Fusion: Some Like It Hot

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Abstract — Fusion research is the longest, most expensive, most scrutinized failure of physics or of any sort of science. Why is the war on cancer, or research on consciousness, gravity waves, etc. and fusion such a disaster? We shall cover the subject from seven different angles, by seven authors through the eyes of outsiders, insiders, optimists and pessimists. What we see is the common root of all blunders, let it be politics, high finance, or military missions.

We shall see ample examples of plain lies, over and underestimations, hubris, stupidity by committees, and endless strings of cover ups. As for fusion, it is obvious: physicists don't read patents, are not interested in mother nature's inventions like biological transmutation. If they ever listen to internal dissent, it is only to suppress criticism. If it comes from the outside, it is simply ignored. All in all, science as an institution collapsed in its real meaning.

When will the general public realize that the emperor is naked? When will the community of physicists realize that thermonuclear controlled fusion is a dead end but other "tabletop" forms are possible? Will they ever realize that apart from the D-T reaction there are a number of other possible fusions/transmutations? There is hardly any open debate on these issues, though our future as a civilization depends on it. At such crossroads, science as an institution always failed. This institution failed to recognize airplanes, transistors and magnetic monopoles, ether in the early 1920s, antibiotics in the 1880s, longitudinal radio waves in 1890, etc. These lessons of history are forgotten like all previous ones. We shall keep on repeating them, till the last drop of oil...

Two theoretical physicists stand in front of a firing squad. The officer in charge grants their last wishes. "I should like to tell about my theory of cold fusion," says one of them. "Shoot me first," says the other.

This little joke characterizes the state of affairs of fusion research—and intolerance in general.

The $200-odd-billion, 60-year-long, most scrutinized, least successful yet most important field of physics is in permanent turmoil. Tension and stakes are high and increasing. How come? Why has hot fusion become such a big project, yet such a fruitless one?

Recently published books yield a rare opportunity to have a look "behind the curtain," into the minds of leading decision makers in hot fusion.

There are two things on which all fusion researchers agree:

1. Fusion is important.
2. The sky is blue.

In terms of media weight, employment, public attention and public investment, cold fusion is completely invisible compared to hot fusion, yet in terms of results, the reverse is true. Hot fusion never broke even, to say nothing about ignition (self-sustaining fusion process), while in LENR "heat after death" is observed from time to time (self-sustained heat production with no input power).

Worse, it is predictable that hot fusion will never achieve a break-even point, neither in the tokamak nor in inertial confinement schemes, but there is still some hope for the electrostatic confinement process.

This situation is insane and irrational. But this is tue everywhere in science as in all of our other major failed research lines: cancer research, mind/brain research, or gravity wave research, etc.

The age of results and spectacular success is over in all areas of science. This is the background from where fusion research is to be observed.

And there is a further difficulty due to a golden rule: In a large project, there are only two objectives out of three which can be achieved: 1) the task, 2) the required funding, 3) the allotted time. That is if the aim and funding is fixed, the time cannot be determined.

For controlled nuclear fusion, the "price tag" and the required time vary by orders of magnitudes, if the required fusion is specified a priori. If "hot" fusion is required, only the Farnsworth-Hirsch (or Bussard type) electrostatic confinement methods have any chance of working economically. The rest is extreme engineering. Tokamak and laser fusion are expensive, useless "white elephants," riddled with a string of turbulent and Bernard-Rayleigh type of instabilities.

Diligent readers of books on the subject might reach the above conclusion if they read between the lines of the following books:

— Daniel Clery, *A Piece of the Sun: The Quest for Fusion Energy* (2014, a bird's eye view of a realistic science writer)
— Stephen O. Dean, *Search for the Ultimate Energy Source: A History of the U.S. Fusion Energy Program* (2013, memoirs of a leader, an insider)
— George H. Miley, *Life at the Center of the Energy Crisis: A Technologist's Search for a Black Swan* (2013, report from the front line of all fusion research)

And to a smaller extent there are four more recent books on the subject, also with important information:

— Francis F. Chen, *An Indispensable Truth: How Fusion Power Can Save the Planet* (2011, a cheering optimist)
— W.M. Stacey, *The Quest for a Fusion Energy Reactor: An Insider's Account of the INTOR Workshop* (2010, report by a

typical committee man)
— Charles Seife, *Sun in a Bottle: The Strange Story of Fusion and the Science of Wishful Thinking* (2008, pessimistic science writer)
— David Goodstein, *On Fact and Fraud: Cautionary Tales from the Front Lines of Science* (2010, a realist loaded with self-doubt)

The Emperor is Naked

Daniel Clery, a physicist by training, and professional science writer, has covered the history of (hot) fusion worldwide since the end of World War II. In fact, he wrote a brilliant thriller on the subject, maybe the best book in my opinion on any science related subject—informative, witty, with realistic coverage.

In *A Piece of the Sun: The Quest for Fusion Energy*, Clery doesn't hide the tensions, the lack of results and the wastefulness of hot fusion projects. For example, he described how a tandem mirror machine at Lawrence Livermore Lab (MFTF-B) was decommissioned right after the inauguration ceremony. It was nine years in the making, costing $372 million, and was ready to go. But it has not been tested for even one minute due to a shift of taste at the DOE.

The Russians had a similar act: their T-15 tokamak was decommissioned after a long construction process in 1991, because they couldn't afford the liquid helium to cool the superconducting magnets. This was due to the collapse of the USSR, but the Russian program never recovered fully. (A similar accident happened at Princeton's Plasma Physics Lab as well, after spending some $170 million.)

In my opinion, any of these expenses alone would have been enough to cover the funding of several successful LENR projects.

As we learn from this book, politics at the highest level makes this funding process possible, or impossible. Even the name of projects are decided at the top level, like ITER—as tokamak sounds too Russian.

Clery clearly writes about the rise of the tokamaks, the change of "fusion fashion," or the "revenge of Russians," as it was later termed.

Site selection of the fusion devices is also at the mercy of high politics, *e.g.* Culham was selected for the JET (Joint European Torus) due to the success of a British anti-terrorist team (SAS) at the Mogadishu airport in 1977. Squabbling European politicians nearly disbanded the Culham team, because each wanted the construction site in their own backyard. The stalemate went on for years until the Mogadishu raid.

Clery also writes about the managerial incompetence of ITER leadership who were all new to hot fusion projects. A management structure designed by a committee of international bureaucrats "which is a millstone around the young organization's neck."

By now it is well known that the ITER project is way over the initial budget, and no one dares to predict a completion date. Only the objective has remained the same.

U.S. inertial confinement (mini H-bomb explosion by laser heating) has a similar troubled history. The National Ignition Facility at the Lawrence Livermore Lab has a final price tag of $4 billion, four times the original cost estimate, and twice the estimated time. Lawsuits and management scandals delayed the construction. Stephen Koonin, a sworn enemy of cold fusion, was at the helm of the oversight committee.

But the real trouble is with the heat (energy) output. A special index, called "experimental ignition threshold factor" (not the efficiency), was used to measure the energy balance. That is, the ratio of incoming laser beam energy versus the generated fusion energy is measured. (Since the efficiency of giant neodymium glass lasers and amplifiers is about 1-2%, it will never be suitable for any meaningful energy production.) But even so, the incoming laser beam energy is about 50% more than the fusion energy output.

This index must be around 1000 to have any meaningful economy. A million times higher repetition rate is needed then we have today—one shot per day.

It is quite clear that inertial confinement will never be a practical energy producing solution, whether it be direct beam heating or indirect heating. The secret simulation software predicted otherwise. It predicted ignition, that is self-sustaining energy relations. It turned out that the National Ignition Facility has no ignition, not even to break-even.

Clery leaves this conclusion to the reader, but it is readable between the lines: the emperor is naked.

Knights, Clowns?

Knights or clowns on a quest for the "Holy Grail of Energy"? Stephen Dean was a top ranking leader of the U.S. hot fusion programs. His book, like a memoir of a military leader, is a top down view of U.S. fusion history. As early as 1972, he became assistant director of magnetic confinement systems, under Robert L. Hirsch and R.W. Bussard, who pioneered the "electrostatic potential well" method. (R.W. Bussard later became a critic of the usual thermonuclear programs, and worked on secret U.S. Navy projects. He was more afraid of fellow U.S. plasma physicists than Russian spies.)

Dean's book is a factual account of the U.S. hot fusion programs. Had it been more flexible and more open intellectually, the program could have been a success, because both Hirsch and Bussard were full blooded researchers, and all three former leaders were of high intellectual capability.

But this is the essence of a real drama with very high stakes: what makes good guys turn into intellectual monsters? How and what made decision makers turn a noble aim—controlled, peaceful fusion—into a nightmare, a ruthless suppression machine?

There is a very strong parallel between the history of hot fusion and authoritarian regimes, or even the history of religions. The common background seems to be the fight for limited resources with the highest return in the shortest term. This risk averse, intellectually oppressing climate is apparent through Dean's book, if one reads between the lines.

The internal fight for the limited and dwindling funds is apparent in lots of interim reports, committee reviews and bureaucratic planning.

The book is an excellent collection of evidence of shallow thinking in fundamental physics, and financial back stabbing and arm twisting. The structure of the book details this in Chapter 5 ("The Carter Plan vs. the Reagan Agenda: 1980-1985") or some subtitles from Chapter 8, like "The 1995 PCAST Fusion Review, Congress Takes Aim, Industry Groups Respond, Fusion Energy Mission Abandoned, TPX Reborn as KSTAR, 1997 PCAST Energy Report," etc.

The reader will get a detailed history about the start of some inertial confinement devices and the tokamak effects. An old joke came to my mind after reading the committee and panel reviews leading to their fruitless efforts: What is a two humped camel? A horse designed by a committee.

So we have a history of committee reports but the physical fundamentals of the hot fusion process is never questioned. This is apparent from Figure 1.3 (page 7) where the reactivity probabilities of fusion reactions are compared to each other, and—no surprise—the D-T reaction is the winner, at about 80 KeV ion temperature.

Between the lines this means that no electrostatic shielding and electrostatic confinement, nor anything else, is considered, only thermonuclear fusion—good old H bomb physics.

Cold fusion (LENR) is not mentioned at all. Clery at least devotes a short chapter to the usual Fleischmann-Pons story (but nothing beyond it).

There is a sub-story in the book about the fight for public support, and a broader publicity and influence in the circles among influential law-makers.

Fusion Power Associates, established by Dean, has institutional members among them like Boeing, General Atomics, and universities like UCLA, Wisconsin University, University of Texas, and all major national laboratories. No wonder, cold fusion advocates are not in this heavyweight group, so this can't be their game.

There is a relevant remark in the book about internal criticism. Dean writes, "In reviewing many proposals over the years, I have observed that it is nearby impossible to get a positive review of a proposal to pursue any idea that is not already being worked on in the government's own fusion program." (A.k.a. — there is nothing new under the sun.)

This sentence is a frank statement about the state of affairs. Dean writes the above statement as their panel turned down Hirsch's proposal for a portable resonant electrostatic confinement device, despite the fact they were impressed with the quality and the results of the work, as they witnessed it to work in their own office.

The highlight of this research history book is Chapter 14, "Perspectives 2012," the opinion of the state of affairs by 14 experts, who were in leading positions in several hot fusion research programs in the U.S. For that alone it is worth reading this book, and the author must be thanked for this idea. Some telling sentences are worth quoting.

Charles Baker, a former director of hot fusion projects at the Argonne and Oak Ridge National Labs, and the U.S. ITER team, says: "We have not been serious enough in the USA in pursuing the potential promise of fusion energy...Insufficient progress has been made on the nuclear, materials, and systems sciences needed for fusion energy...The fusion community carried out a large number of studies…filling many book cases with excellent reports...However the collection of such studies done by advisory and ad hoc groups doesn't constitute a viable strategy..."

Finally, Baker notes, "An essential strategy for fusion must include developments, steps and perhaps smaller fusion energy systems."

N. Anne Davies, Director of DOE Fusion Energy Sciences, writes: "The fusion program is arguably the most reviewed program in the federal government, and every review has found the science to be of high quality, the promise of fusion worth the cost, and the program deserving of additional funding."

William R. Ellis, former head of magnetic mirror and bumpy torus programs, chair of ITER Industry council, said: "…how shortsighted some of our government's high level decisions have been in science and technology areas, including fusion…The federal government has shown itself to be an unreliable partner in supporting high-tech, nationally important R&D programs over the long term, contrary to what many of us expected—and contrary to what in fact we were told to expect by government."

Richard D. Hazeltine, former chair of the DOE Fusion Energy Science Committee and chair of the APS Division of Plasma Physics, wrote: "The campaign to achieve fusion power production is characterized above all by the huge size of its necessary first step. No one has found a table-top prototypical fusion reactor that proves the concept." (Well, except, Tesla, Papp, biological transmutation of bacteria, Moray, Meyer, Chernetzky, Correa, Pons and Fleischmann, Mizuno, Zhang, Arata, Rossi and about 200 others.)

Robert L. Hirsch, former director of the U.S. fusion program, and executive at Exxon and EPRI, wrote: "Tokamak fusion will almost certainly fail to become a viable commercial electric system…engineering considerations of the tokamak fusion turn out to be commercial showstoppers…D-T fusion is not clean as often claimed…"

Dale M. Meade, former leader of the Princeton tokamak program, wrote: "Since the budget collapse in the mid-1990s, the U.S. fusion community has lost its focus on the fusion goal, slipped into a 'no, you can't' approach that dwells on problems instead of finding solutions."

Richard F. Post was a pioneer of the U.S. magnetic fusion program at Lawrence Livermore. In his piece, subtitled "Thoughts After 60 Years of Involvement," Post wrote: "Attitudes must change from tokamak, ITER and its follow-on demo represents the only sure path to magnetic confinement fusion power" to: "the surest path to practical magnetic confinement is to use the trail markers and warning signs that have been established in the last 60 years to define a broadly based R&D program that supports and welcomes the study of promising approaches."

Fred L. Ribe, former head of the fusion program of Los Alamos National Lab, wrote: "ITER is advancing toward a large, low beta, low power density reactor. It is unfortunate that means have not been found to advance the high beta magnetic fusion concepts of the 1950s to 1980s to the reactor regime. Their beta values are an order of magnitude higher than that of the tokamak, and their power densities are two orders of magnitudes higher, which would result in smaller fusion reactor cores...It is important to have a diversified fusion program in case the tokamak doesn't fulfill present expectations."

John Sheffield, former director of the Fusion Energy Division at Oak Ridge National Laboratory, wrote: "We should not forget four important points: To be careful how we present fusion in relation to other energy sources; With D-T reactors, the challenge of developing radiation resistant materials may be greater than producing net energy; The ultimate fusion fuel is deuterium; High availability will be difficult to achieve in a complex fusion reactor."

Alvin W. Trivelpiece, former director of the DOE Office of Energy Research and Oak Ridge National Laboratory, wrote: "An infinite amount of money could not produce a commercially viable fusion power system delivery tomorrow...I doubt that when a working fusion reactor is built that there will be a detailed understanding of processes that can be explained by quasi-linear perturbation theory or magneto hydrodynamics."

And to conclude here are four insightful remarks from Dean himself:

- "The U.S. fusion budget remains on a track that doesn't lead to a U.S. demonstration fusion plant ever."

- "Over 10 MW of fusion power was produced for a few seconds in the mid-1990s in the Tokamak Fusion Test Reactor in the USA and the Joint European Terms (JET) in the U.K." (And nothing better since then!)
- "ITER, for example, is designed to operate at the 500 MW level but only at 15 minutes at a time."
- "National Ignition facility is expected to release 5-10 times more fusion energy than the energy input from the laser on a single shot." (It turned out to be a fiasco).

I guess the reader has got a glimpse about the slightly divided views of mainstream opinions about hot fusion, but there is an agreement on two major issues: 1) More funding is needed. 2) Only thermonuclear fusion exists. When fusion is mentioned, "cold" fusion is never considered; it simply doesn't exist.

All Quiet on the Fusion Front

George Miley wrote a kind of "corporal's battlefield report" from the trenches in the war on energy, *Life at the Center of the Energy Crisis: A Technologist's Search for a Black Swan*.

A world apart from the former two authors, Miley has "hands on" experience in fission reactor design, laser physics "ordinary hot fusion" in inertial confinement fusion and inertial electrostatic confinement fusion (Farnsworth-Hirsch) and low-energy nuclear reactions.

In this latter quality he impressed me with transmutation results with the Patterson cell. In a "normal" world he should have received the Nobel Prize for this work long ago. (But this world is insane, as we know.) This is a refreshing book, full of insights on the works of nature, and we see an exceptional openness.

Miley seems to be everywhere and to be involved fairly successfully in many novel areas. (I just recalled half of them.) He is a man of personal integrity. As the editor of *Fusion Technology* he allowed the publication of papers on "cold" fusion as well. The adjective was not important for him, only the quality of the submitted papers. No one has entered into his footsteps ever since then.

Miley spent most of his long career at the University of Illinois, which is also an exceptionally rare, open-minded place of study, where only quality counts. As for any LENR related work at other places, immediate firing would be the reward. Had he chosen to leave the university, Miley could have founded a sort of nuclear Apple or Microsoft, but he had insight in science, not in the business vein. He had the talent of a diplomat as well to get along with hundreds of students and fellow researchers.

This book has a starkly different angle from the first two and so makes the fusion research chronicles three-dimensional and life-like.

I highly recommend this interesting book, especially for the details in the LENR related memoirs. But a few quotes about inertial electric confinement (IEC) are interesting:

- IEC is well suited for burning advanced fuels like tritium-helium, ^{3}He – ^{3}He and proton-boron.
- With the beam-beam type of fusion possible in the IEC, non-equilibrium electrons have a much lower effective temperature than the ions, bringing down the radiation losses.

Miley describes a case when an EPRI panel of experts selected IEC with advanced fuel as the most promising plan to achieve fusion, because it is much simpler than any tokamak. Yet DOE remained silent; they don't trust and they are not interested in any new concepts.

Though IEC can be studied at the fraction of the cost of the rest of ITER, bureaucrats far from the laboratory environment are averse of any risk. This is a fatal error. Miley has the courage to criticize this risk-averse attitude while being on the firing line.

Miley's encounter with Jim Patterson (CETI) is perhaps the most exciting part of the book. They had a pebble bed, multilayer, high surface area cathode, with superior performance. It worked in a reliable manner, and Patterson was awarded several patents. He had a good working relation with Miley, which yielded exciting transmutation results.

Then the mirage vanished, as Patterson was unable to again manufacture the cathode balls which performed well. This was a missed opportunity to create a LENR giant, a "nuclear Apple." If the old, well performing balls had been scrutinized, and analyzed by every type of electron microscope, they could have been successfully reproduced.

Miley has tried super smooth surfaces with no results. Thus in hindsight, the other direction should have been followed using rough surfaces to the nanometer level. These unfinished possibilities make the book exciting, life-like, worthy of reading and contemplating.

Anyway the transmutation work didn't catch the attention of the cold fusion community. (Except mine – I was sitting in a back row at the Toya meeting.) Miley writes: "Once the original Pons-Fleischmann experiments came under attack, mainstream scientists questioned all cold fusion work, saying that it defied the fundamental laws of physics. Cold fusion researchers responded that these scientists did not have an open mind. However now when something new came along in cold fusion, the cold fusion scientists themselves did not keep an open mind...The unusual aspect was that many of the reaction products could only be explained by assuming multibody reactions, *e.g.* multiple protons and Pd or Ni reacting as apposed to normal single body p+Pd type reactions."

About the future of LENR, Miley writes: "Some workers in the field take the view that if LENR is developed, hot fusion will not be needed. That puts the two into direct competition for the same R&D money and puts cold and hot fusion scientists against each other. I personally view LENR as an alternative approach to nuclear energy. However, I have always felt that both hot and cold fusion, once developed, will assume extremely important niches in the future energy economy."

Here, with due respect, I don't agree because flapping wing airplane ventures stopped when the Wright brothers flew above Kitty Hawk. And tokamak is as practical as a flapping wing airplane.

Though the subject of these three books (Clery, Dean, Miley) is mainly hot (thermonuclear) fusion, in fact they are also about tolerance, inquiring minds, and openness, the very essence of science as method, versus science as an institution.

Pride and Prejudice

Internal criticism is mentioned only in Clery's book. Two MIT eggheads came up with a problem they found with a computer simulation of tokamaks. True, the bigger the device, the less the overall energy loss. But at the same time, magneto-hydrodynamic instabilities will grow to an uncomfortable level, making even the break-even threshold improbable. But their voices were thin and apparently no one listened.

But the arguments of Lawrence Lidsky, a professor of nuclear engineering at MIT, were louder. Lidsky mentioned the following troubling issues:

1) In D-T fusion, highly energetic neutrons mean considerable energetic loss, weaken the steel structure and make the surrounding materials radioactive.
2) A more sensitive point is that all tokamaks are too big and complex, prone to system failures, therefore economically they will not be a viable option. The reactors are too large and have excess parameters so they are expensive to build and maintain.
3) Finally he proposed (along with Miley) the proton-boron reaction instead of the D-T. He published his view not only in *MIT Technology Review* but also in *The Washington Post*. A long debate followed. As usual the hot fusion community closed ranks, and consequently he was fired from his position and became a pariah.

A somewhat similar internal fight took place in the USSR at the same time between the fusion groups of Pjotr Kapitsa (resonant magnetic pinch containment) and A. Sakharov (tokamak). Kapitsa's group was completely ignored (despite his Nobel prize, and being the Academy's president), and after his death his research group was disbanded.

Hot fusion is a state within (and above) all states. As in all authoritarian states, criticism is dealt swiftly and efficiently with eternal silence.

In the public media, the criticism is rare and modest toward hot fusion, yet harsh, swift and unforgiving toward LENR.

None of the above authors deal with the issue of moral and legal responsibility. In the U.S., which is the home of (sometimes ridiculous) legal suits, strangely no one took the federal government or any project leader to court for the apparent waste of public funds.

The public is blind, though the emperor is naked. This fundamental "state of the fusion" assessment issue is missing from all these books. Of course only Miley has the necessary breadth of knowledge to judge the state of affairs but is still lenient toward thermonuclear fusion.

There are ample parallels in history. For example, in World War II after the battles at Moscow, Stalingrad and Kursk, the German Army was in a continuous retreat; the war was lost. Only a small circle (near to the pork barrel) believed in victory. Along the same line, during the U.S. Civil War, the war was lost for the South after Vicksburg and Gettysburg, but they kept on fighting for years.

Lidsky's criticism was the Stalingrad point for hot fusion, and the NIF's failure to reach break-even is the analogue to the defeat of Kursk or Gettysburg. When Russian soldiers fought in the streets of Berlin, and the Third Reich was nothing but a 10 km wide strip, German soldiers were still shot for desertion. That was the work of the fearful military police, with no room for intellectual debate over legal responsibility.

The whole fusion problem is at a stalemate, in limbo. Public funding is clearly not enough for a general attack on all fronts, including LENR. The hot fusion community will continue their fight until their final defeat, or until the first commercial LENR device appears on the shelf of household items.

Will there be a sort of Nuremberg's trial for crimes of science? Will the leaders of hot fusion be held accountable for crimes "against the environment and humanity"? This responsibility issue can't be silenced forever, as it was in previous decades.

The suppression of cold fusion ought to have legal consequences. Guardians of public funding must be held accountable due to the lack of progress and the consequent extreme damage to the environment and the economy. With rights come responsibilities also. But these issues are painfully absent in all the books on the subject.

There is another related important missing issue of equal importance: Is the thermonuclear fusion option the only viable theoretical solution to be considered? What about charge shielding? What about biological transmutation or Joseph Papp's work?

These are painful issues and proof of weak background research. But when so much public funding is at stake, no stone should be left unturned because this is the way real science is done.

Why is this background theoretical issue so important? Because whatever path the U.S. chooses, the world follows, even in North Korea!

So in the future when two physicists will stand before a firing squad and the squad captain allows their last wishes... this is a joke today but what about tomorrow?

My distant relative, Edward Teller, who started all this mess, would surely agree...

Life after LIFE

The Quest for a Fusion Energy Reactor: An Insider's Account of the INTOR Workshop by W.M. Stacey highlights the technical, organizational and political outlines of ITER, and is a very disturbing book.

Stacey and other planners, like the Russian scientist Evgeny Velikhov, started planning ITER in 1979, which is now supposed to be completed around 2030. Apart from physics and engineering considerations, this was to be the longest and most expensive, and most devastating, campaign of hard science. (Clery covers this story nicely.)

Stacey gives a clear account of the mindset of a committee man considering how to start the fusion mega-project. But there is no soul searching, not a speck of doubt about issues of "what if."

What if the very fundamentals of hot fusion are wrong? What if there were other less expensive, faster ways to reach thermonuclear fusion? (Like Farnsworth-Hirsch resonant electrostatic confinement, or Kapitsa's resonant magnetic confinement approach?) What if the complexity of the machine makes the time (60 years) and cost (~$20 billion) unacceptable for society in general? Can grandchildren finish what grandpa has designed? Is there a better, more efficient way to spent $20 billion? No, these minor issues are not considered, only the nice pubs in Vienna, and the internal politics of committees.

This extreme, criminal irresponsibility is well covered in Stacy's book. If you want to infuriate yourself, this is the book to read. For me, the cynicism was really frightening and shocking, but it would be a good document on the desk of the attorney general, if a criminal negligence case is ever brought to trial.

Francis Chen's book, *An Indispensable Truth*, comes with the subtitle: "How Fusion Power Can Save The World."

But this is the trap, when the word fusion is used without an attributive adjective. Inside the book of course we learn quite late that the only fusion is thermonuclear, and there are no other possible fusions. I wholeheartedly agree with Chen, as he puts it in the introduction: "Controlled fusion energy is not a pipe-dream. It can replace fossil fuels and curb global warming. The world will benefit from a concentrated effort to bring fusion reactors into the power grid sooner rather than later." We all agree. The other thing that we also agree on is that the sky is blue.

Chen devotes the first three chapters to the evidence of climate change, fossil fuel reserves and renewable energy. He gives good, decent coverage, which is nearly half the book.

Nearly one-third of the book is devoted to magnetic confinement thermonuclear fusion, with "How fusion works and what it can do." But right at the start: "Fusion: Energy from Seawater" is a gross overstatement (or deliberate lie if one sticks to the facts).

Chen refers to D-D reactions, as only deuterium is found in seawater. Tritium must be bred or manufactured in conventional fission plants.

Chen is extremely biased toward his beloved magnetic containment scheme. It is clear he rises and goes to bed with field lines and instabilities. The clear sign of this tragicomic bias toward magnetic confinement is visible in the naming of chapters such as: "The Remarkable Tokamak" and "A Half Century of Progress." (This chapter's title should be "A Half Century of Failure.") The worst bias is in "Hoaxes and Dead Ends" about cold fusion and electrostatic confinement.

It is strange that Chen is not aware of the disastrous consequences of the long string of instabilities. This is a typical cognitive dissonance, which is the usual attitude of chain smokers towards death. Yes, smoking kills everybody but me. Yes, instabilities are everywhere, but we shall overcome them at stellar cost, but in the meantime we shall silence all critical voices. George Miley has the opposite attitude, a carefully balanced opinion, based on facts and test results.

Experience has shown so far that each new order of magnitude in confinement time, ion temperature, plasma pressure/magnetic pressure, etc. came with an unexpected, unforeseen instability, which required considerable effort to tackle.

If size is plotted on a linear scale, cost is a parabola. A double sized device costs four times as much (at the minimum) if it works at all. All scaling laws are based on the tacit assumption that this highly nonlinear phenomena (burning plasma) has a predictable quasi-linear behavior.

Inertial confinement simulations of indirect heating of pellets by 196 laser beams used similar assumptions. According to computer models, experiments at the National Ignition Facility should have reached ignition long ago. Not even break-even has been reached. Knowledge behind the sophisticated codes are just not satisfactory.

So why should we trust the predictions for more complicated magnetic confinement when even the Bohm diffusion was dismissed as nonsense at the start of magnetic confinement experiments?

No doubt, Chen's views are the mainstream in the higher political circles, funding agencies (like DOE), in mainstream journal editors, and the public media. And there is no dialogue with other minority views.

This makes Chen's proudly stated cheerful opinions fearful. Bias is at the highest level everywhere in his book. There are no careful counter opinions, like Hirsch and Miley.

One doesn't have to go too far with unstable plasma behavior. Turbulent premixed (even diffusion) flames, the staple of industrial furnace designers, are full of instabilities, and so far unsolved. One can live with it but each radical new boiler design must be tested.

Now ITER is ten times as large as J.E.T.—so a brand new type of instability (unknown to us) is highly expected. It is not a known unknown, but more dangerous: an unknown unknown.

The footwork and due diligence is clearly missing in hot fusion research in general. One can't blame Chen alone. The anticipated future instabilities clearly make ITER an expensive gamble, and a very dangerous one. It is not just the clearly lost U.S. $20 billion that is at stake. The lost opportunities for other better solutions are the real losers and consequently society in general.

Who will sue Chen (and thousands of other physicists) for suppressing much better other solutions, like dozens of "cold" fusions? Science as a method is the biggest loser; science as institution will survive with the usual irresponsible cynicism. Maybe some heads will roll, but everything will go on as usual.

Is there life after LIFE? Laser Inertial Fusion Energy (LIFE) research at Lawrence Livermore National Laboratory clearly demonstrated a fiasco, by failing ever to reach break-even (even the incoming laser energy beam, not at the incoming power supply energy). Yet nothing happened. There was no major shake up of research policy. Z pinch, stellators, and mirror machines all failed in the last half century. The only consequence was that instead of billions only hundreds of millions have been poured into the bottomless pit.

On these shaky foundations, Chen even makes economic predictions (Cost of Fusion Energy, pages 359-361). Obviously tokamak fusion is the clear winner over photovoltaic, wind or tidal power. Only fission is better and coal is equal if carbon is sequestrated (hidden in a deep cavity).

For an insider, these fallacies are known and obvious. (Over 100 tokamaks were built during the last 60 years and none of them has ever reached break-even.) But fellow hot fusion researchers don't bite the very hand which feeds them. For the outsiders, all this seems to be reputable science, not pseudoscience, as demonstrated by the long stream of blatant disasters which were portrayed as brilliant victories. The failure of LIFE was a crushing defeat to "big" hot fusion but the propaganda machine, (which is as good as in any authoritarian regime), made it look like business as usual. Hot fusion is too big to fail.

There are a few skeptics, though. Charles Seife's book is a "skeptic's handbook" about fusion, be it hot or cold. The title, *Sun in a Bottle*, as usual has a frank subtitle as well, "The Strange History of Fusion and the Science of Wishful Thinking."

As a former reporter for *New Scientist* magazine, and *Science* as well, Seife is nearly as good a storyteller as Daniel Clery but more cynical—and rightly so. Being a science writer but not a physicist, he omits important details both about hot and "cold" fusion. But starting from mainstream hot fusion projects, he covers the history of fusion down to the little known work of a teenage boy Thiago Olson, who made a "fusor" in his garage (a version of Philo Farnsworth's electrostatic confinement resonant reactor). He briefly mentions Robert Bussard's very interesting talk at Google, Todd Ditmire's (a Livermore physicist) laser fusion in the infrared range. There is also a fascinating short description of Seth Putterman's results with pyroelectric crystals.

Seife covers the Fleischmann-Pons story at length, along with Jones' cold muon catalyzed fusion, as if they were the same. The story is mildly skeptical but he mentions the positive results of John Bockris, and Stanford's Robert Huggins. He even mentions (in a neutral manner) Eugene Mallove (and *Infinite Energy*).

"History occurs twice…the first time as tragedy, the second time as farce." This is Seife's comment about Rusi Taleyarkhan's bubble fusion story, related to sonoluminescence. As a personal acquaintance of Taleyarkhan, Seife wrote a detailed account about the details of a similar storm as with Fleischmann and Pons.

The whole chapter is a detailed, complex story with clashing egos, but very little down to earth science is quoted. I personally miss reference to Julian Schwinger's pioneering work with sonoluminescence. Schwinger, a pioneer of radar and quantum electrodynamics, considered collapsing bubbles as a source of vacuum energy but with ordinary water.

Seife is more openly critical of "hot" fusion than Clery but his tongue would be sharper today, about ten years later, with even less results. But he is rather narrow-minded and doesn't follow the slowly unfolding story of cold fusion. His conclusion: "Unless you are creating fusion in a hot, dense plasma, you are extraordinarily unlikely to produce excess energy. Too many phenomena conspire against you. Tabletop fusion is an interesting curiosity, but not a path to unlimited power."

For the public, let it be state planners of energy or green activists, fusion is no longer an option at all, hot or cold. The overheated optimism of the 1960s and 1970s has vanished and the skepticism is growing in the circles of natural science as well. Hot fusion protagonists like Chen still fight the rear guard's battle with decreasing funding.

In the last decade, a remarkable trench has grown between the protagonists of internal confinement and magnetic confinement. It is only a matter of time when there will be an open fight between them. Both armies have ammunition against the other. The best argument against inertial confinement is that ample, sophisticated series of experiments have failed to reach break-even. If the total energy input requirement is considered, where lasers have incurably low efficiency, it is a completely hopeless case. Certainly there are other interesting applications, to study matter at extremely high density and temperatures which are fundamentally interesting. But to study matter at extremely high density and temperature has no immediate practical application. No matter what the driver is (laser beams, heavy ions, electron beams, X-rays), no matter what the ablation material is (whether direct or indirect heating), there is no break-even, and to make things worse, secret computer codes are unable to describe the process. Their calculated results are way off the test results.

They have the same valid arguments against magnetic confinement methods, and add the spiraling cost and extreme complexity of these growing behemoths.

Fact or Fraud?

But when it comes to "tabletop" devices, they close ranks, and the battle cry is: "Fraud!"

David Goodstein's book *On Fact and Fraud* is a balanced, valuable, short book on the subject, worth reading regardless, whether it describes "hard" or "soft" science. There is a huge gap between deliberate fraud and wishful thinking. The former is the deliberate forgery of test data, rare in hard science; the latter is an overoptimistic interpretation of data and trends, a daily occurrence in science.

Goodstein's book is full of such cases through the history of hard science, but for us the most interesting is Chapter 5, which describes a little known incident of cold fusion science, the "Frascati Affair of Scaramuzzi."

He starts with the usual story of the Pons-Fleischmann history when "science has gone berserk." But this book offers much more, because Goodstein, a professor of physics at Caltech, is a personal colleague of the physicists Steven Koonin and Charles Barnes, and at the same time Goodstein was a friend of the Italian professor Scaramuzzi, also a physicist and solid state researcher.

Scaramuzzi (and his small team) found a "dry" fusion effect—not a wet (electrochemical) one. They have heated and rapidly cooled small chips of titanium in a deuterium atmosphere. They have detected neutrons, even in deep mountain caves, devoid of external "background" neutrons. They produced sporadic positive results and carefully eliminated possible test errors, avoiding the traps of Pons and Fleischmann.

Nevertheless, Scaramuzzi has also faded from the news, though a number of small, inexpensive test devices have been designed and tested, with good results, though not repeatable ones.

Nearly the same story has happened at the Bhabha Nuclear Research Center, in Mumbai, India, with the group of Mahadeva Srinivasan. (The case is not covered in the book.) They also found tritium, neutrons, and even some excess He-3, a clear sign of a nuclear process. They and nearly all other groups faded away due to lack of funding. Mainstream science became allergic to cold fusion. They expected it to obey the same rules of hot fusion which they were familiar with.

A fellow professor asked Goodstein, "Do you believe in cold fusion?" He replied, "No...Certainly I believe quite firmly in the theoretical arguments that say cold fusion is impossible. On the other hand, however I believe equally firmly in the integrity and competence of Franco Scaramuzzi and his group of coworkers in Frascati...Over the years I have looked at his team's cells and at their data, and its pretty impressive. What all these experiments really needed is a thorough critical examination by accomplished rivals intent on proving them wrong. That is part of the normal functioning of science. Unfortunately, in this area, science is not functioning normally. There is nobody out there listening."

To put it another way, self-correction (which science as an institution is so proud of) no longer works. Science as a method is suspended or even dead.

And fusion is not the only area in which this is the case: cancer research, cholesterol research, and even the study of proper dose of vitamins are all riddled by oppression and closed mindedness.