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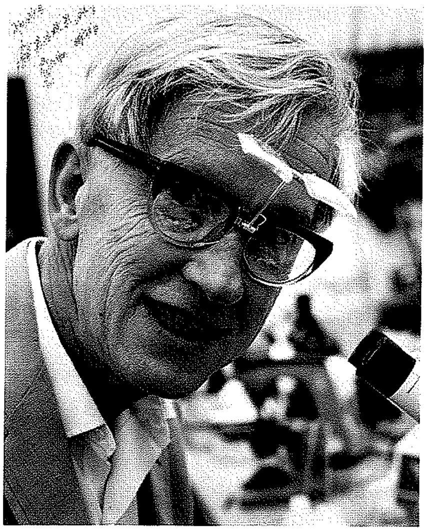
Oral History



Max Delbrück

—How It Was

Max Delbrück was born in Berlin in 1906, first came to Caltech in 1937, received the Nobel Prize in 1969, and became Board of Trustees Professor of Biology, emeritus, in 1977. He began his scientific studies in astronomy, changed to theoretical physics and then to biology, becoming – through his work on phage – a leader of molecular biology by the mid-1940s. The outbreak of World War II prevented his return to Germany, and he spent the years 1940 to 1947 at Vanderbilt University teaching physics and doing research in biology. In 1947 he returned to Caltech, and he has been a member of its faculty ever since.

It would be hard to write a less adequate description of the career of one of the most distinguished and humane scientists of this century, and fortunately the Oral History program of the Caltech Archives has made it possible to flesh out such a bare bones account. Six interviews conducted by Carolyn Kopp cover such topics as Delbrück's family and early education, his university education and postgraduate work, his early career in Germany, his phage work and the phage group, observations on Caltech and on physicists and biology, and his postwar visits to Germany – all sprinkled with fascinating anecdotes. In fact, having to omit more than half of the material so that excerpts would fit in E&S *turned out to be an exercise in making hard choices. We present here the first installment (of two).*

Max Delbrück: My father, Hans Delbrück, 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. He was also editor of a monthly called the *Preussische Jahrbücher* — that's a monthly somewhat analogous to the *Atlantic Monthly.* He singlehandedly edited that for at least 30 years and wrote a column commenting on German politics. There's a book on my father called *Hans Delbrück as a Critic of the Wilhelminian Era,* and that's what he was.

I was the youngest of seven children, four sisters and three brothers over a span of 16 years. My four sisters, Lore, Hanni, Lene, and Emmi, are all still alive. My oldest brother, Waldemar, 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 14 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 and intellectually threatening person, but my whole soul was concentrated on trying to compete not only with him but with the other siblings, and the older ones in our close friends' families, since I was the youngest in all these contexts.

My mother was, I think, 15 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

Berlin, 1927. Hans and Lina Delbrück with four of the seven Delbrück children, from left, Emmi (Bonhoeffer), Max, Justus, and Lore (Schmid).

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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.

CK: What about your next-door neighbors, the Harnacks?

MD: Our nearest relatives who lived next door, the Harnacks, were similar to our family. Like my father, 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, and most important, he became president of the Kaiser Wilhelm Society when it was founded in 1910. The Harnacks 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 about politics and history, and the others had to pipe down.

This whole section of the suburb of Berlin was just crawling with professors with large families; Karl Bonhoeffer, professor of psychiatry, around the corner, and the Max Planck family a little ways down, and the mathematician Hermann Amandus Schwarz, and 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 Bonhoeffers built were straightforward accommodations for large families, nothing very fancy about them.

CK: Were you close to your parents?

MD: 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.

CK: 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?

MD: In science, 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 was 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. My father had a great interest in philosophy, and his hero was Hegel for philosophy of history.

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

MD: I think they must have been until 1914 moderately well off. My father had his salary and his income as editor, and my mother had a dowry from her father, so there was a modest degree of affluence, and apparently the life until 1914 was pretty free and very hospitable. As the war came and life 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. 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 and coal shortages and then the total mess in 1918. So this relatively affluent residential suburb after the war became almost a ghost town.

CK: And the Second World War? There were others lost?

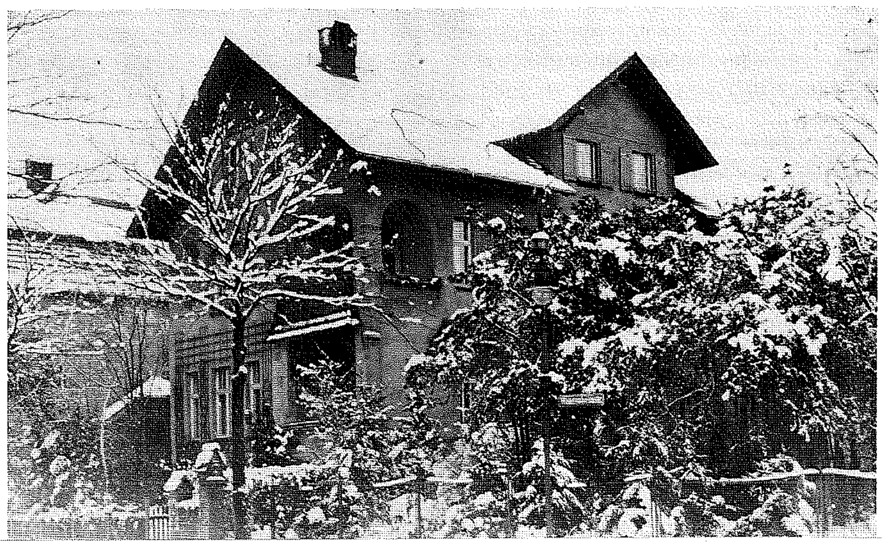
MD: Ernest Harnack participated in the German Resistance during the war and was executed by the Nazis. On the Bonhoeffer side there was, of course, a much greater involvement in the Resistance, and that has been widely documented. They lost two sons and two sons-in-law in the aftermath of July 20, 1944.

My brother Justus was imprisoned by the Nazis but got out during the fall of Berlin. Then the Russians came and arrested him, and he died in a diphtheria epidemic in one of the camps. We didn't find out until two years later.

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

MD: 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. The last two or three years in high school I certainly proclaimed myself an astronomer. I had a two-inch telescope, and I read popular books on astronomy, and I had a little astronomy club with a pal who had similar interests. Also one of the

The Delbrück house in Berlin-Grunewald, built in 1906, destroyed by bombing in 1943.



Bonhoeffer sons, Karl Friedrich, knew much more about astronomy, being a real scientist. 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.

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

MD: 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 parents' bedroom, 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 parents' bedroom thinking they were asleep. I'm sure my mother was worrying herself stiff that I would freeze to death out there. She made me a special, very warm dressing gown.

CK: Was science taught in school as well as mathematics?

MD: 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 by the 1920s. Biochemistry didn't exist. Nor genetics. It was all very descriptive.

CK: And Max Planck lived nearby?

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

CK: The first university you went to — Tübingen in 1924 — did you go there with the intention of studying astronomy?

MD: That I did. Hans Rosenberg was there; he was sort of an astrophysicist, which at that time was a science just beginning. He had a little observatory, and I think we were a total of three students of astronomy. Of course I had just come from high school; I was 17½ 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. I learned more about science from older students, not from any of the professors.

I was at Tübingen for one semester; then I went to Berlin, then to Bonn, then back to Berlin, and then finally to Göttingen. In Berlin I could study free of tuition because my father was a professor there.

CK: Scientifically I would think Berlin would be a very exciting place with Einstein and Planck; or by then was Planck considered somewhat out of it?

MD: Planck was out of it. Einstein never had students, and Max von Laue had students but was not an exciting teacher. He was too uptight in his personality. He was a very fine person, but he was not easy.

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

MD: 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.

They had good experimental physicists in Berlin 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 40 minutes to get there and 40 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.

CK: What was the state of astronomy when you were a student?

MD: 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 in 1837 by the German astronomer Bessel, and that was a tremendous triumph. 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 and making catalogues of the stars. Fifty years of this had ruined German astronomy, because all the young people who trained there, all they did was sit

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every night for hours and hours in unheated observatories and measure transits of stars. 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 in Tübingen, Hopmann in Bonn, and Hans Kienle in Göttingen. Göttingen, of course, was a much more exciting place than the others 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.

CK: How was the intellectual atmosphere at Göttingen different from that at the other universities that you had attended?

MD: Well, of course, it was just after the breakthrough of quantum mechanics which had happened in 1925. In 1925 Werner 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. V. Condon — are just a few of the names that I remember. So you really had a feeling that you were close to where things are really happening, which is a feeling students do not usually have in most places.

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

MD: 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 observatory, 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 at Berlin in the winter of 1925-26. 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?" 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.

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 on novae failed. I was trying to understand the theories that were just being advanced, which was quite impossible for me, because the mathematics was beyond me and because they were in English and I didn't know any English at the time. It was far too ambitious a project and didn't lead anywhere.

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 palling 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 PhD 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. 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.

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.

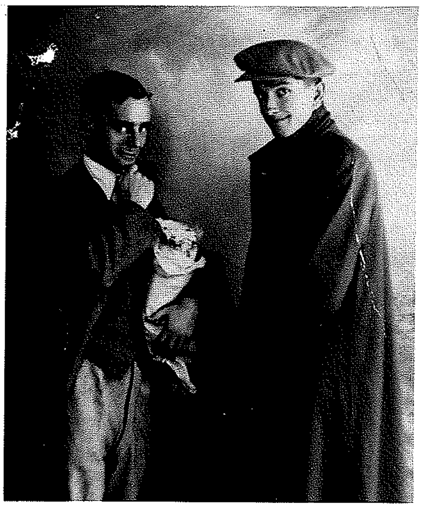
CK: When you finished your doctoral dissertation do you remember how you felt, whether you felt like this was really exciting science and you wanted to pursue it?

MD: 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.

Then I got a job at Bristol University in England. Max Born, my official professor, recommended me to teach some quantum mechanics to a professor of theoretical physics there — John E. Lennard-Jones. I must have gone to Bristol in about September of 1929 not knowing more than a dozen words of English. 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.

CK: Then you had a postdoctoral fellowship to study with Niels Bohr and Wolfgang Pauli?

MD: Yes. 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



Max Delbrück and roommate C. F. Powell (with groceries) in Bristol, 1932.

Colloquium in Copenhagen, 1936. In front row (from left), Niels Bohr, Paul Dirac, Werner Heisenberg, Paul Ehrenfest, Max Delbrück, and Lise Meitner.

been recommended by Max Born and by Karl Friedrich Bonhoeffer. So in the early spring of 1931 I arrived in Copenhagen and was immediately taken in hand by George Gamow. In fact I roomed with him for awhile. 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.

So I spent the summer there, and in the fall I moved on to Zurich and there I shared an office with Rudolf Peierls, Pauli's assistant. From Pauli I went back to Bristol for half a year.

CK: How did you come in contact with Bohr's ideas about complementarity?

MD: During the time I was in Copenhagen 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 Erwin 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 as 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 1927, 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 30 years later — *innumerable* lectures.

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

MD: Enormously. I was interested — well, anybody who was *at all* interested in quantum mechanics 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 in Copenhagen with Bohr and in Zurich with Pauli (and another half year in Bristol), and I had the choice of either going to Berlin to become an assistant of Lise Meitner at the Kaiser Wilhelm Institute for Chemistry, or to Zurich to be an assistant of Pauli, I chose to go to Berlin because of the vicinity of the K. W. Institutes for Biology.

I came to Berlin in the fall of 1932, but during that summer I went back for a short visit to Copenhagen, where I heard that Bohr was giving a big lecture, opening a world congress of light-therapy physicians in the Riksdag, the parliament building. So I went there, 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. He got up, promptly lost his way behind the rostrum, and finally found the lectern. 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 awhile, 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 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. 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. It was a very good thing that he did, because it certainly challenged me to take it seriously, and constituted my motivation to turn to biology. □

Research in Progress

An Eleven-Year Twitch

Sunspots were first observed by Galileo, and their 11-year cycle was noted in the mid-19th century, but the reason for this repeating period of solar activity has remained one of the sun's biggest mysteries. Recent evidence uncovered by Robert Howard and Barry J. LaBonte links this cyclic activity to solar-mass movements, offering a solution to the mystery. Howard is a staff member and LaBonte a research fellow of the Hale Observatories, which are operated jointly by Caltech and the Carnegie Institution of Washington.

What *is* known about sunspots is that they contain highly magnetized material and are associated with violent storms, the largest of which appear as solar flares. At the beginning of the cycle, sunspots appear at the intermediate latitudes (about 35°) in both hemispheres and increase in frequency and size as they drift toward the equator over an 11-year period. As this activity then vanishes at the equator, small new sunspots show up closer to the poles as the start of the next cycle. The polarity of the magnetic field of this new group of sunspots is opposite to that of the previous ones, creating a cycle of 22 years.

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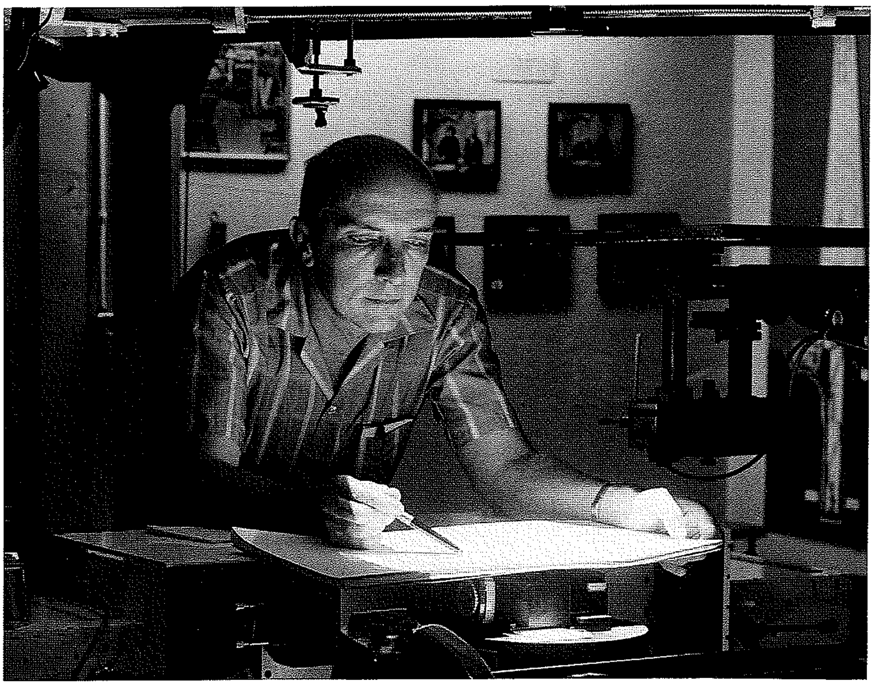

Robert Howard checks a solar image at Mount Wilson's 150-foot Tower Telescope.