Fateful discovery almost forgotten

The discovery of the fission of uranium exactly half a century ago is at risk of passing unremarked because of the general ambivalence towards the consequences of this development. Can that be wise?

JUST over 13 years from now, there will be great celebrations on the fiftieth anniversary of what will then be recognized — as it is, of course, already — to have been one of the great discoveries in the history of science, that of a plausible structure for the nucleic acid polymer called DNA from which has flowed almost the whole of what is now called biology.

Those of an anxious or paranoid disposition should be prepared soon to reserve places at the celebratory symposia that will be held around Easter 2002. They and the rest of us should meanwhile reflect that this year, 1989, is also the fiftieth anniversary of a discovery which has done as much to change the world we live in, and as quickly, as even the discovery of the role of DNA in genetics. Yet the crowds are not fighting for places at the celebratory symposia — indeed, there are hardly any symposia to attend.

Yet nuclear fission is just half a century old. For more than half a decade, Fermi (at Rome) had been gathering data on the consequences of bombarding uranium nuclei (atomic number 92) with neutrons. Artificially radioactive materials abounded in the products. It was natural that Fermi should suppose that neutrons would first be absorbed by the heavy nuclei and should then decay (losing an electron), leaving the plausibly radioactive nucleus of some literally artificial element, with atomic number greater than 92. But there were implausibly many radioactive species to account for.

The key to this puzzle came only at the end of November 1938, when Otto Hahn and F. Strassmann, from the Kaiser Wilhelm Institute in Berlin, announced that Fermi's disconcerting artificial radioactivity included at least one isotope of barium, with atomic number 56 (*Naturwissenschaften* **26**, 756; 1938). The general expectation that nuclear reactions of this kind would effect only small changes in the atomic number, or the unit electric charge, of a nucleus was overturned. Physics was never again to be the same.

The explanation came just a few weeks later, on 11 February 1939, in a letter to the editor of *Nature* from Lise Meitner and her nephew, Otto R. Frisch (*Nature* **143**, 239; 1939). In a simple yet sophisticated argument, they showed that neutron bombardment of uranium nuclei must induce their fission into smaller fragments. What followed — the Manhattan Project, Hiroshima and Nagasaki, Shippingport (if anybody remembers that) and even Chernobyl — is inextricable from our contemporary history. It is also relevant that Meitner, previously at the Kaiser Wilhelm Institute but then evading the Nazis, gave her address as the Academy of Sciences, Stockholm. Her nephew, also Jewish, had made an earlier escape and had spent a year in London before settling with Bohr at Copenhagen.

Both the discovery of fission and its exploitation are, of course, heroic tales. At various times in the past half-century, they have been celebrated as such. But with the passage of the years, and for reasons which are thoroughly familiar, fission has become a dirty word. There are many people, even in research, who would prefer that history had taken a different course. For some, the ideal might have been that the Universe had evolved so that some fundamental constant, Planck's constant for example, had been sufficiently different from what it is as to have made the fission of uranium irrelevant. But that is not what the world is like, so people keep silent instead, hoping that 1989 will pass off quietly.

That view scorns the truth that the discovery and development of fission remains one of the most enthralling demonstrations that ingenuity can be a match for nature, and is thus a disservice to the reputation of science. The discovery of fission was also literally an heroic endeavour whose importance can with hindsight be diminished only by disservice to the reputations of a band of mostly admirable people, many of whom are still alive. To fail to acknowledge that is also to fail to acknowledge that fission quickly became the chief agenda item in the relationship between science and government from which, on balance, both parties have benefited. But were there not Hiroshima and Nagasaki, four decades of fission-provoked cold war and then Chernobyl? May not seemly neglect be the best mark of this uncomfortable anniversary? Only if it is supposed that uncomfortable truths are best forgotten. That supposes that some history is so painful that nothing can be learned from it, which is a disservice to the intellect.

The ingenuity that fission has always commanded is already plain in the article from Meitner and Frisch, which is far more subtle than a mere jumping to the correct conclusion. Their objective is to show that uranium fission is the preferred interpretation of Fermi's data.

First, they show fission to be plausible, using Bohr's theory of the nucleus as a liquid drop held together by its surface tension, which nevertheless diminishes steadily as the charge (or the atomic number) increases. They discard the notion that smaller fragments might be formed from uranium nuclei by the quantum tunnelling of particles much larger than the helium nuclei that make α-particles, concluding that the "whole 'fission' process can thus be described in an essentially classical way". Even so, it is still astonishing that their estimate of the energy released in the fission of a uranium nucleus should have been 200 MeV within a few per cent of the measured value.

Meitner and Frisch then show that fission makes better sense of the data accumulated since Fermi's earliest experiments than any other explanation. The paper is crammed with intuitive suggestions of radioactive β-decay chains for which to look among the light elements — for example, strontium–yttrium–zirconium. But they allow Fermi one transuranic element, supposed to be uranium-

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News and Views

A New Type of Nuclear Reaction

ON p. 251 of this issue of NATURE, an account is
given of recent investigations by Prof. O. Hahn, Prof.
L. Meitner and F. Strassmann on the bombardment
of uranium by neutrons. Prof. Meitner and Dr. O. R.
Frisch have discussed the development and implica-
tions of these results in a letter which also appears
in this issue (p. 239). Experimental confirmation of
these conclusions, it is claimed, is announced in
the following cable, dated February 3, received
from R. D. Fowler and R. W. Dodson, of the
Chemical Laboratory, Johns Hopkins University.
"We have bombarded uranium nitrate in a three
millimetre ionization chamber with deuterium neu-
trons and found that particles causing a very intense
ionization, at least five times that from natural
uranium alpha particles, are produced. Fast neutrons
from one milliampere two hundred and fifty kilovolt
deuterons produced thirty-five particles per minute.
Placing paraffin around ionization chamber increases
this to seventy counts per minute. We believe this
to be confirmation of work of Hahn and Strassmann
(*Naturwissenschaften*, Jan. 6; Frisch and Meitner
NATURE [Feb. 11, p. 239—Editor]) in which activity
ascribed to barium was found after neutron bom-
bardment, and that these particles are barium ions
of about one hundred million volts energy.

"We have also bombarded thorium oxide in
similar ionization chamber, with deuterium neutrons.
Fast neutrons in same intensity as above produce
thirty intensely ionizing particles per minute. Paraffin
does not increase the effect. We believe thorium is
also disintegrated by fast neutrons into fragments
of about one half thorium mass, having energies of
about one hundred million volts."

239, but in reality plutonium-239 (of which much more was to be learned).

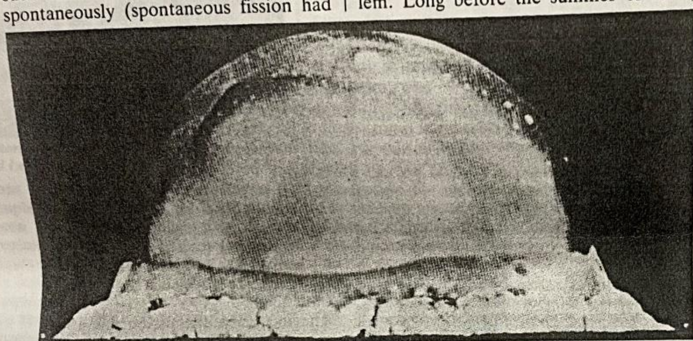
Even by present standards, the excitement generated by the developments is remarkable. Within a week, Frisch was reporting (*Nature* **143**, 276; 1939) the direct measurement of ionization caused by fission fragments and an estimate of the energy of the fragments in excess of 50 MeV. The two papers carry the same date, but one was delayed a week. Did *Nature* take the view that its readers could stomach only so much surprise? The following week (*Nature* **143**, 330; 1939), Bohr himself blessed Frisch's argument that fission could be accurately described by the fission of an oscillating liquid drop, dealing in passing with the question why such unstable nuclei did not disintegrate spontaneously (spontaneous fission had not then been measured) by noting that the latent heat of condensation of a neutron onto a uranium nuclear-drop was necessary to set the nucleus in oscillation.

Fission became more serious on 18 March (*Nature* **143**, 470; 1939) when H. von Halban, F. Joliot and Leo Kowarski at the Collège de France described the experiments by which they had shown that spare neutrons are released in the fission process. The starting point was the observation that uranium nuclei contain as many as a dozen neutrons above the requirements of stable nuclei of half the mass, that some of the extra neutrons are incorporated in the fission fragments (which is why their radioactivity is predominantly β-activity) but that some may be liberated in the fission process.

The experiment consisted simply of the measurement of the diffusion of slow neutrons through aqueous solutions of uranyl nitrate and ammonium nitrate (used as an inert control). The observation was that neutrons diffuse further into uranyl nitrate, for which the only explanation is that those absorbed as triggers of individual fission events are more than replaced by neutrons released by the fission events.

The authors concluded that the "interest of the phenomenon ... as a step towards ... exo-energetic transmutation chains is evident. However, in order to establish such a chain, more than one neutron must be produced for each neutron absorbed. This seems to be the case...." By April (*Nature* **143**, 680; 1939) the same group had estimated that the neutron-catalysed fission of a single uranium nucleus would yield an average of 3.5 neutrons (one to replace the catalyst and 2.5 for other purposes) and, by 3 June (*Nature* **143**, 939; 1939), that some of the spare neutrons liberated in fission carry energy in excess of 2 MeV. The stage was set for putting chain reactions to work.

That so much was accomplished so quickly is both a tribute to the ingenuity of those concerned and a sign that they understood the importance of the problem. Long before the summer of 1939, people in the know in the United States and Europe were openly speculating about bombs, nuclear reactors and perhaps both. It is remarkable that, at this point, with the outbreak of the Second World War in Europe just a few weeks ahead, the Royal Society should have asked Otto Hahn to London to describe his work in public on 23 June, and that Hahn should have accepted. (*Nature* paid Dr Norman Feather as he then was 10 shillings for his report of the occasion at **144**, 46; 1939.) Within a year of the appearance of the paper by Hahn and Strassmann, more than 100 others had appeared.



The first atomic explosion, the Trinity Test, at Alamogordo in the New Mexico desert on 16 July 1945.

The pace of these developments was too great for the transatlantic communications of the times — the telegraph cable and the ocean-going liner. During the first half of 1939, the European work on fission appeared almost exclusively in *Nature*, that in the United States in *Physical Review*, but inter-communication depended largely on chance.

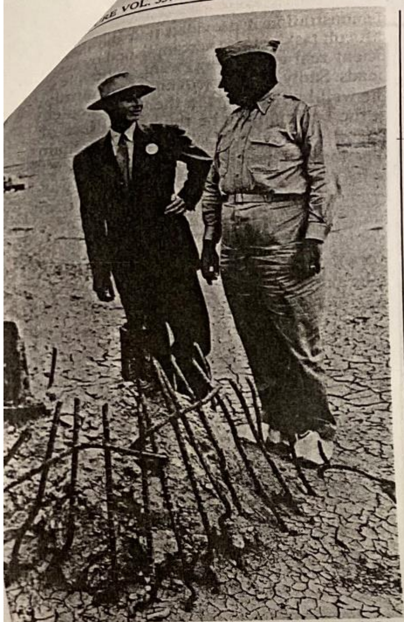
Thus the content of Frisch's paper with Meitner, and of his demonstration of energetic nuclear fragments from the fission of uranium, reached the United States by means of Bohr, who happened to be leaving Denmark for the United States at the point in mid-January at which Frisch was about to put his manuscripts in the mail to *Nature*.

By the spring, the character of the phenomenon of fission had been independently confirmed. Bohr seems to have been the first to raise the crucial question of which of the two predominant isotopes of uranium (235 and 238) is chiefly responsible for fission by slow neutrons. He told the spring meeting of the American Physical Society at the end of April that uranium-235, if separated from the larger bulk of uranium-238, would sustain a chain reaction, thereby starting the argument about the feasibility of separating these isotopes which lasted more or less until the separation plant at Oak Ridge had been commissioned.

Frisch, by then at Birmingham with Rudolf Peierls and the Australian Mark Oliphant, seems to have been the first to recognize that, while the fission of uranium-235 by slow neutrons might allow the construction of a nuclear reactor, only the fission of the same isotope by fast neutrons would make a bomb. He and Peierls persuaded Oliphant of that, and were eventually allowed to persuade F. Simon to divert his attention to the separation of the isotopes of uranium by difussion. By the summer of 1940, the British government had been convinced that what was called a "super-bomb" would be feasible and, with other things on its mind, took to persuading the United States that such an effort would be in the mutual interest.

By the summer of 1940, the other route to nuclear weapons had been signposted in the United States by the discovery a Berkeley of the element with atomi number 93 (now called neptunium) by E McMillan and Philip Abelson (unti recently the editor of the journal *Science*) This proof that trans-uranic elements di indeed exist seems quickly to have put int people's minds the certainty that some o them might be fissile and might therefor provide an easier means of producin materials for bombs and reactors. It was matter of weeks before plutonium mad its appearance.

Hindsight brings many benefits, bu also the disadvantage of making the pro cess of discovery seem a commonplace the stuff with which the introductor pages of text-books are filled. It is not eas now accurately to reconstruct the state o innocence of those who so quickly com prehended the ramifications of the dis covery of nuclear fission. The fact of th neutron was a few years old (and a neu tron source was a speck of radium mixe with beryllium). That there are forc between nucleons (neutrons and proton was phenomenologically established, b Yukawa's guess that they are mediated b mesons would not be established un after the Second World War. But hop that the motion of nucleons within nuc would be analogous with those of ele


Historic sight: Robert Oppenheimer and General Leslie Groves after the Alamogordo test.

trons in atoms, and thus described by simple quantum numbers, had already been disappointed. And while the experimentalists had accumulated several years of experience in the study of nuclear reactions of a familiar character, fission required endless ingenious improvisation. To have planned to recognize radioactive species by the characteristic energy of their γ-rays would have been to anticipate the future.

It is remarkable, and a comment on the way in which the research profession's judgement of its own recent past is compromised by ambivalence towards the consequences, that this achievement is now more often described as "fateful" than as "great". But can it in the long run be wise that such a tale of cleverness in the face of a natural conundrum should be neglected because some of the consequences are uncomfortable? It is as if an applicant for a job were to suppress from his *curriculum vitae* a spell of employment as an income-tax collector, whose practitioners are not always popular.

This neglect is also a disservice to those involved, whose influence then and since has done much to shape the framework within which the profession of science must now work, not only in the West but also in the Soviet Union. Much has rightly been made of the circumstance that refugees from Germany and its occupied territory were conspicuous in the understanding of fission and what followed. Can there ever have been another intellectual migration on such a scale? Meitner, an Austrian Jewess, left Berlin too late for comfort. Britain took (and kept), for example, Simon, Peierls and Mendelssohn, and the United States, the galaxy of talent of Einstein, J. von Neumann, E. P. Wigner, E. Fermi, E. Teller (who had stopped off in London on the way), Weisskopf and a host of others.

The scale of the migration is naturally impressive, but the ease with which it was assimilated is even more so. People newly arrived from hostile states might have expected to spend time in camps for aliens (which is why the Fermi family buried part of Enrico Fermi's Nobel prize money in cash in the garden of their New York house), but in the event were welcomed as urgently needed colleagues. It might have been different if the study of nuclear reactions had been destined to remain an academic subject. But in the event, the migrants played a role going far beyond their narrow expertise.

The immigrants from Europe were the first to make and then to sustain in the face of bureaucratic indifference the argument that the search for a practicable nuclear explosive was a necessary precaution against the possibility that Germany might do the same successfully. Hungarian Leo Szilard, an enthusiastic fixer of a physicist (with a patent of dubious value on energy-generation by nuclear transmutation up his sleeve) was among the first in the United States to urge voluntary restraint on the publication of research on fission. In the end, it fell to immigrant Einstein to write the two letters that persuaded President Franklin D. Roosevelt to launch the Manhattan Project. Even with hindsight, nobody can seriously pretend that the argument was incorrect.

The influence of the hectic months following the discovery of fission on the sociology of the profession of science has been profound. Practical needs led quickly to the practice of what had previously been a precept only — that a person's value as a scientist rests not on age, or background, or even paper qualifications, but on the understanding in his head which he can communicate or the skill in his fingers which he can put to use.

Upstart students who now take as a given their right to function within their sphere of competence as independent scientists must thank for their privileges the utilitarian tradition springing from the hunt to put fission to military use that the value of an idea or a technique can be judged independently of its provenance.

Friendliness is another legacy of those times. Those still alive who were at Los Alamos before 1945 still recall their time there with affection. The proof that it is possible to practise physics in shorts and shirtsleeves was plainly influential. So, too, was the practice of eating lunch from brown paper sacks. High-energy physics laboratories the world over seem to strive to keep these outward traditions alive. But the more durable cement of these relationships is the sense that people appear still to enjoy that they had been given a licence by a government frequently unconvinced to turn a wild idea into a reality and in the process to demonstrate that even abstract science can be useful.

Los Alamos was not of course the only important laboratory of the Manhattan Project (there were also Berkeley, Chicago and the production plants, as well as the British–Canadian enterprise at Chalk River) and nuclear energy was not the only important vehicle by which science made a wartime contribution to military affairs. (Radar and cryptography seem also to have been powerful wellsprings of comradeship.) But it is remarkable that people's affections for Los Alamos appear to have survived even the unfriendly traumas of the post-war period. The unpleasant procedure by which Oppenheimer's security clearance was withdrawn by the Atomic Energy Commission in 1956 has scarred all the participants and many others. By contrast, it is odd that the revelation, at about the same time, that Klaus Fuchs had been disloyal to the Los Alamos cause seems not to have filled those who worked there with the chagrin that might be expected of people betrayed.

But the Manhattan Project was different partly for its scale and, eventually, for its secrecy, and partly because of its daring — the risk of failure must always have seemed uncomfortably great, too many of the calculations might have been made on the backs of envelopes and too many of the careful calculations used numbers that were measured only crudely. Those who enjoy the game of rewriting history should perhaps ask what would have happened if the Manhattan Project had failed. How long the Pacific War would have dragged on after 1945 is the obvious but unanswerable question. That of what the reputation of science would have been is not nearly as difficult to answer. At one stage in the argument between the people at Los Alamos advocating that the two manufactured bombs should not be used in anger but merely demonstrated, the bureaucracy seriously countered that the US Congress would "want to know what happened to all that money".

In the event, science emerged from this adventure with its reputation enormously enhanced. That is not surprising — and, some will say, not particularly creditable either. But the case is not as simple as it may seem after nearly half a century.

Despite the conflicts of the intervening decades, it is difficult now to reconstruct the urgency of the general conviction at the discovery of fission that the world was heading for a war in which the stake was no less than the preservation of the fabric of civilized society.

The energy and flair with which the Third Reich had rebuilt the German Navy

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Lise Meitner, who, with Otto Frisch, first explained the nuclear fission process in 1939.

and created from scratch an aircraft industry lent weight to the fear that it might also be able to make nuclear explosives more quickly than its adversaries. In present circumstances, it is possible to contemplate and even to advocate agreements between potential adversaries that some kinds of weapons systems should be deliberately left on the shelf, but the world knows the difficulties of those projects. The case for building a bomb was irresistible once the feasibility of the project had been demonstrated. The wisdom of the first use, against Hiroshima and Nagasaki, is another matter.

But should not the scientific community have been more alert to the consequences of its adventure? There are two apparently contradictory responses. First, although the explosive power of the first bombs and the effects of radiation and radioactivity on civilian populations appear to have been estimated in advance with reasonable accuracy, and enthusiasm had developed for the promise of endless and cheap electricity from nuclear reactors, nobody had been able to calculate what the political consequences would be. (But P.M.S. Blackett, in Britain, accurately predicted nuclear stalemate between the United States and the Soviet Union and, after a distinguished wartime career in operational research, was for some years afterwards ostracized by the British government for the book in which he said as much.)

Second, however, Los Alamos and Chicago between them produced not only the first nuclear weapons but the tradition that people in research have an obligation towards the use made of their creations. The second thoughts at two laboratories in the early months of 1945 about the proper use of their nuclear explosives were ineffectual, for there was no common view of what might be done with them except what the military were planning, and ineffective, for the argument with the military was never joined. Similarly, the hope nurtured by many that nuclear weapons were qualitatively novel and thus must serve as catalysts of novel kinds of international understandings had been discovered to be wishful thinking by 1947, when the Baruch plan to internationalize nuclear energy inevitably collapsed, partly because of its internal contradictions. Yet the tradition that science need not be neutral on the uses people wish to make of it has flourished fitfully in the intervening decades. If it fades, or is entirely overtaken by the notion that what makes money must be worthwhile, the blame will attach not to the Manhattan Project people but to their successors.

The reputation of science and its relationship with government is another matter. In the United States, the bombs helped to win a place in government for science and scientists. Until the Nixon administration, there was a President's Science Advisory Committee, one of the means by which the federal government was persuaded to shoulder responsibility for the support of research in general, not simply in public health and agriculture. To suggest that the present output and quality of science in the United States is attributable to the Manhattan Project would be a travesty of the truth, but success helped. While the comparison is remote, much the same appears to have happened in the Soviet Union. The relative independence of the Soviet Academy of Science between 1941 and 1986 owes much to the power of the Kapitzas, Kurchatovs and Artsimovitches who carried through the Soviet nuclear weapons project.

The other side of this coin is naturally less endearing. The intimacy of science and government in the Manhattan Project has left government with the impression that science is properly an instrument of national policy. The rights and wrongs of that position are rarely defined with the clarity they rightfully demand. It is, of course, proper that a government seeking to carry through some worthwhile technical project should expect that its nationals who happen also to be scientists will sign up for the enterprise, but there is a worrying temptation to suppose that those who might help have no particular right to hold opinions about the wisdom of the project. People's devotion to the Manhattan Project has led governments to believe that it should be possible to whistle up a crowd of willing workers even for projects whose rationale is much less compelling than, in the early 1940s, was the project to make nuclear weapons. The precedent of the Manhattan Project is awkward in relation to the Strategic Defense Initiative, for example.

It must also be confessed that the success of the Manhattan Project, or the demonstration it provided that the most difficult tasks can be accomplished by sufficient zeal and daring, went to people's heads. Sadly, the consequences are mostly the well-known flaws in the what seemed at the beginning to be the benign face of fission — the use of self-sustaining chain reactions for generating electricity.

In 1955, for example, the British government announced its plan for a dozen nuclear reactors whose construction would begin within a decade and which would span the whole gamut of conceivable designs, from thermal gas-cooled reactors to liquid-sodium-cooled fast reactors. That same year, the first UN Atoms for Peace conference at Geneva was told of how the running cost of generating electricity would be so small that consumers might not have to pay for the number of kilowatt-hours used. The Manhattan Project was the model on which these dreams evolved, but could not but be misleading: making a few tens of kilograms of fissile material, difficult though it had been, was a simpler task than creating a whole new industry. Los Alamos had necessarily little to say about the demands of the long haul — durability and reliability in particular.

But these are not usually the grounds on which the nuclear industry is now commonly regarded as a second malign face of fission. Instead there is the fear of nuclear accidents (of which Chernobyl is the worst so far), of the routine release of radioactive materials from reactors and reprocessing plants and the complaint that there is no sure method for the long-term disposal of nuclear waste. Yet, ironically, these problems have been anticipated more accurately than those of building and operating nuclear reactors economically. It is natural that nuclear engineers should nurse a sense of grievance at the unfairness of these charges, even though their own over-confidence has provoked the distrust they accurately sense.

The truth is that the nuclear industry has everywhere — Chernobyl notwithstanding, even in the Soviet Union — been more diligent about the protection of people from radiation and its consequences than in regard to the engineering and economy of its plants. The view now gaining ground that the risk of further nuclear accidents cannot be contained acceptably is tantamount to the assertion that nuclear technology in unmanageable at the standards it is reasonable to expect of it. If that is how the argument about the future of nuclear power is resolved, the result will be that a potentially valuable technology must be left on the shelf even at a time when the threat of climatic change makes nuclear power particularly valuable. That would be an ironical way of celebrating the half-century of the discovery of fission — and a unique confession of technical incompetence. □