EXAMINATION OF THE UNITED STATES DOMESTIC FUSION PROGRAM

By

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ABSTRACT

Fusion has been "forty years away", that is, forty years to implementation, ever since the idea of harnessing energy from a fusion reactor was conceived in the 1950s. In reality, however, it has yet to become a viable energy source. Fusion's promise and failure are both investigated by reviewing the history of the United States domestic fusion program and comparing technological forecasting by fusion scientists, fusion program budget plans, and fusion program budget history. It is evident that delays in progress were due to both technologic and economic setbacks. In order for the US to become a leader in fusion energy, it must continue supporting domestic fusion experiments while maintaining involvement in ITER.

Thesis Supervisor: Dennis Whyte
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List of Terms and Abbreviations

AEC	US Atomic Energy Commission
ATC	Adiabatic Toroidal Compressor
C-Mod	Alcator C-Mod Tokamak at MIT
CTR	Controlled Thermonuclear Research
DIII-D	Doublet III D
DOE	US Department of Energy
DT	Deuterium-Tritium Plasma
EDA	Engineering Design Activity
ERDA	US Energy Research and Development Administration
FESAC	US Fusion Energy Sciences Advisory Committee
FIRE	Fusion Ignition Research Experiment
FY	US Fiscal Year
ICF	Inertial Confinement Fusion
IFE	Inertial Fusion Energy
INTOR	International Tokamak Reactor
ISX	Impurity Studies Experiment
ITER	International Thermonuclear Experimental Reactor
JCAE	US Joint (House-Senate) Committee on Atomic Energy
JET	Joint European Torus
KMS	Keeve M. Siegel
LDX	Levitated Dipole Experiment
LLL	Lawrence Livermore Laboratory
MFE	Magnetic Fusion Energy
MHD	Magnetohydrodynamic
NACA	National Advisory Committee for Aeronautics
NASA	National Aeronautics and Space Administration
NIF	National Ignition Facility
NNSA	National Nuclear Security Administration (a part of DOE)
NSTX	National Spherical Torus Experiment
OFES	Office of Fusion Energy Sciences
OPEC	Organization of Petroleum Exporting Countries
Ormak	Oak Ridge Tokamak
OST	US White House Office of Science and Technology
PDX	Poloidal Divertor Experiment
PLT	Proto-Large Torus
PPPL	Princeton Plasma Physics Laboratory
R&D	Research and Development
ST	Spherical Tokamak
TFTR	Tokamak Fusion Test Reactor
UFA	University Fusion Associates
ZETA	Zero-Energy Thermonuclear Assembly

1. Introduction

In nuclear science, there are two main ways of obtaining net energy: fission and fusion. Fission, the primary method used today, is the splitting apart of large nuclei, such as uranium, into two smaller atoms, called fission products, which releases a substantial amount of energy. Along with fission products, this splitting produces neutrons, which can go on to split more large atoms and produce more energy. Fusion, on the other hand, is the combining of two smaller atoms, such as hydrogen, to produce heavier atoms and energy. This process occurs naturally in the sun, using hydrogen as fuel and producing mainly helium as waste. Fusion reactions can release more energy than fission without producing radioactive byproducts (as well as being a non-proliferation source of energy that is pollution-free and limitless), but this reaction has not been commercially developed because of daunting engineering and physics problems.

Fusion is a point of contention for many nuclear scientists in the world today. The tokamak, a toroidal device used to contain plasma, is presently the leading design for magnetic fusion. Some, such as Ken Tomabechi, believe that "Use of thermonuclear fusion promises a great potential to contribute to the future world" [1] while others, such as Robert L. Hirsch, argue that "Tokamak fusion will almost certainly fail to become a viable, commercial electric power system" [2]. The world has been researching the subject for decades without much to show for it. Why have fusion reactors not become the new, clean energy source powering the globe?

The biggest problem is that scientists cannot figure out how to get more energy out of a fusion reaction than is required to sustain the reaction. Many fusion researchers, however, continue to propagate the idea that the technology is "only forty years away" [6]. This misleading promise has caused contention in the US government as to whether it is worth it to continue funding fusion [18]. The failure to progress towards viable fusion energy plants and loss of funding has led to cuts in the United States' domestic research program on fusion science [2].

Goals of various fusion projects within the United States will be examined to see whether they were achieved on time—or at all. This historical analysis will reveal whether fusion research is facing problems merely due to technological issues or whether economic issues are contributing. From the historical analysis, the current state of the United States' involvement in the ITER with regards to the domestic fusion program will be assessed, and a proposal for the future will be made.

2. Historical Overview

2.1 The Beginning of Fusion: Early to Mid-20th Century

The origin of fusion is a natural progression in the history of nuclear physics research. It begins in 1905 when Albert Einstein first published his famous equation $E=mc^2$. This equation showed the potential of extracting energy through nuclear means rather than by burning fuels; in fact, the nuclear energy that could be produced by fusion is millions of times more powerful than burning an equivalent mass of organic material [3]. Fission, which is the splitting of heavy elements like uranium, was the first form of nuclear energy that was utilized. Fusion, which is the combining of light elements like hydrogen, was also discovered and found to be about eight times more energetic per unit mass converted than fission but also correspondingly more difficult to achieve in practice [3].

Sir Ernest Rutherford was an early pioneer in nuclear physics research. In 1908 he won the Nobel Prize for his experimentation revealing atomic structure and showing how radioactive elements transform into other elements of the periodic table. In 1919, Rutherford performed experiments showing how the collision of lighter elements could produce heavier elements (this process later came to be known as "fusion"). In the same year, Francis Aston demonstrated the existence of different "isotopes" of the same element using a mass spectrometer, which he had invented and later received a Nobel Prize for. In addition, he realized the then-astonishing fact that the mass of a helium nucleus was less than the sum of the hydrogen nuclei of which it was composed (this is the basis for why fusion produces so much energy). In 1934, Rutherford and his colleagues demonstrated the fusion of deuterium and deuterium to form helium using a Cockcroft–Walton particle accelerator [3].

The first recorded interest in building a fusion experiment in the United States was in 1938 by Kantrowitz and Jacobs at the Langley lab of the National Advisory Committee for Aeronautics (NACA, the predecessor agency of NASA). It was reported that "they built a simple torus (donut-shaped) vessel with magnet coils wrapped around it to produce a 'magnetic bottle' and introduced about 150 W of power from a radio transmitter, hoping to heat the hydrogen gas to a million degrees. The experiment failed to produce the desired result and was abandoned" [3]. Fusion experiments were put on hold in favor of fission, which was much more promising at the time because of the chain reactions that were appealing for their destructive nature.

When World War II began, nuclear research shifted from producing energy to producing weapons. Fission was the main focus, although fusion was brought up from time to time, mainly in the context of how to build a hydrogen bomb. The first tangible steps towards fusion energy were taken in the United Kingdom in 1946 when George Thomson and Moses Blackman at Imperial College in London registered a patent for a thermonuclear power plant. The design superficially resembled present-day fusion experiments but it is clear that this early idea would not have worked [4]. Their initial design was too simplistic and did not take into account many of the plasma instabilities we know about today.

One bizarre incident that seems to have spurred an interest in fusion research was an article published in the *New York Times* in 1951. It featured an announcement from Argentine President Peron that an Austrian physicist working in Argentina had made a breakthrough, and they had made a working fusion power plant. No more details were ever revealed, and the announcement of the fusion plant was later found to be a fake. The publicity, however, managed to draw the attention of scientists and politicians in the USA, UK, and USSR, and serious efforts to investigate the possibilities were launched [3, 4].

At this point, many experiments began in the United States at around the same time. An ambitious classified program was launched by the Atomic Energy Commission (AEC) in 1952 and 1953 to fund fusion energy projects [5]. Three ideas were funded: the Stellarator, conceived by Lyman Spitzer at Princeton; the Perhapsatron, conceived by Jim Tuck in Los Alamos; and mirrors, proposed by Richard Post at Livermore.

Lyman Spitzer, an astrophysicist, quickly got to work after hearing the claims of a fusion power plant in Argentina. In July 1951, Spitzer received a grant from the AEC to begin work on the Stellarator at Princeton, which later became the Princeton Plasma Physics Laboratory [6, 7]. The Stellarator was based around a simple concept that occurred to Spitzer: since plasma is strongly affected by electric and magnetic fields, it should be possible to put it in a "bottle". This bottle would have to be in a certain shape, however, to contain the plasma effectively. Plasma is a very high temperature state of matter composed of free-floating electrons and protons. When moving through a tube, if the plasma was exposed to a magnetic field in the proper orientation, the charged particles would be forced to spiral down the tube in tight helices, which would keep the plasma confined. The trick to keeping the plasma from "spilling out" of the tube was to wrap it into a torus-shape. This torus-shaped bottle would still be leaky, but Spitzer came up with the idea of having a figure eight shape to minimize the leak.

Jim Tuck was an Englishman who worked at Los Alamos during the war and had returned there from Oxford [5]. He brought the idea of a "pinch" fusion device back from England and built one in Los Alamos in 1952. He called it the Perhapsatron because, he said, "perhaps it will work and perhaps it will not" [3]. The pinch effect is a phenomenon that had already been studied in investigations of the passage of electric currents through conducting gases. The idea is that when a strong enough current is discharged through the gas, magnetic forces form in a circular pattern around the current and react back upon the gas to pinch it into a thin filament. Plasma is a conducting gas so Tuck believed it would be possible to contain it using this pinch effect, since it keeps the plasma from hitting the walls of its container [8].

Richard Post first worked at Berkeley but then moved to Livermore, where he began research on magnetic mirrors in the early 1950s. Instead of wrestling with the problems of curving the plasma's path, Post decided to use a straight tube and tweak the magnetic fields in order to cap the ends. Strong magnetic fields at the ends of the tube and slightly weaker magnetic fields at the center would create barriers that would behave almost like a mirror. Some—not all—of the plasma streaming to the end of the tube would be reflected back inside [6].

Within months of Argentina's announcement, not one, but three ideas had been proposed to contain plasma, the first step towards a fusion power plant. The Stellarator, the Perhapsatron, and the magnetic mirror all showed great promise; at least on paper, they were all able to contain plasma in a magnetic bottle. The AEC decided to pursue all of these ideas. By the time Argentina's claim of making a working fusion reactor was revealed as a fraud, the United States had consolidated these three efforts into one project: Project Sherwood [6]. Project Sherwood's initial funding was modest, a few hundred thousand dollars per year.

Project Sherwood started off ambitious and optimistic. Spitzer believed his Stellarator would produce 150 million watts of power and Tuck's Perhapsatron, being of a simpler design, looked like it would achieve fusion sooner. The enthusiasm, however, hid a lot of difficulties and even some infighting. Spitzer and his team thought their idea was the best path to fusion energy and spent some of their AEC grant trying to disprove the Perhapsatron. They managed to prove that the pinch method had some instability, where if a pinching plasma had even the tiniest kink in it, the kink would grow and the plasma would hit the walls of its container, destroying it (this is called the kink instability).

Tuck tried to resolve this by making his pinch machines bigger, increasing the strength and speed of the pinch, which theoretically would be able to get fusion going before the kink instability destroyed it. In early 1955, when Los Alamos researchers tested this idea on their newest and biggest pinch machine, Columbus I, they ended up finding another instability. At first, they believed they were successful because every time they pinched the plasma, they saw a burst of neutrons, which is a good indicator of a fusion reaction. Researchers at Livermore were skeptical of the machine reaching the temperatures advertised, so Los Alamos physicists started taking measurements to find out exactly where the neutrons were coming from. These measurements showed that the neutrons coming out of one side of the pinch machine were more energetic than neutrons coming out of the other side of the machine, which would not happen in a thermonuclear reaction [6]. This discovery revealed the sausage instability, where if a part of the plasma is pinched a bit more than the rest of the plasma, this small pinch grows progressively more pronounced. This causes the plasma to get wasp-waisted and pinch itself off, which looks like a pair of sausages. The sausage instability creates strong electrical fields near the pinch point. The fields accelerate some nuclei in the direction of the pinch current, which strike the relatively chilly cloud of plasma and fuse, releasing neutrons [6]. This type of fusion is worthless, however, because a small amount of very hot nuclei interacting with cooler ones cannot create more energy than consumed. In order to produce energy in a fusion reactor, scientists need a hot cloud of thermalized nuclei, a thermonuclear fusion reaction.

In 1954, Edward Teller (the father of the hydrogen bomb and cofounder of Lawrence Livermore National Laboratory) figured out that plasma held in place by magnetic fields was unstable under certain conditions. Teller described it as such: "The magnetic fields behave somewhat like a collection of rubber bands: as the plasma pressure increases, they try to relieve the increasing tension by writhing. They try to snap inward and let the plasma leak out between them… Even a tiny irregularity in the magnetic field would rapidly get worse, and scientists would lose control of the plasma" [6]. This so-called Teller instability affected the Stellarator and Post's magnetic mirrors.

By the mid-1950s, all three groups in the Sherwood Project had enormous difficulties to overcome, and they spent more and more money trying to do so. The kink instability, sausage instability, and Teller instability severely halted progress on Spitzer's, Tuck's, and Post's research. They kept building bigger and more elaborate machines in an attempt to contain the unstable plasma. The few hundred thousand dollars spent in the early 1950s turned into nearly $5 million by 1955 and more than $10 million by 1957 [6]. The most expensive machine in the Sherwood Portfolio was the model-C Stellarator proposed by Spitzer, which would cost roughly $16 million to design and build [6].

Scientists still remained hopeful. In 1955, the first UN Conference on the Peaceful Uses of Atomic Energy, *Atoms for Peace*, was held in Geneva. Fusion was only mentioned in the opening address of the president of the conference, Indian physicist Homi J. Bhabha, but he made a famous prediction:

> It is well known that atomic energy can be obtained by fusion processes as in the H-bomb and there is no basic scientific knowledge in our possession today to show that it is impossible for us to obtain this energy from the fusion process in a controlled manner. The technical problems are formidable, but one should remember that it is not yet fifteen years since atomic energy was released in an atomic pile for the first time by Fermi. I venture to predict that a method will be found for liberating fusion energy in a controlled manner within the next two decades. When that happens the energy problem of the world will truly have been solved forever for the fuel will be as plentiful as the heavy hydrogen in the oceans. [5]

This prediction, while seemingly optimistic, was not far-fetched for the time. Although scientists were running into technical problems, the post-war attitude of being able to solve anything was still largely prevalent. Scientists had refined fission energy in a remarkably short amount of time and not only were nuclear powered submarines being built, but also commercial nuclear power plants were being constructed. There was no reason to doubt scientists' ability to overcome complex problems in fusion, too. In 1956, while working at Livermore, Post wrote: "It is the firm belief of many of the physicists actively engaged in controlled fusion research in this country that all of the scientific and technological problems of controlled fusion will be mastered – perhaps in the next few years" [9]. Many scientists believed that fusion power plants would be built in the near future, joining their fission power plant brothers in ridding the world of its dependence on fossil fuels. The original predictions about fusion were lofty and ambitious; it seemed only a matter of time before the world had clean energy that was "too cheap to meter". As time went on, however, the goals for fusion energy got more and more humble as scientists learned the difficulties of containing plasma.

There are two social factors that could have influenced the early optimism of fusion researchers in the US. The first factor is the postwar exuberance over the possibilities of technology; anything could be done, many felt, if only the decision was made to make it a matter of national priority. The United States was concerned with new ways of providing electrical power as well as staying technologically ahead of other nations. The most pervasive stimulus for the American fusion program was weapons research. Fusion had already been successfully used in the hydrogen bomb; commissioners in Washington believed there were chances of other, as yet undetermined, military applications. The second factor, as pointed out by Bromberg, was the lack of any scientists who commanded the breadth of scientific and engineering knowledge that underpin the invention of fusion reactors. Neither the disciplines of fusion physics and fusion engineering nor their sociological counterpart, a fusion community, existed in the early 1950s. No one person or laboratory had an overview of all the understanding necessary to build a fusion reactor, and this absence caused a tendency for the fusion scientist to underestimate the difficulties that were likely to emerge in that part of the enterprise that was outside his or her particular expertise [8].

2.2 From Disappointment to the Discovery of the Tokamak: Late 1950s through 1960s

In the 1960s, progress in fusion research seemed slow and painful. The optimism of earlier years was replaced by the realization of just how difficult it was to use magnetic fields to contain hot plasma. Hot plasmas could be violently unstable and, even when the worst instabilities were avoided or suppressed, the plasma cooled far too quickly. New theories predicted ever more threatening instabilities and loss processes.

An important theory that was discovered in 1950 but became important for fusion research around this time was hydromagnetic or magnetohydrodynamic (MHD) theory, which relied on plasma being seen as a conducting fluid. Swedish astrophysicist Hannes Alfvén published the book *Cosmical Electrodynamics*, in which he argued that, on astronomical scales, electrical forces are usually small, but that magnetic forces can have a dominant effect on fluid motion [5]. In fact, Alfvén showed that cosmical plasmas can often be considered as ideal conductors—with negligible resistivity. This led him to the concept of frozen-in magnetic fields and to his prediction of the existence of MHD waves. These are now known as Alfvén waves, and their phase velocity is called the Alfvén velocity. The idea of a magnetic field moving around with a conducting fluid—from which magnetic field lines derive an identity—was foreign to Maxwell's electromagnetic theory. It is valid only in an approximation that neglects high frequency phenomena in which the displacement current plays a role, and Alfvén stressed that the coupling can indeed be broken, also in cosmical phenomena [5]. Yet this way of looking at the dynamics of magnetic fields and plasma had turned out to be extremely fruitful.

A related concept emerging from MHD theory is magnetic pressure. Plasma can disturb a magnetic field—push it aside, bend or twist it or constrict the field lines. Conversely, the field can accelerate the plasma, shape, or confine it. For magnetic confinement, it is necessary that there is a pressure equilibrium between the plasma and the field; and for the equilibrium to be a stable one, it must be such that no small disturbance can grow and destroy the equilibrium [5]. The pressure equilibrium was very important in pinch machines.

Another problem with Tuck's Perhapsatron pinch machine design was discovered by Princeton professors Martin Schwarzschild and Martin Kruskal. When the first experiments on magnetic confinement were initiated, it had already been recognized that the pressure equilibrium could be unstable. As a first step towards a more comprehensive MHD stability theory, Schwarzschild and Kruskal described the flute instability of a plasma supported against gravity by magnetic pressure [5]. They had also contributed to the discovery of the kink and sausage instabilities. All these instabilities were producing huge setbacks for the various fusion experiments.

A small glimmer of hope came in the form of British physicist John D. Lawson's landmark 1957 paper. Lawson calculated the density, temperature, and "confinement time" required for a fusion power plant [3]. He showed that there was a minimum temperature that must be exceeded (called the ideal ignition temperature) but that a trade-off existed between the required density (n) and confinement time (τ): higher density required lower confinement time and vice versa. These requirements (on temperature, T, and the product of density and time, $n\tau$) came to be known as the "Lawson criterion" and constituted a proof of scientific feasibility for fusion researchers. With a temperature of 12-14 keV the required product of thermal pressure and confinement time is 1 MPa-s. Meeting the Lawson criterion seemed within reach to researchers at the time [5]. Another important definition gained from the Lawson criterion is the critical requirement of gain, which is called the Q_p for the plasma. Q_p is defined as fusion power divided by external power. If Q_p equals 1, that is defined as scientific breakeven. If Q_p is 5, that is called "burn", when self-heating from the produced alpha particles, which is about 1/5 of fusion power, is equal to external heating. When Q_p is infinity, there is no external power, which is the idea of the Lawson criterion. For a typical fusion reactor, the Q_p was calculated to be 30-40, which would be enough to produce electricity.

David Bohm, an American theorist working in the 1950s at Berkeley, had devised a theoretical formulation of the relationship between temperature, magnetic field strength, and the rate at which the plasma slips, or diffuses away. According to Bohm's equation, later called "Bohm diffusion", even by tightening the magnetic field, the loss of plasma energy in any magnetic fusion device would always be too great for making surplus power [9]. Bohm had placed a seemingly insurmountable roadblock on the way to fusion achieving breakeven. Increasingly, emphasis shifted from trying to achieve a quick breakthrough into developing a better understanding of the general properties of magnetized plasma by conducting more careful experiments with improved measurements [5].

One important event outside of the US that had a major effect on the fusion community was the British machine ZETA. It was a powerful pinch machine that had cost less than $1 million to build. Its name reflected the optimism of its designers; ZETA was an acronym for Zero-Energy

Thermonuclear Assembly, thermonuclear because it would achieve fusion and zero-energy because it would produce as much energy as it consumed [6]. ZETA began operation in mid-August 1957, and by August 30th the ZETA device starting producing neutrons. Earlier in the year, the British and Americans had decided to share data on fusion reactors with each other, and they were to decide jointly when and how to declassify the data and release it to the public. The American scientists were reluctant to make an announcement about ZETA because Project Sherwood had no promising achievements to present. This was around the time that *Sputnik* was launched by the Russians; America was already losing the space race, they didn't want to lose the fusion race too. Citing security issues, the US tried to delay the announcement for a year so it could be presented at the second United Nations conference in 1958. News of ZETA still got out, however, and Britain was pushing to publish its results. Finally in January 1958, the Americans gave Britain permission to publish the ZETA findings. On January 24, 1958, *Nature* featured the ZETA findings, and the British and American scientists held a joint press conference. The British scientists announced they were "90% certain" that ZETA's neutrons had come from fusion, and outlined a twenty-year research plan that would lead to fusion reactors [6]. Basil Rose, a physicist at Harwell (where ZETA was located), was skeptical; he did not believe the ZETA neutrons came from thermonuclear fusion. Rose ran the ZETA machine twice, once in its normal operating mode and once with the magnetic fields and currents reversed. The reaction should be symmetrical; if the neutrons truly came from thermonuclear fusion, they would have the same energies, no matter whether the machine was running normally or in reverse. Rose found that the energies were not equal; these were the same false neutrons that had been produced by the Columbus I machine. He published his results in *Nature* on June 14, 1958, and ZETA became a public relations disaster [6]. For years after, the hope and subsequent failure of ZETA haunted fusion scientists worldwide.

The US fusion program in the 1960s was overseen in the Congress by the Joint (House–Senate) Committee on Atomic Energy (JCAE). Many of the programs mentioned in the previous section had evolved to become more complicated, and similar ideas were being tested in a variety of labs. Stellarators at Princeton Plasma Physics Laboratory (PPPL) were observing loss rates of plasma more rapid than expected, and the reasons were not understood. In the early 1950s, Spitzer believed his small model-A and model-B Stellarators would lead quickly to a bigger, model-C machine that would serve "partly as a research facility, partly as a prototype or pilot plant for a full-scale power producing reactor" [6]. Spitzer was sure he would have a working fusion reactor by the 1960s, but setback after setback sapped his optimism. However, by the late 1950s, he viewed the $24 million model-C Stellarator then under construction "entirely as a research facility, without any regard for problems of a prototype" [6]. This was a good call by Spitzer. The ZETA scientists were eager to be on the forefront of fusion research and ended up embarrassing themselves by publishing incorrect results. Fusion scientists were reigning in the hopeful optimism of the 1950s, and, with the Bohm diffusion problem looming, it made sense to turn the model-C Stellarator into purely a research facility. Tuck's magnetic mirror at Lawrence Livermore Laboratory (LLL) likewise was struggling. Magnetic mirrors were producing less promising results than pinch machines and still suffered all the same instabilities. But where stellarators and mirror machines seemed to fail, Tuck's new pinch machine seemed to have made a bit of headway.

At Los Alamos, Tuck had developed Scylla, which was a variant on existing pinch machines, in 1957 [8]. Scylla was similar to his previous machines (the Perhapsatron and Columbus I), but its pinch was arranged slightly differently. Instead of producing an electrical current down the length of the plasma, Scylla ran a current around the circumference of the tube of plasma instead. It was just a variant on the existing pinch machines, but this small change made a big difference. Scylla was about to heat deuterium to more than ten million degrees, and scientists began to detect protons, tritium nuclei, and neutrons, which are all the expected products of deuterium-deuterium fusion [6]. They had achieved a tiny fusion reaction in a magnetic bottle in the laboratory. With the recent failure of ZETA, however, Scylla scientists weren't ready to make a formal announcement of their accomplishment. They were uncertain about whether they had truly achieved thermonuclear fusion and were well aware of the damage that a premature announcement could cause. Finally in 1960, Tuck made a formal announcement to Congress (not the press) that he and his research group were "prepared to stake [their] reputations that [they] have a thermonuclear reaction" [6]. Scylla had succeeded where ZETA failed, but the world scarcely took notice. The success of seeing thermonuclear neutrons was not showing a path to a working reactor. Congress was becoming very impatient with fusion scientists' broken promises and began to reduce funding for magnetic fusion research.

Making matters worse, the AEC's budget, which had skyrocketed through the 1950s, stopped growing, and the fusion research budget itself began to pinch. The JCAE was growing impatient with the slow pace of fusion development. At the JCAE hearings on the AEC's FY 1965 budget in the spring of 1964, Chairman John Pastore of Rhode Island said "how long do you have to beat a dead horse over the head to know that he is dead?" [3]. Pastore went on, "Is this not indeed a very expensive way of getting this basic knowledge? We can build these machines until the cows come home. Somewhere along the line somebody has to think that this is a lot of money and maybe we ought to be putting it into some other place where it may be more productive" [9]. Some scientists were also unhappy. Harold Furth, a scientist at Livermore who later went to PPPL, said researchers there "were in a state of dissatisfaction and ferment, feeling that the program was taking the wrong course... too much junk machinery was being built without enough enlightenment" [9]. On December 10, 1964, Post and Furth published a paper together entitled "Advanced Research in Controlled Fusion"; it called for a "new orientation" for the controlled fusion program [9]. It essentially sounded a retreat from the ambitious fusion reactor goal into basic plasma physics.

It was during this dispirited period of the mid-1960s that Spitzer decided to quit the field. Spitzer had left astrophysics behind to study fusion, but he believed it would be a ten-year commitment, not a twenty-year puzzle still unsolved. He also felt he had contributed all he could to the field, quoted as saying at the time: "I felt I'd made most of the contributions that I could make. My method of research was more useful in the early days of plasma physics that it had been in later years. The subject has become more mathematical, becoming more and more difficult to follow in one's mind what the plasma is doing. It's more and more a matter of getting detailed, rather complicated equations... it's a combination of applied mathematics and numerical computations, and I can't do such things" [9]. Spitzer's departure from fusion dampened many spirits in the field.

An important breakthrough occurred in 1960: the invention of the laser. The first important thing lasers did for fusion research was they allowed scientists to accurately measure the temperature of the plasma. Lasers, which are made of photons all of the exact same wavelength (or more simply put, color), produce very tight beams without much scattering or dissipation. When a laser is shined at plasma, the photons will begin with the exact same color. As the photons strike the fast-moving particles in the plasma, they gain energy, which shortens their wavelengths, making them appear slightly bluer. By looking at this change in wavelength after a laser beam hits plasma, scientists can calculate the energies of the particles in the plasma, which reveals the temperature. Knowing the temperature of the plasma is important because plasma temperatures in existing machines at the time had been measured indirectly, which caused debate as to whether the machines were really achieving high enough temperatures to induce fusion. Temperature is a key parameter in achieving the Lawson criterion, so it is critically important to be able to determine it accurately. The second important contribution of lasers was the innovation of a new way to contain plasma. Lasers produce particularly intense yet easily controlled light beams; they are both precise and high-energy. If many lasers were pointed at a pellet of deuterium from all directions, the beams could heat and compress the pellet, which would create a tiny fusion reaction. At the Livermore laboratory, physicists Ray Kidder, John Nuckolls, and Stirling Colgate set to work designing laser fusion schemes soon after the first laser was built. Their calculations seemed to show not only that laser fusion was possible, but also that it might be relatively easy to achieve breakeven [6]. This so-called laser fusion was later called inertial confinement fusion. This idea was not pursued on a large scale until 1974, and will be discussed in the next section.

One very important influence on the American fusion program was Russian research that was conducted in the fifties and sixties. When the third international conference on plasma physics and controlled thermonuclear fusion convened in August 1968 in the Siberian city of Novosibirsk, a major focus of attention was Lev A. Artsimovich's report on the most recent tokamak results [8]. The tokamak was a machine originally conceived by Andrei D. Sakharov and Igor E. Tamm. It was toroidal in shape and used coils of wire to induce magnetic fields, which earned it the name tokamak, an acronym comprising the initial syllables of the word combination "*to*roidal'naya *ka*mera *mag*nitnaya" (where the letter 'g' was replaced with 'k' for euphony), meaning "toroidal chamber with magnetic coil" [6, 10]. Whereas the Stellarator used external magnetic fields to contain the plasma and pinch machines used internal electric currents to squash it, the tokamak did both. The experimental values of temperature, density, and confinement time that Artsimovich claimed for the Soviet tokamaks were dramatically better than the parameters that had been so far recorded on any other toroidal device.

When, three years earlier in 1965, the Soviets had claimed tokamak confinement times 10 times better than Bohm times at the Culham conference, they had been greeted by incredulity. By the Novosibirsk meeting, the situation had changed. Bohm diffusion was no longer feared to be a universal property of toroidal systems. The multipole machines[1] had already achieved confinement times greatly exceeding Bohm times, albeit at low densities and temperatures, and even the Model C Stellarator seemed to be beating Bohm diffusion by a factor of 4 or 5. Moreover, both the confinement and the temperatures of the tokamak plasma had been greatly increased since 1965. The Soviets' 1965 tokamak results had been open to the interpretation that they were essentially identical with Stellarator results, if one allowed for experimental error and the intrinsic differences of the machines; they had been so interpreted by Spitzer at the Culham meeting [8]. The new data had to be treated more seriously. To be sure, the temperature measurements upon which the whole edifice of Soviet conclusions was based were still indirect, which was cause for debate. Many American and British researchers did not believe the claims by the Soviets. To resolve the debate of the indirect temperature measurements, in 1969, a group of British scientists was sent to Artsimovich's lab at the Kurchatov Institute to verify the Soviets' claims. When the British scientists shined a laser beam at Artsimovich's tokamak plasma, they saw that the Soviets were not exaggerating. Their plasma was tens of millions of degrees, dense, and relatively well confined [6]. Within a few years, plasma physicists across the world scrapped their old devices and build tokamaks. The laser measurements of Artsimovich's plasma sparked a fusion energy renaissance in the United States.

The optimism of the 1950s had dampened in the 1960s due to increasing awareness of the difficulty of containing plasma. Many fusion experiments during the 1960s changed their goal from "demonstrating a working fusion reactor" to "investigating plasma properties". Only twenty years into fusion research, some projects realized that it was not possible to build a fusion reactor with their present technology. The US fusion program was losing support and might have died if not for the discovery of the Russian tokamak and its prominence at Novosibirsk in 1968. The tokamak reinvigorated the US program, and predictions of fusion only being "a few

[1] The origin of the multipole machine went back to 1960 when Donald Kerst, at General Atomic, began systematically probing behind the "apparent differences and novel characteristics" of all the various fusion devices in an attempt to resolve them into their basic types and to illuminate their virtues and shortcomings [8]. The multipole, however, was not conceived as a prototype reactor. A multipole could be used as a reactor only if the rings could be made superconducting. This would mean keeping rings at temperatures close to absolute zero in proximity to a plasma 10 times hotter than the interior of the sun. Kerst did not intend to deal with problems like that. On the contrary, he believed that the state of fusion research was such that it did not make sense to be concerned seriously with prototype reactors. There was too much to be learned about plasma properties. The multipole, with its MHD stability, would make an excellent apparatus for studying the new microinstabilities then coming to the fore. For Kerst, "useful experience and interesting plasma results" were the objectives [8].

decades away" started to emerge again in the 1970s (although predictions now had much more careful wording as to not be interpreted as promising a fusion reactor).

2.3 The Rise of the Tokamak: 1970s

By January 1970, the model-C Stellarator at Princeton had been dismantled. In its place, a mere four months later, a tokamak was built; it was renamed the Symmetrical Tokamak (ST). They also built a smaller new tokamak, the Adiabatic Toroidal Compressor (ATC), to study heating by both adiabatic toroidal compression and neutral beams. Over the next few years, Princeton embarked on an aggressive program of tokamak building with two larger machines: the Proto-Large Torus (PLT), a 1 MA machine due in 1975, and the Poloidal Divertor Experiment (PDX), to study poloidal divertors, due in 1978.

Oak Ridge National Laboratory, previously devoted mainly to mirror machines, also moved into tokamaks, initially building the Oak Ridge tokamak (Ormak), equipped with beam heating, and later the Impurity Studies Experiment (ISX), specifically constructed to study impurity problems. The plan for Ormak was to build two different machines, Ormak I and Ormak II, of different aspect ratios (the ratio of the radius of the torus to the radius of the plasma cross section). Because the work at Princeton had eliminated the need for mere duplication of the Soviet results, which had been part of the rationale for Ormak I, the Ormak program sought to focus on reducing the aspect ratio of fusion machines.

At MIT, W. P. Allis, S. C. Brown, and D. J. Rose had established a strong plasma physics tradition, and had been looking into fusion research from their backgrounds in gas discharges, microwaves, and nuclear engineering. Moreover, the Francis Bitter Magnet Laboratory had expertise in producing strong magnetic fields. Bruno Coppi worked with D. Bruce Montgomery at the Bitter laboratory to design the tokamak Alcator (from the Latin for "high-field torus"), which began operation in 1972. Alcator was later renamed Alcator-A because it became part of a series of high-field tokamaks. MIT's tokamak planned to push Ohmic heating to the limit by attaining high densities and temperatures from a combination of large field, small major radius, and anomalous resistivity [5, 8].

Along with the tokamak, inertial-confinement fusion began to be explored in the US at the beginning of the 1970s. The inertial-confinement route to controlled-fusion energy is based on the same general principle as used in the hydrogen bomb—fuel is compressed and heated so quickly that it reaches the conditions for fusion and burns before it has time to escape. The inertia of the fuel keeps it from escaping—hence the name inertial-confinement fusion (ICF) [4]. Of course, the quantity of fuel has to be much smaller than that used in a bomb, so that the energy released in each "explosion" will not destroy the surrounding environment. The quantity of fuel is also constrained by the amount of energy needed to compress and heat it sufficiently quickly. An inertial fusion power plant would have a chamber where these mini-explosions would take place repeatedly in order to produce a steady output of energy.

The first big ICF experiment was built in 1974 at Livermore. It was known as Janus, and, two-faced like the god it was named after, it had two laser beams that shot at a tiny pellet of deuterium and tritium from opposite directions. It was more a test of the laser system than a concerted attempt to initiate fusion reactions. Nevertheless, the Livermore scientists were soon detecting tens of thousands of neutrons coming from the pellet [6]. They had achieved thermonuclear fusion, even though it was on a tiny scale.

An interesting event that occurred during this time was the rogue company—KMS Industries, Inc. of the United States—reporting that it had built its own laser system. By May 1974, KMS, named after its physicist founder and president, Keeve M. Siegel, claimed that it was producing neutrons from laser fusion. Soon after, the *New York Times* touted KMS's achievement as "a significant step toward the long-range goal of nuclear fusion as a source of almost limitless energy" [6]. The AEC was less than thrilled, however, because a private firm was doing an end run around the government. If KMS claims were true, an AEC statement read, it would be "a small but significant initial step toward the achievement of fusion power" [6]. Siegel was making the AEC look bad—and fusion energy look good. Because Siegel was able to use lasers to ignite fusion as the head of a private company instead of as a scientist in a government laboratory, the public began to believe that private industry was embracing fusion reactors as a viable source of energy while the government was holding back or perhaps stalling the development of this technology. Siegel, the entrepreneur, exuded confidence in public; he was sure, he said, that he could "demonstrate the feasibility of laser fusion in eighteen months" and turn lasers into "efficient fusion power" within "the next few years" [6, 7].

The timing of Siegel's announcement could not have been more favorable for public reception of the new fuel technologies. The United States was just getting through its first oil crisis. Since America supported Israel during the 1973 Yom Kippur War, the Arab members of the Organization of the Petroleum Exporting Countries (OPEC) cut off oil supplies to the US, causing gas prices to skyrocket. It was becoming more and more clear that the US needed to find a new source of energy—anything other than petroleum—to avoid being held hostage to

OPEC's interests. It was scarcely two months after the embargo was lifted that the nation learned about KMS and fusion, which seemed like the way to get out from under OPEC's thumb. Congress immediately seized upon it and started pouring money into fusion research. Laser fusion saw a dramatic increase in funding, growing from almost nothing to $200 million per year by decade's end.[2] Livermore and some other laboratories around the country, particularly those at Los Alamos and at the University of Rochester in New York, began to plan massive laser projects with an eye toward creating a viable fusion reactor. Magnetic fusion, too, benefited from the renewed interest in fusion energy. After stagnating for a decade at around $30 million per year, magnetic fusion budgets began to skyrocket. In 1975, more than $100 million went to magnetic fusion; by 1977, more than $300 million; and by 1982, almost $400 million [6]. Siegel's 1974 announcement helped ignite public enthusiasm (and government largesse) for fusion research. Sadly in 1975, he died of a stroke while testifying about his work in front of Congress. Siegel did not survive to benefit from the surge of optimism he generated, but he also did not have to confront the worsening technological problems laser fusion scientists faced as lasers grew more powerful.

In 1975, Janus was already suffering major issues. Lasers were extremely powerful for their day, pouring an unprecedented amount of laser light into very tiny spaces. Livermore's scientists managed to get this level of power by taking enormous slabs of glass made of neodymium and silicon and exciting them with a flash lamp. The slabs of glass were what produced an enormous number of infrared photons in lockstep, and the resulting beam exited the glass and was bounced around, guided by lenses and mirrors to the target chamber [6]. However, the beam was so intense that it would heat whatever material it touched, changing the properties of lenses, mirrors, and even the air itself. When heat changes the properties of a lens or a mirror, it alters the way the device focuses on the beam. Some of these imperfections manifest as hot or cold spots in the beam; the hot spots would pit lenses, destroying them in a fraction of a second. Every time the Janus laser was fired, it would tear itself to shreds [6].

The Livermore scientists had already started to solve this problem on their next iteration of an ICF machine, Argus. By shooting the beam down a long tube and carefully removing

[2] Congress also took the opportunity to reorganize its entire portfolio of energy research. In 1974, it eliminated the AEC and created the Energy Research and Development Administration (ERDA) to take on many of its functions. Just a few years later, ERDA, together with other federal agencies, would become the Department of Energy (DOE) [6].

everything but the light at the very center of the beam, the scientists would be assured of getting uniform, pure light that was free of hot spots. However, the laser had to be housed in a very large building to accommodate the tubes, which were more than one hundred feet long, and some of the laser's power was sacrificed [6]. These were minor issues compared with laser fusion's more serious problem with electrons. When the laser was shined on a sample of matter, the electrons would heat up before the nuclei; the electrons got so hot that the target would explode. The way to fix this was to use a higher frequency of light, which could be achieved by shining the laser through large, expensive frequency-changing crystals. Nevertheless, the use of these crystals provided results that led Livermore's physicists from Argus to push for a full-sized machine, Shiva, that would use twenty beams to zap a pellet of deuterium from all directions [6].

The Shiva machine encountered another instability called the Rayleigh-Taylor instability. This instability occurs at the boundary between two fluids of different density and can be observed if a layer of water is carefully floated on top of a less dense fluid, such as oil. As soon as there is any small disturbance of the boundary between the water and the oil, the boundary becomes unstable and the two fluids exchange position so that the less dense oil floats on top of the denser water [4]. A similar effect causes the compressed capsule to distort unless its surface is heated very uniformly. A uniformity of better than 1% is called for, requiring many separate laser beams, each of which has to be of very high optical quality, uniformly spaced all around the surface of the capsule [4].

An ingenious way of obtaining a high degree of uniformity was developed at Livermore around 1975, although the details remained secret until many years later due to nuclear national security. The capsule of deuterium is supported inside a small metal cylinder that is typically a centimeter across and made of a heavy metal, such as gold. This cylinder is known as a hohlraum, the German word for "cavity" [4, 6]. The laser beams are focused through holes onto the interior surfaces of this cavity rather than directly onto the capsule. The intense laser energy evaporates the inner surface of the cavity, producing a dense metal plasma. The laser energy is converted into X-rays, which bounce around inside the hohlraum, being absorbed and reemitted many times, rather like a light in a room where the walls are completely covered by mirrors. The bouncing X-rays strike the capsule many times and from all directions, smoothing out any irregularities in the original laser beams. Also, the X-rays can penetrate deeper into the plasma surrounding the heated capsule and couple their energy more effectively than longer-wavelength light. Some energy is lost in this conversion, but the more uniform heating compensates for this. This approach is known as indirect drive, in contrast to the direct-drive arrangement, where the laser beams are focused directly onto the capsule [4].

Laser fusion's price tag quickly grew from millions of dollars to match that of magnetic confinement at hundreds of millions of dollars. Shiva's failure occurred two decades after Homi J. Bhabha predicted that fusion power plants were twenty years away. Yet in the 1970s, and even into the 1980s, fusion scientists spoke of power plants being thirty years away [6]. After decades of research, the goal of fusion energy had become ten years more distant.

In the late 1970s, morale in the magnetic fusion community was extremely high. Though they were still far away from breakeven, steady progress had been made over the years. As the machines got bigger and more expensive, scientist were able to get the high temperatures greater than 10 keV required for fusion energy and densities in their plasmas, and to hold them for longer times. Physicists and politicians alike were confident that the new, large tokamaks being built would achieve breakeven, and perhaps go beyond. In 1980, President Jimmy Carter signed into law an act that promised to double the fusion budget in seven years—from nearly $400 million annually—and established the national goal of "the operation of a magnetic fusion demonstration plant at the turn of the twenty-first century" [6].

Congress was starting to pressure the AEC for plans for the future of the domestic fusion program. On the one hand, increasing parameters was one clearly understandable indication of progress in a field whose complexity baffled congressional attempts to monitor it. On the other hand, the Joint Committee members were suspicious that the fusion scientists were attempting to bootleg pure research into programs that the committee was funding as technology [8]. In early May 1971, Edward E. David, Jr., the recently appointed presidential science advisor and chairman of the White House Office of Science and Technology (OST), sent a request to AEC Chairman Glenn Seaborg to develop an outline for the two traditional cases, a "significantly expanded" program and "an 'all out' program that seeks to develop fusion energy in the shortest feasible time" [8]. The Controlled Thermonuclear Research (CTR) Branch was instructed to take the second case "quite seriously" [8]. The laboratory leaders gathered in Washington with the AEC fusion staff on May 14 to discuss the response they wished to submit.

There were two conflicts that lay at the heart of the fusion program's development. The first conflict was between what fusion leaders wanted to buy with the sharply augmented funding that they felt they needed and what they thought the outside world—groups such as the Congress and the Office of Management and Budget—wanted to pay for. The fusion community wanted to explore plasma with higher, more reactor-like, values of temperature, density, and confinement time. To get the new plasma regimes, they thought, they must build larger machines that would be an order of magnitude more expensive than the set of tokamaks that had just been funded. The tokamaks had been budgeted originally at under $1 million, and were actually costing several million dollars apiece [8]. Beyond the Scyllac[3] and PLT, the fusion leaders wanted three feasibility experiments in which the plasma would be brought up to the full temperature and density of reactor operating conditions in each of the three main devices of theta pinch[4], tokamak, and mirror. Each was estimated to cost $30 million (1970s dollars) or more [8].

These new machines would be highly visible budget items, and the fusion leaders anticipated Congress and the administration would therefore expect them to perform as predicted. Yet it was just because they could not trust the theoretical predictions that the fusion scientists wanted the experiments in the first place; it was precisely the unexpected that they wanted to look for. In these unfamiliar regimes of different geometries and higher parameters, wholly new phenomena might manifest themselves, and the scientists felt that it was essential for the program to discover these phenomena, and learn to deal with them, as soon as possible.

The second conflict was between the demand made by Congress and administrators for detailed program plans and the state of fusion research in 1971. Planning in any research and development (R&D) program is provisional [8]. In one that depends as decisively upon new scientific insights as fusion energy did, detailed planning can be foolhardy. The crucial question was whether any existing device was good enough to serve as a frame around which to drape plans. If not, planning now would at best be superfluous. It would be better, as one participant phrased it, to have a slow, steady, relatively undirected research program, over half a century, which wiggled out like an amoeba to encompass new questions as they arose [8]. Neither plasma data nor engineering studies had progressed to the point at which anything more than a hunch could be offered as to whether any of the devices could be a reactor. The fusion reactor of the twenty-first century might be based on a scheme not yet invented.

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[3] An experiment at Los Alamos that was a toroidal theta pinch reactor.
[4] Theta pinch is merely a type of pinch machine.

Knowing the problems at hand, the program leaders nevertheless proceeded to formulate plans. In part, they felt a sense of responsibility to the government that was supporting them, and a sympathetic understanding of the government's need for information in its decision making [8]. In part, they were aware of the political dangers of failing to comply. Were they to admit that they could not predict dates past the first stages, the administration and Congress might choose to delay the increased funding until those stages were completed. Further, none of the scientists believed that an amoeba-like half-century of research had any political chance at all [8]. The first milestone they laid down was a more rigorous definition of scientific feasibility. They decided that the scientific feasibility experiments would "attempt to 'break-even' fusion plasma conditions (threshold reactor values of density, temperature, and plasma confinement time) in laboratory configurations which lend themselves to development into net power-producing systems" [8]. This test was to be carried out under conditions of minimum radioactivity and therefore without tritium. To this end, the leaders introduced the idea of "equivalent-energy breakeven": "test plasma conditions such that if a fusion fuel were substituted for the test plasma gas, the resultant thermonuclear energy production would equal the energy originally invested in the plasma... This demonstration is envisioned without the actual production of a large quantity of neutrons or the handling of significant amounts of tritium, both of which would require significant additional expense and necessitate a time delay" [11]. Where future milestones were concerned, the fusion leaders fell back upon analogies to the development of fission technology. One or more experimental reactors, using fusion fuels and producing net energy, would comprise the next stage. A demonstration reactor would follow; it should include "all of the elements of a commercial power plant", and its successful operation "would be a prelude to commercial sales" [12]. They set 1980 as the target for the first proof of scientific feasibility. The group was reluctant to make any statement at all about the dates for the stages that would follow [13]. In the end, it settled on admirably cautious language: "Estimates of the time and costs... to carry through... experimental fusion reactors and demonstration fusion reactors are very difficult to make" for the all-out program plan [8]. A demonstration plant in the mid-nineties for the first case, and the late eighties for the second, was suggested by "a rough estimate" [8, 14].

2.4 The Decline of the Domestic Budget and the Birth of ITER: 1980s

By the time Ronald Reagan came into office in 1981, the climate for fusion was beginning to change again. The OPEC crisis was fading into memory, and energy research was not a high priority for the new president. He scuttled Carter's plan, and as budget deficits rose, fusion energy money began to disappear from the $400 million annual budget, $50 million hunks at a time [6]. The panoply of glorious experiments planned in the 1970s began to crumble under increasing financial pressure. As magnetic fusion budgets dwindled, researchers struggled to save their precious tokamaks from the budget ax. A huge magnetic mirror project at LLL, called the Mirror Fusion Test Facility, that had already swallowed more than $300 million, was scrapped just as it finished its eight-year construction and was about to be dedicated. The dedication ceremony went on as planned, even though the project had been cancelled weeks before. One of the ceremony's attendees was quoted as saying "I thought I was going to a wake" [6]. One after another, new facilities—such as the Elmo Bumpy Torus[5] and ISX—died on the drawing board. With the budgets in free fall, there was no room for anything other than the tokamak program, and even that was in jeopardy [6].

In the beginning of the 1980s, Livermore was building a $200 million laser named Nova. Researchers there were confident Nova would finally take them to the promised land—igniting fusion fuel, producing more energy than it consumed [6]. Nova came into operation in 1984 and had ten beams. It was capable of producing up to 100,000 joules in a burst of light lasting a billionth of a second. For that brief instant, its output was equivalent to 200 times the combined output of all the electricity-generating plants in the US [4]. However, Nova was subject to severe MHD instabilities. It was dismantled in 1999 to be replaced by other experiments.

The science of ICF was following the same trajectory as that of magnetic fusion. Early optimism in the 1950s led scientists to believe that plasmas could be confined and induced to fuse relatively easily. Cheap, million-dollar machines, they thought, would be able to do the job [6]. But the plasma seemed to always find a way to wriggle out of control. Instability after instability caused leaks in stellarators, pinch machines, and mirrors, and million-dollar machines turned into ten-million-dollar and hundred-million-dollar machines. The larger size would provide the experiments more margin to the troubles caused by the instabilities in order to

[5] An experiment at Oak Ridge that was comprised of a series of magnetic mirrors connected end-to-end to form a closed torus [8].

achieve the Lawson criterion, but simultaneously it greatly expanded the cost and timelines. Laser fusion began with similar optimism. Livermore's scientists thought that their first few lasers could get more energy out than they put in. But instabilities like Rayleigh-Taylor allowed the plasma to escape its confinement. Million-dollar lasers grew bigger and more expensive; soon, laser fusion machines were as expensive as their magnetic counterparts.

Other government-funded experiments in the US were still progressing. At MIT, Alcator-A was replaced by Alcator-C (Alcator-B was designed but never implemented). In comparison to Alcator-A, Alcator-C had a larger minor radius (although it was still the smallest tokamak in the US), increased toroidal field, and increased plasma current. These modifications were aimed at achieving breakeven and getting very close to the minimum value for ignition (if the machine was run at higher temperatures). Alcator-C experimented on Ohmic heated plasmas at high density. At General Atomics in San Diego, there was a doublet program of studying elongated and indented cross-sections that continued with the construction of a larger device Doublet III which came into operation in 1978. After being operated in the doublet configuration as a joint US-Japan venture, it was converted in 1986 into a large D-shaped tokamak with a poloidal divertor and renamed DIII-D [5].

One of the biggest tokamaks was at Princeton: the Tokamak Fusion Test Reactor (TFTR), which promised to achieve breakeven. TFTR was supposed to cost a bit more than $300 million, but as often is the case with cutting-edge science projects, the expenditures ballooned well beyond that to $1.65 billion by the time the project was finished [6]. The TFTR program was delayed, but not cancelled, by Reagan's budget cuts, and began operation in 1983. TFTR, despite budget cuts, was steadily closing in on breakeven. It was holding plasma for seconds at a time and achieving temperatures close to a hundred million degrees. Even with the improvements, breakeven was still about a factor of three to scientific breakeven, but more than a factor of ten away from creating strong self-heating. There was no way, with budgets as they were, that fusion scientists could ever hope to build a magnetic fusion reactor. A tokamak big enough and powerful enough to keep a plasma burning indefinitely would cost billions, and America's fusion budget could never withstand that sort of strain. The story was no different overseas. No single nation could afford to build a tokamak that could achieve breakeven and sustained burn. Perhaps, though, by pooling their resources and joining together in one great effort, fusion scientists around the world could finally build a working fusion reactor [6].

Looking back at the plan proposed in the late 1970s by fusion scientists, to get to a commercial fusion power plant based on the tokamak concept will require three major steps. The first step—to show that fusion is *scientifically feasible*—has been achieved by TFTR. The second stage—usually known as the *Next Step*—requires an even bigger experiment that will test most of the technical features needed for a power plant in order to show fusion is *technically feasible*. The third step, tentatively known as DEMO, will be to build a prototype fusion power plant producing electricity routinely and reliably to show that fusion is *commercially feasible*. Optimistic plans in the 1970s aimed at starting the Next Step in the 1980s and having the DEMO stage operating early in the 21st century, but various factors have combined to delay these plans by at least two decades [4].

Back in the late 1970s, with the construction of the big tokamaks already under way, the next task was to think about the design of the Next Step experiment to validate technical feasibility. Groups in the United States, the Soviet Union, the European Community, and Japan started to design their own versions. They came together to collaborate on a study that was known as INTOR, the International Tokamak Reactor. The INTOR study did valuable work in identifying many of the technical issues for a fusion power plant and stimulated research programs to solve them. However, INTOR fell short on the physics specification—not surprisingly, because TFTR (and other big tokamak projects outside of the US) were still only at the construction stage and it was a very large extrapolation from existing smaller tokamaks. Estimates from the small tokamaks led INTOR to be designed around a plasma current of 8 MA, but even before the study was completed, it became clear that this was too small—at least 20 MA would be needed for a tokamak to reach ignition [4]. (On a side note, this number is now overly pessimistic and in fact ~10 MA machines can reach ignition using improved confinement regimes like H-node.)

The idea of an international reactor had been around since the budgets started dropping, but it truly came to life in 1985. INTOR had helped set the scene for international collaboration. At a summit in Geneva, Reagan and the Soviet leader Mikhail Gorbachev tried to reduce tensions between the US and the USSR. Gorbachev suggested to Reagan the possibility of a joint effort to build a fusion reactor. Reagan jumped at the chance, as did France and Japan. Together, the four countries would build an enormous tokamak that would finally achieve ignition and sustained burn, and the International Thermonuclear Experimental Reactor (ITER) was born [6].

The principal objectives for ITER were to produce an ignited plasma and to study its physics, as well as to test and to demonstrate the technologies essential for building a fusion power plant—especially the superconducting magnets and tritium breeding systems. Although the four partners were supportive of international collaboration in fusion energy research, they were reluctant to commit themselves irrevocably to building the large ITER project. Furthermore, they did not want to decide immediately where it would be built. It was agreed to proceed in phases—conceptual design, engineering design, site selection, construction, and operation—but each phase would have to be approved without commitment to further phases. As of the writing of this paper (May 2014), ITER remains under development. Each approval point has resulted in significant delays before agreement could be arrived at to proceed with the next phase, so the ITER project has taken much longer than originally expected.

2.5 ITER Grows while US Domestic Programs Shrink: 1990s

Work on the conceptual design phase for ITER began in 1988, and a central ITER office was established in Garching (near Munich) in Germany. The conceptual design study, which established the key parameters and requirements for ITER, was conducted mainly as a part-time collaboration between scientists and engineers working in their home countries and was coordinated by round-table meetings in the central office. The successful outcome of the first phase was followed by an agreement signed in 1992 to proceed with the second phase—the Engineering Design Activity (EDA). This phase was scheduled to take 6 years and was carried out by a dedicated full-time "Joint Central Team" of scientists and engineers who were to be supported by many more experts working part-time in so-called "Home Teams" [4]. There was disagreement about where the central team should be placed—Europe naturally thought that the ITER design efforts should continue in Garching—but the US wanted the central team to move to San Diego, and Japan wanted it to be based at Naka. Eventually, a compromise was reached to split the central team (which numbered about 160 professionals at its maximum) among all three places; this difficulty was an early sign of indecision at the political level that recurred in years to come [4].

ITER was to be the largest fusion experiment ever attempted. As design work began on it, scientists realized that it would cost $10 billion. The four countries, working together, could contribute the money, but ITER would devour the total fusion budgets of all the participating countries. Some noted that ITER (pronounced "eater") was a frighteningly apt name for the device [6]. Once the ITER project was under way, there would be no room in the collective fusion budget for anything else; even some of the larger tokamak projects around the world could not survive.

Nevertheless, the engineering design of ITER went ahead with considerable enthusiasm and support from the international fusion community. Technical and scientific objectives were set, and a detailed engineering design was prepared. The results from other tokamak experiments were now at the stage where it could be predicted with confidence that ITER would require a plasma current of 21 MA to reach ignition with an energy confinement time of about 6 seconds. The design for ITER was based on a tokamak with a divertor and a D-shaped cross section—

similar to JET[6] but with physical dimensions about three times larger [4]. The construction was planned to take about 10 years and was predicted to cost about $6 billion [4]. These details were monitored and checked by independent experts, who agreed that ITER was well designed and could be built to schedule and within the estimated costs.

However, by 1998, when the engineering design had been completed and it was time to make the decision about the construction phase and to select a site where ITER would be built, the political climate had changed. The collapse of the Soviet Union and the end of the Cold War meant that a project like ITER could no longer be supported simply as a showcase of East-West collaboration—now it had to stand on its own merits as a new form of energy. Although there had already been considerable discussion of environmental issues during the 1990s, a consensus about the threat of global warming had been slow to find support in some countries. Alarm calls about the risk of future fuel shortages had generated little sense of urgency to carry out the basic research to develop alternative forms of energy. In fact, government funding for energy research, including fusion, had been allowed to fall in real terms. The situation had reached a crisis point in the US, where a drastic cut in research funds caused the government to pull out of the ITER collaboration in 1998 [3, 4].

Domestic fusion programs suffered cuts in funding because of the reduction in funding energy research and the split between funding domestic fusion experiments and ITER. Princeton scientists did not want their facility to disappear. Other fusion researchers, especially those who thought that non-tokamak machines were still worth exploring, were angry that the world was going to gamble all its fusion money on a giant tokamak while ignoring all other possibilities. Almost everyone in the nuclear science community agreed that a big international reactor effort would be a wonderful thing, but at the same time everyone wanted to have a thriving domestic fusion program, too. Fusion researchers would not get both, especially with budgets dropping precipitously [6]. In the early 1990s, with ITER in ascendance, PPPL seemed marked for death, and in 1997 TFTR was decommissioned early [3, 6, 15].

There were some programs that managed to survive the budget cuts. At MIT, a completely new machine called Alcator C-Mod began operating in 1993. It has the highest operating toroidal field on a divertor tokamak, using liquid nitrogen cooled field coils [15]. PPPL began to

[6] Joint European Torus (JET), located in Culham, UK, is currently the world's largest tokamak and began operating in 1992.

collaborate with Oak Ridge National Laboratory, Columbia University, and the University of Washington at Seattle to create the National Spherical Torus Experiment (NSTX). NSTX obtained its first plasma in 1999 and remains a "proof of principle" experiment, developing "components and scientific data for ITER" [16].

As the twentieth century drew to a close, it was clear that the world would not have an operating fusion power plant, as envisioned in the 1971 plan, by the year 2000. The reason was threefold: failure of the US government to provide the necessary funds; failure of the US government to commit to construction of the necessary facilities, such as the fusion engineering test reactor; and failure of the US Department of Energy to manage the program so as to achieve its avowed practical purpose [3]. There was also the continuing problem of new phenomena and instabilities being discovered by fusion scientists. Every time higher parameters were achieved, the plasma would behave in a new way that required more time and research to understand. It did not help that experiments' funding were continually getting cut, which delayed research even further. The combination of these problems practically eliminated the idea of a fusion power plant. Many of the domestic fusion programs turned their research efforts towards mimicking ITER (by testing materials that would be used in ITER's construction or by operating at ITER-like regimes) in order to remain funded. At the turn of the millennium, the best prospect for fusion research was achieving ignition with ITER.

2.6 Rejoining ITER: 2000s through the Present

The American departure in 1998 shook the ITER collaboration—and branded the US as an unreliable partner when it came to international science—but the project limped along. The loss of US funding and researchers caused the remaining countries to rethink their original plan. Russia, Europe, and Japan continued designing ITER but the plans they came up with were much less ambitious than the originals. The plasma in the reactor would span 12 meters instead of 16 and the machine would not be able to achieve ignition, although it would be able to keep a plasma confined for up to an hour and produce 10 times as much power as it consumed. It would cost half as much as the original ITER: $5 billion instead of $10 billion [6].

The DOE reviewed the new design to determine whether or not the US should rejoin ITER. In 2002, the appointed DOE review committee "said that the ITER cost estimate, which they estimated at $5 billion (in constant 2002 dollars), 'is supported by the design and R&D results that are unusually mature for a science project facing the decision to fund construction'" [3]. In January 2003, President George W. Bush issued a statement saying "I am pleased to announce that the United States will join ITER, an ambitious international research project to harness the promise of fusion energy. The results of ITER will advance the effort to produce clean, safe, renewable, and commercially available fusion energy by the middle of this century. Commercialization of fusion has the potential to dramatically improve America's energy security while significantly reducing air pollution and emissions of greenhouse gases" [3]. A DOE press release stated, "The US share of the [estimated $5 billion] construction cost is expected to be about 10% of the total" [3]. The DOE assured that the rejoining of ITER would not be at the expense of the US domestic fusion program. In a speech to fusion researchers at PPPL, Energy Secretary Abraham said, "But let me be clear, our decision to join ITER in no way means a lesser role for the fusion programs we undertake here at home. It is imperative that we maintain and enhance our strong domestic research program—at Princeton, at the universities, and at our other labs. Critical science needs to be done in the US, in parallel with ITER, to strengthen our competitive position in fusion technology" [3].

By the time the US had rejoined the project, China and South Korea had joined as new partners, and India would join at the end of 2005, thus making seven partners. The agreed division of costs was that Europe would bear approximately 45% of the overall cost of construction, and the other six partners would each contribute about 9% each. France, as host country, would provide many of the support facilities, including office buildings, improved road access, and power lines. The ITER Organization was established by an agreement signed by ministers of the seven members in November 2006 and was ratified by their governments the following year. This was good news, although 21 years had elapsed since the project was conceived. The most urgent task of the newly-appointed team was to review the existing 2001 design (which had not been updated to keep pace with the ongoing developments in fusion research and the cost estimates were incomplete in some areas), identify any design shortcomings, and look at possible improvements and changes required to make it consistent with more recent developments in fusion science. The ITER Design Review started in late 2006 and took a year to complete.

During the negotiations to set up the ITER organization, the members insisted that their contributions to ITER would be mainly in the form of hardware, rather than cash, so that their own industries would benefit directly from the ITER contracts [3]. It would have been much simpler and more cost-effective to have funded the ITER construction centrally, but this arrangement was politically unacceptable to some of the members. Responsibilities for all of the component parts of the machine were apportioned among the ITER members. Each component part had a nominal price tag based on the original estimates in the 2001 design proposal. Each member country took responsibility for a package of component parts, with the total nominal value of the package corresponding to that member's contribution to ITER [3].

Each domestic agency now had the task of supplying ITER with this agreed-upon package of components and doing so at whatever the actual costs turn out to be. The ITER Organization remains responsible for the overall design and specification of the machine and its integration, and for giving the domestic agencies the technical specifications for the individual components—but each domestic agency is responsible for procuring and for paying for these components [3].

Although DOE Secretary Abraham said that ITER would not affect the domestic program, this could not have been farther from the truth. In 2003, Patrick Looney, Assistant Director for Physical Science and Engineering at the White House Office of Science and Technology Policy was quoted as saying "There is no agreed upon fusion energy timeline… The US decision to join ITER negotiations is not part of a broader fusion initiative… there were large error bars on the

President's estimate and did not constitute a timeline commitment" [3]. He was very vague about the President's promises and this did not bode well for the domestic fusion program.

In March 2003, the US Fusion Energy Sciences Advisory Committee (FESAC) delivered the "35-Year Plan" requested by the DOE. The FESAC plan envisaged a broad portfolio of both magnetic and inertial fusion energy approaches and associated technologies over the next 15 years at a total cost of approximately $10 billion. At that time the concept for the first generation of fusion power plants would be selected for focused development over the next approximately 20 years. The plan called for initiating the development effort in FY 2004 with a budget of $332 million, although the recently submitted Presidential FY 2004 budget requested only $257 million [3]. For its implementation, the plan required the fusion budget to continue to grow to approximately $570 million in 2008 and to peak at approximately $900 million around 2013. When the plan was requested, the fusion community believed Congress was seriously interested in fusion energy development and this document would help inform the President about the context of his decision to rejoin ITER. However, it turned out that the decision to rejoin ITER was made prior to completion of the plan. When it was delivered, the 35-Year Plan was deemed unnecessary and subsequently ignored.

Worse, the DOE Office of Fusion Energy Sciences (OFES) FY 2004 budget document, though it contained $12 million to restart the US ITER effort, eliminated essentially all the energy-oriented fusion technology programs. FESAC expressed dismay at the fusion technology cuts proposed by the FY 2004 budget submission. In a letter dated March 5, 2003, FESAC chairman Richard Hazeltine told DOE Office of Science Director Ray Orbach:

> FESAC recommendations regarding the burning plasma initiative have emphasized the importance of maintaining scientific and technological breadth in the program. The Secretary of Energy renewed this emphasis in his recent announcement concerning US participation in ITER. Yet the funding for FIRE, a domestic burning plasma experiment that could provide an alternative to ITER, has been eliminated. Similarly, inertial fusion energy (IFE) is an important element of a balanced US fusion program: it provides the principal alternative to magnetic fusion and takes advantages of NNSA investments in the National Ignition Facility. The FY 2004 budget, however, eliminates chamber technology

> for both MFE and IFE... In summary, FESAC finds the Presidential request for fusion research funding in FY 2004 to be not only meager but also harmfully distorted. It terminates components of the program that are truly essential. Fusion research has accepted new challenges and identified new priorities, consistent with the President's stated agenda; fusion scientists want to get on with the job. What is needed is a funding allocation that respects the magnitude and nature of the task at hand [3].

The University Fusion Association (UFA) also sent a letter to the Subcommittee on Energy and Water of the House Appropriations Committee, urging them to add $25 million to the President's FY 2004 budget request. Their letter stated, "Without additional resources, carrying out the necessary preparations for ITER in FY 2004 with the present budget request of $257 million (unchanged from the FY 2003 request) will result in destroying critical elements of the base science and technology part of the fusion program" [3]. The Congress ultimately added $6.8 million to the President's request and specified that it was to be used for "non-ITER-related activities in the domestic program" [3]. The Congress also reduced the DOE-proposed ITER contribution from $12 million to $8 million.

In the FY 2005 budget request, President George W. Bush asked for the same amount ($264 million) that Congress had appropriated in FY 2004. Within this flat budget, however, the DOE proposed providing $38 million to ITER (an increase of $30 million over FY 2004). It was clear that ITER funding was in fact beginning to come out of the domestic program. After a FESAC review of the IFE program, Congress decided to add $12 million to the President's FY 2005 budget request, directing it to the domestic program and directing "the Department to reduce its planned expenditures on ITER" [3]. Meanwhile, the DOE had reestimated the cost of its US ITER contribution, increasing it from $500 million to $1.12 billion.

FY 2006 was to be the first year of ITER construction funding. In the FY 2006 budget proposal, the President requested a $17 million increase in the total OFES budget. However, the budget requested a $51 million increase for ITER, thereby necessitating the proposal of $34 million in cuts to the ongoing domestic fusion program [3]. Testifying on the FY 2006 budget request at a hearing of the House Science Committee Subcommittee on Energy, DOE Office of Science Director Ray Orbach said, "In the FY 2006 budget, we have had to reduce somewhat the domestic (fusion) program, but I would like to look at that in terms of a reorientation of the domestic program rather than a reduction" [3]. It was clear that the DOE Office of Science Director did not understand the magnitude of this proposal. In the fall of 2005, Congress provided the $17 million increase for fusion requested by the President for FY 2006, but declined to authorize the cuts requested in the domestic program. Instead of allowing the DOE to cut the domestic fusion program by $34 million, the Congress directed that $30 million be retained in the domestic program and that proposed ITER funding be reduced accordingly [3].

In late July 2005, the Congress passed and President Bush signed into law the Energy Policy Act of 2005. A section of the Act on fusion policy states "It shall be the policy of the United States to conduct research, development, demonstration, and commercial applications to provide for the scientific, engineering, and commercial infrastructure necessary to ensure that the United States is competitive with other countries in providing fusion energy for its own needs and the needs of other countries, including by demonstrating electric power or hydrogen production for the United States energy grid using fusion energy at the earliest date" [3]. In 2007 and 2008 the President asked for more money in the fusion budget to provide for ITER and increase the domestic fusion budget, but the request was rejected by Congress, and the domestic fusion program suffered a decrease in funding once again.

In early 2006, the President sent his FY 2007 budget request to Congress, requesting $319 million for OFES (an increase of $38 million over FY 2006). In it he requested $60 million for ITER and proposed once again to cut funding for heavy-ion fusion and high-energy-density physics (by $4 million), innovative confinement physics (by $2 million), and fusion materials research (by $2.4 million). In early 2007, President Bush sent to Congress an aggressive FY 2008 fusion budget request for $428 million, compared to $319 million in 2007. He requested the full $160 million needed for ITER construction, along with a slight ($9 million) increase for the domestic fusion effort ($268 million compared to $259 million in FY 2007). Congress eventually rejected the large increase for ITER, providing OFES with only $290 million in FY 2008. This threw the US planned ITER funding profile into disarray. In early 2008, DOE scrapped its previous ITER funding profile and told Congress its new projected contribution to the ITER project would total somewhere between $1.4 and $2.2 billion. Nevertheless, in early 2008 the President asked Congress to provide OFES with $495 million, including a $214.5 million contribution to ITER (the amount contained in the old funding profile). Congress would eventually provide $395 million to OFES, a $100 million increase, all for ITER.

The Congress provided OFES with $403 million for FY 2009, including $124 million for ITER and slightly over $100 million for the FY 2008 allocation. For FY 2010, President Obama requested, and the Congress later provided, $426 million. The OFES program also received $91 million in "stimulus" funding in FY 2009, as part of the nation's effort to stimulate the US economy in response to the economic recession that had been under way in the country for about 2 years [3]. For the ICF program in NNSA, the Congress provided $442 million in FY 2009 and $458 million in FY 2010. The NIF funding within NNSA had never been threatened financially like ITER was within OFES. The NNSA ICF budget had been above $400 million since FY 1998.

In early 2010, President Obama submitted his FY 2011 budget to Congress. It proposed only $380 million for OFES compared to $426 million appropriated in FY 2010. The budget request stated that spending for ITER would be reduced by $55 million ($80 million, compared to $135 million in FY 2010), resulting in a proposed $9 million increase in the domestic fusion program. The decrease in ITER funding reflected a recognition that the ITER program was proceeding more slowly than anticipated, resulting in scheduling slippages. The inertial confinement fusion program in NNSA would receive $482 million, compared to $437 million in FY 2010, mostly for "NIF diagnostics, cryogenics, and experimental support" [3]. The OFES program eventually received only $367 million, including $80 million for ITER.

During 2008 and 2009, international reviews were under way on the management, schedule, and cost of the ITER project. Originally, ITER construction completion was scheduled for 2016. But in June 2008, the ITER Council approved a 2-year slip in first operations to 2018, with the first fusion (DT) plasmas in 2023. At a Council meeting in June 2009, the Council kept (temporarily) the 2018 "first plasma" date but slipped the schedule for DT operations to 2026 [3]. At a February 23-24, 2010 meeting of ITER Heads of Delegations (China, EU, India, Japan, Korea, Russia, and the US), the ITER Organization proposed an additional schedule slippage of a year (to November 2019 for first plasma). ITER design changes were partly to blame, but estimated costs to complete the project were also exceeding the planned expenditures of some of the Parties, most notably the EU and US. In the US, the cost of its ITER contribution had been stated in 2006 as $1.12 billion, but in 2008 the DOE revised this to a "range" between $1.4 and $2.2 billion [3]. At a meeting during the last week of July 2010, the ITER Council approved the 1-year schedule slippage to November 2019 and slipped DT operations another year, to 2027.

The Council approved a new "baseline" cost estimate of $19 billion, which was approximately double the previous agreed-upon estimate (or quadruple the original estimate) [3].

In mid-2010, the ITER Council approved the baseline cost, scope, and schedule. The present authorized schedule aims at completion of the construction in late 2019, followed by the first pump-down of the vacuum vessel and the integrated commissioning of the tokamak systems. This will be followed by several years of plasma operation in helium and hydrogen to progressively optimize systems and plasma performance without activating the machine. The first operation in tritium is planned to start in 2027 [3]. Even this revised schedule is thought by many to be too optimistic (the expected date has now moved to 2034), and it may have to be extended to take account of various issues, including the delays anticipated as a result of the earthquake and tsunami in Japan in 2011 [3, 4].

In the US, the financial situation was worsening due to concern in Congress and the Administration over US deficit spending. Pressure was mounting to hold level or reduce so-called "discretionary" spending, and fusion fit into this category [3]. In President Obama's FY 2013 budget request, the US total fusion effort is held approximately level while providing ITER $100 million less that needed to meet the US commitment to the ITER schedule. ITER was proposed for a $45 million increase over its FY 2012 level ($150 million compared to $105 million) to be paid for by a proposed equivalent reduction in the US domestic fusion effort [3]. Although DOE did not publicly acknowledge that the total cost of its ITER share was continuing to rise, it was common knowledge in the US fusion community that it was approaching $2.5 billion [3].

The reduction in the domestic fusion budget has been felt acutely by a few research centers. The proposed budget caused a decision to shut down Alcator C-Mod at MIT, one of three major magnetic fusion research tokamaks in the US (the other two being NSTX at Princeton and DIII-D at General Atomics in San Diego, neither of which were shut down, they only had their funding reduced). Alcator C-Mod has been shut down but not dismantled, as other funding options are being explored.

There is some hope in the domestic fusion program, however. In 2009 construction of the National Ignition Facility (NIF) at Livermore was completed. This facility uses indirect drive method for ICF. In September 2013, NIF reported that the amount of energy released through the fusion reaction exceeded the amount of energy being absorbed by the fuel [17]. By the same definition, gain was achieved in MFE devices like TFTR as well. But the actual gain was 0.01, and you need 100 in an ICF reactor. This is still far away from the dream of a fusion reactor, but it is a small step in the right direction.

3. Recommendations

Fusion research over the last sixty years has been filled with both ups and downs. The program in the US started off with much post-war enthusiasm in the 1950s, when scientists believed they could have a working fusion power plant in just a decade's time. The 1960s were filled with disappointments as scientists realized how complex a problem it was to try to contain plasma. With renewed hope from the Russians' design presented at Novosibirsk in 1968, research continued and progress was made in the 1970s towards bigger and bigger fusion tokamaks. As the US budget started to tighten in the 1990s, poor estimates were made of when fusion reactors would be realistic due to the worry of losing funding before a project could even begin. As time progressed, the perceived lack of progress led the government not to fulfill its promised budgets and this led to many fusion experiments ending. US involvement in ITER has been a disaster for the domestic fusion program. In the 2000s through the present, US domestic fusion funding has been siphoned to pay for the US contribution towards ITER.

The future of fusion is still unknown; NSTX and DIII-D are still operating, NIF is showing promising results, and Alcator C-Mod has received enough funding to remain active for another year. But it does not look as though there will be a fusion reactor anytime soon, at least not in the US. Too much of the fusion budget is being allotted for ITER; the domestic budget is shrinking and experiments are suffering. The experiments in the US are only "proof of concept" and their main research is for ITER construction and diagnostic systems. Forecasting for ITER is poor because of the multitude of setbacks in the project. A fusion reactor will not be built in the US any time soon; trying to predict when a fusion reactor will be built is a fruitless endeavor because we need machines that can reliably achieve breakeven and then ignition first. After those two milestones are met, the long process of designing a safe, efficient fusion reactor will occur and then the arduous process of construction can begin.

Until then, the US needs to re-evaluate its involvement with ITER. ITER is the most promising fusion experiment to date and I suggest that the US remain a part of it. The reason is twofold: first, the US has already withdrawn and rejoined the project once, which caused the US to look like a bad business partner, and withdrawing again might mean the US will be barred from performing research at ITER; second, ITER is one of the biggest tokamaks and it shows promise for interesting results. However, in order to get these interesting results, ITER needs to be managed better. The dates for final construction cannot be pushed back again. The US and every other country involved needs to put more priority into ITER; for the US, this means increasing funding to the OFES for both construction of ITER and upkeep of the domestic fusion program. As Stephen O. Dean pointed out in a 2012 FESAC meeting:

> The proposed termination (of the Alcator C-Mod program at MIT) is of serious concern, since that program has made, and is making, important contributions to our understanding of tokamak physics and, furthermore, is important to the training of the next generation of fusion scientists. Termination of Alcator C-Mod would mean a "double whammy" for the MIT fusion program, since DOE terminated the other significant experimental facility there last year, the Levitated Dipole Experiment (LDX). Without these two facilities, MIT will lack the facilities to continue providing experience to students doing experimental fusion research… But the problem with the proposed reductions is much broader and more serious than just the role and future of the MIT program. Reductions in other areas, such as High Energy Density Laboratory Plasmas, theory, and systems studies will not only result in a loss of valuable talent and expertise throughout the US fusion program, but will also mean that research results these people and facilities would otherwise provide in the coming years will not be obtained [3].

The best plan for the US is to continue domestic research so that when ITER comes online, the country will be able to perform useful experiments whose results could be used to benefit the smaller tokamaks within the country. Also, if ITER proves successful and domestic funding is strong, the US could start building a demo fusion reactor before any other country, keeping the nation in the lead for technological advances. The US needs to be more involved in the ITER program and ensure that it is being led by competent, experienced people who will finish construction on time and within budget. This would include pressuring the other countries to finish their contributions in a timely manner, which can only be done after the US gets its own affairs in order. If the US can find a way to increase its budget for domestic fusion programs and fund ITER at the full promised amount, this could incentivize other countries to do the same. If the US can do this, ITER could be completed on time (the November 2019 timeline) and the US would have a strong domestic program to back it. There would be plenty of US scientists and researchers ready to learn from ITER and apply that knowledge to the domestic program. This would greatly benefit the US if ITER's operation proved that fusion reactors were a viable energy source. From the US involvement in ITER and having the backing of a strong domestic program, the US would be on the forefront of new, clean energy technologies.

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