Critique of Fusion Energy Research

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Abstract

This paper gives a critical assessment of the prospects for fusion energy research. It first describes the basic physics of nuclear fusion and the plasma conditions required for a fusion reactor. It then briefly reviews the history of fusion energy schemes and discusses some of the persistent difficulties in both magnetic and inertial fusion energy. It concludes with some thoughts about the psychology behind the wildly optimistic claims for fusion energy.

"Fusion is the process that powers the sun and the stars, releasing vast amounts of energy that makes all life on Earth possible. When we bring the process of that power to Earth, it will bring about an age of safe, clean, and unlimited energy that will transform our planet."
Princeton Plasma Physics Laboratory: https://www.pppl.gov/about

"Culham Center for Fusion Energy is turning the process that powers the Sun into carbon-free, safe and abundant electricity for a cleaner planet."
UK Atomic Energy Authority: https://ccfe.ukaea.uk/

"Helion is building the world's first fusion power plant, enabling a future with unlimited clean electricity."
Helion Energy: https://www.helionenergy.com/

"The surest path to limitless, clean fusion energy"
Commonwealth Fusion Systems: https://cfs.energy/

1. Introduction

The quotes above are from the websites of four representative fusion labs, as of May 2023. At that time there was great enthusiasm for fusion energy, especially among recent startup companies such as Commonwealth Fusion Systems and Helion Energy. Even much older and larger government-funded fusion labs such as PPPL (US) and Culham (UK) seemed to share this dream of unlimited and clean fusion energy.

Here I will try to show why these claims for fusion energy are at best wildly optimistic and at worst delusional, dishonest, deceitful, or fraudulent. To use a rough analogy, building a fusion reactor is about as difficult as sending men to the moon. But making an economically practical fusion power plant is about as difficult as making money from a lemonade stand on the moon.

My graduate thesis advisor at Cornell justified fusion research in 1973 by saying that “we have to find out if it will work”. That was a good motivation then, but after 50 more years of worldwide effort and about 10 more generations of graduate students, it is unfortunately nearly certain that a commercially practical fusion reactor will never be made.

2. Is fusion energy real?

There is no doubt that nuclear fusion is real since the fusion reaction rates for hydrogen isotopes have been measured very accurately using test ion beams. However, thermonuclear fusion rates are significant only above an ion temperature T_i above about 50 million degrees Centigrade (or 5 keV in physics units), as reviewed in Ref. [1]. It is true that fusion energy powers the sun and stars, as hypothesized by Arthur Eddington in 1920 [2] and calculated in detail by Hans Bethe in 1939 [3]. Significant fusion energy was first generated on Earth in the early 1950’s in the Hydrogen bomb, but only by using a fission bomb to heat up the fusion fuel.

Serious research on controlled fusion was begun in the early 1950’s at several governmental labs worldwide. These programs were based in part on a hoped-for analogy with uranium fission reactors: fusion seemed like another promising new source of electricity. It is true that an almost unlimited and inexpensive supply of deuterium fusion fuel (heavy hydrogen) is available in water, and that the “burning” of fusion fuel at about 100 million degrees can produce energy without creating CO_2 or chemical pollutants.

The required high temperatures $T_i \geq 5$ keV were reached in tokamaks in the late 1970’s, where “tokamak” is a Russian acronym for “toroidal magnetic chamber”. But the main challenge for the past 70 years has been to do this efficiently: *to create more energy output from fusion than it takes energy input to maintain the fuel at these temperatures*. So far only a few fusion devices have gotten close to this “breakeven” point, as discussed below. However, breakeven is still very far from practical fusion reactor, due in part to the large cost and complexity of these devices.

3. Plasma physics

The very high temperature needed for nuclear fusion causes the fuel atoms’ electrons to become dislodged from the nuclei and become free to conduct electricity. This creates a new state of matter called a plasma, sometimes called an ionized gas, or fourth state of matter (solid, liquid, gas, plasma), which occurs above about 10,000 ºC. Plasmas can be seen on Earth in the glowing insides of neon sign tubes, toy plasma balls, arcs and sparks, and lightning bolts, and in the sun and stars. Almost all the scientific problems of fusion have to do with the complexity of plasma physics, and not atomic physics or nuclear physics.

Plasmas are complicated because their motion can be strongly affected by electric and magnetic fields, which in turn can be created or distorted by the plasma itself. Thus there is a strong self-organization of plasmas which makes them resistant to outside control. Fusion plasma control is made more difficult by the very high speed of the ions, which is typically a million miles per hour. Plasma motion also tends to be very unstable and turbulent, similar to the exhaust of a rocket engine as it lifts off the launch pad (but fusion plasmas are much hotter).

This rapid and hard-to-control plasma motion is the central scientific difficulty of fusion research. Encouragingly, it takes relatively little energy to heat the fusion fuel to the required high temperature, compared to the very large energy released by the fusion reactions. For example, Deuterium (D) and Tritium (T) ions heated to 20 keV can fuse to create 17.6 MeV of fusion energy. Thus, if there are no additional energy losses, the maximum possible fusion energy gain ratio is about (17.6 MeV/40 keV)~450. However, additional loss of hot plasma away from the reaction region cause the fusion energy gain to be less than 1 in almost all experiments so far. This gain ratio needs to be much greater than 1 (perhaps 20) to make a practical fusion reactor.

4. Requirements for a fusion reactor

The physics requirement for a useful thermonuclear fusion reactor was first published by John Lawson in 1957, as described in detail in Ref. 1. This criterion has various related forms, but basically requires that the fusion energy production rate within the plasma is greater than the plasma energy loss rate. For a D-T plasma in the temperature range of T_i=5 keV-20 keV, this requirement is approximately: n(ions/m^3) T_i(keV) τ_E(sec) $\geq$ 2-3x10^{21}. This "triple product" condition favors higher plasma densities "n", to increase the fusion reaction rate, and long energy confinement times "τ_E" to lower rate of plasma energy loss, whatever the mechanism.

The only physics which goes into this criterion is the fusion reaction rate vs. temperature, which depends on nuclear physics of specific fusion fuels and can (almost) never be changed. By far the best fusion fuel is D-T, which has the highest reaction rate from T_i=5-100 keV. The mechanism of plasma energy loss was not specified since it depends on the specific fusion plasma configuration. But since this energy loss is usually dominated by plasma transport due to turbulent diffusion, higher plasma energy confinement times favor larger plasma devices. This is the driving motive for larger fusion reactor devices; it simply takes longer for plasma to diffuse (i.e. leak out) across a larger-sized region than a smaller-sized region.

The triple product criterion $nT_i\tau_E \geq$ 2-3x10^{21}(ions/m^3)(keV)(sec) gives an approximate threshold for "scientific breakeven", at which the fusion power produced by a D-T plasma equals the plasma loss power. Of course, for a practical fusion reactor this product needs to be significantly larger, at least by a factor of x3-5. This condition can be met at very different plasma densities; for example, in magnetic fusion energy (MFE) the near-term goal is a relatively low n and high τ_E, roughly n=10^{20} m^{-3}, and τ_E=5 sec at T_i=20 keV (the density of air is about 5x10^{25}

atoms/m^3). However, in inertial fusion energy (IFE) the density would be much higher and τ_E much lower, roughly n=10^{30} m^{-3}, and τ_E=1 nsec at T_i=10 keV.

5. Plasma Instabilities

Plasma instabilities are the most difficult physics problem in fusion research. In general, these instabilities drive plasma out from the hot core toward the colder edge, just as heat rises in a flickering flame into the cool air above it, leading to a transport of energy. Even though this trend is a general physical principle, the details of plasma instabilities and their resulting energy transport vary significantly with plasma parameters and configuration. Unfortunately, there is no general theory which can predict the energy confinement time for a specific plasma configuration. This is why fusion experiments have improved mainly by trial and error for the past 70 years.

The biggest issue in plasma instability physics is turbulence. Like any liquid or gas which has a large velocity or temperature gradient, the motion of a fusion plasma quickly becomes turbulent. The turbulent plasma fluctuations have a broad range of space and timescales, with an average size scale much less than the plasma size and an average timescale much less than the energy confinement time. This causes random motion of small blobs of plasma, which results in spatial diffusion of plasma energy and a reduction in the plasma energy confinement time. The source for this turbulence will always be present since a very large temperature gradient is necessary for controlled fusion on Earth.

Plasma turbulence in MFE is very difficult to understand because it involves interacting fluctuations in density, temperature, electric fields, magnetic fields, and velocity, whereas turbulence in normal fluids such as water has mainly velocity fluctuations. Furthermore, at high fusion temperatures the plasma electrons and ions move independently of each other and have few collisions, so the details of their orbits and velocity distributions can become important. Ideally, to understand plasma transport in MFE the particles and fields should be calculated over ranges of at least 1000 in time (µsec to msec) and space (mm to m), which is very computationally expensive even for a small sample of the ~10^{23} particles. Hundreds of plasma scientists have been working for many years on increasingly complex and realistic computer simulations of plasma turbulence, but so far with only partial success. Reliable predictions about future fusion plasmas are beyond our present capabilities.

6. Some dreams of fusion

Fusion research has been going on for so long that past mistakes have largely been forgotten. The history of fusion has been discussed in many books such those in Ref. [4], but some examples of this history are mentioned below.

a) the Bomb: By far the worst consequence of fusion research is the Hydrogen bomb. The first H-bomb exploded spectacularly in 1952 after only a few years of intensive design. The details are still classified, but (fortunately) an H-bomb needs a fission bomb as a trigger. The developers of the H-bomb hoped they could tame fusion energy in reactors, as was done with fission reactors. The early optimism of the fusion pioneers is still remembered and influential. For example, I took a course from Hans Bethe at Cornell, who advocated a fission-fusion hybrid reactor. Marshall Rosenbluth, a student of Edward Teller and an advocate of ITER, told me that seeing one of the first H-bomb clouds reminded him of a "diseased brain". Andrei Sakharov, father of the Soviet H-bomb, was also the co-inventor of the tokamak about 1950, which is still the best controlled fusion device.

b) Richter: A German scientist Ronald Richter, working in Argentina, announced in 1951 that thermonuclear reactions had been produced in his lab. This became a hot topic of newspaper stories around the world, including one on the front page of the New York Times. However, no real fusion reactions had been created, and the project was discredited and cancelled in 1952. But the Times story prompted Lyman Spitzer, a plasma astrophysicist at Princeton, to conceive the idea of a stellarator magnetic fusion reactor in 1951, published after declassification in 1958 [5]. This led to the first stellarator experiments at Princeton, and eventually to the Princeton Plasma Physics laboratory (PPPL).

c) ZETA: The ZETA machine, a toroidal pinch at the UK Atomic Energy Authority at Harwell, did produce D-D fusion reactions in 1958. Their results were published in Nature [6], alongside some of the early results from US fusion researchers. There was frenzy of media coverage in England which promised "unlimited power from seawater" and "a sun of our own". However, it quickly became clear that the neutrons in ZETA were not of thermonuclear origin, since they were asymmetrically emitted in space, and so were not useful for a fusion reactor. The UK fusion effort moved from Harwell to Culham in 1965.

d) mirrors: The magnetic mirror is the simplest idea for a fusion reactor: a long magnetic tube with pinched ends, so that plasma particles bounce back-and-forth. It was invented in the early 1950's in both the USSR and Livermore (the center for US H-bomb design). However, mirror machine experiments had rapid plasma leakage and were filled with complex instabilities, which were both gradually reduced by clever modifications. Finally, the huge Mirror Fusion Test Facility (MFTF) was completed at Livermore in 1986, but then shut down on the same day due to funding competition from the tokamak at Princeton.

e) FRCs: The "field reversed configuration", discovered by accident in the late 1950's, is a beautiful theoretical idea spoiled by strong instabilities. The idea is a toroidal magnetic bottle like a free-floating smoke ring. It was first found in short-lived pinch experiments, and later imagined to be a perfect fusion reactor geometry. My first experience in fusion research was on an electron beam driven FRC at Cornell in the early 1970's, but it didn't work well and FRC experiments died out by the 1990's. They were reimagined this century by private fusion companies like TAE Technologies, and Helion Energy, but still don't work well when compared with tokamaks.

f) TFTR: The Tokamak Fusion Test reactor (TFTR) was the largest magnetic fusion device ever built in the US and began operation at PPPL in 1982. It was designed to reach "scientific breakeven" with D-T fuel in the 1980's. The machine was massive and overwhelming, even when it was not running. TFTR initially had a good confinement time of about τ_E=500 msec, but this fell disastrously to about 50 msec as more heating was applied. There were many difficulties, surprises, and failures. After years of trial and error, by 1994 TFTR obtained 10 MW of D-T fusion power with 40 MW of plasma heating power [7], still far short of breakeven. In retrospect, TFTR was at the limits of the capability, resources, and enthusiasm of the US fusion program.

g) NIF: The National Ignition Facility (NIF) is the largest laser fusion experiment in the world. It was funded by the US Defense Department to simulate H-bomb physics, but is also for used for research on inertial fusion energy (IFE). Construction began in 1997 and a D-T fusion yield of 10 MJ per shot with a fusion gain ratio of 10 was expected by 2012. The cost was about $5B. The best fusion yield achieved by 2012 was less than 1% of that expected. The yield increased to about 3 MJ by 2023, still far short of the original expectation. However, "scientific breakeven" was achieved, and ignition was claimed by NIF proponents. There are very serious problems with the feasibility of this technology as a fusion reactor, such as low laser efficiency, high cost of targets, target alignment and chamber clearing at high pulse rate, and tritium breeding.

h) ITER: ITER is a tokamak about 30 m high being built in France and the largest fusion device ever attempted. Its engineering design was started in 1988, construction started in 2013, and full D-T operation expected in 2035. It is being funded by the European Community and 6 other international partners and is expected to cost roughly $50B. Yet it is very far from being a real fusion reactor. ITER is expected to make 500 MW of fusion with 50 MW of plasma heating power for 500 sec pulses, but if this fusion power was converted to electrical power (which it will not be), it would barely be able to power itself. After the start of D-T it will need to be maintained remotely due to its intensively radioactive structure. ITER could be seriously damaged in a fraction of a second by a bad plasma "disruption", a large-scale instability seen in all tokamaks.

7. Dreams vs. reality

In my opinion, ITER will the biggest disaster in the history of science. It will probably never be completed due to design or construction failures or budget overruns. Even if it is completed, it will likely fail to reach its goal due to poor confinement, impurity radiation, wall erosion, water leaks, magnet failure, lack of tritium, or catastrophic major disruption. Even if it reaches its goal, it should become clear that this is not a good way to make electricity due to the huge cost, long downtimes, and high levels of radioactivity. Even if this was not clear, the gap between ITER and a practical fusion reactor is so large that it will very likely never be crossed.

Yet ITER is by far the most likely to succeed path to fusion energy, at least in MFE. Its design was based on the best experimental evidence worldwide and the most reliable technology available. Thousands of capable scientists and engineers worked on its design for over 20 years. There is no little or no new physics expected in ITER. If anything, the design is too conservative, since it is essentially the JET tokamak multiplied in size by two. But ITER is still an extremely large step: JET ended a 40-year run in 2022 with a 10 MW D-T shot of 5 second duration [8], but ITER is supposed to make 500 MW D-T shots with a 500 second duration.

Meanwhile, there are a few modest-sized national fusion programs which are doing some nice work. Rapid progress is being made in the EAST tokamak in China, which has made near-fusion-grade plasmas last for a record 1000 sec [9]. The Korean tokamak KSTAR is doing almost as well in a much smaller country, and the German stellarator W-7X is impressive in the complexity of its design and technology. These programs can be justified by the training of young scientists and by scientific curiosity, independent of a fusion reactor program. Many smaller national and university labs can be justified in the same way.

However, in my opinion NIF and other inertial confinement fusion (ICF) programs are chasing a delusion. There is no way that a NIF 3 MJ pulse is relevant for a fusion reactor: 3 MJ of fusion energy can be converted to just $0.10 in electricity. The idea of doing ICF explosions 100 times bigger than NIF (i.e. about 70 kG of TNT) at a rate of 10 times per second to make a 1 GW electrical power plant is ridiculous. The Livermore press conference [10] touting the 3 MJ fusion "breakthrough" in 2022 was disgraceful, with both gullible media and US Department of Energy (DOE) officials equally to blame. Much more likely than an ICF reactor would be discovery of a new method to trigger a large H-bomb without a fission bomb. This is what NIF is trying to do, but with a very small bomb and a very large driver. In this respect ICF research is dangerous.

Private fusion energy companies such as Commonwealth Fusion Systems (CFS) or Helion Energy which claim to be developing practical fusion power are either deceitful or fraudulent. Many of them are based on ideas developed in cancelled government-funded programs. For example, the SPARC tokamak at CFS [11] is based on earlier DOE-funded designs for a tokamak after TFTR; namely, CIT (compact ignition tokamak), BPX (burning plasma experiment), and FIRE (fusion ignition research experiment). Helion Energy and TAE Technologies are based on DOE work on FRCs by scientists from Los Alamos, the University of Washington, and Cornell. These company founders apparently wanted to continue their research and found that there were enough gullible rich people to fund them, but only if they promised fusion power soon enough.

Clearly these claims of fusion power within 5-10 years are deceitful, since these company founders must know this cannot be delivered. They are probably also fraudulent, where fraud is defined as "deception intended to result in financial or personal gain", since they chose not to be nonprofit companies. Whether they are legally fraudulent has yet to be decided in court.

Some fusion companies like General Fusion of Canada seem to venture well beyond delusion or deceit. Their idea of using massive pistons to compress fusion fuel seems ridiculous, as does the "power of pistol shrimp" touted by First Light Fusion of Oxford, which is "working towards a pilot plant producing ~150 MW of electricity and costing less than $1 billion in the 2030s." It is difficult to compress plasma ions moving at a million miles an hour with a piston moving even at 1000 miles an hour.

Large government-funded fusion labs such as Culham or PPPL sell the long-term promise of fusion to their sponsors every year, but they are certainly not deluded by the dream of near-term fusion energy. Instead, they delicately position these labs in the twilight zone between optimism and deceit by claiming that there might be a practical fusion reactor in about 20-30 years. These labs have been sustaining this optimism for over 60 years.

8. Some specific issues

There is no single physics or engineering issue which completely prohibits a fusion reactor. Instead, there are many independent problems all of which need to be solved together, making a practical reactor nearly impossible. This section describes some of these specific problems of the tokamak, the most successful magnetic fusion device since the 1950's. Over a hundred tokamaks have been built around the world, and all these problems have been known for at least 40 years.

a) Energy confinement:

Plasma energy confinement has been the main physics issue in tokamak research since the 1950's. For example, the energy confinement time of ITER needs to be $\tau_E \geq 4$ sec to achieve its goals, which is about 10 times higher than that of the largest existing tokamak JET. The ITER energy confinement predicted from existing tokamak data using "empirical scaling" is about $\tau_E = 3.0 \pm 0.5$ sec [12], which is marginal for its success, and even higher confinement times are needed for a reactor. Confinement cannot yet be predicted accurately from plasma theory due to the complexity of the small-scale turbulent transport.

b) Impurity contamination:

Impurity ions in the plasma core originating from the vessel wall or from helium "ash" from D-T reactions will reduce the fusion power for a given plasma configuration. The impurity fractions are almost entirely unpredictable in ITER due to the uncertainties in the plasma-wall interaction and the impurity ion particle confinement. The recent fusion performance of JET with tungsten walls as needed for ITER was generally worse than with previous carbon walls due to increased atomic radiation. There is presently no demonstrated method to preferentially remove impurities or helium ash from a tokamak, so the level of impurities in future tokamaks may be unacceptable.

c) Disruptions:

The most dangerous tokamak instability is a plasma "disruption", which causes a very rapid (few msec) loss of the entire plasma energy and plasma current to the wall. Disruptions occur in all tokamaks and can cause extremely large electromagnetic forces and heat loads on the vessel and wall components. Disruptions are caused by large-scale plasma instability, usually triggered by exceeding the plasma density, pressure, or plasma current limits, all of which are near the operating range of ITER. Prediction and mitigation of disruptions is planned, but it is still possible that a single large disruption could significantly damage the ITER tokamak or any future tokamak reactor.

d) Wall erosion:

There will inevitably be a gradual erosion and redeposition of the internal tokamak walls due to plasma heat and particle loss. The location and rate of this erosion are difficult to predict or control since it depends on largely unknown turbulent transport loss in the edge plasma. Excessive wall erosion or cyclic stress could lead to a leak from the water cooling lines just below the walls, which would immediately shut down operation. A serious water leak inside the vessel during operation could cause a loss of coolant accident leading to a steam explosion, which would disable the tokamak. The intractability of this problem has led to a proposed solution of flowing liquid metal walls inside the tokamak.

e) Magnet failure:

ITER will have the largest and most complex set of superconducting magnets ever built, many of which need to be pulsed every shot. All these magnets must be cooled with liquid helium and restrained from huge electromagnetic forces. These magnets can fail due to coolant leaks, mechanical stress, or electrical arcing. Most tokamaks have had magnet failure, including the recently built Japanese superconducting device JT-60SA [13]. It would be very difficult or impossible to repair or replace any of the major coils of a tokamak reactor after D-T operation, since the whole structure will be radioactive, necessitating full robotic maintenance.

f) Tritium inventory:

The tritium fuel for D-T tokamak reactors needs to be created in on-site breeding blankets located outside the plasma but inside the toroidal field coils. This is theoretically possible using neutron-lithium reactions with neutron multipliers such as beryllium. The design of these blankets is extremely complicated due to neutronic, thermal, and mechanical interactions, and none has been tested so far in a D-T neutron environment. There will be an in-vessel tritium inventory limit of only a few kg due to radiological safety, so periodic removal of tritium from the vessel walls, ports, and dust inside the vessel will be needed, which again needs be done robotically.

g) Radiation damage:

In a tokamak reactor the first wall will be subject to very high 14 MeV neutron radiation loads, typically a few MW/m^2 over many years. This will eventually cause radiation-induced damage of the structural materials, typically measured as the average number of displacements per atom of the material lattice (perhaps 100 dpa). Radiation damage causes changes to metals such as softening, swelling, and helium embrittlement which could eventually result in structural failure of the wall. It might be possible to develop new radiation-resistant wall materials, but no good candidates have been proven yet.

h) Availability:

A practical tokamak reactor ought to be operated with a full power availability factor comparable to other electrical power plants, which ranges from nuclear fission at >90% to solar at 25%. At present the longest D-D tokamaks pulses run for about 1000 sec a few times per day, or <5% of the time. Full-power operation of ITER is planned with 500 sec pulses at perhaps 2 per day, or also <5% availability. Long shutdowns are also expected in ITER due to the difficult repair and maintenance needs. An order-of-magnitude increase in availability is needed for a tokamak reactor, which is difficult since expensive external current drive will be needed for long pulses.

i) Safety:

A tokamak reactor will have at least few kg of tritium and radioactive dust inside the vacuum vessel, and so a public evacuation plan will be needed in case of a vacuum accident. A tokamak reactor will also create a huge amount (thousands of tons) of low-level radioactive waste due to neutron activation of the interior walls, which will require a long-term decommissioning and storage process. Finally, a fusion reactor poses a threat of nuclear proliferation since fissile plutonium 239 can be made by placing natural or depleted uranium near neutrons. Therefore the machine would need to be very carefully monitored to prevent clandestine use.

j) Cost:

Assuming a tokamak reactor could be built to produce net electricity, it will be practical only if its cost of electricity is comparable to that from other sources. This seems extremely unlikely based on the $50B cost of ITER, which cannot produce any net electricity. The preconceptual design for the European tokamak DEMO reactor is sobering in its complexity [14], with a 40-year timetable for net electricity production, but with no attempt to assess the cost. Given the simplicity and falling costs and of solar and wind power, it is highly unlikely that a tokamak reactor could ever be cost competitive.

8. Psychology of fusion

Some optimists believe that a fusion reactor can be realized by a brilliant new idea or by a determined effort, like Edison's light bulb or the Wright brothers' airplane. Other optimists hope that fusion could succeed with an engineering *tour de force* like the Channel Tunnel or the International Space Station. Hasn't every technological challenge been overcome? Who can deny that a great fusion breakthrough is possible?

Technological optimism has been the driving force for the fusion reactor program since the early 1950's. For most fusion scientists it doesn't matter that the reactor goal is many years away. Gradual progress has been made by developing better understanding and bigger machines, and old concepts such as the pinch and mirror machines were largely left behind. Fusion research is full of interesting challenges over a wide range of areas: theoretical physics, large-scale computing, experimental planning, machine building, plasma diagnostics, data analysis, and engineering of all kinds. The fusion field has provided enjoyable day-to-day work and long-term employment, so if the funding continues people will continue to chase the fusion dream.

Fusion program leaders have become adept at gaining government support for their long-term programs. There has never been a public admission that a fusion reactor was out of reach, just that it required more time and more money. The machines have become larger and fewer, and some older labs like Oak Ridge and Los Alamos were phased out in favor of single-purpose labs like PPPL. Remarkably few fusion scientists have told the hard truth about the difficulty of fusion [15]. Dissent was discouraged as fusion work became more collaborative both nationally and internationally, eventually leading to ITER. The shrinking scope of fusion research helped stimulate the private fusion start-up companies, which brought back some of the naïve enthusiasm of the 1950s, along with many of the same mistakes and delusions.

There is a partially hidden psychological aspect of fusion research. During the 1949 debate about whether to build the H-bomb, Enrico Fermi wrote [16]: "The fact that no limits exist to the destructiveness of this weapon makes its very existence and the knowledge of its construction a danger to humanity as a whole. It is necessarily an evil thing considered in any light." Fusion bombs have the power to destroy a large city in seconds. Proximity to this evil seems to breed arrogance and overconfidence. This is especially true in the US, where the custodians of the H-bomb extract endless government funding for the "stockpile stewardship" program, which supports NIF and other such work. This psychological proximity to the H-bomb has subtly haunted all fusion reactor dreams.

But surely there must be unanticipated technological spin-offs which make fusion research worthwhile, even if we don't succeed in making a fusion reactor? Unfortunately, there have been none so far. It is true that low temperature plasmas are very useful in many applications, such as in chip making and plasma processing of surfaces. But there have been no applications for the high temperature (keV) plasmas used in fusion research. The main societal benefit of fusion research has been friendly international collaboration, which began shortly after controlled fusion was declassified in 1958. Fusion research has enjoyed an almost unique worldwide community spirit because the goal of fusion energy is so distant.

9. Conclusion

There are three types of obstacles which need to be overcome to make a practical fusion reactor: plasma physics, nuclear concerns, and cost. Unless these are all solved together there will be no fusion reactors.

The plasma physics obstacles can be overcome by building even bigger and more expensive machines. The problems are mainly due to instabilities of the hot plasma, which cause the plasma energy to leak out into its cool surroundings. Both ITER and NIF will be near to achieving the necessary fusion conditions, as described in Sec. 6. Maybe one or two additional generations of larger devices in each case could overcome the plasma physics obstacles, assuming no cost or time constraints. Possibly some new ideas for plasma confinement could help.

The nuclear concerns are a much bigger obstacle. A D-T fusion reactor will be extremely radioactive, whether operating or not, and can only be maintained by remote control, which is very difficult. The inside walls will be damaged by the neutrons over time, and the whole structure will be left radioactive after plant closure. There is some danger of radiation release and of nuclear proliferation. Non-D-T fuels might reduce these problems, but then the plasma physics obstacles could probably never be overcome due to the much higher temperatures and confinement times required.

The fusion reactor cost is by far the biggest obstacle. It is very difficult to imagine that a practical fusion reactor could ever cost less than either ITER or NIF, neither of which can make net electricity, and both of which cost more than a fission power plant. Despite some highly optimistic fusion reactor design studies, there is no way that a large and complex fusion power plant could possibly compete with solar or wind energy, powered by the fusion reactor in the sun.

Unfortunately, these huge obstacles mean that a fusion power plant is just not going to work in the foreseeable future.

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