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October 24, 2011

Mr. Tom Tamarkin

5545 El Camino Avenue

Carmichael, California 95608

Ref: Response to your questions on analysis of Pulsed Jet Magneto Inertial Fusion (PJMIF)

Dear Mr. Tamarkin:

I have finally found some time to offer you a few comments on fusion and PJMIF. I have had a chance to briefly review some of the relevant papers.

First let me say that I didn't realize at first that your Pat Boone was "the" Pat Boone, with whose music my generation grew up. I remember vividly as a teenager going with my cousin to see the movie "April Love" at a drive-in movie theater in a small village in central Pennsylvania. I am now pleasantly surprised to see that he is such a strong supporter of fusion energy and I hope that he will continue to actively advocate a properly funded and balanced fusion program. Please give my deepest regards to Pat.

Because Pat is actively advocating fusion, I thought you and he might be interested in my own assessment of the U.S. fusion program. This assessment is captured in a recent letter to Congress, which I append to the bottom of this message.

Secondly, let me say that I have recently been in e-mail contact with Niels Winsor. I have provided him with the simple computer code used to do the computations and plots in the American Journal of Physics paper, "The fundamental parameter space of controlled thermonuclear fusion," with which you said you are familiar. I hope that he will find it useful in his investigations.

Thirdly, let me note that I am personally acquainted with the physicists who are currently involved with PJMIF (Thio, Witherspoon, Hsu, Awe, etc.). I consider all of them to be excellent physicists capable of carrying out the research necessary to evaluate the scientific issues associated with PJMIF. Any disagreements that I might have with them are at the "state-of-the-art" or philosophical where I would have to agree that my chances of being correct are no higher than their chances.

Now, some technical comments. Magnetized Target Fusion (MTF), a.k.a Magneto-Inertial

Fusion (MIF), involves heating a pre-formed magnetized target plasma to fusion temperatures by compressing it with an imploding higher density shell. The primary heating mechanism after initial plasma formation is compressional heating (as opposed to, for example, neutral beam and microwave heating of tokamak plasmas). MTF/MIF attempts to access a density space somewhere in between the 11-12 orders of magnitude in density that separate conventional ICF and MCF. The fundamental issues the MTF/MIF community would address in detail if sufficient funding was available include: (a) what magnetized plasma is the best target plasma; (b) what is the best implosion driver, first for research purposes, second for energy applications. As discussed briefly in the AJP paper and shown on the attached slide, the vast density-velocity space of MTF/MIF (as compared to the limited space of conventional ICF) means that many possible driver/target-plasma combinations can be considered. Some, but probably not all, drivers may be appropriate for a variety of different plasma formations schemes. Other drivers may have a limited number of plasma formations schemes from which to choose. Some combinations, particularly the higher velocity drivers, may be appropriate for the higher end of the MTF/MIF density spectrum and some may be more appropriate for the lower end of the density spectrum.

Of course, MTF/MIF is a "pulsed" approach, which means that a large part of the fusion community rejects such an approach outright without any serious consideration, as I am sure you realize. Conventional ICF has the same problem. The dream of a "steady-state" reactor (the author Charles Seife calls it "wishful thinking") has been an major obstacle to actually achieving significant fusion energy release. I, for one, am willing to believe that if the physicists can demonstrate net energy production once, the engineers can figure out how to do it repetitively in an economic way.

I am also one who believes that the engineers cannot completely design a fusion reactor until a net energy producing fusion source exists. Andre Sakharov, the Nobel Peace Prize laureate and father of the Soviet H-bomb, has been quoted by his Russian colleagues as saying something like "if you make one false assumption, you can prove anything." With an assumption that we know what a fusion source will look like, many millions of dollars have been spent on reactor studies, which, in my opinion, is "putting the cart before the horse."

I believe that MTF/MIF gives us the best opportunity to provide the first demonstration of a fusion source that can be engineered into a reactor. In conventional inertial fusion, the "cart has been before the horse" in a way: the focus for the last thirty years and more has been the driver, because, after each new generation of driver was developed, it was realized that a bigger driver would be required. Some critics of ICF have always claimed that a driver bigger than NIF was required, but the ICF proponents have always forcefully argued that the next generation of laser would be sufficient and they have effectively discredited the critics. With proper funding, MTF/MIF could move quickly to experiments where the focus was on the fuel physics, not the driver.

Although the principles of MTF/MIF have been recognized for three decades (see, for example, Lindemuth and Kirkpatrick, Nuclear Fusion 23, p. 263, 1983), it is only within the last decade or so that MTF/MIF has attained some acceptance within the general fusion community, primarily due to the enthusiasm of Francis Thio and Dick Siemon. As you probably know, there is now ongoing work at Los Alamos, the Air Force Research Laboratory, Sandia National Laboratories, the University of Rochester, in Russia, and elsewhere. None of these efforts is "mainline" and all are trying to survive on shoestring budgets. Rochester recently published results (Physical Review Letters) that showed that a magnetic field in a laser target increased the temperature and neutron yield, thereby confirming the basic principle of MTF/MIF; in at least some people's eyes, the Sandia "Phi" target of 1978 is actually a more convincing confirmation.

At the recent Symposium on Fusion Energy in Chicago in June, I gave a paper that essentially summarized the AJP paper and used the attached slide. One member of the audience asked me which combinations on the attached slide I would pursue. Because of MTF/MIF's relatively low cost, a properly funded MTF/MIF program could pursue a number of the combinations in parallel. However, to answer the question in a funds-limited situation, I stated that I would evaluate the Russian MAGO concept first, because computationally (e.g., my two-dimensional computations, published in Physical Review Letters) MAGO appears to have the ideal MTF/MIF pre-implosion density, temperature, and magnetic field and because magnetically driven solid/liquid liners are a relatively mature technology when compared with other driver candidates. Experiments with solid and liquid liners have, in my opinion, the best chance of separating plasma issues from liner issues. Furthermore, Russian explosively driven magnetic flux compression generators provide the capability for doing experiments at high energy, i.e., a "Halite/Centurion" approach for MTF/MIF. As with any proposed approach for which there is limited data, there are technical issues with MAGO, such as plasma purity, but I would have to eliminate MAGO before I became a strong advocate for any other approach. Unfortunately, because MAGO originated in the Russian nuclear weapons program, it has been largely ignored in the U.S. in deference to plasma formation systems that are more familiar, even if not as optimum.

Now we come to PJMIF. PJMIF is an attempt to create a driver that would have more favorable reactor "stand-off" properties than, say, a magnetically driven liner that would require electrical contacts near the fusion source. The basic motivation for PJMIF is certainly valid. Little work has been done on determining whether or not a suitable plasma can be created within a plasma jet liner, should the plasma jet liner ultimately be shown to have the necessary properties. Creating the fuel plasma may be an even bigger challenge than assembling the liner from jets. Hence, developing a plasma jet liner does not directly address the issue of demonstrating in the shortest possible time that a magnetized plasma can be compressed to fusion conditions by an imploding shell. However, in the long run, the demonstration of a suitable plasma jet liner could conceivably speed the demonstration of a fusion source by enabling experimentation at a lower cost than some other driver approaches. I answer your specific questions (as highlighted in bold blue below.)

Thank you for giving me the opportunity to comment. If I can provide further assistance, please don't hesitate to contact me. I can be reached by telephone at 520-743-2991.

Very sincerely,

(Irv)

Irvin R. Lindemuth, formerly
Special Assistant for Russian Collaboration
Office of Associate Director for Weapons Physics
Los Alamos National Laboratory

Answers to your questions:

(i) Are there any obvious fundamental physics flaws with the concept that the proponents of the concept have overlooked? If so, please identify and discuss.

The proponents recognize the issues, which include: (1) symmetry of the liner assembled from many jets; (2) the density profile of the liner; (3) magnetized plasma formation within the liner; (4) mixing of the liner material with the target plasma; (5) is high gain required, and if so, how to introduce additional fuel. These issues are in many ways common with any MTF/MIF driver/plasma combination.

(ii) Are there insurmountable engineering challenges associated with the approach that you can foresee at this stage? If so, please identify and discuss.

There are no obvious challenges to building facilities that will allow the study of an array of spherically converging jets. Bigger challenges will be encountered when trying to mate a plasma jet liner with a target plasma formation scheme. If plasma is injected, as in the original PJMIF concept, the plasma formation scheme may significantly alter the symmetry of the liner since the scheme may force the elimination of jets in a significant solid angle. If the target plasma is formed from fusion fuel at the leading edge of the jets, a much more complex problem than injection, then there may be essentially insurmountable issues with fuel symmetry and magnetization.

(iii) Have the proponents conducted plausible computer simulations and analysis to provide a plausible expectation of the fusion gain achievable by the approach?

I believe the calculations are highly idealized. As with any MTF/MIF approach, any computations of fusion gain are in their infancy and should be taken with a grain of salt. Fully integrated liner/plasma computations for any MTF/MIF approach represent a major challenge that are pretty much beyond the state-of-the-art, particularly for PJMIF where fully three-dimensional modeling is required.

(iv) A major challenge for the concept is the ability to produce an imploding liner from the merging of the jets. What is your assessment that the proponents are likely to succeed in achieve this technical goal, given adequate resources? Do they have credible concepts and approaches for achieving this goal?

You are correct--this is a major challenge. Because I am not as familiar with plasma jet technology as I am with magnetically driven liners, I personally don't feel as confident in this approach as I would be with magnetically driven liners. This is clearly a research topic, and, as with all such research, it is difficult to assess a probability of success. Per previous comments, I believe those involved are very capable of determining whether or not PJMIF is a feasible approach for forming a liner.

(v) Another major challenge for the approach is the ability to get the imploding plasma liner to generate pressures up to 50 mega-bars? What is your assessment that the proponents are likely to succeed in achieving this technical milestone, given adequate resources? Do they have credible concepts and approaches for achieving this goal?

The pressure required is determined by the fuel density. 50 Mbar is a reasonable requirement for mid-to-high density MTF/MIF but the fuel pressure at the lower density of the MTF/MIF spectrum could be orders of magnitude less. The idealized, one-dimensional hydro calculations show that a 50 Mbar stagnation pressure can be achieved with a liner alone, but the pressures that could be obtained in a central fusion fuel may be significantly different. Regarding what is actually required, I do know that a major critic of PJMIF is Paul Parks of General Atomic, and he evidently is dead-set against PJMIF. I know that essentially everyone else in the MTF/MIF community disagrees with Parks but some will agree that he has raised some legitimate issues.

(vi) Yet another crucial challenge to any fusion scheme is its ability to reach the temperature needed for thermonuclear fusion reactions to occur. For a mixture of deuterium and tritium, the canonical temperature for this purpose is 100 million degrees K. Please comment on the ability of the PJMIF scheme to reach such temperatures in principle and/or any issues you see in connection with this goal."

As with any other approach, this depends crucially on what type of plasma is imploded by a plasma jet liner. As far as I know, essentially no work has been done on evaluating a plasma formation scheme and whether or not it can be mated with a plasma jet liner system. See also my comments in response to (ii).

(vii) A typical criticism of any pulsed approaches to fusion from the researchers in the mainstreams of government funded research in steady-state magnetic fusion is that pulsed approaches to fusion cannot produce useful or practical power (Ref: Francis Chen: "An Indispensable Truth: How Fusion Energy Can Save the Planet".) I would appreciate any comments or insight you can share with me on that assertion.

See earlier comments on pulsed approaches. There is without doubt a bias in the magnetic confinement fusion community against pulsed approaches. This bias borders on religious conviction, so there really is no serious legitimate scientific discussion. But pulsed approaches may be the only way to utilize fusion. Going from a few seconds to true steady state operation may never be demonstrated by a magnetic confinement scheme.

LETTER TO CONGRESS
Summer 2011

Fusion energy is an absolute necessity for future U.S. energy independence. Therefore, this message is written to you because you are supporters of the fusion energy program and want to ensure that the U.S. receives maximum benefit for the investment being made in fusion research.

The recently released Senate version of the Energy and Water Development bill and the earlier release of the House version clearly indicates that the U.S. Congress recognizes that the U.S. has a major problem with ITER and also recognizes that there are a number of potentially lower cost approaches to fusion. Unfortunately, as long as the Office of Fusion Energy Sciences (OFES) in its present embodiment is essentially the sole office chartered with making fusion energy a reality, the U.S. will not get the maximum benefit from the investment made in fusion and, in fact, fusion may never become a reality. If fusion is to become a reality, OFES must be either completely restructured or completely eliminated. The reasons why such major changes are required include:

I. OFES is not a fusion energy advocate;

II. OFES will sacrifice the U.S. domestic fusion program to build ITER;

III. OFES discourages a healthy scientific dialog;

IV. OFES discredits any perceived competitor; and

V. OFES will circumvent Congress.

In the remainder of this message, I would like to elaborate on these points.

I. OFES is not a fusion energy advocate

OFES does not advocate a broad-based, balanced fusion energy program. OFES advocates only steady-state magnetically confined fusion approaches, and, in practice, only advocates tokamaks, e.g., ITER, as the only viable approach to fusion. OFES has a strong history of minimizing or eliminating competitors to the tokamak, even though the tokamak has never lived up to its projections (e.g., Anne Davies, former head of OFES, told the December 1978 issue of Popular Science, "TFTR [the Tokamak Fusion Test Reactor at Princeton] will achieve not just a power breakeven, but will be a net power producer, in terms of heat.").

Leaders of OFES come from a tokamak background and simply do not have the expertise to objectively evaluate any other approach. For more than 30 years, OFES leaders have understood only tokamaks and believed in only tokamaks. New OFES personnel are recruited with the requirement that they be capable of stewarding tokamak research. By carefully picking those who are chosen to “peer review” proposals, OFES leaders foster a tokamak “in-breeding” in the fusion community.

A recent (July 10, 2011) New York Time op-ed by Stewart Prager, Director of the Princeton Plasma Physics Laboratory, reflects the tokamak-centric attitude of OFES and those who are heavily funded by OFES. Although laser fusion (and not heavy-ion fusion, magneto-inertial fusion, etc.) is mentioned in passing, the clear focus of the article is on ITER and its successors. Noting that "what has been lacking in the United States is the political and economic will," Prager echoes the OFES mantra: all that is needed is more money, i.e., "a rough estimate is that it would take $30 billion and 20 years to go from the current state of research to the first working fusion reactor." There is no hint whatsoever that there are alternate concepts that have potentially lower costs and shorter development paths.

As you know, OFES is attempting to redirect all fusion plasma research to science relevant to ITER. In this context, Edmund Synakowski, OFES head, describes the shift as going away from "exploring such alternative configurations for their own sake" to research that "can contribute to our understanding and optimizing the tokamak configuration…" (Physics Today, September 2011, p. 30). Synakowski evidently views any concept other than tokamaks as something to be evaluated only "for their own sake," not for the sake of deploying fusion energy in the shortest possible time at the least cost.

In the Congressionally initiated High Energy Density Laboratory Physics (HEDLP) arena, it appears that OFES will continue to request minimal funds for some HEDLP work, because it is politically expedient to do so and because conventional inertial fusion energy (IFE) does not involve magnetic fields. However, in spite of Congressional recognition of the inter-relationship between High Energy Density Laboratory Physics (HEDLP) and fusion energy, a recent OFES solicitation (Program Announcement LAB 11-583, "High Energy Density Laboratory Physics," September 8, 2011) excludes fusion energy as an application of HEDLP. To OFES, HEDLP is interesting plasma physics but is simply not relevant to fusion because fusion is tokamaks and tokamaks are not high-energy-density devices.

Because fusion is so important to the U.S. future energy independence, the U.S. desperately needs a properly funded organization that recognizes that ITER is not a guaranteed path to fusion energy. This organization must advocate a balanced approach that fairly evaluates all possible paths, particularly those that have lower cost and shorter development time than the narrow tokamak approach espoused by OFES.

II. OFES will sacrifice the U.S. domestic fusion program to build ITER

OFES continues to ask the U.S. to invest billions of dollars in ITER. Even by present projections that are bound to be optimistic if past history is any indication, ITER will not even produce its first plasma until late 2020 and not demonstrate breakeven until at least 2028. To build ITER, OFES essentially seeks a "blank check" and will try to divert all available funding to this goal.

At a recent FESAC (Fusion Energy Sciences Advisory Committee; July 28) meeting, Office of Science Director Bill Brinkman indicated that U.S. contributions to ITER will have to grow to $300M in FY2013 and be maintained at that level for 3 years. It would seem highly likely that, if OFES is left unchecked, the U.S. contribution to ITER will exceed the $2.2B upper limit of the present "official" U.S. estimate. Any discussion regarding whether or not such a path is the best use of taxpayer funds is outlawed.

At the same FESAC meeting, Synakowski apparently expressed his view that it was more important to do whatever was necessary to make fusion (i.e., tokamak fusion) succeed "globally" rather than to focus on the success of the U.S. program.

If OFES was a true advocate of fusion energy by the quickest and least expensive approach and was seeking to maximize the benefit of the limited funds available in the present budget climate, OFES would recognize that it is in the U.S.' best interest to withdraw from ITER and focus the U.S. on efforts where the U.S. is the clear world leader, such as conventional inertial fusion and magneto-inertial fusion. There is no down side to letting Europe explore the tokamak approach without U.S. participation, and doing so will actually speed up fusion development.

III. OFES discourages a healthy scientific dialog

OFES essentially represses any suggestions that any other approach could possibly lead to fusion energy. The total repression of scientific discourse has created such a negative scientific environment that it is quite obvious that the United States is not getting the maximum possible return on the investment it is making in fusion.

At the recent Symposium of Fusion Energy (SOFE; Chicago; June 26-30) Town Hall Meeting on Advancement of Fusion Energy, Ray Fonck, the former head of OFES who chose to ignore a 2007 GAO audit of the fusion program, asserted, "it is counterproductive to criticize the mainline (i.e., ITER) program." In keeping with this politicized, non-scientific attitude, I have been told that one slogan of Synakowski is "one fusion, one voice," i.e., fusion should speak with one voice so that ITER funding is not jeopardized.

In an article entitled "U.S. narrows fusion research," Physics Today (September 2011, p. 30) described program elements that Synakowski is attempting to eliminate and noted "a couple of researchers would not go on record criticizing the cancellations for fear that DOE would retaliate in future funding competitions." In this environment, nobody who is an active fusion researcher who depends on doing fusion research funding for a living dares to say anything these days against the mainstream and the establishment.

IV. OFES discredits any perceived competitor

It is quite easy to find examples of the "dirty politics" OFES and its dependents will use to discredit any competitors. Most recently, Robert Goldston, former head of Princeton Plasma Physics Laboratory and one of the U.S.'s leading ITER proponents, has recently written an article in the Bulletin of the Atomic Scientists (Vol. 67, No. 3, pages 59-66, July 2011) entitled "Inertial confinement fusion R&D and nuclear proliferation: the need for direct and transparent review." Without being specific, the article asserts,

"uncontrolled dissemination of knowledge gained from inertial confinement fusion research and development (R&D) may risk contributing to the proliferation of <u>highly deliverable and very powerful advanced nuclear weapons</u>" (my underlining for emphasis).

While we cannot discount the possibility of some proliferation risks with IFE, one has to question why this issue is being brought up in such sensational fashion by an ITER advocate who is not an expert in nuclear weapons and proliferation issues. Particularly when the same individual has essentially denied the existence of IFE, bringing this issue up only after the National Academy of Sciences (NAS) has been chartered to review IFE would seem to have suspicious motives.

The irony here, of course, is that the tokamak community will try to suppress the fact that Soviet scientists Tamm and Sakharov invented the tokamak because they were seeking a way to create fissile materials for nuclear weapons. This use, and not fusion energy, may be the only motivation for a proliferant nation to expend the high cost to build a tokamak.

I strongly suspect that OFES will attempt to discredit the writer of this letter, rather than address the points that this letter makes.

V. OFES will try to circumvent Congress

In 2007, under direction of Congress, the GAO conducted an audit of the U.S. fusion program. The GAO report was highly critical of OFES and made several recommendations that were essentially ignored by OFES. The on-going National Academy of Sciences review of the prospects for inertial fusion energy, which could be construed as a partial response to the GAO recommendations, was initiated by Undersecretary Koonin, and not OFES.

OFES has a long history of trying to circumvent any direction from Congress. In the early and mid-90's, Congress was concerned about the emphasis on tokamaks and advocated a more balanced program, including an evaluation of alternate concepts. Although OFES gave "lip service" to alternate concepts for several years, ITER advocates and their insatiable appetite for all available funds have lead to an erosion of all non-tokamak efforts in the U.S. program. As you know, the FY2012 OFES budget request said,

"the magnetic-fusion-relevant component (of alternate concept experimental research) will become more concentrated on projects that solve problems that hinder the tokamak approach..."

The Senate bill mark-up clearly shows that the Senate recognizes the potential of magneto-inertial fusion (MIF), also known as Magnetized Target Fusion (MTF). In addition, it appears that the House of Representatives also recognizes such potential benefit, with wording in the House version of the appropriations bill that includes:

“The Committee urges the Department to fully evaluate existing research capabilities that do not fit easily within the existing weapons-focused inertial and energy-focused magnetic confinement fusion programs, such as krypton fluoride lasers and magneto-inertial fusion, but that may play important roles if an inertial fusion energy program moves forward in future years."

In contrast, the OFES FY2012 budget request also stated:

"one of the three areas presently receiving funding in HEDLP, magnetized high-energy-density plasma, will be significantly redirected and resized to basic science."

If Congress permits this redirection, all research in an approach to fusion that attempts to combine the best features of inertial fusion and magnetic fusion will be terminated. By all estimates, MIF/MTF is much lower cost than either of the two conventional approaches, MFE and IFE, and the development time, because much requisite technology already exists, should be much shorter. The MIF/MTF approach has apparently triggered an OFES Herod reflex (cut off the baby's head before it has a chance to grow up). OFES does not want the scientific world and Congress to know that any approach involving magnetic fields could possibly be cheaper than the path that goes through ITER.

Regarding alternate concepts, OFES will continue to say whatever is required to placate Congress and then totally disregard what has been said. The aforementioned OFES solicitation (Program Announcement LAB 11-583) is yet another example of OFES trying to circumvent Congress by excluding fusion energy as an application of HEDLP. Given the importance to fusion that Congress has put on the HEDLP program, it is very telling that the Descriptions of Research Programs section of this announcement does not even mention the word fusion one single time. This totally contradicts OFES' response to the 2007 GAO audit of the fusion program. In his October 10, 2007 letter to GAO's Mr. Gene Aloise (p. 35-36 of the audit report), Ray Fonck, then head of OFES, very explicitly states:

...The joint program on HEDLP will address underlying scientific issues that will be relevant to future considerations of inertial fusion energy...We disagree with the conclusion that this joint program "will not address most of the scientific issues that would advance inertial fusion energy." The joint program in HEDLP and the large NNSA program in inertial confinement fusion will encompass most of the science issues related to IFE target physics...

The recent Program Announcement should certainly be interpreted as an indication of OFES' intent to defy Congress regarding the role of high-energy-density approaches such as inertial fusion and MIF/MTF. Congress should immediately obtain an explanation of the omission of fusion in this Program Announcement before the contracts are put in place and the money is spent on something for which it is not intended. I would recommend that Congress act swiftly to direct OFES to modify or reissue this solicitation.

Whereas OFES is making great efforts to suppress alternate approaches such as MIF/MTF, I learned recently at the Symposium of Fusion Energy (SOFE; Chicago; June 26-30) that China considers MIF/MTF to be a viable candidate for EDEMO, China's first attempt of putting fusion energy on the electrical grid.

SUMMARY

The U.S. has a major problem with OFES. The task of bringing fusion energy to fruition rests with an organization that has a strong history of minimizing or eliminating competitors to the tokamak, even though the tokamak has never lived up to its projections. Scientific dialog has largely been curtailed in an area that clearly needs such dialog. If fusion energy is to be brought to fruition in the quickest time at the lowest cost, Congress must finally succeed in breaking up the OFES tokamak monopoly. The only way that this can be accomplished is by relegating OFES to a tokamak-only responsibility and creating a new office (or offices) of equal rank that is (are) chartered to develop a balanced alternate concept program and that is (are) given sufficient funding to be competitive. It is highly unlikely that the Office of Science will make sufficient changes if left to its own accord—it is time for Congress to act.

I hope you will circulate this among your colleagues who have oversight of the U.S. fusion energy program. Also, I would appreciate it if you can provide me with the names and contact information of others who are involved in the fusion funding process.

If I can provide additional information, please don't hesitate to contact me.

Very sincerely,

(Irv)

Irvin R. Lindemuth

P.S. I attach to this message a copy of the article, "The fundamental parameter space of controlled thermonuclear fusion," that was published in the May 2009 American Journal of Physics. This article addresses in near-layman terms why tokamaks must operate in a low-energy density regime and why inertial fusion must operate in a high-energy-density regime at a fusion fuel density that is a factor of 1 trillion higher than the fuel density in tokamaks. Most importantly, the article answers the question, "is there anything in between conventional magnetic confinement and conventional inertial confinement." I hope you will find the article interesting and useful.

I also attach "The Report of the Review Panel: Sixth Symposium on Current Trends in International Fusion Research, Washington DC, March 2005." This report addresses such things as:

Quality of Fusion Research; The Potential Role of Fusion Energy; Industrial Applications of Plasmas; Classification of Fusion Schemes; ICF/IFE & NIF; MCF—The ITER Saga; Alternate Concepts; Fusion and Private Investment; Fusion Materials; Fusion and the Electrical Power Industry; Status of Fusion Research in the US; and Recommendations.

In contrast to panels convened by the Office of Science and OFES, the members of this panel received little or no financial support from OFES and, hence, were not constrained by the "don't bite the hand that feeds you" mentality. The problems identified by this panel and the recommendations made remain valid today.

BIOGRAPHICAL SKETCH--IRVIN R. (IRV) LINDEMUTH

Dr. Lindemuth retired from full-time employment in November, 2003 after more than 32 years in the U.S. nuclear weapons physics program with the University of California, first at the Lawrence Livermore National Laboratory and then at the Los Alamos National Laboratory. At Los Alamos at the time of his retirement, Dr. Lindemuth was a Special Assistant for Russian Collaboration in the Office of the Associate Director for Weapons Physics, the Team Leader for Magnetohydrodynamics and Pulsed Power in the Plasma Physics Group, and a Project Leader for Pulsed Power Science, Technology, and International Collaboration in the High Energy Density Hydrodynamics Program. His primary responsibility was to provide technical leadership for a scientific collaboration between Los Alamos and Los Alamos' Russian counterpart, the All-Russian Scientific Research Institute of Experimental Physics (VNIIEF) at Sarov (Arzamas-16). Prior to joining Los Alamos in 1978, he was a technical staff member in A-Division at the Lawrence Livermore National Laboratory where he was involved in fusion research. Dr. Lindemuth received his B.S. degree in Electrical Engineering from Lehigh University in 1965 and his M.S. and Ph.D. degrees in Engineering—Applied Science from the University of California, Davis/Livermore in 1967 and 1971, respectively. His thesis research was conducted under the advisorship of Dr. John Killeen, founder of the National Magnetic Fusion Energy Computer Center. One of his graduate school advisors was Edward Teller. He has been an Adjunct Professor at the University of New Mexico Los Alamos branch, where he has taught engineering and mathematics courses. He spent the 1991-92 academic year as a Visiting Professor in the Nuclear Engineering Department of Texas A&M University, where he taught undergraduate and graduate courses, helped lay the groundwork for the Department's expansion into the controlled fusion area, and assisted the Department in forming collaborations with Russian laboratories and educational institutions. His areas of expertise include thermonuclear fusion and advanced numerical methods for the computer simulation of fusion plasmas and related pulsed power technology. He has published numerous papers in refereed journals and proceedings of major international conferences. He has been involved in a wide range of fusion and high energy density physics programs spanning essentially all of the ten orders of magnitude in density and time space from magnetic fusion energy plasmas to inertial confinement fusion plasmas. An internationally recognized pioneer in the application of implicit, non-split computational methods to magnetohydrodynamics, he has achieved widespread recognition for his large-scale numerical simulations of a variety of fusion and other high-density plasma systems. In addition to his accomplishments in modeling high temperature plasmas, he has formulated a variety of novel pulsed power computer codes that have led to important advances in laboratory programs. His codes have stimulated the development of several types of fast opening switches. He is a US pioneer in Magnetized Target Fusion (MTF) and performed the first comprehensive survey of the parameter space in which MTF was likely to work. Even before the collapse of the Soviet Union, he recognized that the Soviets had developed advanced technology in the areas of ultrahigh magnetic fields and ultrahigh energy electrical pulse generation that significantly exceeded US capabilities and that were motivated by the Soviet MTF program known as MAGO. In January 1992, he became the first American scientist to present a formal scientific seminar at one of the formerly secret, and still closed, Russian nuclear weapons design laboratories. Dr. Lindemuth played an essential role in establishing the collaboration with VNIIEF, a collaboration that has helped integrate Russian weapons scientists into the global scientific community and that has resulted in more than 300 conference papers and archival publications. The LANL/VNIIEF collaboration, and Dr. Lindemuth's role in it, were featured in the Discovery Channel documentary, "Stockpile" first aired in 2001. In 1992, Dr. Lindemuth was the recipient of a Los Alamos Distinguished Performance Award for his work in the formative stages of the LANL/VNIIEF collaboration. In 2004, he was named a Fellow of the Institute of Electrical and Electronic Engineers (IEEE). Dr. Lindemuth currently resides in Tucson, Arizona and is a part-time research faculty member of the Physics Department at the University of Nevada, Reno.

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On 9/22/11 3:02 PM, Tom Tamarkin wrote:
Dr. Lindemuth:

Someone in my circle of "virtual team members"...perhaps Dr. Niels Winsor, retired and living in Albuquerque, New Mexico...mentioned that he thinks you have a winter residence in Tucson, Arizona. That is very nice. I was born and raised in Phoenix and attended NAU in Flagstaff. Of course Flagstaff and Tucson are winter and summer apart...

To be very frank, some "off the cuff" comments are very helpful in that they allow me to have the comfort of some risk analysis and understanding while moving forward with the more formal review.

I have, of course, read and archived the ***"Why Magnetized Target Fusion Offers A Low-Cost Development Path For Fusion Energy,"*** Siemon, Lindemuth, Schoenberg, LANL, 12/97 and the more recent ***"The Fundamental Parameter Space Of Controlled Thermonuclear Fusion,"*** Lindemuth & Siemon, UNR, 08/2008, American Association of Physics Teachers, 2009. Thus I have an appreciation for the level of scientific insight and expertise you bring to this review.

If you are in a position to offer off the cuff comments followed by the more formal response according to your time frame, that would be wonderful and very much appreciated.

One of our contributions to date to the "fusion community" and fusion effort has been a boost to the public's awareness through a series of articles one of my business partners, Pat Boone, has published in the field (for non-scientists of course...) In the fifth and final article, mention is made of the Department of Energy OFSE's Innovative Confinement Concepts program of the late 90s and early 2000s. I am the principal drafter (ghost writer) of these articles before Pat puts them into his laymen's language in his personal style. It was in this capacity that I first became aware of your work. With Pat Boone's personal compliments (and mine) I attached a copy of all five articles.

Thank you for your help and if my theory is correct, enjoy Tucson and the view of Mt. lemon.

Tom Tamarkin
916-482-2000 (O)
016-482-2020 (C)

From: Irv Lindemuth [mailto:irvl@att.net]
Sent: Thursday, September 22, 2011 14:10
To: Tom Tamarkin
Subject: Re: PJMIF Technical Review Request

Mr. Tamarkin,

Thank you for contacting me regarding PJMIF. Your questions are certainly the appropriate ones to be asked, and I would be glad to give you my opinion. However, because of my very busy personal and professional schedule in the near term, including making the semiannual transition from my summer home to my winter home and perhaps including a business trip overseas, I may have difficulty finding time to do the reading and research I would like to do before answering your questions. I can commit to sending you a response in a month or two if that is compatible with your needs and I can attempt to find time sooner. If you need a response sooner, I might be able to give you some "off the cuff" comments.

Please let me know what time frame you have in mind.

--IL

On 9/20/11 3:24 PM, Tom Tamarkin wrote:
To: Dr. Irv Lindemuth
From: Tom Tamarkin, USCL, EnergyCite
Ref: Proposed Technical Review of PJMIF
Date: September 20, 2011

Dear Professor Lindemuth:
I am conducting a technical review of the Plasma Jet Magneto-Inertial Fusion (PJMIF) as part of my due diligence in considering private funding for developing commercial fusion power based on the approach. I have learned of your name from reading the fusion literature and from people I have talked to about fusion. I would very much like to have your thoughts and comments on PJMIF and its potential for commercial exploitation of fusion energy. I am not a plasma physicist; I am a corporate executive who, as an undergraduate majored in physics with a math and chemistry minor in the early 1970s. I am scientifically and technically competent and knowledgeable.

For your convenience, I attach two recent publications on the concept. You are probably aware of more.

(a) S. Hsu, et. al. "Spherical Imploding Plasma Liners as a Standoff Driver for Magneto-Inertial Fusion", submitted for publication in IEEE Trans. Plasma Sci., 2011

(b) T. Awe, et. al. “One-dimensional radiation-hydrodynamic scaling studies of imploding spherical plasma liners,” *Phys. Plasmas*, vol. 18, p. 072705, 2011."

I understand that PJMIF is a new and innovative fusion concept, and the technology knowledge base for the concept remains to be developed. That does not bother me. I would like your comments and assessments on the following questions:

(i) Are there any obvious fundamental physics flaws with the concept that the proponents of the concept have overlooked? If so, please identify and discuss.

(ii) Are there insurmountable engineering challenges associated with the approach that you can foresee at this stage? If so, please identify and discuss.

(iii) Have the proponents conducted plausible computer simulations and analysis to provide a plausible expectation of the fusion gain achievable by the approach?

(iv) A major challenge for the concept is the ability to produce an imploding liner from the merging of the jets. What is your assessment that the proponents are likely to succeed in achieve this technical goal, given adequate resources? Do they have credible concepts and approaches for achieving this goal?

(v) Another major challenge for the approach is the ability to get the imploding plasma liner to generate pressures up to 50 mega-bars? What is your assessment that the proponents are likely to succeed in achieving this technical milestone, given adequate resources? Do they have credible concepts and approaches for achieving this goal?

(vi) Yet another crucial challenge to any fusion scheme is its ability to reach the temperature needed for thermonuclear fusion reactions to occur. For a mixture of deuterium and tritium, the canonical temperature for this purpose is 100 million degrees K. Please comment on the ability of the PJMIF scheme to reach such temperatures in principle and/or any issues you see in connection with this goal."

(vii) A typical criticism of any pulsed approaches to fusion from the researchers in the mainstreams of government funded research in steady-state magnetic fusion is that pulsed approaches to fusion cannot produce useful or practical power (Ref: Francis Chen: "An Indispensable Truth: How Fusion Energy Can Save the Planet".) I would appreciate any comments or insight you can share with me on that assertion.

Your help is much appreciated.

Sincerely,

Tom Tamarkin
President & CEO
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