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DOES MAN HAVE A NATURE?

BY RENÉ DUBOS

These remarks were made by Professor Dubos after receiving the Arches of Science Award from the Pacific Science Center, Seattle, Washington, 19 October 1966, in recognition of his "outstanding contributions for the improved understanding of the meaning of science to contemporary man."

I HAVE OFTEN been asked why I settled in the United States after having spent the first 25 years of my life in France where I was born. It was not to escape from Europe, because I loved the physical and human environment of France, Italy, and England, the three countries I knew when I decided to emigrate. I came without any precise plan, not with a job in view, not even with a student fellowship, simply as an old-fashioned emigrant in search of adventure.

In the small village where I was brought up I read with passion until the age of 14 stories about Buffalo Bill and the Wild West, that were then published in a French weekly magazine. I could not imagine any better life than roaming on horseback over the Great Plains and the Rockies. Then, while a student in Paris, I read everything available about American life and became intoxicated with the phrase "America, the land of unlimited possibilities." I had no clear vision of what these possibilities were, but I wanted to experience them nevertheless.

Much to my regret, it does not seem that the Buffalo Bill aspect of my youthful aspirations will ever become reality. Thanks to you, however, the other phase of my dreams has been fulfilled beyond all expectations. My most euphoric imaginings of the land of unlimited possibilities did not include any adventure as romantic and sumptuous as the one you have brought into being. In expressing my gratitude to you, I feel that I am speaking not only in the name of the person I am today, but also as a representative of all the young people all over the world for whom the name America has meant a land of adventure and the promise of a larger life.

Through a happy accident, the first friend I made after arriving in the United States was a graduate student from Washington State. He was Charles Edward Skinner, who eventually became a professor at the University of Minnesota where he died a few years ago. As a result of conversations with him I aimed for the Northwest as soon as I had enough money for a coast-to-coast trip during my vacation time. This was in the early 1930's — almost 40 years ago!

Crossing the continent on bad roads in a second-hand car during midsummer was then a strenuous and exciting adventure, or rather a series of unexpected adventures. But I felt completely rewarded for our tribulations when we entered Nebraska and

Wyoming — Buffalo Bill's holy hunting grounds. The Far West was equal to everything I had imagined. My mood changed suddenly, however, as we reached the Snake River Valley in Idaho. One of my most vivid memories is the vision of the tall poplar trees, and the greenness of the valley. Physically, the landscape reminded me of some parts of France. Yet all experiences in overnight cabins and inexpensive restaurants revealed a human world far different from what I had known.

From the Snake River we naturally proceeded to the Columbia River. I do not recall by what highway we approached the coast but I do remember the emotion I experienced when we first saw the Ocean from a high elevation in an evergreen forest. Reaching the Pacific Ocean was like the completion of my discovery of America.

The week we spent in Oregon, Washington, and Vancouver Island gave me the occasion for other types of discovery — this time concerning human nature. I remember for example approaching a city — Spokane, I believe — which announced its existence by a huge sign "Watch Spokane Grow." Whereas another city — was it Seattle? — proudly proclaimed "Seattle grows without being watched."

From Seattle, we went to Victoria, British Columbia, where I recaptured all the charm of an English provincial town. While in Victoria I went into a department store with the intention of buying a belt. But the atmosphere in the store was so overwhelmingly proper in a 19th-century sort of way that the thought of a belt appeared somewhat indecent, and I bought suspenders instead.

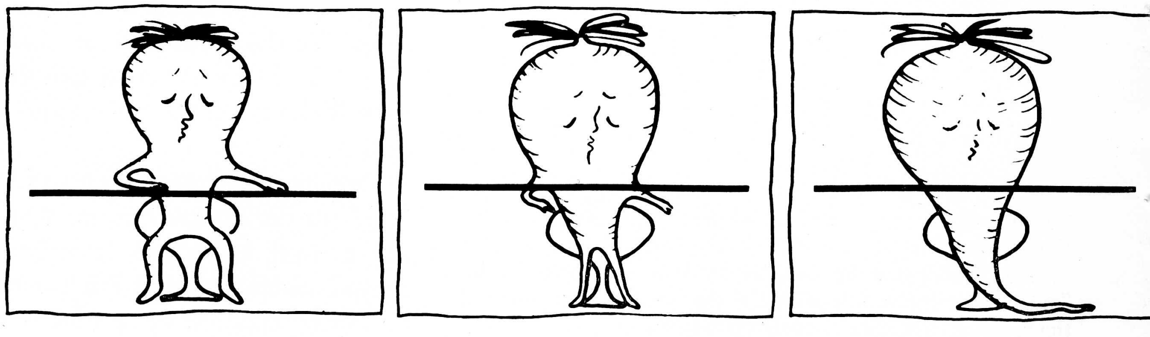
We went down the coast to San Francisco and then proceeded back to New York, but not without stopping for a last pilgrimage west of Denver at Lookout Mountain where Buffalo Bill is buried.

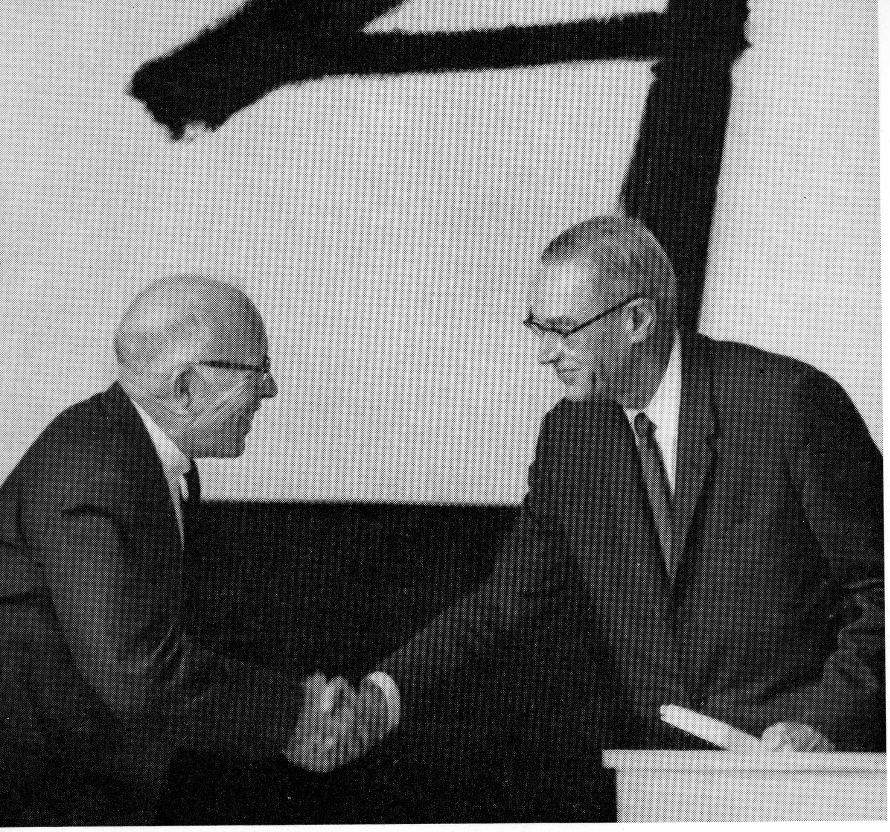
These events and impressions seem on the surface to have little significance. Yet they have been influential in shaping my attitudes first as a laboratory scientist, then as a student of the social aspects of biology and medicine. Let me try to convey to you how the trip to the Northwest sharpened my interest in the interplay between living things and their environment.

I am six feet tall, have blue-green eyes, and my hair was very blond and abundant in those distant days. Physically, I looked more Nordic than most of the Scandinavians and Anglo-Saxons who had settled the lands I had just visited in the Middle West and Northwest. Yet, it was obvious that my French past had made me very different from Westerners; the environment in which I had spent the formative years of my life has left an indelible stamp on me. The differences between Easterners and Westerners and even more between people in Seattle and Victoria (B.C.) made it even more obvious that the personality of human beings is profoundly influenced by their environment. In fact, Seattle and Victoria constituted then, and perhaps still constitute now, the most picturesque illustration of the epigram of the Spanish philosopher Ortega y Gasset: "Man has no nature; what he has is history."

As a young biologist, I had been very much impressed by the fact that the activities of the microbial world are largely determined by the conditions that microbes find in nature. My Western trip made me realize that human beings also are as much the product of their environment as of their genetic endowment. Everything I have done in the laboratory and

"Deprivation of environmental stimuli can turn man into a mental turnip."





Dr. Dael Wolfle, Chairman of the Arches of Science Awards Committee, congratulates Dr. Dubos at the announcement luncheon in Abby Aldrich Rockefeller Hall on October 4

everything I have written has been conditioned by the belief that even though man is constrained by his heredity, he has nevertheless a great deal of freedom in shaping his destiny because he can choose and manipulate his surroundings.

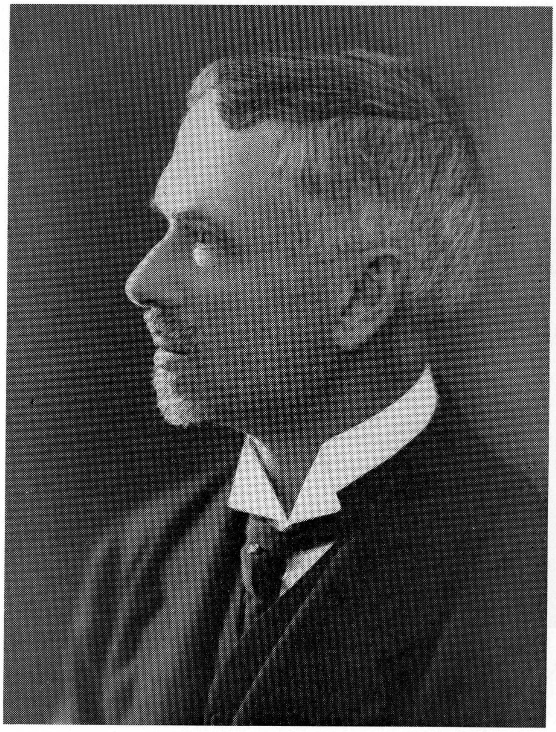
Almost all aspects of our ways of life are now affected by scientific knowledge and by scientific technology. For this reason it is imperative that the performance of scientists be exposed to the public gaze. It is essential that the facts of scientific progress be reported to the lay public as adequately as possible. But this is not enough to help the public pass judgment on the social aspects of science. It seems to me that some more general assessment of science and its implications is also of vital importance. Just as there are professional critics for art, literature, economics, and politics, there should be "science critics."

Last year, Warren Weaver expressed in his own inimitable way the profound social significance of the field of activity recognized by the Arches of Science Award. In his words,

> The relations of science to society, to our total culture, and incidentally to our government are now quite unlike what they were even twenty-five years ago.
>
> In addition to the great innovators of new theories, the penetrators of nature's deep secrets, it is essential that we today have individuals who are capable of understanding science, and who are willing to live their lives partly within science but also partly within the world of affairs.
>
> These may be persons who have the capacity to make science intelligible to all of the citizens, who must have a better understanding of the scientific aspect of public decisions. These may be individuals who can bear the steadily increasing burden of administrative responsibility in activities involving science. These persons, working at the interface of science and society, are more than useful — they have become essential.

Most of the other significant rewards for science are given exclusively for notable contributions to scientific research — in Warren Weaver's words "for what the individual has done *inside* science." There is such an urgent need to develop a scholarly criticism of science, with regard not only to its own inner logic and development, but also to its social implications, that the Arches of Science Award will, eventually, be considered as much *inside* science as conventional scientific research. I believe that the Pacific Science Center has done more than establishing a new award. In my opinion, it is fostering a new trend in science policy. It is directing attention to the fact that the activity of scientists must be integrated with all other social and cultural activities, and therefore must be subjected to creative criticism from the general public.

SCIENTIFIC AUTOBIOGRAPHY



SERGE WINOGRADSKY

BEFORE COMING to the United States, I chanced to read in a French magazine an article by the illustrious Russian microbiologist Serge Winogradsky who was then working in France at the Pasteur Institute. Winogradsky was not writing for specialists and his scientific message was therefore stated in simple words. He asserted that the kinds and numbers of microbes in a particular sample of soil at any given time are determined by the physical characteristics and the chemical composition of that soil sample. Winogradsky emphasized also that the microbial population in turn modifies the properties of the soil and thus conditions its fertility. It was this concrete example of feedback between soil microbes and soil conditions that first sensitized me to the importance of the interplay between environmental factors and living things.

I thought it worthwhile to mention the influence exerted on me by this semi-popular article published exactly 40 years ago, not only to pay tribute to the scientist who has had the deepest influence on my intellectual life, but also to illustrate the significance of the program of general scientific education sponsored by the Pacific Science Center.

Let me try to convey, with one single example, how the article on soil chemistry and microbiology that I read as a young man has conditioned my subsequent activities in various biomedical and sociological fields.

When I became involved in the study of tuberculosis during the early 1940's my attention immediately turned to the effect that environmental factors exert on the virulence of the tubercle bacillus and on the susceptibility of man to tuberculous infection. In particular, I was much impressed by the fact that the social environment profoundly affects the prevalence and severity of tuberculosis and that the frequency of the disease in turn affects many aspects of social life, including literature and the arts. My wife and I then wrote a book in which we tried to analyze the complex and subtle interplays that occur between tubercle bacilli, individual human beings, and society as a whole. Whatever its merits, this book is relevant to the philosophy of the Pacific Science Center because it embodies several different, but closely related, aspects of the relation between scientific knowledge and social problems. Furthermore, the book gave us the opportunity to write about science, not only as a body of factual knowledge, but especially as a matter of social concern.

Interest in the environmental determinants and the social consequences of tuberculosis naturally led me to look at other problems of health and disease from a similar point of view. Step by step, I thus found myself involved in a consideration of the factors that affect the interplay between man and his total environment.

These remarks are excerpted from the address given by Dr. Dubos at the announcement luncheon in Abby on October 4. The article by Serge Winogradsky is "La Méthode Directe dans l'Etude Microbiologique du Sol." *Chimie et Industrie,* Volume 11, 1924.

ADVANCED SCIENTIFIC RESEARCH IN DEVELOPING COUNTRIES

BY ABDUS SALAM

FIVE HUNDRED YEARS ago — around 1470 AD — Saif-ud-din Salman, a young astronomer from Kandhar working then at the celebrated observatory of Ulugh Beg at Samarkand, wrote an anguished letter to his father. In words more eloquent than I could employ, Salman recounted the dilemmas, the heartbreaks, of an advanced research career in a poor, developing country:

Admonish me not, my beloved father, for forsaking you thus in your old age and sojourning here at Samarkand. It's not that I covet the musk-melons and the grapes and the pomegranates of Samarkand; it's not the shades of the orchards on the banks of Zar-Afsham that keep me here. I love my native Kandhar and its tree-lined avenues even more and I pine to return. But forgive me, my exalted father, for my passion for knowledge. In Kandhar there are no scholars, no libraries, no quadrants, no astrolabes. My star-gazing excites nothing but ridicule and scorn. My countrymen care more for the glitter of the sword than for the quill of the scholar. In my own town I am a sad, a pathetic misfit.

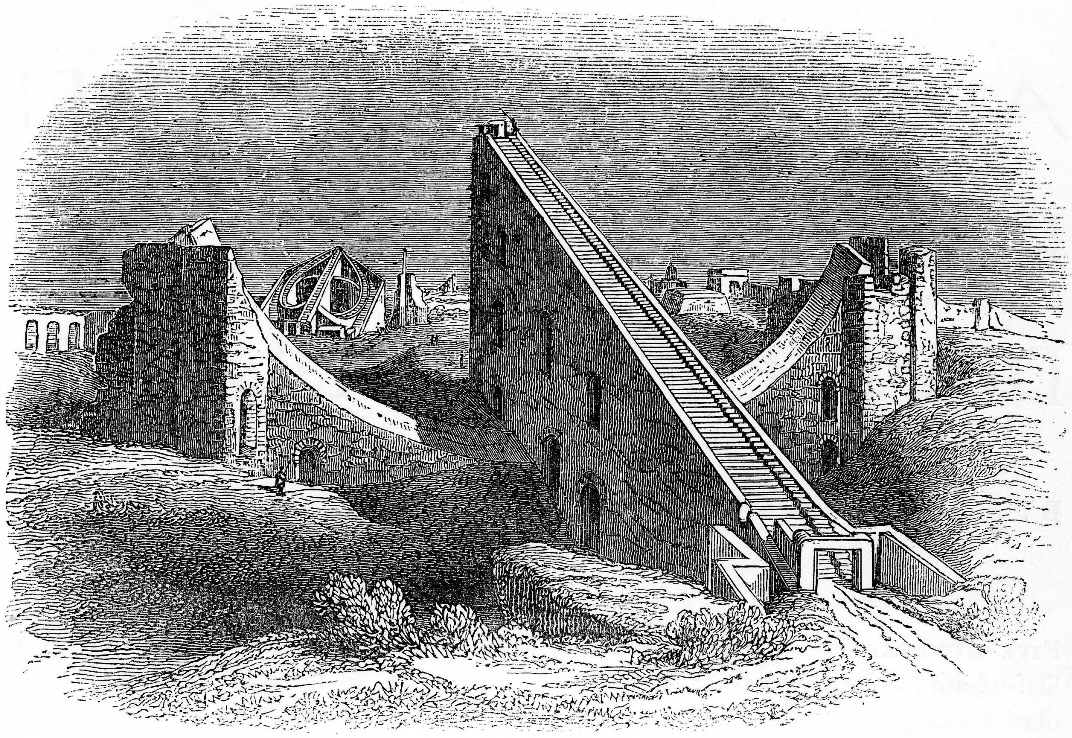
It's true, my respected father, so far from home, men do not rise from their seats to pay me homage when I ride into the bazaar. But some day soon, all Samarkand will rise in respect when your son will emulate Biruni and Tusi in learning and you too will feel proud.

Dr. Salam, Director of the International Centre for Theoretical Physics at Trieste, presented this address in May in Caspary Auditorium at the International Symposium on Science in South Asia jointly sponsored by The Rockefeller University and the New York State Department of Education.

Saif-ud-din Salman never did attain the greatness of his masters, Biruni and Tusi, in astronomy. But this cry from his heart has an aptness for our present times. For Samarkand of 1470, read Berkeley or Cambridge, for quadrants read high-energy accelerators; for Kandhar, read Delhi or Lahore, and we get the story of advanced scientific research and its dilemmas in the developing world of today as seen by those who feel in themselves that they could, given the opportunity, make a fundamental contribution to knowledge.

But there is one profound change from 1470. Whereas the emirate of Kandhar did not have a conscious policy for development of science and technology — it boasted of no ministers for science, it had no councils for scientific research — the present-day governments of most developing countries would like to foster, if they could, scientific research, even advanced scientific research. Unfortunately, research is costly. As we have heard, most countries do not yet feel it carries a high priority among competitive claims for their resources. Not even indigenous *applied* research can command priority over straightforward projects for development. The feeling among administrations, perhaps rightly, is that it is by and large cheaper and perhaps more reliable to buy applied science from the world market. The final picture so far as advanced research is concerned, remains in practice almost as bleak as at Kandhar.

Let us examine some of the factors that affect ad-



Observatory of Delhi built by Rajah Jai Sing in 1710

vanced scientific research. To me, first and foremost, the determining factor for all advanced research is the supply of towering individuals, the tribal leaders, around whom great institutes are built. These are perhaps the five per cent of all men who are trained for research. What is it that we in the developing world are consciously doing to ensure their supply? To my knowledge most developing countries are doing practically nothing. Quite the contrary, it is to me astonishing, miraculous, that considering all the hazards that beset a poor society, any talent at all is saved for science. These hazards are, first, the very poor schooling; second, the Indian Civil Service and its analogue, the Civil Service of Pakistan, which skim off the very top of the subcontinent's intellect; third, the chancy nature of any opportunities for the extended apprenticeship for research. Add to this the greatest hazard of all; one may or may not be fortunate in getting a position with the few men – in the case of India and Pakistan, the Siddiqis, the Usmanis, the Menons, the Sarabhais, the Seshachars, at the few centres of excellence – who appreciate at all the demands of a research career and who run laboratories which are reasonably well equipped. As Doctors Seshachar and Siddiqi told us, it remains a sad fact that, though India and Pakistan may have built specialized institutes outside the university system where advanced research is carried out, by and large the vast university system remains weak, static, uninspired. I shall always remember my first interview with the head of the premier college in Pakistan, which I joined after a spell of theoretical work in high-energy physics at Cambridge and Princeton. My chief said:

> We all want research men here, but never forget we are looking more for good, honest teachers, and good honest college men. This college has proud traditions to uphold. We must all help. Now for any spare time you may have after your teaching duties, I can offer you a choice of three college jobs: you can take on wardenship of the College hostel; or be chief treasurer of its accounts; or if you like, take up Presidentship of its Football Club.

As it was, I was fortunate to get the Football Club.

Admittedly this was twelve years ago. I would be ungrateful if I did not mention that this same college today is contesting with the Atomic Energy Commission of Pakistan for the control of a high-tension laboratory with a 2.5 Mev Cockcroft-Walton set. This is a measure of the change brought about by the heroic efforts of the Pakistan Government since

Headquarters of the Council of Scientific & Industrial Research in New Delhi

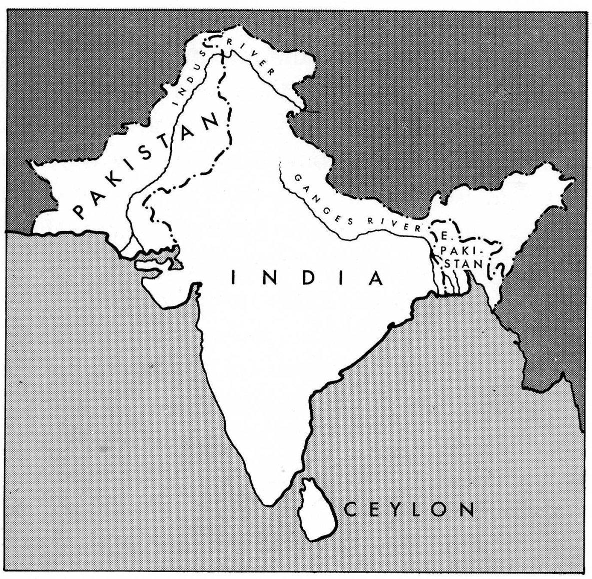
1958, to which Dr. R. Siddiqi referred. Things have changed and I would like to illustrate the present position, the immediate needs, with reference to research in a field I am familiar with, the field of theoretical physics.

My thesis is that in a number of fields, advanced scientific research in developing countries is reaching, and has reached, a stage of first-rate maturity. The indigenous resources are being skillfully employed, but there still is a desperate need for international help. The truth is that irrespective of a man's talent, there are in science, as in other spheres, the classes of haves and the have-nots: those who enjoy physical facilities for furtherance of their work, and those who do not, depending on which part of the world they live in. This distinction must go. Our President, Dr. Bronk, in his very beautiful and wise address yesterday, gave us the keynote of this conference; meaningful, international cooperation in science. I believe the time has come when the international community of scientists should begin to recognize its direct moral responsibility, its direct involvement, its direct participation in advanced science in developing countries, not only in institutional terms but in *personal* terms of the first-rate individual working in these countries. And as I said earlier, I'd like to illustrate my remarks with reference to theoretical physics.

Theoretical physics happens to be one of the few scientific disciplines which together with mathematics is ideally suited for a build-up in a developing country. The reason is that no costly equipment is involved. It is inevitably one of the first sciences to be developed at the highest possible level; this was the case in Japan, in India, in Pakistan, in Brazil, in Lebanon, in Turkey, in Korea, in Argentina. Gifted men work in advanced centres in the West or the USSR. They then go back to build their own indigenous schools. What has happened in the past is this: these men went back to the universities, they were perhaps completely alone, there was no critical size in the groups they were part of, no good libraries, no communication with groups abroad; they were isolated, and isolation in theoretical physics is death. This was the pattern when I joined Lahore; this is still the pattern in Chile, in Argentina, in Lebanon, in Korea. In India and Pakistan we have been more fortunate in the last decade. A number of specialized institutes have grown there for advanced work in theoretical physics: The Tata Institute, the Institute of Mathematical Sciences at Madras, Atomic Energy Centres at Lahore and Dacca, where a fair concentration of good men has been made possible. But this is not enough. These institutes still are small islands; they still do not have vigorous contacts with the world community. Tata and Madras have partly solved their problem, they have funds to invite visitors; they have fewer funds to send Indian physicists abroad, mainly because of the very real problem of foreign exchange.

It was with this type of problem in mind that the idea of setting up an International Centre for Theoretical Physics was mooted in 1960. The idea was that one may set up a truly international centre, run by the United Nations family of organizations, for advanced research in theoretical physics. We planned it with two objectives in view: first, to bring physicists from the East and the West together; second, to provide extremely liberal facilities for senior, active physicists from developing countries.

How does the idea work out in practice? We have normal fellowships which are given mostly to those from developing countries. But in addition the International Centre has instituted a scheme which we call the Associateship Scheme. A number of senior, active physicists from developing countries are selected, they are given the privilege of coming for a period of one to four months every year to the Centre with no formalities except a letter to the Director. The Centre pays for their fares and their stay. The idea is that we should eventually have a cadre of something like 50 senior, active physicists from developing countries who possess this privilege at any one time. They should choose the timing of their coming, whenever it suits them.



Looking back on my own period of work in Lahore, as I said, I felt terribly isolated. If at that time someone had said to me, we shall give you the opportunity every year to travel to an active centre in Europe or the USA for three months of your vacation to work with your peers; would you then be happy to stay the remaining nine months at Lahore?, I would have said yes. No one made the offer; I felt then and I feel now that this is one way of halting the brain drain, of keeping active men happy and contented within their own countries. They must be kept there to build for the future, but their scientific integrity must also be preserved. By providing them with this guaranteed opportunity for remaining in contact with their peers, we believe we are making a dent in the problem.

As I said, we are running the Associateship Scheme at Trieste. Ideally the scheme should be wide enough to cover nearly every active physicist in developing countries. Unfortunately, the International Centre at Trieste does not possess funds to do this, and this is the reason why I welcomed so very much the opportunity afforded by this conference to plead here for an extension of this scheme.

Briefly, what I would like to see happen is that large institutions — Princeton, Harvard, Rockefeller University, New York State University, Imperial College, London — may consider setting up their own associateship schemes, not only in theoretical physics, but also in other subjects. The Rockefeller University, for example, could extend the privilege of giving its freedom not only to Professor Seshachar, but also to most other active microbiologists in most developing countries. We have found the scheme is not too costly. Since we pay no salaries, only the fare and a per diem, it costs us something like $100,000. Already the European Organization for Nuclear Research at Geneva has started following a scheme similar to our own, which covers I believe both experimental and theoretical physics. It is naturally designed only for developing countries within Europe (Greece and Spain). If we could succeed in covering every active, first-rate worker in the developing countries, we would go very far in removing one of the curses of being a scientist in a developing land.

I have emphasized the personal problem of the advanced research worker. In advanced scientific research I believe it is the personal element that counts much more than the institutional. If, through meaningful international action allied with national action, we could build the morale of the active research worker, persuade him to not make himself an exile, we shall have won a real battle.

ONE OF A SERIES of authoritative statements on the role of science and technology in the historical development, present conditions, and future prospects of South Asia—by eminent scholars and administrators from India, Pakistan, England, Sweden, and the United States—to be published in the book, *Science in South Asia,* The Rockefeller University Press, 1967.

BLASPHEMY AND BIOLOGY

BY JUNE GOODFIELD-TOULMIN

More often than we sometimes realize, or care to admit, the pace and direction of scientific advance are determined by a sociological climate external to science itself. A striking example of this was the suppression and denial of copyright protection to a superb series of biological lectures published by an English surgeon in 1819.[1] The chain of events which made this situation even possible goes back to the Star Chamber of Charles I in the year 1637, where on July 11, an act was passed requiring all printers and publishers to be licensed. Two important conditions were specified. First, the printers must not produce any "seditious, schismatical, offensive bookes or pamphlets," and second, the books and pamphlets must bear the name and address of the publisher and also the name of the author. (This latter provision was solely an identification for punishment.) By 1709 the intellectuals of England must have been somewhat discouraged, since we find Parliament passing "An Act for the Encouragement of Learning by Vesting the Property of Printed Books in the Authors." Nevertheless this still carried by implication the tradition that there was no property in that which was blasphemous, seditious, or immoral.

In a spate of Chancery cases in the 1820's the Chief Justice of England, Lord Eldon, ruled definitively on this question. Where there was blasphemy, sedition, or immorality, there was no property; where there was no property, there could be no right. When an author brought an injunction against pirating publishers, the publishers were quick to see the advantage of raising in defense the issue of blasphemy, immorality, and sedition. And if Lord Eldon was the judge, they tended to win their cases. Lord Byron himself was prominent twice in such suits. In February 1822 he lost his case with regard to a pirated edition of his poem *Cain;* and in 1823 he lost it yet again over a pirated portion of *Don Juan.* In the interim between these two cases, in March 1822, a case was heard concerning a pirated scientific work, *Lectures on Physiology, Zoology, and the Natural History of Man.* This work had been withdrawn from publication by the author, William Lawrence, after a public outcry and when in spite of the author's wishes it was reprinted, Lawrence brought an injunction to restrain the publisher. The injunction was contested by the defendant who argued that, because of the inherent blasphemy, Lawrence had no property rights in his manuscript. Having read the book itself and the reviews in the *Quarterly Review,* the *British Critic,* and the foremost medical journals of the day, Lord Eldon gave judgment for the publisher on the grounds that the law did not protect those who contradicted the Scriptures and that Lawrence's lectures

Dr. Toulmin *is Rebecca Bacharach Treves Professor in the History and Philosophy of Science at Wellesley College and Fellow of the Royal Society of Medicine in London. She gave this lecture at The Rockefeller University in March.*

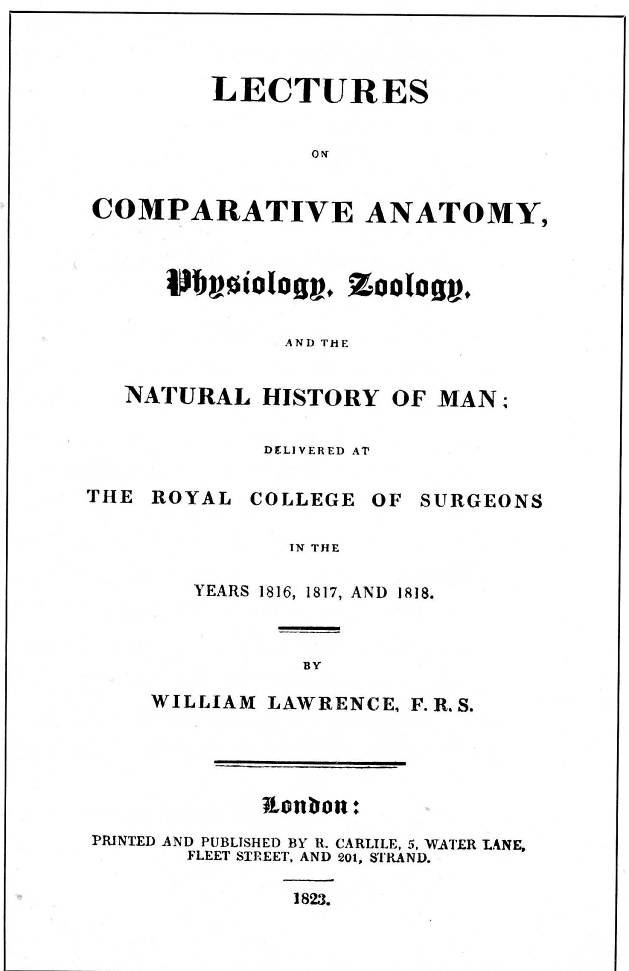
did violate the law as it stood.

There are a number of important elements in William Lawrence's book for the development of biological theory and methodology. Yet it must be said that the objections brought against his physiological arguments—leading to the public outcry, his withdrawal of the book, and ultimately to the court case—were largely irrelevant to strictly scientific issues.

William Lawrence, who eventually became the President of the Royal College of Surgeons twice, was born in 1783 in Cirencester, Gloucestershire, where his father was chief surgeon of the town. When he was sixteen, he was apprenticed to John Abernethy at St. Bartholomew's Hospital in London. Two years later Abernethy, impressed with the young man's talents, appointed Lawrence as his demonstrator in anatomy, and he held this post for twelve years. He became a member of the Royal College of Surgeons at the age of twenty-one, and assistant surgeon at Barts at the age of thirty-one, in 1814. His relationship with Abernethy is important in view of the subsequent controversies, for Abernethy was a devoted admirer of John Hunter who had died in 1793 when Lawrence was ten years old. Hunter's impact and influence on English physiology and medicine were pervasive and profound; in Abernethy's view, his word was gospel and he lost no opportunity of mentioning Hunter's ideas, and especially Hunter's theories of life, in the most eulogistic terms.

Every year at the Royal College of Surgeons two introductory courses were offered to the medical students, one on comparative anatomy and one on physiology. Abernethy gave one of these courses in 1814 and two years later Lawrence was appointed to give the other course. So for about four years they ran parallel with each other on these series of lectures. In view of Abernethy's eminence at the Royal College of Surgeons, and his devotion to Hunter, it was perhaps tactless and certainly unfortunate for Lawrence, in 1816 on the occasion of his first appearance, to devote one of his introductory talks to the problems of "Life";[2] and to use the occasion to deliver an extremely penetrating analysis of Hunter's doctrine of a vital principle, and to criticize especially the particular gloss placed on this doctrine by Abernethy. Given all these circumstances some sort of confrontation between the two men was clearly inevitable.

Hunter's views on the vital principle are to be


LECTURES

ON

COMPARATIVE ANATOMY,

Physiology, Zoology,

AND THE

NATURAL HISTORY OF MAN;

DELIVERED AT

THE ROYAL COLLEGE OF SURGEONS

IN THE

YEARS 1816, 1817, AND 1818.

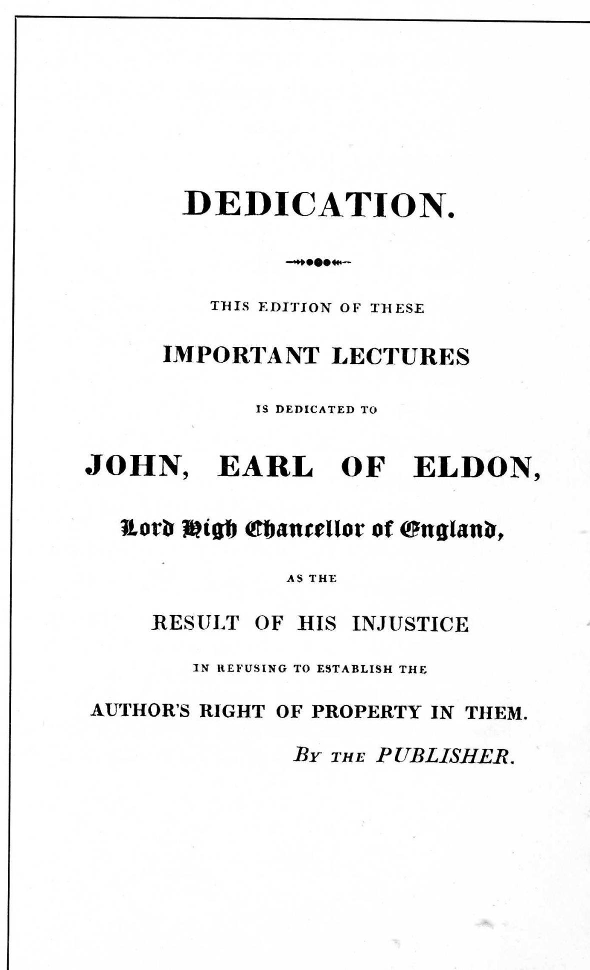
BY

WILLIAM LAWRENCE, F. R. S.

London:

PRINTED AND PUBLISHED BY R. CARLILE, 5, WATER LANE, FLEET STREET, AND 201, STRAND.

1823.


DEDICATION.

THIS EDITION OF THESE

IMPORTANT LECTURES

IS DEDICATED TO

JOHN, EARL OF ELDON,

Lord High Chancellor of England,

AS THE

RESULT OF HIS INJUSTICE

IN REFUSING TO ESTABLISH THE

AUTHOR'S RIGHT OF PROPERTY IN THEM.

By the PUBLISHER.

WILLIAM LAWRENCE

found in lectures that he delivered in 1786 and 1787,[3] at a stage when physiology was just about to enter again onto one of those periods of philosophical questioning; that is, a time when once again the question, "In what terms can we describe or explain living matter?" would take on a pressing urgency, alongside the more routine inquiries and experiments. That it arose once again when it did, and in the form it did, was no accident. It was directly due to the success of the new chemical philosophers who at one and the same time presented physiologists both with a fundamental distinction between living and nonliving matter, and an overpowering paradox for biological method. For, by the time these chemical philosophers had done their work, the idea of a pure chemical substance, as we would now regard it, was more or less defined: i.e., it could be weighed, measured, contained; it reacted, it was ponderable, and so on. More importantly for physiology, they had specified by implication what a chemical substance could *not* do: i.e., chemical substances could not move, eat, excrete, breathe, evince irritability, reproduce, and develop. In other words, matter was essentially passive, and this property was to have important implications both for the problem of life and for the sensitive area of the relationship between mind and matter.

If that had been all, the differences between organisms and inorganic materials could have been safely left at this descriptive level. But as the chemists continued their work, the paradox became evident: for chemical reactions clearly went on in living organisms. So the question then became: "How can we *explain,* at the theoretical level, the obvious differences between the living and the nonliving that we perceive at the observational level; and yet in our explanations, how can we both allow for these differences and for the fact that chemical matter clearly forms part of all living systems and chemical processes go on inside them?" That there must be some *specifiable* difference seemed evident; and the evidence that all physiologists pointed to was the evidence of death. Once an organism died it behaved like an ordinary chemical system. It could be, and was, decomposed by the air; a decomposition which it apparently had the capacity to resist when it was alive.

So, what, in a word, was "life"?

In the early years of the nineteenth century we find several different forms of answer to this question. Here I am concerned only with two. Firstly, the doctrine that life was a "principle superadded to matter" and secondly, the theory that life results both from the very organization of living matter, and the action of mutually interdependent processes. The first view was placed firmly into biological circulation by John Hunter. The second view, even if it did not originate with William Lawrence, found in him one of its most eloquent exponents, and is clearly coming into its own in the second half of the twentieth century.

In essence, Hunter said, "life" was a simple principle, quite distinct from the organization of living matter but superadded to it. Because the chemical matter (such as carbon and nitrogen) of a dead organism was the same as in a living organism, he argued next that the cause of life cannot possibly reside in the matter alone. And even though this chemical matter might be organized in a special way, this organization alone could not be *equivalent* to life. After all, he argued, life was clearly manifested in matter — which was, so far as one can judge, totally devoid of any observable organization — for example, the earliest stages of chick embryos.

For those people who found it difficult to conceive that a nonmaterial principle added to matter could give rise to strikingly new properties, Hunter always appealed to the analogy of magnetism.

Iron appears at all times the same; whether imbued with this property or not; magnetism does not seem to depend on the formation of any of its parts. A bar of iron without magnetism may be considered like animal matter without life; . . .

Now when in 1814[4] Abernethy expounded Hunter's ideas on life he presented them as a rational theory. Unfortunately, not only did he take Hunter's analogies literally, but, as John Bostock[5] was later to point out, he wished ideas of his own onto Hunter. Carrying the same kind of argument as Hunter's still further, he concluded too that life is a principle superadded to matter; and, while he was careful not to identify this "vital principle" with the "electrical principle," he nevertheless moved over from the magnetic analogy of Hunter to an electrical one: adding to it his own gloss that the "vital principle" must therefore be a "subtle fluid," and unjustifiably attributing this view to the great man himself.

The particular gloss was unfortunate, but perhaps not surprising. Whereas Lavoisier had shown that matter was something which could be weighed and was therefore ponderable, he nevertheless had left a whole group of phenomena such as electricity, magnetism, and heat in categories which up to this point had not been susceptible to the same kind of explanation. And so it became necessary for the critics of the "vital principle" doctrine to start by clearing away the "electrical fluid" analogy, before they could get down to the fundamental problem itself. This was not too difficult, and we can let Lawrence in 1816 be the spokesman for the more general and widely held view—going straight to the heart of the matter in his characteristically forthright and uncompromising manner:

The object of explanation is to make a thing more intelligible. In showing that the motions of the heavenly bodies follow the same laws as the descent of a heavy substance to earth does, Newton explained a fact. The opinion under our review is not an explanation of that kind; unless indeed you find, what I am not sensible of, that you understand muscular contraction better by being told that an Archeus, or subtle and mobile matter sets the fibres at work. This pretended explanation in short is a reference not to anything that we *understand better than the object to be explained; but to something that we don't understand at all**. . . .To make the matter more intelligible, this vital principle is compared to magnetism, to electricity, and to galvanism; or it is roundly stated to be oxygen. "'Tis like a camel, or like a whale, or like what you please. . . ."[6]

His forthrightness was hardly calculated to endear him to his contemporaries, nor did it. But had matters been left at this point, the controversy might well have been confined to the walls of the Royal College of Surgeons, and restricted to the not unusual clash between scientific generations. (In his 1817 lecture Abernethy was content to cover the same ground as in his 1814 series, expounding Hunter's views once again, and roundly attacking Lawrence for his skepticism.) Lawrence was clearly a man whose intellectual convictions were profound; though intemperate at times in expression, he was passionately devoted not only to the study of physiology, but also to the younger generation of medical students. In a society which was becoming more and more introverted and insular he was determined to search for scientific truth by observation and reason alone, and speak it out freely. Stung by Abernethy's reaction, which he felt was unreasonable and unconsidered, and pricked by the avuncular hostility already showing itself among the senior members of his profession—notably Charles Bell—he launched in his 1817 lectures into a brilliant and sustained exposition of his views. These are the lectures which, in 1819, were published as *Lectures on Physiology, Zoology, and the Natural History of Man.*

Lawrence writes beautifully, with a wealth of illustrations, both literary and scientific, drawn from past and present writers, and from foreign as well as from British authors. Ironically, in view of what was to happen later, there runs through all these lectures one consistent theme: a plea for liberal attitudes and an open mind, both in science and in thought generally; a realization that a "modest confession of ignorance is better than an overbearing dogmatism"; an insistence that "fair argument and free discussion produce more honorable results than an enquiry into a man's motives, tendencies, and designs"; an emphasis that truth, not victory, should be the only object and the only end in academic discussion. And

*My italics.

all the time he insists, "I will not be silenced." For, to quote one of the many bitter-sweet phrases he is continually using, "Like Ajax, all that truth demands is daylight and a sense of fair play." (This plea relates to Ajax's fury when he realizes that Odysseus has deprived him of Achilles' armor by political trickery.)

What was it in Lawrence's views that led up to his ostracism and to the Court case? Several distinct elements played significant parts, all of which his critics tended to run together: his views on organization and life; his ideas about the relation of the mind and the body; and his willingness to utilize knowledge and help from whatever source he could, even foreign ones.

Organization and Life

He begins by refusing quite unequivocally to constrain the term "life" within one definition, formula, or phrase, for at the simplest level life denotes only what is *apparent to our senses.* It cannot be applied to the "offspring of metaphysical subtlety" or "immaterial abstractions" without a complete departure from the original acceptance of the term. We can, of course, study the phenomena of life; we can study its organization, or the peculiar heterogeneic composition which distinguishes living bodies, as contrasted with inorganic bodies. Further, it is not only the inevitable heterogeneity of organisms that marks them off from nonliving matter, but the fact that the very structures themselves are in constant flux, by virtue of the processes of exchange between the organism and the external world, e.g., digestion, breathing, and growth. We are dealing, he says, not with one *single phenomenon,* which could be covered by a single *word,* but a set of mutual interrelations. Even the expressions we use, such as "organization," "function," "vital properties," and "life," are themselves closely related to each other. "Organization," he said, "is the instrument. Vital properties are the acting power; function, the mode of action; and life is the result."

Certain organic structures, such as muscles and nerves, manifest certain vital properties, such as contractility and irritability. He argued that there was, in the full philosophical meaning of the word, a "necessary" connection between the structures and the properties. The vital properties are the causes of vital functions in the same sense that chemical affinity is the cause of chemical combination and gravitational attraction is the cause of the movements of the heavenly bodies. We observe the phenomena of life, and we trace them back as far as observation and experiment will enable us; and refer them ultimately to a peculiar order of properties which are called "vital properties." For the moment no question of the mechanisms arises. To the question, "Why or how does muscle contract or a sense cell respond," all that Lawrence felt entitled to say was, "As yet I do not know, and I cannot conjecture."

In using the phrase "vital properties" to describe the immediate cause of living phenomena, Lawrence was deliberately alluding to the Frenchman, Xavier Bichat, whose words he returns to many times, and who clearly influenced him greatly. Yet in one interesting and fundamental respect, he moved on from Bichat. Bichat insisted that there was such a variability among vital properties that they could not be studied with the tools of physics and chemistry. It was this view which lay at the heart of Claude Bernard's later criticisms: not Bichat's insistence, which Bernard applauded, that the causes of vital phenomena are to be found in the properties of tissues, but the implicit indeterminism in the phenomena about which Bichat was so emphatic.

In 1960, writing about Bichat,[7] I said that it was a pity he died so young, since he might subsequently have realized that the "internal environment" was capable of a rigid determinism, and that the variability of vital properties has another source. Fifteen years after Bichat's death, in 1818, we see Lawrence taking this next step for him, placing himself, methodologically speaking, halfway between Bichat and Bernard. Yes, Lawrence agrees, vital action does fluctuate. Yes, to calculate the power of the muscle or the velocity of blood is, to use Bichat's comparison, like building an edifice on shifting sand. But why should this be so? By 1818 Lawrence is emphasizing not so much an inherent indeterminism in physiological phenomena, as the complexity and interrelatedness of living processes. He is thus questioning not the calculations themselves, but *what the figures can tell us.*

The laws of physics and chemistry do apply within living matter: this, he declares explicitly, is too obvious to be denied. What he questions is the *total* program of "reductionism." Are the limb movements of the animal machine governed by the laws of mechanics and hydraulics like a system of levers? Yes, he answers, in a general way they are; but we cannot calculate exactly the contraction of living muscle, because we can never be certain that we have all the necessary data before us. Similarly with the circulation. There are innumerable processes and reactions which contribute to the "strength" of the circulation at any one time and these themselves are dependent not only on each other but on variations in the external environment. It is no wonder, says Lawrence, when physiologists do try to estimate the force exerted by the heart for instance, that one gives his result as 8 ounces, another as 180,000 pounds, since with so many elements necessarily entering into the calculation, it becomes impracticable, if not irrelevant.

The same is true of animal chemistry; chemical analysis gives us a kind of "anatomy" of living fluids, but physiological knowledge, Lawrence insists, consists in discovering the innumerable ways in which the composition of the bodily fluids varies and *why* it varies, and how each organ influences and modifies the rest. So chemical analysis yields but one small part of the physiological story. A thorough understanding of chemistry is indispensable for the would-be physiologist, but to resolve life itself into a mere play of "chemical affinities" seemed to Lawrence "injudicious." In the science of life, we are dealing with complex systems, and this students should never forget. In his own words,

> In the physical sciences it is in our power to regulate the conditions of the operation or experiment, and to reduce them by successive analyses to the greatest simplicity. But in physiology we are forced to take and study our subjects in all the complexity of their natural composition, in conditions not regulated by our choice, and in a state of complication requiring close attention, and careful discrimination to search out and determine the precise share of each component part.

So Lawrence sums up both the problem and the methods for the future science of physiology and its relation to medicine.

> By the preceding observations, or by any subsequent ones, I would by no means discourage surgical students from the pursuit of the physical sciences. I regard them, on the contrary, not merely as a desirable accompaniment, but as powerful and indispensable auxiliaries in physiological and medical researches. A close alliance between the science of living nature and physics and chemistry, cannot fail to be mutually advantageous. What we have principally to guard against, in our professional researches and studies, is the influence of partial and confined views (and of those favorite notions and speculations, which, like colored glass, distort all things seen through their medium). Thus we have had a chemical sect, which could discern, in the beautifully varied appointments, and nice adaptations of animal structure, nothing but an assemblage of chemical instruments: a medico-mathematical doctrine, which explained all the phenomena of life by the sciences of number and magnitude, by algebra, geometry, mechanics, and hydraulics; and even a tribe of animists, who, finding that all the powers of inorganic nature had been invoked in vain, resorted to the world of spirits, and maintained that the soul is the only cause of life. It is amusing to observe the entire conviction and self-complacency, with which such systems are brought forward... the ardor, with which wrangling sectaries dispute about their petty divisions and sub-divisions of belief: each sect conceives itself in possession of the truth, yet probably they are all more or less counterfeit.
>
> If the seductive influence of favorite notions, and the disproportionate importance attached to particular sciences, have operated so unfavorably on the doctrines of physiology and medicine, the remedy for the evil must be sought in more enlarged views and general knowledge. We cannot expect to discover the true relations of things, until we rise high enough to survey the whole field of science, to observe the connexions of the various parts and their mutual influence.

Lawrence had, for his time, a very balanced view of life and of the fundamental methodological dilemma facing physiologists. He was not (to use our own arbitrary labels) a "mechanist"; or a "vitalist"; or a "reductionist": he was the first, and might have been one of the greatest, English biologists. I use this word advisedly, for he preferred the newly coined name *"biology"* for the "science of life" to *"physiology"* which meant the "doctrine of nature"; and he seems to have been the man who introduced this term into the English language. The term had been coined by Treviranus of Bremen for whom Lawrence had a great admiration, and whose great treatise on biology — to which Lawrence constantly refers — was still incomplete at the time of these lectures.

Mind and Body

But it was Lawrence's views on the mind-body problem which provoked the most violent attacks. These ideas were a natural extension, both of his belief that the cause of vital functions must be sought in the living tissues of the body, and of his inability

Lawrence made important contributions to the pre-Darwinian debate and saw some of the implications. He had a well-developed sense of humor, and this cartoon from Punch *1861 would have amused him.*

to conceive of any "principle" *superadded* to matter and separable from it. To talk of life as independent of an animal body, or to discuss a function without referring to the organ involved, was absurd. All this applied just as much to the notion of a "mind" separated from, or added to, "matter" as it did to "life" separated from, or added to, "matter." The whole Cartesian dualism was misconceived, unnecessary, and to any physiologist worth his salt, irrational. The doctrine of the *sensorium* had been current for nearly 200 years and Lawrence criticizes it in these terms:

> Physiologists have been much perplexed to find out a common center in the nervous system, in which all sensations may meet, and from which all acts of volition may emanate; a central apartment for the superintendent of the human panopticon. . . . That there must be such a point they are well convinced, having satisfied themselves that the human mind is simple and indivisible, and therefore capable of dwelling only in one place. Now, there are many orders of animals with sensation and volition, who have none of these parts. And this assumed unity of the sentient principle becomes very doubtful, when we see other animals, possessed of nervous systems, which, after being cut in two, form again two perfect animals. Is the immaterial principle divided by the knife, as well as the body?

The same kinds of fact, reasoning, and evidence which show digestion to be a function of the alimentary canal and motion to be the function of the muscles, show too that sensation, perception, memory, judgment, reasoning, thought — in short, all mental and intellectual functions — are the corresponding functions of the appropriate organ: to wit, the brain. Any difficulty or obscurity which afflicts this idea afflicts the former ideas equally; all the evidence which connects the living processes with the material connection in the one, applies just as forcibly in the other.

Porter and Master

Lawrence knew he would be told that the notion of *thought* was inconsistent with that of *matter;* that it was impossible to conceive how the medullary substance can perceive, remember, or judge. He acknowledged that, as yet, men *were* ignorant of these things, just as they remained ignorant of how the liver secreted bile, or how muscles contracted. But, though we might not yet know the mechanism, the constant conjunction of functions and organs was the sole ground for affirming a *necessary* connection between them.

On the other hand, if one took the prevailing view — that thought was *not* a function of the brain itself, but rather the act of an immaterial substance residing in it — the physiologist is then entitled to ask, what then does the brain do? One is presented with a curious physiological situation: an organ which, in the human, receives one-fifth of all the blood from the heart, delicately organized, nicely wrapped up in protective membranes, safely protected in a bony box, better fed, clothed, and lodged than any other part of the body, yet without any apparent function. And with devastating sarcasm, Lawrence concludes:

> [and] . . . its office, only one remove above a sinecure, is not a very honorable one: it is a kind of porter, entrusted to open the door, and introduce newcomers to [mind] — the master of the house — who takes upon himself the entire charge of receiving, entertaining and employing them.

No wonder his critics lambasted him. We know, he goes on, that thought, sensation, and all such functions of the mind must be closely related to the structure of the brain. We can see mental powers grow and develop as a child grows and develops; as the body grows older so, too, does the mind falter. The gradation of brain structure and mental faculties increases through fish, reptiles, birds, horses, elephants, dogs, monkeys, and so on up to man. Animals, too, participate in rational endowments; it is strongly suspected, he says, that a Newton or a Shakespeare excels other mortals because of more ample development of the anterior cerebral lobes. The mind of man is, thus, the more perfect exhibition of *mental* phenomena which man's more complex *anatomical* development would lead us to expect. Insanity is not a disease of an independent mind, for which moral treatment alone can be recommended, but a disease of the brain – a deranged function of a normally healthy organ. "Arguments," Lawrence says, "syllogisms, moral discourses, sermons, have never restored any patient: no real benefit can be conferred without vigorous medical treatment of the insane."

The Explosion

This was extremely strong stuff for the world of England in 1817 and the rest of the story is quickly told. The lectures were published in 1819, as were Abernethy's, and within a few months it was all over. Lawrence had managed to offend, criticize, denounce everything that the Establishment held most dear, whether it was the medical establishment, the political establishment, or the theological establishment. As the row gathered momentum during the months of 1819, one can feel the opposition and the fury snowball until one is unable to disentangle the real issues from the irrelevancies, the sound judgments from the prejudices.

The actual explosion began in the University of Cambridge. The Hulsean lecture, which is given to this day, was founded by John Hulse who provided a sum of money in order that each year, "A Christian Advocate should produce a publication which may be an answer to cavil and objections brought against organized religion...." For the year 1819 the lecturer in question, Rev. Thomas Rennell,[8] felt obliged to call attention to the mischievous tendencies of certain opinions which struck deep at the heart of all religion. We find these mischievous tendencies examined in a chapter which is cheerfully headed: "Mistaken Notions of Life and Organization; Views of M. Bichat, T. C. Morgan, and of Mr. Lawrence."* The focus of his attack is not unexpected: by insisting that all the properties of animals are located within, and *utterly* dependent on, the material tissues of the body (which are destroyed by death), these physiologists deny the possibility of the separate existence of the human soul. We have the authority of the Scriptures to tell us that on death the mind and soul separate from the body and lead an independent existence. This is fundamental Protestant doctrine, and any suggestion that the mind could not exist without its material counterpart inevitably strikes at the heart of contemporary Protestant belief. For, when we have argued ourselves out of a separate mind as an immaterial entity, we can then with equal facility argue ourselves out of a separate soul as an immaterial entity, and by a continuation of the very same logical process dismiss the possibility of the existence of Almighty God, who is also a spirit. So materialism of this sort inevitably leads to atheism; in fact, materialism and atheism go hand in hand.

Several things must be said straight away about Rennell's position. Though given the current intellectual situation his attitude is understandable, he invaded a physiological issue with theological arguments. Like so many natural theologians in the Protestant tradition, he also staked far too much on an intellectual position in a territory which was not truly his. But the trouble was that practically everyone followed Rennell's lead, and evaded those scientific questions which one day would *have* to be answered; making the issue Lawrence's supposed atheism and materialism, rather than his physiology. So by July 1819, when the two most influential literary journals of the day, the *Quarterly Review*[9] and the *British Critic*,[10] are examining the questions, we find the *Quarterly Review* beginning in this manner:

> We find our attention called by the pamphlets before us to a subject of no ordinary importance, the doctrine of materialism, an open avowal of which has been made in the metropolis of the British Empire in the lectures delivered under public authority by Mr. Lawrence... in the Royal College of Surgeons.[11]

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*N.B. He is attacking Lawrence's first lectures delivered in 1816.

In some 6,000 words Lawrence is roundly attacked. The focus of all the attacks is generally identical with Rennell's and he is chided not only for his materialism and atheism but for the misguided, pernicious influences upon him of the free-thinking physiologists of Germany and France, especially Bichat.

The defenders of Lawrence were few and for the most part felt obliged to publish anonymously, but they were at least unanimous in trying to disentangle the scientific from the theological issue and to delineate the boundary between science and religion. Moreover, in the two most brilliant defenses Rennell is attacked not for his bad science but for his bad theology. A Protestant writer presents the issue neatly: immaterialism and immortality need not be, as Joseph Priestley had pointed out, one and the same thing; we are enabled to regard thinking matter as material yet God and the soul as spirit. The Scriptures teach us morality, but they no more teach us physiology than they do astronomy.[12] And with the earlier warning of Galileo before us, theologians would be well advised to think carefully before they condemn Lawrence's book permanently to the "Index Anglicanus Expurgatorius!"

Another brilliant and amusing publication[13] comes from a Catholic, Foster of Chelmsford, who takes the same line: physiologically speaking, Lawrence is on irreproachable grounds. And Foster, too, begs people not to turn to the Bible for scientific knowledge, but to take note again of the example of Galileo "imprisoned in a dungeon for truths afterwards confirmed by Newton."[14]

But in 1819 the pressures were far too great. This was England four years after the Battle of Waterloo when every attempt at reform was still ruthlessly suppressed. The trauma of the French Revolution and especially the Terror had not passed: if anything, fears were more intense, and Englishmen were absolutely determined that in their country at least the status quo would remain. The existing climate of the time is well summed up in a second passage from the *Quarterly Review* of July 1819.

> Mr. Lawrence contends that the doctrines which he promulgates were true, and that truth ought always to be spoken . . . it is not to be justified, we must inform him, on any sound principle, that a man should, at all times and under all circumstances, give currency to opinions of every description, on the mere ground that, in his private judgment, he believes them to be true. A considerate person will always feel a certain distrust of his own opinions, and above all, he will most seriously weigh the tendency, and the probable consequences of their general reception. Apply this to the opinions maintained by Mr. Lawrence. . . . Mr. Lawrence, we apprehend, would much sooner entrust his life and property to a person who believed that he had an immortal and accountable soul, than to one who believed with him that medullary matter thinks, and that the whole human being perishes in the dissolution of the body.[15]

And whereas such views were only to be expected in France, this was England. As the *British Critic* said:

> Melancholy it is indeed to think that Bichat has mixed up with his physiological speculations, so many that are adverse to the best interests of mankind; but it must be considered as an apology for the man, even though it be no justification of his opinions, that he was only 7 years of age when the French Revolution broke out. He was consequently brought up and educated in atheism. It might, indeed, have been hoped that a man of so superior an understanding would have been above the influence of shallow sophistry, as it is, however, he is fairly entitled to allowance. With respect however to Mr. Lawrence, he certainly cannot take the benefits of the apology which may be offered for Mr. Bichat. What we mean to observe is simply this, that Mr. Lawrence was extremely wrong and censurable in supposing that because a French professor, in a country and at a time when all principles of every kind were treated with ridicule, might talk atheism to his pupils, and treat the religion of Christianity with contempt, that, therefore, an English professor may innocently, and without violating any confidence, take the same liberty.[16]

(Those of us who have been accustomed to seeing how soundly Bichat was trounced for his vitalism, find it ironic to see him here attacked for his materialism. The English critics had the wrong target. They could have attacked La Mettrie and Hartley with more justification.) The *Quarterly Review* called on the Royal College of Surgeons to force Lawrence to expunge the offending paragraphs from his book and to insist as a condition of his continuing employment as a lecturer that he strictly abstain from propagating any similar opinions. The medical journals joined in, and within one month of publication Lawrence withdrew his book and resigned his lectureship. The pressures on him must have been intense. Clearly the alternatives were either to withdraw his book, or leave his medical practice and follow the example of Joseph Priestley and emigrate to America. In the only written communication we have he spoke of his withdrawal as "a matter of expediency."* Presumably had he not resigned voluntarily, the Royal College of Surgeons might have forced him to do so. (He went on eventually to a splendid career as a medical practitioner, becoming Surgeon-General to Queen Victoria.) The pirated version of his book ran into nine editions, and we may attribute the large sale almost entirely to the supposed blasphemy. It was discussed for years, but with few exceptions the issue was the one of materialism, and Lawrence's genuine biological arguments were bypassed. The focus of the methodological debate shifted out of England back into France and it was left to Claude Bernard to give the real first and brilliant synthesis of biological experiment and biological philosophy in terms not unlike Lawrence's. Forty-four years later we find a small vignette of Lawrence written by the young Thomas Henry Huxley in the preface to his book, *Evidence on Man's Place in Nature,*[17] describing Lawrence as "one of the ablest men [he] had ever known," but he writes how Lawrence even then warned him not to broach the dangerous topic of the evolution of man.†

Clearly, whether we like it or not, the broad patterns and traditions held by a society do affect those specific aspects of the intellectual tradition which we are prepared to select and in this way a man's real contributions can be totally bypassed. Lawrence wanted to talk about biological method; his critics were only interested in his supposed blasphemy. But perhaps the whole outcome was inevitable since scientific issues do become invested with extrascientific significance and we must accept the consequences. For the isolation of scientific ideas from the rest of the community's life can be as traumatic for society as episodes like these were for an individual's career.

Since this article went to press my attention has been drawn to an interesting article by Owsei Temkin which sets this controversy in a somewhat broader framework. Happily there seem to be few differences of interpretation between us. Temkin, Owsei. *Basic Science, Medicine and the Romantic Era,* Bull: Hist. Med. xxxvi. 1963. pages 97-129.

*Letter to William Hone (bookseller), undated but on paper watermarked "1820."

†Lawrence made important contributions to pre-Darwinian discussions, particularly to the theories of inheritance. These I have not discussed here, since they were not the focus of attack on him in 1819. See also C. D. Darlington, *Darwin's Place in History,* 1960.

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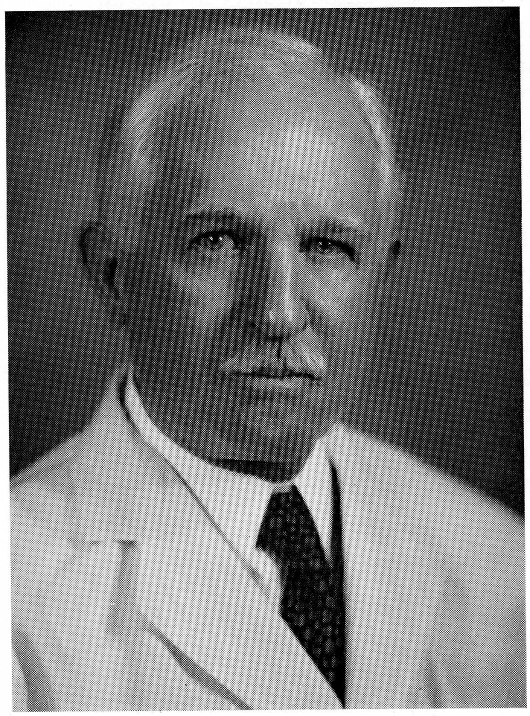
RUFUS COLE
1872-1966

It was in the spring of this year that Dr. Rufus Cole journeyed to Washington with his niece, Mrs. Ruth Moore, to receive a singular honor, the Jessie Stevenson Kovalenko Award. This medal, to be bestowed upon him by the National Academy of Sciences, was in recognition for his achievement in medical science. He made the journey for still another purpose, one close to his heart, to visit various galleries, for he was an amateur painter of no mean enthusiasm and skill. He had been in Washington but a short time when he contracted a cold which progressed with startling rapidity. On the 20th day of April he succumbed and, as fate would have it, to lobar pneumonia, the very same disease which he had studied with such brilliance and enthusiasm a half century before. His death was a loss not only to his children and grandchildren, who were with him at the time, but to his many devoted friends and to the whole scientific world as well. He was indeed a great and influential man and a person of warmth and understanding whose demise has been felt throughout the world.

Those of you who have lately come to our University should be reminded that Dr. Cole was the first Director of our Hospital. He served in this capacity from the time of its inception until his retirement in 1937. He was chosen for the post by the Board of Scientific Directors upon the recommendation of the great Dr. William Welch, then Dean of the medical school at Johns Hopkins University.

The first decade of this century was a period of transition in American clinical medicine. It was Rufus Cole who brought to our University, then The Rockefeller Institute for Medical Research, the revolutionary concept that the treatment of disease should go hand-in-hand with its study in the laboratory.

I first knew Dr. Cole in the autumn of 1924 when I was a timorous fledgling in search of a position as research assistant in the Hospital's Department of Bacteriology headed by Dr. Oswald T. Avery. I had the good fortune of serving there for two decades. There was ample opportunity to witness the astute judgements which Dr. Cole exercised in selecting the scientifically trained young physicians who were to serve as residents. Over the years there passed a procession of young men, well over a hundred in all, whose abilities led them to outstanding positions in our medical schools and universities. I knew them all and I knew them well, too. It can be truthfully said that it was the searching and critical mind of this



RUFUS COLE

modest and kindly gentleman which in no small measure helped mold these young men and brought them to the positions of leadership which they eventually achieved. I think we one and all feel a great personal loss in the death of Dr. Cole. It was he, as Director of our Hospital, who laid many of the foundation stones of our now great University.

Sir William Osler termed pneumonia the "Captain of Death." It was his pupil, Rufus Cole, and his associates who reduced this dread disease in rank.

Walther F. Goebel

CARL TENBROECK

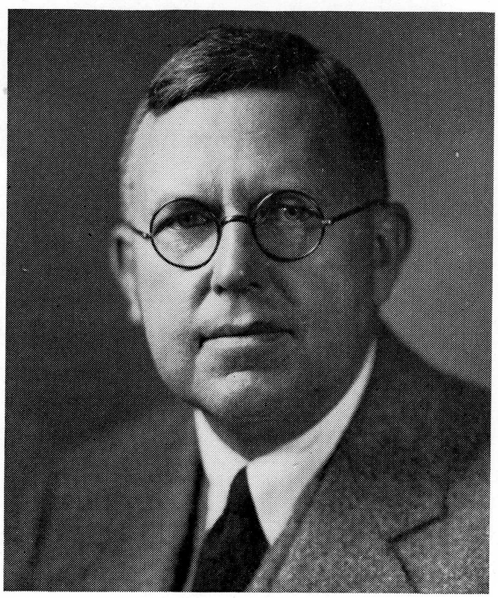
1885-1966

Dr. Carl TenBroeck was associated with The Rockefeller University for over forty-four years; twenty-nine with the Department of Animal and Plant Pathology at Princeton, where he was Director from 1930 until his retirement in 1951; and fifteen as Member Emeritus. Dr. TenBroeck died in his sleep at his home in Bar Harbor, Maine at eighty-one years of age. The lasting admiration of friends and colleagues everywhere assuages the loss felt for this distinguished leader in comparative pathology.

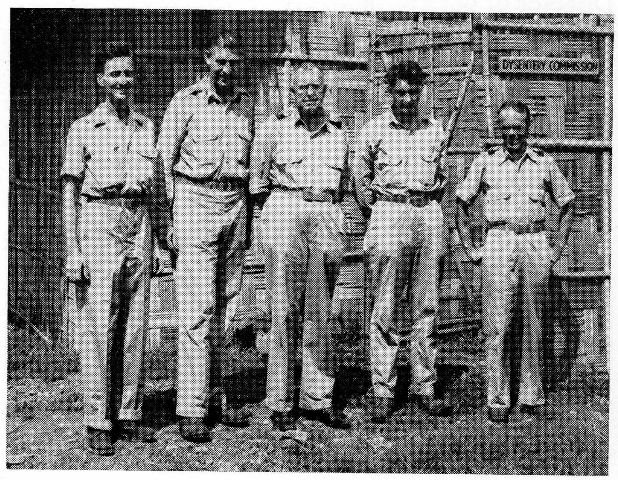
During his period as Director, the Princeton laboratories of The Rockefeller Institute for Medical Research — as the University was then called — were internationally recognized for their uniquely important contributions to biological and medical research, including pioneer work in the chemistry of enzymes and initiation of the modern study of viruses, to name only a few.

Dr. TenBroeck had come to the Institute from Harvard in 1914 as first assistant to Theobald Smith, Director of the new division of animal pathology at Princeton. His research at that time — which was on the hog cholera virus and on bacilli of the paratyphoid group — was interrupted by World War I and service in the Medical Corps. From 1920 to 1927 he was Professor of Bacteriology at the Union College of Peking where he devoted his research chiefly to the tetanus bacillus, especially with regard to immunity reactions. Meanwhile his former professor of comparative pathology at Harvard, Theobald Smith, had announced his retirement, so the Board of Scientific Directors prevailed upon TenBroeck to return to the Institute early in 1928, and appointed him Director of the Department of Animal Pathology in 1930. After settling down in Princeton, TenBroeck — working with a colleague, Malcolm Merrill, now Director, Health Service, Agency for International Development — demonstrated that the bite of a single mosquito can transmit encephalitis to a horse, and following up this clue, showed for the first time in an animal virus, that an animal virus can multiply inside the body of the mosquito.

Under Dr. TenBroeck's leadership the Princeton laboratories became the training ground for com-



CARL TENBROECK



Dr. Carl TenBroeck, center, in 1944. The Army bacteriological laboratory at Assam, India is in the background and Dr. John B. Nelson — now Emeritus — is at the extreme right. Major Gustave J. Dammin is to the left of Dr. TenBroeck, and the two enlisted men are unidentified.

parative pathologists from all over the world. He was resolute in stimulating the study of comparative pathology in invertebrates and plants as well as in vertebrates, and was a prime mover in changing the emphasis of the day from bacteriology to comparative virology and to the study of the diseases of animals as well as man.

In his years at the Princeton laboratories, scientists of outstanding ability were selected and encouraged. In this regard, the late Professor Richard E. Shope paid warm tribute to TenBroeck's influence on his own career.

During World War II Dr. TenBroeck was a consultant to the Secretary of War, and his missions to India and elsewhere with Dr. John B. Nelson — now Emeritus — not only furnished the Army with valuable information for coping with bacillary dysentery and paradysentery infections, but served to demonstrate the feasibility of bacteriological diagnosis under field conditions.

Dr. TenBroeck was awarded the Medal of Freedom — the nation's highest civilian award — and was a member of the American Academy of Arts and Sciences.

RICHARD EDWIN SHOPE

1901-1966

Professor Richard E. Shope, a greatly gifted pathologist who was the first investigator to find the cause of influenza, died on October 2. Dr. Shope was a Member of the National Academy of Sciences and of the American Philosophical Society, and recipient of many awards including the Kober Medal of the Association of American Physicians, the Albert Lasker Award, the John Philips Memorial Medal of the American College of Physicians, and the Medal of the New York Academy of Medicine. A year ago last January he received with his usual modesty the medal of the Academy of Medicine at the hands of Dr. Peyton Rous, who then remarked:

WE PHYSICIANS are all naturalists, though now often at several removes. The medal of our Academy is to be presented now to Dr. Richard E. Shope, a medical naturalist of the most immediate sort, although he deals with creatures other than man. No need to remark that he has had to become an acute diagnostician. Not a few of his achievements have been unique, and I have been asked to tell of them.

Influenza had been sweeping again and again across the world and nobody knew its cause until, in 1931, Dr. Shope discovered that in swine this disorder is due to a virus acting in association with a bacillus. This soon led to the demonstration by English workers that human influenza is also caused by a virus and that it is conditioned by the same bacillus. Now, at last, the influenzas could be coped with in an understanding way.

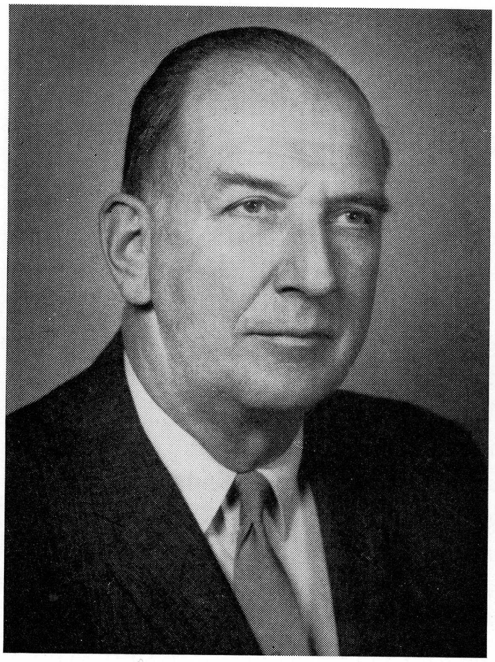
Thus did Dr. Shope begin the huge task at which he has toiled ever since: to find and control the virus diseases of animals. He has dealt not only with diseases of the farm but of wild creatures as well. Within two years after his work on influenza he found in the cottontail rabbits of the Southwest the first virus causing tumors in a mammal. This discovery still resounds, as bearing directly on the cancer problem. For the virus not only induces and actuates benign tumors of the epidermis, but it initiates carcinomas that originate from the cells of these growths. The parallel with the consecutive neoplastic changes occurring in certain human instances of cancer is complete.

By the time the United States entered World War II Dr. Shope had come upon still more virus causes of disease in animals, and he was already renowned. He had enlisted promptly and was a Lieutenant Commander in the Navy when summoned by our Secretary of War, who was concerned lest the enemy scatter throughout our land the virus of rinderpest,

These remarks by Dr. Rous first appeared in the *Bulletin of the New York Academy of Medicine* and are reproduced here with its permission.

the worst of cattle plagues, and he asked Dr. Shope to try to make a vaccine to prevent the disease.

Dr. Shope took on the task forthwith. It meant bringing the rinderpest virus into this country for test purposes. Where could this safely be done? The virus was so infectious that he decided it must be at a site where not only his scientific equipment but his staff and himself would be completely isolated. So, with the concurrence of Canadian authorities — for the project had become a joint enterprise — his laboratories, living quarters, and cattle pens were built on an island, Grosse Ile, situated in the St. Lawrence River below Quebec. In earlier days the island had been used for quarantine against typhus, but now it



RICHARD EDWIN SHOPE

was a desolate spot. Throughout more than a year and a half Dr. Shope and his associates were marooned there, and then they emerged with a vaccine that has since saved many multitudes of cattle in distant regions wherever rinderpest has threatened. But this rescue did not happen of itself. Dr. Shope had to travel far and wide to ensure that all went right.

A few years later he made another vaccine that had important social consequences — this time of a quaint sort. Many may recall seeing, not many years ago, from the window of the train from Southampton to London, wide stretches of land that was good for nothing because it was riddled with burrows that looked as if made by crowded, giant moles. Actually they were made by rabbits, and this was but a sample of a condition present elsewhere in England and Scotland; it had always existed within the memory of man. But shortly after World War II a South American virus deadly to rabbits was purposely set free in England by a British pathologist, and thus the riddled land was rescued, as were also immense expanses in Australia.

The story was different in France, where wild rabbits are a traditional food of the farmer and large rabbitries are maintained to provide them. Here the introduction of the South American virus spelled disaster, for through carelessness it had been let in. Soon it had destroyed most of the rabbits. But again Dr. Shope was ready. He had come upon a virus in American cottontails that produced an almost harmless, transient disease, yet which appeared related to the South American virus in some respects; and with a premonition of later need he had devised a vaccine with it that protected rabbits completely against this scourge. Through the Pasteur Institute his vaccine was supplied to all the French rabbitries and, as a result, the farmer again has his traditional food.

Not long ago Dr. Shope found a second benign-tumor virus, this time in wild American deer, and also another virus that causes a fatal hemorrhagic disease. Of late he has been engaged much of the time by the government in determining whether meat from countries where foot-and-mouth disease prevails in cattle shall be let into the United States. The financial welfare of several nations would be much furthered were this done; but the problem remains knotty. At present, Dr. Shope is searching for the intermediate host of hog cholera which has become epidemic in some parts of the United States.

Dr. Shope's theme throughout his scientific life has been the meaning of animal diseases for mankind, though he would never say so. Yet not alone from this theme has he drawn his wisdom and his strength. He knows human nature well. How does he value his fellow creatures including ourselves? Much as he does the lower animals — with understanding, indulgence, humor, and love.

Richard Shope, this medal is a token not only of our admiration, but of our congratulations as well. For the facts that you have brought to light will shine on into the future.

BARKLIE McKEE HENRY

1902-1966

BARKLIE MCKEE HENRY, Trustee of The Rockefeller University since 1949, died at Blue Mountain Lake, New York on September 4, 1966. Mr. Henry was graduated *cum laude* from Harvard in 1924 and also spent a year at Balliol College. In 1926, he became managing editor of *Youth's Companion* and in 1927 joined The Guaranty Company of New York. In 1930, he was forced by the death of his parents to retire from business and give full attention to the affairs of his family and to his manifold interests in projects for the advancement of human welfare.

In the earliest days of the depression he was active in the Emergency Unemployment Relief Organization in New York and set up that group's Block Community Organization. He later became President of the New York Association for Improving the Conditions of the Poor and organized the merger of that group with the Charity Organization Society to form The Community Service Society. He was the first President of The Community Service Society, the largest private family welfare organization in the United States. He was also one of the organizers of the Greater New York Fund.

Mr. Henry found great satisfaction in assisting work in the fields of medicine and psychiatry. He was a Governor of The Society of the New York Hospital, and later its Vice-President and President. He strongly supported measures encouraging a rapprochement between medicine and psychiatry and was active in strengthening the psychiatric services of New York Hospital. He served on the Board of Managers of the New Jersey State Diagnostic Center at Menlo Park and was a member of the New Jersey State Board of Control of Institutions and Agencies. In 1958 he was appointed by the Governor of New Jersey to be a member of a commission to study the Department of Institutions and Agencies, a body responsible for all of the state's correctional activities, mental hospitals and clinics, and welfare activities.

Mr. Henry had been Trustee of the Pierpont Morgan Library, The American Academy in Rome,

BARKLIE McKEE HENRY

The Institute for Advanced Study in Princeton, the John Hay Whitney Foundation, and The Cooper Union. He was Chairman of the Executive Committee of the Carnegie Institution of Washington, a Member of the Overseers' Committee to Visit Harvard College, a member of Princeton University Advisory Council to the Department of Philosophy and a director of the Milbank Fund. During the second World War, Mr. Henry was on active service with the Navy and commanded a coastal minesweeper, a sub-chaser, and finally a destroyer escort. He was called upon by Secretary Forrestal to help with contract terminations and also helped to draft the report of the Dubose Board on relationships between regular and reserve officers in the Navy. After the war he served on a Civilian Advisory Committee to the Bureau of Naval Personnel.

Mr. Henry was a trustee of The Rockefeller University for 19 years. In 1951, he was appointed to the committee of trustees that reviewed the fundamental policies of the institution and formulated the plans for creation of the modern Rockefeller University.

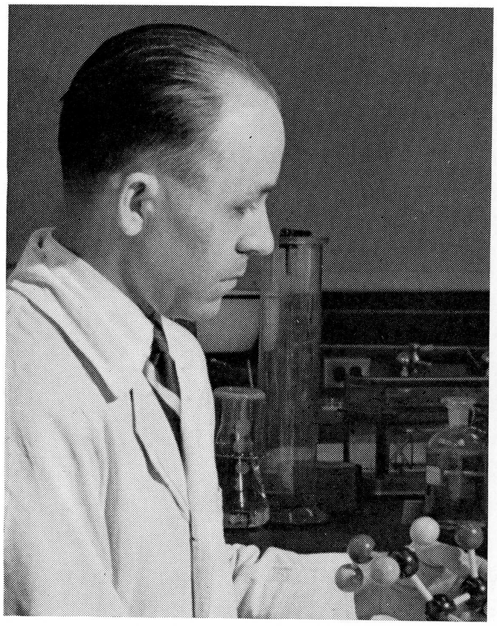
During reconstruction of the five existing buildings, the construction of eight new buildings, and the landscaping of the campus, Mr. Henry was chairman of the Committee on Buildings and Grounds and gave constant support to President Bronk throughout those ten years of great development.

In speaking of his friend, Dr. Bronk recently said: "I think with gratitude of Barklie Henry's many gifts: his ever loyal friendship that was enriched by wise advice and encouragement when encouragement was needed and deserved; his always cheerful spirit; his broad tolerance; his perceptive discrimination between trivial and significant matters; his wide knowledge of which he gave generously to the many institutions he served selflessly; the noble example of a truly good man who made all who knew him better."

DILWORTH WAYNE WOOLLEY

1914-1966

PROFESSOR D. WAYNE WOOLLEY – pioneer scientist in the study of vitamins and antimetabolites – died on July 23 high in the Andes Mountains of Peru. To the end, his deep devotion to science and his acute sense of history were with him as he sought to view at first hand remnants of the ancient civilization of the Incas before continuing on to a scientific meeting in Brazil.



DILWORTH WAYNE WOOLLEY

Dr. Woolley's work as scientist and scholar was reflected in his writing, in his seminal ideas and discoveries, and in rich conversations his friends remember so well. He was not content to undertake the obvious and the routine in the laboratory – "Life is too short for that," he often said – his joy and satisfaction were found in breaking new ground where others were not wise enough or brave enough to venture.

In the earliest days of the vitamins and the antimetabolites he showed his leadership by his originality and his incisiveness. While still a graduate student at the University of Wisconsin, for example, he succeeded in isolating and characterizing nicotinamide as the preventive agent in canine black tongue disease, a finding which preceded recognition of nicotinamide as the human anti-pellagra vitamin. In 1939 he came to the University – then The Rockefeller Institute for Medical Research – and his interest in the vitamins continued with the first demonstration that an anti-vitamin, pyrithiamine, could be effective in animals. He was thus among the earliest to appreciate the importance of the antimetabolites and was soon received as a world authority on the subject. In his book, *A Study of Antimetabolites*, written in 1952, is a passage on how scientific principles are discovered. Invariably, he suggests, such discoveries result from one man's capacity to draw unified meaning from isolated and diverse facts. This was one of his own best qualities.

He clearly demonstrated this ability during his extensive investigations on serotonin. Out of his work on the role of the hormone in hypertension came his recognition in 1954 that serotonin was intimately involved in mental processes. This opened a vast new field to which his second book, *The Biochemical Basés of Psychoses,* serves as a guide. Dr. Woolley's most recent undertaking was the very difficult problem of the nature of hormone receptors, but this work was cut short before the full significance of his findings could be developed.

In recognition of his achievements Dr. Woolley twice received the Eli Lilly Award – once in bacteriology and once in chemistry – the Mead Johnson Award, and the American Pharmaceutical Association Award. He was a member of the National Academy of Sciences and the American Academy of Arts and Sciences. He was granted an honorary M.D. degree from the University of Amsterdam. In 1958 the University of Alberta, from which he had been graduated with a B.S. degree some 23 years before, presented him with an honorary LL.D. degree.

Further qualities which set Dr. Woolley apart from others were his great courage and determination. A scientific pioneer who did not tread the beaten path, he was often exposed to criticism but stood up squarely to opposition. Although suffering from severe diabetes since early childhood and from total blindness just as his scientific career was beginning, he carried on with vigor and enthusiasm. I recall so well being shown for the first time the beautiful view from the porch of his home overlooking the Hudson Valley. Not one detail was missing – from the reflections on the river below and the glimpse of Hyde Park on the south, to the tanager perched in a tree nearby.

His many friends and colleagues will remember and will miss this intrepid scientific discoverer and devoted scholar. ROBERT B. MERRIFIELD

NEW STUDENTS

THIRTY-THREE new students from twenty-seven colleges and universities were accepted as candidates for the degree of Doctor of Philosophy:

ANDREW E. BALBER, B.A. Haverford College

SAMUEL D. BALK, A.B. Wesleyan University, M.D. New York University School of Medicine

EDWARD J. BENTZ, JR., B.S. Rensselaer Polytechnic Institute

STEVEN BLAHA, B.S. University of Notre Dame

DOMINICA BORGESE, University of Paris, Faculty of Science

BYRON T. BURLINGHAM, B.A., M.S., M.D. State University of Iowa

RICHARD M. CARON, B.A. Florida Atlantic University

BRUCE A. DINER, B.S. The City College of the City University of New York

ROBERT J. DONAGHEY, S.B. Massachusetts Institute of Technology

DONALD A. ELLIOTT, B.A. Texas Technological College, M.D. Baylor University College of Medicine

NINA V. FEDOROFF, B.S. Syracuse University

CRAIG I. FIELDS, S.B. Massachusetts Institute of Technology

SAIMON GORDON, M.B., Ch.B. University of Cape Town

PETER O. GRAY, A.B. Columbia College

FRANCISCO GRUNBAUM, Lic. Math. University of Córdoba

HARRY WMS. HARPER, A.B. Trinity College of Duke University

CARL D. HOPKINS, A.B. Bowdoin College

SALLY C. HOUSEHOLDER, B.A. Wellesley College

L. MARTIN JERRY, M.D. University of Toronto, Faculty of Medicine

MARC W. KIRSCHNER, B.A. Northwestern University

STUART L. LAIKEN, B.S. The University of Chicago

MARY LEE STEWART LEDBETTER, B.A. Pomona College

HENRY A. LESTER, A.B. Harvard College

PAUL M. LIZARDI, B.S. University of Puerto Rico

CHRISTIAN MATHOT, BEI, Ecole Nationale de Chimie

PATRICK E. O'NEIL, S.B. Massachusetts Institute of Technology, M.S. The University of Chicago

SETH J. PUTTERMAN, B.S. California Institute of Technology

ROLLIN C. RICHMOND, A.B. San Diego State College

HWA-LING SZU, B.Sc. Taiwan Provincial Cheng Kung University, M.S. University of Detroit

ALAN M. TARTAKOFF, A.B. Harvard College

ZENA WERB, B.Sc. University of Toronto

R. HAVEN WILEY, JR., A.B. Harvard College

RICHARD E. ZIGMOND, A.B. Harvard College

As an undergraduate at Dartmouth, Dr. McKean recalls, his interest in mathematics and particularly statistical mechanics, won out over oceanography only in his last year. Then at Princeton and during his first years at MIT, the focus of his research interests gradually shifted to more abstract mathematics and to probability theory. More recently he has taken up the study of statistical mechanics again. Dr. McKean is a member of the American Academy of Arts and Sciences, Editor of the *Transactions of the American Mathematical Society,* and co-author with K. Ito of the book, *Diffusion Processes.*

HENRY P. McKEAN, JR.

NEW FACULTY

THIRTY-ONE new appointments to the faculty were announced by President Bronk in September. These appointments include four mathematicians, four psychologists, three cytologists, two philosophers, two physicists, and three biologists.

Henry P. McKean, Jr., has been appointed Professor. Dr. McKean, a mathematician, received the Ph.D. degree from Princeton University in 1956. After a year as a Fulbright Fellow at the University of Kyoto, Japan, he became Assistant Professor of Mathematics at the Massachusetts Institute of Technology, and by 1963 progressed to Associate Professor and Professor.

Doctors I. Arthur Mirsky, Ernest Nagel, Hao Wang, and Tai Tsun Wu have been appointed Visiting Professors.

New appointments to Associate Professor include Marshall Cohen, Paul Cranefield (also appointed Editor of *The Journal of General Physiology),* Richard M. Krause, and Norton Spritz. Laura F. Garnjobst has been appointed Emeritus Associate Professor.

Visiting Associate Professors will be Howard E. Conner, Zbigniew J. Ciesielski, David C. Glass, and Virendra Singh.

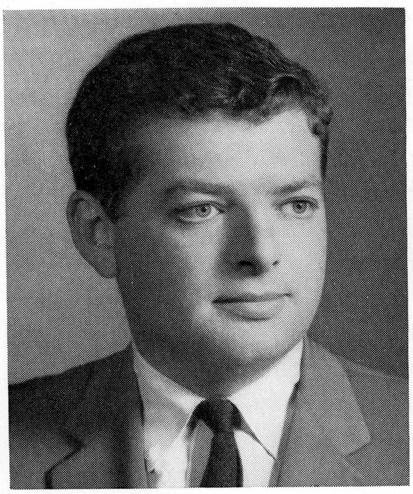
New Assistant Professors are Gordon G. Ball, Rudy A. Bernard, Walter H. Doerfler, Martha Fedorko, Huminori Kawata, Robert S. Lees, Stephen D. Litwin, Bruce S. McEwen, Michael F. Moody, Miklós Müller, David D. Sabatini, Thomas T. Struhsaker, Robert E. Webster, Steven H. Weisbroth, and George Wolf.

The appointments of Peter Marler and Neal E. Miller as Professors, and Roger S. Payne and Richard L. Penney as Assistant Professors were announced in the *Review* for January-February 1966.

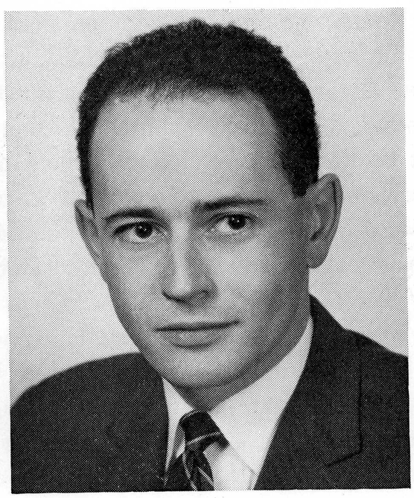
FACULTY PROMOTIONS

SIX MEMBERS of the faculty of The Rockefeller University — Doctors Zanvil A. Cohn, Gerald M. Edelman, Robert B. Merrifield, Floyd Ratliff, Alexandre Rothen, and Philip Siekevitz — have been promoted to the rank of Professor.

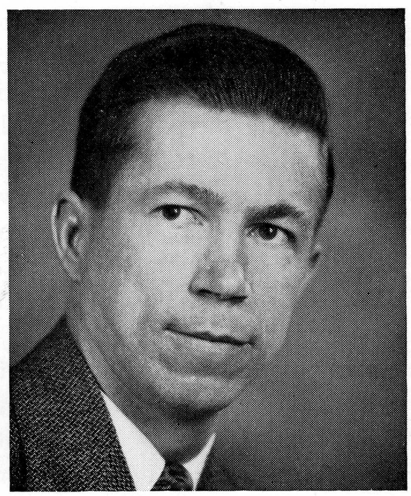
Professor Zanvil A. Cohn joined Rockefeller in 1957 as a Research Associate in Dr. Dubos' laboratory and as Assistant Physician to the Hospital. He progressed to Associate Professor and Physician in 1962. Dr. Cohn was born in New York City and received the M.D. degree *summa cum laude* at Harvard in 1953, serving his internship at Massachusetts General Hospital. He was made Assistant Resident Physician the following year, and in 1955 entered the U. S. Army as Chief of the Rickettsial Biological Section of the Walter Reed Army Institute for Research.



ZANVIL A. COHN



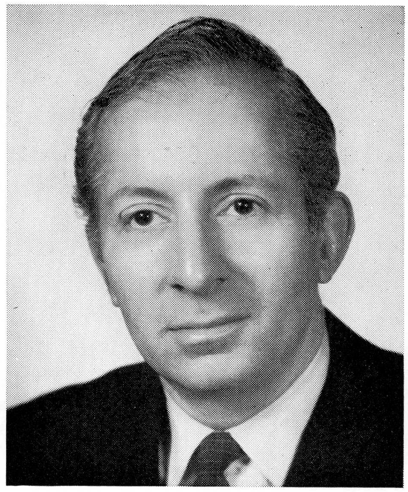
GERALD M. EDELMAN



ROBERT B. MERRIFIELD

FLOYD RATLIFF

ALEXANDRE ROTHEN



PHILIP SIEKEVITZ

Professor Cohn's special area of research interest at the University has been cellular immunology and experimental pathology, including study of phagocytic cells. He is currently engaged in studies of pinocytosis and the formation of lysosomes, factors involved in host defense mechanisms. Dr. Cohn received the Boylston Medal from Harvard in 1961, and is Associate Editor of the *American Journal of Epidemiology.*

Professor Gerald M. Edelman received the M.D. degree at the University of Pennsylvania and joined Rockefeller as a Graduate Fellow and Assistant Physician to the Hospital in 1957. He received the Ph.D. degree in 1960 and for three years was Associate Dean of Graduate Studies. In his research activities at the University Dr. Edelman has been interested in the structure of antibody molecules. While still a graduate student he showed that immunoglobulin molecules consist of several types of polypeptide chains – a finding which has clarified the relationship between the structure and biological activity of antibodies. In the past five years he has gained international recognition for his work on myeloma globulins and Bence-Jones proteins, and in 1965 he received the American Chemical Society Award in Biological Chemistry sponsored by Eli Lilly and Company.

Professor Robert B. Merrifield is best known for methods he has developed recently, in collaboration with his associates, for the automated synthesis of peptides – a discovery with broad implications for medical treatment and research which are yet to be fully explored. Dr. Merrifield came to the University as an Associate in Biochemistry in the fall of 1949, from the University of California at Los Angeles where he had received the Ph.D. degree earlier in the same year. It was in the laboratory of M.S. Dunn at UCLA that Dr. Merrifield's early interest in analytical methods for determining amino acids first developed. In the laboratory of Dr. Woolley at Rockefeller, Dr. Merrifield's research interests logically expanded into work on peptide growth factors, and from growth factors to chemical synthesis of the compounds. More recently his work has focused on insulin synthesis.

Professor Floyd Ratliff, a physiological psychologist, is noted for his work on the physiology of vision, and in particular the spatial and temporal properties of mutual inhibitory interactions of elements in the retina. Earlier this year he was elected to membership in the National Academy of Sciences and received The Howard Crosby Warren Medal of the Society of Experimental Psychologists. Dr. Ratliff received the M.S. and Ph.D. degrees from Brown University. Before coming to Rockefeller in 1954, Dr. Ratliff was a National Research Council fellow in Dr. Hartline's laboratory of Biophysics at the Johns Hopkins University and then an Assistant Professor of Psychology at Harvard University. Dr. Ratliff served as an Artillery Officer in The United States Army from 1941 to 1945; he is the author of the book, *Mach Bands: Quantitative Studies on Neural Networks in the Retina.*

Professor Alexandre Rothen joined The Rockefeller University as Assistant in Biochemistry in 1927. He was born in Geneva, Switzerland and received the Ch.E. and Ph.D. degrees in 1924 and 1925 from the University of Geneva. For the next three years he continued at Geneva as Assistant in Physical Chemistry in the University and also for a while in the laboratories of the Radium Institute. Dr. Rothen's research interest has been stereochemistry, rotatory dispersion, and the interaction of biologically important molecules at interfaces and the nature of the forces involved. He is author with P. A. Levene of a chapter on rotatory dispersion in the book *Organic Chemistry* edited by Henry Gilman, and also the author of a chapter on surface films technique in the book, *Physical Techniques in Biological Research.*

Professor Philip Siekevitz came to Rockefeller as an Assistant in Cytology in 1954. He had received the Ph.D. degree from the University of California at Berkeley in 1949, and for the next three years was a U. S. Public Health Service Research Council Fellow at the Huntington Laboratories of Harvard University, where he worked under P. C. Zamecnik and was the first to show that protein synthesis could be studied by isolated cellular organelles. On leaving Harvard he joined the McArdle Laboratory of the University of Wisconsin and worked from 1951 to 1954 with V. R. Potter. Dr. Siekevitz' research interests at Rockefeller have been in the areas of protein synthesis, secretion, and the relation of cellular function to cellular structure. He is President of The American Society for Cell Biology, member of the Molecular Biology Study Section of the National Science Foundation, co-author with Dr. A. Loewy of the book, *Cell Structure and Function,* and advisory editor to *Archives of Biochemistry and Biophysics* and the *Journal of Cellular Physiology,* and *Bioscience.*

Faculty promotions to Associate Professor include Doctors David J. L. Luck and Bruce R. Voeller. Newly promoted Assistant Professors are Doctors Stuart Brody, Lawrence Eisenberg, Larry Harper, Clyde D. Hill, Virginia C. Littau, William O. McClure, Bruce McCutcheon, and Angel O. Pogo.

THE ROCKEFELLER UNIVERSITY

NEWS

Dr. Peyton Rous is shown at the press conference in Caspary Auditorium on October 13, the day after the Karolinska Institute in Sweden announced that he would receive the 1966 Nobel Prize in Medicine. The presentation—to be made in Stockholm December 10 by King Gustav VI Adolph—will be reported in the next issue of the *Review.* Sharing the prize of $60,000 and also receiving a gold medal is Dr. Charles Brenton Huggins, Director of the Ben May Laboratory for Cancer Research at the University of Chicago.

Nobel Laureates
Associated with The Rockefeller University

Peyton Rous	1966
Edward L. Tatum	1958
Fritz Lipmann	1953
John H. Northrop	1946
Wendell M. Stanley	1946
Herbert S. Gasser	1944
Karl Landsteiner	1930
Alexis Carrel	1912

Trustees

Vincent du Vigneaud	1955
George H. Whipple	1934
Edgar Douglas Adrian	1932

¶ The Estate of Duncan MacInnes, of which the University was the sole beneficiary, included his summer home at Fort Montgomery overlooking the Hudson River and adjoining the extensive open land of the United States Military Academy. The Trustees of the University decided not to sell the property at this time, but to test the desirability of retaining it as a weekend resort for faculty and students. During the first five months in which the MacInnes House was thus made available, it was used by 180 students, faculty, and their families.

¶ During this autumn the corporate associates of the American Institute of Physics and of the American Chemical Society held their annual meetings in Caspary Auditorium; the University of Pennsylvania Medical Alumni dinner in honor of General Sarnoff and the Orton Society annual dinner, and various committees and panels of the National Academy of Sciences, the President's Science Advisory Committee, the American Association for the Advancement of Science, and the Health Research Council of New York City met in the conference rooms of Caspary and Abby Aldrich Rockefeller Halls.

¶ At the bicentennial celebration of Rutgers University and at the 75th anniversary celebration of the California Institute of Technology, The Rockefeller University was represented by President Bronk. Professor Ratliff represented the University at the inauguration of Ray Lorenzo Heffner as President of Brown University; Vice President McCarty at the inauguration of John S. Toll as President of the State University of New York at Stony Brook; alumnus C. Peter Wolk at the inauguration of S. Douglas Cornell as President of Mackinac College, and President Bronk at the inauguration of Howard Wesley Johnson as President of the Massachusetts Institute of Technology.

¶ The first of fourteen concerts in The Rockefeller University Concert Series for 1966-1967 was performed by the Smetana String Quartet on October 12th.

¶ President Bronk has been elected a member of the 24-man Board of Managers of The Franklin Institute. The Institute was founded in 1824 to perpetuate Benjamin Franklin's ideals for the study and promotion of the mechanic arts and applied science. In its monumental building on the Benjamin Franklin Parkway in Philadelphia, there is housed one of the world's greatest museums of science and technology. The New York Hall of Science, of which Dr. John R. Dunning, Dean of the School of Engineering at Columbia, is President, and Dr. Bronk is Vice President, is being patterned after the museum of The Franklin Institute.

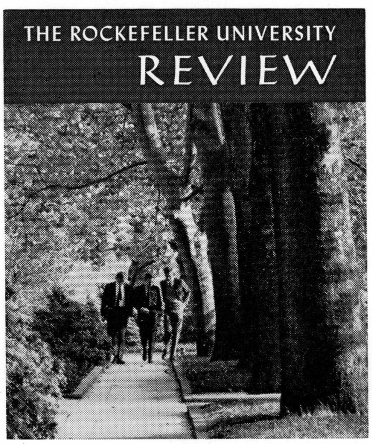
¶ Vice President McCarty has been elected a member of the Board of the New York Community Trust, an organization established in 1923 to manage an unlimited number of funds for charitable purposes. Lindsley F. Kimball, a Trustee of the University, is also a member.

¶ The 1966-67 exhibits in the gallery of Caspary Auditorium opened during October with a display of Egyptian art on loan from The Metropolitan Museum of Art and from Professor Samuel Goudsmit.

¶ Bertil Hille and Larry Simpson are two of 12 life scientists among 45 recipients of North Atlantic Treaty Organization (NATO) Postdoctoral Fellowships in Science, according to an announcement of the National Science Foundation and the Department of State. Mr. Hille will spend a year in Cambridge University in the laboratory of Professor Alan Hodgkin following the completion of his graduate studies at the Rockefeller; Mr. Simpson will use his Fellowship in the laboratory of Jean Brachet at the Free University of Brussels.

¶ Among the faculty receiving honorary degrees from other universities and colleges this fall were Dr. René Dubos, Doctor of Science, Carleton College, and Dr. Paul Weiss, Doctor of Medicine and Surgery, University of Helsinki.

¶ New members of the faculty and new students were welcomed at a reception given by President Bronk and the University Trustees on October 17. This year over 400 were present in Welch Hall for this traditional fall gathering, including many of the Trustees, who had held their meeting on the same day.



THE COVER shows first-year Graduate Fellows on a fall morning strolling past the Abby. Photographed by Joseph Barnell.

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