



MAX DELBRÜCK
(1906-1981)

INTERVIEWED BY
CAROLYN HARDING

July 14-September 11, 1978

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Pasadena, California



Subject area

Molecular biology

Abstract

Interview in 1978 with Max Delbrück, professor of biology emeritus, begins with his recollections of growing up in an academic family in Berlin. Trained at Göttingen in the late 1920s as a theoretical physicist, he later switched to biology, inspired by Niels Bohr to investigate the applications of complementarity to biological phenomena. After postgraduate work at Bristol and Copenhagen, he returned to Berlin in 1932 to work for Lise Meitner and formed a “club” of theoretical physicists, biologists, and biochemists, who met for discussions at his mother’s house. Recollections of the advent of the Nazis in 1933. In 1937 Delbrück left Berlin for Caltech on a Rockefeller Fellowship; he defends the decision of other German scientists, notably Heisenberg, to remain in Germany. At Caltech he began working in *Drosophila* genetics but quickly shifted to phage work with Emory Ellis. Moved to Vanderbilt University in 1940, where he remained for seven years; comments on Oswald Avery’s identification of DNA as the “transforming principle.” Recalls his association with Salvador Luria and summer phage group at Cold Spring Harbor in the 1940s; joint letter with Linus Pauling to *Science* in 1940 on intermolecular forces in biological processes; his

reaction to 1945 publication of Erwin Schrödinger's *What is Life?* Returned to Caltech in 1947 as professor of biology; comments on activities of Biology Division under chairmen George W. Beadle and Ray Owen, and the psychobiology of Roger Sperry. Recalls 1953 Watson-Crick discovery of the structure of DNA; comments on Watson as director of Cold Spring Harbor and on *The Double Helix*. Comments on receiving (with Luria and Alfred Hershey) the 1969 Nobel Prize in Physiology or Medicine. Recalls his later work on *Phycomyces*. The interview ends with Delbrück's overview of the history of German science and its travails under the Nazis, and recollections of his postwar visits there.

Administrative information

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Caricature of Max Delbrück by Hans Gloor, 1950s.

California Institute of Technology
Oral History Project

Interview with Max Delbrück

by Carolyn Harding

Pasadena, California

Caltech Archives, 1979

Errata:

p. 2: “*Preussische Jahrbücher*”—Correct spelling is *Preussische*.

p. 6 fn: Should be *Schweigermutter* (one word) and *im neuen Haus*.

p. 26: “He’s still alive”—Hans Kienle died in 1975.

p. 63: “tobbaco mosiac virus”—Correct spelling is tobacco mosaic virus.

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CALIFORNIA INSTITUTE OF TECHNOLOGY
ORAL HISTORY PROJECT

Interview with Max Delbrück
Pasadena, California

by Carolyn Harding

Session 1	July 14, 1978
Session 2	July 18, 1978
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Harding: Suppose we begin with your family, your father.

Delbrück: My father was a professor of history at the University of Berlin and 58 years older than I, so he was practically my grandfather, and I never knew him in the part of his life when he was still struggling. His specialty was the history of the art of war and material criticism of the sources. Previous historians had largely compared written sources, and by comparing them had tried to figure out what was right and what was wrong. He went much further and saw in the written sources a generally very corrupted description of a battle. He actually tried to find out the logistics available then and what was the terrain: could soldiers actually run these distances and could that many soldiers of this kind be provisioned and so on, and eliminated thereby many things that had been perpetuated as legends. He did this through all periods of history - comparisons of the feudal regime of the Persians with the feudal regime of the Middle Ages, and the first organized infantry of the Greeks with the first organized infantry of the Swiss in the fifteenth century, and so on. So he illuminated history in that way. He taught a two-year course in general history ranging from the Egyptians to modern times, "The history

of the art of war in the framework of political history," which was published in a number of volumes incorporating the results of vast numbers of Ph.D. theses.¹ Then after he retired he also published his lecture series as a Weltgeschichte.²

I was the youngest of seven children, four sisters and three brothers. My four sisters are still all alive, the oldest is going to be 88 this year. My oldest brother was killed in action in the First World War; I knew very little of him because he was sent to a boarding high school, and then he was at the University, and then he was in the war and was killed; he was fourteen years older than I. My other brother, Justus, was four years older and of him I saw an enormous amount; we shared a room for quite a number of years of my adolescence and my relation to him was a very great mixture of admiration and competition and all things that siblings can have. Now looking in retrospect he was an exceptionally kind and friendly and by no means a domineering or intellectually threatening person, but my whole soul was concentrated on trying to compete not only with him but with the other older siblings, and the older ones in the Bonhoeffer team, and the older ones in the next house, since I was the youngest in all these contexts.

My father was not only this professor of history and scholar in the sense described, which kept him very busy, but he was also editor of a monthly called the Preussische Jahrbücher - that's a monthly maybe somewhat analogous to the Atlantic Monthly. He single-handedly edited that for at least thirty years and this included also writing a political column every month of sixteen printed pages in which he commented on German politics, internal and external, every month year in and year out, so he was a busy man.

Harding: What was the circulation of this journal?

Delbrück: I never found out what the circulation was, but it was not very wide, but several thousand I'm sure. It was read quite a bit in

1. Hans Delbrück, Geschichte der Kriegskunst im Rahmen der politischen Geschichte, 7 vols. (Berlin: G. Stilke, 1900-36).
2. Hans Delbrück, Weltgeschichte: Vorlesungen, gehalten an der Universität Berlin, 1896/1920, 5 vols. (Berlin: O. Stollberg, 1924-28).

government circles and among the high school teachers and academic circles, so it had some influence but not nearly as much as it should have. There's a book on my father called Hans Delbrück as a Critic of the Wilhelminian Era,³ and that's what he was; in his political column he was often very critical of what went on and several times he had run-ins with the imperial government.

Harding: Was his job ever in jeopardy?

Delbrück: I don't think his job was ever in jeopardy. That would have been an extreme thing for a German professor, but there were some bizarre things. I think once Herr von Jagow, the President of Police of Berlin felt insulted and challenged him to a duel. My father declined; he thought it was a very improper thing for the President of the Police to challenge a journalist to a duel if he didn't like his criticism. At one time, I've forgotten, there were some unpleasantnesses with the government. The Kaiser at times was very angry at him and tried to do him in but was prevented from doing so.

Our nearest relatives who lived next door, the Harnacks, were similar. The old man Harnack, Adolf von Harnack, was also very much in public life and also had historical interests. He was a church historian and public servant. He was Director of the Prussian State Library and of all Prussian libraries, sort of an office to supervise and develop the libraries. Most important he became President of the Kaiser Wilhelm Gesellschaft when it was founded in 1910, I think. So he stood very much in public life, but in contrast to my father he was a very diplomatic man, although he was also very critical of what went on and very perceptive, and loved to discuss all these matters with my father every Sunday night. He never expressed himself in public so as to incur the wrath, at least not the way my father did. He also had a great deal of ugly business but that had more to do with church questions. When he was made Professor of Theology at Berlin there was a great question of whether he was orthodox Protestant enough; in fact the whole proposition was almost scuttled. There

3. Annelise Thimme, Hans Delbrück als Kritiker der Wilhelminischen Epoche (Düsseldorf: Droste-Verlag, 1955).

exists a biography of him by his daughter, Agnes von Zahn Harnack, a very good biography.⁴ Anyhow they had numerous children that were on the average ten years older than we were, and the Harnacks and the Delbrücks assembled almost every Sunday night either at the Harnacks or at the Delbrücks. It started out very informally and everybody talked with everybody and also played games but gradually it led to these more serious conversations and the others had to pipe down.

Harding: At these Sunday evening dinners...

Delbrück: Not dinner. I think it was dinners before the First World War but then life became difficult during the war and after the war and then it was just after dinner.

Harding: I see, and food got short. Were the discussions usually about politics or were they also about history?

Delbrück: Also about history quite a bit. Whatever they happened to be interested in at the moment - both politics and history.

Harding: How about your mother?

Delbrück: My mother was, I think, fifteen years younger than my father. She was 42 years older than I and so I did not know her as a young woman. I have heard her described as on the timid and shy side. She, I think, also was the youngest of her family and she got married when she was 19 or something and my father was 35 and she was expected to be and was very submissive. She also was of fragile health, which is no surprise, having had a large number of children and having gone through very difficult times during the First World War. You see I was born eight years before the war so my recollections essentially start with the first war and the hunger periods during that time.

Harding: Did she remember her grandfather, Justus von Liebig?

4. Agnes von Zahn Harnack, Adolf von Harnack (Berlin: Walter de Gruyter & Co., 1951).

Delbrück: I don't think so, no. She grew up in Leipzig. Her father was the surgeon general of the allied armies in the Franco-Prussian war in 1870 and Professor of Surgery at Leipzig, so that family grew up in Leipzig. Her mother's father was Leibig. Liebig lived the second part of his life in Munich so I don't think there was much contact between these families. I don't recall her ever mentioning him. Liebig died about 1873 and then my mother would have been about 10. I remember my mother's mother who was Leibig's daughter very well, but she was a widow. I don't remember my mother's father, the surgeon. He had probably died before I was born. But my mother's mother lived on in Leipzig for many years and occasionally we visited there. She apparently was a very intelligent and attractive woman. Her other son-in-law, Adolf von Harnack, quite often traveled with her. He abandoned his own family and traveled with his mother-in-law. They went to Italy together. I remember at her funeral Harnack made a very, very nice speech characterizing her. I don't remember what he said but it was obvious that there was a very friendly and admiring relation between the two.

Harding: How did it happen that the Delbrück and Harnack families lived next door to each other?

Delbrück: They had both been living in Berlin, I think, and then one moved out and built a house, or bought a house that had been built, and then the others; I think we were probably the second. We bought a lot and built a house there in 1906, the year I was born. This whole section of the suburb of Berlin was just crawling with professors with large families: the [Karl] Bonhoeffers around the corner, and the [Max] Planck family a little ways down, and the mathematician Hermann Amandus Schwarz, and probably quite a few others. Professors with large families intermingled with moderately successful businessmen. Some of the houses were quite palatial, but the houses that the Harnacks and the Delbrücks and the Bonhoeffers built were straightforward accommodations for large families, nothing very fancy about them.

Harding: Did you share rooms or have your own?

Delbrück: We shared rooms, indeed yes, first with my sister Emmi who was a year and a half older and then with my brother Justus until we left home.

Harding: Were you close to your parents?

Delbrück: I was very close to my mother and I had a very ambivalent relation to my father, of which I was not conscious when I was a child, but in retrospect it was just absolutely classically Freudian. Not until many, many years later did I resolve this subconscious hatred and jealousy mixed with admiration and fear and respect. Once many years later, when we lived in Nashville, my wife and I, one Saturday night as we were visiting some friends and having drinks, this friend of ours asked me, "Why do you work so hard?" (These were not scientists, the husband was a businessman). And the man said, "I'll tell you, I'll tell you. He works for the woman he loves". In thinking about it, I do in retrospect think that was more true than he intended, meaning that I did not work for my wife, but for my mother to outshine my father. This can be presumed to be a strong motivation.

Harding: Your sister's recollections suggest that your father was very interested in hearing his children's opinions about things and in encouraging them to develop arguments.⁵ Would you say in the sense of emphasizing logic and reasoning and forming opinions that he was an influence on you?

Delbrück: I would say he wasn't that systematic an educator or in any way took a detailed interest in our education. He just liked open talk and it's true that he fostered that. My sisters of course, again in standard fashion, all loved and admired him and were very, very fond of him.

5. Emmi Delbrück Bonhoeffer, "Meiner lieben Schwieger mutter Paula Bonhoeffer zum ersten Geburtstag in neuen Hans Marienburger Allee 42, 1936," Max Delbrück Papers, Box 9, California Institute of Technology Archives.

Harding: I haven't been able to find the names of all your sisters and brothers, and when they were born.

Delbrück: My oldest sib is Lore (Laura), a sister, and she, as I say, will be 88 this year so that means she was born in '90. The next one, Waldemar, a brother, was born two years later, let's say 1892. And then the next one, Hanni (Johanna), she must have been three or four years younger, born in 1896. And then the next one, Lene (Helene), she will have her 80th birthday this year so she would have been 1898. And then my brother, Justus, 1902, and then my sister Emmi (Emilie), 1905, and then I in 1906. So that gives a total span of 16 years.

Harding: Can we talk a little bit about the intellectual and cultural environment in your home? Besides history and politics was there much interest in the arts, literature, philosophy, science?

Delbrück: Okay, let's start with science. There was none. There was no knowledge and no interest and no competence at all. In art I would say it was very modest and conventional. In music neither my father nor my mother were musically gifted or trained; my father not at all and my mother had very modest competence in singing and piano playing. But some of my sisters and I played a little bit of various instruments and there was occasionally chamber music. It was much less than at the Bonhoeffer's and more than at the Harnack's. None of these groups were really outstanding in musical performance. Philosophy, my father had a great interest and his hero was Hegel for philosophy of history. In fact in his study he had two busts, Hegel and Ranke. I never understood what he saw in Hegel and I still find Hegel a very unprofitable author to read.

Harding: When did you first read Hegel?

Delbrück: Well, I tried at various times, especially in my student years when I had this older friend, Werner Brock, who made me read

a lot of things and also made me read Hegel, so I got first glimpses of what Hegel is talking about from him. And then at various times later I tried again to read him but, as I say, it's mostly not my language. I guess Hegel did make his greatest impact really through his influence on Marx and that's an entirely different story. His attempts to influence philosophy of science have been, I think, a total and dismal failure. And political philosophy I never got close to in my younger years.

Harding: Did your father talk much about Marx?

Delbrück: He talked occasionally about Marx and he also wrote a little book about Marx, very negative, taking Marx to pieces especially with respect to his predictions as to world revolution and classless society and all these things.⁶ Strangely, the Marxists themselves were very sympathetic to him because in a way he represented a movement in historical research which was very close to their hearts, namely, investigating the material basis of history. So when he died actually the Marx-Engels Institute of Moscow came around and wanted to buy his library. They didn't because it just didn't work out. So there was this strange relation between him and the Marxists.

Harding: Economically was your family pretty well off until the war?

Delbrück: I think they must have been until 1914 moderately well off, I would say. My father had his salary and his income as editor and my mother had a dowry from her father, the surgeon. The dowry, though, apparently was very much less than my father had expected, because this surgeon happened to be the exception to the rule, and was not grabbing for money but actually amassed only a very modest fortune. But my father was a very careful manager and he managed to increase what he had acquired by marriage so there was a modest degree of affluence and apparently the life until 1914 was pretty free

6. Hans Delbrück, Die Marx'sche geschichtsphilosophie (Berlin, 1921).

and very hospitable. Emmi, one of my sisters, gets quite ecstatic about the wonderful parties they had and how hospitable they were. As the war came and became more and more of a nightmare in every respect of course all this darkened. In a way the First World War was much worse than the second one because I think many more people were killed. On my mother's desk she had a complete circle of nephews and young friends of the house who had been killed in the war. I think three quarters of the young men in the family were killed. So that was all very sad and in addition then there came these pretty severe food restrictions and then the total mess in 1918. So this Villen-Kolonie, this relatively affluent residential suburb after the war became almost a ghost town. Every one of these houses was subdivided into smaller apartments and there were very few children around and it became very sad, but gradually it filled up again. Our house also was subdivided then; the apartments that were made in the house were sometimes rented out to strangers and some were rented out to married children. Two of my married sisters with their families lived in the house for a number of years. One was the cause of tremendous tension because that branch of the family for awhile was very Nazi sympathetic, whereas the rest of the family was not. So that gave rise to some extremely violent scenes in the early thirties and middle thirties until they moved out. That breach in the family was not bridged over until many years after the war.

Harding: How did the war (W.W.I) affect you personally?

Delbrück: Well, the worst thing was that during the war every boy was expected to join the Boy Scouts, whether he liked it or not. Not only had he to join the Boy Scouts but he was expected to love it. The Boy Scouts went out every Sunday morning from 8:00 a.m. to 1:00 p.m. rain or shine or snow and engaged in some kind of military games, marches and fights and what not. It was just ghastly, especially if you were a small and timid boy as I was and had all these other characters to contend with. And my family tried in every which way to get me out of this nightmare, building me golden bridges, but I felt

constrained to pretend that it was my greatest wish to participate. Finally I think it was arranged that the family physician just put a real veto in. So that's how the war affected me. Otherwise,... food was getting pretty scarce, food and coal heating.

Harding: Do you remember being cold and hungry?

Delbrück: Oh yes, oh do I remember! Yes, first of all you never got enough to eat at any meal. An hour after any meal you were hungry again and then my mother sent us to the bakery to buy some certain cookies there, but these cookies apparently were baked with flour that had been made by hydrolyzing wood with alkali. Wood is cellulose, you know. You hydrolyze it and you get sugar out of it. However this was done not enzymatically but with strong alkali, ammonia. So these cookies smelled very strongly of ammonia but we ate them. So that was the food part, especially the winter of 1917-18. And coal, our house was coal heated and coal was just not available for several winters, and so the house was instead heated with some makeshift ovens. My brother and I became great experts in rope jumping to keep warm; between doing our school work every ten minutes we jumped up. Both of these are not serious things because neither of them had lasting aftereffects. Others were subjected to much more serious damage; in fact our life as children was not bad.

Harding: Was there much interest in or discussion of religion in your family?

Delbrück: I would say not. No, it was assumed that you believed in God and that you were protestant. My father more or less ceremoniously at every Christmas time assembled the family and read the Christmas story from St. Luke, and the family went off to church for the Christmas service and maybe also at Easter time, but otherwise there was very little religious education. We went to hear the Passion music, the St. Matthew Passion of Bach. That was almost required, and I developed a great fondness specifically for this; in my later

student years I played quite a bit of the piano part. But no, neither the Delbrücks nor the Harnacks [discussed religion much], even though the Harnacks might have had more occasion to do so.

My sister Emmi had religious scruples and there exist two letters to her from her uncle Harnack who was then 70 and she was 18. He answered her letters at great length - interesting letters, I think I translated them recently. I didn't know about these letters and my sister sent them to me a few years ago. And I thought it was quite remarkable that Harnack who was such a very busy man took time out to write long handwritten and very well considered letters to a young girl. But Harnack had a tremendous ability to write and speak eloquently, amazingly, in contrast to my father who was not an easy speaker...Even such things as speeches at wedding parties there were always obligatory speeches of various kinds to the mother of the bride, to the bride and groom. My father would have to think for several days about what he was going to say. He asked Harnack once after the wedding of one of my sisters, when Harnack had made an absolutely marvelous speech for my mother, how long he had thought about it. Harnack said, "I assure you I had not given it a single thought until the moment I knocked on my glass and got up." It was remarkable. I believe that he was telling the truth.

Harding: Let's continue discussing the friendships with the Bonhoeffers and the Harnacks. The families were quite close.

Delbrück: The families were quite close insofar as there were overlaps in age; there wasn't much between the sibs, although I don't know about my older sisters and brothers because they were too far above me. There was a fairly good relationship with Agnes von Zahn Harnack, who was one of the daughters, because she was a teacher. She was a very well-educated woman; she had a Ph.D. and was a high school teacher in a private school. I think several of my sisters went to that school, so they knew Agnes as a teacher, and I also got to know her a little later. She was very friendly, was interested in young people and I was one of the young people. She and her husband,

Karl von Zahn, had a play reading circle. Four or five or six people got together and divided the play in parts and read and I was included a few times, which was very nice.

Another son of the Harnack family, Ernst Harnack, a Social Democrat in 1918-19, when Germany changed from Imperial Germany to the Weimar Republic. This young man, who had been a lieutenant in the Hussars, made a career in public administration and ended up as the Regierungspräsident in the Regierungsbezirk Köln... At that time the mayor of Cologne was Mr.[Konrad]Adenauer who at that time was just a mayor of a big city. They were on a par as it were, one was a Regierungspräsident, the other was mayor of the biggest city. In 1933 they both got fired by the Nazis very quickly, so they both lived in retirement in the suburbs of Berlin and I saw this fellow Ernst von Harnack quite often. He was a very restless person, and now with nothing to do except write his memoirs, but writing wasn't really his cup of tea at all. He was also a musician, played the flute. We were quite often at his house. One Sunday winter morning he invited me to come on a walk with him... He said he would take Adenauer along. Adenauer at that time was already a well-known name because although he was just mayor of a big city he had been quite a spectacular mayor, and had also played a role apparently in political constructions that aimed at secession of the Rhineland from the rest of Germany during the time of the early twenties. But he had been careful enough so that later on this never could be proved, and he was too smooth to really let that block his later career. Anyhow that Sunday morning we went there to go on a walk with him in the snowy forest and beforehand Harnack had told me, "Now watch out. He probably doesn't want to go for a walk because it's so mushy snow but we'll insist." And I said, "Well, we'll insist." And this Harnack was not a man to be fooled with and if he insisted then he insisted. But it was just immensely impressive because the minute we got to Adenauer's house, he came out, "Ah, good morning gentlemen. Let's go for a walk... Let's, let's have a little drink before we go. That's always good for the road." So we sat down and for two hours Adenauer talked, and talked incredibly interestingly about personalities and politics, and Harnack never said a word

mentioning a walk. Finally after two hours Adenauer said, "Well, gentlemen, shall we step out for a few minutes?" At that moment I knew that this was a man of a different order of magnitude. And after the war when I came back to Germany one of my first questions was "What about Adenauer? Has he reemerged?" Because he kept totally quiet during that period. He lived in his little house near Bonn and had his rose garden there and never said a peep. So after the war I asked, and people said, "Yes, I think he is founding a new party which is a mixture of Catholic, Protestant and so on." That was in '46-'47. Within two years he was on top. But in '46-'47 there was just barely a rumor that he might be coming to the fore and then very quickly he played everybody else against the wall, including the French, British and the American occupying forces, and they each had their top man - whoever was running the German government and had to play these three against each other, be on good terms with all three. And Adenauer did all his maneuverings with the same kind of style - never did it arise that anybody could have another idea than his idea or that anything else would count. That was very impressive to me to see such a man with such savoir faire in action.

Ernst Harnack was one of the ones who participated in the German Resistance during the war and was executed by the Nazis. There exists a writeup about him by his younger brother Axel Harnack⁷... On the Bonhoeffer side there was, of course, a much greater involvement in the Resistance and that has been very widely documented. They had this terrible thing that they lost two sons and two sons-in-law in the aftermath of the 20th of July 1944. One of the sons-in-law, Gerd Leibholz, the husband of Sabine Bonhoeffer, is still alive; he must be in his late seventies. He was a Staatsrechtsler - political science and constitutional law. He was Jewish; he emigrated with his family and they lived in Oxford. After the war he came back and became a Professor of Constitutional Law in Göttingen and a judge at the supreme court for constitutional law... A very able man and a very nice man, too. He belonged to this group of my brother Justus,

7. Axel Harnack, Ernst von Harnack, 1888 bis 1945: ein Kämpfer für Deutschlands Zukunft (Schwenningen a.N.: Neckar-Verlag, 1951).

and Klaus Bonhoeffer and Hans Dohnanyi and Gerd Leibholz. The four of them were very close; of this group he was the only survivor after the war.

My brother was imprisoned by the Nazis; his trial was pending but was incomplete at the end of the war. During the fall of Berlin when the prisons were opened he got out and all of a sudden turned up at one of my sister's. This was the nightmarish time, the fall of Berlin when the Russians closed in from all sides. Two of my sisters were living there in different suburbs and while machine guns were already shooting in all directions and everybody was hiding in their cellar one of my sisters heard the telephone ring upstairs -- it's a Kafka-like story that you hear the telephone ringing-- and she sneaked up and the other sister was on the phone and told her, "Justus has just turned up." So he hung around for awhile looking for Klaus. It was not known yet that Klaus had been executed or murdered rather, in one of the camps. And while Justus was engaged in that the Russians came and arrested him - apparently wanted him as a witness for the forthcoming Nuremberg trials. He was in one of these camps and was not heard from and only two years later we found out that in October of that year, '45 - that means four or five months after his arrest - there was a diphtheria epidemic and he and many others in this camp died.

So this year '45 was one of total chaos and standstill. The first things that the Allied forces had to do after the fall of Germany were to reestablish communication and public health and food and emergency services, but for four or five months none of that existed and it was just a free-for-all. The Russian zone was worse off because in addition to the local population they had to cope with the several million people who streamed in from the eastern parts that had been occupied by the Russians and the Poles. These people trekked first to East Germany and then spilled over to West Germany... So there was a tremendous amount of chaos.

Harding: How did your sisters manage?

Delbrück: Well, they managed, they managed to survive and all their children, except one little baby girl of Justus' widow, survived. At the end of the war I found myself like Joseph in Egypt - all of a sudden the uncle in rich America of 17 fatherless nephews and nieces in Germany. My wife and I spent an enormous amount of time wrapping and mailing packages, at first by various contrived routes through military people, and then when the Care packages came in it was simpler. They all survived and are healthy in body, if not in soul. I mean there are traumas that remain...but they are relatively little. Well, everybody shows the scars of what he has gone through.

Harding: Why don't we back up again to your childhood and talk a little bit about the development of your own interest in science and other areas?

Delbrück: I really am not sure whether I remember accurately and in sequence. I think I did have a special interest in math but I don't know whether that preceded my interest in astronomy or followed it. I certainly had a high school teacher who was very friendly and took a great interest in me and gave me lots of private instruction on the side.

Harding: Do you remember his or her name?

Delbrück: His name was Simon. I would say that he was a very friendly person but he was certainly not the most inspiring teacher. I mean he was marvelous in taking this personal interest in me and taking me out on Sunday walks picking up mushrooms and things like that, and occasionally showing me some things about mathematics that was not in the class. Other teachers were more impressive, especially the Greek teacher, Walter Krantz. This is referring to high school now, the last four years. And this transition from pre-high school to high school, from Obertertia to Untersekunda was

a fantastic one because it coincided with the end of the war. So the real teachers all of a sudden turned up, whereas until then we had limped along with the 4F's, and you can imagine the combination of 4F's with boys in their rowdy years was disastrous. So Easter 1919 - we changed grade always at Easter time - these teachers who had been in the war came back. This teacher of Greek actually had been a captain in the army and as such automatically demanded and received respect and discipline with no problem at all. Where the classroom had been total chaos until then, all of a sudden we were [well-behaved]. This also coincided with the transition in Germany to where you are addressed formally, from "du" to "sie". So all of a sudden we were addressed as gentlemen and we behaved as gentlemen. It was very impressive. Quiet reigned, scholarliness reigned, gentlemanliness reigned. It was unbelievable and everybody relaxed - relaxed from that point of view but also became quite dedicated to their work.

Harding: What did you read in your Greek class?

Delbrück: What did we read? Everything. It starts with just a textbook and then it very quickly jumps to Xenophon and Thucydides and then Plato and then the Tragedies. We didn't quite get to Aristotle. I would say I never got so I could read any text fluently, but we stumbled through these texts and got some passages in German, and Latin similarly. We had two varieties - 8 hours a week or 12 hours a week, the small Greeks and the big Greeks. I belonged to the small Greeks.

Harding: Did you have the sense that science was what you wanted to do?

Delbrück: I don't think so. No, excuse me - the last two or three years I certainly proclaimed myself an astronomer.

Harding: Did you have a telescope?

Delbrück: And I had a telescope, two-inch, and I read popular books on astronomy, and I had a pal who had similar interests who is still a good friend of mine. He lives in Germany. He's a high school teacher, retired, and we correspond occasionally. His name is Manuel Michaelis de Vasconcellos, a Portuguese name; he is one-quarter Portuguese and in fact by citizenship he was Portuguese.

So we had our little astronomy club and after this had been going for awhile one day I got to talk to this older Karl Friedrich Bonhoeffer about it and it turned out that he knew much more about astronomy, being a real scientist. And he asked me a number of questions from which it was obvious that I had really very little insight about planetary motions or parallaxes or stellar properties and so on. So he quickly found out that I really didn't know much, and he told me a fair amount, and from that developed our friendship. He took a great liking to me and I, of course, admired him. I was very pleased that an older friend took an interest. (Almost all through my student years I had older friends, from whom I learned a great deal. I shifted universities for quite awhile, and in each situation I think I developed a particular friendship with some older person.) So I proclaimed myself an astronomer and then I almost became an astronomer. My interpretation of this, in retrospect (and this retrospect dates back now 40 years or something) is that I did that because I found it a convenient way to establish my identity for myself - that I knew something where nobody else knew anything. And it's true - none of the Harnacks, none of the Delbrücks and only this very much older Bonhoeffer was a scientist. So here I had my own thing which I could claim to know.

Harding: Did your parents encourage this interest in astronomy and science?

Delbrück: My father was very tolerant of it and my mother was very helpful in it. Tolerant is maybe the right expression because I really made myself a tremendous nuisance. I had my telescope set up on a little balcony which was adjacent to my parent's bedroom and then my bedroom was adjacent to that, so during the night to get

to this telescope I had to go through their bedroom. I remember a number of winter and summer nights where it was necessary for me to look at my telescope at 2:00 in the morning. Of course I had a sleep that could only be awakened by the loudest of alarm clocks, so I had this enormously loud alarm clock which awakened everybody in the house except me. And then finally I roused myself and crawled through my parent's bedroom thinking they were asleep - I'm sure my mother was worrying herself stiff that I would freeze to death out there. I spent an hour at the telescope and then crawled back to bed. So that's what I mean. They were very tolerant, and she made me a special, very warm dressing gown or something.

Harding: Do you happen to remember what books, scientific or semipopular, you read on astronomy?

Delbrück: No. They were probably standard, semipopular books, summarizing the planetary and stellar astronomy, such as it was at the time. I took a special interest in Kepler. I don't know why, but somehow he seemed to be the most romantic, so I had his portrait over my bed. At the final high school commencement exercises I had to make the valedictorian speech and I chose to talk about Kepler. I still blush to think that I plagiarized a large part of a speech on Kepler that I found in my father's library by some astronomer who had given his speech 40 years earlier.

Harding: Was it well received?

Delbrück: Oh yes, it was embarrassingly well received. I never admitted the plagiary.

Harding: If you were valedictorian you must have been quite a good student, especially with all those professors' children in school.

Delbrück: I don't know whether "valedictorian" is really the right word. I mean somebody was chosen to give a speech and I don't know

how I came to be chosen. We were all fairly good students by the time we got there - Ja, I was a fairly good student. There was a rough division between those who had to take oral exams and those who didn't, and I belonged to those who didn't have to take oral exams.

We cheated also in one other way, but that I don't blush about because that was really a sporting event. The Greek written exam consisted in translating a text, and the Greek teacher had to submit to the School Board, which is higher up than the school, several texts, three for the small Greeks and three for the big Greeks. The teacher had the right to bring in the vocabulary of these texts, so that we wouldn't be simply stymied by the vocabulary. But he said in the beginning, "I have checked. I know that Delbrück's father has so and so's Greek dictionary, which has references to all the places where these things occur in classical literature, and I have checked that these words are not in there, so don't try and figure out the text from that." Well, we were just absolutely overjoyed because he had misremembered and my father had a different dictionary. So of course we immediately formed a committee of three of us who got together every afternoon, and looked up all the words that he had given us and all the references, and so made a compilation of coincidences, and indeed we figured out three of the six texts. And in fact our little Greek text was among them. That was figured out the night before the exam so we had only a little time... I never learned as much Greek as in these weeks. So I don't blush for that. I think our teacher caught on to it actually and didn't mind. Maybe the whole thing was a plant on his part to make us really work on this vocabulary... It was a nice school, a good school on the whole.

Harding: Was science taught as well as mathematics ?

Delbrück: We had a very modest amount of physics and practically no chemistry. We had actually one small group who were taught a little bit of chemistry sort of as an aside by one of the teachers.

I didn't learn anything from that. We had in earlier years some absolutely miserable biology courses, unbelievably bad biology courses. Just something about the classification of animals and plants, unbelievably bad; nobody had an appreciation of biology. Biology at that time was not considered an interesting science. I mean the 19th century was essentially a century of systematics. Experimental embryology had begun to exist at the beginning of the century but hadn't penetrated into any high school texts at all. Genetics certainly hadn't penetrated into any high school texts by the 1920's. Biochemistry didn't exist. It was all very descriptive.

Harding: Would you say that there was a very great division, as I think there is today, between high school and college education?

Delbrück: That was true at that time. Whether it's true now I don't know in Germany.

Harding: Did you ever hear anything about Einstein? And Planck lived...?

Delbrück: Yes, Planck lived down the street but none of the family knew what he had done, even that he had gotten the Nobel Prize, or not sure whether he had. It was all very vague.

Harding: Though Harnack was president of the Kaiser Wilhelm Gesellschaft.

Delbrück: Yes, he probably knew something about it. I mean everybody knew that Planck was secretary of the Academy, of the Academy of Sciences and so on, that he was somehow a great scientist but what on earth he had done nobody knew.

Max Delbrück

Session 2

18 July 1978

Begin Tape 2, Side 1

Harding: Why don't we begin with the first university you went to, Tübingen. Did you go there with the intention of studying astronomy?

Delbrück: That I did.

Harding: Was there a particular reason for going there as opposed to some place else?

Delbrück: If so, I have forgotten. I guess I asked around where there were good astronomers and there was a reasonably good one, [Hans] Rosenberg, and he was sort of an astrophysicist, which at that time was a science just beginning. Astrophysicist meant that you didn't measure the position of the stars but the spectra of the stars, and used all kinds of physical devices that were just coming in - like thermocouples and spectrosopes and so on. He had a little observatory, the university observatory there, and I think we were a total of three students of astronomy. Of course I had just come from high school; I was seventeen-and-a-half and had to take lots of other courses besides, mathematics and physics. I took mathematics courses quite seriously and I took the physics courses much less seriously and I took one chemistry course. I mean I didn't take it — I went to one lecture and actually I think attended one or two chemistry lab sessions, but this wasn't my cup of tea at all. And so I never learned any chemistry while I was a student. I had to learn physics and chemistry the hard way later on.

Harding: You were only at Tübingen for one semester, the summer of 1924.

Delbrück: Well this was partly because this was just after the end of inflation and that means that people were very poor at that time. My father decided he couldn't afford to send me away to study, because I could study free of tuition at Berlin but not at another university, in Berlin because he was a professor there, even though he was emeritus. So the first semester my older sisters intervened on my behalf and prevailed upon him to let me go, but by winter he decided that it was too expensive and I had to come back.

Harding: Do you remember what you took in Berlin that winter semester of '24-'25?

Delbrück: Well I guess more regular astronomy, stellar astronomy, from the Director of the Berlin Observatory [Paul] Guthnick, a very dull person, and the Director of the Astronomischesrechen Institut, that's the one that puts out the astronomical almanac. I have forgotten the details but I also took classes, I think, from [Max] von Laue, a physicist, and Planck. I was a terrible student as far as attending lectures goes. I mean I did attend but I always was late; because I was late I sat in the back row, and never could get myself organized to take proper notes and look at them afterwards.

Harding: Why, would you say?

Delbrück: I don't know. I never learned to really make use of the university offerings at that time. I wasn't mature enough at all.

Harding: When did this attitude change and you began to feel that you wanted to do theoretical physics seriously?

Delbrück: Oh, not till much later, till my second or third year in Göttingen. I went to Tübingen, then to Berlin, then to Bonn,

and then back to Berlin. I think during that winter semester in Berlin I took on a part-time job working at the tower telescope of the Einstein Foundation, which was a high-resolution telescope spectroscope, designed and built by a man named [Erwin] Freundlich; and he hired me as an undergraduate helper. This tower telescope was erected in order to confirm or deny one of the implications of general relativity--the red shift of the lines in the gravitational field of the sun. So I worked there as an undergraduate helper. I wasn't much help.

One bizarre thing happened there. This tower telescope was located on the premises of the Potsdam Observatory outside of Berlin, which was a big observatory with quite a number of telescopes - quite a famous observatory. The Director of that observatory at the time was a Professor [Friedrich] Ludendorff, who was in fact a brother of the General [Erich] Ludendorff, who had been the principal agent in the final collapse of Germany; I mean he had lost the war for Germany. My father had been on government committees of the Weimar Republic, parliamentary commissions investigating on the one hand the origins and causes of the war, and on the other the origins and causes of the collapse and defeat. He had been in this commission extremely critical of Ludendorff, and had also published a little pamphlet called "Ludendorff's Self-portrait."⁸ This was actually a book review of several volumes of memoirs by a number of people who had been principals in the war, but especially of Ludendorff (that's why he called it "Ludendorff's Self-portrait"), and it was a very scathing condemnation. Well, when this fellow Ludendorff, the Director of the observatory, found out that I had been hired he thought this was a special taunt against him by Freundlich--that

⁸Hans Delbrück, Ludendorffs Selbstporträt (Berlin: Verlag für Politik und Wirtschaft, 1922).

Freundlich had sneaked me in there just in order to irritate him. He called up Freundlich and said, "I will not permit this man to walk across the premises, this son of the person who insulted my brother so much." Rather a strange situation. It was finally resolved - he couldn't do anything about it - but whenever I met him each of us looked the other way.

Harding: Do you remember being interested in Einstein's theory of general relativity?

Delbrück: Well, yes. Interested but quite incapable of mastering the technical aspects of it at that time. Gradually I got around to learning enough mathematics that goes with it to get a fair understanding...

Harding: What was the reason for going to Bonn?

Delbrück: Oh, just, I suppose, it was supposed to be a good place.

Harding: By then had your family fortunes improved?

Delbrück: Not really. My allowance was still so meager that I could afford to have a cup of coffee only once a week.

Harding: Since you didn't particularly enjoy studying or attending classes, what were the things that you did enjoy doing?

Delbrück: Well, I guess I studied from books and I talked a good deal to older students.

Harding: About science?

Delbrück: Science and other things. I learned most about science from older students, not from the professors much. Actually, in Germany astronomy was altogether pretty bad at that time. It had been ruined by the overambition of the generation of astronomers 50 years earlier. The first parallax of a star had been measured by [Friedrich Wilhelm] Bessel in 1837 and that was a tremendous triumph. You know, one of the most elementary implications of the Copernican hypothesis is that if the earth moves around the sun, then you should see the stars wiggle back and forth. This very elementary implication of the Copernican system had not been verified for 300 years - from 1543 when Copernicus was published till 1837 - and it required a tremendous refinement in position measurements. As a result the Germans had taken great pride in improving these methods more and more, not only measuring parallaxes but also the proper motions of the stars. If you wait long enough the star will move even if it is very far away. So making position catalogues of the stars was a thing in which the Germans took great pride; one of the first ones, Bonner Durchmusterung, I think was in the 1850's. But then in the 1880's or '90s they started a Katalog-der-astronomischen Gesellschaft which was a more ambitious undertaking, more stars, more positions; they figured that if they did that at that time, and then again 50 years later, they would have a vast number of proper motions and could really derive the structure and dynamics of the galactic system. By the time I was a student the 50 years were not up, but they were getting close to being up. But by then they had discovered that they had over-estimated the accuracy with which they had measured the positions 30 years earlier; they then decided that either they would have to wait another 50 years before they could really get sufficiently accurate proper motions, or would have to throw away all the earlier measurements and start afresh with the now improved methods. Well, you can imagine this had involved most of the observatories. So this agitated the meetings of the Astronomische Gesellschaft (which was really a European society that some Germans started),

and in the end I think they decided to throw away the old measurements and start afresh again. But that had ruined German astronomy, because all the young people who trained there, all they did was sit every night for hours and hours and measure transits of stars, which was a very painful thing to do because you couldn't even heat the building (otherwise the air was not quiet enough). It really had a disastrous effect on the intellectual quality of the German astronomers. And I came in just as there were a few people who decided it was time to really apply more sophisticated physics to astronomy. Rosenberg was one of them and I think a corresponding younger man in Bonn was called Hopmann, and in Göttingen the corresponding one was Hans Kienle, a very nice young fellow. He's still alive, I think, over 80 now. I think he lives in Turkey and has set up a popular observatory there teaching the Turks the elements of astronomy. He was a very charming man. He was the one in Göttingen who was so avant-garde. Göttingen, of course, was a much more exciting place than the other two because the mathematics was absolutely tops. It was the place where [David] Hilbert was and quite a galaxy of other mathematicians; in physics it also was tops because Max Born and James Franck were there.

Harding: Before we go on to Göttingen I'd just like to ask a few more questions. Scientifically I would think Berlin would also be a very exciting place with Einstein and Planck; or by then was Planck considered somewhat out of it?

Delbrück: Planck was out of it. Einstein never had students and Laue had students but was not an exciting teacher. He was too uptight in his personality. He was a very nice person but he was not easy. They had good experimental physicists there but the thing was that the university was right in the center of this very big city, and it took from our house where I lived about forty minutes to get there and forty minutes

to get back. The amenities in those days for big city universities were very poor; they had practically no public rooms at all, you just had to go there to the lecture and then go home again.

Harding: So that was basically commuting.

Delbrück: Yes.

Harding: While you were in Berlin did you become interested in drama or art? Did you see any plays directed by Max Reinhardt, for example?

Delbrück: Sure.

Harding: Do any stand out in your mind, any of the Shakespeare?

Delbrück: Yes, there was a Tolstoy, "The Living Corpse," and "A Midsummer Night's Dream," and several Gerhart Hauptmann plays. There was symphony, opera. There were other much more avant-garde things but I guess that most of the time I couldn't afford to go there. I went more to concerts. Berlin had a very outstanding symphony orchestra.

Harding: Did you hear Arnold Schoenberg?

Delbrück: Schoenberg was not conducting.

Harding: Did you hear any of his pieces?

Delbrück: No, I don't think so. Among the opera, the most exciting was Wozzeck by Alban Berg, who was a Schoenberg student, which I still think is one of the best operas there is. I heard it there but I'm not so sure I was impressed with it at that time. I was

absolutely overwhelmed with it when I heard it here in Los Angeles about 20 years ago. I can't remember what I thought of it then. I guess I thought it was pretty tremendous. On the whole, ... my tastes in art were more conservative.

Harding: Did you become interested in Freud at this time or later?

Delbrück: Later. No, I think at that time Freud was still very much frowned upon, if mentioned at all. A little later Freud became the rage ... Let's see, politically, well, we are talking about the student days in Berlin and that was '26, '25-'26, and politically I wasn't interested at all or aware of anything going on. Certainly I don't recall I had any friends who were politically either to the right or to the left. Those that were politically on the right weren't showing their colors yet, although they certainly existed in large numbers. And those politically on the left also, I think, were not very evident. Of course, the Communists were very bitter that the revolution in Germany had misfired. It was really a bourgeois revolution and not a Communist proletarian revolution at all. But at that time I think the Communists were still a pretty small minority. It was only after the crash in '29 as the depression deepened and deepened, and there were more and more unemployed people, that the radicalization both to the right and to the left gained so much momentum. But from '26 to '29 and even early '30 I was in Göttingen. That means I was doing a semester in Göttingen, and then during semester vacation came home or went on a trip, or something like that.

Harding: Why don't we move on and talk about Göttingen? How was the intellectual atmosphere there different from that at the other universities that you had been at?

Delbrück: Well, of course, it was just after the breakthrough of quantum mechanics which had happened in '25. In '25 Heisenberg had

discovered quantum mechanics and a flood tide of publications on this subject came out, most of which were out of date by the time they were published - everybody who was "in" had seen them circulate in preprint form. There was a very considerable influx of foreigners; [Paul] Dirac was there, [J. Robert] Oppenheimer was there, [Yoshikazu] Sugiura from Japan, [H. P.] Robertson from here at Caltech, [E. U.] Condon, are just a few names that I remember. So those were the physicists and the mathematicians also were in large numbers from abroad. So you really had a feeling that you were close to where things are really happening, which is a feeling that students do not usually have in most places.

Harding: Was it Heisenberg's paper, or the impact of his ideas, that stimulated you to go to Göttingen?

Delbrück: No, I went to Göttingen still as an astronomer, because of Kienle ... I guess I had heard while in Berlin, while working at the Einstein Tower, I had heard about Heisenberg's paper, rumors that a breakthrough had happened in this quantum thing. And I think Heisenberg came to give a seminar in the winter of '25-'26. I went to the seminar, didn't understand a word, but I remember as I walked into the building - the grimy old building, the physics institute in downtown Berlin, the lecture hall on the third floor, enormous staircases - as I walked in there, at the same time Einstein came in from one side and [Walther] Nernst from the other side. And I heard Nernst ask Einstein, [whispering] "Do you think there's anything to this? Do you think there's anything to this?" And Einstein said, "Ja, ja, I think it's a very good paper, very important." So they walked up there and the place was packed, standing room only. In the front row on the right were sitting Einstein and Planck and Nernst and von Laue. In the second row, the associate professors and on down, standing room only for the others.

Heisenberg describes this visit and I don't know how accurate it is. In Heisenberg's autobiography, which is an extremely interesting book, he has sort of stylized history. Conversations that took place over months he makes into a fictitious conversation; he describes this talk there, and that Einstein took him home afterwards and they talked for several hours. I asked Heisenberg a few years ago, whether they also had talked about a particular paradox of statistics, Einstein-Bose statistics, which is a separate and interesting story, how that came about. But Heisenberg said he did not talk about it at that time. I think we had some correspondence on that.

So I had heard about this sensational thing while still in Berlin and then I went to Göttingen, I guess that in Göttingen I essentially did not pal around with the physicists in the beginning but more with mathematicians and astronomers, [which changed] only when my attempts to write a thesis in astronomy failed. I was trying to write a theory on novae. Novae are stars that suddenly flare up and the question is why do they increase in luminosity, and how can you predict how that should happen, and so on. Well, to make a theory of that you would have to have a pretty good understanding of stellar dynamics, that means what happens inside the stars, how the temperature, pressure, ionization, radiation density, how they vary as you go from the periphery to the inside. There were beginning to exist theories of that, mostly by [Arthur S.] Eddington, the English astrophysicist, and [E. A.] Milne and [James] Jeans, all three of them English. I was trying to understand them, which was quite impossible for me, because the mathematics was beyond me and because they were in English and not in German and I didn't know any English at the time. Maybe some of these books had been translated, and some of it I heard in the lectures of Kienle, who also struggled with these theories. Anyhow this attempt to do something about the theory of novae was far too ambitious a project and didn't lead anywhere.

Harding: Did you choose it yourself or was it suggested to you?

Delbrück: I don't remember. Anyhow by then as a result of trying to understand this astrophysical theory of the interior of the stars, I had had to learn a good deal of quantum mechanics, and therefore had started paling around with some of the theoretical physicists, among them Pascual Jordan and Eugene Wigner and Walter Heitler. In fact I wrote a minute little paper on group theory in quantum mechanics, which was just filling out a proof that Wigner had somehow skipped in his paper. And then I asked Heitler whether he didn't know of a quick topic for a Ph.D. thesis. He suggested that since he and Fritz London had just made a quantum mechanical theory of the hydrogen molecule, which explained reasonably satisfactorily the strong bonding of the two hydrogen atoms in terms of what was called an exchange integral, it might be interesting to look into the lithium molecule. Lithium is the next higher homologue of hydrogen. Its innermost shell the K shell, is filled, and then there is one electron in a 2s orbit instead of a 1s orbit. So why do two lithium atoms which have electrons in the 2s orbit not make a strong bond the same way that the hydrogen atoms make a strong bond, having electrons in the 1s orbit? So I thought that's fine, that looks like something manageable. And that turned out to be a nightmare, because this is wave mechanics and perturbation theory; it involves calculating integrals over the space of the two electrons involved - that means six dimensional integrals with wave functions around two different centers.

Harding: And no computers.

Delbrück: No computers, no. Computers were not yet even on the horizon. So the question was what could you do by way of approximations, and by way of actually complete analytical integrations.

Well, by hook or by crook I finally put a thesis together. I have not dared look at it again and I understand that quite a few other papers have been written on this problem meanwhile, and maybe by now they know the answer to the problem. It was not a terribly interesting problem... Well in a way maybe it was interesting but it was not manageable. Anyhow it was accepted as a thesis and on the strength of this I was recommended by my professor, the official professor was Max Born, but Heitler was the one who directed or helped me a little - but I don't know how much he helped me. Anyhow Max Born recommended me for a job in Bristol University in England, and that was because the professor of theoretical physics from there had come to Göttingen also to learn quantum mechanics, and he was there only for three months and didn't know any German, so he decided he needed some more tuition; so I was recommended to teach him some more quantum mechanics.

Harding: This was [John E.] Lennard-Jones?

Delbrück: Lennard-Jones, yes. So after my thesis was accepted and before the oral exam I set out for Bristol to go there as a research fellow.

Harding: Before we go on to Bristol can we talk a little about your friends at Göttingen? You were there at such an interesting time with so many interesting people, and I wonder if you have any recollections of Born or some of the other students you were with such as [Victor F.] Weisskopf and [Edward] Teller?

Delbrück: I don't think Teller was there, and I think Weisskopf came there only the last year I was there, and I was not close to him then as I was later. I saw him a number of times. Yes, I guess maybe we did get quite friendly.

Harding: He says in his reminiscences that he remembers you and himself and Maria Mayer and Teller all learning quantum mechanics together and what an exciting experience it was.

Delbrück: I wonder whether he's mistaken about Teller. I thought Teller was in Leipzig and Weisskopf was also in Leipzig for a while. So he switched from Leipzig to Gottingen... Maria Mayer certainly was there.

Harding: How did you get along with Born?

Delbrück: Oh, very well.

Harding: Did he have students over to his house often?

Delbrück: Yes. Not only to his house but also on outings, outings to Nikolausberg. He had students and assistants to his house and then music was performed, or there were outings to Nikolausberg, and drinking beer and playing silly games, nice games, very relaxed.

I had two friends who were not physicists or astronomers but philosophers. One of them was Robert Heiss, and he later became professor of philology and psychology at Freiburg. The other one was Werner Brock. Werner Brock was the most important influence on me. I spent very much time with him, and also went on various trips with him -- a very close friendship. He was enormously knowledgeable, literally knowledgeable, but also insightful. He had studied psychiatry and art history and philosophy and medicine, and he had strong views on everything - and very interesting views - and he was also keen on expounding them, so that was wonderful. He also made me read a lot more than I had done before.

Harding: What did he make you read, besides Hegel?

Delbrück: Oh, almost anything. Even commentaries on the New Testament. Probably also political literature, psychiatric literature, Freud probably, and Jaspers -- Jaspers started out as a psychiatrist. And Heidegger, and a good deal of art history. Also poetry: Rilke, Hofmannsthal, George, Shakespeare's sonnets

Brock was half Jewish, and very unstable anyhow, and quite often played with suicide, so when the Nazis came and he had to emigrate it was really total disaster for him... Although he lived till a few years ago, he never got into any kind of a normal career, and he died in an institution. I visited him twice in the last years before he died in the asylum in Freiburg. A very sad situation. He was Werner Brock. Ja, I think compared to him the others were a very slight influence on me intellectually.

Harding: Did he encourage you to continue your work in science?

Delbrück: I think so, yes. Yes, hard science was the only thing in which he was not an expert, and I guess, come to think of it, that must have played a role in our relationship.

Harding: When you finished your doctoral dissertation do you remember how you felt, whether you felt like this was really exciting science and you wanted to pursue it?

Delbrück: No, I didn't feel that my dissertation was exciting science. No, I didn't feel that I was doing very well. I had not felt that I had been doing well in astronomy and I did not feel that I was doing well in physics; and I was just hoping that something would happen that I was doing well and was willing to carry on with. So then I got this job in Bristol. I must have gone to Bristol in September or something of that year [1929] not knowing more than a dozen words of English. So I spent the first

three months frantically learning English there. Bristol was an attractive place in the sense that the physics department there had just gotten a large sum of money and had expanded and had hired several young fellows, mostly from Cambridge, who were experimental physicists; they had good facilities there and were very spirited. One was C. F. Powell who rose to great fame as the discoverer of the pi meson, and several other important things in elementary particle physics, for which he got the Nobel Prize. He was my roommate and a very good friend. Another was H.W.B. Skinner, who in contrast to Powell did not have a very sunny disposition. He was more intelligent than any of us, I think, but he was a misanthrope in a way. A very interesting person. Another was Ray Appleyard. He was a cripple of some kind, I think a polio victim. He died a few years later. But he was also a very interesting person.

The main upshot of the Bristol thing for me was the culture shock. It was the first time I was outside of Germany and I got into an entirely different culture, languagewise, and also it was more provincial than Göttingen had been. After the first three months, which I just spent learning English, the next three months I was very unhappy. I thought this was a terrible country and terrible people and I couldn't stand it at all, and so on, until I suddenly realized that their way of life was actually a very good one and I became tremendously Anglophile. I felt reborn.

Harding: Did you travel around the English countryside much when you were there?

Delbrück: At that time people were beginning to have cars so we went on some lovely tours of Devonshire and Cornwall; actually, with Skinner I went on a big continental tour through France, Switzerland, Austria, Hungary, Czechoslovakia, Poland, and back through Germany.... There's a woman involved in that, too.

Harding: Do you want to talk about that?

Delbrück: No. It's a complicated story.

Harding: Were you friends with Dirac also?

Delbrück: That would be an exaggeration. He was my hero, I mean I had an infinite admiration for him, and studied every one of his papers and his book when it came out, but I was far too much in awe of him to be close. It's a pity because I should have been less shy and tried to learn more directly from him. Dirac was not in Bristol; he was from Bristol and he occasionally came there to visit his mother who was, I think, a grammar school teacher.

Harding: How did you like Lennard-Jones?

Delbrück: Lennard-Jones I did not get along well with. Our personalities were not well matched.

Harding: Were you successful in teaching him more?

Delbrück: No. I wrote one paper there on the quantum mechanics of interactions of inert gases. A very poor paper; I wasn't ready. It was a formal paper... It's the only paper I've ever published in the Proceedings of the Royal Society. My relations with Lennard-Jones were not bad; I mean there was just no love lost; neither felt at ease in the presence of the other.

Harding: But you went back there?

Delbrück: Yes, I forget why that was. I didn't go back there just to be with Lennard-Jones; I went, I guess, to be with my other friends of whom I was very fond by then; altogether I was very fond of the place. I went back for only half a year, and I guess that was just somehow to fill the time until the job in Berlin became available in the fall of '32. In between was the

Rockefeller year; do you want to talk about that?

Harding: Yes.

Delbrück: So somehow by hook and by crook I got this Rockefeller fellowship to go to Copenhagen and Zurich. I guess by hook and by crook means I must have been recommended by Max Born and by Karl Friedrich Bonhoeffer. I imagine that those two were the deciding recommendations. They were both people who thought well of me and who had a high influential voice. So in the early spring of '31 I arrived in Copenhagen and was immediately taken in hand by George Gamow. In fact I roomed with him for a while. I came to Copenhagen without much of an idea of what I was going to work on, and I fell in with Gamow and did a little work on nuclear physics. I also had some notions that I had an idea about spinors. Spinors were operators representing spins. It turned out to be nothing, but I don't remember the details.

So I spent the summer there, quite an eventful summer; actually I don't know why it was so eventful, but it seemed to me very eventful largely because of the many practical jokes that Gamow dreamed up and made me do with him... Almost every day he dreamed up another thing. I have described a few of them in the Gamow memorial volume.⁹ At the middle or end of this summer Gamow's visa expired and he had to go back to Russia. He thought he would come right back and I really longed for him to come back; it was a great vacuum after he had left because he was such a tremendous, vital force. But he didn't get out of Russia for several years.

In the fall I moved on to Zürich for the winter, and there I shared an office with Rudolf Peierls from Berlin, now Sir Rudolf Peierls. Peierls was Pauli's assistant, and a very

⁹Max Delbrück, "Out of this World," in Cosmology, Fusion & Other Matters. George Gamow Memorial Volume, ed. Frederick Reines (Boulder: Colorado Associated University Press, 1972).

competent theoretical physicist, very competent in handling enormous numbers of equations. But Pauli didn't like him, I'm afraid.

Harding: Pauli seems to have been a man of extremely strong feelings.

Delbrück: Yes, Pauli liked me and didn't like Peierls, but Peierls was infinitely more competent. But Pauli rode him mercilessly. It was just terrible the way he was rude to him.

Harding: You had the chance to become Pauli's assistant, didn't you?

Delbrück: Yes, later. I had forgotten. Yes, from Pauli I went back to Bristol for half a year, and during that time there was a possibility of going on from there either to Berlin or to Pauli, and I opted for Berlin because I wanted to be near the biology institutes there in Dahlem. At least that's what I have said somewhere else - I mean I remember only what I said about it at some later time, and I don't remember what the actual motivation was. Maybe I was also afraid that I wouldn't be up to par with Pauli.

Harding: We haven't discussed Bohr and complementarity yet.

Delbrück: Let's not do that today.

Max Delbrück

Session 3

20 July 1978

Begin Tape 3, Side 1

Harding: We were going to discuss Bohr and complementarity.

Delbrück: I came to Copenhagen, I think, February 1931, and stayed there five months. During that time, as I mentioned last time, I associated mostly with George Gamow doing some work on nuclear physics. I had come with notions of working on relativistic theory of the electron, on spinors, but that evaporated very quickly. During that time, and during all those years, Bohr incessantly worked and reworked his ideas on the deeper meaning of quantum mechanics. Quantum mechanics had been discovered as a technique in 1925 by Heisenberg, matrix mechanics, and in 1926 the other technical form of quantum mechanics had been discovered by Schrödinger, wave mechanics; the interconvertibility of these two forms of quantum mechanics had been shown very quickly. In 1927 Heisenberg had formulated the uncertainty principle as the real root of meaning of the quantum of action, and Bohr in a lecture at Como had given his version of what the deeper meaning was, and had formulated what was called the "complementarity argument." The essence of this argument was that for any situation in atomic physics, it is impossible to describe all aspects of reality in one consistent space-time-causal picture. The various experimental approaches that you use will reveal one or another aspect of reality, but these various experimental approaches are mutually exclusive; that means they are such that you cannot get the information that you get out of one arrangement, and simultaneously use the other arrangement to get other information. So these various experimental arrangements stand in a mutually exclusive relationship. The nature of the formalism of quantum mechanics is to permit you to derive the predictions for the outcome of the experiment of one kind from the results of experiments made with the mutually

exclusive arrangement (if they are done successively); these predictions are of a statistical, probabilistic nature. This feature of atomic physics, expressed in the way Bohr expressed it, or in the more popular way that Heisenberg expressed it as an uncertainty relation, was, of course, a total shock to everybody concerned; in fact, so much a shock that Einstein never got over it. During the rest of his life Einstein tried somehow to get back to the classical picture where reality is just one reality, and if you can't get at the full reality with present methods, then presumably there must be other methods to get at reality; whereas Bohr was insistent on saying that this limitation to the classical picture of reality was not a preliminary stage to be replaced by a return to classical notions, but was an advance over classical notions--that we now had arrived at a new dialectical method to cope with the feature of reality that was totally unexpected. That was the formulation of Heisenberg in '27, and Bohr in maybe the same year, maybe the next year. But Bohr continued to elaborate and restate his position year in and year out until he died thirty years later, innumerable lectures.

Harding: Were you interested in the idea of complementarity when he first...?

Delbrück: Enormously. I was interested--well, anybody who was at all interested in the result of the questions couldn't help but be fascinated. It also motivated me to look at the writings of Kant on causality to see how Kant, who was so clever and thoughtful, could have overlooked this possibility. So for the first time, and with a real motivation, I looked at Kant, and it was very clear that this situation was just utterly removed from anything that Kant had thought of--so there was no doubt that the physicists had been pushed into an epistemological situation that nobody had dreamed of before. Bohr then very vigorously asked the question whether this new dialectic wouldn't be important also in other aspects of science. He talked about that a lot, especially in relation to biology, in

discussing the relation between life on the one hand, and physics and chemistry on the other--whether there wasn't an experimental mutual exclusion, so that you could look at a living organism either as a living organism or as a jumble of molecules; you could do either, you could make observations that tell you where the molecules are, or you could make observations that tell you how the animal behaves, but there might well exist a mutually exclusive feature, analogous to the one found in atomic physics. He talked about that in biology and in psychology, in moral philosophy, in anthropology, in political science, and so on, in various degrees of vagueness, which I found both fascinating and very disturbing, because, it was always so vague. It was vague largely because the basic situation wasn't clear enough, and also in many respects Bohr wasn't sufficiently familiar with the status of the science. So it was intriguing and annoying at the same time. It was sufficiently intriguing for me, though, to decide to look more deeply specifically into the relation of atomic physics and biology--and that means learn some biology. So when the question came up of what job I would take after this year with Bohr and Pauli (and another half year in Bristol), and I had the choice of either going to Berlin to become an assistant of Lise Meitner or to Zurich to be an assistant of Pauli, I chose to go to Berlin because of the vicinity of the Kaiser Wilhelm Institutes for biology to the Institute [Kaiser Wilhelm Institut für Chemie] I was going to work in.

Harding: Did you know people in the Kaiser Wilhelm Institute for Biology before you left?

Delbrück: Yes. Apparently I did know Curt Stern, a fly geneticist at the Kaiser Wilhelm Institute for Biology, because he once showed me a journal entry in his journal, where he wrote down all the visitors he had, which showed that I had visited him some time before I took the job. I was puzzled by that, because I thought

before going to Copenhagen I hadn't taken any... it may have been during the summer I was first in Copenhagen that I went back to Berlin once and visited him. Otherwise I don't know whom I knew.

Harding: When did you meet N. W. Timoféeff-Ressovsky?

Delbrück: That was only after I came to Berlin. Let's see, I came to Berlin in the fall of '32, and during the summer of '32 I went back for a short visit to Copenhagen and arrived there on the night train from Berlin to Copenhagen. I was met at the station in the morning by Bohr's associate, Léon Rosenfeld, who told me that Bohr was giving a big lecture, opening a world congress of light therapy physicians, physicians who send you high into the mountains to cure you of tuberculosis and things like that. They would have an opening meeting in the Riksdag, the parliament building, and he would give the opening lecture there, and he was really insistent that I should come. So Rosenfeld and I had breakfast and got there, and were sitting in the gallery. And after five other people had greeted the solemn assembly of several hundred of these characters (with the Prime Minister sitting in the front row and the Crown Prince of Denmark all in morning coat), Bohr finally was called upon to give the opening lecture. So he got up, promptly lost his way behind the rostrum, and finally found the lectern from which he was supposed to lecture. In his usual way he whispered away, almost inaudible; so it was impossible to decide whether he was speaking English or Danish, and fiddling, fidgeting away. After he had talked a while, while fidgeting around he must have actuated a mechanism which caused a hydraulic mechanism to lift the lectern, and he gradually disappeared behind the lectern, very slowly--it was really like a Charlie Chaplin movie. It was slow enough and long enough for the Crown Prince to notice it, and poke the Prime Minister in the ribs, and everybody was watching with utter fascination whether this would stop or not, and finally Bohr took it and pressed it down and continued. From then on, of



Tom Lauritsen, Max Delbrück, Niels Bohr and Paul Epstein enjoy a moment on Caltech's sunny campus in June, 1959. Caltech Archives.

Click on the image to listen to Delbrück's description of Bohr's mishap during his 1932 Copenhagen lecture

course, everybody riveted their attention on him to see whether this was going to happen again. This was the great lecture entitled "Light and Life," which was published quite a bit later.¹⁰ In it he went out on a limb to predict such a complementarity; for once he was spelling things out so explicitly that later on it could be said that his prediction was wrong. Usually he was very careful never to say anything that could be definitely called "wrong"; he was so cautious in his formulations. But here he did. It was a very good thing that he did, because it certainly challenged me to take it so seriously, to follow it up.

Harding: What was the reaction of other biologists and physicists?

Delbrück: Oh, they ignored it. I mean the physicists didn't know enough biology, and didn't care about it on the whole, and the biologists, for them anything like quantum mechanics was utterly beyond their ken. At that time biologists didn't know any atomic physics, so a few biochemists like Otto Meyerhof, who was outstanding, dismissed it out of hand. I think [Otto] Warburg also did not take it seriously. The biochemists at that time were superconfident that eventually everything would turn out to be biochemistry, even though they were beginning to be confronted with this paradox in biochemistry; in living organisms you have small molecules and big molecules, proteins and nucleic acids (nucleic acids were just beginning on the horizon with proteins), and the proteins control the conversions of the small molecules, the synthesis and so on. So the question is how could the proteins be synthesized? Do you have then superproteins, or do you need 100 proteins for each step in putting on one amino acid to the next one? It was sort of a divergent problem. We now know how this is done, in a very

10. Niels Bohr, "Light and Life," Nature 131:421-23 and 457-59 (1933); Die Naturwissenschaften 21:245-250.

ingenious way, but at that time it looked like a hopeless proposition of being able to reduce everything--[that is], if you thought a little more in detail, it looked like a hopeless proposition. But it was also, we can say, much too early to really say anything very definite, because enzymology was just an emerging science, the very first enzymes were being purified. It wasn't known whether proteins were really well-defined molecules, and so on and so forth. So this was Bohr's bold step, and constituted for me the motivation to turn to biology.

When I did go to Berlin my job was to be a theoretical physicist, as it were consultant, for Lise Meitner. Lise Meitner was an experimental physicist working on radioactive substances, a very good experimental physicist, and there were quite a few new developments all the time. I was supposed to keep up with the theoretical literature and watch out what happened, and also presumably be productive as a theoretical physicist, and write theoretical physics papers. And I did write a few theoretical physics papers, not very interesting ones. I did write, together with Gert Molière, a very learned paper on statistical mechanics and quantum mechanics.¹¹

Harding: Why do you say "learned"?

Delbrück: Well, it was not directed to any direct applications, but rather to the question of whether quantum mechanics had really changed some of the puzzling aspects of classical statistical mechanics. In classical statistical mechanics, you want to explain the increase in entropy--that means you want to explain the arrow of time--but you want to explain it in terms of the laws that govern the motion of the individual particles, and equations of motion of the individual particles embodied the principle of time reversal.

11. Max Delbrück and Gert Molière, "Statistische Quantenmechanik und Thermodynamik," Abh. d. K. Preuss. Akad. d. Wiss., Phys. Math. Klasse, Nr. 1, 1-42 (1936).

(They are symmetric with respect to form and there is no arrow of time in that). So you want to get out of the foundations, which don't contain the arrow of time, a set of predictions that will involve the arrow of time, and that's a tricky business. And the question was, was this paradoxical thing less paradoxical in the quantum mechanical formulations than in the classical one. After looking into that very thoroughly, we decided it was not different. Superficially one had thought that because of the observation, which plays such a great role in quantum mechanics, that that introduced a directionality in time because you intervene, and then you change thereby the future but not the past--that there was an asymmetry in time. But it turned out that this didn't really cure the problems of statistical quantum mechanics. That was one thing.

Another theoretical thing was the following. One of the graduate students of Lise Meitner had studied the scattering by lead of gamma rays of ThC^{11} ; ThC^{11} is a gamma ray source with relatively hard gamma rays, as I recall, 2.6 million electron volts. If you scatter these gamma rays on lead, then, according to then current theory, you should find very little coherent scattering. Most of the scattered light should be Compton-scattered--that means scattering where the electron acts as if it were a free electron. And after scattering you find, at right angles then, a Compton-scattered light quantum which is very much less energetic than the incoming one. This student, H. Kösters had found a scattered component which was much harder than the expected one. I put out the conjecture that this had something to do with the new theory of the electron that Dirac had proposed, according to which the negative energy states of an electron (with energies below minus mc^2) were all filled, and the electron never jumped from plus energy to minus energy because these were filled (because of Pauli's exclusion principle). I made the conjecture that these negative energy electrons in the vicinity of the nucleus are not free electrons, but that their wavefunction was distorted by the nucleus of the atom and therefore that they could scatter. If they are free electrons

then they wouldn't scatter, but if they are disturbed by the field of the nucleus then there could be virtual transitions from minus to plus energy, and there would be corresponding scattering.

This problem is related to the problem of scattering of light by light. In the classical theory, two light beams just go right through each other and don't interact, but in quantum electro-dynamics if you take into account these negative energy electrons, then the first light beam polarizes the vacuum, and the second light beam then is scattered on the first one. So I made a conjecture that these hard scattered rays should be due to this scattering of underground electrons. The fate of this conjecture was that it turned out that the scattered light, the scattered quanta observed by Kösters, were not due to that effect. Instead, they were due to the effect that the negative energy electrons actually absorbed a quantum, and thereby created a hole there, a positive electron. This positive electron then could recombine with some other electron and make annihilation radiation, and that is very much harder than the Compton-radiation. Actually that was an obvious implication that I had overlooked. And that came out very quickly. Nevertheless the effect that I predicted ought to be there also, and the question was how to calculate it, and I slaved on that and it turned out to be a nightmare to calculate that. With the help of some advice by Hans Bethe I got so far as to predict that this effect should be proportional to the fourth power of the nuclear charge, z^4 , and that's about all that I predicted; it was published in a short appendix, I think, to the paper by Kösters.¹²

That's where my contribution ended to this problem, and I never heard of it again until about 20 years later, in the fifties, when I was long since in biology. Somebody told me that there had been published two papers in Physical Review on "Delbrück scattering," by Bethe and some graduate students of his who had made some progress in calculating them.¹³ So since then this name, "Delbrück scattering"

12. M. Delbrück, "Zusatz bei der Korrektur," in L. Meitner and H. Kösters, "Über die Streuung Kurzweiliger γ -Strahlen," Zeitschrift für Physik 84:137-144 (1933), 144.
13. F. Rohrlich and R. L. Gluckstern, "Forward Scattering of Light by a Coulomb Field," Physical Review 86:1-9 (1952); H. A. Bethe and F. Rohrlich, "Small Angle Scattering of Light by a Coulomb Field," Physical Review 86:10-16 (1952.)

exists, and if you ask theoretical physicists then I am known scurrilously for that little incident. I understand that the actual calculation of this effect, and experimental verification of it, still has been lingering on for the next 20 years after that, because it turned out to be just very, very difficult to calculate; also, in order to observe it you need to go to much higher energies--I think the optimal energy is about 10 million electron volts rather than 2.7--and I think now it has been confirmed to exist. So that was one thing in physics.

Now, I came to Berlin in the fall of '32, and during the winter of '32 and the spring of '33 was the takeover of power by Hitler, and with it very quickly the beginning of the emigration of a large number of colleagues, especially Jewish colleagues, and the harrassment of those who didn't leave; they either lost their jobs, or were not permitted to come to the Institutes any more, or to attend seminars. It was quite ridiculous.

Harding: What was the situation then at the Kaiser Wilhelm Institute?

Delbrück: Lise Meitner, herself, was half Jewish, but the thing I think that protected her was that she was an Austrian citizen, and also that she was at the Kaiser Wilhelm Institute and not at the University; therefore she was not a state employee--nominally I think the Kaiser Wilhelm Gesellschaft was a private organization. Also, she was pretty powerfully protected by Max Planck, who had just then become the president of the Kaiser Wilhelm Gesellschaft. So at the Kaiser Wilhelm Gesellschaft many of the Jewish colleagues could stay.

At the neighboring institute, the Kaiser Wilhelm Institute for Physical Chemistry, the Director was Fritz Haber, a very outstanding man, and sort of the senior man in the whole Kaiser Wilhelm group because his was the first institute that had been founded. He was also a man of great fame because he had instituted the Haber-Bosch process of chemical nitrogen fixation, of great importance in

replacing natural fertilizer by synthetic fertilizer, and also during the First World War he was the one who had invented chemical warfare. So he was a man not easy to attack, and he had in the whole institute quite a large number of Jewish associates. ([Karl Friedrich] Bonhoeffer had been there, and Bonhoeffer and Harteck had done the very important work also on ortho-and para-hydrogen at his institute. Bonhoeffer by then had moved to Leipzig, as professor.) Anyhow there appeared very violent attacks in the press, in the Nazi press, very quickly on Haber, and Haber preferred to leave the country and not come back at all. He did not want to have anything to do with this kind of mention, and he died a year later, or even less. When he died there was no memorial service of any kind, but a year later the Kaiser Wilhelm Gesellschaft decided to make a memorial service in Dahlem where most of the Institutes were, and that became a bone of contention. The Nazi government tried to prevent it, and forbade any state employee, that means any professor, to attend. The principal speaker at this memorial, one of four speakers but really the main speaker, was supposed to be my friend Bonhoeffer, who was then in Leipzig. So here he was; he had come to Berlin and had received this strict order from the Minister not to attend this meeting. So what was finally arranged was that Otto Hahn, who was a codirector with Lise Meitner of the Kaiser Wilhelm Institute for Chemistry, said he would read Bonhoeffer's speech. And Bonhoeffer and I walked around and around the place and tried to decide whether he should go in or not, and finally he decided not to go in, but I went in and sat in the back row and Hahn read this memorial. Actually it was a very dignified and well-attended affair, very impressive, and Planck also had personally picked up Hahn and taken him to the place. So that was that; it was just a confrontation. There were not any particular further developments at this point. Now this by way of background of how things began to heat up.

I don't know how this came about, but after a while there was a group of, as it were, exiled, internal exiled, theoretical physicists, I and five or six of them, who met fairly regularly and

mostly at my mother's house to have private theoretical physics seminars among ourselves; at my suggestion we soon brought in also some other people, some biologists and biochemists. And one of the people we brought in was [N. W.] Timoféeff-Ressovsky, who was a staff member of a Kaiser Wilhelm Institute for Brain Research, which was located at the other end of Berlin--enormously far away, just about an hour and a half by various public conveyances, in Berlin-Buch, now East Berlin or maybe even in East Germany. Anyhow we had Timoféeff over at my house a number of times and we also went to his place just to see some flies, and talked about fly genetics and mutation research. His main line of research at that time was to study quantitatively the induction of mutations by ionizing radiations. In order to do this quantitatively, we had to have quantitative dosimetry of the ionizing radiation, and the person responsible for that was [K. G.] Zimmer. So out of that grew a rather lengthy paper, which summarized all the experimental data and methods, and then a big theoretical Schmus about interpreting it, for which I was mostly responsible.¹⁴

As I recall, and I have not reread the paper, the experimental conclusions were that the number of recessive mutations that you find in the X chromosome was proportional to that dose, if one measures the dose in terms of ion pairs produced, or small clusters of ion pairs. And this was true whether the X-rays were hard or soft or even gamma rays, or whether the dose was fractionated or not fractionated, or whether it was given a high intensity or low intensity. It looked as if it could be interpreted to say that one is altering genes, and the genes have a rather high stability against spontaneous temperature-induced alteration, and that the ionization energy was plenty high enough to push it over this hill. About the spontaneous mutation rate, the only thing that was known was that it was relatively little temperature-dependent, indicating a high activation energy; that is one of the elementary results of

14. N. W. Timoféeff-Ressovsky, K. W. Zimmer, and M. Delbrück, "Über die Natur der Genmutation und der Genstruktur," Nach. Ges. Wiss. Göttingen, Math.-Phys. Klasse, Fachgr. 6, Nr. 13, 189-245 (1935).

chemical kinetics--that the higher the activation energy, the smaller is the " Q_{10} ", the factor by which the rate increases when you increase the temperature by 10° . So in a crude way one could say that this all meshed together to the picture that the genes were relatively stable macromolecules.

I think and I have heard, but I have not ever studied in detail, that the argument really wasn't that good, in the sense that at that time, there was no means of clarifying whether these mutations were point mutations, or deletions, or rearrangements, and so on; especially there was no way of determining whether the spontaneous mutations and the radiation-induced mutations were the same kind of mutations. I think it is now clear that they are not, and I don't even know whether it's known now what fraction of either of them are point mutations in the modern sense. So a great deal of effort has been made by radiation biologists to extract more information by this approach from radiation genetics. But I have never continued to work in this field, because I thought very quickly that it was clear that this was not an optimal way to get closer to the nature of the gene. There is only one small second paper as I recall, or maybe there are two; one that concerned the question whether the spontaneous mutations actually could be due to cosmic rays. That could be ruled out by a very simple comparison with the rate expected from the known relation between ionizing dose and effect. The degree to which cosmic rays could contribute to this spontaneous rate was only 1/1000th of the observed rate. So that was one other thing.

The major paper got a funeral first class. That means it was published in the Nachrichten der gelehrten Gesellschaft der Wissenschaften in Göttingen, which is read by absolutely nobody except when you send them a reprint.

Harding: Had you tried to publish it in a more widely circulated journal?

Delbrück: No, we thought it was sufficient to publish it in this place, because we could get plenty of reprints, and we could send it to the people whom it would interest. Timofeéff must have sent

it around to all the major geneticists; when I came to Caltech two years later, [A. H.] Sturtevant, for instance, was quite interested, although again, he didn't know enough physics. It was all a matter of bridging physics and genetics at that time--there just weren't any people who could do that. Sturtevant wanted to know what was in the paper, and so I gave a seminar here, and he was very pleased with that and said, "Now you have told us exactly what I wanted to know."

Harding: Did he believe it?

Delbrück: Well, there was really nothing to disbelieve or believe. I mean our arguments had a certain moderate amount of strength; I think I wasn't exaggerating the strength, I mean I tried to make a fair presentation of how strong or how weak they were. Maybe I was a little too optimistic, but nobody could really judge these questions--as to how uniform these classes of mutations were.

Harding: But there was general acceptance that the gene was a molecule?

Delbrück: Most people would have thought so anyhow, so it was not an upsetting thing. Now, you say you have looked up when this meeting in Copenhagen was?

Harding: Yes, I have. You visited Copenhagen in April of 1936, and then there was a conference in September attended by you, Muller, and Timoféeff and Bohr. In fact, in October you and Timoféeff wrote a summary of the discussion.

Delbrück: I see. Well, okay, that's good because I don't remember... I only remember that we traveled--Timoféeff, Muller, and I--together from Berlin to Copenhagen, and the first thing was that I had forgotten my passport--and to forget your passport is just absolutely suicidal if you want to cross a frontier. Nevertheless we managed to get across by making a loud noise that there was a meeting arranged

by Professor Bohr, and it was important that we should arrive on this train. Timoféeff with his booming voice also tried to intervene, and the man asked, "Well, who are you? Where were you born?" And Timoféeff had to admit that he was born in Russia. The train goes from Berlin to Stettin, and then on a ferry over to one of the islands on which Denmark lies. Muller was very nervous, and was preparing notes all the time for the talk that he was going to give, and he managed three times during this trip to write down notes and lose them again. That's how nervous he was. So we had a very good laugh at him, both on the way going and even more on the way back.

Harding: He had been in Russia, right?

Delbrück: I can't be sure. I think he was just coming back from Russia. Muller was first very enamored with Communism, and went there for several years, and helped very powerfully to build up Russian genetics, especially Drosophila genetics. Then, of course, the whole thing got under a terrible cloud, and Lysenko rose to power and to favor, first with Stalin and then surprisingly also with Khrushchev. So Muller became very antiCommunist...I am trying to remember. In 1948, I think, there was an international genetics congress in Stockholm, and Muller was president of that congress (it's an honorary title, and gives you the privilege of giving a one-hour lecture on anything you want to lecture on). So Muller made this lecture a tremendous indictment of Lysenkoism, then just really rising to power, which was counterproductive--it really made Lysenko rise more than ever before. And that was a very interesting phenomenon. This is my somewhat vague recollection, and I am not sure, but I think that Muller's indictment at this International Congress had a great deal to do with the rise to power of Lysenko. It shows how difficult it is for a scientist to do something effective in politics.

So where were we? Back to the 1930's. This sort of black market research was going on, I mean it was moonlighting; I was supposed to be the theoretical physics advisor to Lise Meitner, but actually took all this time out to work in biophysics.

During that time Hahn and Meitner (who were great experts on radioactivity and the chemistry of radioactive substances for decades) followed up the discovery of [Enrico] Fermi that you could irradiate a large number of chemical elements with neutrons and obtain radioactive substances; especially that you could irradiate uranium with neutrons, and obtain quite a number of radioactive substances with apparently new chemical properties, which Fermi suspected to be transuraniums. Hahn and Meitner picked that up, and indeed discovered that when you irradiate uranium with neutrons, a large number of products arose which could be characterized by their half-lives and by the type of radiation that they gave off, these were interpreted to be elements 93, 94, 95, 96, 97, but very soon it became obvious that there were quite a few more than that, and so they were supposed to be isomers of the transuraniums. I was very quick in interpreting all of these as isomers of these things, and in retrospect this was really immensely stupid of me; I should have guessed what was really going on, namely fission, but I, like everybody else, lacked imagination to see that.

Harding: The theoretical physical problems never seemed to have really caught your wholehearted interest.

Delbrück: Yes, that's true. Well, this wasn't really a theoretical physics problem almost. It was too trivial to be a theoretical physics problem. It was something that any experimental physicist could easily have [figured out]. You didn't need any calculation; all you needed to know was that there was excess energy there; the neutron enters and there is enough energy there to blow the nucleus to pieces. You needed to just be able to add and subtract, and it just didn't occur to anybody; and it didn't occur to anybody until they were literally forced to this conclusion, and they were forced to it only the year after I left. I left in '37 and came here to Caltech and gave here a seminar in physics which then a few weeks later turned out to be everything wrong. The way they found out was that the people in Paris, who were also in the game, thought that they had found a decay product which was radium chemically.

Radium was four numbers below uranium, 92-88, so this should have been a decay where two alpha particles would have been lost. Hahn and Meitner wanted to confirm that and really make sure that it was radium. The classical way to characterize radium was to precipitate it with barium chloride, I think. Barium was the lower homologue of radium, and if it was radium then it would be precipitated with the barium chloride, but not completely identical. It would be impoverished relative to the barium. Well, they found that it precipitated like barium, and they made a little publication which said that we are sorry, but we find that this decay product... as chemists we have to call it barium. And they did go a little further; if so, then that means that we have a real split of the uranium atom. Well, and then the rest is well known. But that happened only the year after I left.

Let's see, I must have worked a little bit also in biology on something that might be called population genetics--I don't think I published a paper on that--on natural selection and asexual reproduction, because I gave a talk on that in Eindhoven and two other places in Holland. That must have been in '36 or so. It can be dated accurately because it was the week in which the King of England resigned because he married a certain lady, which upset people quite a bit. Anyhow I gave a talk on that, and this is in retrospect amusing, because when Hahn heard that I was going to give a talk at Phillips in Eindhoven, he said, "Oh, Delbrück, at Phillips they once made metallic uranium, just to see what it was chemically like. They must have still some sample bars of metallic uranium. (Nobody else had it at the time. What you used was, I think, uranium nitrate.) Why don't you ask them whether they could let me have a piece of metallic uranium?" And I asked the Director of Research and he said, "Yes, sure." And so after my lecture he gave me a piece this size [ball-point pen] of metallic uranium which I put into my pocket, and brought with me from Holland to Germany. From the point of view of radioactivity it is harmless, because the radiation is very weak, but it's sort of a nice and informal way that some things are done. And so some of the work on uranium radiation was done with this piece.

This little club which started out as theoretical physics, and then brought in genetics, also brought in biochemists and photosynthesis physiologists. The photosynthesis man was Hans Gaffron, and he and Kurt Wohl lived together in the same house in Dahlem. As a result of the talks that we had in our club on photosynthesis, they published a series of papers on the kinetics of photosynthesis. The interesting problem there is this, that in order to reduce one molecule of CO_2 , you need several quanta of light. The minimum from the point of view of energy is four [quanta], but experimentally one found 8 to 10. Let us say a CO_2 molecule has to accumulate 8 or 10 quanta before it becomes the structure which becomes sugar, and the oxygen gets liberated as molecular oxygen. You would think when you start with a leaf in the dark (where all intermediate products presumably have disappeared) and start irradiating, there would be a slow beginning of the evolution of oxygen. But the experimental observation is that the evolution of oxygen starts immediately at the maximum rate. There were some more sophisticated experiments on this kinetics that had been published. Wohl and Gaffron discussed these experiments, and essentially already described what is now accepted; namely, that photosynthesis is done in photosynthetic units, which consist of about 1000 molecules of chlorophyll all funneling their energy into one photosynthetic reaction center. So that was also an important piece of what could be called "molecular biology" that came out of these discussions.

Harding: How often did this little club meet?

Delbrück: I wish I could remember. I think it met irregularly and I would imagine sometimes every week, sometimes once a month, and so on, I don't recall. We had no secretary of the society, no record keeping or anything. We were just a handful of people. Let me name the people.

Harding: Yes, I have that already.

Delbrück: What names do you have?

Harding: I have Gert Molière, Werner Bloch, Ernst Lamla, Werner Kofink, Kurt Wohl, Hans Gaffron, K. G. Zimmer, and of course Timoféeff.

Delbrück: That's almost all. There was one other who was an enzymologist, whose name I've forgotten, who gave us a talk on alcoholic fermentation. That was a poor talk. He didn't do anything further.

I did study further population genetics, a book by R. A. Fisher, I think.

Harding: You were thinking of going back to London on the Rockefeller Fellowship?

Delbrück: Well, this is putting the cart before the horse. What was happening was that I was just studying this book by R. A. Fisher, when one day I got a visit from a gentleman of the Paris office of the Rockefeller Foundation, who was just checking up on what former Rockefeller Fellows were doing. I told him what I was doing, and since I was reading this book by R. A. Fisher, he suggested, "Don't you want to go to London and study with these people?" And I said, "Well, why not?" And then, however, after I reconsidered, I said, "I'm not really that interested. If I want to do something like a Rockefeller Fellowship I would rather go to Pasadena." And to my surprise he acceded to that without batting an eyelid, and to my surprise Hahn and Meitner--not to my surprise, I knew that I had their good will and friendship--they acceded to it, and facilitated it by giving me a guarantee that I could come back and get my job back--that's what Rockefeller insisted on. And so the next thing was to get an exit visa to get permission to leave Germany. Before the Nazis, this problem would not have existed. There was no such thing as an exit visa, but at that time already, I guess you needed some sort of an exit permit because they had reinstated military service. I was beyond the age of military service. In '37 I was 31 already, and I guess they called up only people from 20 to 25,

then from 25 to 30. I was lucky that I always was one or more years ahead of the thing. But I think it may have been in connection with that that I needed a permission and somehow I got it.

Harding: You say in the Royal Society questionnaire that one of the reasons that you wanted to go to the United States was because it seemed as if political factors would bar you from further advancement in Germany.

Delbrück: Yes, I haven't mentioned that. So while I was the assistant of Lise Meitner I also tried to become a lecturer at the University; that means Habilitation, become a privatdozent and obtain a venia legendi, permission to lecture. It's not really a job because you don't get paid for it; you get the permission to lecture. This procedure was made more complicated by the Nazis very quickly by dividing it into two steps. One, you were supposed to get an advanced degree, as it were, the Dr. Habil.; that means essentially presenting all the publications that you have made, [demonstrating] that you are scientifically, scholarly, qualified. In addition, you were, however, supposed to pass also some political tests. To do so you had to go to a Dozentenakademië, an indoctrination camp, which was quite a fascinating thing. A "free" discussion group, you know, where you got lectures on the new politics and the new state. So we had "free" discussions, and after three weeks of "free" discussions they decided whether you were sufficiently politically mature to become a lecturer at the University. So I went to two of these. The first one, I think, the very first one that they had run themselves, and that was a lovely place opposite Kiel. Kiel is a north German city on the Baltic on a bay, and the bay with an island or peninsula across. There we lived on a very nice estate with a large park, they housed us all, and we also had our daily discussions and other exercises and social events. There were about 30 of us, I would say, three of us to a room. And in a way it was a marvelous thing, because it was the first time in my life I got thrown together closely with people from other disciplines. I was together with an economist and a psychiatrist and we got to know lots of other people. I learned more

about other sciences at this academy and at the next one than anywhere else. But of course there was also the business of having these wonderful lectures by reliable party members, and everybody was terribly nervous because you really didn't know what was going on, and what you could say and couldn't say, and so and so forth. Anyhow I obviously was too incautious, and I was informed afterwards that I wasn't quite mature enough but that I could try again.

So I tried again. The next time it was in an equally beautiful place, Thüringen, which is in the central part of Germany, now East Germany, beautiful mountains there. There things ran much more smoothly; everybody knew by then what he could say and couldn't say and everything was much more relaxed. But still I must have shot my mouth off. It must have been transparent that I wasn't in great love with the new regime, so I don't know whether I was officially informed that I wasn't mature enough, or whether they just didn't answer my letters. I have forgotten now. Anyhow it was pretty clear that a University career was not likely to be open for me. I don't remember. I went to considerable lengths to prove that I was not Jewish, which was also part of the business, which involved supplying real authenticated copies of all the baptismal certificates of your four grandparents, and their Christian marriage certificates, maybe even to the great-grandparents; in the Archives there is a file of all this. And this was quite amusing to get those...but this was all to no avail. So when this Rockefeller thing came around it seemed like a good idea to see something of the world and see what was going to happen, because at that time it was anybody's guess how long the mess was going to last. Some people said six months and some people said much longer. I was immensely lucky that I had this opportunity. Many nasty things have been said about those who could have left and didn't leave, like Heisenberg, he's the most outstanding case, I don't agree at all with these derogatory comments. I don't think that it was anything to my credit that I left at all. I think it was a question which could be answered one way or the other, and there is great merit on both sides.

Harding: What is the moral argument for staying?

Delbrück: Well, I mean, what is the moral argument against running away? It's just running away, that you take advantage that you can run away. If you imagine that the thing may last only a short while, then it's important to see to it that some of the good people are staying.

Harding: Laue was an example of someone who stayed and persistently fought the regime.

Delbrück: Well, you could cite also here he had to make his compromises like everybody else. He was telling me a story that he and Otto Warburg wrote a letter to the Nazi Minister of Education, where they wanted to get something done, and then the question was, how would they sign it, with "Heil Hitler" or not? The choice was either "Heil Hitler" or the old conventional formula, "Mit vorzüglicher Hochachtung" (With our great respect). They discussed it for a while and finally Laue said, if he said "with great respect" it would be just a big a lie, so I assume they wrote "Heil Hitler". So if you want to stay then you have to make your compromises, and that's what everybody had to do. Bonhoeffer stayed. All the Bonhoeffers stayed, and some of them were active in the Resistance and some of them were not active in the Resistance. That was a second choice. It was whether you thought you should personally get involved in this movement. You still had the choice of being resistant on the Communist side or on the liberal side, and whether you should wait until the generals would agree, join you and act, and so on. These are very difficult questions.

Harding: It seems that the choices seem to be much more clear-cut in retrospect than perhaps they were at the time.

Delbrück: Of course, yes...It's not that the choices seem clear-cut in retrospect, but they seem clear-cut to people who have no sense of the reality of the situation. I mean going away was in any case

only a chance...Going away without any kind of security--that means having a job somewhere else--was limited to those who had professions that were salable in another country and who had already professions or had some other ways of having private funds, or large funds that they could transfer, and could start a new life in a different country. But that was an infinitesimal part of the population. And if you were a Jew and didn't have funds and left, you could certainly count on the help and cooperation of the Jewish communities in other countries. If you were non-Jewish and left you were certainly very suspect and couldn't expect much help from the Jewish organizations. I mean that's what I did find when I left, that I was constantly under suspicion. Why would the fellow leave if he didn't have to? That was more the attitude really at the time. I mean I wasn't applauded for leaving, but I was suspected of leaving by having some sinister motive imputed. And rightly so. There were certainly quite a few Nazi agents did leave posing as adversaries. So, a difficult business...

Max Delbrück

Session 4

24 July 1978

Begin Tape 4, Side 1

Harding: Today, then, we're going to discuss the phage work and the phage group.

Delbrück: Well, last time we talked about my emigration from Germany and the visit to Bohr a little before leaving Germany; I went via England and visited a Faraday Society meeting in Manchester, I think, and then took a boat to New York. In New York I visited the Rockefeller Foundation offices on the 52nd floor of the RCA building (very startling offices, with a very wonderful view of midtown New York), and then spent a post-season month in Cold Spring Harbor; I think it was something like late September to late October. There I talked mostly to [M.] Demerec, and learned a little about work on Drosophila cytogenetics, using salivary gland chromosomes with their wonderful banding. Demerec also made me do a little experimental work, that is, actually dissect Drosophila larvae and fish out the salivary glands and squash them and stain them, and that's as far as I ever got with Drosophila genetics experimentally. [I was] fairly unhappy during that month, because it was the post-season Cold Spring Harbor, which was at that time very, very limited in the number of people there, and those people were all very quiet; nobody really talked to each other, so after closing hours there was just nothing you could do except go for a walk in the woods.

After that month I went west, and made only one stop on the way in Columbia, Missouri, to visit Louis Stadler. Stadler was sort of the counterpart to Timoféeff, in the sense that he [Stadler] had discovered the mutagenic activity of ultraviolet light; this in contrast to the work of the other people [who worked] with ionizing radiations. Stadler had applied [UV radiation] to corn pollen, and

I was interested in the details of that work, so I decided to visit there; also because it was a nice way to make a break in the trip across the country and see something of the Middle West. And that was a very nice visit. I liked Stadler enormously and we got along very well. Then from there (there were no planes yet at that time) I continued by train and must have arrived in Pasadena on the Santa Fe train one evening in late October. I was met at the train station by one German fellow, [Georg H.M.] Gottschewski, a Drosophila geneticist, and somebody else. They took me out for a beer, and dropped me at the Athenaeum, and Gottschewski got me all upset, because he said that Thomas Hunt Morgan was very upset about my coming; he didn't know what to do with this theoretical physicist, and really thought it was crazy for a physicist to come. Well, that turned out to be entirely wrong, but it was sufficiently unsettling for me, having traveled 8,000 miles to get here, that I from that day on was utterly confused about north and south in Pasadena. (I could not tell you without thinking hard whether north is over there now. Really every time I try to have it instinctively, I get turned around.)

The next morning, then, I visited Morgan, who was very cordial, and I explained that I had done these somewhat theoretical studies with Timoféeff--Timoféeff did the experiments and I did the theory on mutagenesis and ionizing radiation in Drosophila--and that I wanted to learn more about the actual Drosophila genetics, and see how the whole subject could be advanced further. Morgan suggested that I should work with [A. H.] Sturtevant. I talked to Sturtevant, who was also very nice, and he suggested that it would be interesting to try to clear up some confusing results on linkage in the fourth chromosome. He gave me some reprints to read, which I tried and failed to understand. By then the Drosophila terminology had become so specialized and esoteric that it would have taken me weeks even to understand all their terminology. I sat poring over these papers pretty disconsolately for some time, in the room across from Calvin Bridges, who was another very wonderful Drosophila geneticist. So I consulted with him quite a bit and became very good friends with him. Calvin Bridges lived a "hippie" type of life, very simple. He had a small frame house here on one of the streets

nearby, cooked for himself and occasionally had friends come in, but all very unobtrusive and very friendly. He and I almost regularly went for lunch together, which consisted of going to the corner of Lake and California and buying there in the market for 10 cents some peanuts, and for 5 cents a little bottle of milk, and then walked back and sat on the bench at the bus stop, and consumed our peanuts and milk and chatted about everything, both science and many other human things.

Harding: I read somewhere that he was a friend of Theodore Dreiser.

Delbrück: That could well have been. Yes, he was a friend of a number of interesting people, but never in a snobbish way. Everything that he did was utterly unpretentious, low key. It was a completely new experience to me. In the Old World I had never met a person so unpretentious in a way that only an American can be unpretentious. although he was a really outstanding scientist. I mean so intellectually unpretentious.

Harding: But he died shortly thereafter.

Delbrück: He died a year later, yes. And in fact he came to me to take him to the hospital, but then I was going East a few days later and he died while I was away. He died of cardiac insufficiency, congestive heart failure. Anyhow I consulted with him for quite a bit and tried to learn some Drosophila genetics and, and, as I say, I didn't make much progress in reading these forbidding-looking papers; every genotype was about a mile long, terrible, and I just didn't get any grasp of it. So then one day I read that a seminar on bacteriophage had been given by E. L. Ellis, while I was away on a camping trip with Frits Went, the plant physiologist. I was unhappy that I had missed it, and went down to ask him afterwards what it was all about. I had vaguely heard about viruses and bacteriophages, and I had read the paper by [Wendell M.] Stanley on the crystallization of the tobacco mosaic virus before I had left Germany. I had sort of the vaguest of notions that viruses might be an interesting experimental object for a study of reproduction at a basic level.

Well, Ellis was very cordial and showed me what he had accomplished by then, which was really very impressive; starting from zero knowledge concerned with anything about microbiology, viruses and so on, he had gotten together very primitive kinds of equipment--an autoclave and a sterilizing oven, and a few dozen pipettes and a few dozen petri plates, and some agar--and had taught himself how to pour plates and to use sterile technique. He had gone down to see his friend, Carl Lindegren at USC, who was in the Bacteriology Department, and had gotten from him this strange organism that nobody had heard of before, called E. coli, which is now the thing that you hear about in grade school. And he had gone to the Los Angeles Sewage Department, and gotten himself a liter of Los Angeles sewage, and from this sewage had isolated a phage [active] against E. coli. With that he had taught himself how to get plates that would produce nice plaques of the phage, and had, in essence, already shown something like a one-step growth curve. I don't know really how far he had gotten with that.

Anyhow I was absolutely overwhelmed that there were such very simple procedures with which you could visualize individual virus particles; I mean you could put them on a plate with a lawn of bacteria, and the next morning every virus particle would have eaten a macroscopic 1 mm hole in the lawn. You could hold up the plate and count the plaques. This seemed to me just beyond my wildest dreams of doing simple experiments on something like atoms in biology, and I asked him whether I could join him in his work, and he was very kind and indeed invited me to do so. And so I did, after asking some other people like Bridges and Frits Went whether they thought this was a good idea. They encouraged me, so I dropped Drosophila and teamed up with Ellis. And that was just marvelous. We had a tremendous time; a tremendous time because it was all really new, at least to us and certainly to everybody in this building [Kerckhoff Laboratories of Biology], and pretty soon we also did a few things that were not generally known.

A few weeks or months afterwards Ellis gave a seminar on phage, and he brought some petri plates along to show these plaques; these

were passed around and everybody said, "Ah!" A few days later I met Mrs. Morgan, and I asked her whether she was impressed with these [plaques]. She said, "You know, the light was very poor. I couldn't see them." It turned out that nobody had been able to see them. Everybody had taken it on faith that there were plaques there, which I thought was quite hilarious. It reminded me of the story of the emperor's new clothes. I told Mrs. Morgan about it and she didn't know that story, so I made a special trip downtown to a secondhand bookstore [to see] whether I could dig up a copy of Andersen's fairy tales, and I did find a copy and bought it and took it home. (It was an old copy of 1880 or something like this and the story was in there; then I looked at other stories, and the more I looked the more puzzled I became, because many of the stories didn't seem to be Andersen's fairy tales, but Grimm's fairy tales, which are an entirely different thing. Andersen's are invented ones and Grimm's are folklore stories. Well, indeed, it turned out that the publisher of this book had simply stuffed his Andersen's fairy tales, which weren't enough but who was a popular name at the time, with some Grimm's fairy tales just for good measure. Those must have been the book publishing practices in America in the 1880's. Quite amusing.)

Ellis and I worked together for a year, and after a year, unfortunately and to my great regret, Ellis dropped out of the phage thing, and went back to what he had done before, namely, doing cancer research on transplantable tumors in mice. Apparently, the fellowship under which he worked, which was funded by the physician Seeley Mudd, stipulated that it should be on cancer research. When Ellis--who was not an M.D. or even a biologist, but a physical chemist--had gotten this fellowship, Ellis had had the marvelous good sense to study the cancer literature, and thought that an interesting aspect of cancer literature was that there was a virus component to it; then he thought, well, if you want to study viruses, maybe the best viruses are the bacterial viruses. But apparently Seeley Mudd discovered that after a year, and decided that this was too far away from cancer, and made him go back to cancer research. Ellis came

in [to the lab], and certainly continued to take an interest in what I was doing in my second year here.

Harding: Did you have any trouble renewing your Rockefeller fellowship for another year?

Delbrück: Not really. No, that was relatively simple. I came in the fall of '37, and it was renewed to start in September '38. This ran out after the war had started, which made it virtually impossible for me to go back to Germany; not that I was keen on going back, but it also left me high and dry without visible means of subsistence. For several months I lived on money borrowed from friends.

Harding: There was no possibility of a position at Caltech?

Delbrück: There might have been, but Morgan didn't come forward. He thought maybe that wouldn't have been a healthy thing to do, although I'm sure he had a high regard for me--this was not the way he handled things. However, then the Rockefeller Foundation itself took a mild interest, and drew my attention to this job at Vanderbilt. In fact, an arrangement was made by which the Rockefeller Foundation paid half of my salary--the full salary was \$2500 a year--in return for a gentleman's agreement that I would have half time free for research, would not be just loaded down with teaching physics. So a few days after Christmas of 1939, I left Pasadena and drove East, and arrived in Nashville on New Year's Eve in a driving snowstorm, and booked myself into one of the two hotels. The next morning I met the physics professor, Francis Slack, and gradually got started there; indeed, I had an office in Physics, and did some teaching there, and I had a lab, one the size of this office, over in Biology, kindly put at my disposal by the head of the Biology Department, [E. E.] Reinke.

Harding: Had you continued to be interested in physics while you were at Caltech?

Delbrück: Not much.

Harding: I know you gave one physics research conference.

Delbrück: Well, I gave a physics topics seminar on the findings of Hahn and Meitner on irradiation of uranium by neutrons. It might be interesting to check when I gave this seminar, whether that was...Yes, it must have been before the break. The break was in December '38, January '39, and it was sometime during '38 that I gave the seminar, but I think it was just a few weeks before the actual break; so within a few weeks actually all the interpretations I had given of the experiments turned out to be wrong.*

I got myself again set up at Vanderbilt in Biology. I used the incubator and the sterilizing facilities of the Department of Bacteriology, which was a one-man department, [consisting of] Mr. [W. McA.] Deacon. My room was sort of in the no man's land on that floor between the Physiology [Department] of Dr. [Charles S.] Shoup and the Bacteriology Department of Mr. [W. McA.] Deacon. I used the sterilizing facilities of the Bacteriology group, I think. I may have gotten my own equipment after a while. I diddled along there, and then, I don't know in what sequence, I was joined by various other people.

Harding: You met Salvador Luria in December of 1940.

Delbrück: And he did not come to Nashville until nine months later. I don't know whether by then I had some other people working there. Some of the earliest were A. H. Doermann, who had just gotten his degree in Neurospora genetics, with [George] Beadle at Stanford; and E. S. Anderson, who had gotten his degree with [C. B.] van Niel at Pacific Grove, the Marine Station of Stanford University; and gradually we took up contact with A. D. Hershey, who was at that time in the Microbiology Department of the Medical School at St. Louis. And then T. F. Anderson, Tom Anderson, the electron microscopist; we first contacted him one summer when he was in

*In fact Delbrück's seminar, "Recent Work on the Trans-Uranium Elements," was 11 January 1938.

charge of using the RCA electron microscope at Woods Hole. He had an exhibit instrument there, and collaborated with anybody who wanted to use it. He and Luria had already started the summer of '42 [working] on phage, and I joined them also for a few weeks. Actually, it turned out that the findings we made that summer had been made previously by [H.] Ruska in Germany, but during the war there was very little communication. So the fact that some of these phages had this very odd shape, with a head and a tail, and very startling morphology, had been seen in the electron microscope by Ruska, and had been published in the Naturwissenschaften.¹⁵ We did it a little more quantitatively, since we paid great attention to [controlling] two things quantitatively; that is, really control the concentrations of bacteria and phage, and the time in which they interact, so we could be a little more precise as to the adsorption process.

Harding: I gather that in some of the photographs it looked like the phage were actually swimming towards the bacteria, but you resisted taking that interpretation, and that Anderson later showed that it was an artifact. Was that an intuitive sort of guess on your part?

Delbrück: Well, I suppose so. It would have been very unlikely that they could swim. I don't want to rehash the whole early phage history.

Harding: Well, maybe we could talk about how the first course at Cold Spring Harbor was set up in 1945, when you got the idea for that. You put together an exam for the students to take before they were admitted.

Delbrück: Yes, we wanted to be sure the students could multiply

15. H. Ruska, "Die Sichtbarmachung der bakteriophagen Lyse im Uebermikroskop," Naturwissenschaften 28:45-46 (1940); H. Ruska, "Ueber ein neues bei der bakteriophagen Lyse auftretendes Formelement," Naturwissenschaften 29:367-368 (1941).

and divide, which was necessary; I had tried to give such a course at Vanderbilt to the local undergraduates there, and a few of the students who had taken the bacteriology course had tried to sign up, but that was hopeless. I mean they could not multiply and divide large numbers, 5×10^7 divided by 3×10^4 . It would have taken another three weeks to learn that. So I thought [an exam] was a good idea.

Harding: Did you find that the level of students in the United States at the undergraduate level was lower than in Germany?

Delbrück: I have no way of judging that, because I had never been teaching in Germany, so what did I know [about what] other students knew, except a few of my friends.

So how this course at Cold Spring Harbor came about? I don't remember who suggested it, but that must have been already the fourth summer then; the first summer that we did phage work in Cold Spring Harbor was '41, and I think from then on we were there every summer. So in '45 then we gave this first course, which had a marvelously motlied crowd of students; the list of students I think must be on record.

Harding: Would you say that there was a sense that you needed to convert people to join in the research?

Delbrück: You mean why did we give this course? I think Luria was the promoter of that. Luria thought that if phage ever was to become an important line of research, and its potentialities really developed, more people would have to be brought into it. And therefore one should make an effort to bring more people into it this way, by giving the course. I think it was Luria more than I, but I may be wrong. I don't know. Anyhow it helped, even though only a few of the people who took the course actually became phage workers. At least this way we recruited quite a number of people who could read the phage literature with understanding.

Let's see, you were asking [before the interview] about [O.T.] Avery. Avery made his great discovery in 1943, but we knew about his working on this problem for at least a couple of years before then, and I think both Luria and I had gone to visit with him. And also Demerec knew quite well that there was a very interesting problem. It had been shown that you could use an extract of one bacterium, and expose another bacterial strain to it, and then get some kind of transformation, and the transformation was expressed in producing a particular capsular polysaccharide. The feeling had been that the transforming agent was the polysaccharide itself, that somehow that was sort of a crystallization process, or rather, a nucleation process; you add a piece of this polysaccharide, and then more is produced; that was the obvious interpretation at the time. If that was true, then it showed that here you had a genetic property which was not transmitted by genes, but by something more like a whole organism, you might say; like every little piece of polysaccharide was a little apple tree that could grow into a big apple tree; however, this little apple tree did not contain genes, but was just a form principle that had made it possible to accrete more in the same form--more like a crystallization process. If you dump into a saturated solution a crystal of a particular substance, then you can get more of that crystal; it's a nucleation process. And if that had been true, it would not have been so overwhelmingly interesting, because it was obvious that this could not be the general principle of genetics. So it came as a total shock and surprise when Avery and his associates discovered that the transforming principle was DNA. He communicated this discovery to his brother Roy Avery at Vanderbilt University, who was in the Department of Microbiology in the Medical School (not where I was, in the Biology Department), in a 17-page-long handwritten letter, which Roy Avery showed me just about the day he received it, and which I read there standing in his office in the spring sunshine, I think it was. It was quite an amazing letter, and has been published of course. Did I tell you how it was retrieved?

Harding: No.

Delbrück: A number of years later, the National Academy had a symposium highlighting this discovery of DNA as an information storage molecule, and I was supposed to be chairman of that symposium. I thought it would be nice to read that letter, and so I wrote to Roy Avery (O. T. Avery had died meanwhile), and asked him whether he could find that letter. Quite some time later he said, "Oh, you put us to so much bother. My wife and I have been spending a week going through old boxes of letters, and finally we think we found the letter you had in mind." He sent it to me and I intended to read from it, but when I was on the plane from here to Washington I discovered that I had forgotten the letter, and next morning was my talk. So I told the President of the National Academy that I really would like to get hold of the text of this letter. He suggested that I should make a telephone call early in the morning from his office to my wife, who somehow would have had to get hold of the letter meanwhile, and his secretary would be there, and my wife would dictate it to her over the telephone. And indeed this was done, and the letter was transcribed this way; that was quite a tour de force.

This discovery [by Avery et al.], of course, was just the beginning of the battle, because immediately the scientific world split into those who believed that [their experiments] showed that DNA is an information storage molecule, and those who believed that the DNA preps were contaminated with a small amount of protein, that the protein was the important part. During the subsequent years it was essentially the work of [Rollin D.] Hotchkiss who gradually tightened the proof more and more to show that the DNA is the essential thing.

Harding: Would you like to talk about your collaboration with Pauling?

Delbrück: There is a note in Science by Linus Pauling and myself on the nature of the intermolecular forces involved in genetics.¹⁶ The

16. Linus Pauling and Max Delbrück, "The Nature of the Intermolecular Forces Operative in Biological Processes," Science 92:77-79 (1940).

origin of this paper is the following. I came here in the summer of '40 to immigrate properly--I was here only as a visitor. To convert myself into a real immigrant I had to first emigrate to Mexico and then back here. So that summer I came here and met Pauling on the campus, and asked him whether he had read some recent papers by Pascual Jordan. Jordan was one of the founders of quantum mechanics and a friend of mine from the Göttingen days. In these papers Jordan had claimed that quantum mechanics shows that identical macromolecules had a special quantum mechanical resonance attraction for each other, and that that had something to do with both gene replication and with the synopsis of like molecules on the strands of homologous chromosomes during meiosis. Pauling came with me over to the Physics Library to read this paper, and he looked at it for about five minutes and then said, "That's baloney." I was impressed how firm he was in his opinion, because I was not sufficiently familiar with the applications of quantum mechanics to more complicated chemical systems to be sure of my ground, although I thought it looked a little unlikely. A few days later I met him again (I guess after I had been down to Mexico and back) and he said, "Oh, I have written a little note to Science about this. Would you like to join me in publishing this?" So I went over there and he showed me this letter which didn't say that what Jordan said was baloney, but almost in those words. So all I could do was mitigate it a little, and he asked me to sign it and, well, I didn't want to be impolite. Ever since then Pauling has referred to this note as our "joint paper", and has also claimed that we stated as our firm belief that the principle of replication of the gene involved the synthesis of a complementary, rather than an identical structure. Well, you have to read the paper very closely to find this view expressed in it, and I don't think Pauling has read it over very closely since then. Anyhow it didn't strike anybody else as having been very prophetic. Certainly the other application that Pauling made of this complementarity argument, when he developed a theory of antibody formation, implying that the antibody is formed around the antigen, forming a complementary structure, that we now know is wrong; whereas in the case of DNA replication, we have every reason to think that it is right. So that

was the origin of this little note.

Harding: Since we are on the subject of chemistry, a number of people have commented on your deprecation or even hostility towards chemistry in the investigation of biological systems.

Delbruck: I think what did happen was that I was impatient with biochemistry in the sense of metabolic pathways converting one small molecule into another, and with the idea that the further pursuit of this kind of biochemistry would lead to the understanding of the nature of the gene, and its replication, and its effects. It was obvious that you could do this kind of conventional biochemistry ad infinitum, and that it was enormously bewildering in the number of compounds that they handled; you had to learn a special language for it, but you didn't really learn what I was interested in. Also the so-called biochemical genetics, the Neurospora genetics, that tied together genetics and biochemistry so beautifully, only highlighted the difficulty even more. You could learn an enormous amount about actual biosynthetic chains and their interrelations, but you did not learn at all how the enzymes came about; and if you say, "One gene, one enzyme," then the question remained, how does the gene make the enzyme, and how does the gene make the gene, and this was in fact not answered at all by any of the biochemical approaches. So in a sense I think my reservations about the powers of biochemistry were appropriate, and if in addition I was glib and arrogant about it, then that was just a personality defect. I mean it was, of course, true that I had never learned any chemistry or biochemistry, and just did not want to take the time to do so. In recent years I have had to learn quite a bit more, and I wish I knew more, because it's all book learning. I still haven't mastered any of the elementary procedures used in chemistry and biochemistry, but I can at least talk to those who have in a meaningful way.

Harding: I have just a few more questions. When [Erwin] Schrödinger's

book What Is Life? was published in 1945, what was your reaction to it? Had you known that he had discussed the model that you had put forward ten years earlier?

Delbrück: No, it was a total surprise to me. No, I had not seen or heard anything from Schrödinger, or by Schrödinger, for years, and when the book came out it was other people who drew my attention to it. I was puzzled how he had gotten hold of the paper--that was the one with Timoféeff and Zimmer in the Nachrichten--that he obviously had read, and which then formed a central chapter in the book. I have recently learned, I think from [Robert] Olby or somebody else, that it was not I who had sent a copy of the paper to Schrödinger, but that [P. P.] Ewald had shown him a copy.

Harding: Did that book have the effect of increasing people's interest in what you were doing then in 1945?

Delbrück: Insofar as it was read by a large number of younger, and not so young, people and physicists it was publicity for me, although not specifically publicity for phage, more for genetics and for the problems posed by genetics. I mean I didn't need publicity, I would say, but maybe I owe my job at Caltech to it, I don't know. I doubt that I did, because Beadle knew me personally quite well when he offered me the job, and also the people here in the Division had seen me around for two years; I don't think they needed Schrödinger's book when the question came up whether they should offer me a job here, which was done in December 1946, and the book came out about a year earlier. But I don't know what went on here.

Harding: Another question I have is whether you experienced any sort of culture shock, coming to the United States from Germany, and how you found this period, which must have been very difficult before you were naturalized, and even once you were naturalized, being a German in this country?

Delbrück: I think the culture shock was very much less than when I went to England eight years earlier, and really had to move into a different language. That was both an enormous culture shock and tremendous enrichment; the greatest enrichment of my life was really to learn another, totally other language--not totally other, but very different, language--so as to live within that language. That was, I think, the most important experience of my life--besides being born and being raised in one language--this addition of another language, and through the other language, then, the other culture. By the time I came here, I had seen many Americans and was fluent in the language, and I don't recall the culture shock as being traumatic in any way, or especially liberating; I was very happy. From '39 to '41, I was just from a different country; theoretically the United States was still neutral, but then after Pearl Harbor I was an enemy alien. But in Nashville that was not serious. There were some minor restrictions of movement, and I am sure there were some people who were suspicious that I might be engaged in espionage or something, but that was only natural. We had very good friends there, especially one non-University person, Alfred Starr and his family, a businessman who befriended us and some other University and non-University people; we were all friends of the Starrs, and both my wife and I felt very comfortable in this circle. Maybe you should bring her into the conversation the next time. I really think that from now on we should, because she has a very good perception of many of these more human aspects, both in Nashville and later on here. Better interview her separately from me, so there's no mutual inhibition.

Harding: And yet although you were happy at Vanderbilt you were quite sure that you wanted to move. That's the impression I get from some of your correspondence.

Delbrück: Well, then you know better. When the question really came up, to stay or not to stay at Vanderbilt--I mean when the people

at Vanderbilt realized that I was very much in demand after the war, and then I got offers from Illinois, and Cold Spring Harbor, and from here, and from Manchester, England--then all of a sudden they tried to really promise me anything, and I think I was quite willing to listen, but I think Manny was not, as I recall. In any case when the offer from Caltech came, it was irresistible.

Harding: Why don't we talk about Caltech next time? Shall we finish by discussing the "Principle of Limited Sloppiness"?

Delbrück: That was a casual remark that I made at a meeting in Oak Ridge after the war where I was chairman. The meeting was called to discuss photoreactivation, which had recently been discovered by [Albert] Kelner in bacteria, and by [Renato] Dulbecco in phage. It just amazed me that this very striking effect had not been discovered before. Many scientists had irradiated bacteria and phage with ultraviolet light, including Luria, myself, Dulbecco, and so on and so forth, and had measured survival rates. It turns out that if you measure the survival in the presence of daylight, then you get entirely different values than when you measure survival in the dark or in red light. The reason that it hadn't been discovered was because whoever had done the measurements had done them very carefully under controlled conditions, always the same light. Both Kelner and Dulbecco had done the experiments in a little more sloppy way, sometimes putting the plates here, and sometimes putting the plates there, sometimes having the water bath near the window, and sometimes not near the window, and then noting that there was something grossly different. So in introducing this little symposium, I said it shows the usefulness of limited sloppiness. If you are too sloppy, then you never get reproducible results, and then you never can draw any conclusions; but if you are just a little sloppy, then when you see something startling, you say, "Oh, my God, what did I do, what did I do different this time?" And if you really accidentally varied only

one parameter, you nail it down, and that's exactly what happened in both of these cases. So I called it the "Principle of Limited Sloppiness". Apparently nobody had pointed that out before in this slightly funny way, and everybody thought it was a very convincing thing, except Kelner. He was absolutely mad as a tomcat at me, that I had accused him of limited sloppiness. "I was not sloppy!" he said.

Harding: You were insulting him, not complimenting him.

Delbrück: Yes, it was very funny. I had a very hard time calming him down after this. This principle has been quoted a number of times since then.

Harding: So do you encourage it in your students?

Delbrück: Well, it's difficult to encourage because most of them are too sloppy anyhow. I mean I have to encourage the limited part, but it is true. I mean everybody has his own style of sloppiness, and Luria certainly was very careful to do things exactly alike, as I remember. I think for that reason he missed a number of discoveries that he would otherwise have made, that others made instead. It's not really a useful principle, because it's difficult to say what "limited" means. It was more a topic of conversation than a real principle.

Max Delbrück

Session 5

1 August 1978

Begin Tape 5, Side 1

Harding: You came to Caltech in 1947, and were Beadle's first faculty appointee in Biology, and I was wondering what changes you see in the Biology Division from 1947.

Delbrück: Well, it got bigger which is not necessarily fortunate, and its emphasis shifted.

Harding: To chemical biology?

Delbrück: Well, it shifted to chemical biology when Beadle came, more to molecular biology at first; then very soon the psychobiology was added, the [Roger] Sperry group, and that was an interesting move. This was made possible by a fund that Caltech had received, the so-called Hixon Fund, which was obtained for research that would do something about juvenile delinquency. From year to year the Hixon Committee struggled to find something that could be interpreted as having even the remotest connection to juvenile delinquency, and at the same time be compatible with the general attitude at Caltech of doing basic research. After having struggled for a number of years with that--arranging conferences, having visiting professors, and so on--the Committee disembarassed itself by appointing Roger Sperry as the Hixon Professor, so from then on he had to worry about how to reconcile this. (I was a member of that Committee.) That was an important move, and the contributions of Sperry have been enormous. It's very unfortunate that there wasn't a happy relation between Sperry and the chairman who succeeded Beadle, Ray Owen. I mean there would have been an unhappy relation between Sperry and Beadle too, but it didn't come to that because there were only a few years of [overlap], I think. I don't really know what their relations were; but the relation in subsequent years between Sperry and the chairman and the administration were

unhappy, because Sperry thought that behavioral biology and psychobiology should be set up as a separate division, and not develop out of Biology. The administration and Ray Owen just wouldn't go along with that at all. They said Caltech was so many divisions and that's it, and whatever you want to do, you have to develop within the present Divisional framework.

I could see the reasoning of Sperry; if there had not been a Biology Division, and you had been brought here as a biophysicist and then expected to develop biophysics out of Physics, it would have been entirely the wrong starting place, and the wrong things would have been emphasized. I think in effect the same thing has happened with our present behavioral biology. It has grown out of Biology, and therefore has never developed the breadth and freedom that it could have developed if it had been set up de novo in a new division. But the difficulty was that Sperry himself was not willing to take on the responsibility of setting up such a new division. He was always very ambivalent and wanting things done in that way, but not wishing to do them himself. So that has been a very unfortunate part of the development of the Biology Division, which is still very, very manifest now in my opinion. The newer biology, itself, of course, came into a new phase with the building of the lab across the street, the Beckman Laboratories, and the corresponding new appointments. It's too early to say how much good will come out of that. There are good people there, so presumably good will come out of it.

Another development that took place was that when Beadle came here, one of the strong members of the faculty was Frits Went, the plant physiologist, and he had magnificent plans for a controlled plant physiology laboratory, the phytotron, and that also led to personality conflicts. Frits Went was very imaginative as to how that should be set up and how it should be engineered, but I don't think he was a good businessman or an engineer. That led to many headaches and complications, and eventually Frits Went left, and was followed by Anton Lang in using the phytotron, who was the only

plant physiologist far and wide that we thought was a really good scientist. He carried on for a number of years, and did very constructive work, but then he left to go to Michigan. After that the Division tried very hard to find a successor, but it was just impressive how low the quality was of the people that were recommended to us as the best in the field. So eventually all of plant physiology was essentially scrapped at Caltech, and the one principal member who remained, James Bonner, turned more and more to the direction of molecular genetics, and has been completely out of plant physiology for many years now.

Harding: Do you think the Division has made an effort to identify new and coming fields?

Delbrück: Well, they considered bringing me here as being a new and coming field, and in recent years, certainly, they have in eukaryotic molecular genetics made several important appointments like Eric Davidson, and Tom Maniatis, and before that, Giuseppe Attardi. Then there was, of course, a period where Caltech went into animal viruses quite strongly. That was initiated in 1950 and, similarly to the Hixon business, came about through a stimulus from the outside; namely, a wealthy citizen of San Marino, who suffered from Herpes zoster, was persuaded to offer Caltech \$100,000 to start working on animal viruses. Beadle discussed that with me and with others quite a bit, and we decided since none of us knew anything about animal viruses, we should have a meeting here. We brought together animal virologists, plant virologists, and our local bacterial virologists--that was 1950--and a little book came out of that, which was very hastily put together.¹⁷

The upshot was that we didn't think any of the people in either plant virology or animal virology was really doing any very

17. M. Delbrück, ed. Viruses 1950 (Pasadena, California: California Institute of Technology, 1950).

innovative or creative work. I think it was I who suggested to Beadle that we try the other tack; namely, send [Renato] Dulbecco, who was a postdoc with me, to visit the labs and see whether he wouldn't come up with some new ideas. Dulbecco was enthusiastic about it, and he went on a three-month tour. During that tour he developed the idea that it should be possible to really make a quantitative plaque assay of some animal viruses, which everybody else told him couldn't work and wouldn't work, and which he decided he would try anyhow. A preliminary lab facility was set up for him across from the Huntington Hospital. (All of this has been described.¹⁸) For a while, then, we had a strong animal virus group in the subbasement here.

Harding: Was there much contact between the animal virus group and your group?

Delbrück: Oh yes, naturally. This contact has been described by [George] Streisinger in the phage book.¹⁹ He shared the room with Harry Rubin.

Harding: Did you continue to take a very active interest in the phage work that was continuing even after your interests had switched to Phycomyces?

Delbrück: I had to because it was still my group, and in fact in the late '50's I had a partial return to molecular genetics, in the sense that I did some work on UV photochemistry of nucleic acids, or tried to. Harold Johns, who was on sabbatical working with me, and I developed a monster UV monochromator--bigger than a grand piano--and that monochromator was used for a while to do some experiments.

18. Renato Dulbecco, "The Plaque Technique and the Development of Quantitative Animal Virology," in Phage and the Origins of Molecular Biology, John Cairns, Gunther S. Stent, James D. Watson, eds. (Cold Spring Harbor, New York: Cold Spring Harbor Laboratory of Quantitative Biology, 1966), 287-291.

19. George Streisinger, "Terminal Redundancy, or All's Well that Ends Well," *Ibid.*, 335-340.

A copy of it was built when I went to Cologne in 1961-63, and a copy of it was built by Harold Johns when he went back to Toronto, and the original version from here was later on given to somebody at Baltimore.

Harding: I am curious as to whether, when the Watson-Crick structure of DNA came out, there was a general feeling among biologists that this really marked a revolutionary point in biology.

Delbrück: Let's put this question into two questions, whether I thought so and whether there was a general feeling.

Harding: I know you thought so. You wrote to Bohr that you thought it equalled the Rutherford discovery of the nucleus of the atom. Do you still think in retrospect that was...

Delbrück: Oh, sure. Easily.

Harding: And the other half of the question?

Delbrück: The other half of the question; I think there was considerable hesitation as to whether the structure was true. Shortly afterwards there was a Cold Spring Harbor symposium, and some of the more knowledgeable chemists were quite doubtful, a) whether it's true, and b) whether it would ever be possible to prove that it was true. Now it's an interesting fact that there are several aspects to the structure. One is the two-strandedness. Everybody now knows that that is true for the majority of DNAs, and we also know other cases where it's true that it is single-stranded. Secondly, the two strands run antiparallel, and everybody knows that that is absolutely true; that's been proved beyond any doubt. The third thing is the complementarity of the bases, and that everybody agrees is absolutely true. And the fourth thing is the interlocking--that means that the two strands run around each other--and that, in fact, has come under a cloud

recently again. People have become aware that the alternative models that have been proposed cannot be dismissed out of hand, that this double-helicity has never been adequately proved. Presumably it will be proved in the next year or two, by synthesizing short 10- or 12-nucleotide pieces, and complementary pieces, and then crystallizing the double helix, and then proving the absolute structure of that. It will be very fascinating to see whether it comes out right. The reason that it hasn't been absolutely proved is that you don't really have true crystals of DNA, under any circumstances. In those cases where you have crystalline double-strandedness, in transfer RNA, there it has been proved that the helix is as in the model, but that's RNA and that's certainly limited sequences of RNA, so the generality of it is still not totally proved. Well, then the next question was, granted that the model is true, is the replication occurring in the way the model suggests; namely, each strand making its complementary strand. And that immediately poses a problem as to how the two daughter double helices are taken apart, how their knots are resolved. And that problem is still unresolved, or incompletely resolved.

Harding: You wrote a paper on that with [Gunther] Stent, right?

Delbrück: I wrote two papers on that, one with Stent which just formulated the problem, and one where I proposed a model that involved a lot of breakage and reunion.²⁰ In recent years all these enzymes have been found--the nicking-closing enzymes, and gyrase, and anti-gyrase and what not--that do a lot of cutting and gluing together, different from the one that I had in my model. This debate is still continuing as to how the intertwining is resolved -- but on the whole, the most basic feature, that the replication is done by synthesis of complementary strands, that is very clearly true.

20. Max Delbrück and Gunther S. Stent, "On the Mechanism of DNA Replication," in A Symposium on the Chemical Basis of Heredity, William D. McElroy and Bentley Glass, eds. (Baltimore: The Johns Hopkins University Press, 1957), pp. 699-736; M. Delbrück, "On the Replication of Deoxyribonucleic Acid (DNA)," Proc. Natl. Acad. Sci. U.S.A. 40: 783-788 (1954).

The next question was, what do you do with this information that is stored there in the DNA? How do you go from there to really making proteins? And that has been largely resolved in the sense that we know how the amino acid sequences in the proteins are coded for a template code, but here again in the last couple of years it has been found that in eukaryotes, all kind of monkey business occurs; that the gene that codes for a certain messenger RNA-- which then is translated into protein--that this gene contains interstitial pieces that are eliminated later, and the meaning of that nobody knows yet. So there are still surprises.

Harding: I get the feeling that you are always interested in the questions that still remain.

Delbrück: Well, that's what you are asking me, what people thought in the beginning. I guess my feeling from the beginning was that the structure is convincing, and that what it suggests about replication is also convincing, but how the knots are resolved is a mystery. And as I say it still is a mystery. And how this information is really used to code genetic information, that was totally nebulous in the beginning; the first incredibly bold attempt to cut through this fog was [George] Gamow's who suggested a direct method of using DNA as a template; to lay the amino acid sequence on it in the big groove of the DNA. That was manifestly wrong in its chemical details, and was therefore rejected by everybody including me, but surprisingly in the end it turned out to be relatively close to the truth. Of course, that was before transfer RNA and ribosomes and all this other jazz had been discovered. But the general principle that the coding follows such simple rules turned out to be true, and that was a very great surprise to me. I was certainly not prepared to believe in such a simple procedure.

Harding: I'd like to ask you a somewhat more personal question. You have known Watson for quite a long time, and I've seen a couple of letters which you've written, in which you describe him as being a very creative scientist but, on the other hand, very concerned, or

solely concerned, with his own scientific and personal problems. Would you be willing to talk about his personality, since he is certainly one of the dominant figures of molecular biology?

Delbrück: Well, he has astonished me at every turn of his career. This letter to which you refer is a letter that I wrote to Beadle in '54. The question came up of getting Watson here on the faculty; he had been here for half a year, and had been a total failure. He was very depressed at that time and totally uncooperative, and actually, you might say, a disruptive influence. He was really in the dumps at that time, so I didn't expect him to come to, and be constructive; I think when he then went to Harvard, he did in fact continue to be very unhappy and unproductive for some time, but I don't know the details. I think he only got going and got an experimental group going, and became enthusiastic and productive again when Alfred Tissières joined him. I don't know what year that was. That was the beginning of his setting up and developing a first-rate research group. So he certainly showed then that he could be a very good judge of quality in people, and also could hold a group together and foster first-rate work. That surprised me favorably, and then again I was very surprised when he took over Cold Spring Harbor as an administrator.

Harding: Do you think he has done a good job running it?

Delbrück: Oh, he has not only done a good job running, he has almost recreated it, you might say; as a fund raiser he has completely reconstructed the place. He has gotten an endowment together, which the place never had; he has gotten money to rebuild all the buildings from the inside completely new, and has done a very good job of almost overdeveloping, you might say. Many people think it has lost its intimacy and has just become a high pressure research group. In the old days, and by the old days I mean the '40's and '50's, it was never a high pressure place, but it certainly was a very creative place; but creative in a leisurely way, not leisurely exactly, but in an unpressured way. Now it has

become quite a big operation, yet we have enjoyed every summer we have been there, which is up until two years ago; even though they run three different courses simultaneously, and every three weeks a new course, thus there is a total of nine courses plus about five symposia. So there is a tremendous influx of people every summer, and you can hear at least three high-powered seminars every day, and yet you don't have to go to them. You can go to the beach, and just confine yourself to sit next to the people you want to sit next to at luncheon, breakfast and dinner, and talk to the people you want to. He has done a first-rate job, no doubt about it. He is still a neurotic person, I think. The most he has surprised me with is that he got successfully married after numerous attempts to get married, which were all dismal failures. He finally married a very, very nice girl--a very successful marriage.

Harding: What did you think of The Double Helix?

Delbrück: Well, that shows that you haven't read a letter that I wrote to him about that. That probably isn't in any collection. He showed the manuscript to quite a number of friends of his before it was published, and I thought it was an important confession on his part, that it was a need for him. People have criticized that he says so many nasty things about other people, but the thing that strikes me most is that he says nastier things about himself than about anybody else, and he obviously had a need to do so. I was surprised by the book, because there are many nasty things about himself I was not aware of, although I thought I knew him. I think my letter was just a formal criticism; the book was written when he was about 37, and is about the time when he was 23, and often as you read the book you don't know who is speaking, the boy of 23 or the man of 37. But he said, "Never mind. It has to be vigorous writing. I wanted to really write a readable book, not a scholarly book." And I guess it was a point well taken.

Harding: Did you read the book about Rosalind Franklin that came out a few years ago?

Delbrück: No. I am not interested in this controversy. Why should I worry about guilt or no guilt? I mean if Jim was worried about his guilt, and writes a book about it, that's fine. And that's a readable book and that's a genuine book because he writes about it. I think these questions of credit for this and credit for that are not very interesting in science. In the end the personalities are really irrelevant to the science itself. What remains on the one hand is the science, and not who contributed what. It's a different thing if you want to write a biography of that person, but that's quite separate from the science.

Harding: Although you may read The Double Helix primarily as a confession by James Watson, it seems that many people, and especially many nonscientists, read The Double Helix as this is the way science is done. In fact science is not what you read in the textbooks, or science is not the pure search for knowledge, but is rather this race to see who can find the answer first.

Delbrück: I think science is done many different ways, and I think that Jim's presentation represents one way that is real for a certain section, but it is not representative of all of them. Anybody can figure that out for himself. You can't expect Jim to write representative for all scientists. I certainly think that the way the development of science is represented in most textbooks is completely asinine; I mean at least the way I have seen it presented as progressing from hypothesis via experiment to conclusion or something like that. The progress of science is tremendously disorderly, and the motivations that lead to this progress are tremendously varied, and the reasons why scientists go into science, the personal motivations, are tremendously varied. I have said what I have to say about that in the Beckett lecture, at least one particular point that seems to be missed; that science is a haven for freaks, that people go into science because they are misfits, and that it is a sheltered place where they can spin their own yarn and have recognition, be tolerated and happy, and have approval for it.²¹

21. Max Delbrück, "Homo Scientificus According to Beckett," in Science, Scientists, and Society, William Beranek, Jr., ed. (Tarrytown-on-Hudson, New York; Bogden & Quigley, Inc., 1972), 132-152.

Harding: But you would agree, wouldn't you, that compulsive behavior which is directed at solving a puzzle is very different from compulsive behavior which is aimed at winning the Nobel Prize?

Delbrück: I don't think that you can separate the two. I mean this Nobel Prize thing is overdone. I guess some people are interested in it. That is certainly a sign of tremendous immaturity, but I come back to the same thing, it is not so different. Scientists are freaks and they are immature and unbalanced and like many other people have dreams of glory, especially boys from the age of 15 and 16. They dream of glory, and they have stuck with these dreams of glory, like my son, Toby; he's going to be a student at UCSD and wants to go into systems analysis. I am sure he has great dreams that this is the way to obtain power, and control of power, and things like that. You can speculate about it ad lib, in any form or manner, Freudian or otherwise, so I don't think that is restricted to science. The particular thing about science is to combine that with a retreat from the world. Other people want to obtain power by going out into the world, but the scientist really wants to obtain power by retreating from the world. I can say also that I very early decided for myself that when you do science, you potentially change the world much more than Caesar or any of the great military or political figures ever did, and you can sit very quietly in a corner and do that. So this dream of power is a realization that the great things in the world are not done necessarily with great expense of mechanical energy or chemical energy, but by small perturbations causing great effects, and that's what I mean. Since this is so manifest in science--that the person who thinks and puzzles things out can cause tremendous effects, and maybe even get the Nobel Prize if you want to focus attention on that, if that particular thing gives you the satisfaction--then it's not so different.

I don't want to run down the Nobel Prize because when it came I would say that I wasn't overwhelmed by it, because I knew that what I had done was comparable to what other people had done, and so I was on a par with many of them; I knew also that it was very arbitrary who

gets it, and who doesn't get it, because there are a large number of people whose contributions turn out to be valuable. This whole business of merit is a very arbitrary one. So when it came it was an especially pleasant thing to happen. Let me go back. It's not like if you are a writer, let us say, and you have struggled for 30 years to establish a name for yourself, and all of a sudden you get this bonanza, all this recognition. For many scientists that is not so. I mean by the time they get the Nobel Prize they have long since become full professors, they have all the grants, they have got everything they want. It doesn't mean anything except that they now get a lot of solicitations to contribute to that, and a lot of solicitations to put their name onto this, and it's a lot of minor nuisances and minor ego trips involved with it. But the trip itself to Stockholm was a very pleasant surprise, because the Swedes themselves take so much delight in the festivities. It was especially nice that in this case we went together with the Lurias, and the Hersheys, and the Gell-Manns. The families were there, and we all stayed together on the fifth floor of the Grand Hotel, and did a lot of kidding of each other. It was really a family party and none of us was too uptight about it.

Harding: And it's very fitting in a way that you won the same year that Beckett did, although he didn't come.

Delbrück: He didn't come, the dog...Yes, I'm not going to talk about Beckett.

Harding: Okay. How would you like to talk about teaching?

Delbrück: I don't think my teaching has been anything... well, my teaching has been poor all the way through, in the sense that I never really was on top of the subject that I was teaching.

Harding: We're talking about class teaching.

Delbrück: Well, let me specify. The only teaching I have done is this; one year Beadle asked me to teach General Biology, and that was a hair-raising experience for me since I didn't know anything about it. The only satisfaction I had was when I talked about a botanical subject, I found that none of the people brought up as zoologists knew anything, and vice versa. So my ignorance was not greater than the intersection of the ignorance of the faculty, but the students made some very funny comments about it. And then I taught every year one term, the winter term, a course called "Selected Topics in Biophysics," and that has been every year something different, and every year something I wanted to learn. Every year I spent an enormous amount of time learning a new subject, some current topic or something fairly remote from actual biological research. The subject was always new, and I always was not on top of it. So it was chromosome mechanics, and nonlinear differential equations, and membranes, and receptor physiology. Occasionally I repeated the topic, but changed it in substance. The last two times was this epistemology business, that still ran under this pseudonym, "Selected Topics in Biophysics."

This teaching has had a minimum of attendance. In the old days I never had more than really a handful of students, and in later years, maybe a dozen or something like this, and in addition a number of kibitzers, postdocs and sometimes faculty, etc. I think maybe the most influential part has been the membrane lectures, that I started taking up teaching membranes. That was before anybody here at Caltech paid attention to membranes, and I think that had some influence. That was interesting, because none of us knew anything about lipids, or membrane proteins, or mechanical or electrical or permeability properties of membranes; so that was useful as a general preparatory ground, and led to quite a number of people taking an interest in membranes.

Harding: It seems like you have also been a teacher in a less academic sense; that is, in the sense of having had so many research fellows. Seymour Benzer tells a story about how, when they weren't writing their

papers, you used to take them down to Corona del Mar, and lock them in a room until they would write.²²

Delbrück: Not quite. Yes, legends have grown up on that. Well, any professor does that with his students, try to make them do this or that. These expeditions to Corona del Mar were very nice. This was before Corona del Mar had been converted into its modern form, when it was in very little use, and had four small bedrooms, so that was a very nice place to go and write a paper.

Harding: I get the impression that you never withheld criticism if you thought it was deserved. I just think of the person visiting a couple of days ago, who was giving a seminar in the group meeting, and you told him, no holds barred, that he was giving a terrible seminar.

Delbrück: He did, didn't he? Well, they all know that, so they expect it.

Harding: So you feel that criticism like that shouldn't be taken in any case personally.

Delbrück: No, certainly not. Well, what do you mean "personally"? I mean it's certainly personal. If he gives a terrible seminar, it is he who gives a terrible seminar, but there is always room for improvement; I mean you won't improve unless you are told that it's lousy.

Harding: But I can imagine people who might be embarrassed or somewhat upset if they were told that.

Delbrück: Occasionally people have reacted in a stupid way, very occasionally. I think very few people. I think there is much too little outspoken criticism, out of a misunderstood sense of considerateness. The same as with walking out of seminars. I have been often

22. Seymour Benzer, "Adventures in the rII region," in Phage and the Origins of Molecular Biology, op. cit., 158.

accused of walking out of seminars, but I think that is done much too little. I know of hundreds of seminars where people afterwards said, "I didn't understand a word," but yet they sit there docilely like sheep, and let the seminar finish just because they don't want to offend the speaker. A very inefficient way of doing it.

Harding: Do you think there is a general lack in science education of teaching people how to convey their ideas and express them clearly?

Delbrück: Oh, there sure is! I think the Chemistry Division makes a special effort to teach students how to give a seminar. They have a course in that. The Biology Division never had. You just learn gradually. I gave lousy talks too when I was a student, and most students have very little opportunity to give talks, even graduate students, very little. One of the purposes of our group meetings is to give people a chance to stand up against the blackboard and talk, organize their thoughts; I'm also always trying to force them to write it down beforehand, and make a handout.

Max Delbrück

Session 6

11 September 1978

Begin Tape 6, Side 1

Harding: Today we're going to discuss the relation between physics and biology, and the role of physicists in modern biology. It occurred to me that I never asked you why, in retrospect, you think that the principle of complementarity made such an impression on you.

Delbrück: Several places I have explained that back in the old days, the way matter behaved when it was integrated into living organisms seemed to display properties that it doesn't have in the nonliving state, so that for thousands of years it was thought that there was something entirely different involved. Then the nineteenth century showed a reversal in that, and people showed that after all there are the same elements there; that you could actually synthesize some of the molecules, that energy is conserved, that entropy increases, and so on; that insofar as it was testable, it seemed to comply with the rules to which matter is subjected in the inorganic world. And yet, all the textbooks of biology start out with listing a number of properties of living matter that set it apart from non-living matter. And the question was, was that some fundamental principle that set it apart, or was it just a matter of complexity? That was difficult to understand, and Bohr's point of view seemed like a very intriguing one.

Harding: What I was trying to get at is something perhaps more psychological; for example, in retrospect, one could argue that or suggest that Bohr was a very influential person in your life, and that his ideas had particular significance because of...

Delbrück: Well, that was utterly true. Bohr had an enormous

influence but that in itself would not have done it. On the contrary, I would say that for quite some time he talked about these matters and it never made any sense to me at all, until one day it clicked, when he used this very simple analogy: here you have the hydrogen atom, and you have a proton and an electron running around, and you can do classical physics until your dying day and you'll never get a hydrogen atom out of it. In order to get a hydrogen atom out of it you have to use this complementarity approach. His analogy was that maybe, if you look at even the simplest kind of cell, you know it consists of the usual elements of organic chemistry, and obeys otherwise the laws of physics; you can analyze any number of compounds in it but you'll never get a living bacterium out of it, unless you introduce a totally new and complementary point of view. That, together with the very recent success that had happened in quantum mechanics, the uncertainty principle, showing in a hopeless situation a great simplicity, was an intriguing idea.

Harding: The other possibility that occurred to me was, I wondered if paradoxes in general had intrigued you, not only in physics but in other areas of philosophy perhaps, and if perhaps just the notion of working on a paradoxical kind of problem was important.

Delbrück: Not so much the paradoxical kind, as the hope of finding some simple solution in a situation where the straightforward approach seemed to bode only more and more complexity, going from one molecule to a more complicated molecule, biosynthetic cycle after biosynthetic cycle; whereas here we were hoping to establish the laws of heredity, which Mendel had found, actually, as a simple algebra, and that there was a correspondingly more simple basic algebra that would account for many of the phenomena of biology. That's always attractive, if you can do it. It's not so much the paradoxical that was attractive, as the hope of finding a simplicity behind complexities, I would say.

Harding: Where by simplicity you don't necessarily mean unity, because one of the conclusions that one draws, I guess, from the principle of complementarity is that there is not one unified way of looking at the universe, but that one needs to look from at least two angles.

Delbrück: Still Quantum Mechanics is a unified theory, but one renounces an ideal that had prevailed previously of locating everything causally in space and time.

Harding: The other comment I have is that I see a kind of paradox in this application of the principle of complementarity to biology which I would state like this: The conclusion that there will be phenomena which are not reducible to the phenomena of atomic physics is deduced from a principle of atomic physics, namely, the principle of complementarity; so that atomic physics becomes both, as it were, the weapon and the victim as far as this reducibility question is concerned. Do you see what I'm saying?

Delbrück: Yes, I see what you're saying, but I wouldn't put it that way. No, it was just a dialectical approach that had been discovered in quantum mechanics, and that Bohr thought surely would not be unique to that situation, in science, and if you discovered something that original, probably it has wider applicability; that quantum mechanics, as such, would again be subject to the need of a complementary approach for something else seemed very natural. And it still is.

Harding: Is what you're saying that there would emerge more general dialectics, perhaps, like...

Delbrück: Yes, like psychology and molecular biology. I mean they certainly are in a complementary relation which nobody still can formulate very well. They haven't been pursued to that bitter end

where you have to make some kind of a new dialectical approach.

Harding: So did Bohr believe, and do you believe, that it is an historical accident that the principle of complementarity emerged first in physics?

Delbrück: Oh yes, yes, definitely. Okay, let's talk about something else.

Harding: Okay. Would you like to say something about what you think physicists have contributed to biology, both in terms of concepts and methodology, and anything else?

Delbrück: Well, in methodology and technology they have contributed immensely. All of the analytical procedures used today are very heavily based on physics: centrifuging, electrophoresis, X-ray structure, radioactive tracers, refined optical methods, which are still becoming more sophisticated every day, including resonance raman spectroscopy, and still more refined aspects of it. It's just an avalanche of physical techniques that is still rolling, and is going to continue to roll. So in that respect there has been an immense input from physics to biological science. Conceptually, I don't think there has been that much. I mean there has been a difficulty with those physicists who came with a particular technique that they had learned in physics, and then tried to do biology; they have been largely unsuccessful, because this business of a man with a technique in search of a problem is often very unfertile. It's better if you are fairly widely educated in basic physics, and then look around in biology for an interesting problem, and then learn the particular techniques in physics that might be useful.

Harding: What do you think a general education in physics gives somebody in terms of ability to identify interesting problems in biology?

Delbrück: Well, if you have a good feeling for how molecules behave, you are better able to see what are really interesting problems. If you have just the vaguest notion of how matter behaves in the inorganic world, you think everything is possible and all your theories become arm-waving theories. You have to be able, if you want to make a theory, to really think it through in detail, see whether it's quantitatively reasonable, and so on.

Harding: Do you think physics training gives physicists a much better background in constructing theories and following through on their implications?

Delbrück: Some of them, yes, some of them. Many physicists nowadays are trained much too narrowly; if they are high energy physicists, or if they are low temperature physicists, or if they are solid state physicists, then they have a very specific and very limited training really in physics. In order to apply physics successfully in biology you have to know more physics than you have to know to do physics, not less but more. That's what several of the more intelligent ones find right away, since so many aspects of physics are involved even in the simplest biological phenomenon, such as chemotaxis in bacteria. You get involved with everything--with motility, and with diffusion, and with viscosity, and with hydrodynamics, and with electrical phenomena and electrochemistry and so on. And very few physicists are at all familiar with several of these subjects. If you know basic physics, certainly, at least you can go to the library and read the relevant literature with some understanding, but if you never had any physics or math then it's hopeless.

Harding: What do you think your own physics training did for you when you turned to biology?

Delbrück: Just this--that I was able to learn what I needed, with effort, at least learn on the theoretical side. Experimentally I

have always been very weak as far as physics experimentation goes. I never had any real familiarity with constructing equipment, and that has been my weakness throughout. Later on I had some associates who were good at that, and it was helpful.

Harding: But I think there must have been more involved than just an ability to learn, because you also chose a certain organism to work on, and certain kinds of problems to look at. Do you think, in retrospect, that these were influenced in some way by your training in physics?

Delbrück: No...No, they were influenced by this philosophical motivation to get at basic problems, and find the suitable organism and the suitable technique for that.

At one point, when I took the microbiology course from van Niel-- I think it was in 1940--he drew my attention to phototactic bacteria. He mentioned experiments showing that these bacteria could detect intensity differences of 10⁰% , and they could detect these intensity differences of 10⁰% irrespective of whether the intensity was very low, or a million times higher or even a billion times higher. That fascinated me, because what could be the mechanism would have to be a very general and a very simple mechanism that was able to do this detection business over such an enormous dynamic range. So when I first went into this business of signal handling as another aspect of molecular biology, I had my first graduate student here, Rod Clayton, work with these phototactic bacteria. Well, it turned out that the experiments were wrong; the dynamic range was not 10⁹ but about 100, but aside from that, the mechanism of how they detect these differences is still unknown.

A corresponding thing exists in Phycomyces. Phycomyces is not phototactic but phototropic; it grows to the right or to the left, and also detects intensity differences of about 10⁰%, and it operates over a very large range, and we still don't know how it does that. So the question was, is there some very fundamental law and fundamental simple phenomenon involved that is unknown, or is it just a combination of a number of tricks that enables the organism to do this detection over a wide range, by just having evolutionarily

adapted a variety of mechanisms that all lead to the same result, because it's obviously useful for the organism to have this discrimination threshold? That's still an unresolved question. I pointed that out in this lecture, "A Physicist Looks at Biology,"²³ that this is an ambiguity that pervades all of biology. You never know whether a law that you find, a quantitative law like this Weber-Fechner law in this case, represents a physical law employed by the organism, or whether it's an adaptation, and something very complicated which looks simple because it serves a simple purpose. It actually might be a very complicated mechanism, to be able to detect these differences over a wide dynamic range, and that it is not actually based on some very simple physical principle, but on a very complicated mechanism, and maybe at various intensity levels there are different mechanisms. This is an ambiguity that pervades all of biology.

Harding: I don't completely understand this ambiguity. It seems like the adaptation part would just apply to how the mechanism came into its present state.

Delbruck: Say you start out with a mechanism that permits it, at a given intensity, to discriminate between this intensity and 10^0 more. Now this organism moves to another habitat where the intensity level is 10 times higher. Then it will evolve modifying mechanisms that enable it to do this same discrimination at a higher intensity, and still higher, and still higher, intensities. So that it can do that over a range of 10^9 is not in the nature of some simple elementary process, but is the result of a combination of a large number of auxiliary mechanisms. Just like we can see over a wide range of intensities; part of it is due to our neural mechanisms

23. "A Physicist Looks at Biology," Trans. Conn. Acad. Arts Sci. 38: 173-190 (1949). Reprinted in Phage and the Origins of Molecular Biology, John Cairns, Gunther S. Stent, and James D. Watson, eds. (Cold Spring Harbor, New York: Cold Spring Harbor Laboratory of Quantitative Biology, 1966).

changing from one intensity level to another; our pupils are opening and closing, and so on and so forth. So there may be just a very complicated mess that's involved, and not some simple fundamental law that's valid over a wide range.

Harding: It's clear to me how organisms become more complex over thousands, millions years of adaptation. It's not so clear to me how this influences the sorts of laws that one finds in biological organisms.

Delbrück: Well, it does. It should.

Harding: Are you saying that because organisms have evolved over a long period of time, then the laws that you find in biological systems will be more complicated, the more complex the organism?

Delbrück: Not necessarily. The organism doesn't have to be so very complex. Phycomyces and these bacteria are not, as organisms go, very complex, but they have to cope with a great range of physical environments. They manage to adapt to greater and greater ranges by modifying some simple mechanism and supplementing it with auxiliary mechanisms, like the pupil opening and closing, and God knows what; or having in the retina many of the sensory elements gather light just for one neural input, and so on and so forth. I said that in fair detail in this "A Physicist Looks at Biology." This was written in 1949, long before the DNA story, but I think that's still correct, what I said there. And again in the "Geleitwort" for the German edition [of Phage and the Origins of Molecular Biology], it is also restated there.²⁴

Harding: Assuming that there is this ambiguity, what implications does that have for physicists coming into biology? Do you think that

24. "Geleitwort Zur deutschen Ausgabe," in Phagen und die Entwicklung der Molekularbiologie, John Cairns, Gunther S. Stent, and James D. Watson, eds.; E. Geissler, ed. German edition (Berlin: Akademie Verlag, 1972).

they really need to take cognizance of evolution in their training, and in their interpretation of their experimental results?

Delbrück: Hundreds of physicists have come to me and said, "I know very well UV-spectroscopy," or "I know X-ray diffraction," or "I know infrared spectroscopy, and what can I do? Name an interesting problem in biology that I could apply this to." I think it's an unprofitable thing to do. I think you should become a biologist and look around and find an interesting problem.

Harding: Well, it sounds almost as if the problem is not so much with the physicists, although if they have a particular technology one can understand that they might be particularly susceptible to this, but perhaps more, the problem reflects a more general disinclination to look around for new interesting problems, and then to learn or develop the methods that could produce important insights.

Delbrück: Okay, let's talk about something else.

Harding: Okay. Shall we talk about science in Germany, or something else you want to talk about?

Delbrück: Science in Germany was immensely successful in the nineteenth century; that was university science, and was largely a result of the reform of the universities after 1806. At that time the Berlin University was founded, on the basis of a memorandum drafted by Wilhelm von Humboldt, that placed considerable emphasis on research at the universities. The other universities in Germany followed the model of the Berlin University. A great deal of success was due to the fact that the universities were not federal universities, but universities of the various Länder: Bavaria, Prussia, Baden, and Hessen, and so on. The Ministries of Education of these various Länder competed with each other for the best; the system was that the faculties of the universities proposed, and the Minister chose from the propositions, the idea being that the faculties generally knew who was the best, but rarely wanted the best. So from this

profound human insight the construction was made that the faculties could propose, but that the minister would choose, because he could find out actually who was the best. So there was a great deal of competition, and the best people got the best places by and large, and they were well-endowed.

By the beginning of this century the big universities had too many students, and the teaching became too cumbersome, and then this Kaiser Wilhelm Gesellschaft idea came up. The Kaiser Wilhelm Gesellschaft was founded in about 1910, nominally as a private organization endowed largely by industrial contribution, and also somewhat by the government. So the very best people were taken out from teaching, and were made heads of these pure research institutes. That was in a way immensely successful, but in the long run, in my opinion, disastrous, because it took the best people out of teaching, and made the contact with the best students much poorer. Anyhow the whole thing got into disarray by the First World War, because financial sources dried up. During the Weimar Republic, with a great effort, it was resuscitated; by then the federal contribution was much larger, so that in the twenties and early thirties, the Kaiser Wilhelm Institutes were again quite flourishing. And then the Second World War knocked everything out again. The whole Nazi period had two effects: one, an enormous amount of emigration or expulsion of Jews, and other people who left because of the Nazis, and two, an enormous amount of physical destruction. Not as much killing; the First World War killed far more people than the second one in this class of people, but the physical destruction was just fantastic. And then there was the fragmentation of Germany into the Russian zone and the French zone and the British zone and the American zone. At first it was considered that the Kaiser Wilhelm Gesellschaft had been involved in warmongering and preparing for the war, which was completely stupid; the K.W.G. was forbidden and then it was gradually allowed in the various Länder, and then gradually it was reconstituted as a federal thing. I don't know what the present nominal set-up is; in effect it is a federal entity, and a very large entity.

My first visit back to Germany after the war was in '47 when things were still very chaotic, very chaotic. It was really hard to

move about and all the railroad stations were still beleaguered by thousands of people who were just camping out, and very little transportation, and food and everything. The currency hadn't been reorganized yet; cigarettes were still a large part of the currency, and so on.

Harding: What was the psychological state of the scientists that you met at that point? So many people had emigrated during the Nazi period, and of course the whole status of Germany during the war... was there much guilt among the scientists that you met? How did they feel about this experience of the last fifteen years?

Delbrück: It depended on who. No, I have explained earlier that if anybody feels guilty, I feel guilty of not having stayed, because I had so many friends who I admire for having stayed, and having tried to save what was to save, rescue it across this disaster. I have seen many of those; Karl Friedrich Bonhoeffer was one of them, Hans Kopfermann was another one, and many others for whom I have the greatest admiration--VonLaue, Heisenberg, too; Otto Hahn certainly.

Harding: Was there more a sense of relief, then, when you got there that here the war was over, and that they could start rebuilding?

Delbrück: Yes.

Harding: And they felt that they had done as much as they could do during the war to...

Delbrück: To save things, and to protect some of the younger people.

Harding: What sorts of things did they do to protect the young people?

Delbrück: My first contact, I think, was in '47 indirectly with a

young fellow, Carsten Bresch, who is now Professor of Genetics in Freiburg. He was a student at that time, and he had read about bacteriophage, and wanted me to bring him some of the phages that I had been working with. And that developed into a complicated maneuver, whether I was permitted to give him the phage. I gave the phages to the American control officer in Berlin, and he, after a great deal of soul searching, passed these phages on to Bresch. Bresch started doing some experiments at the Robert Koch Institute with very primitive equipment; it took him about two hours from where he lived to get to the place, and he had to construct his own constant temperature bath, and things like that. And he and his friend came and visited me at my sister's (Lene Hobe) where I was visiting and talked for several hours. The enthusiasm for going back to work and to research was just immense, a real explosion of enthusiasm getting back to something really worthwhile.

I visited also, I think, Otto Warburg at that time in Berlin, who arranged immediately for me to give a talk in the Harnack House. The Harnack House was the lecture hall of the faculty club of the Kaiser Wilhelm Gesellschaft and had been taken over by the American officers' club, which to many Germans was the ultimate insult--that this sacred place of the Kaiser Wilhelm Gesellschaft should not be some stupid American officers' club. Anyhow, Warburg was very happy to use this occasion of having an American visitor, namely me, come there so he could arrange a talk for me to give. He arranged it, I think, at 2:30 in the afternoon, and I said, "Well, 2:30 in the afternoon is a pretty bad time. I mean German professors have a good midday meal, and then they need a siesta. They wouldn't be able to stay awake."

"Oh," he said, "they don't get that much to eat that they can sleep afterwards." He could be pretty unexpected in his responses.

Harding: He was a director of one of the Kaiser Wilhelm Institutes. He had maintained that position all during the war?

Delbrück: He had maintained his position, although he was at least

half Jewish or something. He was a special protégé of Goering's. He was sort of extraterritorial.

Harding: You didn't care much for Warburg, did you? I've seen something you wrote to Hans Krebs that Warburg's style was enough to keep you from going into photosynthesis.

Delbrück: Well, maybe his polemical style, yes. Otto Warburg was a very great biochemist, the greatest maybe, but he was also the most impossible character, and he was the source of more outrageous stories than any other character of the century, practically more than Hilbert even. Warburg stories was an infinite source; I mean the world is poorer for Warburg not supplying any outrageous stories anymore.

Harding: You thought he was a latent homosexual.

Delbrück: Did I say that?

Harding: Yes.

Delbrück: I don't know how latent he was. I don't know. Maybe Krebs said that. Krebs wrote a very good article, biographical memoir.

Harding: I'm pretty sure you said it; you were talking about his paranoia.

Delbrück: I didn't know him that well and he obviously made many great discoveries and many mistakes. So where were we? The reconstruction after the war. My best Warburg story is that when I visited him in '47, we talked about this and that, and he said, "The Russians came and they took everything from the lab, instruments and papers, even my father's scientific papers." (His father had been

also an outstanding physicist.) "That's really a scandal isn't it," he said. I mean this was at the time when Berlin was in ruin and still practically smoking, and thousands of dead people were still trapped in the subways or something like that, and three-quarters of Berlin was just in ashes. But that the Russians had taken his father's scientific papers, that was really a scandal! It shows his sense of proportion. Impressive.

Harding: What happened after the war to those scientists in Germany who had been outspoken Nazi sympathizers?

Delbrück: I really don't know. At the time that I left, which was in '37, there were damn few who were outspoken Nazi sympathizers, a very, very few, and all of them very minor ones. Later on some more may have done some more lip service, or actually sympathized and gotten themselves into big positions, and then presumably they were ostracized for a while or lost their jobs or something like that. I don't know, but actually, in the circles that I was familiar with, there were very, very few.

So this was in '47, and then I must have visited a number of times afterwards, but the first time I came for longer was in '54. Then I came for three months and went to Göttingen, and was a guest of what was then already, I guess, the Max Planck Institute for Physical Chemistry, with [Karl Friedrich] Bonhoeffer as director. I came out of friendship for Bonhoeffer, and also because I was interested, and did set up some Phycomyces work there. Bonhoeffer was very liberal in what he permitted to happen at his physical chemistry institute; he also actually took on Bresch, who was a geneticist, for several years, which was a very useful thing. And I guess during that time the first contact was made to the people in Cologne, and then I came back for three months in 1956; namely, to Cologne as a guest of Josef Straub, who was a professor of botany, and who wanted me to bring molecular genetics to the University. At that time his institute was still in a bunker in the Botanical Gardens,

sort of subterranean caves, very preliminary. At that time the first new university institutes were being built, his among them. In fact, I think I gave a phage course there in this new building. They had no electric light yet, and no cement floors, but yet we moved in and gave a course there, which was quite a tour de force.

Harding: This was in '56?

Delbrück: '56.

Harding: How had they kept up with what was going on in the United States and Great Britain and France?

Delbrück: Well, very little. I mean they were trying to catch up. Everybody was working extremely hard.

Harding: Were they reading the journals?

Delbrück: Insofar as they had the money to buy them, yes, and so far as they knew English, which was not as common as it is now, and so far as they had the training. Most of the people in Germany who studied botany and zoology had no training in mathematics at all, and also had relatively little training in English, because up to that time most of the literature had been in German. I mean this tremendous transition of scientific writing from German to English took place just about that time.

Harding: So the government was not putting very much money into science still at that point?

Delbrück: It put a lot of money in, but it didn't have much money. At that time it was just barely digging out from under the ruins. I mean most of the work was still just reconstructing the barest minimum of facilities. Just an incredible amount of ruins still there in '56. Every time I went to Cologne I climbed to the top of

the cathedral, which is 505 steps, to look down on the city. It was interesting from year to year to see, first, the city in ruins and rubble, and then gradually these being cleared away; then first the banks and insurance companies being built up again, and then all kinds of new roads being constructed, and the churches being restored, and then gradually other things being built up again. There was an immense housing shortage through at least the early sixties. When we came in the early sixties there was still very little new housing.

So this was '56, and at the end of this stay there--they always wanted me to come there permanently, they wanted to offer me a job and I just couldn't see myself moving from Pasadena to Cologne--so in the end I made a mistake. Straub said always, "Name your conditions," and in the end I said, "I can name the conditions, but they are so astronomical that it would be ridiculous." "Okay," he said, "name them." So the last day I was there I named conditions which I hoped would be so astronomical that the matter would end there. But then due to the fantastic negotiating ability of Straub, the thing finally became a reality in '61, five years later. He always played me against the administration; he lured me by saying the administration was going to do that, and he lured the administration saying if you do that, then Delbrück will come. It was very, very difficult, because even where they had to build the building, that was really real estate earmarked for the new zoology institute, so they had to persuade the zoology professor to cede half of his territory and instead making our building twice as high. All kinds of ridiculous things like that. Within five years the thing finally came up, so then we went there from '61 to '63.

At that time already all over Europe there were new universities being founded, and similarly in Germany, they created a number of new universities. One of them, in Konstanz, was conceived at first as something analogous to an American private university, with high admission standards. Private universities don't exist in Europe; they are all state universities, even Oxford and Cambridge; although the English universities are nominally private, they de facto all

depend on the university grants committee, which is a government organization. So there were plans afoot to create in Germany such a private university, but it turned out to be not feasible because the accreditation of such an institution, the accreditation of the degrees awarded by it, would have had insuperable difficulties. There was also a movement afoot to create an international European institute of technology on a large scale. The plans for that went pretty far, but it fell through in the end. So what was founded in Konstanz was a state university, but with somehow higher admission standards or more exclusive. By hook and by crook they involved me in the founding committee, as a consultant for the natural sciences faculty. This led to a natural sciences faculty which was essentially all molecular biology, even the chemistry and the physical chemistry was all molecular biology.

Harding: What did you think of that?

Delbrück: Well, I was responsible for it, so I mean I tried to eliminate the classical disciplines of botany and zoology, and streamline it to contemporary avant garde research. It functions reasonably well now. This was for the first stage of it. Meanwhile much more has accreted around it. We went there at an early stage, for the summer semester '69. That was my last long stay in Germany.

The German universities have had their revolution like the rest of the world's universities from this country, and Japan and Europe and everywhere; the student revolutions and trying to reform, and especially to break the stranglehold on the university of the full professors. I understand that now after a tremendous amount of havoc being wreaked by all this commotion, now the reaction is in full swing, and the full professors are pretty much back where they used to be. But I haven't seen much of it. The Max Planck Institutes have expanded enormously; I think they by now have 80 institutes, some of them quite monster institutes, like the one in Göttingen, the Biophysical Chemistry Institute, and even more so the

one in Martinsried near Munich, the Biochemistry Institute--huge places, much too large, real megalomaniac constructions, and I don't think they are as productive as they should be.

Harding: Do you think that things should be decentralized more, or is there any partial answer to the megalomania tendency?

Delbrück: Well, I think they should be more integrated with the universities. The Martinsried institute is about 20 miles away from downtown, out in the country, and unless you really have absolutely topnotch people there who have a tremendous amount of inner drive--you sit out there and you have elegant labs, and you have a guaranteed budget, and you have a guaranteed staff--the chances are you will fall asleep, or you do just what you have always done; it's not competitive enough. I don't have the impression that it's very lively. It has been urged on them that they should at least do what they do in Cold Spring Harbor, have summer courses, and while that has been greeted with enthusiasm, it hasn't been done. It hasn't been done because it's a lot of work, and nobody who isn't really forced to it is likely to do a lot of work that isn't his own work, so the right twist hasn't been found. They have a nice and quite large Institute for Molecular Genetics in Berlin, in Dahlem, which is working reasonably well. They have the EMBO lab at Heidelberg--EMBO stands for European Molecular Biology Organization--and it's supposed to be an analogue to CERN [Conseil Européen pour la Recherche Nucleaire], and that is slowly getting off the ground. But I doubt that that will amount to very much. On the whole I have a feeling that nobody really knows whither research and education is going to move.

Harding: In Germany or generally?

Delbrück: In Europe, and probably here too.

Harding: Because things have just been getting larger and larger, and there must be a breaking point, or why?

Delbrück: For reasons as explained in the commencement speech.²⁵
The pristine faith in science has been punctured, and it's obvious that science is not going to solve our problems. Science is just as much a destabilizing force as it is a stabilizing force in the world. That's a very general thing. Specifically in Germany it's weighted down with all these problems of institutional lethargy and vested interests that go with it.

Harding: I wonder, do you find now that, as expressed in your commencement address, you really have strong doubts about pursuing science the way it has been pursued in this country and other countries for the last twenty years?

Delbrück: Yes. The honeymoon is over.

Harding: When you say the honeymoon is over, that has a variety of interpretations. On the one hand, money has been cut, so the honeymoon is over in that sense; it's also...

Delbrück: No, I don't think that's important.

Harding: It's over also in the sense that there seems to be, as you say, a sense that science does not solve all our problems and there is also perhaps somewhat of a distrust of science by the public.

Delbrück: Even by the scientists.

Harding: And by scientists themselves. Is the honeymoon also over in the sense that science is no longer rewarding in some ways, do you think?

Delbrück: To the individual?

25. "The Arrow of Time - Beginning and End," Engineering and Science 42 (Sept.- Oct. 1978), pp. 5-9.

Harding: Yes.

Delbrück: It's still rewarding for business, and it's still rewarding for the military, although how much? I wonder how much it is really rewarding for the military. They have all this overkill material, and I don't know how much more, of more sophisticated science, would be of practical use to them. Look at South Africa. Everybody is wondering whether South Africa has atomic weapons. If it had, what would it do with them? It can't use them against guerilla warfare inside, and there are no external enemies...Yes, I guess one would like to know more where really our values come from. You're asking me for the value questions. And so you can ask where do the values come from, and you can ask what should our values be, and if you have an answer to what our values should be, how do we get them to be our values, if you first decide what the values should be. These are not questions of science, but they are the questions the answer to which will decide the further course of history more than anything else. I think the further course of history will not be decided by further discoveries in science, but by these questions about human values. It is obvious that the Israelis and the Arabs have different human values, and how do you arbitrate between them and how do you understand really where they come from, and how can you manipulate them so as to become compatible? I think these are questions people will be more anxious to know the answer to than whether black holes exist. The newspapers may still write about black holes but that is just a palliative; I mean people like to read something that's utterly harmless and yet diverting. You can devote whole universities to these questions, but the public may not be so enthusiastic to support universities that tell them about black holes, rather than how to resolve conflicts between Kenya and Uganda, or South Africa and black Africa, or India and China.

Harding: Do you think it's possible that science will continue but that scientists will become more involved in value questions?

Delbrück: No. I think the scientist, insofar as he is a scientist, has to do what he did before. Scientific institutions, like Cal Tech, will have to become more involved in value questions.