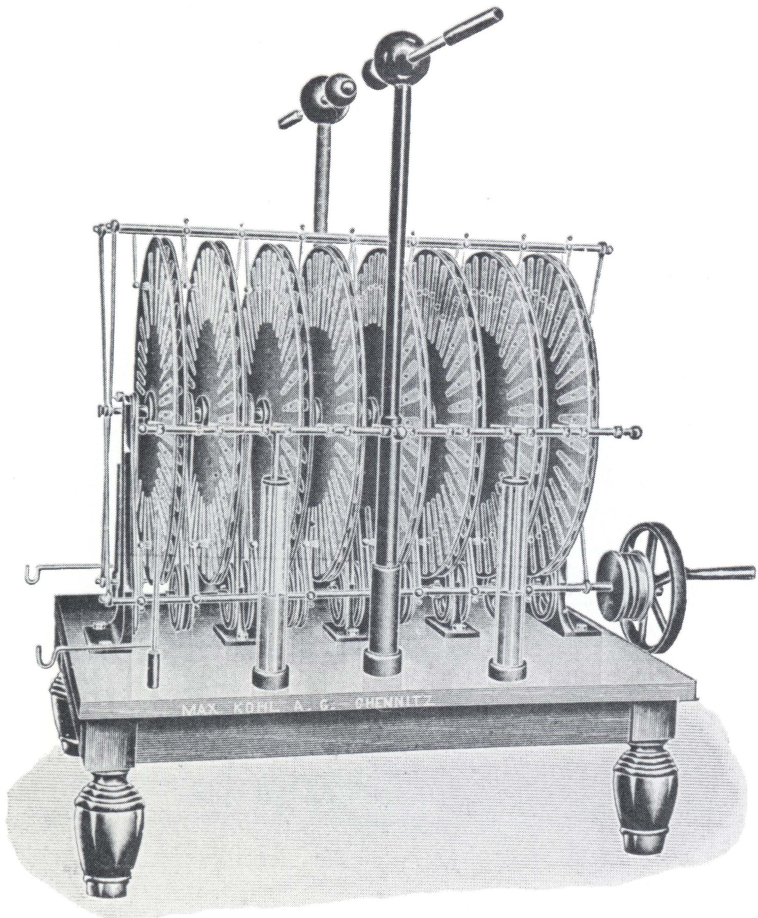


Fig. 6. Electrostatic generator of the Toepler type, disc diameter 90 cm.

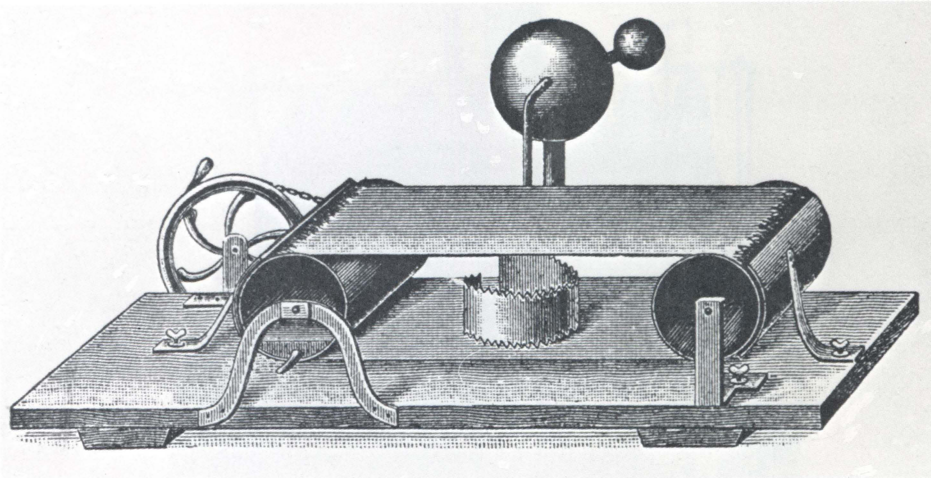


Fig. 7. The commercial belt generator by R. Busch 1893.

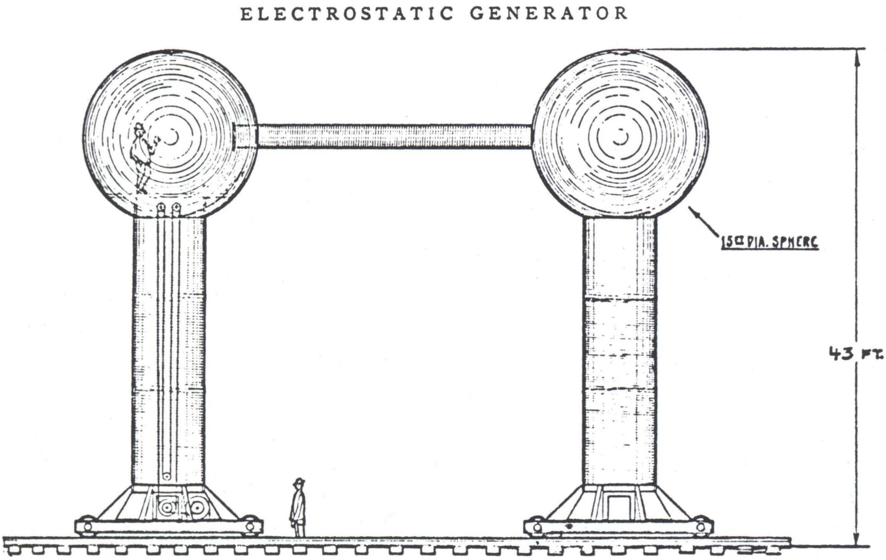


Fig. 8a. Sketch of the 2 x 7.5 MV "Round Hill" generator.

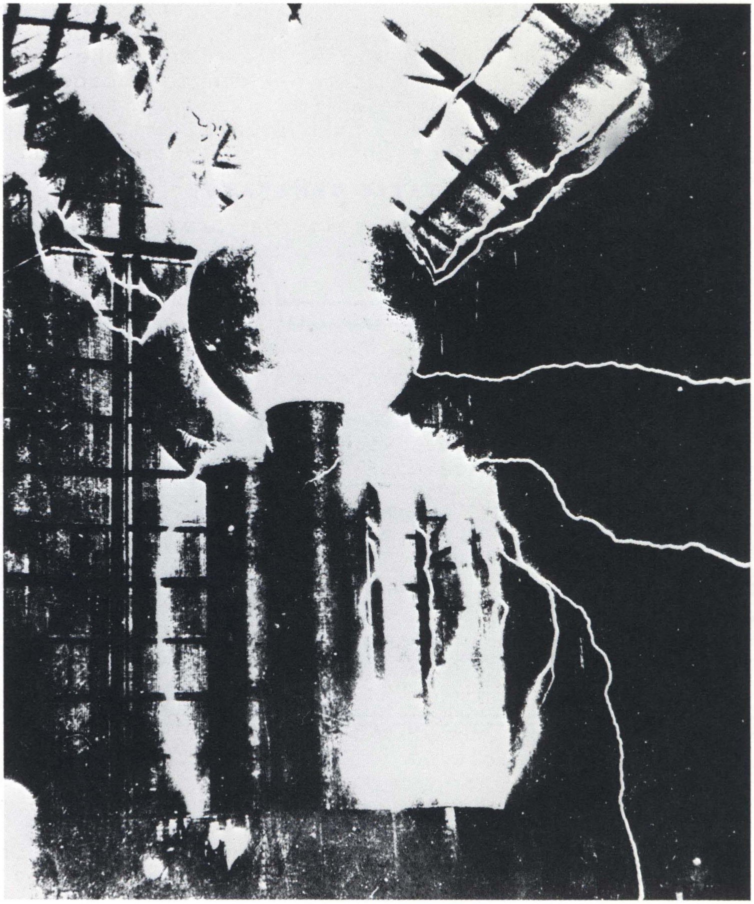


Fig. 8b. Discharges between the voltage terminal and the hangar walls.

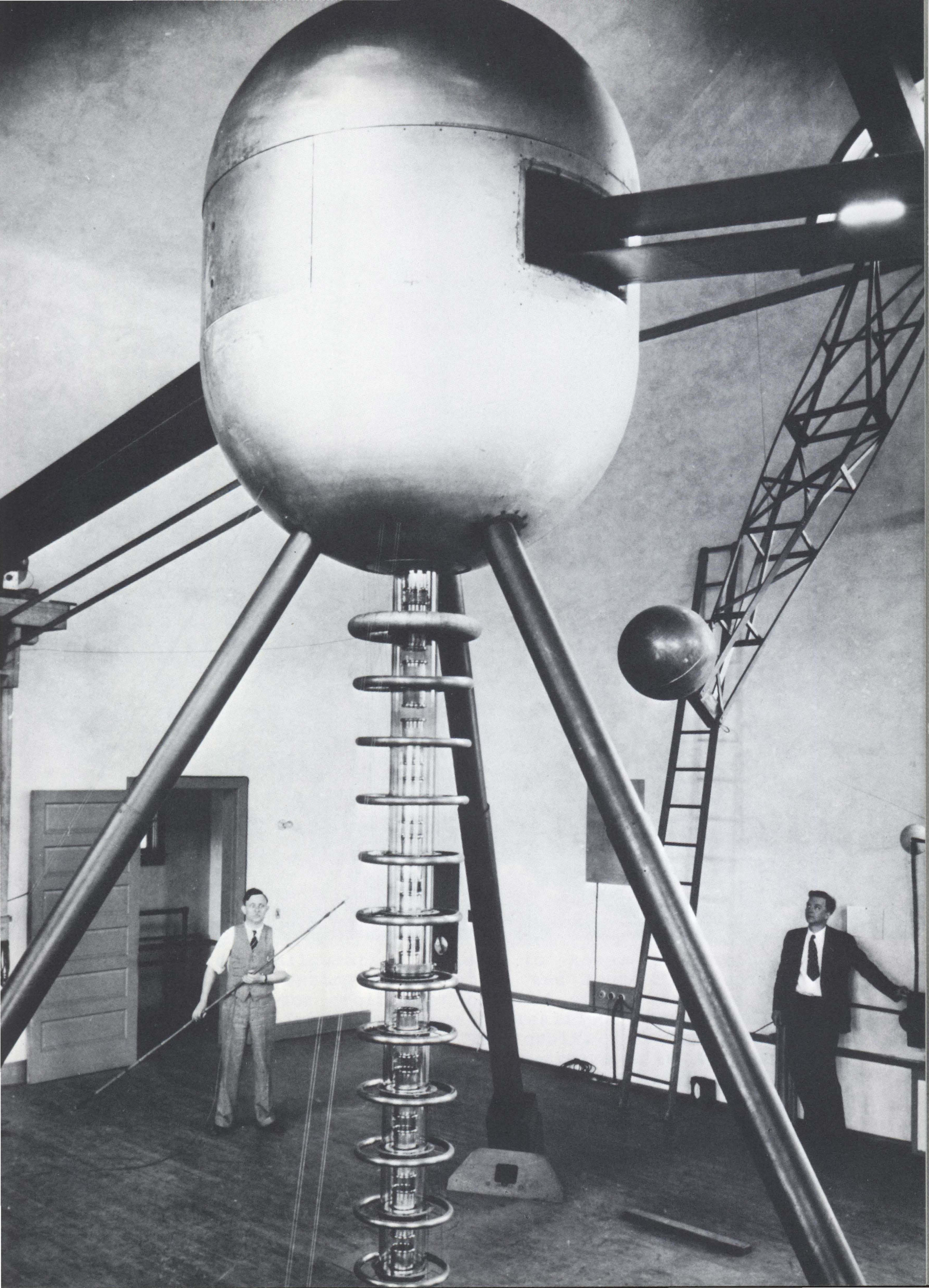
The annoying dependence of these generators on the atmospheric conditions was overcome by Herb, Parkinson and Kerst by installing the generator in a pressure vessel (He 35), which at the same time allowed higher voltages due to Paschen's law of voltage breakdown. But this technique also was anticipated in 1885 by Hempel in Dresden (He 85), who studied the performance of a Toepler induction generator in an iron pressure tank filled with dry air up to 6 atm. Even the driving electromotor was placed in the vessel. He found a significant increase in the achieved current, and better insulation properties. He also investigated the behavior of different gases such as H_2 and CO_2.

After the war, Van de Graaff's generators became for many years the standard accelerator not only in nuclear physics, but also in many other fields of research and industry where they are still standard today. The most impressive installations are under construction in Oak Ridge and Daresbury for heavy-ion research. They are designed for 30 MV voltages and housed in 70 m high towers. These generators will work according to the tandem principle. In such a device one starts with negative ions which are accelerated to the voltage terminal. In a gas flow or a metal foil, electrons are stripped off, the sign of the ion charge is changed and the particles are accelerated back to ground, gaining in that way double the energy. This method was applied to Van de Graaff generators in 1955 but was already invented thirty years earlier. In 1932, in a paper *Method for Multiplying Canal Ray Energies for Nuclear Disintegration*, Gehrtsen (Ge 32) reported on an experiment in which protons were accelerated to a tube electrode. Inside the tube they became neutralized by gas collision in order to avoid their deceleration in running against the subsequent opposite voltage. They were charged again and accelerated in a second step with the same voltage gaining double the energy. Gehrtsen's Ph.D. student Peter, in a subsequent paper (Pe 36), reported that he applied this method five times obtaining a 300-KeV proton beam of the order of nanoamps with a 60 KeV dc power supply. Dempster (De 32) in Chicago, independently reported also on such a device on a smaller scale. H. I. Kallmann (Ka 33) in the same year worked with multicharged ions, changing the charge between accelerations. (Later, in 1949, he introduced photomultipliers to nuclear physics.)

Other groups approached the problem of getting high voltages in different ways. I bring to your attention the experiment of Breit and Tuve in 1928 using Tesla coils. They were quite successful after putting the coil in an insulating coil under a pressure of about 40 atmospheres. In a letter to *Nature* (Br 28), they reported to have reliably reached 5 megavolt with a repetition rate of 60 cycles/sec and 10 μ sec pulse length.

In 1932 Brasch and Lange (Bra 31) used a powerful Marx generator. For the first time they were able to accelerate electrons up to 2.4 MeV. The pulse current was extremely high, about 1000 amps with a 10 sec pulse length and a repetition rate

Fig. 9 . Belt generator built by Tuve, Hafstad, and O. Dahl, 1933.



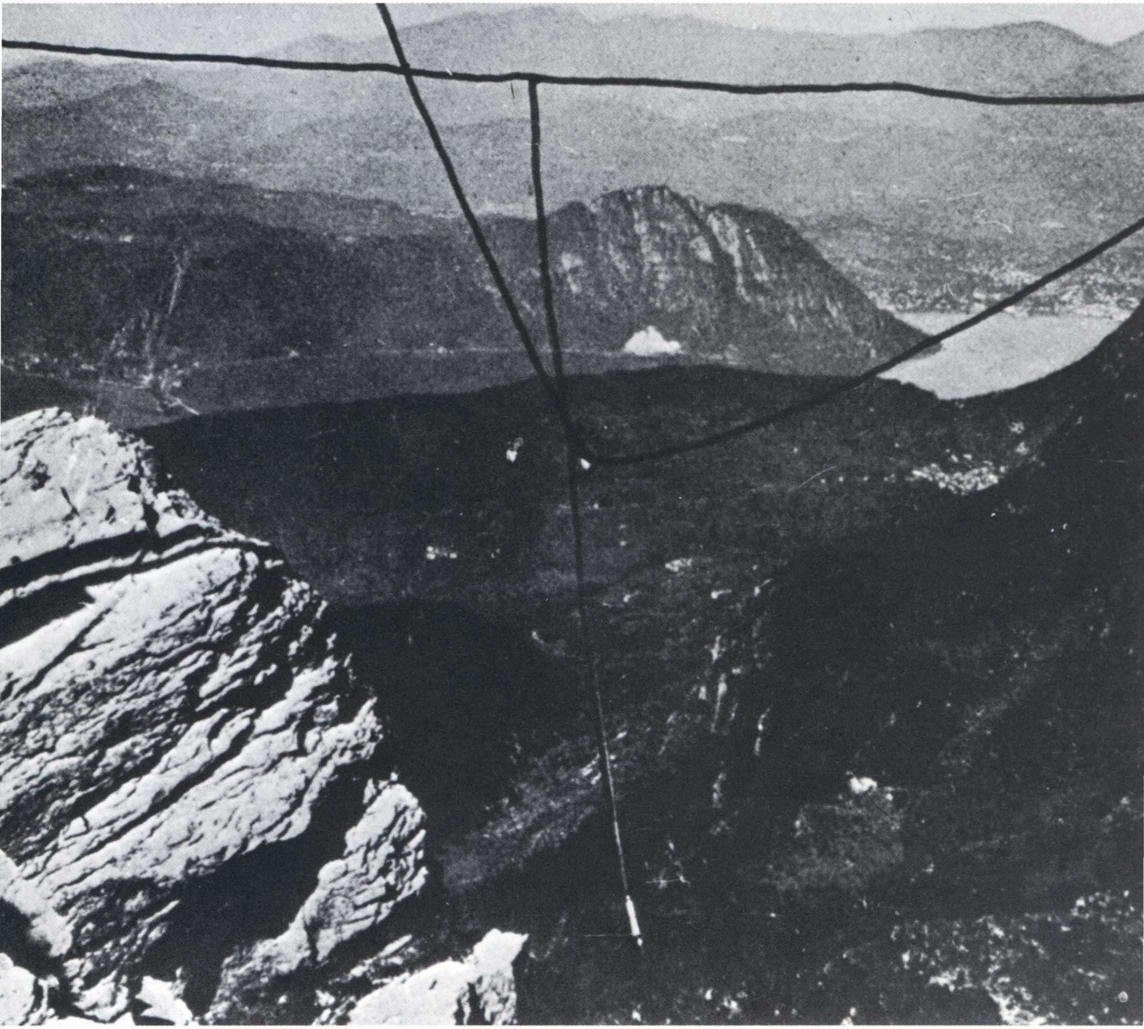


Fig. 10. The "antenna" installed at Monte Generoso by Brasch and Lange, 1930.

of 2/sec. The intensity of the x rays were so strong that behind 10 cm of lead a photographic film was still blackened. As far as I know, such devices are now used in experiments for collective acceleration.

Brasch and Lange (Bra 31) also tried a very unusual method of getting high voltages using thunderstorm electricity. For that purpose they stretched an isolated metallic net between two peaks 700 m apart on Monte Generoso at the Swiss-Italian border. This "electrode" collecting atmospheric electricity was connected at the ground to a discharge tube and parallel to a spark gap for measuring and limiting the voltage. They observed voltages up to 15 megavolt and 8 megavolt with a reasonable rate (Fig. 10).

The experiment was stopped in 1933 because one of the co-workers had a fatal accident at the experiment. Lange emigrated in 1933 to the Soviet Union, Brasch to the United States.

These experiments led to a blind alley but they were of great value for the development of discharge tubes. The technique of dividing the tube in many sections made of ceramics or resins and separated by metallic iris diaphragms has been in common use since that time.

II. The Betatron

Our next candidate is the Betatron or the accelerator of electrons by magnetic induction.

The idea to use the electric field around a time-varying magnetic flux for acceleration of electrons which are guided in a magnetic field has many fathers. S. Slepian in 1922 (Sle 22) took a patent on the idea, which was published in 1927, but he never made an experiment (Fig. 11). Breit and Tuve, at that time at the Carnegie Institution used high-frequency coils but were without success as they did not achieve stabilization of the electron orbit.

The first, who had in principle all the ingredients in his hands, was the Norwegian Rolf Wideroe. In 1923 as a very young student he was fascinated by the problem. He calculated the magnetic field configuration needed to keep the radius of electrons constant during acceleration and found the condition that the field at the orbit must be half of the average field within the orbit. Two years later he realized that in addition the electrons must be stable in the orbit. He came to the conclusion that the guide field must decrease with the radius according to a power law with a field index $n = (\Delta B/B_0)(r0/\Delta r)$ between 0 and 1. He did not publish his results at that time but one can still read it in his notebook (Fig. 12). His published experiments in 1928 (Wi 28) were not successful. As an electrical engineer he had no experimental experience with free

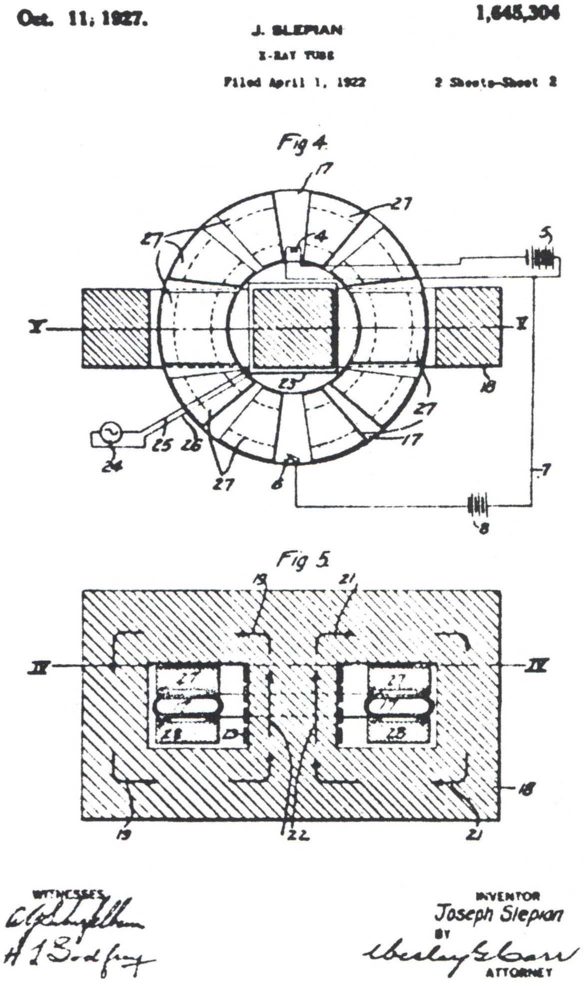


Fig. 11. Slepian patent: "X-Ray Tube," electron acceleration by induction, 1922.

electrons and surprisingly in the experiment he made no use of the field conditions he had calculated for the orbit stabilization. But it is worth mentioning that in his notebook he gave the parameters for a 100-MeV accelerator. In 1923, and for a 21-year old student, this is an astonishing feat.

In 1928 Rutherford stimulated Walton (Wa 29) to study the problem. He also found mathematically the focusing conditions. For the experiment he used a low pressure ring discharge as the electron source and failed. Such a Plasmabetatron, as we would call such a device today, was brought to some success by Budker and by Drees and Paul and others 30 years later, but it turned out not to be very useful for the purpose envisaged.

In 1935 M. Steenbeck at the Siemens Company also took up the problem of acceleration by magnetic induction. Knowing the Wideroe 1:2 conditions, he formulated the magnetic focusing condition for electrons on the stable orbit: $0 > n > 1$, as Wideroe already knew. An experimental set up was built. He certainly observed accelerated electrons, but with extremely low intensity (Ste 35,43), due to the fact that he did not preaccelerate the electrons before injection into the orbit.

The first successful induction accelerator was built by Donald Kerst in 1940 (Ke 40). He was just the better, more steadfast and more careful experimentalist than all the others, especially in the magnet design. His first apparatus brought 2.3 MeV for the electrons. He called the apparatus at first Rheotron, later Betatron. In a paper with Serber (Ke 41) he gave in 1941 the full theory of its operation.

This success of Kerst, published in the last issue of the *Physical Review* to come to Germany during the war, strongly influenced my personal life. At that time I was an assistant to H. Kopfermann, who worked for years on the isotope shifts in atomic spectra, in order to get information about the charge distribution in nuclei. Immediately he realized that scattering experiments with high-energy electrons would be a superior method. He stimulated me to give up spectroscopy and to convert to the new technique of electron accelerators.

K. Gund had started also at Siemens a new attempt in constructing for medical purposes an "Elektronenschleuder" (centrifuge), as he called the Betatron in those days. In 1943 6 MeV was reached, and Gund succeeded in 1947 in extracting an electron beam out of the vacuum tube for the first time (Gu 47). At the same time, Wideroe with R. Kollath and B. Touschek, built in Hamburg a 15-MeV "ray transformer," which successfully came into operation in 1944. Fifteen years later Touschek made important contributions to the development of electron storage rings.

It is characteristic for the time before the war that the physicists involved in such developments worked completely

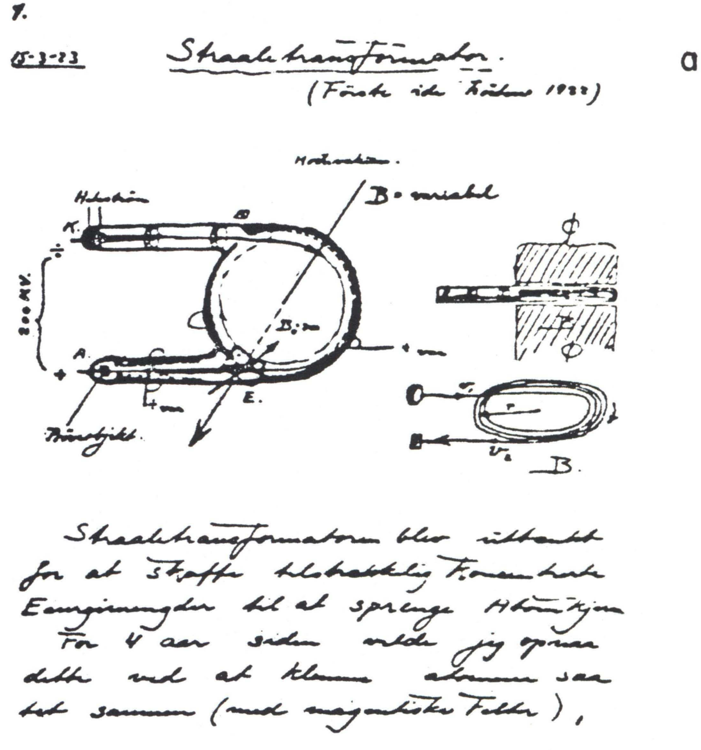


Abb. 2 und 3 *Auszüge meiner ersten Notizen (in norwegisch)*

Fig. 12. Pages of the notebook of R. Wideroe concerning acceleration by induction. (a) "Strahlentransformator"; (b) Derivation of the focusing condition, Aachen 1925.

b

Wir wollen jetzt die Gültigkeit der Vernachlässigung der Tangentialbeschleunigung näher untersuchen.

Bewegungsgleichung:

$$t = \frac{1}{\sqrt{M}}\left[\arcsin \frac{M\cdot s - N}{N} + \frac{\pi}{2}\right]$$

woraus:

$$t = \frac{1}{\sqrt{1+K}}\,\frac{r_0}{v_t}\cdot \arccos\left(1 - \frac{s}{\Delta r}\right)$$

oder:

$$s = \Delta r\left(1 - \cos\sqrt{1+K}\cdot\frac{v_t}{r_0}\cdot t\right)$$

Wir wollen zuerst die Gleichung der Bahnkurve (in Polarkoordinaten) aufstellen. Für den Radius Vektor haben wir:

$$r = r_0 + \Delta r - s = r_0 + \Delta r\cos\left(\sqrt{1+K}\,\frac{v_t}{r_0}\cdot t\right)$$

$$K = \frac{\Delta B}{B_0}\cdot\frac{r_0}{\Delta r}$$

Abb. 4 *Foto einer Seite meines Notizbuches aus Aachen (Sommer 1925)*

Prinzip einer Methode zur Herstellung von Kanalstrahlen hoher Voltzahl.

Von

GUSTAF ISING.

Mit 2 Figuren im Texte.

Mitgeteilt am 12. März 1924 durch C. W. OSEEN und M. SIEGBAHN.

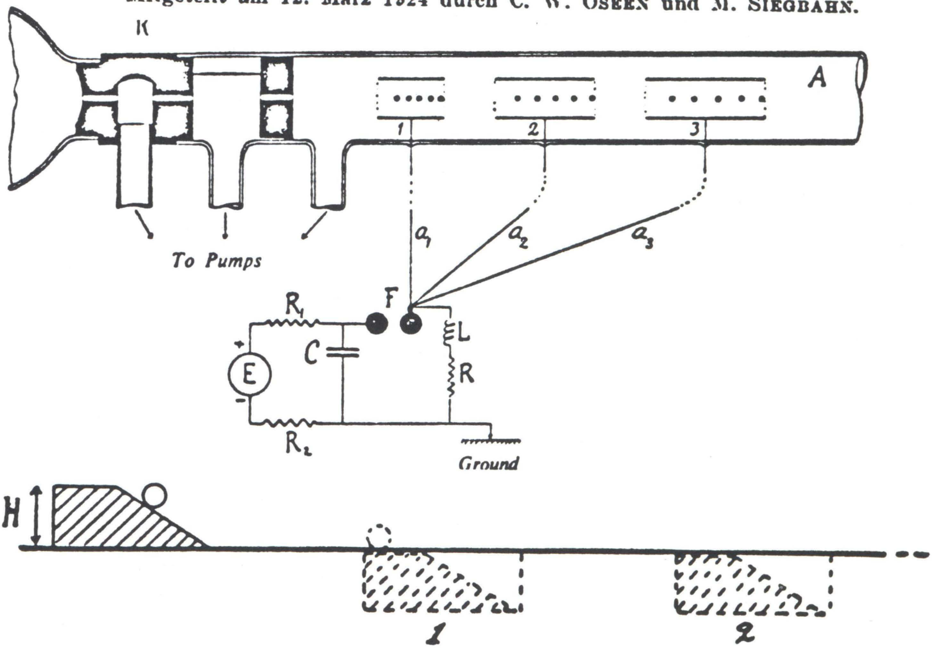


Fig. 13. Linear accelerator proposed by G. Ising 1924.

independently without knowing each other. There was no exchange of ideas or experiences; symposia or special conferences which we feel today to be indispensable were almost never held. Nuclear tourism did not yet exist.

III. Resonance Accelerators

When Wideroe failed in accelerating electrons according to the Betatron principle, he turned in order to save his doctoral thesis in Aachen, to another method proposed by the Swede, G. Ising in 1924. In a paper *Principle of a Method for the Production of Canal Rays of High Voltage*, Ising (Is 24) described the possibility of accelerating ions in the front of a radio-frequency wave traveling along a tube as is shown in Fig. 13. The wave front arrives at the accelerating electrodes at the same time as the ions. But Ising performed no experiment. Wideroe modified the method a little bit using drift tubes alternately connected to ground and an rf voltage generator. The travel time inside the field-free drift tubes is kept half the oscillation time of the rf voltage U_o by increasing their length proportional to the ion velocity. In that way the ions see in all gaps an accelerating field. The total energy is then given by the voltage U_o multiplied by the number of gaps. This time Wideroe was successful in a pilot experiment with only two gaps using Na- and K-ions (Wi 28). He showed that the method worked according to his theoretical prediction of resonance acceleration (Fig. 14). As an example he gave the parameters for a heavy-ion (Cesium) accelerator for 2 MeV. With an rf voltage of 170 KV and a frequency of 1.7 MHz the length of the apparatus would be only 1.20 m.

With this experiment based on Ising's idea, the method of resonance acceleration was born, followed by many variants and improvements leading to the powerful cyclotrons, synchrotrons, and linacs of today. One story reported by Wideroe (Wi 64) is worth mentioning. In a discussion at Aachen in 1928 he was asked by Flegler, a professor of electrical engineering, if it is possible to bend the ion beam in a circle in a magnetic field and to use the same accelerating gap repeatedly. Wideroe answered that it would be possible in principle, but the beam would not be stable. He had calculated the stabilization by a proper magnetic field but the space-charge effects would compensate the focusing forces already at a current of 10 mA. And in another note that Wideroe sent to me, he says that for him as an electrical engineer accustomed to many amps, milliamps were negligible. At that early time he did not know that physicists were already happy with microamps. However, Wideroe missed the invention of the cyclotron in which Ernest Lawrence succeeded two years later.

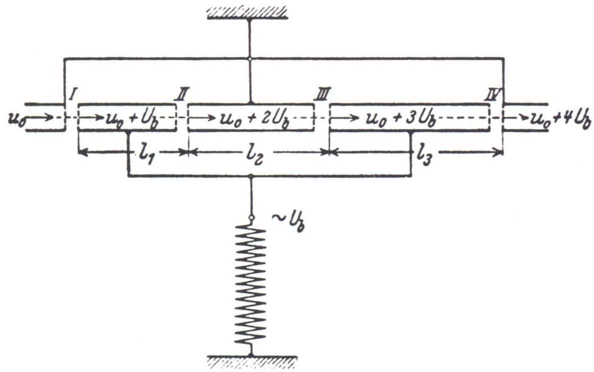
The *History of the Cyclotron* was extensively reported by M. S. Livingston and E. M. McMillan in memorial lectures to E. O. Lawrence in 1959 and published in *Physics Today* (Liv 59). Lawrence, in his Nobel Prize Lecture (1951), tells us about the background of his great invention: When he became a physics professor at Berkeley in 1928 he planned to join the new exciting field of nuclear physics and he was taking into consideration all types of high-voltage generators for an accelerator in Berkeley. One evening in early 1929 he was glancing over current periodicals in the library and came across the article by Wideroe on multiple acceleration. He could not read German well, but from the formulas and figures he was aware that this method was just what he needed to compete in the race for higher energies. Thinking big he realized that for millions of electron volts it would be advantageous to bend the linear beam of Wideroe to a circle in a magnetic field. He realized soon that the rotation frequency of the particles is independent of the velocity as the orbit radius increases. Therefore a fixed frequency on the electrodes can be used depending on (e/m)B. For realization he gave the problem at first to the student N. E. Edlefsen. In spite of the fact that the experiment did not work so well, the method was published (La 30).

Then the task was passed to Stan Livingston. He was just the right man for it and in his Ph.D. thesis he reported on the first cyclotron resonance with a quickly built small model for 80 KeV protons with D-shaped electrodes with an applied potential of only 1000 volts (Fig. 15a,b). In December 1932 with a larger device 1.2 MeV were achieved and the first nuclear disintegration was observed. A new chapter in nuclear experimental technique was opened. Figure 16 shows the next stage, the 27-inch cyclotron with the proud-looking Lawrence and Livingston in front of it.

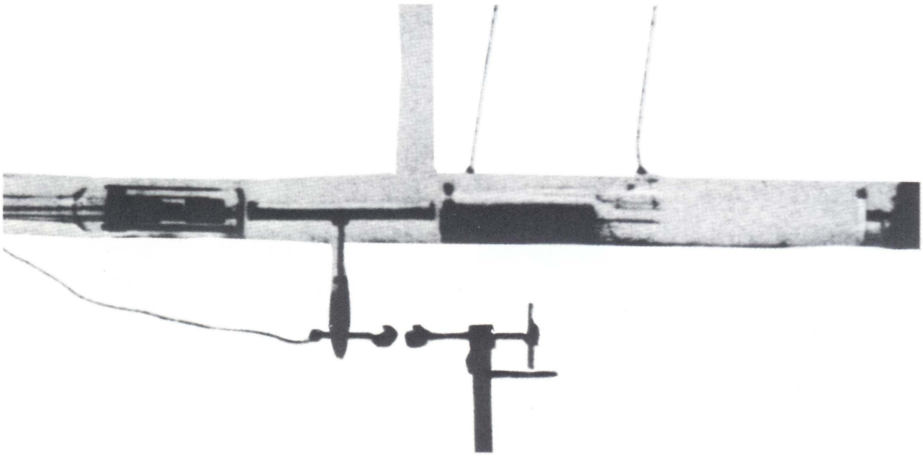
Lawrence was quite lucky that his experiment fulfilled the two conditions necessary for a successful operation of a cyclotron: beam focusing and isochronism. The magnet with its wide gap automatically gave a field which decreases with the radius necessary for radial focusing and the gap between the dee's provided an electric field suited for axial stabilization. Qualitatively this behavior was understood by the authors but the full theoretical treatment and understanding of these problems came after the experiment. R. R. Wilson, also in Berkeley, published the relevant paper on *"Magnetic and Electrostatic Focusing in the Cyclotron"* (Wil 38). Only seven days earlier M. E. Rose (Ro 38) submitted his paper dealing with the focusing problem and with the maximum available energy in the cyclotron. H. A. Bethe (Be 37) had just remarked that due to the relativistic mass increase the particles may fall out of isochronism during the acceleration process.

Lawrence not only developed the cyclotron with Livingston, but also followed at the same time the other way opened by Wideroe, the linear accelerator. In this field D. Sloan was his co-worker.

In 1931 Lawrence reported in the *Proceedings of the National Academy of Sciences* (La 31) that they had succeeded in



Principle of voltage transformation by means of potential fields.



The experimental tube.

Fig. 14. Wideroe's first linear accelerator.

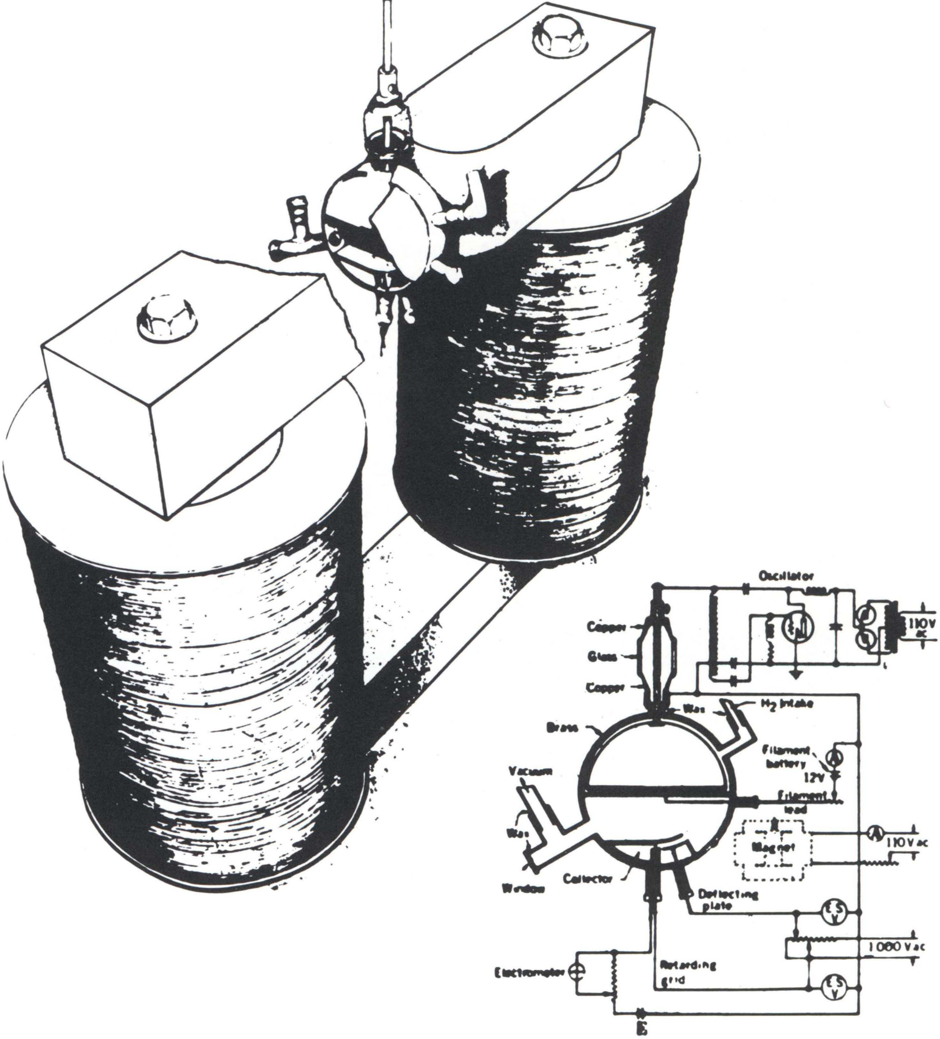


Fig. 15a. First cyclotron by E. O. Lawrence and M. S. Livingston.

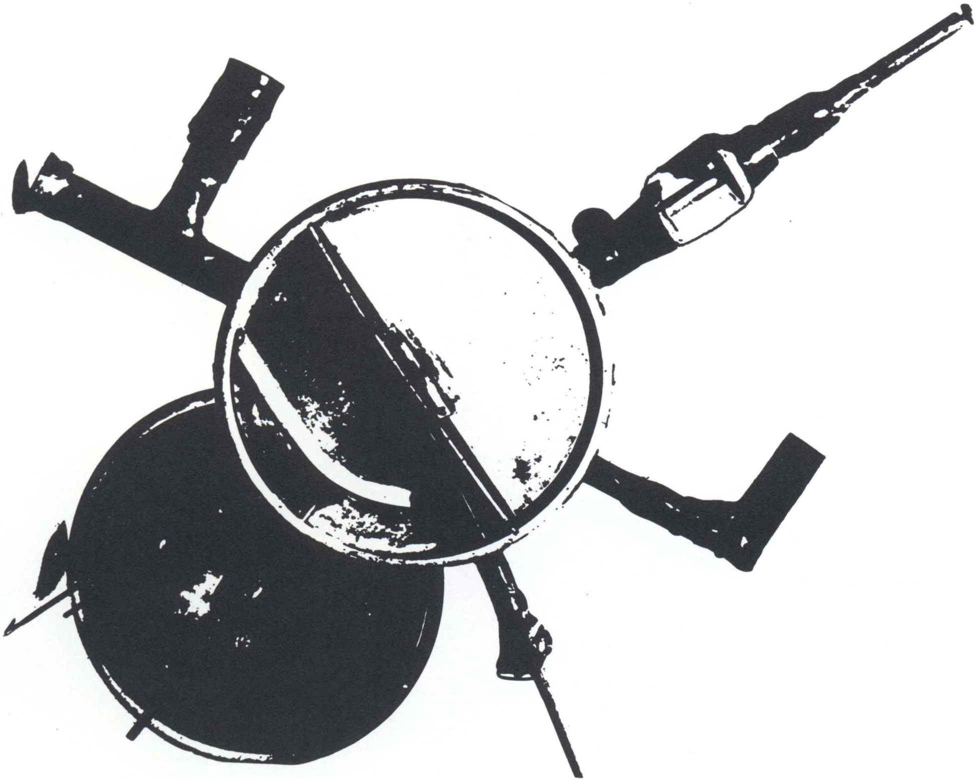


Fig. 15b. The vacuum chamber with the D.

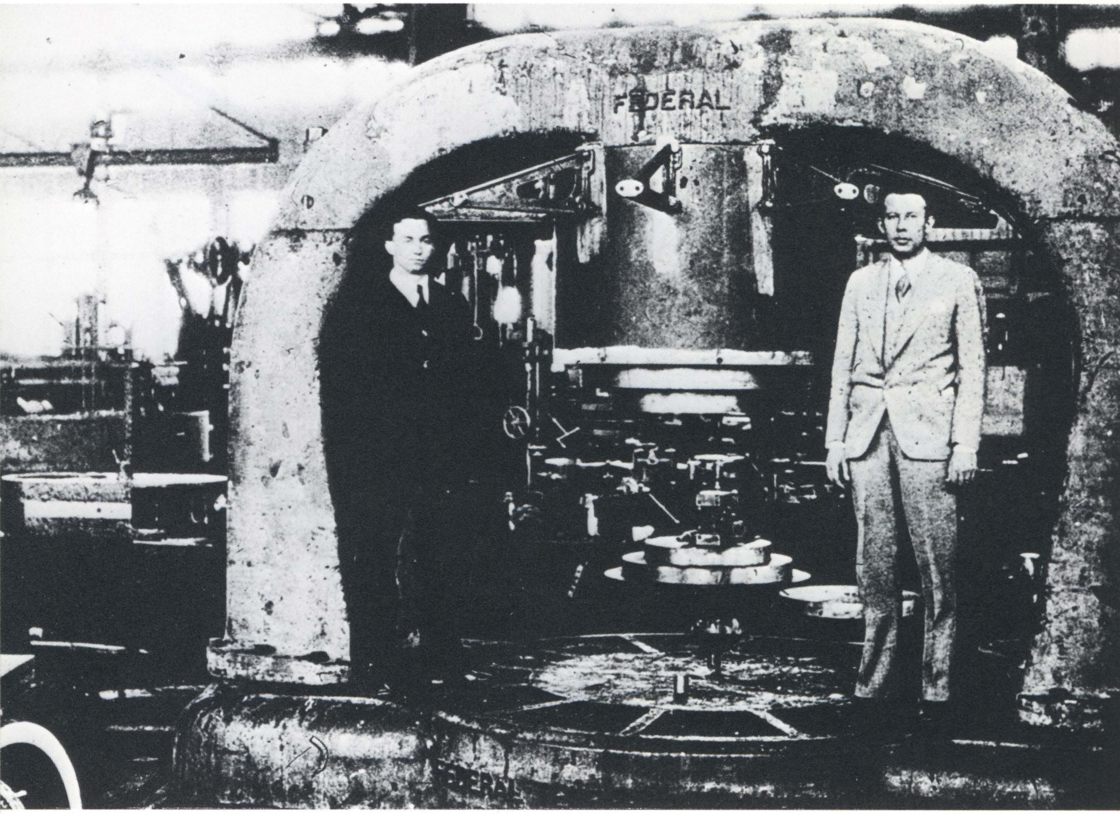


Fig. 16. The 27-inch cyclotron (Lawrence and Livingston posing in front of it).

accelerating mercury ions in a linac 1.07 m long with 20 stages up to 205 KeV with only 6.2 kV rf voltage of 4.5 mHz. The year after, with increased voltage they achieved 1.26 MeV at a current of 10^{-7} amps.

All these devices depend strongly on radio-frequency generators of high power and high frequencies. During the war such generators were developed for radar systems. High power magnetrons and klystrons became available, and the first proton linacs were successfully constructed by L. Alvarez (Al 46) at Berkeley and by the late John Williams in Minnesota. The largest linacs of this type are at present the proton meson factory at Los Alamos (800 MeV) and for heavy ions up to uranium the Unilac in Darmstadt combining the Wideroe and Alvarez acceleration structures.

Electron linear accelerators with their special problems of relativistically moving particles profited from the radar experiences in the so-called S-band. W. W. Hansen (Ha 48) is especially deserving of mention for his work in developing these traveling wave-guide accelerators, which Ising had proposed 20 years earlier in a primitive way. Another 20 years later the gigantic 2-mile long Stanford accelerator for 20 GeV came into operation.

IV. Synchronous Accelerators

As mentioned above, particles in a cyclotron fall out of resonance with the accelerating radio-frequency field due to the relativistic mass increase, and cease to gain energy. In a profound theoretical paper Thomas (Tho 38) treated all the connected problems in detail and showed quantitatively how one should overcome them experimentally. In order to achieve high energies he proposed that the magnetic field should not only vary with the radius but also with the azimuth. On his ideas the isochron - and the spiral-ridge cyclotron are based. But it took 20 years before these types of accelerators were realized. The strict mathematical treatment of the problem was not easily understood by the experimentalists.

In addition to the method proposed in Thomas's paper, there are two other ways to achieve high energies, the synchrocyclotron and the synchrotron. In both devices the parameters of the accelerator are changed adiabatically with increasing particle energy. In the *synchrocyclotron* one changes the frequency of the rf generator during the acceleration process according to the mass increase while the particle spirals out in the constant magnetic field.

In the *synchrotron* one keeps the orbit radius constant, therefore the magnetic field has to be increased according to the momentum. As long as the velocity of the particles is still increasing, the radio-frequency has to be changed too, but it can be kept constant when the particles approach the velocity of

Fig. 17. The Fermilab 500-GeV synchrotron with the first superconducting magnets for the Energy Doubler.

light as is the case for electrons already at low energies. Due to the fixed-orbit radius only a ring magnet is required. But one has to pay a price for it. It is obvious that in these types of accelerators no continuous acceleration is possible anymore. They must operate in a pulsed mode, one acceleration process after the other.

Independently these principles were developed in 1945 by V. Veksler in Russia (Ve 45) and E. McMillan (Mil 45) in Berkeley and even Wideroe in 1946 took a patent on it (Wi 46). The authors showed that if the field or frequency variations in time proceed adiabatically, synchronism between revolution and radio frequency is automatically maintained. This result opened the door for the acceleration to ultra-high energies. At first the method was applied to electron acceleration and later to protons, culminating in the large 500-GeV accelerators at CERN and Fermilab, where one envisages in the near future 1000 GeV (Fig. 17). But these machines would not have been feasible either from the technical or from the economic point of view, if the method of strong focusing had not been invented.

Strong Focusing and the Use of Multipole Magnetic Lenses

The first synchrotron used the rules which were developed for the Betatron for focusing the particles on the orbit. The particles were guided by a magnetic field which was rotation symmetric and decreased with the radius according to a power law $B \sim r^{-n}$. In such a field the particles oscillate with a Betatron" frequency (number of oscillations per revolution) $Q_v = n^{1/2}$ in axial direction, whereas the radial frequency is given by $Q_v = (1-n)^{1/2}$ due to the fact that the centrifugal force is proportional to r^{-1}. Therefore one has to compromise in the choice of n between 1 and 0. This restricts the restoring force to a relatively low value which results in large amplitudes of the particles. Due to this weak focusing the guiding ring magnet must be relatively wide and becomes expensive.

In 1952 Courant, Livingston, and Snyder (Cou 52) from the Brookhaven Laboratory found a very sophisticated method of substantially improving these conditions in order to achieve a "strong-focusing" device. But soon they realized that a Greek engineer N. Christofilos (Chri 50), had already taken a patent on the same idea in 1949, despite the fact that he had no previous experience with accelerators (Fig. 18). They proposed to abandon the concept of a magnet ring with a field configuration constant in azimuth and instead to cut it into sections with a field-index alternating between high positive and negative values. In this case one gets alternating focusing and defocusing forces in both the radial and vertical directions. In such a field configuration the equation of motion is represented by the Hill equation which results in stable and unstable orbits. In the case of stability, which depends only on the field parameters, the oscillation amplitudes of the particles are substantially reduced compared with the previous weak-focusing method, in spite of the

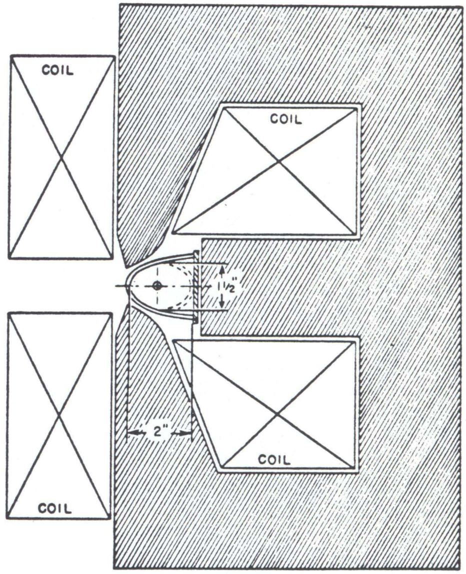


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FIG. 5. Cross section of E-magnet with poles shaped to give $n = 3600$ at an orbit radius of 300 ft. The vacuum chamber illustrated has an internal aperture of about 1×2 inches.

E. D. Courant, M. S. Livingston,† and H. S. Snyder

The Strong-Focusing Synchrotron—A New High Energy Accelerator*

Fig. 18. Strong-focusing magnets. (a) Courant et al., 1952; (b) Christofilos, 1950.

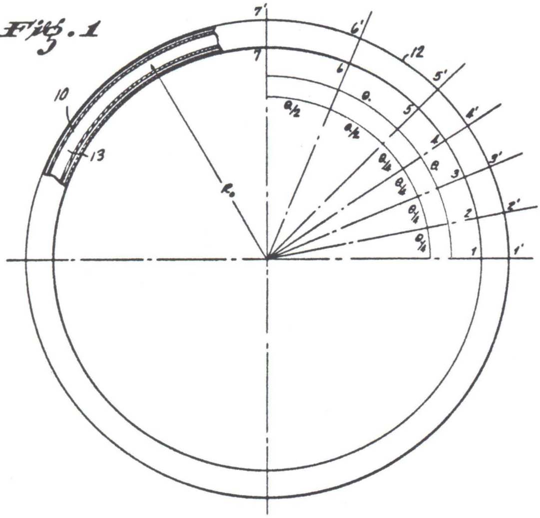
b

Nicholas Christofilos

Focussing System for Ions and Electrons

[U.S. Patent No. 2,736,799 (filed March 10, 1950, issued February 28, 1956).]

Fig. 1



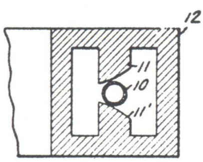


Fig. 2

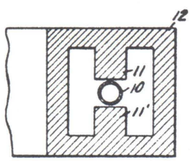


Fig. 3

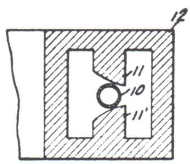


Fig. 4

fact that for half the time they see strong-defocusing forces. Figure 19 illustrates the size of the vacuum tube in weak- and strong-focusing accelerators and thus the width of the respective magnet gaps. The decisive progress is obvious.

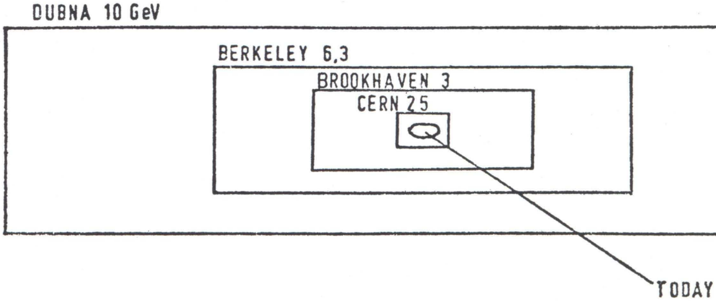


Fig. 19. The size of vacuum chambers. The synchrotron (Dubna) the Berkeley and Brookhaven accelerators are wesk-focusing synchrotrons, the others use the strong-focusing principle.

Immediately after the new principle worked out in Brookhaven became known, the newly founded European Laboratory for Nuclear Research (CERN) took up the idea. The already approved plans for a conventional weak-focusing proton synchrotron for 10 GeV were changed. A rough estimate showed that for the same cost a 25-GeV accelerator could be built. It came into operation in 1959, only a few months in advance of the Brookhaven machine. But it is worth mentioning that the first strong focusing synchrotrons to deliver beam to experiments in the USA and Europe, were the electron synchrotrons at Cornell University and the University of Bonn.

In the same paper, Courant et al., showed that quadrupole magnets are good cylinder lenses for particles and that a system of alternate polarity quadrupoles can serve as a beam-transport system over long distances. Only a few weeks later, Kitagaki from Tohoku University in Japan (Ki 53) proposed to separate in a synchrotron the task of focusing and bending the particles in a circle by using alternating quadrupole lenses in straight sections between dipole bending magnets. Already at that time, Kitagaki gave the parameters for a 100-GeV proton accelerator. Only in 1962 did this idea of a separate-function machine come up again and was first realized at the Fermilab 500-GeV synchrotron.

However, the conception of non-circular symmetric "magnetic or electrostatic lenses for charged particles" existed already in electron optics. In 1947, a paper by O. Scherzer (Sche 47) dealt with this problem and introduced lenses of even higher symmetry than quadrupoles for achromatic and non-linear corrections. Friedburg and Paul had also shown that such multipole configurations are good lenses even for neutral atoms and neutrons.

In this first period of accelerator development, experimental physicists and a few engineers were pioneering with imagination but relatively low theoretical background. Building accelerators was an art; the principles were understood only in first approximation and the success in properly shimming the magnets or matching the primitive rf generators relied strongly on the skill or intuition of the experimentalist. In a next step, high-rank theoreticians joined the field, calculating the complicated motion of particles moving nearly relativistically in complex magnetic and electric fields and teaching the experimentalists the theoretical background for the proper design of better and better accelerators.

V. Storage Rings

The development of storage rings was started later, initiated in 1956 by D. Kerst (Ke 56) and his co-workers from the MURA group. Shortly thereafter O'Neil (On 56) submitted a proposal for an electron colliding-beam device. Touschek was the first to point out the importance of the physics with electron-positron colliding beams and proposed a technical solution for using only one magnet ring. In an early experiment, together with a Frascati group, he succeeded in storing electrons for a considerable time (Tou 60) (Fig. 20). Again, the principal idea was anticipated by R. Wideroe in a patent in 1943 (Wi 43), but his patent did not treat the stability problems in any detail. Stability problems are however of decisive importance in such devices with 10^{12} and even more revolutions of the particles such as one observes in the ISR at CERN, a thousand times more than the earth has circulated around the sun. A proton current of 40 amps at an energy of 30 GeV is circulating in this extraordinary device. The method of filling so many particles in the rings is based on the rf beam-stacking method worked out by the MURA group. All in all, it means that the physicists have to master particle optics to the utmost.

At present we are again in the early days of a new technique. The dream of experimentalists to observe antiproton-proton collision in the center-of-mass system at high energy now seems to be feasible. Antiprotons are produced in high-energy collisions but with a large phase space. For filling these particles in a storage ring one has not only to match their phase space to that of the ring but also to increase the phase-space density. In other words, one has to cool the antiprotons.

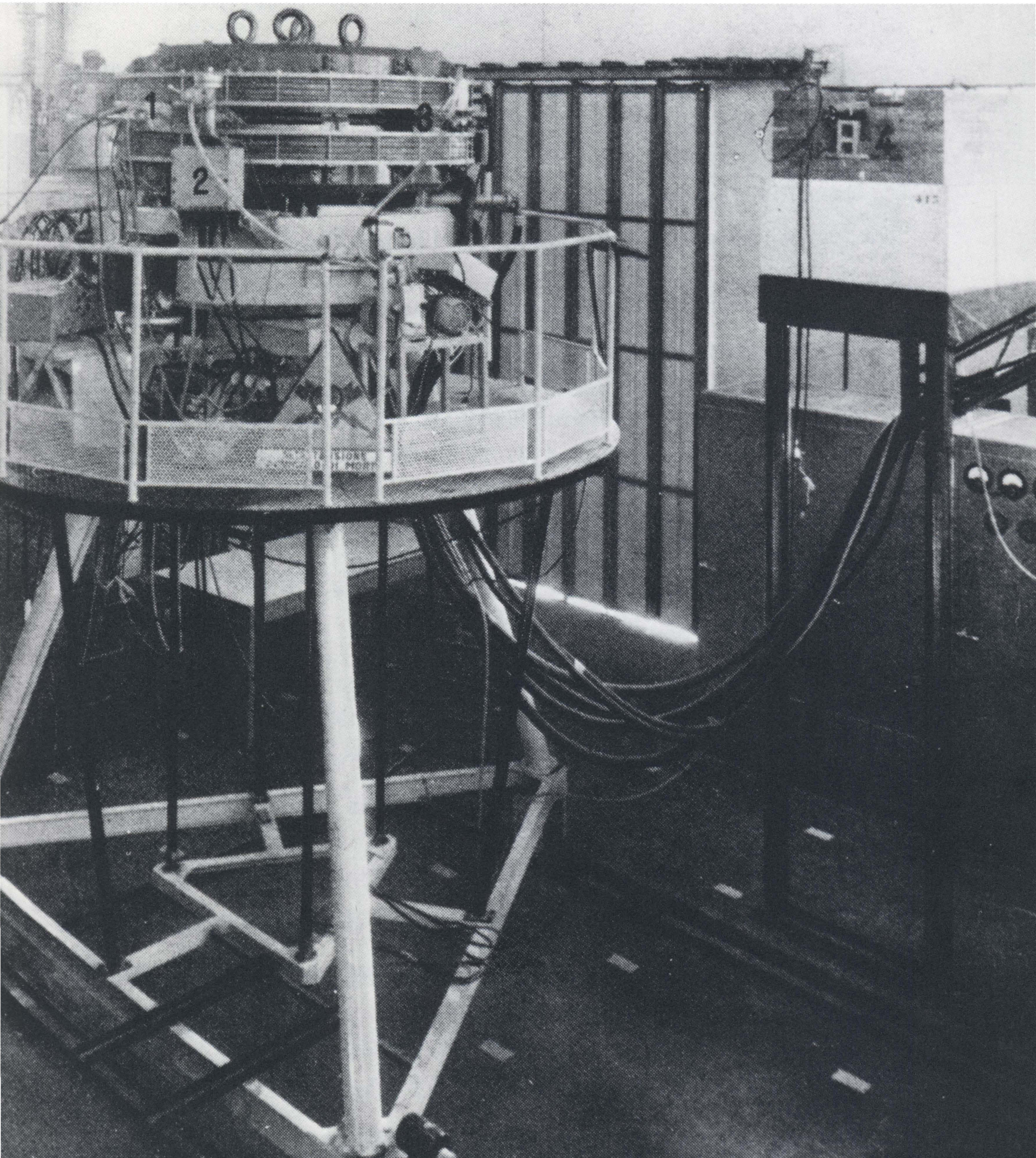
In 1966, Budker (Bu 66) in Novosibirsk had the first idea of how to do it. He used the method of a heat-exchanging device. Along the straight section of a storage ring for the protons a "cool" electron beam of nearly the same velocity as the protons is brought in close contact to them. Both particles exchange momentum which is taken away by the always new cool electrons.

Van der Meer at CERN (Me 72) invented a second method--the process of stochastic cooling. It makes use of a "Maxwellian demon" who measures the statistical fluctuations in the ensemble of "hot antiprotons." With this information, taken from a section of the circulating proton beam, one controls an rf field on the opposite side of the ring in order to minimize the fluctuations, or to decrease the entropy of the particles in the beam. As the protons are running on a circle and the correcting signal propagates along the diameter, the correcting signal can be applied in time.

As both cooling devices look very promising in many respects, accelerator physicists are faced again with a new task; they have to learn statistical mechanics!

All in all, accelerator physics is a fascinating field, covering many disciplines and always prepared to follow new theoretical ideas or to incorporate new experimental techniques.

Fig. 20. The electron storage ring "Ada" at the Frascati laboratory.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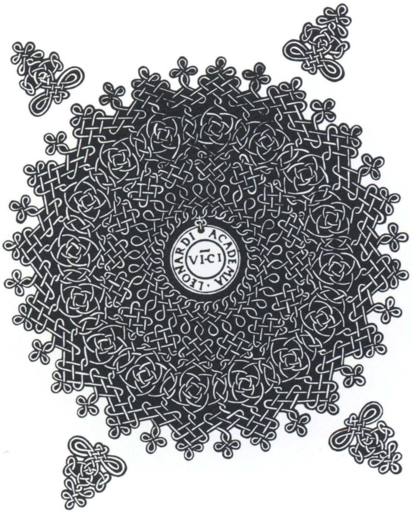
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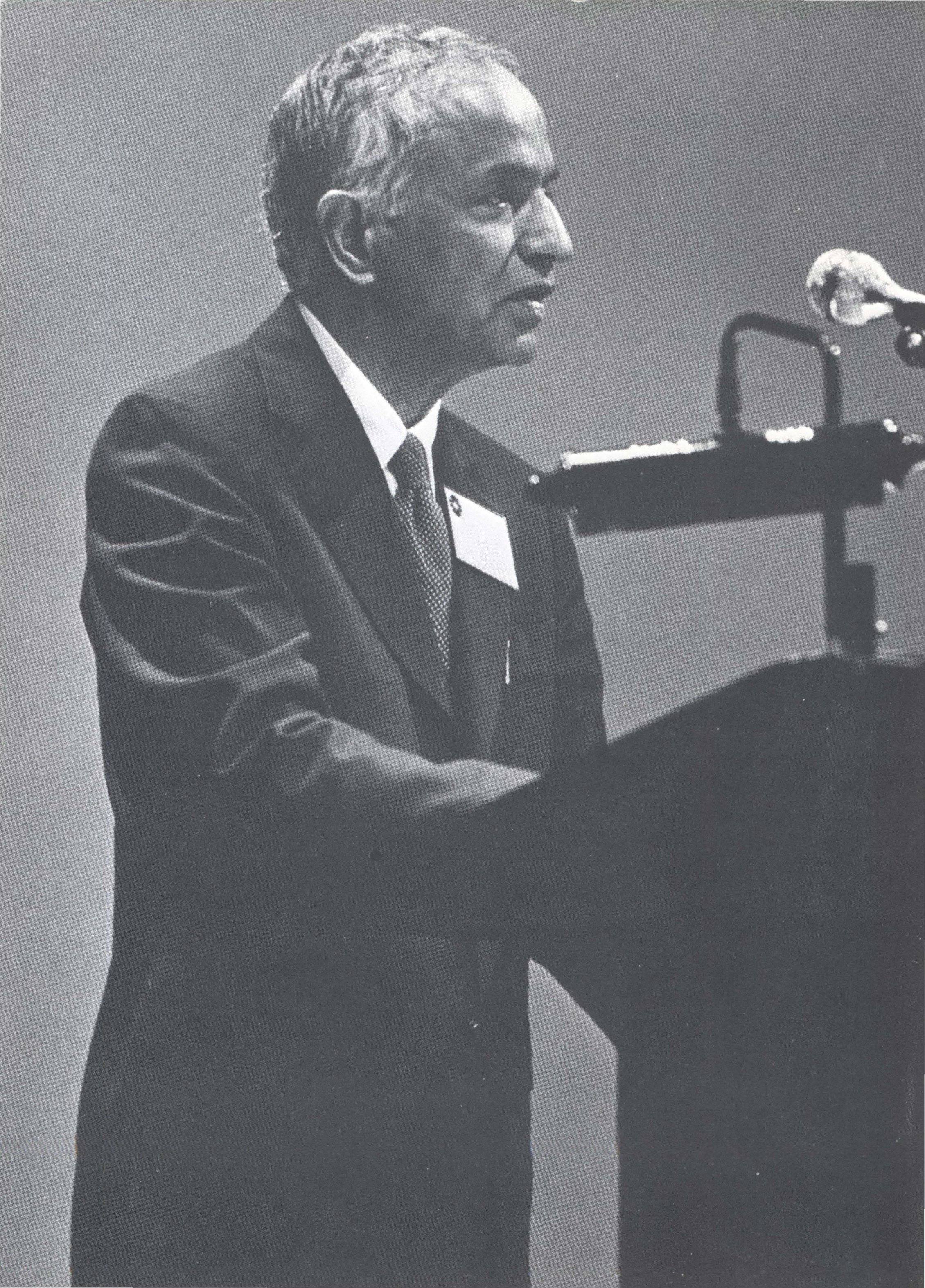


Beauty And The Quest For Beauty In Science

S. Chandrasekhar

I am afraid that I am a stranger amongst you. An audience assembled to pay tribute to Robert Wilson, for his immense contributions to physics and to the community of physicists, is necessarily interested in exploring the nature of the ultimate things by means which are equally the ultimate. I cannot profess to have been in these realms. Besides, the topic to which I have been asked to address myself is a difficult one, if one is to avoid the trivial and the banal. And, moreover, my knowledge and my experience, such as they are, compel me to limit myself entirely to the theoretical aspects of the physical sciences - limitations, most serious. I must, therefore, begin by asking for your patience and your forbearance.

All of us are sensitive to Nature's beauty. It is not unreasonable that some aspects of this beauty are shared by the natural sciences. But one may ask the question as to the extent to which the quest for beauty is an aim in the pursuit of science. On this question, Poincaré is unequivocal. In one of his essays he has written:

> The Scientist does not study nature because it is useful to do so. He studies it because he takes pleasure in it; and he takes pleasure in it because it is beautiful. If nature were not beautiful, it would not be worth knowing and life would not be worth living. . . . I mean the intimate beauty which comes from the harmonious order of its parts and which a pure intelligence can grasp.

And Poincaré goes on to say,

> It is because simplicity and vastness are both beautiful that we seek by preference simple facts and vast facts; that we take delight, now in following the giant courses of the stars, now, in scrutinizing with a microscope that prodigious smallness which is also a vastness, and, now, in seeking in geological ages the traces of the past that attracts us because of its remoteness.

Commenting on these observations of Poincaré, J.W.N. Sullivan, the author of perceptive biographies of both Newton and Beethoven, wrote (in the *Athenium* for May 1919):

> Since the primary object of the scientific theory is to express the harmonies which are found to exist in

> nature, we see at once that these theories must have aesthetic value. The measure of the success of a scientific theory is, in fact, a measure of its aesthetic value, since it is a measure of the extent to which it has introduced harmony in what was before chaos.
>
> It is in its aesthetic value that the justification of the scientific theory is to be found, and with it the justification of the scientific method. Since facts without laws would be of no interest, and laws without theories would have, at most, only a practical utility, we see that the motives which guided the scientific man are, from the beginning, manifestations of the aesthetic impulse. . . . The measure in which science falls short of art is the measure in which it is incomplete as science . . .

In a perceptive essay on *Art and Science*, the distinguished art critic, Roger Fry (who may be known to some of you through Virginia Woolf's biography of him), begins by quoting Sullivan and continues:

> Sullivan boldly says: "It is in its aesthetic value that the justification of the scientific theory is to be found and with it the justification of the scientific method." I should like to pose to S. [Sullivan] at this point the question whether a theory that disregarded facts would have equal value for science with one which agreed with facts. I suppose he would say No; and yet so far as I can see there would be no purely aesthetic reason why it should not.

I shall return to this question which Roger Fry raises and suggest an answer different from what Fry presumes that Sullivan would have given. But I shall pass on now to Fry's observations comparing the impulses of an artist and of a scientist.

> From the merest rudiments of pure sensation up to the highest efforts of design, each point in the process of art is inevitably accompanied by pleasure: it cannot proceed without it. . . . It is also true that the recognition of inevitability in thought is normally accompanied by pleasurable emotion; and that the desire for this mental pleasure is the motive force which impels to the making of scientific theory. In science the inevitability of the relations remains equally definite and demonstrable, whether the emotion accompanies it or not, whereas, in art, an aesthetic harmony simply does not exist without the emotional state. The harmony in art is not true unless it is felt with emotion. . . . In art the recognition of relations is immediate and sensational - perhaps we ought to consider it as curiously akin to those cases of

> mathematical geniuses who have immediate intuition of mathematical relations which it is beyond their powers to prove. . . .

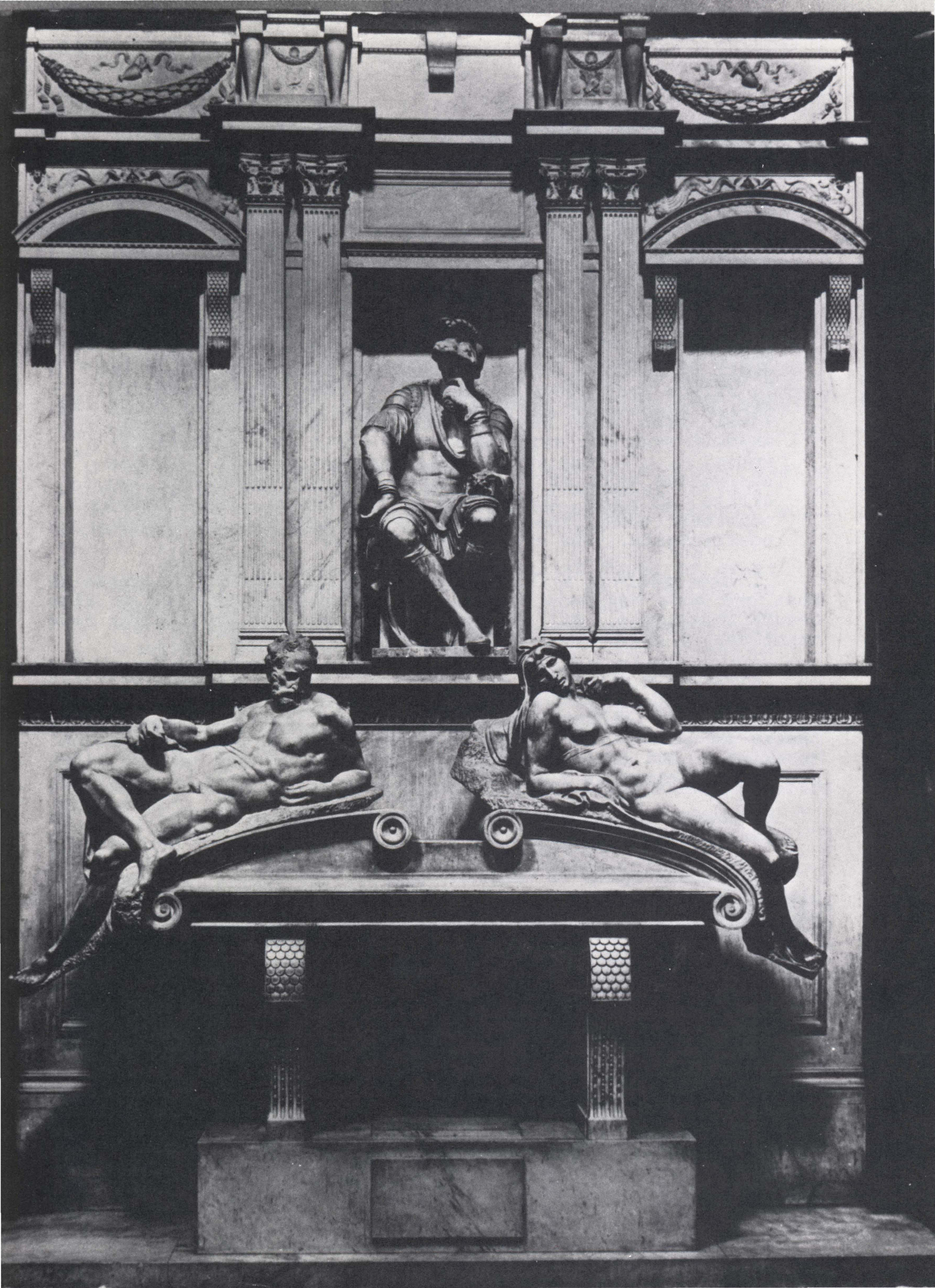
Let me pass on from these generalities to particular examples of what scientists have responded to as beautiful.

My first example is related to Fry's observation with respect to what mathematical geniuses perceive as true with no apparent cause. The Indian mathematician, Srinivasa Ramanujan (whose dramatic emergence into mathematical fame in 1915 may be known to some of you) left a large number of notebooks (one of which was discovered only a few years ago). In these notebooks Ramanujan has recorded several hundred formulae and identities. Many of these have been proved only recently by methods which Ramanujan could not have known. G. N. Watson, who spent several years of his life proving many of Ramanujan's identities, has written:

> The study of Ramanujan's work and the problem to which it gives rise, inevitably recalls to mind Lamé's remark that, when reading Hermite's papers on modular functions, "on a la chair de poule." I would express my own attitude with more prolixity by saying that such a formula as,
>
> $$\int_0^\infty e^{-3\pi x^2} \frac{\sinh \pi x}{\sinh 3\pi x}\, dx = \frac{1}{e^{2\pi/3}\sqrt{3}} \sum_{n=0}^{\infty} e^{-2n(n+1)\pi}(1+e^{-\pi})^{-2} \times (1+e^{-3\pi})^{-2} \ldots (1+e^{-(2n+1)\pi})^{-2}$$
>
> gives me a thrill which is indistinguishable from the thrill which I feel when I enter the Sagrestia Nuova of Capelle Medicee and see before me the austere beauty of "Day," "Night," "Evening," and "Dawn" which Michelangelo has set over the tombs of Guilano de' Medici and Lorenzo de' Medici.

An example of a very different kind is provided by Boltzmann's reaction to one of Maxwell's papers on the dynamical theory of gases in which Maxwell shows how one can solve exactly for the transport coefficients in a gas in which the intermolecular force varies as the inverse fifth power of the intermolecular distance. Here is Boltzmann:

> Even as a musician can recognize his Mozart, Beethoven, or Schubert after hearing the first few bars, so can a mathematician recognize his Cauchy, Gauss, Jacobi, Helmholtz, or Kirchhoff after the first few pages. The French writers reveal themselves by their



> extreme formal elegance, while the English, especially Maxwell, by their dramatic sense. Who, for example, is not familiar with Maxwell's memoirs on his dynamic theory of gases? . . . The variations of the velocities are, at first, developed majestically; then from one side enter the equations of state; and from the other side, the equations of motion in a central field. Ever higher soars the chaos of formulae. Suddenly, we hear, as from kettle drums, the four beats "put n = 5." The evil spirit V (the relative velocity of the two molecules) vanishes; and, even as in music, a hitherto dominating figure in the bass is suddenly silenced, that which had seemed insuperable has been overcome as if by a stroke of magic. . . . This is not the time to ask why this or that substitution. If you are not swept along with the development, lay aside the paper. Maxwell does not write programme music with explanatory notes. . . . One result after another follows in quick succession till at last, as the unexpected climax, we arrive at the conditions for thermal equilibrium together with the expressions for the transport coefficients. The curtain then falls!

I have started with these two simple examples to emphasize that one does not have to go to the largest canvasses to find beauty in science. But the largest canvasses do provide the best examples. I shall consider two of them.

Einstein's discovery of the general theory of relativity has been described by Hermann Weyl as a supreme example of the power of speculative thought, while Landau and Lifshitz consider the theory as probably the most beautiful of all existing physical theories. And Einstein himself wrote at the end of his first paper announcing his field equations:

> Scarcely anyone who fully understands this theory can escape from its magic.

I shall return later to consider wherein the source of this magic lies. Meantime, I want to contrast, in parallel with Einstein's expressed reaction to his theory, the feelings of Heisenberg at the moment of his discovery of quantum mechanics. We are fortunate in having Heisenberg's own account. He writes:

> It had become clear to me what precisely had to take the place of the Böhr-Sommerfeld quantum conditions in an atomic physics working with none but observable magnitudes. It also became obvious that with this additional assumption, I had introduced a crucial restriction into the theory. Then I noticed that there was no guarantee that . . . the principle of the conservation of energy would apply. . . . Hence I concentrated on demonstrating that the conservation law held; and one evening I reached the point where I was

> ready to determine the individual terms in the energy table [Energy Matrix]. . . . When the first terms seemed to accord with the energy principle, I became rather excited, and I began to make countless arithmetical errors. As a result, it was almost three o'clock in the morning before the final result of my computations lay before me. The energy principle had held for all the terms, and I could no longer doubt the mathematical consistency and coherence of the kind of quantum mechanics to which my calculations pointed. At first, I was deeply alarmed. I had the feeling that, through the surface of atomic phenomena, I was looking at a strangely beautiful interior, and felt almost giddy at the thought that I now had to probe this wealth of mathematical structure nature had so generously spread out before me.

In the context of these statements by Einstein and by Heisenberg on their discoveries, it is of interest to recall the following conversation between Heisenberg and Einstein which Heisenberg has recorded. Here is an extract:

> If nature leads us to mathematical forms of great simplicity and beauty - by forms, I am referring to coherent systems of hypotheses, axioms, etc. - to forms that no one has previously encountered, we cannot help thinking that they are "true," that they reveal a genuine feature of nature. . . . You must have felt this too: the almost frightening simplicity and wholeness of the relationships which nature suddenly spreads out before us and for which none of us was in the least prepared.

These remarks of Heisenberg find an echo in the following lines of Keats:

> Beauty is truth, truth is beauty - that is all
> Ye know on earth, and all ye need to know.

At this point, I should like to return to Roger Fry's question, I quoted earlier, namely, what one should make of a theory which is aesthetically satisfying but which one believes is not true. Freeman Dyson has quoted Weyl as having told him:

> In my work, I have always tried to unite the true with the beautiful; but when I had to choose one or the other, I usually chose the beautiful.

I inquired of Dyson whether Weyl had given an example of his having sacrificed truth for beauty. I learned that the example which Weyl gave was his gauge theory of gravitation which he had worked out in his *Raum-Zeit-Materie*. Apparently, Weyl became convinced that this theory was not true as a theory of gravitation; but still it was so beautiful that he did not wish to abandon it and so he kept it alive for the sake of beauty. But much later, it did turn out that Weyl's instinct was right after all, when the formalism of gauge invariance was incorporated into quantum electrodynamics.

Another example which Weyl did not mention, but to which Dyson drew attention is Weyl's two-component relativistic wave equation of the neutrino. Weyl discovered this equation and the physicists ignored it for some thirty years because it violated parity invariance. And again, it turned out that Weyl's instincts were right.

We have evidence, then, that a theory developed by a scientist, with an exceptionally well-developed aesthetic sensibility, can turn out to be true even if, at the time of its formulation, it appeared not to be so. As Keats wrote a long time ago,

> What the imagination seizes as beauty must be truth - whether it existed before or not.

It is, indeed, an incredible fact that what the human mind, at its deepest and most profound, perceives as beautiful finds its realization in external nature.

What is intelligible is also beautiful.

We may well ask; how does it happen that beauty in the exact sciences becomes recognizable even before it is understood in detail and before it can be rationally demonstrated? In what does this power of illumination consist?

These questions have puzzled many thinkers from the earliest times. Thus, Heisenberg has drawn attention, precisely in this connection, to the following thought expressed by Plato in the *Phaedrus*:

> The soul is awestricken and shudders at the sight of the beautiful, for it feels that something is evoked in it that was not imparted to it from without by the senses, but has always been already laid down there in the deeply unconscious region.

The same thought is expressed in the following aphorism of David Hume:

> Beauty in things exists in the mind which contemplated them.

Kepler was so struck by the harmony of nature as revealed to him by his discovery of the laws of planetary motion that in his *Harmony of the World*, he wrote:

> Now, it might be asked how this faculty of the soul, which does not engage in conceptual thinking and can therefore have no prior knowledge of harmonic relations, should be capable of recognizing what is given in the outward world. . . . To this, I answer that all pure Ideas, or archetypal patterns of harmony, such as we are speaking of, are inherently present in those who are capable of apprehending them. But they are not first received into the mind by a conceptual process, being the product, rather, of a sort of instinctive intuition and innate in those individuals.

More recently, Pauli, elaborating on these ideas of Kepler, has written:

> The bridge, leading from the initially unordered data of experience to the Ideas, consists in certain primeval images pre-existing in the soul - the archetypes of Kepler. These primeval images should not be located in consciousness or related to specific rationally formulizable ideas. It is a question, rather, of forms belonging to the unconscious region of the human soul, images of powerful emotional content, which are not thought, but beheld, as it were pictorially. The delight one feels, on becoming aware of a new piece of knowledge, arises from the way such pre-existing images fall into congruence with the behavior of the external objects. . . .

Pauli concludes with

> One should never declare that theses laid down by rational formulation are the only possible pre-suppositions of human reason.

This congruence between pre-existing images and external reality, to which Pauli refers, once intensely experienced appears to have the consequence that it develops over-confidence in judgment and values in the person who has had such an experience. For otherwise, how can one understand statements, such as these, made by some of the great scientists:

> "It is thermodynamics gone mad," by Lord Kelvin, one of the founders of thermodynamics, commenting on Boltzmann's derivation of Stefan's law and Wien's derivation of his displacement law.

> "You look at it from the point of view of the star; I look at it from the point of view of Nature," by Eddington in a controversial discussion with me.

> "I disagree with most physicists at the present time just at this point," by Dirac in the context of his views on the extant methods of renormalization in quantum electrodynamics.

> "It really looked as if, for the first time, we had a framework wide enough to include the entire spectrum of elementary particles and their interactions fulfilling my dream of 1933," by Heisenberg in 1957 in the context of his ill-fated collaboration with Pauli on a unified field theory.
>
> "God does not throw dice," by Einstein; or, even more provokingly,
>
> "When judging a physical theory, I ask myself, whether I would have made the Universe in that way, had I been God," also by Einstein.

In the context of these last statements by Einstein, it may be well to remember Bohr's remonstrance that

> Nor is it our business to prescribe to God how he should run the world!

Perhaps it is in terms of this over-confidence that one must try to understand the comparative sterility of once great minds. For as Claude Bernard has said,

> Those who have an excessive faith in their ideas are not fitted to make discoveries.

I am clearly treading on dangerous ground. But it does provide me the opportunity to draw attention to a fact which has been a source of considerable puzzlement to me: it concerns the very different ways - at least, so they seem to me - in which great writers, poets, and musicians on the one hand and great scientists on the other, appear to grow and to mature.

It is not uncommon that in considering the works of a great writer or a great composer one distinguishes an early, a middle, and a late period. And it is almost always the case that the progression from the early, to the middle, and to the late periods is one of growing depth and excellence. In some cases, as in the cases of Shakespeare and Beethoven, the latest works are the greatest. This fact is forcibly described by J. Dover Wilson in his delineation of the growth of Shakespeare's art in his great tragedies.

> From 1601 to 1608 he is absorbed in tragedy; and the path he treads during these eight years may be likened to a mountain track which, rising gently from the plain, grows ever narrower, until at the climax of the ascent, it dwindles to the thinnest razor-edge, a glacial arete, with the abyss on either hand, and then once again grows secure for foothold as it broadens out and gradually descends into the valley beyond.

> Eight plays compose this tragic course. The first, *Julius Caesar*, written a little before the tragic period proper, is a tragedy of weakness not of evil. In *Hamlet* the forces of evil are active and sinister, though still the prevailing note is weakness of character. *Othello* gives us Shakespeare's earliest creation of a character wholly evil, and at the same time Iago's victim is blameless - human weakness is no longer allowed to share the responsibility with heaven. *King Lear* carries us right to the edge of the abyss, for here horror is piled upon horror and pity on pity, to make the greatest monument of human misery and despair in the literature of the world. . . . Shakespeare came very near to madness in *Lear*.

Yet he pushed forward: *Macbeth*, *Antony and Cleopatra* (one of the very greatest of Shakespeare's plays), and *Coriolanus* followed in succession. And Dover Wilson asks:

> How did Shakespeare save his soul alive in this, one of the most perilous and arduous adventures ever undertaken by the spirit of man?

Shakespeare survived; and he survived only to follow his great tragedies by those wonderful plays, *Winter's Tale* and *Tempest*.

I am afraid that I have, perhaps, digressed a little too long in detailing to you the growth of Shakespeare's art. But I did want to emphasize to you the magnitude of that growth. And I am sure that one can say very similar things about Beethoven's late compositions which include the *Hammerklavier Sonata*, the *Missa Solemnis*, and above all, his last quartets.

While Shakespeare and Beethoven are probably unique in treading the razor-edge at the end of their lives and surviving, there are others who illustrate, at a somewhat more modest level, the same consistent ascent to higher peaks of accomplishment. But I am not aware of a single instance of a scientist of whom the same can be said. His early successes are often his last successes.* In any event, he seems unable to sustain a constant and a continuous ascent. Why is this the case? I shall not, however, attempt to answer this question but pass on to some more concrete considerations.

The question to which I now wish to address myself is how one may evaluate scientific theories as works of art in the manner of literary or art criticisms. The case of general relativity provides a good example, since almost everyone is

*I am here excluding the cases of those who, like Coates, Galois, Abel, Ramanujan, and Majorana, died in their youth. In these cases, we do not know how they may have fared had they lived past their prime.

agreed that it is a beautiful theory. I think it is useful to inquire wherein the source of this beauty lies. It will not do, I think, to dismiss such an inquiry with an assertion such as Dirac's (made in a different context):

> [Mathematical beauty] cannot be defined any more than beauty in art can be defined, but which people who study mathematics usually have no difficulty in appreciating.

Nor do I think that one should be satisfied with a remark such as Born's

> It [the general theory of relativity] appeared to me like a great work of art, to be enjoyed and admired from a distance.

[Parenthetically, may I say, quite frankly, that I do not know what to make of Born's remark. Has the general theory of relativity to be admired only from a distance? Does it not require study and development like any other branch of the physical sciences?]

In spite of the inherent difficulties which beset such discussions, I shall attempt to clarify why the general theory of relativity appeals to our aesthetic sense and why we consider it as beautiful. For this purpose, it is necessary to adopt some criteria for beauty. I shall adopt two.

The first is the criterion of Francis Bacon:

> There is no excellent beauty that hath not some strangeness in the proportion!

[Strangeness, in this context, has the meaning "exceptional to a degree that excites wonderment and surprise."]

The second criterion, as formulated by Heisenberg, is complementary to Bacon's:

> Beauty is the proper comformity of the parts to one another and to the whole.

That the general theory of relativity has some strangeness in the proportion, in the Baconian sense, is manifest. It consists primarily in relating, in juxtaposition, two fundamental concepts which had, till then, been considered as entirely independent: the concepts of space and time, on the one hand, and the concepts of matter and motion on the other. Indeed, as Pauli wrote in 1919,

> The geometry of space-time is not given; it is determined by matter and its motion.

In the fusion of gravity and metric that followed, Einstein accomplished in 1915 what Riemann had prophesied in 1854, namely, that the metric field must be causally connected with matter and its motion.

Perhaps the greatest strangeness in the proportion consists in our altered view of spacetime. As Eddington wrote:

> Space is not a lot of points close together; it is a lot of distances interlocked.

There is another aspect of Einstein's founding of his general theory of relativity that continues to be a marvel. It is this.

We can readily concede that Newton's laws of gravitation require to be modified to allow for the finiteness of the velocity of light and to disallow instantaneous action at a distance. With this concession, it follows that the deviation of the planetary orbits from the Newtonian predictions must be quadratic in v/c where v is a measure of the velocity of the planet in its orbit and c is the velocity of light. In planetary systems, these deviations, even in the most favorable cases, can amount to no more than a few parts in a million. Accordingly, it would have been entirely sufficient if Einstein had sought a theory that would allow for such small deviations from the predictions of the Newtonian theory by a perturbative treatment. That would have been the normal way. But that was not Einstein's way: he sought, instead, for an exact theory. And he arrived at his field equations by qualitative arguments of a physical nature combined with an unerring sense for mathematical elegance and simplicity. The fact that Einstein was able to arrive at a complete physical theory by such speculative thought is the reason why, when we follow his thoughts, we feel as "though a wall obscuring truth has collapsed" (Weyl).

The foregoing remarks apply only to the foundations of the theory leading to the field equations. We must now ask, whether on further examination, the theory satisfies the second criterion for beauty, namely, "the conformity of the parts to one another, and to the whole." The theory most abundantly satisfies this criterion while revealing at every stage a "strangeness in the proportion." Let me give a few illustrations.

Consider, first, the solutions which the general theory of relativity allow for black holes. As is known, black holes partition the three-dimensional space into two regions, an inner region, bounded by a smooth two-dimensional null-surface, which (the inner region) is incommunicable to the space outside which is asymptotically flat. It is a startling fact that with these very simple and necessary restrictions, the general theory allows for stationary black holes a single unique two-parameter family of solutions. This is the Kerr family in which the two parameters are the mass and the angular momentum of the black hole. What is even more remarkable, the metric for this family of solutions is explicitly known. The Kerr metric is axisymmetric and represents a black hole rotating about the axis of symmetry.

The axisymmetric character of the Kerr geometry clearly guarantees that the energy of a test particle describing a geodesic, as well as its component of the angular momentum about the axis of symmetry, will be conserved. In addition to these two conserved quantities, the Kerr geometry unexpectedly allows for the test particle a third conserved quantity (discovered by Brandon Carter). In consequence, the Hamilton-Jacobi equation, governing the motion of a test particle, is separable in its variables; and the solution of the geodesic equations can be reduced to quadratures. This was surprising enough. But what is even more surprising is that all the equations of mathematical physics - the scalar wave equation, Maxwell's equations, Dirac's equation, and the equations governing the propagation of gravitational waves - all, are separable in Kerr geometry (even as they are in Minkowskian geometry) and can, therefore, be solved explicitly.

One experiences similar astonishment when we realize that the singularity theorems of Penrose and Hawking require that our universe must necessarily have originated in a singularity and that, in consequence, we are compelled to contemplate the nature of the physical processes that will occur at densities of the order of 10^{93} gms/cm^3, in volumes with linear dimensions of the order of 10^{-33} cm, and in time intervals of the order of 10^{-44} seconds - dimensions which must stagger even this audience.

Or again, Hawking's theorem that the surface area of a black hole must always increase suggests the identification of the surface area with the thermodynamic entropy of the black hole; and this leads to an intimate connection between thermodynamics, geometry, and gravity.

There is clearly no lack of strangeness in the proportion in all these!

Everything I have said so far is in conformity with the two criteria of beauty with which I started. But there is yet another aspect of the matter which remains to be considered.

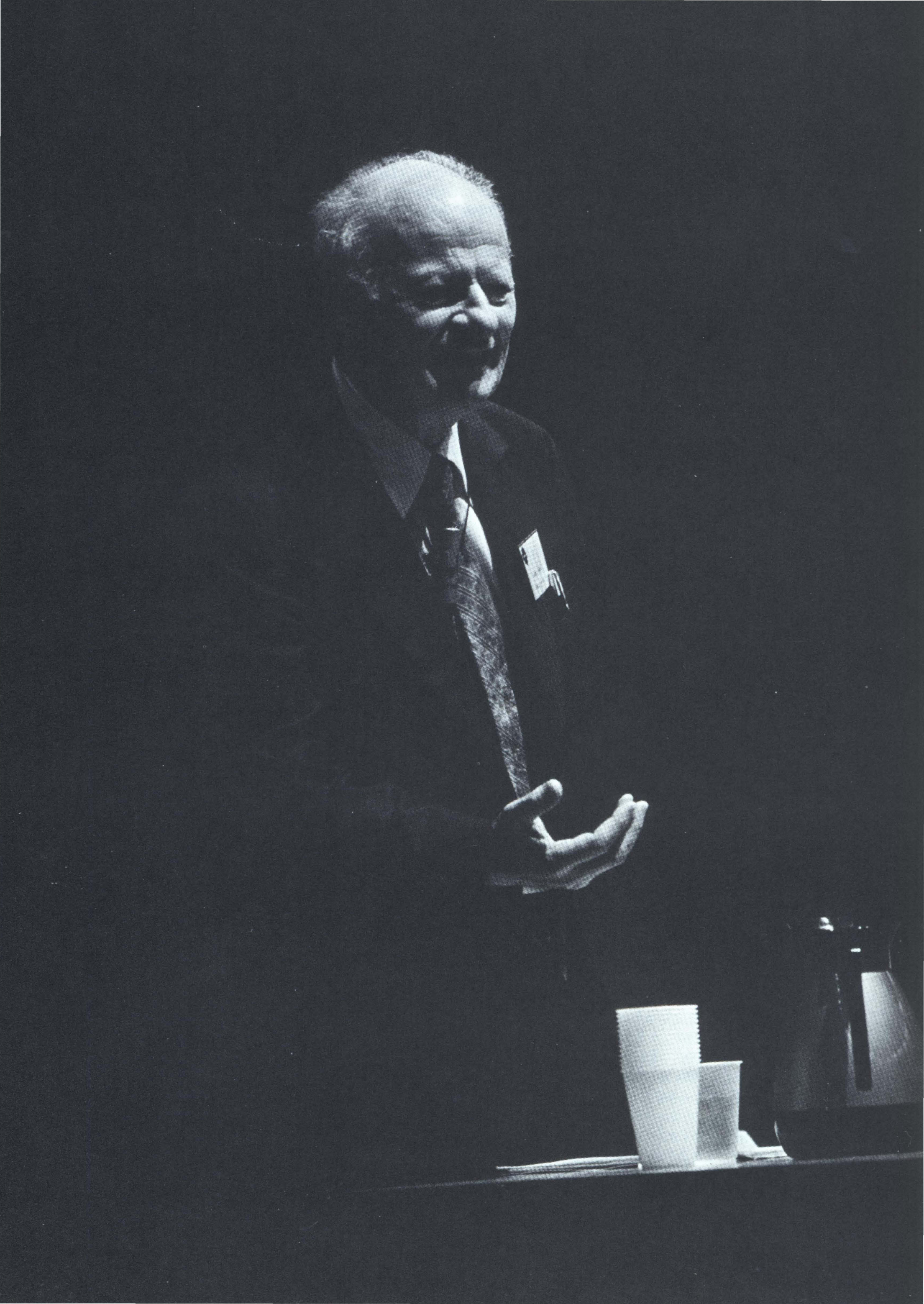
When Henry Moore visited the University of Chicago some ten years ago, I had the occasion to ask him how one should view sculptures: from afar or from near by. Moore's response was that the greatest sculptures can be viewed - indeed, should be viewed - from all distances since new aspects of beauty will be revealed in every scale. Moore cited the sculptures of Michelangelo as examples. In the same way, the general theory of relativity reveals strangeness in the proportion at any level in which one may explore its consequences. One illustration must suffice.

If one enlarges Einstein's equations to the Einstein-Maxwell equations, i.e., the field equations appropriate for space pervaded by an electromagnetic field, and seeks spherically symmetric solutions, one obtains a solution describing a black hole with a mass and an electric charge. This solution was discovered by Reissner and Nordström as a generalization of the well known one of Schwarzschild. Because of the charge of the black hole, it is clear that if an electromagnetic wave is incident on the black hole, a certain fraction of the incident electromagnetic energy will be reflected back in the form of gravitational waves. Conversely, if a gravitational wave is incident on the black hole, a certain fraction of the incident gravitational energy will be reflected back in the form of electromagnetic waves. The remarkable fact is that the two fractions are identically the same, i. e., for all frequencies. This result was not expected and the underlying cause for it is still not known. This example illustrates how strangeness in the proportion is revealed by the general theory of relativity at all levels of exploration. And it is this fact, more than any other, that contributes to the unparalleled beauty of the general theory of relativity.

So far, my remarks have been confined to what we may all concede as great ideas conceived by great minds. It does not, however, follow that beauty is experienced only in the context of great ideas and by great minds. This is no more true than that the joys of creativity are restricted to a fortunate few. They are, indeed, accessible to each one of us provided we are attuned to the perception of strangeness in the proportion and the conformity of the parts to one another and to the whole. And there is satisfaction also to be gained from harmoniously organizing a domain of science with order, pattern, and coherence. Examples of such organizations are Jacobi's *Vorlesungen über Dynamik*, Boltzmann's *Vorlesungen über Gas Theorie*, Sommerfeld's *Atombau und Spektrallinen*, Dirac's *Principles of Quantum Mechanics*, and the various gems of exposition which Schrödinger wrote in his later years. The translucence of the eternal splendor through material phenomena (of which Plotinus spoke) are made iridescent in these books.

May I conclude then by suggesting that each of us, in our own modest ways, can achieve satisfaction in our quest for beauty in science like the players in Virginia Woolf's *The Waves*:

> There is a square; there is an oblong. The players take the square and place it upon the oblong. They place it very accurately; they make a perfect dwelling place. Very little is left outside. The structure is now visible; what was inchoate is here stated; we are not so various or so mean; we have made oblongs and stood them upon squares. This is our triumph; this is our consolation.



Los Alamos And Cornell

H. Bethe

How did Robert Wilson enter my life? He wrote me a letter to tell me that I was wrong, or at least incomplete in a calculation I had done showing that particles in a cyclotron could not be focused above a certain energy, perhaps 40 MeV. This happened when Ernest Lawrence was collecting money to build a cyclotron which would go to a much, much higher energy, so my name was not popular at Berkeley. There was a graduate student at Berkeley, by name of Bob Wilson, who was working on focusing in the cyclotron. He had noticed one point which M. E. Rose and I had not, namely that there is focusing by the electrostatic field between the D's of the cyclotron. We had only looked at the magnetic focusing, so there was first an exchange of letters between him and me, and then the next more senior person came in, namely Ed McMillan, and then finally Ernest Lawrence. He said, "Well, never mind what you say, there is always a way to skin a cat." The cat was skinned eight years later by Ed McMillan, who invented the FM cyclotron.

My second encounter with Bob Wilson was in Princeton about 1940 when he was working on the range energy relation and made the best measurements up to that time on the range energy relation of protons. He also investigated quite a number of proton-induced nuclear reactions. All this interested me very much, and we had very interesting conversations. I couldn't now reproduce details, and neither, I think, could he. Then came World War II and Wilson, like many of us, felt that he should contribute something to the War effort. He invented a machine which was called the Isotron. I don't know to the present day just how it is made, but it was supposed to separate the uranium isotopes U^{235} and U^{238}. I have the suspicion, but I don't really know, that a very similar principle is now being used at TRW for isotope separation. Since I don't know either device, my comparison may be quite off resonance.

The work on the Isotron was stopped by decree of the higher-ups in the uranium project, partly because Wilson was necessary in what seemed to be a more vital part of the uranium business, namely Los Alamos, where he became a group leader. Harvard had a cyclotron, which I think had never worked. Wilson (in the name of Los Alamos) stole it and his group made it go. Wilson brought with him from Princeton a large number of faithful people and added more from other places, including McDaniel from Cornell, who became Wilson's successor at Cornell some years ago. This group was an extraordinary group; not only did they do lots of experiments, probably more than most other groups, but in addition they took an interest in absolutely everything in the laboratory. I remember one day at a meeting of what was called the Coordinating Council, which was the assembly of all group leaders in Los Alamos, our director Oppenheimer said, "Well, everybody should take an example from Wilson's group. They know everything in the Laboratory, and are interested in solving the problems of everybody else." At one time it became necessary in Los Alamos to have a second experimental Physics Division, one not concerned with nuclear physics, but concerned with observations of the assembly of model nuclear weapons. The man who, until that time, was the head of the Physics Division, Bob Bacher, was shifted to be the leader of this new division, and it was necessary that the Nuclear Experimental Division get a new head. There were several group leaders; of course, the one who was chosen to head the division was the youngest of them, Bob Wilson. This choice was clearly approved by all the other group leaders, including even Emilio Segré , who was very senior to Bob, and who does not easily recognize anybody else's authority.

At the end of the War, we all dispersed, or most of us did. Somebody said, "Robert Wilson has as many offers from universities as a dog has fleas." Cornell did not add to the fleas at that time; instead of that, Bob repaid the debt to Harvard, and, having stolen a rather primitive cyclotron from Harvard, he now set out to build there a much bigger and better one. However, history repeated itself. The director of our Nuclear Studies Lab at Cornell, Robert Bacher, who had been the first leader of the Physics Division at Los Alamos, again was called away, this time to become the scientific member of the first Atomic Energy Commission. Again it was obvious that the person who should replace him was just the same as before, Bob Wilson. It took a great deal more effort this time to persuade Bob to fulfill his manifest destiny. He had a joint project at Harvard, and both Dale Corson and I spent many long conversations on the telephone to persuade him to come to Cornell. Why he decided to come I don't know. It certainly was much to our advantage that he did, and maybe he even enjoyed it.

At the time, Cornell had decided to build a synchrotron for accelerating electrons to 300 MeV; 300 MeV was chosen because we thought we had to produce two mu mesons, whose total mass would be 210 MeV. Well, we were wrong about that, but we were right about choosing the energy. We were not alone in choosing that energy. There were, I think, three other places which were doing the same thing, and by that time Lawrence's cat had been skinned. McMillan and Veksler had discovered the principle of phase stability which makes it possible to have synchrotrons or synchrocyclotrons, and such machines were going up all around the country. It was fairly easy to build these machines but not so easy to make them go. When ours at Cornell had been built and when it was finally all assembled, it wouldn't work at all. And neither would the one at Berkeley. There was great consternation and it wasn't until Dale Corson measured the magnetic field of our synchrotron that the cause was discovered: The magnetic field was supposed to be 8 G all around the circuit. Instead, it was maybe +20 in one place and -10 in some other place; the electrons could hardly be blamed for losing their way around this mess. It was Bob Wilson who found the solution: eliminate this residual magnetic field as much as possible by raising the field to a rather low level electromagnetically, then getting down to zero field, and then repeating this many times so as to demagnetize the iron. Hardly had he done this, when the electrons merrily started to circle and could even be accelerated. No sooner accomplished than he phoned his old friend, Ed McMillan, and told him how to do it. And this, as he later said in print, may have been a mistake, because McMillan had beaufiful radio-frequency acceleration systems, so that once he had electrons at all, they got accelerated to 300 MeV, while at Cornell all we could do was 200.

Well, 200 MeV is more than the mass of a π meson by quite a lot, so we should have been able to observe π mesons, and indeed we did observe some, but very few, and especially we could not observe any neutral π mesons. At 300 MeV, the neutral π mesons are abundant; so York and Panofsky, using McMillan's synchrotron, discovered the π^0 in short order, and found that it was produced in very large quantity. However, it was useful to have our machine so miserable, because that way it was found out that the cross section for π mesons increases enormously from 200 MeV to 300, and particularly so for π^0's. So this was the discovery of the resonance now known as the delta resonance, or the 3,3 resonance of the nucleon. Brueckner was the first theorist to recognize this resonance clearly. The resonance was then established close to here, at Chicago, by Fermi's group using, not electrons, but protons as the particles to generate the π mesons. Fermi himself, I think, never believed the resonance, although it was there for everybody to see. It became even clearer, however, when the synchrotron at Cal Tech got to about 500 MeV, and one could follow the cross section through a beautiful maximum and down again on the other side, to show that really there was a resonance. All this activity at energies above his machine's capacity was of course something Bob Wilson couldn't tolerate for long, and very soon he set out to increase "his" energy, but let me pause for a moment.

When we first set out to build the 300-MeV synchrotron, we had two things in mind. The first was that synchrotrons clearly were quite a lot cheaper than cyclotrons to accelerate protons, and we didn't think that the government would give us enough money to build a big cyclotron at Cornell. Berkeley, being a much more famous laboratory, was in a better position to get enough money. So first we chose the synchrotron because it was easier and cheaper. Second, we thought that the interaction between electrons and hadrons was simple--it was just an electromagnetic interaction. We thought we undersood electromagnetic interaction, whereas we thought it would be much more difficult to understand the strong interactions, and this situation hasn't really changed. In fact, it has changed so little that many people now believe that indeed electron machines are especially useful at very high energy, in the many-GeV region, just because of the simple interaction; and I remind you of the results from SLAC and DESY--the colliding beams, electrons and positrons, which offer relatively simple situations to create new hadrons, psi particles and so forth. But, as far as the cost is concerned, the shoe is now on the other foot. To build a machine for 500-GeV protons costs $243 M, as we learned this morning, but you certainly couldn't get a 500-GeV beam of electrons for that money. Well, maybe Bob will invent a way to do so. So I think now people would love to find a way to make electrons at high energy as cheap as protons at the same energy.

I wanted to talk about the research that went on at Cornell, and I want to mention a couple of things which are not high-energy particle physics. One is that the Cornell synchrotron was used by DeWire and collaborators to measure very accurately the pair production cross section. This interested me personally very much because I had calculated it a few years earlier with Heitler, and it was shown that indeed the cross section behaved the way it ought to as a function of energy. In particular, it went up logarithmically with energy, up to a certain point, and then it saturated which was due to screening of the electric field of the nucleus by the electrons. I may mention parenthetically that when Heitler and I wrote that paper, Heitler would never believe screening would give saturation. Another point was that the cross section was not exactly proportional to Z^2. After all, Z^2 is the Born approximation and why should that be right for lead? But I had a graduate student at the time, Maximon, who calculated the deviations from Z^2 and found, I am happy to say, something which was in very close agreement with the experimental results on the pair production cross section.

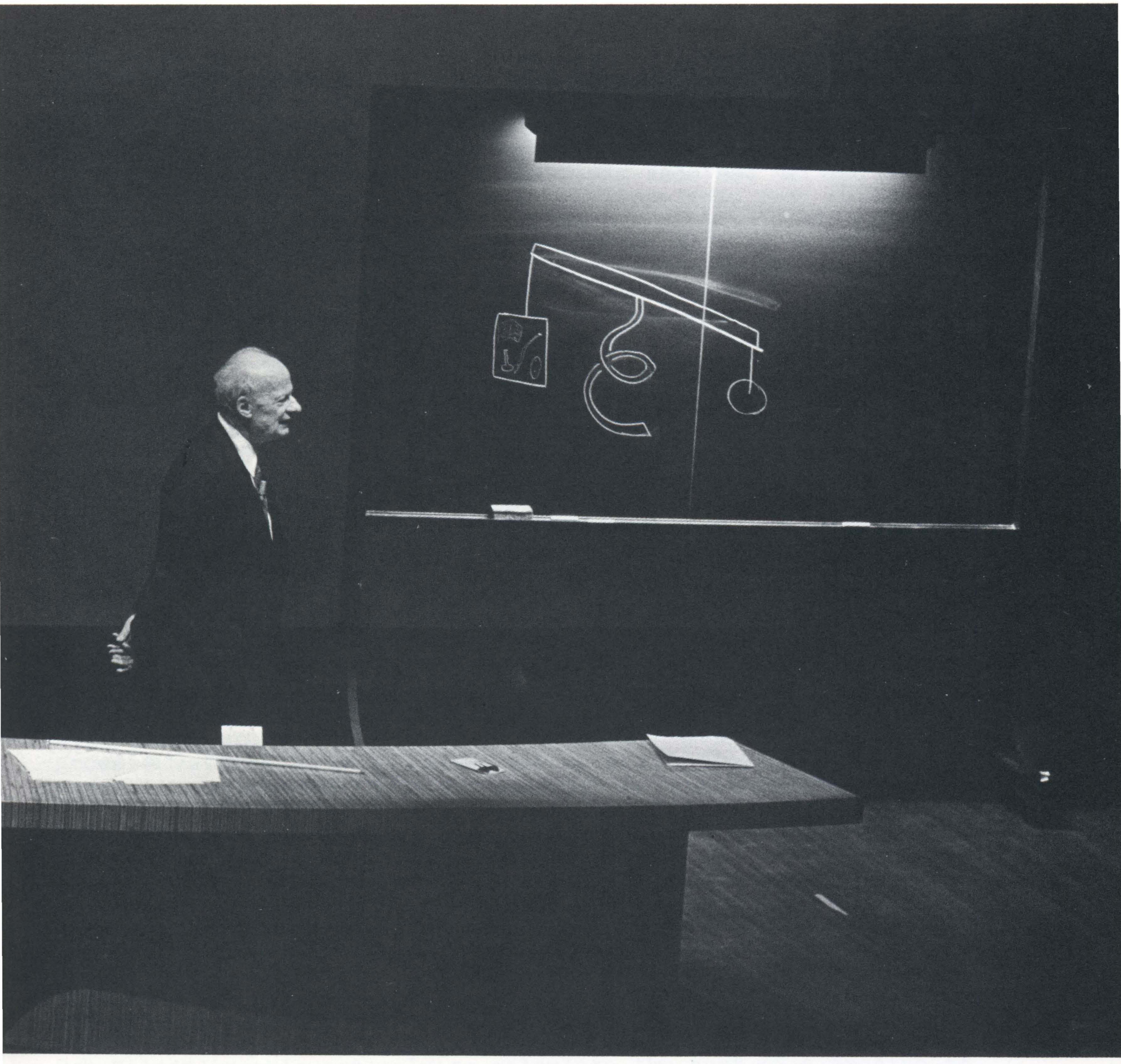
The second point I want to mention outside of particle physics is the quantitative measurement of the synchrotron radiation in our synchrotron, which was done by Corson. Synchrotron radiation is one of the beautiful ways to prove special relativity theory. Of course, the most beautiful proof today is the lifetime of the μ meson in the storage ring at CERN, which gives the time dialation to an accuracy of a percent or so, and if anybody still doubts the twin paradox, he only need to look at these measurements to be convinced otherwise. Synchrotron radiation is a nuisance, because it means that you have to apply an awful lot of energy to just keep the electrons at constant speed and even more to accelerate them against the radiation. It is a nice way to show that the velocity of an electron of, let us say, 10 GeV is very close to the velocity of light. Just for fun and in order to give a talk on special relativity the other day, I calculated that the difference between the velocity of our electrons at Cornell of 10 GeV and the velocity of light is 25 cm/sec. That's a very, very slow walk. At Stanford it's 6 cm/sec. Now what consequence does it have that the velocity is near the velocity of light? The consequence is that light emitted by an electron at various points of its path is nearly in phase, because the electron goes almost fast enough to catch up with its own radiation. Therefore there is a very long distance over which the emitted light is in phase, and this distance, of course, is inversely proportional to the difference, c-v. This tells you that the electric field which you've produced by synchrotron radiation is proportional to 1/(c-v), and this in turn is proportional to γ^2, the energy squared, and therefore the intensity is proportional to γ^4, as everybody knows who builds a synchrotron. By measuring this radiation intensity you measure directly the difference c-v, and this was done for the first time accurately at Cornell by Corson, who found very close agreement with the calculated value. So this is, if anybody needs it, still another proof of special relativity.

The third point was not done on the synchrotron, but it was done by Wilson himself, and that was Delbrück scattering, the scattering of light by a potential field, and of course the potential field you choose is the Coulomb field of a heavy nucleus, in this case lead. This scattering is extremely small, but it had been calculated, and it was interesting to measure it. Unfortunately, there are other scatterings which are bigger--one is the Thomson scattering by the nucleus, and the other is the Rayleigh scattering by the K electrons of the heavy atom. Well, we calculated all this, and Bob went to work to measure it using Co^{60} γ rays, of energy 1.3 MeV. Delbrück scattering happens to be a maximum at just about twice the mass of the electron. Wilson found what seemed to be a definite indication that Delbrück scattering was there, and was just about the magnitude calculated. I think most people have forgotten that.

Now this was Wilson as an experimenter, but I suppose that very few of you know that he is also a theoretical physicist. One of his theoretical papers was a calculation of the development of a shower in a heavy material such as lead, using the cross sections for emission of radiation, and for pair production. When you have a material such as lead, the cross sections change very much with energy, so you cannot just use the usual Bhabba-Heitler radiation shower theory. Wilson knew that you have to take into account the real variation of cross section, and for this he used a computer. This was in 1952 when computers were not very plentiful, so he made one himself using an erector set, and it worked very well. With this he calculated the shower production, which then was used to make his precision quantameter where he uses the energy of the pair electrons to measure the energy of quantum radiation.

Another theoretical paper he did with me. He had the idea, and I did the algebra. This paper was about the scattering of pions by protons. The cross section for pion production had been measured, and the mean free path had also been measured at Cornell for pions of various energies in nuclear matter. In particular, for pions of about 50 MeV, which we could easily make because we had low-energy electrons. You find that the mean free path of these pions is very long; it's about 4 fermi. In addition, the Cornell experimenters were able to measure the diffraction scattering of pions from a nucleus, and if you know the mean free path for collisions inside the nucleus and also know the diffraction scattering, you can deduce from this the amplitude of scattering by a single nucleon. Of course, from a single nucleon you get scattering in all directions, but only some of that adds coherently to give you the scattering by the nucleus, so what you want to calculate really is the potential of the pion in nuclear matter. Bob Wilson told me how to do that from the scattering amplitude, and so I set to work and did the actual algebra, and we published the paper together. I think it's the only paper we published together, which I find very regrettable. We concluded that the coherent scattering amplitude of nucleons for π mesons at this low energy was very small, and while our numbers don't hold up at present, on the whole our result was qualitatively correct: The scattering is very small. The scattering in the p state is still very small because you are far below the resonance, and the scattering in the s state is also small, because the scattering amplitude has opposite signs for isospin 3/2 and 1/2, so that the average scattering, and hence the coherent scattering, is indeed very small.

Not all the work of the director of the Nuclear Laboratory was in physics. We had an administration, and I'm not sure whether it was the dean or the president, one of them inquired, what on earth were the physicists doing that cost the university so much money? I suppose this is somewhat familiar to some of you, and so, let us say it was the dean who said, "Now please make me an organization chart." There were two parts to the Physics Department--one was the solid state part under Lloyd Smith, and the other was the Nuclear Laboratory under Bob Wilson. So two weeks later when we faced the dean again Lloyd Smith presented a beautiful organization chart with lists of people and of different research being done and who was doing what and who was teaching what and so on, and it went over a great big page, maybe several pages. Bob Wilson had his organization chart made in the machine shop, and it looked like this:



There was a support which was a Cornu spiral, just for beauty's sake. On top of this was a balance, a rather husky piece of metal, and at its two ends there were weights. At one end were some lead weights which represented the dead weight of the faculty. At the other end were all the positive factors which made the Lab go. There were products of the machine shop like screws and electronic tubes (in those days there were still tubes), and there was something symbolizing the students, perhaps a book. Most important, there was a dollar bill, the support from the Office of Naval Research, which should be praised once more as one of the most generous and least troublesome sponsors of research that ever existed. One of the strange phenomena was that this dollar bill stayed in that glass jar for years and nobody ever took it away, except once or twice somebody borrowed it for lunch and then gave it back.

I mentioned much earlier the Caltech synchrotron which went to about 500 MeV and got the first full picture of the resonance. Obviously this didn't let Wilson sleep. He got two ideas, both stimulated by some rumors he heard from Brookhaven. One was a rumor and the other was fact. The rumor was that Brookhaven had operated their Cosmotron with a gap of, I think, two inches instead of six inches. Wilson said to himself, "Well, if they can do that, our machine is much smaller, so we can do it with a one-inch gap." This tremendously decreased the size of the magnet and the size of the doughnut containing the beam of electrons. Everything got cheaper by a factor of ten or more. And so the next Cornell synchrotron was designed with this in mind. Later, Wilson writes, he discovered on visiting Brookhaven that nobody had ever heard of operating the Cosmotron with a two-in. gap. The second news from Brookhaven was for real, and that was strong focusing. This immediately seemed most plausible, and Wilson said, "Of course, our machine is much simpler than the Brookhaven machine of 30 GeV which was being constructed, so let's try it out with electrons." And indeed, the first experiments with strong focusing were done by Wilson and his collaborators at Cornell showing that the principle really worked. In this manner, he and the others designed a second synchrotron, which started out with 1.2 GeV, and then gradually went up to 2 GeV. This one worked almost immediately. Nowadays machines do seem to work very quickly after they are built, in contrast to the first synchrotrons. I think the main secret is that one injects now at high energy so that one avoids the irregularities of the magnetic field at injection.

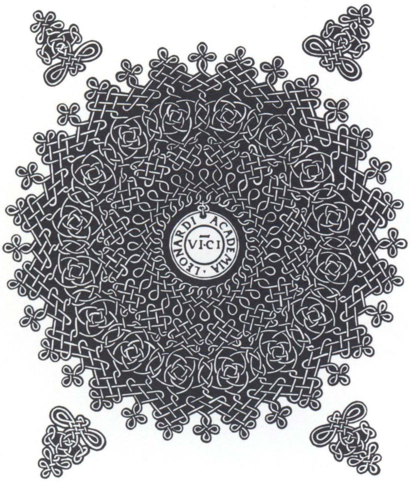
But that was not the only secret, and I will now quote from a paper he wrote which says, "If we had any secret in constructing machines cheaply and rapidly, it was our willingness to make mistakes." I think this is the example which everyone should be urged to follow. I know of one case in nuclear reactor development, which gives a beautiful example of the truth of this statement. For many years the United States has wanted to develop a breeder reactor using high-energy neutrons, and as a first step to do this they wanted to have a test facility at Hanford in Washington, known as the Fast Flux Test Facility. I believe it was to be finished in 1969. I believe it is now scheduled to be finished in September of this year, 1979. It overran by ten years and it overrran by, I think, a little more than a factor of ten in cost. The reason for this was that the AEC, at the time, decided that no mistake must be made in constructing this facility. Everything must be designed in such a way that it is absolutely impossible to make a mistake. In my opinion, there is no greater mistake possible than trying to make no mistake.

Well, the 1- and 2-GeV synchrotrons went into operation. Wilson had the satisfaction to discover in collaboration with many other people at Cornell the next resonance, and then still another, showing many isobaric states of the proton. Nowadays there are still many more, and you can read the little book which will give you a list of lots and lots of excited states of the nucleon. This is what V. Weisskopf calls, "the third spectroscopy," namely that in the GeV range, and this morning we heard from Professor Paul how similar the three spectroscopies actually are. That doesn't make it less interesting to work on the GeV spectroscopy.

Of course, while the experimental results kept coming in, there was again ambition in the entire Cornell group, and especially in Bob Wilson, to go to the next step, to yet higher energy. But they said, "This time we'll take the easy way, we'll let an industrial company build it for us." They would simply copy the Cambridge Electron Accelerator, which was being built at the same time. But Cornell could get money only for a small copy of that at CEA, a copy that was to give 3 GeV and was to cost about $10 M. Cornell was supported by the NSF which was quite willing to provide the $10 M. But now the entire experimental group at Cornell, very much infected by the Wilson spirit, decided this was really a bore to have their machine built by an industrial company: "It will take a long time, it won't give us very much energy, we are now at 2 GeV with our synchrotron which was built for 1 GeV, so why don't we do something more interesting?" So they designed a much bigger ring, a much bigger synchrotron, which first gave 10 and later 12 GeV instead of 3 for the same money, $10 M. Well, NSF insisted maybe $10 M was not really quite enough for this bigger machine, why don't you take $12 M? I'm told, (I hope the information is correct) that it took a lot of persuasion on the part of our vice-president for research to persuade Bob Wilson to accept the extra $2 M. And of course it was built, I suppose, for $11.5 M. I don't know how Bob Wilson is able not only to calculate the cost of a machine so accurately, but also the rate of inflation, so I think one of the things he might now go into is economics prediction of the inflation rate? How about it? In the midst of constructing this machine, Bob Wilson, as you all know, was asked to come here as the director of the National Accelerator Laboratory. Nobody at

Cornell liked that idea but it was very clear, certainly to me, that there was no choice for him, that he had to go. And I think in retrospect maybe we don't even begrudge him this decision. This morning you heard from Leon Lederman how it went here at the National Accelerator Laboratory, how Bob Wilson predicted the time when the machine would work, and the money that would be spent for it, and that he never met the goal, spending $243 M instead of $258 M.

We are here to honor Bob Wilson, but what I mainly want to do is to say that he would be an example, not only to all of us, but to the country. At present the United States is pervaded by a philosophy of not taking any risks, not making any mistakes. This paralyzes any decision; it paralyzes our enterprise. We are this year celebrating the hundredth anniversary of the discovery of the electric light. A hundred years ago, the United States was a leading country in enterprise, and even more so 50 years ago, about the time when I came here. I'm sorry to see that the spirit of enterprise has gone out of this country. Enterprise still is alive in such people as Bob Wilson and in the people who are here in this room, some working here at Fermilab, some at other places. But the whole nation should learn from Bob Wilson, "do not fear to make mistakes, but be resourceful enough to correct the mistakes when they are discovered." It is far less risky to do this than trying to avoid taking any risk.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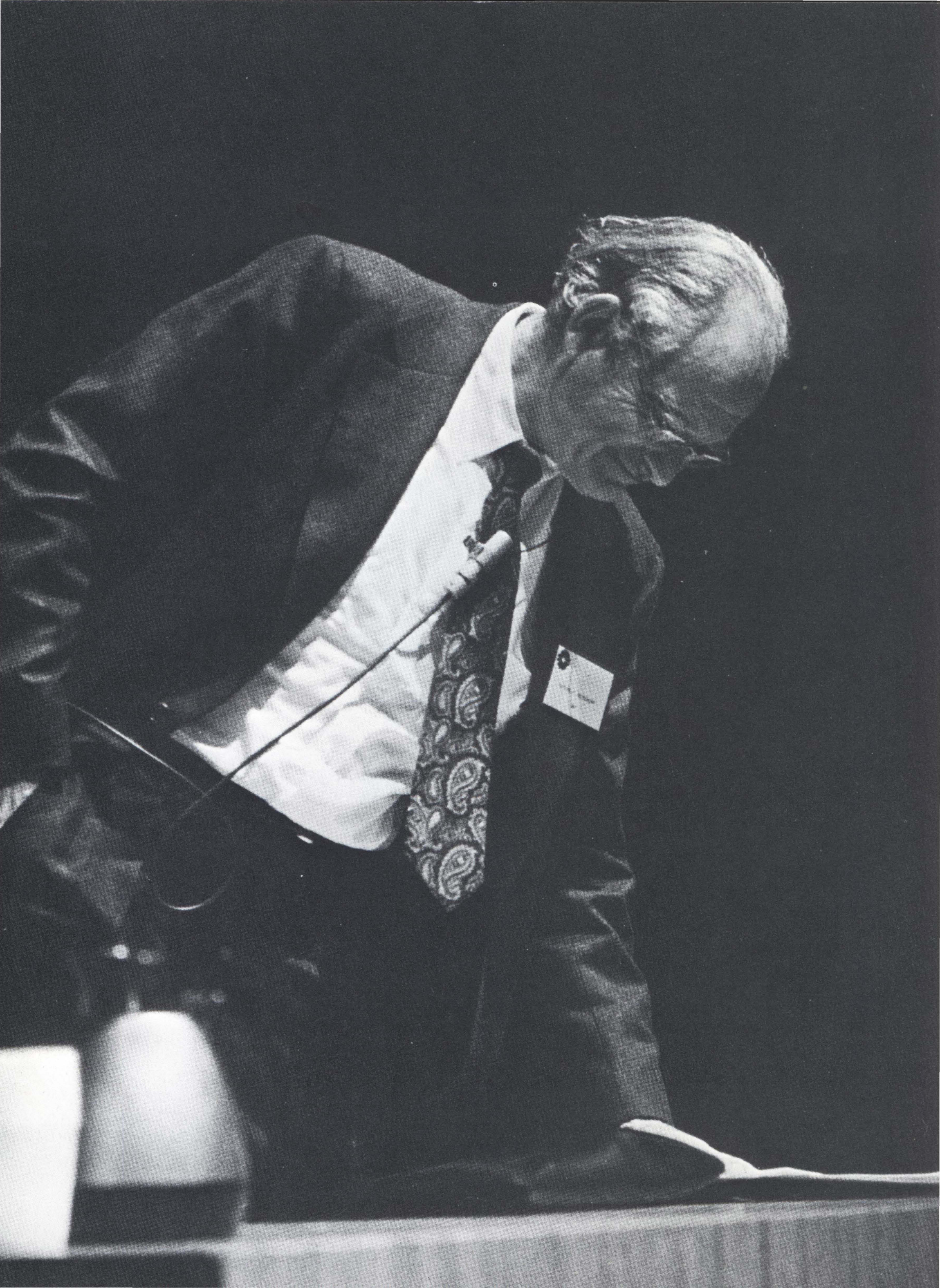


Art And Science

V. F. Weisskopf

I would not know of a better occasion to talk about Art and Science than a celebration in honor of Bob Wilson. Very few people have been active simultaneously in these two areas of creativity. He is a physicist and a sculptor. Oh no, he is more than that. Bob is also a great architect. Wrong again. I left out a fourth area: engineering. Just look around here at Fermilab to see what he has created: Beauty in Physics and Physics in Beauty. To prove the first, a beautiful bubble chamber may suffice; here the beauty lies not only in the pattern of the tracks but in the insight into the deep structure of matter that those tracks reveal. To prove the second, a few examples of Bob Wilson's creations are shown. They are the symbols that give sense and meaning to the laboratory which they adorn.

I. Space Is Blue

Here are a few ideas and thoughts about the broad and many faceted subject of Art and Science. Some of them may contradict what our friend Chandra has said this morning. Niels Bohr used to say: "A shallow truth is a statement whose opposite is false; a deep truth is a statement whose opposite is also a deep truth."

First I want to draw your attention to the diversity of human experiences and the diversity of what we are doing with them. There are outer and inner experiences, rational and irrational ones, social experiences between two or many human beings, and experiences with the non-human part of nature. Our reactions to these experiences are manifold and varied. We think and ponder about these experiences; we make use of them to improve our lives and to avoid material and emotional hardships, we are oppressed or elated by them, we feel sadness and joy, love and hate. We are urged to act and communicate them to others; we try to relate them to the pattern of our lives. We want to influence people and our environment. All this is the raw material of human creativity. What are its manifestations?

The creative spirit deals with our experiences and shapes them into various forms: the myths, the religions, the philosophies, the diverse arts and literatures, architecture, the sciences, medicine and technology, and the social structures. These manifestations are directed towards many aims, practical and spiritual; the actual effect upon humankind is sometimes positive and constructive, sometimes negative and destructive, often without much relation to what the creators intended.

Most forms of human creativity have one aspect in common: the attempt to give some sense to the various impressions, emotions, experiences and actions that fill our lives, and thereby to give some meaning and value to our existence. Meaning and sense are words difficult to define but easy to grasp. We cannot live without meaning; oh yes, we can, but life is empty, cold and "meaningless." It is the crisis of our time in the Western world that search for meaning has become meaningless for so many of us.

The different forms of human creativity often seem to be incommensurable, mutually exclusive, or even contradictory; I believe, however, a better word is *complementary*, a term that has acquired a more focussed significance since its use by Niels Bohr. The main purpose of my talk will be to point out the complementarity, in the sense of Bohr, between the different avenues of human creativity, in particular, between the arts and the sciences. Even within physics itself, we deal with concepts and discourses that, on the surface, are contradictory and mutually exclusive, but on a deeper level they are what Bohr aptly has called "complementary." They represent different aspects of reality, one aspect excluding the other, yet each adding to our understanding of the phenomenon as a whole. The quantum state of an atom evanesces when it is observed by a sharp instrument designed to locate the electron. The state is restituted when the atom is left alone and given enough time to return to its original state. Both aspects--quantum state and location--are complementary to each other; they are necessary concepts to get a full insight into atomic reality.

Similar complementarities appear in all fields of human cognition as Bohr often has pointed out. They have to do with the question of relevance. In the atom the wave picture (quantum state) is relevant for certain aspects of its reality, the particle picture for others. There are different ways of perceiving a situation, ways which may seem unconnected or even contradictory, but they are necessary to understand the situation in its totality. A simple example may suffice for the moment. A waterfall may be an object of scientific study, in which case the velocity distribution and the size of the droplets and their electric charge are relevant; or it may be the object of a poem describing the beauty of the phenomenon in which very different properties become relevant. I remind you of the well known conversation between Felix Bloch and Werner Heisenberg about the subject of space. Bloch reported to Heisenberg some new ideas about the relevance of certain mathematical structures of space when Heisenberg, his mind drifting into other avenues of experience, exclaimed: "Space is blue and birds are flying in it!"

II. The Holistic Approach

Let us now try to discern certain categories within the vast expanse of human experience. We face a world of many dimensions and infinitudes, of which the "world" of the natural sciences is only a subdivision. The separation of the natural world "outside" ourselves from the "internal" world of the mind is an ever recurring problem of philosophy and subject to questions and doubts.

Let me emphasize, however, that I do not consider modern quantum mechanics as a source of such doubts. I cannot accept the view that the complementarity within physics establishes a direct relation between mind and matter. The "influence" of the observer upon the observed which is often correctly quoted as the basic tenet of quantum mechanics, plays an important role only for the definition of the concepts that are used for the description of atomic phenomena. However, the actual phenomena are independent of the observer. In most cases we are not interested in the exact position of an electron; hence the quantum states of the atoms are not destroyed by attempts to localize an electron. For example, when quantum mechanics describes the light emitted by an electrical discharge in a gas, or the properties of a metal, we do not interfere with the reality of the object. How could quantum mechanics be so successful for the understanding of what is going on in the stars, where any direct influence on the object certainly is excluded?

Natural science, of course, is built upon some kind of separation of the external world; it regards the objects of its study as distinct and independent from the emotions and ideas that permeate the inner self. But science is a relatively new creation of the human intellect. Before its appearance the approach to human experience has been essentially holistic. Myths, religions and philosophies have tried to derive the totality of human experience, external and internal, from one leading principle and thus provide it with a well-defined meaning.

Art, which is one topic of this essay, has always played an essential part in this holistic approach. It was to a large extent a servant of myth, religion and philosophy, being a most suitable instrument to transmit holistic thoughts and emotions by transforming them into concrete visible or audible entities. Think of Greek sculpture, of Homer's poetry, of the Gothic cathedrals, of Bach's passions. There they stand, the works of art, representing ideas and symbols immediately and directly with all their spirit and power. They impose upon any beholder their meaning and their general validity, their grandeur and beauty, if the beholder is part of the human soil from which the myths or religions grew.

It is often said that there is another source of art: the immediate urge to embellish and decorate objects of special value and significance. I do not see a great difference between this and the intensification of symbols and ideas. The embellished objects are symbols that art renders significant; they acquire a meaning beyond their ordinary role through decoration and embellishment.

Whenever the mythologic and religious fervor begins to weaken, art tends to separate from these realms and acquire an independent role. It then replaces myth and religion to an increasing extent. It continues to create realizations of ideas and emotions that are important and meaningful for the culture of the time, although they may no longer be derived from a myth or a religion. Then it is art that serves as a powerful synthesizer of human experiences of the day, presenting to us meaningful messages of joy or sadness, greatness or meanness, beauty or terror, salvation or torture that cannot be transmitted in any other way. Two periods of separation between art and religion come to my mind: one is Hellenistic-Roman art, the other is the period in which we live, that started in the Renaissance and resulted in an almost complete separation in modern times.

Art, just as myth and religion, is a holistic approach to human experience. Every true work of art transforms and molds a complex of many varied impressions, ideas or emotions, into one unique entity; it compresses a great variety of internal or external perceptions into a single creation. It expresses a whole truth, if this word may be applied here, and not a partial one or an approximation to the truth. If it is a great work of art, it cannot be improved or changed or redone in order to comply with new insights that were not taken into account in the first creation. It is an organic whole that says what it says in its own unique way. At different epochs it may mean different things to the beholder or listener or reader. It will be interpreted in different ways; it may be more meaningful at one period and less at another. It may mean different things even to different groups of people. But it is valid and effective only in its original unique form.

III. The Scientific World View

The tradition of holistic approach to the totality of human experience suffered an important change with the birth of natural science in the Renaissance. A new era began. Instead of reaching for the whole truth, people began to ask limited questions in regard to the natural world. They did not ask questions such as: What is matter? What is life? What is the nature of the Universe? Instead they asked: How does the water flow in a tube? How does a stone fall to the earth? What makes the blood flow through the veins? What happens if you rub two objects against each other? The general questions were shunned in favor of the investigation of separable phenomena, where it was easier to get direct and unambiguous results.

Then, the great miracle happened: by the systematic study of many detailed phenomena whose relevance were not obvious at all at the start, some fundamental insights into the basic structure of nature emerged. The renunciation of immediate contact with absolute truth, the detour through the diversity of experience paid off. The restraint was rewarded as the answers to limited questions became more and more general. The study of moving bodies led to celestial mechanics and an understanding of the universality of the gravitational law. The study of friction and of gases led to the general laws of thermodynamics. The study of the twists of frog muscles and of voltaic cells led to the laws of electricity that were found to be the basis of the structure of matter. Some sensible answers emerged to those holistic questions that were shunned at the beginning. The non-holistic approach led to holistic results. Einstein said once, "The eternally incomprehensible fact about the world is its comprehensibility."

The holistic character of scientific insights greatly differs in character from that of myth, religion and art. First of all, it does not directly include what we commonly refer to as the human soul, our feelings of awe or desolation, our ambitions, our convictions of right or wrong. It includes only the physiological phenomena accompanying these realities. The holistic character refers to the unity of natural phenomena outside of our "souls." Furthermore and equally characteristic, the scientific insights are always tentative, open to improvement and changes; they have restricted validity. They appear as incomplete perceptions of parts of a greater truth hidden in the plenitude of phenomena, a truth that is slowly but steadily revealed to us. Every step toward more insight adds to the value of previous steps. The scientific creations do not stand each by themselves as the works of art; they cannot be regarded as separable entities. They are parts of a single edifice that is collectively assembled by the scientists and whose significance and power is based upon the totality of contributions. In German it is referred to by the untranslatable term: "Das 'Weltbild' der Naturwissenschaften." Newton said: "I stand on the shoulders of giants." His work, as that of Einstein or other great scientists, comprise only a few stones of this edifice, albeit rather large ones at pivotal locations.

IV. The Complementarity of Art and Science

Both art and science are here to give us deeper insights into our environment. But this environment is not all the same. For science--I consider here only natural sciences--it is the natural world in which we live, including our own body and brain. For art, it is different; it also contains the natural world, albeit in a different way (remember Heisenberg's space), but it mostly consists of the vast realm of personal ideas, feelings, emotions, reactions, moods, attitudes, and relations between human beings. One might object to this and assert that all these elements are also subject to a scientific approach as phenomena within our brain. This is certainly true but, just as art approaches external natural events in a thoroughly different way than science, so does it approach the internal landscape of what one may call our souls.

This difference has very much in common with Niels Bohr's complementarity. There are several contradictory, mutually exclusive approaches to reality. The scientific approach to a phenomenon is complementary to the artistic approach. The artistic experience evanesces when the phenomena are scientifically explored, just like the quantum state is temporarily destroyed when the position of the particle is observed. We cannot at the same time experience the artistic content of a Beethoven sonata and also worry about the neurophysiological processes in our brains. But we can shift from one to the other.

Both aspects are necessary to get at the full reality of the phenomenon. We can admire the starry sky by being overwhelmed by the vastness and variety of star patterns or by contemplating the physical nature of the stars and star system, of their motions and their developments from the big bang to their present stage. We can be impressed by a clear sunset because of the beautiful blending of colors or because of some thoughts connected with this symbol of the end of a day in human life; but we also can be impressed by the processes of refraction and scattering of light in the atmosphere or by suspended particulate matter.

The contrast between those different approaches is not necessarily the one between rational thinking and emotional feeling; one can and does talk rationally about emotional impressions and about music, painting or other arts. But it is a very different type of discourse, lucid and concise within its own intrinsic scale of values, but fragile and indefinite when judged by the peculiar requirements of scientific intercourse. One view complements the other. We must use all of them in order to get a full experience of life. In particular, as a scientist one may be aware of this need, since his or her professional life is rather one-sided in this respect: "In the morning I go from mystery to reality, in the evening from reality to mystery." But mystery is another form of reality. No wonder that so many scientists are actively or passively interested in music, the most irrational of arts.

The vast difference or complementarity ought to be obvious to anybody who has to do with art and science; it should need no further comment. But I have encountered a sub-group of scientists who do not subscribe to this statement. I call them the science chauvinists. They maintain that the progress of neurophysiology and brain science will finally lead to an adequate scientific understanding of what is going on in our brain when we create or enjoy a work of art or when we are spiritually elevated by art or religion, so as to sense a deeper meaning in it. Going one step further--now the sub-group becomes noticeably smaller--they maintain that we then may be able scientifically to create art or replace it by certain nerve stimulations since we then would know what its neurological function is.

I believe that the notion of a scientific insight into the essence of art is based on a number of fallacies. Sure, there is no imaginable limit to our understanding of brain action, and of the identification of definite nerve-processes with emotional, moral or aesthetic thoughts or feelings. We may expect tremendous progress in this field of science within a few decades. But there are several reasons why I believe there is a definite limit to fundamental scientific understanding of such matters. One reason has to do with the fact that any scientific research is based upon reproducibility of results. Certain phenomena in our soul that are relevant to the arts are not reproducible. Not only has every human being a different set of genes; more importantly, he or she was subject to a different set of impressions. Some of these differences may be considered as irrelevant in certain respects. A medical doctor will treat disease successfully by the same methods, whether the patient be Einstein or a halfwit. But for the development of human culture and traditions the differences become most relevant. Human culture is an amplifier for both the genetic differences and those acquired by experience. A non-recurring unique combination of such differences makes an artist or poet capable of creating a work of art. It also determines the unique way in which an individual experiences that work of art. How can such a process be scientifically analyzed when it occurs only once? Do we not face here a typical complementary situation between the structure of the nervous system on the one side and the creation and perception of a work of art on the other? Indeed does not the specific uniqueness of a work of art represent a fundamental obstacle to the application of scientific analysis to the creative and perceptive process?

I maintain that the same problem also appears in the social sciences. Non-recurring and unique events occur frequently in the minds of human beings and they have decisive impact upon the social fabric of society because of the amplifier effect of human culture. This may turn out to be a serious impediment to reliable scientific predictions in social science; it may also be a fundamental difficulty when animal sociobiology is applied to human societies.

I must confess that I may run into the same error that the great Niels Bohr committed when, some time ago, he argued that the processes of life are complementary to physics and chemistry. He based his conclusion upon the fact that a strict chemical analysis of life processes requires the death of the investigated creature. Therefore, he considered it possible that matter alive may represent a different state of matter, complementary to the non-living state, in analogy to the atomic quantum state which is destroyed by any attempt to look at its detailed structure. He was wrong as Watson and Crick, and all that followed them, have clearly shown. I do not think that I commit a similar error. If I do I am in good company. Indeed, I believe that there are fundamental differences between art and science which cannot be bridged over, just as no new physical theory will ever get rid of the wave-particle complementarity.

Art and science have this in common--that they provide meaning and sense to human experience. But the sense of the meaning is thoroughly different. It has been observed that art transforms general experiences into a single and unique form, whereas science transforms detailed single experiences into a general form. Either of the two transformations results in a holistic product: the work of art and the law of nature. But there are vast differences between the two. We already have mentioned the tentative and unfinished character of our scientific perception of nature. It represents only part of a truth that is developed step by step, whereas a work of art is finished and transmits its full message at all times, although the messenger may not be always interpreted in the same way.

An important difference between art and science comes from the collective character of the scientific "Weltbild." Even the most impressive single scientific creation makes sense only within the web of other contributions. Surely, the significance of a work of art also depends to some extent upon what has been created before: Beethoven could not have composed his music without the framework of Bach, Haydn, and Mozart. Michelangelo's art builds upon the development of Greek art and of the early Renaissance. But this dependence is much more tenuous and different in character. The interdependence between scientific creations is such that individual contributions have no significance whatsoever in isolation. They are bricks in a common edifice, and it is the edifice that corresponds to a work of art and not the individual bricks.

The collective character of science leads to another typical difference between a work of art and a law of nature. The presentation of the latter is not bound to the formulation given to it by the creator. On the contrary, the very essence of a natural law elevates it far beyond any personal formulation. Nobody but a historian of science is interested how Maxwell formulated his equations. Their significance is much better understood from later more comprehensive presentations. The uniqueness of a work of art is a notion completely different from the uniqueness of a law of nature. The former represents a personal entity, which is transmitted to and reexperienced by other individuals again as a personal experience. The latter is an impersonal entity, an abstraction from a multitude of specific direct or vicarious experiences and creative ideas of many individuals; it is understood by other individuals as an impersonal general intellectual entity. The work of art produces in the recipients feelings of joy, sadness, spiritual elevation or tragic dejection that are an essential part of the message. The insight into a law of nature also produces feelings and emotions, such as awe, joy of insight, satisfaction and the like. But they are not an essential part of the message.

It is often said that the role of intuition is a common factor in art and science. There is rarely a progress made in science without an intuitive perception of some idea or of some hidden relations. In art, of course, intuition is the essential driving force of creativity. However, scientific and artistic intuition are not always of the same character. True enough, the first spark of an idea or the first glimpse of some grand unification may come to the scientist in a similar unexplainable flash of insight as an artistic revelation. But, more often than not, scientific intuition comes from an unconscious or half-conscious awareness of existing knowledge or of connections between concepts that have not yet been consciously realized. But any intuitive scientific insight must be rationally validated afterwards before it can be incorporated into the scientific edifice. In contrast, artistic intuition is the main instrument of creation and does not require any additional validation; it reigns superior and is the highest instance of judgment, over and above the mold of style and fashion.

V. Hope

In what sense does the universe make sense? In the sense you sense a sense. Every true scientist feels a sense, consciously or unconsciously. If he did not, he would not go ahead with that fervor, so common among scientists, in his search for something that he calls the truth. Surely there is a large amount of ambition, mixed into this fervor--acclaim, tenure and Nobel prize--but there is no denying that this great fervor exists. It is based upon a conviction that what he does is worthwhile and will lead to an increase of insight, something that is great and valuable beyond any doubt, even if the fallibility of mankind makes the wrong use of it. Great insight leads to great power; great power always leads to great abuse.

The decay of a sense for meaning and the increase of cynicism in our culture has also contaminated the community of natural scientists and has shaken that conviction in various degrees for various members of that community; but there is still a good deal of belief in the purpose and meaning of their collective work. I cannot help feeling that they represent a "happy breed of men" among so many others who grapple with the problems of meaning, sense and purpose.

The emerging scientific "Weltbild" contains much to support the enthusiasm and fervor of its propagators. The great unifying principles that underlie the plenitude of events become clearer with every decade. An outline of a history of the universe from the big bang to the human brain is taking shape and becomes ever more convincing with the discoveries and insights that emerge from year to year. What is more startling and uncanny than the recent observation of the optical reverberation of the origin of the universe in form of the cold radiation that fills all space? What is more impressive than the steady extension of our insights into the structure of matter, from molecules and atoms to nuclei, electrons, nucleons and quarks, steadily approaching the basic entities of matter, and the growing understanding of nature's fundamental forces? What is more overwhelming than the recognition of the chemical basis of life, in which the stability of the atomic quantum state emerges as the main cause of the fact that the same flowers appear again every spring.

Do we find a similar fervor and a sense of purpose among other groups? Surely we do; we find it among those who are devoted to creative, artistic activities and among those who try to improve the social fabric of our times in many different ways. However, they face a much greater challenge. The problems of natural science are much less messy and much less interwoven with the complexity and fragility of the human mind. It is much easier to perceive an underlying order in the flow of natural events if human behavior is excluded.

The decay of the previously existing sources for meaning, sense and purpose, such as myth and religion, has left a big void in our bellies, as a friend of mine said, a void that craves to be filled. Every human being craves for a meaning and a sense to his life to endow it with luster and light. With the decay of myth and religion all that was left was an autonomous art that has made itself independent of any prevalent religion, and a new most vigorous intellectual development: science. Can these two enterprises serve as providers of meaning and sense? Goethe has said

> He who has Art and Science
> Has also a religion
> But those who do not have them
> Better have religion.

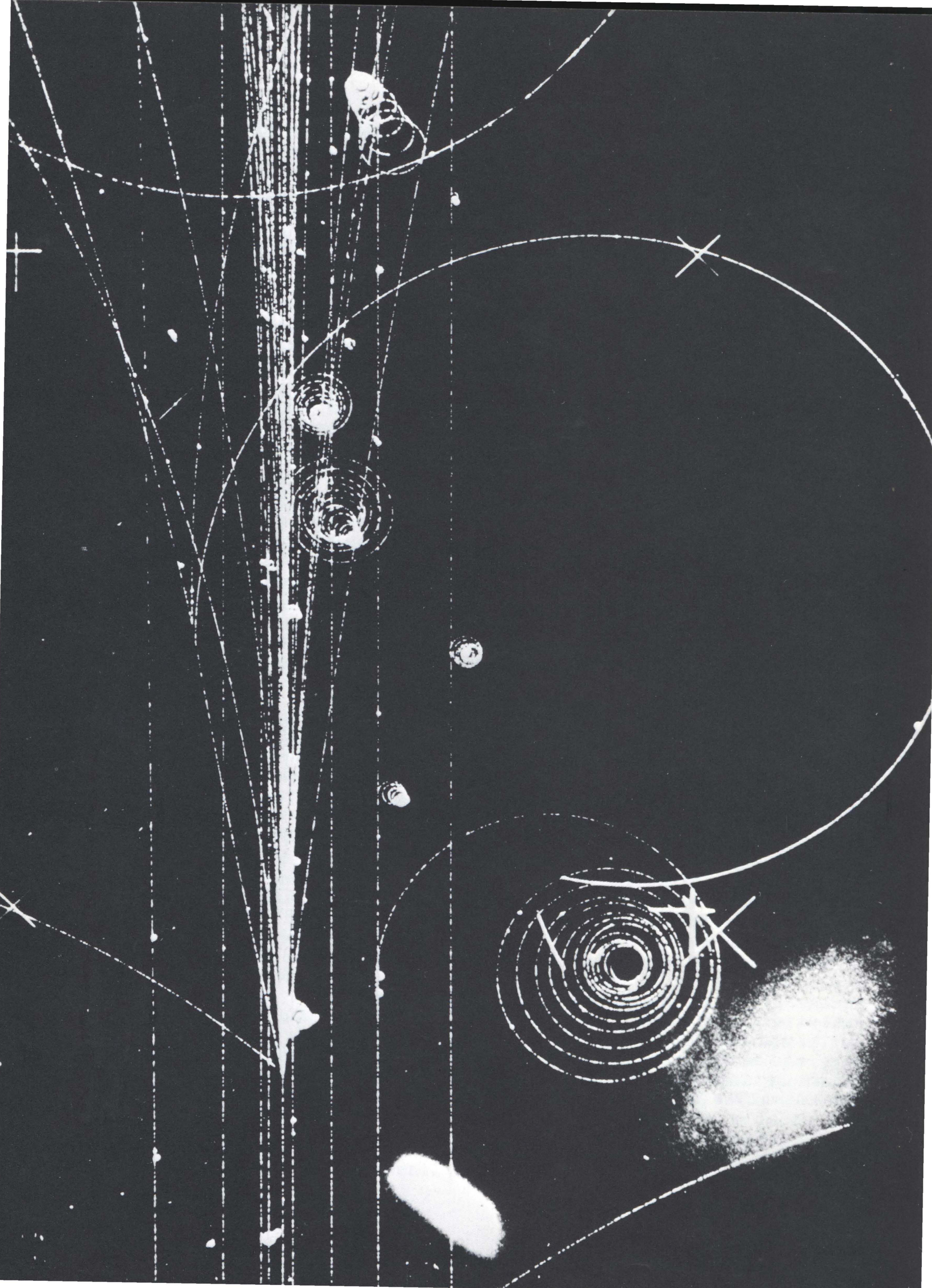
Goethe's remark points out one important element common to both expressions of the human mind. Their true significance is not easily accessible to a large part of mankind. Of course, there are many expressions of art and some of science that are indeed appreciated by large groups of people, such as folk art, popular art, popular science and science fiction. However, these manifestations are not the most effective providers of sense and meaning. The grandest creations and achievements of art and science serve as inspirational sources only to a small minority of humans; their values seem to be not suitable for a wider spread. The large majority cannot get meaning, sense and purpose from these sources. They must have some sort of religion as Goethe says. Perhaps it is the greatest problem of our day that this craving is no longer fulfilled by the conventional religions and that there is nothing to replace it.

The kind of meaning that science provides to its perpetrators has not proven to satisfy this craving, in spite of the fact that everybody is fully aware that we live in an age dominated by science and technology. On the contrary, this awareness is tied to a large extent to practical applications among which the military ones and the destructive effects of technology on the environment play an important role. The scientific insights into the greatness and unity of the universe in the large and in the small have not penetrated much into the minds of the people. It probably is the fault of the scientists who do not try hard enough transmitting the elation they feel at the peak moments of their work. They are too much immersed in their narrow specialities and do not sufficiently seek to express the deep connections which their insights have provided. It also is partly the fault of the artists and writers of today who neglect this task. Is it not the duty of art to remold all that is great and awe inspiring in our culture and to lend it a form that stirs the souls of men? It may be, however, that the great ideas of science are not suitable for inspiring outsiders with any true elation.

What is it then, that contemporary art expresses? Almost exclusively it deals with the tragedy and the depth of our lack of purpose and meaning. In this effort our art is powerful, heart-rendering and deeply depressing. It acts as an amplifier of what is meant with the aforementioned void in the belly; it follows the great tradition of art by elevating it to grand tragedy. Even cynicism has been ennobled by contemporary art. But rarely do we find the ingredients that permeated art in past centuries: beauty or hope.

In the meantime the members of the Goethe group get some luster of life from scientific insights if they can, or from works of art; not in the least from the classical works of art which have retained their power and significance; perhaps they seem today even more powerful and significant because they contain so much of those ingredients that are missing in contemporary art.

For the others among our fellow men, and that is the vast majority, the burden is much harder. Our material and spiritual world is in disorder and in danger of destruction. The great insights and elations of science as well as of art have not much impact on most of the people because these values are not connected with a ground swell of meaning permeating the collective mind. But there are many signs and portents among the younger generation of a mounting craving for sense and purpose and for the dignity of the individual. This ground swell appears in various forms, some are constructive, some are destructive. There are promising efforts to improve the social and spiritual climate, there are cults and semi-religious sects. All too often, some of these cults and sects have led to a misconceived mysticism and to a concentration on the inner self without the necessary relations to society. Maybe there will come a day when scientific and artistic meaning will combine and help to bring forth that ground swell of meaning and value for which there is so great a need. The growing awareness of this need is in itself an important element that brings people together and creates common values and even elations. There is always hope--for hope.

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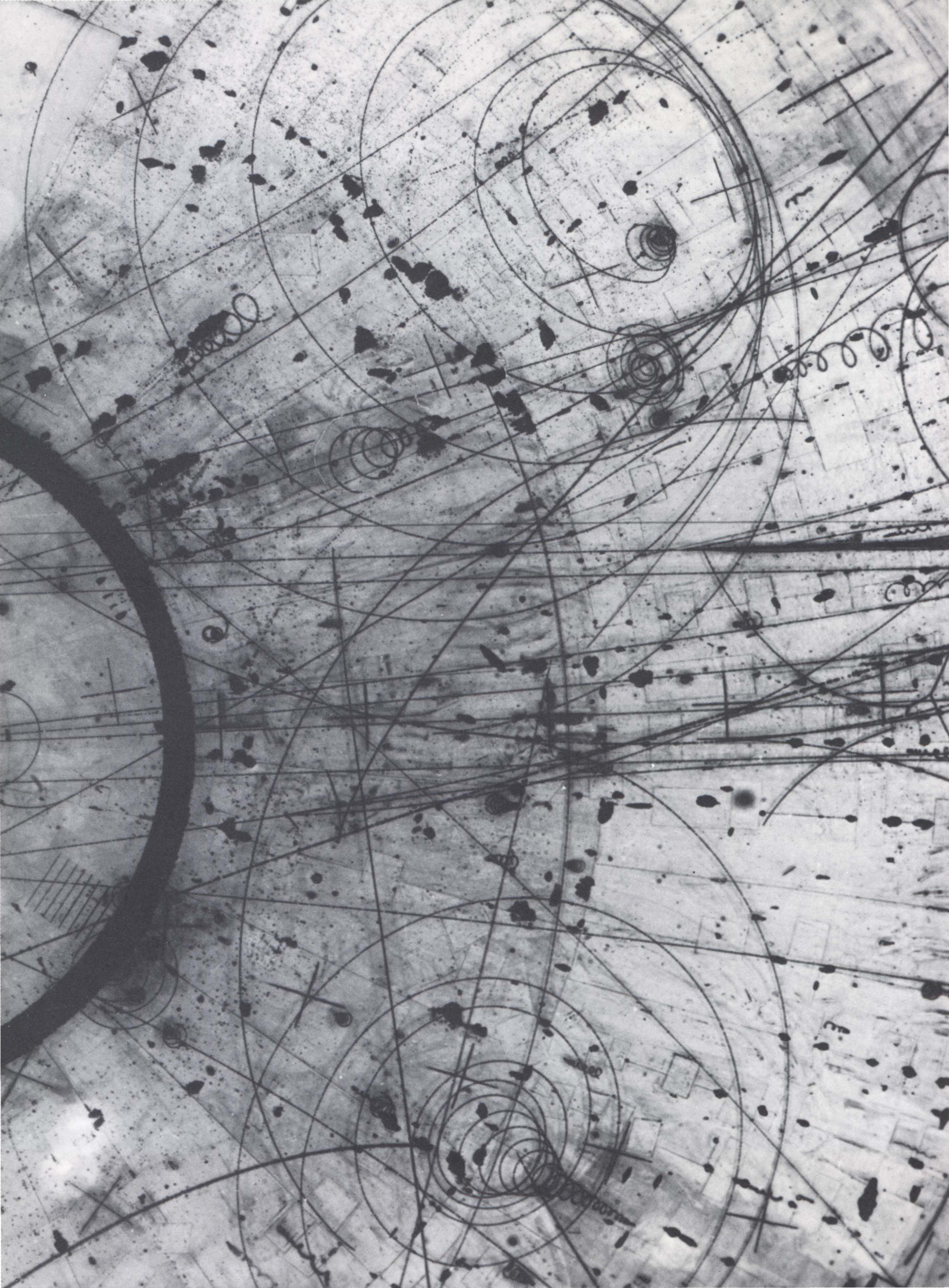

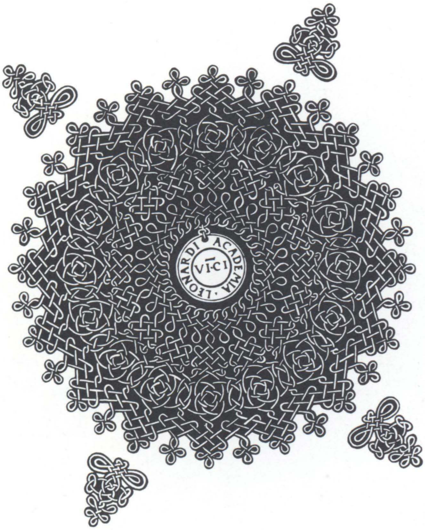
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