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Cathryn Carson

*Building Physics After World War II:
Lawrence and Heisenberg*



University of California, Berkeley
1997

Morrison Library Inaugural Address Series

No. 6

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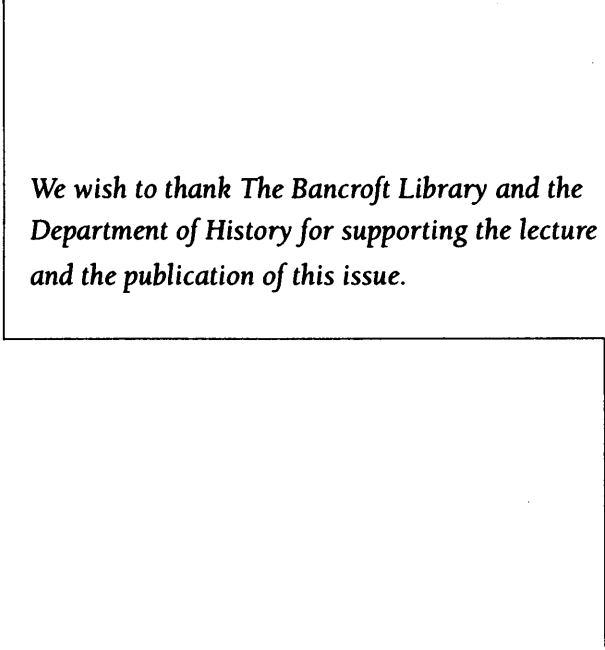
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PREFACE

The goal of this series is to foster scholarship on campus by providing new faculty members with the opportunity to share their research interest with their colleagues and students. We see the role of an academic library not only as a place where bibliographic materials are acquired, stored, and made accessible to the intellectual community, but also as an institution that is an active participant in the generation of knowledge.


New faculty members represent areas of scholarship the University wishes to develop or further strengthen. They are also among the best minds in their respective fields of specialization. The Morrison Library will provide an environment where the latest research trends and research questions in these areas can be presented and discussed.

Editorial Board




BUILDING PHYSICS AFTER WORLD WAR II:
LAWRENCE AND HEISENBERG





I want first to express my thanks to Charles Faulhaber and to the Bancroft Library for organizing this program of inaugural lectures, and for offering me this opportunity to speak about my work before this audience. This opportunity, I should say by way of introduction, brings with it a certain dilemma. Historians of science, and particularly historians of modern physics, often find themselves trying to address at least four distinct publics. The first of these is the science studies community, historians and philosophers and sociologists of science, who have all made a commitment, for whatever idiosyncratic reason, to understanding the development of the institution we call science. The second audience is the broader society of historians—“regular historians,” as historians of science sometimes call them—who share their general concerns and approaches to the past, but escape the particular compulsion to apply them to science. A historian of physics quite frequently also ends up addressing physicists, whose expectations of her field are often different from her own.¹ And finally, there is the non-historian, non-physicist public at large, academic and nonacademic, that finds its draw in the enormous significance of modern physics—intellectual, social, economic, military—for the world we live in. So a historian of physics faces quite a number of different audiences. The catch is, however, that she rarely ends up facing all of them at the same time; and that is the dilemma, and the promise, of this occasion. What I will be trying to do today is to describe something of my work on the history of modern physics, in a way that will be meaningful to as many as possible of my different publics. I wonder, in the end, if any one of them will be fully satisfied, but that is one of the risks of the game.

Before I launch into my topic for today—Professor Faulhaber mentioned my title, “Building Physics after World War II: Lawrence and Heisenberg”—I suspect I should say a few words about what I and historians of science more generally are trying to do in our work. As I see it, the history of science has three distinct tasks, none of which it can really afford to neglect. First, it aims to explain the development of scientific concepts; second, it tries to understand science as it fits into “regular history”; and third, it seeks to make sense of how scientific ideas emerge from the broader context—the “regular history” context—that frames them. Now, no one of these is completely uncontroversial. To start with the first, it seems that the conceptual development of scientific ideas is often viewed in science studies as somehow out of date, or at least not the part of the field where the real action is. With so much attention being devoted to other kinds of inquiries—and with so much truly excellent scholarship coming out on those topics—interest in the actual development of ideas has often come to seem somewhat, well, behind the times. I think this is rather unfortunate, although given all the boring chronicles of scientific ideas that have been produced over the years, I can understand how the appeal may have faded. But this kind of work does not have to be the simple registering of developments that has filled so many books. And without wanting to reverse the developments in historiography that have brought us to this point, I do think that a history of science that loses too much of its interest in scientific ideas also loses one of its richest traditions. That is the effort to explicate the evolution of those ideas—often peculiar and counterintuitive—as they unfolded in their time (and not, I would add, as preserved in the scientists’ origin stories).




A historian of science's second task, I think, is to work towards understanding science as it fits into "regular history." Compared to the first task, this second has emerged more recently. I remember being told in graduate school that the work that I was doing on the role of the scientist in West Germany, however fine it might be, just was not "history of science." For all my respect I do have for this senior scholar's views, I think they are mistaken. History of science *has* to address the integration of its subject into the more general framework of history: partly for its own sake, as it seeks to understand the forces shaping the scientific enterprise, and partly, I say with some regret, for a lack of other people to do the job. It still surprises me that more "regular historians" do not take up the interaction of that power, science, with culture, politics, society. But until they do so, *and* also beyond, the historian of science will have to deal with these issues.

Finally, in something of a synthesis, history of science has to try to make sense of how scientific ideas emerge from the context that surrounds them: that is, to see if the general framework of history—cultural, political, social—can help us understand the development of scientific concepts. This intellectual program has become controversial of late, with complaints that it constitutes a "postmodern" historiography, whose ideological aim is to tear down science by showing it to be merely "constructed" (note the "merely"). But the label of "postmodern" and the attribution of destructive intent are for the most part out-of-place. The real issues behind the program—How much leeway is there for human choice in the shaping of scientific concepts? And how, in concrete cases, is such choice actually get enacted?—are questions that exercised philosophers of science (and some scientists) for decades before postmodernism. And the charge of wanting to tear down

science seems quite odd to many historians, who are often trying above all to understand how the thing works. As I look over my field's development, I think the reason this approach has flowered so dramatically has more to do with its explanatory successes than with some ideology supposed to be shared among its practitioners. And without being able to think of myself as any sort of science-basher, I cannot imagine trying to do history of physics without it: I cannot imagine, for instance, how we can understand the settling-in of the Copenhagen Interpretation of quantum mechanics without dealing with the cultural situation in which it emerged. The program is an essential part of the historiography of science, and if we are still working on implementing it with nuance and sophistication, finding where it helps and where it does not, what that indicates is a direction for our future efforts.

So the history of science and the history of physics are occupied with these three tasks; and some of the most thoughtful scholarship of recent years has taken on all three at once.² In my own work, particularly my continuing project on Heisenberg in West Germany, I have also tried to show how all three concerns can and should be knit together. For today, however, I will be somewhat less ambitious. As I turn to my topic, "Building Physics after World War II," my approach is going to be somewhat more limited, first limited by time, and also limited by your interest, which I am figuring does not extend deeply into the details of the conceptual development of postwar physics. Instead, drawing on a good deal of recent scholarship, I will focus on linking up the history of physics during the first postwar decade with broader historical changes, understanding it in connection with demobilization after World War II and remobilization for the Cold War, with postwar construction and reconstruction and with longer-




range ambitions. Within these limitations I hope at least to convey a sense of the historiographical interest of the period and the questions that it poses. The transformations that were worked in physics by the Second World War were as great, in their own way, as any the discipline had seen before. And it is the character of this transformation, the new modes of building physics after the war, that I want to explore by means of a contrast between two individuals each influential within his own context.

In a sense it is apropos to address this period by comparing Ernest Lawrence and Werner Heisenberg, each representing much wider developments. Let me just indicate, to start, where the contrast might lead us. Lawrence, as I am sure you know, was one of the men who made Berkeley physics into what it is today. The Lawrence Berkeley Lab on the hill above the campus was to no small extent his creation, and the physics program here rose to international prominence in the 1930s in large part through his efforts to finance, build, and exploit new generations of ever more energetic particle accelerators. From the end of the World War II he headed up a standard-setting program of experimental high energy nuclear physics or, as it came to be called, particle physics, and his story can exemplify the new direction of physics research in the years after the war. By contrast, Heisenberg stands for physics pursued under very different circumstances and with very different means. When the United States won the war, Germany lost it; and the consequences of that experience would have crucial ramifications. Like Lawrence, Heisenberg had been one of the key players in prewar physics, exploring quantum mechanics and then quantum field theory, the theoretical complement to Lawrence's experiments. After the war Heisenberg also emerged as a national leader for physics. In his case, however, it was in a

devastated, divided, and occupied country, one where research in nuclear physics was stringently controlled and where physicists' clout was limited at best. The process of adaptation to these conditions will point up the contrasts in early postwar physics, highlighting the circumstances that made Lawrence's project so dominant and Heisenberg's so difficult.

So with the general line of argument sketched out, let me begin with Lawrence's story. Here I am fortunate to draw on a substantial body of scholarship on Lawrence and his laboratory, much of it (with acknowledgment to the Bancroft, where Lawrence's papers are stored) the work of historians here at Berkeley, including John Heilbron, Robert Seidel, and Bruce Wheaton. A biography of Lawrence by Gregg Herken is also underway.³ For many of you this story will be familiar. We can start with Lawrence's arrival at Berkeley in 1928 as an associate professor in the Department of Physics. Although he had previously made his name in atomic physics, he quickly switched his interests to the new possibilities for nuclear science in accelerating charged particles in a device soon named the cyclotron. Lawrence and a graduate student built their first cyclotron in late 1930; an inexpensive, roughly palm-sized device that would soon be dwarfed by its successors in size and in energy. Through the hard years of the Depression and the recovery, Lawrence planned a sequence of new machines, each larger and more powerful than the last: first an 11-inch cyclotron in January 1932; then a 27-inch one in December, enlarged to 37 inches in 1937. The next machine, 60 inches in diameter, was completed in 1939, the last before the war; but Lawrence was already drawing up plans for another one, a machine whose magnet weighed 4300 tons and measured 15 feet across, so energetic it could accelerate particles to 100 million




volts and so large it could only find its site on the hill above campus. Costs were projected at several million dollars; construction began in 1940. All during this period, moreover, Lawrence's growing school of cyclotron physicists was spreading out across the country and beyond, building new machines at their new institutions, helping make the U.S. the world's leader in cyclotronics. During the same years colleagues came to Berkeley to learn the hands-on tricks necessary to make the thing work.

Now one might assume from this prewar sequence of ever-larger accelerators (the first intimation of an unmistakable postwar trend), that Lawrence's lab must have been doing exceptional physics. The peculiar thing is that the machines' contributions to nuclear physics were, on the whole, rather modest: the great advances of the 30s—particle discoveries, artificial disintegration, induced radioactivity, nuclear fission—came by other means, either cosmic-ray studies or lower-energy techniques. Where the laboratory really shone was in the creation of particle beams, and the study and manufacture of artificial radioactive isotopes. It was actually the latter project, in particular, that underwrote much of the lab's expansion, as Lawrence explained to great effect the medical uses of the cyclotron (particularly the 60-inch one, designed for that purpose) to the university and to donors. Beyond that, much of Lawrence's effort was directed less towards doing nuclear physics than towards making improvements on the machines. The push to ever higher energies was justified in itself, because the money happened to be there, or for the sake of medical physics. It had little to do in Lawrence's argumentation with either the breakdown of quantum field theories, which, as we will see, was anticipated by many theorists at high energy, or even much expectation of discovering new heavy particles, which really

first became a leading concern in the late 40s and early 50s.⁴ And the fixation on hardware and technique, as it often appeared to European physicists, seemed a displacement of physics by engineering. So by the end of 1930s the achievements of Lawrence's team were clearly impressive—they would celebrate his Nobel Prize in 1939 for the cyclotron and the artificial radioactive elements—it was not obvious that this would be the direction of physics in the future.

In any case, the Second World War would intervene, and two projects growing out of Lawrence's lab would prove vital in the Manhattan Project. Beginning in 1939, beams from the cyclotrons were put to use irradiating samples of uranium. It had recently been shown, in a discovery made in Berlin without a cyclotron, that neutrons could cause the fission of certain uranium isotopes; but the point of the present project was to study not the fission process, but the artificial radioactivity created in non-fissioning nuclei. The outcome, as I am sure is well known here at Berkeley, was the identification of plutonium and the characterization of its properties. This line of work grew directly from one of the laboratory's strengths of the 1930s, its studies of materials produced by bombardment with cyclotron beams.

By contrast, the second war-related project emerged from Lawrence's programmatic inclination to push science, engineering, and equipment to their limits. The magnet for his huge new cyclotron, begun in 1940, was soon put to other purposes, as physicists began to explore the possibilities for separating uranium. Of the two principal isotopes of uranium, only one is suitable for use as an explosive; and that isotope, a small fraction of the normal mixture, can in theory be separated out with a magnetic field




because of its slightly smaller mass. So Lawrence turned his cyclotron magnets to the building of prototype mass spectrometers, their massive size being necessary to achieve a detectable enrichment of U-235. The “calutrons” (note the name) that resulted from this program formed one of the major routes to isotope separation implemented at Oak Ridge. Besides drawing on the equipment in place at the Berkeley lab, the project owed much to its assembled expertise and staff, and to Lawrence’s capacity to imagine and inspire colossal technical achievements.

Now as the conflict was winding down in 1944 and 1945, Lawrence began to turn his attention to building physics after the war. Not surprisingly, he envisaged Berkeley’s program as continuing in the same direction as before, building accelerators of ever higher energy, beginning with the completion of the cyclotron that had been begun in 1940. This was still not yet the obvious line for physics as a whole to be pursuing, even if it remained the obvious line for Lawrence. Back in 1937 a totally new particle, the meson, had been discovered, not in the cyclotron, of course, but, like the positron of 1932, in the cosmic-rays that impacted on the earth’s atmosphere from space. Already before the war, in promoting his plans for the fifteen-foot machine, Lawrence had started holding out the hope that such particles might actually be created with a sufficiently energetic accelerator. Here was a crucial reconception of the purpose of the cyclotron, and also a justification of the push for higher energies—a push, we may note, that had to that point borne relatively little fruit for physics. Added onto the pure machine-building impetus, the possibility of creating particles and the associated chance to study them would drive the proposals for new accelerators (though the particles that would be created were still assumed to be ones already known). By the end

of the war, however, it still remained unrealized; and the scale of the proposals that Lawrence set out often had as much to do with the limits of his budgets and the availability of his materials, particularly as these fluctuated with the circumstances, as with the still tenuous predictions of particle creation thresholds.

For the character of the postwar program, however, the crucial factor was that the limits of Lawrence's budgets and the availability of his materials saw a dramatic jump from the prewar possibilities. For reasons to which I shall return, Lawrence now found ready support for his ambitions and millions of dollars in yearly funding from the federal government and, in particular, from the Manhattan Engineer District and its successors. This support constituted a qualitative change from even the best of the prewar times, when the Rockefeller Foundation was making its most generous contributions. It made it possible for the lab to go on operating at nearly a wartime scale, when it had employed hundreds of people on its war-related projects; and it made it feasible to imagine implementing Lawrence's existing vision of new generations of accelerators. The result, as I am sure you know, is the laboratory familiar from the reports of the 1950s, with its envelope-pushing accelerators and its research program of a scale, scope, and ambition rarely matched elsewhere.

At the same time, I should note, Lawrence continued to play a role that he had already taken on during wartime: that of the scientist as advisor on matters of national security. From his place within this network he pressed for continued attention to the military applications of physics, in favor of strategic bombing, plutonium production, the nuclear submarine, and the hydrogen bomb. This, too, would have its impact on the work done under his aegis,




particularly at the new laboratory that went up in Livermore. But for today I will mostly leave this aside and focus on the impact of his program on the building of postwar physics. For Lawrence, then, building physics after World War II meant building up his lab's existing program, pushing for bigger machines and higher energies. In a sense, he was well placed to take advantage of the new situation created by the war, with a new patron for research and new orders of magnitude of funds. And it was in no small part these circumstances that would make him into a preeminent exemplar of physics after the war.⁵ Concerns might be expressed elsewhere about the way the field was headed—murmurs of “Berkelitis”⁶—but Lawrence evidently never doubted.

For Heisenberg in Germany the situation was rather different. To understand why this was so, we need examine his career up to this point, for which David Cassidy's biography is the essential starting point, at least for the years through 1945.⁷ Heisenberg and Lawrence were practically the same age, though Heisenberg was the earlier in making his mark on modern physics. Heisenberg's studies in atomic theory in the capitals of continental physics, Munich, Göttingen, and Copenhagen, put him at the center of the quantum theory by the early 1920s. A few years after his Ph.D. he saw the way to the creation of a new quantum mechanics that, together with the contributions of other, primarily European, often German physicists, became the principal basis for all future work on the structure of matter. Already at age twenty-five, in 1927, he was accepting a call to Leipzig as full professor of theoretical physics. The extraordinary success that this appointment rewarded included not only the 1925 paper on quantum mechanics and his famous work of 1927 on the uncertainty principle, but also his explanations of such phe-

nomena as two-electron atomic systems and ferromagnetism that truly seemed the sign of a golden touch. These advances, together with others over the next years (for instance, his work of 1932-33 laying the theoretical foundations of nuclear physics) secured him a reputation as one of the world's foremost physical theorists.

In the late 1920s Heisenberg started on a project that would occupy him for the rest of his career, the creation of a consistent and correct quantum theory of fields. The effort began in two papers with Pauli on quantum electrodynamics, which set up a canonical formalism for extending quantization procedures of quantum mechanics to the infinite number of degrees of freedom of the electromagnetic field. That work on quantum electrodynamics (or QED) suggested how to connect the continuous phenomena of the field with the discrete character of the particle, and how to construct a unifying theory of the interaction of different sorts of particles (electrons, photons) and to account for the forces amongst them. It also raised substantial problems, however, as peculiar infinities appeared in crucial places in the calculations, giving rise to expectations that quantum electrodynamics and the whole structure of quantum field theory would somehow collapse at high energies. So evidence for that collapse was sought in the high energies of the cosmic-rays; and though the boundary for the onset of peculiarities was pushed back through this research, the conviction remained that the quantum theory of the electromagnetic field was at some level fundamentally wrong.

Nonetheless, QED's complementary successes could serve as a basis for extending the quantum field theoretic treatment to other kinds of particles, an effort that occupied many of the leading theorists of the 1930s (and be-




yond). This enormously challenging program got a boost in 1937, when the meson was discovered in cosmic-rays: a new kind of particle demanded a new sort of field. (The positron, which had been found in 1932, had quickly found a place in QED and did not demand a new theory, while the neutron, also discovered in 1932, was given its own field and built into a problematic theory of the nuclear force.) Conveniently enough, the meson now seemed to fit much more nicely as the particle associated with the field thought to carry the force holding nuclei together. That this expectation later proved unfounded became the “ten-year joke” of particle physics, once the real quantum of the nuclear force (such as it was) was found in 1947; at that point the meson was recognized to be something totally different. But in the late 30s it seemed a crucial element in the theories of the nuclear force. Information on it was sought indirectly through nuclear physics, and more directly in the cosmic-rays. And some physicists, Heisenberg among them, began to suspect that further peculiarities in the cosmic-ray data indicated that field theory might break down at high energies in meson theory as well as QED.⁸

In all this work Heisenberg was an active participant through the 1930s. In the early part of the decade he also began to build up something of a school of theoretical physics; an effort, however, that was pretty much put to an end in 1933. Over the next few years large numbers of German physicists were dismissed or emigrated because of the Nazi racial laws. In conjunction with other leading figures of the German physics community, Heisenberg took a role in behind-the-scenes efforts to reverse, ameliorate, and in one case criticize the decrees--to what little effect only gradually became clear. Nor did the award of the Nobel Prize in the fall of 1933 (for the year 1932) do much

to improve the new predicament of theoretical physics in Germany. On the contrary, the so-called *deutsche Physik*, or Aryan physics, on the ascendant in the mid-30s, attacked modern physics as a Jewish construct and Heisenberg personally as a “white Jew.” Despite the savage public battle that ensued, Heisenberg chose to remain in Germany. Alongside some efforts to persuade the regime of the damage it was doing to science—a memorandum from the mid-1930s, for instance, contrasting the sorry state of German physics with the vitality of the U.S., as exemplified by Lawrence’s cyclotrons—he evidently preferred to retreat as much as possible into the circle of friends and family and into physics.


Now Heisenberg’s actions under the Nazi regime obviously raise multiple questions for historians to argue about, as do the actions of many German scientists; and the questions are heightened in Heisenberg’s case by his involvement, from the outbreak of war, with the regime’s program to exploit nuclear fission. This project, as you may know, involved on the order of a hundred or so physicists and chemists, of whom Heisenberg was unofficially the leading theoretician; and it explored some of the same topics as the Manhattan Project, though in much less depth, before concluding in 1942 that a bomb could not be built in short order under the conditions of the war. Although my main topic today is the postwar period, I want to make a short excursion here to talk about these issues, particularly as they bear on the relationship between science and the state. The general view, so far as I can tell, is that while Heisenberg avoided joining any of the National Socialist organizations, his work on the nuclear project indicated a level of sympathy with the regime that was common to many German scientists, or at least indicated the absence of any fundamental objection. Moreover, on this view,



Heisenberg and his colleagues badly compromised themselves after the war with apologies, falsely suggesting that the German scientists' moral scruples had made them hold back from building a bomb for Hitler. This argument, which is partly based on recent scholarship,⁹ seems in any case to be the view that has made its way into popular and semipopular perceptions.

It was also, I should say, the view I held myself when I started studying Heisenberg's postwar career. But as I began to work my way through his private correspondence and public statements, I found that the standard picture, interestingly enough, did not work in all respects. If you go back and read Heisenberg's own statements, paying attention not to read into them what you have long heard is supposed to be there, you notice that he did not actually claim that he had deliberately kept the bomb from Hitler. Instead, he pointed out that the German nuclear project had never been in a position to produce a weapon, constrained as it was by the conditions of the war. What he did lay claim to was at least to have thought about the morality of making a bomb—but without suggesting that this concern had had a role in preventing it.¹⁰ So, in a conclusion I find revealing about the postwar public sphere, his explanations turn out to be more subtle and differentiated than those that circulated under his name, particularly those given by popular writers of the mid-1950s.¹¹ Though on this occasion I cannot lay out all the evidence for this,¹² I do want to raise the issue.¹³ (I will also say parenthetically, that I think the recent suggestions that the German project did not build a bomb because its leading physicists misunderstood it are also marred by misconceptions of some important technical points; but again I will not go into this here.)¹⁴


This leaves, however, other questions surrounding what Heisenberg and his scientific colleagues did do under the Third Reich. These questions play quite crucially into more general arguments, particularly prominent since the 1960s, about the role of the scientist in politics and society; and Heisenberg's case is quite interesting in its complications. On the one hand, it seems clear that he came to reject the regime's ideology. Along with his behind-the-scenes actions beginning in 1933, this is evident in a closely-guarded manuscript he wrote during the war, which makes coded references to the student resistance group the "White Rose" and contrasts the "strange this-worldly religions" of communism and National Socialism with the Anglo-Saxon spirit of law and justice.¹⁵ In this document it seems fairly clear where his allegiances lay. How he chose to act on those allegiances is a trickier matter, however. One can of course argue that by working for the Third Reich Heisenberg, like his colleagues, demonstrated that he had no objections to the regime, or else that his postulated convictions were worth very little. Rather than answering the question of the relation between thought and action, this approach simply avoids it, or else turns it solely into a question of courage. The matter, I think, can be considerably more complicated. In Heisenberg's case, for instance, it involves not only his general political judgment (also evident after the war) about working within the system, but also his estimates, sometimes mistaken, of the Nazis' strength and staying power. What the question comes down to is how actors calculate action under compulsion, and how historians can come to understand their calculations. In this sense I am of course not arguing that we should see what Heisenberg (or other scientists) did as right or good, since I do not think I want to take that view myself. But if we are going to understand



his thoughts and actions, and the thoughts and actions of his colleagues, we need to be on our guard about the problems of interpretation.

Now I have gone off on this excursus partly for its own interest, of course, but partly because it also has consequences for Heisenberg's postwar efforts. What I want to do now is turn to Heisenberg's efforts to rebuild physics after the war. Here the situation was, again, rather different from Lawrence's. While Lawrence was petitioning the Manhattan Project in the summer and fall of 1945 to support an expansive accelerator program, Heisenberg was sitting in Allied custody in Britain. He and nine other German nuclear scientists had been taken there at the end of the European war, and they were not released to Germany until early 1946. Heisenberg was returned to the British zone of occupation and instructed to rebuild the research institute he had come to lead during the war. Like a number of other institutes, the Kaiser-Wilhelm (later Max-Planck) Institute for Physics had been relocated from a devastated and divided Berlin to the university town of Göttingen, relatively spared by the bombing. What Heisenberg now faced was the task of starting up an institute minus any and all equipment: what apparatus had not been confiscated by the occupying forces had come to rest in the French zone and was thus inaccessible. There was no Manhattan Engineer District, or even a federal government, to which he could apply for funds; for that matter, there was hardly any good paper to write up an application, or telephone connections to call up an agency. The rebuilding of Germany's scientific institutions was beginning, like the rebuilding of its universities, but the hope of re-attaining international standards, of returning German physics to a position of respect on the international scene, was mostly, and manifestly, a long way from realization.

So this is the state of affairs Heisenberg faced in the early years after the war. The strategies for building physics that Heisenberg chose in this situation form an interesting contrast to Lawrence's, and they make clear some interesting features of the two men's actions and commitments. Let me bring out three major contrasts, in each case looking first at Heisenberg's situation and then using it to illuminate Lawrence's. Heisenberg's first strategy was encapsulated in one observation: theory is cheaper than experiment. Experimentalists need apparatus, to start with; but they also need an infrastructure and a support system: gas lines, electrical outlets, companies to supply materials, technicians to maintain things. For multiple reasons, many of these were lacking in early postwar Germany. By contrast, theorists need scrap paper, and sometimes a blackboard. So part of the reason why Heisenberg put such an early emphasis on theory in rebuilding his institute—which had started out, like most institutes, as the domain of experimentalists, and which later, still under Heisenberg's leadership, would again become more experimental—was that the circumstances recommended it. Under these conditions, it was also clear that theory would provide a quicker reconnection to the international level of scientific research: Germany might eventually, in years or decades, become competitive again in experimental work, but for the time being theory was more promising. (This is not to say, of course, that theorizing was completely independent of material needs and infrastructure; one finds Heisenberg continually struggling to get hold of the newest issues of the American journal *The physical review*, which he ended up obtaining as a personal gift from his former student Edward Teller. But still the volumes could be months delayed, and Heisenberg only learned about the great advances in renormalized QED, for in-




stance, long after they were discussed widely outside of Germany. So theorizing had its constraints too.) The contrast, at any level, to Lawrence's situation is obvious. Though of course Lawrence was an experimentalist and Heisenberg a theorist, their choices of emphases also reflect the circumstances in which they worked. And Heisenberg's example makes intensely clear the fundamental preconditions, rarely recognized as such, that made Lawrence's entire experimental program, and that of the main line of postwar physics, even a thinkable projection.

Heisenberg's second strategy is also interesting as a contrast to Lawrence's. As Heisenberg began to find the resources in the late 1940s to build up experimental physics, he initially placed his primary emphasis on cosmic-ray research. The reasons here are again revealing, and help to set off interesting features of Berkeley's situation. First of all, Heisenberg needed to be able to say that cosmic-ray research had no conceivable military utility or application. Any sort of applied nuclear research was placed under serious restrictions or forbidden outright in postwar Germany, at the demand of the occupying authorities. The fact that cosmic-rays were absolutely useless for military purposes was something Heisenberg stressed over and over in his early postwar lectures and interviews. This is not a theme one often hears in U.S. discussions of the field.

Second on Heisenberg's list of reasons to pursue cosmic-ray studies was the fact that these experiments were strikingly simpler and cheaper than accelerators. With the limited budgets and infrastructure available to postwar German physicists, cosmic-ray experiments were comparatively easy to arrange and carry out. This indeed held more broadly, as European researchers generally (except in the

few centers of accelerator work) found it more feasible financially to focus on cosmic-rays. Here the equipment and costs were rather limited: all one had to do was to expose one or another sort of detector to cosmic-rays and see what happened. The high energy of the particles came for free, instead of having to be paid for with a massive accelerator. What costs there were, largely associated with getting at the cosmic-rays, were still manageable: either trips to mountain peaks (like the Zugspitze near Garmisch) or balloons or occasionally airplanes, with the help of the Allied forces. Accelerator programs like that at Berkeley were really not an option.

Finally, Heisenberg, like many physicists, was not yet convinced that accelerators were necessarily the ideal way to go about high-energy physics. The great discoveries of the late 40s and early 50s—the pion and the V particles—finally opened up the suspicion that higher energies would reveal an entire zoo of new particles. But these discoveries were made with cosmic-rays, not accelerators. When the large Berkeley cyclotron first produced its man-made mesons in 1947, a decade after the particles had been discovered in cosmic-rays, it was indeed a great achievement, since it allowed for much more intense study than had been possible in other ways. But in discovering new particles the machine was still behind the cosmic-rays, and even the man-made mesons required the assistance of a cosmic-ray physicist to get the detectors functioning. Along with the general time lag that Lawrence's machines had demonstrated in the past, and the concern about the cost-effectiveness of the entire program, these were reasons why some physicists might still find cosmic-rays the more effective means of experimentation. One sees signs of this, for instance, in the arguments that men like Bohr and Chadwick raised in the early 50s against making CERN




into a massive accelerator center on the American model: their sense was that accelerators were not necessarily the immediately obvious instruments. By the mid-50s, however, things *were* beginning to change, as the new postwar accelerators were beginning to deliver results. And by that time Heisenberg too accepted the argument that CERN had to have a large accelerator. But one has to keep in mind that Germany's arguments for pushing the CERN project were multifaceted; up "to 80% of the cost," Heisenberg stated in 1953, "should be considered from the angle of European cooperation."¹⁶

Given all this, it is worthwhile returning to the Berkeley case and looking again at the reasons for the promotion of Lawrence's postwar accelerator program. I spoke earlier about the continuation of the machine-building push, which seemed to have a certain independence from the physics itself. Part of the scale of Lawrence's proposals, and his focus on accelerators, is to be understood in this way. But the scale of his proposals had to do with something else as well. The historians who have researched the lab have found two interesting postwar planning statements, one from the early winter of 1944, the other from the late summer of 1945. In the former, Lawrence was expecting that the end of the war would bring a dramatic *shrinkage* of the laboratory, including a 99% budget cut to just \$85,000 a year. This was about the same level as before the conflict, and reflected very clear assumptions about who would be paying for physics and why. By the time of the second estimate, however, after the successes of the summer of 1945, expectations had shifted. Now Lawrence approached Leslie Groves, the head of the Manhattan Project, proposing an annual postwar budget of several million dollars. Obviously something had changed.

Part of what changed, of course, was the emergence in mid-1945 of striking new approaches to accelerating particles, ideas that opened up grand options for a laboratory like Lawrence's. But that is not all that changed. The notion that the federal government—and more specifically, the Manhattan Engineer District—would be appropriate for, and interested in, supporting peacetime scientific research, represented a new development, and one strikingly more characteristic of the U.S. than of the emerging German republic. Although the state *had* made its contributions to prewar research at Lawrence's lab, those contributions had come primarily from the National Cancer Institute. And the idea that the government might support physics on such a scale, with millions of dollars at a single institution, reflected some fundamental rethinking in this country, between 1944 and 1945, of the utility to the state of fundamental scientific research.

Many historians have in fact commented on the U.S. government's new appreciation for postwar physical research, trying to explain why basic science, say, at particle accelerators might be of interest to the state. Interestingly, Lawrence certainly indicated that research on the meson might help clarify nuclear forces, and his sponsors certainly found that promising. But the reasons for support actually often had less to do with hopes for application of the knowledge, and more to do with the technique and technology instantiated in the machines, for the new accelerators cannibalized hundreds of thousands of dollars of war-surplus radar sets and electronics, and promised in turn new useful developments. Above all, the reasons for support had to do with the desire to hold onto the corps of committed scientists at the laboratory and elsewhere.




In the Manhattan Project, the radar effort, and the other wartime programs, U.S. physicists, theorists and experimentalists, had proven themselves valuable beyond all belief as versatile, innovative designers of practical devices, even in fields far removed from their original training. This pool of scientific talent had to be preserved and enlarged for redeployment in the case of a future national emergency. This is not simply historians' speculation; there are clear arguments to this effect in the documents of the administering agencies.¹⁷ Of course the physicists saw it differently, and this is not to say that their research was somehow "tainted"; but if we are looking for the motives of the state, this is principally where they are to be found.

The interesting thing, I think, is that the federal government was betting on an expensive envelope-pushing accelerator program even in the months immediately after the war, *before* the strong conviction had arisen that new particles lay awaiting at higher energies and *before* accelerators had yet shown themselves more efficient than other methods (like cosmic-ray experimentation, or, for that matter, lower-energy studies). Obviously accelerators had worked in the past, and with the new design principles they would work even better in the future; but their effectiveness *vis-a-vis* other approaches was hardly clear. In this sense the prewar record of Lawrence's lab was not all that inspiring. The willingness of the U.S. government to support the research nonetheless is a crucial feature of the postwar setting, and the striking character of its support is brought out even more clearly in the contrast between Lawrence's aims and Heisenberg's.

The connection to the state brings me to the last of Heisenberg's strategies. For him, building physics after World War II finally meant bringing the case of science

before the state authorities, particularly, come 1949, to the federal executive in Bonn. In the early postwar years, from 1949 to 1951, Heisenberg emerged as the leading figure in an attempt to institute an organized science policy within the federal government, cooperating directly in this matter with the Chancellor, Konrad Adenauer. Interestingly—and here the previous contrasts with Lawrence’s case are partly replaced by similarities—Heisenberg deliberately designed his efforts following British and U.S. models, aiming to establish in West Germany a scientific advisory function comparable to that he saw in those two countries. His “German Research Council” was made up of eminent scientists, chosen by cooption, who would advise the government on all scientific-technical matters while remaining, explicitly, politically independent. The Council also pressed for drastically increased funding for the sciences, and it made the case for the federal coordination of research efforts in the service of national rebuilding. In all these aspects, Heisenberg was pushing for a role for the scientist in the government to match those in the victorious Allied countries, where the lessons of the war had installed scientists in positions of power. What was different for the West German case, and thus valuable as a means of comparison with the U.S., were the resistances that Heisenberg ran up against in his bid for scientific advising.

First, it was not self-evident in the German case that the proper authority was the federal government. State support for science was nothing new in Germany, since much of the country’s research was done in its universities, and the universities were controlled by the state. But by long-standing tradition, at least before 1933, state responsibility for the universities, and for “cultural” matters more generally, belonged not to the central government




but to the *Länder*, the individual states (Bavaria, Lower Saxony, etc.).¹⁸ To have scientific research attached to the federal government, as it was in the U.K. and the U.S., Heisenberg's German Research Council now had to argue against this view. Of course, in Lawrence's case it would be obvious that the federal government would be the patron, since it was naturally the federal government that had directed the wartime mobilization of science. That contingency would set the patterns of postwar funding in the U.S., but it was not a foregone conclusion in nations without the crucial wartime experience.

A second set of resistances derived from Heisenberg's way of framing the significance of the sciences. The disciplines he brought into the German Research Council were quite emphatically the natural sciences. These were the fields, as he saw it, that were essential for Germany's short and long-term rebuilding, from the reestablishment of basic infrastructure in construction, agriculture, and health to the development of technical industries to sustain a new economy. So while the Council proposed to speak for all of German "research," it effectively gave voice to the natural sciences alone. This was not unproblematic, however, at least not in the German context. In fact, representatives of the humanities and the social sciences rapidly raised an angry protest at their exclusion, and at the Council's truncated conceptions that stressed practical application over pure learning. Of course, when Lawrence addressed the state he never faced anything of this sort. Whether in funding or in advisory posts, he could count on a special place for the sciences in the postwar political economy. Here humanists and social scientists had little of the power that they possessed in Germany, where the ideal of *Wissenschaft* gave them cultural capital that scientists often lacked. And likewise Lawrence could implicitly count on deference

towards the employment of the physicist as advisory jack-of-all-trades; in the Allied prosecution of the war physicists had secured their place as government advisors and experts on all possible subjects. In Germany, by contrast, Heisenberg was also facing objections from engineers for the arrogation of their expertise. He could not simply point to the Manhattan Project to prove that scientists were natural advisors on all topics.

Finally, Heisenberg had to persuade the politicians to allow him to create the role for the scientist that he was hoping to put in place. Heisenberg's convictions about scientific advising went back not only to the need for support of the sciences, but also to deep-seated beliefs about the crucial need in politics for scientific rationality. This was scientific rationality less in the familiar sense of technocratic calculation, and more as a bulwark against such deeply irrational political movements as, in Heisenberg's mind, National Socialism. And so one of the tasks of the scientific advisor, along with special pleading for funding, was to inoculate postwar politics with the spirit of scientific rationality. Thus the German Research Council was to aim at the very highest levels of government, establishing itself as advisory to the Chancellor—but also maintaining political independence. Now although this seemed natural to Heisenberg, it struck many politicians as disputable, particularly for the claims it made that scientists should be accorded political power without being held to political standards. The federal president, Theodor Heuss, wondered aloud whether scientists, *qua* scientists, could give what could only be political advice. And though Adenauer, the chancellor, was much more favorable to the initiative—perhaps, as Heuss seems to have viewed it, because it centered scientific advising within his own office—he was reluctant to allow the Council and its scientists to exercise




truly independent authority. Responding to these critiques, and more generally persuading politicians to listen to him, demanded a large part of Heisenberg's efforts on behalf of the German Research Council. And here we find perhaps a final, striking contrast with the U.S. case. When Lawrence had a case to make to the politicians—whether for building cyclotrons, for developing fission weapons, or for pushing the hydrogen bomb—the channels for exerting influence were already in place, sometimes already institutionalized. Whatever the postwar struggles over the shape of the government's scientific organization, Lawrence and his colleagues did not have to spend their time convincing the state that it ought to listen to them at all. In West Germany this entire mechanism did not yet exist; it would have to be created and put into place against the wishes of some very powerful players. The German case makes clear what phenomena simply could be taken for granted in the U.S. as a result of the wartime experience, things that took intense political negotiation in countries without that past to draw on.

As if to reinforce the point, the German Research Council failed. In the end it had too many enemies—too many different enemies—and not enough friends. In 1951 it was formally incorporated into a body that was to combine the allocation of research funds (an organization for this purpose had existed separate from the Council) with the duties of scientific advising. But not much came of the scientific advising part—though the German Atomic Commission, set up to advise on nuclear energy after the regaining of sovereignty in 1955, came to stand in, at least for physics, for a full advisory apparatus. And Heisenberg himself would function for several years as an informal consultant to Adenauer, becoming (at least in popular perception, if perhaps less so in reality) the *eminence grise*

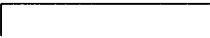
of science policy. But by the middle 1950s that connection had broken down, over Heisenberg's sense of scientific subordination to the Chancellor's political calculations and over disagreements about the nuclear arming of the federal defense forces. Building up a scientific advisory structure like that in which Lawrence already operated proved far harder than Heisenberg had imagined.

At the beginning of this discussion I referred to the important transformations that World War II would work in physics. As I come now to a close, I want to bring out two of these transformations, ones that I have tried to shed some light on in our comparison of Lawrence and Heisenberg. The first, and perhaps the more obvious, is the recasting of the patronage relationship within which physics operated. As many scholars have pointed out, Lawrence's accelerator program is an ideal example of the new dispensation, supported by massive government funding and sustained by Lawrence's advisory function. This dramatic alteration of the prewar state of affairs originated, as we saw, in the wartime experience, and it persisted after the war because of the state's interest in maintaining a reservoir of technique and personnel. And however natural the situation might come to seem in the years after the war, it was not by any means an inevitable development. The conditions that made it possible were hardly universal, but rather quite specific, as becomes especially evident in Heisenberg's failed efforts to build up a West German science policy along lines Lawrence could have approved. And the resistances Heisenberg ran into make it clear, by contrast, what sorts of opposition Lawrence and his colleagues never had to face.

And the other transformation, in closing, that I want to highlight is a reworking of the self-imagination of the



field of physics itself. Lawrence's accelerator program envisaged a definite direction to physics: towards ever higher energies. From the 1930s onward this drive was due at least as much to the satisfactions of machine-building as to new physical discoveries. We saw how, just on the cusp of the war, Lawrence began—*began*—to talk about putting the big cyclotron to use to explore the new regimes of particle energy it was intended to make available. So Lawrence held out the hope of making mesons—though he did not seem too perturbed when their theoretical production threshold fluctuated into the space beyond his machine's capacity. In all of this we see very little of the expectation that the new realms of energy will reveal new sorts of particles, a conviction that, if now completely second nature to us, really took hold only in the late years of the 40s and the early years of the 50s. And the massive new accelerators of the early postwar era, together with the push towards higher energies that their design instantiated, were likewise conceived with little reference to the discovery of new particles. Of course, in the years since those discoveries, as even men like Bohr dropped their resistance to huge accelerator projects, we have reconceived the impetus to higher energy as an impetus towards new physics; and we have rewritten the history of the discipline, to follow the physicist-historian Abraham Pais, as a story of "inward bound."¹⁹ This imperative of "inward bound" is the postulated journey from macroscopic to microscopic, from bulk phenomena to atomic structure to nuclear physics to elementary particles. But the conception of physics as "inward bound," while it has precursors in the 1930s, is really, I sometimes think, a formulation characteristic of the early postwar years. One finds surprisingly little mention of it among the physicists of the first half of the century, scientists whom we now



suppose to have been pursuing it. But by the 1950s on, with the domination of the big accelerators, it had become a standard theme in explanations of the discipline's grand goals. It may be that the field's self-imagination, and its framing of particle physics as the vanguard of the discipline, was also shaped, and shaped dramatically, by the material possibilities of the early postwar period.



FOOTNOTES:

1. Cf. J.L. Heilbron, "A historian's interest in particle physics," in *Pions to quarks: particle physics in the 1950s*, ed. Laurie M. Brown, Max Dresden, and Lillian Hoddeson (Cambridge: Cambridge University Press, 1989), 47-54.

2. One example is the extraordinary biography of the nineteenth-century physicist William Thomson that Crosbie Smith and M. Norton Wise published a few years ago, *Energy and empire: a biographical study of Lord Kelvin* (Cambridge: Cambridge University Press, 1989). I could also point to the work of John Heilbron, my predecessor here at Berkeley.

3. J.L. Heilbron and Robert W. Seidel, *Lawrence and his laboratory: a history of the Lawrence Berkeley Laboratory*, vol. 1 (Berkeley: University of California Press, 1989); J. L. Heilbron, Robert W. Seidel, and Bruce R. Wheaton, *Lawrence and his laboratory: nuclear science at Berkeley, 1931-1961* (Berkeley: Lawrence Berkeley Laboratory and Office for History of Science and Technology, University of California, Berkeley, 1981); Robert Seidel, "The origins of the Lawrence Berkeley Laboratory," in *Big science: the growth of large-scale research*, ed. Peter Galison and Bruce Hevly (Stanford: Stanford University Press, 1992), 21-45; Robert W. Seidel, "Accelerating science: the postwar transformation of the Lawrence Radiation Laboratory," *HSPS* 13:2 (1983): 375-400; Robert Seidel, "The postwar political economy of high-energy physics," in *Pions to quarks*, 497-507.

4. Thus Lawrence seems to have made little use of Oppenheimer-type arguments, often deployed in cosmic-ray physics, that such techniques would let physicists reach into the energy realm where quantum field theories were


expected to break down. For such arguments see David Cassidy, "Cosmic-ray showers, high energy physics, and quantum field theories: programmatic interactions in the 1930s," *HSPS* 12:1 (1981): 1-39; Peter Galison, "Particles and theories," chapter in *How experiments end* (Chicago: University of Chicago Press, 1987). The relevant energy regimes (beginning around 100 MeV for meson field theories) were just out of reach of the last cyclotron Lawrence was planning before the war.

5. For historical studies of particle physics after the war see, for instance, Ulrike Merseits, "From cosmic-ray and nuclear physics to high-energy physics," in *History of CERN*, ed. Armin Hermann, John Krige, Ulrike Merseits, and Dominique Pestre, vol. 1, *Launching the European Organization for Nuclear Research* (Amsterdam: North-Holland, 1987), 3-60; Laurie M. Brown, Max Dresden, and Lillian Hoddeson, "Pions to quarks: particle physics in the 1950s," in *Pions to quarks*, 3-39.

6. Arthur Roberts, "Take away your billion dollars," *Physics Today* 1:7 (1948): 17-21.

7. This is David C. Cassidy, *Uncertainty: the life and science of Werner Heisenberg* (New York: W. H. Freeman and Company, 1992), which, however, has relatively little to say about the three decades of Heisenberg's life after 1946. See also David C. Cassidy, "Heisenberg, Werner Karl," in *Dictionary of scientific biography*, ed. Frederic L. Holmes, vol. 17 (New York: Charles Scribner's Sons, 1990); and Armin Hermann, *Die Jahrhundertwissenschaft: Werner Heisenberg und die Physik seiner Zeit* (Stuttgart: Deutsche-Verlagsanstalt, 1977).

8. Here see especially Galison, "Particles and fields," and Cassidy, "Cosmic-ray physics."



9. Mark Walker, *German National Socialism and the quest for nuclear power, 1939-1949* (Cambridge: Cambridge University Press, 1989); Cassidy, *Uncertainty*; Mark Walker, *Nazi science: myth, truth, and the German atomic bomb* (New York: Plenum Press, 1995).

10. I would note, in parenthesis, that this position is quite different from the arguments recently revived that Heisenberg had actually deliberately hindered the work. See especially Thomas Powers, *Heisenberg's war: the secret history of the German bomb* (New York: Alfred A. Knopf, 1993).

11. See Robert Jungk, *Heller als tausend Sonnen: Das Schicksal der Atomforscher* (Stuttgart: Scherz & Goverts, 1956); idem, *Brighter than a thousand suns: a personal history of the atomic scientists*, trans. James Cleugh (New York: Harcourt Brace Jovanovich, 1958).

12. The argument can be found in part in "Making the postwar physicist into a political role model: Heisenberg and representations of the Third Reich's nuclear project in postwar West Germany," MS for a talk delivered at Stanford University on 13 November 1996.

13. This also has ramifications for our understanding of his earlier actions, as the postulated terminus of his postwar explanations has shaped the interpretation of the path that led up to them.

14. See, for instance, Jonathan Logan, "The critical mass," *American scientist* 84 (1996): 263-277. In the past Paul Lawrence Rose has put forward similar arguments and is presently at work on a book of his own. I would also argue that there are problems with many (though not all) of the suggestions of scientific error in Jeremy Bernstein, ed., *Hitler's uranium club: the secret recordings at Farm Hall*

(Woodbury, NY: AIP Press, 1996). Bernstein does not argue, however, that these errors were responsible for the fact that the Germans did not make a bomb. I am very grateful to Jeremy Bernstein for discussions on this subject over several months in 1996, though I do not know what his present views are.

15. Werner Heisenberg, *Ordnung der Wirklichkeit* (München: R. Piper, 1989), 131, 173-174, 161, 152. The text was first published after Heisenberg's death in Werner Heisenberg, *Gesammelte Werke/Collected Works*, ed. Walter Blum, Hans-Peter Dürr, and Helmut Rechenberg, vol. C.I (München: R. Piper, 1984).

16. Armin Hermann, "Germany's part in the setting-up of CERN," in *History of CERN*, vol. 1, 383-429, p. 387.

17. Along with the sources cited above, see, e.g., S.S. Schweber, "The mutual embrace of science and the military: ONR and the growth of physics in the United States after World War II," in *Science, technology and the military*, ed. Everett Mendelsohn, Merritt Roe Smith, and Peter Weingart (Dordrecht: Kluwer, 1988), 3-45; Peter Galison, "Physics between war and peace," in *Science, technology, and the military*, ed. Everett Mendelsohn, Merritt Roe Smith, and Peter Weingart (Dordrecht: Kluwer, 1988), 47-86.

18. The violation of the principle during the Third Reich, with the creation of the Reich Education Ministry, did nothing to help Heisenberg's case.

19. Abraham Pais, *Inward bound: of matter and forces in the physical world* (New York: Oxford University Press, 1986).

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