AUTUMN BOOKS

Prometheus of the atomic age

Otto R. Frisch

Scientists in Power. By S.R. Weart. Pp.343. (Harvard University Press: Cambridge, Massachusetts, and London, 1979.) $17.50; £11.35.

IT is widely known that Enrico Fermi and his team started the first controlled nuclear chain reaction on December 2, 1942, in Chicago. This book tells us how a Frenchman, Frédéric Joliot, rather than Fermi, might have become the Prometheus of the atomic age, but for the German occupation of France. Madame Curie had picked the promising youngster for her assistant in 1925; she soon saw that he was a "skyrocket". Two years later he married her daughter Irene, the chemist, and thus entered the scientific circle, in the highly stratified society of Paris. Some saw him as a gate-crasher; and he always felt happiest with workers or fishermen.

His research at first brought him some disappointments. In 1931 he published observations which led James Chadwick to the discovery of the neutron and to the Nobel Prize; the next year he narrowly missed the discovery of the positron. But then at last came success: early in 1934 he and his wife published their observation of "artificial radioactivity". That important discovery brought them jointly the 1935 Nobel prize for chemistry and secured for Joliot the first Chair for Nuclear Chemistry, at the Collège de France.

In January 1939 Otto Hahn and Fritz Strassmann published their famous paper which indicated the fission of uranium nuclei struck by neutrons; Irène Joliot-Curie, who had narrowly missed that discovery, "bitterly regretted that she and her husband had not worked together on the problem, for their combined brains might well have solved it". At this point, about a quarter through the book, begins the main story.

Within a few days, Joliot had seen the stupendous implications of that new phenomenon: the possibility of a nuclear chain reaction which could release energy on a large scale. Leo Szilard had been looking for such a reaction since 1934, and Joliot had said in a public lecture in 1935 that "compared with the neutron, the philosophers' stone of the alchemists was a childish fantasy; doubtless people would soon be able to change lead into gold, but this would be an insignificant by-product compared with the enormous quantity of energy that would be released". Now, he felt, it was up to him to create that chain reaction which would present almost unlimited energy to France and hence to the world.

First of all he had to make sure that the fission of uranium nuclei released neutrons, as expected. Hans von Halban, son of an Austrian-born professor, was in Paris and keen to join Joliot in tackling that tricky problem, having gained experience with neutrons in Copenhagen (jointly with the reviewer). He in turn recruited Joliot's "little typist", a huge Russian called Lew Kowarski who was determined to become a scientist and did secretarial work to get a foot in the door and to supplement his meagre income from design work in a factory of gas pipes. Trained as a chemical engineer (see my obituary notice of Kowarski in *Nature*) he brought a valuable new way of thinking into the team. Those were the three musketeers who soon agreed that in publishing their joint work they would always share the credit for all ideas, whoever had thought them up.

By the end of February they knew that a chain reaction was not impossible: uranium produced more fresh neutrons than it swallowed. But were there enough of them? Some would escape from the material or be absorbed by its impurities. The team had to know the average number of neutrons released when a single uranium nucleus suffered fission, and by the end of April they thought that number was about 3.5. Today we know that the number is nearer 2.5, but perhaps the error was to the good; it made a chain reaction look easy to achieve and thus encouraged the team to redouble their efforts.

From the beginning they had studied uranium salts dissolved in water, in a tank about 1 m in diameter, with a neutron source at the centre. The neutron distribution in the tank was measured by placing small foils of dysprosium at various distances from the source and, after a standard time, measuring their radioactivity with a Geiger counter; that gave information on the number and the average speed of the neutrons. The water prevented most neutrons from leaving the tank and thus escaping scrutiny; it also served as a 'moderator' which slowed down the neutrons and made them more effective in causing fission. Promising as the results were they came nowhere near a self-sustaining reaction; it was soon clear that much more uranium would be needed.

In May 1939 Joliot approached the Union Minière du Haut-Katanga, a Belgian copper-mining firm which, almost as a sideline, extracted radium from its uranium ore. They had plenty of surplus uranium, and Joliot got them to ship five tons of pure uranium oxide to his Laboratory for Nuclear Synthesis at Ivry (near Paris), with its huge hangar and heavy handling equipment. In July Halban began the tedious neturon density measurements on spherical metal containers filled with a black mud, a mixture of uranium oxide and water, up to three atoms of hydrogen to one of uranium; the oxide would not soak up more water. When War broke out in September they had tested

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spheres up to half a metre in diameter and had clear evidence of a chain reaction that roughly doubled the number of neutrons that came from the source; but it was still far from self-sustaining. Water was not a good moderator; too many neutrons failed to cause fission because they were captured by its protons.

But what other moderator could one use? Carbon, as graphite, was available by the ton, but with its nuclei twelve times heavier than protons, neutrons needed many more collisions to become slow. Would they survive so many collisions without being captured? Much time was lost experimenting with piles of solid carbon dioxide ('dry ice') and industrial graphite; the results were promising but not conclusive. In the USA, similar work was done later and more carefully, convincing Fermi that graphite could be used; about the same time, a wrong result caused the Germans to abandon graphite as a moderator.

In the meantime Kowarski urged his team to consider heavy water: instead of protons it contains deuterons, only twice their mass, and it hardly absorbs neutrons. But it has to be extracted, at great cost, from ordinary water, of which it forms a minute fraction; in the USA, available only by the tumbler, its use was quickly dismissed. But in Europe the Norwegian firm Norsk Hydro had made almost 200 litres and were ready to increase production. In February, lieutenant Jacques Allier, a former employee of the French bank that controlled Norsk Hydro, went to Norway and persuaded them to hand over their whole stock, free of charge. The Germans were expected to interfere by force, and the entire operation reads like a spy thriller. Until the heavy water arrived in Paris in the middle of March, those two dubious foreigners, Halban and Kowarski, were interned for three weeks on two separate islands!

But in May the German army invaded France. After a brief attempt to set up a new laboratory in Clermont-Ferrand, Joliot decided that the team must split up; he would remain in France while Halban and Kowarski were to resume the work abroad. On June 17 they drove west in their cars, with their families, laboratory notes and the heavy water; in Bordeaux they embarked on a British collier which safely got them to London on June 22, just as France signed an armistice.

The British installed the team and gave them help at the Cavendish Laboratory, Cambridge. An aluminium sphere was constructed and filled with mixtures of uranium oxide and heavy water; it was made to rotate slowly to prevent the oxide settling to the bottom. After five months they had good evidence that a still larger sphere would give a self-sustaining reaction. But Fermi was still sceptical. Moreover, the USA were suspicious of Halban who stressed the French patents and got help from ICI (Imperial Chemical Industries); why help those probable post-war competitors? Negotiations dragged on for nearly three years.

To remove the work from this beleaguered island, a transfer to Canada was prepared; but when it at last took place late in 1942 — just as Fermi won the race for the chain reaction — some of the Cambridge physicists decided to stay with Kowarski, who had refused the inferior position Halban had offered him in Canada. Both teams had to live through another year of slow progress and low morale.

The Quebec Meeting of Churchill and Roosevelt finally enabled Canada to get information and heavy water from the USA. Replacing Halban, (Sir) John Cockcroft took charge of the Canadian team which was now joined by the Cambridge scientists. It took Kowarski less than a year to build a heavy-water reactor, which began to operate — the first outside the USA — a month after the War came to an end.

Joliot led a double life during the occupation. He kept his Paris laboratory going, but subtly sabotaged any research which Germany tried to conduct there; and he worked in the Resistance and came to admire the communists for their courage, idealism and competence. When France was liberated he was tireless in reviving fission research, amidst political strife and a ruined economy. Equipment was requisitioned or home-made; young people, attracted by the glamour of nuclear energy, were trained. Before Christmas 1948 the first reactor in France began to operate a heavy-water reactor, again built by Kowarski.

After that, the power of the scientists began to decline as the engineers, administrators and politicians took over. The declaration that France would never build nuclear weapons was gradually forgotten. Joliot's communist stance caused increasing problems, and after a rally in April 1950 when he declared that France would never make war on the Soviet Union he was removed from his post as head of the Commissariat à l'Energie Atomique. Large reactors were built, optimised for the production of plutonium, and the first French Bomb was exploded over the Sahara in 1960.

Joliot is clearly the hero of the book, a scientist who combined hard logic with lively imagination, and a persuasive personality with considerable skill as organiser. In his bid for nuclear power he was up against Fermi, probably the greatest all-round physicist of our time, as experimenter and theoretician alike, while France was short on theory. But Fermi was cautious and a foreigner in the USA while Joliot was full of fire and had the support of his country and his government. The author quotes Lord Blackett for saying "had the war not intervened, the world's first self-sustaining reaction would have been achieved in France".

This is a very well researched book, with its 12 pages of bibliography, 35 of footnotes, and a good index. But in spite of that scholarship the book is eminently readable and contains deft pen portraits of its many characters. It is written with a quiet dignity that yet does justice to the dramatic events with which it deals. Spencer Weart focusses on the French story but gives fair attention to efforts made elsewhere. Scientific explanations are kept fairly brief; much more space is given to the delicate interplay of social, economic and political forces; of great families and great schools; of scientists and administrators. Whichever way you look at it, this is a valuable and fascinating book. □

Otto R. Frisch was Professor Emeritus at Trinity College, Cambridge, UK. In Copenhagen he had contributed to the discovery of nuclear fission early in 1939. Professor Frisch died on 22 September, 1979.

Curiosity of a scientist

Hermann Bondi

Disturbing the Universe. By Freeman Dyson. Pp. 283. (Harper and Row: New York and London, 1979.) $12.95; £6.50.

OCCASIONALLY I feel unkind towards my many friends and colleagues in the education industry by pointing out that every small child makes a nuisance of itself by constantly asking "Why?", that society has developed a marvellous defence mechanism called education, and that this is so effective that it stops almost everyone from keeping on with this questioning. The few failures of this cure are, I claim, called scientists.

Curiosity must indeed be the mainspring of scientific endeavour. The constant nagging feeling of *wanting* to know something or other that is well hidden is indeed a powerful drive. To find out what a particular problem really is like and has as its solution, to worry the problem like a dog worries a bone, to gain insight by following to their end innumerable false turns, to see the whole confusing jumble eventually form a surprising and unexplored pattern, that is the real compulsive urge and joy of a scientist's labours.

To an extent that varies markedly between individuals there is associated with this an urge to communicate, an urge to teach, an urge to show to others the lovely pattern one has seen (whether one has found them or made them is a moot point) in one's own sandbox. Thus, research and teaching are naturally linked, but the link may take many different forms. To interest suitably qualified colleagues or research students in one's own researches is natural enough, but to watch a research student learn by making his own mistakes, perhaps messing up a lovely corner of one's sandbox is more of an acquired taste needing much self-discipline, but with its own rich satisfaction when one's pupil finally soars off under his own power. Undergraduate teaching is often seen as a chore, but only by those who have not got the urge to get across the beauty one sees in the structure of one's subject, the pleasure of conveying the personal pattern that one has seen (or made) in a well known part of the field.

Thus, there is substance to the notion of the normal academic career where one's researches (the value of which can be judged reasonably well) lead to posts in which teaching (the quality of which it is much harder for a university to assess) forms a major part of one's duties, along with continuation of one's research. However, justification for the curious habit of academic tenure (other than the mental comfort of the holder) can surely only be found if it enables the incumbent to follow daringly different unconventional and potentially fruitful pursuits on which he would otherwise not have embarked. Unhappily it is only a minute minority who so use their freedom. The vast majority of tenured academics, even of full professors who have nowhere further to rise, turn their academic positions or chairs, not so much into comfortable beds, as into narrow grooves of dried-up specialism often made most uncomfortable by making an 80 hour week out of administrative tasks that would take a competent bureaucrat half of an afternoon. Rare indeed is the person who really uses the freedom given by academic tenure to enrich the world and to enjoy himself by working hard and effectively at each in turn of the wide variety of pursuits he finds intriguing. Pre-eminently one such is Freeman Dyson, and his book is an exciting journey through the broad landscape of his experiences and thoughts.

Perhaps this is the point at which I should interpose the story of my period of friendship with the author which illustrates some of the bits he so oddly left out of the book. He does not mention his extraordinary brilliance at school at Winchester and as undergraduate at Trinity College, Cambridge, in the bumper class that had in it James Lighthill and Freeman Dyson, a record pair not equalled before or since. After the painful interlude of Bomber Command (fully described in the volume) he returned to Trinity as a young Fellow a few years my junior. An inveterate talker like myself (I recall his interrupting me at High Table dinner with "Not another word now. We are still on the soup while the other are finishing their desserts") he clearly had a most enjoyable year, but found that Cambridge, with its then rigid barrier between Physics and Mathematics was not a good place to learn and work in the lively field of theoretical quantum physics he wished to pursue. Hence his move to Cornell and Hans Bethe, where his book restarts. Why this odd omission of Cambridge?

He relates how he learned, travelled, and did brilliantly, none of which surprised me. But I was astonished and a little worried when he accepted a permanent position at the Institute of Advanced Studies in Princeton. That such an able and enterprising young man should take a post from which there was no escape upwards in academic life, a post that would hang round his neck for nearly 40 years, that would prevent him from teaching anyone but a few highly selected graduate students, that would isolate him in a closed esoteric community, all this saddened me greatly. How wrong I was to worry is so beautifully shown in his book. To him the Institute post was simply a springboard for pursuing