Fall 1963

The Rockefeller Institute Review 1963, vol. 1, no. 4

The Rockefeller University

THE ROCKEFELLER INSTITUTE

FALL 1963 **REVIEW**

The Rockefeller Institute Review, Fall 1963. The Review is issued bimonthly. This is volume 1 number 4. Published by The Rockefeller Institute, Sixty-sixth Street and York Avenue, New York, New York 10021. Second class postage paid at New York, New York.

REMINISCENCES ABOUT NIELS BOHR

BY A. PAIS

As a very young man, Pais went from Holland to work with Niels Bohr. From then until last year, the two were close friends in Copenhagen and at the Institute for Advanced Study in Princeton. This deeply personal account of friendly associations was prepared for a memorial volume of the immortal scientist, vital human, and loyal friend. It is especially appropriate that it be published in The Rockefeller Institute Review *because it was at the Institute that Bohr received from a friend of forty years the last of his many honorary degrees a few months before his death.*

Professor Pais joined the faculty of The Rockefeller Institute this year after thirteen years as Professor of Physics at the Institute for Advanced Study. He had held that high post from the age of 32.

In January, 1946, I went for the first time to Copenhagen from my native Holland, as a Rask Oersted fellow. I was the first one of the postwar generation to come to Bohr's Institute from abroad for a longer period of study. The morning after my arrival I went to Mrs. Schultz who told me to wait in the journal room adjoining the library where she would call me as soon as Professor Bohr was free to see me. I had sat there reading for a while when someone knocked at the door. I said, "Come in." The door opened. It was Bohr. And my first thought was, what a somber face.

Then he began to speak.

I have later often been puzzled about this first impression. It vanished the very moment Bohr started to talk to me that morning, never to return. True, one might correctly describe Bohr's physiognomy in repose as unusually heavy or rugged. Yet Bohr's face is remembered by all who knew him for its intense animation and its warm and sunny smile. Only once did I hear of another observation not entirely dissimilar to my own first superficial one. That summer, on the beach in Tisvildeleje, Bohr's old aunt, Miss Hanna Adler, was to tell me of an experience she once had long ago when she sat in a Copenhagen streetcar together with Bohr's mother and the two young boys, Harald and Niels. The sons were hanging on their mother's lips as she was telling them a story. Apparently there was something peculiar about these two young faces in concentration, for Miss Adler overheard one lady in the streetcar remark to her neighbor, "Stakkels Mor [That poor mother]."

I did not see much of Bohr during the next month or so. After a brief trip to Norway he was very busy with plans for the extension of his Institute. However, I was soon invited for Sunday dinner at Carlsberg, and that evening I had my first opportunity to talk physics with Bohr in his study. I told him of things I had worked out during my years in hiding in Holland. This concerned the self-energy problem in quantum field theory. Briefly, in such a theory one considers a basic particle, like the electron, as an object without extension, a point. This leads to the apparent difficulty that the electron thereby acquires an infinite energy due to the electromagnetic field which it generates. At the time I was concerned with the question whether such infinities could be compensated by a hypothetical coupling of the electron to another field of short range. While I was telling Bohr about this, he smoked his pipe, looked mainly to the ground and would only rarely look up at the blackboard on which I was enthusiastically writing

This essay will appear in a memorial volume about Niels Bohr to be published in Denmark.

down various formulae. After I finished, Bohr did not say much and I left a bit disheartened with the impression that he could not care less about the whole subject. I did not know Bohr well enough at the time to realize that this was not entirely true. At a later stage I would have known right away that Bohr's curiosity was aroused, as he had neither remarked that this was very, very interesting, nor that we agreed much more than I thought.

World Politics

After this discussion we went back to the living room to rejoin the company. Then, as on later occasions, I felt fortunate to be for a while in the invigorating atmosphere of warmth and harmony which Mrs. Bohr and her husband knew to create wherever they were in the world, but above all in their home. The conversation now turned to more general topics, and that evening I caught a first glimpse of Bohr's intense preoccupation with the problems of the international political scene. In this area all his thoughts were focused on one central idea, the unique opportunities for an open and peaceful world due to the advent of atomic weapons. I shall not dwell upon this subject, which will be commented on with much more competence by others. All I wish to record here was the deep impression which Bohr's sense of urgency on this issue made on a young man who just had emerged from life in occupied Europe. "The release [by the U. S.] of atomic data for purely scientific purposes is but a side issue. The essential point is the political issue. The current political problems of Poland, Iran, etc., however important, are but side issues." Such remarks may now seem obvious. They were not at all so widely accepted then. On such topics, as in matters scientific, Bohr's strength lay in the single-minded pursuit of one given theme. At the time, he was still optimistic that in not more than a year or two such views as he then expressed would find acceptance by the governments most vitally concerned.

It would be wrong to suppose, however, that an evening at the Bohrs' was entirely filled with the discussion of such weighty matters. Sooner or later, for the purpose of illustrating some point or just for the pleasure of it, Bohr would tell one or more stories. I believe that at any given time Bohr had about half a dozen favorite jokes. He would tell them, we would get to know them. Yet he would never cease to hold his audience. For me, to hear again the beginning of such a familiar tale would lead me to anticipate not so much the denouement as Bohr's own happy laughter upon the conclusion of the story. At that time he had one joke of which, for the life of me, I cannot remember the thread but only the punch line. "The question is not whether the Irish are human, but whether the humans are Irish."

Shortly thereafter, on March 2, the twenty-fifth anniversary of the Institute was celebrated. True to Bohr's style, it was an intimate occasion, the high point of which came as Bohr reminisced about the people and the events of that heroic period. There was no pomp, only a few brief speeches. It was my pleasant task to express the gratitude of the first installment of postwar visitors from abroad. And, of course, that evening there was a feast of Parentesen, the graduate students' club. It was the time I learned to sing "Videnskabens Faedre [The Fathers of Science]," of which the last stanza is to be rendered while the participants stand on their chairs, beer in hand: "Nobelmanden Niels Bohr, ved vej blandt alle vildspor. . . . [. . . knows the way amidst all false tracks. . . .]." It gave us all a sense of pride to have Bohr in our midst that moment.

During the following weeks it became clear that Bohr had gotten quite interested in the problems of quantum field theory which I had mentioned to him. Every now and then he would call me to his office to have me explain one or the other aspect of them. He was particularly intrigued by those arguments which showed that many elementary particle problems (such as the self-energy question mentioned earlier) are fundamentally quantum problems which cannot be dealt with by the methods of classical physics. It may be noted that this view did not have as wide an acceptance at that time as was to be the case two years later when the modern version of field theory known as the renormalization program started to develop.

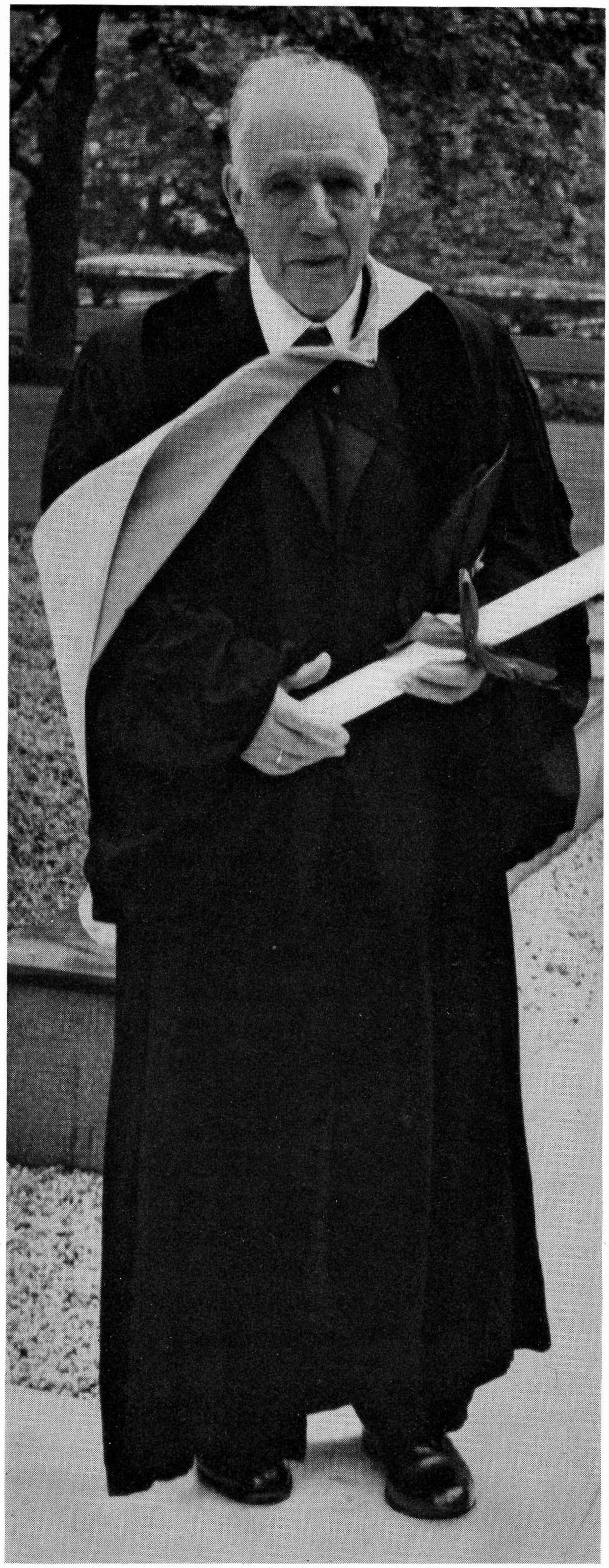
Meanwhile, I had become involved in several other enterprises that went on at the Institute. I would like to mention one of them, work with Hulthén on nucleon-nucleon scattering, as it is interesting to recall how bold we felt in extending the numerical work to the unheard-of energy of 25 Mev.

Then, one day in May, Bohr asked me whether I

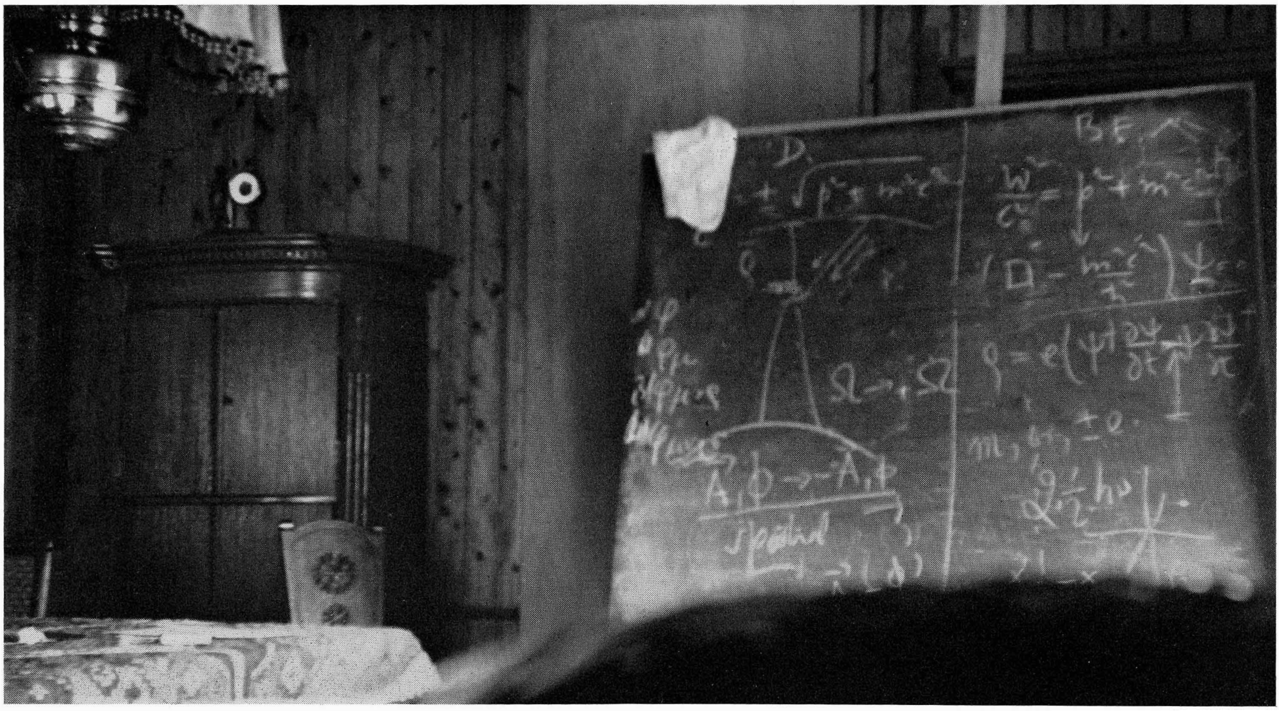
would be interested to work together with him day by day for the coming months. I was thrilled and accepted. The next morning I went to Carlsberg. The first thing Bohr said to me was that it would only then be profitable to work with him if I understood that he was a dilettante. The only way I knew to react to this unexpected statement was with a polite smile of disbelief. But evidently Bohr was serious. He explained how he had to approach every new question from a starting point of total ignorance. It is perhaps better to say that Bohr's strength lay in his formidable intuition and insight, not at all in erudition. I thought of his remarks of that morning some years later, when I sat at his side during a colloquium in Princeton. The subject was nuclear isomers. As the speaker went on, Bohr got more and more restless and kept whispering to me that it was all wrong. Finally he could not contain himself and wanted to make an objection. But after having half raised himself, he sat down again, looked at me with unhappy bewilderment and asked, "What is an isomer?"

Sparring Partner

The first subject of work was the preparation of Bohr's opening address to the International Conference on Fundamental Particles to be held in July in Cambridge, England. It was the first meeting of its kind of the postwar era. Bohr planned to make a number of comments on the problems of quantum field theory, alluded to earlier. I must admit that in the early stages of the collaboration I did not follow Bohr's line of thinking a good deal of the time and was in fact often quite bewildered. I failed to see the relevance of such remarks as that Schroedinger was completely shocked in 1926 when he was told of the probability interpretation of quantum mechanics, or a reference to some objection by Einstein in 1928, which apparently had no bearing whatever on the subject at hand. But it did not take very long before the fog started to lift. I began to grasp not only the thread of Bohr's arguments but also their purpose. Just as in many sports a player goes through warming-up exercises before entering the arena, so Bohr would relive the struggles which it took before the content of quantum mechanics was understood and accepted. I can say that in Bohr's mind this struggle started all over every single day. This, I am convinced, was Bohr's inexhaustible source of identity.



Niels Bohr at The Rockefeller Institute 1962 Convocation



Interior of Bohr's pavilion at Tisvilde

Einstein appeared forever as his leading spiritual sparring partner — even after the latter's death, Bohr would argue with him as if Einstein were still alive.

Consolidator

I can now explain what was the principal and lasting inspiration which I derived from the discussions with Bohr. In Holland I had received a solid training as a physicist. It is historically inevitable that men of my generation received quantum mechanics served up ready-made. While I may say that I had a decent working knowledge of the theory, I had not and indeed could hardly have fathomed how very profoundly the change from the classical to the quantum mechanical way of thinking affected both the architects and the close witnesses of the revolution in physics which took place in 1925. Through steady exposure to Bohr's "daily struggle" and his ever-repeated emphasis on "the epistemological lesson which quantum mechanics has taught us," to use a favorite phrase of his, my understanding deepened not only of the history of physics but of physics itself. In fact, the many hours which Bohr spent talking to me about complementarity have had a liberating effect on every aspect of my thinking.

Of course the purpose of the foregoing remarks is hardly to edify the reader with what goes on in the mind of the present author. Rather, they are meant to exemplify the way in which the direct close contact with Bohr affected physicists of the post-quantum-mechanical era. To earlier generations he had been a leader in battle at the frontiers of knowledge. This was no longer so in the times I am referring to and thereafter; such is destiny. To us who knew him then, Bohr had become the principal consolidator of one of the greatest developments in the history of science. (It would be inappropriate to speak of Bohr the philosopher, at this stage of his life, as his attitude toward professional philosophy was always skeptical, to say the least.) It is true that, to the end, Bohr was one of the most open-minded physicists I have known, forever eager to learn of new developments from younger people and remaining faithful to his own admonition always to be prepared for a surprise. (In these respects he was entirely different from Einstein.) But, as it has to be, in new fields his role shifted from actor to spectator. Bohr created atomic physics and put his stamp on nuclear physics. With particle physics, the next chapter, the post-Bohr era begins. Actually, the Cambridge paper of 1946

represents Bohr's farthest penetration into the more modern problems.*

At about that time Bohr proposed to me to "lay aside titles," as the Danes say, which means that one goes over to addressing the other person with the familiar "thou" form. I recall how in the beginning I twisted sentences around in the most awkward ways, to avoid the allocution; but I got used to it.

Some time later the Bohr family went to their summer house in Tisvilde, and I was invited to stay with them so that the work could continue. It was a wonderful experience. A good deal of the day was spent working in a separate little pavilion on the grounds. All during this period, Aage Bohr joined in as well. We would go for a swim in the afternoon and often work more at night. In fact, after Aage and I had retired, Bohr would still come in sometimes, on a shoe and a sock, to impart to us just one further thought that had occurred to him that very minute.

Other evenings were spent in the family circle, and sometimes Bohr would read one or more of his favorite poems. I marked them in my own books: Goethe's "Zueignung," Schiller's "Sprüche des Konfuzius," "Breite und Tiefe," "Mädchen's Klage," etc. Bohr liked the following lines especially:

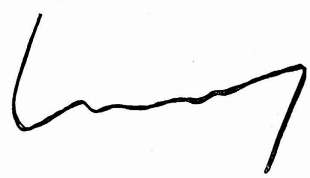
> . . . .Wer etwas treffliches leisten will,
> Hätt' gern etwas groszes geboren,
> Der sammle still und unerschlafft
> Im kleinsten Punkte die höchste Kraft.

Like everything Bohr did, large or small, he was able to put his whole being into it, and he could convey beautifully how small the point was, and how great the force.

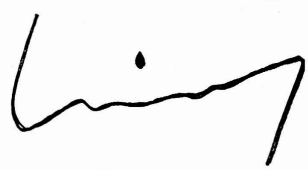
Bohr was an indefatigable worker. When he was in need of a break in the discussions, he would go outside and apply himself to the pulling of weeds with what can only be called ferocity. At this point I can contribute a little item to the lore about Bohr the pipe smoker. It is well known that to him the operations of filling a pipe and lighting it were commutative, but the following situation is even more extreme. One day Bohr was weeding again, his pipe between his teeth. At one point, unnoticed by Bohr, the bowl fell off the stem. Aage and I were lounging in the grass, expectantly awaiting further developments. It is hard to forget Bohr's look of stupefaction when he found himself holding a thoughtfully lit match against a pipe without bowl.

Bohr devoted tremendous effort and care to the composition of his articles. However, to perform the physical act of writing, pen or chalk in hand, was almost alien to him. He preferred to dictate. On one of the few occasions that I actually did see him write himself, Bohr performed the most remarkable act of calligraphy I shall ever witness.

It happened during that summer in the pavilion in Tisvilde, as we were discussing the address Bohr was to give on the occasion of the tercentenary celebration of Newton's birth. Bohr stood in front of the blackboard (wherever Bohr dwelt, a blackboard was never far) and wrote down some general themes to be discussed. One of them had to do with the harmony of something or other. So Bohr wrote down the word harmony. It looked about as follows:



However, as the discussion progressed, Bohr became dissatisfied with the use of harmony. He walked around restlessly. Then he stopped and his face lit up. "Now I've got it. We must change harmony to uniformity." So he picked up the chalk again, stood there looking for a moment at what he had written before and then made the following change:



with one triumphant bang of the chalk on the blackboard.

*Some of the arguments in this paper have since been superseded. Bohr believed that there could conceivably exist an inconsistency in quantum electrodynamics in that the cross section for the scattering of light by light might exceed the *unitarity limit*. Further developments have shown that there is no reason for this belief.

At that time Bohr had an interesting idea about zero-point energies of quantum fields. He thought that it might be fruitful to explore whether the positive infinite zero-point energies of boson fields could be compensated for by the negative infinite energies of the "seas" of fermions, in such a way that this compensation would determine the relative number of distinct particles of either kind. As a consequence of more modern developments in field theory, these zero-point-energy questions now appear to be less significant.

And then the time came to return to Copenhagen. We went by car. It was an act of faith to sit in an automobile driven by Bohr. On that particular occasion he complained that he felt too hot, and actually let go of the wheel to take off his jacket. Mrs. Bohr's rapid intervention saved the situation. Shortly afterwards Bohr went to England. I saw him on his return, and then left Denmark.

I stopped in the Netherlands on my way to the United States, and had occasion to call on Fokker in Haarlem. I mentioned to him some recent experiences in Denmark. This led Fokker to tell me of his own contact with Bohr in a much earlier period. He had interesting things to say. In 1913/14 he studied with Einstein in Zurich and gave there the first colloquium on Bohr's theory of the H-atom. Einstein, Laue and Stern were in the audience. Einstein did not react immediately, but kept a pensive silence. In 1914 Fokker spent six weeks with Rutherford, where he met Bohr. Bohr asked everyone, "Do you believe it?"

Divinely Bad

I met Bohr again two months later, at the celebration of the Bicentennial of Princeton University. Bohr then asked me to spend some time with him on the preparation of a talk for that occasion. I did so and I know how well prepared Bohr was with carefully structured arguments. However, I recall my amazement at the talk which Bohr actually gave, which was done without a worked-out manuscript before him. I should say that this amazement was due to the fact that till then I had never heard Bohr speak publicly.

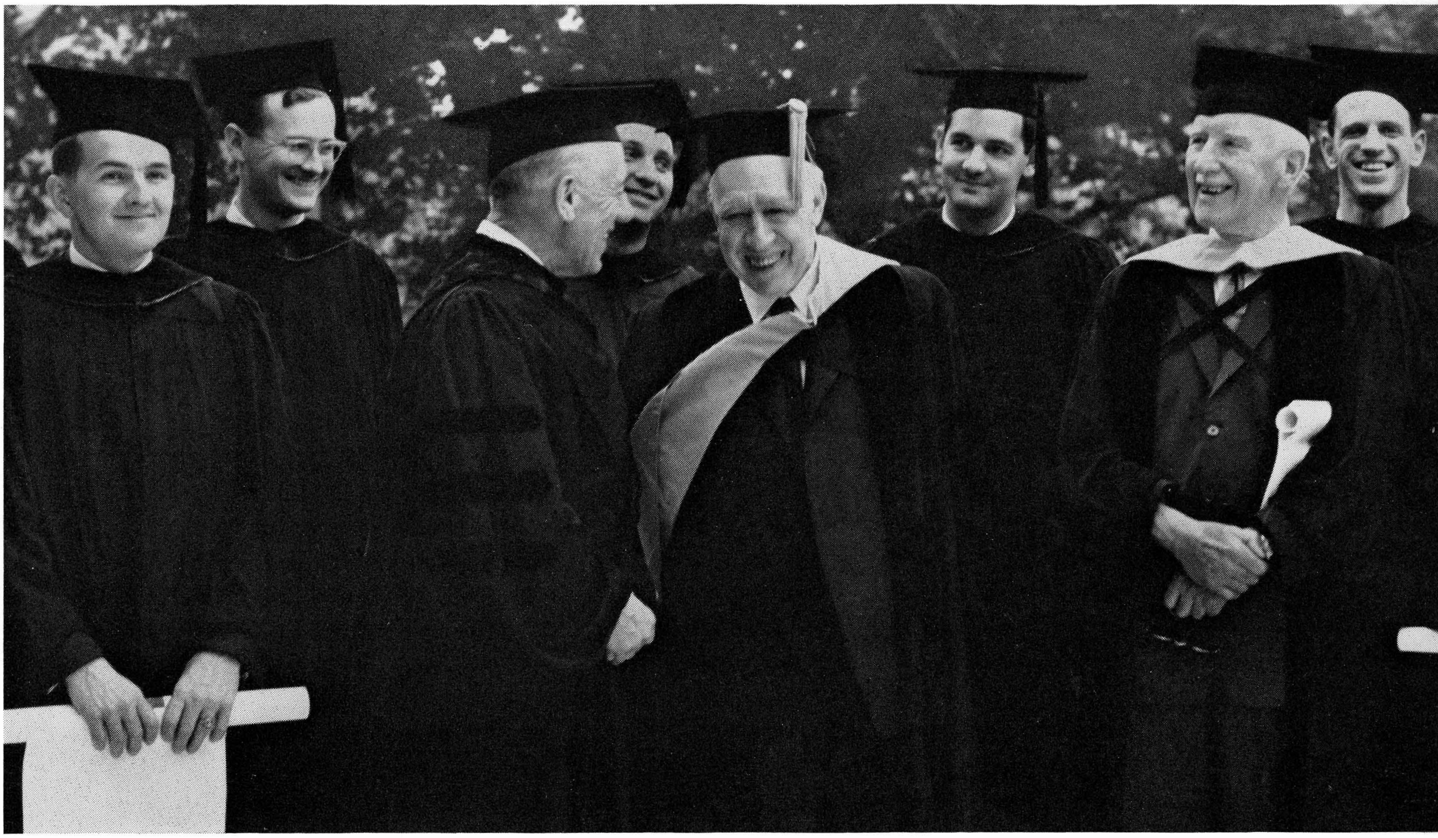
In attempting to describe the experience of listening to Bohr in public, I am reminded of a story about the violinist Eugène Ysaye, who at one time had a member of a royal family as his pupil. Another musician of great renown (to whom I owe this tale) once asked Ysaye how this pupil was doing. Whereupon Ysaye opened his hands heavenwards and sighed, "Ah, her royal highness, she plays divinely bad."

However different the context, these are the words which best characterize the situation. Bohr was divinely bad as a public speaker. This was not due to Bohr's precept never to speak more clearly than one thinks. Had he done so, the outcome would have been quite different, as Bohr was a man of the greatest lucidity of thought. Nor is it due to the fact that Bohr's voice did not carry far, which made it impossible to hear him at the back of a large audience. The main reason is that Bohr was in deep thought as he spoke. I remember how that day he had finished part of the argument, then said, "And . . . and . . . ," then was silent for at most a second, then said, "But . . . ," and continued. Between the "and" and the "but" the next point had gone through his mind. However, he simply forgot to say it out loud and went on somewhere further down his road. To me, the story was continuous, as I knew precisely how to fill in the gaps Bohr left open. And so it has come to happen more than once that I have seen an audience leave a talk by Bohr in a mild state of bewilderment, even though Bohr had toiled hard in preparing himself in all detail. Still, when he would come up to me afterward with his characteristic question, "Jeg håber det var nogenlunde [I hope it was tolerable]," I could assure him that it was much more than that. Whatever might have been the limitations in technique, a strong inspiration had come across from this unsparing struggle for truth.

At the same time, it should be emphasized that Bohr's best way of communication actually was the spoken word, but with just one or at most a few persons present. Bohr's needs for verbal expression were great, as the following occurrence may illustrate. On a later occasion (1948) Bohr arrived in Princeton after a trip by sea from Denmark. For about a week he had had no opportunity to discuss scientific matters; he was quite pent up. Pauli and I were walking in a corridor of the Institute when Bohr first came in. When he saw us, he practically pushed us into an office, made us sit down, said, "Pauli, schweig," and then talked for about two hours before either of us had a chance to interrupt him. Had Bohr's words been recorded, it would have constituted a fascinating document on the development of quantum theory.

Einstein

I may add a word here about Bohr's use of the English language. This he mastered fully, but he had some fixed and very endearing mispronunciations. The one I remember best is his way of referring to one of the great menaces of our time as the "atomic bum" [my attempt at phonetics]. Also, he



Niels Bohr, after receiving honorary degree from President Bronk, enjoys a relaxed moment with A. V. Hill, Dr. Bronk, and graduates.

used to denote a well-known investigative agency by "FIB," which somehow seemed to take the sting out of Mr. Hoover's organization.

During the bicentennial conference Bohr was approached by Dr. P. Schilpp with the request to contribute to an anniversary volume for Einstein's 70th birthday. I shall come back to this below. At this point I wish to relate my own first direct experience of the impact of Einstein on Bohr. It happened a few weeks later that Bohr came to my office at the Institute for Advanced Study, of which I then was a temporary member. He was in a state of angry despair and kept saying, "I am sick of myself" for several times. I was concerned and asked what had happened. He told me he had just been downstairs to see Einstein. As always, they had gotten into an argument about the meaning of quantum mechanics. And, as remained true to the end, Bohr had been unable to convince Einstein of his views. There can be no doubt that Einstein's lack of assent was a very deep frustration to Bohr. It is our good fortune that this led Bohr to keep striving at clarification and better formulation, and not only that. It was Bohr's own good fortune too.

Bohr left the U. S. in late November. In February, 1948, he returned to the Institute in Princeton, of which he was then a permanent member. Bohr's first membership there dates from 1939; it was during that period that he published his famous brief note on the distinct fission properties of Uranium 235 and 238. It may be stated that as soon as the news of Bohr's escape from Denmark in 1943 reached the United States, immediate attempts were made by the Director, Dr. F. Aydelotte, to get Bohr to come to the Institute. But there were other things to be done. In a letter to Dr. Aydelotte, Vannevar Bush stated that Bohr had other involvements, "which I do not yet fully understand." Under Dr. Oppenheimer's directorate Bohr was appointed a permanent non-resident member, which meant that provisions were made for him to come and go at his own pleasure. Bohr used his membership for the spring term of 1948 and again for the fall term of 1954.

In the 1948 period I saw a lot of Bohr, as he and

his wife lived at 14 Dickinson Street, the same house in which I occupied the top floor. When I came home at night, the following charming little comedy would often be re-enacted. As I opened the door, Bohr would always just be walking in the corridor, his back towards me, on his way to the kitchen. In that way he would let me notice him first. He would then turn around in apparent surprise and ask if I would not care for a glass of sherry. And then we would settle down to talk about the political problems. For at that period Bohr had become disillusioned with the official reactions to the atom. It was now his desire to make a direct attempt to get his views considered by those in positions of responsibility, and he was preparing a memorandum to this effect which was discussed over and over during those evenings. It formed the basis for Bohr's open letter to the United Nations in 1950.

Apart from this, Bohr spent most of his time by putting the finishing touches to his article in the Einstein volume, mentioned earlier. This paper is Bohr's masterpiece. Nowhere in the literature can a better access to his thinking be found, and it is a must for all students of quantum mechanics, now or later. During that period I was witness of an amusing moment which involved both Bohr and Einstein.

One morning Bohr came into my office and started as follows: "Du er så klog . . . . [You are so wise. . . .]." I started to laugh (no formality or solemnity was called for in the contact with Bohr) and said, "All right, I understand." Bohr would like me to come down to his office and talk. We went there, and it should be explained that Bohr at that time used Einstein's own office in Fuld Hall. At the same time, Einstein himself used the adjoining small assistant's office; he had a dislike of the big one which he did not use anyway. (A photograph in the Einstein anniversary volume of the *Reviews of Modern Physics*, 1949, shows Einstein sitting in the assistant's office.) After we had entered, Bohr asked me to sit down ("I always need an origin for the coordinate system") and soon started to pace furiously around the oblong table in the center of the room. He then asked me if I could put down a few sentences as they would emerge during his pacing. It should be explained that, at such sessions, Bohr never had a full sentence ready. He would often dwell on one word, coax it, implore it, to find the continuation. This could go on for many minutes. At that moment the word was "Einstein." There Bohr was, almost running around the table and repeating, "Einstein . . . Einstein. . . ." It would have been a curious sight for someone not familiar with Bohr. After a little while he walked to the window, gazed out, repeating every now and then, "Einstein . . . Einstein. . . ."

At that moment the door opened very softly, and Einstein tiptoed in.

He beckoned to me with a finger on his lips to be very quiet, his urchin smile on his face. He was to explain a few minutes later the reason for his behavior. Einstein was not allowed by his doctor to buy any tobacco. However, the doctor had not forbidden him to steal tobacco, and this was precisely what he set out to do now. Always on tiptoe, he made a beeline for Bohr's tobacco pot which stood on the table at which I was sitting. Meanwhile Bohr, unaware, was standing at the window, muttering, "Einstein . . . Einstein. . . ." I was at a loss what to do, especially because I had at that moment not the faintest idea what Einstein was up to.

Then Bohr, with a firm "Einstein," turned around. There they were, face to face, as if Bohr had summoned him forth. It is an understatement to say that for a moment Bohr was speechless. I myself, who had seen it coming, had distinctly felt uncanny for a moment, so I could well understand Bohr's own reaction. A moment later the spell was broken when Einstein explained his mission, and soon we were all bursting with laughter.

The periods of closest contact which I had with Bohr are those described above. In the subsequent years I saw him often, either in Denmark or in the U.S., but no longer for protracted periods of time.

In the fall of 1961 we were both present at the Solvay Congress. It was the 50th Anniversary of the first one, and Bohr gave an account, both charming and fascinating, of the developments during that period. He was present at the report I gave at that meeting, after which we walked in the corridor and spoke of the future of particle physics. It was the last time I spoke to him.

Bohr's was a rich and full life. As I write these last lines, I hear Bohr reciting two lines of another of his favorite poems:

> . . . Nur die Fülle führt zur Klarheit,
> Und im Abgrund wohnt die Wahrheit.

EARLY CULTURAL INTERCHANGES BETWEEN PHILADELPHIA AND NEW YORK

Caspar Wistar was one of a remarkable group of 18th-19th century scientists who were inspired by Benjamin Franklin. He was President of The Royal Medical Society of Edinburgh a year after his graduation from the University of Pennsylvania; he was Professor of Chemistry and Anatomy in his Alma Mater, founder of the Wistar Institute of Anatomy and Biology, and succeeded his friend Thomas Jefferson as President of the American Philosophical Society.

In 1800, Dr. Wistar began the pleasant custom of inviting a score of friends to dine with him on Saturday or Sunday evenings for the enjoyment of good food and learned discourse. Excepting a brief period during and after the Civil War, 24 members of the American Philosophical Society have continued that tradition.

Among the members of the Wistar Association have been many eminent scientists and humanists: Alexander Dallas Bache, first President of the National Academy of Sciences; William Pepper, Provost and Professor of Medicine in the University of Pennsylvania; William Keen, the noted surgeon; Horace Howard Furnas, eminent Shakespearean scholar; Paul Cret, the architect; Frank Aydelotte, President of Swarthmore College and Director of The Institute for Advanced Study; E. G. Conklin, beloved biologist of Princeton and Woods Hole.

Since the founding of the Association more than 160 years ago, every Wistar Party was held in the City of Philadelphia or its suburban surroundings until April of this year. Then the Wistar Party for the first time left the City of Benjamin Franklin to dine at The Rockefeller Institute in the City of New York. On that occasion, Henry Allen Moe, President of the American Philosophical Society and President of the New York Historical Association, spoke as follows:

BY HENRY ALLEN MOE

IT DOES NOT COME to memory that I ever said "No" successfully to Dr. Bronk, nor even that I ever tried. This time, moreover, I said "Yes" straight off; because I figured that he – being he – really had me in his sights. I mean, he surely knew – when he telephoned – that I am President of the American Philosophical Society, and I figured that he knew that I am President of the New York State Historical Association. And, accordingly, if I said "No" to his asking for a talk on early cultural interchanges between Philadelphia and New York, I'd be making a pretense of incompetence in both offices which might not be true.

Of course, I might well have responded to Dr. Bronk, "Why don't you speak for yourself, Det?" For Dr. Bronk's ancestor, Jonas Bronck, was here in 1639, sailing up and down this East River – which is the name the Dutch gave it, distinguishing the North River (the Hudson) and the South River (the Delaware).

Jonas Bronck had a farm not far north of here, more than 300 years ago. He called that farm by a Biblical name, "Emmaus," but other people called it "Bronck's Land." When later the area, under the English, came to be called Morrisania, the pioneer's name – for Jonas Bronck was the first white settler on the mainland north of Manhattan Island – the pioneer Bronck's name stuck to a little river and

thence went to the Bronx Park and still later to the Bronx Borough.

Jonas spelled his name Bronck, but the ending in "x," in the modern version, is not without 17th-century authority; for there is a drawing, a plat, dated 1639 and now in the State Archives in Albany, which is endorsed, "The plat of Bronckx his land."

Jonas Bronck was a Dane who came to New Amsterdam in company of another Dane named Kuyter who had served as a naval commander in the West Indies; he came, by special permission of the Dutch West India Company, in an armed vessel chartered for the voyage. As you will understand, he came in a style suitable to his descendant!

Treaty with the Indians, 1642, at the house of Jonas Bronck (leaning over the table) for whom the Borough of the Bronx is named.

It is an error, commonly made, to think that New Netherlands was settled entirely by the Dutch. For example, Bergen, in what is now New Jersey, got its name, in the early 17th century, from settlers who came from the Norwegian city of that name. And the first white child born in New Amsterdam was named Jan Vinje, and however it be spelled — Vinje, Vienje or Finje — it couldn't be other than a Norwegian name — a name in my mother's family, indeed — pure Norwegian if there be any such thing.

So much for the ancestor of our host, who did not choose to speak for himself!

And I now shall move from the 17th century into the 18th century, and up to the headquarters of the New York State Historical Association at Cooperstown, New York, where we have museums and a great library, all munificently established and endowed by Stephen Carlton Clark.

I mention Cooperstown because Cooperstown was founded by a native of Philadelphia, William Cooper. William Cooper became a judge and a Member of Congress; but it is as a settler — and father of his son, James Fenimore Cooper — that he deserves to be remembered. For he went into the 18th-century wilderness up by Otsego Lake — a hundred miles west of Albany — and there settled a half-million acres with farmers and craftsmen, and established a village which was to become a vigorous cultural and scientific center — thanks again to Stephen Carlton Clark. Judge Cooper was, so far as we of the New York State Historical Association have been able to learn, the first great merchandiser of real estate to sell land on the installment plan. It was his invention: Philadelphia bred good businessmen then and now!

Serious Philadelphia

New Amsterdam, later New York, was founded in 1623, Philadelphia in 1682. But by the mid-18th century, Philadelphia was the larger city and in many ways the more important; and certainly was the more interested in the promotion of science, the arts, architecture and education. Philadelphia, I think it may be said in truth, was more serious, more devoted to good causes, New York more worldly, more international.

But one is bound to wonder a bit what would have been the relative positions and comparative virtues of these two cities if Benjamin Franklin had decided to settle here in New York City. For almost everything which gave Philadelphia its pre-eminences derived from Franklin's genius. But these are facts which need not be detailed to members of the American Philosophical Society and of the Wistar Society.

In the 18th century, the imports and exports of New York and Philadelphia were almost identical. They were not commercial rivals because the areas in which they sold goods did not overlap, and there

was little exchange of goods between them. Such intercity trade as there was, left a little balance in Philadelphia's favor.

Both cities, even in the 18th century, were blessed with mixtures of citizens from many countries, giving variety and color to their populations. Dr. Alexander Hamilton, the traveler — *not* the first Secretary of the Treasury — wrote of Philadelphia as a sober town, devoted more to getting than to spending; but, of New York, he remembered the "good company and conversation always on tap at Manhattan's tavern clubs early in the evening [which] usually descended to bawdy talk with excessive topping" before the night was out.

Let us not forget that this evening we are in New York!

The firm morality of the Quakers was long a force in Philadelphia. Since New York had not been under the domination of any religious group, its views on matters of morality were more flexible. For example, Quaker Philadelphia was hostile to the theatre long after New York had accepted it with enthusiasm and critical enjoyment.

On the other hand, Philadelphia had the Library Company of Philadelphia founded by Benjamin Franklin in 1731, while no such institution, where books were generally available, appeared in New York until the New York Society Library was founded in 1754. Both still thrive.

During the period 1743-1776 fifty bookshops opened in Philadelphia while only sixteen opened in New York City.

There must have been many men in the 18th century who made contributions to — or took contributions from — both cities. An analysis of 18th-century newspaper advertisements shows an increasing movement between the two towns as the century progressed. And one senses a rising interest in cultural matters in New York as the years go by. For example, The New York *Daily Advertiser* wrote on April 7, 1786,

> It must give pleasure to the citizens of this place to find that New York bids fair to outvie the sister states in becoming the seat of the arts. Today we are informed on the arrival of Mr. Joseph Wright from Philadelphia, a gentleman of abilities in painting . . . he means to follow his profession of limner here.

Three years later in 1789 Charles Willson Peale and Rembrandt Peale brought their Collection of Portraits of Americans from Philadelphia to New York and also modestly announced that they were available for portrait painting. In 1795 Charles Willson Peale thought enough of the scientific and cultural resources of New York City to ask in the *New York Weekly* for the contribution of suitable objects to his Philadelphia Museum.

Assuming that the newspaper advertisements are a good indication, many craftsmen came to New York from Philadelphia and felt that coming from Philadelphia in itself was a recommendation. One suspects that it was not only because it was the larger city but also because it had high standards of craftsmanship.

For example, in 1750 Henry Wileman, a button maker who had served his apprenticeship with Caspar Wistar, set up shop to make "Philadelphia buttons and buckles in New York."

A bell hanger, a dry scourer, a silk dyer, a flax dresser, a chair maker, a ship's figure-head carver, an upholsterer and paperhanger, a German flute teacher and harpsichord tuner, all moved from Philadelphia to New York and, in advertising their arrival, bragged about their association with the city on the South River.

Interurban

Because I have at hand no analysis of Philadelphia advertisements — we do not have many 18th-century Philadelphia newspapers in our Library at Cooperstown — I cannot tell exactly to what extent this was a two-way street; but I am sure it is safe to assume that it was. One also is aware that only a few such craftsmen advertised in the papers, so my examples are merely indications of variety, not of frequency.

The list of 18th-century men who had a foot in both towns should by no means be limited to craftsmen. I shall remind you of three or four persons of distinction:

Andrew Hamilton, d. 1741, architect of the Pennsylvania State House (Independence Hall), and sometime Attorney General of Pennsylvania, was rushed from Philadelphia to defend Peter Zenger when he was charged with seditious libel in 1735 — for publishing in his New York *Weekly Journal* articles that attacked the Royal Governor, Cosby, and his prerogative. The issue was freedom of the press;

and Hamilton's victory is one of our judicial milestones. It has been called "the greatest oratorical triumph won in the Colonies prior to the speech of James Otis against writs of assistance."

After the trial, Hamilton was given a great celebration dinner in New York, was presented with the freedom of the city in the form of a seal enclosed in a gold box and, as he returned to Philadelphia, guns on the ships in the harbor saluted him.

In sum, it was a New York man who had the guts to write the articles; it was a Philadelphia man who successfully defended his right to do it.

I don't want it thought that there were not good lawyers in New York in 1735; there were, but they were in danger of disbarment, and indeed some were disbarred by Governor Cosby on suspicion that they would defend Zenger.

CADWALLADER COLDEN, 1688-1776, one of the most widely ranging minds of the colonial period, first landed in Philadelphia in 1710 from his native Scotland. He moved to New York in 1718 but, all his life, kept in correspondence with Benjamin Franklin and John Bartram, and with other members of the American Philosophical Society. For not only was Cadwallader Colden the Lieutenant Governor of New York from 1760 to 1776 and author of *The History of the Five Indian Nations,* but also he was a leading American botanist, a cartographer, physician and physicist.

It is unfortunate, from our view, that he was a Tory bulwark—but I guess I suspect that an awful lot of our friends would have been the same. But, of course, none of us!

Transit of Venus

Lest the Philadelphia contingent here tonight feel that they are getting the best of my talk, I remind them of what Cadwallader Colden wrote following the Stamp Act riots in December, 1765: "Whatever happens in New York has the greatest influence on the other colonies. They have their eyes perpetually upon it and they govern themselves accordingly."

ROBERT HARPUR, 1731-1825, Irish born, stern, brilliant and difficult, Professor of Mathematics, later of Natural Philosophy, and still later Librarian of King's College, was another New Yorker with Philadelphia connections. He was first Secretary of the Board of Regents of New York State, had been an ardent patriot in the Revolution. He was the correspondent in his area for the American Philosophical Society to report the Transit of Venus, January 8, 1768, as shown in Volume I of our *Transactions.* After a long career in New York City he went north and west to settle in the wilderness where is now Binghamton, New York, near which place is Harpur College, named for him.

Both New York and Philadelphia represented defeats for General Washington in the Revolution; both cities were long occupied by British troops and were found each to have its quota of Tories and Revolutionaries. Yet as the cards of fate fell, Washington was first inaugurated President in New York on April 30, 1789; but, by his second Inauguration, the capital had moved to Philadelphia, where he took that oath of office. These in truth were the first two cities of the Republic.

Contributions

President Eliot of Harvard once listed five contributions to civilization which, he said, "I hold to have been eminently characteristic of our country": "peacekeeping, religious tolerance, the development of manhood suffrage, the welcoming of newcomers, and the diffusion of well-being."

Three of these characteristics — religious tolerance, manhood suffrage, welcome to strangers — were displayed by cosmopolitan New Amsterdam as they definitely were not by its English neighbors, in the period when the distinctive American type of civilization was in process of formation.

The exception among the English-founded colonies was Pennsylvania. There the colonists from many lands, who flowed into the Province established by William Penn, stood with the descendants of the New Netherlanders to exert a modifying and molding influence upon the descendants of the English Puritans at the North and the English churchmen at the South.*

If the New Amsterdamers and the early Philadelphians had not been the way they were, many of us would not be here. And it is entirely possible that the best of what we are today derives directly from the spirit of enlightenment which has marked the cultural interchange between the two cities.

*The preceding three paragraphs I owe to Mrs. Schuyler van Rensselaer's fine *History of the City of New York in the Seventeenth Century:* Vol. II, pp. 161-162.

CONVOCATION FOR CONFERRING DEGREES

12 JUNE 1963

The fifth annual Convocation for conferring degrees of The Rockefeller Institute was held during June. On that occasion, our young School of Graduate Studies awarded its 50th degree of Doctor of Philosophy. In accordance with the now established custom, each graduand was presented for his degree by a member of the faculty.

ALAN ROBERT ADOLPH

B.E.E., Rensselaer Polytechnic Institute

S.M., Massachusetts Institute of Technology

BY H. KEFFER HARTLINE

ALAN ADOLPH came to The Rockefeller Institute with a background in engineering and a keen interest in the physiology of the nervous system. There is good precedent for such a combination of interests.

Early in his course of study Adolph determined to apply to neurophysiology those parts of modern probability theory and statistics that are so useful in many branches of physics and engineering. In detail the activity of nerve cells perhaps is precisely determined by the physical and chemical influences acting upon them, but diverse and complex influences are usually not obviously interrelated, and a statistical description of their net effects is often the only practical one. Indeed, even in detail, influences on individual cells and the physical mechanisms underlying the activity of cells are themselves more often than not subject to uncertainty and variability, and can properly be described only in statistical terms.

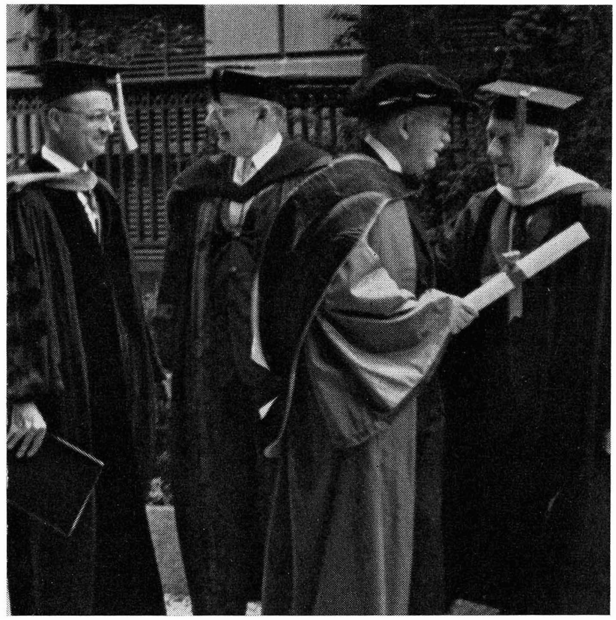
One of the two examples chosen by Adolph for his thesis research concerns the excitation of muscle by its motor nerve. When the fine terminations of a motor nerve fiber are excited, they liberate a chemical substance capable of producing muscular contraction. This chemical transmitter is given off in discrete, small packets. Even at rest, occasional packets are given off sporadically, and are manifested by small randomly occurring fluctuations in the electrical potential difference across the surface of the contiguous muscle cell, too small to elicit contraction. When a nerve impulse is sent down a motor fiber, it causes many of these packets to be released in a burst; the individual effects quickly build up to cause the muscle fiber to twitch. But even within the burst, liberation of the individual packets is not perfectly synchronized; only by specifying the statistical parameters of their release can the total effect of the burst be described accurately.

This first example is well known to neurophysiologists, and was chosen by Adolph so that he could exercise the statistical methods he was developing on a system already well understood. His second example is less familiar. It involves the input side of the nervous system.

In the dark, a photoreceptor cell exhibits small sporadic fluctuations in its membrane potential very reminiscent of the miniature fluctuations just described for muscle. Weak illumination speeds up the rate at which these random fluctuations occur. If the light is strong enough, overlapping fluctuations will build up to a sufficient degree to cause the receptor to discharge impulses in its nerve fiber. As with muscle, the summed effects of the random potential fluctuations can only be described statistically.

To provide the statistical description of these forms of cellular activity, Mr. Adolph drew upon the theory of the shot effect familiar to physicists, as for example in the photomultiplier tube. This theory he extended and used to analyze the subthreshold activity of the photoreceptor. His research has yielded substantial contributions to our understanding of receptor physiology. These advances would have been impossible by any other approach.

The acquisition of specific knowledge and the education derived in its pursuit were of course Mr. Adolph's primary goals. But Alan Adolph also has a deep philosophic interest in the very nature of knowledge itself. He viewed his researches with a philosophic detachment, seeking his own answers to questions about the nature of human understanding.



BARRY ROBERT BLOOM

B.A., Amherst College

BY MERRILL W. CHASE

Barry Robert Bloom entered the student program of The Rockefeller Institute in 1958, an unusually qualified candidate and no stranger to inquiry through research. Born in Philadelphia in 1937, he enjoyed a high-school training in which faculty standards have been kept high in our age. His summers thereafter were used with deliberate and characteristic purpose. At Bar Harbor, the Precollege Science Program at the Roscoe B. Jackson Memorial Laboratories provided the excitement of mind that is offered by a first acquaintance with the experimental method; and it may be well to state here that Mr. Bloom has shared this enthusiasm in the summer course offered by the students of The Rockefeller Institute to a limited number of precollege high-school graduates in our own area. Pursuing his collegiate work at Amherst College, Mr. Bloom employed his summers wisely. Two successive vacations were spent at the Worcester Foundation for Experimental Biology, the latter in the role of assistant to an Investigator, while in the following summer he carried out a research project at the Lankenau Institute for Cancer Research in Philadelphia. At Amherst, his honors work dealt with the area of cellular physiology, being a study of radiation effects on phosphorylation in isolated particulates of cells. His professor commended him in warm terms and ended his letter with the truly scientific understatement, "I am sure that he will make valuable contributions in research."

This experience in cellular physiology doubtless bore on Mr. Bloom's decision to work in the field of cellular hypersensitivity. For, as was first shown clearly in this Institute, hypersensitivities of so-called delayed type appear to depend on new properties that have been acquired by wandering mononuclear white cells during the process of sensitization. To typify this particular sort of hypersensitivity in distinction to hay fever and asthma, two examples may be cited. First are the eruptions that occur on the skin, developing slowly after contact with some offending substance — be it poison ivy, or nickel, or some simple chemical substance. Second are the more hidden sensitivities that arise following invasion of the body by micro-organism or parasite, requiring skin tests with an extract of the causative agent to reveal the sensitivity: best known perhaps are the intradermal injections performed with tuberculin, or histoplasmin, and so on.

For two decades, we have known that white cells from the blood or lymphoid organs of sensitized guinea pigs can confer a transient sensitivity on normal guinea pigs. We know that the transferred cells appear to be rejected about the time at which transplanted skin would be rejected, whereas cells transferred between genetic twins convey a sensitivity

that probably persists for more than a year and a half. Further, it is now generally accepted that the skin of the hypersensitive individual possesses no inherent sensitiveness but owes its capacity to react to a gradual accumulation of wandering white cells at the site of skin tests, which there initiate the local response.

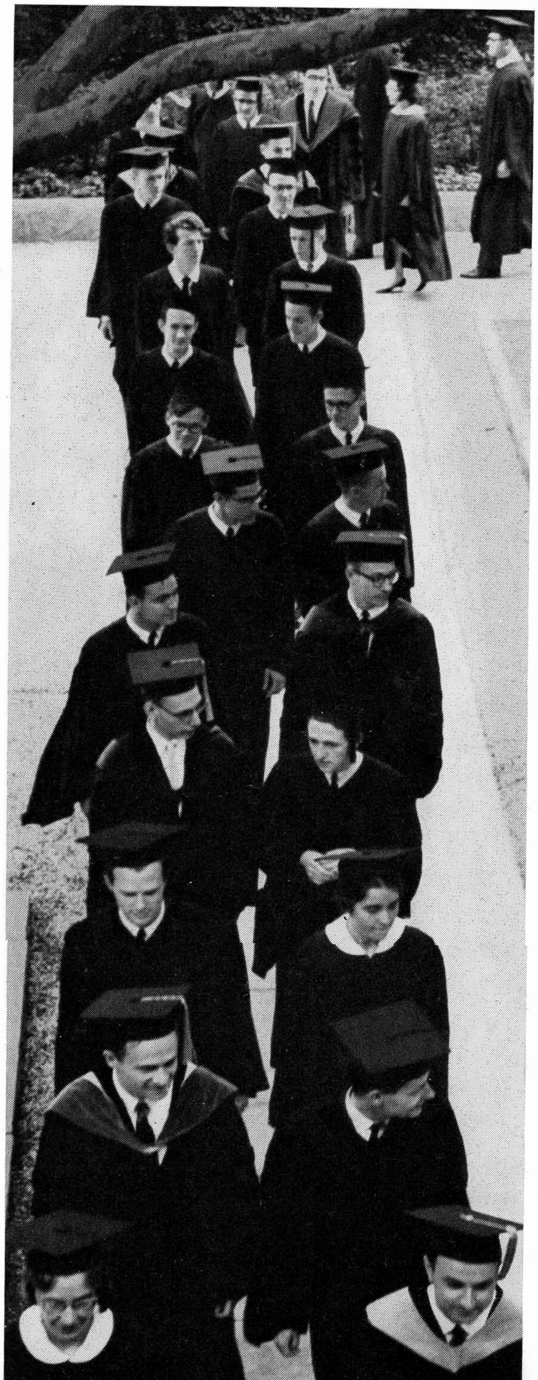
A great hindrance to study at the cellular level was the seeming necessity for the cells to be living at the time of transfer. Studies with cellular transfer in man appeared to offer new vistas: for microbial hypersensitivities, successful transfer was reported with use of disrupted cells, and transfers appeared to be long-lasting irrespective of genetic similarity between donor and recipient. Indeed, there arose reports that transfer could be effected even between guinea pigs with use of disrupted cells and with soluble cellular constituents.

Here was the challenge that Barry Bloom took up, first indirectly from the hands of others, later with developing genius of his own. Would it be possible to approach the problem of hypersensitivity through isolated intracellular constituents, perchance some cell-bound antibody? And if not, what reason could exist for others' reports of success?

This is not the place for minutiae. It is enough to state that during the course of this meticulous effort, which engaged the willing labor of every available person in the laboratory, three additional reports appeared claiming transfer in guinea pigs with nonliving subcellular material. Barry Bloom was able to show, by experiments far more refined in design than others had used, that living cells seem requisite indeed for transfer between guinea pigs, and he became able to specify the several sources of technical errors inherent in such experiments that can lead and have led to misinterpretation.

The cells of hypersensitive guinea pigs differ in still another respect from the described behavior of human cells. Living human cells reportedly remain viable but lose their capacity to effect transfer upon being exposed to solutions of tuberculin, while an active factor is said to be released, one that is capable of passing dialysis membranes. In sharp contrast, Mr. Bloom has shown that guinea pig cells remain fully competent to transfer, and they do not pass any reactive principle into solution by reason of contact with tuberculin. Such dissimilarities appear to transcend species differences. They call for further effort with both species, for parallel transfers with living cells to study durability of the transferred sensitivity, for selection of experimentally induced hypersensitivities that exceed all probability of previous exposure on the part of the intended recipient, and for the withholding of all test injections prior to cellular transfer.

Further, from first experiments employing living cells and graded doses of metabolic inhibitors, Barry Bloom was able to reach the conclusion that successful transfer with living cells demands ordinarily that



cells continue to metabolize and synthesize some required material after being placed in their new host. This new contribution opens the way to searching study. The conclusion appears to question whether cells as harvested will ordinarily yield, by direct extraction, sufficient of the "end product" that appears to arise through further metabolism.

Contributions of other nature need not be cited. Doors have been opened within the corridors of knowledge, and in the doing, this candidate has matured.

ROBERT DONALD CAMPO

B.S., M.S., St. John's University

BY DOMINIC D. DZIEWIATKOWSKI

MANY PEOPLE CONTINUE to regard the skeleton as if it were lifeless. To many it is a framework onto which energetic and interesting organs are attached. Such too was the opinion of Robert Campo a few short years ago. But he saw the light. Indeed, he saw many lights one foggy morning while driving across an intersection; his car was struck by another car. He saw stars and his bones began to ache. He realized that his skeleton was alive, impressively so. The skeleton now became interesting. He began to examine bones of growing animals closely, using chemical, histological, and autoradiographic techniques. He demonstrated to his own satisfaction, as did his scientific predecessors, that bones were first modeled in cartilage and that subsequently minerals were deposited therein to give them rigidity. He thereby also realized that a basic question, which many had attempted to answer unsuccessfully, was the question of how and why a cartilage calcified. In pursuit of an answer, he demonstrated, using radioactive isotopes in the form of precursors, that cells of cartilages produce a combination of protein and complex sugars. With these large, complicated materials they surround themselves. Moreover, he went one step further; he showed that this combination of protein and complex sugars is probably enzymatically modified before the cartilage accepts minerals. This is indeed a contribution to an understanding and appreciation of how bones develop. It may be the clue others have sought. In any case, he has directed our attention to a path along which future work may yield answers to the exact mechanisms involved in the development of bones.

Representative members of the faculty have assessed these observations. In their opinion they are a worth-while, scholarly contribution to knowledge.

STEPHEN COOPER

B.A., Union College

BY NORTON D. ZINDER

VIRAL INFECTION of a cell has been viewed as the imposition of one genetic system upon another. The essential conclusion that has been hitherto developed is that the cell is a passive though complex medium for viral growth, with the specific structural and regulatory information for the viral components residing in the infecting viral material. A corollary would be that the smaller the virus and the fewer its components, the simpler the events following infection and perhaps more readily capable of total resolution.

Utilizing this line of reasoning, Stephen Cooper began the study of the biochemical events that ensue following the infection of a bacterium by a bacteriophage. The phage used was one of the smallest known viruses and contained as its genetic material RNA rather than the more usual DNA.

The first question that he posed was whether DNA synthesis was necessary for phage RNA synthesis. Using a variety of procedures that inhibit DNA synthesis, he was able to show that phage RNA and phage itself could be synthesized when DNA synthesis was prevented. This was taken to indicate that some special biosynthetic pathway was directed by the phage for the synthesis of its RNA. This conclusion predicted that there would be some protein synthesis necessary after infection which would allow phage RNA synthesis to proceed. The major difficulty in the experimental approach lay in the fact that there was no simple way to differentiate phage RNA from cellular RNA. By a number of ingenious experiments, Cooper was able to show that there indeed was a period of protein synthesis necessary after infection subsequent to which phage RNA could be synthesized. He also showed that this protein was not the structural protein of the phage. With the now almost inescapable conclusion that there was a new enzyme which used RNA to direct the synthesis of new RNA, the enzyme was sought and

the appropriate activities were found. This latter finding opens new areas of approach to the understanding of events following viral infection and also provides the missing enzyme in the macromolecular synthesis of genetic material.

This analysis developed and was carried forth in a logical fashion. As I watched this work develop, I watched simultaneously the birth of Steve as a scientist. The early experiments were carried forth with what amounts to, for Stephen Cooper, some trepidation. I guess you have to know him to appreciate fully this remark. There was a growth of confidence as the results came in, and a falling into perspective of many areas of science as evidenced by his now often too-quick-to-be-followed way of thinking. He evoked tremendous enthusiasm upon arrival in the lab and often left us in chaos.

In addition, Cooper actively guided the work of a young high-school student who this year was a recipient of a Westinghouse Science Talent Award.

BRIAN ALBERT CURTIS

A.B., The University of Rochester

BY GEORGE W. CORNER

Brian Curtis grew up in a scientist's household, and since boyhood has known the interest and importance of biological studies. As an undergraduate at the University of Rochester, he majored in biology and attained high standing, otherwise he would not have been here today.

His association with The Rockefeller Institute resulted from a summer spent at the Brookhaven National Laboratories, where he made the acquaintance of a distinguished emeritus member of the Institute, our beloved Donald Van Slyke, whose encouragement and recommendation helped to bring him here.

Joining us in 1958, Curtis soon attached himself to the Laboratory of the Physiology of Reproduction, of which I was then the chief, and began to work with my colleague, Dr. Arpad Csapo. That able scientist should himself properly be at this rostrum today to present Mr. Curtis, but he is abroad – either in London or in Brazil, we are not sure which. We may be certain, however, that wherever he is at this moment, and whether he is speaking English, or Portuguese, or Hungarian, Dr. Csapo is talking to somebody, somewhere, about muscle and that mysterious phenomenon, muscle contraction.

Because Brian Curtis shared this interest, Dr. Csapo encouraged him to investigate one of the most essential activities of the body, the response of the muscle cell to stimulation by the nervous system. It is well known that the contraction of muscle involves the interaction of calcium salts with actomyosin, the contractile protein of the muscle cell; and it has long been supposed that the nerve stimulus acts by driving calcium from the outside of the muscle cell into the interior, to combine with the actomyosin and render it contractile. This idea seemed to be proved by the observation that a piece of muscle kept in calcium-free fluid will not respond to stimulation through its connecting nerve.

Curtis has found, on the contrary, by experiments much more complicated than I can describe here, that such a muscle, even in calcium-free surroundings, will contract if it is stimulated directly by an electric current. This means that the calcium which is somehow caused by nerve stimulation to interact with the actomyosin, is that which is present within the cell itself. What the stimulus does is not to drive calcium into the cell from outside, but to direct the behavior of calcium that is already there.

Having achieved this important further step toward the understanding of muscle excitation, Curtis has gone on to develop a new theory of the interplay of nerve, contractile protein, and calcium ions when a muscle contracts. These studies he will continue in the new post to which he goes later this year.

Research on a problem of such depth as this constitutes a broad education in scientific method. Curtis has had to acquaint himself with the anatomical structure of muscular tissue, with the biochemistry of muscle, with the physiology of the nervous system, and with the methods of physical chemistry and electronics by which such research is made possible.

It is a fortunate circumstance, Mr. President, that a young scientist capable of dealing with a question as fundamental, as intricate as this, has been able to carry on his investigation in an institution fully equipped for such work, under the direction of an accomplished and ingenious leader, and in association with so many experienced men of science having command of the various disciplines that bear upon his problem.

ERIC HARRIS DAVIDSON

B.A., University of Pennsylvania

BY ALFRED E. MIRSKY

WHEN ERIC DAVIDSON came to The Rockefeller Institute five years ago for an interview, I was one of those whom he saw. It was, however, difficult for me to see Eric, because his face was covered with a thick growth of whiskers; but even so, he looked good to me.

What has happened to Davidson between that day and this? Five years ago a change in the climate of opinion was taking place in biology. Cell biologists were recapturing the vision of the biologists of an earlier epoch – the era of Theodor Boveri, E. B. Wilson, and T. H. Morgan. Five years ago some of us were beginning to see that whether or not a gene in a chromosome is active depends on influences coming to it from the cytoplasm – on feedback from the cytoplasm to the chromosomes. Recognition of variable gene activity affects profoundly our conception of the cell and indeed our conception of the whole organism.

The development of an organism, the still mysterious process in which a myriad of differentiated cells arises from a single cell is approached in a different way, once one recognizes the variability of gene action, variability dependent on feedback from the cytoplasm. It was the importance for development of cytoplasmic control of chromosomal activity that was so well understood by Boveri, Wilson, and Morgan; and this was the vision that was being recovered when Eric Davidson came to the Institute.

Davidson's research has been on variable gene activity in relation to cell differentiation. Needless to say, he has learned much from Wilson's great book published in 1925; and, of course, we like to think that Eric has learned something from us. I say this only that I might go on to say something quite different about learning. I have spoken of the importance of the feedback from cytoplasm to chromosomes, a feedback that was not perceived for many years because the effects of chromosomal genes on the cytoplasm are so very striking. It is much the same with teaching. Of course we like to think that students learn from us, but it would be an unrewarded teacher who did not learn from his student. When it comes to the feedback process, a teacher need not guess about results; he knows whether he is learning. And so I can say that I have learned much from Eric – in a delightful way, too. I can certify that others will have my experience.

FREDERICK ARTHUR DODGE, JR.

B.A., University of Pennsylvania

BY CLARENCE M. CONNELLY

HAVING COMPLETED his undergraduate training in biology at the University of Pennsylvania, Frederick Dodge came to The Rockefeller Institute to participate in its new program of graduate education in the biological and related sciences. By the end of his first year of study here, Mr. Dodge found his interest focusing on problems of neurophysiology. He was impressed by the advance achieved by Hodgkin, Huxley, and Katz in their description of the events underlying the generation of the nerve impulse in the giant nerve fiber of the squid, and he was particularly intrigued by the sensitive, yet powerful, techniques they had used in their investigations – the techniques of electronics, of physical chemistry, and of mathematical analysis. Thus, quantitative biology gained a disciple, and Dodge undertook studies in physics, physical chemistry, and mathematics to prepare himself for research in neurophysiology.

At one point, Mr. Dodge proposed that it should be possible to apply the electronic-control technique used by the Cambridge group to the technically more difficult study of excitation in the myelinated nerve fibers of vertebrates. Soon thereafter, we learned that Dr. Bernhard Frankenhaeuser of the Caroline Institute in Stockholm was developing an electronic-control system for just such a study. Dr. Frankenhaeuser graciously welcomed Dodge as a student and colleague, and working together for a year, they perfected and began to use the new control system.

At The Rockefeller Institute, Dodge has used a similar electronic-control system to secure complete sets of electrical measurements from nerve fibers of the frog. He has resolved the measured electrical currents into components carried by sodium ions and components carried by potassium, and he has analyzed the kinetics of the changes in ionic permeability produced by changes in the potential difference across the nerve membrane. The four simultaneous

differential equations that epitomized each set of observations were solved by computer to predict action potentials, thresholds, refractoriness, and other characteristics that could be compared with characteristics measured separately in each nerve; in each case, the agreement between prediction and observation demonstrated the adequacy of the analysis.

Dodge's work has improved our knowledge of the way in which nerve fibers generate the coded messages they conduct between sensory receptors and the brain or between motor-nerve cells and muscles. His work provides solid footing for future investigations into the molecular mechanism of the ionic permeability changes underlying nerve action.

This report would not be complete if I did not tell you that Dodge, with his scholarly understanding of biological processes and with his quick grasp of the potentialities and limitations of experimental techniques, is frequently called upon by his fellows, students and faculty alike, for advice and for critical discussion of their research. In brief, Mr. Dodge is an effective member of the community of scholars that characterizes this Institute as a University.

ALAN FINKELSTEIN

A.B., Washington and Jefferson College

BY ALEXANDER MAURO

THE PROCESS OF intellectual maturation proceeds by subtle, almost imperceptible, steps and indeed to many it appears so elusive that one wonders how it happens at all. Hopefully, the objective of a graduate school is to promote this process by every stratagem and maneuver so as to make possible the blossoming forth of the young scholar. If one were to make an attempt at setting a few basic criteria to help measure in some way the attainment of this maturity, perhaps it would not be too amiss to suggest, on the one hand, tenacity and persistence in pursuing fundamentals and, on the other, critical and independent thinking in this pursuit. To this end, the unique facilities and ethos here at the Institute were tailored for our candidate. As close observer over the past four years, and three more previously at Yale, I have enjoyed watching Alan Finkelstein move along this path; he has equipped himself well in the basic disciplines for the tasks ahead.

As for his choice of research, he has sought out physical mechanisms that might shed light on the workings of the excitable cell membrane as found, for example, in the nerve and muscle cell. His work has proceeded along both theoretical and experimental lines. On theoretical grounds it was clear that in a system of diffusing ions, electromotive force and electrical resistance describe two different properties of the system; thus during the change of the electrical state across the cell membrane — a characteristic variation called the action potential — either or both of these quantities can change with time. In one of the theoretical schemes adhered to by many electrophysiologists, the electromotive forces in the membrane are absolutely constant, while the resistances of various ions can change dramatically with time during the action potential. At this point further theoretical analysis indicated that in order to realize unambiguously such a system, specific channels or regions must be arranged side by side in the cell membrane for different ions. Concurrently, Finkelstein conducted experimental work on two old stalwart friends to whom physiologists have turned again and again for help in their investigations, the frog and the toad. Two unlikely organs were examined for excitability: the skin and the bladder. In the frog, the skin displayed the characteristic action potential when a suitable current was applied, while in the toad the bladder was found to be excitable. To be sure, this was a fascinating new finding, but the really significant discovery was made when, upon allowing the electromotive force to fall to zero by depriving the organ of oxygen, the preparation continued to display an action potential with a suitable stimulating current. We have then for the first time a clear-cut demonstration of the independence of electromotive force and electrical resistance, and indeed of the fact that in the frog skin and toad bladder excitability is due exclusively to a change in resistance. To students of excitability this will be of great interest, since it strongly suggests that all excitable membranes have this property and, furthermore, that ions move in separate regions across the membrane.

Lest his friends feel disappointed in hearing only of his intellectual attainments, it is fitting to mention that our young scholar has many other laudable attributes. One in particular is a delightful sense of humor, which very often comes forth at the appropriate occasion as a thoroughly amusing fable or maxim

drawn from a seemingly inexhaustible repertory. That he can call upon his amiable talents even when engaged in a serious intellectual presentation has been amply demonstrated on many occasions. All of these facets will serve him most effectively as a teacher and colleague in the years to follow.

PETER JOHN GOMATOS

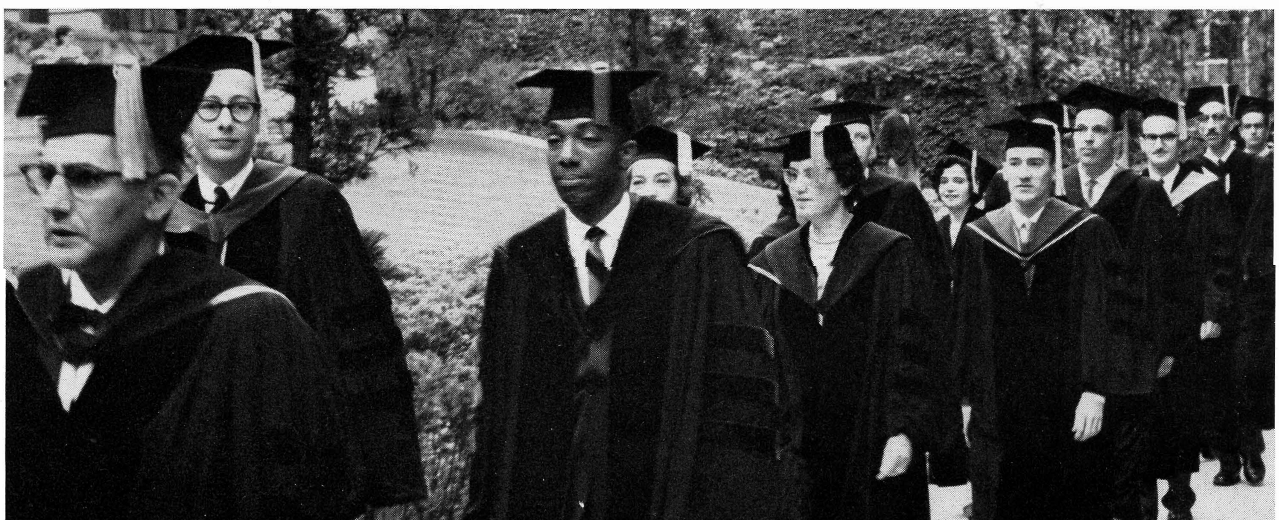
S.B., Massachusetts Institute of Technology
M.D., The Johns Hopkins University School of Medicine

BY IGOR TAMM

LESS THAN TWENTY years ago, in this Institute, there was made what André Lwoff has called the great discovery of modern biology — the identification by Avery, MacLeod, and McCarty of deoxyribonucleic acid as the carrier of genetic information. Since that time, and especially during the last ten years, studies of nucleic acids have moved forward so rapidly that many of the features of their structure, synthesis, and function have been worked out. The results of the investigations of Peter John Gomatos provide a fresh new note in this field, and show that much that is exciting is yet to come.

Gomatos came to the Institute because he had become deeply interested in the molecular basis of biological phenomena and, through his medical background, in the problem of cellular changes in virus infection. He was aware that a serious study of virus-induced processes requires knowledge and skills in many disciplines, including cytology, genetics, and various branches of chemistry. His lively mind took delight in studying these sciences, and both his conceptual thinking and his experimental ability expanded to cover a wide variety of fields. Many in this Institute contributed to his intellectual growth and development.

In studying a little-known but widely distributed virus, called reovirus, Peter found indications that the unusual growth characteristics of this ribonucleic acid-containing virus might be due to a structural peculiarity of its genetic material, the RNA, and with his broad background, he was ready to tackle the difficult problem of isolating the RNA and studying its structure. Through his work, he has established that the genetic material in reovirus is a ribonucleic acid of a new kind: it is a large, double-stranded molecule, possessing extraordinary stability. In studies which stand out for their originality and skill, he has shown that this type of genetic material is present not only in an animal virus of wide distribution, but also in a plant virus, the wound tumor virus, whose intermediate host is an insect. The fact that the plant virus causes tumors has stimulated Peter to think about what fundamental properties may be characteristic of the genetic material of tumor viruses. He has proposed a useful hypothesis, namely, that a double-stranded structure may be a necessary requirement for tumor production.



Peter Gomatos has also made significant contributions to our understanding of what goes wrong in the biosynthetic machinery of cells infected with reovirus, and he has begun studies of the detailed biochemical mechanisms whereby new virus materials are made.

Perhaps the most remarkable fact about Gomatos is that throughout his studies in molecular biology, he has remained a physician at heart. His ultimate goal is not only to continue his investigations and to teach virology, but also to return to the bedside to care for the sick and to guide future physicians. Most will agree that this promises to be a difficult undertaking; some will say it is an impossible task for any one man. Be this as it may, I deeply admire Peter Gomatos for his strength and courage, and most warmly wish him well.

GUIDO GUIDOTTI

M.D., Washington University School of Medicine

BY LYMAN C. CRAIG

In presenting a candidate for his degree at The Rockefeller Institute one has an obligation to say something uniquely suited to the man and to the particular contribution which he has made. Guido Guidotti selected a subject for his research which makes this task pleasant and rather easy.

Almost immediately after his exposure to our biochemistry course, he chose to study hemoglobin — the beautiful red protein in the red cells of the blood which carries oxygen from the lungs throughout the body and removes CO_2. It is not a very stable substance outside its natural environment, but Guidotti did not worry. He had a good supply always at hand. His own blood. No wonder his results have been extraordinarily reproducible.

There is something about the red color of blood that seems vital and has been of great fascination since antiquity. Faust, for instance, said he found it to be a very interesting kind of juice. A systematic chemical investigation of hemoglobin was begun by Hoppe-Seyler in Germany about 100 years ago. Many famous chemists have studied it, including a number here at the Institute — Van Slyke, Mirsky, Granick, and others. It was so complicated that only in recent years have experimental techniques been adequate to reveal the chemical details of its nature. It is now known to be an association complex of four heme fragments and four protein chains. The protein chains are of two kinds, one with 141 amino acids and the other with 146. A team of Rockefeller Institute scientists including Hill, Konigsberg, Guidotti, and Goldstein have established the order in which all these amino acids are connected. This information was also derived independently by a team in Munich headed by Braunitzer and in the California Institute of Technology under Schroeder. The way the chains and the hemes are arranged in space in the crystal has been shown by improved techniques of X-ray diffraction. Perutz at Cambridge was awarded the most recent Nobel prize for this X-ray achievement.

It is of great interest, however, that the hemoglobin molecule is not in the crystalline form in the cell. Guidotti in his thesis has presented excellent data to show that the larger complex is in dynamic equilibrium with smaller associated forms of hemoglobin. This makes the chemistry even more involved and fascinating. His work will continue to develop.

JOHN WILLIAM BAKER HERSHEY

B.A., Haverford College

BY D. WAYNE WOOLLEY

Because most living processes are the result of enzyme action, there has been great enthusiasm to understand the basic mechanism of enzyme action. The best people have spent much time in the past and in the present attempting to solve this problem. It is one of the basic ones in science. Although there is much information available, the fundamental mechanism is still not understood.

When John Hershey came to our laboratory four years ago, we were engaged in an attempt to prove a mechanism of enzyme action by synthesizing a substance which would have the specific activity of the enzyme chymotrypsin. There is no better way to show that you understand something than to be able to make a model which will do the same thing. Hershey was much intrigued by this sort of work and decided to attempt to synthesize a compound which would have the specific enzymic activity of trypsin. Synthesis of such compounds is very difficult, in fact so difficult that it has never been done before. By means of very skillful organic chemistry,

he was able to synthesize the monomer of the desired substance which should mimic the action of trypsin. What remains to be done is to incorporate this monomer into a polymer and to test it for enzymic activity.

The skill and the achievement in making this monomeric substance is of the highest order. I might mention to Dr. du Vigneaud that it is of the same order of magnitude of difficulty as the synthesis of penicillin which was so widely hailed a few years ago.

JOAN LOUISE KENT

B.A., Barnard College

BY ROLLIN D. HOTCHKISS

MAN IS SUFFICIENTLY compulsive about matters of health to visit swift retribution whenever he discovers bacteria taking special and unfair interest in another of his own species. The intruder is likely to be divested of capsule, coverlet and calories and be bombarded with manifold ingenious soaps and poisons, to be stained by dyes to all the rainbow hues, and then to be ensconced in a catalog forever with an unpopular and all but unpronounceable Latin name, which vaguely suggests one of its more obscure properties.

With the somewhat more friendly bacteria, however, man has become more open-hearted. The pneumococcus is one of these which, largely because of the efforts of our predecessors at this Institute, now seems almost kindly to man. The microbial geneticist has lately come to find that such friendly little creatures have their own ways of studying and adapting to respond to him and other objects of the biological world. As man and the bacteria nowadays take peep sights at each other along the slender, doubly helical tubes of deoxyribonucleate molecules, it is often difficult to decide in which direction the information travels the faster. One of our number here today, Joan Kent, has been uniquely successful in experiencing and interpreting to her own kind the way in which bacteria speak to their kindred about the very experiences she has given them.

Joan Kent came to Barnard College from what I take to be a chain of modern and progressive schools, and at that college, along with much science, filled herself with language, literature, art and music. I learned for example that in one early year she folded history back upon itself and enrolled in two courses, one covering European history from ancient times to 1815 and, simultaneously, another covering Europe from 1870 to the present. She has never vouchsafed to me when she plans to probe the missed years, those from 1815 to 1870. By the time she moved on to this modern and progressive school, however, she had decided to take up microbial genetics. Summer laboratory work at Cold Spring Harbor and Woods Hole contributed to this stimulus.

In her studies here, it has been noted that Miss Kent has characteristically sought the large and broad view first, then used that as the basis to choose the most suitable and rewarding sites for concentrated attention. This is in some contrast to an uncompromising older view which holds that it is unbecoming of a young person to ask large questions until he has satisfied his elders that all of the smaller questions have first been solved.

For example, in a student paper which she wrote in early 1958, she ventured to predict that the polymerase which builds up deoxyribonucleic acids might well be able to build in occasional units of the ribonucleic acid kind. This type of thought, had it been circulated, would have then seemed like heresy to the literal-minded, word-dependent thinker; yet one of the great laboratories working on the subject in this country has since told us that that very thing turns out to be true.

That Joan Kent does move on down energetically to concrete and factual levels can be testified on any of the various times when she has engaged fifty per cent, more or less, of the entire supply of culture tubes in our laboratory in her skillfully planned counter-intelligence raids on the bacterial information centers.

What Joan has found will be largely for other times and agencies to record. In brief, however, she has arranged to control the rate at which bacteria are confronted with new messages inscribed in the deoxyribonucleates from more knowledgeable cells that she has selected as teacher strains. It turned out that, like good students, bacteria are able to browse impartially amidst all this molecular information — this she followed quantitatively in all respects — and to adopt without fear or favor items of the good and the useful; by a kind of instinct we did not know they had, rejecting always the false and the

distorted messages. She is now engaged in testing the cells' ability to discriminate more sensitively among nucleic acid molecules that have been brainwashed by ever milder detergents.

WILLIAM CAREY PARKER

B.S.E., Princeton University
B.A., University of Oxford

BY ALEXANDER G. BEARN

IT WAS DARWIN's enduring achievement to realize that variation was the raw material of evolution. Without variation life not only becomes dull, it becomes extinct. Extension of the Darwinian concepts of variation to macromolecules had to await the development of methods designed to separate closely similar macromolecular species.

Carey Parker's research has centered around the macromolecule transferrin, a serum protein which has the important task of transporting the metal iron from the intestinal tract to various parts of the body where it is manufactured into hemoglobin and other iron-containing compounds. Iron is not an optional nutrient; it is vital. You will recall that without it the ladies of the 17th century suffered from love-melancholy—an affliction happily cured by a refreshing draft from the ever popular chalybeate springs.

Transferrin was first recognized as the specific iron transport protein of serum in 1945. It was isolated and characterized and even achieved the dignity of having a number of reassuring physical constants assigned to it. Variations in the quantity of this protein were long recognized, but the suspicion that transferrin could exist in different molecular species was disclosed only when electrophoresis was performed in a gel composed of hydrolyzed potato starch. Examination of the serum from a large number of persons revealed that certain healthy individuals possessed an unusual transferrin which was inherited according to simple Mendelian laws. One particular genetic variant, occurring in about ten per cent of Negro populations, was of particular interest because the very same variant was also reported to be present in Chinese populations.

In view of the widely differing nature of the two populations, the likelihood that they had been subjected to the same selective pressures appeared remote. Perhaps these two variants were not identical. To test this idea, the existing separatory techniques had to be improved to achieve additional resolution. This Carey Parker soon successfully accomplished. Using this modified method, since adopted by many laboratories, he was able to demonstrate beyond doubt that these two forms of transferrin, the one in Chinese populations, the other in Negro populations, were indeed structurally different. Additional studies on a Navajo Indian population from Arizona disclosed that they too had their own private transferrin. These important observations have lent substance to the concept of balanced polymorphism, and argue eloquently against the hypothesis that there is an optimum genotype perfectly fitted for all environments.

When Parker came to the Institute, there were eight known genetic variants; now, largely as a result of the improved methodology, he has identified five more. Those of us who still wistfully cherish the notion that an elegant simplicity permeates science may secretly hope that he will shortly turn his attention to another protein. Isolation and chemical characterization of normal transferrin and three of the genetical variants were next undertaken. Although Parker's interests are decidedly anthropocentric, he took a quick glance backwards and compared human transferrin with that of a cynomologos monkey and disclosed interesting differences both in amino acid composition, a study performed in the laboratories of Stein and Moore, and in antigenic structure.

Although his experimental output has been considerable, he has not neglected the theoretical aspects of his subject. He has ingeniously adapted the existing models for the control of protein synthesis, developed in bacteria, to problems in human biochemical genetics.

The mere recitation, Mr. President, of Carey Parker's accomplishments during his time as a student does not give adequate recognition to his wide scholarship nor the personal pleasure he has imparted to those of us who have been closely associated with him. We who know him best will remember with affection his equanimity, his persistence, and his infectious enthusiasm. At times his agility in defending the seemingly indefensible has betrayed his early legal training, but with T. H. Huxley, he has ever been prepared to admit that even the most beautiful hypothesis can be destroyed by a single ugly fact.

CLIFFORD LEROY SLAYMAN, JR.

B.A., Kenyon College

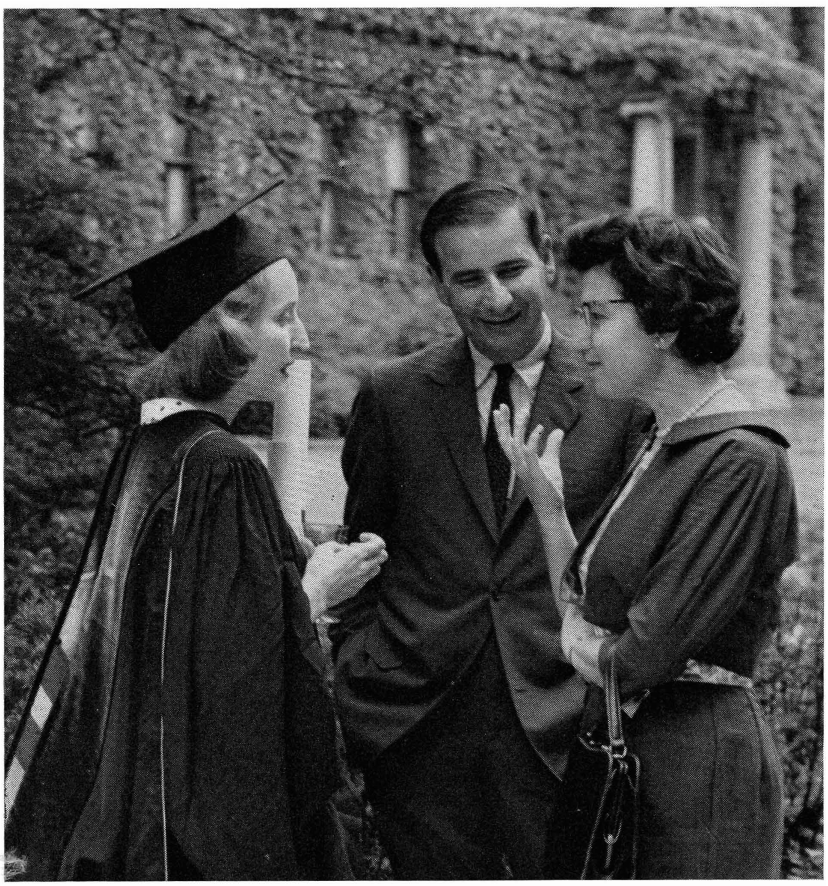
BY EDWARD L. TATUM

CONVOCATIONS AT The Rockefeller Institute have been occasions of many "firsts," truly appropriate enough for the first graduate university in this country. This year is no exception. Each degree awarded is, of course, a "first" in the sense of recognition of the growth to scientific maturity of a new member of the scientific community. Apart from this, however, this convocation is uniquely marked by being the first time at the Institute that a husband and wife are receiving their Doctor of Philosophy degrees at the same commencement exercises, as now are Clifford and Carolyn Slayman. Because of these unusual circumstances, and since some of his work has been done in my laboratory, I, rather than his research adviser, Dr. Hartline, am now presenting Clifford Slayman.

Clifford Slayman's early interest was in brain physiology, and he began his studies here at the Institute with Vernon Brooks. Then, becoming interested in receptor function, he began work with Ratliff and Hartline. Most work in neurophysiology nowadays, whether on cortical neurons or on receptors, is deeply concerned with the basic mechanisms underlying the electrical events involved in nervous action. Thus, it was quite logical for Slayman to become interested in the electrical properties of the cell membrane.

At this stage Slayman decided to study these membrane properties in the mold Neurospora, which in spite of its apparently appropriate name was then an organism new to neurophysiology. This choice could be attributed to the rapid spread and growth of Neurospora when given a chance, or to his wife's enthusiasm for this organism, which she was studying in our laboratory.

In electing to study the electrical properties of



the plasma membrane of Neurospora, Slayman was not choosing an easy problem, for the hyphae of Neurospora are extremely fine. But Neurospora, so thoroughly studied by geneticists and biochemists, offers such a unique opportunity for the genetic control of membrane properties that the idea of using this material was not to be dismissed without trial.

The electrical properties of the plasma membrane of Neurospora are indeed interesting. Puncture of the cell wall by a micropipette shows the cell interior to be negative with respect to the external medium by an amount almost twice that found in nerve cells — too great to be accounted for on the basis of simple diffusion gradients of the common ions, sodium and potassium, across a selectively permeable membrane.

An especially interesting result of Slayman's research is the demonstration of the strong dependence of the membrane potential on cellular metabolism. Metabolic poisons which reduce the oxygen consumption of Neurospora diminish the membrane potential proportionately. The effect is abrupt and the recovery equally rapid. During exposure to a respiratory inhibitor, a fraction of the original potential difference across the membrane persists, and remains about as sensitive to changes in the concentration of external ions as was the original potential difference. This fact suggested to Mr. Slayman that two distinct sources contribute to the net potential difference, one of which is essentially an ion diffusion potential for sodium and potassium, the other a charge-transport system driven by oxidative metabolism.

In his research, Slayman has thus demonstrated, to a notable degree, originality of concept in opening a promising and important new field of work, an ability to devise, modify and master difficult techniques in both electrophysiology and microbiology, and a keen insight in analysis and interpretation of his experimental findings. In addition he has shown restraint and good judgment in confining this initial study to the wild type of Neurospora, insisting on laying a solid groundwork on which his future research with a variety of mutants can be built.

It is a source of gratification and pride for the faculty of The Rockefeller Institute to see the development of its students, such as Clifford Slayman, to scientific maturity, particularly when their development involves new approaches to basic biological problems, and leads to the opening of new, promising fields of research.

CAROLYN WALCH SLAYMAN

B.A., Swarthmore College

BY EDWARD L. TATUM

Carolyn Walch came to The Rockefeller Institute from Swarthmore College via the Johns Hopkins University, where she first became interested in biochemical genetics of the mold Neurospora. In the course of her early work here, which involved a genetic study of two ascospore-lethal mutant strains of Neurospora, she undertook the extracurricular responsibility of becoming Mrs. Clifford Slayman, a step which was to have a profound influence on the development of the research interests and careers of both.

The exchange of ideas and concepts from the two fields of electrophysiology and biochemical genetics led Clifford to his use of Neurospora for studies of membrane properties. It also led Carolyn to choose for her major research a related and equally new and exciting area, that of the role of membrane function in the growth and metabolism of Neurospora.

Imaginatively, Carolyn set as her goal the experimental verification of the prediction that, as are all properties of living organisms, the properties of the plasma membrane of Neurospora are under genetic control, and hence subject to mutational change. Specifically, she undertook a study of potassium ion transport across the cell membrane of Neurospora, and of its alteration through gene mutation.

In approaching this objective, Mrs. Slayman, through the development and use of suitable culture techniques, determined that potassium is an ion required for growth of Neurospora, measured the internal levels of sodium and potassium, and showed that potassium is concentrated in the cells by an active process, dependent in part on oxidative energy. She then effectively used radioactive potassium to study the flux, or kinetics, of potassium transport, and its relationship to sodium and rubidium ions.

Next, Mrs. Slayman ingeniously devised a method of producing and isolating strains with defective potassium transport systems. By this method she has already obtained one such mutant strain. Her analysis of this strain showed that it differs from the nor-

mal by a single gene mutation, and that it appears to lack the membrane system needed for active uptake of potassium from the external medium.

Thus, by learning, modifying, and devising a variety of biochemical, biophysical, physiological, and genetic techniques, Mrs. Slayman accomplished her original goal, and in so doing has opened up an exciting new area in which biochemical genetics and electrophysiology are inextricably intertwined.

Finally, if I may be permitted the use of analogy, The Rockefeller Institute has made a successful investment in the Slaymans which promises high returns, both through their training for careers as productive scientists, and through the opening of new scientific vistas. To extend the analogy, this investment can even be stated in terms of stocks and bonds: stocks of mutant strains of Neurospora, which can be crossed, as the boundaries of the disciplines of physiology, biochemistry, and genetics were crossed in the Slaymans' work; and bonds, those of matrimony and of the sharing of scientific interests. Already the investment in these stocks and bonds has directly changed the course of two lives, and promises to yield continuing and expanding returns in the future. The Rockefeller Institute is justly proud of its investment in Clifford and Carolyn Slayman.

CECIL CHEUNG-CHING YIP

B.Sc., McMaster University

BY REGINALD M. ARCHIBALD

FROM HONG KONG, Cecil Yip came to us in 1959 through McMaster University in Hamilton, Ontario. At that time he was a bachelor with respect to both academic and marital status. His arrival was preceded by his reputation for three noteworthy characteristics. He was already recognized as an accomplished artist with water colors, an expert in Chinese cooking, and perhaps most important, he was recognized as an exceptionally hard worker. Whether or not either of these first two attributes was responsible for his subsequent wooing and winning of a charming wife, is not for me to say. However, I can attest to his industry and perseverance. From the time he first arrived in my laboratory he has been, quite unintentionally, a refreshing example to his colleagues. He was found consistently on evenings and weekends still hard at work in the laboratory. One sensed that the excellent facilities were being used to almost maximal advantage and wished only that even more of his colleagues were around to profit from his example. Cecil Yip, although industrious, was not grim, for his industry stemmed from enthusiasm and intellectual curiosity. This combination of enthusiasm and industry coupled with an unusual ability to design and execute experiments almost insured his productivity. Under the able supervision of Klebanoff, he moved quickly from one accomplishment to another. Successively he tackled and solved problems of increasing complexity. Some might say he quickly "got in the groove." However, Yip is not one to confuse a groove with a rut. At no time have I known him to be in a rut.

Although thyroxine, the chief hormone of the thyroid gland, had been synthesized chemically more than thirty years ago, it had not been synthesized by cell-free extracts of the thyroid gland. This Yip set out to do, and did for the first time. Each of the several reactions involved in the process was studied in detail: the iodination of tyrosine and of its deamination product, parahydroxy phenylpyruvic acid, and the condensation of these two iodinated compounds to form thyroxine. Thus, for the first time, thyroxine can be said to have been synthesized from its amino acid precursor by cell-free preparations of the thyroid gland. This helped, among other things, to open the way to analysis of the mechanism by which the synthesis of this hormone is controlled in disease states characterized either by over- or underproduction of thyroxine. In addition to three publications of merit which arose from his undergraduate work, three more have already appeared as a result of his work at the Institute. Still others are in press. From the industry which he has displayed since his thesis work was completed and from plans for future work which he has already outlined, it seems apparent that this industry will not stop once his degree is granted.

At the close of the Convocation, the honorary degree of Doctor of Science was conferred on Henry Allen Moe, President of the John Simon Guggenheim Memorial Foundation and President of the American Philosophical Society, and on Alan Tower Waterman, Director of the National Science Foundation and President of the American Association for the Advancement of Science.

NEWS AND NOTES

CREATIVE WRITING

Impetus to a new generation of literary talent in the sciences may come from The Richard Prentice Ettinger Creative Writing Program which has just been established with headquarters at the Institute. The other sponsoring institutions are New York University and the University of Pennsylvania.

An all-day symposium on scientific writing inaugurated the Program in Caspary Auditorium on October 29th. In the evening, the 200 distinguished scientists, editors, and publishers who participated in the symposium, dined in Welch Hall. At the dinner Dr. Paul B. Sears, Professor Emeritus of Yale University and author of *Deserts on the March,* received the first Richard Prentice Ettinger Medal.

Dr. Loren C. Eiseley, Visiting Professor at the Institute, directs the Program, which honors the co-founder and Chairman of the Board of Prentice-Hall, Inc., publishers. The first Fellowships will be awarded in the spring of 1964.

HONORARY DEGREES

Professor Peyton Rous received the honorary degree of LL.D. from St. Lawrence University at its June commencement.

President Bronk received the degree Doctor *honoris causa* from the University of Brussels at a special convocation in November.

PROFESSORS

Dr. Abraham Pais has been appointed Professor in the Institute. After receiving his baccalaureate and doctoral degrees at Amsterdam and Utrecht in 1932 and 1941, he was associated with Niels Bohr in Copenhagen. Pais was then appointed Professor of Physics in the Institute for Advanced Study in Princeton, a post he held for 13 years until coming to the Rockefeller. Much of his work in recent years has dealt with the "strange" particles of physics. Dr. Pais is a member of the National Academy of Sciences and of the Royal Academy of Sciences in the Netherlands.

Dr. Vincent G. Allfrey has been promoted to Professor (*Review,* June 1963). A native New Yorker, he was first associated with the Institute in 1941 as a technician in the laboratory of Professor Alfred E. Mirsky, with whom he was to share honors two decades later for significant advances in the knowledge about nucleic acids.

Dr. E. G. D. Cohen, formerly of the University of Amsterdam, has been appointed Professor in the Institute. As a leader in the younger generation of theoretical physicists, his recent research has attracted notice for his work on the phenomena of plasmas, now recognized as the fourth state of matter (in addition to the classical states of liquid, solid, and gas).

Dr. Loren C. Eiseley is as widely acclaimed for his writing as for his anthropological studies. During his residence here as Visiting Professor, he is directing The Richard Prentice Ettinger Program for Creative Writing. Dr. Eiseley was Provost at the University of Pennsylvania for several years, resigning to accept the first University Professorship, a post from which he is on leave while at the Institute.

Dr. Edward J. McShane has been appointed Visiting Professor at the Institute while on leave of absence from his post as Professor of Mathematics at the University of Virginia. Dr. McShane has done considerable work in the calculus of variations, and during World War II was Chief Mathematician in the Ballistics Research Laboratory at Aberdeen Proving Grounds.

NEW STUDENTS

Twenty-nine new students were awarded fellowships and admitted as candidates for the degree of Doctor of Philosophy at the opening of the academic year in September:

NICHOLAS H. ACHESON, A.B. Harvard College
BARBARA ALEXANDER, B.A. Reed College
BARBARA SUE ANDREWS, B.A. Russell Sage College
JACK BRADBURY, B.A. Reed College
DAVID BRYSON, M.D. Yale University School of Medicine
RONALD I. CARR, B.A., M.D. University of Toronto
MICHAEL CHANOWITZ, A.B. Cornell University
RICHARD W. COMPANS, B.A. Kalamazoo College
SAMUEL W. CUSHMAN, A.B. Bowdoin College
W. EINAR GALL, A.B. Hamilton College
MICHAEL E. GOLDBERG, A.B. Harvard College
SCOTT C. GUTH, B.S. University of Arizona; M.D. St. Louis University School of Medicine
C. DONALD KNIGHT, A.B. Baker University
LYNDELL LARSEN, B.S. University of Florida
JOHN K. MARSHALL, A.B. Harvard College
RONALD MILLECCHIA, S.B. Massachusetts Institute of Technology
THOMAS J. MURPHY, B.S. Fordham University
KENNETH D. NADLER, B.S. Rensselaer Polytechnic Institute
EARL PARR, B.A. University of Kansas
GLENN L. PAULSON, B.A. Northwestern University
BRIAN POOLE, B.Sc. McMaster University
MARY E. REUCHLIN, B.A. Bryn Mawr College
STANLEY SAJDERA, B.S. California Institute of Technology
SANFORD R. SIMON, A.B. Columbia University
D. MAX SNODDERLY, JR., S.B., S.M. Massachusetts Institute of Technology
E. MARTIN SPENCER, B.A. Dartmouth College; M.D. Harvard Medical School
LEE VAN LENTEN, A.B. Colgate University
CHARLES E. WAHL, A.B. Columbia University
ERIC WEINBERG, A.B. The University of Rochester

CELEBRATION

The 90th birthday of Eugene Opie was celebrated at the Institute on July 5. Dr. Opie was a Member of the Institute from the time the first laboratories were opened in 1904 until 1910. After serving as Professor and Dean of the Washington University Medical School, Director of the Phipps Institute of the University of Pennsylvania and Professor in the Cornell University Medical School, he returned to the Institute in 1941. Now, 70 years after graduating from Johns Hopkins and 60 years after first joining our faculty, he works long hours each day in his laboratories.

AWARD WINNER

All biochemists are trained to separate and identify unknown substances, but with Professor Lyman C. Craig of the Institute, this aspect of the science became a primary interest early in his career in Iowa. He came to the Institute in 1933, and this November received one of the highest honors in American Medicine, the Lasker Medical Research Award of $10,000, for basic research.

Cover Photograph: View of Founder's Hall by Stephan Pischinger. Page 3: photograph by Heka. Page 4: photograph by A. Pais. Page 7: photograph by The Rockefeller Institute Press Illustration Service. Page 10: detail from the painting "A Treaty with the Indians" by John Ward Dunsmore, courtesy of The Title Guarantee Company, New York. Pages 14, 15, 20, 24: Convocation photographs by The Rockefeller Institute Press Illustration Service.