DOCUMENT RESUME

ED 257 661 SE 045 757

TITLE	Views on Science Policy of the 1983 U.S. Nobel Laureates in Science. Hearing before the Committee on Science and Technology, U.S. House of Representatives, Ninety-Eighth Congress, Second Session, March 8, 1984.
INSTITUTION	Congress of the U.S., Washington, D.C. House Committee on Science and Technology.
PUB DATE	85
NOTE	73p.
PUB TYPE	Legal/Legislative/Regulatory Materials (090)
EDRS PRICE	MF01/PC03 Plus Postage.
DESCRIPTORS	Astronomy; Botany; Chemistry; Economics; Federal Aid; *Financial Support; Genetic Engineering; *Government Role; Hearings; Higher Education; Mathematics; *Opinions; Physics; *Policy; *Sciences; Scientists
IDENTIFIERS	Congress 98th; *Nobel Prizes; *Science Policy

ABSTRACT

Four of the 1983 United States Nobel laureates in science provided the U.S. House of Representatives Committee on Science and Technology with their views about the current state of American science and U.S. government science policy as well as comments on one or more aspects which they believed should be brought to the committee's attention. These laureates are: (1) Gerard Debreu (laureate in economics for research on market equilibrium); (2) William Fowler (one of two astrophysicists awarded the Nobel prize in physics for research on stellar evolution); (3) Barbara McClintock (laureate in medicine and physiology for her discovery of "mobile genetics elements"); and (4) Henry Taube (laureate in chemistry for creative work in inorganic chemistry). One of the major areas addressed by these laureates was the continued financial support for science programs, laboratory equipment, and training of future scientists. (JN)

VIEWS ON SCIENCE POLICY OF THE 1983 U.S. NOBEL LAUREATES IN SCIENCE

HEARING

BEFORE THE

COMMITTEE ON SCIENCE AND TECHNOLOGY U.S. HOUSE OF REPRESENTATIVES

NINETY-EIGHTH CONGRESS

SECOND SESSION

MARCH 8, 1984

[No. 133]

Printed for the use of the Committee on Science and Technology

U.S. GOVERNMENT PRINTING OFFICE
41-057 O WASHINGTON : 1985

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WITNESSES

VIEWS ON SCIENCE POLICY OF THE 1983 U.S. NOBEL LAUREATES IN SCIENCE

THURSDAY, MARCH 8, 1984

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
Washington, DC.

The committee met, pursuant to notice, at 10 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the committee) presiding.

Present: Representatives Fuqua, Winn, Walker, Ottinger, Sensenbrenner, Gregg, McGrath, Walgren, Skeen, Gore, Volkmer, Bateman, Boehlert, McCandless, Lewis, Dymally, Valentine, Reid, and Boucher.

Staff present: Harold P. Hanson, executive director; John Holmfeld, science policy staff; David S. Jeffrey, minority staff director; and Dave Clement, minority counsel.

Mr. FUQUA. The committee will be in order.

It is both a pleasure and honor to again have these hearings with the Nobel laureates in science. This is our sixth annual hearing with the Nobel laureates in science. In the past years we have, with one exception, only been able to have the American laureates come before the committee.

This year, however, for the first time since 1976 all the laureates are Americans and we are especially pleased that all but one of these distinguished U.S. scientists can be with us today.

The fact that since 1955 American scientists have been strongly represented among the science laureates is, I believe, testimony to the strength of America's science in general. It suggests that the system of Federal support for science which we have evolved in this country following World War II is basically sound.

That basic strength does not mean that there is no room for improvement and for adjustment to emerging conditions and circumstances. We have therefore invited our witnesses today to give us the benefit of their views about the current state of American science and our Government science policy.

We have asked you to discuss one or more aspects of those issues which in your view should be brought to our attention. Following your comments, members of the committee will probably have some questions that we will ask you to respond to, if you so choose.

We want to welcome you here today and thank you for taking time from your busy schedules to join us in this consideration of the state of American science.

I would like at this time to recognize the distinguished gentleman from Kansas, Mr. Winn, who is the ranking Republican on the committee, for any comments that he wishes to make.

Mr. WINN. Thank you, Mr. Chairman.

I appreciate the opportunity to join you in welcoming our distinguished guests today. The awarding of Nobel Prizes in Science to American citizens has become such a common event that many people are not even aware that this distinct honor can be bestowed on citizens of any nation of the world.

While each of us is certainly aware that your individual, tremendous achievements in science overreach national boundaries, at the same time, we cannot help but be proud as American citizens. And as a Member of Congress and a member of the prime committee for the authorizing of Federal funds and Federal support for science research and development, I believe each of us here takes pride in the fact that each of you is an affirmation of the success of our efforts to create the proper climate nationally for the continuation of fruitful scientific endeavors.

I would be remiss if I did not take this opportunity to mention briefly a topic that is probably foremost on the minds of the Members of Congress these days. That topic is the size of the Federal deficit.

As I hope you all are aware—and I know you are—we are looking at a national deficit for the fiscal year 1985 somewhere in the range of $180 to $200 billion. Faced with these kinds of frightening figures, Members of Congress have to be extremely prudent when it comes to authorizing or appropriating money for either new or existing programs.

Certainly, in an ideal world the members of this committee would like to see a great deal more money spent in fostering scientific research and development. Unfortunately, in this real world that cannot be the case.

It thus becomes the difficult job of this committee to set priorities for Federal support of science. We would certainly appreciate any words of advice that each of you could give to the committee on the problem of setting priorities in science from your perspective in the scientific community.

Mr. Chairman, as in years past, it has always been a great honor to have such distinguished scientists appear before us as witnesses and guests, and I look forward with a great deal of interest to their testimony.

Thank you, Mr. Chairman.

Mr. FUQUA. Thank you, Mr. Winn.

We are pleased to have as our guest today the 1983 Laureate, Dr. Gerard Debreu, a Laureate in Economics from the University of California at Berkeley; Dr. William A. Fowler, a Laureate in Physics from the California Institute of Technology; Dr. Barbara McClintock, a Laureate in Medicine and Physiology from Cold Spring Harbor Laboratory, Long Island, and Dr. Henry Taube, a Laureate in Chemistry from Stanford University.

Dr. Debreu, we will be pleased to hear from you. I know some of you have prepared statements, others may not. But we would be pleased to hear from you and we will go down the line. If the panel—the distinguished panel, I should say—wishes to make any comments, and at the end there may be other comments.

Dr. Debreu, we will be pleased to hear from you at this time.

[The biographical sketch of Dr. Debreu follows:]

BIOGRAPHY OF GERARD DEBREU

Born in Calais (France)	July 4, 1921
Ecole Normale Supérieure, Paris (France)	1941-44
French Army (North Africa - Germany)	1944-45
Agrégé de l'Université, Paris (France)	1946
Research Associate of the Centre National de la Recherche Scientifique, Paris (France)	1946-48
Rockefeller Fellow (U.S.A.-Sweden-Norway)	1948-50
Research Associate of the Cowles Commission for Research in Economics, University of Chicago	1950-55
D.Sc., Université de Paris (France)	1956
Associate Professor of Economics, Cowles Foundation for Research in Economics, Yale University	1955-61
Professor of Economics (and Mathematics since July 1975), University of California, Berkeley	January 1962-
President of the Econometric Society	1971
Fellow of the American Academy of Arts and Sciences	1970-
U. S. Citizen	July 1975
Chevalier de la Légion d'Honneur	July 1976
Dr. rer. pol. h.c., University of Bonn	April 1977
Member of the National Academy of Sciences of the U.S.A.	1977-
Docteur en Sciences Economiques h.c., Université de Lausanne	October 1980
Doctor of Science h.c., Northwestern University	June 1981
Distinguished Fellow of the American Economic Association	1982
Docteur h.c. de l'Université des Sciences Sociales de Toulouse	June 1983
Nobel Memorial Prize in Economic Sciences	1983
Commandeur de l'Ordre National du Mérite	January 1984

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Information

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THIS YEAR'S ECONOMICS PRIZE IS AWARDED FOR RESEARCH ON MARKET EQUILIBRIUM

The Royal Swedish Academy of Sciences has decided to award the 1983 Prize in Economic Sciences in Memory of Alfred Nobel to Professor Gerard Debreu, University of California, Berkeley, USA, for having incorporated new analytical methods into economic theory and for his rigorous reformulation of the theory of general equilibrium.

General market equilibrium

This year's Prize is awarded for penetrating basic research work in one of the most central fields of economic science, the theory of general equilibrium.

In a decentralized market system, individual consumers and firms make decisions on the purchase and sale of goods and services solely on the basis of self-interest. Adam Smith had already raised the question of how these decisions, apparently independent of one another, are coordinated and result in a situation whereby sellers usually find outlets for their planned production, while consumers realize their planned consumption. Smith's answer was that, given price and wage flexibility, price systems automatically bring about the desired coordination of individual plans. Towards the end of the 19th century, Léon Walras formulated this idea in mathematical terms as a system of equations to represent consumers' demand for goods and services, producers' supply of these same goods and services and their demand for factors of production, and equality between supply and demand, i.e. equilibrium in each market. But it was not until long afterward that this system of equations was scrutinized to ascertain whether it had an economically meaningful solution, i.e. whether this theoretical structure of vital

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THE ROYAL SWEDISH
ACADEMY OF SCIENCES

17 October 1983

importance for understanding the market system was logically consistent.

Gérard Debreu's major achievement is his work in proving the existence of equilibrium-creating prices. His first fundamental contribution came in the early 1950s, in collaboration with Professor Kenneth Arrow. Arrow received the 1972 Prize in Economic Sciences in Memory of Alfred Nobel for his work in this and other adjacent fields.

Arrow and Debreu designed a mathematical model of a market economy where different producers planned their output of goods and services and thus also their demand for factors of production in such a way that their profit was maximized. Thus connections were generated within the model between the supply of goods and demand for factors of production on the one hand and all prices on the other. By making additional assumptions about consumer behaviour Arrow and Debreu were able to generate demand functions or "correspondences", i.e. relations between prices and supplied and demanded quantities. In this model Arrow and Debreu managed to prove the existence of equilibrium prices, i.e. they confirmed the internal logical consistency of Smith's and Walras' model of the market economy.

Subsequent to these pioneering efforts, there has been considerable development and extensions of such proofs with Gerard Debreu at the forefront. His book "Theory of Value" from the late 1950s has already become a classic both for its universality and for its elegant analytical approach. The theory developed in this study lends itself to many far-reaching interpretations and applications. The concept of "goods", for instance, is defined so broadly that the theory may be used in pure static equilibrium analysis, the analysis of the spatial distribution of production and consumption activities, intertemporal analysis and the analysis of uncertainty. Thus, within the same model Debreu's general equilibrium theory intergrates the theory of location, the theory of capital, and the theory of economic behaviour under uncertainty.

Efficient resource utilization

An essential issue which is related to the market economy and which can also be traced back to Adam Smith concerns the normative properties of the market allocation of resources. Will the fulfillment of self-interest through the "invisible hand" of the market mechanism lead to efficient utilization of scarce resources in society? Will the resources be used and production adapted so as to result in a situation where any attempt to make one individual better off necessarily means taking away from other individuals, i.e. a situation without any waste whatsoever? It has long been known that in certain circumstances market price formation has such efficiency properties, but the exact nature and full extent of the conditions which must be satisfied in order to guarantee them had not been determined. Through the work of Debreu and his successors, these condition have been clarified and analysed in detail.

Stability

The idea of the market economy also concerns stability of equilibrium. An interesting property of stability involves the question of whether it would be profitable for any group of market agents to withdraw completely from the market economy so as to ensure, independently, improvement of the agents' own economic position. Debreu has made enduring contributions in this field, especially in a joint article from the early 1960s with Herbert Scarf, and through subsequent major accomplishments. He has shown that in very large economies (i.e. with numerous market agents) it will not be profitable for any group to cease trading in the markets. Hence in this respect the market equilibrium will be stable.

Debreu has also made significant contributions to the theory of consumer behaviour. He has indicated possible representations of consumer preferences in terms of so-called utility functions, and has also studied the feasibility of consistently aggregating individual demand functions over groups of individuals.

Debreu's influence

Debreu's foremost contribution is perhaps of a more indirect nature, however. His clarity, analytical stringency and insistence on always making a clearcut distinction between a theory and its interpretation have had a profound and unsurpassed effect on the choice of methods and analytical techniques in economics.

Gerard Debreu

Born in Calais, France, on 4 July, 1921
Became citizen of USA in 1975
Doctorate at the University of Paris, 1956
Professor of Economics, University of California, Berkeley, 1962
Professor of Mathematics, University of California, Berkeley; 1975

Address

Professor Gerard Debreu
Department of Economics
University of California
Berkeley, CA 94720
USA

tel.: (415) 642-7284

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STATEMENT OF DR. GERARD DEBREU, 1983 LAUREATE IN ECONOMICS, AND PROFESSOR OF ECONOMICS AND OF MATHEMATICS, UNIVERSITY OF CALIFORNIA, BERKELEY, CA

Dr. DEBREU. Thank you, Mr. Chairman.

American scientists were given a special accolade in 1983 when five of them received every Nobel Award in the fields of physics, chemistry, physiology, medicine, and economics.

Only three times before, in 1946, 1968, and 1976, were all the science prizes awarded to the United States. Our appearance before your committee, therefore, marks an exceptional occasion which is made more exceptional still by the presence among us of one of the seven women laureates in the 83-year history of the Nobel Science Prizes. Another statistic further underscores the success of scientific research in this country. Out of a total of 180 living science laureates, 103 are American citizens. This record should permit us to be objective when we check the health of our research establishment.

The environment provided for scientific work by our universities, private foundations, corporations, Government agencies, and especially the National Science Foundation, has been superb during the greater part of the last half century. I vividly recall the deep impression that it made on me at the time I discovered it in 1949 as a Rockefeller fellow making a 1-year visit to the United States, and especially after June 1, 1950, when I became a research associate of the Cowles Commission for Research in Economics at the University of Chicago. In the summer of 1955 I moved to Yale University with the Cowles group, and in 1962 I joined the University of California at Berkeley where I am presently a professor of economics and of mathematics.

The three great universities with which I have been associated during the past 34 years provided a nearly perfect setting for the research I have wanted to do. Moreover, during the last two decades, the Economics Division of NSF has given me, more than anything else, time for that research. To all these institutions I owe a great debt. There is perhaps no more appropriate opportunity for me to acknowledge it than that offered by this hearing.

But it is precisely because that environment has been so valuable for me that I have become concerned about its preservation. Many scientists have made statements to this committee about the support of fundamental research by the Federal Government. Since the initiation of these special hearings, however, in 1979, only one of the five new American Nobel laureates in economics testified, and mathematics was never represented. Therefore I take it as my particular responsibility to speak today about the two fields with which I am associated.

The least perceptive observers of the influence of science on our society are impressed by its spectecular manifestations, among them the exploration of space, the harnessing of nuclear energy, and the development of computer technology. But few of those observers fully understand how each one of those achievements necessarily rests on a sophisticated mathematical basis. Because abstraction is of the essence in mathematics, it is difficult for mathematicians to explain to nonscientists, sometimes even to scientists, the results obtained in their field.

Moreover, the impact of a mathematical idea on other scientific fields may be felt only after a significant time lag, and on technological realization after an even longer delay. The misperception of the role of mathematics in our scientific culture has powerfully contributed to an underestimation of its value and, indirectly, to its underfunding.

Thus, from fiscal 1970 to fiscal 1982, the support of NSF in constant dollars increased by 26 percent in physics and by 43 percent in chemistry. In the mathematical sciences it decreased by 2 percent. The effects of this small decrease were magnified by the drastic reduction in the support of basic research in mathematics by the Department of Defense, beginning in the midsixties.

The consequences have been deeply felt in many ways through the entire mathematical community of the United States. Mathematicians in their most productive years have not been given the research time that they need. An increasing number of exceptionally gifted students of mathematics have turned to other activities.

However, after more than 13 years of level support by NSF mathematics and of sharply decreased support by the Department of Defense, the danger signals sent by mathematicians were perceived in Washington. From fiscal 1982 to fiscal 1983 the constant dollar support of the mathematical sciences by NSF increased by 5 percent. This was followed from fiscal 1983 to fiscal 1984 by an 8-percent increase that carried the support of the mathematical sciences by NSF to $41.6 million.

Thus, the first steps have been taken to restore the momentum of mathematical research in the United States. A full restoration of that momentum will require a doubling of the 1984 level of support.

While mathematical sciences suffer because too few try to comprehend their accomplishments, economic sciences suffer because too many readily believe that they can pass judgment on their performance. The resulting misconceptions are great and widespread. According to a narrow view, science must predict. Yet, the natural sciences have great theories that are nonpredictive. The theory of evolution is one of them. Another is the history of the universe that physicists are currently writing. Both the theory of evolution and the history of the universe attempt to explain, as does a large part of economic theory. When economists undertake to predict, the imperfections of their forecasting are often judged without reference to the imperfect forecasting of meteorologists or to the immense complexity of economic systems and to the numerous noneconomic shocks to which they are subject.

The limitations on the scope of economic theory and the imperfections of economic forecasts have sometimes led to an agnostic approach that consists of accumulating statistical data in the hope that regularities will suggest themselves. That this methodology has led to disappointments would not surprise the natural scientists who are guided by theoretical hypotheses in collecting their data.

These, and other factors, have resulted in the dramatic decrease in the constant dollar budget of the economics program of NSF by

45 percent from fiscal 1979 to fiscal 1982. The devastating 45-percent cut that took place within that 3-year period was followed by a further 2-percent cut from fiscal 1982 to fiscal 1983, and then by a sudden 26-percent increase from fiscal 1983 to fiscal 1984, carrying the NSF economics budget to $9.4 million. The 26-percent increase expérience in 1 year indicates an abrupt reversal of the recent trend. It must be followed by an increase in the order of 70 percent in the support of the core subjects of economics to bring that support back to the level of fiscal 1978.

The field of mathematical economics, in which I have worked for some 35 years and that I more specifically represent today, might seem to have been put in double jeopardy by the budgetary developments in mathematics and in economics that I have reviewed.

In fact, the impetus that it received in the forties has not yet spent itself completely. It will, however, need reinforcement in the near future. For that impetus a large share of the tribute must be paid to the memory of one of the great scientists of our period. John von Neumann at the same time made contributions of the first magnitude to mathematics, to theoretical physics, to the development of computers, and to economics. The last field occupied him for several years of his research career. He appreciated its intellectual challenges and overcame several of them. He would have been an outstanding witness at a hearing of your committee similar to the present.

John von Neumann was one of the many magnificent gifts that Adolf Hitler made to the United States. This country deserved those gifts and was able to receive them because of its open door policy. The continued migration of scientists to the United States following World War II is reflected in the fact that at the present time 30 of the 103 living American Nobel science laureates are foreign born. It should therefore also be a matter of concern for the science policy of this country that its door is no longer so wide open as before to scientists who wish to move to the United States.

Thank you for the privilege to address you.

[The prepared statement of Dr. Debreu follows:]

Statement of Gerard Debreu
Professor of Economics and of Mathematics,
University of California, Berkeley
before
The Committee on Science and Technology
U. S. House of Representatives
Thursday, March 8, 1984

Mr. Chairman and Members of the Committee on Science and Technology,

American scientists were given a special accolade in 1983 when five of them received every Nobel Award in the fields of Physics, Chemistry, Physiology-or-Medicine, and Economics. Only three times before (in 1946, 1968, and 1976) were all the science Prizes awarded to the United States. Our appearance before your Committee therefore marks an exceptional occasion, which is made more exceptional still by the presence among us of one of the seven women Laureates in the 83-year history of the Nobel science Prizes. Another statistic further underscores the success of scientific research in this country. Out of a total of 180 living science Laureates, 103 are American citizens. This record should permit us to be objective when we check the health of our research establishment.

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judged without reference to the imperfect forecasting of meteorologists or to the immense complexity of economic systems and to the numerous non-economic shocks to which they are subject. The limitations ~~on the~~ scope of economic theory and the imperfections of economic forecasts have sometimes led to an agnostic approach that consists of accumulating statistical data in the hope that regularities will suggest themselves. That this methodology has led to disappointments would not surprise the natural scientists who are guided by theoretical hypotheses in collecting their data. These, and other factors, have resulted in the dramatic decrease in the constant dollar budget of the Economics Program of NSF by 45% from Fiscal 1979 to Fiscal 1982. The devastating 45% cut that took place within that three-year period was followed by a further 2% cut from Fiscal 1982 to Fiscal 1983 and then by a sudden 26% increase from Fiscal 1983 to Fiscal 1984 carrying the NSF-Economics budget to 9.4 million. The 26% increase experienced in one year indicates an abrupt reversal of the recent trend. It must be followed by an increase on the order of 70% in the support of the core subjects of economics to bring that support back to the level of Fiscal 1978.

The field of mathematical economics, in which I have worked for some 35 years and that I more specifically represent today, might seem to have been put in double jeopardy by the budgetary developments in mathematics and in economics that I have reviewed. In fact the impetus that it received in the forties has not yet spent itself completely. It will however need reinforcement in the near future. For that impetus a large share of the tribute must be paid to the memory of one of the great scientists of our period. John von Neumann at the same time made contributions of the first magnitude to mathematics, to theoretical physics, to the development of computers, and to economics. The last field occupied him for several years of his research career. He appreciated its intellectual challenges and overcame several of them. He would have been an outstanding witness at a hearing of your Committee similar to the present.

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Mr. FUQUA. Thank you very much, Dr. Debreu.

Dr. Fowler, we will be pleased to hear from you at this time.

[The biographical sketch of Dr. Fowler follows:]

FOWLER, WILLIAM ALFRED, Institute Professor of Physics, California Institute of Technology, 1565 San Pasqual St., Pasadena, Calif. b. Aug. 9, 1911, Pittsburgh, Pa. s. John McLeod (deceased) and Jennie Summers (Watson) Fowler(deceased) Lima, Ohio. m. Ardiane Foy Olmsted of Pasadena, Calif., Aug. 24, 1940. c. Mary Emily Fowler, Martha Summers Fowler Schoenemann.

Education: Lima Central High School, 1929; Bachelor of Engineering Physics, Ohio State Univ., 1933; Ph.D. in Physics, Calif. Inst. of Tech., 1936; Ph.D. Dissertation: Radioactive Elements of Low Atomic Number.

Positions: California Institute of Technology: Research Fellow in Nuclear Physics, 1936-39; Asst. Prof. of Physics, 1939-42; Assoc. Prof. of Physics, 1942-46; Prof. of Physics, 1946-70; Institute Professor of Physics, 1970-1982. Institute Professor of Physics, Emeritus, 1982. Fulbright Lecturer, Cambridge University, 1954-55; Guggenheim Fellow, 1954-55, 1961-62. Numerous named lectureships, American universities & colleges, 1957-

Affiliations:
American Physical Society 1938, President 1976
National Academy of Sciences 1956, Council 1974-77
International Astronomical Union 1958
American Astronomical Society 1958
American Philosophical Society 1962
Cosmos Club 1965
Fellow, American Academy of Arts and Sciences 1965
Fellow, Royal Astronomical Society 1968, Associate 1975
Benjamin Franklin Fellow, Royal Society of Arts 1970
Honorary Member, Mark Twain Society 1976
Astronomical Society of the Pacific 1979
Planetary Society (Founding Member) 1980-
National Association of Railroad Passengers 1981-
Society for Scientific Exploration (Founding Member) 1982-

Member:
Naval Research Advisory Committee (NRAC) 1962-70
Space Science Board (NAS) 1970-73, 1977-80
National Science Board (NSF) 1968-74
Nuclear Science Advisory Committee (NSF/DOE) 1977-81, Chairman 1977-79
Visiting Committee for NRAO 1977-1981
Advisory Committee, Physics Division, NSF 1981-1984
Member, Commission on Phys. Sciences, Mathematics & Resources NAS/NAE/NRC 1981-1984
Chairman, Office of Physical Sciences, AMPS NAS/NAE/NRC 1981-1982
Los Alamos Advisory Committee on the National Underground Science Facility (NUSF)1982-
Member, Panel on Public Affairs, APS & other Committees APS/AIP 1981-

Awards:
Naval Ordnance Development Award, USN, 1945
Medal for Merit, USA, 1948 (President Harry Truman)
Ohio State University Lamme Medal 1952
Liège Medal, Université de Liège, 1955
California Co-Scientist of the Year 1958 (with H. Fraenkel-Conrat, Biochemist, Berkeley
Barnard Medal for Meritorious Service to Science for 1965, Columbia University
Apollo Achievement Award, NASA, 1969
Tom W. Bonner Prize. American Physical Society. 1970
G. Unger Vetlesen Prize, Columbia University, 1973
National Medal of Science, USA, 1974 (President Gerald Ford)
Honorary Doctor of Science. University of Chicago. [illegible]
Eddington Medal, Royal Astronomical Society, London,
Honorary Doctor of Science. Ohio State University. 1978
Bruce Gold Medal. Astronomical Society of the Pacific, 1979
Doctorat Honoris Causa. Université de Liège. 1981
Doctorat Honoris Causa. Observatoire de Paris. 1981 (Awarded 1982)
Honorary Doctor of Science. Denison University. 1982
[illegible] Prize [illegible] 1983

Research: Studies of nuclear forces and reaction rates; nuclear spectroscopy; the structure of light nuclei; nuclear astrophysics; thermonuclear sources of stellar energy and element synthesis in stars and supernovae; supernova models; isotopic anomalies in meteorites and the origin of the solar system; nucleocosmochronology; study of general relativistic effects in quasar and pulsar models.

Defense Record: Research and development proximity fuses, rocket ordnance, and atomic weapons; Research staff mem., Sec. T, NDRC; Research staff mem., Div. 4, NDRL 1941; Asst. Dir. of Research, Sec. L. Div. 3, NDRC 1941-45; Technical [illegible] of [illegible] New Developments Div., War Dept., [illegible] and Southwestern Pacific [illegible] 1944; Acting Supervisor, Ord. Div., R and D. NOTS 1945; Sci. Dir., Project VISTA, Dept. Defense 1951-52.

Information

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THIS YEAR'S PRIZE IN PHYSICS TO TWO ASTROPHYSICISTS

The Royal Swedish Academy of Sciences has decided to award the 1983 Nobel Prize in Physics to Professor Subrahmanyan Chandrasekhar, University of Chicago, Chicago, USA, for his theoretical studies of the physical processes of importance to the structure and evolution of the stars, and to Professor William A. Fowler, California Institute of Technology, Pasadena, USA, for his theoretical and experimental studies of the nuclear reactions of importance in the formation of the chemical elements in the universe.

The common theme of this year's Nobel Prize in Physics is stellar evolution. A star is formed from the gas and dust clouds which exist in the galaxies. Under the influence of gravity, there is a condensation which slowly contracts to form a star. In this process, energy is released which leads to the heating of the newly-formed star. Finally the temperature is high enough to set off nuclear reactions in the interior of the star. As a result, the hydrogen, forming the major part, is burnt to helium. This creates a pressure which stops the contraction and stabilizes the star so that it can exist for millions of years. When the hydrogen has been consumed, other nuclear reactions take over, particularly in the more massive stars, and increasingly heavy elements, up to iron, are formed. When the evolution has reached this stage, the star can no longer resist gravity, and it undergoes some form of collapse, the exact nature of which depends on the mass of the star. In some instances the collapse takes the form of an explosion whose visible result is the creation of a supernova. This brings about a brief but extremely intense flow of neutrons, which leads to the formation of the very heaviest elements. For less heavy stars having a mass of the order of our Sun, the collapse gives rise to a so-called white

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ACADEMY OF SCIENCES

19 October 1983

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dwarf. The matter has here been compressed so that one cubic centimetre weighs around 1 kilogramme. The electron shells of the atoms have been crushed and the star consists of atomic nuclei and electron gas. For slightly heavier stars, the final stage is an even more compressed state in which electrons and nuclei unite to form neutrons. For the heaviest stars having a mass in excess of 2-3 Solar masses, the force of gravity becomes so strong that the matter simply disappears in the form of a so-called black hole.

This should indicate that stellar evolution gives examples of a number of physical processes of fundamental importance. Many scientists have studied these problems, but Chandrasekhar and Fowler are the most prominent.

Chandrasekhar's work deals with a large number of features in stellar evolution. A major contribution is the study of the stability problem in different phases of the evolution. In recent years he has studied relativistic effects, which become of importance because of the extreme conditions arising during the later stages of stellar evolution. Chandrasekhar's possibly best-known achievement, accomplished when he was in his 20's, is the study of the structure of white dwarfs. Al - though many of these investigations are of older dates, they have through the great progress of astronomy and space research in recent years gained renewed interest.

Fowler's work deals with the nuclear reactions which take place in the stars during their evolution. In addition to generating the energy which is radiated, they are of importance because they lead to the formation of the chemical elements from the original matter, which chiefly consists of the lightest element, hydrogen. Fowler has done extensive work on the experimental study of nuclear reactions of astrophysical interest, as well as carried out theoretical calculations. Together with a number of co-workers, he developed ,during the 1950s, a complete theory of the formation of the chemical elements in the universe. This theory is still the basis of our knowledge in this field, and the most recent progress in nuclear physics and space research has further confirmed its correctness.

Subrahmanyan Chandrasekhar

Born in Lahore, India, on 19 October, 1910
Became citizen of USA in 1953
Ph.D., Trinity College, Cambridge, England 1933
Sc.D., 1942
Distinguished Service Professor of theoretical astrophysics, Yerkes Observatory, University of Chicago, 1946
Foreign member of the Royal Swedish Academy of Sciences, 1973

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William A. Fowler

Born in Pittsburgh, USA, on 9 August, 1911
Ph.D., California Institute of Technology, Pasadena, USA, 1936
Professor of Physics, California Institute of Technology, Pasadena, USA, 1946

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STATEMENT OF DR. WILLIAM A. FOWLER, 1983 LAUREATE IN PHYSICS, INSTITUTE PROFESSOR OF PHYSICS, EMERITUS, THE CALIFORNIA INSTITUTE OF TECHNOLOGY, PASADENA, CA

Dr. FOWLER. Mr. Chairman, members of the committee, slightly over 2 years ago, on March 4, 1982, I testified before your Subcommittee on Science, Research and Technology. Mr. Walgren was in the chair and Mr. Dymally was present, on the then critical situation concerning instrumentation in university and college laboratories.

It is gratifying to know that substantial progress has been made in alleviating this situation in the last 2 years. NSF, DOE, and DOD have in place programs dedicated in one way or another to funding the purchase of scientific equipment, especially in universities with graduate departments.

There is still some concern in my mind regarding the situation in undergraduate colleges which give bachelor's degrees based on studies in the sciences. It is my contention that undergraduate colleges should be provided with research grants and the appropriate instrumentation if they offer degrees in the sciences. Aspiring young scientists, whether headed for teaching or research, should be given hands-on training in real research not just laboratory exercises.

One cannot learn modern physics playing with inclined planes and pulleys. Even those destined for a career in theory ought to have the option of doing a hands-on experiment of some real relevance. All of this will require college science faculties with a few members who are interested in research as well as teaching. There will be a bonus: bett · people will be interested in college positions.

Now, in my testimony 2 years ago I said you are not going to hear from me a litany of what science can do for the general welfare, for the economy, or for the national defense. Let me assure you, I do not deprecate the applications of science to engineering and engineering to technology. I do hope that these applications will be benign.

My field of research is the application of nuclear physics to astronomy. It is a benign application in comparison to reactors and bombs. The whole matter has been a paradox for me all of my professional life. I do not know the answer to that paradox but of one thing I am certain. The science of nuclear physics which explains the origin of sunlight must never be used to raise a dust cloud which will obscure that sunlight.

Well, be that as it may, my primary concern is that science be supported by the Government in part because of its contributions to the intellectual needs of the American people and all the people in the world, for that matter.

Men and women have intellectual needs as well as cultural and physical needs. These intellectual needs can only be satisfied by the continuing effort to obtain new knowledge about the universe and about the physical laws which govern it.

Now, permit me to discuss briefly an example which is very close to my mind and my heart. In doing so, I may overstep the bounds of propriety regarding self-interest and special pleading, in that I am suggesting the support of a search for knowledge in which I am greatly interested. I can assure you, however, that the search will involve no monetary gain for me nor for the institution with which I am affiliated.

I begin at the beginning. Since earliest times, mankind has been interested in the Sun. When it became clear that all life on Earth depends on light from the Sun, the purely intellectual question arose—what is the ultimate source of energy for the rays of light from the Sun?

Early in this century, Lord Rutherford used radioactivity to measure the age of terrestrial rocks and found values exceeding 1 billion years. Much later, his methods applied to the meteorites and to the Moon yielded ages around 4½ billion years. In spite of tectonic activity on the Earth, some terrestrial rocks have been closed, undisturbed systems for close to 4 billion years. Sophisticated arguments indicate that the Sun and the other components of the solar system formed at the same time, 4.5 billion years ago.

What has provided the source of energy of the Sun over that prodigious length of time? Well, to make a long story short, it is now believed that the source of energy is the fusion of hydrogen into helium at the center of the Sun.

We know that fusion can power the Sun for about 7 billion years more before the Sun changes its present characteristics in any marked way. But to get to the point, there is a problem. I refer to it as "The Case of the Missing Solar Neutrinos."

The case of this: In the fusion of four hydrogen nuclei into one helium nucleus, two elementary particles we call neutrinos are released. These neutrinos have no electrical charge, no magnetic moment, probably no mass, and they interact only weakly with matter. Only 1 in 100 million neutrinos produced at the center of the Sun is intercepted on the way to the surface. At night they penetrate the Earth and reach us willy-nilly.

A simple calculation yields the fact that 60 billion neutrinos penetrate every square centimeter of the Earth and our bodies per second. Just as they pass through the Sun and Earth, they pass through our bodies without interacting and without producing any damage.

That is what physicists and astronomers believe. But we have no positive proof. In fact, fundamental problems have been raised by attempts to detect the rather rare high energy solar neutrinos made by Dr. Raymond Davis of the Brookhaven National Laboratory in the Homestake Gold Mine near Lead, SD. Davis has installed a 100,000-gallon tank of liquid perchloroethylene—ordinary cleaning fluid—1 mile deep in the mine. The chlorine in the perchloroethylene is the detector. It can only detect the higher energy neutrinos from the Sun and not the much more numerous low energy solar neutrinos.

In 60 observations over 13 years, Davis has found one-third—one-third, mind you—of the flux of these high energy solar neutrinos relative to what we expect if we understand what is going on in the Sun. Something is wrong. Many explanations have been suggested but most raise more problems than they solve.

It is generally agreed that attempts to determine the flux of the low energy neutrinos must be made, and the best detector for those neutrinos is the rare element gallium. There is the rub. Between 30 and 60 tons of gallium will be needed and gallium costs $500,000 per ton. There is one source of relief. The Federal Republic of Germany is a partner in this proposed set of observations. The Federal Republic of Germany will provide one-quarter of the gallium needed. They will match 1 to 3 that supplied by the United States, even though the costs of the gallium and the operation of the observations for 5 years will run to a total cost of at least $20 million. Who is to pay?

Well, before answering that question, I want to enter into the record the support of the scientific community for the gallium observations:

The field report, astronomy and astrophysics for the 1980's concludes:

> The committee urges continued financial support of the gallium detector experiment by this international collaboration, the State of Israel is also collaborating in this work, and the completion and operation of the full gallium neutrino detector at the earliest opportunity.

Second, a long-range plan for nuclear science, the NSAC report to NSF/DOE just last year:

> The solar neutrino problem raises a number of important scientific questions and has to be pursued. At the present time, scientific grounds dictate the choice for the next neutrino detector to be gallium on the basis of its demonstrated feasibility and recovery efficiency.

Third, review of the gallium solar neutrino project at Brookhaven National Laboratory, 1981. It was chaired by Glen Seaborg, Felix Boehm, Jerry Brown, George Field, myself, Terry Goldman, Martin Schwarzchild of Princeton, and Tony Turkevich from Chicago:

> The members of our panel have come to the conclusion that the gallium solar neutrino project has outstanding scientific merit on a broad interdisciplinary scale. We strongly recommend that the project be approved and authorized to proceed.

I mention these things, gentlemen, because I do not want to be accused of recommending a program that has not been thoroughly peer reviewed previously.

The trouble is, it is interdisciplinary. It is very hard to get any one discipline to carry the ball, but that is another argument I am sure you gentlemen are quite aware of.

Well, I return to who is to pay. The American people have paid billions of dollars—billions of dollars—to learn about the Moon, and Jupiter, and other planets. I submit that the Sun is far more important in their daily lives than the Moon and the planets. I have given my lecture on the case of the missing solar neutrinos over 100 times. Always, there is intense public interest in the source of energy for the life-sustaining light from the Sun. It is my deep feeling that $20 million of Government money would be well spent to solve a deep intellectual problem which can be appreciated by everyone.

Well, what are the practicalities? The Department of Energy has generously supported the Brookhaven program in solar neutrino research. I have no connection with Brookhaven whatsoever. Brookhaven should carry on. The Congress and the administration should provide the Department of Energy with the additional funds—and I stress additional—that are necessary to enable Brookhaven to do so. Whatever the results, the gallium observations will bring intellectual satisfaction to Americans, scientists, and nonscientists alike. Our generation may solve an ancient problem, where does the light from the Sun come from, and open up new vistas on how the universe operates.

Well, Mr. Chairman, members of the committee, forgive me for being so specific in my recommendations. Invite me back after you have solved "The Case of the Missing Solar Neutrinos," and we can discuss generalities to our heart's content.

Thank you.

[The prepared statement of Dr. Fowler follows:]

STATEMENT

WILLIAM A. FOWLER

NOBEL LAUREATE IN PHYSICS 1983

INSTITUTE PROFESSOR OF PHYSICS EMERITUS

CALIFORNIA INSTITUTE OF TECHNOLOGY

BEFORE THE

COMMITTEE ON SCIENCE AND TECHNOLOGY

U. S. HOUSE OF REPRESENTATIVES

MARCH 8, 1984

Mr. Chairman and Members of the Committee,

I welcome this opportunity to respond to the invitation to present my views and recommendations with respect to the Government's role in support of scientific research and education.

Slightly over two years ago on March 4, 1982 I testified before your Subcommittee on Science, Research and Technology on the then critical situation concerning instrumentation in university and college laboratories. It is gratifying to know that substantial progress has been made in alleviating this situation in the last two years. NSF, DOE and DOD have in place programs dedicated in one way or another to funding the purchase of scientific equipment especially in universities with graduate departments. There is still some concern in my mind regarding the situation in undergraduate colleges which give bachelor's degrees based on studies based in the sciences. It is my contention that undergraduate colleges should be provided with research grants and the appropriate instrumentation if they offer degrees in the sciences. Aspiring young scientists, whether headed for teaching or research, should be given hands-on training in real research not just laboratory exercises. One cannot learn modern physics playing with inclined planes and pulleys. Even those destined for a career in theory ought to have the option of doing a hands-on experiment of some real relevance. All of this will require college science faculties with a few members who are interested in research as well as teaching. There will be a bonus: better people will be interested in college positions. There will be another bonus: students in other fields will learn that scientists do not live cloistered lives in ivory towers. Experimental scientists have to work hard in the laboratory in ways more similar to that of the workman than to that of the office worker. These students will communicate this to their friends and co-workers and help to change the image of the scientist in the public mind.

In my testimony two years ago I said, "You are not going to hear from me a litany of what science can do for the general welfare, for the economy and for the national defense." Let me assure you that I do not deprecate the applications of science to engineering and engineering to technology. I do hope that these applications will be benign. My field of research is the application of nuclear physics to astronomy. It is a benign application in comparison to reactors and bombs. The whole matter has been a paradox for me all of my professional life. I don't know the answer to the paradox but of one thing I am certain. The science of nuclear physics which explains the origin of sunlight must never be used to raise a dust cloud which will obscure that sunlight.

Be that as it may, my primary concern is that science be supported by the Government in part because of its contributions to the intellectual needs of the American people and all the people of the world for that matter. There is no more profound biblical quotation than that of

Matthew "Man shall not live by bread alone." Men and women have intellectual needs as well as cultural and physical needs. Those intellectual needs can only be satisfied by the continuing effort to obtain new knowledge about the universe we inhabit and about the physical laws which govern it.

Permit me to discuss briefly an example which is very close to my mind and heart. In doing so I may overstep the bounds of propriety regarding self-interest in that I am suggesting the support of a search for knowledge in which I am greatly interested. The Committee will have to be the judge. My conscience is clear. The search will involve no monetary gains for me nor for the institution with which I am affiliated.

I begin at the beginning. Since earliest times mankind has been interested in the sun. When it became clear that all life on earth depends on light from the sun the purely intellectual question arose—what is the ultimate source of energy for the rays of light from the sun?

The quantitative study of chemical reactions in the eighteenth and nineteenth centuries made it clear that chemical processes—burning in simple language—could provide solar energy for at most about ten thousand years. In 1854, Helmholtz and in 1861, Lord Kelvin suggested gravitational energy released in a gradual decrease of the sun's radius. Their own calculations showed that this would suffice to maintain the sun's present luminosity for at most a few million years. At the same time geologists were showing that geological processes on the earth required at least 100 million years. Early in this century Lord Rutherford used radioactivity to measure the age of terrestrial rocks and found values exceeding one billion years. Much later, his methods applied to the meteorites and to the moon yielded ages around 4.5 billion years. In spite of tectonic activity on the earth some terrestrial rocks have been closed, undisturbed systems for close to 4 billion years. Sophisticated arguments indicate that the sun and the other components of the solar system formed at the same time, 4.5 billion years ago.

What has provided the energy of the sun over that prodigious length of time? To make a long story short it is now believed that the source of energy is the fusion of hydrogen into helium at the center of the sun. Through the work of Bethe, Critchfield, Lauritsen, Cameron and myself we think we know the detailed nuclear reactions taking place in the fusion of hydrogen into helium.

We know too that fusion can power the sun for about seven billion years more before it changes its present characteristics in any marked way. But to get to the point there is a problem. I refer to it as "The Case of the Missing Solar Neutrinos."

The Case is this. In the fusion of four hydrogen nuclei into one helium nucleus two elementary particles we call neutrinos are released. These neutrinos have no electrical charge, no magnetic moment, probably no mass and they interact only weakly with matter. Only one in 100 million neutrinos produced at the center of the sun is intercepted on the way to the surface. At night they penetrate the earth and reach us willy nilly. A simple calculation yields the fact that 60 billion neutrinos penetrate every square centimeter of the earth and our bodies per second. Just as they pass through the sun and the earth they pass through our bodies without interacting, without producing any damage.

That is what physicists and astronomers believe. But we have no positive proof. In fact, fundamental problems have been raised by attempts to detect the rather rare, high energy solar neutrinos made by

Dr. Raymond Davis of the Brookhaven National Laboratory in the Homestake Gold Mine near Lead, South Dakota. Davis has installed a 100,000 gallon tank of liquid perchloroethylene (cleaning fluid) one mile deep in the mine. The chlorine in the perchloroethylene is the detector. This detector can only detect the higher energy solar neutrinos and not the much more numerous low energy solar neutrinos.

In 60 observations over 13 years Davis has found one-third of the flux of these high energy solar neutrinos relative to what we expect. Something is wrong! Many explanations have been suggested. Most raise more problems than they solve. The two most reasonable are:

First, there is the possibility that the solar neutrinos transform in part to the two other known forms of neutrinos on their way from the sun to the earth. These two other forms of neutrinos do not trigger the chlorine detector. Sophisticated experiments here and abroad are underway to prove or disprove this explanation involving neutrino transformations.

Second, our understanding of the sun and thus our models of solar structure are wrong. The flux of the higher energy solar neutrinos depends critically on the details of our solar models. We know that the flux of the lower energy solar neutrinos is practically independent of the models of solar structure. On very general grounds we know what to expect.

Thus it is generally agreed that attempts to determine the flux of the lower energy neutrinos must be made. If their flux is also down to one-third then neutrino transformations are the likely explanation since high and low energy neutrinos are affected in the same way by these transformations. If their flux is what we expect on general grounds then the deficit in high energy neutrinos will indicate that our solar models are wrong, or in other words, we do not understand the sun. There is the horrifying possibility that we are completely wrong and hydrogen-to-helium fusion does not power the sun. That will set us back on square one!

It is generally agreed that the best detector for these lower energy neutrinos is the rare element gallium. There's the rub! Between 30 and 60 tons of galliu will be needed and gallium costs one-half million dollars per ton. There .. one source of relief. The Federal Republic of Germany will provide one-quarter of the gallium needed. Even so the cost of the gallium and operation of the observations for five years will run to a total cost of at least 20 million dollars. Who is to pay?

Before answering that question I wish to enter into the record the support of the scientific community for the gallium observations:

1. Astronomy and Astrophysics for the 1980's (Field Report). Vol. 1, p. 115: A pilot experiment employing 1.3 tons of gallium was successfully completed during the summer of 1980. The U. S. Department of Energy and the Max Planck Institutes in Germany are now supporting the development of a larger detector that will be calibrated with a laboratory source of neutrons produced in a reactor. The Committee urges continued financial support of the gallium-detector experiment by this international collaboration and the completion and operation of the full gallium neutrino detector at the earliest opportunity.

2. A Long Range Plan for Nuclear Science. NSAC Report to NSF/DOE, 1983. p. 40: The solar neutrino problem raises a number of important scientific questions, and has to be pursued. At the present time scientific grounds dictate the choice for the next neutrino detector

to be ^{71}Ga, on the basis of its demonstrated feasibility and recovery efficiency, but options for alternate detector schemes which could help resolve the present problem should be explored further.

3. Review of the Gallium Solar Neutrino Project at Brookhaven National Laboratory, 1981 (Report by G. T. Seaborg, Chairman, F. Boehm, G. E. Brown, G. B. Field, W. A. Fowler, T. J. Goldman, M. Schwarzschild, and A. Turkevich)

pp. 8,9: The members of our Panel have come to the conclusion that the Gallium Solar Neutrino Project has outstanding scientific merit on a broad interdisciplinary scale. We have made an analysis with the aim of reducing the cost to the minimum value commensurate with obtaining a definite and meaningful result and have come to the conclusion that a satisfactory measurement can be made with 45 tons of gallium (i.e., a reduction from the proposed 50 tons). We strongly recommend that the Project be approved and authorized to proceed.

There is little to add and I return to "Who is to pay?." The American people have paid billions of dollars to learn about the moon and Jupiter and other planets. I submit that the sun is far more important to their daily lives than the moon and the planets. I have given my lecture on "The Case of the Missing Solar Neutrinos" over one hundred times. Always there is intense interest in the source of energy for the life-sustaining light from the sun. It is my deep feeling that 20 million dollars of Government money would be well spent to solve a deep intellectual problem which can be appreciated by everyone.

What are the practicalities? The Department of Energy has generously supported the Brookhaven program in solar neutrino research. Brookhaven should carry on. The Congress and the Administration should provide the Department of Energy with the additional funds--and I stress additional-necessary to enable Brookhaven to do so. Whatever the results, the gallium observations will bring intellectual satisfaction to Americans, scientists and non-scientists alike. Our generation may solve an ancient problem and open up new vistas on how the universe operates.

Mr. Chairman and Members of the Committee, forgive me for being so specific in my recommendations. Invite me back after you have taken action on "The Case of the Missing Solar Neutrinos" and we can discuss generalities to our hearts' content.

Thank you.

Mr. FUQUA. Thank you very much, Dr. Fowler.
Dr. McClintock, did you wish to make any comments?
[The biographical sketch of Dr. McClintock follows:]

DR. BARBARA MCCLINTOCK

Dr. Barbara McClintock received the 1983 Nobel Prize in Medicine for her discovery in the 1940s of "mobile genetic elements." Today, these elements are understood to be segments of DNA that have the ability to move around the genome and control many different genes. They have also been referred to as "controlling elements", "jumping genes" and "transposable elements." Dr. McClintock's work was far ahead of its time, achieved when the "demonstration that chromosomes carried the hereditary factors was only a decade old; the concept of the gene was still just that, a concept and a contentious one at that; and the discovery of DNA as the chemical basis of genes, the helical structure of DNA, and the genetic code were all way into the future." (Science, Oct. 28, 1983, p. 402.)

Dr. McClintock first presented her ideas to the scientific community in 1951 at a symposium in Cold Spring Harbor, New York. Her presentation was based on six years of work studying the variegation of kernel pigmentation in corn. The inheritance of coloration in corn kernels was known to be genetically instable, that is, the patterns of corn kernel pigmentation could not be reliably predicted using the principles of genetics known at the time. Dr. McClintock concluded that movable controlling elements were responsible for the observed color patterns. The essence of Dr. McClintock's insight was that there were two types of genes: structural genes, that specified the manufacture of enzymes and other proteins; and controlling elements, which regulated the activity of the structural genes.

Since Dr. McClintock's ideas were complex, and contradicted the established doctrine of that time, they did not make an impact on the field of genetics until several other scientific discoveries were made. Ten years later, in 1961, nonmobile gene regulating sequences were discovered in bacteria. In the mid-1970s mobile genes (transposable elements or transposons), which can carry drug resistance, were found in bacteria. By the 1980s, many kinds of mobile elements in numerous organisms, including humans, were discovered. Recently scientists at several research centers, including the Cold Spring Harbor Laboratory and at the Carnegie Institute of Washington, have been able to confirm at the molecular level much of what Dr. McClintock had intuitively deduced.

Dr. McClintock's discovery of movable genetic elements has had an impact on a number of different facets of genetics: bacterial drug resistance; antibody production in vertebrates; parasites' ability to evade their host's immune system; and the evolution of retroviruses (which are thought to cause cancer). Perhaps the most direct application of Dr. McClintock's discovery, however, is in genetic engineering as a potential method for introducing desirable genes into cereal crops, such as corn. Work on this topic is currently under way in Australia, Germany and the United States.

PRESS RELEASE, OCTOBER 10, 1983

The Nobel Assembly of the Karolinska Institute has today decided to award the Nobel Prize in Physiology or Medicine for 1983 to

Barbara McClintock

for her discovery of "Mobile Genetic Elements"

Summary

Barbara McClintock discovered mobile genetic elements in plants more than 30 years ago. The discovery was made at a time when the geneti code and the structure of the DNA double helix were not yet known. It is only during the last ten years that the biological and medical significance of mobile genetic elements has become apparent. This type of element has now been found in microorganisms, insects, animals and man, and has been demonstrated to have important functions.

Genetic instability was originally discovered in maize (Zea mays) in which it was found to cause altered patterns of pigmentation of the kernels. Instead of being evenly pigmented, the kernels have sectors of more intense pigmentation. The spots vary in size and colour. At the sam time, the cells show chromosome breaks and other abnormalities. McClintock examined the relationship between the pigmentation pattern of the kernels and chromosome changes. Variegation in the colour of the kernels was found to be parallelled by transposition of structural elements within or between chromosomes. Because transpositions result in inactivation of neighbouring genes, McClintock used the term "controllin elements" to describe the mobile chromosome structures. Another effect o transposition was chromosome breaks at points where the mobile elements were integrated.

During the mid-1960s mobile genetic elements were demonstrated in bacteria and shown to play a role in the transmission of resistance to antibiotics from one bacterium to another.

Such elements were also found to have an important function in th ability of unicellular parasites (trypanosomes) to change their surface properties, thereby avoiding the immune response of the host organism. Recombination of DNA segments proved to be an essential factor in the ability of lymphoid cells to produce a seemingly infinite number of different antibodies to foreign substances. In recent years, evidence ha accumulated that transposition of genes or incomplete genes are involved in the transformation of normal cells into tumour cells. Thus, genes controlling cell growth have been found to undergo translocation from on chromosome to another during cancerogenesis. The initial discovery of mobile genetic elements by Barbara McClintock is of great medical and biological significance. It has also resulted in new perspectives on how genes are formed and how they change during evolution.

oo0oo

When McClintock began the work that led to the discovery of mobile genetic elements, genetic instability had been demonstrated in plants and insects (Drosophila). In maize, the instability caused the kernels to show differently coloured patches. This variegation was believed to reflect a greater fragility of certain chromosome regions, causing genes for pigmentation to mutate more easily than other genes. As daughter cells multiplied and inherited the mutant genes, colonies of cells with an altered pattern of pigmentation were formed.

McClintock first examined the structure of chromosomes in maize plants showing variegation in pigmentation. By combining results from these studies with those from genetic crosses she was able to localize genes for e.g. type of starch, storage protein, anthocyanin pigments on the individual chromosomes. Of the ten pairs of chromosomes pair number 9 turned out to be of particular interest.

The choice of maize presented several experimental advantages. Each ear (Fig. 1) has several hundred kernels, each of which is the result of an independent fertilization event. The inheritance of a series of characteristics can easily be studied simply by examining the structure, starch content or pigmentation of the individual kernels. Mutations affecting pigmentation are particularly useful, not only because they can be easily observed, but also because they do not harm the multiplication of the cells. Therefore, if a single cell undergoes a mutation or other form of heritable change during the development of the kernels, this will result in altered pigmentation of several successive generations of daughter cells. The number and size of the differently coloured spots, therefore, provides important information on the extent of genetic instability and the point during development at which the genetic change took place.

Another advantage of maize as an experimental system was that individual chromosomes are easily studied. During the 1930s McClintock made an important contribution to plant genetics by describing the detailed morphology of normal and altered maize chromosomes. This work was a necessary condition for the discovery of mobile genetic elements.

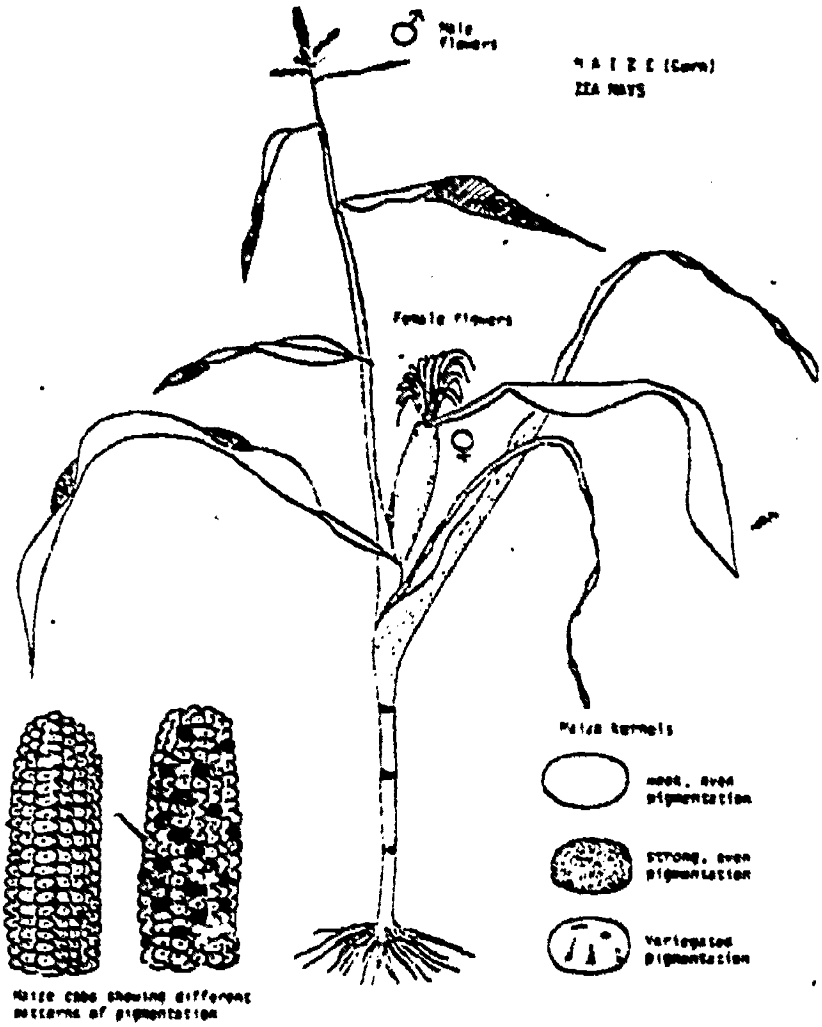

Figure 1. Schematic summary of the maize plant, its maize cobs and maize kernels.

The first mobile element characterized by McClintock was found on chromosome number 9, where it caused chromosome breaks (Fig. 2). Since the chromosome was divided into two parts, this element was named "dissociation" or Ds. As it was transposed along chromosome 9 it caused breaks and inactivitation of neighbouring genes. McClintock, therefore, referred to the mobile elements as "control elements". In order for Ds to be transposed, a second genetic element called "activator" (Ac) had to be present. Together Ds and Ac represented a two element system controlling gene activity. McClintock also identified different forms of Ds, some causing complete gene inactiviation while others resulted in different degrees of partial gene inactivation. The role of the Ac element was shown to be a coordinating one. By signalling to Ds elements, Ac triggered the transposition of one or several such elements. Also the Ac element occurred in different forms. Some of these produced signals early during the development of the kernel, while others induced transpositions late in development. The type of Ac element could be detected by examining the size of differently pigmented spots on the surface of the kernels.

In later work, McClintock demonstrated regions of genetic instability on other maize chromosomes. Also in these cases the phenomena observed turned out to be due to genetic elements moving from one chromosome to another.

The most important features of the control elements discovered by McClintock are the following:

The control elements behave as ordinary genes in genetic crosses, and can be localized to specific chromosome regions. When they transpose along, or between, chromosomes, they cause inactivation of neighbouring genes. In some cases, they also result in structural instability at the sites of integration, causing chromosomes to break easily at these sites. When control elements leave a certain region, the previousloy inactived genes resume normal functions.

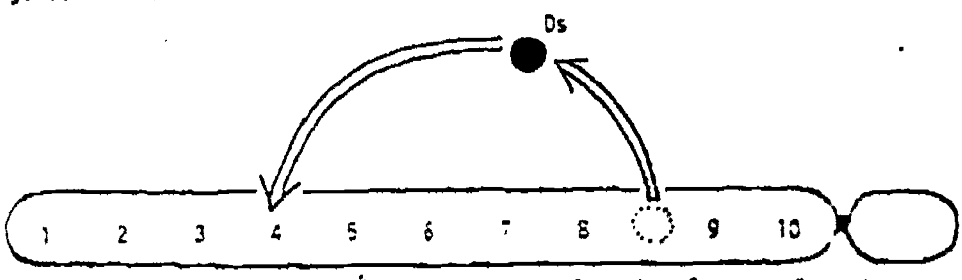

Figure 2. When the control element Ds jumps from its "resting" position between genes number 8 and 9 to a position close to gene number 4, the latter is switched off. If Ds later moves to another position, gene 4 will resume its function and the corresponding protein will again be synthesized.

The first mobile element characterized by McClintock was found on chromosome number 9, where it caused chromosome breaks (Fig. 2). Since the chromosome was divided into two parts, this element was named "dissociation" or Ds. As it was transposed along chromosome 9 it caused breaks and inactivitation of neighbouring genes. McClintock, therefore, referred to the mobile elements as "control elements". In order for Ds to be transposed, a second genetit element called "activator" (Ac) had to be present. Together Ds and Ac represented a two element system controlling gene activity. McClintock also identified different form of Ds, some causing complete gene inactiviation while others resulted in different degrees of partial gene inactivation. The role of the Ac element was shown to be a coordinating one. By signalling to Ds elements, Ac triggered the transposition of one or several such elements. Also the Ac element occurred in different forms. Some of these produced signals early during the development of the kernel, while others induced transpositions late in development. The type of Ac element could be detected by examining the size of differently pigmented spots on the surface of the kernels.

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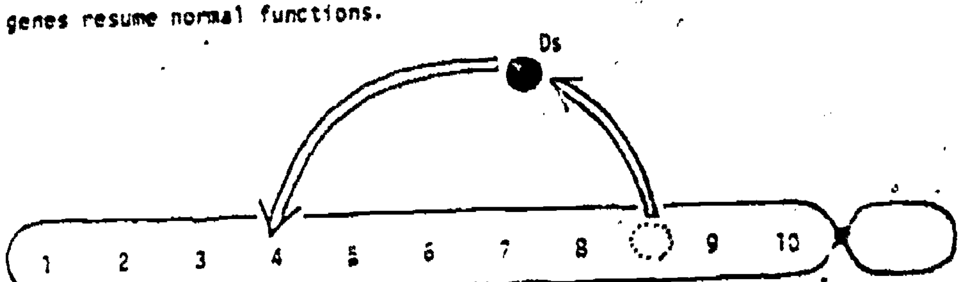

Figure 2. When the control element Ds jumps from its "resting" position between genes number 8 and 9 to a position close to gene number 4, the latter is switched off. If Ds later moves to another position, gene 4 will resume its function and the corresponding

Control elements can be classified into groups. Within a certain group, one element acts as a superior element (regulator) signalling to subordinate elements (receptors) when to transpose. By doing so, the superior element controls the exact time during development when transpositions are to occur.

Control elements can assume different states. They can be part of regulatory systems consisting of two or more elements. They can also appear as independent or autonomous elements. Some elements act by programming neighbouring genes to become active at a later time, which may be several cell generations later.

McClintock's experiments were carried out with great ingenuity and intellectual stringency. They reveal a whole world of previously unknown genetic phenomena. In spite of this, they failed to attract the attention of contemporary scientists. This might have been due to the fact that her results were reported in not so widely read publications such as the annual report of the institute where she worked and in special newsletters exchanged by plant breeders working with maize. A contributing factor was that she was far ahead of the development in other fields of genetics. Her most important results were published before the structure of the DNA double helix and the genetic code had been discovered. Furthermore, useful as they were from an experimental point of view, the pigmentation patterns of maize kernels was of little practical significance.

In recent years, mobile genetic elements have been demonstrated in a number of species. This has given new insights into the mechanisms involved in the evolution of genes and has resulted in a much more dynamic picture of the organization and function of genes. In bacteria, short DNA segments known as "insertion sequences" or IS elements have been found to move from the bacterial chromosome to smaller DNA molecules known as plasmids or from one plasmid to another. The effect of their transposition is inactivation of genes. Genes surrounded on both sides by IS elements become mobile (Fig. 3). This type of gene is known as "transposon," and is of great importance in clinical medicine. Often, these structures carry genes for resistance to antibiotics. The spread of such resistance genes from resistant to sensitive bacteria is a major problem in the treatment of infectious diseases.

Mobile genetic elements have also been found in bacteriophages, i.e., viruses that infect bacteria. In trypanosomes, a type of parasite that causes African sleeping sickness, mobile genetic elements cause

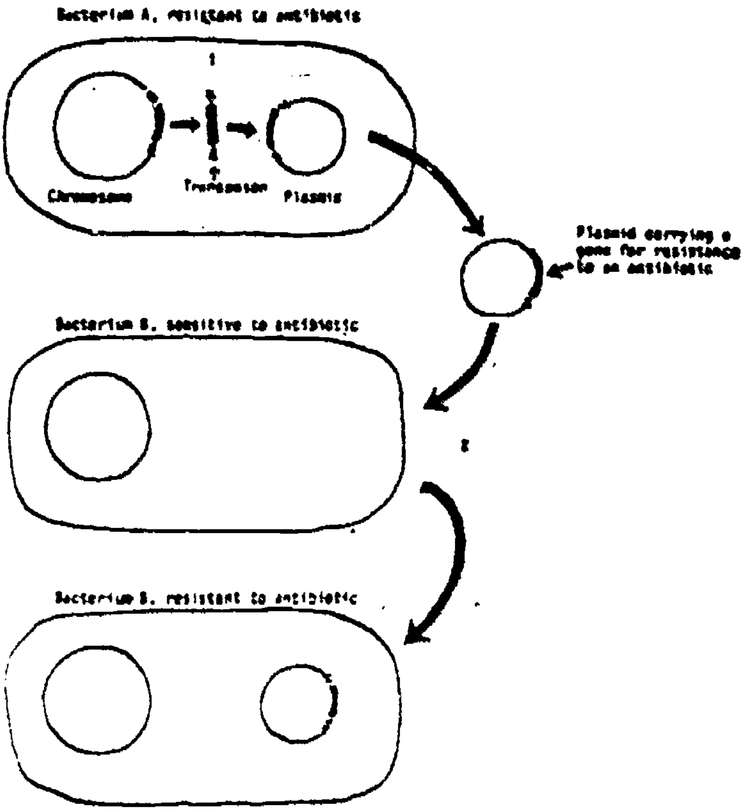

Figure 3. Schematic summary of how a gene for resistance to an antibiotic can jump from a bacterial chromosome to a plasmid using a transposon as a vector (step 1). The plasmid (R-factor) may then be taken up by a sensitive bacterium (step 2) which becomes resistant to the antibiotic. In this way resistance to an antibiotic may spread from one bacterium to another making treatment difficult.

changes in the surface molecules of the parasite, making it possible for the parasite to evade the immune response of the host organism.

In insects (Drosophila), several mobile genetic elements have been identified and shown to be closely related to genes found in RNA tumour viruses. One such element, known as "copia", can occur in nuclear DNA as a mobile gene. It can also be copied into RNA and become part of an RNA virus. The RNA form can again be copied back into DNA when a new cell is infected. The DNA copy then becomes a mobile gene in the nucleus of the infected cell.

The correlation between mobile genetic elements and RNA viruses (retroviruses) is of interest also in relation to animal and human cells. Some genes that cause normal cells to become tumour cells (oncogenes) can occur both as viral genes (v-onc) and as cellular genes (c-onc). In some cases the abnormal growth pattern of tumor cells has been linked to transposition of c-onc genes or to integration of mobile genetic elements close to the c-onc genes.

The discovery of mobile genetic elements by McClintock is of profound importance for our understanding of the organization and function of genes. She carried out this research alone and at a time when her contemporaries were not yet able to realize the generality and significance of her findings. In this respect, there are several similarities between her situation and that of another great geneticist active 100 years ago, Gregor Mendel, who, studying the garden pea, discovered other basic principles of genetics.

STATEMENT OF DR. BARBARA McCLINTOCK, 1983 LAUREATE IN MEDICINE AND PHYSIOLOGY, RESIDENT SCIENTIST, COLD SPRINGS HARBOR LABORATORY, LONG ISLAND, NY

Dr. McClintock. When I received the invitation to be here today, I recognized right away that I was not qualified, that my background and experiences would not lead me to make any recommendations that were based on information or judgments that I thought could be satisfied by my information.

I think I better tell you why I felt that way. In the first place, I never had a grant of any kind. I have only once served on a panel, and that was in the 1944 granting agency. I was such a maverick that they never invited me back. [Laughter.]

I have not had graduate students that were under my direction since the mid-1930's. I had 5 years of being at the university and I had very nice graduate students, really good ones, which I enjoyed very much. But I left there.

I did not have graduate students at the institution I am in now that were part of the institution, but I had graduate students and I was imported for them at Cal Tech, at Columbia, at Yale, and so forth—especially in North Carolina, I had four down there all at the same time. I had to commute from New York down there but it was not my responsibility to provide any funds or see to it on that score.

I have done a great deal of consultations with people, young people in research, which I have enjoyed immensely and I hope was effective in some way or another.

Under these conditions of not having anything that I can put my fingers on, if it would be useful for you, I can only say that in my field, which is biology, and in my associations, which are with molecular biology at my institution, I am quite disturbed.

I am disturbed because the funds have not been generally distributed among biology. The people must be supported now in molecular biology by grant funds. They spend much of their time writing grants. They have to work very hard, they are very nervous about them. When they are rejected, it is a catastrophe and you see it for them.

I do not know what we can do about this, actually but it is true that it is a difficult situation, there is not enough money to go around.

I think much of it started because we probably have centered our funds in certain areas. Maybe that is why a group of people felt they had to go into that area. If I go to a symposium at our place, there may be ten people talking about the same subject, doing the same experiments. There is something wrong. It is something that cannot be corrected, I believe, immediately. That has to come through some restructuring of how we think about biology and how it should be spotted.

I am very disturbed that botany—I know plants reasonably well—I am very disturbed that botany just went out of the picture in the last 20, 30 years. And yet, plants are marvelous. I think many of us do not realize that much of our pharmaceuticals have come from plants. And yet, we are letting them get out of hand.

I remember when I was young I was at Cornell at a very good botany department—it is no longer there. So, I feel that is very important. I see it as very important that we should train naturalists. Many people in biology do not understand the diversity of organisms—there are a few, of course. But I think we need to have more naturalists, more people that are trained in that direction but they cannot be supported.

So, I think we need to have a restructuring of our thoughts on what should be supported in biology, but I do not know how to give it to you.

I think that gives you the idea of why I felt incompetent to write anything specific for today.

Mr. FUQUA. Thank you very much, Dr. McClintock. I think you are very well qualified to advise this committee because the things that you are talking about. We will get into this later in some questions that we think are very important.

Next, we will have Dr. Taube. I hope I am pronouncing your name correctly or some semblance of that.

Dr. TAUBE. I recognize whom you are addressing. [Laughter.]

Mr. FUQUA. Thank you, sir, we are pleased to have you.

[The biographical sketch of Dr. Taube follows:]

HENRY TAUBE

Born Neudorf, Saskatchewan, Canada, November 30, 1930
naturalized U.S. citizen

Scientific Field: Inorganic Chemistry

Education: University of Saskatchewan, B.S. 1935
M.S. 1937
University of California, Ph.D. 1940(chemistry

Professional Experience:	Instructor of Chemistry University of California, 1940-41
	Assistant Professor Cornell University, 1941-46
	Research Associate National Defense Research Committee, 1944-45
	Professor of Chemistry University of Chicago, 1946-61
	Professor of Chemistry Stanford University, 1961-present
Concurrent Positions:	Guggenheim Fellow: 1949 and 1955
Honors and Awards:	Award, American Chemical Society, 1955 Howe Award, 1960 Distinguished Service Award, 1967 Nichols Medal, 1971 Willard Gibbs Medal 1971 Chandler Award, Columbia University, 1964 Kirkwood Award, Yale University, 1966 National Medal of Science, 1977

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CHEMISTRY PRIZE AWARDED TO ONE OF THE MOST CREATIVE CONTEMPORARY WORKERS IN INORGANIC CHEMISTRY

The Royal Swedish Academy of Sciences has decided to award the 1983 Nobel Prize in Chemistry to Professor Henry Taube, Stanford University, Stanford, USA, for his work on the mechanisms of electron transfer reactions, especially in metal complexes.

Chemical reactions were known to man long before chemistry had attained the status of science. It was observed that substances changed their properties under certain external conditions, which is a characteristic of chemical reactions. Thus the ancient Egyptians found that if malachite, a green ore, was fired with charcoal, a red metal was obtained, called copper. It was also found that when clay was baked, ceramic products with properties quite different from clay were obtained.

Much earlier than this, man had found that a piece of dry wood caught fire if it could be made hot enough; changes in the properties of substances occurred only on certain conditions. Temperature was early the factor which was varied in order to bring about changes, and it was also found at an early stage that the speed with which the changes occurred frequently depended on the temperature. With the discovery of black powder it was also noted that processes could take place very rapidly, leading to explosions. The branch of chemistry concerned with how fast chemical reactions take place is known as chemical kinetics, and the scientist

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ACADEMY OF SCIENCES

19 October 1983

engaged in explaining how is said to study the mechanism of chemical reactions.

Millennia of hypotheses, experiments and observations, new hypotheses and new experiments and observations were to pass before a fairly firm scientific structure had been created. At the beginning of this century, progress had been considerable. In particular, a physical-mathematical description of the reactions had been produced, and it was possible in figures and formulas to express the conditions determining whether a chemical reaction would occur, and it was possible to provide mathematical equations for how rapidly it took place. A beginning had also been made in the treatment of reactions which did not pass completely in one direction, as opposed to those mentioned above. It was realized that chemical equilibria existed, and it was possible to deal with these theoretically. It is a characteristic of chemical equilibria that the reacting ions or molecules, although on average bound to another a given bond is not permanent and that the bonds are always being broken down and restored. Three major types of equilibrium reactions have come to be of dominant importance in chemistry. The concepts of acid and base were combined in the acid/base reactions and the pH associated with this.

Metal ions dissolved in water may attract ions or molecules. This is known as complex formation and usually, although not always, occurs as an equilibrium reaction. Finally the combustion of the burning piece of wood and the production of metallic copper from its ore through a reaction with charcoal have been generalized as oxidation and reduction. As a further generalization it has been found that oxidation and reduction are associated with a transfer of electrons, e.g. in metal ions such as cobalt and chromium. Under certain conditions it is possible to make cobalt with three positive charges react with chromium having two positive charges, where cobalt gets only two but chromium three positive charges. The effect is thus that an electron having a negative charge has been tranferred from the two-valent chromium to the three-valent cobalt. This is particularly frequent phenomenon in complex compounds of metal ions. Taube has today been awarded the 1983 Nobel Prize for his studies of the mechanisms of electron transfer in metal complexes. Better than anyone else he has helped us understand how these electron transfers take place. It is particularly the structural preconditions governing electron transfers in metal complexes which he has studied. The electron transfer process as such is a separate major problem in theoretical chemistry and physics, where other scientists have contributed more than Taube.

What are the experiments made by Henry Taube and what conclusions has he been able to draw? In his studies, he started from the fact that three-valent ions of cobalt and chromium do not form equilibrium complexes (an example of the exceptions already referred to). The ions or molecules which are bound

to these metal ions are therefore joined to them without ever leaving them. But the corresponding two-valent ions form equilibrium complexes. If an ion or molecule bound to the three-valent ion (in this instance, three-valent cobalt) could somehow be marked so that it is possible to find experimentally whether this marked ion or molecule in the electron transfer has at the same time been transferred to the other metal ion (in this instance, two-valent chromium), that is, in the opposite direction as the electron in this case. This was exactly what Taube found, and from this he drew the conclusion that before the electron transfer could
fig. take place, a bridge was formed between the metal ions of the ion or molecule which changed places. He proved this in a large number of case and investigated how the electron transfer was affected by changes in the bridging molecule.

His next step was lengthen the bridge between the metal ions (while using molecules which could bind two metal ions) and he found that in some instances there was still an electron transfer in spite of the greater distance between the metal ions. There was thus a form of what Tabue calls "distant attack".

A logical continuation was the bonding of three-valent ions to the two ends of the bridge before reducing this complex with a two-valent ion (in this instance, europium). This reacted rapidly with one of the metal ions and Taube could then follow the slow transfer within the complex (in this case from ruthenium to cobalt) free from all assumptions on how rapidly the bridge was formed.

Finally Taube let the three-valent metal ions on either side of the bridge be identical and could then study if in reduction with an electron this was captured by one of the identical metal ions or it belonged to both, a phenomenon known as delocalization. (Delocalization generally gives rise to strong colours, such as in Prussian blue.)

This entire development was dominated both experimentally and theoretically by Taube, who according to one of the nominations has in eighteen listed instances been first with major discoveries in the entire field of chemistry. The examples selected here, which are all included in the prize award, may seem rather specialized, not to say esoteric. However, during the last ten years it has become increasingly apparent that Taube's ideas have a considerable applicability, particularly in biochemistry. All respiration which is associated with oxygen consumption is thus also associated with electron transfers, and a growing number of scientists in this field are basing their work on Taube's concepts of electron transfers in metal complexes.

It should be added that, as already pointed out, Taube has made major contributions throughout the chemistry of complexes. Thus he was the first to produce a complex beteen a three-

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valent metal ion, which was based on the ideas developed by Taube in his electron transfer studies.

Finally a quotation from one of Nobel Committee's reports on Taube: " There is no doubt that Henry Taube is one of the most creative reseach workers of our age in the field of coordination chemistry throughout its extent. He has for thirty years been at the leading edge of research in several fields and has had a decicive influence on developments."

<u>Henry Taube</u>

Born in Saskatoon, Canada, on 30 November, 1915
Became citizen of USA in 1942
Ph.D., University of California, 1940
Professor in Chemistry, Stanford University, 1962

<u>Address</u>

Professor Henry Taube
Department of Chemistry
Stanford University
Stanford, CA 94305
USA

tel.: (415) 497-2300

STATEMENT OF DR. HENRY TAUBE, 1983 LAUREATE IN CHEMISTRY, PROFESSOR OF CHEMISTRY, DEPARTMENT OF CHEMISTRY, STANFORD UNIVERSITY, STANFORD, CA

Dr. TAUBE. Mr. Chairman, committee members, thank you for the opportunity to testify before this committee.

I shall begin with some general comments on how the practices followed by the Federal Government in allocating research support affects the immediate users, and then turn to some specific items which trace the effects of current practice on academic institutions.

I have been doing independent research since 1940, in fact since I began my research toward the Ph.D. degree at the University of California in 1937. My first federally supported grant was made by the ONR in 1949, and the first advanced research instrument I acquired was a mass spectrometer, awarded by the AEC, cost about $12,000.

After a time, these agencies lost interest in my work but support was then provided by the National Science Foundation and later and concomitantly, by the NIH. Without Federal support, my work would not have prospered. Such support has been the essential ingredient in the enormous progress made in all fields of chemistry in the last three to four decades.

Is it all peaches and cream? Almost, but not quite. The proposal format exacts a price at the level of the principal investigator from all but the most confident, and also at the level of those in training, and this cost is not inconsiderable in the way it affects the quality of the work.

Preparation for the Ph.D. provides the first exposure to independent research, and it is critical in the development of a research chemist. It is essential at this stage when conditions should be optimum for the growth of curiosity and interest, that the candidate be given the utmost freedom to choose directions. The graduate students supported by a research assistantship must, however, report to a mentor who has a responsibility not to stray too far from the spirit of the research proposal that won the support. The mentor is subject to a second constraint: In a climate in which each graduate student expects support, the principal investigator feels an obligation to maintain continuity for each member of the research group.

As a result, there is a tendency on the part of the PI in a large part of his effort to opt for the safe and sound instead of what may be more imaginative but is at the same time riskier.

The constraints I have mentioned tend to stifle creativity, but their effect is to some extent ameliorated when there are plural sources of support—this is an important plus of our system—and also when grants are made to cover support for an extended period. They would be further ameliorated were a large fellowship program instituted with each fellowship carrying also a reasonable allottment to cover the expenses of research. By allocating funds to some student-centered rather than proposal-centered awards, a great deal of flexibility would be introduced both for the student and for the research mentor.

It is pertinent in this connection to draw attention to the system followed in the United Kingdom. There each graduate student derives from public funds a stipend which, in most cases, covers the cost of living. The PI still has the problem of raising the funds needed to cover the expenses of research, but even with this requirement, I think that more freedom of choice exists than in our system. It is, of course, proper to ask whether the system followed in the United Kingdom works.

If we use as criterion the number of Nobel laureates in chemistry named since 1950—a good starting date for comparison because it is at about the time that the effects of public support of research began to be felt—we would have to conclude that it works very well indeed. For the United Kingdom the count is 16, for the USA, 19. When allowance is made for the differences in population, which should reasonably well reflect the differences in the number of investigators, the United Kingdom wins handily. It is, of course, not proven that that system of allocating research support is responsible for the high quality of their effort, but it is likely a significant factor.

I turn now to the second theme: Federal support of science in relation to academic institutions. There is a present and active committee under the chairmanship of Dr. George C. Pimentel, organized by the Board on Sciences and Technology, which is surveying "intellectual frontiers in the chemical sciences and the opportunities they present for meeting critical societal needs." Please note ". . . for meeting critical societal needs." Chemistry is competent to do this.

I served as a member of an earlier similar committee under the chairmanship of Dr. Frank Westheimer which, in 1965, in a spirit of optimism, filed its report "Chemistry: Opportunities and Needs," and I want to comment on some of the concerns we then expressed and the changes which have taken place since then.

One of the strong recommendations made by the Westheimer Committee is that there be substantial allocation of funds for the purchase of major instruments by departments and also by individual users. Instrumentation is extremely important to us. I refer you to appendix A for illustrative matter, and progress has in fact been made in meeting needs. But reasonable needs, needs which I have felt in my own research, have still not been met, and underinstrumentation remains a critical issue and is recognized as such also by the Pimentel committee.

Closely related to this is the virtual disappearance of Federal funds for university science building, which were at a level of in excess of $120 million in 1965. In 1979, the level was at about $30 million, "targeted on major national facilities intended to serve large and diverse populations of researchers."

These data are taken from a report of the Association of American Universities published in 1981. According to this report, the costs incurred for science building by universities over the 4 earlier years amounted to $400 million, and the figure given for those needed for the next 3 years, even without allowance for expansion, were much larger—$765 million.

The problem is exacerbated by the need each institution faces to bring the existing buildings up to current health and safety standards. Contrary to what many believe, it is not easy for a university to raise money for new buildings. Funds for this purpose are given capriciously and there is no relationship between the quality of the institution and its capacity to raise building funds from private donors.

A concern of the Westheimer committee was the difficulty there is in providing startup funds for young investigators. They are the ones who have the most daring ideas, and it is essential to the health of the science that their research programs be established expeditiously. The startup costs for a young investigator in our department is on the average about $150,000, much of this cost being assignable to instruments and equipment. It does not cover other costs such as laboratory modification which can involve an even larger figure, nor summer salaries, nor graduate student stipends. The Presidential Young Investigator Awards Program represents a step in the right direction, but it is a small step compared to the needs.

In closing, I want to return to chemistry and societal needs. Whatever contributions I have made are at the level of basic science and have not been directed to a specific process or composition of matter. But these and like advances build the foundations of a science which, taken as a whole, manifests itself in practical applications which are ubiquitous, for the science concerns itself with all forms of matter, its transformations and its properties. Matter makes up materials, and George Pimentel in testifying a fortnight or so ago to a subcommittee of this committee, dilated on the control that the chemist can exercise over the properties of materials.

Among the materials he cited was his shirt. I start with his example and proceed further to remind you that chemistry speaks also to what lies under George's shirt: living matter and emotions. Chemistry is concerned also with animate matter, its function and dysfunction, and some of the most challenging problems are those of trying to understand the chemistry of living cells.

I and my fellow chemists appreciate the efforts of this committee to keep science in all its forms vigorous, exciting, and rewarding.

[The prepared statement of Dr. Taube follows:]

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I turn now to the second theme: federal support of science in relation to academic institutions. There is at present an active committee under the chairmanship of Dr. George C. Pimentel, organized by the Board on Sciences and Technology, which is surveying "intellectual frontiers in the chemical sciences and the opportunities they present for meeting critical societal needs." Please note--"for meeting critical societal needs." Chemistry is competent to do this. I served as a member of an earlier, similar committee, under the chairmanship of Dr. Frank H. Westheimer, which in 1965, in a spirit of optimism, filed its report "Chemistry: Opportunities and Needs," and I want to comment on some of the concerns we then expressed, and the changes which have taken place since then.

One of the strong recommendations made by the Westheimer Committee is that there be a substantial allocation of funds for the purchase of major instruments by departments and also by individual users. Instrumentation is extremely important to us (I refer you to Appendix A for illustrative matter), and progress has in fact been made in meeting needs. But reasonable needs, needs which I have felt in my own research, have still not been met, and under-instrumentation remains a critical issue and is recognized as such also by the Pimentel Committee.

Closely related to this is the virtual disappearance of federal funds for university science building, which were at a level of in excess of $120 million in 1965. In 1979, the level was at about 30 million dollars, "targeted on major national facilities intended to serve large and diverse populations of researchers." These data are taken from a report of the Association of American Universities published in 1981. According to this report, the costs incurred on this account by universities over the four earlier years amounted to $400 million, and the figure given for those needed for the next three years, even without allowance for expansion, were much larger--$765 million. The problem is exacerbated by the need each institution faces to bring the existing buildings up to current health and safety standards. Contrary to what many believe, it is not easy for a university to raise money for new buildings. Funds for this purpose are given capriciously, and there is no relationship between the quality of the institution and its capacity to raise building funds from private donors. (I have appended an account of the difficulties a university faces in raising gift funds for science buildings.)

A concern of the Westheimer Committee was the difficulty there is in providing start-up funds for young investigators. They are the ones who have the most daring ideas, and it is essential to the health of the science that their research programs be established expeditiously. The start-up costs for a young investigator in our department is on the average about $150,000, much of this cost being assignable to instruments and equipment. It does not cover other costs such as laboratory modification, which can involve an even larger figure, nor summer salaries, nor graduate student stipends. The Presidential Young Investigator Awards represent a step in the right direction, but it is a small step compared to the needs.

In closing, I want to return to chemistry and societal needs. Whatever contributions I have made are at the level of basic science and have not been directed to a specific process or composition of matter. But these and like advances build the foundations of a science which taken as a whole manifests itself in practical applications which are ubiquitous, for the science concerns itself with all forms of matter, its transformations and its properties. Matter makes up materials, and Dr. George C. Pimentel in testifying a fortnight or so ago to a subcommittee of this committee, dilated on the control that the chemist can exercise over the properties of materials. Among the materials he cited was his shirt. I start with his example and proceed further to remind you that chemistry speaks also to what lies under George's shirt: living matter, and emotions. Chemistry is concerned also with animate matter, its function and dysfunction, and some of the most challenging problems are those of trying to understand the chemistry of living cells.

I and my fellow chemists appreciate the efforts of this committee to keep science in all its forms vigorous, exciting, and rewarding.

Appendix A

The power of modern instrumentation is illustrated by the progress which has been made in the development of one of the most important tools needed by and available to the chemist: the determination of molecular structure by X-ray diffraction on crystals. In 1940, a full Ph.D. thesis would be built around the determination of a rather simple structure. Now, owing to the advances that have been made, small molecule structures (say less than 75 atoms) require only on the order of a week to collect data, and two weeks to solve and refine the structure.

Two technological advances led to a revolution in chemical crystallography. The advent of accessible modern digital computers to the community in the early sixties made feasible much more extensive calculations in a dramatically shorter time. Not only could electron density maps be computed, but non-linear least squares methods could be used to refine structures to a high degree of precision. Second, the development of computer controlled X-ray diffractometers (also in the sixties) provided the means to collect accurate diffraction data rapidly and routinely. These instruments eliminated much of the tedious manual labor involved in collecting diffraction data using film. Together these two advances coupled to make almost routine the solution of small molecule crystal structures (protein structures still require several years).

A typical contemporary crystallographic facility would consist of a computer controlled diffractometer ($125,000) and a small computer system for in house calculations ($30-75,000 depending on speed and size). Display of results is facilitated by a good high resolution plotter ($5-10,000) or a high resolution color graphics system to allow real time rotation and manipulation of the images ($30-70,000 depending on features). Various estimates have been made about the actual cost per structure--these clearly depend on such factors as labor cost, facility usage, etc. However, a good number for a typical inorganic structure is around $2500-3000.

(Excerpted from statement prepared by Prof. K. O. Hodgson, Department of Chemistry, Stanford University.)

Appendix B

The Seeley G. Mudd Chemistry Building at Stanford was dedicated in 1977. At the time the campaign to raise funds was started, and that was in 1967, the Stanford Department of Chemistry was already recognized as among the leading ones in the U.S. The two largest gifts were announced in 1973. Other gifts were accumulated before and after that date toward a final eventual cost of $6.5M, (which would be about $20M in today's dollars). The receipts fell short by about $1.3M. That sum was borrowed and is being repaid by University unrestricted funds.

"Providing adequate space for science is one of Stanford's most urgent and sizeable priorities. We spend several hundreds of thousands of dollars annually to renovate, upgrade, equip, and otherwise make safer and more useful the space used by faculty members in science, engineering, and technology areas. There are $2M worth of gift supported renovations under way or planned in the Chemistry Department presently; we budget from University funds between one and two million dollars annually for health and safety improvements, lab modifications, and deferred laboratory maintenance.

Funds for facilities are the hardest to raise. That is Stanford's experience, and it is the experience of our sister institutions as well. The highly technical nature of modern science buildings, their size, the high and rising cost of construction, and the delays encountered while seeking diligently after private financial support all conspire to hike the price tag well into the economic stratosphere. Moreover, with so large a price tag on individual buildings, few individuals can afford to provide the 50-75% of the total that would propel fund-raising to a speedy completion. (A 60,000 gross square foot laboratory building would cost about $20M, in total, to build if we started to plan that building today.) The Mudd building was named for a gift equivalent to a bit more than a third of the total cost.

In many ways, capital funds to renovate old and build new space and ongoing funds to pay for the operating expenses of the high technology working environment that is required by modern science are our greatest financial needs--and the lack thereof the greatest barrier to productive scientists."

(Excerpted from statement prepared by Vice Provost R. F. Bacchetti of Stanford University.)

Mr. FUQUA. Thank you very much, each of you. We are deeply grateful for you being here and sharing with us your thoughts about subject matters of great interest to us in the Congress in trying to fund, and make priorities and tough decisions with limited resources for the various research programs that are funded by the Federal Government.

I noticed one thing that seemed to be, I think, a thread of each of your statements, and maybe you might like to elaborate upon that, the fact that the dire need that we have for instrumentation in our colleges and universities, and research organizations, as well as fellowships to bring in young scientists into research.

Maybe some of you might wish to further elaborate on that comment. Dr. Taube?

Dr. TAUBE. I would like to follow up on my statistics on Nobel Prizes. In physics, the ratio is more than 4 to 1 in favor of the United States, and I think that the difference might well be—Willie Fowler will correct me if I am wrong—I suppose physics is relatively better funded in this country than in the United Kingdom. It is expensive research. It is good research and expensive research.

This contrast between the two fields I really find quite interesting. I have not really thought deeply about the matter, the suggestion that because physics is a more expensive subject this is partly responsible for the disparity in Nobel laureates in the two fields.

Mr. FUQUA. Dr. Fowler.

Dr. FOWLER. Well, Mr. Chairman, I realize that the Congress cannot or should not micromanage the operations of the agencies. But I would call to your attention that, at least on my part, there is some underlying dissatisfaction with the way the instrumentation funds have been allocated.

Particularly in the National Science Foundation the funds are primarily allocated through the program directors. Now, in the study that I chaired, and which I testified on to your subcommittee, and which has subsequently been published by the National Academy of Sciences, we took this stand that instrumentation money should be instrumentation money, not for salaries, not for overhead, not for secretaries because we are all human beings and in tough times when you are a PI or one of the principal workers on a given project, you tend to keep salaries going; you tend to keep shopmen; you tend to keep secretaries. I mean, people come first.

But there is a point at which you just cannot continue to do that. There is no use having good people if they do not have good instrumentation to work with. It is not like it was when I was a young fellow in the laboratory and we built all our own equipment. You cannot do that any more. You just cannot compete with IBM or with the rest of them that make sophisticated computers for you. There is just no way any more.

So, I would like to see—to get to my point—that the instrumentation funds not be done through the program managers—not that they are not good but they are going to use the same criteria that they have used all along was very difficult to change your habits. There should be some independent place where anybody can go and say, "Look, I need this instrumentation; I have to have it to do what I want to do. I cannot get it through the regular program which is already overburdened, but Congress has given you characters some additional funds."

I think the foundation got $40 million, something like that. That should be up for competition on the basis of merit, not on the basis of some hierarchy that has been established. All is disciplinary. The National Science Foundation is a disciplinary organization, it is very hard to get something if you are a physicist and you want to do something in astronomy; if you are a chemist and want to do something in cosmo-chemistry which has to do with the science, it is tough.

I realize that you gentlemen cannot twiddle the dials too closely, but I would sure like to see some effort to make instrumentation a completely separate funding, not run through the program directors.

Mr. FUQUA. That is not micromanagement. You have struck a sympathetic nerve because I was under the impression that it was going for what it was intended for and not for overhead and salaries.

I can assure you, we will——

Dr. FOWLER. If you think there is a sympathetic nerve here, wait until my friends in the National Science Foundation hear what I just told you. [Laughter.]

Mr. FUQUA. You may not get another grant. [Laughter.]

Dr. FOWLER. Mr. Chairman, one other thing. I hope you will pay some attention to what I said about colleges. I have been on the lecture tour for years, and I go to small colleges. I even went to Ripon College at one time which is in Ripon, WI, the home of the Republican Party. It is a very good little college. It is a very good little college, and it is trying to teach students physics.

There were six students who were taking a physics option—nothing in their laboratories. It is just disgraceful. And you know, at Cal Tech at least a great number of our better students come from the small colleges in the Middle West and we have to essentially start them 1 year back because they have never seen anything to actually do a physics experiment with where they were not told the answer in the lab book.

Well, I am quite evangelical about this but you see, there is the other thing: All the peer reviewing is done by characters like me, members of a prestigious faculty, of a prestigious university or institution—mine is. How the devil can people at a small college like Ripon or Dennison—I can name them all over this country—how can they compete when they are being peer reviewed by those of us who are essentially in graduate departments in large universities? I mean, Cal Tech is not all that large.

So, I think it is very important that some new standards be established because I feel strongly that the place you start, the skills in physics and skills in science, is in the colleges and it is not necessarily in the colleges that are large and have big graduate establishments. The small colleges make a very real contribution, but we are going to wipe that contribution out if we do not give them something to work with.

Mr. FUQUA. I think you make a very good point. One of the questions I was going to propound was that basically our science policy rests with our university science departments and the large research facilities like our national laboratories that are built by the Federal Government.

But we also see the importance of singular work, like Dr. McClintock did. I guess the question is: Is our present system of funding research sufficiently attuned to the needs of that style of research? How do we get the Dr. McClintocks funded, our people at maybe smaller, less prestigious schools? Are we fostering a system that makes the rich richer—I am speaking of the colleges—and the other colleges poorer and they can never achieve greatness, particularly as a graduate program?

I am sure that is a subject that has been dear to my heart for a long time and I can make a long speech about. But I will try to condense it.

Dr. McClintock, what are your thoughts about that? You have been victimized or something by that.

Dr. McCLINTOCK. No; it was my choice. [Laughter.]

Mr. FUQUA. That, I appreciate.

But I noticed looking here, Cal, Berkeley, Cal Tech, and Stanford, little tiny schools but great and prestigious schools in this country, and they do very good work, and then Cold Spring Harbor Laboratory.

Dr. McCLINTOCK. That was a very good laboratory.

Mr. FUQUA. I am wondering, how do you fund singular work—whether you by your own personal choice chose not to seek Federal funding—but how can we help small researchers or singular researchers that maybe need some help for instrumentation, or a graduate, or some assistance in some of the work that they are doing?

Dr. McCLINTOCK. Well, from my own lab, I am a little concerned about instrumentation maybe because I do not think that the people using the instruments recognize that they should take the kind of care.

In the 1950's when there was lots of money these people grew up. I do not mean the faculty, I mean the people that come to our place as mainly postdocs. They come for a short number of years. They make their reputation and then they go on.

But they were funded in one way or another. They did not have the responsibility for the instruments, and I see that the instruments are frequently badly, badly treated, and these people grew up with the idea that all you have to do is throw it out and get a new one.

Somewhere along the line I think that there should be some way in which these people understand that these are not easily replaced and that you should do something about taking care of them.

Also, maybe there should be fewer instruments around. That is another thing that happened in the 1960's and early 1970's, there were duplicates of a lot of instrumentation. That money is not there now, but it is the state of mind that, I think, needs to be changed more than anything, to utilize what is there carefully. If it has to be replaced, it can be replaced but not because it has been mistreated. I find mistreatment is very serious. I do not know whether you find it in your places or not. Do you?

Dr. FOWLER. Well, Barbara, there are always people who abuse certain privileges, there is no question about it. But my feeling, my experience has been—and we have tried to inculcate the view into the teaching of our graduate students and undergraduates in our lab. We have tried to make it clear to them that an instrument is almost a sacred thing. This is the thing that you are going to do your job with if you do an experimental thesis. You have to have it. You have to treat it right because you are not going to get another one tomorrow.

So, my experience in general has been that students will respond to that. I am sure you know. I do not think it is quite fair for you to generalize——

Dr. McCLINTOCK. May I make a statement?

Dr. FOWLER. Please.

Dr. McCLINTOCK. I am not generalizing. In the field of biology there are many, many instruments used at all levels. It is not as precious to have everything just so in many of the instruments, as in your line. You cannot work unless your instruments are working right.

You can work with a microscope, for instance, that has a little bit of dirt on it, not too much. You see what I mean?

Dr. FOWLER. Yes.

Dr. McCLINTOCK. There are various grades. In the area in which I am involved, these grades show up. It is not too important until it gets very bad. So, in your line it is very different from my line.

I am thinking about just ordinary small instrumentations that are expensive and they can be misused very fast.

Mr. FUQUA. Mr. Dymally, do you have a question?

Mr. DYMALLY. Thank you. I just wanted to make a couple of observations. Of the four laureates, three come from California, and of the five who received the prizes here, three were foreign born. As a foreign born, I am really proud of that fact.

Given the kind of bigotry that creeps in legislation dealing with immigration, I think we ought to be proud of the fact that of the 103, 30 are foreign born living in America. That, to me, is a source of great pride. We ought to keep that in mind when we get up tight about undocumented workers taking people's jobs and all of these generalizations about foreigners. So, that is of interest to me.

Dr. Fowler, under the master plan for higher education in California, your institute has a monopoly on research, and it would do well for you to send a copy of your testimony to Assemblywoman Hughes, chair of the Assembly Committee on Education, and Senator Hart, chair of the Senate Committee on Education, about undergraduate research because no other institution fights to monopolize research as the University of California, in the legislature.

Dr. FOWLER. Mr. Dymally, I am from Cal Tech.

Mr. DYMALLY. I am sorry, I blew that. Yes. I should really direct this to Dr. Debreu, that is what I really intended to say.

So the university, I believe, should really permit the state colleges to receive some of these research funds. A good example, the State University System in California dominates the training of teachers and yet, they get very little research money for teacher training. The University of California no longer wants to handle teacher training and they get all the research money.

But still, Dr. Fowler, I think it is in your testimony—those two persons—because undergraduate research is very important and we have been, my staff, has been trying to get the NSF to give research money to the undergraduate schools, and they should be thinking about the community colleges also in some specialized areas.

Thank you very much, Mr. Chairman.

Mr. FUQUA. Thank you, Mr. Dymally. Mr. Boehlert.

Mr. BOEHLERT. Thank you, Mr. Chairman.

Dr. Fowler, one of your distinguished colleagues and a Nobel Laureate himself, Dr. Kenneth Wilson, pointed out last year in testimony before the committee that the graduate student, the typical graduate student, in the United Kingdom, France, Germany, and Japan, has access to the latest in computer technology. Yet he, a Nobel Prize winner, a national asset here in America, does not have that same access.

We spend a good deal of time talking about the need for more funding for supercomputers for our university research centers. I would like to have your comment on the need and some suggestions on direction on which we should proceed.

Dr. FOWLER. Well, Kenneth Wilson sat in my classrooms about 30 years ago and he did not listen to what I told him. [Laughter.]

But he is a lot smarter than I am. I am not much of a computer buff. But it is perfectly true that in our universities—and I think Henry and Gerard will agree with me—we had to bootleg computer facilities. We never funded in a proper way, in the early days, the computer facilities in universities whereas one of the things that was done in Europe was to go the other way. They installed rather more computer facilities than I would have thought were necessary.

For example, for years in our laboratory we had a telephone line to the big computer in Berkeley and we essentially had to work at night and on the weekends in order to use that big computer.

Now, that is changing and not in the way that Ken Wilson wants. But practically every laboratory now has a VAX, and in fact there must be about 20 VAX at Cal Tech at the present time.

The old idea of the centralized computer system has not worked out all that well. But now there is a new generation, the supercomputers, which are going to have to be done, I take it, on a national scale and made available to everybody.

I was quite serious when I said that about young Wilson, and his father was a colleague of mine, Paul Wilson. So, one has to be a little careful. Computers are necessary, and they are expensive.

But to get back to my pitch, instrumentation in the laboratory is necessary, otherwise you do not need a computer—well, that is not true either because you can do a lot of theory with computer nowadays. But if you are going to do experimental science you have to have instrumentation and computers. Then, when you get into these fancy things that are needed nowadays, unfortunately, science is getting, at least physics, is getting tough. It is tougher and tougher. That is why we have to have these enormous computer facilities because the easy problems have been solved. So, the hard ones take, as Wilson said, supercomputers.

But you have to balance that against the other needs, I would say.

Mr. BOEHLERT. Thank you very much.

One further question. I am concerned We have just gone through, in the State of New York, a great debate about adequacy of science and math education at the secondary level. Essentially, I think, we have sort of backed down instead of moved forward.

Do you think we are challenging sufficiently the young people at the secondary level in the area of math and sciences, and should we not be requiring a good deal more than we are? The typical response is that we are taxing them too much. I do not share that view, but I would rather have your informed view, the panelists.

Dr. FOWLER On the math I would like to hear Dr. Debreu.

Dr. DEBREU. Yes; science education in this country obviously raises critical questions. I do think that students are not sufficiently challenged.

There is the problem of the insufficient number of qualified teachers in high schools. So, on both counts I think that there is a great deal to be done. There is a great paradox, a culture that is becoming more and more scientific while the science education programs in high schools are not following pace with the development of science.

Mr. BOEHLERT. Then, you must be pleased with this committee's action last year to provide $425 million for upgrading math and science and foreign-language education at the secondary level.

Dr. DEBREU. Certainly.

Mr. BOEHLERT. Thank you.

Thank you, Mr. Chairman.

Mr. FUQUA. The bells now indicate we do have a rollcall vote going on on the floor. We will take a short recess and be right back. Mr. Gore will be back in a few moments and resume the meeting. The rest of us will get back as soon as we can get back.

So, the committee will stand in short recess.

[Recess.]

Mr. GORE [presiding]. If we could start back up again. The committee will come back to order. Chairman Fuqua is over voting and will return shortly. He asked me to reconvene the hearing so as to make the best use of this valuable opportunity to exchange ideas with such a distinguished panel.

I would like to personally express my thanks to the four of you for coming here. Such a distinguished group really honors the committee and we are delighted to have a chance to hear from you.

Since most of my colleagues are also voting at the time, I thought I would take this opportunity to ask a few questions myself.

Dr. McClintock, it took almost 30 years for the scientific establishment generally to appreciate and recognize your discoveries. Why do you think that was?

Dr. McCLINTOCK. We were not ready for it.

Mr. GORE. Pardon me?

Dr. McCLINTOCK. We were not ready for such a drastic change. You are all aware of genetic engineering at the present time. We have come a long way since those days. We did not know what a gene was. There were lots of guesses, but it could not be DNA. It had to be a protein because that is the only thing we knew that was complex.

Also, we had the idea at that time—and I do not know why it was so strongly felt—but it was a preconceived notion, a tacit assumption, that the gene home could not change. I do not know why these things happened, but they happened all along the line, it does not make any difference which subject it is, this type of thing will come up.

When the person has a tested assumption, even if you could give them the best kinds of information and support for a particular end result, a particular conclusion that seems inescapable, we are unable to pass even the first part of it, the first demonstration. The tested assumption, the words you use mean something very different.

So, I had a very pleasant time being left alone. [Laughter.]

Mr. GORE. Well, that is great. I appreciate that response. Your discoveries then, really took place well before the groundwork was in place for the rest of science to understand the significance of what you had discovered.

Dr. MCCLINTOCK. Well, it all comes by, you do an experiment, and you get a very strange, unexpected answer. It leads you into a certain direction of trying to find that you knew something was very strange, very important. You stopped everything else and worked on it, because it was so important in your own mind.

Mr. GORE. Yes.

Dr. MCCLINTOCK. It makes no difference, you cannot stop a person who is quite convinced that this is going in the right direction, and it kept going in the right direction.

So. I think that happens in all sciences, that you meet something unexpected. It gives you a notion of—even if it is radical—that something very different is going on. You cannot stop yourself from working on it.

Mr. GORE. Very interesting.

Let me ask a more general question of the panel. It is expected that the number of students entering colleges is going to level off and could even decline according to some projections. For many universities this may mean that they cannot, because they already have a large tenured faculty, cannot hire new, young professors.

What is your view of this issue, and what do you think could be done about it? Whoever wishes to, respond.

Dr. MCCLINTOCK. I think we have to define our goals of what we are after. That is, how many students should we have and what areas should they be in? I think that just to expand and expand or, even in what way we should, focus the students' attention—for instance, in biology many of the people who get into graduate work have not had any courses.

They start in biology with the M.A. I think somehow or other some change has to occur in the orientation in the high schools and certainly the colleges.

Mr. GORE. Yes.

Dr. MCCLINTOCK. I have the feeling that we are a little out of focus.

Mr. GORE. Dr. Fowler.

Dr. FOWLER. Well, the problem of us old boys holding onto jobs is a very serious one. I have actually retired and Cal Tech policy is you retire completely at the age of 70; at 68, you go on half retirement and get half salary.

But the solution that my institution, Cal Tech, has found is to establish an attractive program of voluntary retirement. Although we have lost some really very good people, who have taken advantage of that voluntary retirement scheme at the age of 62 rather than waiting until 68 or 70, nonetheless, on the whole it has been a very good thing because for every old college professor, full professor, when he retires you can hire two young assistant professors nowadays. One of them may not be quite the equal of the old boy, but two of them are a lot better, I can tell you that.

So, I do not know whether the Congress can help in any way, but universities should be encouraged to offer voluntary retirement schemes. That would certainly be one solution of this problem.

Whether we get more students or less, there is the other problem. I mean, we are all living so long and able to be active and productive, right?

I mean, look at us. Henry, are you the junior member here?

Dr. TAUBE. Gerard. Fowler. Gerard, are you the junior member?

So, there are very serious problems and the way to do it is to help the universities to encourage voluntary retirement, not to make laws like some Senator from some Southern State wants to make, that you cannot retire people until they are—what is his age limit now? That is dreadful, just dreadful.

Mr. GORE. Dr. Debreu.

Dr. DEBREU. I would like to make a comment about another aspect of the shortfall of universities. Universities and foundations have benefited greatly from the tax treatment of gifts, and I know this is under discussion. If tax laws were changed in such a way that those gifts no longer enjoy the privilege that they now enjoy, the effects on universities would be very profound and negative.

That is something about which we should seriously worry for the future.

Mr. GORE. Dr. Taube.

Dr. TAUBE. I wanted to point out that there is sort of a built-in stability of inertia in universities, and I will illustrate that with the example taken from my department.

Enrollment in freshman chemistry tripled a few years after about 1960, and the department tried very hard to get additional so-called fluff approved. It finally has been approved in 1983, and this gives you an idea of how a university will respond to those fluctuations in population. It really is a reflection of the stability that the tenure system affords.

So, if this decline that you mentioned lasts only for a few years, we will simply ride over it. I do not think it will make any real difference at the university level. The places that are much more sensitive are farther down, community colleges, high schools, and so on.

Mr. GORE. We have had this remarkable achievement with all of the science prizes going to Americans this year. It happened in 1976. In 1983, it happened, and, in 1976, it happened as well. In the intervening years, American scientists have won a very large number of the prizes.

But it is often noted that the work for which the Nobel is awarded has often been done anywhere from 10 to 40 years in the past.

Is that true of all four panelists here? I know, Dr. McClintock, your work was done 30 years ago, the work recognized with the Nobel.

Dr. Taube.

Dr. TAUBE. Mine started at the University of Chicago, I would say, about 1950. It was given for, I think, an accumulation over the years.

Mr. GORE. Yes; of course.

Dr. TAUBE. And the way it is justified is, the work is beginning to be appreciated as of this time.

Mr. GORE. Yes. And the same is true?

Dr. FOWLER. Well, I have been working in the same business for 50 years. I went to Cal Tech as a graduate student in 1933, although I did not really start specific work in the area for which the prize was given until about 1939.

It is true that some of the early developments—as in Henry's case and Barbara's case, and I suppose in your case, too—were recognized. Nonetheless, I think all of us are in a field in which there is a great deal of activity now. In fact, in my case—to be frank with you—I was told that the fact that the space program has shown as many of the ideas that my colleagues and I have had, have been verified, recently.

You see, in space you can see a lot more of the spectrum than you can from the ground. I mean, x-ray and gamma-ray astronomy, in particular, have corroborated much of what we have been talking about all these years.

So, it is the general recognition of the validity that you have been working on that I think the Nobel Prize Committee recognizes.

Dr. DEBREU. Yes; in my case also the work I have done has extended over several decades, but the initial impetus was given in the early 1950's. Specifically, what the Nobel citation recognizes is that early work.

Mr. GORE. Yes.

Dr. DEBREU. I should point out there that Alfred Nobel's will specifies that the prize should go to achievements obtained in the last year. So, all our prizes are in contradiction of Nobel's will and probably invalid in a court of law. [Laughter.]

Mr. GORE. How injudicious of you to note that fact. [Laughter.]

Dr. FOWLER. He did not say he was going to give the money back. [Laughter.]

Mr. GORE. Well, the point that I was going to make, which may not be valid at all, that sometimes you hear Americans say, "Well, we must be giving adequate support to science, because we keep winning all the Nobels." It is true that American science is vital and alive and energetic, and has world leadership.

But it is also true that we cannot use the Nobels as a truly accurate indicator of our current leadership in the scientific field because the burst of activity in the decades following World War II was largely responsible for the inquiries that you all began and perfected during your careers, and which has only recently become recognized as deserving of this high honor.

The work now going on in other countries may or may not be equal to or better than the work going on now in the United States. But the current Nobel Prizes are not a fully accurate measure of that standing.

Well, those are all the questions that I have, Mr. Chairman, and I want to turn the podium back over to you.

Mr. FUQUA. Mr. Lewis.

Mr. LEWIS. Thank you, Mr. Chairman.

I want all of you to know, and particularly Dr. Fowler and Dr. Taube, that I certainly share your concern with instrumentation, and you certainly have a supporter here, having been in research and development for 17 years, trying to do combustion analysis on rocket and jet engines without a mass spec certainly creates many, many problems.

It is true that we did sweep the scientific field this year with the Nobel Prizes. I am concerned with not only this year, but I am concerned with 10 years down the pike, and maybe even 20.

This committee has passed legislation, as Mr. Boehlert from New York mentioned to you, on science and math, and it rests in the Senate. I believe we have to, in Congress, to some extent, establish the atmosphere for additional science and math instructors.

But what I am concerned about and would like to hear from you for the record is this. How are we going to continue to move our younger students into these fields? What kind of character do we have to put out there? What kind of recruiting do we have to do to continue so we will remain No. 1 in science and maintain the strength through the years?

Dr. TAUBE. I can respond to that in part. This is a very difficult problem. I have four children, they are all bright. None is a scientist. Most do not even care about science.

Mr. LEWIS. Could you get just a little bit closer to the mike, Doctor?

Dr. TAUBE. I mentioned that I have four children. They are all bright. None is interested in science. So, obviously I do not even know how to do it in my own home. [Laughter.]

But one of the things that I am sure about is that it needs well-trained teachers who understand the subject, and teachers at an early stage because teachers can so easily turn the young away from science rather than interest them in it.

You mentioned that you are going to spend a lot more money in New York State for improvements of science and math education. I am curious about what form this support will take. To me the essential thing is to get good people into teaching and people who like to teach.

In order to do that, you have to pay them enough so that other people who give greater awards do not compete for their services.

Mr. LEWIS. I see. Dr. Fowler, do you have any words of wisdom you could apply?

Dr. FOWLER. Well, I do not know how wise they are, sir. I go back to what I referred to in my testimony, that we somehow or other have to train people who are going to teach the sciences even at the grammar school level. We have to somehow or other get across to them what science is really like.

What it amounts to is that we really have to change the image of the scientist that most American people have. They tend to think of us all as Einsteins. That is what you do when you are a scientist, you dream up deep, fundamental ideas.

That is not the way science is for most of us. It is true, it was that way for Albert Einstein, he was a very great man. But science, especially experimental science, can be a lot of hard, grubby work. But it can be intellectually satisfying.

It is that intellectual satisfaction, completely independent of any economic benefits that you may eventually get out of it, that is important to get across, plus the idea, as I quite frequently say, the work that a physicist does in the laboratory is much more similar to the work of a workman than it is to the work of an office worker.

I mean, that has to be made clear that we are not all eggheads of longhairs, that there are people actually working and working hard in the laboratory to find out the things which we consider important in the sciences.

I think it is very essential, as I said, that anyone who is going to be teaching science somehow or other have some experience with what on-going science is like. There is no point to telling them about what science was like in the 1800's. You have to bring the teachers of today—either in grammar schools or in high schools—you have to make them acquainted with what science is really like, and it changes all the time. It changes all the time.

How to do that is a very difficult problem. How far does the Federal Government go in regard to grammar school and high school education? That is the problem of the States, in general. But the States just are not doing a good job, there is no question about it in my mind.

The Federal Government pays the price for this because eventually, when these youngsters come into the schools that the Federal Government supports through programs, a lot of that money has to go essentially to retraining them and teaching them what it is all about.

So, there are very serious problems. Let me just say, we in science in the United States, in my lifetime, have been the luckiest people in the world. We have been supported very, very generously by the American people, let me make no bones about that.

All that we are talking about—at least all that I am talking about—this morning is a little bit of fine tuning on how it is done. We have to say that there has never been—now, I know, Barbara, you would not take it—but if my laboratory had not had Federal funding we would not have had all of the graduate students and postdocs that we had, in addition to doing what we did.

We have been very fortunate, gentlemen. It is just the point, I mean, you have to keep changing. You have to be aware of what the new problems are. I think that is our job in communicating to you and that is why Henry and I jump up and down so much about the instrumentation problem because it is still not completely solved, at least so far as I am concerned.

Mr. LEWIS. Dr. Debreu.

Dr. DEBREU. Usually I agree with Dr. Fowler, but I disagree on this point. I think that mathematics of the numbers I have quoted, and economics have not been treated as well as physics in the recent time.

Mr. LEWIS. Dr. McClintock.

Dr. MCCLINTOCK. It seems to me that science has expanded, has enlarged so many fields in so many parts. How are we going to treat it with a student of high school level? How are we going to get across a background?

What you need is more or less a background, but it must be something that the student understands. With so many different fields now for them to know about, I do not know how they are going to choose or how they are going to be exposed to it.

What we have now is difficulties because of this. There is no easy way of handling all of the fields that maybe they would like to know about, and neglecting the other fields that one has to have in high school. I think it is a very difficult problem that we have not, probably, faced.

Mr. LEWIS. Would you all agree that by passing legislation that would allow sufficient funds to provide additional education for continuing education for students at the college level or even instructors to go into graduate degrees in science and math would be a plus. And if the Congress passed legislation to do this, it would be a plus in helping to build up that pool of scientists that we need 10 to 20 years down the pike? Anyone.

Dr. TAUBE. I think one has to realize that with an infusion of funds like this the response time—you mentioned it—it is very long.

I think it is more important to look to the future. I do not know how this can be arranged, but I think it is important to pay teachers enough so that you attract some of the very best. It is not enough to sort of retread people that are here now—I am not saying that all of them are bad. I think the important thing is to look more to the future and arrange things so that you get the best kinds of people going into teaching.

Dr. FOWLER. Well, Henry, you are not talking about university teachers.

Dr. TAUBE. No, no, I am talking about——

Dr. FOWLER. Because we are overpaid if anything.

Dr. TAUBE. Well, all right. I am talking about elementary school and high school.

Dr. FOWLER. Yes; but there is a very fundamental problem in the United States; namely, elementary and high school teaching has been in the province of faith and the local communities. In many ways, that is a very good thing; it is very fundamental to our society that it be done that way.

But, boy, when you see the salaries that the high school teachers, even in a place like Pasadena, CA, are receiving compared to the salaries of an assistant professor at Cal Tech, it is just criminal, in my book, it is just criminal.

It is because Cal Tech can go to the Federal Government and they can pay the professors' salaries partly out of the grant funds, about 35 percent now. The high schools and grammar schools of Pasadena cannot go to the Federal Government, and California does not have a property tax any longer. So, there are no funds from that source as there used to be.

So, there are really critical problems in the United States. I agree with yo.. that mathematics has not been supported as well, but that I consider the tuning of the problem, that mathematics should be given more than it has in the past because it is a very integral part of the whole business.

But back in the grade schools it is really serious for this country. How we can do anything without changing our whole system of supporting the public schools is a very real problem.

Mr. LEWIS. Thank you.

Thank you, Mr. Chairman.

Mr. FUQUA. Thank you, Mr. Lewis. I just want to ask Mr. Volkmer who has some very good news that you might be interested in.

Mr. VOLKMER. I would just like to say, Dr. Fowler, as one of those—and I am sure the vast majority of this committee has been listening to your cry and others' cry for need for instrumentation on the university level—that next week when the Space Science and Applications Subcommittee takes up markup on NASA reauthorization, we plan—at least I do, and I think the subcommittee, hopefully, will go along with it—to provide specific funds for instrumentation in the research-analysis part of the NASA budget, which will follow along basically what has been done in DOD, NSF, and others.

So, we are trying to do our best.

Mr. FUQUA. Let me thank all of our distinguished laureates for being here this morning, sharing your views. It has been very helpful to us.

I think, Dr. Fowler, that you will see some positive action in some of the recommendations that you specifically made.

Dr. Debreu, we understand the problem you are speaking of in mathematics, and I think we have seen an effort by this committee to try to reinforce that commitment, maybe not enough, but we are still, as Dr. Fowler said, trying to fine tune and to accomplish that.

To all of you, we thank you very much. Congratulations. We are very proud of you and we are proud to have people of your caliber before our committee to give us the benefit of your thoughts on matters that are very important to us and we are very concerned with.

Thank you very much.

I would like to recognize Mr. Gore for submission of a report from the Subcommittee on Investigation a. ' Oversight on structural failures in building facilities. Mr. Gore.

Mr. GORE. Thank you very much, Mr. Chairman. I will not be lengthy on this. I want to ask for approval of the subcommittee's report entitled, "Structural Failures in Public Facilities."

Mr. FUQUA. Is there objection? The Chair hears none.

[The document follows:]

U.S. HOUSE OF REPRESENTATIVES

COMMITTEE ON SCIENCE AND TECHNOLOGY

SUITE 2321 RAYBURN HOUSE OFFICE BUILDING
WASHINGTON, D.C. 20515
(202) 225-6371

February 22, 1984

Hon. Don Fuqua, Chairman
Committee on Science and Technology
U. S. House of Representatives
Washington, D. C. 20515

Dear Mr. Chairman:

I am pleased to transmit to you a report prepared by the Investigations and Oversight Subcommittee entitled, "Structural Failures in Public Facilities".

The report discusses the findings and recommendations from the Subcommittee's extensive investigation of structural failures in public buildings, dams, and bridges in the United States and draws upon Subcommittee hearings held in August 1982, a survey of the construction industry conducted by the Subcommittee, and the Subcommittee's ongoing review.

The review was undertaken by the Subcommittee because of a concern about the apparent increase in the number of structural failures reported in the media, failures such as that which took place at the Hyatt Regency Hotel in Kansas City, Missouri, in July 1981 and the Hartford Civic Center in Hartford Connecticut in January 1978.

The Subcommittee did not seek to assess blame in the case of any particular failure, but rather to identify common problems associated with such failures, the elimination of which could decrease the number of failures. The Subcommittee's review identified six significant factors (and several factors of lesser importance) which, in the opinion of the Subcommittee, contributed most significantly to the occurrence of a structural failure. The six factors are:

(1) communications and organization in the construction industry;

(2) inspection of construction by the structural engineer;

(3) general quality of design;

(4) connection design details and shop drawings;

(5) selection of architects and engineers; and

(6) timely dissemination of technical data.

In general, the Subcommittee recommendations are designed to institute checks in the building process on the creation of errors that lead to structural failures. The Subcommittee has recommended a variety of actions that the building industry and professional societies can undertake without the necessity of government action, steps such as the establishment of peer review procedures, development of guidelines, and institution of quality control criteria. The Subcommittee also recommended certain actions by state and local governments.

The principal recommendation addressed to the Federal Government calls for the creation, within the National Bureau of Standards, of a national investigative body to obtain and disseminate information on structural failures. This body would be modeled on the widely respected and successful National Transportation Safety Board and represent a reorganization and small enhancement of some of the activities already performed by the National Bureau of Standards. Combined with the cooperative efforts of private industry, the Subcommittee's recommendations could help prevent the causes of many structural failures.

I would like to express my appreciation to the many people in the design, fabrication, and construction industries, who are identified in the report and who cooperated and assisted the Subcommittee staff in developing the hearings, questionnaire, and report, and to Ronald Williams, Technical Consultant, who assisted the Subcommittee staff in preparation of the hearings, questionnaire, and report.

Sincerely,

Albert Gore, Jr.

Albert Gore, Jr.
Chairman
Subcommittee on Investigations
and Oversight

Mr. GORE. I would like to particularly commend Mr. Winn, the ranking minority member of the full committee, for his active participation and assistance during this investigation.

It has lasted over two Congresses. We had a hearing in August 1982 and, with the able assistance of Ron Williams of the full committee staff, we have continued that investigation. The recommendations, I think, speak for themselves, and I would ask that it be approved.

Mr. FUQUA. Without objection, the report is approved.

Mr. GORE. Thank you.

Mr. FUQUA. Without objection, there will be 3 days, until Tuesday at the close of business, for any minority, dissenting, or additional views.

Mr. GORE. I would also ask unanimous consent that the staff be allowed to make technical amendments to the report, and we will consult with the minority on those.

Mr. FUQUA. Without objection.

Mr. GORE. Thank you.

Mr. FUQUA. The committee stands adjourned.

[Whereupon, at 11:44 a.m., the committee adjourned, to reconvene subject to the call of the Chair.]

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