HEISENBERG, GOUDSMIT AND THE GERMAN 'A-BOMB'

In Mark Walker's strange new view ("Heisenberg, Goudsmit and the German Atomic Bomb," January 1990, page 52), Germany's uranium project, so feared by the Allies, so surprisingly inconsequential in fact, was not a failure at developing atomic weapons.[1] Nuclear *power* was its animating goal, and progress, under sound National Socialist management, was highly creditable.

But how accurate is Walker's account? Much of it derives from material accessible only in Germany, but much else is based on open sources available in the archives of AIP's history library in New York, and familiar to me from the year and a half I worked with Samuel Goudsmit at the *Physical Review*. Comparing Walker's account with the available records turned up some remarkable differences, of which I can list only a few here:

▷ Walker extravagantly dismisses as the product of Goudsmit's "profoundly ahistorical and noncontextual preconceptions" his conclusion that the Germans decisively overestimated the difficulty of making bombs. In fact this overestimate was the controlling assumption of the uranium project's agenda. This much was later acknowledged by Werner Heisenberg[2] ("we regarded the necessary technical effort as rather greater than, in fact, it was") and by Carl-Friedrich von Weizsäcker[3] ("I must admit that we also overestimated the difficulty of the problem "; "we had thought it would be even more difficult and so this was sufficient reason not to try it").

▷ Though Walker makes much of Goudsmit's refusal to do so, Goudsmit did, after a long, painstaking investigation, acknowledge that the senior German physicists had an accurate theoretical conception of fast-fission uranium bombs and of plutonium breeding.[4]

▷ The Alsos mission did *not* destroy apparatus. Heisenberg did *not* write a letter to German authorities on behalf of Goudsmit's parents; he replied to a plea from the Dutch physicist Dirk Coster. Heisenberg's letters to Goudsmit "never mentioned his intervention on behalf of Goudsmit's parents," as Walker states, presumably because Goudsmit by then had long known about the letter, via Coster and Max von Laue, and perhaps because Heisenberg's oddly vague response arrived too late to be of any use. Goudsmit was *not* a reserve officer (he declined a military commission) and did not, of all things, write a "heroic" account of the mission in his book *Alsos*[5] (see the index entry under "Rabi, I. I.").

▷ Goudsmit never held the stereotype view of science as a series of isolated works of great minds, as Walker has it. He advanced the opposite view (in, for example, "It Might as Well Be Spin"[6] and "Guess Work: The Discovery of the Electron Spin,"[7] whose titles suggest how Goudsmit thought of his own "great" contribution).

▷ The basis of Goudsmit's assessment of the uranium project's bomb plans nowhere includes the idiotic identification of spherical reactors with spherical bombs, as in Walker's caricature. Nor did Goudsmit assert that any existing reactors were meant to be bombs, as Walker suggests by substituting the literal "Germany's atom bomb" for the rhetorical "Germany's 'atom bomb' " in Goudsmit's figure caption.

Goudsmit did err, but for reasons very different from those Walker gives, in concluding that the German physicists missed the concept of fast-fission bombs. Germany's uranium work, as the Alsos mission found it at the end of the war, stood roughly where the Allies' had been in late 1941 to early 1942, when the feasibility of fast-neutron explosives had just been established by Merle Tuve's fast-neutron cross-section measurements on uranium-235. Prior to March 1941, it was anyone's guess whether a reasonably small mass of uranium really could, as Otto Frisch and Rudolf Peierls had suggested the year

before, support an explosive chain reaction, or whether it would prove necessary to take advantage of the larger cross sections at lower neutron energies by using moderators. Finding no indication that the German project had resolved this question, and ample evidence of interest in weapons based on slow-neutron fission,[8] Goudsmit reasonably but incorrectly concluded that the project physicists had not grasped the possibility of fast-fission weapons. This point he eventually set right.

Goudsmit's encounter with the German uranium project impressed him with how badly physics had fared under the Nazis. Where the Manhattan Project had advanced with such conspicuous success under a coordinated leadership involving respected scientists at high levels in government, military and industrial decisions, the Nazi system had slighted science. Where autonomy was allowed, as in the Luftwaffe research organization, success (in rocketry, in jet fighter development) was forthcoming. But, lacking a unified and influential organization like the American National Defense Research Council until late in the war (and then mainly on paper), the uranium project was weakly represented and fragmented. Thus, at the time Enrico Fermi's Chicago pile CP-1 went critical with 6 tons of uranium metal, in late 1942, the German project had some $7\frac{1}{2}$ tons in hand—but divided between competing groups unwilling to share scarce uranium, heavy water and other resources. Eugene Wigner and Hans Bethe, who were well placed to assess the difficulties, had reckoned that Germany could stockpile bombs by the end of 1943. That not even a self-sustaining chain reaction had been achieved by war's end came as a great surprise. Yet CP-1 cost no more than one million dollars, an amount available to the Germans for the asking.[9] Nor did Germany lack the industrial sophistication or the desire: Nuclear weapons are mentioned enthusiastically in the project documents again and again.

Why then did the project get no further? Goudsmit emphasized four factors:

1. That the Germans, mistakenly believing themselves far ahead of the Allies, felt no competitive urgency.
2. That Nazi political control and interference burdened the project in a variety of ways.
3. That the uranium project physicists decisively overestimated the difficulty of the task (because they failed to appreciate fully the plutonium alternative; because, as Goudsmit initially thought, they conceived of bombs as depending on slow-neutron reactions; and because the academically oriented theoreticians lacked aptitude and enthusiasm for industrial undertakings, even on the scale of cyclotrons).
4. That wartime conditions worked against the project.

Walker allows only the last of these, with a passing acknowledgment of the first. The project administrators, as he sees it, chose with admirable correctness to forgo an industrial-scale weapons program as incompatible with wartime priorities. And so, Walker says, a considered decision was taken in 1942 not to press forward. ("This was the final verdict, which never was reassessed.") Walker's principal evidence for this remarkable conclusion, a single, anonymous Army Ordnance Office report, suggests an extraordinary faith in committee organization charts. Certainly, the Allied enterprise followed no such orderly course, as Richard Rhodes's vivid account makes particularly clear.[10] Powerful persuasion from the Allied physicists themselves time and again overturned the cautious positions of government officials formally vested with power. Not least for such reasons, surely, did Heisenberg stress the importance of his meeting with the armaments minister, Albert Speer, whose considerable power—to provide funds, priorities, influence—lay beyond any Ordnance Office committee's "final verdict."

That political interference impeded the project Walker dismisses as so much prejudice (on Goudsmit's side) or self-serving misrepresentation (on Heisenberg's). The uranium project administrators were "professionally respectable physicists," Walker insists, not political hacks. Erich Schumann, the head of army ordnance research, was "qualified to teach physics at the university level," he tells us, but he doesn't mention Schumann's specialty (the physics of piano strings), his title (professor of military physics) or the mock title that suggests his standing with the project physicists themselves—"the professor of military music." If the senior administration (with the isolated exception of Walther Gerlach, at the end of the war) was distinguished in more than political guile, Walker has not given us evidence for it. The six talented physicists he lists as "involved in the scientific work and the administration" include only one (Gerlach) with authority beyond laboratory level. Goudsmit's opinion that

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such figures as Schumann, Rudolf Mentzel and Bernhard Rust were scientifically incompetent political men is hardly belied by their credentials. Rust, Hitler's minister of education, was "scientifically illiterate," in Rhodes's words. His subordinate Mentzel, the chief of all research in German universities, has not in any account other than Walker's been described as a capable scientist. Both, however, held high honorary ranks in the SS (*Obergrüppenführer* and *Brigadeführer*, respectively); Schumann was a Wehrmacht general.

One point that no reader of *Alsos* could miss, in some three dozen pages dealing with these men, is Goudsmit's own low opinion of four particular political administrators and his reasons for it (though "nincompoops," the word he used for two of them, was edited out of the book's page proofs by the publisher). *Alsos* makes the important point that the Gestapo, too, held critical reports on several of these men. Walker, however, telescopes the entire picture into a single, completely misleading sentence: "Using Gestapo records that he himself considered suspect, Goudsmit unfairly dismissed Schumann and other National Socialist science policy administrators as incompetent. . . ."

For one to understand the Heisenberg–Goudsmit letters, some of the record omitted by Walker needs to be restored: the letters themselves, and the conversations of the German physicists interned at Farm Hall, secretly taped by British intelligence.[11–13] (Key portions of the transcripts, which are still classified, were reproduced in *Alsos*, though secrecy restrictions forced Goudsmit to do this with a coy device that did not do justice to the certainty of his evidence.[11] Walker ignores the actual conversations in favor of the memorandum prepared by the Germans for public release.) The Farm Hall transcripts confirm three points of interest here: first, that the German physicists conceived the construction of atomic weapons to be vastly difficult—far beyond even the combined resources of the Allied nations; second, that such basic questions as the critical mass for a uranium bomb had not, by war's end, been settled in Germany; third, that von Weizsäcker and others planned an artful reinterpretation of their embarrassingly slight showing—namely, that the physicists had chosen, on principle, not to pursue atomic weapons.[11,12] Heisenberg, at the time *Alsos* was written, still endorsed this comforting fiction, and it did not escape Goudsmit's notice that the point on which the Heisenberg letters so strongly insisted—that the physicists had well understood how to create fission bombs—was the very premise of von Weizsäcker's invention. Goudsmit thus had reason to question whether the physicists really had understood fission bombs and plutonium, and to view Heisenberg's later protests to the contrary with a certain wariness.

Heisenberg's letter of 5 January 1948 conveys his evidence that the German physicists understood fast-neutron fission weapons and plutonium breeding, but it is evidence of an oddly thin, inferential kind. Among the wartime progress reports, he points out, is a speculation (by Walther Bothe) that protactinium might support an explosive fast-neutron chain reaction. As proof that such reactions had also been contemplated in uranium, Heisenberg reproduces from memory a slide on which he had illustrated the neutron multiplication to be expected in a large mass of pure U-235. The slide itself, from a 1942 lecture to Luftwaffe officials "adapted to the intelligence level of a Reich Minister of that time," he presumes lost. Lastly, as evidence that the plutonium alternative had been appreciated, Heisenberg cites a 1940 report in which von Weizsäcker reasoned that slow-neutron capture by U-238 in a natural-uranium-fueled reactor should produce the transuranic element neptunium (Eka Re-239, as it was then called), with expected fission properties similar to those of U-235. That Heisenberg's evidence consisted of no more than this—an open-ended speculation about an impractically rare element; a theoretical sketch on a single, lost lecture slide; and a plausible conjecture—could only have encouraged Goudsmit's doubts.

Heisenberg, we might surmise, felt entitled to be taken at his word, yet Goudsmit was less concerned with what Heisenberg knew than with the common currency available to the uranium project. Why, for example, to the thousand-plus experiments undertaken by the Allies to investigate plutonium, had Germany done none—not even the vital cross-section and yield measurements needed to confirm or exclude the plutonium option, measurements Emilio Segrè and Glenn Seaborg had done with cyclotron-generated microsamples in May 1941? That Germany had been slow to acquire cyclotrons was, as Goudsmit observed, more a statement of the problem than an explanation of it. Von Weizsäcker's 1940 speculation on plutonium was noteworthy, yet Louis Turner, at Princeton, had outlined the idea (correctly identifying plutonium rather than neptunium as the fissile end product of U-238 neutron capture) at about the same time.[14] For Turner, however, such bare speculation was so far from real knowledge as hardly to warrant his withholding it from publication in the *Physical Review* on secrecy grounds. "It seems as if it was wild enough speculation so that it could do no possible harm," he had written to Leo Szilard.

Alsos, according to Walker, is unreliable because after Goudsmit's parents were killed at Auschwitz he "no longer was completely objective." The insupportable implication here, that the word of the oppressors is intrinsically more, rather than less, reliable than the word of their victims, was addressed in the very *Bulletin of the Atomic Scientists* article (April 1948) Walker examines at length. As Walker renders it, Philip Morrison's "Reply to Dr. von Laue" seems an apologetic qualification of his earlier *Bulletin* statement attacking Heisenberg's claim that the Germans had not tried to create nuclear weapons. Morrison's actual words, however, are quite different:

> I am of the opinion that it is not Professor Goudsmit who cannot be unbiased, not he who most surely should feel an unutterable pain when the word Auschwitz is mentioned, but many a famous German physicist in Göttingen today, who could live for a decade in the Third Reich, and never once risk his position of comfort and authority in real opposition to the men who could build that infamous place of death.

Goudsmit's views are "oversimplified," Walker tells us, "deeply based in scientific stereotype" or "the result of sloppy research." To judge by what can actually be checked against the documents, however, the confusion, exaggeration and distortion here are Mark Walker's own contribution to the record.

References

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JONOTHAN L. LOGAN
Westford, Vermont

11/90

Mark Walker states that "contrary to accounts based on Heisenberg's claims, the German fission research effort in World War II was indeed a nuclear weapons program." I contradict this statement on the basis of the *same* documents Walker used: formerly secret reports[1] on the German nuclear fission research of World War II (henceforth called the German uranium project) kept at the Kernforschungszentrum in Karlsruhe, as well as letters and documents in the Werner-Heisenberg-Archiv, Munich.

To decide this question one should first recall very briefly the historical facts. The German uranium project was started—after some preliminaries—in September 1939 by the Heereswaffenamt (Army Ordnance Office). The following problems were presented to the scientific experts assembled at the inaugural meeting:

> It is the task of the participants to work out all preparatory steps in order to answer uniquely the question of whether nuclear energy can be produced on a technical scale. Of course, it would be very nice if the answer turned out to be positive and if one succeeded in opening a new source of energy. This would very probably also have military importance. A negative result, however, would be likewise important, since one could then be certain that the enemy has no access to it [that is, nuclear energy] either.[2]

The research carried out up to early 1942 did indeed nearly answer these questions. Theoretical and experimental work made it apparent that a self-sustaining nuclear chain reaction could very likely be established in a machine (reactor) containing natural uranium as fuel and heavy water as moderator; also, the creation of nuclear weapons seemed to be feasible in principle by proceeding along either of two paths: production of a suitable amount (tens of kilograms) of U-235 by isotope separation, or breeding enough fissionable transuranium material in an already functioning uranium machine.[3] The scientists reporting to the German authorities (the Heereswaffenamt and Reichsforschungsrat) in February 1942 and later also declared that a "nuclear explosive" (*Kernsprengstoff*) would not be available without several years of enormous technical, industrial and financial effort. At that time the Heereswaffenamt retired from the uranium project, which continued, however, to be a secret project rated "important for the war" (*kriegswichtig*). Further experiments were carried out to achieve a critical reactor, on the one hand, and isotope separation (of uranium and hydrogen), on the other hand. In spite of some progress, at the end of the war (May 1945) neither a functioning reactor nor larger amounts of U-235 existed in Germany. (It might be mentioned that the isotope separation effort was directed toward getting material for a smaller uranium machine or a machine running with light water as moderator.)

Careful study of the material documenting the above story does not uncover any serious work, theoretical or experimental, on a "nuclear weapon," not even during the time when the German uranium project was supervised by the Heereswaffenamt, a military authority. True, the possibility of such weapons was mentioned occasionally, more or less in passing, in some of the reports. (Only one of the roughly 150 reports submitted through the end of February 1942 dealt explicitly with "the requirement for the utilization of uranium as an explosive"—Paul O. Müller's sketchy report of six pages, dated 31 May 1940. Müller proposed to use a mixture of water and uranium oxide in which the isotope U-235 was enriched—anything but an efficient explosive.) It is also true that in the beginning of 1942, when the project was given up by the Heereswaffenamt and the danger arose that it might be dropped altogether, the scientists involved tried to rescue their research by emphasizing that it was *kriegswichtig*—because otherwise they would not obtain the required funds, nor the necessary materials (uranium, heavy water, steel and rare metals), nor the junior scientists and assistant scientific personnel (freed from military service) needed for the work. Their reports and talks stressed any possible military use of nuclear energy, be it for machines propelling tanks and submarines or for explosives. No action or work followed from these words; nevertheless the state authorities (including Albert Speer's ministry for war production) kept the project alive. Hence I do not see any justification for calling the German uranium project a "nuclear weapons program."

Let me finally mention a further weak point in Walker's argument. In discussing the details of a 1946 review of the German uranium project by Werner Heisenberg—a decent English translation appeared in *Nature*[4]—Walker claims that "when this review is compared with sources documenting the history of the German nuclear fission project, several important discrepancies emerge." Whatever one thinks about these discrepancies—I have some trouble discovering any in the examples mentioned by Walker—one must always keep in mind that any report given some years later on an extended project will strongly reflect the personal recollections and opinions of the writer; it certainly cannot yield a detailed, "document proof" account of the historical events (especially if the documents were, as in this case, not available to the writer). Instead of accusing Heisenberg personally of any inaccuracies, Walker should have scolded those historians who base their reconstructions of the whole story on a report by a single actor in the game.

References

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HELMUT RECHENBERG
Max-Planck-Institut für Physik und Astrophysik
Munich, Germany

2/90

Mark Walker's conclusions that Samuel Goudsmit "clung tenaciously to mistaken ideas," that his approach was "profoundly ahistorical and noncontextual" and that Werner Heisenberg played a relatively minor role in the German nuclear effort are controversial, misleading and at best only partially correct.

Since the following comments have a distinctly personal flavor, it is proper to explain my own direct involvement in the events of the times. Like Goudsmit, I was born and educated in the Netherlands. I came to the United States to complete my PhD with Enrico Fermi, who had just emigrated to America. Fermi was scheduled to lecture in Ann Arbor the summer of 1939 (on cosmic rays), and I came to Ann Arbor a few months before the start of the summer session. While there, I met Goudsmit, Otto Laporte and of course Fermi, as well as a number of visitors to the summer symposium. All during the summer discussions focused on cosmic rays and nuclear physics, with a comparable amount of time (and intensity) spent on the turbulent and frightening political events of that summer. Heisenberg visited for a week late in July and stayed at Goudsmit's home. He left to return to Germany early in August. I had met Heisenberg before, in Hendrik Kramers's seminar in Leiden, and I saw quite a bit of him during his visit to Ann Arbor. I also saw a lot of Goudsmit while at Michigan. And well after the war, from 1964 on, when I was a professor at the State University of New York at Stony Brook, I saw Goudsmit, who was at Brookhaven, very frequently.

Like Goudsmit's family, my parents, many members of my family and many friends were exterminated in the Holocaust. Goudsmit and I often discussed our fluctuating respective reactions of anger and guilt—and our fear and even terror of the possibility of a renewed wave of barbarism (anywhere in the world). But painful as these reactions may be, to dismiss Goudsmit's conclusions concerning the successes and failures of German and American science on the grounds that he could not be objective because of his personal experiences is totally unjustified and ignores the intellectual integrity of Goudsmit (and myself). This of course doesn't mean Goudsmit's analysis is correct. Emotions do affect attitudes; they influence the assignment of personal guilt and personal responsibility; but they do not invalidate an argument and do not alter the facts. Goudsmit's arguments deserve to be discussed and analyzed on their own merits, not ignored in a cavalier fashion, as Walker does.

Perhaps the most serious objection to Walker's article is that he totally fails to place the Heisenberg–Goudsmit confrontation in the proper scientific and personal context. The relationship between Heisenberg and Goudsmit involved three distinct elements. The background of their earlier personal and scientific interactions, the changing relations between American and European physics, and their widely differing political opinions were all crucial ingredients in their angry exchanges.

In 1925 both Goudsmit and Heisenberg were members of the brilliant new generation of quantum physicists. It was widely expected that both would make major contributions to quantum theory. Both of them surely did. But after about three or four years, atomic spectroscopy, the field of Goudsmit's special expertise, became less central in physics, and his contributions to physics started to diminish and become less basic. Heisenberg, by contrast, continued (at least for some time) his brilliant exploits. Right after Goudsmit discovered the electron spin, Niels Bohr invited him to come to Copenhagen to study the problem of *ortho* and *para* helium. Goudsmit went, but made no progress whatsoever and returned a little disillusioned to Leiden. Heisenberg followed Goudsmit to Copenhagen and solved the helium problem completely. It was that very achievement that was mentioned in his Nobel Prize citation. Goudsmit often mentioned that episode. He stated on numerous occasions, "Heisenberg's solution was way beyond me." It is hard to know the effect of a single incident, but this one must have had a substantial impact. It is certain that by 1939, Goudsmit felt that physics had passed him by. He was disappointed about his contributions to physics, and he had severe doubts that he was capable of understanding, let alone contributing, to the then current physics. (Heisenberg never had such doubts.)

Goudsmit's disappointments made him at times depressed, often angry and always cynical. These cynical attitudes, combined with strong anti-Nazi feelings, caused him to be abrasive. He worried incessantly about the future of Europe. Goudsmit was not particularly interested in politics, but his tendencies were liberal rather than conservative, international rather than national. By contrast, Heisenberg was a strong German patriot, a true believer in Germany's historic destiny. He often said during the war that he hoped that Germany would win. Although hardly surprising, this hope was totally unacceptable to Goudsmit.

Yet another source of tension had to do with Goudsmit's and Heisenberg's shared belief that as leading members of the international physical community, they were expected to meet certain standards of behavior, intellectual integrity, personal compassion and individual accountability. Goudsmit felt this was incompatible with an allegiance to Nazi Germany. He thus felt that Heisenberg had not lived up to these standards. By the same token, Heisenberg argued that those who had not been subject to the insidious pressure of a ruthless totalitarian regime had no right to sit in judgment of those who had suffered through it.

A third area of conflict was the shift of the center of theoretical physics from Europe to the United States. As beautifully analyzed by Samuel Schweber,[1] by the middle of the 1930s, American physics, helped by a large influx of foreign physicists, had evolved into a powerful independent discipline with a style and approach all its own, combining the abstract, theoretical European approach with the more direct, pragmatic American methodology. This American approach was particularly successful during (and after) World War II. The resulting shift, accelerated by the deterioration of European physics, was difficult to accept for Heisenberg and for many others (Wolfgang Pauli, Kramers, Carl-Friedrich von Weizsäcker). In fact they never did fully accept it.

Goudsmit understood better than Heisenberg that the change of the scientific hegemony from Europe to the US was an important element in their personal conflict. That is why Goudsmit was so irritated by the automatic assumption of German superiority and was outraged at Heisenberg's suggestion that he would be willing to lecture on the "uranium problem" to the American physicists (including Fermi, Eugene Wigner, J. Robert Oppenheimer and Hans Bethe—the very people who had built a bomb and constructed a pile).

In early August 1939, while Heisenberg was staying with Goudsmit, Laporte, another old friend of Heisenberg's, gave a party for him. I and a few other graduate students were asked to function as bartenders and

waiters. There was actually not much to do, so we could pay close attention to the conversations. There was really only one central topic. Fermi had just left Fascist Italy to come to the US; Heisenberg had decided to return to Nazi Germany. The crucial part of their argument was whether a decent, honest scientist could function and maintain his scientific integrity and personal self-respect in a country where all standards of decency and humanity had been suspended. Heisenberg believed that with his prestige, reputation and known loyalty to Germany, he could influence and perhaps even guide the government in more rational channels. Fermi believed no such thing. He kept on saying: "These people [the Fascists] have no principles; they will kill anybody who might be a threat—and they won't think twice about it. You have only the influence they grant you." Heisenberg didn't believe the situation was that bad. I believe it was Laporte who asked what Heisenberg would do in case of a Nazi–Soviet pact. Heisenberg was totally unwilling to entertain that possibility: "No patriotic German would ever consider that option." The discussion continued for a long time without resolution. Heisenberg felt Germany needed him, that it was his obligation to go back. Fermi did not think there was anything anyone could do in Italy (or Europe); he was afraid for the life of his wife (her father was later killed); and so he felt it was better to make a fresh start in the US. But none of the decisions had come easy. The role of physics and physicists was mentioned off and on.

After the party was over everybody left in a state of apprehension and depression. Although there was no clear anticipation of the turbulent events to come, it was evident that theoretical physicists would no longer be a happy, unconcerned group of brilliant young men matching their intelligence against the secrets of the universe.

Reference

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MAX DRESDEN
Stanford Linear Accelerator Center
8/90 *Stanford, California*

Mark Walker's interesting article brought me back to September 1944, when Samuel Goudsmit's intelligence team visited the physics lab of the N. V. Philips Gloeilampen Fabrieken in Eindhoven, the Netherlands, right after Eindhoven was liberated.

I had joined the Philips labs in Eindhoven as a research physicist, working on vacuum tube electronics and noise problems, after obtaining my PhD degree in 1934. Before that I had studied experimental physics at the University of Groningen, the Netherlands, from 1928 to 1934. My physics professors were Dirk Coster (experimental physics), Frits Zernike (theoretical physics) and Ralph de Laer Kronig (wave mechanics), and my mathematics professors were J. G. van der Corput (analysis) and B. L. van der Waerden (linear algebra).

Coster was very active in helping the Jews. In 1939 he had traveled to Berlin and led Lise Meitner to safety. He also had pleaded in vain with the German authorities to release Goudsmit's elderly parents from the concentration camp and spare their lives.

Goudsmit's intelligence team consisted of several groups. In hindsight the most important was the nuclear physics group. It had to inquire about German atomic bomb development without revealing Allied progress in the field. I showed the electronics group some German crystal diode detectors with 5-cm half-wave antennas. They were much surprised, for it was their first concrete evidence that the Germans were working on 10-cm radar. A third group worked with the commercial department to detect large shipments of vacuum tubes to German locations that were not on the Allied intelligence list. Some time earlier an alert commercial administrator had noted huge shipments of vacuum tubes to an obscure village called Peenemünde and alerted Allied intelligence via the underground. Allied bombers bombed Peenemünde heavily, retarding the German rocket program substantially.

During one of the breaks Goudsmit took me aside and asked me what I knew about his parents. I told him of Coster's efforts—that he had not succeeded in having Goudsmit's parents released and that therefore the worst had to be feared. Later it turned out that they had died in Auschwitz. It was difficult for me to be the bearer of such sad tidings.

Walker mentions that Heisenberg also intervened with the German authorities on behalf of Goudsmit's parents. The question is now whether Coster's and Heisenberg's intercessions were isolated events or part of a larger effort. Knowing Coster, and bearing in mind that he knew practically all the important German physicists, I believe the latter.

Van der Waerden played a very active role in the early years of wave mechanics. He must therefore have known Heisenberg since 1925. After 1930 they were colleagues in Leipzig, though it is not known how much they interacted during that time. Either fact makes it understandable why van der Waerden came to Heisenberg's defense after the war.

Our research lab did not suffer from German interference, but after 1 January 1944 we had to work on German research contracts. Slowdown tactics were successfully employed at first, but it was soon clear that this was not a long-term solution. Much stricter rules were announced in early September 1944, with the threat of death penalties. But on the same day Antwerp fell, and two weeks later Eindhoven was liberated. Our ordeal was over.

There is not enough understanding about what it means to work under an unfriendly totalitarian regime. Your options are quite limited. Outright refusal leads to prison or execution. Slowdown techniques are possible but cannot become obvious. I therefore have considerable sympathy for the positions taken by van der Waerden, Heisenberg and especially Max von Laue.

A. VAN DER ZIEL
University of Minnesota
2/90 *Minneapolis, Minnesota*

WALKER REPLIES: I am grateful to the editors of PHYSICS TODAY for an opportunity to respond to my critics. First of all, I would like to direct all interested persons to my book[1] and to my supplementary article on the postwar controversy surrounding the "German atomic bomb."[2] My article in PHYSICS TODAY, like all articles that appear there, was restricted in regard to length and the number of footnote references. The issues under discussion are complex, and I recognize that "Heisenberg, Goudsmit and the German Atomic Bomb" does not and could not have done them complete justice. Taken together, my book and supplementary article not only provide extensive references for the evidence I present but also place the postwar controversy between Samuel Goudsmit and Werner Heisenberg in the proper context. I will respond to my critics by referring to my book and that article, not merely to my publication in PHYSICS TODAY.

I consider Jonothan Logan's criticism of me to be unfair. Rather than my work being "strange," "extravagant," a "caricature," "misleading," "confused," "exaggerated" and "distorted," I have provided a fair and objective account of what happened and why. For the sake of brevity, I will respond to the most important of

Logan's assertions in the order they appear in his letter.

Goudsmit's conclusion that the Germans overestimated the difficulty of making atomic bombs rested on the assumption that the Germans believed that such a bomb would require tons of uranium. This assumption is false, as I have demonstrated in my book, especially on page 48. The postwar statements by Heisenberg and Carl-Friedrich von Weizsäcker cited by Logan refer to the great costs of the industrial production of nuclear weapons and were written at a time when German physicists were concerned with explaining why they had not made more progress toward their goals.

Logan writes that Goudsmit did acknowledge that these German scientists had known about fast-fission uranium bombs and plutonium breeding, and he refers the reader to a letter Goudsmit published in *The New York Times* in 1949 and to two publications from 1976. Goudsmit did recant at the very end of his life, but that in no way contradicts my argument that he had not in the period covered by my book. When I reread Goudsmit's 1949 letter, however, I saw the following passage, which can hardly be reconciled with Logan's account: "Finally, the German physicists also missed the crucial point that a bomb is a reaction produced by fast neutrons in plutonium or in U-235. Fast neutrons are mentioned by them only in the hope that they might perhaps produce a chain reaction in the abundant isotope U-238." This assertion by Goudsmit is false.

I do not understand Logan's distinction between writing a letter and answering a plea. As I understand the English language, it is possible both to respond to a plea and to write a letter. I stand by my characterization of Goudsmit's account of the Alsos mission as "heroic."

Goudsmit repeatedly portrayed the German conception of an atomic bomb incorrectly, for example, in *The Bulletin of the Atomic Scientists*: "The German line of thought was as follows.... An atomic bomb is an uranium engine which gets out of control.... To make a bomb of pure plutonium never entered their minds, or at least was not considered feasible and not taken seriously. The idea of using a pile to produce plutonium and to make a bomb out of that material came to them only slowly, after the detailed radio descriptions of our bomb in August 1945."[3]

I do not rely on a "single, anonymous Army Ordnance Office report," as Logan claims. This report contains a bibliography of the 134 scientific and technical reports on which it was based. I not only read through each and every one of those 134 reports; I also researched thoroughly the dozens of letters exchanged between scientists and Army Ordnance in regard to this research.

Arguably it is Logan's criticism of my portrayal of Erich Schumann that is most typical and most revealing of his approach to history. There is no reason to doubt that Goudsmit told Logan that Schumann was incompetent, and I am willing to believe that Goudsmit believed this himself. But it is not true. I have seen Schumann's personnel file at the archives of the Humboldt University in Berlin, and these documents provide a very different picture. Schumann received his *Habilitation* (the right to teach) in 1929 at the University of Berlin—arguably the most prestigious teaching institution for physics in the world at the time—in "Systematic Musical Science," which included acoustics, an area of physics. His command of acoustics was examined and approved at that time by (among others) Max Planck, Max von Laue and Walther Nernst. Schumann received the *Venia Legendi* (the right to teach an additional subject) for the entire discipline of experimental and theoretical physics in 1931. His command of physics was examined and approved at that time by (among others) Nernst, von Laue and Erwin Schrödinger. But Adolf Hitler did not come to power until January of 1933. Schumann taught physics in Berlin from 1929 until the Second World War forced him to devote all his time to administration. Moreover, all the courses he taught in the Third Reich, including classes with titles like "Military Science," he had already taught in 1931. Does the fact that Schumann was interested in musicology necessarily mean that he was a bad physicist? Schumann was a ruthless and unscrupulous administrator, he was as convinced a National Socialist as they came, and he had no qualms about using science to create weapons with previously unknown destructive capacities, as proven by his sponsorship as an administrator in Army Ordnance of the rocket research in Peenemünde. But he was not incompetent.

Logan's reference to the so-called Farm Hall tapes is irrelevant. First of all, these recorded conversations, if they exist, have not been released. Thus the brief excerpts that some have claimed are genuine cannot be checked for accuracy. Second, and what is far more important, the portrayal these excerpts suggest, that these German scientists did not understand how an atomic bomb would work, is contradicted by evidence that they understood very well by 1942 how to make such a bomb in principle, and it is unlikely that they would have forgotten this knowledge in the meantime.

Logan argues that Heisenberg's postwar letters to Goudsmit are speculative, thereby implying that Goudsmit was correct to be skeptical of his German colleague's claims. But Logan has unaccountably refrained from mentioning the other evidence I present in my book on page 218. (Since Logan cites my book in his first footnote, I assume that he took the time to read it thoroughly and carefully before criticizing me.) Bartel van der Waerden visited Goudsmit and subsequently wrote Heisenberg and told him that he had seen documents in Goudsmit's office that verified Heisenberg's claims about what the Germans had known about plutonium and nuclear weapons.

Finally, it is unfortunate that Logan has accused me of putting words into Philip Morrison's mouth, for my account of what Morrison published is fair. But what I consider to be especially unjust is for both Logan and Max Dresden to accuse me of arguing that Goudsmit's claims should be rejected *because* he had suffered at the hands of Germans and therefore was no longer completely objective. I have never written or said any such thing. Goudsmit's claims about the quality of the German research on nuclear power during World War II are *objectively* false, as anyone who examines the reports the German scientists composed during the war can see. I mentioned Goudsmit's loss of objectivity to suggest an explanation for why Goudsmit not only made false claims about the German work but also refused to correct those claims in public even after evidence had been presented to him that demonstrated that he had been incorrect. My book deals with a controversial topic, and I expect to receive criticism, but no other critics have so gravely misconstrued my words and intent.

I also believe that Dresden's criticism of me is unfair. My conclusions are not "misleading" or partially incorrect, and my work is not "cavalier," as I believe Dresden might see if he were to take the time to read my book thoroughly and carefully. Similarly, I believe that a reader who examined the 204 pages in my book that precede my discussion of the controversy between Goudsmit and Heisenberg would see that I have

tried very hard to place this debate in the proper context. The history and anecdotes that Dresden relates in his letter are interesting but not relevant for the issues raised in my article.

I am grateful to Helmut Rechenberg for his tacit willingness to "agree to disagree." As far as I can see, our difference of opinion can be summed up in the following question: Did the German scientists *try* to make nuclear weapons during the Second World War? But this question has no one answer. It depends on what one means by "try." If trying to make nuclear weapons means making the massive industrial efforts, spending the billions of marks, employing the thousands of scientists and engineers, and building the factories that were all obviously needed to manufacture nuclear weapons, then the Germans did not try. However, if trying to make nuclear weapons means making efforts to produce known nuclear explosives—plutonium and uranium-235—in steadily increasing amounts as quickly as possible without interfering with the war effort, then the Germans did try. In my book, I tried to leave this question open, so that each reader could decide for him- or herself which interpretation is justified. In my condensed article in PHYSICS TODAY, this discussion unavoidably was simplified.

Finally, I would like to say that I agree completely with A. van der Ziel. I have considerable sympathy for individuals who have to work and live under any totalitarian regime, and I have tried very hard to express this sympathy in my work.

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MARK WALKER
Union College
3/91 *Schenectady, New York*

Unemployment Rates and Reactions

After reading Leon Lederman's reply (October 1990, page 122) to the critics of his Reference Frame column "Low Pay and Long Hours" (January 1990, page 9), I feel compelled to comment about his optimism regarding future and current employment opportunities for physicists.

I have strong reservations about the statistics Lederman uses to support his claims. I have worked in industry for nearly 20 years, and the salary surveys and manpower projections of AIP and NSF seem more optimistic than experience warrants. The positive projections may be self-serving, since negative results can cause funding problems for the NSF and for members of AIP member societies. Further, using unemployment rates supplied by the government to argue increased employment of any group is extremely questionable. The rates are based on the number of persons currently collecting unemployment insurance. A person who exhausts his insurance is dropped from the ranks of the unemployed. A person who doesn't register or who doesn't qualify for unemployment insurance isn't even counted.

Lederman claims that "to first order, there was 100% unemployment" during the Great Depression. The *World Book Encyclopedia* says that at the height of the Depression unemployment was at 13 million, or 25% of the work force. Using this method of "first order" estimation the current unemployment rate is also 100%.

Lederman's belief that there will always be work for physicists because the world needs technology to solve its ever growing problems is unduly hopeful. The problems of pollution and diminishing resources are not new, yet the plight of the unemployed physicist is growing. The only thing clear is that there are fewer jobs for physicists because there is less economic need for physicists.

The trend of American business is increasingly toward short-term goals. I have witnessed many companies once involved with R&D drop it, continue to reduce staffing or go out of business. My own career is a testament to the pursuit of new employment due to reduced staffing or the elimination of R&D.

The solution to this crisis is political. Economic incentives must be created to make it profitable for American business to increase the priority of long-term goals. The APS should be trying to convince legislators to provide these incentives. The current commitment by the APS to promote science education is almost folly given the declining demand for physicists.

It is unlikely that there will be in the foreseeable future a demand for physicists comparable to that of the 1960s. There are, however, things you can do to get through the difficult times:

▷ Contact your local unemployment office; you may be eligible for benefits.
▷ Do not expect job advertisements to be what they appear. Many companies advertise because of corporate policy, but most positions are filled through professional recruiters (also called headhunters). So find yourself a good headhunter. His fee is paid by the hiring company. He will advise you on how to write a resume and conduct an interview.
▷ Use as many contacts as possible to learn of openings or gain access to those hiring. The more influential the contacts, the better. Many job openings are filled before they get listed; the listings are often pro forma.
▷ Be open to changing your direction. The ability to carry on thesis work for a lifetime is seldom an option when you need to survive. Most physicists have many marketable skills, such as the ability to do advanced engineering and project management, and many jobs held by physicists have corporate engineering and management titles. So be sure to list engineering skills in your resume, particularly if you include smaller corporations in your job search. Smaller companies often need their technical staff to perform a variety of functions.
▷ Consider being a consultant. If you have many contacts then try to consult on your own; otherwise sign up with consulting firms. The problem with consulting is that you are self-employed and work is irregular.
▷ Write a book. A well-prepared prospectus demonstrating that your book is marketable is indispensable in getting your book published.
▷ Should you find suitable employment, assume it will not be permanent. That is, plan for the unexpected. Try to set aside money in safe income-producing investments as if you were planning your own retirement fund. You may need that income when you are between jobs.

Finally, do not expect too much useful help from the APS or AIP. As long as officers are elected on the basis of awards, publications, committee memberships and name recognition, I don't think you will hear them make realistic proposals for improving your employment or economic condition.

MURRAY ARNOW
10/90 *Skokie, Illinois*

THE EXECUTIVE DIRECTOR OF AIP REPLIES: The American Institute of Physics is widely known and respect-