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Nuclear Fission: Reaction to the Discovery in 1939

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ABSTRACT

A large body of literature exists on the scientific and political history of nuclear weapons. There is little, however, concerning the reaction in 1939 to news of the discovery of nuclear fission. This study is a detailed examination of worldwide views during the preceding four decades about "harnessing the energy of the atom," a brief survey of the scientific accomplishments of 1939, a close look throughout that year at the thoughts, hopes, fears, and actions that fission inspired, primarily in America, and an analysis of why the discovery came as such a surprise, and why it generated relatively little moral or ethical introspection.

INTRODUCTION

Pandora, of Greek mythological fame, incautiously opened a box and allowed to escape the ravages of human mind and body. Only hope remained to comfort mortals. The discovery of nuclear fission at the end of 1938 presented some scientists with a modern counterpart to Pandora's box. Would the splitting of uranium lead to inexpensive energy that might revolutionize the world's economy, or would the product be instead a bomb of awesomely destructive power? The story of the development of both nuclear weapons and civilian reactors already claims a large literature and promises still more. Modern Pandora's *reaction* to the potential of her discovery, however, has not been examined in any detail. Why were scientists so surprised by the phenomenon of uranium splitting in two? Did they recognize immediately the possible fruits of this discovery? Did they comprehend or acknowledge any sense of social responsibility regarding its applications? Could they foresee, given the dark picture of world politics, that their profession might undergo a fundamental redirection from basic to mission-oriented research and from genteel poverty to permanent dependence upon governmental funding?

This is a study of the reaction in 1939 to news of the discovery of nuclear fission. We start with an extensive background section in order to document the worldwide discussion of atomic energy over the preceding four decades. In optimism and pessimism, with technical knowledge and without, the possibility of energy release was debated to a surprising extent and by an eminent cross-section of the scientific community. Equally surprising, in view of this exposure, the discovery of fission—and its almost immediate recognition as a likely mechanism for "harnessing the atom"—came as a shock.

Section 2 is a brief survey of the research highlights of 1939; section 3 a close look throughout that year at the thoughts, hopes, fears, and actions that fission inspired. Scientists commented on the phenomenon more than others, and our story focuses

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mostly upon them. But when newspapers, magazines, or radio joined their voices, we include them also. The coverage, though focusing upon reaction in the United States, includes also a few examples from abroad.

That a bomb of awesomely destructive capability was now closer to reality was no secret; indeed, it was common knowledge. Yet the analysis of this potential, upon personal morality, the scientific community, and international relations, was neither widespread nor profound. The concluding section draws together a number of themes to explain this reaction. To nuclear physicists in 1939, the application of this phenomenon was too distant to explore its significance carefully; if a weapon was to be made, the next generation would do it. Further, scientists are no more endowed with foresight than other humans; the only head start they have in contemplating the future is their proximity to the tools that may shape that time. Assessment of technology, difficult at best, must include specialists from many other disciplines. This is no revisionist attempt to hold scientists morally or ethically responsible for nuclear weapons—or to absolve them. Rather, it is a historical presentation of their behavior in the immediate aftermath of a major scientific discovery having profound societal implications, and an analysis of their actions and inactions.

Background

Ernest Rutherford provided good newspaper copy in his remarks at the 1933 meeting of the British Association for the Advancement of Science: "Any one who says that with the means at present at our disposal and with our present knowledge we can utilize atomic energy is talking moonshine."[2] A vigorous, bluff, New Zealand farmer's son who had been raised to the British peerage for his scientific contributions, Rutherford was not usually so quotable. Irritation over what he considered to be wild speculation probably generated this much-cited "moonshine" comment. And since the BAAS meeting had come to be the major event at which scientific information was conveyed by the press to the public—where "*ex cathedra* statements of the convocation of the church of science" were uttered, in the words of John D. Bernal—Rutherford could be sure that his views would be widely circulated.[3]

In a way, Rutherford had himself to blame. It was his own experiments published in 1919 that first showed the nuclear disintegration of nitrogen when bombarded with alpha particles. In the following years Rutherford and James Chadwick found similar reactions in many of the other light elements, and this work encouraged speculation about "harnessing the energy of the atom." Then, in early 1932, somewhat over a year before the moonshine comment, two remarkable discoveries in nuclear physics were announced from Rutherford's famous Cavendish Laboratory at Cambridge University.

Chadwick identified the long-sought neutron, a particle of mass almost identical with the proton, whose lack of electrical charge would enable it to strike nuclei without being repelled by their charge. And the team of John Cockcroft and E. T. S. Walton perfected another means of promoting nuclear reactions, namely a machine in which they fired accelerated protons at a lithium target, causing it to split into pairs of helium nuclei (alpha particles) in each interaction. Rutherford, the world's leading nuclear physicist, was proud that "his boys" had been the first to induce nuclear transmutations with a particle accelerator (and before Ernest Lawrence obtained similar results in his cyclotron). Still, optimistic newspaper stories about boundless energy—"Nothing less than the complete abolition of irksome manual labour and a new era of prosperity for all"—were not to his taste.[4] For, while it was true that each reaction yielded alphas of great energy, only a tiny fraction of the proton projectiles collided with lithium nu-

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clei. Far more energy was expended raising all the protons to high voltage than was gained in the very few transmutations. The nucleus, Rutherford believed, would remain a sink of energy rather than a reservoir.

Expectations (and fears) of harnessing the energy of atoms may have peaked at the time of the Cockcroft-Walton experiment, but they formed a backdrop to all of twentieth-century science. Shortly after Henri Becquerel's discovery of radioactivity in 1896, considered then an inexplicable outpouring of energy from uranium atoms, Marie and Pierre Curie found that the purified radium compounds they had separated were self-luminous. American chemist Henry Carrington Bolton in 1900 could scarcely contain his eloquence: "Are our bicycles to be lighted with disks of radium in tiny lanterns? Are these substances to become the cheapest form of light for certain purposes? Are we about to realize the chimerical dream of the alchemists,—lamps giving light perpetually without consumption of oil?"[5]

As early as 1902, Rutherford's chemical colleague at McGill University, Frederick Soddy, commented on the enormous energy emitted by radioactive bodies, his evidence being the radiations and luminosity produced. Was this the source of the sun's heat, he wondered?[6] The next year he expanded on his understanding of radioactivity. The earth, he wrote, was "a storehouse stuffed with explosives, inconceivably more powerful than any we know of, and possibly only awaiting a suitable detonator to cause the earth to revert to chaos."[7] Soddy thus became the oracle first to call public attention to the potential dangers of this new science—although the medical effects of exposure to radium (burns) were known earlier.

Also in 1903, Pierre Curie and Albert Laborde discovered another facet of the energy inherent in radioactive substances: a sample of radium maintained itself at a temperature slightly higher than its surroundings. When this microscopic laboratory phenomenon was editorially transmuted to understandable macroscopic terms, the press proclaimed the existence of a cornucopia of energy. For example, it was claimed that the energy stored in one gram of radium could raise five hundred tons a mile high, while an ounce would suffice to drive a fifty-horsepower automobile at thirty miles per hour around the earth.[8]

This energy of radioactive decay, understood to manifest itself via collisions of the alpha, beta, and gamma radiations with neighboring particles, threw light on the geological age of the earth and found both medical and commercial applications (e.g., in the treatment of dermatological problems and cancer, and in the production of self-luminous watch dials).[9] But the prediction of energy in quantities sufficient for vehicle propulsion or home heating did not materialize. Nor, it should be added, were scientists pursuing such goals. Their efforts were focused upon learning more about the structure first of the atom, and then the nucleus.

Nonetheless, speculation continued, and some visionaries besides Soddy could sense potential danger. Even Rutherford was not immune to such speculation and in 1903 playfully suggested to a colleague "the disquieting idea that, could a proper detonator be discovered, an explosive wave of atomic disintegration might be started through all matter which would transmute the whole mass of the globe into helium or similar gases, and, in very truth, leave not one stone upon another." The colleague published Rutherford's remark, calling it a "nightmare dream of the scientific imagination," but useful to illustrate the vistas created by the discovery of radioactivity.[10] Despite his disclaimer, Rutherford thus became associated with the concept that "some fool in a laboratory might blow up the universe unawares."[11]

Soddy, who returned to England in 1903 after his collaboration with Rutherford in

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Montreal, gave talks in Cambridge and London which were published in a book and also delivered the prestigious Wilde Lecture in Manchester. From all these platforms he spoke of the enormous store of energy within atoms, far larger than in chemical reactions, and the explosive violence of a radioactive transformation. The difference from chemical explosives, however, was that the disruption of one radioactive atom did not lead to another.[12]

Rutherford, too, was intrigued with the energy inherent in atoms. In his classic text *Radio-Activity*, published in 1904, he wrote:

> Since the other radio-elements only differ from radium in the slowness of their change, the total heat emission from uranium and thorium must be of a similar high order of magnitude. There is thus reason to believe that an enormous store of latent energy is resident in the atoms of the radio-elements. . . . The difference between the energy originally possessed by the matter, which has undergone the change, and the final inactive products which arise, is a measure of the total amount of energy released. There seems to be no reason to suppose that the atomic energy of *all* [emphasis added] the elements is not of a similar high order of magnitude. With the exception of their high atomic weights, the radio-elements do not possess any special chemical characteristics which differentiate them from the inactive elements. . . . If it were ever found possible to control at will the rate of disintegration of the radio-elements, an enormous amount of energy could be obtained from a small quantity of matter.[13]

Pierre Curie, a gentle and sensitive man, soon expressed his own concern. In the Nobel lecture (6 June 1905) delivered on behalf of Marie and himself for their share of the 1903 physics prize, he spoke of the way in which radium had added to scientific knowledge and become a tool for combatting cancer. But radium could also burn healthy tissue and cause harm, even to the point of paralysis and death. It was foreseeable, Curie said, "that radium could become very dangerous in criminal hands." He was convinced, nonetheless, that scientific understanding was desirable, and humanity ultimately would reap more benefits than evil.[14]

British chemist William Ramsay felt the problem was by no means limited to radium. In his 1911 presidential address to the British Association, he noted that "If radium were to evolve its stored-up energy at the same rate that gun-cotton does, we should have an undreamt-of explosive; could we control the rate we should have a useful and potent source of energy." But, Ramsay argued, the supply of radium is extremely limited, so hopes should not be raised too high. "If however," he continued,

> the elements which we have been used to consider as permanent are capable of changing with evolution of energy; if some form of catalyser could be discovered which would usefully increase their almost inconceivably slow rate of change, then it is not too much to say that the whole future of our race would be altered.[15]

Aside from his off-hand remark in 1903, Rutherford characteristically was less speculative and expansive. In fact, he preferred to throw cold water on whatever he regarded as science fiction. In the first series of William Ellery Hale lectures to the U.S. National Academy of Sciences, in April 1914, he noted that radioactive decay is spontaneous and uncontrollable, and that attempts to alter the rate have failed. While a recent novel by H. G. Wells depicted human control of this rate, resulting in enormously powerful atomic bombs, Rutherford cautioned that this "does not at present seem at all promising."[16] He returned to this theme two years later, by which time Europe was plunged into warfare. At a public meeting in February 1916, he acknowledged scientific interest in releasing at will the energy equivalent of a hundred million pounds of coal that is contained in a pound of matter, and hoped success would not be achieved until mankind had learned to live peaceably.[17]

Not long before, Frederick Soddy had

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again raised the specter of atomic energy misused. This brilliant British radiochemist whose compassion for humanity was often obscured by his nastiness to humans observed that:

> in the atoms of matter exists a store of energy beyond comparison greater than any over which we have obtained control. In the slow changes of the radioactive elements there is known to be an evolution of energy nearly a million times as great as has ever been obtained from a similar weight of matter before. The energy is there, but the knowledge of how to liberate it at will and apply it to useful ends is not—not yet.[18]

Soddy conjectured that a way might be found to release, in an instant, "the energy which now oozes out, so to speak, from radioactive materials over a period of thousands of millions of years. . . ." One pound of matter would be the equivalent of 150 tons of dynamite. Soddy's point was that mankind must learn to live without recourse to war, but it is his profound conviction that atomic energy would be harnessed that is more noteworthy.[19]

The American historian Henry Adams, whose autobiography appeared in 1918, equated the progress of Western civilization since the Middle Ages to the supply of energy. When graphed, his curve showed a quickening pace. The discovery of radium bent the plot even more sharply upward, and Adams expressed concern about its uncontrolled use.[20]

More optimistic was Oliver Lodge, one of the grand old men of British science. In 1919, he pointedly noted that *all* matter, not just the radioactive substances, contains enormous amounts of energy, and felt that society would benefit from a source of ashless, dirtless, and smokeless fuel. There might be, he conceded, occasional explosions when the energy was liberated too quickly, but on balance the development would be desirable.[21] A contemporary of Lodge, the noted Irish engineer Charles A. Parsons, was equally interested in the power which science offered humanity, but feared its menacing, uncontrolled use. The example he cited in his 1919 British Association presidential address was the potential instantaneous release of the energy in radium—equal to 2.5 million times the energy in an equal weight of TNT.[22]

Rutherford, however, confided to his old friend, the chemist Arthur Smithells, that "You need not be alarmed about any possibilities of atomic disintegration; if it had been feasible it should have happened long ago on this ancient planet. I sleep quite soundly at nights."[23]

By the early 1920s, Albert Einstein's $E = mc^2$ relationship, coupled with ever-more-accurate atomic weights provided by F. W. Aston's mass spectrometer, allowed further public discussion of the source of atomic energy. In many nuclear reactions the mass of the products is less than that of the initial ingredients, and this mass loss is converted into enormous amounts of energy. British astrophysicist Arthur Stanley Eddington in 1920 was among the first to suggest the fusion of four hydrogen atoms into a helium atom as the probable mechanism that keeps stars hot. Stars draw "on some vast reservoir of energy by means unkown to us," he stated. As others had done, he pointed to the "sub-atomic energy which . . . exists abundantly in all matter" as the only likely source. Eddington, too, wondered out loud if "man will one day learn how to release it and use it for his service," and whether its use will be "for the well-being of the human race—or for its suicide." But his special contribution to the discussion was his drawing attention to the energy available when four hydrogen atoms of weight 1.008 synthesized one helium atom of weight 4. Referring to Rutherford's induced nuclear transformations, Eddington wryly noted that "what is possible in the Cavendish Laboratory may not be too difficult in the sun."[24]

Aston, who produced the data on which such speculations were based, was one of

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J. J. Thomson's associates who remained as an independent worker when Rutherford took over the Cavendish Laboratory. Aston was quite optimistic about capturing nuclear energy on earth and sanguine about the consequences. In an address to the British Association in 1922, he restated Eddington's insight:

The quantity of matter so transmuted is indeed almost inconceivably small, but it is the first step towards what may well be the greatest achievement of the human race, the release and control of the so-called "atomic energy." We now know with certainty that four neutral hydrogen atoms weigh appreciably more than one neutral helium atom, though they contain identically the same units, 4 protons and 4 electrons. The change of weight is probably due to the closer "packing" in the helium nucleus, but whatever the explanation may be transmutation of hydrogen into helium must inevitably destroy matter and therefore liberate energy. The quantity of energy can be calculated and is prodigious beyond the dreams of scientific fiction. If we could transmute the hydrogen contained in one pint of water the energy so liberated would be sufficient to propel the *Mauretania* across the Atlantic and back at full speed. With such vast stores of energy at our disposal there would be literally no limit to the material achievements of the human race.

The possibility that the process of transmutation might be beyond control and result in the detonation of all the water on the earth at once is an interesting one, since, in that case, the earth and its inhabitants would be dissipated into space as a new star, but the probability of such a catastrophe is too remote to be considered seriously. A recent newspaper article pointed out the danger of scientific discovery, and actually suggested that any results of research which might lead to the liberation of atomic energy should be suppressed. So, doubtless, the more elderly and apelike of our prehistoric ancestors grumbled at the innovation of cooked food, and gravely pointed out the terrible dangers of the newly-invented agency, fire, but it can scarcely be maintained today that subsequent history has justified their caution.[25]

Another supporter of the view that scientific investigation must proceed, in this case the argument being related to the value of chemical warfare, was J. B. S. Haldane. The British biochemist remarked in 1924: "If we could utilize the forces which we now know to exist inside the atom, we should have such capacities for destruction that I do not know of any agency other than divine intervention which would save humanity from complete and peremptory annihilation."

Haldane recognized the inadequacy of present-day techniques of bombarding nuclei, but thought success in releasing usable amounts of energy would ultimately be achieved. His timetable, however, placed that date *after* travel to the moon. His purpose in mentioning nuclear energy was to pose a contrast to the far more humane capabilities of chemical warfare.[26]

If Haldane's remarks bore tangentially upon the social responsibility of scientists, the Russian mineralogist Vladimir Vernadsky was direct. In 1922 he wrote:

We are approaching a great revolution in the life of humanity, with which nothing . . . earlier . . . can be compared. The time is not far away when man will take atomic energy into his hands. . . . This can occur in the near future; it may happen after a century. But it is clear that it will inevitably happen. Does man know how to use this power, to direct it to good and not to self-destruction? Has he . . . the ability to use this force, which science will inevitably give him? Scientists must not close their eyes to the possible consequences of their . . . work, of . . . progress. They must consider themselves responsible for the consequences of their discoveries. They must relate their work to the best organization of all humanity.[27]

In Italy, a virtually unknown Enrico Fermi contributed an appendix to the translation of a book on relativity which was published in 1923. Not the more fashionable issues of space and time, but the equivalence of mass and energy, was the most important feature of Einstein's theory, he wrote. One gram of matter equaled more than the total energy

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of a thousand-horsepower motor running for three years.[28] Fermi expressed no views on the social responsibility of scientists, but it was a topic receiving increased attention. If those such as Haldane and Vernadsky proposed a measure of scientific self-control, others found this inadequate. Indeed, they questioned the very value of science, fearing both that it might get out of control and that it was subverting other, more important, activities. One of the most notable of this class was the bishop of Ripon, who chose to beard the lion in his own den. In a sermon attended by many participants of the 1927 meeting of the British Association the clergyman proposed "that the world is going too fast and that humanity would be benefitted if physicists and chemists suspended operations for ten years." The time "saved" could well be used to improve the interactions between humans.[29] This idea of a research moratorium was not new, as historian of science and technology Carroll Pursell has pointed out. Evidence that man's sense of social responsibility weighed less on the balance than did scientific knowledge and capability was seen by some in Galileo's struggle with the Inquisition, in the stories of Faust and Dr. Frankenstein, in the development of poison gases in World War I, and most recently in the trial of Thomas Scopes for teaching evolutionary biology in a Tennessee school.[30]

While the bishop (who had made the suggestion with tongue in cheek) gathered some supporters, most scientists and newspapers recognized that scientific research could not be turned off. None reacted with more hostility to the idea of a holiday for science than Caltech's leader, Robert A. Millikan. If the bishop outranked the physicist in church hierarchy, some of the latter's colleagues suspected that Millikan believed that *he* alone was capable of walking on water. However, Millikan's concern here was not theological, but the public's perception (and support) of science. He objected to

> so-called humanists. . . .advocates of a return to the "glories" of a pre-scientific age, [who] have pictured the diabolical scientist tinkering heedlessly, like the bad small boy, with these enormous stores of sub-atomic energy and some sad day touching off the fuse and blowing our comfortable little globe to smithereens.[31]

His anger was directed primarily at Frederick Soddy, who first "raised the hobgoblin of dangerous quantities of available subatomic energies" a few decades ago, and only secondarily at the bishop of Ripon and others who kept such fears alive. Millikan was convinced "that the creator has put some foolproof elements into his handiwork and that man is powerless to do any titanic physical damage."[32]

Lest it appear that Millikan was being hypocritical in denouncing Soddy's speculations while engaging in fanciful ideas himself with regard to the creator's handiwork, one must grant that Millikan felt he was arguing from good data. Aston's binding energy curve showed that the heavy elements, essentially those that are radioactive, are the ones that would release energy in disintegrations, yet these elements comprise less than one percent of all matter. The vast majority of the elements, he said, were already in "their state of maximum stability." With hindsight, we can see that Millikan not only ignored the potential of fusion, but failed to reckon that even one percent of the earth's crust available to mining was a very large amount of material.[33]

Not everyone focused on the dangers conjured up by the release of nuclear energy. In a public lecture in January 1933, Arno Brasch of the University of Berlin cast doubt on any near-term benefits, especially by the current techniques of bombarding atoms in accelerating machines. But he professed optimism in the experiments he and a colleague conducted atop a Swiss mountain, in which lightning was tapped to impart high voltage to their projectiles.[34] M.I.T. President Karl

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Compton was less inclined to dismiss accelerator technology, both because it was so new and because his institution was building a ten million volt generator. The headline he spawned, just a day after Brasch, declared "Science Now Ready to Harness Vast Force."[35]

Rutherford's September 1933 moonshine comment, therefore, was not made in a vacuum. There had been some three decades of speculation about useful atomic energy, with success seemingly imminent upon Cockcroft and Walton's achievement in "smashing atoms." And the moonshine had a kick to it, apparently satisfying a need for such a statement. Under a headline in the *New York Herald Tribune* proclaiming "Atom-Powered World Absurd, Scientists Told," an Associated Press story quoted a number of scientists who agreed with the technical limitations cited by Rutherford. I. I. Rabi, of Columbia University's physics department, Victor La Mer, a chemist also at Columbia, and Samuel C. Lind, of the University of Minnesota's chemistry department, all concurred in Rutherford's prediction. However, physics chairman H. H. Sheldon, of New York University, was unwilling to believe "that the end of discovery of new and fundamental principles" had come, and Ernest Lawrence, at the University of California's Berkeley campus, while uncertain of ultimate success, felt that "this is purely a matter of marksmanship," and "we're going to keep on trying to make a larger percentage of the 'shots' reach their mark."[36]

As might be guessed, such equivocal support did not end the discussion. Interest remained sufficiently high that when Albert Einstein visited Pittsburgh in December 1934, he generated the headline "Atom Energy Hope is Spiked by Einstein. Effort at Loosing Vast Force is Called Fruitless."[37] Lev Landau, the Russian theoretician, was more circumspect. When asked in 1934 if nuclear energy was science fiction, he replied that it appeared to be with charged particles. "But if one day somebody finds a reaction initiated by neutrons that releases secondary neutrons, one is all set."[38] At the Nobel Prize ceremonies the following year, Frédéric Joliot-Curie expressed great confidence in use of the atom:

> If we look back and take a glance at the progress achieved in ever-increasing measure in science, then we are justified in supposing that the investigators who are able to build up or break down elements at choice, will also learn how to realize transformations of an explosive character; veritable chemical chain reactions.[39]

The conjectural pendulum then swung in the opposite direction. A. S. Eddington in 1935 wrote that the practical application of nuclear energy could not be "more than a dream for idle moments." Prometheus' precedent of stealing fire from the gods for mankind was not likely to be copied. And yet, mindful of international tensions of the day, and recognizing that "unlimited energy means unlimited power for war and destruction," Eddington admitted that, tiny though it was, the cloud on the horizon was ominous.[40] More conservative than Eddington, the distinguished atomic and nuclear physicist Niels Bohr felt that harnessing the atom's energy was becoming *less and less likely*. In an address to the Royal Danish Academy of Sciences in January 1936, he reviewed the transformations that occur when neutrons and charged particles penetrate into a nucleus. If still more "violent impacts" could be induced, even though that admittedly was far beyond present capabilities, they might lead to the emission of several particles at a time, or even "an explosion of the whole nucleus." Yet Bohr did not equate this explosion with a weapon, though he did note that the process would not translate into controlled nuclear energy for practical use. "Indeed," he added, "the more our knowledge of nuclear reactions advances the remoter this goal seems to become."[41]

Even on the eve of the discovery of nuclear

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fission the matter was chewed over again, this time by H. C. Dickinson, chief of the U.S. National Bureau of Standards' Heat and Power Division. His analogy to the complete conversion of one pound of fuel was the operation of a 1,000 horsepower motor between the birth of Christ and six months after George Washington's. Admittedly, such efficiency was not expected, but the conversion of four hydrogen atoms into one of helium, with a mass loss of about one percent, would allow that motor to run for fifteen years on one pound of hydrogen. Of course, if means were found to release large quantities of atomic energy without at the same time devising methods of control, the event would, he contended, "probably be recorded by the inhabitants of other solar systems as the formation of a new star." However, because of the inefficiency of particle accelerators, "the utilization of atomic energy [should be] classed as possible, but unattainable practically."[42]

Was Rutherford's rejection of usable nuclear energy unconditional? Apparently not. He could foresee no improvements in known processes that would yield a net output of energy, and considered "loose and uninformed talk of the possible dangers to the community of the unrestricted development of science and scientific invention" as unhelpful to the progress of science.[43] But he was a man without pretensions of any sort, especially to omniscience, and seems somewhat to have hedged his bet. At a Royal Society social event around 1930, Rutherford approached Maurice Hankey, the long-time secretary of the Committee of Imperial Defence, with the thought that experiments on nuclear transformations might someday, somehow, be relevant to the nation's defense. It is difficult to evaluate whether Rutherford was engaging in idle chatter or revealing an intuitive feeling, but Hankey believed the latter.[44]

Rutherford, too, seems to have had a suspicion that the neutron might be the "magic bullet" that would allow a profitable smashing of atoms. He was especially impressed with the discovery of Fermi's group in Rome that neutrons slowed to low (thermal) velocities were far better at causing nuclear transmutations than were fast neutrons. In a lecture commemorating the bicentenary of the birth of James Watt—appropriate in view of Watt's improvement of the steam engine, the energy source that powered the Industrial Revolution—Rutherford, in January 1936, presented the audience with a, by then, usual combination of visionary attainments tempered by present-day technical limitations. In particle accelerators, he noted, the efficiency generally rises with the projectile's energy. One million volt protons, for example, produce far more transmutations than 20,000 volt protons, although in both cases, when lithium nuclei are split, some seventeen million volts are released. The problem, of course, was that only one proton in 10^8 engaged in a successful encounter, and more energy was supplied than was yielded. The long-term benefits of such a program seemed doubtful. "On the other hand," Rutherford concluded,

> the recent discovery of the neutron and the proof of its extraordinary effectiveness in producing transformations at very low velocities opens up new possibilities, if only a method could be found of producing slow neutrons in quantity with little expenditure of energy. At the moment, however, the natural radioactive bodies are the only known sources for gaining energy from atomic nuclei, but this is on far too small a scale to be useful for technical [i.e., industrial] purposes.[45]

A copious supply of slow neutrons, thus, was posited as the most likely key, but if we wish to label Rutherford's disbelief in useful energy from accelerating machines as "Moonshine I," then we should designate success with neutrons as "Moonshine II."

Once the cause of radioactivity was seen early in the century to be within atoms instead of due to an external influence, it was clear that atoms possessed enormous

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amounts of energy. Initially, indications came from the alpha, beta, and gamma radiations, and from temperature differentials. Later, mass-difference calculations provided even more accurate figures. The ability to cause nuclear transformations (and their associated mass changes) was achieved in the 1920s for most of the light elements by Rutherford and Chadwick. The development of particle accelerators around 1930 was, to a great extent, in response to the need for more energetic projectiles, and a greater supply of them, to attack the nuclei of heavier elements. Cockcroft and Walton's accomplishment was not only to be the first to produce transmutations by machinery. Unlike the earlier reactions, which only chipped a proton off the target nuclei, in theirs the target was literally split in two.

Throughout these decades, some people were confident that nuclear energy would be harnessed, for good or ill, while others hewed more closely to the known facts and phenomena and saw no reasonable extrapolation to such applications. Public interest was at a high level not only because of the subject's intrinsic importance but because of the fame of those who participated in the debate. Nobel laureates, present and future, included Rutherford, Curie, Ramsay, Soddy, Aston, Fermi, Millikan, Rabi, Lawrence, Einstein, Landau, Joliot-Curie, and Bohr.

The forty-year-long discussion was not, with hindsight, especially innovative or profound. Some of the latest discoveries were incorporated into the arguments, to be sure, but other ideas were curiously absent. Only Leo Szilard seems to have seriously considered a chain reaction (see pp. 208–9 below), and he kept that secret. Speculation about the possible splitting of heavy elements seems to have been absent, even though the packing fraction and binding energy curves were well enough known to recognize that both of the processes that came to be called fission and fusion would convert mass to energy.

Scientists and the public lacked a high degree of fanciful thinking, while at the same time many exhibited an almost blind confidence in the future conquest of Nature. This is not meant as criticism of scientists and the public. Rather, it is an effort to emphasize that scientists, even the most creative ones, usually behave as ordinary human beings, and the human condition is basically one of hope, with few glimpses of distant peaks or even the paths toward them.

Early Research

Many technical aspects of the discovery of nuclear fission and the early exploration of the phenomenon have been described elsewhere.[46] Since the purpose of this paper is to explore the *reaction* to fission, the research of 1939 need only be surveyed briefly.

Enrico Fermi and his colleagues at the University of Rome were in the forefront of those investigating the creation of new radioactive species by neutron bombardment in the 1930s. Chadwick's discovery of the neutron in 1932 and the Joliot-Curies' discovery of artificial radioactivity in 1934 were combined in Rome in a systematic attack on the periodic table. One remarkable finding was that the likelihood of forming a radioactive isotope of the target element, or a radioactive daughter product of it, was *not* proportional to the neutron's velocity; indeed, slow neutrons were more efficient. Another spectacular result was a proliferation of beta activities when neutrons were fired at uranium, the heaviest element known. Since emission of a negative electron (the beta particle) leaves the target nucleus with one more unit of positive charge, it appeared that uranium was being transmuted to the next higher element in the periodic table. Several different beta decay patterns suggested that a whole series of transuranic elements was being created by man!

Although Fermi thought it was premature to claim the production of elements not

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found in nature, the Italian press labored under no such circumspection and the story was picked up by papers around the world. The scientific community was equally interested, and between 1935 and 1938 radiochemical identification was attempted for as many as sixteen sources of activity from the neutron irradiation of uranium. The leaders in this work were Otto Hahn, Lise Meitner, and Fritz Strassmann, in Berlin, and Irène Joliot-Curie and Paul Savitch, in Paris. Not all the activities were allegedly from transuranics (ten were); some of particular interest seemed to be emitted from radium isotopes, although there was no unambiguous evidence of the two alpha particles that must be emitted to turn uranium into radium.

Hahn was the world's most experienced radiochemist, having been at his trade since 1904, when he worked first under William Ramsay and then Ernest Rutherford. The standard technique used to isolate radium was to separate it, by dissolution and precipitation, with larger amounts of barium, a chemically similar element suitable as a carrier, and then separate these two by fractional crystallization. The procedure had been so long in use that there could be little doubt about its effectiveness. When Meitner escaped from Germany before Hitler's racial laws could place her in a concentration camp, the careful investigation of the "radium isotopes" was left to Hahn and Strassmann.

To their amazement, they found in December 1938 that the activity separated not with the radium but with the barium fraction. As chemists, they had no doubt of the accuracy of their work, and were forced to conclude that the substance previously thought to be radium was really barium. Almost to convince themselves that uranium *could* somehow produce such an element, they reasoned that the sum of the mass numbers of any two elements in the middle of the periodic table, where barium was located, was about that of uranium. But they found the concept of mid-table-element production "a drastic step which goes against all previous experience in nuclear physics," and left themselves the loophole that perhaps "a series of unusual coincidences" had given them "false indications."[47]

Before this paper was published in January 1939, Hahn wrote to Meitner, who now was in Sweden. At Christmastime 1938 she shared one of his letters with her physicist-nephew, Otto Frisch, who visited her from Copenhagen, where he worked in Niels Bohr's institute. Together they reasoned that the analogy of the nucleus to a drop of liquid was applicable here. The neutron would add sufficient energy to the uranium nucleus, causing it in some cases to deform from its usual spherical shape into an elongated one. Mutual repulsion of the positive charges at each end of the elongation would overcome the surface tension and break the drop at its narrow neck. Indeed, a back-of-the-envelope calculation of the mass loss agreed with a similar rough figure for the kinetic energy of the fragments: 200 million electron volts (MeV). This was recognized as extraordinarily significant, since the most energetic chemical reactions involve but a few electron volts, while ordinary radioactive processes involve only several MeV.[48]

Frisch returned to Copenhagen and, by mail and telephone to his aunt in Stockholm, composed a report on their interpretation of the Hahn-Strassmann results. From a biologist colleague, William Arnold, he learned that the process of cell division was called fission, and borrowed the word. Another colleague, George Placzek, encouraged Frisch to hunt for the large ionization pulses that must be caused by the fission fragments, an experiment he performed successfully on 13 January 1939. The Berlin team's chemical evidence was now supported by physical data, and two papers soon were sent off to the British weekly, *Nature.*[49]

Bohr had learned from Frisch on 3 January about the fission concept while in the bustle of departing (7 January) for a trip to the

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United States; a telegram informed him of the experimental confirmation. He hoped to keep the matter quiet until the Europeans' papers appeared, thereby ensuring their priority, but the news was inadvertently made public to a journal club at Princeton University on 16 January. This disclosure as well as private conversations by Bohr initiated the investigations at Columbia University involving Herbert Anderson, John Dunning, Enrico Fermi, and others. Fermi, like Meitner, had been forced to escape a fascist country, though his leadership in neutron research easily survived the transfer from Rome to New York. The Columbia team confirmed fission, in a physical procedure similar to Frisch's, on 25 January 1939. Dunning wired the news to Fermi, who had left earlier for the Fifth Washington (D.C.) Conference on Theoretical Physics. When the meeting began the next day, both Bohr and Fermi discussed the Hahn-Strassmann work—and created a sensation. Soon, additional confirmation of the energetic fission fragments was reported from the Carnegie Institution of Washington, Johns Hopkins University, the University of California at Berkeley, and Frédéric Joliot-Curie's laboratory in Paris.[50]

Although Hahn for a while left open the possibility that some of the activities he investigated might indeed be from transuranic elements, experiments by others soon allowed this idea to die.[51] In contrast, another idea was retained as mental gears shifted from heavy-element production to fission: the great efficiency of slow neutrons in provoking the well-studied transmutations of lighter elements was shown by Frisch to be true also for uranium fission.[52] Further, the calculated energy release was quantitatively confirmed at Columbia: about 175 MeV were detected in the recoil energy of the main fission fragments, while the remaining 25 MeV were attributed to the beta decays, gamma rays, and neutrons assumed to be emitted from the fragments.[53] Whether neutrons were indeed emitted as uranium fissioned was the key question once the reality of fission was assured. Experiments conducted in Paris and New York in March and April indicated that an average of 2 or 3.5 neutrons were produced in each fission (the correct value is about 2.5). If only a small number escaped through the surface of the uranium, or were captured by impurities or uranium itself (without fissioning), then the remaining neutrons would be free to strike other uranium nuclei and maintain a chain reaction.[54]

On theoretical grounds, Bohr suggested in February that the rare isotope U-235, of 0.7 percent abundance, was being fissioned by the slow neutrons.[55] Dunning's group decided to pursue this line of investigation, which proved successful a year later when spectroscopist Alfred Nier, of the University of Minnesota, separated enough U-235 to confirm Bohr's hypothesis.[56] This work led, of course, to the various isotope separation processes conducted on an industrial scale at Oak Ridge, Tennessee, during World War II, and to the bomb exploded over Hiroshima.

Fermi, on the other hand, chose to pursue the chain reaction in natural uranium, which consists primarily (99.3 percent) of the U-238 isotope. Using carbon in the form of graphite bricks as a moderator to slow the neutrons, and securing graphite, uranium, and other materials of heretofore unknown purity, he and his team built dozens of experimental "piles," first at Columbia and then at the University of Chicago. Success, on 2 December 1942, in initiating, controlling, and shutting off a chain reaction gave confidence that no natural laws were likely to preclude detonation of a nuclear weapon.[57]

Fermi's first reactor gave added significance to other work done earlier. Uranium-238, besides fissioning under fast neutron bombardment, has a resonance for absorbing neutrons of lesser energy. The product, U-239, is unstable, with a half-life of about 23

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minutes, and decays with the emission of a beta particle. The daughter product, of mass 239 and charge 93, was clearly a transuranic element, and it was also expected to be unstable, but Emilio Segrè's search for it in 1939 bore no fruit.[58] Success in detecting the element named neptunium was finally achieved in 1940 by Edwin McMillan, like Segrè part of Ernest Lawrence's Radiation Laboratory in Berkeley, and Philip Abelson, a recent Rad Lab alumnus.[59] Louis Turner, of Princeton University, about this time suggested that neptunium's daughter product, element 94, would most likely be highly fissionable with slow neutrons, a possibility that meant a bomb would not have to rely solely on U-235.[60] As is well known, Glenn Seaborg soon discovered this second transuranic element, plutonium, which could be created in the intense neutron flux of reactors, several of which were constructed at Hanford, Washington. The plutonium was purified by ingenious, yet "conventional," wet-chemistry processes, which were more certain of success than the separation of uranium isotopes having such close mass numbers. Hanford plutonium was detonated in the Trinity test at Alamogordo, and then at Nagasaki.

Many other discoveries of great value were made in 1939, the year that concerns us—fission occurs instantaneously, some delayed neutrons are emitted (vital for controlling a reactor), thorium fissions with fast neutrons only, heavy nuclei split in numerous ways, as shown by the many fission fragments identified, etc. Most important, in September Bohr and his former student John A. Wheeler, who was just starting his career at Princeton, published a classic paper on the theory of fission, including numerous quantitative calculations.[61] Though we have given only a sketch of the research conducted in that period, it suffices to indicate the major lines pursued.

Reaction

When Otto Frisch explained the concept of nuclear fission to Niels Bohr, the great Danish physicist struck his forehead and exclaimed "Oh, what fools we have been! We ought to have seen that before."[62] This was the general reaction among physicists.

There was an exception, however: Leo Szilard. This farsighted, ebullient, rotund Hungarian, who was at his best in organizing the activities of others, and who reputedly never dirtied his hands with an experiment, had left his faculty position at the University of Berlin just days after the Reichstag fire on 27 February 1933. When Hitler came to power the month before, he accurately assessed the evil times to come, simply picked up his bags and took the train to Vienna. Soon he turned up in London to encourage formation of the Academic Assistance Council, whose task was to find jobs for Jews fleeing Germany.[63]

When Rutherford made his moonshine remark at the September 1933 meeting of the BAAS, it struck a chord in Szilard's mind. He recalled reading H. G. Wells's novel *The World Set Free* (1914), in which atomic bombs destroyed the world's metropolises. While Szilard regarded the book as nothing but fiction, harnessing atomic energy in reality came to preoccupy him. He recognized the need for a process that yielded energy *and* the means to sustain itself. This latter feature involves the concept of a chain reaction, in which each event establishes at least the basis for the next event. In 1939, the idea of a chain reaction did not occur immediately to all of those dazzled with the first evidence of nuclear fission. But Szilard (and Landau) had conceived it five or so years earlier.[64]

Bothered by Rutherford's 1933 comment, Szilard visited the Cavendish Laboratory and proposed to its director that a chain reaction might be achieved in two stages: alpha bombardment of light elements would produce protons, as in the many transmutation experiments of Rutherford and Chadwick, and these protons would disintegrate lithium into two alphas, as in the Cockcroft-Walton experiment. Rutherford, who knew the low cross-sections or efficiencies involved, was

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vexed enough at this obviously impractical suggestion (by a scientist who was not yet a nuclear physicist) to pour his astonishment into the ear of the first person he encountered in the corridor, spectroscopist Kenneth Bainbridge, newly arrived from Harvard to spend a year in Cambridge.[65] Szilard confided to his friend Edward Teller: "I was thrown out of Rutherford's office."[66]

His cool reception in the Cavendish did not stop Szilard from contemplating chain reactions, but it may have redirected his thoughts from charged particles to neutrons. While strolling in London it occurred to him that a chain reaction might be sustained if an element could be found which, upon absorbing one neutron, yielded two in the ensuing process. When the phenomenon of artificial radioactivity was discovered the next year—non-radioactive elements, upon irradiation, emit particles—Szilard thought that the tools now existed for testing the elements, but he found no enthusiasm for such a venture among British physicists.[67]

Still, Szilard considered the potential application of nuclear energy so significant—if means to release it could be found—that he decided to file a patent on the process. In fact, he filed several patent applications in Britain, the first in the spring of 1934, covering such concepts as a neutron chain reaction, the production of radioactive materials, and the chemical separation of radioactive elements from non-radioactive isotopes. Subsequently, he assigned the chain reaction idea to the British Admiralty as a means of preventing its publication and possible conversion by others into a weapon. While Szilard's belief that the reaction could be propagated with beryllium was wrong, the fundamental concept was accurate.[68]

Denied the opportunity to work in the Cavendish Laboratory during the 1934–1935 academic year,[69] Szilard nevertheless became a *nuclear* physicist with a discovery significant enough to carry with it eponymic fame (the Szilard-Chalmers effect). Rutherford's opinion of him may also have changed with their presumed interaction at the Academic Assistance Council, of which Rutherford was president. During the latter part of the 1930s, Szilard continued to develop those approaches so characteristic of him: patenting neutron applications, but not for personal benefit; attempting to organize other scientists, in this case to control the patented processes; and seeking to restrict the spread of certain ideas by self-censorship. While he sought again to convince Rutherford that "nuclear transmutations on a large scale" were feasible, and that "it is very unlikely that the misuse of chain reactions could be prevented if they could be brought about and became widely known in the next few years," there is no evidence that he succeeded before Rutherford's death in 1937.[70]

When the fission of uranium was discovered a few years later, Szilard was already living in New York City and in contact with the able group of neutron investigators working at Columbia University. His fertile mind immediately recognized the possibilities now presented, and saw what experiments would provide the necessary data. With Walter Zinn he investigated the emission of neutrons in the fission process,[71] and with Fermi and Herbert Anderson he studied the geometry and materials that would lead to a sustained chain reaction.[72] Indeed, Szilard's contributions over the next several years to reactor design led the U.S. government to assign the basic patent on this invention jointly to him and Fermi.[73]

In the closing days of 1938, Szilard wrote to the British Admiralty requesting that his patent be withdrawn; a chain reaction in indium, an element that, like beryllium, once seemed promising, was out of the question, and Szilard seemed ready to turn to other matters.[74] A month later, indeed, the day after Dunning's group at Columbia detected the large energy pulses of fission, and just after receiving the issue of *Die Naturwissenschaften* containing the Hahn-Strassmann paper, Szilard cabled to London to disregard his letter.[75]

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Even before this Szilard had been thrilled by the news Bohr brought from Copenhagen. The physics department at Princeton, where he spent a few days, "was like a stirred-up ant heap," but their attention, Szilard felt, was riveted only upon the fundamental scientific interest in Hahn and Strassmann's discovery. Szilard, by contrast, was fascinated by the amount of energy released in fission. He did not think that a nuclear power source would be cost-effective, but believed that the large-scale production of radioelements was a possibility (useful in medicine), and also, unfortunately, atomic bombs. The man on whom Szilard unburdened these thoughts on 25 January was New York investment banker Lewis Strauss, a layman whose taste for science complimented Szilard's compulsion to maintain liaison with the business community. The two, in fact, had been in contact for over a year, endeavoring to raise funds to build an accelerator that would produce medically useful radioactive bodies. The irony is that Szilard thus introduced Strauss to the subject of nuclear weapons, on which they held strongly opposing views in the postwar period when Strauss was a member of the first Atomic Energy Commission and, subsequently, its hawkish chairman. The irony is compounded by contrasting Szilard's low estimate of reactor efficiency with Strauss's oft-repeated comment in the 1950s that the electricity produced by nuclear power plants would be too cheap to meter.[76]

But was Szilard's 1939 assessment of the reaction to fission accurate? Was attention focused overwhelmingly on basic, not applied, science? Otto Frisch recalled that he did not even think about the chance that neutrons might be emitted, although Lisa Meitner did. However, to neither did the concept of a chain reaction occur. When a colleague in Copenhagen, Christian Møller, suggested it, Frisch concluded that the existence of uranium deposits meant that there were inadequate neutrons for a chain reaction.[77]

Eugene Wigner, a prominent member of the "ant heap" at Princeton, some four decades later commented cryptically, "We immediately recognized that it could mean very good things and very bad things."[78] Philip Morrison, at the time a graduate student in physics at Berkeley, recalled his reaction in one word: "Bombs."[79] Morrison's classmate, Robert R. Wilson, remembered a different emphasis, an energy machine, though with the same vigorous interest.[80] E. U. Condon, then associate director of the Westinghouse Research Laboratories, commented on the initial excitement of the basic science, *followed* over the next few months by the realization that a bomb or a controlled power source might be constructed.[81] Condon, as an applied physicist, thus saw applications discussed in clear sequence to the fundamental science, but one must remember that he likely was more distant from those hotbeds of discussion and his recollections may be less reliable.

Documents from 1939 are, of course, better sources of attitudes and ideas than are reminiscences. The laboratory notebook of John Dunning, at the time an associate professor of physics at Columbia University, shows his immediate comprehension of fission's significance. On the night of 25 January 1939, when he, Eugene Booth, and Francis Slack physically confirmed the Meitner-Frisch interpretation of the Hahn-Strassmann chemical experiment (not yet knowing that Frisch had done so some twelve days earlier), Dunning's entry reads: "Believe we have observed new phenomenon of far reaching consequences. . . . Here is real atomic energy!" Checking their work the next day, Dunning wrote: "*Secondary neutrons are highly important!* If emitted would give possibility of a self perpetuating neutron reaction which I have considered since 1932–35 as a main hope of 'burning' materials with slow neutrons and release atomic energy." On 27 January, Dunning's notebook gives a glimpse of the self-imposed censorship question soon to be debated: "So

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far no one knows except immediate group. Agreed to keep it rigorously quiet in view of serious implications of atomic energy release internationally."[82]

At that stage, however, there was no chance of suppressing such information, for scientific ideas occur when their time is ripe, and independent and simultaneous discovery is common. Moreover, self-censorship requires the cooperation of the *community* of scientists involved; rarely can an individual attempt succeed. Dunning, too, knew that Bohr and Fermi were in the nation's capital, where they intended to announce the European discovery of fission.

The site of this announcement was the Fifth Washington Conference on Theoretical Physics, a small, prestigious meeting of invited participants, sponsored jointly by the Carnegie Institution of Washington's Department of Terrestrial Magnetism and George Washington University. Merle Tuve, of the DTM (which did research in nuclear physics as well as geophysics), and Gregory Breit, a former member of the DTM, collaborated with GWU physicists George Gamow and Edward Teller to plan a series of programs which, so far, had featured nuclear physics, molecular physics, elementary particles, stellar energy, and in 1939 low-temperature physics. While neither Bohr nor Fermi claimed cryogenics as a major interest, there were aspects of concern to them, and certainly they would have been welcomed at *any* theoretical conference.[83]

Before the scheduled program began on 26 January, Bohr and Fermi dropped their bombshell. Tuve immediately asked Richard Roberts and Lawrence Hafstad to look for the energetic fission pulses using the DTM's Van de Graaff generator. They departed as soon as Fermi finished speaking and spent the next few days overcoming leaks in the accelerator's vacuum system. On the afternoon of 28 January, Roberts and R. C. Meyers bombarded uranium with neutrons and were rewarded by the sight of unusually long spikes on their oscilloscope trace. That same evening they repeated the demonstration of these large kicks before Bohr, Fermi, Breit, Rosenfeld, Teller, and (presumably) Tuve. The possibility of atomic power was recognizably closer.[84]

In Baltimore, fission also was confirmed the same day by R. D. Fowler and R. W. Dodson, who may have been alerted by one of their five Johns Hopkins University colleagues at the Washington Conference, or by the Hahn-Strassmann paper, which had reached the United States by then. Berkeley apparently was on no one's "grapevine." The physical chemist William Giauque, who later would win the Nobel Prize for his work at low temperatures, was the only Californian invited to the Washington Conference, and he did not attend.[85] Consequently, newspaper reports of fission provided those on the West Coast with their first information about the phenomenon. Luis Alvarez remembers coming across an article in the *San Francisco Chronicle* while having his hair cut and, before the barber finished his work, rushing out of the shop to tell his graduate student, Philip Abelson, who had been studying the bombardment of uranium by neutrons. For further details, he sent a telegram to Gamow (the use of long-distance telephones being less common than it is today). Alvarez noted that the next day he and Kenneth Green

> observed the large fission pulses on an oscilloscope. Before this observation, Alvarez had told Robert Oppenheimer what he had learned from Gamow, and Oppenheimer "proved" it was impossible, by arguments about potential barriers. But a few hours later, when Oppenheimer had seen the pulses, it took him only minutes to suggest that neutrons should accompany the fission, and he was soon talking of power-producing devices and bombs. Alvarez was not surprised at his friend's quick reversal in position—that is what he expected a good physicist to do, when faced with new evidence—but he *was* surprised at how quickly Robert Oppenheimer arrived at the concepts of power production, both controlled and explosive.[86]

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The *San Francisco Chronicle* was not the only newspaper to tell the story. Thomas R. Henry, one of the few science reporters in the country, reported for the *Evening Star* (Washington, D.C.) that it was just "dumb luck" that he decided to drop in on the theoretical physics conference. He was rewarded with an extensive page-one story on 28 January, featuring the headlines "Power of New Atomic Blast Greatest Achieved on Earth," and "Physicists Here Hail Discovery Greatest Since Radium." The sensational nature of fission, Henry emphasized, was in the understanding of matter and energy it provided; "as a practical power source, the new finding has at present no significance." Henry returned to page one on 30 January with another long story, this time on the confirmation of fission at Columbia, the Carnegie Institution, Johns Hopkins, and in Copenhagen.[87]

The Associated Press wire service picked up these accounts, and stories appeared in the *New York Times* and the *Los Angeles Times*, and in newsmagazines, such as *Newsweek*.[88] The *New York Times* editorialized that Rutherford, Millikan, and others might have been wrong after all, since "the possibility of harnessing the energy of the atom crops up again,"[89] and the *Times*'s science editor, Waldemar Kaempffert, devoted much of his weekly column to just such a likelihood. Power plants would have to be constructed with sufficient shielding, he predicted, to protect workers from high levels of radiation.[90] *Science News Letter*, an authoritative periodical for the scientifically-interested layman, cautioned its readers to discount any prophesies that the world might be "blown to bits" by these experiments. Despite the nay-saying, this 11 February article appears to be the first connection between fission and the possibility of explosives made in print. Peaceful uses of atomic energy, such as propelling an ocean liner across the Atlantic with the atoms in a glass of water, were discounted as premature. Despite the large amount of energy released in the fission of a uranium nucleus, still more energy was required to produce the neutron that initiated the event, for many additional neutrons must be created as well, only to be absorbed by various nuclei in this inefficient process, and thus "lost" as far as causing more fissions.[91] For an analogy, *Newsweek* pulled from its file of quotations Albert Einstein's remark that "It is like shooting birds in the dark in a country where there are not many birds." A further measure of fission's widespread notoriety is comedian Fred Allen's query to a fictional atom-smashing professor about the practical results of such work. The professor answered: "Well, some one may come in some day and want half an atom."[92]

Scientists were clearly aware of the stir created by fission both in their own community and among the public. Dean George Pegram of Columbia saw the discovery enhancing the already very attractive field of nuclear physics.[93] Niels Bohr was impressed both by the American enthusiasm and the "rush . . . to compete in exploring the new field."[94] Ernest Lawrence's laboratory in Berkeley was certainly one of these centers of activity, where not only the research staff and apparatus, but the director's typewriter were pressed into action. In early February, Lawrence sent at least half a dozen letters to colleagues around the world, detailing the laboratory's success across a broad front. Their curiosity was overwhelming, he reported, and many were studying the reaction, even to the point of committing "heresy" by temporarily abandoning construction work on the new cyclotron:

> . . . within a day of reading about it in the paper, Alvarez and Green observed the energetic particles with ionization chamber and linear amplifier. Then, Thornton and Corson photographed them in the cloud chamber. Abelson identified one of the activities as iodine by the iodine K x-rays, and this piece of work is the prettiest of all. Alvarez has been looking for neutrons in the reaction and

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so it goes. . . . For obvious reasons, we want to find out whether neutrons are given off in the splitting process.

Lawrence's letters generally concluded with variations of an optimistic refrain: "It may be [that] the day of useful nuclear energy is not as far distant after all."[95] Members of the Rad Lab also made a policy of sending correspondence and wires to John Tate of Minnesota, editor of the *Physical Review*, requesting rapid publication of their papers, "rapid" meaning within about two weeks.[96] And, certainly, there was extensive shop-talk in Berkeley, both informal and formal. Glenn Seaborg remembered "a seminar in January 1939 when the new results . . . were excitedly discussed; I do not recall ever seeing Oppie [J. Robert Oppenheimer] so stimulated and so full of ideas."[97]

One must be cautious, however, in extrapolating the Berkeley experience to other laboratories. There had to exist the necessary preconditions of an active research group, adequate apparatus, and, most important, research interests in nuclear physics close enough to the fission reaction to make its investigation a reasonable extension of the earlier work. Oppenheimer, who held joint appointments at Berkeley and the California Institute of Technology, told his Pasadena colleagues about the recent news, but neither Charles Lauritsen nor William Fowler nor anyone else at Caltech pursued it. They did not think ahead to applications, regarding fission only as a scientific problem, and they were already concerned with reactions involving light elements, not uranium and others at the heavy end of the periodic table.[98] Oppenheimer also served as the source of fission news for physicists at the University of Illinois. A letter from him reached Robert Serber before the newspaper stories, and that very evening Serber gave a report at their journal club. Oppenheimer mentioned explosives in his communication, and Illinois physicists did subsequently investigate fission.[99]

For institutions having nuclear physicists, Illinois seems more characteristic than Caltech; fission and its applications were indeed of concern to others. In mid-March, George Pegram informed the technical assistant to the Chief of Naval Operations that Fermi, who planned soon to be in Washington, would be happy to describe the experiments performed at Columbia. Although he personally thought the likelihood was small, Pegram believed the possibility of liberating "a million times as much energy per pound as any known explosive" must be explored. To naval and army officers, and civilian scientists from the Naval Research Laboratory, Fermi explained the status of the investigations. The Navy's interest lay more in the potential of a power source than in explosives; with such an engine that required no oxygen, submarines could remain submerged for indefinite periods. Three days after Fermi's conference, NRL director Admiral Harold G. Bowen recommended that the Navy's Bureau of Engineering underwrite some of the costs of such research. The Bureau responded by offering $1,500 to the Carnegie Institution of Washington, not an inconsiderable research grant in those days. The CIW declined the funds for internal policy reasons, but agreed to do the work. The NRL also exhibited an interest in isotope separation techniques, and made contact with centrifuge expert Jesse W. Beams at the University of Virginia.[100]

Explosives, however, remained on the minds of the scientists. Was it possible to construct a bomb? Bohr, who spent the first part of 1939 at Princeton, on 16 April discussed the question with George Placzek, Eugene Wigner, Leon Rosenfeld, John Wheeler, and others. The answer had to be "no," Bohr reasoned. It was preposterous to expect to separate enough U-235. "It would take the entire efforts of a country to make a bomb."[101] To others the possibility remained strong. In a February letter to George Uhlenbeck, at the University of Michigan,

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Oppenheimer speculated that a "ten cm cube of uranium deuteride . . . might very well blow itself to hell."[102] At the end of March, despite all the technical uncertainties, Szilard wrote to Victor Weisskopf at Rochester that, "It appears very likely that a fast neutron bomb will be too heavy to be carried by aeroplane. It could, however, probably easily be carried by boats, and it seems to be possible to devise engineering tricks for setting the reaction off in such a way as to cause an explosion the destructive power of which goes beyond imagination."[103] Merle Tuve commented upon the "war scare" atmosphere created by the possibility of a chain reaction, and the discussions among Fermi, Bohr, and Szilard about secrecy.[104] On Easter Sunday 1939, Gale Young, a mathematical biophysicist/nuclear physicist at the University of Chicago, was moved enough to pen a short story called "Road to Tomorrow," in which the destruction of cities by nuclear fission killed millions and condemned surviving generations to cancerous deaths.[105]

The newspapers and newsmagazines in March and April also kept returning to the idea of an explosive. Waldemar Kaempffert, of the *New York Times*, speculated that a Martian observing earth's cataclysmic end might comment, "Some imbecile has been annihilating matter." With evidence of both delayed and instantaneous neutron emission, a chain reaction of fission events looked possible. The key was isolation of enough pure material.[106] Since the efficiency of the fission process was not yet well known, many scientists, such as Fermi, preferred to downplay the chance of a dramatic explosion.[107] But the concept was too enticing for reporters to overlook and, while they acknowledged many hurdles yet to be overcome, they seemed to delight in writing about "an explosion that would make the forces of T.N.T. or high-power bombs seem like firecrackers."[108] When the American Physical Society held its spring meeting in Washington, at the end of April, the headlines ranged from the extreme "Vision earth rocked by isotope blast," to the more modest "Physicists here debate whether experiments will blow up 2 miles of the landscape." Again, Bohr and Fermi were at the eye of the storm. Bohr insisted upon the theoretical possibility of a chain reaction in pure U-235 that could destroy the neighborhood of a laboratory for many miles. If a thermal diffusion process suggested by Yale's Lars Onsager worked well, enough U-235 might be separated to wreck even the entire area of New York City.[109] Wendell Furry, of Harvard, agreed that a mass of this isotope a yard in diameter would "tear a very large hole in the landscape," but sought to calm fears by arguing its very small likelihood. As for the chance of "literally blow[ing] up the world," that certainly was "eyewash." Physicists at the meeting seem agreed on this latter point, but were of different minds as to the practical possibility of a bomb.[110] If anyone raised any moral reservations about weapons of such destructive power, or ethical questions about turning one's professional goal from increasing knowledge about the universe to investigating military applications of science, it has gone unrecorded.[111] Fission inspired awe and fear; doubt and revulsion would come much later. And even fear was not directed at the application itself. Rather, its origin lay in the expectation that German physicists also recognized the explosive possibilities of fission.

At this point censorship made sense to Szilard. Why give the Nazis information that might help them? His earlier patents, especially the secret one assigned to the British Admiralty, clearly fit this mold of controlling scientific concepts and applications. But when he approached Fermi around 1 February 1939, with the idea of self-censorship, the Italian showed his mastery of colloquial English with the reply: "Nuts!" To Fermi,

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the chance of a chain reaction occurring was too remote for such a violent assault on the cherished tradition of openness in science. By March, however, with evidence in hand of neutron emission in the fission process, Szilard and Teller convinced Fermi to reassess his position. Papers were sent to the *Physical Review* (to date priority) with a request that publication be delayed a while. This self-imposed ban was soon lifted, though, when it was learned that Joliot's team had submitted for publication a paper on secondary neutron emission, disregarding Szilard's plea for censorship.[112] More than Fermi's cooperation was lost by this action, for Szilard and his Central European compatriots also lost the agreement they had secured from the several other people at Columbia, the Carnegie Institution group, the *Physical Review's* editor, John Tate, Bohr, John Cockcroft and Patrick Blackett in England, and others. It took the outbreak of hostilities in September 1939 to generate the attitude in the belligerent nations that secrecy was necessary, but it was not until mid-1940 that self-censorship was effective in the United States.[113]

Scientists in England, France, and America saw their fears realized in June, when Siegfried Flügge, of the Kaiser Wilhelm Institut für Chemie, in Berlin-Dahlem, published a paper in *Die Naturwissenschaften* entitled "Can nuclear energy be utilized for practical purposes?" It was a review of worldwide research on fission, and contained a calculation showing that "all available uranium [in a supercritical mass] will be transformed in less than 10^{-4} sec." To Flügge it was obvious that "the energy liberation should thus assume the form of an exceedingly violent explosion."[114]

However, German scientists had not waited for Flügge to point the way. It was instead the 22 April paper in *Nature*, by Hans von Halban, Frédéric Joliot, and Lew Kowarski, that galvanized them to action. The French report of 3.5 neutrons released per fission could not be ignored.[115] A few days later, Wilhelm Hanle delivered a paper on an energy source, a "uranium burner," to the Göttingen physics colloquium, and his superior, Georg Joos, wrote a letter to the Reich Ministry of Education, which controlled the universities. With famed German efficiency, rather than bureaucratic bumbling, the Ministry quickly named Abraham Esau, president of the Physikalisch-Technische Reichsanstalt (the national bureau of standards), to head a conference. This was held on 29 April, just a week after the Paris group's paper appeared, and it was decided to obtain all uranium stocks in Germany. Esau also banned the export of uranium compounds and negotiated for radium from the recently captured Czechoslovakian mines at Joachimsthal. Events moved on a parallel track as well. On 24 April, again reacting to the *Nature* article, the physical chemists Paul Harteck and Wilhelm Groth, at Hamburg, wrote to the War Office, suggesting the possibility not of a reactor but of an explosive. Kurt Diebner, an Army expert on nuclear physics and ordnance, was placed in charge of this project and, by summer 1939, despite the skepticism of his superiors, established an independent office for nuclear research within the Army Ordnance Department. "By the time war broke out, Germany alone—of all the world powers—had a military office exclusively devoted to the study of the military applications of nuclear fission."[116]

Other countries were not far behind. Soviet scientists likewise reacted to the Joliot team's results. Igor Tamm is reported to have asked a group of students: "Do you know what this new discovery means? It means a bomb can be built that will destroy a city out to a radius of maybe ten kilometers."[117] In Leningrad, Igor Kurchatov, who became the Russian equivalent to Fermi, Oppenheimer, and Vannevar Bush rolled into one, and his

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associates investigated neutron-uranium reactions. Though interrupted by what became more pressing wartime scientific needs, they were working on an atomic bomb by the time of Hiroshima and Nagasaki in 1945.[118]

The British also were heavily involved in uranium research. The French paper of 22 April struck George P. Thomson, of Imperial College, London, as vitally important, a view shared by Rutherford's successor at the Cavendish Laboratory, W. Lawrence Bragg. While a power source was an even chance, an explosive was far less probable. Yet, even such a slim possibility had to be investigated. With remarkable speed they began turning the wheels of government, and on 26 April the Minister for the Co-ordination of Defence asked the Treasury and Foreign Office to obtain all the Belgian uranium they could. As it turned out, the Belgian Union Minière Company, which extracted radium from uranium ore mined in Africa, had only small stocks on hand; a large amount had recently been shipped to America. But the president of the company agreed to inform Britain of any unusual interest in uranium and its source. Experimental work was undertaken by Thomson and by Mark Oliphant at Birmingham. Yet when war came in September, it was easy to turn to other matters, for the chief scientific adviser to the government, Henry Tizard, felt that a bomb could not be made, and Frederick Lindemann of Oxford, who usually disagreed with Tizard, had urged his friend Winston Churchill to write to the Secretary of State for Air with the same advice.

Despite some semi-optimistic analyses in the autumn by James Chadwick, the neutron's discoverer, then at Liverpool, uranium research was in danger of being abandoned. Resurrection came in the spring of 1940, when Otto Frisch, by now at Birmingham, and his host there, Rudolf Peierls, calculated that not tons but only a few kilograms of metallic U-235 would suffice to make a bomb. Besides establishing a much smaller critical size than was earlier thought necessary, they suggested a means of assembly to avoid predetonation. In overcoming these grounds for disbelief that such a weapon could be constructed during the present conflict, Peierls and Frisch not only gave new life to the British efforts, but similarly inspired a faltering American project. Eventually, the work done in England was transferred to the United States and Canada and became part of the Manhattan Engineer District.[119]

But to return to the events of 1939, news of fission continued to receive as good press from May through the summer, as it had in the earlier months. The British scientific magazine *Discovery* editorialized in May that "this result [of secondary neutron emission] is of great importance and, with a little imagination, one can easily regard this as the first real step towards the elusive goal of harnessing the vast store of nuclear energy which is released only very occasionally in processes so far investigated."[120] *New York Times* science writer William L. Laurence used far less restraint in extolling the virtues of uranium-235 as the "philosopher's stone" that would enable one to tap "the vast stores of atomic energy." He recognized the difficulty of separating appreciable quantities of this isotope, but cited an optimistic statement by Arthur H. Compton that thermal diffusion might achieve this task. Disregarding the more cautious estimates made at the American Physical Society meeting just a week earlier, Laurence wrote that a tiny amount of U-235 "would blow a hole in the earth 100 miles in diameter. It would wipe out the entire City of New York, leaving a deep crater half way to Philadelphia and a third of the way to Albany and out to Long Island as far as Patchogue."[121]

A thoughtful editorial in the July issue of *Scientific American* indicated that the dilemma physicists faced was becoming more apparent. Any time man found a new force, means were sought to turn it to destructive

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uses. Physicists might initially decide, therefore, to leave the subject, but upon reflection would realize that they would only be abandoning it to the "world conquerors." "The physicist cannot stop." Nor can he be confident whether any discovery "will be a curse or a boon."

> He is a part of the stream of only slightly foreseeable human events. He cannot control his own discoveries, once they are given out, for he is far outnumbered. And if the human race won't leave its new playthings alone, and gets badly hurt, that's its own funeral. In a few years we may have the answer.[122]

In time, a number of scientists would come to disagree with the editorial's tone of inevitability and its attitude that the human race deserves whatever it gets. But in 1939 the sense of social responsibility so widely acknowledged today and widely discussed even then was weak. In that milieu, few scientists could, for moral or ethical reasons, renounce work on a weapon. Moreover, fission in 1939 is not the best yardstick of social responsibility, for the Nazi regime in Germany was considered the greatest evil.

Nevertheless, the explosive potential of fission led Swiss physicist Auguste Piccard, famous for his balloon ascents to high altitude and his attempts to set similar records for underwater descents, to call it a "diabolical discovery." He hoped that it would prove impossible to create a fast chain reaction, although a slow, controllable process would be a boon to humanity.[123] The problem received further attention in *Discovery*, where the danger of a runaway experiment was discussed, but the more serious choice between "a pinch of salt" to fuel the *Queen Mary* and Wellsian chaos created by nations "dropping bouquets of uranium bombs" was emphasized.[124] *Discovery's* editor, British scientist and novelist C. P. Snow, summarized the situation on the eve of World War II. "The power of most scientific weapons," he wrote, "has been consistently exaggerated; but it would be difficult to exaggerate this." Snow was pessimistic should a bomb prove possible. "We have seen too much of human selfishness and frailty to pretend that men can be trusted with a new weapon of gigantic power." Aviation, he claimed, perhaps with the April 1937 destruction of Guernica in mind, had impoverished, not enriched, life. "We cannot delude ourselves that this new invention will be better used." And yet, Snow concluded, the bomb must be made if physically possible. "There is no ethical problem," because there is no secret. Every large laboratory on earth will achieve the same results, and it must be done sooner in America than in Germany.[125]

Snow, of course, was correct. Research proceeded in a great many laboratories. In his own country, specifically at the University of Birmingham, Rudolf Peierls, in June 1939, faced that very same question of secrecy that had confronted others. He wrote a paper on the nature of a critical mass and then had reservations about publishing it, "not because of the power of the mathematical result it contains, because that wasn't very deep, but because one didn't want to draw unnecessary attention to something as delicate as that." He went ahead when he was convinced that the likelihood of a chain reaction in ordinary uranium was negligible; it would require the separation of tangible quantities of U-235, an improbable task.[126]

Szilard did not know of this particular quandary, and because he felt he knew so little about research activities elsewhere he could only engage in "worst case" analyses. Indeed, he was becoming increasingly frustrated. At the beginning of the year he had raised money from friends to rent the radium needed to generate neutrons. In typical Szilardian fashion he also had filed a patent application covering a power source, the production of radioactive elements, and an explosive, had coordinated research efforts at Columbia and the Carnegie Institution, including negotiating with the Radium

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Chemical Company for a gram of the element to be used by Tuve, and had even incorporated a tax-free "Association for Scientific Collaboration," useful to impress potential patrons. Private funds now were being used to rent the enormous amount of 2.35 grams of radium, to purchase 500 pounds of uranium oxide, to buy released time for Walter Zinn from his post at the City College of New York, and to employ a graduate chemist full time as a technician.[127] But progress elsewhere on chain-reaction physics seemed to have come to a halt, and the attempt at self-censorship had failed. Szilard was investigating a chain reaction in a system of uranium and carbon, the carbon showing signs of being a better moderator of neutrons than ordinary water, and was in contact with Union Carbide, Monsanto, U.S. Graphite, National Carbon, and other suppliers for graphite of highest purity.[128]

Coincident with this frenzy of Szilard activity, Fermi, who had collaborated with Szilard on a uranium-water system, left New York for the summer to study not nuclear fission, but cosmic rays, at Ann Arbor. Szilard kept him informed by mail of plans for a large uranium-carbon experiment.[129] In Paris, Joliot's group worked on a uranium-heavy water system, with, as they believed, more of an emphasis on obtaining "quick-and-dirty" proof that a chain reaction could be initiated, than on what they viewed as the American focus on accurate scientific data. The French were concerned far more with a power source than a bomb, for they recognized the industrial potential (with possible financial benefit to science) and wished to secure their nation's position.[130]

At a meeting of the American Physical Society at Princeton in June, Szilard spoke to Ross Gunn about fission applications. Gunn was the technical adviser of the Naval Research Laboratory which, since Fermi's approach to the government in March, had maintained an interest in uranium. Despite the Navy's desire to assist, Gunn soon wrote to Szilard that regulations prohibited negotiating any meaningful contract. That both Fermi and Szilard were aliens may also have reduced the motivation to seek a way around the restrictions.[131]

Another factor in the difficult process of getting academic scientists and government to embrace warmly instead of dancing at arm's length was the sheer novelty of it. Scientists feared for their independence and government had doubts about the value and propriety of underwriting research. As late as 1945, the idea of government subsidy of research in universities was anathema to such pillars of the Establishment as Robert Millikan, and Frank Jewett, the president of the National Academy of Sciences. Both pressed such views on Vannevar Bush when he was composing *Science, the Endless Frontier* (1945), his blueprint for the postwar relationship between science and government.[132]

Yet, awkward as this connection was in the United States, Szilard had no alternative, and he was, after all, thinking ahead to a weapon. By mid-1939, he and Wigner decided that the rich supply of uranium ore from the Belgian Congo must be kept from German hands. Albert Einstein knew the Belgian queen, and Szilard knew Einstein, having, in fact, taken out a few patents with him in Berlin. Would Einstein ask the queen to prevail on her countrymen not to sell uranium to the Germans? Einstein was tracked down to a summer cottage on Long Island Sound and told for the first time about the possibility of a chain reaction. He preferred an approach to a member of the Belgian cabinet and, on Wigner's suggestion, agreed to allow the American State Department to see the letter first, since they were concerned about the appropriateness of communicating with a foreign government. Szilard then had some second thoughts and sought advice on the best course of action from another ref-

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ugee with better-developed diplomatic antennae. This led him in turn to an economist with direct ties to the White House, Alexander Sachs, who offered personally to deliver Einstein's letter to President Roosevelt.[133]

The letter was drafted by Szilard, with input from Einstein, Sachs, and, presumably, Wigner and Teller. It indicated the possibility of a nuclear chain reaction, with power, radium-like elements, and an explosive as conceivable products. "A single bomb of this type, carried by boat and exploded in a port, might very well destroy the whole port together with some surrounding territory. However," he added, "such bombs might very well prove to be too heavy for transportation by air." The request for action was minimal. If the president desired, he might appoint a liaison between his administration and the physicists. This person would keep government departments informed of progress and convey requests for government action, such as acquisition of uranium ore. The liaison might also seek funding for the research from private individuals and material help from industrial laboratories with the appropriate equipment. Note that no suggestion was made for direct government subsidy of the research; federal blessing, not money, was sought.[134]

The letter was dated 2 August 1939. With it Szilard enclosed a letter of transmittal to Sachs and a memorandum of some technical points to the president, both dated 15 August.[135] The remainder of August passed, then September, and then part of October. Sachs was waiting for the right opportunity to deliver his documents to Roosevelt—a time when the president's mind would not be completely occupied with the war that had just erupted in Europe, but Szilard and his Hungarian cohorts found the delay difficult. Sachs finally visited the Oval Office on 11 October, where he explained his purpose to Army and Navy ordnance experts as well as to the president. The presence of these officers made it clear that prime consideration was being given to an explosive. Roosevelt recognized the significance of Einstein's letter and ordered action on it.[136]

Action consisted not of referring the matter to the National Academy of Sciences, whose track record in advising government was spotty, or to an established federal bureau, where secrecy might not be maintained, but to a newly-constituted Advisory Committee on Uranium, chaired by Lyman Briggs, director of the National Bureau of Standards. At a meeting on 21 October, of Briggs, the two military ordnance specialists who had been at the White House, two Washington-based physicists, plus Sachs, Szilard, Wigner, and Teller (Einstein declined to attend), discussion centered on the promising uranium-graphite experiments. Specifically, the absorption cross section of carbon for neutrons had to be determined. Almost incidentally, it seems, conversation turned to the possibility of government financing. After an exchange over whether troop morale or new weapons won wars, and Wigner's gentle suggestion that, if the former, the Army's procurement budget could be cut significantly, the Army representative growled, "All right, you'll get your money." The committee recommended the purchase of four tons of pure graphite and, upon satisfactory preliminary results, acquisition of fifty tons of uranium oxide. Six thousand dollars, a considerable sum, was provided by the military in early 1940, from which time private and industrial funding became a dead issue.[137]

It is unlikely that Szilard had any philosophical reservations about government support. More probably, he was anxious to get on with the job, and welcomed assistance whatever its source. Moreover, he had been rebuffed by the Union Carbide and Carbon Company in October 1939, when he asked merely for the loan of two thousand dollars'

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worth of graphite.[138] Also in October, he sought support from a Canadian corporation.[139] Earlier, R. B. Roberts's physicist brother, who was employed by the Radio Corporation of America, found RCA uninterested in patenting a uranium pile and its control mechanisms.[140] In this context, perhaps, Roosevelt's encouragement is all the more extraordinary. The president's interest sufficed to surmount the traditional barriers to government support of university research. No one could, of course, predict the final cost of two billion dollars, or history might have been different. Also, one should not conclude that the path to Hiroshima and Nagasaki was smooth; the Briggs committee, for example, was far too lethargic for Szilard and others, and, unprodded, might have relegated the Einstein letter to an inconsequential footnote of World War II. Then, too, a bomb project was likely as the result of stimuli other than Einstein's letter. Yet, the document remains important not only for its drama, but for its role in the process of increasing government interest in, and support of, basic science.

During the closing months of 1939 there was less published about fission because of the war in Europe. That the Germans also appeared to restrict such articles was apparent to American physicists. Nevertheless, the possibility of constructing nuclear weapons continued to be discussed openly, in such places as a review article on fission in *Scientific American,*[141] and in the *New York Times's* annual roundup of science, where fission was called the year's greatest discovery.[142] Likewise, the two major scientific reviews of events in 1939, by Otto Frisch for the Chemical Society of London, and by Louis Turner in the *Reviews of Modern Physics,* cited the possibility of an explosive chain reaction.[143] The year's end was also marked by a public address in Copenhagen by Bohr in which he discussed the destructive potential of the fission process.[144]

While the Allied effort to construct atomic bombs in World War II, popularly called the Manhattan Project, was cloaked in secrecy, it is clear that no literate scientist or layman in 1939 need have been unaware that such weapons were conceivable. Bombs and power plants were widely discussed; ethical and moral questions also were raised, but in the political climate of that year were regarded as inconsequential.

Conclusion

The discovery of nuclear fission obviously created a sensation. Why? Not only was there a long tradition of scientific discussion about the likelihood of harnessing atomic energy, but there was a strong literary tradition as well. Anatole France, in *Penguin Island* (1908), H. G. Wells, in *The World Set Free* (1914), Harold Nicolson, in *Public Faces* (1932), and J. B. Priestley, in *The Doomsday Men* (1938), were some of the authors who brought fictional atomic destruction to their considerable readership. The reason why these scientific and literary predictions of usable atomic energy were so widely discounted was that no scientist was prepared to believe that an atom could break off large pieces. Yet, in order to benefit from the conversion of mass to energy, large chunks of heavy atoms had to be detached.[145] All optimistic claims, therefore, were based on improving the efficiency of known processes. No one anticipated the discovery of a new phenomenon of nature, such as fission.

It is true that Rutherford and his good friend at Yale, the radiochemist Bertram Boltwood, in 1905 contemplated the rupture of radioelements by processes other than alpha- and beta-particle emission;[146] that Boltwood's Yale colleague, Henry Bumstead, spent a 1904–1905 sabbatical year in the Cavendish Laboratory where J. J. Thomson encouraged him to "smash up atoms with Röntgen Rays" (J. J. was "so anxious to bust atoms artificially that . . . he would have

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tried it with a cold-chisel before long"),[147] and that Rutherford's former student at McGill, Samuel J. Allen, who spent his professorial career at the University of Cincinnati, in 1909 conducted research whose stated goal was to decide if the atom "can be disintegrated by outside agency."[148] But the only reactions they and others observed, either in natural radioactivity or induced transformations, involved electrons, protons, or at most alpha particles. After looking for, and *not* detecting other types of reactions, and with over four decades of such experience, the discovery of large-fragment fracture was understandably surprising. Equally, fusion was discussed as a possible process to yield energy, and experimental success was achieved in 1934, when Mark Oliphant, Paul Harteck, and Rutherford bombarded deuterium with deuterons.[149] But, again, more energy was consumed in accelerating a great many particles than was realized in the relatively few interactions.

Regarding another significant process, the concept of a chain reaction was sufficiently well understood to raise no eyebrows, although its application in physics was infrequent. From the mid-1920s, N. N. Semenoff and his Leningrad colleagues added much to the knowledge of branching chain reactions of chemical kinetics,[150] and one could easily visualize a firecracker (or molecule of high explosive) igniting a neighbor, such that Szilard's patents in the 1930s quite logically included a chain reaction. Yet, Rutherford's claim that the nuclei of atoms are so far from each other that the disruption of one (as, e.g., in radioactive decay) would likely go unnoticed by its neighbors made sense to scientists.[151] The discovery of fission thus overturned "established" patterns of nuclear disintegration, and the emission of neutrons in the process toppled the "logic" that a chain reaction could not be maintained. These upheavals, and the research opportunities they presented, suffice to explain the intense scientific interest in fission, while the obvious applications were fascinating to scientist and layman alike. Yet, these applications raise moral and ethical problems.

Let us take as simple working definitions the following: morality concerns whether something is right or wrong (is it personally right or wrong to design weapons?), and ethics is confined to matters of proper professional behavior (should science be used for destructive purposes?).[152]

Did scientists express moral positions concerning fission research? Merle Tuve, although active in nuclear physics, chose to work on the proximity fuse, which he regarded as a defensive weapon.[153] It may be noted in this connection that the MIT Radiation Laboratory, where another partially defensive weapon, radar, was developed, had no trouble recruiting scientists.[154] Richard G. Fowler, then at the University of Michigan, and for most of his career at the University of Oklahoma, entered on his War Manpower Commission form a refusal to work on weapons of mass destruction, although he notes, "I did not know at that time that they were possible!"[155] Joseph Platt, at the time on the staff at the University of Rochester, and for many years later president of Harvey Mudd College, declined Manhattan District job offers and went, instead, to work on radar. The choice, in part, was based upon a moral or ethical (he calls it aesthetic) feeling that it was a "perversion of a major intellectual accomplishment . . . to make the first practical application of that knowledge a huge bomb."[156] Alexander Langsdorf, first of Washington University, St. Louis, and then of the Chicago Metallurgical Laboratory and Argonne National Laboratory, states that he "consciously avoided going to Los Alamos when I might have."[157] Volney C. Wilson, at the University of Chicago, performed some chain-reaction calculations for Arthur Compton, and then asked to be taken off such work. But after the Japanese attack

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on Pearl Harbor he changed his mind and ultimately joined the Los Alamos staff.[158] Max Born, forced from his chair at Göttingen and during World War II at the University of Edinburgh, was never even asked to work on the bomb. He explained, "My colleagues knew that I was opposed to taking part in war work of this character which seemed to me horrible."[159] For apparently similar reasons other scientists in Great Britain declined to join in research on fission explosives,[160] and a number of men at Los Alamos had profound reservations about their work, before Hiroshima as well as after.[161] Indeed, after the war some physicists switched to biophysics in a conscious effort to leave a field that was useful to the military.[162]

Note, however, that not a single one of these examples occurred in 1939; most took place in 1942 or later. By 1945, the most quoted reaction was that of Oppenheimer: "We have made a thing, a most terrible weapon, that has altered abruptly and profoundly the nature of the world. We have made a thing that by all the standards of the world we grew up in is an evil thing."[163] But in 1939, it was simply too soon for morality and ethics to be of more than evanescent concern. While many scientists immediately saw the possibility of a bomb and even spoke openly of it, as shown above, there was little extended discussion either of its construction or its implications. Most recognized the difficulty of the problems that had to be solved before even a chain reaction was proven possible. For a bomb, the matter was more complex still. The likelihood of isolating enough U-235 was considered remote, and the ability to achieve the necessary speed of assembly of a supercritical mass was doubtful.[164] Because they felt that the application of fission must be a long-term project, unlikely to be pursued during the threatening war, and unlikely to be pursued by *them*, shop-talk of fission could focus upon its quite fascinating scientific aspects.[165] It was still basic research. For those involved, the drift towards an application was unreal in a sense: they did not believe a weapon could be constructed, but they had to prove that negative themselves.[166] The alternative, if their prediction was wrong, was that Hitler might acquire the bomb. Meanwhile, they were "intrigued with a fascinating and difficult scientific and engineering problem."[167] And if a bomb could be made, it was inevitable that some country would do so; the United States must be first. Victor Weisskopf summarized these ideas well:

> The year 1939 changed many things. It witnessed the beginning of the most destructive war in history. It has also changed science. Many physicists who never were interested in applications of science devoted their skills to the necessities of war and became applied physicists. They faced new problems, new experiences, different from the accustomed academic environment. But the deepest change in the character of our science came from the discovery of fission. Many of us hoped at that time . . . that the number of neutrons released would have been small enough to prevent a chain reaction. But soon enough it was clear that, on the forefront of the most esoteric and basic part of our science, a phenomenon was discovered, full of tremendous destructive and constructive potentialities. It was not yet ready for exploitation; many staggering problems had to be solved, but the way was clearly indicated. Many physicists were drawn into this work, by fate and destiny rather than enthusiasm. A threat hung over us, the frightening possibility of finding this new and incredibly powerful weapon in the hands of the powers of evil, but there is no doubt that we were also attracted by the unique challenge of dealing with nuclear phenomena on a large scale, with taming an essentially cosmic process.[168]

There was the opportunity for personal choice, but it rarely presented itself clearly. Given the international conditions in 1939, there seemed little option but the pursuit of applications of science. When a bomb was discussed, it usually was done abstractly—*if*, not *when*, it were made—and the moral dimension ignored, or implied by common statements to the effect that civilization must

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be saved from Nazi domination. Note that the Einstein letter to President Roosevelt avoided moral or ethical matters, and most other comments by scientists in 1939 were similarly constructed. Although history has shown that they were overly impressed with German scientific and technological capabilities, they were fully justified at that time to fear Germany's threat to the world.

Some Europeans believed war would be avoided, and many Americans doubted that the United States would become involved. For them there was no pressing need to consider fission explosives.

In addition to these above reasons why in 1939 the construction of a bomb provoked relatively little *serious* consideration in America, and similarly its attendant moral and ethical questions, other factors highlight the nature of this reluctance. The goal of the British Social Relations of Science movement, prominent in the 1930s, was to encourage applications of science, not discourage them. Whether or not one sympathized with this group's left-leaning politics, many scientists could share the desire to make their work "useful." Moreover, the common view that "pure" scientists consider applications of science beneath their dignity is probably far too extreme. Lord Kelvin, Albert Einstein, Frederick Soddy, Leo Szilard, and many others have patented devices. It is conceivable that applied science is not such a radical departure for basic scientists.

Long-standing traditions of international contact in science,[169] coupled with the flow of refugees from Germany, made the scientific community especially aware of the Nazi threat. Consequently, many would not hesitate to embark upon a weapons project.[170] In fact, Donald Hornig, who was successively at Harvard, Brown, Princeton, in Washington as the president's science adviser, then back at Brown as president, recalls now at Harvard the attitude he saw in 1939 and 1940 of men actively seeking "out ways to participate in the war effort vs. Germany," and deploring "the slowness of the government in making use of academic scientists in military research."[171] For most, there was no self-consciousness about research for military purposes until the war ended in Europe, and before this time fission raised no more moral or ethical questions than did conventional weapons.[172] Henry Linschitz, at Los Alamos during the war, and later at Syracuse and Brandeis, argues that "a strong sense of professional community and shared intellectual adventure probably operated to bring scientists *into* the bomb development project, rather than to keep them *out* of it."[173] If fission weapons were not extensively and deeply discussed in 1939, it was despite these attitudes that normally would encourage such analysis.

Another point concerns the nature of scientific activity and the attitudes of scientists. Although we can point to military accomplishments by Thales and Archimedes in antiquity, and to poison gas warfare in this century, scientists before World War II generally felt that their work had little effect upon society. Their mobilization for the development of weapons, although a real application of their knowledge, was not part of their normal activity. They were not professionally socialized to think about the implications of their work, and they certainly were no better at futurism than the rest of the population. Nor should they have been so. The vast majority of scientists are inherently non-speculative and non-philosophical; they prefer to stick closely to the data and extrapolate from them no more than prudence allows. While one can, of course, find notable examples of scientists who *do* speculate to varying degrees—including the rather superficial predictions described in this paper's background section—most in this profession tend not to inflate their statements' value or paint them in purple prose. Indeed, public remarks generally are carefully crafted, because professionally they engage in constant self-criticism. When they do speak, they

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usually limit themselves to statements of scientific fact and possibility. Rarely do they make statements of warning or of promise. Rarely do they engage in fantasy; that is better done by novelists. Cries of catastrophe more often come from those who *do* speculate beyond the data. While scientists are creative people in their own realm, they tend elsewhere to be no more imaginative, prescient, or possessed of extraordinary foresight than other humans. To expect that in 1939 they should have focused quickly upon the significance of a new discovery, explored carefully its potential consequences, and weighed in the balance the question of their own involvement exaggerates the abilities and orientations of scientists.

What was true individually seems also to be accurate collectively. There was time for group decision-making, as shown by the censorship attempt, but the opportunity for ethical, professional choice regarding work on a bomb was unperceived and the community drifted (or marched) into weapons development. There were no vocal protests from individual scientists and no organized opposition. That would have been unlikely in any case, since scientific societies, the basis of organized activity, regarded themselves far more as scholarly bodies than vehicles for activism. It was improper to make pronouncement on public issues. The one domestic organization committed to a social role, and inspired by the Social Relations of Science movement in England, was the American Association of Scientific Workers, and it was silent on this issue.[174] Indeed, a moratorium on fission research would have been a violation of the normal scientific behavior of investigating an interesting phenomenon. Its achievement would have been far more striking than a curtain of self-censorship, and the latter succeeded only after war broke out.

This does not mean that scientists always turned a blind eye to controversial interactions with society. In a lecture delivered in 1936, Rutherford spoke as an elder statesman on the question of the social responsibility of scientists:

> During the last few years, there has been much loose and uninformed talk of the possible dangers to the community of the unrestricted development of science and scientific invention. Taking a broad view, I think that it cannot be denied that the progress of scientific knowledge has so far been overwhelmingly beneficial to the welfare of mankind. . . . It is, of course, true that some of the advances of Science may occasionally be used for ignoble ends, but this is not the fault of the scientific man but rather of the community which fails to control this prostitution of Science. . . . It is sometimes suggested that scientific men should be more active in controlling the wrong use of their discoveries. I am doubtful, however, whether even the most imaginative scientific man except in rare cases is able to foresee the ultimate effect of any discovery.

Rutherford proposed, however, that the government create a "Prevision Committee," somewhat akin to the U.S. Congress's present Office of Technology Assessment, which would seek to determine when the application of a discovery might have an adverse effect on the public.[175]

This attitude, that, on balance, science has been beneficial and that scientists generally have little control over the uses of their work, has been the dominant view throughout the twentieth century. Scientists were free to be oblivious to the implications of their research. Their links to the military were not yet firmly established. Government support was still limited primarily to research done in government laboratories. The whole system of research and its patronage left scientists responsible to the scientific community itself and to a few philanthropic foundations. This was a period of adolescence, before the military found science too important to be left to the scientists, and before science found research too expensive to do without government funding.

In 1939, if one were to define where social

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responsibility entered science, it would most likely have been where basic turned into applied science. There was no precedent of being held responsible for fundamental research that had been perverted for evil purposes. Certainly, in a subject still so opaque as fission physics, and with no concept that basic research would lead to harm of society, one could be unaware that ethical or moral questions existed. In more recent years, the definition of social responsibility has been enlarged to include basic research with potentially undesirable consequences. The prime example here is of biologists working with recombinant DNA who, in the 1970s, considered a moratorium and did establish guidelines for themselves. There was, it must be observed, an immediacy in the 1970s that was lacking in 1939. In addition, attitudes have changed to the point where many scientists consciously refuse to engage in any military-related research.

Another insight to the change in our perspective since that prewar period lies in Szilard's inability to conceive that government would, or should, fund the investigation of fission. His goal was to obtain private or corporate philanthropy. It is true that Pegram quite early in 1939 did arrange for Fermi to speak with Navy representatives, but the distinction may be that it was felt to be proper to inform the government of a military application, while at the same time anticipating no financial support. In those days, Szilard's orientation was hard-headed; the tradition of government support of research was weak. During the period from 1935 to 1939, for example, neither the Army nor Navy spent more than a meager seven million dollars a year on research.[176] Moreover, there may have been a conscious aversion toward seeking government support. The recent interaction between science and government in some countries was not marked with happy memories. Nazi racial theories and their condemnation of theoretical (i.e., Jewish) physics undercut Germany's strength in science,[177] while the detention of Peter Kapitza by Soviet authorities in 1934, after he had worked in England for thirteen years, was regarded as a blow to the international character of science.[178]

This search of the first year of fission research has uncovered no "smoking gun"; there was no widespread but submerged current of morally- or ethically-based opposition either to the fundamental investigation or to potential applications. Does this mean, therefore, that scientists should not be expected to help society avoid disasters? This cannot be answered with certainty, of course, but a qualified "no" is possible. As argued above, scientists have no claim to greater foresight than others, and are even conditioned not to be speculative. But events of the past four decades have sensitized them to the role of social responsibility. Individuals have spoken out on many issues, leading to beneficial public debate, and scientific societies now feel comfortable in supporting some "causes." A perfect early warning system will never be erected, yet we may expect ever more attention to be paid to the implications of scientific research.

NOTES

1. The authors express their thanks to the University of California's Institute on Global Conflict and Cooperation for funding this project. Our appreciation is extended also to the many scientists who responded to our questionnaire, to Alan Beyerchen for his unpublished 1968 study entitled "Prelude to the Atom Bomb," and, for help and/or permission to reproduce certain items, to Judith Goodstein, Caltech Archives; Lynda Claassen, Department of Special Collections, University of California, San Diego; Spencer Weart, Center for History of Physics, American Institute of Physics; Jean St. Clair, National Academy of Sciences Archives; Lewis Brown, Department of Terrestrial Magnetism, Carnegie Institution of Washington; Robin Rider and the staff of the Bancroft Library, John Heilbron and Bruce Wheaton, Office for History of Science and Technology, both at the Uni-

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versity of California, Berkeley; Richard G. Hewlett; Luis Alvarez; Mrs. Ernest O. Lawrence; Roger Stuewer; Paul Hoch; and Sonja McCoy.
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80. Interview of R. R. Wilson by S. Weart, 19 May 1977, p. 55, CHP/AIP.
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82. J. R. Dunning, note 50. Also see "Fateful night," *New Yorker,* 21 (18 Aug. 1945): 15–16.

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83. Details of these conferences appear in many papers in the DTM archives, CIW, as well as the published *Yearbooks of the Carnegie Institution of Washington*
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89. *New York Times,* 3 Feb. 1939, p. 14.
90. *New York Times,* 5 Feb. 1939, sect. 2, p. 9.
91. Watson Davis and Robert D. Potter, "Atomic energy released," *Science News Letter,* 35 (11 Feb. 1939): 86–87, 93. Potter, a reporter, worked as a volunteer in the DTM/CIW laboratory to gain a better understanding of his material.
92. *Newsweek,* 13 (13 Feb. 1939): 35–36.
93. G. B. Pegram letter to M. Tuve, 3 Feb. 1939, Tuve collection, box 17, Library of Congress.
94. N. Bohr letter to O. R. Frisch, 3 Feb. 1939, BSC, reel 19, note 49.
95. E. O. Lawrence letter to Alex J. Allen, 7 Feb. 1939, Lawrence collection, Bancroft Library, University of California, Berkeley. Other letters in this collection are E. O. Lawrence to James M. Cork, 7 Feb. 1939; to E. Fermi, 7 Feb. 1939; to J. Stuart Foster, 7 Feb. 1939; to Donald G. Hurst, 9 Feb. 1939; to John Cockcroft, 9 Feb. 1939.
96. P. Abelson nightletter to J. Tate, 2 Feb. 1939; P. Abelson letter to J. Tate, 13 Mar. 1939; E. O. Lawrence letter to J. Tate, 11 Apr. 1939, Lawrence collection, Bancroft Library, University of California, Berkeley.
97. G. T. Seaborg, "Public service and human contributions," in I. I. Rabi, et al. (eds.), *Oppenheimer* (New York: Scribner's, 1969), 48–49; quoted in Alice K. Smith and Charles Weiner (eds.), *Robert Oppenheimer: Letters and Recollections* (Cambridge: Harvard University Press, 1980), 207.
98. Interview of W. A. Fowler by C. Weiner, 8–9 June 1972, 75–76, CHP/AIP. Also see J. R. Oppenheimer letter to W. A. Fowler, 28 Jan. [?] 1939, printed in A. K. Smith and C. Weiner, note 97: 207–8.
99. Questionnaire returned by R. Serber, Summer 1983.
100. R. G. Hewlett and O. E. Anderson, note 46: 15. M. Tuve letter to L. Szilard, 27 Mar. 1939, "uranium" file, DTM archives, CIW.
101. J. A. Wheeler, "The discovery of fission," *Physics Today,* 20 (Nov. 1967): 49–52; quote on p. 52. In "Some men and moments in nuclear physics," in Stuewer, note 38: 282, Wheeler gives a different date of 16 Mar. 1939.
102. J. R. Oppenheimer letter to G. Uhlenbeck, 5 Feb. [1939], printed in A. K. Smith and C. Weiner, note 97: 209.
103. L. Szilard memorandum to V. Weisskopf, 31 Mar. 1939, Szilard collection, file sbs-6, Library, University of California, San Diego.
104. M. Tuve letter to G. Breit, 12 Apr. 1939, DTM archives, CIW.
105. Unpublished typescript sent to the authors by G. Young.
106. W. Kaempffert, "The week in science," *New York Times,* 5 Mar. 1939, sect. 2, p. 9.
107. Anon., "Exploding uranium atoms may set off others in chain," *Science News Letter,* 35 (11 Mar. 1939): 149.
108. *Newsweek,* 13 (27 Mar. 1939): 32. For other media references to fission, especially power-plant applications, see Anon., "Confirm release of neutrons from splitting uranium atoms," *Science News Letter,* 35 (1 Apr. 1939): 196. Anon., "Atomic energy cannot compete as power source," *Science News Letter,* 35 (8 Apr. 1939): 217. Anon., "Scientists split uranium, create record discharge of atomic energy," *Life,* 6 (24 Apr. 1939): 52.
109. "Vision earth rocked by isotope blast," *New York Times,* 30 Apr. 1939, sect. 2, p. 35.
110. "Physicists here debate whether experiments will blow up 2 miles of the landscape," *Washington Post,* 29 Apr. 1939, p. 30.
111. Luis Alvarez, for example, remembers no discussion at that time of the social implications. See interview of Alvarez by C. Weiner, 14–15 Feb. 1967, 44, CHP/AIP.
112. L. Szilard, note 63: 107–10. Activities in Joliot's laboratory, and the reasons why the Parisian group decided to publish, are described in the interview of Lew Kowarski by C. Weiner, 20 Mar. 1969, pp. 74–75, 79–82, CHP/AIP.
113. The story of censorship attempts is treated in detail in Spencer Weart, "Scientists with a secret," *Physics Today,* 29 (Feb. 1976): 23–30.
114. S. Flügge, "Kann der Energieinhalt der Atomkerne technisch nutzbar gemacht werden?" *Die Naturwissenschaften,* 27 (9 June 1939): 402–10. Translation by Eugene Rabinowitch.
115. H. von Halban, F. Joliot-Curie, and L. Kowarski, "Number of neutrons," note 54. Note that similar papers by H. L. Anderson, E. Fermi, and H. B. Hanstein, and by L. Szilard and W. H. Zinn, appeared a week earlier in the *Physical Review,* note 54, but the Europeans apparently saw, and reacted to, *Nature* sooner.
116. David Irving, *The Virus House* (London: Kimber, 1967), 32–39; quote on 39.
117. Herbert York, *The Advisors: Oppenheimer, Teller, and the Superbomb* (San Francisco: W. H. Freeman, 1976), 29.
118. I. N. Golovin, *I. V. Kurchatov* (Bloomington, Indiana: Selbstverlag Press, 1968), 31–33. David Holloway, "Entering the nuclear arms race: the Soviet decision to build the atomic bomb, 1939–45," *Social Studies of Science,* 11 (1981): 159–97.

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119. Margaret Gowing, *Britain and Atomic Energy, 1939–1945* (London: Macmillan, 1964), 34–41. Churchill's letter to Kingsley Wood, Secretary of State for Air, 5 Aug. 1939, is reproduced in W. Churchill, *The Second World War. Vol. I. The Gathering Storm* (New York: Macmillan, 1948), 386–87.
120. "Notes of the month," *Discovery*, 2 (May 1939): 239–40.
121. W. L. Laurence, "New key is found to atomic energy," *New York Times*, 5 May 1939, 25. Laurence has quote marks in the newspaper, from the word "leaving" to the end of the sentence. He gives no indication whom he is quoting.
122. "Incomparable promise or awful threat?" *Scientific American*, 161 (July 1939), 2.
123. A. Piccard, "Scientists' pandora box," *Living Age*, 356 (Aug. 1939): 550–52.
124. Douglas W. F. Mayer, "Energy from matter," *Discovery*, 2 (Sept. 1939): 459–60.
125. C. P. Snow, "A new means of destruction?" *Discovery*, 2 (Sept. 1939): 443–44.
126. Interview of R. Peierls by C. Weiner, 11–13 Aug. 1969, 80, CHP/AIP. R. Peierls, "Critical conditions in neutron multiplication," *Proc. Cambridge Philosophical Soc.*, 35 (Nov. 1939): 610–15.
127. Notes by Szilard, undated but probably 1939, Szilard collection, file B-15, Library, University of California, San Diego. Additional documentation regarding radium rental and the Association for Scientific Collaboration may be found in files B-2, B-3, and M-18. Information on patent application 263, 017 of 20 Mar. 1939, may be found in files B-20 and F-52. The application is reproduced in B. T. Feld and G. W. Szilard, note 68: 652–90. It built upon his earlier British patent application on a chain reaction, but Szilard abandoned the application in 1941 for reasons that are not clear. Also see L. Szilard letter to M. Tuve, 22 Mar. 1939, "uranium" file, DTM archives, CIW.
128. Correspondence on graphite, 1939, Szilard collection, files B-5 and B-17, Library, University of California, San Diego.
129. L. Szilard, note 63, 110–11. For the correspondence with Fermi in early July, see B. T. Feld and G. W. Szilard, note 68: 193–98.
130. Interview of L. Kowarski by C. Weiner, 20 Mar. 1969, 94–95, CHP/AIP. Spencer Weart, *Scientists in Power* (Cambridge: Harvard University Press, 1979), 94–103.
131. R. G. Hewlett and O. E. Anderson, note 46: 15–16. Interview of Philip Abelson by Elizabeth Hodes, 18 July 1983. R. Gunn letter to L. Szilard, 10 July 1939, printed in S. Weart and G. W. Szilard, note 70: 89. L. Szilard, note 63: 113–14.
132. J. Merton England, *A Patron For Pure Science* (Washington, D.C.: National Science Foundation, 1982), 15, 20, 35.
133. L. Szilard, note 63: 111–13. R. G. Hewlett and O. E. Anderson, note 46: 16.
134. A. Einstein letter to F. D. Roosevelt, 2 Aug. 1939, FDR Library, Hyde Park, NY. This letter is reproduced in B. Feld and G. W. Szilard, note 68: 199–200. Also see L. Szilard, note 63: 115.
135. These are reproduced in L. Szilard, note 63: 142–45. The letter also appears in B. T. Feld and G. W. Szilard, note 68: 201–3.
136. R. G. Hewlett and O. E. Anderson, note 46: 17. L. Szilard, note 63: 113–14.
137. R. G. Hewlett and O. E. Anderson, note 46: 19–21. L. Szilard, note 63: 114–16. Also see L. Szilard memorandum to L. Briggs, 26 Oct. 1939, on a large-scale experiment, printed in B. T. Feld and G. W. Szilard, note 68: 204–6. Here Szilard still contemplates foundation or industrial support.
138. L. Szilard, note 63: 116–17.
139. L. Szilard letter to A. B. Purvis, 3 Oct. 1939, Szilard collection, file 3bS-8, Library, University of California, San Diego.
140. Unpublished autobiography by R. B. Roberts, 36, written ca. 1978, "Roberts" file, DTM archives, CIW.
141. Jean Harrington, "Two elements for one," *Scientific American*, 161 (Oct. 1939): 214–16.
142. W. Kaempffert, "Highlights of science during the year," *New York Times*, 31 Dec. 1939, sect. 2, p. 7.
143. L. Turner, note 46, esp. 20. O. R. Frisch, note 46, esp. 15–16.
144. The lecture is mentioned in Christian Møller and Mogens Pihl, "Review of Niels Bohr's research work," in S. Rozental (ed.), *Niels Bohr. His Life and Work as Seen By His Friends and Colleagues* (Amsterdam: North-Holland, and New York: Wiley, 1967), 259–60. We are indebted to Roger Stuewer for this reference. Bohr was well known as a humanitarian and deep thinker, both before and after World War II, but if he discussed the consequences of nuclear weapons in 1939, such information has not been found.
145. In other terms, the process of fission was so surprising because it was not just a case of a particle penetrating the potential barrier surrounding the nucleus, but a deformation of the nucleus into a dumb-bell shape and a splitting at that narrow waist.
146. E. Rutherford letters to B. B. Boltwood, 28 Sept. 1905 and 5 Dec. 1905, Boltwood collection, Yale University Library. B. B. Boltwood letters to E. Rutherford, 18 Nov. 1905 and 10 Dec. 1905, Rutherford collection, Cambridge University Library. Printed in L. Badash (ed.), *Rutherford and Boltwood, Letters on Radioactivity* (New Haven: Yale University Press, 1969), 85–86, 105, 108, 110.
147. H. Bumstead letters to E. Rutherford, 18 June 1905 and 30 Sept. 1905, Rutherford collection, Cambridge University Library.
148. S. J. Allen, "On the secondary radiation produced from solids, solutions, and pure liquids, by the β rays of radium," *Phys. Rev.*, 29 (Sept. 1909): 177–211; quote on 211.
149. M. L. Oliphant, P. Harteck, and E. Rutherford, "Transmutation effects observed with heavy hydrogen," *Nature*, 133 (17 Mar. 1934): 413, and, same title, *Proc. Roy. Soc.*, 144A (1 May 1934): 692–703.
150. George B. Kistiakowsky, "The four anniversaries," *Bull. Atomic Scientists*, 38 (Dec. 1982): 2–3.

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151. E. Rutherford, "The transmutation of the atom," BBC National Lecture, 11 Oct. 1933, typescript in Rutherford collection, item 122, p. 25, Cambridge University Library.
152. These definitions admittedly are arbitrary and limited. Philosophy professor David Hawkins points out "The perennial moral problem is how to stay far enough from the centers of power to avoid their constraints and corruptions, yet close enough to have some chances for critical influence." (D. Hawkins letter to L. Badash, Fall 1983.) Personal morality is of a lesser dimension. Also, it should be obvious that the use of a weapon may save more lives than it takes, by ending a conflict sooner. These definitions, therefore may be inadequate, controversial, and philosophically imprecise, yet for our simple historical needs they will suffice.
153. Questionnaire returned by Philip H. Abelson, Summer 1983. Questionnaires were sent to 70 scientists active in 1939, and 37 responses were received. The sample was centered on scientists who published on fission.
154. Questionnaire returned by Hans A. Bethe, Summer 1983.
155. Questionnaire returned by R. G. Fowler, Summer 1983.
156. Questionnaire returned by J. B. Platt, Summer 1983. Platt, in a letter to L. Badash, 27 Mar. 1984, has elaborated on his attitude: "As it happened, I became project engineer for a particularly effective blind bombing system, so I have never been able to feel self-righteous about my choice."
157. Questionnaire returned by A. Langsdorf, Summer 1983.
158. A. H. Compton, *Atomic Quest* (New York: Oxford University Press, 1956), 41–43.
159. R. W. Clark, note 44: 82.
160. Interview of James Chadwick by L. Badash, 11 Feb. 1970. Also see R. W. Clark, note 44: 81.
161. Questionnaires returned by Cyril Stanley Smith, Marshall Holloway, Donald Hornig, and Charles L. Critchfield, Summer 1983, and by Henry Linschitz, 3 Jan. 1984.
162. Questionnaire returned by William A. Higinbotham, Summer 1983.
163. J. R. Oppenheimer, "Atomic weapons," *Proc. Amer. Philos. Soc.*, 90 (Jan. 1946): 7. From a 1945 lecture.
164. Questionnaires returned by P. H. Abelson and W. A. Higinbotham, Summer 1983.
165. Interview of Edwin McMillan by C. Weiner, 1 June 1972, p. 146, CHP/AIP.
166. Questionnaire returned by Kenneth T. Bainbridge, Summer 1983.
167. Questionnaire returned by David L. Anderson, Summer 1983.
168. V. F. Weisskopf, "A memorial to Oppenheimer: The Los Alamos Years," *Physics Today*, 20 (Oct. 1967): 39–40.
169. L. Badash, "British and American views of the German menace in World War I," *Notes and Records of the Royal Society of London*, 34 (July 1979): 91–121.
170. Questionnaire returned by Henry Linschitz, 3 Jan. 1984.
171. Questionnaire returned by D. F. Hornig, Summer 1983.
172. Questionnaire returned by Herbert L. Anderson, Summer 1983.
173. Questionnaire returned by H. Linschitz, 3 Jan. 1984.
174. Elizabeth Hodes, "Precedents for Social Responsibility Among Scientists: the American Association of Scientific Workers and the Federation of American Scientists, 1938–1948," unpublished doctoral dissertation, University of California, Santa Barbara, 1982.
175. E. Rutherford, note 43.
176. Daniel J. Kevles, *The Physicists* (New York: Random House, 1979), 289.
177. Alan Beyerchen, *Scientists Under Hitler* (New Haven: Yale University Press, 1977).
178. L. Badash, *Kapitza, Rutherford, and the Kremlin* (New Haven: Yale University Press, 1985).

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