NATIONAL ACADEMY OF SCIENCES

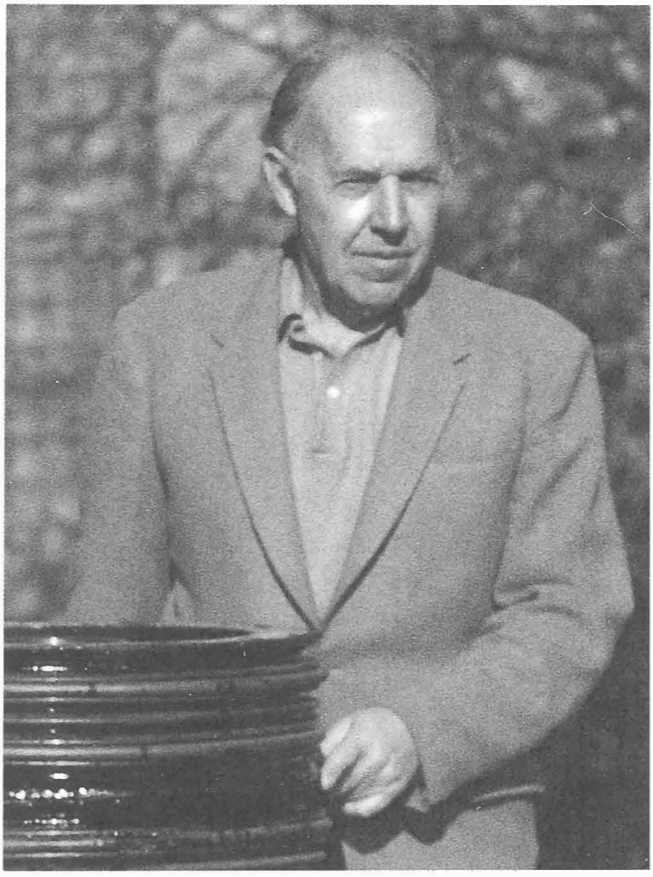
RICHARD BROOKE ROBERTS

1910—1980

A Biographical Memoir by

ROY J. BRITTEN

Biographical Memoir



Richard Brooks Robert

RICHARD BROOKE ROBERTS

December 7, 1910–April 4, 1980

BY ROY J. BRITTEN

DR. RICHARD BROOKE ROBERTS spent most of his career in the biophysics group at the Department of Terrestrial Magnetism of the Carnegie Institution of Washington. Dick contributed importantly to many scientific advances in this period in microbiology, the beginnings of molecular biology, and study of the brain. One high point was the proof (with Kenneth McQuillen and me) that in *Escherichia coli*, protein synthesis occurred on ribosomes. Dick also named the ribosome. Dick started out as a nuclear physicist and among several discoveries showed that delayed neutrons were emitted in uranium fission (1939,5). This discovery was of great practical consequence because delayed neutrons slow the responses in a pile enough to permit control by mechanical movement of cadmium rods. This made fission piles practical for all of their uses in weapons making as well as power. As a result, Dick was involved in early planning of what became the Manhattan Project, although he decided that it was too long range a project for the emergency. He chose to work on more practical weapons and showed that vacuum tubes would survive being fired from a gun; he also developed a radio-controlled proximity fuze which made antiaircraft guns very effective (the first "smart" missile), forever changing

the course of war. He earned the Congressional Medal of Honor.

Dick's career was marked by an independence of mind and a very practical style. He was a professional physicist and biologist with few equals and a severe and irreverent critic of the illogical and imperfect. Nevertheless, his attitude was close to that of amateur in the best sense. He had a love of what he did and a noncompetitive desire to help everyone else achieve "good" science. Perhaps the greatest of his contributions were the ideas and cooperation he gave to others.

FAMILY

Dick Roberts was fortunate in his forebears, many of whom must have had some of the same definite, practical, and inventive cast of character. In the early days of oil in Pennsylvania, his grandfather's brother, Colonel Edward A. L. Roberts, invented and patented shooting explosives in an oil well to improve the flow. He and his brother formed the Roberts Torpedo Company, and the time was ripe since the wells were beginning to clog. The family fortune was helped by the $200-a-well charge and by many successful suits against infringers. When they heard of nitroglycerin, they immediately started manufacturing it in 100-barrel lots in old barns for well shooting. Dick wrote a document[1] (AB) which has been useful for this memoir, and I quote from the first paragraph.

> I have just given 9 volumes of the Academy Biographical Memoirs to the library. The sight of these volumes always provokes the horrible thought that someday someone will have to prepare one of them for me. The thought of being dead is not horrible at all. I have had a very fine serving of life and would not feel cheated if I went tomorrow. However, writing such a piece is not easy and I have often wondered how I could handle such an assignment for somebody—. . . . And so it seems almost mandatory

to write out quite a bit myself so that the poor devil will only have to cut out the meanderings and leave the hard core.

As ever, I continue to appreciate Dick's help.

The next paragraph of AB, entitled "Genetics," starts: "Since I am convinced that the genetic endowment is by far the most important factor in an individual I will begin by recording a few items about my ancestors." There was a Roberts on General Washington's staff and there was Lucius Quintus Cincinnatus Roberts who traveled to China and set up trade. L. Q. C.'s father-in-law (Mr. van Braam) gave the set of china to Martha Washington which is now at Mount Vernon. Dick's grandfather, Walter B. Roberts (brother of Edward A. L.), was a businessman and state senator; the brothers were dentists who invented and sold dental equipment, and there was a banker in the family as well. His grandmother on the Roberts side was Emily Titus, and Titusville was where Dick was born. The family on his mother's side was also involved successfully in Pennsylvania oil, having started a refinery, sold to Standard Oil in 1870. In AB, Dick states, "For a memoir probably all this would boil down to one sentence like[:] His ancestors were active, intelligent, well educated etc. Active is the key word for LQC Roberts . . . for W.B. Roberts. . . . Intelligent is the key word for the Titus family and my father."

CHILDHOOD AND EDUCATION

Dick was born on December 7, 1910, in Titusville, Pennsylvania, the third child with two much older (perhaps twelve and fifteen years) brothers. The family moved to Princeton in 1916 and to New York in 1921 but continued to summer every year in Titusville into the 1930s. His schooling was that of well-to-do families of the time, never setting foot in public school—Miss Fine's School in Princeton,

Lawrence Smith School in New York, St. Paul's School, and Princeton. His interest in science and math started at St. Paul's School, and the reasons given in AB were that he enjoyed them and was not so good at sports (though his love of golf started there) and that the supervision was not too strong. He and a friend (Charlie Thayer) forced the school to give them a calculus class for two, which was rare before college in that era. A quote from AB: "I liked to use the calculus for physics problems and baffle the physics teacher who didn't know calculus."

In AB, Dick raises the question of why none of the other very bright youngsters in the school went into science and answers it as follows: "Probably it was because most were very rich. The class list read like the NYSE. Our family was always very comfortably well-off but I felt like a pauper at SPS. For example I was the only one of the 6th form who did not have a raccoon coat. The free time to play in the lab. was particularly valuable. Too much supervision may be deadly."

For Dick, Princeton was a great success as he grew up, but there is no comment in AB that suggests that great interests in physics were formed or that the classes were particularly good. Apparently, in his senior year he did drift down to the basement at Palmer lab, where research was going on, and got hooked. There seemed no possible choice but to do graduate work at Princeton and go for a Ph.D. Of course, his older brother Walter, who was an RCA radio engineer (with valuable circuits named after him), had been living in Princeton for many years, having graduated from Princeton during the First World War and returned for a Ph.D. (Later he often worked for RCA in a lab attached to his Princeton house.) One quote from AB about mathematics is interesting:

I think math is one subject that has to be learned in the classroom. The assignments are essential. Who has the will power to go through it without some compulsion. I have frequently tried to go through the book on group theory but never got beyond the first chapter. History, economics, etc. even biochemistry were not hard to pick up but not math. And it's not that I was dumb at it. I never had worse than 1st group. But my math stopped with the differential equations and complex functions or whatever it was that junior year. Possibly this was because math was an applied subject for me. I liked to solve problems with it but did not care much for elegant proofs. Heaviside's approach appealed to me (the man who used operators without formal proof . . .).

Dick did graduate first in the class shared with a St. Paul's School comrade (Lew Van Duzen). For those who don't know, the quality of students is high at Princeton and the competition is strong.

There was never any doubt in his mind that he was as intelligent as anyone around. Meeting him, one would quickly recognize that this was a man of importance, yet there was a complete lack of pomposity. As with all such persons, he made demands on the world around him and instantly recognized a bore. His circle of friends was large, and the parties at Linnean Avenue, with barrels of oysters and steamship roasts, are to be remembered. It was hard for him to take much of anything seriously except for a prime list: science, family, golf, new weapons, the fate of humanity, and money. His open-mindedness was remarkable. As a minor example, ESP caught his eye as a student at Princeton and remained a lifelong interest, with total objectivity as far as I could tell. But in writing the history of genetics, some have tried to cut him down for this. Dick's informal approach to mathematics was just right for the main part of his career in biophysics, where a major contribution was an analytical quantitative approach. His skill was in crossing the line between a problem or sets of ob-

servations and the mathematical formulation. An important part of his time at Princeton was spent in the ROTC, and he became a first-rate artillery officer.

About the start of graduate school, AB states: "Arriving at the beginning of the fall term 1932 I was told that I was assigned to work with Prof. Ladenburg in Nuclear Physics. My reaction was fine, but what is nuclear physics? . . . . It had not been included in the undergraduate work. Thus began 4 years of battle to get the degree." There is detailed reminiscence about troubles with high-voltage equipment (Cockcroft-Walton) and ion sources. This was ultimately all resolved, and his thesis was on deuteron-deuteron reactions. By virtue of Ladenburg's extensive absences and a lot of independence, he had become an experimental nuclear physicist. There is, of course, more to it, and I quote AB again: "Somehow the theorists did not resonate with the experimental people. I picked up more from Ed Salant while working with him than any other time. . . . 32–34 were big years in nuclear physics. Artificial disintegrations, the neutron, the positron and induced radioactivity. And the deuteron. At Princeton we had enough equipment to follow along closely but not enough insight to contribute anything. Ladenburg had 1 curie of radium and so he could add a little beryllium and show neutrons within a week after the announcement arrived." It may be worth remembering the fate of that curie which Ladenburg was assembling into a sealed brass cylinder, quoting AB again: "Just before the solder hardened the water pressure (or rather steam pressure) blew the top and his whole curie. He had to take treatment to reduce his radium dose, the whole chemistry stockroom . . . where the explosion occurred . . . was sealed off . . ." It was still sealed off when I arrived as a graduate student thirteen years later.

PREWAR YEARS AT THE DEPARTMENT OF TERRESTRIAL MAGNETISM OF THE CARNEGIE INSTITUTION OF WASHINGTON

The thesis was finished in late 1936, and on a visit to his wife Adeline's family in Washington he drove up to see Merle Tuve and told him of his plan of measurements for a scheduled year at Cavendish; Merle said, "Why not do them here? We have better equipment." Dick accepted the temporary arrangement, and later a fellowship was squeezed out of the Carnegie Institution of Washington (CIW) administration by Merle. So easily began a lifetime. The early work was on scattering experiments (protons, deuterons, and helium), which was hard fundamental work with N. P. Heydenburg. There followed some lithium work with Rumbaugh, and "the main event scientifically of 1938 was pinning down the Be7. This was really very satisfying as isotopes were being discovered right and left and this was my first (and last)" (AB).

The Department of Terrestrial Magnetism (DTM) pressurized Van de Graaf generator split its first atoms on December 23, 1938. A quote from a January 1939 letter to Dick's father is included in AB:

> We have had a very exciting week in Physics. The annual theoretical conference started Thursday with an announcement by Bohr that Hahn in Germany had discovered a radioactive isotope of Barium as a product of bombarding uranium with neutrons. . . . Fermi also discussed the reaction and described an obvious experiment to test the theory. The remarkable thing is that this reaction results in 200 million volts of energy liberated and brings back the possibility of atomic power. Hafstad and I left the meeting as soon as Fermi finished to go to the lab to try the reaction. We had some trouble with a leak in the tube so it wasn't till Saturday afternoon that Meyers and I finally made the test. We had Uranium in our ionization chamber and bombarded with neutrons. We soon observed tremendous pulses corresponding to very large energy release. [There follows a drawing.] I told Tuve after supper and he immediately called Bohr and Fermi

and they came out Saturday night and we ran the test again for them and they were immediately convinced. What we did of course is of no particular credit to us but it is nice to be the first to observe the actual splitting of a uranium atom.

Another quote from AB to keep the history exactly straight: "We later found out that the Columbia group had done the same on Wednesday and Johns Hopkins on Thursday. Frisch was two weeks ahead."

One witness that Saturday evening, Enrico Fermi, had done the same experiment in 1936, using a radium beryllium source of neutrons and had (for a good technical reason) placed a very thin aluminum foil between the uranium and the ionization chamber, which stopped all of the fission fragments. But for that foil the Italians, the Germans, and possibly the world would have known about atomic power and explosions much longer before the Second World War.

Continuing to quote valuable history from AB:

The following weeks were also hectic. The key to atomic power was the neutron emission that might accompany fission. Since it was technically difficult to observe the relatively few additional neutrons released during bombardment we looked for (and found) neutrons emitted after the beam was turned off. . . . In March Adeline and I went to Florida for a few days on the beach with my brother Walter . . . he recorded all our long discussions about uranium fission, how to make a pile, and particularly arrangements to control the pile as the delay in the neutron emission gave ample time for control. On his return to New York in May he presented his usual stack of applications for circuit patents. He then said here are a few along a different line and handed over the applications for the pile and its control. His superiors (?) decided RCA was not interested. I had no interest in the patent side as CIW had a policy of no patents. . . .

After Florida I continued work with Salant on neutron scattering but my main efforts went into measuring cross-section for fission for neutrons of various energies. These were essential in calculating whether a chain reaction would run. By summer I wrote a long paper on the possibility of a

chain reaction based on these cross sections but it was never published because of an agreement to keep such data out of the journals. I did however write one article which I believe is the first serious discussion of the possibility of atomic power for the Journal of Applied Physics. It concluded "The day of free atomic power is probably not in sight."

One other historical note on fission. Tuve was scheduled to be present at the first meeting between the scientists and the government following the letters from Einstein and Sachs to Roosevelt. For some reason he could not attend and sent me along in his place. The meeting decided to appropriate $6000—an enormous sum in those days—for graphite so Fermi could estimate the possibilities of a graphite moderated pile. Also I remember an Army colonel saying a 20 KT bomb wouldn't do much—he had survived such a explosion (at Halifax?). In short he was not impressed.

The next phase is 1939 to 1940, when Dick mostly helped in building the cyclotron at DTM, which was supposed to be a supplier of isotopes for half the days and for nuclear physics the other half. He designed the RF system and much else. In this period, many contacts were made that set up the future biophysics group at DTM, including Philip Abelson and Dean Cowie. The biophysics work had actually started much earlier, including a study with Louis Flexner on the transfer of ^{24}Na from rat mother to embryo. The cyclotron did useful work during the war but, of course, it was short lived as a supplier of isotopes. From AB: "At the end of the war the cyc supplied isotopes around the world for the period before the AEC was ready to sell them. . . . Abelson and I used it for my one post-war physics experiment which showed that the neutron(s) from D plus light elements were mainly forward." It is worth noting that this was the first evidence that the Bohr compound nucleus did not apply everywhere and it motivated the theory of stripping reactions. AB continues: "In retrospect the cyclotron was a fine machine that came at the wrong time." The permanent benefit was the cyclotron building which housed the biophysics group for nearly thirty years.

WARTIME

In AB, Dick states that the idea for a proximity fuze came from England by way of the Tizard mission in the summer of 1940. After development by a group Dick led (that grew to 1,000 people), the fuze was very effective against German V1 bomb attacks on London, as well as for many other uses. One afternoon in 1940, Merle Tuve had asked Dick to find out whether a glass radio tube could be designed to stand 22,000 *g* and that evening Dick ran an 8,000 *g* test on an old 38 tube which survived. In the morning, a 954 acorn tube survived a 20,000 *g* test attached to a lead sphere dropped from the top of the cyclotron building, and "Section T" was off to a quick start. Merle Tuve was boss and put Dick in charge of the radio fuze, while others got projects such as photoelectric and acoustic. In two years, production was going and tests of improvements were being run, a remarkable record. Just one quote for atmosphere from AB: "The next problem was to get some action out of Crosley. They told us that two weeks were required to put a change into effect. New blue prints (of resistors) were required and seven approvals plus signatures. On the assembly line I found that all that was needed was a different basket of components to introduce the change."

Dick with Section T under Merle's direction (which became a part of the Applied Physics Laboratory of Johns Hopkins University) developed many things such as radar jammers and fire control. Later he went on to guided missiles and to ramjets. This period was very important to Dick's life, and his contributions were first rate but will not be described in detail since they involve military engineering and not science. He kept in touch, and his commitment deepened during the Korean War. In review, it is

appropriate to say that the proximity fuze made a difference in the survival of England by protection against German aircraft and V1 bomb attack. It was also a factor in turning the Battle of the Bulge, as well as in much naval activity.

PEACE, ARMS CONTROL, AND SOME POLITICS

This section is not sequential in time but attempts to encapsulate what was a main force in Dick's life. Many scientists, realizing what a disaster modern warfare could become and somewhat guilty over their part in the creation of the weapons, have attempted to forestall the disaster and improve the chances for disarmament. It is fair to say that Dick met with some success (much more than most of us), through writings, his military contacts, and his role in a science advisory group for the Democratic Party (the Science and Technology Committee of the Democratic Advisory Council). The committee initiated something called the National Peace Agency, which ultimately evolved into the Arms Control Agency, in which Dick played a role in its early days (1963). From AB:

> For a year previous to this time there had been a voluntary unilateral ban on testing while the negotiations were in progress. Then I heard one day from friends in the Pentagon that Eisenhower had decided to resume the tests and had given orders for the preparations to begin. This seemed a bad step backwards to me so I called all the members and received unanimous agreement that we should issue a quick statement. This was prepared—I think by the Washington group of McClure, Lapp and me—and then I took it to the Council which was meeting in New York. It was approved and issued by the Council as one of the main items from their meeting. I don't remember the exact words but it was to the effect that resumption of testing at that time would be terrible. Evidently this unexpected blast shook Eisenhower because he countermanded the order to resume testing.
>
> This was very satisfying as I felt (and still do feel) that my efforts in

calling our committee, going to the council, etc., had changed history a slight bit in the right direction.

He was a strong proponent of U.S. and U.S.S.R. submarines as the only deliverers of nuclear missiles as the ultimate safe strategic deterrent. The logic, at least, is still sound. From AB:

The idea was to have two Polaris fleets. Ours would be stationed in the Caribbean and the Russian one behind Japan. Both would be out of range but could move out to attack after a week's cruise. Both sides would want the other to know the fleets were at home and out of range so both sides would allow the other to observe and verify that there was no danger. But if necessary they could move out. It was like the solution to a double dummy bridge problem. Levering Smith, who was then in command of the Polaris fleet, said it would suit him. We published it but of course nothing happened. Implementing a good policy is far more difficult than inventing it.

THE BIOPHYSICS GROUP AT THE DEPARTMENT OF TERRESTRIAL MAGNETISM

This group grew out of a wartime cyclotron-oriented biophysics group, and initially Phil Abelson was group leader. It was really created when Dick Roberts joined Abelson and Dean Cowie after convincing Merle Tuve (director of DTM) and Vannevar Bush (president of CIW) that a permanent group might do good science. Soon it was joined by Ellis Bolton and later by me. For a long period all of us were jointly listed as the authors of all of the work in the annual reports. The group members were all very cooperative, and the research interests ran in parallel for many years as a result of continuous discussion, but it never operated as a research team with appointed jobs. Dick had chosen to give up personal power when he turned down a good offer from the Kellex Corporation and left the Ap-

plied Physics Laboratory. He could afford to just be interested in science. That attitude and the opportunities offered by CIW mostly suppressed personal ambition. The five of us formed the core of the group for the longest period, though many others, including Brian McCarthy, Dave Kohne, Bill Hoyer, Nancy Rice, Tom Bonner, and many important fellows and visitors, were part of it.

Dick Roberts was, as I remember it, responsible for the "philosophy" or underlying set of attitudes which set the strategy. One was that basically simple processes were responsible for biological complexity and another was that (anathema to many biologists) a physicist could step in and devise ways to isolate these processes. Whatever one may think about their validity, these are fruitful attitudes and have changed the face of biological knowledge. This summary is best divided into three periods corresponding to two volumes which record the research published in 1955 and 1964 and then the succeeding decade or so. For a fuller history, see page 656 of the second of these volumes or page 172 of *CIW Year Book 74* (1975).

STUDIES OF BIOSYNTHESIS IN *E. COLI*

Biophysics was redefined as "quantitative research in biology carried out by investigators trained in physics" (*CIW Year Book 50* [1951]), in preference to the customary meaning of the time which was instrument development in support of biological research or medicine. After Dick took the phage course, the attitude developed that the host *E. coli* was more interesting and should be studied during exponential growth so that "normal" pathways of synthesis and processes could be examined. The early interest was in transport and permeability and later moved to biochemical pathways. They both represented opportunities for new insight deriving from radioactive tracers. It is hard, even

for me, to sympathetically reimagine a period in which it was not yet proved that DNA stored the genetic information and in which the only useful radioactively labeled materials were targets bombarded in the cyclotron and from Oak Ridge, ^{14}C as barium carbonate, ^{32}P as orthophosphate, and ^{3}H as hydrogen gas or water. Anyone interested should get hold of this volume (1955,3), which became known as the *E. coli* bible. Dick initiated this writing project and was the driving force, though it cost us all a year. I met someone only this year at a meeting in Cambridge who took the trouble to come up and say how much it had helped him in the lab. While the term "feedback inhibition" was devised by others, its existence was proved by the work of the group during this period. A high point of Dick's contributions might be the quantitative analysis and proof that the Krebs cycle (previously recognized as a component of carbohydrate metabolism) was important in the synthetic activities of *E. coli.*

MACROMOLECULAR BIOSYNTHESIS

The next period is reflected in a book which Dick put together (1964,1) including all of the reprints of the group for the period and selected annual report sections with comments interpreting their current significance. It reports the transition from investigation of pathways of synthesis of small molecules to studies of ribosomes. In this period, Dick became interested in the code and particularly studied a doublet code, which reflected what we would now refer to as degeneracy, but the influence of this work was minor. The application of the sucrose density gradient to macromolecules came out of Dick's attempts to use layers of sucrose to fractionate ribosomes for kinetic studies. The measurements (with Kenneth McQuillen) of the presence on ribosomes of nascent protein were driven by

Dick's enthusiasm to manually carry out experiments fast enough to catch the process where the turnover time was only a few seconds. The result was the proof that ribosomes rather than some other component of the microsome fraction were responsible for protein synthesis.

THE BRAIN

Over the years, Dick's interests moved to the intractable subject of the operation of the brain and the mechanisms of memory, and much of the work of this period was in cooperation with Louis Flexner. The rest of the group never became closely involved. The experiment Dick and Louie initiated was an attempt to determine whether protein synthesis was involved in the establishment of long-term memory. Mice were trained, and their brains were injected with puromycin. They obtained positive conclusions but later withdrew this interpretation, since memory was restored by intracerebral injections of saline. This was pioneering work which I am assured by experts is now carried out successfully. Dick states in 1974 (*CIW Year Book 75*, p. 178): "Puromycin blockage appeared to be caused by the formation of puromycin-peptides, which adsorbed to receptor sites and blocked certain synapses. Presumably these are receptors for catecholamines as the puromycin has a structural resemblance to these compounds. Thus, experiments designed to demonstrate a role for protein synthesis in memory formation ended in implicating the catecholamines. . . . Roughly 20 papers have been published. . ."

In the same review of the history of the group, Dick stated: "We are pleased to have participated in this exciting period in the development of biology. We believe that we did make significant contributions and that, since some of us will carry on in different places, our history is not

complete. We are especially pleased to note the contributions being made by 22 Fellows who received a part of their training with us."

During graduate school, Dick married Adeline Furness (November 1935), and their children, Dick junior and Julie, were born during the early days at DTM. After a divorce, Dick married Irena Zuzanna Eiger (December 1948) and they had a son, Tommie. After Irena died, Dick married Josephine Taggart Rice (January 1967). I have known all of Dick's children, and though they did not always agree with their father and have not gone into science, they are excellent human beings and reflect credit on Dick. On Saturday, April 4, 1980, Dick was playing golf, his lifetime favorite game, and collapsed of a heart attack, dying as he might have preferred.

NOTE

1. The document is called "Autobiography of Richard Brooke Roberts," written from 1977 to 1979. It has sections called "Genetics" (describing his family), "Chronology," etc., and I will quote freely from it, referring to it as "AB." It is typed by Dick's own hand, mostly on paper headed "Quarterly Review of Biophysics." I have asked the Academy to include the whole in their files because it creates a sense of presence in many circumstances that I will not be able to quote. There is a freedom of style to it that reflects his sense of fun and is responsible for some of the looseness in this memoir. It is not a balanced autobiography (35 pages) but is the sort of thing historians may value for certain gems.

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