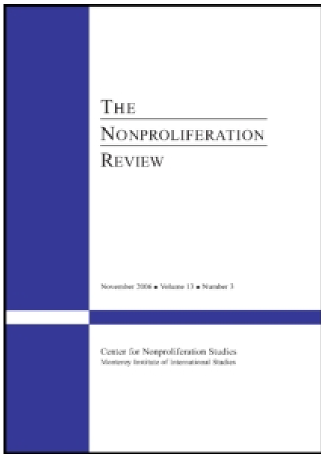

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THE REAL STORY BEHIND THE MAKING OF THE FRENCH HYDROGEN BOMB
Chaotic, Unsupported, but Successful

Pierre Billaud and **Venance Journé**

Based on the first-person account of coauthor Pierre Billaud, a prominent French participant, this article describes for the first time in such detail the history of the development of the French hydrogen bomb in the 1960s and the organization of military nuclear research in France. The authors illustrate the extent to which French defense and governmental authorities did not support research on thermonuclear weapons until 1966. Billaud, a project insider, relates the historical episodes that led to France's successful 1968 thermonuclear test, including the names of the individuals involved and how a timely tip from a foreign source hastened the success of the first H-bomb test.

KEYWORDS: France; nuclear weapon; thermonuclear; hydrogen bomb

The first successful French nuclear test—code-named *Gerboise Bleue*, with a 65-kiloton yield, four times that of the Hiroshima bomb—occurred on February 13, 1960, in the Sahara desert.[1] The scientists working for the Direction des applications militaires (DAM), the military applications department of the French Atomic Energy Commission (Commissariat à l'énergie atomique, or CEA), had no doubts about continuing to the next step, the hydrogen bomb. Everyone in France and many people abroad assumed that France would swiftly reach the thermonuclear level.

After all, three other nuclear powers had already developed thermonuclear devices—and relatively quickly, too. On November 1, 1952, the United States conducted its first thermonuclear test, "Ivy Mike," seven years and three and a half months after its Trinity test. It took the Soviet Union four years (August 29, 1949–August 12, 1953) and the United Kingdom four years and seven months (October 3, 1952–May 15, 1957) to achieve thermonuclear capacity. And in the following decade, China did it, with its sixth test, in fewer than three years (October 16, 1964–June 17, 1967). Yet after *Gerboise Bleue* it took France eight and a half years to reach the same landmark, detonating its first thermonuclear device on August 24, 1968. Why such a long delay, especially since the French were pioneers in nuclear research? (In early May 1939, Frédéric Joliot-Curie, Hans Halban, Lew Kowarski, and Francis Perrin had registered in secret three patents, including the first ever on the chain reaction in uranium and another for a "method for perfecting explosive charges.")

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This account relates the episodes of the development of the first French thermonuclear device and illustrates how France's H-bomb program suffered from a lack of support from French authorities. It also explains how and by whom the technical solution was found in 1967.

The Organization of Nuclear Weapons Research in France

Initially, the French military nuclear program proceeded in secret. The Fourth Republic (1946–1958) was the time of colonial wars in Indochina and Algeria, and many decision makers did not favor embarking on a long-term program that would require considerable financial and human resources. Moreover, domestic conditions were unfavorable as governments were changing at a fast pace; the political climate was very unstable, and apart from a few rare exceptions—such as mathematician and astrophysicist Yves Rocard—scientists were opposed to a French nuclear weapon. Internationally, the United States was against an independent French nuclear deterrent, and disarmament treaties under discussion were increasing the pressure to limit, or even renounce, nuclear testing. Nevertheless, French authorities proceeded to establish all the necessary infrastructure for a nuclear program.

Although the CEA had been created in 1945 with, among other tasks, the specific purpose of developing nuclear weapons, the nuclear military program really started in the early 1950s. The CEA had a unique status with an unusual level of autonomy, enabling it to maintain a continuity of views and action. It was directly under the authority of the Président du Conseil, whereas the funds for the military activities were under the authority of the Defense Ministry. The Office of General Studies (Bureau d'études générales, BEG) was created inside CEA in 1954 with Colonel Albert Buchalet as its head, and I (coauthor Pierre Billaud) joined that year. It was a tiny office with only five employees, who were in charge of implementing the technical means to prepare the studies for the first atomic bomb test.

One of the first tasks for the BEG was to choose suitable sites near Paris to establish the necessary research facilities. The two main departments interested in nuclear military activities were the Defense Ministry's Department of Studies and Manufacture of Armaments (Direction des études et fabrications d'armement, DEFA), and the Explosives Department (Service des Poudres). The Explosives Department realized early on, in 1950 and perhaps even earlier, the value of studying the pyrotechnic processes for triggering a nuclear explosion and other possible areas of chemistry or physical chemistry related to applications of atomic energy. The department was eager to collaborate with the CEA. On the other hand, DEFA had always coveted the technical responsibility for the development of nuclear weapons, and as a result it was in direct competition, or even confrontation, with the CEA. On May 20, 1955, Defense Minister General Pierre Koenig and Gaston Palewski, the state secretary in charge of CEA, signed a memorandum of understanding that explicitly gave the CEA the responsibility for the development of nuclear weapons.[2]

In order to reduce the risks of a dangerous accident in the vicinity of the capital, Colonel Buchalet created two large research and manufacturing centers in the Paris region, located far away from each other, with one devoted to pyrotechnic research and the other to nuclear research. The Vaujours Research Center (northeast of Paris) was created first, to work on the explosive necessary for the manufacture of nuclear devices. In addition to a head office and administration, it had three technical departments: theory, physics, and devices and explosives. It was followed soon after by the Bruyères-le-Chatel Research Center (also known as B3), where research on nuclear physics, metallurgy, and nuclear chemistry were carried out in order to prepare the experimental devices. The departments in this center were: experimental nuclear physics (where I was the director), mathematical physics, electronics, metallurgy, and chemistry, along with such necessary support activities as drawing and mechanical fabrication.

Buchalet quickly realized that more sites would be necessary. For one thing, the quantity of chemical explosives necessary to trigger implosions, which would have to be tested at full scale, was too large to be stockpiled near Paris. Similarly, it was undesirable to keep the necessary quantity of plutonium (several kilograms) so close to Paris. And so an annex of Vaujours for testing high-explosive implosion devices was created in Moronvilliers, in the Champagne region, on a plot of land still littered with dangerous memories from World War I that had to be cleaned up before the scientists moved in, in February 1958. The plutonium issue was addressed through a search for a new site in a sparsely populated part of Burgundy, and an annex of the B3 center finally opened in Valduc, where the operational devices would also be constructed.

The BEG became the Department of New Techniques in February 1957, and I was given the task of coordinating the preparation of the first nuclear device and test. Within a short period of time, 600 people were working for the department. The following year, the Department of New Techniques was transformed into the Direction des applications militaires. The DAM was organized into a large complex, including the Department of Studies and Fabrication, which included all the centers and equipment under construction, was in charge of designing and fabricating a plutonium device, and included two other subdivisions—the Department of Military Programs and the Department of Tests. In addition, there were several support agencies, including the Bureau of Scientific Information (BRIS). The DAM maintained this organizational structure until the first French nuclear test in 1960.

A fort situated at Limeil (southeast of Paris) was under the authority of the Army's Office of Studies and Weapons Fabrication for the armed forces. The neutron source, which would be used as a trigger for the chain reaction in the planned device, had already been developed there. In 1959, the center at Limeil was officially integrated into the CEA-DAM.[3] This center, which I directed from 1962 until 1966, was the intellectual force behind France's nuclear weapon design. It had three departments: mathematical physics, with three branches—fission, fusion, applied mathematics; general physics—experimental physics and dense plasmas; and nuclear devices.

Military Attitudes Toward the H-bomb Program

Although surprising in retrospect, the DAM's desire to work on the H-bomb met with opposition from the Defense Ministry, which was responsible for allocating the funds for the development of nuclear weapons and had the authority to decide which projects would be carried out. For atomic questions dealing with military applications, the main contact people for the DAM were the defense minister and his representatives, notably the Ministerial Delegation for Armament's official atomic representative—all of whom steadfastly refused to give any budgetary support to thermonuclear studies.

When members of the staff from Limeil would bring up the issue of H-bomb funding with engineers or military personnel at the ministry, we were met with instant refusal. Why? Given the expense of the war in Algeria, the French Armed Forces were worried about the cost of developing nuclear weapons, which would divert money from certain needed conventional weapons. Indeed, staffing and equipping laboratories for nuclear research required significant financial investments.

Although the first military planning law (1960–1964) endorsed thermonuclear weapons, military authorities gave priority to planned weapons for which a basic atomic charge was considered sufficient. "As a consequence, the scientific studies devoted to thermonuclear weapons were marking time."[4]

At the time, our top-priority programs concerned fitting a warhead to the Mirage IV jet bomber (first tested in 1962) and developing a ballistic missile warhead for the nuclear submarines that were to follow. The Air Force also wanted a ground-to-ground missile system; missiles for the system were later deployed (beginning in 1971) on the Plateau d'Albion. France had limited means compared to the superpowers, but the government had decided to pursue land, air, and sea weapon systems—something that can be now considered a waste of resources. Later the DAM would also develop warheads for the Pluton tactical ballistic missile. Thus, the DAM was mainly focused on these systems (fission and boosted fission) and was not commissioned to do any work on the hydrogen bomb program. The main task of the Limeil fusion branch was to design the boosted fission systems; thermonuclear research was only hinted at in its mission.

Each time a choice had to be made, the reflex of the ministry was to select the less innovative solution. For example, for the missiles deployed on the Plateau d'Albion, we had proposed a boosted charge in which a deuterium-tritium gas is injected in the fissile core to increase the number of neutrons and therefore the yield. This is an elegant and efficient solution. But the ministry representatives preferred the non-boosted solution.

In 1961, when discussions began about an Oceanic Strategic Force—the French nuclear-powered ballistic missile submarine force—the issue of thermonuclear weapons surfaced again in full force. Our wish was to develop a true, high-yield thermonuclear device. The Army refused and requested fission weapons with yields of 300 kilotons. President Charles de Gaulle confirmed this choice during a Defense Council meeting held on May 6, 1963, increasing the requested yield to 500 kilotons.[5] We designed a boosted fission charge with enriched uranium, quite big and inelegant about 75 centimeters in diameter and a little more in length. It was bulky, difficult to insert in the rockets, and, moreover, had serious safety problems due to the high mass of highly enriched uranium. In 1964, when the DAM began work on the 1966 testing series, the ministry thought it inadvisable to justify these tests in terms of thermonuclear research.[6]

The second military planning law (1965–1970) referred only in passing to the H-bomb in the following terms: "improvement of nuclear charges, in particular thermonuclear." No mention was made about the thermonuclear bomb during the parliamentary debate.[7] Following France's withdrawal from the North Atlantic Treaty Organization (NATO) integrated military structure in 1966, the military hierarchy had more urgent priorities, since they had anticipated that the Army and the Air Force would be deprived of the tactical nuclear weapons the United States had made available to other NATO members. In 1967, France didn't consider developing thermonuclear weapons to be as urgent because its ballistic missile submarines were still under construction. (The submarines would be put into service in 1972 and equipped with boosted weapons of 150–600 kilotons. They were upgraded with thermonuclear weapons in 1976.)[8]

The Defense Council, still headed at the time by General de Gaulle, had clearly accepted—if not actually approved—of this "H" non-priority. Alain Peyrefitte, state secretary in charge of information, provides evidence of this in his account of his brief conversation with the general in July 1962 after a Council of Ministers meeting during which Gaston Palewski, then minister in charge of scientific research and space and atomic matters, mentioned 1970 as the possible date for an eventual thermonuclear experiment. Peyrefitte recalled asking de Gaulle, "Don't you think that 1970 is a long way off for the H-bomb?" De Gaulle replied, "Well, yes. I wonder if we couldn't shorten the timeframe. But, you see, these types of things take a lot of time."[9] Nevertheless, this lack of official support did not prevent the scientists working at Limeil from thinking about the H-bomb.

1965: What We Knew About the Technical Aspects

From 1955 to 1960, as we prepared for the first French atomic test, we were also pondering thermonuclear weapons. But the prospect of hydrogen weapons seemed so far into the future that we did not work seriously on it. However, the proceedings of the 1958 Atoms for Peace Conference included an article about the combustion of lithium-6 deuteride (Li6D) written by a French team from DEFA. Li6D was commonly considered the best fuel for thermonuclear weapons, but we did not have any idea about how to burn it. All the problems with the thermonuclear bomb can be summarized by this question: how to discover the process that will allow the Li6D to undergo a fusion reaction?

The main advantage of these mysterious H-bombs, apart from their compactness and their high yield, was their complete safety with regards to any accident—fire, impact, or a fall—a major concern at the time because of the difficulty of making a safe fission weapon. Compared to our American colleagues in 1948, French scientists had many advantages: we knew that hydrogen bombs existed and worked and that they used Li6D, and we understood the reactions at work. We also had powerful computers, of U.S. origin, which were not available in the late 1940s. And we knew, more or less, the dimensions and weights of the nuclear weapons deployed at NATO bases in Europe and their yields. This information was obtained from tips we had managed to get, as well as from articles in the open literature from such publications as *Aviation Week* or the *Bulletin of the Atomic Scientists*.

But the information we had on these classified matters was so sibylline that we did not know what to do with it. Initially, we followed the same reasoning as those who preceded us in atomic research—the Americans, the Russians, the British and the Chinese: since a fission charge produces a temperature on the order of several tens of million degrees Kelvin, which is sufficient to initiate fusion reaction, we could place a light element and a heavy element side-by-side, make the heavy element go super-critical, and then observe the result. At the time, we did not think at all of separating fission and fusion in two different stages.

In our designs in which Li6D was closely fitted to a fissile core, the heating was too rapid and the resulting efficiency was very low. I kept Jacques Robert, the head of the DAM, informed of these disappointing results. In 1965 he asked me to organize regular meetings at Limeil, inviting people from outside DAM such as Professor Jacques Yvon, a distinguished scientist in the CEA.[10] But most of the participants were very busy or unfamiliar with the topic, and would listen politely but not really participate. I suggested reconsidering the problem from its fundamentals—the physical conditions for a good combustion of these light elements—but this did not seem to raise any interest. I explained to Robert that this was a completely new domain and that only people who were completely free to think about it full time would be able to make a real contribution. I also much regretted that our country, unlike the other members of the nuclear "club," did not realize the usefulness of creating the necessary means to acquire information from the nuclear tests of other countries, especially through analyzing their radioactive fallout. We would have gained interesting information, for example, from the Chinese atmospheric tests that had started in 1964.

In June 1965, internal turmoil erupted into a crisis at the department of mathematical physics at Limeil. The head of the fusion branch proposed that all the available computing power be allocated to design a new code, stressing that it was the only way to be sure to solve the H problem. As a matter of fact, some scientists were convinced that the computer codes were wrong or inadequate. But it was not possible to give up all the other tasks. Moreover, the problem seemed to be of a conceptual nature—how to build the weapon—rather than one of refining the computer code.

I was convinced that we had to reconsider the fundamental problem: how to burn the Li6D? And what were the temperature and density conditions to obtain a high-yield combustion? In order to test various hypotheses on these physical parameters, we had to do many runs on our IBM Stretch computer. Because I was the director of the Limeil center, my assistants had to take the lead in this matter. Nevertheless, the head of the fusion branch still insisted on pursuing a different goal, requesting all computers and creating a serious malaise in the department; although he was a capable physicist, I had to remove him. Things began to work again.

His successor was Luc Dagens, who had studied physics at Ecole Normale Supérieure. The appointment of this young scientist significantly improved the innovative potential of the department. When Dagens joined the team, we still had mistaken ideas about some parameters of the processes involved in thermonuclear reactions. His studies and calculations represented a major contribution and were critical in allowing us to greatly simplify the design of the thermonuclear stage.

Dagens and I had a common understanding of the problem. We initiated computer simulations on thermonuclear stages that showed the determining influence of initial density and temperature for the resulting yield. The combustion would work provided that the initial Li6D density, 0.8 grams per cubic centimeter, could be increased up to 12–15 g/cm^3. We had no idea how to reach such high values, but the results showed us that this was a good reasoning.

To understand the difficulties, one must realize that the surface of an explosive material moves at a very high speed, creating a shock wave that can compress adjacent material. If this adjacent material behaves like a perfect gas, hydrodynamics teaches that the compression coefficient cannot be greater than 4, whatever the value of the initial pressure, even a very high one. So reaching 20 seemed impossible, and my colleagues objected when I proposed looking for a cold compression before the Li6D would reach a high temperature. They had assumed that the compression would result from a single shockwave pass. So they missed the fact that in a closed system, such as a thermonuclear stage, an inward shockwave sustained from behind by high pressure would undergo several reflections, each bringing a further compression factor of 4, until the inner pressure matched that of the shock front, resulting in a considerable bulk density increase, suitable for a high-efficiency burning.

I had come to the conclusion that the obligatory condition for obtaining a good thermonuclear yield lay in acting on the light combustible in two successive and quite distinct stages, first by a strong compression without heating, and then by a temperature increase. My colleagues did not initially approve the idea; Dagens in particular did not believe in the method I proposed, nor in its physical validity. I was unable to convince them, and I left it idle for the time being. This was the status of our research when General de Gaulle visited Limeil on January 27, 1966.

1965–1966: A Policy U-Turn and Unrelenting Pressure on DAM

President de Gaulle's 1962 remarks about the slow pace of work on the hydrogen bomb were of a relaxed nature, to say the least. They contrasted sharply with the near hysteria that he suddenly displayed in 1965 upon realizing that China—which had put a lot of effort into an H-bomb from inception of its program—was going to get the hydrogen bomb before France. Apparently forgetting that only a few short years before he had supported contrary directives, on January 10, 1966, the president admonished Alain Peyrefitte, who had recently been named minister of research and of atomic and space matters, saying:

> Find out why the CEA hasn't managed to make an H-bomb. It's taking forever!. . . I want the first experiment to take place before I leave! Do you hear me? It's of capital importance. Of the five nuclear powers, are we going to be the only one which hasn't made it to the thermonuclear level? Are we going to let the Chinese get ahead of us? If we do not succeed while I am still here, we shall never make it! My successors, from whatever side, will not dare to go against the protests of the Anglo-Saxons, the communists, the old spinsters and the Church. And we shall not open the gate. But if a first explosion happens, my successors will not dare to stop halfway into the development of these weapons.[11]

Peyrefitte asked, "How much time are you giving me?" De Gaulle responded, "1968 at the latest." Peyrefitte then "threw up [his] arms in a gesture of helplessness," and de Gaulle said, "Figure it out!"[12]

De Gaulle's impatience was mainly due to his well-known concern about national independence and also by his obsession with the "*grandeur de la France*." On January 27, 1966, he again said, "If the first explosion does not happen before I leave, they will give up everything and we will be downgraded. They won't go beyond the A-bomb, and our efforts will have been in vain. France will lose its rank."[13] This feeling would have been exacerbated by the first Chinese thermonuclear test in June 1967.[14] De Gaulle believed that reaching the thermonuclear level was the only way to ensure the irreversibility of the French nuclear deterrent, and he was convinced that his successors would not have enough political will or courage to carry it through.

It is important to stress that neither the military hierarchy nor the rest of the government shared De Gaulle's concerns. As explained above, funding for thermonuclear research was still not included in our budget. Recounting a discussion with Prime Minister Georges Pompidou on September 27, 1966, Alain Peyrefitte quotes Pompidou: "In all cases, we will stop at the level we will have reached in 1970. If we have reached the H-level, all the better, if not, then too bad." Pompidou then added in a lower, confiding tone, "What does it matter anyway?" Peyrefitte replied: "You know as well as I do that the General can't stand the idea." Pompidou, with a mocking smile, said, "The General, yes, but what about us?"[15]

On January 27, 1966, de Gaulle, along with his ministers Peyrefitte and Pierre Messmer, came to Limeil to check for himself on our progress. DAM's director, Jacques Robert, and CEA's general administrator, Robert Hirsch, were also present. As the director of Limeil, I was left alone to explain the state of studies and future prospects. In my presentation to de Gaulle, I mentioned a few new ideas, but because I had not yet convinced my subordinates of the validity of the approach I was considering, it was difficult for me to make promises on a deadline. Moreover, because of the recent crisis and the reorganization that had followed, I could not present clear perspectives. I said that four years was the minimum amount of time necessary to develop a thermonuclear weapon. I meant a *weapon*, not a convincing experiment, which could occur a lot earlier, in 1967 or 1968. But, absent a promising lead, I declined to give a date for our first valid thermonuclear test.

General de Gaulle remained silent. In the car back to Paris, he questioned the merit of the scientists in charge: "As long as the authorities themselves evaded the question, one could not expect the scientists to be more determined than we. But now that we have made up our mind, isn't it possible to hire capable people?"[16] The requests from the political authorities were vague until that date, a fact that de Gaulle acknowledged. But then, once the priorities were clearly set, the DAM was considered solely responsible for the previous lack of progress. The last testing campaigns had produced results that were unconvincing or difficult to explain, and this may have raised a certain amount of suspicion with regard to the value of the teams.

In early February, Hirsch was informed of the bad impression made on de Gaulle during his visit to Limeil. Hirsch bravely defended his teams but nevertheless exerted on them the same pressure that he was receiving from above. And so, in 1966 and 1967, although the wait-and-see attitude of the Defense Ministry had not been modified, the DAM was subject to destructive harassment, and its director, Jacques Robert, was constantly threatened with dismissal if promises to bring results were not delivered quickly or, failing that, if changes were not immediately made in the scientific hierarchy. Mine indeed was the first "head to fall."[17]

The organization of the theoretical research department (mathematical physics) was well suited to the work arising from the existing defense programs (which excluded the hydrogen bomb), i.e., the development of high-performance fission weapons, the research for which involved merely an extrapolation of already acquired data, additional probing, optimization of methods, and so forth. By contrast, the H-bomb was a completely different objective and represented a scientific challenge. The design of a thermonuclear device required understanding of energy transfers in dense and very hot plasmas, which required expertise in new areas such as molecular physics and fluid mechanics, as well as a conceptual jump and innovations beyond the known domain—in short, a pure discovery. Anyone who has had occasion to participate in a real discovery knows that such a result cannot be obtained "under the gun" or by leading comments such as, "So what's the news with the H-bomb? Is it going to be ready today or tomorrow?"

France's human resources were of high quality and in all likelihood perfectly capable of dealing successfully with the H-problem, as later events would prove. As mentioned above, in December 1965, simulations had produced new insights. Once the political priorities were clearly stated, all that was necessary was to let the teams think and focus on the problem and only intervene in case of a clearly unproductive tangent.

When, in March 1966, under pressure from Peyrefitte, Jacques Robert mentioned that I should leave Limeil, the research situation, though encouraging, was not such that I could promise short-term results without bluffing. I thus resolved to leave, sad and disappointed. Because I had been responsible for the first *Gerboise* test, I remained in the circuit, and I became technical advisor to the head of the DAM.

Until that time, the Limeil director had been the *de facto* highest scientific authority for theoretical nuclear research at the DAM. I was replaced by Jean Berger, a learned scholar in condensed matter and shockwave physics who had little or no expertise in nuclear physics and related disciplines. As a result, thermonuclear research was no longer seriously directed at the highest level and was basically left to the initiative of Luc Dagens. Unfortunately, he had embarked on a wrong path, designing very voluminous and heavy systems, one called thermonuclear symmetrical (TS) and the other thermonuclear asymmetrical (TAS). They included a first fission stage with very high energy (400–500 kilotons) associated with a sizable mass of Li6D. Fusion would occur, but with very low efficiency: the Li6D was heated at the same time as compression occured, which would lead to a poor result. The total yield was increased, eventually doubled, but with only a very tiny thermonuclear contribution, while abroad the existing thermonuclear warheads were known to release 1 megaton and were triggered by a fission stage on the order of 10 kilotons. Moreover, it was impossible to imagine how to weaponize these enormous objects of 1 meter in diameter and 3 meters long.

After my departure from Limeil, and parallel to our work, in order to increase the chances for a useful breakthrough Jacques Robert created an informal study group (Groupe d'etudes thermonucléaires) that included the best engineers and scientists from other departments at Limeil to compete with Luc Dagens' department and thus stimulate inventiveness with the hope that new ideas would emerge. However, 1966 drew to a close without any truly encouraging new results.

1967: The Solution Emerges, Unnoticed

In January 1967, I published a voluminous report wherein I presented and developed my idea from late 1965, left idle since, explaining why the current studies were going in the wrong direction and producing a ridiculously low thermonuclear efficiency. I proposed a scheme with two consecutive steps: a cold Li6D compression increasing the density, from the normal value of 0.8 g/cm^3, by a factor of at least 20, followed by a sufficient temperature increase (the ignition). In this report, I also gave orders of magnitude of the energies involved in each step. The energy level was relatively low but nevertheless needed fission reactions to be attained. I sketched some practical and economical mechanisms to do the ignition, once the compression was supposedly reached. One of these was later successfully implemented in the 1970 Dragon test of the VM2 device. In the same report I also proposed possible device designs, but Dagens did not consider them credible, although no calculation was made to assess them.

Though it did not solve the entire problem, my report unleashed a new round of reflections and indirectly promoted a positive stir among the many engineers and scientists at Limeil, who firmly wanted to meet the challenge and win it. In the first three months of 1967, the intellectual atmosphere at Limeil was such that the ferment of ideas had spread to all theoretical divisions: advanced studies, the new name of the fusion branch, headed by Luc Dagens; assessment of devices, headed by Bernard Lemaire; applied mathematics, headed by Jean Guilloud; and experimental devices, headed by Jacques Bellot. Frequent spontaneous discussions brought together scientists and engineers from the three divisions, enabling an open exchange of information.

Jean Berger, the head of the Limeil center, convened a meeting at which I was invited to present my report, and Dagens agreed that several scientists and engineers would study my proposal. This meeting prompted a series of informal discussions at Limeil—in which I did not participate, but in which Dagens, Carayol, Bernard Lemaire, Joseph Crozier, and Gilbert Besson took part—to find a way to compress the Li6D. I would like to acknowledge the assistance of Jean Ouvry, who helped evaluate the energy required to put my idea into practice; of Edouard Moreau, who devised the ideal mathematical law for the compression of the thermonuclear combustible medium; and of Michel Carayol, for the first simulation of a thermonuclear assembly close to the final objective.

In early April 1967, Carayol had the idea that the x-rays emitted from the fission explosion could transport the fission energy to the thermonuclear fuel chamber to induce the necessary compression. He published a brief paper wherein he presented, and justified mathematically, his architectural idea. This was the key to the solution for an efficient thermonuclear explosive device, consistent with the current data about U.S. hydrogen weapons. Carayol had rediscovered the radiative coupling concept first introduced by Americans Stanislaw Ulam and Edward Teller in January 1951.

However, because I had left the direction of Limeil, my office was in Paris, and I had no opportunity to join the decisive discussions that led to Carayol's discovery. Bernard Lemaire and Jacques Bellot were major witnesses of these discussions. According to Bellot, who was head of the Experimental Devices Department and would later be in charge of directing the preparations for the first French H-bomb experiment:

> There were many informal working meetings of small groups in the X department discussing at the blackboard. The usual participants were Bernard Lemaire, Gérard Lidin, Michel Carayol, Gilbert Besson, Joseph Crozier, and myself, sometimes other people, and occasionally Luc Dagens. I thus had an insider's view of the events leading up to the "Carayol note." Later on, I discussed the discovery process with the main protagonists of these events, and we all agreed on the following. The starting point was an observation by Crozier who, in certain computation sessions, recorded a disturbing phenomenon that he could not explain. In fact, it was a local phenomenon of "radiative compression," and it was Lemaire who, to his credit, was able to explain this physical phenomenon. The idea of exploiting this began to enter our minds (Lemaire in particular made efforts in this direction). Carayol's discovery consisted in giving this a concrete shape, and imagining the geometry and modus operandi which we know today. There is no doubt in my mind that the "fundamental" idea must be ascribed to Carayol.[18]

Therefore, equipped with other newly acquired knowledge—in particular, the means to burn Li6D—the solution had been found by April 1967. All the parts of an efficient system had been sketched out, if not precisely defined. In particular, all the essential phenomena

Michel Carayol, the Genuine Father of the French H-Bomb

Michel Carayol was born in 1934 and died in 2003. His father was an industrialist and his mother a teacher. He entered Ecole Polytechnique in 1954, graduated in 1956, and joined the Armament. In 1962, he was part of the DEFA assigned to CEA-DAM at Limeil. In 1967, Carayol was part of the advanced studies branch.

Carayol was involved in the small group established to discuss ways to design a configuration in which the Li6D would be initially compressed using the energy from a first, separated fission stage. Very soon Carayol tried a simulation of a new type of thermonuclear stage using a spherical geometry, the most efficient design for an inward crush. This system included a substantial quantity of Li6D. The originality of the scheme was its thick external layer, made out of a metal of intermediate atomic number, moderately transparent and moderately opaque vis-à-vis the photonic rays coming from the fission stage when the chain reaction was ending.

To start the calculation, he hypothesised that this external layer would be at high temperature, probably several millions or tens of millions of degrees Kelvin, without any explicit specification of how this would be reached. This simulation confirmed the possibility of a very strong Li6D compression before the heating and produced a very good thermonuclear yield. The reliability of this encouraging result depended directly upon the validity of the physics included in the codes and of the data used. Two previous French tests (Rigel on September 24, 1966, and Sirius on October 4, 1966) had been disappointing from the perspective of thermonuclear studies, but they had validated the simulation codes and the physical data.

Carayol did not talk much, and he did not tell us at that moment exactly what he had in mind, nor did he see any need to write a report on this successful numerical experiment. He presented his results to several people, including Jean Ouvry, Edouard Moreau and myself.[a] By doing this simulation, Carayol had shifted the focus of the problem. The question was now how to find a way to convey enough energy to the coated sphere, such that it would heat up the external layer in a short time and, if possible, in a uniform fashion.

Bernard Lemaire writes:

> The studies and assessments made for this test [the Antarès test, on June 27, 1967, based on Dagens' design, had been disappointing, but the preparatory studies and calculations referred to in this quote had been made in March 1967] had led us to think of final architectures including two different stages. Moreover, these studies had led to the fundamental idea that had been lacking. Some engineers of the Applied Mathematics Department, and particularly J. Crozier, noticed some unexpected effects in the results of the calculations that they mentioned to Luc Dagens, Michel Carayol, and Bernard Lemaire. The explanation was found straight away. It showed the role of radiation as a vector of the energy. These unexpected effects were soon exploited by Michel Carayol and Gilbert Besson. Carayol then devised an architecture of the thermonuclear device well adapted to the conditioning of the [Li6D], along the lines proposed on this point by Pierre Billaud.[b]
>
> Soon after, in April 1967, Carayol wrote a brief report describing his proposal for a cylindrico-spherical case in dense metal, containing a fission device on one side and a thermonuclear sphere on the other. The report showed that the photons radiated by the primary—still very hot—in the X-ray frequency range, swept into the chamber rapidly enough to surround completely the thermonuclear sphere before the metal case would be vaporized. Carayol had discovered independently a scheme equivalent to the concept developed by Ulam and Teller in the 50s.

[a] Certain that Carayol would not write anything, I wrote a summary of this presentation for the record in one of my internal DAM reports.

[b] Bernard Lemaire, *La naissance du thermonucléaire,* p. 6. This DAM report, dated November 29, 1993, was unclassified and was supposed to be published in the DAM's monthly bulletin, but the publication was vetoed by Robert Dautray, the high commissioner at the time, and it has only been distributed to a very limited number of people.

had been identified, worked out, and, in part, evaluated. And yet, by a twist of fate, Carayol's draft was not welcomed with the interest or seriousness that it merited. DAM's deputy, Paul Bonnet, wanted to pursue its development, and Luc Dagens really believed in this new design, telling me, "It must work!" But Dagens did not take any measures to move it forward, and he continued studying his own TS and TAS designs. Personally, I was hesitant and perplexed, as were most other scientists who were not directly involved. When Dagens showed me Carayol's design, I did not react positively, which I greatly regret, as I was disturbed by the dissymmetry and by its exotic and unconventional nature. However, had I still been in charge of the H-bomb research, I would certainly have asked for particular efforts with regard to this option.

Given the lack of enthusiasm, of positive response, and of action to assess this new proposal, even among the members of the informal study group—which is difficult to understand up to now—these results remained practically confined within Limeil, considered still in a groping phase, and thus were not fully appreciated by the DAM or any higher authority. No claim of a significant advance was issued.

Anxious to accelerate things, however, Peyrefitte happened to think of creating a special "H Committee" that would bring together the main directors of the Commissariat in a monthly secret meeting.[19] Asked to report on results they had ignored or had little knowledge of, these highly ranked directors could do nothing but get entangled in inadequate and hazy explanations, thus simultaneously increasing the minister's mistrust and his frenzied efforts to get things moving.

Under increasing pressure from the Elysée, at the beginning of the second quarter of 1967, Jacques Robert was forced to dismiss Michel Périneau, the director of DAM's research sub-directorate, replacing him with Jean Viard, who had previously been in charge of testing. Like Berger, Viard had originally been trained in detonations and condensed matter physics but was not familiar with nuclear disciplines. It took him five months to evaluate the situation and to prepare his actions.

In August, Viard decided to organize a meeting intended to bring things up to date and discuss approaches and conclusions vis-à-vis the H-bomb. This conference took place September 4-5, 1967, in the DAM center in Valduc (in Burgundy), and it brought together the twenty or so scientists and engineers who had worked on the problem. Carayol was on holiday, so Besson, one of his colleagues, presented his paper, reminding the audience that the design was along the line of the "cold" compression that I had proposed.

At the conclusion of the meeting, Viard, who was still not very comfortable with thermonuclear physics, decided upon a test schedule for the summer of 1968 that would include two experiments of Dagens' TS and TAS models and a device according to Carayol's design. This latter project, which had been more or less disdained until then (even by Professor Yvon), was thus brought out of mothballs in extremis. This decision would prove of utmost importance for the future of the H-bomb program, although this was not then realized. As a matter of fact, the design later proved to be the key to the thermonuclear explosive.

Strangely enough, the meeting in Valduc, although intended to generate ideas and strip away controversies, was particularly dull and uninteresting. No disagreements, no debates. One can easily imagine that this general inhibition was largely due to the trauma inflicted on the DAM over the course of the preceding eighteen months. Before then, there had been precious bonds of friendship and confidence, bonds that transcended the hierarchical structures and that favored the sharing of ideas. Alas, these bonds had been foolishly destroyed. Perhaps it was also the fact that everyone was waiting to observe the actions of both the new director, Viard, who hosted the meeting but lacked confidence on nuclear questions, and the newly appointed scientific director, Robert Dautray.[20]

In May 1967, Dautray had arrived at the DAM with a title of scientific director appointed to the research sub-directorate, subordinate to Viard. Peyrefitte, the minister charged by de Gaulle to get results "at any price," was very insistent. He did not limit himself to threatening and shaking up the existing teams. Though a stranger to the world of scientific research, Peyrefitte did not hesitate to carry out his own diagnosis, and decided that results could only be obtained through a change in the management of the research teams. He decided that he would find the "adequate" replacement himself.[21] As a man of letters, knowing science and the scientific process only through a few stereotyped notions, Peyrefitte was convinced that titles and diplomas were a sure guarantee of the greatest inventiveness, a belief that is—as is demonstrated in laboratories every day—totally wrong. And so he cast his eye on a young physicist from the Nuclear Piles Department at CEA-Saclay, Robert Dautray, whom he sought to impose on the CEA so that Dautray should direct the thermonuclear research effectively. The general administrator, Robert Hirsch, was quite annoyed. He could not refuse an order from the minister, and yet he knew perfectly well that this order could not be carried out in this way. One cannot drop in an unknown research director who has little awareness of the scientific domain in question without running the risk of a wait-and-see attitude from the researchers. Moreover, the hierarchical responsibility for the project was on the level of the research sub-director (Viard) and, higher up, on that of the DAM (Robert).

Imposing a new director of thermonuclear research with full authority over the relevant departments at Limeil meant essentially relieving Viard and Robert of their responsibilities, without substituting a similar degree of competence. For that reason, Viard and Hirsch together came up with the solution, at least for the duration of an initial observation period, of granting Dautray an official title of scientific director without giving him any actual hierarchical authority.[22] This position allowed Dautray admittance everywhere and free access to all the technical information on past or current activities. Of course, he was free to express himself orally or in written form, and would even have been able to actually "take control" if he managed to impose his authority over the researchers by displaying unquestionable capabilities, which would then certainly have been enshrined in a more explicit official title. In any case, Dautray was welcomed within the DAM openly and without reservation, as any other new fellow researcher.

For his first five months at the DAM (that is until the important meeting of September 19, 1967), Dautray studied documents and visited the departments involved in the H problem. To everyone's surprise, he remained totally in the background, stayed silent during meetings, and issued no papers, notes, reports, or anything else. Normally, a high-level scientist in such a situation, knowing that he had been designated as the potential savior of a situation in jeopardy, and aware of the short time remaining—before de Gaulle's 1968 deadline, which left only one year to achieve results—might have considered it a clear obligation to express himself as soon as possible, say after a month or two at the very most, by making known his initial conclusions as to the best direction for research and experiments. Even during the Valduc meeting, intended to settle these questions, Dautray remained completely silent.

A Timely Confirmation Spurs the March Toward Success

During the first months of 1967, Viard had told me, ''A British physicist is showing some interest in what we do.'' At several embassy parties, a first-rate British atomic scientist, Sir William Cook, former director during the 1950s of thermonuclear research at Aldermaston, the British center for atomic military applications, had approached the military attaché at the French Embassy in London, André Thoulouze, an Air Force colonel, and had hinted to our nuclear research program. Thoulouze had previously been in charge of an air force base and knew René David, who would later work at the DAM. For this reason, instead of contacting the French main intelligence services, Thoulouze directly contacted our information bureau at CEA, the BRIS, where David was working at the time. In analyzing the fallout from the French tests, the Americans, the British, and the Soviets knew that we had not made any real progress on the thermonuclear path. In 1966 and 1967 we had tested some combination of fission with light elements. Cook told Thoulouze that we had to look for something simpler.

Two weeks after the Valduc seminar, on September 19, and while the work resulting from the Valduc decisions had not yet concretely gotten under way, Thoulouze came from London bearing information from this qualified source. Jacques Robert immediately convened a meeting, in the DAM's headquarters in Paris, to debrief this information. Only three other people attended the meeting: Viard, Bonnet (DAM's deputy), and Henri Coleau (head of the BRIS). The information, very brief and of a purely technical nature, did not consist of outlines or precise calculations. Nevertheless, it allowed Bonnet to declare immediately that the Carayol design, proposed unsuccessfully as early as April 1967, could be labeled as correct.[23] Had this outline not already been in existence, we would have had a difficult time understanding the information and might have suspected an attempt to mislead us. In fact, this was a reciprocal validation: Carayol's sketch authenticated the seriousness of the source, while the latter confirmed the value of Carayol's ideas. Without realizing it, as very few were aware of Carayol's discovery (and surely not Cook), he had given us a big tip and unexpected assistance, as this information also freed us from the ministerial harassment to which we had been constantly subjected. From that moment, things moved briskly.

Two days later, on September 21, during a meeting presided over by Jacques Robert, the news was communicated to all interested scientific management personnel that the test schedule was henceforth redirected toward the Carayol design. A few days after that, two devices were specified, one of them entrusted to Bellot (with an objective of several megatons), and the other to me (approximately 1 megaton, with an advanced thermonuclear yield). A third device was also planned in case we had been misled by the British, and that was also entrusted to me.

Right away, the DAM's efficient machine started working toward its objectives, deploying its considerable resources of scientific know-how, precision, and, when necessary, audacity bordering on risk, for example in metallurgy and in machining certain delicate pieces. Because I was responsible for a device, I was in constant contact with the cooperating departments of the DAM, thanks to correspondents from each department who had been assigned to my project. Everything was coordinated during regular meetings, and I immediately arbitrated any problems or possible conflicts. As soon as possible, I fixed material choices: shapes, sizes, masses, and other important parameters, so that the technological departments could work without delay and upon definite and stable data. Most often, these choices resulted from hand calculations, followed by complete simulations carried out by the teams of Dagens and Lemaire and specialists from the Applied Mathematics Department. Every week, I informed the DAM meeting of the progress of the project, and I remember that I hardly had to ask for a single technical decision from higher level, as I always stayed exactly within the bounds that had been set for me. In all of these engrossing tasks, I was very ably seconded by my deputy, Jean Ouvry. Bellot operated in a similar manner on his side, with the assistance of the members of his team, notably of Aubépin De la Mothe-Dreuzy, André Deléaval, and Claude Farrugia.

During the September 21 meeting, we were told that information had been received from abroad and that it should remain confidential. The people attending the meeting were to refrain from discussing it with others, and a list of "initiates," twenty-odd people, was established and kept up to date. Very few people outside the DAM were on this list; in particular, no one from around the minister for research and atomic questions had been included, which would later have unexpected and unfortunate consequences.

The existence of decisive information from a foreign source remained a state secret that, to the best of my knowledge, was not divulged in a significant way within the CEA until 1996.[24] The information went up to Maurice Schumann, the new minister in charge after April 1967. According to Maurice Schumann, de Gaulle "almost had a stroke" when learning about it. De Gaulle's great surprise shows unquestionably that neither he nor his government had approached the United Kingdom government on this matter.[25] Although a great deal of secrecy still surrounds this important episode, there is almost no doubt that Cook took the initiative to make contact—with the approval of the British authorities—for the purpose of bargaining with France over the United Kingdom's entry in the Common Market, which de Gaulle was opposing because he thought the British were too subservient to the United States.[26]

The CEA authorities secretly decided to ask Dautray to be the only link with our foreign informer. The apparent reason was that CEA would be represented by a real scientist in potential future contacts. Henri Coleau and René David of BRIS, who had been the first effective intermediaries, were removed abruptly from this operation with no explanation. In this way, Dautray was included again in the H process. This remained so confidential that nobody knew it inside the DAM, apart from Robert and Viard. It is possible that Dautray continued the contact with Cook. Nevertheless, I can testify that Bellot and I, the leaders of the test projects, did not get any outside information during the design and definition phases of our devices. But at the last moment, when our devices were already at the test site in Polynesia, Viard ordered us, with no explanation, to add a peripheral component to our devices. Afterward, I realized that this addition did not improve anything, at least for the design for which I was responsible.

I arrived in Papeete, Tahiti, for Bellot's experiment, Operation Canopus, on August 24, 1968, and watched it from the headquarters of the nuclear experimentation center. Although thick clouds prevented immediate visual observation, it appeared that the experiment was a total success. Two weeks later on September 8, 1968, my device functioned perfectly in its test, Operation Procyon, and the weather was perfect, allowing for photographs (much used subsequently by the media). The third device was not tested.

Ministerial Recognition

On October 10, 1968, one month after the second experiment, Robert Galley, the new minister for research and atomic questions, held a special luncheon to celebrate recent successes. Nine people were invited, including DAM's main management personnel who were involved in the H-bomb program. The friendly meal suddenly turned into an official award distribution ceremony when Galley unexpectedly addressed the floor and identified Dagens, Carayol, and me as the three main scientists responsible for the recent successes. He listed respective contributions: Dagens, for the complete elucidation of the decisive reactions in the thermonuclear combustible material; me for my cold compression thesis that proved indispensable for the proper development of the reactions; and Michel Carayol for his original idea of the two-stage architecture coupled only by radiation. In addition to these scientists and the minister, the others present included Hirsch, Robert, Viard, Dautray, Jean-Luc Bruneau, and Bellot.[27]

Given that these discoveries were classified at the time, all of this remained confidential and known only within the CEA. Shortly thereafter, on March 22, 1969, in the Cour des Invalides, Viard and I were awarded by de Gaulle himself an exceptional promotion in the Légion d'honneur.[28] Subsequently, Carayol decided to return to his original domain (armaments). The others returned to their normal activities and, for my part, I would, two years later, give concrete shape to the development of the primaries (first fission stages) intended for the future thermonuclear weapons of the submarine fleet. Bellot was in charge of developing the whole of the assembly, in particular the thermonuclear part. I was responsible for the design and preparation of the Andromède and Cassiopée primary experiments (May 15 and 22, 1970), as well as those for the Dragon operation (May 30, 1970), an innovative megaton scientific experiment that had been entrusted to me by Viard.

And If the British Had Not Tipped Us in September 1967?

Nobody can reshape the past. But one may perhaps wonder about what would have been the course of events within the DAM, left alone on the prospects outlined at the end of the Valduc meeting. At that time, Viard had scheduled three tests for the summer of 1968, two along the Dagens' T line, and one along the Carayol design. I know from inside how things work after decisions to test a device are taken; a kind of steamroller is set in motion.

Very probably, before the end of the month, one person would have been designated responsible for the Dagens devices and one for the Carayol device (perhaps Bellot and me). Teams would have been formed and the work would have started. After preliminary computations, the feasibility of the Carayol device would have been asserted and Dagens' machines recognized as inefficient; Viard would have intervened and instructed us to focus efforts on the Carayol line alone.

I suspect that, in the absence of any decisive remark from outside, we would have chosen to proceed in two successive steps. First, to check the validity of the radiative coupling phenomenon with one or two two-stage devices, in which a fission primary would trigger a secondary of medium energy, say 100 kilotons, just to observe the efficient compression of the secondary assembly. Very simple and immediate measurements would have sufficed to settle the result, good or not. In case of success, one or two megaton devices would follow, ready to be fired.

Therefore my assessment is that the information from Cook saved us one or two months, allowing us to give up the TS and TAS designs straight away. The important point is that Viard had decided at the Valduc meeting in September to include the Carayol design in the 1968 campaign.

Conclusion

France developed its nuclear arsenal in a chaotic fashion. In the first phase during the Fourth Republic, the nuclear activities were conducted in secret due to internal instability and foreign pressure. Apart from the assistance of the United States during the Mission Aurore in 1958, which was helpful only for the analysis of the 1960 plutonium bomb tests, France received no foreign assistance toward the development of its nuclear force.[29] The U.S. Atomic Energy Act prohibition on collaboration with foreign nuclear development efforts (apart from the United Kingdom) was secretly and informally reversed in the early 1970s, only after the French nuclear force had become a *fait accompli*, and then a secret Franco-American collaboration was initiated (this collaboration was formally recognized in a 1985 agreement).[30]

The slow progress of French thermonuclear research was the combined result of the reluctant attitude of the defense authorities, who were worried about the financial burden of the development of these new weapons, and of the priorities dictated by the Pompidou government for the quick development and manufacture of the classical fission charges ordered for the strategic triad, rather than for work on the H-bomb program.

A clear governmental request accompanied with adequate means after 1960 not only would have prevented unpleasant and unnecessary crises and suspicion among DAM personnel, but would also have led to a successful outcome much earlier. As a matter of fact, once the priorities were clearly set, it took less than two years to find the missing clue, although it was not recognized as such at the time.

This history demonstrates one of the basic problems facing collective research: an idea, as good as it may be, does not have real strength when first put forward. Fellow researchers can be reticent or even opposed if they already have their own opinion on the issue—which was the fate of my idea of cold compression when first raised in 1965—or the author of the idea might be too self-effacing, such as the case of Carayol. Carayol was very reserved and would present his results without trying to highlight them. The note in which he presented his results was very short, laconic, not pedagogical, and did not contain any comments on the architecture of the device; he did not even classify it, as if it were a small unimportant report. Dagens had his own devices to work on; I did not really try to understand Carayol's idea, and Bonnet, who was much in favor of it, did not push it openly. Unfortunately, the hierarchy directly in charge did not include any real nuclear physicists and therefore did not intervene, as it should have done, to promote more work on promising designs. In any case, a good idea takes time to be implemented. The role of the hierarchy is to insure that all resources are made available—technical means of course, but also human resources and good working conditions, including peace of mind and trust.

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NOTES

1. Pierre Billaud, "Souvenirs d'un pionnier de l'armement nucléaire français: Les facteurs déterminants du succès de l'engin M1 (Opération *Gerboise Bleue* 13-2-1960)" [Recollections of a French Nuclear Weapon Pioneer: Determinants of Success of the MI Device (Operation *Gerboise Bleue* 13-2-1960)], Spring 1989, <pbillaud.club.fr/nuc3.htm>.
2. Jean-Pierre Ferrand, *Genèse de la DAM* [Genesis of the DAM], 1983, <pbillaud.club.fr/atom4.html>.
3. Ibid.
4. Bertrand Goldschmidt, *Le complexe atomique* [The Atomic Complex] (Paris: Editions Fayard, 1980), p. 190.
5. Jacques Chevallier, "Histoire de la Direction des Applications Militaires du CEA, 40ème anniversaire de la DAM" [History of the Directorate of Military Applications of the CEA, 40th Anniversary of the DAM], December 3, 1998, <pbillaud.club.fr/atom12.html>.
6. Jean Damien Pô, *Les moyens de la puissance, les activités militaires du CEA (1945–2000)* [The Means of Power: The Military Activities of the CEA, 1945–2000] (Paris: Editions Ellipses, 2001), p. 121.
7. Marcel Duval, *A la recherche d'un "secret d'Etat"* [In Search of a "State Secret"], Défense Nationale, August-September 2004, pp. 84–96.
8. Ibid.
9. Alain Peyrefitte, *C'était de Gaulle* [It Was de Gaulle] (Paris: Fayard, Paris, 1994), Volume I, p. 167.
10. Yvon would serve as CEA high commissioner from 1970 to 1975.
11. Alain Peyrefitte, *Le mal français* [The French Malaise] (Paris: Plon, 1976), p. 81.
12. Ibid.
13. Peyrefitte, *C'était de Gaulle*, Volume III, p. 112.
14. Duval, *A la recherche d'un "secret d'Etat."*
15. Peyrefitte, *C'était de Gaulle*, Volume III, p. 159.
16. Peyrefitte, *C'était de Gaulle*, Volume III, p. 111.
17. Chevallier, "Histoire de la Direction des Applications Militaires du CEA."
18. Private communication with Pierre Billaud, "Commentaires de Jacques Bellot sur la véridique histoire de la Bombe H française" [Comments by Jacques Bellot on the True Story of the French H-Bomb], <pbillaud.club.fr/h2bel.html>. See Chevallier, "Histoire de la Direction des Applications Militaires du CEA."
19. Peyrefitte, *Le mal français*, p. 82.
20. Robert Dautray (original name Ignace Kouchelevitz) was born in France in 1928 to a family of Russian Jewish migrants. From a very young age, he showed uncommon ability and progressed easily through his studies from 1940 to 1950, despite the adverse conditions in France due to the German occupation and the consequences of the war. In 1949, he entered the high-level advanced scientific school, Ecole Polytechnique, where he graduated first in his class. In 1967, he was appointed to the DAM to spur hydrogen-bomb research. But when the major breakthrough occurred in September 1967, he had not

availed himself of the opportunity to participate significantly to the decisive steps. Jean Viard, head of the research department of DAM, charged him in October 1967 with keeping the minister informed of the progress of preparations for the summer 1968 tests. It appeared later that Dautray had taken advantage of his practical monopoly of contact with the minister's assistants to depict himself as the one who directed the studies and led DAM to the successful thermonuclear tests of 1968, thus deserving the title of ''father of the French H-bomb.'' This was a quite important qualification that opened to him the gates of the prestigious Académie des Sciences and, in 1993, his appointment as high commissioner of the CEA. The title of scientific director attributed to Dautray satisfied the minister, Maurice Schumann (who had replaced Alain Peyrefitte in April 1967), who never asked for a clear explanation of the exact extent of Dautray's responsibilities, and who thus remained convinced that Dautray was truly directing the H research efforts. This version of the facts was first published in 1976 by Peyrefitte in his book *Le mal français*. In February 2007, Dautray published his memoirs, *Mémoires, du Vel d'Hiv à la bombe H* [Memoirs, from Vel d'Hiv to the H-Bomb] (Paris: Ed Odile Jacob), in which he claimed his paternity of the French H-bomb. This book contained also a number of other errors that are commented on in Pierre Billaud, ''Une insulte à la probité scientifique'' [An Insult to the Scientific Probity], April 2007, <pbillaud.club.fr/h2007.html>.

21. Peyrefitte *Le mal français*, p. 83.
22. Scientific Director ''at'' the DAM, not ''of'' the DAM.
23. I was not present at this first meeting; this is according to what Jean Viard told me.
24. In 1996, I (coauthor Pierre Billaud) was contacted by a journalist from the weekly news magazine *Le Nouvel Observateur* who was planning to write an article on the French H-bomb program following the private printing of my manuscript. He admitted to me eventually that he had learned, through a French non-CEA source, that England had provided secret H-bomb information to which he had found veiled references in my text, and that he was planning to focus his article on that point. I could not help him without breaking my former oath of secrecy. And yet, the events in question had occurred almost thirty years earlier, the usual time for maintaining secrecy on such subjects. Moreover, I was convinced that the foreign source had provided this information to France on the orders of his government, or at least with its agreement; consequently, I was almost sure that such a disclosure would have no negative consequences on his reputation. The journalist, moreover, had incorrectly convinced himself of the existence of the classical elements of such an event, including a mole, a case officer, mysterious letter boxes, suitcases full of banknotes, and so on. I was thus practically obliged to set him straight, fearing an absurd crisis among our British friends, and informed him that it was the British source that had initiated the contact, and had done so in a most banal manner. I didn't convince him, and he and his editors decided to give his article a sensational title. In an effort to correct unjustified deviations, I decided to offer the scientific magazine *La Recherche* a summary article of a primarily historical nature including the clear identification of the English source. See V. Jauvert, ''Comment les Français ont volé le secret de la bombe H'' [How France Stole the Secret of the H-Bomb], *Le Nouvel Observateur*, March 28, 1996, pp. 110-112; and Pierre Billaud et Hervé Kemp, *Comment la France a fait sa bombe H* [*How France Made Its H-bomb*], *La Recherche* 293 (December 1996), pp. 74–78.
25. Duval, *A la recherche d'un ''secret d'Etat.''*
26. Private communication with author (Billaud) by participants of the September 19 meeting.
27. Unfortunately Michel Carayol, Robert Hirsch, Jacques Robert, and Jean Viard have since passed away.
28. Some weeks before, on February 20, 1969, Berger, Bellot, Bonnet, Carayol, Coleau, Dagens, Dautray, and David had been awarded, or promoted in, the Légion d'honneur at Villacoublay.
29. In early 1958, Lewis Strauss, the chairman of the U.S. Atomic Energy Commission, had authorized a secret French mission (Mission Aurore) with a visit to the Nevada Proving Ground. The aim was to learn about the sophisticated methods of analysis related to all aspects of nuclear tests. It did not include any information on the design of atomic bombs. This is discussed in more detail in Pierre Billaud, ''Souvenirs d'un pionnier de l'armement nucléaire français'' [Recollections of a French Nuclear Weapon Pioneer], September-October 1998, <pbillaud.club.fr/nuc2.htm>.
30. Richard H. Ullman, ''The Covert French Connection,'' *Foreign Policy* 75 (Summer 1989), pp. 3–33.