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# THE DESIRE TO UNDERSTAND THE WORLD

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## THE WAY TO ASTROPHYSICS<sup>1</sup>

### *Saving My Life*

July 1, 1943: I was joining the Observatory of Haute-Provence. I was full of emotions and feelings, which certainly had, in a very subtle way, an influence on my scientific life. It was the beginning of an illegal life. I had a complete set of papers (false papers), identity card, food card, and most important the *Carte du Service du Travail Obligatoire* (the government of Pierre Laval in Vichy had negotiated an agreement with the German Government: boys born between 1920 and 1922 had to go to work in Germany), with the notation “*trente-quatre mois de captivité*.”<sup>2</sup> When I went to Digne a few days later to meet my young wife Ruth who was coming from Nice, these papers demonstrated their validity. Just as I got off the bus, I had an identity check by a gendarme<sup>3</sup> and he let me go without any problem. I learned much later (after the liberation) that the gendarmerie in this part of France was closely connected with the resistance movement. Did the gendarme simply feel comfortable seeing a young man, who was of an age to work in Germany, carrying the proof that he was exempted from this kind of constraint, or was he connected with the resistance movement and supporting illegal activities? I shall never know.

I had stopped in St. Michel, a few days before my marriage, in order to register in due time at the butcher shop, the bakery, and the grocery store, with my fresh food tickets. The tickets were “legal,” as well as the food card, even

<sup>1</sup>It is impossible to follow the chronological order! The logic of things has determined the presentation of the biography.

<sup>2</sup>Prisoner thirty-four months.

<sup>3</sup>There are three kinds of police in France: the national police, which is under the orders of the Interior Ministry; the city police, which in all big cities is under the orders of the city council; and in the countryside, the gendarmerie, which is a special part of the national army.

if they had been obtained in a complex way where false documents had been used to obtain legal documents. I was settling in a quiet place, in a region that was still under Italian occupation, with the feeling that I had greater chances to survive than in any other place. I wished to do science, but at the time of my arrival, this was far from my mind. To survive: Survival was my only worry.

Coming from Nice by this extraordinary 130 kilometer railroad and going through the valleys and the hills of the Alps of Provence with an engine working with wood instead of coal took seven hours. Then came the bus, and, after the St. Michel stop on the main road, I rode five kilometers, with my suitcase on my bicycle, to the Observatory. I still remember the extraordinary smell coming from the fields and from the trees, and this characteristic noise filling the landscape: the buzzing of the cicadas. I had “gone underground” (metaphoric expression for illegality); I was feeling free, even if the fear of being arrested and sent to a concentration camp in Germany, like my father nine months before, had not left my mind.

I knew that my only duty was to work as a night assistant to the assistant director of the Observatory, Ch. Fehrenbach. Otherwise, I was a guest of the Observatory, and I was deeply grateful to Jean Dufay, Director of the Lyons Observatory and the *Observatoire de Haute Provence*, who had given me a place to hide, a place that was incredibly safe. And in fact, even after the collapse of Fascist Italy and the complete occupation of the Italian occupation zone by the German troops, the Observatory of Haute-Provence remained a safe place. The nearest German soldiers were the four men working in the railroad station of Manosque, 25 kilometers away, and there was a larger garrison in Digne, at a distance of 62 kilometers. Why do I give all these details? I want my readers to understand what was going on in the mind of a young man, who had lost his civil rights, who was under the threat of the racist decisions taken by the Nazis and the Vichy government.

To survive: Survival came first.

### *Student Life: 1939–1941*

I was already attracted by science, during my younger years, perhaps influenced by my father, who very much liked reading popular science publications. I was educated in school without any problem and studied for one of those competitive exams that are so characteristic of the French educational system (Ecole Normale Supérieure or in short ENS or l'École).<sup>4</sup> I passed it in July 1939.

<sup>4</sup>The ENS was created by a decree of the parliament (*Convention*) in 1794, as were many other Écoles the same year. It was only at the beginning of the twentieth century that a large fraction of ENS science students obtained academic and research positions. For example, Eugène Bloch, Director of the physics lab (ENS Class of 1897) was a high school teacher for 19 years before getting a University position. He used all his free time to do research.

The war started soon afterwards, with the declaration of war on Germany by France on September 2, 1939. At that time, the academic year started the first week in November. I was just 19 years old—too young to be drafted. I remember reading before the start of the academic year *Men Like Gods* by HG Wells. I remained seduced during all my life by this view of a free society, where the main aim was knowing and understanding the world better and better.

Because of the mobilization, the École Normale was almost empty. The boarding school was closed except for non-Parisian students. I had dreamed of leaving the family, but it was not possible. Only the few *Élèves* who were discharged from military service, or the youngest ones, who were not drafted, about 40 altogether, were there. *Élèves* of the second, third, and fourth year of studies were in the army. Once, during the year, a large fraction of these *Élèves*, most of them officers, succeeded in getting leave on the same day and organized a banquet at the ENS, with the first-year students taking part in this enjoyable meeting.

I went to the École everyday, attending lectures on mathematics and physics at the Sorbonne. Twice a week, École students attended premilitary officer training: artillery for sciences students and infantry for humanities students.<sup>5</sup> The material of the University exams appeared so easy compared to the level of the competitive exam that little work was required to be successful. This left time for other occupations.

One of the ENS students who entered the school with me, Louis Le Blan, was very fond of experimental work. He had found it possible to build electronic systems in the physics laboratory. He told me, a couple of weeks after the opening of the University (the first Monday in November at that time), that the Director of the lab, Eugène Bloch, wanted a student to help him set up his lab. Eugène Bloch was a specialist in spectroscopy, and the laboratory had just moved from the old building of the ENS, on Rue d'Ulm, to the new building, on Rue Lhomond. He wanted to use his Rowland grating again, in a completely new room, in order to begin his research again. There was a circular concrete base, completely isolated from ground vibrations. In the old building, it had been noticed that sometimes all the spectral lines on the photographic plate, after a long exposure, were double: The vibrations produced by a truck going by on the nearby street had displaced the grating by a small amount, large enough to modify the recorded spectrum. The aim was to put the grating and the support of the photographic plates in the correct position to take spectra. This was done

<sup>5</sup>Usually the military preparation for an ENS student lasted two years, but in war time, in order to have more officers, the density of preparation had doubled, and we were supposed to pass the exam giving us the grade of sous-lieutenant in June. We learned about the 75-mm gun, ballistics, and how to hit the target successfully.

with an iron arc as a light source. I was asked to measure the wavelengths and to identify spectral lines with the help of a catalog of iron lines. I measured hundreds of wavelengths but could identify only a very small number of them. Many years later, I realized that a large number of faint molecular lines appeared on these spectrograms.

I do not know whether I really contributed to setting up the large grating spectroscopy room, or whether what I was doing was just a way of initiating me to laboratory work and experimental research. All I remember is that doing this experimental work in complete freedom made me very enthusiastic. I was very shy, but during the year, having looked here or there at various books, I asked Eugène Bloch about a theoretical problem. I do not remember his exact answer, but his meaning was that I should first be concerned with experiments.

At that time, there was no “modern physics” in the physics program of the University. The famous textbooks of Georges Bruhat contained practically no quantum mechanics; the volume on optics had some development on spectral lines. The first books of Louis de Broglie were easy to find in bookstores, and I bought a couple of them. But these books were devoted to the basis of quantum mechanics, and there were no applications to practical physical problems, like those found in the old classical book of Mott & Massey. Although reading Louis de Broglie’s books was fascinating, there was no possible practical use for them. In fact, walking around in the second-hand area of the well-known Gibert bookstore, I had found a very attractive theoretical book on atomic structure, which I judged a few years later to have been written by one of these crazy crackpots whose work winds up on the second-hand shelves. This gives the reader an idea of the intellectual landscape facing a young student.

### *Disorderly Flight*

Real war in France began during the night of May 9, 1940. Very soon afterwards troops and civilians began to flee in the face of the German army. At the beginning of June, it appeared that Paris was going to be taken. I asked the assistant director of the École Normale, Georges Bruhat, who was in charge of the science section, what to do. At that time he was certainly much worried. Because the physics lab had been doing research for the army during the *drôle de guerre*, it was necessary, as I learned years later, to destroy documents. He just gave me an appointment in Bordeaux (reminding me of historical memories of the Franco-Prussian War of 1870—would the government be set up in Bordeaux like that of Gambetta?), telling me that I had to leave Paris by bicycle by the Porte d’Orléans at the time German troops would be entering Paris by the Porte de Clignancourt. I left Paris, riding on a bicycle with the smallest possible amount of luggage, early on the morning of June 10th. I avoided an incredible traffic jam, left Etampes just a few minutes before a terrible bombing, finally

took a small road, and after a few kilometers found a small village. I went to the railroad station, asking for a place to sleep. The station master gave me hospitality, opening a folding bed in the waiting room, with perfectly fresh white sheets. He did not want me to pay anything. This peaceful image, at the time of this disaster, is still very vivid in my mind. Early the next morning, I started riding again in the direction of Blois. On the way I was joined by a 16-year-old factory worker who had been ordered to go to his factory, which had moved to the southwest of France. The road was less boring with him. There were not so many cars as before, as people were crossing the Loire river. In their mind a great battle could stop the German army there! At the end of the day, we got off the main road. We found a farm, where the farmers offered us a peasant dinner. We shared a large bed for the night, and next morning I went to Blois. I expected to find a train going to Bordeaux, got on one, and after 25 hours, including several stops in the country because of bombing alarms, arrived at the Bordeaux railroad station. Cousins gave me hospitality.

The next day I went to the University and found the École Normale Supérieure professors gathered there. In the University courtyard, the chemistry professor Georges Dupont was demonstrating the possible use against tanks of what was going to be called a few years later "Molotov cocktails." I did not know what to do. Alfred Kastler, who at that time was a professor at Bordeaux University, gave me physics papers of bachelor candidates to grade. In Bordeaux I met one of the ENS students who had taken the competitive exam with me, Jacques Polonowski, and another one, Jean Mayer, who had been drafted in April and had escaped from his barracks not far from Bordeaux. The father of Jean Mayer was a well-known professor at the Collège de France, and Jean Mayer was probably much more aware than I of what could happen. He was terribly nervous, was frightened that terrible antisemitic decisions of the Nazis would fall on us. He took me to the harbor to try to board an English boat for England. It was not successful! His mother and sister were staying in Valence d'Agen, where his family had a property. He convinced me to go there, where I spent a few days. Jean Mayer was already dreaming of hiding in the country and taking part in resistance actions against the Germans. In fact, he left France with his family for the States later. In the States he became a well-known professor of biology, member of the American Academy of Sciences, and member of the French Academy of Sciences. It was during the short time I spent in Valence d'Agen that I first heard the famous appeal of de Gaulle to the French to reject the capitulation by Petain and to keep fighting against the Germans.

I do not remember how I got a message from Albert Lejeune, another ENS student who had reached Montpellier, suggesting that I should come to take my physics and math exams there. This seemed the most important thing to do at

that time. I went to Montpellier, got a bed there at the boarding high school for boys and, as a student of the École Normale Supérieure, obtained some money from the Rector, Henry Pariselle. I met other students from ENS. I registered for the special exams organized for refugees and was able to get in touch with my parents.

During this time in Montpellier, I spent a long time in the library, reading some of the volumes of the physics course of Bouasse,<sup>6</sup> but I also went walking in the *garrigue* (sun-drenched hills). Maurice Pariselle (same ENS class), who had been drafted in April, was in Montpellier barracks. Visiting him, I discovered that I could take the military preparation exam, but the collapse of the French army had raised in my mind such a lack of trust in the military that I did not want to have any connection with them anymore. The great speech of de Gaulle was known, but London seemed so far away, and I had so little taste for arms and their use that after the failure with Jean Mayer in Bordeaux it did not enter my mind that I could begin to look for an escape from defeated and occupied France. The other students of the *ENS* whom I met in Montpellier were only thinking about continuing their studies and, in the end, I did as they did. I finally succeeded in passing the two exams for which I had prepared during the year.<sup>7</sup>

At the end of July 1940, I did not know what to do. I left Montpellier and went to Martres-Tolosanes, a small village near Toulouse, where I could be lodged by a niece of my father. At the end of September, I learned from an exchange of letters with my mother that Bruhat had decided that I had to come back to Paris and attend the normal activities of the School.

### *Return to Paris*

It remained to organize my trip back to Paris. I went to the railroad station of Toulouse to buy a ticket. A poster, above the ticket office, was frightening: "*Le passage de la ligne de démarcation est interdit aux nègres, aux chiens et aux juifs.*"<sup>8</sup> I nevertheless bought the ticket. I left Toulouse on September 19. The train stopped in Chateauroux, for a check of the situation of the passengers. The French officer in charge asked me if I was Jewish. Surprised, and not prepared for that question, I just said "no," shaking my head. His comment was: "*Tenez-bon!*"<sup>9</sup> I prepared myself for the border crossing. But my name,

<sup>6</sup>Bouasse did not believe in quantum physics, but he wrote a physics treatise of 48 volumes, which had the advantage of going into details that are usually skipped. These books were present in all University libraries. Easy reading! Completely obsolete now!

<sup>7</sup>The French university system is such that degrees delivered by one university are valid all over the country.

<sup>8</sup>Crossing the border of the occupied zone is forbidden to negroes, dogs and Jews.

<sup>9</sup>Stand firm!

which looked like a German name,<sup>10</sup> did not raise any question and I could renew breathing when the train started.

The École was almost full. There were several Élèves who were war prisoners. After the demonstration supporting de Gaulle on November 11, at the Place de l'Etoile, the University was closed for several weeks and we had to sign in everyday at the nearest police station, on the rue Vauquelin.

Second-year students had to study chemistry and classical mechanics. I liked mechanics, but I did not like the courses in chemistry, except those in physical chemistry. Here I took my first step in research. In Spring 1941, studying binary mixtures, I was curious to examine ways of measuring the chemical composition of the solid and liquid phases obtained during solidification or melting. It appeared to me that fast cooling, or tempering, would produce a wrong relation between the chemical composition of the solid or liquid phase and the temperature of solidification or melting, by some departure from thermodynamic equilibrium. With a simple picture of the chemical composition of the layer separating the liquid from the solid phase I tried to estimate the deformation of the classical diagram describing binary mixtures.

That this work was published is due to chance. I was sent by Bruhat to visit Paul Pascal,<sup>11</sup> President of the Jury of chemistry, to get in advance the marks of the written part of the chemistry exam of ENS students. Pascal, after giving me the marks, and noticing that I was not happy with the mark he had assigned to me, appeared to be pleased to talk with me and asked me about my interests. I mentioned this computation on tempering of binary mixtures. He offered to let me present a short paper on the subject in the *Comptes Rendus de l'Académie des Sciences*. I brought him the paper one week later. It was published in the *Comptes Rendus* in June 1941. Later I tried to publish a complete paper in *Cahiers de Physique*. Was it delayed because Jews were not allowed to publish,<sup>12</sup> or because of the slowness of the publication? It appeared only in 1945.

That same year I studied for an exam on probabilities. The main course was given by the famous Professor Emile Borel. It was also necessary to make a choice among several optional subjects, and I chose statistical mechanics. The

<sup>10</sup>It is definitely not a German name, but a Jewish name. Fortunately, this was apparently not known by German officers! In the spring of 1943, as I was going from Lyon to visit friends of my father in Annonay, it happened that there was an identity card check. The German officer asked me kindly: "Sprechen Sie deutsch?" I said "nein" instead of saying "non." The officer smiled!

<sup>11</sup>Pascal was a well-known specialist in physical chemistry and Member of the French Academy of Sciences.

<sup>12</sup>Obviously, Academician Paul Pascal did not care about it. I do not think that there was any checking of the *Compte Rendus*.



course was delivered by Francis Perrin. The introduction to Fermi-Dirac and Bose-Einstein statistics was absolutely exciting.

During the summer of 1941, students were called up to help harvesting. To avoid that, I studied for an exam of mathematics: *mécanique analytique*. There was a very large program from Lagrange-Hamilton theorems to elasticity, hydrodynamics, and shock waves. At home I enjoyed studying elasticity, fluid mechanics, Lagrangians, and a terrible course of Gaston Julia on matrices. In October, I discovered that the professor responsible for the exam was no longer Gaston Julia, but Jean Chazy. The subject of the exam was not matrices, but an elementary application of Hamiltonians to eastward deviation of projectiles. I was the only candidate, and Jean Chazy asked me to come to his apartment for the oral part of the exam. There was no heating at that time, and Chazy was wearing a blanket on his shoulders. He sharply criticized my solution as being not very elegant and asked me a few questions on elasticity. I was awarded the degree of *Certificat de Mécanique Analytique*.

### *Deportation of My Father*<sup>13</sup>

It is perhaps difficult today to make the reader appreciate the emotions and feelings of a young Jewish student in the fall of 1942. I had lost my civil rights. In October 1940, Jewish teachers were forbidden to teach: They should not face Aryan pupils; in June 1941 *numerus clausus* of Jewish students was set up. Becoming a student of ENS in 1939 was the opening of a career as a science teacher, and this prospect had vanished. The first arrest of Jews in Paris took place in May and the first roundup in August 1941. They were sent to a concentration camp. That camp was in France, but they were going to be deported later. After the murder of a German colonel in Paris by the resistance, 742 Jewish intellectuals (physicians, graduate engineers, writers, lawyers, dentists: my father was one of them) were arrested and sent to the Compiègne *lager* on December 12, 1941. At that time, only men were arrested, either individually or in roundups. With the stamp “JUIF” on my identity card I was facing a real danger. After consulting several people, it appeared that the best thing to do was to flee to the south, to the nonoccupied zone, to Lyons. I obtained the agreement of Georges Bruhat, who took the necessary administrative decisions, especially transmitting my student fellowship from Paris to Lyons. It was a long trip; I received unexpected help crossing the border illegally between north and south zones and arrived in Lyons on January 16, 1942. I felt safer in Lyons than in Paris, but I was worried about my mother,

<sup>13</sup> An excellent book concerning the situation of the French Jews is *Vichy France and the Jews*, by Michael R Marrus and Robert O Paxton (New York: Basic Books, originally published as “Vichy et les Juifs” by Calmann-Lévy, Paris, 1981).

left alone in Paris. Drastic decisions about Jews in prison camps had not been taken yet and she could send letters and food packages to my father; she could receive letters from him as well. I had the feeling of a rope around my neck, strangling me little by little.

In January 1942, the war in Russia was terribly impressive. London was broadcasting information, and even with their optimistic presentation of the news and the extraordinary firmness of England, the deep penetration of the German army into the Soviet Union looked like an irreversible process, raising the fear of a possible final victory for Hitler. In that case, what kind of slave would I become? The decision of the *final solution* was not known at that time.

September 23, 1942 is the day of deportation of my father. He died in Auschwitz, probably on September 25, at the age of 65. He had dropped a letter to my mother on the railway tracks, thrown through the opening of the cattle wagon, where he had written: “nous sommes quarante cinq dans un wagon à bestiaux. . .”<sup>14</sup>. The letter was picked up by a railroad employee who sent it to my mother. As soon as she received it, she sent me a short note giving me the horrible news, which I received only on October 2. My father had been sent to the camp of Pithiviers in August. On September 21, the Prefect of Loiret, noticing his First World War services, tried to save my father by moving him to the camp of Beaune-la-Rolande. But he was not French born and the Prefect was obliged to move him again to Pithiviers, from where he was deported.

### *Protection from Racism*

My parents believed that French Jews were safe. In fact, my father, a dentist, did not imagine how he could make a living in the southern zone<sup>15</sup> and never mentioned the idea that perhaps it was safer there than in the north.

As already mentioned, after the arrest of my father on December 12, 1941, I left Paris and arrived in Lyon on January 16, 1942. It was the beginning of a new life.

By May 1943, I was wondering how to escape from the Compulsory Working Service (STO: *Service du Travail Obligatoire*). In the physics lab of Lyon, there was an assistant lecturer, Cavassillas, who sometimes went to St. Michel, to help Jean Dufay observe the night sky. He suggested that I hide in the Observatory and advised me to see Dufay<sup>16</sup> and ask him if he would agree. I waited for Dufay to come out from lecturing at the University and began explaining who I was: a student of the Ecole Normale Supérieure, whose father was deported. . . .

<sup>14</sup>We are forty five in a cattle wagon, 25 women and children, 15 children, 9 without parents. . . .

<sup>15</sup>Until November 1942, the German Occupation forces were not present in the southern part of France.

<sup>16</sup>I learned after the war that the “Conseil national de la Résistance” had been holding meetings at Lyons Observatory.

I asked him if he would accept me under a false identity at the Observatory. Before I had finished my sentence, he had said yes. Furthermore, he obtained a fellowship for me.<sup>17</sup>

Eugène Cotton, who entered ENS in 1934, was also working in the physics lab. Since we both came from the École we had a feeling of friendship. He came once to see me in the winter of 1942 and gave me a small paper from an underground resistance movement. I did not want it. It is true that I was depressed by the fact that my father was in a concentration camp, but at that time I did not believe that doing something against the occupation forces was possible. At the time of this Service du Travail Obligatoire story, I knew Eugène quite well. He introduced me to another former *Élève* of ENS, of the same class as he, who was also a physicist. His name was Vigneron, and he was already deeply involved in the underground resistance movement. Vigneron provided me with a paper attesting liberation from *Stalag 1A* (in fact, I never used it!). I got in touch with David Donnoff, who was working in the Jewish resistance. He provided me with a blank birth certificate, which I later filled in with the birth on January 5, 1920 of Antoine Emile Louis Sellier. I went to the “Prisoners House” to obtain an official paper concerning the time spent in a stalag; I was very much afraid of not being able to answer questions. I arrived in an office on the first floor of a building and found a man of about 50, to whom I explained that I had been liberated from Stalag 1A for health reasons. The man joked: “You know well that prisoners from Stalag 1A are not liberated for health reasons”; he asked for my name and filled out the papers; a few minutes later I was in the street with this precious document. There remained the problem of getting an identity card. I had the address of a police station in Grenoble, connected with the resistance, where I could obtain it. But when I arrived in front of the police station, I read a posted notice that listed the documents necessary for an identity card and found that I had only one instead of two. I needed a witness. I asked a grocer, in front of his shop, if he would agree to testify for me. He looked at me, asked me to give him my document (the blank identity card with just the fiscal stamp on it), told me to wait a few minutes, went to the police station, and indeed came back within a short time with my false-true identity card. With it, and the document from the Prisoners House, I got the card and tickets concerning food products the next day. When registration of young men for compulsory work was open, I got the card with the words already mentioned:

<sup>17</sup>Not being registered in Paris, I had lost my ENS student fellowship in the Fall of 1942. Jean Dufay procured the help of Bruhat to obtain a fellowship of the *Aide à la recherche scientifique*. Breaking racist discrimination rules, Champetier, assistant director of the CNRS, sent a check to Dufay without stating the name of the payee. I received the money regularly through the St. Michel post office.

“prisoner thirty-four months” without any trouble. I wish to express here my gratitude:

to Georges Bruhat, dead in deportation, December 31, 1944, for his constant support;

to Jean Dufay, who provided me hospitality at the *Observatoire de Haute Provence*;

to Charles Fehrenbach, who protected us during our stay at the Observatory; and

to those members of the communist resistance movement, Léopold Vigneron, Eugène Cotton, Cavassilas, and to the member of the Jewish resistance, David Donnoff, killed later in Lyon by the Gestapo, who helped me to go under cover.

### *Research: Second Step*

Beginning with my third year of the ENS (Fall 1941), we had to practice some elementary research, which for physicists was preferably experimental. My little knowledge of statistical mechanics and chemical physics led me to imagine that water molecules would not have their electric dipole isotropically distributed in the vicinity of ions in an ionic solution. Because of the scale of the process, it seemed to me that it would be possible, using X-ray diffraction, to test the effect. Georges Bruhat sent me to the laboratory of crystallography of Jean Wyart to present my project. It was accepted, and I started immediately, trying to accomplish the experiment. The idea was to put the ionic solution in between two thin mica plates. The beam of X rays would be limited by a slit of variable width. However, by the time I had completed the first items in the experiment, I had decided, as mentioned before, to go to Lyons to find a refuge.

Lyons had an important laboratory of mineralogy and crystallography, headed by Henri Longchambon. I was immediately accepted there. I was given a small lab, where there was a relatively old fashioned X-ray source, which had not been used for several years. The first thing I had to do was to produce X rays. There was a Holweck vacuum pump, which could only be started after preliminary pumping. I failed.

Visiting the physics lab, I became acquainted with Professor Max Morand, also a former student of the ENS (class of 1920), who had been a professor of physics at the University of Liège. After the bombing of Liège in May 1940, with all his instruments destroyed, he left Belgium and found a refuge at the Faculté des Sciences of Lyons. He was trying to rebuild a Van de Graaf

generator. I told him the trouble I was having with my instruments. He came to my lab and showed me that the entire instrument was dirty and had to be cleaned carefully, in order to avoid dirty vapors preventing a good vacuum from taking place. I complained about the fact that I had to obtain my degree, the *Diplôme d'Études supérieures de Physique*, before the end of the academic year. He then suggested I should build a hot-wire manometer and make measurements of gas pressure with it. The hot wire was in fact a gold foil. When measuring its electric resistance, I found that it had a ten times larger resistance than expected from its cross section. When looked at with a microscope, it appeared to be full of holes. Following a complex transform that I had found in Bouasse during my stay in Montpellier, I calculated the actual resistance, due to a large number of holes in the gold foil.

Usually, to get the degree, students would present the results of their experiment and give a short lecture on a subject chosen by the examining board. However, having explained to Prettre, professor of chemical physics, my work on mixtures tempering, he asked me to present it. For the degree I presented two original subjects: the hot gold-foil manometer and the quenching of binary mixtures.

The audience was quite limited. However, I had the honor of the presence of Eugène Bloch and his brother Léon, who had left the northern zone of occupation for Lyons. I spoke almost two hours, as no one had told me to prepare a shorter presentation of my work. At the end, Eugène Bloch, who was famous for his teaching of didactics of physics to the *Élèves*<sup>18</sup> first congratulated me, and then said that, for such an elementary subject, my talk was perfectly incomprehensible. I never forgot the remark. Eugène Bloch was certainly a very important person in French academia. In 1943 he had a visit from Jean Chazy, and I went to his office to meet him. When I arrived there, Jean Chazy was making surprising comments about the migration of a large number of Jews from the occupied northern zone to the southern zone. He was in fact comparing the system of the two zones to communicating vessels and the number of Jews in each zone to fluid heights in communicating vessels. His statement was that the height of the “jewish liquid” in the southern zone being too large, the situation was incompatible with the properties of communicating vessels. Eugène Bloch was smiling! Obviously, having been a University Professor of Rational Mechanics did not enable him to have a good view of the actual situation. In 1944, Eugène Bloch, 66 years old, was deported and died in Auschwitz. His wife and his brother escaped.

<sup>18</sup>During the last year of ENS, students prepared a difficult competitive exam called the *agrégation*, with an important test consisting of a lesson in physics and a lesson in chemistry for high school students.

During the next year, 1942–1943, I kept my office in the physics lab of Max Morand. With the help of Longchambon, I found a small job, using polarized light to test stresses in a plexiglass model of a railroad bridge over the Rhône. This lasted until my departure to Saint Michel.

## IDEOLOGY, POLITICS, AND SOCIETY

### *Introduction*

Having taken a small part in the resistance and having been seduced by an ideology, dreaming of a new and better society, I felt it necessary after the war to become active in politics and trade unions. I felt the need to write impassioned articles in periodicals. This has been known in France and abroad, including the States. It played a part on my relationship with the scientific community. I find it necessary to tell that story, a story that goes from an enthusiastic attitude at the beginning to pain, regrets, and melancholy, feelings that have not passed away.

### *Resistance and Belief*

To understand what follows I have to say a few words about my father. Born in Tulcea (Rumania) in 1877, he was taken by his parents to Palestine in 1882, participating in the emigration movement “Zion lovers.” A good pupil, he was sent in 1896 by *Alliance Israélite Universelle*, under the sponsorship of the Baron Rothschild, to study agriculture in Grignon.<sup>19</sup> This was at the time of the Dreyfus affair, and he discovered in France the battle between right and left, the fight of the famous *Ligue des Droits de l’Homme* in favor of Dreyfus and against antisemitism. Although brought up religiously he became completely atheistic. Back in Palestine in 1899 he was sent to work in the region of Metula. At that time this area was full of swamps and he caught malaria. Because he had learned modern agriculture he got into trouble with the old Jewish peasants. He finally went to New Zealand in 1902. There, he discovered the Labour Party and appreciated the quality of its politics. As he could not settle in New Zealand under the conditions he had hoped for, he came back to France in 1905. He would have liked to study medicine, but he did not have the proper degree to do it. However, he was able to register in the best dentistry school and got his degree in 1908. That same year he obtained French nationality. He married one of the daughters of the Secretary of the Jewish Paris Consistory, Léon Kahn, who had died in 1900. Despite his atheistic attitude, he was very respectful of the faith of my grandmother. I was educated with a mixture of atheism, skepticism, and Jewish tradition.

<sup>19</sup>There is still a high level school in Grignon, training graduate engineers in agriculture.

My father, being a great admirer of Jaurès, became a member of the French Socialist Party. During the First World War, he had to treat disfigured soldiers and found that work difficult to bear. When the “Third Internationale” was created he joined the French Communist Party, but he resigned when the Organization asked him to leave the *Ligue des Droits de l’Homme*. He was so grateful to the *Ligue* for its fight in favor of Dreyfus that he could not stand such a requirement.

I became acquainted with the political ideas of my father essentially at the time of the “Front Populaire” and the fight against French fascists. The political victory of the left in the 1936 elections was very exciting, and at that time I certainly did not see the difference between socialists and communists. During the last two years before the beginning of World War II, I was so busy studying for the competitive exam that I did not closely follow international events. My father was certainly very much worried by the great trials in the USSR, but I did not feel concerned. Then came the Munich agreement and the victory of Franco in Spain—all that had the smell of the coming war.

A short time after the terrible news of the deportation of my father, I went to see Eugène Cotton, telling him that I was ready to join the Communist Party. I needed to do something to give me the conviction that I was contributing to the fight against the Nazis. A few days later, I started to stencil at night, in the lab, packages of 50 or a 100 copies of short leaflets calling students to resistance. Leaving the lab, I had to walk just 20 meters in order to drop the stencil in the Rhône river; the following day, the leaflets were given to someone I did not know, to be quickly thrown in a lecture room. Once I made a photocopy of a document about the use of plastic bombs.

Obviously, Eugène Cotton, acting like the intellectual he was, wanted to let me know not only the 1942 slogans, but also Marxist theories. He lent me Engels’s *Origin of Family, Property and State*. The student of the underground communist organization with whom I had a direct connection lent me *Materialism and Empiriocriticism* by Lenin, but I was not ready to read that kind of philosophy. On the contrary, Engels seemed to me amazingly clear. I was seduced, as if I had an unknown need for an ideology, which at last was fulfilled. In fact, looking today at my intellectual behavior at that time, it looks as if I had taken holy orders. Suddenly it was like sunshine enlightening my life. The theories of Marx and Engels, explaining human history completely and definitely, the success of the Communist Revolution in Russia in 1917, the idea that practical applications of the knowledge of history’s laws would lead to a new society, were incredibly attractive. This kind of behavior, a sort of faith, with a complete respect for these written “scientific” laws of history, looks today just like the respect of dogmas, and at that time touching these dogmas was sacrilegious.

The situation was so desperate; the fear of being arrested as a Jew and sent to a camp was turning over in my head and in my stomach. The help I received from the Communist organization certainly brought me to a feeling of gratitude. Coming back to Paris in October 1944, I did not join the Party again, although I was feeling guilty for not expressing my thanks and not being a communist militant.

At the end of 1945 or beginning of 1946, I joined the Party again. Communist publishers were publishing the most important Marxist writings, little by little. The ideology in these books went much beyond the daily political attitude of the Communist Party. The idea that a “scientific” knowledge of the laws of history, economy, and production could lead to a better society was very appealing. Becoming known among scientists, I joined the trade union *Syndicat de l’Enseignement Supérieur et de la Recherche Scientifique*. It was at that time a very small union, which was actually in the hands of the communists. I quickly went down the path of a more intellectual Marxist activity; I published several ideological articles, completely in agreement with the dogmas of that time in *La Pensée*, founded by Paul Langevin in 1939. I became known as an active Communist in the French astronomical community, especially when I became general secretary of the Union in 1949. I kept this responsibility for eight years.

In the spring of 1950, A Danjon organized an international meeting on astrometry in Paris. There was a Soviet delegation, which included Zverev (chief of the delegation), Ambartsumian (for whom it was sort of an award), Nemiro and Orlov from Pulkovo, and Batruchevitch (a “secretary” of the Academy). It was well known at that time that Soviet people were not allowed by the KGB to have private relationships in the country they were visiting. Nevertheless, I dared to invite them for dinner at home. We were expecting them at 8 pm, but would they come? Three of them, Ambartsumian and the two others arrived at 9:30 pm, explaining that Zverev was sick. Batruchevitch was not there either. Since both Ruth and my mother-in-law spoke Russian, the guests enjoyed the dinner and it was a pleasant evening.

Through the channel of something akin to cultural exchange sponsored by the Soviet Embassy, I was invited to go to Moscow for the celebration in November of the anniversary of the Revolution. When I arrived in Moscow, I discovered that it was essentially a tourist visit, but I asked again and again to see astronomers. Finally, I had a meeting in the building of the Soviet-French Association with Kukarkin and Kulikovsky; Kulikovsky who spoke French also acted as translator. I was also invited to meet the Dean of Sciences. Because I was not a mathematician, he looked for an astrophysicist and found Alla Mashevitch, who came to his office. Meeting Alla was a great pleasure, for I was familiar with several of her publications on the internal constitution of



the stars. As we came out of the University (which at that time was in an old building close to Red Square), I left her at the bus stop; we both hoped to see each other again. Many years later, she told me that she desperately tried to see me again without success. When I insisted on seeing Alla Mashevitch again, I was told that she was sick, then that she was in her “datcha,” and finally, I had another meeting with Kukarkin and Kulikovskiy. It is difficult, after 45 years, to estimate the results of these meetings. Perhaps the simplest thing is to say that I did get acquainted with astronomical research in the USSR.

### *Stalinist Regime, Stalinist Antisemitism*

I had a painful experience in 1956. In order to make it understandable, I have to give some information on my family. My father-in-law, Joseph Ariel (Fisher), had been expelled from the USSR in 1924 for Zionist activities. He worked in the Keren Kayemeth LeIsrael in France. He had always closely followed the situation, events, and politics of the USSR. He settled in Israel in 1950 and became the Israeli ambassador in Brussels (1952–1956). He was terribly affected by the antisemitic campaign in the USSR at the end of 1952, the accusation against the Jewish physicians, and its consequences at the level of the French Communist press.

In 1956 I was invited to take part in a colloquium in Burakan. This took place a few months after the famous Khrushchev speech. Because of the softening of the Communist dictatorship I was able to stop for two days in Moscow before the colloquium and visit the two living sisters of my father-in-law. I stayed for these two days at the home of the oldest sister. She had had a French nanny when she was a young child, before the First World War, and spoke relatively good French.

I am still under the shock of that visit.

I heard stories of antisemitism. Israel Gradshtajn, who was the brother of my mother-in-law, had been expelled in 1952 from his Institute of Mathematics, but was accepted as a teacher of mathematics in a high school. Gradshtajn! The author of the famous book of mathematical formulae. He apologized for giving me only a Rumanian translation of an old edition of his book.

I heard stories about arrests and deportations. That was the way in which these two sisters had lost their husbands, one deported in 1937, the other in 1945. The last one was freed in the summer of 1953, came back to Moscow by his own means, and died six weeks later of dysentery.

I want to be clear. They were deported *not* because they were Jewish. They were just carried along to the camps with millions and millions of people. The existence of *gulags* was known officially at that time, but it was deeply moving to be in touch with people who experienced the Stalinist dictatorship. This was much more moving than the events surrounding Lysenko. French biologists

never accepted the claims of Lysenko, and I was aware of this early, due to the relationships I had with some of them in the Trade-Union of University Professors, especially the geneticist Georges Teissier and the biologist Marcel Prenant.

Coming back to western Europe, I stopped in Brussels to see my father-in-law and my mother-in-law. My father-in-law, who had a diplomatic passport, was waiting for me in the arrival hall before the police check point. I began immediately to tell him what I had heard and what I had learned. Despite his continuous interest in events in the USSR, this was far beyond what he actually knew, and his first reaction in this public place was to tell me to keep silent.

### *Sorrow*

This contact with Soviet reality was painful. I had given time and thought to this political activity, I had run risks, and I had been mystified, deceived, and betrayed. It was unbearable. Back in Paris, I visited officials of the Party to tell them that it was necessary to tell the truth, to have a critical look at the Stalinist dictatorship. I believe now that these minor officials were frightened. They rejected my arguments gently, as if I had been the victim of a lie and was some kind of irresponsible child. That is exactly it: What I was saying was childish, and they had only to reprove me in order to put me back on the right path.

All of that was useless. In fact, when I began to read the classics mentioned above, I had already considered that Marxist theories were just a step in the direction of a better understanding of history and of present society. Just a minor incident had stopped this thinking for a while. In 1948, I was giving a lecture in one of suburbs of Paris. I was trying to explain the *Dialectical Materialism* of Engels. One finds there a comparison between the discontinuous change from liquid to vapor and the discontinuous change of a society through a revolution. Then I explained that, just as we can bring liquid water to vapor water by going around the critical point, it was possible to avoid a revolution and to go directly to the new society. The result, during the discussion, was an acid criticism of this metaphorical description of historical changes in society, as if it had been against the Revolution. At that time, I concluded that I had to be respectful of the dogmas.

I have already mentioned the origin of my emotional link to communism. But in 1956 I was beginning to judge what happened in the USSR. It took me two years to decide to leave the Party. It was then 1959. It is perhaps difficult for the reader to understand this slow, too slow change of mind. I was able to reject Stalinism, but I still believed in the basic dogmas of Marx and Engels. The horrible dictatorship of the Communist Party in the USSR seemed to me an inheritance of Russian history, a deviation from the path drawn by Marx and

Engels. It was only in the 1970s that it became clear to me that knowledge of society is not of the same nature as science.

At that time, in the 1970s, it became apparent to me that I had not only been betrayed by the Communist Party, but that I had betrayed myself by the belief that it was possible to use a social, historical, and economic theory to build up a new society, exactly as it is possible to use a new physical theory to achieve a successful new physical experiment. I had the feeling that I had been a supporter of an unacceptable dictatorship, that I had been an accomplice, even if only in a very remote way, in unbearable crimes.

### *Philosophy*

What remains on one hand of this ideological experience is just a philosophy. If I use different words to speak of the physical, chemical, . . . sciences on the one hand, and the social sciences on the other hand, I would say first that it is not possible to give to “science” and to other “knowledge” the same basic epistemological meaning. Next, Nature can be known and understood, and the most fascinating research subject of a theoretician is to look at the contradictions between theory and observational and experimental data. Although this seems elementary (*elementary, my dear Watson!*), it nevertheless is a powerful research motivation.

On the other hand, I remain worried about the future of science. Education does not provide the majority of citizens with the understanding of the aim of science: knowing the laws of Nature. People confuse science and technique. I have the feeling that science is not really accepted by society<sup>20</sup> and that elementary teaching of science is not satisfactory and has not been adjusted to serve the future of mankind.<sup>21</sup> It is very difficult to transmit such a message. Can I dare to say that it is not only a question of understanding, but that all over the world most scientists, simply, do not care?

## RESEARCH

### *White Dwarfs in Saint Michel*

On July 1, 1943, I was in the hills of Haute-Provence, facing wild sunlight and heat. There were four families living in the Observatory: Fehrenbach, the assistant director; two technicians, Pissavin and Blanc; their wives and children; and Sellier (myself!) and my wife, Ruth. Later on, David Belorizky, an astronomer at the Marseille Observatory, and his small son, came to find

<sup>20</sup>There was a remarkable editorial on this subject in *Physics Today* (September 1970).

<sup>21</sup>I have tried to approach these questions in two books: *Science et Société* (1971) and *La science menacée* (1988).

a refuge from antisemitic roundups. Jean Daudin, a specialist in cosmic rays and a very active member of the resistance also found a temporary refuge there, with his wife Alice Daudin, who wanted to become an astrophysicist and finally remained after the war working in the field of nuclear physics. I was committed to help Fehrenbach as a night assistant. The library of the Observatory consisted at that time of just one cupboard of books. It included the first edition of the *Handbuch der Astrophysik*, the first edition of Unsöld's *Physik der Sternatmosphären*, the proceedings of the 1938 Paris colloquium on *Novae, Supernovae and White dwarfs*, the complete collection of *Zeitschrift für Astrophysik*, and a few other books. Fehrenbach advised me to read Unsöld. My knowledge of German was not good, and I had to translate what I was trying to read into French. I translated perhaps one hundred pages, but I did not understand their meaning until I had to practice radiative transfer three years later. In order to give a scientific life to the Observatory, Fehrenbach organized a seminar for the five scientists who were there. He asked me to give an account of the 1938 meeting. It turned out that, having attended the lectures of Francis Perrin in 1941 on statistical mechanics, I knew Fermi-Dirac statistics and I could easily understand the problem of the internal structure of white dwarfs. It also happened that some time before I had read in a popular science periodical, *Science et Vie*, a paper written by Paul Couderc on stellar energy sources in which he cited the name of Bethe. I gave the CNRS an order for a microfilm of Bethe's paper. I found an older paper of Gamow's in *Z. Astrophys.* on thermonuclear reactions and discovered immediately that a white dwarf having the same chemical composition as the Sun would explode. Hydrogen should not be present inside white dwarfs. I also found that, due to the high gravity field in white dwarfs, hydrogen should float to the surface, and that the size of the transition region from pure hydrogen to metals had to be very thin.

Can I mention the origin of my passion? Doing research gave me great pleasure. I had the impression of exploring unknown countries, discovering one after the other extraordinary things, and bringing them piece by piece into the realm of knowledge.

When I arrived in Paris, in October 1944, I had to study for the competitive exam called the *Agrégation*, which at that time was compulsory for ENS Éléves and was a prerequisite for a career as a high school teacher. On the other hand I wanted to finish the first step of a research project leading to the doctoral degree. However, the date at which the competitive exam was to take place was not known, and it was not even known if there was going to be one during war time. This gave me the opportunity to continue my research.

However, it was also necessary to attend lectures and perform student duties. Starting the study of "classical" physics and chemistry again was difficult after

having experienced research activity. It was necessary to know all classical physics and all chemistry to be ready for the exam. And both fields were much less attractive than quantum physics and statistical mechanics. I remember the difficult conditions of the winter of 1944–1945: not much heating, just a very small stove. I made myself a vest with several layers of paper cut out of newspapers (good thermal insulation!) but had the greatest difficulty in using it, due to the noise produced by any motion.

Finishing my work on white dwarfs was very absorbing, and I visited Louis de Broglie several times, bringing him manuscripts to be published in the *Compte Rendus de l'Académie des Sciences*. These short papers, called *Notes*, were published during the winter of 1944–1945.

It was during these months that I became acquainted with Jean-Claude Pecker, who was also finishing his studies at the ENS. His parents had been deported in May 1944 and he was deeply affected by this. I remember walking with him in the corridors of the school and talking, talking about everything, but essentially about astrophysics. From that time on we have had a friendship, a brotherhood that has never weakened.

Finally, we learned in March that the written part of the competitive exam was going to take place in April. I hurried up, desperately reading hundreds of pages of physics and chemistry. The oral part took place three months later. Thirteen young physicists were successful. I was the thirteenth. Undoubtedly, initiation in research work was not a good preparation for a competitive exam!

Many years later, in 1967, I was elected as a member of the CNRS Scientific Committee of Astronomy, Astrophysics and Geophysics, and elected immediately afterwards as a member of the Scientific Council of the CNRS. There I met the well-known physicist, Louis Néel, a Nobel prize winner, who had been the chairman of the *Jury d'Agregation* in 1944. The problem he had given in 1944 consisted in building up, step by step, the theory of the Lyot monochromator. He told me what I felt was perhaps the best compliment I ever had: “I have recently been sorting out old papers. I found my notes concerning your part in the competitive *agregation*. And it was clear that I was not mistaken about you.” After the exam, successful candidates were received by the chairman, and I was asked which kind of position I wanted. I said that I was expecting a fellowship in the States. This was not true, but it was not wrong either, as GP Kuiper, in the fall of 1944, had offered me an invitation to the States. Because I had experienced illegal life, escaping from the compulsory working duty, I was considered a special case, and I avoided having to take a teaching position in a high school.

I still had to complete the writing of my thesis and I worked hard to have it published in due time in the *Annales d'Astrophysique*. I presented it in March 1946 to the examining board that had Louis de Broglie as chairman.

### *Chargé de Recherche*

I applied for a position in the CNRS. My file happened to be supported by Francis Perrin and I finally got a position in physics in the fall of 1945, although it was not until February 1946 that I began to get the salary of *Chargé de Recherche*. The rank of the position was very exceptional, as I was entering the CNRS not at the lowest rank of *Attaché de Recherche*, but at the rank above. In fact, I think that for a beginner who had not yet obtained the doctoral degree (I got it in March 1946), this has never been done again.

A relatively short time after publication of my thesis in *Annales d'Astrophysique*, I received a very nice letter from L. Biermann in Munich, drawing my attention to papers I had not quoted. I gave a curt answer, mentioning the deportation of my father and the difficult conditions under which I had been working at St. Michel. I met Biermann much later at a meeting on the edge of the lake of Tägernsee in 1959. He wanted to talk with me; we entertained Biermann and his wife at home in Paris a couple of weeks later. Biermann said that he had no knowledge of the extermination camps during the war. We met the Biermanns again in Boulder in 1968 and in Copenhagen in 1978, on the occasion of the seventieth birthday of Bengt Strömgren. Our relationship with Biermann has been rare but friendly.

### *Institut d'Astrophysique (IAP)*

I became a member of the Institut d'Astrophysique in Paris. During my stay in St. Michel, I had known Daniel Chalonge and Daniel Barbier. At that time the IAP building was not finished, and I did not get an office. I was working at home, often visiting the IAP. The library was very small—it was in a room that later became one of the guest rooms for IAP visitors. Henri Mineur was the Director. Chalonge and Barbier had offices. Volodia Kourganoff was working in a tiny space, which had become the kitchen of the guest area. I had lots of discussions with all of them. Kourganoff was beginning his work with Chalonge on the role of the negative hydrogen ion in solar limb darkening. Several times I met Rupert Wildt who often visited Chalonge. He was famous for his paper on the negative hydrogen ion. Tuominen arrived in 1946; he got a position in Paris for two years and an office at the IAP. I talked with him almost every day.

I had constantly in mind the proceedings of the 1938 Paris colloquium, and taking into account a remark of Biermann on instabilities, I imagined a model of collapsing white dwarfs, leading to a supernova explosion. In 1939 Zwicky had considered the collapse of a white dwarf into a neutron star. I had no knowledge of Landau's model of neutron stars, and my theory of supernovae was based on the idea of an instability leading to an explosion and the ejection of matter by a shock wave. Strangely enough, the orders of magnitude of radius,

stellar density, and velocity of ejection were not so far from what we presently know, even if our knowledge of SN maximum luminosity was wrong by six or seven magnitudes. Altogether the theory was incredibly naive. Barbier, who was editor of *Annales d'Astrophysique* submitted my paper to Strömgren. Strömgren turned out to be very laudatory, and my paper was published in 1946. In July 1948, a short time before the IAU meeting in Zürich, I received a very kind letter from Paul Ledoux, criticizing my view on instability. There was some kind of heroism in building up theories based on an incomplete knowledge of physical processes with the aim of explaining poor observational data.

I considered thermonuclear reactions as having a great astrophysical importance, and I continued several years to work on the subject.

Barbier wanted to keep me in astrophysics. At that time, I often visited Edmond Bauer, formerly a Professor in Strasbourg and later Professor in Paris. He had his office at the Institut Curie, and I had him as a professor for the preparation of the *Agregation*. He told me once that he had convinced Danjon, during the time he was in Strasbourg, of the reality of quantum mechanics. These ideas of modern physics were seductive. Bauer persuaded me to apply for a position in Dublin, to study quantum mechanics with Walter Heitler. I began to exchange letters with Cecile Morette (now DeWitt) who was working there, but before any conclusion was reached, Barbier obtained the agreement of Bengt Strömgren to receive me in Copenhagen. I was sufficiently involved in astrophysics to accept the invitation immediately.

My first initiation into astrophysics was in St. Michel. For my second initiation I was to have the guidance of a prestigious astrophysicist and so did not to have to use books alone.

When I arrived in Copenhagen on April 9, 1947, the anniversary of the invasion of Denmark in 1940, the street were almost empty. I had little money, was afraid of boarding a streetcar with my small knowledge of Danish, and did not take a cab. I walked from the railroad station to the Observatory, appreciating the beauty of the city.

### *Copenhagen*

Strömgren was waiting for me. I asked him: "What shall I do?". His answer was "You have worked on the inside of white dwarfs; now you should work on white dwarf atmospheres." Rudkjöbing helped me to find my way. I had no doubt that I had to study a pure hydrogen atmosphere. I started reading Unsöld, having completely forgotten my reading of *Physik der Sternatmosphären* four years before. I discovered the limitation of the number of excited states, due to the presence of a microscopic random electric field and pressure ionization, and I finally computed atmosphere models and line profiles with a small desk hand-computer. I was so much seduced and attracted by the publications of the

Copenhagen Observatory that I asked Strömgren to have my work published in this series. I did not know at that time that it is better to publish in international periodicals. That work, which was published only in 1950 because the Copenhagen Observatory did not have the money to publish it earlier, remained entirely unknown. It was the first complete description of pressure ionization and the Stark broadening of hydrogen lines in white dwarfs. The MHD (Mihalas, Hummer, Däppen) paper, published in 1989, was certainly much more elaborate than my 1950 one. But I confess that I was not pleased at not being quoted! I wrote to Mihalas and discovered that he was completely unaware of the existence of that paper. I sent him a photocopy of it. Many thanks to Mihalas for his apologies.

I regularly attended the seminar at the Bohr Institute and enjoyed having a direct view of the most recent developments in physics. I had an appointment with Niels Bohr, gave a lecture on white dwarfs, and became acquainted with a number of young physicists, including Cecile Morette, with whom I had the exchange of letters about Dublin.

### *Beginning an Astrophysicist's Life*

I was still in Copenhagen when I received an invitation from Lyman Spitzer for a stay in Princeton in 1948–1949. I was going to have a fellowship, a sum that seemed large to me. The aura of Princeton was such that I felt much flattered and honored. I accepted immediately.

In the spring of 1948, Danjon asked me if I would be willing to teach astrophysics at the University of Paris. Danjon was teaching fundamental astronomy and Jacques Lévy, an astronomer at the Paris Observatory, was taking care of training students. These courses were given under the common title *Astronomie Approfondie* and each of us had to lecture two hours a week, during the first semester, from November 1 to February 28. As far as I know, this offer was made on the advice of Otto Struve. I felt very flattered. I accepted, but as I was going to the States, this appointment had to be postponed until the fall of 1949. When I came back from Princeton in June 1949, I went to see Danjon and asked him if he was still willing to give me this lecturing opportunity. It was the beginning of a long story. I taught for 27 years, but I shall tell more later about this extraordinary experience.

In spring of 1948, I was invited by Jacques Cox, former Rector of Brussels University and a professor of astronomy, to give a few lectures at the University. I remember speaking about supernovae. There were in the audience several well-known professors: Émile Picard, who went up in balloons to measure cosmic rays; Bourgeois, the Director of the National Belgian Observatory; and naturally Jacques Cox. At the end of my stay, Cox asked me if I would agree to teach astrophysics at Brussels University. I was not going to have a full position



there, just a small salary, enough to pay for my trips from Paris and my hotel expenses. But this gave me a feeling of recognition, and I accepted. I started this teaching also in 1949. I kept this position for 15 years.

## RESEARCH AND UNIVERSITY EDUCATION

### *Learning New Fields of Physics*

Arriving in Princeton, I had no precise subject in mind, except perhaps some concern about solar granulation, resulting from discussions with Jean-Claude Pecker. It was through discussions with Martin Schwarzschild and Lyman Spitzer that I came to the two subjects I dealt with during my stay in Princeton: heating of the solar corona by shock waves and the effect of a  $\mu$ -gradient due to gravitational settling on a white dwarf structure. I talked much with Martin and Lyman and discovered the way of thinking of theoreticians who had a great knowledge of observational data. They were critical and direct in the discussion. I had the feeling of my great ignorance: The trouble is that all possible physical processes can be important in astrophysics.

Let me give an example here. During June 1950 I spent a week at Leiden Observatory, gave a lecture on white dwarfs, and talked much with Oort and Van de Hulst. Oort asked me if I could give an explanation of some spectroscopic properties of the Crab Nebula. After my visit, I spent a couple of months working on the problem of radiative transfer in the nebula and did not find any explanation, so I finally dropped the subject. Schklovsky, who had many more relationships with physicists than I (as he told me many years later), concluded, as did others, that the continuum of the Crab was due to synchrotron radiation.

When I started lecturing on astrophysics in Paris, in the fall of 1949, Danjon left me free to choose the program. I felt that I had to cover the whole field, from stars to galaxies and cosmology. I had to learn how to present basic data from the Hertzsprung-Russell diagram and binaries to the structure of galaxies. This experience, which lasted 27 years, was also the origin of many publications, owing to the number of cases presenting unexplained facts, which actually turned out to be easily explainable. This is the reason for the great variety of subjects that I approached, even if they remained essentially in the field of stellar physics: the structure, evolution, and origin of stars.

### *Scientific Publications and Reputation*

If I remember correctly, I once heard Otto Struve saying that Soviet astronomers had been clever enough to avoid something similar to the famous catastrophic Lysenko affair. Their solution had been to organize meetings on cosmogony and to publish yearly *Questions of Cosmogony*. Kukarkin, during the IAU meeting in Rome, asked me to write a report on *Cosmogony and Cosmology in Western*

*Countries.* I collected about four hundred microfilms during the academic year 1952–1953, and during summer vacation I wrote a long paper, which was published in this Soviet periodical in 1954. I felt proud of that analysis, which was the result of quite hard work, but I did not like having it published only in Russian. The French text, after undergoing a few modifications to make it sufficiently popular, was published in France by the publisher Albin Michel (1957), under the title *Origine et évolution des mondes*. A Spanish translation by Raquiel Rabiela de Gortari and Arcadio Poveda was published in 1960. In between, I had been asked in 1959 to bring the book up to date, in order to have it published in English. Spending one month in Pasadena in September 1959, I discovered a fantastic amount of literature on cosmogony and cosmology published between 1952 and 1959. I felt unable to achieve a complete updating and finally wrote just a few adjustments. It is true that, at that time, I was working on the loss of angular momentum in stars, which was the continuation of the short paper I had given during the IAU meeting in Moscow in 1958. I was more eager to complete a paper on this subject than to write corrections to the book on cosmogony and cosmology. Anyhow, I sent these few corrections to the publisher and the English translation, by Annabel and Bernard Pagel, of *The Origin and Evolution of the Universe* was published in 1966.

The draft of the paper on loss of angular momentum was finished when I left Pasadena at the end of September 1959, and a preprint was available.

It is true that my publishing *Questions of Cosmogony* confirmed the rumor about my membership in the French Communist Party. During an IAU meeting, I was trapped by GP Kuiper and W Iwanowska; the latter was Director of Torun Observatory, whose fiancé had been killed in Katyn. They asked me to give my opinion about this horrible murder of 15,000 Polish officers in Katyn. Was it the Nazis or the KGB?

In 1967, after the death of André Danjon, I decided to be a candidate for membership in the Academie des Sciences. At that time, the rule was to pay a visit to academicians, to introduce oneself. Francis Perrin who had helped me in 1945 to get my position in the CNRS asked me: “Is it true, as Danjon told me, that you stopped working on the internal constitution of the stars, because it was condemned by Russian communists?”. This was incredibly surprising as I had never stopped working on this subject. I still remember the bright look in Perrin’s eyes. Can I mention here the fact that several people never forgave me for my former membership of the Communist Party? They did not want to see me joining the highest institutions.

In 1954 I received the offer of a professorship in Manchester. I did not want to leave France. Continuing to contribute to the development of astrophysics in France was a way of expressing my gratitude for the help I had received

during the war. Because of the political situation and the statutes of CNRS research workers, was there also a threat of losing my CNRS position? There were rumors about it. I asked Danjon if I could become a university professor in Paris (actually, an associate professor or something equivalent). He agreed to support my application and I secured the new position in October 1954. As noted in the last edition of *Petit Larousse*, this was the creation of the the first Astrophysics Chair in France.

It was in these years that I asked JC Pecker if he would be willing to write an astrophysics textbook with me. On the advice of Alfred Kastler we approached the publisher Masson, and *Astrophysique Générale* was published in 1957.

### *Students*

Almost immediately after starting my astrophysics lectures, I began to have contact with students who wanted to prepare their PhD with me. The French doctoral degree, at that time, was obtained after several years of research: Four or five years was considered normal. People preparing a doctoral degree already had jobs, such as *Attaché de Recherche* (CNRS position), assistant lecturer at the University, or assistant astronomer at the Observatory.<sup>22</sup>

When discussing topics with a beginner, I would mention a variety of unsolved theoretical problems until enthusiasm sparked in the mind of the student. Some of those attending my lectures at the University got interested in important questions. They found their research subject by themselves, asking me only to be their sponsor and the person to talk with. I have sponsored many doctoral students; I do not remember how many. Some of them were remarkable. This situation opened up the possibility of developing theoretical astrophysics in France.

A symposium on plasma physics in Stockholm (1955) gave me the firm belief that it was absolutely necessary to develop this field in France. With Jean-Loup Delcroix, Jean-François Denisse, and Theo Kahan, I organized in the fall of 1955 a graduate course in plasma physics, which immediately met with great success. I taught magnetohydrodynamics. This interest in plasma physics and magnetohydrodynamics led me to meet R Lüst, A Schlüter, E Parker, and many others, and I kept in touch with them for several years.

There was a great change in the physics programs in 1957, and I succeeded in having astrophysics become one of the optional subjects of the undergraduate courses for the *Licence*. Next I found that it was necessary to create a graduate course in astrophysics (1961). As I could not deliver courses on all subjects by myself, I solicited the help of many fellow astrophysicists. Some of these

<sup>22</sup>The present situation is completely different. Essentially, students have to complete their doctorates in two or three years. They have a fellowship.

courses were very good. This graduate course in astrophysics was the origin of the training and recruiting of many astrophysicists, both French and foreign.

After creating the graduate course in astrophysics in 1961, I found that leading these two teams of graduate studies was too heavy a load, so I gave up plasma physics in 1969. This experience gave me some training in both plasma physics and MHD and is the origin of several applications of this knowledge to astrophysics.

University studies were reorganized again in 1967. This afforded the chance to teach astrophysics at three levels: beginning, for undergraduate studies (optional), advanced, for undergraduate studies (optional), and, as mentioned above, graduate studies.

### *School of Astrophysics*

Graduate courses in astrophysics and plasma physics attracted students. Whereas the numbers of girls and boys attending the courses in plasma physics were respectively 1 and 20, or something like that, there was roughly an equal number of girls and boys attending the astrophysics courses. Many of them remained in the field and had brilliant careers. There was little space in the Institut d'Astrophysique, and at the beginning, despite the fact that the number of young research workers was small, they were squeezed together on the second floor. In 1964 I started the necessary steps that led to the building of the Laboratoire d'Astrophysique de Meudon or LAM, inaugurated in 1972.

In Princeton I discovered the value of a weekly seminar. Nothing like that had been put into practice in the French astronomical community. I introduced it at the IAP as soon as I came back from the States in 1949. This gave a new life to the IAP.

I never noticed any differences between male and female students, except perhaps that, at the beginning of their careers, girls were more shy than boys. I cannot give names here. I do not remember all of them, and naming some might make the others jealous.

Today I find myself surrounded by the product of what I can call the "astrophysics school." In other words, there are now in France many people with whom I can talk about astrophysics; many of them have reached a level that I consider higher than mine at the same age. When I was young, traveling mainly to the States and taking part in meetings was not only a way of becoming known to foreign colleagues, but was necessary to find people to talk with. This time has passed.

About 250 students have taken my graduate courses. Perhaps two thousand students have taken my undergraduate course. I have often discovered that some of my younger colleagues have attended my lectures. Many of my former students are now in charge of astronomical life in different places. There

are now groups of astrophysical theoreticians in Nice, Toulouse, Lyons, and Strasbourg (chronological order). Ramon Canal is at the origin of such a group in Barcelona.

This clearly shows that theoretical astrophysics now occupies its proper place in France.

### *Publications*

Beginning in the fall of 1949, in order to prepare my lectures at the University, I had to get acquainted with the most recent literature. And it was also necessary, for the consistency of the lectures, to have at least some knowledge of the large variety of fields of physics involved in astrophysics. It was easy, years ago, when considering a physical property, to notice references to an obvious physical process. This is essentially the origin of the large number of papers (more than 200) that I have published. When I talked with Van de Hulst in the 1960s, he already noticed that I had not specialized in a narrow domain of astrophysics. In recent years, this rate of publishing has decreased. Does the writing of a paper at the present international level take more time? I would like to mention here what I consider my most important papers.

I first recall some of the work mentioned previously.

The obvious fact that the vertical scale of gravitational separation was much smaller than the radius of a white dwarf led me to the description of white dwarf structure (Schatzman 1946).

Similarly, to give a proof of the absence of hydrogen inside white dwarfs I found it possible to take into account the effect of screening on thermonuclear reaction rates (Schatzman 1948b). For this, I used knowledge of the Debye field that I had acquired in 1941. Great improvements in this theory were made by Van Horn & Salpeter (1969) and by DeWitt, Graboske & Cooper (1973). Many years later, I convinced Bernard Jancovici, from Orsay University, to apply better statistical methods to the treatment of the thermonuclear rate at high density. The result was an important paper by Alastuey & Jancovici (1978).

The conditions of wave propagation in the solar atmosphere, and my knowledge of the properties of shock waves, led me to a theory of the heating of the solar chromosphere and corona (Schatzman 1949). I knew nothing at that time about plasma physics and was unable to take into account the presence of magnetic fields.

The fact that after a nova outburst a star is still visible means that internal energy of the star is certainly larger than energy of the explosion. This means that the star has a small radius and is necessarily a white dwarf. I already had proposed the idea of a shock wave in novae (Schatzman 1948a). Reading a paper by Alpher on the origin of the elements, which included cross section estimates, led me to the description of the pp I cycle closing by the  ${}^3\text{He}({}^3\text{He}, \text{pp}){}^4\text{He}$  reaction, and I considered the possibility of a detonation wave due to that

reaction (Schatzman 1951). However, the difficulty of starting the detonation led me to a theory of novae based on the idea of instabilities generated by tidal effects in a white dwarf companion in a binary (Schatzman 1958). Furthermore, it raised the possibility of explaining the presence of rings in the gas cloud ejected by a nova. I greatly appreciate the fact that Sumner Starrfield studied these papers carefully before beginning his work on novae. It is well known that he studied the role of accretion. Why did I not consider that possibility? Herman Bondi and Fred Hoyle had produced an accretion theory that ignored the role of radiation. I have shown, at the 1952 Cambridge IAU meeting, that because of ionization due to radiation the rate of accretion was much smaller (Schatzman 1955). The consequence of this result is that I had in mind the idea that accretion was not important.

The polarization of light by interstellar matter was supposed to be due to the presence of anisotropic particles. However, there was no model of formation of such particles, and there was the question of the magnitude of the interstellar magnetic field. In a lecture at the IAP, Pol Swings had mentioned the presence of soot particles in the atmosphere of N-dwarfs. Subsequently, Cayrel and I considered the possibility of polarization of light by interstellar graphite particles (Schatzman & Cayrel 1954). May I mention again a case of nostalgia? In 1962 Hoyle and Wickramasinge made the same assumption but ignored my paper. We know today that this model is too simple and disagrees with the observed wavelength dependance of polarization.

After attending a remarkable lecture by O Struve in the 1950s at the Institut d'Astrophysique and reading his book on stellar evolution, I got the idea of looking at the question of loss of angular momentum. But losing angular momentum from stellar surfaces meant too much mass loss and was incompatible with our knowledge of stellar evolution and the absence of a big cloud around main sequence stars. When I began to learn plasma physics in 1956, I proposed the idea of spindown due to mass loss in the presence of a magnetic field (Schatzman 1959, 1962). I still think that the relatively large magnetic field above sunspots is more important than the weak one around the whole star.

Another product of my teaching of magnetohydrodynamics is the idea of an acceleration mechanism of cosmic rays by shock waves (Schatzman 1963).

Since my early years at the Observatory of Haute-Provence, I have been skeptical about the idea of complete quiescence inside the stars. I began to look at the effect of macroscopic diffusion on gravitational separation (Schatzman 1969), then I looked at the generation of turbulent diffusion by differential rotation (Schatzman 1970), considered the effect on lithium (Schatzman 1977)—but unfortunately used a bad model of giants, and finally got the idea of applying turbulent diffusion to give an explanation of the solar neutrino de-

ficiency (Schatzman et al 1981). The oversimplified diffusion model led to results in disagreement with helioseismology data.

Talking with Roland Omnès in the late 1960s about his idea of annihilation in the early Universe, I gained at least some knowledge of the annihilation process. In the early 1980s, I was approached by Jean-Marie Souriau about his result (presently not accepted by cosmologists) concerning the presence of a missing zone of quasars. He found that to explain the space distribution of quasars it was necessary to assume a spherical, closed Universe, with a nonzero cosmological constant. His main idea was that half of the sphere was made of matter and half made of antimatter. From this picture I tried to see if the order of magnitude of the annihilation processes and its effect before galaxy formation would explain the size of the missing zone. I was surprised to find the proper order of magnitude. Why not try to do better? With Xavier Desert, we published a paper on “Physical constraints on the bi-partition of the Universe” (Desert & Schatzman 1986), where the theory of the missing zone was in agreement within 10% with the results of the group of Marseille-Luminy.

The difficulty of having a consistent model of lithium depletion (Schatzman & Baglin 1991), and discussions with Zahn, led me to take into account another physical process. Random gravity waves can generate a diffusion process. The first step is to obtain an estimate of the amplitude of gravity waves generated in the radiative zone by turbulent flow in the convective zone. The second step is to give an estimate of the diffusion coefficient. I started this work in 1991 and I am not yet through. This diffusion process provides a good contribution to the understanding of lithium depletion. But the problem of neutrino deficiency is much more difficult. Being a member of the GALLEX group since 1984, I have signed (with the other members) several papers concerning the results of neutrino counting. The first papers of the series were published in 1992 (GALLEX 1992a,b). The GALLium EXperiment uses a tank containing 30 tons of gallium, including 12 tons of  $^{71}\text{Ga}$  (exactly  $1.044 \times 10^{29}$   $^{71}\text{Ga}$  atoms). The 1995 result is an average solar neutrino flux of 87 SNU (SNU = solar neutrino unit =  $10^{-36} \nu \text{ cm}^{-2} \text{ s}^{-1}$ ). This is in agreement with the results of the Russian-American experiment (SAGE), but in complete disagreement with the predictions of the Standard Solar Model (SSM) of about 130 SNU. There is presently a preference for explaining the discrepancy between the experimental results [Homestake goldmine chlorine experiment, GALLEX and Kamiokande, the direct counting of high-energy neutrinos (above 7 MeV)] and the predictions of SSM by matter enhanced neutrino oscillations: the so-called MSW effect (Mikheyev, Smirnov, Wolfenstein effect; e.g. GALLEX 1992b). Considering that there are still many uncertainties in solar models, I think that, before taking into account the MSW effect, it is necessary to assume massless neutrinos and

to try taking into account more physical processes and their effect on stellar structure, to explain the deficiency.

### *Life of a Scientist*

Since 1957 I have taken part in a large number of colloquia, symposia, and meetings, and I have been invited abroad to give lectures or courses in summer or winter sessions. I was invited to give astrophysics lectures at the 1959 Summer Course of Les Houches and contributed to the organization of the 1966 Summer Course on High Energy Astrophysics. I was invited by Robert Oppenheimer to take part in the 1958 and 1964 Congrès Solvay, and I was invited again to the 1974 one. Leon Van Hove invited me to spend one year at CERN, and I came for the academic year from October 1969 to September 1970. Because of these opportunities, I feel that I have had a better relationship with the international scientific community than with the French one.

My ideas and my work have been appreciated by H Mineur, D Chalonge, D Barbier, B Strömgren, M Schwarzschild, and L Spitzer, and A Danjon gave me the task of teaching astrophysics at the university level. But because I was kept away from scientific responsibility by French astronomers of an older generation than mine, and felt that most of them never gave me proper credit for my work, I have always had the impression of being kept at the border of the French scientific community. This explains why it was not until 1971 that I had for the first time a real feeling of recognition in France. I got the Robin award from the French Physical Society and delivered a speech at the meeting of the Society in Evian. Although in 1966 I had received from the Académie des Sciences the award in memory of the French scientists killed by the Nazis during the war, I could not forgive the fact that the short description of my scientific work, giving the reasons for the award, was completely wrong: It referred to a research field that I had never touched! I felt misunderstood. In 1975, I got the Holweck award given both by the Institute of Physics of Great Britain and the French Physical Society. I gave a lecture in Cambridge and, following what I considered as a great honor, I was elected President of the French Physical Society (1976–1977). That same year, I founded the French Society of Professional Astronomers (SFSA or *Société Française des Spécialistes d'Astronomie*), which is now an effective association.

After the student revolution of May 1968 (in which I took much part) the number of meetings at the University increased so much that I felt that my duties were becoming more and more heavy. On January 1, 1976, I again became a member of CNRS as *Directeur de Recherches*. I gave up the direction of my small laboratory of theoretical astrophysics and moved part time to the Nice Observatory. It gave me the chance to be close to my four-year-old son and his mother Annie Baglin. I appreciated the working conditions in Nice. When



he was Director of Nice Observatory, JC Pecker had organized an excellent library there. I was delivered from all obligations, which meant being a full-time research worker. This marked the end of lecturing and the beginning of more efficient research activity again.

With the new generation, I had other relationships with the French scientific community, and it is under these conditions that I got the Gold Medal of the CNRS in 1983. The situation in the Académie des Sciences had changed and I was presented as a candidate by JC Pecker; I have been told that I received excellent support from several physicists, and I was elected in 1985. I was named a member of the High Scientific Council of Paris Observatory for four years in 1989 and named a member of the Advisory Committee of the CNRS High Administration of Astronomy and Geophysics (INSU or Institut National des Sciences de l'Univers) in 1994. It was the "young" generation who organized a large meeting in 1990 to celebrate my seventieth anniversary. The proceedings have been published by *Union Rationaliste*.<sup>23</sup> Jean-Claude Pecker (1991) gave a complete description of my scientific career; Jean-Paul Zahn (1991) gave the impression of beginners in the 1950s to 1960s. I want to quote his testimony concerning my relationship with the astronomical community:

We were admiring him also for his commitments in politics, trade unionism, and philosophy. We knew these commitments only indirectly, as he had not allowed himself to proceed to any kind of proselytism in the working place: Commitments that have not always been understood and have produced strong enmities. Should I confess that we are still deeply shocked by the late recognition of his role by the official authorities of our community?

This confirms the fact that professionally I never mixed science and politics, and it justifies my feelings concerning my relationship with the community. I conclude with the 1993 Jubilee colloquium "Physical Processes in Astrophysics," organized by Ian Roxburgh with the chairmanship of Lyman Spitzer, and, to my great surprise, announced by a wonderful poster drawn by Pecker. It was terribly moving.

Things have changed since the old times!

I retired in the fall of 1989. I still have an office at Meudon Observatory, I am still the leader of young beginners, and I am trying to remain as active a scientist as possible!

<sup>23</sup>The *Union Rationaliste* was founded by Paul Langevin in 1932. I am presently its president. The *UR* is a small association that bears some similarity with the American Association, *CSICOP*, whose president is Paul Kurtz, Professor emeritus of Philosophy, State University of New York at Buffalo.

### *Returning to Philosophy*

We have in France the philosophical heritage of Auguste Comte's positivism. We can smile now when recalling that Comte said that we shall never know the chemical composition of stars, as we shall never be able to go there and take a sample. It is already remarkable, after all, that he was assuming that stars were made of matter! But this idea that we can know only what we can touch has not disappeared. There are still many scientists in France who are skeptical about any kind of theory. We can naturally smile again when we look at the book of Georges Bruhat, *Les Etoiles* (1937), and notice in the foreword that he was not going to write about the interiors of the stars, as we shall never go there to touch them. Remember that Eddington's book was published in 1926 and that there were many papers on the subject in *Ap. J.*, *MNRAS*, and *Z. Astrophys.* But in 1971, a few months before moving from the Institut d'Astrophysique to the new laboratory that I created in Meudon (LAM, now world renowned), I was talking with André Lallemand, then Director of the IAP, about people who were moving with me to Meudon. Suddenly he said : "You, theoreticians, you are not concerned with the important things." I was shocked and I replied by mentioning some of my subjects of research. He stopped me saying: "Oh! You are different, but the others. . .". That sentence was painful for me. It expressed such a strong prejudice against theory that I find it necessary to emphasize this. A large fraction of French astronomers are "instrumentalists." Some of them are remarkable inventors and are essentially interested in instruments that bring in new data. But they do not understand that this is not enough. We must also be concerned equally with the physical problems that can be solved by obtaining proper data. Of course not every one is like this. We can consider, for example, the case of Lacroute, a man of the same generation as Lallemand. When I met Lacroute for the first time in 1946, he questioned me with great interest about the work of Bethe on thermonuclear energy sources. He is behind the *HIPPARCOS* space program , which is producing an incredible amount of data (luminosity and, for binaries, better values of masses) important for the understanding of stars.

### *Message*

I have always tried to transmit my message. Astrophysical objects are the seat of a large variety of physical processes. Understanding the phenomena rests on the knowledge and the understanding of those physical processes. Philosophical ideas play an important role. The philosophical conviction of the reality of the world surrounding us is important; it inspires the search for these processes that we do not see, but which are the origin of what we observe. This intellectual behavior is so deeply rooted in me that it has always been working

unconsciously. I can even say that it was more by an instinctive materialistic attitude (Louis de Broglie used the word *realistic*, but with the same meaning) than by any philosophical reasoning that I considered gravitational separation in white dwarfs or electron screening in thermonuclear reactions. I think that it is important to keep in mind the occasional contradictions between models and observational data. It is a glory to improve the fit between theory and observations, but there always remains some discrepancy. I think that the most important attitude is to find which forgotten physical processes are responsible for something we do not understand. I have tried to transmit this message to the scientific community.

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