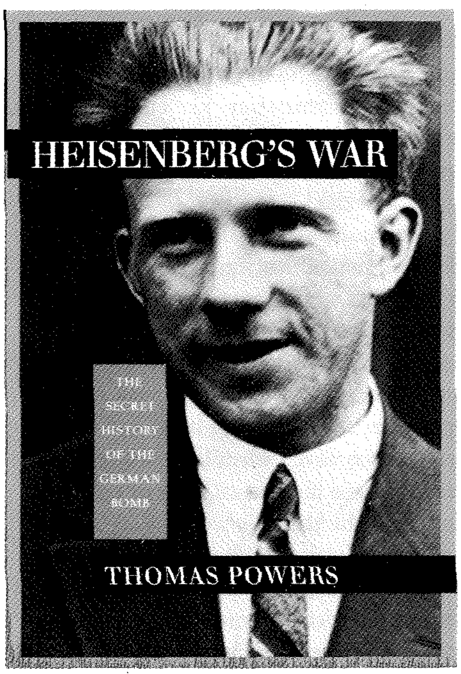


Alfred A. Knopf, 1993
$27.50
608 pages

Heisenberg's Uncertainties

by Daniel J. Kevles

During the Second World War, the nightmare that German physicists would deliver an atomic bomb into Hitler's hands haunted the inner circles of American science. Like most nightmares, this one melded foreboding with facts. Hitler's government controlled rich natural-uranium mines and the world's only plant for manufacturing heavy water, an essential ingredient in nuclear-reactor research. Germany had been a superpower in world physics, a Mecca for American students, its scientists mighty contributors to the recent revolution of quantum mechanics. Despite the loss of many world-class scientists as refugees from Nazism, Germany still appeared formidable. Otto Hahn, who, in 1938, had identified the phenomenon of nuclear fission using tabletop apparatus in his Berlin laboratory, remained in Germany; so did Werner Heisenberg, a theorist of towering talents, who had conceived the famed uncertainty principle and, in 1932, at the age of 31, had won a Nobel Prize for his co-invention of quantum mechanics.

In the mid-1930s, Heisenberg, unimpeachably German but no Nazi himself, had defended Einstein's physics—"Jewish physics," Hitler's minions called it—and had found himself labeled a "white Jew," his career and his life at risk. While he was on a visit to the United States in the summer of 1939, close scientific friends, like the physicist Samuel Goudsmit, had pleaded with him to emigrate, but he had returned home, insisting that he was a German patriot with a duty to help maintain havens of decency in his country and protect German physics for the future. Wartime intelligence reports revealed that his patriotism had deepened to include the hope of a German victory, because it would counter the inroads of Soviets and Slavs threatening from the east. The reports also indicated that the Germans had initiated an atomic project, and that Heisenberg—"the most dangerous possible German in the field because of his brain power," as a distinguished British physicist told American physicists—was involved in it. However, in December of 1944 an American scientific team, sent to Europe to ascertain the state of German nuclear affairs as part of a United States Army intelligence mission code-named Alsos, tentatively concluded that the German atomic-bomb project was paltry and was several years behind the Manhattan Project.

In 1947, Heisenberg explained in the British scientific journal *Nature* that he and his colleagues had known how to make a bomb but had been reluctant to build one for Hitler, and he added that in any case they had not had to face up to the moral decision of whether to

The question of why German physicists did not produce an atomic bomb remains highly charged. It bears upon how we judge any scientist who participated in the Nazi machine or, for that matter, scientists anywhere who for the sake of science or ideology enter into a potentially Faustian bargain with the state.

proceed toward an atomic weapon, because even the military agreed that the task was too large for wartime Germany. They had therefore directed their energies toward making a reactor—which they called an "engine"—to exploit nuclear energy as a source of power to drive ships and electrical generators. Samuel Goudsmit, who headed the Alsos scientific team, promptly responded, in *Life* (and elsewhere), declaring that German physicists *had* tried to build a bomb, that they had failed because of the meddling of Nazi administrators and their own commission of serious technical errors, and that Heisenberg's account disingenuously covered their blunders with a newly minted morality. To Goudsmit, who was Dutch by birth, and whose parents had died in the Holocaust—and to many other Allied physicists—the aim of trying to protect physics did not justify collaborating with the architects of Auschwitz.

The question of why German physicists did not produce an atomic bomb remains highly charged. It bears upon how we judge any scientist who participated in the Nazi machine or, for that matter, scientists anywhere who for the sake of science or ideology enter into a potentially Faustian bargain with the state. Scrutiny of the official German records by several historians has revealed that Heisenberg and his colleagues did in fact understand a good deal about the fundamentals of bomb physics, and that early in the war they raised the prospect not only of a power source but also of "an explosive of unimaginable consequence," as Heisenberg put it in a lecture to a gathering of high German officials in February of 1942.

In *Heisenberg's War: The Secret History of the German Bomb* (Knopf, $27.50), Thomas Powers argues that we can judge Heisenberg only if we can know his intentions, and those the official historical record does not reveal. To ferret them out, Powers has meticulously searched through what he calls "the shadow history of the war," seeking in letters, diaries, recollections, and intelligence files what Heisenberg and his friends "said to each other in the small hours of the night." An accomplished investigative journalist and historian of national security, Powers has exhumed a trove of material and deployed it brilliantly, though somewhat repetitiously, to illuminate the hidden history. His book is provocative and often gripping, and it inventively compels a reconsideration not only of Heisenberg's war but of the relationship to it of several key Manhattan Project scientists, Goudsmit among them.

Powers notes that the German nuclear effort, called the Uranium Club at the time, comprised "an unruly mailing list of competing scientists whose only shared hope was to survive the war." Some in Heisenberg's branch of the club, including Heisenberg himself and his close younger friend Carl Friedrich von Weiszäcker, who was the son of the second-highest official in Hitler's Foreign Office, also wanted to exploit the government's interest in nuclear matters to rescue German physics from Nazi know-nothings. This was a hazardous game, as Heisenberg knew.

Some of Heisenberg's intimates revealed in a trail of leaks intended for the Allies how he was playing the game. Consistent in content, the leaks were exemplifed in a remarkable, unequivocal message that one of Heisenberg's confidants—the theorist Fritz Houtermans, a socialist who had spent time in both Nazi and Soviet prisons and was under suspicion by the Gestapo—had asked a Jewish-refugee physicist named Fritz Reiche to carry by memory to the United States. A contemporary handwritten summary of Reiche's report, which he delivered to a group of physicists in Princeton in March of 1941, reads:

> Reliable colleague who is working at a technical research laboratory asked him to let us know that a large number of German physicists are working intensively on the problem of the uranium bomb under the direction of Heisenberg, that Heisenberg himself tries to delay the work as much as possible, fearing the catastrophic results of a success. But he cannot help fulfilling the orders given to him.

In September of 1941, Heisenberg paid a visit to his mentor and conscience, Niels Bohr, in occupied Denmark, attempting, it seems, to convey that he was at work on a nuclear-reactor project

Theodore von Kármán (soon to arrive at Caltech) took this snapshot of Heisenberg (light suit) in 1927 at the International Physics Conference at Lake Como, Italy. Wolfgang Pauli is the other hatless gentleman at right.

and wanted to keep his research confined to that, but he so fumbled the try that he left Bohr furious, and convinced that he was designing a bomb.

The options for delay were inherent in the details of bomb physics. By late 1941, the Germans, like the Allies, recognized that two types of atomic weapons could be fashioned —one of pure uranium 235 (U-235), the readily fissionable isotope of the element, and the other of plutonium, a newly discovered element that would be produced in the controlled chain reaction of a nuclear reactor from a sister isotope, uranium 238 (U-238). They also knew that U-235 represents less than one percent of natural uranium and cannot be chemically separated from its far more abundant sister. Separation by nonchemical means would be extremely difficult, so obtaining enough pure U-235 to make an explosive—what physicists call a "critical mass"—would require at least several years and untold millions of marks. Powers points out that whenever Heisenberg touted the destructive power of a uranium bomb he also stressed the difficulties, thus discouraging pursuit of a U-235 weapon and encouraging the investment of resources primarily in the creation of a reactor that would produce power and—ultimately, perhaps—element 94, as the Germans called plutonium.

But did he, as his disbelievers insist, emphasize the difficulties because he had committed the key technical blunder of overestimating them? The issue turns on how large the critical mass of pure U-235 would have to be. In several wartime comments, Heisenberg implied that as much as a thousand kilograms would be required—a quantity that would indeed have been impossible to obtain soon enough to affect the outcome of the war. He was heard to make a comparable estimate on August 6, 1945, the day the uranium bomb was dropped on Hiroshima. At the time, Heisenberg and nine other German nuclear physicists, including Hahn and Weiszäcker, were interned at Farm Hall, an estate outside Cambridge, England, where the bedrooms and common rooms were electronically bugged and the conversations routinely recorded and transcribed. The night of August 6, the microphones picked up an unguarded, emotional discussion that started with skepticism that the Americans had succeeded in producing a nuclear bomb, because the Germans did not think that anyone could possibly have obtained enough pure U-235—perhaps two tons, Hahn remarked that Heisenberg had said at one point—to form a critical mass.

Several times during the war, however, Heisenberg had indicated that the critical mass would be only a few tens of kilograms, which was in the right ballpark and was small enough to be considered obtainable; indeed, at a meeting in Berlin in 1942, responding to a question from Field Marshal Erhard Milch, of the Air Force Ministry, Heisenberg had said that London could be leveled with a bomb about as large as a pineapple. The Farm Hall transcripts, which the British kept secret until February of 1992, and which Powers has examined, reveal that on August 6 Hahn recalled Heisenberg's telling him more than once during the war that a uranium bomb could be made with only 50 kilograms of the pure metal. That same night, Heisenberg admitted to Hahn that he had never actually calculated the necessary mass. Eight days later, in a lecture to his colleagues on bomb physics, he led them through the exercise of designing a weapon, showing that it could be done with 16 kilograms of U-235, which was very close to the actual critical mass of the metal. The lecture was stunning in its technical mastery, but also impressive was the fact that Heisenberg had adumbrated part of his analysis in calculations that he had made just two days after Hiroshima. Powers contends that Heisenberg's resolution of the critical-mass problem was so quick that he must have worked out the intricacies of a uranium bomb much earlier, and the Farm Hall

Calculation of the critical mass requires certain essential numbers that characterize the fissioning behavior of U-235. . . . It is clear from the Farm Hall transcripts that Heisenberg had not acquired these numbers experimentally in the course of his wartime research and that no one else had, either.

transcripts, he says, offer strong evidence that Heisenberg "cooked up a plausible method of estimating critical mass which gave an answer in tons, and that he well knew how to make a bomb with far less, but kept the knowledge to himself."

Powers gives too much credit to Heisenberg. Calculation of the critical mass requires certain essential numbers that characterize the fissioning behavior of U-235. These numbers can be determined reliably only by actual measurement. It is clear from the Farm Hall transcripts that Heisenberg had not acquired these numbers experimentally in the course of his wartime research, and that no one else had, either. The news of Hiroshima—that it had been bombed with a uranium device enormous in explosive power yet compact enough to be carried in an airplane—had provided him with a giant hint toward determining the numbers: they had to conform to the reality of the working weapon, and that constraint enabled him to figure out the critical mass in a tour de force of rapid, but advantaged, estimate and deduction. At Farm Hall, Heisenberg explained to Hahn that the reason he had not calculated the critical mass of the isotope precisely was that he had believed that U-235 could not be separated out—which is to say that Heisenberg must have judged obtaining even tens of kilograms of pure U-235 a virtually impossible task. He was right that separation would be costly; the principal American installation for the purpose, at Oak Ridge, Tennessee, was huge. Yet Manhattan Project scientists, a number of whom had in the 1930s built sizable cyclotrons and big laboratories to go with them, had obviously been undaunted by the obstacle, and wartime Germany had been able to provide the immense resources that Wernher von Braun required for his Peenemünde rocket projects. Heisenberg had niether the Big Science temperament nor the experience to envision an industrial-scale separation effort. Physicists in other branches of the Uranium Club did, but he did not throw his prestige behind their ambitions.

Not that he lacked opportunity: he was party to several crucial meetings that high Nazi officials held during the six months starting in December 1941 to evaluate military-research programs for their pertinence to the war effort. Powers, taking an original tack, probes Heisenberg's silences in these colloquies—what he did not say or did not do to advance a bomb project. In all the meetings, Heisenberg accorded no more than brief and casual mention to the alternative route to a bomb—reactor-produced element 94, which did not pose a severe separation problem—nor did he call for a crash program to pursue it. He apparently did not even mention element 94 at a meeting in Berlin on June 4, 1942, where he had the attention of Albert Speer, the boss of the German economy and an enthusiast of big-payoff projects (like von Braun's, for example, for which he would ultimately provide tens of thousands of slave laborers). When Speer asked how much money was needed to press ahead with the nuclear effort, Heisenberg mentioned a figure so ridiculously low that Speer decided—and so informed Hitler—to relegate the project to a low priority. In Powers' view, Heisenberg

In the second row, Moe Berg, the celebrated and cerebral major-league catcher . . . sat with a .32-caliber pistol in his pocket, listening to Heisenberg talk about physics, and resolved to kill him if his remarks indicated that he was seriously at work on an atomic bomb.

managed, without conspiring with friends like Weiszäcker or revealing enough to raise the suspicions of the Gestapo, "to guide the German atomic research effort into a broom closet, where scientists tinkered until the war ended."

Despite the downgrading and the wartime reports of Heisenberg's foot-dragging intentions, the fear that Heisenberg was devoting his mighty brain to the cause of achieving a German nuclear weapon remained undiminished among Manhattan Project personnel, including many of its key scientists and its director, General Leslie R. Groves. The suspicion led the physicists Hans Bethe and Victor Weisskopf, both normally levelheaded, to propose formally, in October of 1942, that Heisenberg be kidnapped in Switzerland, where, as they had learned, he was to lecture later that year. Although the Bethe-Weisskopf initiative was soon rejected—any such move would surely have alienated the neutral Swiss—it eventually helped inspire several operations to deny the German nuclear effort its scientists, and particularly Heisenberg.

Powers devotes substantial space to this campaign against Heisenberg, and his account is chilling. One strategy was to bomb the research institutes that Heisenberg and Hahn directed in Berlin, the objectives to include, as General George C. Marshall learned in an explanatory memorandum from an Army Assistant Chief of Staff for Intelligence, "the killing of scientific personnel employed therein." Another strategy was a revival of the kidnapping idea and then its transmutation into an operation that Bethe and Weisskopf knew nothing about—the assassination of Heisenberg.

Powers tells the story mesmerizingly, having compiled evidence that the scheme was real, that it was fostered by General Groves and the members of the intelligence operations that he established for the Manhattan Project, and that it reached its climax on December 18, 1944, in a lecture hall in Zurich. In the second row, Moe Berg, the celebrated and cerebral major-league catcher, who had finished his career in 1939 with the Boston Red Sox and was now an intelligence operative, sat with a .32-caliber pistol in his pocket, listening to Heisenberg talk about physics, and resolved to kill him if his remarks indicated that he was seriously at work on an atomic bomb. Berg scribbled a note: "As I listen, I am uncertain—see: Heisenberg's uncertainty principle—what to do to H . . . discussing math while Rome burns—if they knew what I'm thinking."

Although Berg obviously did not pull the trigger, Powers holds that the operation that began with Bethe and Weisskopf and eventually put Berg in the Zurich lecture hall contributed to the clouding of Heisenberg's reputation after the war. Goudsmit was complicit in the early kidnapping proposals and, according to Powers, in the assassination scheme itself, having been one of the last of Groves' men to brief Berg, in Paris, before he left for Switzerland. Berg later wrote, in notes about the Paris talks, "Nothing spelled out, but Heisenberg must be rendered *hors de combat.*" Powers implies that not only Goudsmit, but Bethe, Weisskopf, and others were psychologically disposed to reject Heisenberg's account of his passive moral resistance to a German bomb because to

Caltech's Robert A. Millikan (left) and Marie Curie engage in thoughtful conversation, while the dashing Heisenberg (behind them, right) seems to have found something more amusing to discuss. But in Rome in 1931 no one was yet talking about a fission bomb.

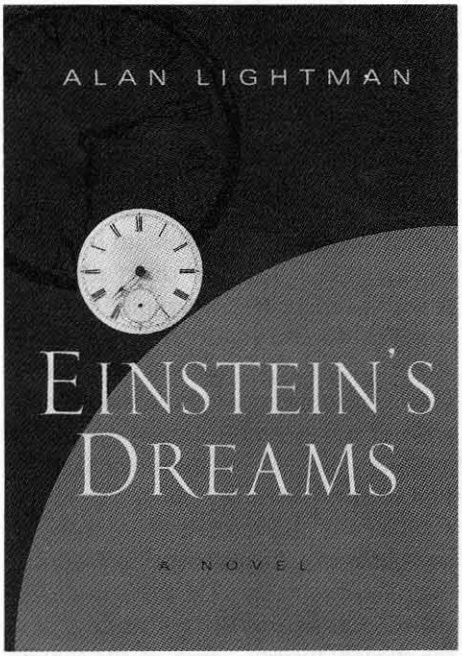
Heisenberg was an unusual man in unusual circumstances, forced to make difficult choices concerning himself, his physics, and his country in a viciously dangerous environment. Even so, it is difficult to accept him as a paragon of moral purpose.

accept it was to indict themselves for their involvements in one or another of the get-Heisenberg projects—and, beyond that, for their own determination to build the world's first atomic bomb.

Here is Powers' evident larger aim: to spotlight Heisenberg as a moral witness against scientists who would forge weapons of mass destruction for their governments. In Powers' judgment, Heisenberg visited Bohr in 1941 primarily with the hope that if he revealed that German scientists were not building an atomic bomb Allied scientists might be persuaded to forgo the construction of one, too. Powers proposes that Heisenberg's postwar claim that moral scruple had figured in his thinking about the bomb could be taken as a rebuke to Manhattan Project scientists for what they had done during the war, noting that if some of them were outraged by the assertion, it was because they were "extremely sensitive to any suggestion that they had done something wrong in building the atomic bomb—especially any suggestion which came from Germans." At the time of Heisenberg's visit, however, Bohr, probably rightly, interpreted his purpose as self-serving patriotism—to stop the Allies from building atomic bombs and dropping them on Germany, a possibility that, in the recollection of Heisenberg's wife, terrified him throughout the war. More important, Heisenberg himself understood that resistance to building bombs for Hitler's totalitarian state could hardly be taken as establishing a moral standard applicable to scientists who devised them for the democratic governments that were Hitler's enemies.

Heisenberg was an unusual man in unusual circumstances, forced to make difficult choices concerning himself, his physics, and his country in a viciously dangerous environment. Even so, it is difficult to accept him as a paragon of moral purpose. Unashamedly eager to use the war to serve German physics, he ingratiated himself with Hitler's henchmen by laying out the requirements for a bomb, thereby obtaining support for nuclear research, his own appointment as director of the Kaiser Wilhelm Institute of Physics in Berlin, and the symbolic reestablishment of modern physics (his as well as Einstein's) in the German scientific hierarchy. He was seemingly tone-deaf to the moral dimensions of politics, uncomprehending of the revulsion that Hitler's domination of Europe stimulated in Bohr and in so many others. Still, while Heisenberg was not a saint, neither was he the devil that Goudsmit saw. The Farm Hall transcripts confirm Powers' reading of the shadow history—that, in the context of Hitler's Germany, Heisenberg and his circle were deeply ambivalent about their nuclear project, that a moral reluctance to see it succeed contributed to its failure, and that Heisenberg himself, as he confessed to his friends on August 6, 1945, was at "the bottom of my heart really glad that it was to be an engine and not a bomb." □

Daniel Kevles, a historian of science and a member of the Caltech faculty since 1964, is the J. O. and Juliette Koepfli Professor of the Humanities. His books include The Physicists: The History of a Scientific Community in Modern America *and* In the Name of Eugenics: Genetics and the Uses of Human Heredity.



Pantheon Books, 1993
$17.00
179 pages

Einstein's Times

by Jay A. Labinger

First, a little truth-in-advertising. The book jacket calls *Einstein's Dreams* a novel, and notes that author Alan Lightman is a physicist (Caltech MS '73, PhD '74) who teaches physics and writing at MIT. Shouldn't we expect, then, that the book will teach us something about Einstein's contributions to physics? It doesn't. Maybe it's more about Einstein the person? No, not that either. Nor is the book really a novel, in any traditional sense. What is it, then?

The book's basic premise is straightforward: While working on the Special Theory of Relativity, published in 1905, Einstein experiences a series of dreams about time. Each dream portrays an alternate world for which the nature of time is different. The dreams are framed by a prologue and epilogue—in which we see Einstein, early one morning, waiting for the typist to come in and do his completed paper—and are punctuated by several interludes, describing meetings between Einstein and his friend and colleague Besso. According to the prologue, one of the dreams provides the key inspiration: "Out of many possible natures of time, imagined in as many nights, one seems compelling. Not that the others are impossible. The others might exist in other worlds."

Only in some of the 30 worlds does time appear to be *physically* different, and most of those concepts are not unfamiliar—the world where time runs backwards; the world that comes to an end; the world where everything happens over and over again. In other worlds *people* are different—they live forever; they have no memory of the past; they cannot imagine the future. In still others neither the physical world nor its inhabitants seem very different from ours, but people perceive and react to time differently. One world is virtually indistinguishable from the "real" world —time passes more slowly at higher altitudes, but the effect is so tiny that it can only be measured with the most sensitive instruments—nonetheless everyone insists on living in the mountains.

Which is the "compelling" vision that inspires Einstein's theory? None of them—or perhaps all of them. To be sure, some of the dreams tease us with relativistic-sounding concepts. In one, everyone is always moving at high speed, since time thereby passes more slowly (just like our world, with the speed of light reset to somewhere around 55 mph). In another, time depends on relative location, rather than on relative velocity. Gradually, however, as we move from one world to the next, distinctions between the physical, human and perceptual natures of time become less and less important, as do the differences between these alternative worlds and the one we are used to. In the world in which people live for only one day, "either the rate of heartbeats and breathing is speeded up . . . or the rotation of the earth is slowed. . . . Either interpretation is valid." The world of immortals is split into the Laters, who feel no pressure to do anything, since they have infinite time; and the Nows,

who are always busy, since they want to be able to do everything that an infinite life allows. (Does this sound at all like anyone you know?) An understanding of relativity arises not out of any single dream, but from the *global* vision of how time is constituted by interactions between the physical world, its people, and their conception of time.

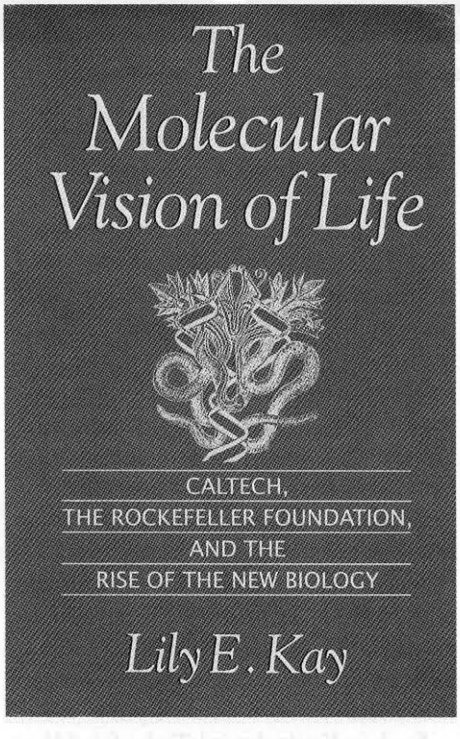
Even though there may be no overt scientific lesson here, Lightman still provides us much to think about. What is the role of metaphor in scientific discovery? What does the conception of time mean for the novelist? To write a novel, after all, is to construct a world; and consciously or otherwise, the novelist must define the nature of time for that world: Does it proceed linearly or cycle back? Move rapidly or slowly? Smoothly or unevenly? While such issues are not raised explicitly, it is hard to imagine that they did not influence the writing of this book.

One of the dreams can perhaps stand for the entire book: "a world in which there is no time. Only images." Such a world is no world at all—but Lightman makes it a beautiful thing to look at. In like fashion, a book like this can be no novel at all—so don't read it as a novel. Read it as poetry— even though it is not written in any form of verse—for the beautiful writing, the thought-provoking ideas, and above all for the lovely images that arise from the making of the worlds, individually and collectively. □

Jay Labinger is the administrator of Beckman Institute. Also a lecturer in chemistry, he's been a member of the professional staff since 1986 and has written several book reviews for E&S.

Caltech's Visions?

by Robert L. Sinsheimer



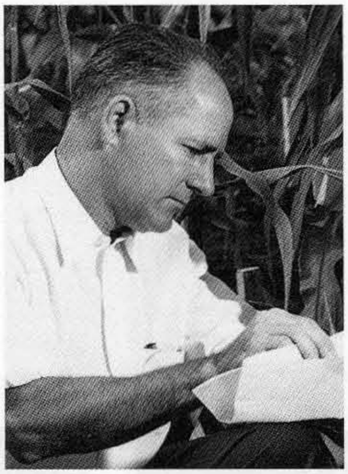
Oxford University Press, 1993
$49.95
304 pages

This is a curious, disturbing, and ultimately scandalous book—and Caltech and those of us whose research is identified with the Institute are the scandalized. It is startling to be told that, throughout one's entire scientific and academic career, one has been a pawn—worse, an unwitting pawn; and, worse yet, an intellectual progenitor of still more unwitting pawns. But, if Lily Kay is to be believed, that was my life.

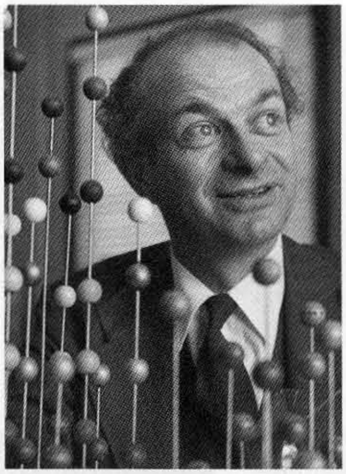
On one level this book presents the history of Caltech's Division of Biology (and therewith the "molecular vision of life") from the mid-1920s to the late 1950s when the division had the generous and sustained support of the Rockefeller Foundation. Arthur Amos Noyes is portrayed as the intellectual father of that vision at Caltech, and it was subsequently implemented by Thomas Hunt Morgan, George Beadle, Linus Pauling (in the Division of Chemistry and Chemical Engineering), and Max Delbrück, to mention only the most famous (all won Nobel Prizes). This history is a work of considerable scholarship and interest. It is densely documented from many sources, including the archives of Caltech and the Philosophical Society, and especially those of the Rockefeller Foundation. Almost every chapter has at least 50 footnotes.

The 1930s through the 1950s (and

Thomas Hunt Morgan



George Beadle



Linus Pauling

Max Delbrück

'60s) were a period of revelation in biology, when the bases for the functional and genetic characteristics of living cells were found to lie in macromolecular structures—in the specific architecture of definable molecules. Many of these advances were made at Caltech. The genetic analyses of Morgan paved the way for Beadle's insightful research, which first linked genes to the performance of specific enzymatic reactions. Pauling's imaginative and painstaking structural studies led finally to the first correct molecular models for proteins and also to the first description of the molecular basis of a genetic disease. And Delbrück's introduction of bacteriophage as a tool for molecular biology research led to a detailed understanding of the genetic role of DNA in replication, mutation, and recombination and in transcription.

Much of this research was indeed made possible by generous support from the Rockefeller Foundation. Kay recounts these advances knowledgeably. Invariably, however, her account of this development of molecular biology is ideologically slanted and hostile, all of it being embedded in, and interwoven with, a subtext—a subtext that purports to reveal a hidden design behind the philanthropy of the Rockefeller Foundation. According to the author, this design was implemented through the skillful guiding hands of its officers Max Mason and Warren Weaver, as they controlled the flow of funds and thereby delicately selected the directions of research. This hidden design was nothing less than the "social control of human behavior," to be achieved through a knowledge of its basic biological origins; the "betterment of man" toward an ideal conceived as the responsible, Protestant-ethic-bound, "Nordic" (Northern European) variety of *homo sapiens*—an ideal reflective of the trustees of the Foundation itself.

Such is Kay's thesis. To be fair, she does not accuse Morgan and the others of being knowing accomplices to the execution of this design. But she does consistently focus her selective vision upon those aspects of their personalities that appear congruent with such a plot—on Morgan's instances of anti-Semitism, on Pauling's small-town-preacher background and his scientific arrogance, on Delbrück's lineage to the German elite and his fostering of a "personality cult," on Beadle's interest in the industrial application of his research. I did not know Morgan, but I knew Beadle, Pauling, and Delbrück well. These men were true scientists, independent thinkers deeply dedicated to the pursuit of knowledge. Each had his personal idiosyncracies, but to suggest that they were somehow manipulated or suborned, their research guilefully co-opted to the hidden designs of the Rockefeller Foundation, I find close to ludicrous.

The author's bias is consistently evident in her choice of language and her persistent (and gratuitous) attribution of motive. For example:

> Graciousness notwithstanding, by retrieving Garrod's "forgotten" work Beadle, of course, was engaging in legitimating his own findings; by setting the record straight he also carved a historical space for his own contributions to biochemical genetics.

> With Pauling's own enthusiatic promotion, in both scientific circles and the popular media, the work [on sickle cell hemoglobin] was regarded as a spectacular achievement . . .

> Robert Sinsheimer at Caltech *rejoiced* in the new powerful technologies . . . (italics mine).

Throughout the book, scientific progress is invariably coupled with a goal of social control. To quote a few examples:

> The program expressed the perception that mechanisms of upward causation were necessary and sufficient explanations of life and the most productive path to biological and social control.

> Equally significant, when the precise

Each had his personal idiosyncrasies, but to suggest that they were somehow manipulated or suborned, their research guilefully co-opted to the hidden designs of the Rockefeller Foundation, I find close to ludicrous.

mechanisms by which nucleic acids exerted their putative power as the chemical blueprints of life were elucidated, molecular biology would claim greater cognitive authority and technological potential when addressing the unresolved problems of biological deterioration and rational social planning.

> Something more profound was at work: a cognitive and social resonance. The Foundation's technocratic vision of social engineering and its representational strategies were articulated on the discursive level of program and policies; the scientist's technocratic vision of life was represented at the bench. The primacy of Caltech on the Rockefeller Foundation's roster reflected these deeply shared interests and convergent social and scientific ideologies.

Kay's implication is clear: Caltech and these particular scientists received the support of the Rockefeller Foundation because its astute officers perceived an underlying "resonance," a shared vision of science and society, that blended the long-range goals of the Foundation, the ethos of the Institute, and the personalities of these faculty. Their science was important but their social perspective was decisive in the Foundation's choices.

That there was a shared vision of science seems likely. That there was a shared vision of social goals is uncertain; if so, knowing these scientists, I cannot believe that it was the program of "social control" or "human betterment" postulated by Kay, although the phrase did apparently find its way into Robert A. Millikan's mouth. But even with the absence of any "written record" linking Pauling to this idea, the author manages to implicate him anyway:

> The synergy between intellectual capital and economic resources buttressed the technocratic vision of progress. With the Foundation's support and the generous help of prominent Pasadena families, Millikan predicted that the Institute could "scarcely fail to win the race for human betterment" through chemical and biochemical advances.
>
> The term "human betterment" must be viewed within a politics of meaning with its own historicity. "The race for human betterment" had a specific linguistic meaning during the 1930s, grounded in eugenic discourse. As the *New York Times* announced, the Rockefeller gift to Caltech was aimed at "the biological improvement of the race." . . . Although there is no written record that during the 1930s Pauling was directly motivated by the social goals of the Rockefeller Foundation's agenda "Science of Man" or by the eugenic campaign of the Human Betterment Foundation, his interests in human applications of biochemical research are documented.

It is not unreasonable for Kay to presume that when its trustees committed the Rockefeller Foundation to "human betterment," they had in mind a world governed by the principles that had led to their personal success—principles of personal responsibility, the work ethic, rationality. And given the evidence that much of human behavior in the world is irrational, it was not without sense at the time to seek biological bases that might explain differences in behavior. To leap from such a relatively benign concept, however, to a

Machiavellian plot, incorporating Caltech and some of the most distinguished scientists of their day and intended to control "human behavior on a global scale," is the stuff of conspiracy buffs.

Accordingly, Kay rejects the thesis that molecular biology was simply the logical outcome of developments in biochemistry, biophysics, and genetics. She writes:

> Current discourse on genetic engineering technologies often characterizes these developments as a natural consequence of the theoretical research that took place during the 1950s, 1960s, and 1970s, a logical evolution from the pure to the applied. The lessons from this book imply the reverse: that from its inception around 1930, the molecular biology program was defined and conceptualized in terms of technological capabilities and social possibilities. Representations of life within the new biology were a priori predicated on interventions that, in turn, aimed from the start at reshaping vital phenomena and social processes.

In one sense, were it not so snide, this view (and indeed the whole book) could be viewed as highly flattering. The very notion that these Caltech scientists could have produced to order such a major scientific breakthrough as molecular biology merely in order to implement the (postulated) social objectives of the Rockefeller Foundation is implicitly a remarkable tribute—although far beyond the possible.

Surprisingly, Kay completely overlooks the historical connection between the conquest of infectious disease by the introduction of antibiotics and vaccines and the increased concern with the residual panoply of genetic diseases. This concern led naturally to a much broader interest in genetics. Instead, she sees only one straight trajectory:

> Molecular biology was mission-oriented basic research. The ends and means of biological engineering were inscribed into the Rockefeller Foundation's molecular biology program, and eugenic goals played a significant role in its design. The program, in turn, formed a key element in the Foundation's new agenda, "Science of Man," a cooperative venture between the natural, medical, and social sciences. This agenda sought to develop a comprehensive science of social control and a rational basis for human engineering.

Thus, she distorts the meaning of statements such as Pauling's in a 1958 broadcast on "The Next Hundred Years":

> Like some of his peers, Pauling saw the deterioration of the human race as the most compelling challenge for the new biology. "It will not be enough just to develop ways of treating the hereditary defects," he said. "We shall have to find some way to purify the pool of human germ plasm so that there will not be so many seriously defective children born. . . .We are going to have to institute birth control, population control."

That "seriously defective" children are born is a human tragedy, and the author's tendency to regard proposals to reduce such tragedy merely as "interventionist concepts of social control," as she does in the next sentence, is simply wrong-headed.

Likewise Kay's perception that emphasis upon the "molecular vision of life" resulted in a diversion of support and interest so that: "important biological problems, such as differentiation, growth, the organization of cells into organs, selection, adaptation, and speciation have remained unsolved for decades." This is also off the mark. On the contrary, these fields are now undergoing dynamic advances thanks specifically to the introduction of the maturing concepts and methods of molecular biology.

It is distressing that such detailed scholarship should have been placed in the service of a distorting, revisionist ideology. Kay clearly belongs to the school of historical determinism that maintains the view that the course of scientific progress cannot be autonomous, but is always a response to cultural, usually political and economic, forces. While this ideology likely has instances of some validity—more so as applied to technology than to science—her attempt to force the development of molecular biology into this mold is misconceived and has led her to an invidious caricature of a great institution and several great scientists. □

Robert Sinsheimer is currently professor emeritus in the Department of Biological Sciences at UC Santa Barbara. He was professor of biophysics at Caltech from 1957 and chairman of the Division of Biology from 1968 until leaving to become chancellor of UC Santa Cruz in 1977. During his 20-year career at Caltech he worked with bacteriophage and was a frequent contributor to E&S *on the ethics of genetic research.*