Leó Szilárd and the Danger of Nuclear Weapons: A Case Study in Risk Mitigation

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Contents

1. Summary

Leó Szilárd patented the nuclear chain reaction in 1934. He then asked the British War Office to hold the patent in secret, to prevent the Germans from creating nuclear weapons (Section 2.1). After the discovery of fission in 1938, Szilárd tried to convince other physicists to keep their discoveries secret, with limited success. In 1939, he wrote a letter to Franklin D. Roosevelt with Albert Einstein warning of the possibility of nuclear weapons and recommending action.

We are interested in how similar Szilárd's efforts are to contemporary efforts to mitigate risks from strong artificial intelligence and also the extent to which Szilárd was successful. This historical data should help us evaluate present efforts and also perhaps design them better.

Szilárd's actions were all intended to respond to a perceived risk of Germany creating and using an atomic bomb. This was a relatively novel threat, one that was to strike abruptly in the future with little opportunity for feedback and learning. Nonetheless, the processes apparently leading to the development of nuclear weapons were more incremental and at least partly observable.

The hypothesis that large amounts of concentrated energy might be created from nuclear reactions was extremely speculative and was not given much credence by relevant physicists until 1939 or later, though its main supporters were also physicists. From 1939 the possibility of weapons became more widely engaged, though it was still generally considered implausible even within the physics community. Concern about applications to the present war were probably seen as highly premature in 1939.

In these ways—novelty, little opportunity for feedback, speculativeness, and low degree of interest from the relevant scientific community—the problem of nuclear weapons resembled the present problem of AI.

We can also compare characteristics of the proposed solutions. How long before the problem is expected are the protective measures to be carried out? How specifically are they directed at the problem (versus being more robustly useful)?

The patent was taken out at least a decade (probably longer) before Szilárd is likely to have anticipated a German nuclear threat. By 1939—the time of the secrecy efforts and the letter—it was plausible that Germany had begun such work. The timeline then appears to have been around two to ten years for a moderate probability of German weapons. AI risk mitigation is probably more farsighted than Szilárd's efforts, but Szilárd's efforts are plausibly farsighted enough to test the efficacy of farsighted interventions.

Szilárd's actions were sometimes directed at forwarding American or British nuclear research, sometimes at slowing German research. Slowing German research was nar-

rowly directed at the problem, whereas speeding local research had broader applications. AI risk efforts are arguably narrowly directed, so in this way they may arguably be less likely to succeed than Szilárd's actions.

Szilárd's success in these endeavors is varied and unclear. The patent did not obviously help. The secrecy efforts probably slowed the German program somewhat, though they very narrowly failed at a particularly important act of censorship. They also brought about more elaborate secrecy institutions, which probably helped but were not anticipated at the time. The letter prompted the government to begin nuclear research. However, it isn't clear whether the resulting organization provided useful impetus for the project, or whether the research would have been begun slightly later with more force anyway.

In sum, Szilárd's endeavors have many close parallels with contemporary AI risk mitigation efforts. Among the parallels we are interested in, they also have differences. None of Szilárd's efforts were clearly successful. However, the secrecy efforts may have been, and they and the letter look like they would have been quite valuable if not for chance. The secrecy efforts are probably the strongest contender for exemplifying risk mitigation similar to AI risk efforts, and are also probably the strongest contender for being successful.

2. Background

2.1. The Nature of the Threat

Leó Szilárd conceived of the nuclear chain reaction in September 1933 or soon afterwards.[1,2] By 1934 at the latest, he had realized that chain reactions could produce weapons.[3] He was concerned about "certain powers" achieving a chain reaction first, and this producing explosives "very many thousand times more powerful than ordinary bombs."[4,5] In particular, he perceived a risk of Germany creating an atomic bomb and using it in war.[6]

2.2. The Patent

Szilárd filed a patent on the chain reaction in 1934.[7] In amendments dated June 28 and July 4, 1934, he described a chain made from particles with no charge, of approximately the mass of a proton; the critical mass; tamper; and the potential for explosives.[8]

Szilard said he took out the patent because he believed the release of large amounts of energy may be imminent and that a suitable body would have to be created to ensure proper use.[9] He apparently did not expect to gain commercially from it.

Patenting was looked down upon in the British science community. However, Szilárd was a recent refugee who may not have been exposed to this norm in the past. He was also unemployed and living off the proceeds of past patents at the time, so it isn't surprising that the idea of patenting a new discovery would readily occur to him.[10,11,12]

Szilárd asked the British War Office to keep the patent secret but was turned down.[13] In February 1936, he offered it successfully to the British Admiralty with the help of a letter from physicist Alexander Lindemann, director of Clarendon Laboratory at Oxford University.[14] Szilárd explained in the letter to the Admiralty that the patented process could contribute to bombs "very many thousand times more powerful than ordinary bombs" and drew attention to the threat of foreign powers using them.[15]

One might wonder how patenting the chain reaction would aid secrecy relative to saying nothing of it. Though a patent is generally public information, prior to the Admiralty accepting Szilárd's patent, the process was probably relatively secret or at least obscure, so the act of patenting at all probably did not much hinder secrecy.[16,17] Beyond that, one can imagine intellectual property rights helping with secrecy, though the details of such intentions are obscure here. Nuclear historian Alex Wellerstein thinks Szilárd was probably not primarily intending to promote secrecy with the patent, but rather hoping to draw UK military attention to it.[18]

2.3. Secrecy

Following the astonishing discovery of fission in late 1938, Szilárd, along with fellow physicists such as Teller, Wigner, and Weisskopf, pushed for other physicists to withhold publication of sensitive papers.[19,20] By May 1940, Szilárd was known throughout the US physics community as the leading proponent of secrecy regarding fission research.[21]

Through conversations and letters, Szilárd and his collaborators convinced many key scientists and journal editors to maintain secrecy.[22,23,24,25]

To aid in promoting secrecy, schemes to improve incentives were devised. One method sometimes used was for authors to send papers to journals to establish their claim to the finding but ask that publication of the papers be delayed indefinitely.[26,27,28,29] Szilárd also suggested offering funding in place of credit in the short term for scientists willing to submit to secrecy and organizing limited circulation of key papers.[30,31]

Szilárd and Weisskopf finally contacted the important French group led by Joliot asking that they join in delaying papers.[32] Joliot's group declined and proceeded to publish a paper revealing the number of neutrons released in fission and commenting that a chain reaction might be feasible.[33,34,35] This paper was highly influential, convincing many physicists that chain reactions were plausible and triggering multiple nuclear weapons programs.[36,37] At this point, the situation seemed futile to decision-maker Pegram, and Szilárd was forced to abandon the effort.[38] Some previously withheld papers were published.

While Szilárd's efforts in a sense failed then, they probably contributed to the development of further censorship and primed physicists to think about the applications of their research.[39] Also, several important papers were withheld, apparently as a result of Szilárd's efforts (see Section 4.2.4).

Other forces for secrecy were also coming into action. Many scientists complained when a particularly risky paper was published about the production of neptunium, which probably helped the secrecy cause.[40,41] As war occurred and worsened, secrecy was imposed in the warring nations.[42] Some people in neutral countries probably also felt more inclined toward secrecy.[43]

In around May 1940, Gregory Breit of the National Academy of Sciences put in place a successful scheme to ensure secrecy in nuclear fission research.[44,45] He organized a committee to filter papers before publication, with sensitive papers to be withheld and circulated narrowly. He rapidly got the support of journals and leading scientists. Breit learned of the secrecy problem via discussions with Szilárd and Wigner, so this was perhaps to some extent a success of their efforts.

2.4. The Letter

In March 1939, Szilárd had been pursuing the construction of a new civilian organization to guide the development of atomic energy and maintain secrecy.[46] Wigner argued that the problem was too serious for them to handle and that they should alert the US president. They called a contact in the Navy and arranged for Fermi to meet with Navy representatives the next day to discuss the issue.[47] However, little came of this meeting.[48]

Later in 1939, Szilárd, Teller and Wigner were again scheming about how to respond to the situation. Wigner again urged that the US Government be informed.[49] Szilárd and his friends were also concerned about the Germans getting large quantities of uranium from the Belgians. In considering how to respond to that problem, it occurred to them to talk to Einstein, who was a friend of both the Queen of Belgium and Szilárd.[50,51] When Szilárd and Wigner visited Einstein on July 16, 1939, the three decided that it would be better to write to a member of the Belgian cabinet and that it would be better still to write to the State Department before approaching a foreign government.[52,53]

Szilárd contacted Gustav Stolper in around July 1939 to ask for his advice in contacting the U.S. government.[54] Stolper discussed the issue with Dr. Alexander Sachs.[55]

Sachs convinced Szilárd that U.S. President Roosevelt should be informed.[56] They gave Sachs a letter to deliver to Roosevelt, and after some delay he delivered it on October 11, 1939, along with his own summary and pitch.[57]

Roosevelt agreed that the issue required action and created the Advisory Committee on Uranium consisting of Lyman J. Briggs, director of the Bureau of Standards, and representatives from the Army and Navy.[58]

The committee was skeptical of the research, and things moved slowly.[59] In mid-1940, the committee was absorbed by the newly created National Defense Research Council, but it continued to be run by Briggs.[60] The NDRC decided not to fund Fermi and Szilárd's research: a uranium-graphite experiment at that point.[61] By the summer of 1941, fission studies were close to being dropped from the war program.[62]

In the meantime, a British nuclear project was active and had sent a report detailing the feasibility of the project to the Americans. Surprised by their limited response to this remarkable missive, Mark Oliphant traveled to America to investigate and discovered with shock that Briggs had declined to share the report with anyone.[63] Oliphant stayed and vocally advocated for American investment in nuclear weapons. He is credited with a substantial part of pushing America into action, alongside other advocates who became more determined at that point.[64,65]

3. Features of Interest

We now investigate the degree of similarity between Szilárd's various efforts to prevent German nuclear advantage and contemporary campaigns to deal with emerging artificial intelligence (AI) risks. We will answer with respect to several axes selected through discussion with Alexander Berger of GiveWell as ones which make AI risk efforts appear particularly futile.[66] To the extent that the efforts are similar on these axes, Szilárd's efforts provide evidence about the promise of contemporary efforts. They might also tell us something about what goes wrong in practice and what is important to get right.

3.1. Timing

Let us consider how long before the perceived advent of German nuclear weapons Szilárd acted to avert the threat. We are interested in this because it seems that AI risk efforts are taking place at least fifteen years before almost anyone expects a problem from AI, and this may be reason to doubt their efficacy.[67]

Szilárd thought of the chain reaction in 1933 and patented it in 1934, eleven years before the first nuclear weapon was used in war.[68] For perspective, this was also around five years prior to World War Two beginning, and four years before the discovery of fission, which prompted other physicists to take an interest in the possibility of a chain reaction.

From early on, Szilárd feared a German threat from nuclear weapons and sought to avert it. However, it is hard to tell how soon he anticipated German nuclear weapons.

3.1.1. A Baseline Estimate of Nuclear Prediction Timelines

One way of estimating when Szilárd would have expected nuclear weapons is to see how long it actually took for nuclear weapons to be developed and adjust this based on any other factors we know about. As we saw earlier, the first nuclear weapon attack was around eleven years after the patent. It was over six years after the secrecy efforts and letter in 1939.

We can perhaps improve these estimates by taking into account whether nuclear weapons development would have been surprisingly fast or slow for any reason and any other evidence regarding the optimism or pessimism of Szilárd's predictions.

Perhaps the largest reason to think US nuclear weapons development would have been surprisingly fast was the massive scale of investment. Around $1.9 billion nominal US dollars were spent on the Manhattan project,[69] which was roughly 0.8 percent of annual US GDP at the time.[70] Germany's economy was less than half the size, so such an investment would have been even more surprising.[71] This investment suggests predictions made ahead of time would tend to be further out unless forecasters predicted the investment, which it seems Szilárd did not, at least, not at that scale.[72] He was apparently frustrated by the slowness of the Manhattan project during it, though this appears to have been relative to the apparent potential of the resources being expended, rather than regarding absolute time.[73]

3.1.2. How Good a Forecaster was Szilárd?

Our baseline estimate was of when one might reasonably predict nuclear weapons. We can modify this according to what information we have on Szilárd's abilities as a forecaster.

In fact, Szilárd was known as an excellent forecaster. He was passionate about politics[74] and was good at predicting world events, according to observers.[75]

He has an impressive anecdotal track record.[76,77,78,79] While selection bias makes this a weaker sign of accuracy than it may seem, it is still good evidence that Szilárd and others thought of Szilárd as a canny observer of world events. This suggests that among people he probably had fairly competent predictions. "Among people" is a big qualifier, and we will not presently investigate biases associated with human forecasters. However, given what we know, it seems that the likely timing of events absent funding on such a massive scale is a reasonable proxy for what Szilard might have expected.

3.1.3. Did Szilárd Worry Early Because He Predicted Early?

One big consideration against taking the actual time of completion as an estimator of Szilárd's expectations is that Szilárd attempted to avert a threat much earlier than others were inclined to. This suggests he may have expected that threat to come to fruition sooner than other physicists did. In that case, perhaps we should take the actual time of completion as an estimator of common expectations and presume Szilárd had much earlier estimates. However, Szilárd's disagreement with others may not have been over timelines—he may have found the risk more probable, or the solution more promising, or had a different opinion on when one should act on things.

In fact, we know about some of Szilárd's disagreements with others. Szilárd characterized his disagreement with Fermi as a disagreement over whether it is "conservative" to suppose a chain reaction is possible (i.e., conservatism means erring toward safety) or conservative to suppose that it is not possible (i.e., conservatism means skepticism about improbable hypotheses).[80] This suggests one main difference is about whether it is correct to act on low probability risks. Other evidence suggests Szilárd favored acting on small chances of catastrophe.[81,82]

We also know that many people thought that a chain reaction was close to impossible, while Szilárd apparently put non-negligible probability on it from early on and high probability on it after large neutron emissions had been observed from fission.[83,84]

Finally, Wellerstein claims that Szilárd prided himself on thinking of things ahead of time.[85]

Given that we can explain Szilárd's early action with his unusually high credence in the problem and solution existing and also with his inclination to act on low probability and far-off outcomes, Szilárd's early action seems weak evidence that he was particularly optimistic about timelines, assuming nuclear weapons were possible.

3.1.4. Later Expectations

There seem to be more records of people's expectations from 1939 onwards. The events of interest are in 1939 at the latest; however, later predictions can still inform our guesses about what people believed earlier. For instance, if in 1941 a person believes something will take a decade, then we might expect that two years earlier, they believed that it would take more than a decade, unless there is particular reason to think they updated upwards.

In 1939, Szilárd claimed that one potential process for partially making a bomb would take five to ten years to run at "technical" scales.[86] He may not have thought this the fastest route, however.

At the initial 1939 Briggs meeting, physicist Richard Roberts thought the answers to questions about isotope separation (a potential step in bomb manufacture) might require several years of work.[87]

When Churwell asked Churchill to invest in bomb manufacture in 1941, he claimed that others working on nuclear research in Britain thought the odds of success within two years were ten to one in favor, though he was more pessimistic.[88]

In 1942, Heisenberg estimated that given maximum support it would take at least two years for Germany to develop a bomb.[89] In 1943, a Japanese Navy physics colloquium estimated that it might take Japan ten years to build an atomic bomb, and likely nobody else could afford one in time for the present war either.[90]

In 1942, Bush estimated that it would take around two years to begin bomb production at the same time he decided to invest heavily in it.[91] Also in 1942, Szilárd's friend Wigner thought a German bomb may be less than a year away.[92]

These predictions collectively point to a bomb seeming at least five to ten years away in 1939.

3.1.5. Conclusions

In 1934, Szilárd probably expected nuclear weapons substantially more than eleven years later: they came eleven years later after huge investments, and Szilárd was a keen forecaster. Thus it is fairly plausible that he expected them at least fifteen years later.

In 1939, nuclear weapons probably appeared to be five to ten years away at least, so it was still plausible though less likely that they appeared to be more than fifteen years away to Szilárd.

Thus, for all of Szilárd's actions, it was plausible though not overwhelmingly likely that Szilárd expected the danger from German nuclear weapons to be at least fifteen years away. For the patent, it is arguably more likely than not.

3.2. Speculativeness and the Views of Scientists

We are interested in whether the purported risk is a clear and straightforward extrapolation of familiar phenomena or a more complex and speculative prediction.[93] We are also interested the extent to which esteemed scientists agreed with the concerned parties. We will discuss these together as they are closely connected.

Naturally, the opinions of other scientists changed between Szilárd's patent and the beginning of the Manhattan project, as did the speculativeness of nuclear weapons. This section will outline the state of opinion and understanding as it changed over time.

In brief, the concept of a nuclear chain reaction alone was extremely speculative at first, and the concept of a weapon based on a nuclear chain reaction much more so. Both were initially almost universally ignored or denounced. Years later, in 1939, the discovery of fission suggested that chain reactions were possible, and physicists began to take the possibility seriously. By 1941 enough of the relevant people (physicists, government officials, and military representatives) found bombs sufficiently plausible that a nuclear program in the US became worth investing in.

3.2.1. Prior State of Scientific Knowledge

In 1933, when Szilárd conceived of the possibility of a nuclear chain reaction, the idea of obtaining energy from atoms in some fashion had been around for at least two decades.[94] Neutrons had been demonstrated to exist less than two years earlier, however, and protons fourteen years earlier.[95,96] Electrons and radioactivity had been discovered around thirty-five years earlier, but the modern picture of an atom as consisting of a nucleus surrounded by mostly empty space was only twenty-two years old.[97,98] It was not until five years later, in 1938, that the atoms of some elements—such as uranium—were discovered to "fission" into multiple smaller atoms when bombarded with neutrons.[99] This is particularly relevant because without knowing about nuclear fission, the idea of a nuclear chain reaction was especially conjectural.[100]

3.2.2. The Hypothetical Chain Reaction Patent

Szilárd's patent on the nuclear chain reaction was theoretical and premised on the existence of some substance giving rise to a nuclear phenomenon with characteristics that nobody had yet observed.[101,102] Without funds to experiment, he guessed at elements that might undergo the required reactions.[103]

Wellerstein emphasizes the reasonableness of other people ignoring Szilárd's claims given their extreme speculativeness and points out that much of Szilárd's understanding of how such phenomena might exist was indeed wrong.[104]

3.2.3. The Early Doubts of Other Scientists

Between Szilárd's patent of the chain reaction in 1934 and the discovery of fission in 1938, it seems other physicists uniformly believed nuclear chain reactions to be extremely implausible. Szilárd claims none of the physicists were interested,[105,106] and several famous physicists spoke out against any such possibility.[107,108,109,110]

Szilárd and his Hungarian friends were not prestigious physicists.[111,112] In 1933, Szilárd, a recent refugee of Nazi Germany, had no job and lived out of hotels.[113,114] Szilárd did have some prestigious friends, such as Einstein, and Lindemann,[115] director of the Clarendon Laboratory at Oxford. Nonetheless, he had trouble obtaining funds to experiment.[116]

Thus, prior to fission, approximately no well-respected scientists appear to have believed that a chain reaction was plausible.

3.2.4. The Likelihood of War in the 1930s

On top of the scientific basis for nuclear weapons being speculative, the prospect of a war where Germany might use such weapons against the UK was probably not obvious in the early 1930s.[117]

It is hard to judge how clear it was. In 1933, Adolf Hitler had become Chancellor of Germany,[118] and in the following months Jewish businesses had been boycotted by government decree and Jews were beaten in the streets while police looked on. There was an ordinance that civil servants of non-Aryan descent must retire. A quarter of the physicists in Germany had become unemployed.

Szilárd fled Nazi Germany himself at around this time and—with no job of his own—worked tirelessly helping exiled academics find new support overseas.[119,120] Thus, by 1934, it was probably clear to many that there was going to be some kind of conflict in the vicinity of Germany. In fact, Szilárd predicted "something would go wrong in Germany" as early as 1930,[121] and by 1934 he predicted war with Germany within a few years.[122]

Szilárd's predictions during these times were probably not representative of common views however. He was a keen forecaster and prided himself on being a step ahead of the game. He made a number of good predictions about the German situation. He left Berlin a day before the trains became full and the borders guarded.[123] He began his plans to rescue fleeing academics two weeks before they were forced to flee, causing physicist Leibowitz to characterize him as "the best prognosticator."[124] He said he would move to America one year before the war started, and was only off by four months.[125] Before leaving Germany, Szilárd disagreed with others about whether "anything really rough" would happen in the civilized country and was proven correct.[126]

He told his friend to flee Austria in 1936, accurately foreseeing that she wouldn't be able to work there in two years.[127]

These anecdotal examples suggest that Szilárd had a better—and more pessimistic—understanding of current events at the time than most, and that in particular he foresaw war more confidently and earlier than others did. The prospect of war appears to have been salient yet speculative in the early 1930s.

3.2.5. Szilárd's Abandoned Hope

Szilárd gradually gave up hope on the chain reaction and eventually wrote to the British Admiralty to withdraw the patent in late 1938.[128,129] Coincidentally, fission was discovered at the same time, so Szilárd's hope was abruptly renewed.[130] He cabled the Admiralty, asking them to disregard his previous communication, and thus the patent persisted.[131] This episode suggests the chain reaction was quite speculative, however:

years after it was conceived of, its main proponent—a man not previously put off by tiny probabilities—found it too implausible to keep in secret.

3.2.6. Interest Stemming from the Discovery of Fission

The discovery of fission in late 1938 was very exciting in its own right.[132,133] It was also the turning point in physicists seriously considering chain reactions, nuclear energy, and ultimately nuclear bombs.[134] Fission appeared to be the kind of reaction that Szilárd had sought to power a chain reaction. His hopes and fears were quickly renewed.[135]

Joliot independently thought of the possibility of a large energy release and thought about a chain reaction, as Szilárd predicted he would.[136] Oppenheimer and Fermi were also soon thinking about nuclear energy, chain reactions, and bombs.[137] Fission prompted three relevant labs to investigate chain reactions, though several other candidate labs did not.[138]

Though some physicists were now considering the matter, it took time to determine to many people's satisfaction that chain reactions were possible and then that bombs were. In 1939, one still risked looking foolish by raising the alert about nuclear weapons.[139,140,141]

3.2.7. The Uncertainty of Neutron Emissions

At first there was the question of whether fission would release enough neutrons to sustain a chain reaction. Fermi thought there was around a ten percent chance that any would be emitted at all.[142] This was quickly resolved in the positive, which encouraged further interest in chain reactions.[143] It also encouraged physicist G. P. Thompson (and perhaps others) to investigate weapons, though Thompson worried that his efforts might look "absurd" to outside observers.[144]

3.2.8. The Uncertainties of Weaponization

Many physicists found the idea of weaponizing fission highly implausible.[145,146,147,148,149,150,151,152,153] Relevant administrators agreed, and such research had little support.[154,155,156]

There were many reasons to doubt the military relevance of chain reactions, even beyond their theoretical nature. For one thing, it seemed potentially important to separate uranium-235 from natural uranium, which consists almost entirely of uranium-238. Such separation appeared to many to be too costly to take seriously.[157] Physicist Frisch reasoned at one point that explosion from chain reactions was implausible because any expansion from the heat would move the materials apart too fast for the reaction to continue.[158] It also seemed at first that a uranium bomb would have to be infeasibly large.[159]

3.2.9. Obscurity

Though a number of physicists were thinking about chain reactions and bombs from 1939—and they were sometimes even joined by the popular press[160]—these ideas were apparently not as widespread as one might imagine. Einstein had not heard of the possibility of a chain reaction in July 1939 when Szilárd and Wigner came to visit him, though this was surprising to Szilárd.[161] As late as March 1941, Conant, the Chairman of the chemistry and explosives division of the the US National Defense Research Council (NDRC), had not heard about the prospect of a bomb.[162] Note that the NDRC was the body which absorbed the Advisory Committee on Uranium. At a conference in June 1942, the secretary of the Kaiser Wilhelm Society heard of the idea of a bomb for the first time and estimates from the rest of the audience's reactions that this idea was also new to them.[163] Others in the Kaiser Wilhelm Society had been pursuing energy from nuclear reactions for over a year at that point.[164]

On the other hand, apparently by the early 1940s, popular media had many stories of nuclear power and bombs.[165] Churchill had also heard of the idea by August 1939 and arranged a briefing to the cabinet saying that devastating new explosives would not arrive in the next few years, fearing that Hitler would raise the idea as a threat.[166]

3.2.10. Scientific Authorities Become Convinced

By early 1940, Frisch and Peierls, two physicists who were living in Britain, calculated that separation of uranium isotopes might be feasible.[167] They wrote a memorandum (now known as the Frisch-Peierls Memorandum) describing a radioactive "super-bomb" and sent it to chemist Henry Tizard, the chairman of the Scientific Survey of Air Defense—a committee concerned with the application of science to war.[168]

Frisch and Peierls envisioned a bomb that would destroy practically everything in its vicinity and spread deadly radioactive substances on the wind. They suggested Germany might be working on such a weapon and claimed that if so there were no shelters that could protect from it. Only a similar weapon could effectively respond.[169]

Tizard formed a group that later became known as the MAUD Committee to determine what should be done.[170,171] They assessed the Frisch-Peierls memorandum and determined, in spite of some skepticism, that the issue justified small-scale experiments.[172] By their second meeting, more calculations had been done, and the participants were excited at the prospects.[173]

They conducted further investigations, estimating the cost of separating uranium and the size of a critical mass.[174,175] By mid-1941, MAUD member Chadwick reports becoming convinced that nuclear weapons were not only possible but inevitable.[176]

The British MAUD committee finally prompted action in America, with the help of some US physicists. Wellerstein describes these events, and Rhodes concurs:[177,178,179]

> The real turning point in the US program had nothing to do with the Advisory Committee at all. In 1941, scientists in the UK (in a similar committee) had concluded that atomic bombs might not be very hard for the US to build at all (but too hard for the UK), and had sent a report to the US explaining this. The chair of the US Advisory Committee did not share the report. An emissary from the UK eventually came over to see what had happened to the report, and instead spent his time convincing several important US scientist-administrators, most notably Vannevar Bush, of the report's conclusions. Bush then staged something of a coup to wrest control of fission research from the original Advisory Committee and to accelerate the research program. By late 1942 it was decided that they should move the program into a production phase and the Army Corps of Engineers was brought in . . . this is when it became the Manhattan Engineer District, and thus the Manhattan Project, and became a bomb-making program. (Wellerstein and Grace 2015, 14)

Thus it wasn't until 1941 that the idea of weaponizing nuclear chain reactions became widely enough accepted to attract the attention of those coordinating military science.

3.2.11. Conclusions

In summary, chain reactions, bombs, and bombs relevant to the current war all started out overwhelmingly speculative and with support from a minuscule fraction of relevant scientists. On the other hand, their support was from scientists—just a very small number of them. Over time—especially after the discovery of fission in late 1938—these ideas became better accepted, becoming a serious military research project in around 1941.

At the time of the secret patent (1934), the risks were not straightforward. It wasn't clear what kinds of nuclear reactions might produce a chain reaction. Nuclear reactions in general were only beginning to be understood. At the time of the secrecy efforts and letter (1939), scientists were coming to accept the chain reaction, and soon after some were beginning to seriously consider bombs, though bombs were probably still largely unheard of and generally seen as implausible. Even once fission was identified, there were many practical obstacles to using fission to produce a sizable explosion in a portable form.

3.3. Novelty

The more novel a threat is, the less experience anyone has with it, and so the harder it is to correctly guess what to do about it ahead of time. Thus we might expect more novel problems to be less successfully dealt with by early action. The technological threats emerging around artificial intelligence are fairly novel. We are interested in the extent to which the nuclear threat was also novel.

"Novelty" is not usually associated with any objective metric. Straightforward "unprecedentedness" won't do, because it is possible to define reference classes that make almost any event historically unprecedented or precedented. For instance, "this morning was the very first time that the sun rose on the 7th of December 2014" or "the emergence of life was just a continuation of the same physical processes that have forever proceeded."

I suggest an event is more novel (in a sense relevant to our purposes) if it is easier to think of axes on which it is an extremal example, if it is more extremal on those axes, and if the axes are more natural or simple or relevant to the case at hand.

On these standards Szilárd's situation was quite novel. Some axes that are particularly natural or relevant, or on which the situation was relatively extremal:

1. **Growth in Weapon Power**

   Nuclear weapons represented a sharp discontinuity in the energy that could be released per pound of explosive. Over the thousand years between the 800s and 1890s, developments in explosives increased the energy released per mass of explosive from 0.5 times that of TNT to to 1.66 times that of TNT. The Fat Man nuclear weapon was 4,500 times the effectiveness of TNT.[180] Abrupt progress at this scale appears to be rare in technologies in general.[181]

   Also note that Szilárd expected nuclear weapons would represent such a dramatic surge in explosive power from early on, so this was not an unexpected after-the-fact feature of the situation.[182]

2. **Weapon Power**

   The first nuclear weapons were the most powerful weapons ever. This isn't impressive in and of itself: sustained progress in weapons development means that one is always dealing with the most powerful weapons ever. Still, this counts as an example of novelty in the relevant respect: if technological progress makes some amount of novelty commonplace, this novelty can still impede our ability to effectively address risks.

3. **Scale of the War**

   World War II was the largest and most deadly war ever.[183]

4. **Coincidence of War and Weapon Progress**

   It was especially surprising for the largest leap in explosive technology ever to coincide exactly with the start of the largest war ever.

5. **New Source of Energy**
   Atomic energy was a new source of energy for human use. There are only around ten such substantial sources of energy in use,[184] and it appears that all of the others have been exploited by humans to some degree for thousands of years.[185] This shift to a new source of energy appears to account for the large change in explosive efficiency—chemical explosives appear to have fundamental limits to energy density, which nuclear explosives are not subject to.[186]

Here are some features of the response that are both relatively natural and extremal:

1. **Arms Race Needed**
   The suggested response to the threat was in large part to develop nuclear weapons for the home military before anyone else did. Scaling up military power is a common occurrence, and the need to do so to ward off the threat of others' growing weaponry is presumably also common. However, it seems rare for even civilian scientists to be asking for a race to weapons development, so this is probably not a situation they would have been prepared for.

2. **Scientific Secrecy Needed**
   Another key part of the response was secrecy. It is hard to find other cases where scientists made discoveries that they considered so potentially harmful that there was a voluntary movement to keep them secret. However, if such a movement was successful, the issue may not be well known. Military research is often secret but is not usually started in a civilian setting and rushed into secrecy. Research into potentially weaponizable diseases is sometimes thought to warrant secrecy.[187] It has been suggested that the coincidence of dramatic improvements in weaponry and the beginning of a huge war produced this unusual state of affairs.[188]

3. **Presidential Alert Needed**
   People write letters to the president at least ten thousand times per day.[189] Presumably not all are drawn to his attention with such care, but presumably many things are drawn to the president's notice. So this does not seem a hugely novel response.

The sharp growth in weapon power and the appropriateness of voluntary scientific secrecy in particular seem to make this a highly novel case. These are both fairly natural qualities of the situation to consider, and the situation was extremal on both. Probably the nuclear case is sufficiently novel to be outside the bounds of what anyone had much experience dealing with. Arguably it is still not as novel as the AI risk case, which might end human labor, or lead to human extinction. We will leave this for the reader to judge.

3.4. Opportunities for Feedback

If your efforts to solve a problem are interspersed with information about whether you are succeeding, we say you are receiving feedback. More feedback tends to make problems easier to solve. We are interested in whether Szilárd's projects allowed for feedback.

You are more likely to receive feedback if a problem occurs gradually and the efforts to solve it coincide temporally, or if the problem can be reversed after it happens. If the problem is sudden and permanent, there is generally less feedback.

However, there might also be substantial feedback if there are good indicators of success even without the problem having occurred. For instance, consider a town that fears a dam breaking near them. If it breaks, this will happen suddenly and the disaster will be hard to reverse. However, before it happens, engineers may have a precise understanding of what precautions will avert the risk, and constructors may know exactly how far they have come with building the desired improvements. In this case, there is a lot of feedback about how they are influencing the problem, though the problem (hopefully) never arrives.

The availability of this kind of feedback is related to the novelty of the problem. Where we have seen similar problems before, we are likely to have a finer understanding of what intermediate goals are useful and can confidently use success at those goals as a proxy for success.

The advent of nuclear weapons was more or less a one-off event, where the first to have them might have been expected to have a substantial military advantage. On the verge of war, it was plausible that if the Nazis developed nuclear weapons first, this would make a huge difference to the outcome of the war.[190,191] Germany was expected by some to bomb the US nuclear facilities early in their hypothetical nuclear attack, further destroying America's ability to catch up.[192] Thus this situation was not conducive to feedback in the form of failing and then doing better next time.

Building nuclear weapons was presumably more conducive to feedback from progress on intermediate steps known to be useful. For instance, you can tell whether or not you have developed a plausible method of separating uranium isotopes, and then you can tell if you have tested it.

However, compared to normal construction projects, it was less clear which directions were useful because many things were being done for the first time.[193] This uncertainty about useful directions is well illustrated by the costly decision to develop several different processes for extracting U-235 in parallel.[194,195] Processing plants were built quickly before the technology underlying them had been thoroughly developed.[196]

Even this level of uncertainty was after the project was well under way. When Szilárd patented the chain reaction, and later when he pushed for secrecy and action, progress toward the goal was presumably even harder to track. There were not closely analogous situations from which to derive the useful steps (see Section 3.3); social systems are hard to predict; and the physics was speculative (see Section 3.2). It was initially unclear even which nuclear processes were relevant: recall that after years of effort Szilárd was about to give up when fission was discovered, and he realized it was what he had been looking for (see Section 3.2.5).

Feedback was also limited because of secrecy between the warring sides—if the aim is to beat Germany, a critical question is whether you are on track to develop nuclear weapons *before* them, not merely whether you are on track to develop nuclear weapons. In fact, Americans erroneously thought Germany was probably ahead of them as late as 1943.[197]

In summary, projects to avert German nuclear dominance could have little feedback of the trial and error form at a high level. There was also little feedback about progress relative to Germany. There was more feedback about progress on apparently useful intermediate steps, though this feedback was probably unusually tenuous because the problem was relatively novel. This landscape of feedback seems fairly comparable to that around AI risk.

3.5. Breadth of Applicability

If a project to mitigate some distant threat simultaneously supports a number of more immediate goals, we might expect it to inspire more interest and so to better succeed. For instance, if you were interested in preventing climate change and deciding whether to invest in basic climate science or build a specific geoengineering technology, basic climate science has the advantage that it will be useful well beyond its applications to climate change.

To investigate whether efforts to address nuclear risks were broadly applicable, we will ask whether the people involved in Szilárd's various initiatives conceived of their actions as narrow bets on the reality of German nuclear weapons and worthiness of specific responses, or as robustly useful steps toward a variety of good outcomes.

3.5.1. Selfish Motives

People often have personal motives for their role in events. For instance, a cause advocate might choose one cause over another to work with people they respect. These are in a sense side effects of the action, which might create a drive for otherwise distant and abstract missions. However, we will for the most part ignore them here, for three reasons.

Firstly, the nuances of personal motivation are likely too complicated and many for us to usefully investigate from this distance.

Secondly, personal motives do not seem intrinsic to projects. It might be hard to make planning for a nanotechnology disaster more broadly socially valuable in the short term, but such a project could easily become attractive to specific individuals if it had the right social connections or location or crossover with another interest of theirs. This means the lack of such personal attractions is unlikely to be an overwhelming problem for any specific project.

Thirdly, personal reasons for projects should not correlate tightly between people. Where they do, we will class it as a larger scale motive. Fairly uncorrelated personal motives seem unlikely to account for large differences in the success of projects. For instance, if Szilárd had some particular ulterior motive to push for secrecy, this shouldn't make much difference to the broader success of the effort unless many others share his feelings.

3.5.2. Patent

To what extent was taking out a patent broadly useful? According to Wellerstein, the primary purpose of making the patent secret was probably to interest the military rather than to keep the idea secret from the Germans.[198] This is a closely related goal: both aim to prevent Germany having nuclear weapons before others. The difference is just in whether the goal is accomplished via slowing German progress or speeding others' progress.

This makes some difference to the robustness of the secret patent's value, however. Speeding home progress on nuclear research is more robustly useful than slowing German progress. If Germany were never to have weapons, America would still benefit from having them. Even without a war or nuclear weapons, nuclear research is valuable. Lack of German nuclear research, on the other hand, mostly has value to someone in conflict with Germany, which is a rarer state of affairs.

To the extent that the patent was intended to forward secrecy, the robustness of its value should be similar to that of other secrecy efforts (see Section 3.5.3). To the extent the patent was intended to forward home nuclear research, its value should be as broad as that of the letter (see Section 3.5.4).

3.5.3. Secrecy

Successful secrecy efforts would presumably have slowed overall nuclear research because their intention was to slow German nuclear research, which would otherwise contribute to global research. As discussed above, projects to slow other parties' research tend to be less robustly useful than those intending to speed home research. This is because their value is limited to winning in conflicts between the relevant parties, while speeding home research is helpful for that and also any goal that benefits from greater scientific progress.

The general unpopularity of secrecy among the scientific community (see Section 2.3) also suggests they didn't see broader benefits and were concerned about the risk of doing something generally harmful in pursuit of a specific speculative goal.

In sum, secrecy promoted nuclear advantage over Germany at the expense of forwarding nuclear research and thus was relatively narrowly directed.

3.5.4. Letter

The Einstein-Szilárd letter explicitly warns of nuclear weapons and suggests taking action on them before Germany does. This sounds relatively narrowly directed at competing with Germany, but is intended to advance America's nuclear weapons research, which at the time would probably have also forwarded nuclear research in general, with many civilian uses as well as military uses beyond competition with Germany.

For Szilárd and his collaborators, American military domination *per se* was probably not a high priority beyond its instrumental value in providing safety from Nazi Germany. Szilárd and his collaborators also had other explicit motivations for pursuing nuclear research. They wanted long-term peace and apparently thought devastating weapons might bring it.[199] The extent and seriousness of this belief is unclear.

If indeed world peace was a substantial motive, then it is another source of value from nuclear weapons research but not a more immediate or direct one: world peace was more speculative and temporally distant than nuclear war. Thus, even if the combination of motives is unclear, we can conclude that for Szilárd and his collaborators the value of producing nuclear weapons was heavily concentrated in speculative and distant propositions unless they had further goals not uncovered in this investigation. For instance, a general desire to see progress in nuclear physics would be plausible.

In sum, the Einstein-Szilárd letter was intended to forward nuclear weapons research. Nuclear weapons research perhaps seemed most useful for beating Germany to nuclear weapons, but probably also seemed useful in forwarding other military and civilian technology. Nuclear weapons might also have been expected to change balances of power in global politics either in favor of America or in favor of peace generally. Both of these goals were likely of interest to some involved parties.

3.5.5. Summary of Breadth of Applicability

The secrecy efforts were fairly narrowly targeted at slowing German weapons research. The letter aimed to promote American weapons research, which was probably more broadly valuable. The patent nominally aimed to promote secrecy but was probably also intended to forward local (then English) weapons research. To the extent it was intended to promote weapons research, it probably also had broad value, in much the same way as the letter.

3.6. Conclusions on features of interest

The threat from nuclear weapons has much in common with the threat from artificial intelligence. They are both highly speculative, not strongly feared by a large portion of the relevant scientific community, novel, and not especially conducive to direct feedback. Suggested solutions to both involve a mixture of work that has broader applicability, and work that is narrowly directed. Some efforts were probably made on nuclear risk at least fifteen years ahead of when the risk was expected, though this is unclear. Concern about AI risk probably precedes the perceived risk by a longer period. On these factors, the two cases differ in degree, but they seem similar enough to make the nuclear case a good source of evidence on the AI case.

4. Success

In a straightforward sense, Szilárd had success: he didn't want Germany to attack other countries with nuclear weapons, and they did not. However, success is complicated. You might get what you want but fail to have brought it about causally, for example. Or you might take the perfect action but get unlucky. You can have all kinds of partial success: you can make a reasonable prediction; you can make a correct prediction; you can choose good responses to your predictions; you can achieve some instrumental goals; you can make a positive difference to the final outcome; and, if you are lucky, you might get the outcome you want. We are interested in disentangling different kinds and parts of success because for instance if people usually make good predictions but take inappropriate actions, then we face a different problem than if they usually fail at the prediction stage. We will discuss several kinds of partial success in turn.

4.1. Success of Predictions

Some key non-trivial predictions prompting Szilárd's actions might be broken down as follows:

1. A nuclear chain reaction was possible.
2. A nuclear chain reaction could produce a bomb thousands of times more powerful than existing bombs.
3. Physicists in Germany would also conceive of the chain reaction and a bomb.
4. Conflict with Germany would worsen.
5. Such bombs might be developed within the time frame of the coming conflicts.
6. Germany might build such bombs and conduct a nuclear attack.

All of the events in these predictions came to pass except for the final one: German nuclear attack. Given that German nuclear attack was the event that Szilárd (and later many others) intended to avert, it is not obvious anyone should be faulted for predicting it—it is hard to immediately distinguish between the prediction being bad and the action to avert it being successful.

Incidentally, Wellerstein suggests that at least the first of the above predictions was made by luck. Prior to the discovery of fission (i.e., while Szilárd's views were most controversial), he says there was little reason to have hope in Szilárd's chain reaction idea.[200]

We are especially interested in whether early predictions tend to be poor and so give rise to an unwarranted struggle against an imaginary problem. To inform that question, we are interested in whether Szilárd and his collaborators had poor predictions as a result of being early. We have moderate evidence suggesting these predictions were not much worse for being early, including the one that did not come to pass. This is that exactly the same predictions were more widely accepted as late as 1943. Since the early predictions were the same as commonly held later predictions, it seems predicting early did not deprive Szilárd of information that would likely have changed his beliefs in the following years.

Both Americans and Germans believed Germany was ahead on nuclear weapons research in early 1943.[201] The threat of Chicago in particular being bombed or more subtly poisoned with radioactivity appeared so real that some people moved their families out.[202,203] It remains possible that early bad predictions caused people later to have bad predictions during the war; however, it is at least unlikely that there was much contradictory evidence to be revealed once the problem was imminent. Thus, overall, Szilárd's predictions were mostly successful and at a minimum probably not worse for being early.

4.2. Success of Actions

We are interested in the extent to which Szilárd's actions made a positive difference. We will consider each of the actions in turn, but it will be useful to first consider the background question of how close Germany ever was to having a bomb. If Germany was far from having a bomb, then the *ex post* value of any efforts to beat them is probably low on the margin: they probably would have lost anyway. This would not rule out actions being successful in other senses—for instance, being worthwhile given reasonable expectations ahead of time.

4.2.1. How Close Was the Threat of German Nuclear Attack?

In April 1939, a secret conference in Germany gave rise to the German nuclear research program.[204] Germany banned uranium exports and investigated procuring radium supplies from Czechoslovakia. At around the same time, Hamburg physicist Paul Harteck and his assistant Wilhelm Groth wrote to the German War Office to alert them to the recent developments in physics and the possibility of bombs "many orders of magnitude more powerful than the conventional ones."[205] The War Office decided to pursue the research.[206] By 1940 the research program was apparently thriving.[207]

However, the research program remained in the laboratory: Germany never came close to developing the industrial capacity to actually build nuclear weapons.[208] Rhodes attributes the German nuclear program's failure to advance beyond laboratory scale research to several factors. Hitler was skeptical about the program's value, and there was a principle in his military that no new technology should be developed if it would take more than 18 months to complete.[209] German scientists also intended for a longer term, more careful project.[210] The German program was also probably slowed by lack of trust between the governments and scientists.[211]

This list of issues makes no mention of any activities Szilárd was involved in; however, they also don't exclude the possible importance of Szilárd's projects. For instance, Hitler may have been less skeptical of the program's value if secrecy had not obscured promising foreign research findings. In fact, if the secrecy efforts were highly effective, this is plausibly what you would see: early research promise, followed by nothing.

On the other hand, if we only expect moderate changes as a result of any of Szilárd's efforts, then Germany was fairly far from beating the US, so no moderate change is likely to have made the difference alone. These issues are too complicated to sort out here, but hopefully we have a picture of the general situation.

4.2.2. Remarks on Szilárd's Role

Before we investigate the success of the patent, the secrecy efforts, and the letter in more detail, I should remind you that this analysis is patched together from various historical writings and records, and these are somewhat open to interpretation. In particular, the extent to which Szilárd should take credit for any events appears to be controversial. Wellerstein warns about the temptation to overemphasize Szilárd's influence in pop-history accounts because he is such a lively character.[212]

On the other hand, Eugene Wigner claims that people usually underestimate Szilárd's role and praises Rhodes (a source for many facts in this document) for appreciating it, though he thinks Rhodes may even overstate it.[213] Whichever of them is right about the overall bias of authors, they both suggest that to the extent this document leans on Rhodes, it is likely to be overoptimistic about Szilárd's importance.

4.2.3. The Patent

Rhodes says little came of the patent.[214] Wellerstein agrees that the patent didn't end up being useful except in keeping Szilárd interested in the topic.[215] Szilárd's early interest prepared him to think more about the problem when it became popular. For instance, it helped him to immediately see the explosive implications of fission once it was discovered.[216,217,218,219] However, this realization would probably not have taken long anyway: it reportedly took Frederic Joliot around a week.[220]

If the patent was not useful, in what ways did it fail? Szilárd successfully handed the patent to the military—on his second attempt—and it was kept secret until 1949.[221] To the extent the goal was to keep the information in the patent secret, it probably couldn't succeed prior to the discovery of fission because nobody cared about the idea anyway. After the discovery of fission it appears to have failed because the relevant Germans quickly learned of the chain reaction, either by other means or by independently thinking of it. At least, by April 1939 German physicists were aware of the possibility of bombs many orders of magnitude more powerful than traditional weapons, so presumably they were also aware of the chain reaction.[222]

As noted earlier, Wellerstein believes secrecy was not the primary aim of the patent. It was intended instead to attract military attention to the idea. This apparently failed because the military was not interested. Why did they fail to become interested? That other physicists found it incredible until 1939 suggests it objectively didn't look promising. Wellerstein also claims that there was little reason for it to look interesting at the time.[223]

Had Szilárd secured funds early on as a result of the patent, it is also unclear whether this would have helped substantially because his research agenda would not have come across fission for some time.[224] Thus had he succeeded at the first step of eliciting funds, it is not obvious that he would have achieved what he wanted.

4.2.4. The Secrecy Efforts

Szilárd had pushed fruitlessly for secrecy before 1938[225] (e.g., his secret patent), but with the discovery of fission and the associated interest of other scientists, Szilárd began a concerted campaign to keep nuclear science secret (for more details, see Section 2.3).[226] This campaign was surprisingly successful: Szilárd and his allies convinced almost every relevant group of scientists to withhold their sensitive papers.[227] However, the French scientist Frédéric Joliot-Curie declined to be secretive and published a paper announcing that fission produced numerous secondary neutrons and thus that a chain reaction seemed possible.[228] This paper prompted scientists in numerous countries to alert their governments to the value of nuclear weapons programs. In this way, Szilárd's efforts failed substantially.

Though Szilárd's secrecy efforts did not muffle Joliot's paper, other important papers were withheld successfully. Szilárd himself withheld a paper on the plausibility of making nuclear reactors that Weart thinks would have stimulated nuclear reactor work in other countries.[229] When Fermi eventually agreed to secrecy, it was in time to protect a paper measuring neutron absorption of carbon. In combination with an error in the German estimate, this appears to have prevented further German experiments on graphite, which was in fact a cheap and effective moderator.[230,231] Weart says this may conceivably have changed the course of World War Two. Szilárd also convinced Louis Turner to withhold a paper introducing the idea of fissioning plutonium, which was in fact the easiest route to building an atomic bomb.[232]

Beyond the direct withholding of papers, the secrecy efforts probably encouraged other secrecy efforts. In 1940, Gregory Breit of the National Academy of Sciences organized a scheme under which potentially sensitive papers submitted to journals would be forwarded to a committee, then potentially circulated to a limited audience, then published when the danger had passed with their original dates. He got the support of editors of scientific journals and other leading scientists and quickly imposed censorship on all fission research in the US.[233] Breit had long known Szilárd and Wigner and learned about the problem through them, so it seems likely that their efforts had some effect here.[234] Wellerstein also says that Szilárd's efforts were a foundational block for further secrecy efforts and encouraged others to think about the problem.[235,236,237,238]

It is worth noting that the secrecy efforts that failed to hide Joliot's results came close to being hugely influential. Everyone except Joliot's group cooperated, and it was plausible that Joliot would join them. The paper that Joliot published convinced many physicists that nuclear chain reactions were possible and helped trigger nuclear research programs in Britain, Germany, and probably elsewhere.[239,240]

As it turned out, had Szilárd's collaborators managed to censor Joliot's paper, it probably would not have made a difference to the final outcome—the Germans did not develop nuclear weapons first, even with Joliot's help. For the apparently lesser successes of Szilárd's secrecy efforts, it is harder to say how influential they were. They probably slowed the German nuclear project in expectation, so they probably contributed to America's overall success in beating Germany to the bomb. However, as discussed earlier, Germany was so far behind America that small changes in their rate would not make much difference to the final outcome. And it is unclear from the present research how large the changes were in Germany's rate of progress.

In sum, Szilárd's secrecy campaign is apparently often regarded as a failure, largely because it failed to censor a particularly valuable paper. However, Szilárd was central in censoring several other important papers, and his secrecy project appears to have led into important further secrecy efforts and institutions. These successes probably slowed German research, though it is unclear by how much, or how much this mattered.

4.2.5. The Letter

The Einstein-Szilárd letter straightforwardly resulted in President Roosevelt's creation of the Advisory Committee on Uranium (hereafter, ACU). However the ACU was famously inactive, and it wasn't until interference from British scientists that America seriously pursued nuclear weapons (see Section 2.4). Thus it is not clear whether in fact prompting the creation of the ACU was causally useful in creating nuclear weapons faster. Wellerstein thinks had the Einstein-Szilárd letter never been sent, little would have changed. He explains his views:

> **Is it true that the Einstein-Szilárd letter was very influential?**
>
> The Einstein-Szilárd letter's importance is in my view somewhat debatable. It was quite limited in its actual scope. FDR was already being steered towards coordinating work on fission—his advisor, Alexander Sachs, had been aware of fission's possibilities. Sachs used the letter as the final excuse to get FDR's attention on the matter. What FDR did was authorize a very modest coordination of fission work under the auspices of the National Bureau of Standards. It was absorbed into Vannevar Bush's National Defense Research Committee (NDRC) in 1940. I suspect that had the Advisory Committee on Uranium not been created in 1939, a similar committee would have sprung up organically within another part of the government (e.g. the Naval Research Laboratory, which was already looking into these matters, and certainly by the time the NDRC was created). The work of the committee was not to work towards building an atomic bomb—it was simply to finance experiments that would help determine whether atomic bombs or atomic reactors were feasible for the current war. By 1941 or so, the US conclusion was that nuclear reactors were probably feasible but bombs were probably too difficult to demand the effort that would be required.
>
> The real turning point in the US program had nothing to do with the Advisory Committee at all. In 1941, scientists in the UK (in a similar committee) had concluded that atomic bombs might not be very hard for the US to build at all (but too hard for the UK), and had sent a report to the US explaining this. The chair of the US Advisory Committee did not share the report. An emissary from the UK eventually came over to see what had happened to the report, and instead spent his time convincing several important US scientist-administrators, most notably Vannevar Bush, of the report's conclu-

> sions. Bush then staged something of a coup to wrest control of fission research from the original Advisory Committee and to accelerate the research program. By late 1942 it was decided that they should move the program into a production phase and the Army Corps of Engineers was brought in . . . this is when it became the Manhattan Engineer District, and thus the Manhattan Project, and became a bomb-making program.
>
> So the point here is that the impetus for the Manhattan Project did not really come from the Advisory Committee and thus really didn't come from the Einstein-Szilárd letter. It came from a completely different source . . . the British. If the Einstein-Szilárd letter had never been sent, and the Advisory Committee had never been created, I doubt it would have affected anything much at all in the timeline of whether and when the atomic bomb was made, simply because it wasn't that influential on the timeline at all. Of course, this is speculative . . . there may have been other useful aspects of the Advisory Committee (e.g. it did involve getting several scientists involved who might have gotten tied up with other war work had they not been told about the uranium question). But I think it is worth emphasizing that it was not really the start of the Manhattan Project at all, and it was not a bomb-producing program in 1939. The bomb-producing program did not really start until late 1942. (Wellerstein and Grace 2015)

In summary, there is a reasonable case to be made that the letter did not substantially improve the timing or other important features of the eventual US nuclear program. However, this is speculative, and given that the letter did nominally begin such research, it still seems plausible that it had some effect.

4.2.6. Would Later Action Have Been Better?

Because we are especially interested in the value of early efforts to avert risks, let us ask whether these efforts would have been better if they had been later.

The patent might have had more success attracting attention later, however had it attracted early attention the attention would have been more valuable because it would have brought funding to explore the problem earlier, and such research would plausibly have been fruitful.

The secrecy efforts appear to have been too late, if anything. Though they were dealing with a potentially far-off problem, the papers fueling that problem were being written at that moment. They were just in time to block some and too late for others. This suggests earlier would have been better, but perhaps earlier secrecy efforts may have been less popular given that attitudes changed with war (discussed briefly in Section 2.3).

It seems fairly ambiguous what would have happened differently if the letter was sent substantially earlier or later.

4.3. Conclusions on Success

Szilárd's predictions were fairly successful. The secret patent was probably not very successful in any sense. The secrecy efforts may have made a substantial difference to Germany's failure to develop nuclear weapons, though this is unlikely and hard to judge. Szilárd also came very close to being hugely successful at withholding information, which for all that was known at the time could have been very useful, though in practice probably would not have been. The letter was of ambiguous value: nominally it started nuclear research which eventually led into the Manhattan project, but, on closer inspection, it may have been worse than useless.

In sum, it looks plausible that Szilárd had little effect, largely because the Germans did not come close to obtaining nuclear weapons before the US. However Szilárd's actions appear to have been close enough to being highly valuable that they were likely very good in expectation, given the information available at the time. There also appears to be a small chance that they were highly valuable in fact, if the secrecy efforts made a meaningful difference to slowing the German nuclear program.

Given the similarities to the AI case (see Section 3.6), what should we infer about efforts to avert AI risk? Perhaps that it is plausible for small groups of people to make a meaningful difference to far-off and difficult problems, but also that it is easy enough for them not to, even after impressive intermediate successes.

A. Appendix: A Few Relevant Notes on the Incidents

A.1. Interest in Saving Humanity

An interesting question for those who seek to do especially large amounts of social good is whether people who set out to do great good tend to succeed better than those who pursue a more regular career path and take opportunities when they stumble upon them. Szilárd is an example in favor of those who try doing better. He had been explicitly interested in "saving the world" since childhood:

> Szilárd thought in maturity that his "addiction to the truth" and his "predilection for 'Saving the World' " were traceable first of all to the stories his mother told him. (Rhodes 1971, 107)

And, though it's unclear how much good he did in the end, he at least came much closer than most.

A.2. Belief That Science Should Be Free

Scientists were sometimes opposed to Szilárd's secrecy schemes because they felt scientific secrecy was wrong on principle.[241,242,243,244,245,246,247] This parallels some of the opposition to constraints on recombinant DNA research in the 1970s on grounds that, in spite of dangers, science should be free on principle. This weakly suggests such views will commonly arise near issues of scientific safety.

Notes

1 "In London, where Southampton Row passes Russell Square, across from the British Museum in Bloomsbury, Leó Szilárd waited irritably one gray Depression morning for the stoplight to change. A trace of rain had fallen during the night; Tuesday, September 12, 1933, dawned cool, humid and dull. Drizzling rain would begin again in early afternoon. When Szilárd told the story later he never mentioned his destination that morning. He may have had none; he often walked to think. In any case another destination intervened. The stoplight changed to green. Szilárd stepped off the curb. As he crossed the street time cracked open before him and he saw a way to the future, death into the world and all our woe, the shape of things to come. . .

" 'As the light changed to green and I crossed the street,' Szilárd recalls, 'it . . . suddenly occurred to me that if we could find an element which is split by neutrons and which would emit two neutrons when it absorbs one neutron, such an element, if assembled in sufficiently large mass, could sustain a nuclear chain reaction.' " (Rhodes 1971, 13, 28)

2 "The morning of Rutherford's lecture, September 11 1933 . . ." (Lanouette 1992, 132)

"[Szilárd's] 'chain reaction' idea occurred either the day after Rutherford's speech or within a few weeks, depending on which of Szilárd's versions is true. It seems unlikely that he would be out walking the day he awoke with a bad cold, and other evidence also suggests at least a few days' delay in his contemplations." *Memoirs*, dictated in 1960 (Lanouette 1992, 133, Footnote 4)

3 "That Szilárd saw beyond 'energy for industrial purposes' to the possibility of weapons of war is evident in his next patent amendments, dated June 28 and July 4, 1934. Previously he had described 'the transmutation of chemical elements'; now he added 'the liberation of nuclear energy for power production and other purposes through nuclear transmutation.' He proposed for the first time 'a chain reaction in which particles which carry no positive charge and the mass of which is approximately equal to the proton mass or a multiple thereof [i.e., neutrons] form the links of the chain.' He described the essential features of what came to be known as a 'critical mass'—the volume of a chain-reacting substance necessary to make the chain reaction self-sustaining. He saw that the critical mass could be reduced by surrounding a sphere of chain-reacting substance with 'some cheap heavy material, for instance lead,' that would reflect neutrons back into the sphere, the basic concept for what came to be known (by analogy with the mud tamped into drill holes to confine conventional explosives) as 'tamper.' And he understood what would happen if he assembled a critical mass, spelling out the results simply on the fourth page of his application:

> If the thickness is larger than the critical value . . . I can produce an explosion." (Rhodes 1971, 214)

4 "The patent, Szilárd explained in the letter Lindemann enclosed [to the British Admiralty], 'contains information which could be used in the construction of explosive bodies . . . very many thousand times more powerful than ordinary bombs.' He was concerned about 'the disasters which could be caused by their use on the part of certain Powers which might attack this country.' " (Rhodes 1971, 224–25)

5 "To this illustrious personage, a vegetarian who daily consumed copious quantities of olive oil and Port Salut, Szilárd turned in the early summer of 1935 to discuss 'the question whether or not the liberation of nuclear energy . . . can be achieved in the immediate future.' If 'double neutrons' could

be produced, Szilárd wrote Lindemann on June 3 [1935], 'then it is certainly less bold to expect this achievement in the immediate future than to believe the opposite.' That meant trouble, Szilárd thought, if Germany achieved a chain reaction first, and he argued for 'an attempt, whatever small chance of success it may have . . . to control this development as long as possible.' Secrecy was the way to achieve such control: first, by winning agreement from the scientists involved to restrict publication, and second, by taking out patents." (Rhodes 1971, 223–24)

6 Leó Szilárd had the opposite problem: he didn't want his idea of creating a neutron chain reaction to release nuclear energy to be shared, for fear it would allow Nazi Germany to build atomic bombs. (Rhodes and Grace 2015, 1)

7 See Szilárd 1934.

8 See endnote 3.

9 "Michael Polanyi had cautioned Szilárd late in 1934 that 'there is an opposition to you on account of taking patents.' The British scientific tradition that opposed patents assumed that those who filed them did so for mercenary purposes; Szilárd explained his patents to Lindemann to clear his name:

> Early in March last year it seemed advisable to envisage the possibility that . . . the release of large amounts of energy . . . might be imminent. Realising to what extent this hinges on the "double neutron," I have applied for a patent along these lines. . . . Obviously it would be misplaced to consider patents in this field private property and pursue them with a view to commercial exploitation for private purposes. When the time is ripe some suitable body will have to be created to ensure their proper use." (Rhodes 1971, 224)

10 "So Szilárd turned to a parallel tradition which he had some experience with, which was patent law. He and Albert Einstein had worked together in Germany on theoretical physics, and had actually taken out a number of patents together as well. Thus it made sense to Szilárd to take out a patent on the chain reaction, but then to give it to the British Admiralty, which would classify it secret and hide it away." (Rhodes and Grace 2015, 1)

11 "Rhodes reminds us that these scientists were just thrown out of Nazi Germany with nothing but the shirts on their back; or as Ernest Rutherford famously said, 'Living on the smell of an oil rag.' They were just trying to survive: find a place where they could settle down to make a living doing their work. Szilárd was living off the earnings from his patents but lacked a place to work. They were trying to find a place that would take them in and accept them and give them a job and so forth." (Rhodes and Grace 2015, 9)

12 "Szilárd came out of a generation of physicists where patenting had just started to be an important way for the scientists to assert control over their work, and had become a means of making sure that any commercial benefits the work produced would be re-invested back into science. Szilárd was already a prolific patent filer for this reason, perhaps even more so than the average physicist in the 1930s." (Wellerstein and Grace 2015, 1)

13 "Director of Artillery J. Coombes turned them down on October 8, noting that 'there appears to be no reason to keep the specification secret so far as the War Department is concerned.' " (Rhodes 1971, 224)

14 "Since the beginning of his rescue work in England Szilárd had been in occasional contact with the physicist Frederick Alexander Lindemann, who was professor of experimental philosophy at Oxford and director of the Clarendon Laboratory there. . . . The following February 1936, [Lindemann] intervened on Szilárd's behalf with the Admiralty . . . writing the head of the Department of Scientific Research and Development cannily:

> I daresay you remember my ringing you up about a man working here who had a patent which he thought ought to be kept secret. I enclose a letter from him on the subject as you suggested. I am naturally somewhat less optimistic about the prospects than the inventor, but he is a very good physicist and even if the chances were a hundred to one against it seems to me it might be worth keeping the thing secret as it is not going to cost the Government anything.

". . . Wisely and withal inexpensively the Admiralty accepted the patent into its safekeeping." (Rhodes 1971, 224–25)

15 See endnote 4.

16 "My understanding regarding the patent is that when he filed it, he explicitly requested it be kept secret. The patent law in question allows the inventor to assign weapons patents to the UK Crown and to have them kept secret (see *Patents and Designs Act* 1907, Section 30)." (Wellerstein and Grace 2015, 1)

17 "Rhodes is only familiar with American practice, but assuming British practice is similar, patents really don't see the light of day until they're finally filed. Even then, it's not as if they turn up on the front page of *The New York Times*. They are awarded and filed without fanfare, just another public document prepared and passed among law firms and government bureaus. And giving the patent to the British Admiralty with the proviso that they would keep it secret added another layer of protection from German awareness." (Rhodes and Grace 2015, 1–2)

18 "That being said, secret patents were unusual at the time. What Szilárd appears to have been doing was drawing the idea to the attention of the British authorities in particular. Filing a patent like this and declaring it as needing to be secret was his way of demonstrating that the idea could be made into a concrete technology and forcing it to go through the kinds of channels that a military patent might be put through—a place where you are guaranteed that security people and technical people are going to be scrutinizing it closely. He was hoping they would take interest in it enough to start funding a project to investigate whether it might actually work—but they weren't . . . Szilárd's approach was always to try and get official (Allied) interest (either the UK or USA), and to secretly pursue this work. So his patenting activities were always secret for this reason. He didn't think a patent would stop the Germans at all . . . just that it would get the 'good guys' interested in the topic." (Wellerstein and Grace 2015, 2)

19 "The next day [shortly after January 1939] Szilárd discussed his plan for voluntary secrecy with Teller, then entrained for Princeton to pursue the same subject with Eugene Wigner, who was still drydocked in the infirmary with jaundice." (Rhodes 1971, 281–82)

20 "In the spring of 1939 one group, foreseeing the unprecedented power of nuclear weapons, made a concerted attempt to restrict knowledge of chain reactions. . . . While Pegram deliberated, Szilárd and his friends were determined to waste no time. Several of them talked the matter over, among

them Victor Weisskopf, an émigré Austrian physicist. . . . Meanwhile another of Szilárd's Hungarian physicist friends, Eugene Wigner, wrote P.A.M. Dirac and asked him to support Blackett." (Weart 1976, 23, 26)

21 "Leó Szilárd was known by now throughout the American physics community as the leading apostle of secrecy in fission matters. To his mailbox, late in May 1940, came a puzzled note. . ." (Rhodes 1971, 346)

22 "At the same time Weisskopf also cabled P.M.S. Blackett, a leading British physicist, asking whether it would be possible for *Nature* and the Royal Society's *Proceedings* to cooperate in delaying publication of fission research. Meanwhile another of Szilárd's Hungarian physicist friends, Eugene Wigner, wrote P.A.M. Dirac and asked him to support Blackett. The matter was rather urgent, Wigner said; although American scientists were willing to cooperate, they realized that their interests might be prejudiced if scientists in other nations published results and they did not. Blackett and another prominent physicist, John Cockcroft, promptly replied that they would support the secrecy plan. *Nature* and the Royal Society were expected to cooperate.

"Szilárd, Teller, Weisskopf and Wigner also talked the problem over with Niels Bohr, who was visiting the United States. Bohr doubted very much that fission could be used to cause a devastating explosion. And he thought that at any rate it would be difficult if not impossible to keep truly important results secret from military experts—the matter was already public. Nevertheless he agreed to go along with the attempt and drafted a letter to his Institute in Denmark (which apparently he did not immediately mail):

> The Columbia group is busy organizing cooperation among all the physics laboratories outside the dictatorship countries, to keep possible results from being used in a catastrophic way in a war situation, and I must therefore ask you, if work along these lines is going on in Copenhagen, to wait before you publish anything until you have cabled me about the results and received an answer.

"But the conspirators still had to win the agreement of other American laboratories.

"The most immediate problem was a group headed by Richard Roberts working under Merle Tuve at the Carnegie Institution in Washington, DC. They too had recently seen some neutrons released from uranium. But the neutrons they saw were emitted over a period of some seconds after fission occurred: These were not the true fission neutrons, but occasional neutrons produced as a side effect of the radioactivity of the fission fragments. The development was announced in a news release of Science Service dated 24 February, written by Robert D. Potter, a science writer who kept in touch with the Columbia physicists and was infected with their excitement over chain reactions. Potter headlined the possibility of an explosive chain reaction propagated by neutrons. He carefully noted that Roberts's delayed neutrons might not be enough to sustain a chain reaction—in fact they are not—but he quoted Fermi as saying that the possibility of a chain reaction was certainly present." (Weart 1976, 26)

23 "The debate was hardly ended, nor Wigner's long day done. He returned to Princeton with Szilárd in tow for an important meeting with Niels Bohr. It had been planned in advance; John Wheeler and Leon Rosenfeld would attend and Teller was making a special trip up from Washington. If Bohr could be convinced to swing his prestige behind secrecy, the campaign to isolate German nuclear physics research might work." (Rhodes 1971, 294)

24 "Szilárd and his friends quickly approached the Washington group, who promised to cooperate in withholding future publications. The proposal was spread further within the United States by word of mouth and letter. Maurice Goldhaber of the University of Illinois was included and Ernest Lawrence of Berkeley was probably informed of the matter when he visited New York on 3 April. John Tate, editor of the *Physical Review*, was brought in, for nearly all important physics papers in the United States passed through Tate's office; anyone else who showed an interest in fission neutrons could thus be put in touch with the conspirators. The attempt to restrict the circulation of information to physicists outside the dictatorships was well begun. It lacked chiefly the acquiescence of the French." (Weart 1976, 28)

25 "At the end of January 1939, still ill with a feverish cold that had laid him low for more than a week but determined to prevent information on the possibility of a chain reaction in uranium from reaching physicists in Nazi Germany, Leó Szilárd raised himself from his bed in the King's Crown Hotel on West 116th Street in Manhattan and went out into the New York winter to take counsel of his friend Isador Isaac Rabi. . . . Szilárd learned from Rabi that Enrico Fermi had discussed the possibility of a chain reaction in his public presentation at the Fifth Washington Conference on Theoretical Physics that had met the week before. Szilárd adjourned to Fermi's office but did not find him there. He went back to Rabi and asked him to talk to Fermi 'and say that these things ought to be kept secret.' Rabi agreed and Szilárd returned to his sickbed.

"He was recovering; a day or two later he again sought Rabi out:

> I said to him: "Did you talk to Fermi?" Rabi said, "Yes, I did." I said, "What did Fermi say?" Rabi said, "Fermi said 'Nuts!' " So I said, "Why did he say 'Nuts!'?" and Rabi said, "Well, I don't know, but he is in and we can ask him." So we went over to Fermi's office, and Rabi said to Fermi, "Look, Fermi, I told you what Szilárd thought and you said 'Nuts!' and Szilárd wants to know why you said 'Nuts!' " So Fermi said, "Well . . . there is the remote possibility that neutrons may be emitted in the fission of uranium and then of course perhaps a chain reaction can be made." Rabi said, "What do you mean by 'remote possibility'?" and Fermi said, "Well, ten per cent." Rabi said, "Ten per cent is not a remote possibility if it means that we may die of it. If I have pneumonia and the doctor tells me that there is a remote possibility that I might die, and it's ten percent, I get excited about it."

"But despite Fermi's facility with American slang and Rabi's with probabilities Fermi and Szilárd were unable to agree. For the time being they left the discussion there." (Rhodes 1971, 279–80)

26 "Fermi therefore arranged to ask the *Physical Review* to delay the publication indefinitely." (Weart 1976, 25)

27 "The telegram asked Halban to advise Joliot that papers on neutron emission had already been sent to the *Physical Review*, but that the authors had agreed to delay publication for the reasons indicated in Szilárd's letter to Joliot of 2 February. The telegram continued:

> NEWS FROM JOLIOT WHETHER HE IS WILLING SIMILARLY TO DELAY PUBLICATION OF RESULTS UNTIL FURTHER NOTICE WOULD BE WELCOME STOP IT IS SUGGESTED THAT PAPERS BE SENT TO PERIODICALS AS USUAL BUT PRINTING BE DELAYED UNTIL IT IS CERTAIN THAT NO HARMFUL CONSEQUENCES TO BE FEARED STOP RESULTS WOULD BE COMMUNICATED IN MANUSCRIPTS TO COOPERATING LABORATORIES IN AMERICA ENGLAND FRANCE AND DENMARK. . .

"The proposed scheme was similar to the one Szilárd had conceived in 1935, with the additional idea that papers should be sent to journals, not for publication but to certify priority of discovery." (Weart 1976, 26)

28 "At the same time Weisskopf also cabled P.M.S. Blackett, a leading British physicist, asking whether it would be possible for *Nature* and the Royal Society's *Proceedings* to cooperate in delaying publication of fission research." (Weart 1976, 26)

29 "Szilárd wrote back at once to say that his own paper was secret, implying that there was an official move underway to withhold papers. He persuaded Turner to write the *Physical Review* and delay publication." (Weart 1976, 29)

30 "Presumably to counter objections he had faced from younger men at Columbia, he wrote:

> For a physicist, who has not yet made a name for himself, refraining from publication means a sacrifice which he should not be asked to make without being offered some compensation. Some addition to the salary which he is normally drawing from the university might therefore be desirable and might require the creation of some special fund." (Weart 1976, 29)

31 "In a letter to F. A. Lindemann, the head of physics at Oxford, he offered a mechanism to ensure secrecy . . . an agreement to make experimental results in the dangerous zone available only to those working in nuclear physics in England, America and perhaps one or two other countries, while otherwise keeping quiet." (Weart 1976, 23)

32 "As Weisskopf said in a recent interview, he had met Joliot's collaborator Halban years earlier and the two had become close personal friends, so Szilárd and Weisskopf drafted a telegram to Halban, which Weisskopf signed. The telegram asked Halban to advise Joliot that papers on neutron emission had already been sent to the *Physical Review*, but that the authors had agreed to delay publication for the reasons indicated in Szilárd's letter to Joliot of 2 February. The telegram continued:

> NEWS FROM JOLIOT WHETHER HE IS WILLING SIMILARLY TO DELAY PUBLICATION OF RESUL TS UNTIL FURTHER NOTICE WOULD BE WELCOME STOP IT IS SUGGESTED THAT PAPERS BE SENT TO PERIODICALS AS USUAL BUT PRINTING BE DELAYED UNTIL IT IS CERTAIN THAT NO HARMFUL CONSEQUENCES TO BE FEARED STOP RESULTS WOULD BE COMMUNICATED IN MANUSCRIPTS TO COOPERATING LABORATORIES IN AMERICA ENGLAND FRANCE AND DENMARK. . ." (Weart 1976, 26)

33 SZILARD LETTER RECEIVED BUT NOT PROMISED CABLE STOP PROPOSITION OF MARCH 31 VERY REASONABLE BUT COMES TOO LA TE STOP LEARNED LAST WEEK THA T SCIENCE SERVICE HAD INFORMED AMERICAN PRESS FEBRUARY 24 ABOUT ROBERTS WORK STOP LETTER FOLLOWS
JOLIOT HALBAN KOWARSKI

". . . Weisskopf having left New York, Szilárd answered on his behalf. . . . That same day the French sent their final answer:

QUESTION STUDIED MY OPINION IS TO PUBLISH NOW REGARDS JOLIOT."
(Weart 1976, 28)

34 "Szilárd also hoped to talk to Fermi: 'I thought that if neutrons are in fact emitted in fission, this fact should be kept secret from the Germans. So I was very eager to contact Joliot and to contact Fermi, the two men who were most likely to think of this possibility.' " (Rhodes 1971, 267)

35 "Joliot published the number of secondary neutrons detected (he found that uranium gave off 3.5 neutrons on average, which is 1 larger than the currently accepted number of 2.5) and commented that chain reactions seemed possible." (Wellerstein and Grace 2015, 4)

36 "On April 7, the day of the final exchange of cables with Szilárd, the French sent *Nature* the results of experiments and calculations that estimated the number of neutrons emitted per fission at between three and four. The report was duly published on 22 April 1939. This note convinced many physicists that uranium chain reactions were a real possibility. In Britain, George P. Thomson decided to warn his government of the dangerous prospects and meanwhile to begin experimenting with uranium. In Germany, Georg Joos wrote a letter to the Reich Ministry of Education; independently and simultaneously, Paul Harteck and Wilhelm Groth wrote a joint letter to the War Office. News of the French work may also have played a role in the startup of Soviet nuclear energy research, perhaps provoking the letters on uranium which I.V. Kurchatov and others sent the Soviet Academy of Sciences about this time. Thus in Britain, Germany and perhaps the Soviet Union, publication of the French results precipitated offically-supported programs of research into nuclear energy. The effort of Szilárd and his friends, after coming within an inch of success, had failed disastrously." (Weart 1976, 28)

37 "In Germany, the French publication precipitated a high-level meeting within a week of publication. The meeting took place secretly on 29 April in the Ministry of Education's headquarters in Berlin, and it resulted in a decision to commandeer all existing stocks of uranium and obtain fresh supplies from the newly captured mines in Czechoslovakia." (Goodchild 2004)

38 "This reply, along with the preceding French publication of the fact that fission does produce some neutrons, doomed the attempt to restrict publication. Pegram, who was not aware how much progress Szilárd and his friends had made aside from the French, after some days of deliberation decided that any attempt to impose secrecy was hopeless. Szilárd was forced to give in. The Columbia scientists asked the *Physical Review* to print their papers." (Weart 1976, 28)

39 "Even though the self-censorship regime failed, it primed the other physicists for thinking about weapons and power possibilities, and created the first serious system of publication review (for security purposes) amongst physicists. This continued to be in effect when the government started to look into fission problems and eventually transitioned into part of the secrecy regime of the Manhattan Project." (Wellerstein and Grace 2015, 4)

40 "Before much progress had been made, the 15 June issue of the *Physical Review* appeared, containing a letter from Edwin McMillan and Philip Abelson at Berkeley. They had observed the production of neptunium when uranium was bombarded with neutrons. This was the first and most essential step of the process that Turner had predicted should lead to plutonium. But Abelson and McMillan had simply failed to see the connection between their work on transuranic elements and the fission problem.

"This publication brought down a flurry of protest, which helped to settle the secrecy issue. From as far as Britain, scientists interested in fission protested the publication of such revealing information." (Weart 1976, 29–30)

41 "With a different perspective on the matter, James Chadwick at Liverpool was so uncharacteristically incensed by the publication of the McMillan-Abelson paper reporting element 93 that he asked for, and got, an official protest through the British Embassy. An attaché was duly dispatched to Berkeley to scold Ernest Lawrence, the 1939 Nobel laureate in physics, for giving away secrets to the Germans in perilous times." (Rhodes 1971, 351)

42 "After war broke out in September, scientists in France, Germany and Britain withheld publication on fission and any other subject remotely of military interest. But in the United States, the Soviet Union and other neutral countries, publication was scarcely impeded." (Weart 1976, 28–29)

43 " 'As recently as six months ago,' Lawrence wrote Breit, 'I should have been opposed to any such procedure, but I feel now that we are in many respects essentially on a war basis.' " (Weart 1976, 30)

44 "In May 1940, eight months after the outbreak of war in Europe, the U.S. physicist Gregory Breit undertook a nearly single-handed effort to halt publication of scientific articles in the newly discovered field of nuclear fission." (Marwell 2003)

45 "But the most important news came from Gregory Breit at the University of Wisconsin. Breit had known Szilárd and Wigner for years and was awakened to the secrecy problem through long conversations with them. Around the beginning of June Breit found a way to circumvent the problems Szilárd and others were running into. Recently named to the National Academy of Sciences, he had been put in the Division of Physical Sciences of the Academy's National Research Council. . . . Breit was made chairman of a subcommittee concerned specifically with uranium. Acting on his own initiative, he immediately began writing letters to journal editors, proposing a voluntary plan under which papers relating to fission would be submitted to his committee before publication. Sensitive papers would be circulated only to a limited number of workers. Breit added that he expected ultimately to publish the papers in book form or otherwise with a statement of the original fade of the paper and with a suitable acknowledgment of the public spirit of the authors.

"There were some raised eyebrows, but the editors of scientific journals and other leading scientists agreed to the plan. . . . Within a few weeks Breit, who swiftly set up close communications with Fermi, Urey, Wigner and others involved in parallel efforts at secrecy, had imposed total censorship on American fission research." (Weart 1976, 30)

46 "On the morning of March 16 [1939] he met with Szilárd, Fermi and George Pegram in Pegram's office. Since at least the end of January Szilárd had been promoting a new version of his Bund—he called it the Association for Scientific Collaboration—to monitor research, collect and disburse funds and maintain secrecy, a civilian organization that might guide the development of atomic energy. He had discussed it with Lewis Strauss on the train to Washington, with Teller after the night of the hard bed, with Wigner in Princeton the weekend Bohr drew his graphs. As far as Wigner was concerned, the time for such amateurism was over. He 'strongly appealed to us,' says Szilárd, 'immediately to inform the United States government of these discoveries.' It was 'such a serious business that we could not assume responsibility for handling it.' " (Rhodes 1971, 292)

47 "[Pegram] knew someone in Washington, he told Wigner: Charles Edison, Undersecretary of the Navy. Wigner insisted Pegram immediately call the man. Pegram was willing to do so, but first the group should discuss logistics. Who would carry the news? Fermi was traveling to Washington that afternoon to lecture in the evening to a group of physicists; he could meet with the Navy the next day. His Nobel Prize should give him exceptional credibility. Pegram called Washington. Edison was unavailable; his office directed Pegram to Admiral Stanford C. Hooper, technical assistant to the Chief of Naval Operations. Hooper agreed to hear Fermi out. Pegram's call was the first direct

contact between the physicists of nuclear fission and the United States government." (Rhodes 1971, 293)

48 "Neutrons diffusing through a tank of water: it was all too vague. Except to alert Ross Gunn, the meeting came to nothing. 'Enrico himself . . . doubted the relevance of his predictions,' says Laura Fermi. The Navy reported itself interested in maintaining contact; representatives would undoubtedly visit the Columbia premises. Fermi smelled the condescension and cooled." (Rhodes 1971, 295)

49 " 'Dr. Wigner is taking the stand that it is our duty to enlist the cooperation of the [Roosevelt] Administration. A few weeks ago he came to New York in order to discuss this point with Dr. Teller and me.' Szilárd had shown Wigner his uranium-graphite calculations. 'He was impressed and he was concerned.' Both Teller and Wigner, Szilárd wrote in a background memorandum in 1941, 'shared the opinion that no time must be lost in following up this line of development and in the discussion that followed, the opinion crystallized that an attempt ought to be made to enlist the support of the Government rather than that of private industry. Dr. Wigner, in particular, urged very strongly that the Government of the United States be advised.' " Leó Szilárd (quoted in Rhodes 1971, 303)

50 "But the discussion slipped away from that project into 'worry about what would happen if the Germans got hold of large quantities of the uranium which the Belgians were mining in the Congo.' Perhaps Szilárd emphasized the futility of the government contacts that he and Fermi had already made. 'So we began to think, through what channels could we approach the Belgian government and warn them against selling any uranium to Germany?'

"It occurred to Szilárd then that his old friend Albert Einstein knew the Queen of Belgium. Einstein had met Queen Elizabeth in 1929 on a trip to Antwerp to visit his uncle; thereafter the physicist and the sovereign maintained a regular correspondence, Einstein addressing her in plainspoken letters simply as 'Queen.' " (Rhodes 1971, 303–04)

51 "As soon as it became clear to Szilárd that physics was his real interest, he introduced himself, with characteristic directness, to Albert Einstein." Einstein was a man who lived apart—preferring originality to repetition, he taught few courses—but Wigner remembers that Szilárd convinced him to give them a seminar on statistical mechanics. . . . Szilárd began explaining [his apparently impossible idea]. 'Five or ten minutes' later, he says, Einstein understood. After only a year of university physics, Szilárd had worked out a rigorous mathematical proof that the random motion of thermal equilibrium could be fitted within the framework of the phenomenological theory in its original, classical form, without reference to a limiting atomic model—'and [Einstein] liked this very much.'

". . . Between 1924 and 1934 he applied to the German patent office individually or jointly with his partner Albert Einstein for twenty-nine patents." (Rhodes 1971, 17, 19)

52 "They called Einstein to arrange a day. . . . Wigner picked up Szilárd on the morning of Sunday, July 16, and drove out Long Island to Peconic." (Rhodes 1971, 304)

53 "Einstein hesitated to write Queen Elizabeth but was willing to contact an acquaintance who was a member of the Belgian cabinet. Wigner spoke up to insist again that the United States government should be alerted, pointing out, Szilárd goes on, 'that we should not approach a foreign government without giving the State Department an opportunity to object.' Wigner suggested that they send the Belgian letter with a cover letter through State. All three men thought that made sense." (Rhodes 1971, 305)

54 "At this time Szilárd also furthered Wigner's proposal to contact the United States government by seeking advice from a knowledgeable émigré economist, Gustav Stolper, a Berliner resettled in New York who had once been a member ofthe Reichstag. Stolper offered to try to identify an influential messenger." (Rhodes 1971, 304)

55 "A message from Gustav Stolper awaited Szilárd at the King's Crown. 'He reported to me,' Szilárd wrote Einstein on July 19, 'that he had discussed our problems with Dr. Alexander Sachs, a vice-president of the Lehman Corporation, biologist and national economist, and that Dr. Sachs wanted to talk to me about this matter.' Eagerly Szilárd arranged an appointment." (Rhodes 1971, 305)

56 "Sachs heard Szilárd out. Then, as Szilárd wrote Einstein, he 'took the position, and completely convinced me, that these were matters which first and foremost concerned the White House and that the best thing to do, also from the practical point of view, was to inform Roosevelt. He said that if we gave him a statement he would make sure it reached Roosevelt in person.' " (Rhodes 1971, 306)

57 "As September extended its violence Szilárd grew impatient. He had heard nothing from Alexander Sachs. Pursuing Sachs' previous suggestions and his own leads, he arranged for Eugene Wigner to give him a letter of introduction to MIT president Karl T. Compton; recontacted a businessman of possible influence whom he had once interested in the Einstein-Szilárd refrigerator pump; read a newspaper account of a Lindbergh speech and reported to Einstein that the aviator 'is in fact not our man.' Finally, the last week in September, he and Wigner visited Sachs and found to their dismay that the economist still held Einstein's letter. 'He says he has spoken repeatedly with Roosevelt's secretary,' Szilárd reported to Einstein on October 3, 'and has the impression that Roosevelt is so overburdened that it would be wiser to see him at a later date. He intends to go to Washington this week.' The two Hungarians were ready to start over: 'There is a distinct possibility that Sachs will be of no use to us. If this is the case, we must put the matter in someone else's hands. Wigner and I have decided to accord Sachs ten days' grace. Then I will write you again to let you know how matters stand.'

"But Alexander Sachs did indeed travel to Washington, not that week but the next, and on Wednesday, October 11, presented himself, probably in the late afternoon, at the White House. Roosevelt's aide, General Edwin M. Watson, 'Pa' to Roosevelt and his intimates, sitting with his own executive secretary and military aide, reviewed Sachs' agenda. When he was convinced that the information was worth the President's time, Watson let Sachs into the Oval Office.

". . . Sachs had made a file for Roosevelt's reading of Einstein's letter and Szilárd's memorandum. But neither document had suited his sense of how to present the information to a busy President. 'I am an economist, not a scientist,' he would tell friends, 'but I had a prior relationship with the President, and Szilárd and Einstein agreed I was the right person to make the relevant elaborate scientific material intelligible to Mr. Roosevelt. No scientist could sell it to him.' Sachs had therefore prepared his own version of the fission story, a composite and paraphrase of the contents of the Einstein and Szilárd presentations. Though he left those statements with Roosevelt, he read neither one of them aloud. He read not Einstein's subsequently famous letter but his own eight-hundred-word summation, the first authoritative report to a head of state of the possibility of using nuclear energy to make a weapon of war. It emphasized power production first, radioactive materials for medical use second and 'bombs of hitherto unenvisaged potency and scope' third." (Rhodes 1971, 312–14)

58 "Roosevelt called in Watson. 'This requires action,' he told his aide. Meeting afterward with Sachs, Watson went by the book. He proposed a committee consisting initially of the director of the Bureau of Standards, an Army representative and a Navy representative." (Rhodes 1971, 314–15)

59 “Their early meetings with the military were nearly disastrous. Teller asked for only $6,000 to continue the water-tank experiments, which the general who listened skeptically to their story only grudgingly granted. When you consider that this program ended up costing more than two billion dollars (in 1945 dollars at that), six thousand dollars was obviously the barest of beginnings.” (Rhodes and Grace 2015, 5)

60 “In the summer of 1940, the uranium committee was absorbed into the newly formed National Defense Research Council (NDRC), headed by James Conant of Harvard and Vannevar Bush of the Carnegie Institution. Briggs was kept on as head of the fission project, but he now reported to Conant.” (Monk 2013)

61 “The National Defense Research Council immediately absorbed the Uranium Committee. . . . When Briggs wrapped up his pre-NDRC committee work in a report to Bush on July I he asked for $140,000, $40,000 of it for research on cross sections and other fundamental physical constants, $100,000 for the Fermi-Szilárd large-scale uranium-graphite experiment (the military had decided to grant $100,000 on its own through the Naval Research Laboratory to isotope-separation studies). Bush allotted Briggs only the $40,000. Once again Fermi and Szilárd were left to bide their time.” (Rhodes 1971, 338)

62 “The American program was in danger for its life that summer, Compton thought: ‘The government’s responsible representatives were . . . very close to dropping fission studies from the war program.’ ” (Rhodes 1971, 368)

63 “Oliphant flew to the United States in late August—he considered the Pan-American Clipper through Lisbon too slow and usually traveled by unheated bomber—to work with his NDRC counterparts on radar. But he was also charged with inquiring why the United States was ignoring the MAUD Committee’s findings. ‘The minutes and reports . . . had been sent to Lyman Briggs . . . and we were puzzled to receive virtually no comment. . . . I called on Briggs in Washington, only to find that this inarticulate and unimpressive man had put the reports in his safe and had not shown them to members of his Committee.’ Oliphant was ‘amazed and distressed.’ ” (Rhodes 1971, 373)

64 “Mark Oliphant helped goad the American program over the top. ‘If Congress knew the true history of the atomic energy project,’ Leó Szilárd said modestly after the war, ‘I have no doubt but that it would create a special medal to be given to meddling foreigners for distinguished services, and Dr. Oliphant would be the first to receive one.’ Conant in his 1943 secret history thought the ‘most important’ reason the program changed direction in the autumn of 1941 was that ‘the all-out advocates of a head-on attack on the uranium problem had become more vocal and determined’ and mentioned Oliphant’s influence first of all.” (Rhodes 1971, 372)

65 “The émigré scientists didn’t make much of a dent in the U.S. But there were also Jewish scientists in England who had similarly escaped from Nazi Germany, and because the British were already at war as of September 1939, they paid attention. It was, in the end, the British scientific establishment, which came to the U.S. in the summer of 1940 with a trunk full of technical secrets which it freely gave to the U. S. government, which convinced its American counterparts to take the possibility of an atomic bomb seriously.” (Rhodes and Grace 2015, 5)

66 “Alexander [Berger] and Holden [Karnofsky] claim broadly that people very rarely attempt to respond to novel future problems far ahead of their emergence. They also expect such attempts are unlikely to be successful. Alexander expressed broad agreement with this list of characteristics that Katja Grace had compiled that might make an action better fit the class they consider unlikely and unpromising:

- The prediction and beginnings of action take place fifteen years or more before the event was predicted to occur
- The prediction is more complicated than a linear extrapolation (e.g. CFCs or climate change don't count because the underlying physical dynamics were well-understood)
- The event has not arisen yet when the response occurs (e.g. CFCs or climate change don't count because ozone destruction and temperature changes were already observable prior to policy action)
- The problem is novel in the sense that the process producing it can't be broken down into steps, all of which presently occur (e.g. saving for retirement doesn't count, because other people are retiring now, so you can see it works)
- Highly credentialed and credible scientists are not concerned
- The policy response was narrowly useful for the predicted problem (e.g. not broad capacity building, or basic research)
- The response is a one-shot effort rather than an iterative process with frequent feedback about success (e.g. like the Maginot line rather than carbon emissions reduction technology)"
  (Berger, Christiano, and Grace 2015, 1–2)

67 See Berger, Christiano, and Grace 2015.

68 America bombed Hiroshima on 6 August 1945. (British Broadcasting Corporation 2004)

69 Brookings lists the 'grand total' in 'then year dollars' as $1,889,604,000. (The Brookings Institution 2001)

70 US nominal GDP in 1945 was around $230 billion.

71 See table 'Wartime GDP of the Great Powers' (Zuljan 2003).

72 "Even though Wigner and Szilárd were trained as engineers, they didn't expect the Manhattan Project to reach the size and scope that it did." (Rhodes and Grace 2015, 7)

73 " 'If each necessary step requires ten months of deliberation,' Leó Szilárd had complained to Alexander Sachs in 1940, 'then obviously it will not be possible to carry out this development efficiently.' The American program was moving faster now than that, but not by much." (Rhodes 1971, 369–70)

74 " 'When I was young,' he told an audience once, 'I had two great interests in life; one was physics and the other politics.' " (Rhodes 1971, 14)

75 "Liebowitz characterized the German situation . . . in Early May:

> . . . Dr. Leó Szilárd . . . proved to be the best prognosticator—he was able to foresee events better than anybody else I know. Weeks before the storm broke he began to formulate plans to provide some means of helping the scientists and scholars of Germany." (Rhodes 1971, 194)

76 "He remembers informing his awed classmates, at the beginning of the Great War, when he was sixteen, how the fortunes of nations should go, based on his precocious weighing of the belligerents' relative political strength:

I said to them at the time that I did of course not know who would win the war, but I did know how the war ought to end. It ought to end by the defeat of the central powers, that is the Austro-Hungarian monarchy and Germany, and also end by the defeat of Russia. I said I couldn't quite see how this could happen, since they were fighting on opposite sides, but I said that this was really what ought to happen. In retrospect I find it difficult to understand how at the age of sixteen and without any direct knowledge of countries other than Hungary, I was able to make this statement." (Rhodes 1971, 14)

77 "His happiness darkened to gloom as soon as he looked ahead: 'It is quite probable that Germany will rearm and I do not believe that this will be stopped by intervention of other powers within the next years. Therefore it is likely to have in a few years two heavily armed antagonistic groups in Europe, and the consequence will be that we shall get war automatically, probably against the wish of either of the parties.' " (Rhodes 1971, 194–95)

78 "[*c.* 1936] He [Szilárd] told me he would be surprised if one could work in Vienna in two years. He said Hitler would be there. And he was" —the *Anschluss*—" almost to the day." (Rhodes 1971, 237)

79 "He was convinced in the mid-1920s that 'the parliamentary form of democracy would not have a very long life in Germany' but he 'thought that it might survive one or two generations.' Within five years he understood otherwise. 'I reached the conclusion something would go wrong in Germany . . . in 1930.' Hjalmar Schacht, the president of the German Reichsbank, meeting in Paris that year with a committee of economists called to decide how much Germany could pay in war reparations, announced that Germany could pay none at all unless its former colonies, stripped from it after the war, were returned. 'This was such a striking statement to make that it caught my attention, and I concluded that if Hjalmar Schacht believed he could get away with this, things must be rather bad. I was so impressed by this that I wrote a letter to my bank and transferred every single penny I had out of Germany into Switzerland.' " (Rhodes 1971, 22)

80 "Many years later Szilárd succinctly summed up the difference between his position and Fermi's. 'From the very beginning the line was drawn,' he said. '. . . Fermi thought that the conservative thing was to play down the possibility that [a chain reaction] may happen, and I thought the conservative thing was to assume that it would happen and take all the necessary precautions.' " (Rhodes 1971, 281)

81 "Experiments in the physics laboratory at Columbia University reveal that conditions may be found under which the chemical element uranium may be able to liberate its large excess of atomic energy, and that this might mean the possibility that uranium might be used as an explosive that would liberate a million times as much energy per pound as any known explosive. My own feeling is that the probabilities are against this, but my colleagues and I think that the bare possibility should not be disregarded." – George B. Pegram; Professor of Physics; Columbia University; Letter of introduction for Enrico Fermi to carry to the Dept. of the U.S. Navy in Washington, DC; March 1940. (MPHPA 2005)

82 "That meant trouble, Szilárd thought, if Germany achieved a chain reaction first, and he argued for 'an attempt, whatever small chance of success it may have . . . to control this development as long as possible.' " (Rhodes 1971, 223)

83 " 'PERFORMED TODAY PROPOSED EXPERIMENT WITH BERYLLIUM BLOCK WITH STRIKING RESULT. VERY LARGE NEUTRON EMISSION FOUND. ESTIMATE CHANCES FOR REACTION NOW ABOVE 50%.'

"Szilárd had known what the neutrons would mean since the day he crossed the street in Bloomsbury: the shape of things to come. 'That night,' he recalled later, 'there was very little doubt in my mind that the world was headed for grief.' " (Rhodes 1971, 292)

84 "If 'double neutrons' could be produced, Szilárd wrote Lindemann on June 3 [1935], 'then it is certainly less bold to expect this achievement in the immediate future than to believe the opposite.' " (Rhodes 1971, 223)

85 "Szilárd prided himself on being ahead of his time, of thinking about the *next* big thing." (Wellerstein and Grace 2015, 3)

86 "Szilárd wrote Lewis Strauss at his Virginia farm on February 13 [1939] 'to see whether you could sanction the expenditures' and presciently briefed the financier on the meaning of the latest developments. The letter's crucial paragraph addresses Bohr's new hypothesis that U235 is responsible for slow-neutron fission in natural uranium:

> If this isotope could be used for maintaining chain reactions, it would have to be separated from the bulk of uranium. This, no doubt, would be done if necessary, but it might take five to ten years before it can be done on a technical scale. Should small scale experiments show that the thorium and the bulk of uranium would not work, but the rare isotope of uranium would, we would have the task immediately to attack the question of concentrating the rare isotope of uranium." (Rhodes 1971, 289)

87 "The DTM physicist also pointed out that other lines of research might be more promising than a slow-neutron chain reaction in natural uranium. He meant isotope separation. At the University of Virginia Jesse Beams, formerly Ernest Lawrence's colleague at Yale, was applying to the task the high-speed centrifuges he was developing there. Roberts thought answers to these questions might require several years of work and that research should be left in the meantime to the universities." (Rhodes 1971, 316)

88 "People who are working on these problems consider the odds are ten to one on success within two years [*c.* 1941]. I would not bet more than two to one against or even money. But I am quite clear that we must go forward. It would be unforgivable if we let the Germans defeat us in war or reverse the verdict after they had been defeated." (Rhodes 1971, 372)

89 "Speer questioned Heisenberg directly. . . . 'His answer was by no means encouraging,' Speer remembers. 'He declared, to be sure, that the scientific solution had already been found. . . . But the technical prerequisites for production would take years to develop, two years at the earliest, even provided that the program was given maximum support.' " (Rhodes 1971, 404)

90 "Between December 1942 and March 1943 the Navy committee organized a ten-session physics colloquium to work through to a decision. By then it was understood that a bomb would necessitate locating, mining and processing hundreds of tons of uranium ore and that U235 separation would require a tenth of the annual Japanese electrical capacity and half the nation's copper output. The colloquium concluded that while an atomic bomb was certainly possible, Japan might need ten years to build one. The scientists believed that neither Germany nor the United States had sufficient spare industrial capacity to produce atomic bombs in time to be of use in the war." (Rhodes 1971, 458)

91 "In late 1942 when Bush made the final push for a bomb production project, he thought it would cost $400 million and that 'bomb production should start about the end of 1944 or the beginning of 1945.' " (Wellerstein and Grace 2015, 7)

92 "The émigré scientists not only suspected that the Germans were working on a bomb but that they might be a year or more ahead. After all, the discovery of nuclear fission had been made in Germany, nine months before Germany began the Second World War by invading Poland. The Hungarian Eugene Wigner, for example, was so worried about a German bomb that in 1942, when he was working at the Met Lab at the University of Chicago designing the first production reactor for breeding plutonium, he estimated that if the Germans had begun work promptly after the Hahn-Strassmann discovery, then they could have a bomb in hand by Christmas. Logically, he thought, assuming the Germans knew about the Met Lab's work, the likeliest target for that first hypothetical German bomb would be the University of Chicago. Wigner took the possibility so seriously that he moved his family out of the city." (Rhodes and Grace 2015, 5)

93 See Berger, Christiano, and Grace 2015.

94 " 'Probably the most important problem before the physicist today,' the senior Pegram told the North Carolina Academy of Sciences in 1911, 'is that of making the enormous energy [within the atom] available for the world's work.' " (Rhodes 1971, 293)

95 "In 1930, when Herbert Becker and Walter Bothe sent alfa particles (helium nuclei) against beryllium, strong penetrating radiation was emitted, there was a hypothesis that this could be electromagnetic radiation of high energy. However, in 1932 James Chadwick could show that in the reaction, a neutral particle with a mass about that of a proton, was emitted. Ernest Rutherford had earlier proposed that such a neutral particle might exist in nuclei. The particle was discovered and was named neutron." (nobelprize.org 2013)

96 "And then he made his stunning announcement, couching it as always in the measured understatement of British science: 'From the results so far obtained it is difficult to avoid the conclusion that the long-range atoms arising from collision of [alpha] particles with nitrogen are not nitrogen atoms but probably atoms of hydrogen. . . . If this be the case, we must conclude that the nitrogen atom is disintegrated.' Newspapers soon published the discovery in plainer words: Sir Ernest Rutherford, headlines blared in 1919, had split the atom.

"It was less a split than a transmutation, the first artificial transmutation ever achieved. When an alpha particle, atomic weight 4, collided with a nitrogen atom, atomic weight 14, knocking out a hydrogen nucleus (which Rutherford would shortly propose calling a proton), the net result was a new atom of oxygen in the form of the oxygen isotope 017: 4 plus 14 minus 1." (Rhodes 1971, 137)

97 "At the turn of the century, there was little known about atoms except that they contained electrons. J. J. Thompson discovered the electron in 1897, and there was considerable speculation about where these negatively charged particles existed in nature. . . . One popular theory of the time was called the 'plum pudding model'. This model, invented by Thompson, envisioned matter made of atoms that were spheres of positive charge spiked with electrons throughout. . . . In 1911, Ernest Rutherford performed an experiment to test the plum pudding model. . . . Rutherford's result lead him to believe that most of the foil was made of empty space, but had extremely small, dense lumps of matter inside. No other model accounted for the occasional wide angle scattering of the α. With this experiment, Rutherford discovered the nucleus." (Gustafsson Group 2001)

98 "It was the month of February in the year of 1896. Antoine Henri Becquerel, a French scientist, was conducting an experiment which started with the exposure of a uranium-bearing crystal to sunlight. . . . What he had discovered was radioactivity! He attributed this phenomenon to spontaneous emission by the uranium." (Duke University Department of Chemistry 2001)

99 "[Otto Hahn's] most spectacular discovery came at the end of 1938. While working jointly with Dr. Strassmann, Hahn discovered the fission of uranium and thorium in medium heavy atomic nuclei and his first work on these subjects appeared on 6th January and 10th February, 1939, in Naturwissenschaften." (nobelprize.org 2001)

100 "I find Szilárd a tricky character, historically. He's half-crank and half-visionary. . . . I don't think there was any good reason in 1934 to think that Szilárd's idea was a workable one. However, once you add nuclear fission to the picture, it suddenly becomes quite obvious and brilliant. In retrospective, it is easy to see Szilárd's patent application as visionary, but only if you filter out all of the wrong and confused parts of it." (Wellerstein and Grace 2015, 4)

101 ". . . it is a speculative patent which says that if a neutron-based chain reaction could be created, it would be useful for liberating nuclear power or creating more radioactive elements. It was entirely theoretical when filed: Szilárd had no idea whether a nuclear chain reaction was possible, or what materials might sustain one." (Wellerstein 2011a)

102 "It was also a cheeky piece of bravado. Szilárd had only theoretical grounds for believing that neutrons might induce radioactivity artificially. He had not done the necessary experiments. Only the Joliot-Curies had carried out such experiments so far, and they used alpha particles. . . . He wondered which element or elements might emit two or more neutrons for each neutron captured." (Rhodes 1971, 204)

103 "What else besides beryllium? Leó Szilárd asked himself in London. Beryllium looked suspicious. What other elements might chain-react? He answered with an amended patent specification on April 9, 1935: 'Other examples for elements from which neutrons can liberate multiple neutrons are uranium and bromine.' He was guessing, and without research funds he saw no way to experiment." (Rhodes 1971, 221)

104 "The reason they weren't interested in it is an important one. It isn't that they were fools or unimaginative. It's that the patent requires the presumption of a nuclear phenomena that in 1934-1935 was not known to exist. This is the part that people often get very confused about: Szilárd wasn't filing a patent on nuclear fission bombs or nuclear fission reactors. What he was filing was a patent that said, 'if there is a nuclear phenomena that is triggered by one neutron and produces more than one neutron subsequently, then you could create a neutron-based nuclear chain reaction that would release a lot of energy and produce a lot of neutrons.'

"But he didn't know what the nuclear phenomena would be. Nuclear fission wasn't discovered until late 1938/early 1939. What Szilárd was imagining wasn't fission . . . he was imagining something more like artificial radioactivity, where a neutron makes an unradioactive atom radioactive. In his scheme, a neutron would make an atom radioactive, which would make it release another neutron, or maybe two, which would then continue the reaction. This is more or less what he had in mind, though his patent describes many elaborations regarding substances that would work in tandem, hypothetical dineutrons and tetraneutrons, and linear (as opposed to exponential) reactions. These reactions would be low-energy, some of them wouldn't release energy at all, and the dineutrons aren't stable. There was a lot of confusion in his work on this topic . . . he had the germ of a good idea, but he also had a lot of bad and wrong ideas. And absolutely none of his speculations were supported by experiment." (Wellerstein and Grace 2015, 3)

105 "[early 1934] Several weeks earlier, looking for a patron, he [Szilárd] had sent Sir Hugo Hirst, the founder of the British General Electric Company, a copy of the first chapter of *The World Set Free*. 'Of course,' he wrote Sir Hugo with a touch of bitterness, still brooding on Rutherford's prediction, 'all this is moonshine, but I have reason to believe that in so far as the industrial applications of the present discoveries in physics are concerned, the forecast of the writers may prove to be more accurate than the forecast of the scientists. The physicists have conclusive arguments as to why we cannot create at present new sources of energy for industrial purposes; I am not so sure whether they do not miss the point.' " (Rhodes 1971, 214)

106 "[at around the time of the patent] 'None of the physicists had any enthusiasm for this idea of a chain reaction,' he [Szilárd] would remember. Rutherford threw him out. Blackett told him, 'Look, you will have no luck with such fantastic ideas in England. Yes, perhaps in Russia. If a Russian physicist went to the government and [said], "We must make a chain reaction," they would give him all the money and facilities which he would need. But you won't get it in England.' " (Rhodes 1971, 204)

107 "Even so, in the mid 1930s, Einstein, Rutherford, and even Bohr . . . the three leading physicists in the world . . . all said at one time or another, 'There's no way we're ever going to get energy out of the nucleus,' with Rutherford famously calling the idea, 'Moonshine.' Szilárd's response to that was, 'Well, I'll show you!' " (Rhodes and Grace 2015, 3)

108 "Thus by the mid-1930s the three most original living physicists had each spoken to the question of harnessing nuclear energy. Rutherford had dismissed it as moonshine; Einstein had compared it to shooting in the dark at scarce birds; Bohr thought it remote in direct proportion to understanding." (Rhodes 1971, 228)

109 "For still more violent impacts, with particles of energies of about a thousand million volts, we must even be prepared for the collision to lead to an explosion of the whole nucleus. Not only are such energies, of course, at present far beyond the reach of experiments, but it does not need to be stressed that such effects would scarcely bring us any nearer to the solution of the much discussed problem of releasing the nuclear energy for practical purposes. Indeed, the more our knowledge of nuclear reactions advances the remoter this goal seems to become." Bohr *c.* 1935 (quoted in Rhodes 1971, 227)

110 "And indeed the leading physicists in Britain were cool to Szilárd's obstreperous advice. They thought his proposed chain reaction entirely unworkable (as was in fact the case for the mechanisms Szilárd was then considering)." (Weart 1976, 23)

111 "Their names—Edward Teller, Enrico Fermi, Leó Szilárd, Eugene Wigner, are known well enough today, but back then, nobody knew who these Middle Europeans were." (Rhodes and Grace 2015, 5)

112 "So in 1934-1935, Szilárd was—perhaps accurately—interpreted as kind of a crank. He was trying to drum up attention (and money) for a problem that wasn't at all clear it existed. That the British authorities humored him enough to make the patent secret is perhaps a demonstration of their patience." (Wellerstein and Grace 2015, 3)

113 "He came up for air in September. By then he was living at the Imperial Hotel in Russell Square, having transferred £1,595 from Zurich to his bank in London. More than half the money, £854, he held in trust for his brother Béla; the rest would see him through the year. Szilárd's funds came from his patent licenses, refrigeration consulting and Privatdozent fees. He was busy finding jobs for others and couldn't be bothered to seek one himself. He had few expenses in any case; a week's lodging and three meals a day at a good London hotel cost about £5.5; he was a bachelor most of his life and his needs were simple." (Rhodes 1971, 26)

114 See endnote 11.

115 "Since the beginning of his rescue work in England Szilárd had been in occasional contact with the physicist Frederick Alexander Lindemann, who was professor of experimental philosophy at Oxford and director of the Clarendon Laboratory there. . . . The following February 1936, [Lindemann] intervened on Szilárd's behalf with the Admiralty . . . writing the head of the Department of Scientific Research and Development cannily:

> I daresay you remember my ringing you up about a man working here who had a patent which he thought ought to be kept secret. I enclose a letter from him on the subject as you suggested. I am naturally somewhat less optimistic about the prospects than the inventor, but he is a very good physicist and even if the chances were a hundred to one against it seems to me it might be worth keeping the thing secret as it is not going to cost the Government anything." (Rhodes 1971, 224–25)

116 "[*c.* 1935] . . . without research funds [Szilárd] saw no way to experiment. The physicists he talked to remained profoundly skeptical of his ideas. 'So I thought, there is after all something called "chain reaction" in chemistry. It doesn't resemble a nuclear chain reaction, but still it's a chain reaction. So I thought I would talk to a chemist.' " (Rhodes 1971, 221)

117 "I don't think he was as concerned about the Germans in 1933-1934 than it was in 1939. He knew the Germans were bad . . . he had fled Germany when Hitler took power, just before things got really bad for the Jews . . . but it wasn't obvious that world war was going to be the consequence. . . . Szilárd prided himself on being ahead of his time, of thinking about the *next* big thing." (Wellerstein and Grace 2015, 2–3)

118 "At noon on January 30, 1933, Adolf Hitler, forty-three years old, gleefully accepted appointment as Chancellor of Germany . . . Hitler decided on a boycott of Jewish businesses as an opening sally. The national boycott began on Saturday, April 1 . . . Jews caught in the streets were beaten while the police looked on. . . .

". . . The Third Reich promulgated its first anti-Jewish ordinance on April 7. The Law for the Restoration of the Professional Civil Service, the harbinger of some four hundred anti-Semitic laws and decrees the Nazis would issue, changed Teller's life, Pauli's, Frisch's, the lives of their colleagues decisively, forever. It announced bluntly that 'civil servants of non-Aryan descent must retire.' A decree defining 'non-Aryan' followed on April 11: anyone 'descended from non-Aryan, especially Jewish, parents or grandparents.' Universities were state institutions. Members of their faculties were therefore civil servants. The new law abruptly stripped a quarter of the physicists of Germany, including eleven who had earned or would earn Nobel Prizes, of their positions and their livelihood. It immediately affected some 1,600 scholars in all. Nor were academics dismissed by the Reich likely to find other work. To survive they would have to emigrate." (Rhodes 1971, 184–85)

119 "In the summer of 1933, another scientist who had fled Hitler's Germany was living on Russell Square. Leó Szilárd hard brought his two suitcases to the Imperial Hotel, less costly than Haber's hotel but just down the road." (Smith 2008, 210)

120 "Always a visionary, Szilárd, sacrificing many years of his career and having no permanent post for himself, worked tirelessly to find suitable positions for many of the other scientists fleeing Germany. Often working by himself, at the detriment of his own safety and career, Szilárd was responsible for numerous colleagues being offered positions. He organized several groups and worked with the Academic Assistance Council, a London-based group headed by Ernest Rutherford that helped refugee scientists and scholars." (atomicheritage.org 2014)

121 See endnote 79.

122 See endnote 77.

123 " 'I took a train from Berlin to Vienna on a certain date, close to the first of April, 1933,' Szilárd writes. 'The train was empty. The same train the next day was overcrowded, was stopped at the frontier, the people had to get out, and everybody was interrogated by the Nazis. This just goes to show that if you want to succeed in this world you don't have to be much cleverer than other people, you just have to be one day earlier.' " (Rhodes 1971)

124 See endnote 75.

125 "During the same period Szilárd wrote Michael Polanyi he would 'stay in England until one year before the war, at which time I would shift my residence to New York City.' The letter provoked comment, Szilárd enjoyed recalling; it was 'very funny, because how can anyone say what he will do one year before the war?' As it turned out, his prognostication was off by only four months: he arrived in the United States on January 2, 1938." (Rhodes 1971, 237)

126 "An older Hungarian friend, Szilárd remembers—Michael Polanyi, a chemist at the Kaiser Wilhelm Institutes with a family to consider—viewed the German political scene optimistically, like many others in Germany at the time. 'They all thought that civilized Germans would not stand for anything really rough happening.' Szilárd held no such sanguine view, noting that the Germans themselves were paralyzed with cynicism, one of the uglier effects on morals of losing a major war." (Rhodes 1971, 25)

127 "From Oxford in late March 1936 he had written Gertrud Weiss in Vienna that she should consider emigrating to America. . . . 'He [Szilárd] told me he would be surprised if one could work in Vienna in two years. He said Hitler would be there. And he was . . . almost to the day.' " (Rhodes 1971, 236–37)

128 "[In 1938] Szilárd was discouraged. 'As my knowledge of nuclear physics increased,' he said later, 'my faith in the possibility of a chain reaction gradually decreased.' If other kinds of radiation also induced radioactivity in indium without producing neutrons, then he would have no more candidates for neutron multiplication and he would have to give up his belief in the process he still nicknamed 'moonshine.' " (Rhodes 1971, 246–47)

129 "Leó Szilárd's work at the University of Rochester confirmed that no neutrons came out when indium was irradiated. On December 21, as Hahn and Meitner exchanged their excited letters, Szilárd advised the British Admiralty by letter:

> Further experiments . . . have definitely cleared up the anomalies which I have observed in 1936 . . . In view of this new work it does not now seem necessary to maintain [my] patent . . . nor would the waiving of the secrecy of this patent serve any useful purpose. I beg therefore to suggest that the patent be withdrawn altogether.

"Szilárd's faith in the possibility of a chain reaction, as he said later, had 'just about reached the vanishing point.' " (Rhodes 1971, 254)

130 "This new discovery revives all the hopes and fears in this respect which I had in 1934 and 1935, and which I have as good as abandoned in the course of the last two years. At present I am running a high temperature and am therefore confined to my four walls, but perhaps I can tell you more about these new developments some other time." (Rhodes 1971, 268)

131 "In New York that day Szilárd dragged himself to the nearest Western Union office and cabled the British Admiralty:

> KINDLY DISREGARD MY RECENT LETTER STOP WRITING

"The secret patent had revived." (Rhodes 1971)

132 "I [Szilárd] feel I ought to let you know of a very sensational new development in nuclear physics. In a paper . . . Hahn reports that he finds when bombarding uranium with neutrons the uranium breaking up. . . . This is entirely unexpected and exciting news for the average physicist. The Department of Physics at Princeton, where I spent the last few days, was like a stirred-up ant heap." (Rhodes 1971, 267)

133 "I repeat that there is a chain-reaction mood in Washington. I only had to say "uranium" and then could listen for two hours to their thoughts." Teller (quoted in Rhodes 1971, 290)

134 "In Szilárd's case, he talked to people for several years before he found anyone serious enough to join him in the idea. After that, it really took the discovery of fission itself before people were prepared to take Szilárd seriously. Then, of course, everybody took it seriously. Not only the chain reaction, but also the idea that you could get energy out of the nucleus." (Rhodes and Grace 2015, 13)

135 "After Bohr's arrival Szilárd traveled down from New York to visit his sick friend and won a long-overdue surprise:

> Wigner told me of Hahn's discovery. Hahn found that uranium breaks into two parts when it absorbs a neutron. . . . When I heard this I immediately saw that these fragments, being heavier than corresponds to their charge, must emit neutrons, and if enough neutrons are emitted . . . then it should be, of course, possible to sustain a chain reaction. All the things which H. G. Wells predicted appeared suddenly real to me.

"At Wigner's bedside in the Princeton infirmary the two Hungarians debated what to do.

"... Szilárd also hoped to talk to Fermi: 'I thought that if neutrons are in fact emitted in fission, this fact should be kept secret from the Germans. So I was very eager to contact Joliot and to contact Fermi, the two men who were most likely to think of this possibility.' He had borrowed Wigner's apartment and had not yet left Princeton." (Rhodes 1971, 266–67)

136 "*Naturwissenschaften* reached Paris about January 16. One of Frederic Jolot's associates recalls that 'in a rather moving meeting [Joliot] made a report on this result to Madame Joliot and myself after having locked himself in for a few days and not talked to anybody.' The Joliot-Curies were once again appalled to find they had barely missed a major discovery. In the next few days Joliot independently deduced the large energy release and considered the possibility of a chain reaction, as Szilárd had thought he might. He tried to track down the neutrons from fission first, found that approach difficult, then set up an experiment somewhat like Frisch's. He detected fission fragments on January 26." (Rhodes 1971, 271)

137 "Alvarez wired Gamow for details, learned of the Frisch experiment, then tracked down Oppenheimer:

> I remember telling Robert Oppenheimer that we were going to look for [ionization pulses from fission] and he said, "That's impossible" and gave a lot of theoretical reasons why fission couldn't really happen. When I invited him over to look at the oscilloscope later, when we saw the big pulses, I would say that in less than fifteen minutes Robert had decided that this was indeed a real effect and . . . he had decided that some neutrons would probably boil off in the reaction, and that you could make bombs and generate power, all inside of a few minutes. . . . It was amazing to see how rapidly his mind worked, and he came to the right conclusions.

". . . The next day, in a letter to George Uhlenbeck at Columbia, 'quite something' became 'might very well blow itself to hell.' One of Oppenheimer's students, the American theoretical physicist Philip Morrison, recalls that 'when fission was discovered, within perhaps a week there was on the blackboard in Robert Oppenheimer's office a drawing—a very bad, an execrable drawing—of a bomb.' Enrico Fermi made similar estimates. George Uhlenbeck, who shared an office with him in Pupin Hall, was there one day to overhear him. Fermi was standing at his panoramic office window high in the physics tower looking down the gray winter length of Manhattan Island, its streets alive as always with vendors and taxis and crowds. He cupped his hands as if he were holding a ball. 'A little bomb like that,' he said simply, for once not lightly mocking, 'and it would all disappear.' " (Rhodes 1971, 274–75)

138 "If Bush was initially less willing to invest in chain-reaction experiments than Teller would have liked him to be, he kept good company; neither Ernest Lawrence at Berkeley nor Otto Hahn in Dahlem nor Lise Meitner, visiting Copenhagen that February to work with Otto Frisch, chose to pursue moonshine. Only Columbia and Paris mounted early experiments, though the DTM would soon follow the Columbia lead." (Rhodes 1971, 290)

139 "He was willing to assume responsibility for sounding the alarm even though it was quite possible that the alarm might prove to be a false alarm. The one thing most scientists are really afraid of is to make fools of themselves. Einstein was free from such a fear and this above all is what made his position unique on this occasion." Leó Szilárd (quoted in Rhodes 1971, 306)

140 "He signed both letters and returned them to Szilárd in less than a week with a note hoping 'that you will finally overcome your inner resistance; it's always questionable to try to do something too cleverly.' That is, be bold and get moving. 'We will try to follow your advice,' Szilárd rejoined on August 9, 'and as far as possible overcome our inner resistances which, admittedly, exist. Incidentally, we are surely not trying to be too clever and will be quite satisfied if only we don't look too stupid.' " (Rhodes 1971, 307)

141 "[In 1939] G. P. Thomson, J.J.'s son, who was professor of physics at Imperial College, London, heard [Joliot's claim that uranium would probably chain react]. 'I began to consider carrying out certain experiments with uranium,' he told a correspondent later. 'What I had in mind was something rather more than a piece of pure research, for at the back of my thoughts there lay the possibility of a weapon.' He applied forthwith to the British Air Ministry for a ton of uranium oxide, 'ashamed of putting forward a proposal apparently so absurd.' " (Rhodes 1971, 296)

142 ". . . Fermi said, 'Well . . . there is the remote possibility that neutrons may be emitted in the fission of uranium and then of course perhaps a chain reaction can be made.' Rabi said, 'What do you mean by "remote possibility"?' and Fermi said, 'Well, ten per cent.' . . .

". . . Fermi was not misleading Szilárd. It was easy to estimate the explosive force of a quantity of uranium, as Fermi would do standing at his office window overlooking Manhattan, if fission proceeded automatically from mere assembly of the material; even journalists had managed that simple calculation. But such obviously was not the case for uranium in its natural form, or the substance would long ago have ceased to exist on earth. However energetically interesting a reaction, fission by itself was merely a laboratory curiosity. Only if it released secondary neutrons, and those in sufficient quantity to initiate and sustain a chain reaction, would it serve for anything more. 'Nothing known then,' writes Herbert Anderson, Fermi's young partner in experiment, 'guaranteed the emission of neutrons. Neutron emission had to be observed experimentally and measured quantitatively.' No such work had yet been done. It was, in fact, the new work Fermi had proposed to Anderson immediately upon returning from Washington. Which meant to Fermi that talk of developing fission into a weapon of war was absurdly premature." (Rhodes 1971, 280)

143 "The following month, on April 22, Joliot, von Halban and Kowarski published a second paper in *Nature* concerning secondary neutrons. This one, 'Number of neutrons liberated in the nuclear fission of uranium,' rang bells. Calculating on the basis of the experiment previously reported, the French team found 3.5 secondary neutrons per fission. 'The interest of the phenomenon discussed here as a means of producing a chain of nuclear reactions,' the three men wrote, 'was already mentioned in our previous letter.' Now they concluded that if a sufficient amount of uranium were immersed in a suitable moderator, 'the fission chain will perpetuate itself and break up only after reaching the walls limiting the medium. Our experimental results show that this condition will most probably be satisfied.' That is, uranium would most probably chain-react.

"Joliot's was an authoritative voice. G. P. Thomson, J.J.'s son, who was professor of physics at Imperial College, London, heard it. 'I began to consider carrying out certain experiments with uranium,' he told a correspondent later. 'What I had in mind was something rather more than a piece of pure research, for at the back of my thoughts there lay the possibility of a weapon.' He applied forthwith to the British Air Ministry for a ton of uranium oxide, 'ashamed of putting forward a proposal apparently so absurd.' " (Rhodes 1971, 296)

144 See endnote 141.

145 "However, even once fission was discovered/announced, it wasn't obvious it could be *weaponized*. There were plenty of quite brilliant scientists—like Enrico Fermi—who thought in 1939 that the fear of fission-based bombs was still too speculative to worry much about, but it was just realistic enough a possibility, especially on the eve of war, that he found some limited success in convincing them to self-censor and to look seriously into the issue." (Wellerstein and Grace 2015, 3–4)

146 "Bohr doubted very much that fission could be used to cause, a devastating explosion." [describing events in 1939] (Weart 1976, 26)

147 "However, like Bohr and Fermi, the French believed an atomic bomb was not likely to be built for many years, if ever. . . . For all these reasons, the team cabled Weisskopf a discouraging reply around 5 April [1939]." (Weart 1976, 28)

148 "[G.P. Thomson] had concluded after neutron-bombardment experiments that a chain reaction in natural uranium was unlikely and a war project therefore impractical." (Rhodes 1971, 329)

149 "Pegram prepared a letter of introduction for Fermi to carry along to his appointment. It stated a hesitant case dense with hypotheticals:

> Experiments in the physics laboratory at Columbia University reveal that conditions may be found under which the chemical element uranium may be able to liberate its large excess of atomic energy, and that this might mean the possibility that uranium might be used as an explosive that would liberate a million times as much energy per pound as any known explosive. My own feeling is that the probabilities are against this, but my colleagues and I think that the bare possibility should not be disregarded." (Rhodes 1971, 293)

150 "Bohr's skepticism, says Wheeler, concerned 'the enormous difficulty of separating the necessary quantities of U235.' Fermi noted in a later lecture that 'it was not very clear [in 1939] that the job of separating large amounts of uranium 235 was one that could be taken seriously.' At the Princeton meeting, Teller remembers, Bohr insisted that 'it can never be done unless you turn the United States into one huge factory.' " (Rhodes 1971, 294)

151 "But while such a plan might demonstrate a potential future source of power, the American scientists and administrators who were advising Briggs could not yet identify any military use. In April the British Thomson committee asked A. V. Hill, a scientific adviser to the British Embassy in Washington, to find out what the Americans were doing about fission. According to the official history of the British atomic energy program, Hill talked to unidentified 'scientists of the Carnegie Institution,' whose opinions he reported pungently:

> It is not inconceivable that practical engineering applications and war use may emerge in the end. But I am assured by American colleagues that there is no sign of them at present and that it would be a sheer waste of time for people busy with urgent matters in England to turn to uranium as a war investigation. If anything likely to be of war value emerges they will certainly give us a hint of it in good time. A large number of American physicists are working on or interested in the subject; they have excellent facilities and equipment: they are extremely well disposed towards us: and they feel that it is much better that they should be pressing on with this than that our people should be wasting their time on what is scientifically very interesting, but for present practical needs probably a wild goose chase." (Rhodes 1971, 334)

152 "[In the summer of 1942] Hans Bethe, now thirty-six and a highly respected professor of physics at Cornell, had resisted joining the bomb project because he doubted the weapon's feasibility. 'I considered . . . an atomic bomb so remote,' Bethe told a biographer after the war." (Rhodes 1971, 415)

153 " 'By separating the 235 isotope,' Herbert Anderson emphasizes in a memoir, 'it would be much easier to obtain the chain reaction. More than this, with the separated isotope the prospect for a bomb with unprecedented explosive power would be very great.' Fermi urged Nier in similar terms; Nier recalls that he 'went back and figured out how we might soup up our apparatus some in order to increase the output. . . . I did work on the problem, but at first it seemed like such a farfetched thing that I didn't work on it as hard as I might have. It was just one of a number of things I was trying to do.' " (Rhodes 1971, 298)

154 "Even if the physicists get all that they expect, I believe that there is a very long period of engineering work of the most difficult nature before anything practical can come out of the matter, unless there is an explosive involved, which I very much doubt." Vannevar Bush (quoted in Rhodes 1971, 366)

155 "But to American physicists and administrators in and out of government a bomb of uranium seemed a remote possibility at best." [in around 1939 or 1940] (Rhodes 1971, 317)

156 "Bohr figured out that U235 would be the bomb material. The Berkeley radiochemist Glenn Seaborg figured out that a uranium reactor could be configured to breed a transuranic (manmade) element, which he named plutonium, and that plutonium would be even more fissile than U235.

"But to get U235 out of natural uranium would mean building truly vast isotope separation complexes, on the scale of oil refineries, and to breed plutonium in adequate quantities would mean building large production reactors charged with tens of tons of uranium metal and moderated with more tens of tons of highly purified graphite. And that meant major support from the U. S. government. The military had to be convinced . . . and it was profoundly skeptical . . . and so did the scientific establishment. That's why the Manhattan Project wasn't organized for almost three years after the discovery of nuclear fission." (Rhodes and Grace 2015, 4)

157 "Bohr insisted that we would never succeed in producing nuclear energy and he also insisted that secrecy must never be introduced into physics.

"Bohr's skepticism, says Wheeler, concerned 'the enormous difficulty of separating the necessary quantities of U235.' Fermi noted in a later lecture that 'it was not very clear [in 1939] that the job of separating large amounts of uranium 235 was one that could be taken seriously.' At the Princeton meeting, Teller remembers, Bohr insisted that 'it can never be done unless you turn the United States into one huge factory.' " (Rhodes 1971, 294)

158 "Meanwhile the British Chemical Society asked Frisch to write a review of advances in experimental nuclear physics for its annual report. . . . Frisch's review article mentioned the possibility of a chain reaction only to discount it. He based that conclusion on Bohr's argument that the U238 in natural uranium would scatter fast neutrons, slowing them to capture-resonance energies; the few that escaped capture would not suffice, he thought, to initiate a slow-neutron chain reaction in the scarce U235. Slow neutrons in any case could never produce more than a modest explosion, Frisch pointed out; they took too long slowing down and finding a nucleus. As he explained later:

> That process would take times of the order of a sizeable part of a millisecond [i.e., a thousandth of a second], and for the whole chain reaction to develop would take several milliseconds; once the material got hot enough to vaporize, it would begin to expand and the reaction would be stopped before it got much further. So the thing might blow up like a pile of gunpowder, but no worse, and that wasn't worth the trouble." (Rhodes 1971, 319–20)

159 "Plugging in the fission cross section of natural uranium, which was essentially the fission cross section of U238, gave a critical mass, notes Peierls, 'of the order of tons.' As a weapon, an object of that size was too unwieldy to take seriously. 'There was of course no chance of getting such a thing into any aeroplane, and the paper appeared to have no practical significance.' Peierls was aware of the British and American concern for secrecy, but in this case he saw no reason not to publish." (Rhodes 1971, 321)

160 "The *New York Times* [April 30, 1939] account accurately summarizes the divisions in the U.S. physics community at the time:

> "Tempers and temperatures increased visibly today among members of the American Physical Society as they closed their Spring meeting with arguments over the probability of some scientist blowing up a sizable portion of the earth with a tiny bit of uranium, the element which produces radium.
>
> Dr. Niels Bohr of Copenhagen, a colleague of Dr. Albert Einstein at the Institute for Advanced Study, Princeton, N.J., declared that bombardment of a small amount of the pure Isotope U235 of uranium with slow neutron particles of atoms would start a 'chain reaction' or atomic explosion sufficiently great to blow up a laboratory and the surrounding country for many miles.
>
> Many physicists declared, however, that it would be difficult, if not impossible, to separate Isotope 235 from the more abundant Isotope 238. The Isotope 235 is only 1 per cent of the uranium element.
>
> Dr. L. Onsager of Yale University described, however, a new apparatus in which, according to his calculations, the isotopes of elements can be separated in gaseous form in tubes which are cooled on one side and heated to high temperatures on the other.
>
> Other physicists argued that such a process would be almost prohibitively expensive and that the yield of Isotope 235 would be infinitesimally small. Nevertheless, they pointed out that, if Dr. Onsager's process of separation should work, the creation of a nuclear explosion which would wreck as large an area as New York City would be comparatively easy. A single neutron particle, striking the nucleus of a uranium atom, they declared, would be sufficient to set off a chain reaction of millions of other atoms." (Rhodes 1971, 297)

161 "Similarly settled, Szilárd told Einstein about the Columbia secondary-neutron experiments and his calculations toward a chain reaction in uranium and graphite. Long afterward he would recall his surprise that Einstein had not yet heard of the possibility of a chain reaction. When he mentioned it Einstein interjected, 'Daranhabe ich gar nicht gedacht!' —'I never thought of that!' " (Rhodes 1971, 305)

162 "He [Lindemann] introduced the subject of the study of the fission of uranium atoms. 'I reacted by repeating the doubts I had expressed and heard expressed at NDRC meetings.' Lindemann brushed them aside and pounced:

> "You have left out of consideration," said [Lindemann], "the possibility of the construction of a bomb of enormous power." "How would that be possible?" I asked. "By first separating uranium 235," he said, "and then arranging for the two portions of the element to be brought together suddenly so that the resulting mass would spontaneously undergo a self-sustaining reaction."

"Remarkably, the chairman of the chemistry and explosives division of the NDRC adds that, as late as March 1941, 'this was the first I had heard about even the remote possibility of a bomb.' " (Rhodes 1971, 359)

163 "More crucial was a June 4 conference at Harnack House that Speer, Fromm, automobile and tank designer Ferdinand Porsche and other military and industrial leaders attended. In February Heisenberg had devoted most of his lecture to nuclear power. This time he emphasized military prospects. The secretary of the Kaiser Wilhelm Society was surprised: 'The word "bomb" which was used at this conference was news not only to me but for many others present, as I could see from their reaction.' It was not news to Speer." (Rhodes 1971, 404)

164 "Heisenberg had larger plans. He had allied himself with von Weizsdcker at the KWI. In July [1940] they began designing a wooden laboratory building to be constructed on the grounds of the Kaiser Wilhelm Institute for Biology and Virus Research, next to the physics institute. To discourage the curious they named the building the Virus House. They intended to build a subcritical uranium burner there. . . . The Virus House was finished in October." (Rhodes 1971, 344)

165 ". . . by the early 1940s the subject of atomic power and atomic bombs had become a staple of science journalists and science fiction authors." (Wellerstein 2014a)

166 "It had seen the light of day before, most notably in an August 5, 1939, letter from Member of Parliament Winston Churchill to the British Secretary of State for Air. Concerned that Hitler might bluff Neville Chamberlain with threats of a new secret weapon, Churchill had collected a briefing from Frederick Lindemann and written to caution the cabinet not to fear 'new explosives of devastating power' for at least 'several years.' " (Rhodes 1971, 320)

167 "Frisch found a friend that year in a fellow emigre at Birmingham, the theoretician Rudolf Peierls. . . .

> I had worked out the possible efficiency of my separation system with the help of Clusius's formula, and we came to the conclusion that with something like a hundred thousand similar separation tubes one might produce a pound of reasonably pure uranium-235 in a modest time, measured in weeks. At that point we stared at each other and realized that an atomic bomb might after all be possible.

" 'The cost of such a plant,' Frisch adds for perspective, 'would be insignificant compared with the cost of the war.' " (Rhodes 1971, 321–324)

168 " 'Look, shouldn't somebody know about that?' Frisch then asked Peierls. They hastened their calculations to Mark Oliphant. 'They convinced me,' Oliphant testifies. He told them to write it all down.

"They did, succinctly, in two parts, one part three typewritten pages, the other even briefer. . . .

". . . The first of the two parts they titled 'On the construction of a "super-bomb"; based on a nuclear chain reaction in uranium.' It was intended, they wrote, 'to point out and discuss a possibility which seems to have been overlooked in . . . earlier discussions.' They proceeded to cover the same ground they had previously covered together in private, noting that 'the energy liberated by a 5 kg bomb would be equivalent to that of several thousand tons of dynamite.' They described a simple mechanism for arming the weapon: . . .

". . . The second report, 'Memorandum on the properties of a radioactive "super-bomb," ' a less technical document, was apparently intended as an alternative presentation for nonscientists. This study explored beyond the technical questions of design and production to the strategic issues of possession and use. . . .

"... Frisch and Peierls finished their two reports and took them to Oliphant. He quizzed the men thoroughly, added a cover letter to their memoranda ('I have considered these suggestions in some detail and have had considerable discussion with the authors, with the result that I am convinced that the whole thing must be taken rather seriously, if only to make sure that the other side are not occupied in the production of such a bomb at the present time') and sent letter and documents off to Henry Thomas Tizard, an Oxford man, a chemist by training, the driving force behind British radar development, the civilian chairman of the Committee on the Scientific Survey of Air Defense—better known as the Tizard Committee—which was the most important British committee at the time concerned with the application of science to war." (Rhodes 1971, 324–25)

169 "The second report, 'Memorandum on the properties of a radioactive "super-bomb," ' a less technical document, was apparently intended as an alternative presentation for nonscientists. This study explored beyond the technical questions of design and production to the strategic issues of possession and use; it managed at the same time both seemly innocence and extraordinary prescience:

1. As a weapon, the super-bomb would be practically irresistible. There is no material or structure that could be expected to resist the force of the explosion. . . .
2. Owing to the spreading of radioactive substances with the wind, the bomb could probably not be used without killing large numbers of civilians, and this may make it unsuitable as a weapon for use by this country. . . .
3. . . . It is quite conceivable that Germany is, in fact, developing this weapon. . . .
4. If one works on the assumption that Germany is, or will be, in the possession of this weapon, it must be realised that no shelters are available that would be effective and could be used on a large scale. The most effective reply would be a counter-threat with a similar weapon." (Rhodes 1971, 324–25)

170 "Tizard, who had been skeptical to begin with and had taken Thomson's conclusions as support for his skepticism, appointed Thomson chairman of the small committee; James Chadwick, now at Liverpool, his assistant P. B. Moon and Rutherford protege John Douglas Cockcroft were added to the list. Blackett was busy with other war work, although he would join the committee later. The group met informally for the first time on April 10 in the Royal Society's quarters at Burlington House." (Rhodes 1971, 329–30)

171 "In late June G. P. Thomson gave his committee a new name to disguise its activities: MAUD." (Rhodes 1971, 340)

172 " 'We entered the project with more scepticism than belief,' the committee would report later, 'though we felt it was a matter which had to be investigated.' Thomson's minutes make that skepticism evident." (Rhodes 1971, 330)

173 "At a second meeting on April 24 Thomson recorded laconically that 'Dr. Frisch produced some notes to show that the uranium bomb was feasible.' Many years later Oliphant recalled a more expansive response: 'The Committee generally was electrified by the possibility.' " (Rhodes 1971, 330)

174 "At Oxford in December 1940, Franz Simon, now officially working for the MAUD Committee, produced a report nearly as crucial to the future of uranium-bomb development as the original Frisch-

Peierls memoranda had been. It was titled 'Estimate of the size of an actual separation plant.' Its aim, Simon wrote, was 'to provide data for the size and costs of a plant which separates I kg per day of 235U from the natural product.' He estimated such a plant would cost about £5,000,000 and outlined its necessities in careful detail." (Rhodes 1971, 343)

175 "The first test of this theory," wrote Peierls in March 1941, "has given a completely positive answer and there is no doubt that the whole scheme is feasible (provided the technical problems of isotope separation are satisfactorily solved) and that the critical size for a U sphere is manageable." (Rhodes 1971, 355)

176 "I remember the spring of 1941 to this day. I realized then that a nuclear bomb was not only possible—it was inevitable. Sooner or later these ideas could not be peculiar to us. Everybody would think about them before long, and some country would put them into action." (Rhodes 1971, 356)

177 "It was, in the end, the British scientific establishment, which came to the U.S. in the summer of 1940 with a trunk full of technical secrets which it freely gave to the U. S. government, which convinced its American counterparts to take the possibility of an atomic bomb seriously." (Rhodes and Grace 2015, 5)

178 "Then for the first time a ranking American physicist joined the debate whose voice could not be ignored. Even before Seaborg and Segré confirmed the fissibility of plutonium, Ernest Lawrence had measured the prevailing American skepticism and conservatism against the increasing enthusiasm of his British friends and responded with characteristic fervor. . . . Warren Weaver, the director of the division of natural sciences at the Rockefeller Foundation, visited Berkeley in February to see how construction was progressing on the 4,900-ton, 184-inch cyclotron for which the foundation had awarded a $1,150,000 grant less than twelve months earlier. Lawrence took time to complain about the Uranium Committee's sloth . . .

"Lawrence rehearsed his complaint again in March when Conant, back from London, traveled out to deliver an address. 'Light a fire under the Briggs committee,' the energetic Californian badgered the president of Harvard. 'What if German scientists succeed in making a nuclear bomb before we even investigate possibilities?' " (Rhodes 1971, 360–61)

179 "Yet they were sufficiently seized with Lawrence's fervor that Compton telephoned Vannevar Bush almost as soon as Lawrence left the room and dictated a follow-up letter the same day. Briggs was 'by nature slow, conservative, methodical and accustomed to operate at peacetime government bureau tempo,' Compton wrote, conveying Lawrence's blunt complaints, and had been 'following a policy consistent with these qualities and still further inhibited by the requirement of secrecy.' " (Rhodes 1971, 361)

180 See Grace and Christiano 2014, Table 1.

181 See Grace and Christiano 2014.

182 ". . . The patent, Szilárd explained in the letter Lindemann enclosed, 'contains information which could be used in the construction of explosive bodies . . . very many thousand times more powerful than ordinary bombs.' " (Rhodes 1971, 224–25)

183 "The 40,000,000–50,000,000 deaths incurred in World War II make it the bloodiest conflict, as well as the largest war, in history." (Hughes 2014)

184 The US Energy Information Administration lists the following major energy sources for electricity generation in the US: petroleum, coal, natural gas, nuclear, hydro, biomass, geothermal, solar, and wind. (U.S. Energy Information Administration 2015)

185 "The Persians used incendiary arrows wrapped in oil-soaked fibres [sic] at the siege of Athens in 480 BCE." (Atwater 2013)

"Although no authentic record is available, coal from the Fushun mine in northeastern China may have been employed to smelt copper as early as 1000 BCE. Stones used as fuel were said to have been produced in China during the Han dynasty (206 BCE–220 CE). . . . Coal cinders found among Roman ruins in England suggest that the Romans were familiar with its use before 400 CE." (Kopp 2014)

"The use of natural gas was mentioned in China about 900 BCE." (Carruthers 2014)

"The Greeks used water wheels for grinding wheat into flour more than 2,000 years ago." (Energy.gov 2004)

"Here we show that . . . intact sediments at the site of Wonderwerk Cave, Northern Cape province, South Africa, provide unambiguous evidence—in the form of burned bone and ashed plant remains—that burning took place in the cave during the early Acheulean occupation, approximately 1.0 Ma. To the best of our knowledge, this is the earliest secure evidence for burning in an archaeological context." (Berna et al. 2012)

"There is evidence that Native Americans used geothermal energy for cooking as early as 10,000 years ago." (Lund 2015)

"Depictions of cloth sails appear in predynastic (*c.* 3300 BC) Egyptian art, and ships from other early Mediterranean civilizations were equipped with sails." (The Editors of Encyclopædia Britannica 2014)

Aristophanes mentions burning glasses as early as 417 BC in *The Clouds*: "Soc. Do you mean the burning-glass?" (Aristophanes, 417 B.C.)

186 "Pentaerythritol tetranitrate does appear to be among the most powerful chemical explosives known, despite being only a small factor more effective than gunpowder. This appears to reflect fundamental limits on the energy density of chemical explosives. Once these limits were surpassed, attainable explosive densities increased rapidly, at first discontinuously and subsequently extremely quickly." (Grace and Christiano 2014)

187 "For the first time ever, a government advisory board is asking scientific journals not to publish details of certain biomedical experiments, for fear that the information could be used by terrorists to create deadly viruses and touch off epidemics." (Grady and Broad 2011)

188 "The extraordinary coincidence that history's most dangerous scientific secret appeared at the moment history's greatest war began made possible this unique case of scientific self-censorship." (Weart 1976, 30)

189 "Tens of thousands of letters, e-mail messages and faxes arrive at the White House every day." (Parker 2009)

190 "A meeting on May 23 brought all the program leaders together with Conant to decide which of several methods of making a bomb should be moved on to the pilot-plant and industrial engineering stages. The centrifuge, gaseous barrier diffusion, electromagnetic and graphite or heavy-water plutonium-pile approaches all looked equally promising. Given war-time scarcities and budget priorities, which should be advanced? Conant used an arms-race argument to identify the point of decision:

> While all five methods now appear to be about equally promising, clearly the time of production of a dozen bombs by the five routes will certainly not be the same but might vary by six months or a year because of unforeseen delays. Therefore, if one discards one or two or three of the methods now, one may be betting on the slower horse unconsciously. To my mind the decision as to how "all out" the effort should be might well turn on the military appraisal of what would occur if either side had a dozen or two bombs before the other.

"To that point Conant reviewed the evidence for a German bomb program, including new indications of espionage activity: information from the British that the Germans had a ton of heavy water; Peter Debye's report when he arrived in the United States eighteen months earlier that his colleagues at the KWI were hard at work; and 'the recently intercepted instruction to their agents in this country [that] shows they are interested in what we are doing.' Conant thought this last evidence the best. 'If they are hard at work, they cannot be far behind since they started in 1939 with the same initial facts as the British and ourselves. There are still plenty of competent scientists left in Germany. They may be ahead of us by as much as a year, but hardly more.'

"If time, not money, was the crucial issue—in Conant's words, 'if the possession of the new weapon in sufficient quantities would be a determining factor in the war'—then—'three months' delay might be fatal.' It followed that all five methods should be pushed at once, even though 'to embark on this Napoleonic approach to the problem would require the commitment of perhaps $500,000,000 and quite a mess of machinery.' " (Rhodes 1971, 406–07)

191 In summary, "present opinion indicates that successful use is possible, and that this would be very important and might be determining in the war effort. It is also true that if the enemy arrived at results first it would be an exceedingly serious matter. The best estimate indicates completion in 1944, if every effort is made to expedite [said Vannevar Bush in 1943]." (Rhodes 1971, 406)

192 "The Hungarian Eugene Wigner, for example, was so worried about a German bomb that in 1942, when he was working at the Met Lab at the University of Chicago designing the first production reactor for breeding plutonium, he estimated that if the Germans had begun work promptly after the Hahn-Strassmann discovery, then they could have a bomb in hand by Christmas. Logically, he thought, assuming the Germans knew about the Met Lab?s work, the likeliest target for that first hypothetical German bomb would be the University of Chicago. Wigner took the possibility so seriously that he moved his family out of the city." (Rhodes and Grace 2015, 5)

193 "If time, not money, was the crucial issue—in Conant's words, 'if the possession of the new weapon in sufficient quantities would be a determining factor in the war'—then—'three months' delay might be fatal.' It followed that all five methods should be pushed at once, even though 'to embark on this Napoleonic approach to the problem would require the commitment of perhaps $500,000,000 and quite a mess of machinery.' " (Rhodes 1971, 407)

194 "Uranium-235, the essential fissionable component of the postulated bomb, cannot be separated from its natural companion, the much more abundant uranium-238, by chemical means; the atoms

of these respective isotopes must rather be separated from each other by physical means. Several physical methods to do this were intensively explored, and two were chosen—the electromagnetic process developed at the University of California at Berkeley under Ernest Orlando Lawrence and the diffusion process developed under Urey at Columbia University. Both of these processes, and particularly the diffusion method, required large, complex facilities and huge amounts of electric power to produce even small amounts of separated uranium-235. Philip Hauge Abelson developed a third method called thermal diffusion, which was also used for a time to effect a preliminary separation. These methods were put into production at a 70-square-mile (180-square-km) tract near Knoxville, Tennessee, originally known as the Clinton Engineer Works, later as Oak Ridge." (The Editors of Encyclopædia Britannica 2015)

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196 ". . . their best production was around 15 percent. Groves counted on improvements and forged ahead.

"He had to begin building before he knew precisely what to build." (Rhodes 1971, 490)

197 "Looking back to the end of 1942 and the early months of 1943 . . . The Americans, having succeeded in producing the first chain reaction in a uranium pile, and seeing the atom bomb as a definite possibility, were certain that the Germans must know as much and more. After all, they reasoned, it was a German scientist, Oto Hahn, who had first discovered the principle of fission and another German had published the first paper on the theory of the chain reacting pile. They had begun their uranium research two years before ours. And especially, and above all, everyone knew that German science was superior to ours. The Germans were just as positve that their science was superior to ours. To be sure, they were still groping in the dark, but, they figured, if they were in the dark where must the Americans be? . . . The thought of German superiority drove [American scientists] almost to panic. . ." (Goudsmit 1996)

198 See endnote 18.

199 "By this time the Hungarians at least believed they saw major humanitarian benefit inherent in what Eugene Wigner would describe in retrospect as 'a horrible military weapon,' explaining:

> Although none of us spoke much about it to the authorities [during this early period]—they considered us dreamers enough as it was—we did hope for another effect of the development of atomic weapons in addition to the warding off of imminent disaster. We realized that, should atomic weapons be developed, no two nations would be able to live in peace with each other unless their military forces were controlled by a common higher authority. We expected that these controls, if they were effective enough to abolish atomic warfare, would be effective enough to abolish also all other forms of war. This hope was almost as strong a spur to our endeavors as was our fear of becoming the victims of the enemy's atomic bombings." (Rhodes 1971, 308)

200 See endnote 100.

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possibility, were certain that the Germans must know as much and more. After all, they reasoned, it was a German scientist, Oto Hahn, who had first discovered the principle of fission and another German had published the first paper on the theory of the chain reacting pile. They had begun their uranium research two years before ours. And especially, and above all, everyone knew that German science was superior to ours. The Germans were just as positive that their science was superior to ours. To be sure, they were still groping in the dark, but, they figured, if they were in the dark where must the Americans be? . . . The thought of German superiority drove [American scientists] almost to panic. . ." (Goudsmit 1996)

202 "The fear was so real that the scientists were even sure of the place and the date of Hitler's supposed radioactive attack. The Germans must know, they thought, that Chicago was at that time the heart of our atom bomb research. Hitler, loving dramatic action, would choose Christmas day to drop radioactive materials on that city. Some of the men on the project were so worried they sent their families to the country." (Goudsmit 1996)

203 "The Hungarian Eugene Wigner, for example, was so worried about a German bomb that in 1942, when he was working at the Met Lab at the University of Chicago designing the first production reactor for breeding plutonium, he estimated that if the Germans had begun work promptly after the Hahn-Strassmann discovery, then they could have a bomb in hand by Christmas. Logically, he thought, assuming the Germans knew about the Met Lab's work, the likeliest target for that first hypothetical German bomb would be the University of Chicago. Wigner took the possibility so seriously that he moved his family out of the city." (Rhodes and Grace 2015, 5)

204 "More ominously, two initiatives originated simultaneously in Germany as a result of the French report. A physicist at Gottingen alerted the Reich Ministry of Education. That led to a secret conference in Berlin on April 29, which led in turn to a research program, a ban on uranium exports and provision for supplies of radium from the Czechoslovakian mines at Joachimsthal. (Otto Hahn was invited to the conference but arranged to be elsewhere.)" (Rhodes 1971, 297)

205 "On April 24, 1939, the Hamburg physical chemist Paul Harteck, . . . and his assistant Wilhelm Groth, wrote a letter to Erich Schumann, who was the head of the weapons research office in Berlin of German Army Ordnance . . . of the War Office. The letter outlined possible applications of nuclear fission, which had been discovered a few months earlier, to weapons. . . 'We take the liberty of calling to your attention the newest developments in nuclear physics, which, in our opinion, will probably make it possible to produce an explosive many orders of magnitude more powerful than the conventional ones,' Harteck and Groth wrote." (Bernstein 1996)

206 "The Harteck letter reached Kurt Diebner, a competent nuclear physicist stuck unhappily in the Wehrmacht's ordnance department studying high explosives. Diebner carried it to Hans Geiger. Geiger recommended pursuing the research. The War Office agreed." (Rhodes 1971, 297)

207 "Auer, the thorium specialists, purveyors of gas mantles and radioactive toothpaste, delivered the first ton of pure uranium oxide processed from Joachimsthal ores to the War Office in January 1940. German uranium research was thriving." (Rhodes 1971, 326)

208 "This is actually a very important point, and one which shines light onto a lot of other questions regarding nuclear weapons. For example, one of the questions that people ask me again and again is how close the Germans were to getting an atomic bomb. The answer is, more or less, not very close at all. Why not? Because even if their scientific understanding was not too far away—which it was not, even though they were wrong about several things and behind on several others—they never

came close to the stage that would be necessary to turn it into an industrial production program, as opposed to just a laboratory understanding. That sheer fact is much more important than whether Heisenberg fully understood the nature of chain reactions or anything like that." (Wellerstein 2013)

209 "Such a program would have been beyond Germany's industrial capacity in the midst of a full-scale war. But the main reason the Germans didn't get beyond laboratory-scale research seems to have been that Albert Speer couldn't convince Hitler that it was important, or even possible. Lacking scientific literacy, Hitler was more interested in rockets than in atomic bombs even though his rockets would have needed atomic warheads to be effective. The overconfident Hitler also assumed Germany would defeat Britain and the Soviet Union by 1943, so there was a standing rule that no new technology should be brought under development if it would take more than 18 months to complete." (Rhodes and Grace 2015, 6)

210 "German scientists—who seemed to have been an academically rigid bunch—envisioned a more cautious, longer-term project where they would step things up one scale at a time: build a small reactor and then the next larger and then the next larger and slowly get to a bomb. In the end, they never got close. By the end of the war, they were experimenting with a half-scale, sub-critical reactor. For full criticality—for actual operation—it would have had to be doubled in size." (Rhodes and Grace 2015, 6)

211 "One of the scientists pointed out that, in order to get to the bomb, you had to go through an intermediate stage that, while it proved that there was such a thing as a chain reaction, was rather expensive. Basically, you built a nuclear reactor and demonstrated a chain reaction that way. So in order to get to the bomb, you have to spend a lot of money on something that may or may not work and doesn't look at all like a bomb. Therefore the scientists had to trust the governments during this intermediate period and the governments had to trust their scientists.

"Hitler didn't trust anybody and Stalin didn't trust his scientists, particularly since they were Jews and he was at least as bad an anti-Semite as Hitler. Later, these discoveries would be clear and logical but, at that time, these human factors got in the way. Roosevelt trusted the scientists, and the scientists trusted Roosevelt, and that was an important reason why the U.S. was first to the bomb." (Rhodes and Grace 2015, 11)

212 "Most historical accounts present Szilárd as an unambiguous visionary and the Einstein letter as the starting point of the Manhattan Project. My approach attempts to be a bit more historical about these matters, looking at them in the context of their own time and not 'smoothing' the narrative by removing the errors, hiccups, missteps, and so on. Amongst serious historians of science and serious historians of physics I do not think my account would be terribly controversial, though my interpretations are stronger than one usually finds in the historical narratives.

"The problem is that much of the scholarship in this field is easily drawn into the drama of emphasizing the role of Einstein, Szilárd, the individual scientists, etc. It's more fun to have Szilárd be this great visionary who changed the direction of history and so forth. And he's a fun character to write about. So it is easy to make him take center stage. The fact that he also later became an opponent of the bomb and the arms race makes it an appealing narrative as well—the scientist who regrets his actions, etc. The fact that Einstein is always so front-and-center despite his really quite minimal role in this story (and the quite minimal role of his science in it as well—$E = mc^2$ matters a lot less to atomic bomb production than people generally realize) is further testament to the power of a good yarn.

"But if you start drilling down into the narrative, beyond the more pop-history accounts, you find that Szilárd's patent and his letter just weren't as influential in the long run. I think Szilárd's most important role here is his self-censorship campaign, which did get the scientists thinking concretely about secrecy and security and did result in the creation of what became very long-term institutions of secrecy. I think it is fair to say that among serious historians of this topic, the real emphasis is on the importance of the *institutions* that were being created . . . both Szilárd's secrecy institution and the importance of someone like Vannevar Bush, who was probably the most important single person with regards to getting the Manhattan Project started, since he was the only person who really had the power and will to move the program from a small experimental investigation into a full bomb-production program. Where more popular accounts of the bomb focus on individual ideas or people making the key contributions, the more scholarly, academic accounts focus on how the big systems of production that got the actual results were created." (Wellerstein and Grace 2015, 6)

213 ". . . what I read already impressed me with the author's knowledge of much of the history of the science which led to the development of nuclear energy and nuclear bombs and of the personalities which contributed in the U.S. to the development of these. I was particularly impressed by his realization of the importance of Leó Szilárd's contributions which are almost always underestimated but which he fully realizes and perhaps even overestimates. I hope the book will find a wide readership." Eugene P. Wigner, Nobel Laureate for Physics, 1963 (quoted in Rhodes 1971, 8)

214 "Rhodes doesn't think Szilárd's patent was particularly useful. Nothing eventuated from it. Burying it in the Admiralty files without pursuing further research was effectively a dead end. It was more Szilárd's conversations with his fellow émigré scientists—in the UK and then the US—that slowly got the ball rolling. It wasn't even Einstein's famous letter to Roosevelt. It was more the later efforts by the émigré scientists to convince the British and American governments that there was a real possibility of a radically different new weapon. . . . Szilárd's efforts were real and they did ultimately prevail over the general incomprehension of anything so absolutely radical as this new discovery was. But Rhodes doesn't think the patent got it very far." (Rhodes and Grace 2015, 2)

215 "I don't think the patent ended up being very useful in the end, except so much as it helped keep Szilárd interested in the topic, which put him in a good position once fission was discovered. I don't think it did anything for him or really did much for getting authorities interested in the topic. The 'unknowns' were bigger than the 'knowns.' " (Wellerstein and Grace 2015, 2)

216 "But once nuclear fission was announced, Szilárd was primed better than any other to realize that this nuclear reaction might actually be the candidate reaction he was looking for, since he had already been thinking about this problem for five years. He was able to quickly see which of his ideas had been good ones, and which had been bad ones." (Wellerstein and Grace 2015, 3)

217 "When Szilárd heard the announcement of fission he, in his recollection, immediately realized it could be a candidate for an exponential, neutron-based nuclear chain reaction. What he needed to know to be sure was whether the number of neutrons produced per fission reaction was more than one on average. He cabled numerous scientists and asked them to not publish on this possibility or the possibility of chain reactions. In the meantime he set up a small experimental apparatus to see if indeed 'secondary' neutrons were produced from fission reactions—they were." (Wellerstein and Grace 2015, 4)

218 "Wigner told me of Hahn's discovery. Hahn found that uranium breaks into two parts when it absorbs a neutron. . . . When I heard this I immediately saw that these fragments, being heavier than corresponds to their charge, must emit neutrons, and if enough neutrons are emitted . . . then it should be, of course, possible to sustain a chain reaction. All the things which H. G. Wells predicted appeared suddenly real to me." Leó Szilárd (quoted in Rhodes 1971, 263)

219 "A short, round, exuberant Hungarian, Szilárd in 1939 had neither a job nor a home. But he was uniquely qualified to face the issues of nuclear energy and secrecy because for over five years he—and he alone—had been concentrating on these problems." (Weart 1976, 23)

220 "Naturwissenschaften reached Paris about January 16. One of Frederic Jolot's associates recalls that 'in a rather moving meeting [Joliot] made a report on this result to Madame Joliot and myself after having locked himself in for a few days and not talked to anybody.' The Joliot-Curies were once again appalled to find they had barely missed a major discovery. In the next few days Joliot independently deduced the large energy release and considered the possibility of a chain reaction, as Szilárd had thought he might." (Rhodes 1971, 271)

221 "Szilárd's 1934 patent is easily available these days. . . He filed the application first in late June 1934, updated it in early July, and finalized it by April 1935. The UK Patent Office accepted it as valid in late March 1936, but it was 'withheld from publication' at Szilárd's request under Section 30 of the Patent and Designs Act. It was eventually published in late September 1949, 15 years after it had been originally applied for." (Wellerstein 2014b)

222 "The same week a young physicist working at Hamburg, Paul Harteck, wrote a letter jointly with his assistant to the German War Office:

> We take the liberty of calling to your attention the newest development in nuclear physics, which, in our opinion, will probably make it possible to produce an explosive many orders of magnitude more powerful than the conventional ones. . . . That country which first makes use of it has an unsurpassable advantage over the others." (Rhodes 1971, 297)

223 See endnote 100.

224 *"If Szilárd had managed to interest the military with his patent, and get funding for such research, would the research he wanted to do have been useful? e.g. Would it have found fission earlier? Or was it on the wrong track because he was imagining a metastability related chain reaction?"*

"Szilárd's research plan involved shooting neutrons at a lot of materials and seeing if any of them created a chain reaction. It wasn't a terrible idea, but he didn't really know how the chain reaction would work, and wasn't imagining fission would be the mechanism. Because Szilárd wasn't thinking about fission, he was starting on the wrong end of the periodic table (he wanted to start with iridium, element 77, some 15 elements from uranium, element 92). I don't know how he would have done if his research was funded; he was not an especially clever experimentalist—he was better with ideas and theory. It is worth noting that by 1935, Fermi had already found that slowed (moderated) neutrons produced interesting radioactive byproducts in uranium. Neither he, nor Szilárd, nor pretty much anybody else until Hahn and Meitner, realized that fission was going on, or that neutrons might be a by-product of uranium bombardment by neutrons." (Wellerstein and Grace 2015, 2)

225 "Rhodes thinks Szilárd's early secrecy efforts didn't make that much difference. There was nothing going on in any official level in the direction of nuclear weapons until well after the discovery of fission in 1938. The British Admiralty wasn't interested. The Soviet Union wasn't interested, if it even knew. There was work in nuclear physics going on in the Soviet Union at the time but it was slowed down by Stalin's purges in 1937 and 1938 and Soviet scientists were not yet at the point where they were thinking about chain reactions." (Rhodes and Grace 2015, 11–12)

226 "In the spring of 1939 one group [Szilárd's], foreseeing the unprecedented power of nuclear weapons, made a concerted attempt to restrict knowledge of chain reactions. But it was not until over a year later that censorship—imposed by the community of physicists on itself—became fairly complete." (Weart 1976, 23)

227 "His self-censorship campaign had a modest amount of success (he convinced US, UK, and Danish scientists not to publish on chain reactions) but he could not convince the French scientist Frederic Joliot. Joliot published the number of secondary neutrons detected (he found that uranium gave off 3.5 neutrons on average, which is 1 larger than the currently accepted number of 2.5) and commented that chain reactions seemed possible." (Wellerstein and Grace 2015, 4)

228 "This campaign, led by Leó Szilárd, is generally remembered as a failure: Frédéric Joliot-Curie completely blew off Szilárd's concerns and published a very optimistic assessment of the possibility of chain reactions, which did lead scientists in half a dozen countries to urge their governments to start nuclear weapons programs." (Wellerstein 2011b)

229 "Szilárd had already taken the single step that was entirely within his power: He withheld from publication a paper of his own. This paper, completed in February 1940, contained elaborate calculations of the characteristics of a nuclear reactor and concluded that there was a strong possibility of making one work. Had the article been published, it surely would have been a great stimulus to nuclear reactor work in various countries. But when Szilárd sent it to the *Physical Review* he requested that printing be delayed until further notice." (Weart 1976, 29)

230 "The previous fall Szilárd had assaulted Fermi with another secrecy appeal:

> When [Fermi] finished his [carbon absorption] measurement the question of secrecy again came up. I went to his office and said that now that we had this value perhaps the value ought not to be made public. And this time Fermi really lost his temper; he really thought this was absurd. There was nothing much more I could say, but next time when I dropped in his office he told me that Pegram had come to see him, and Pegram thought that this value should not be published. From that point the secrecy was on.

"It was on just in time to prevent German researchers from pursuing a cheap, effective moderator." (Rhodes 1971, 345)

231 "This decision came late, but still in time: If the value for the carbon cross section had been published, the course of World War II might conceivably have been changed. For German scientists, using experiments they carried out later in 1940, wrongly concluded that carbon had a substantial neutron-absorption cross section. From that point on they abandoned carbon as a moderator and attempted to use the extremely rare isotope deuterium, which they never managed to get enough of. Soviet scientists too at first did not seriously consider carbon as a moderator. The French scientists were also committed to deuterium. They escaped to England when France fell to the Germans, and thereafter the British followed their lead in matters of reactors, regarding carbon as an unlikely choice. Anderson and Fermi's work could have put all these groups on a different track." (Weart 1976, 29)

232 "This was not the only hole in the dike that had to be plugged. In late May, Louis Turner at Princeton sent Szilárd a copy of a paper on "Atomic Energy from U^{238}." In this paper Turner pointed out that if U^{238} were bombarded by neutrons, as would happen in a nuclear reactor, a series of steps would give rise to a new element. This he predicted to be fissionable—it was the element later named plutonium.

Although Turner had not realized it, he had written the prescription for the easiest route to building an atomic bomb.

"Szilárd wrote back at once to say that his own paper was secret, implying that there was an official move underway to withhold papers. He persuaded Turner to write the *Physical Review* and delay publication. It was well he did so: Turner's paper could have been an essential clue for the Germans and others." (Weart 1976, 29)

233 See endnote 45.

234 "Breit had known Szilárd and Wigner for years, and was awakened to the secrecy problem through long conversations with them." (Weart 1976, 30)

235 "However, in my work I argue that we should really be more surprised at how successful Szilárd was. The idea of chain reactions actually being a short-term (that is, World War II-sized) problem was pretty much science fiction at that point, and Szilárd did manage to get real checks on publication put in place on physics journals in the US, UK, and Denmark. This system continued even after Joliot-Curie published, and became a foundational block for the monitoring of scientific work in the area. If we measure the success of the system by one result (did people know that more than one neutron was released per fission of U-235?) then the censorship was a failure; if we measure success by the creation of a new organization and new interest in the problem, despite it appearing to be a very long-term threat, then it was something of a success." (Wellerstein 2011b)

236 See endnote 39.

237 "I think Szilárd's most important role here is his self-censorship campaign, which did get the scientists thinking concretely about secrecy and security and did result in the creation of what became very long-term institutions of secrecy. I think it is fair to say that among serious historians of this topic, the real emphasis is on the importance of the *institutions* that were being created—both Szilárd's secrecy institution and the importance of someone like Vannevar Bush, who was probably the most important single person with regards to getting the Manhattan Project started, since he was the only person who really had the power and will to move the program from a small experimental investigation into a full bomb-production program." (Wellerstein and Grace 2015, 6)

238 "The main trajectory is that the secrecy started as 'self-censorship' (individuals) and then something like 'community censorship' (physics journals). It transitioned under the NDRC/OSRD period into 'civilian secrecy' . . . government secrecy but under entirely civilian auspices. When the Army took over it became 'military secrecy' of the sort we are familiar with. The transition between 'self-censorship' and 'military secrecy' was gradual between 1939 and 1943, and reflected increasing institutionalization of the bomb program itself as it moved through its various stages." (Wellerstein and Grace 2015, 7)

239 See endnote 36.

240 See endnote 37.

241 "We tried to convince [Bohr]," Teller writes, "that we should go ahead with fission research but we should not publish the results. We should keep the results secret, lest the Nazis learn of them and produce nuclear explosions first. Bohr insisted that we would never succeed in producing nuclear energy and he also insisted that secrecy must never be introduced into physics." (Rhodes 1971, 294)

242 "More crucial for Bohr was the issue of secrecy. He had worked for decades to shape physics into an international community, a model within its limited franchise of what a peaceful, politically united world might be. Openness was its fragile, essential charter, an operational necessity, as freedom of speech is an operational necessity to a democracy. Complete openness enforced absolute honesty: the scientist reported all his results, favorable and unfavorable, where all could read them, making possible the ongoing correction of error. Secrecy would revoke that charter and subordinate science as a political system—Polanyi's 'republic'—to the anarchic competition of the nation-states. . . . If U235 could be separated easily from U238, that misfortune might be cause for temporary compromise with principle in the interest of survival. Bohr thought the technology looked not even remotely accessible. The meeting dragged on inconclusively past midnight." (Rhodes 1971, 295)

243 "And [Bohr] thought that at any rate it would be difficult if not impossible to keep truly important results secret from military experts . . . the matter was already public." (Weart 1976, 26)

244 "Finally, [the leading British physicists] found the idea of scientific secrecy entirely alien. Even those scientists who felt most keenly the responsibility of scientists for the consequences of their discoveries traditionally felt that secrecy is abhorrent and that interference with the normal process of open criticism would not only impede scientific progress but pervert it." (Weart 1976, 23)

245 "Szilárd's arguments were forceful, but others at Columbia replied that an attempt to restrict publication was both futile and an undesirable breach of scientific custom." (Weart 1976, 26)

246 "As Szilárd recalled the meeting, he and Teller pressed for keeping their work secret, but Fermi was repelled by this idea, holding that publication was basic to scientific morality." (Weart 1976, 25)

247 "For one thing, Joliot believed strongly in the international fellowship of scientists, and in principle had little sympathy with secrecy." (Weart 1976, 28)

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