

A Community-based R&D Roadmap for Magnetized Target Fusion

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1. Purpose of Document and Process for Preparation

This is the third version of a community-based statement on the importance and logic of Magnetized Target Fusion, a new thrust in fusion research.

2. Definition of Magneto-Inertial Fusion (MIF) and Magnetized Target Fusion (MTF)

MIF represents the all-inclusive set of pulsed high-pressure (inertially confined) approaches to fusion that involve magnetic field in an essential way. MTF is the subset that involves an imploding pusher (or liner) for pdV heating and magnetic field to suppress thermal conduction.

3. The basic argument in favor of the pulsed high-density pathway to fusion

The density and time scale intermediate between conventional magnetic fusion and inertial fusion appears to offer the lowest-cost approach to fusion energy.

4. Proof-of-Principle Proposal

A proposal to test the principal of heating to thermonuclear temperature using imploding liners and field-reversed configurations (FRCs) was favorably peer-reviewed in June 1998.

5. Energy Applications

Recent workshops have reviewed two possibilities for practical energy first discussed in the 1970s: 1) fast liners proposed at Los Alamos, which are seen today as an IFE look-alike system without the need for an expensive driver; and 2) slow liners (LINUS) proposed at the Naval Research Laboratory, which utilize a liquid metal first wall as a reusable liner material. A third possibility is to compress a magnetized plasma with IFE-like drivers, which reduces the driver power and power density considerably.

6. Technical Issues

Plasma issues include 1) understanding new equilibria that have plasma pressure bearing directly on material walls, 2) the consequences of various MHD modes on global energy transport, and 3) wall-plasma interactions that can lead to impurity radiation. In addition, liners must be designed to be compatible with the imploded plasma configuration.

7. A political issue: proliferation of nuclear weapons.

Research on MTF contributes to plasma science and the development of fusion energy. The physical regime studied with MTF is far removed from that of nuclear weapons.

8. Development Plan

The proposed Proof-of-Principle MTF experiment requires three years (if fully funded at \$7 million per year) Following that, energy breakeven appears possible on the ATLAS facility in about two additional years. The process of commercialization, involving target plasma optimization, an Engineering Test Reactor, and a DEMO, is estimated to require about 25 years and \$2-3 billion, less time and cost than estimated for conventional approaches to fusion.

9. Pulsed Power Facilities

Currently available and next-generation facilities are described, including Shiva Star at the Air Force Research Laboratory, the Z facility at Sandia National Laboratory, and Pegasus (current) and ATLAS (future) facilities at Los Alamos National Laboratory.

10. Governance of Program

A national council on MTF, patterned after the organization of NSTX, is proposed to assist OFES in guidance of this program. About 10% of MTF funding would be used for exploratory work as part of the Proof-of-Principle program. Pulsed power facilities can be used to test plasma target designs developed in small experiments at many possible institutions.

11. Conclusions

Magnetized Target Fusion represents an opportunity for the US to assume leadership in an exciting new direction of fusion research.

1. Purpose of Document and Process for Preparation

The purpose is:

- 1) to review the present status and understanding of Magnetized Target Fusion (MTF),
- 2) to identify and prioritize to the extent possible the technical issues, and
- 3) to suggest a research plan including theory, computation, and experiments needed to resolve the issues and to develop fusion energy based on MTF.

The original plan responded to a request from the Office of Fusion Energy Sciences for a community-based R&D plan for the various innovative concepts under consideration by the magnetic fusion community. Issues for MTF were identified and discussed at the First International Magnetized Target Fusion (MTF) Workshop held in conjunction with the IAEA Innovative Confinement Concept Workshop in Pleasanton, CA, October, 1997. Discussions have also been held at ICC workshops and APS meetings. The first version of this document was prepared by an ad hoc drafting committee consisting of:

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Jim Hammer	LLNL
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Since then it has been maintained on the web (fusionenergy.lanl.gov) and comments are regularly received from many quarters. The this version (July 1999) was prepared for the Snowmass Summer Fusion Study meeting.

2. Definition of Magneto-Inertial Fusion and Magnetized Target Fusion

MIF represents the all-inclusive set of pulsed high-pressure (inertially confined) approaches to fusion that involve magnetic field in an essential way. Examples of MIF include laser heated solenoids, cryogenic-fiber z pinches, flow-stabilized z pinches and the composite Z/θ pinch studied by Wessel and colleagues at UC Irvine.

Magnetized Target Fusion (MTF) is the subset of MIF ideas that involve an imploding pusher (or liner) for pdV heating and magnetic field for suppression of thermal conduction as discussed in pioneering work by Kirkpatrick, Lindemuth, and others.¹ Recently Ryutov² has provided new insights to MTF and there is a growing community of interested researchers. The examples of MIF mentioned above overlap strongly with MTF. Usually MTF assumes an imploding metal liner is the pusher that provides compressional heating and inertial containment, although other possibilities have been considered.

3. The basic argument in favor of a pulsed high-density pathway to fusion

There are not many parameters to adjust in the design of a fusion system. We have argued recently³ that density is the main variable to consider in seeking a significantly lower-cost development path. The fusion energy production per unit volume can be written:

$$P_{\text{fusion}} \sim n^2 \langle \sigma v \rangle E_f$$

The losses per unit volume can be characterized with a loss time τ_E :

$$P_{\text{loss}} \sim nT / \tau_E$$

The ratio of these is Q, which depends only upon temperature and the well-known Lawson product $n\tau_E$:

$$Q \sim n\tau_E \langle \sigma v \rangle (E_f / T)$$

If we limit attention to those systems that can be characterized as having diffusive losses, presumably anomalous, we can assume:

$$\tau_E = R^2 / \chi$$

Because we have a cost perspective in mind, we express the required system size as a required plasma energy, which gives a rough indication of cost.

$$E_p \sim nT R^3 \sim (\chi^{3/2} / n^{1/2}) (T^{5/3} Q / \langle \sigma v \rangle E_f)^{3/2}$$

One sees from this expression that density is the major variable available to affect the required energy in a fusion system. Thermal diffusivity is of course important, but relatively difficult to affect. One way to think about MTF is that the effort presently directed towards reducing thermal diffusivity is relaxed by going to very high density (by magnetic fusion standards, not inertial fusion standards). The second factor depends only on temperature (well known to be best at 10 keV for DT fusion) and Q which must be greater than one. Using conventional magnet technology, and the engineering of steady-state power handling, the density is well known to be pegged at about 10^{14} cm^{-3} . We refer to magnetic concepts that work with density of about this magnitude as “conventional magnetic fusion energy.” However, we reason that if Inertial Fusion works with a pulsed system at much higher density, then an approach that incorporates magnetic field might be quite interesting if it utilizes orders of magnitude larger density than conventional magnetic fusion. Fig. 1 shows a plot of the required plasma energy for classical cross-field thermal conductivity, Bohm confinement, and unmagnetized (inertial fusion) electron thermal conductivity. The advantage of working at high density is immediately apparent.

Anomalous transport leads of course to larger energy requirements than classical transport. Figure 1 also shows the required energy if cross-field thermal conduction has a fixed anomaly factor over classical corresponding to $\chi \sim 1 \text{ m}^2/\text{sec}$. In that case for conventional magnetic fusion density, the required thermal energy is about 1 GJ, which is the same as ITER. More interesting is the consequence of a Bohm global energy confinement time. Because the density scaling with Bohm is more favorable than classical as density increases, there is a regime of density at around 10^{20} cm^{-3} where Bohm becomes acceptable. This is in strong contrast to conventional density where Bohm is seen to be completely unacceptable in terms of the required energy. Ryutov has recently argued (see Appendix B) that drift-wave instabilities in the high-density regime of MTF probably

lead to transport much slower than Bohm, which gives further motivation for examining plasma in the MTF regime.

Unmagnetized plasma has the strongest scaling of all with density. Accordingly ICF seeks to work at the maximum possible density where the energy is small. Power requirements can also be estimated using this line of reasoning, and the power scaling is found to be:

$$P \sim E_p / \tau_E \sim (\chi^{3/2} n^{1/2}) (T^3 Q / \langle \sigma v \rangle E_f)^{1/2}$$

Thus power goes up while energy goes down with density, and there appears to be a rough minimum in cost in the regime labeled MTF in Fig 1. This general argument applies to the entire category of magneto-inertial fusion.

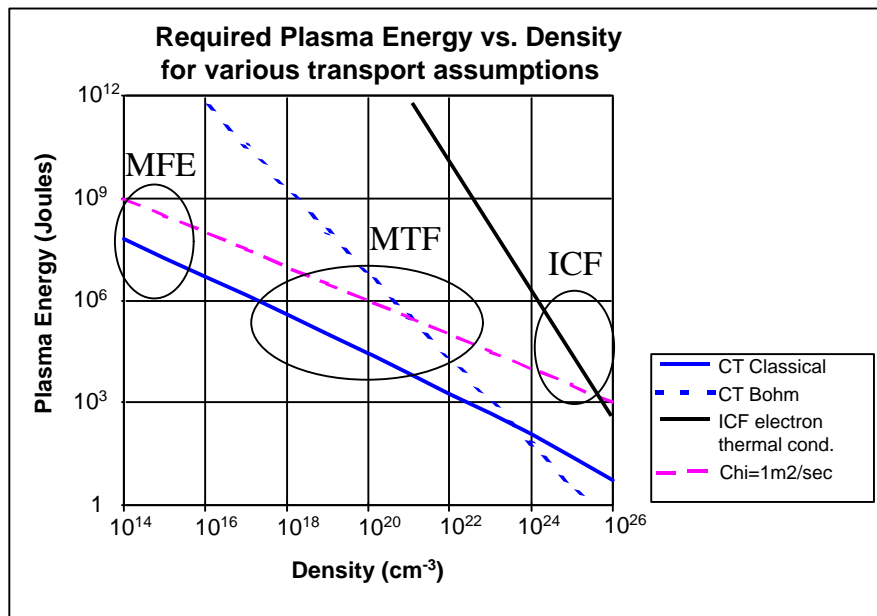


Figure 1. Required thermal energy in a plasma to achieve Lawson ($n\tau \sim 3 \times 10^{14}$ sec/cm³) as a function of density at T=10 keV, assuming a sphere for unmagnetized plasma, and an FRC geometry with plasma $\beta = 1$ for magnetized plasma.

4. Proof-of-Principle Proposal

The preceding arguments are a top-down justification for pursuing research on MTF. It is equally important to consider a bottoms-up argument. What performance is expected for a target plasma placed in a liner imploded with available power supplies? What scientific issues can be addressed and at what cost? Last year a national MTF team was assembled to consider these questions, and a Proof-of-Principle proposal was submitted to OFES. The team concluded that results of FRC research in the past 25 years, combined with liner technology developed in the past decade, allow

a highly significant integrated test of fast compressional heating to keV temperatures. The mission of the proposed program is:

To explore the intermediate-density pathway to fusion conditions by liner compression of a compact toroid target, and to demonstrate the effectiveness of magnetic insulation for reducing driver requirements, thereby opening potential avenues to low-cost energy-producing plasmas and practical fusion power.

The full text of the proposal is available on the web site fusionenergy.lanl.gov and will be made available with this whitepaper at the Snowmass meeting. The proposal received an intense peer review in June, 1998, and the review committee recommended that MTF be advanced to Proof-of-Principle status.

Historically, very few integrated tests of liners imploded on a plasma have ever been done, and the available data is extremely sparse. Fast compression by 10:1 in the size of a self-organized plasma would give major new insights into the properties of these interesting objects. Experimental data on wall-plasma interactions at high energy density is needed to benchmark existing theory and computational models. A major purpose of the proposed MTF proof-of-principle program is to obtain such data. At the same time theory and computation will be advanced along with energy systems analysis to allow a broad examination of the fusion pathway called MTF.

Most magneto-inertial ideas are at the Concept Exploration stage in the sense that significant progress can be made using small facilities with a plasma science emphasis and with research relatively unencumbered by multi-institutional involvement. The MTF subset of MIF would also benefit from a spectrum of small-scale experiments and theory, and ten percent of the MTF proposal is for Concept Exploration. However, MTF has several features that recommend it for the Proof-of-Principle stage. The committee that peer-reviewed the MTF proposal last summer agreed with this assessment. Still, the question is frequently raised: "Why is MTF ready for Proof-of-Principle when it is not currently underway as an exploratory concept?" This question was raised for example by FESAC in their May 1999 Princeton meeting. The answer including some of the discussion that occurred during the peer review process is attached to this document as Appendix A.

The question is partly a reflection of the history of our fusion program. Compact-torus research has not received much attention in the tokamak community. The notion of combining compact torus research with DOE's Defense Program liner research is even less familiar to most fusion researchers. However, in terms of maturity as defined in 1995 by the FESAC alternates concept panel, compact torus research has established the basic existence of equilibria, has a tentative understanding of gross stability in terms of operating limits, and has a reasonable characterization of global energy confinement. There is little question that both Field Reversed Configurations (FRCs) and spheromak compact toroids are good candidates to form a target plasma for implosion by a liner, and experiments could be carried out that would given the needed data on compressional heating. Equally important, the science and technology of liner implosions has been developed in the past decade to the stage where one has reasonable assurance of success in imploding a metal liner with appropriate parameters. In this regard, tests of a liner (no plasma) with the exact dimensions needed for MTF experiments were done in May and June, 1999, at the

Air Force Research Laboratory in Albuquerque. Preliminary results, recently reported by Degnan¹¹, were extremely encouraging. Greater than 13:1 radial compression was observed, whereas 10:1 is required for the proposed MTF experiments.

5. Energy Applications

The MTF Proof-of-Principle proposal concentrates mainly on the scientific issues relevant to MTF. Since then, more thought has been given to the issue of practical energy production. In May a white paper addressing this subject was presented to FESAC, and the paper can be viewed at the web site fusionenergy.lanl.gov.

Many of the ideas contained in the MTF energy whitepaper were discussed at workshops held at Los Alamos National Laboratory in February and at Sandia National Laboratory in April 1999. Two possibilities for practical energy that were identified in the 1970s still appear to be promising today. They can be characterized as “fast” liners examined at Los Alamos⁶ and “slow” liners or LINUS developed at the Naval Research Laboratory.⁷

In the fast liner approach, as with the MTF PoP proposal, a large current pulse implodes a metal liner, which compresses a preheated magnetized plasma. The system engineering examined in the 70s can be viewed today as an IFE look-alike system without the need for an expensive high-power IFE driver. The critical issue, usually termed the “kopeck” problem, is the cost of each target cassette containing a disposable current feeds for the liner and plasma formation.

The LINUS approach involves a reusable rotationally-stabilized Li-Pb liquid metal first wall as the liner material. Similar ideas have been examined recently assuming a spheromak plasma target by researchers at LLNL and UC Berkeley. Critical issues involve the hydrodynamic engineering of the pneumatically driven liquid metal walls and the limit on energy density (fuel density of order 10^{18} cm^{-3}) which implies larger plasma size (see Fig. 1).

Other possibilities that deserve examination would use arrays of beams (particle, laser, or plasma) to accomplish target compression of a magnetized plasma. Auxiliary charged particle beams or a rotationally polarized laser on the polar axis of the target could be used to magnetize the target. The main difference with respect to IFE is that the compression pulse would be orders of magnitude lower in power and power density, though somewhat larger in energy. Much of the IFE reactor study work, such as HYLIFE II, would be applicable to this MTF analog of IFE. The MTF PoP program of compressing MTF targets using capacitor discharges to implode a liner would serve to address critical physics issues, such as fuel contamination by wall interactions, before going to the expense of developing an IFE-like driver specifically designed to match the requirements of MTF.

More details can be found in the MTF energy whitepaper available on the web, and reprints will be made available at Snowmass.

6. Technical Issues

The following discussion from the original MTF R&D roadmap served as a useful guide for writing the MTF PoP proposal. Four categories of important issues can be identified:

- Plasma formation and pre-implosion confinement properties
- Liner implosion technology
- Integrated liner-on-plasma implosion
- Energy system technology

For pdV heating, the rules for adiabatic compression (best possible assumption) can be used to determine the initial conditions required. With liner-heating the volume compression ratio is usually assumed to be between 100 and 1000. In that case the initial conditions for Lawson proof-of-performance must be:^{2,3}

- Temperature 100-400 eV
- Density 10^{17} - 10^{18} cm⁻³
- Minor radius 1-10 cm
- Global energy confinement time > Bohm
- Small enough impurities that radiated power is smaller than thermal conduction
- A magnetic topology that will provide thermal insulation during implosion

The leading technical issues for each of the four categories can be summarized as follows:

- Plasma formation and pre-implosion confinement properties
 - Formation method at high density
 - energy efficiency
 - compatibility of technology with liner implosion
 - voltage and power requirements
 - magnetic topology
 - Stability of magnetic configuration
 - nonlinear evolution of modes
 - influence on global energy confinement
 - Impurity sources and levels
 - electrodes and/or walls during formation
 - liner interaction with plasma
 - influence of possible surface coatings such as LiD
 - Dominant transport process(es) and global energy confinement time
- Liner implosion technology
 - Stability during implosion and maximum compression ratio
 - Achievable velocity
 - Liner thickness needed for required velocity
 - Composite multi-layer liners
 - Liners with shapes and variable thickness to achieve 3-D compression
 - Energy deposited in compressing liner material
- Integrated liner-on-plasma implosion
 - Efficiency of energy transfer from liner kinetic energy to plasma thermal energy
 - Values of compressed density and temperature

Global energy confinement time during compression and at peak compression
Thermonuclear energy production compared to liner kinetic energy (gain)
Enhanced impurities from liner material during implosion and radiation power
Adiabatic plasma response during implosion
Alpha particle confinement and plasma heating (near peak compression)

- Energy system technology
 - Studies of optimum energy recovery methods from MTF neutron pulses
 - Identification of major cost drivers
 - Devising methods for stand-off power supplies
 - Materials available or development needed
 - Technical feasibility of advanced methods such as MHD energy conversion

In the following two paragraphs we expand upon two particular issues, which receive the most comment and discussion among researchers at the present time.

Plasma configuration and formation method. Creating and characterizing a suitable plasma target is generally seen as an important issue for MTF research. Fortunately compact torus experiments have provided much of the needed physical understanding of formation, although the density regime in recent years is lower than desired as quantified by the above discussion. It may be noted that a high-density plasma formation method of a different sort has been developed in Russia at VNIIEF called MAGO.⁸ MAGO is an interesting possibility, but more experimental diagnostics are needed to determine the plasma global energy confinement.

In the long term for MTF, an optimization process will be needed to choose the best plasma target formation method. Energy efficiency, simplicity, reliability, compatibility with advanced liner/pusher design, and so forth become important.

For now however, the FRC has been chosen as a suitable target for PoP experiments. FRCs have a well-established high-beta equilibrium. They can be formed and translated into a liner with little loss of plasma energy. By adjusting the magnetic bias field in the liner, the thickness of the magnetic sheath separating the FRC plasma and the liner can be varied, which gives a knob for adjusting wall-plasma interactions. FRCs can be compressed inside a straight cylinder, and field line tension will compress them axially. This means that good diagnostic access is available for the PoP experiments, even though ultimately one will probably prefer to compress in the axial direction as well using a shaped liner in order to achieve the highest beta and highest energy efficiency. High-density FRC formation was observed in early experiments of the 1960s, and present understanding of formation indicates that parameters needed in the PoP experiment can be achieved with available high-voltage capacitor banks.

A survey of the many possible plasma targets that can be used for MTF has recently been compiled by Ryutov and Siemon.¹²

Plasma-wall interactions and impurities. The issue most frequently cited by researchers, especially in the high-energy-density physics community, is the issue of plasma-wall interactions

and the mixing of pusher material/impurities with the relatively low-density magnetized plasma. During implosions, the magnetic field increases to the megagauss range, at which point ohmic heating melts the inner surface of the liner. Quantifying the effects of wall-plasma interaction under such high-energy-density conditions is a major issue and goal for the MTF program.

As plasma density increases near the wall with temperatures in the eV to keV range, radiation can become significant even though it is insignificant for the hot central core given low impurity levels. Optically thick layers of plasma are expected near the liner. The radiation is absorbed in a thin layer at the first wall and causes additional wall ablation. With a metal conducting liner, transport processes tend to cause a thin magnetic sheath between the hot plasma and the unmelted liner. If the liner has an insulating layer facing the plasma, then the plasma pressure should be directly supported by the liner. However, there is no experimental data on this point and experiments would be extremely interesting. Computer codes are available to address some of these issues, and results from multi-dimensional models are expected to become available in the same time frame as proposed MTF experiments (next few years). Thus integrated liner-on-plasma experiments combined with good numerical models should allow progress on the understanding of wall-plasma interactions, the most critical of MTF issues.

7. A political issue: proliferation of nuclear weapons.

There are occasional expressions of concern that MTF research would somehow contribute to proliferation of nuclear weapons. In addition, some critics have attempted to associate MTF with “pure fusion” weapons and have claimed that MTF should not be permitted under a Comprehensive Test ban Treaty (CTBT). In fact, MTF is an unclassified research program that contributes to plasma science and the development of fusion energy.

Similar questions have been raised regarding inertial fusion (ICF) research and the NIF project. Given the much larger size and visibility of the ICF program, proliferation questions about ICF have been thoroughly reviewed by scientists and policy makers both inside and outside a classified context. In 1994, the prestigious JASON committee of academic experts completed a study on science-based stockpile stewardship. The study addressed the potential proliferation implications of the NIF project and concluded, "The NIF technology is not a nuclear weapon, cannot be adapted to become a nuclear weapon, and demands a technological sophistication far more advanced and difficult than required for nuclear weapons." The JASONs findings also suggest that, "Advances in understanding fusion as a possible energy source should be shared openly, consistent with the NPT [nuclear non-proliferation treaty] bargain." Both DOE and LLNL web pages contain considerable information showing why ICF and NIF do not imply proliferation.

The case for MTF is even stronger. Unlike ICF, MTF does not have direct stockpile stewardship applications and is not presently an element of the stockpile stewardship program. MTF operates in a density/pressure/time-scale regime that is orders of magnitude away from that of ICF and so the physics understanding to be gleaned from MTF experimentation is substantially different. Furthermore, the fusion power production rate in MTF, as quantified for example in proposed liner-imploded, field-reversed configurations, is orders of magnitude lower than that required to ignite “cold-fuel” and achieve high gain in the same way as ICF.

Attempts to connect MTF with “pure fusion” weapons may have arisen because of the fairly visible and historic scientific collaboration between Los Alamos and its Russian counterpart, the All-Russian Scientific Research Institute of Experimental Physics (VNIIEF) at Sarov (formerly Arzamas-16). Beginning in 1993, the collaboration has completed through 1999 more than a dozen experimental campaigns in the US and in Russia. Although MTF is only one element of this very successful collaboration, three of the experimental campaigns provided an opportunity for US scientists to evaluate VNIIEF advances in plasma formation techniques relevant to MTF (the Russian acronym for MTF is MAGO, or magnitnoye obzhatiye, meaning “magnetic compression”). One campaign provided the first US access to VNIIEF magnetic flux compression generator technology.

Building on ideas tracing back to Nobel Peace Laureate Andre Sakharov, VNIIEF scientists, many of whom worked with Sakharov, have recognized the potential of MAGO/MTF as an energy source. VNIIEF’s role in the collaboration is funded by the International Science and Technology Center and by direct contracts with LANL. Collaboration in MTF represents an ideal “conversion” activity for Russia’s nuclear weapons scientists. Former Russian Minister of Atomic Energy Victor Mikhailov wrote, in a 1995 letter to DOE Assistant Secretary V. Reis,

"a joint MTF program would provide tangible evidence to the people of the US and the Russian Federation that we have begun the process of converting our nuclear weapons design expertise to peaceful purposes."

Some people cite the pulsed nature of MTF and the VNIIEF use of “one-shot” explosively driven magnetic flux compression generators in MAGO research, and the resultant perception of “portability,” as the basis for their proliferation concerns. It must be recognized that there is nothing specific about MAGO that requires flux compression generators to provide the intense electrical pulse required for plasma formation and liner acceleration. The simple fact of the matter is that the economy in the former Soviet Union could not sustain the long-term development of high-energy capacitor bank technology such as Pegasus (LANL), Shiva-Star (Air Force Research Laboratory), and Atlas (LANL). Consequently, Russian scientists pursued the much cheaper magnetic flux compression techniques. The US also pursued similar technology, but at a much lower level. The Russian technology provides energetic high-current pulses of electricity, but the required equipment is massive and very difficult to characterize as “portable.”

The fact that MAGO research does not require flux compression generators is readily recognized by anyone who understands the nature of the research. Evgeni Velikov, at the time Chairman of the ITER Council, wrote in a June 1995 letter to DOE Assistant Secretary V. Reis:

"the basic physics will have applications well beyond the use of explosive generators...the results mentioned above were presented to the (Russian) Academy of Sciences...MAGO is not limited to demonstration experiments...we expect this general approach to have applications to long term energy production as well."

Russians simply view the explosive flux-compression generator as an expedient low-cost method for doing the physics of a fusion energy source that will eventually incorporate rep-rated high-power electrical circuits.

Whether or not any pulsed energy-producing system has potential as a militarily useful system depends on many factors in addition to its “portability.” Among the most important criteria is the “yield-to-weight” ratio, i.e., ratio of the energy released to the amount of energy that would be released by an equivalent weight of TNT. For this reason the VonHippel team at Princeton University, probably the most responsible and thoughtful group to raise concerns about MTF, addressed the possible yield-to-weight of an explosive-generator-based MTF system. Their analysis led to the publication of a predicted 3 ton weight for a system that would produce a fusion energy release equivalent to 0.25-2.5 tons of TNT (Physics Today, September 1998), i.e., a yield-to-weight ratio of less than unity. This should be compared to the yield-to-weight ratio of 1 million (10^6) of modern nuclear weapons.

During late 1997 and early 1998, Los Alamos worked with the VonHippel team as they were conducting their research on the proliferation aspects of MTF. Details of the August 1996 LANL/VNIIEF implosion driver experiment were provided by LANL, since this experiment represents the best information available to US scientists on the performance characteristics of Russia’s flux compression technology. The 3-ton weight quoted by the VonHippel team represents a correct interpretation of the actual weight of the August 1996 experiment. However, that experiment delivered 20 MJ of kinetic energy to a liner. The VonHippel team also quotes a 1995 LANL/VNIIEF publication (Phys. Rev. Lett. 9/95) that predicted an energy release of 1 GJ (0.25 ton TNT equivalent) for a liner energy of 65 MJ, not 20 MJ. To zeroth order, it would be expected that the weight of a system would be proportional to the liner energy delivered. Hence, present understanding would suggest that the 3 ton weight stated by the VonHippel team would be perhaps a factor of 3 underestimation for a 0.25 ton fusion energy release, and perhaps more than a factor of 30 underestimation for a 2.5 ton release. In other words, present understanding says that the yield-to-weight of an optimized MTF system is much less than unity.

Even with relatively minor misinterpretations of LANL publications, the VonHippel team concluded

“we see no immediate danger of a militarily attractive new type of weapon from the current unclassified research programs.”

This conclusion was consistent with the team’s previous statement exempting the joint LANL/VNIIEF research from the team’s concerns that spherical high-explosive-driven implosions for fusion should not be permitted under a CTBT. In a letter to former Secretary of Energy H. O’Leary and presidential advisor J. Gibbons, VonHippel and R. Kidder wrote, in a footnote:

“A Los Alamos/Arzamas-16 collaboration is currently experimenting with the ignition of fusion in plasmas using magnetic fields generated by explosion-driven pulsed power generators. We are not arguing here against such indirect uses of cylindrical--as distinguished from the direct use of spherical--HE implosions.”

The recent, heightened concern about “pure fusion” weapons and the CTBT originates with a letter written by Nobel Laureate H. Bethe to President Clinton. Some critics have attempted to include MTF under Bethe’s concerns. However, former LLNL Director H. York, after speaking with LANL scientists about MTF, spoke with Bethe. York concluded (private communication, June 1997)

“In any even MAGO/MTF is not a weapon in my view and I believe in his (Bethe’s) also, so its not what he had in mind.”

Until recently, the DOE policy regarding “pure fusion” weapons has been “no comment.” However, a December 1, 1998 classification review stated:

“The following statements are unclassified: 1. Fact that the DOE made a substantial investment in the past to develop a pure fusion weapons; and 2. That the United States does not have and is not developing a pure fusion weapons; and, 3. That no credible design for a pure fusion weapon resulted from the DOE investment.”

The debate about what is permitted and what is not permitted under a CTBT will no doubt continue, and MTF will no doubt periodically emerge in the discussions. In the context of a CTBT, it is worth noting that for more than 20 years, parties to the CTBT have not challenged the US position that ICF is permitted under the CTBT. The US statement that was published as part of the negotiating record of the 1975 Nonproliferation Treaty (NPT) says, in part,

“it (ICF) does not constitute a nuclear explosive device within the meaning of the NPT or undertakings in IAEA safeguards agreements,”

and it is reasonable to assume that similar logic would have exempted MTF had MTF then been an element of the international controlled fusion research program. Furthermore, nothing in the CTBT negotiations indicates that any party wants to restrict basic energy research.

Los Alamos will continue to cooperate with those concerned about the proliferation potential of MTF. The national MTF team welcomes interactions with anyone interested in understanding the exciting physics opportunities of MTF. MTF research will be conducted in a completely open and unclassified fashion that will allow progress to be continually monitored so that those concerned about proliferation can be assured that no unforeseen development leads to unintended consequences.

8. Proposed Research Plan

In the next 5 years (near term), the focus of MTF research will be a proof-of-principle experiment followed by a demonstration of energy breakeven. We consider this to be the most reasonable ambitious or “stretch” goal in terms of technical challenge as well as likely available resources. Following that, we envision in the long term a sequence of steps leading to commercialization that would take approximately 25 years and cost \$2-3 billion.

The near-term plans to complete a proof-of-principle are fully discussed in the MTF PoP proposal. The plan is to spend two years testing FRC formation at high density, and liner implosions compatible with an injected FRC. Then in the third year, integrated liner-on-plasma experiments will be conducted and compared with theoretical models. If the PoP is successful, then the same technical approach of an FRC inside an imploding liner using larger FRC size and liner kinetic energy is expected to allow a demonstration of near breakeven, at least in the sense of DT-equivalent gain using a deuterium plasma. The possibility of using tritium would have to be analyzed carefully when the break-even experiments are better defined. A pulsed-power facility called ATLAS, which is under construction by Defense Programs for other purposes, will be available in this time frame and appears to have enough energy for such a demonstration.

The recently proposed roadmap for fusion development provides a useful framework to discuss the long-term development of MTF. As discussed in the MTF energy whitepaper, we imagine the following sequence of stages for MTF.

PoP	Use Shiva Star at Phillips Laboratory to document FRC heating to keV temperatures by liner implosion, with $Q_{\text{equiv}} = (\text{DT equivalent fusion energy})/(\text{liner KE}) = 0.01-0.10$ 3 years at \$7M/year (\$10M facility already exists) is the optimum funding profile, but a slower plan with funding per year of \$4M, \$5M, \$6M, \$6M was discussed recently with FESAC.
Perf.Enhanc.	Expand efforts to optimize plasma targets (spheromaks, etc...) Use ATLAS at Los Alamos to obtain $Q_{\text{equiv}} = 0.1-1.0$ in ~2 years Optimization and assessment requires ~ 7 years at ~ \$20M/year (\$50M ATLAS facility will be available)
ETR	Choose LINUS or FLR approach. Test rep-rated power supply in finite-duration burst mode. 8 years at ~ \$30M/year (requires \$250M facility)
DEMO	250-MW unit; 1-10 GJ yield; 0.1-1 Hz; Reliable rep-rated containment. Issues of nuclear materials and tritium handling. 12 years at \$80M/year (requires \$800M facility)

Probably the most important aspect of demonstrating energy breakeven as soon as possible is that it opens the door to optimization. The door is opened both with respect to scientific confidence and with respect to potential funding from government as well as commercial interests. The process of optimization needs to be reasonably comprehensive (thus expensive) and involves much more in the way of engineering development than is needed for an initial breakeven demonstration. As with inertial fusion energy, MTF has the advantage that optimization of the “physics” can be done at relatively low cost on a single-pulse basis. Integrating the physics and technology can be postponed until later stages. During the optimization phase, the main engineering issues are ones of feasibility and down selecting between alternative solutions. Issues include:

- comparison of various plasma targets such as FRCs, spheromaks, and diffuse z pinches from a perspective of cost and engineering simplicity
- thermal hydraulics of blanket/coolant for either the fast or slow liner approaches to liquid first wall,
- materials issues focused mainly on the various performance issues such as electrical properties as opposed to neutron damage (the first structural wall can be highly shielded from neutrons)

It becomes evident as one considers such a development scenario that the engineering and practical system issues for MTF are qualitatively quite different than the development issues for conventional magnetic fusion energy. With MTF there is no divertor, no profile shaping, no resistive-wall stability issue, no disruption issue, no refueling or ash control, etc. The physics is close to magnetic fusion, but the engineering in most instances is closer to inertial fusion energy.

Fusion research is at the stage of R&D where R needs to be larger than D. When a technology is ready for serious development, industry will become interested, and serious development activity must focus more on cost and schedule than on research. At present fusion should manage its

portfolio in the same way that an investor does for long-term growth. Diversity is essential to ensure that difficulties that might develop for one part of the portfolio do not impact on the other parts of the portfolio. Thus the qualitative differences of MTF represent great value for the fusion portfolio, because difficulties or showstoppers that emerge for conventional magnetic or inertial fusion are fairly unlikely to apply to MTF and vice versa. Portfolio elements that are independent in this sense can also be called “orthogonal.”

For more information on the development scenario described above, the reader is referred to the MTF energy whitepaper, which again is available on the web and in hard copy at the Snowmass meeting.

9. Pulsed Power Facilities

A number of universities have pulsed power facilities that could be used for MIF research. Examples (not all inclusive) include UC Irvine, U. Washington, U. Nevada at Reno, Univ. of New Mexico, and Cornell University.

In addition there are large pulsed power facilities at various national laboratories. The following table summarizes the major capacitor banks (and parameters) suitable for high energy liner implosions:

The Z bank parameters are well suited for driving very fast liner implosions, and impressive progress has been reported on the generation of X ray power with wire array implosions.

Shiva Star has been pulse sharpened in the past, using a Plasma Flow Switch (PFS), to 0.25 μ sec current risetime, to drive 5 cm radius, 2 cm long plasma liner implosions. Operated at 5 MJ, it has delivered 10 MA to such loads, resulting in \sim 1 MJ, 6 Terawatt VUV/soft X-ray pulses. Presumably, Atlas could also be operated in such a pulse shortened mode.

Bank	Brief description	Maximum stored energy	Peak current (load)	Current rise time (load)	status
Shiva Star	2 stage Marx, 1300 μ f, 120 KV (erected)	9.4 MJ	22 MA (35 nH) 30 MA (20 nH) (12.5 MA to 39 nH, safety fused solid liner load at 5 MJ operation)	3 μ sec (3 nH) 5 μ sec (8 nH) 8 μ s (20 nH) 10 μ s (35 nH) (10 μ s for 39 nH, safety fused solid liner load)	operational
Atlas	Marx, 1250 μ f, 240 KV (erected)	36 MJ	45 MA (35 nH)	5 μ sec (8 nH) 10 μ s (35 nH)	operational in 2002
Z	Marx-water-MITL $C_{eq} = 0.34 \mu$ f, $V_{eq} = 5.5$ MV	16 MJ (Marx) ($0.5 C_{eq} V_{eq}^2 = 5$ MJ)	19 MA (12 nH) (4 cm radius, 2 cm long liner)	0.1 μ sec	operational
Pegasus	2 stage Marx 600 μ f, 120 KV (erected)	4.3 MJ	6 to 12 MA (affected by inductance and series resistor)	4 μ s (10 nH) 8 μ s (40 nH)	operational

Shiva Star and Atlas parameters are well suited for driving solid liner implosions of the type suitable for MTF experiments.

10. Governance of Program

An MTF community of interested researchers has grown rapidly in the past year or two. Further growth is certain to occur as funding is made available. Our goal as a community is to encourage wide participation.

Because of MTF's intrinsic nature involving small size, large dedicated facilities are not required for the plasma science to advance. We imagine numerous small groups investigating ideas on plasma target formation, which are then subjected to tests on Defense Program facilities. Ideas based on either exploratory theoretical work or exploratory experimental work might provide refinements of the proposed initial MTF proof-of-principle campaign, or significant variations aimed at long-term advantages. For the next decade at least the needed pulsed power facilities are either available or under construction with budgets for basic operation coming from Defense Programs. This presents an extraordinary opportunity for the fusion energy community to investigate MTF without the need for large capital investment. Various universities have pulsed power facilities, and the large laboratories with facilities include Sandia National Laboratory, Los

Alamos National Laboratory, and the Air Force Research Laboratory in Albuquerque (formerly the Phillips Laboratory).

D. Ryutov has proposed that a user facility for MTF be created, which would be equipped with the basic diagnostics and support facilities to be made available for users from around the world. He notes that exciting progress can be made using fairly small sub-megajoule pulsed-power facilities. Such an approach would be especially interesting if the shot rate could be increased over the “once or twice per week” rate that characterizes the larger facilities. A generalization of this idea would be to have all the laboratories and universities with pulsed power facilities join in a consortium of MTF facilities. Then a process could be developed in which users would propose experiments, typically in small teams with all the necessary expertise represented. Ideas would be ranked and scheduled on whichever pulsed power facility was most appropriate. Diagnostic equipment and expertise, developed as an MTF community responsibility with minimal duplication, would be made available for all experimental campaigns. The appropriate managers from the larger pulsed-power laboratories have all expressed support for this general idea. Some resources to help prospective users design hardware and interface with these facilities are expected to be available. Funding arrangements will of course need to be negotiated, but the basic principle of “paying for what you destroy” represents a very low buy-in cost for fusion energy researchers.

A national council on magneto-inertial fusion, patterned after the NSTX national program, could provide advice to OFES regarding MTF funding decisions. Making 10% of MTF funding available for an annual competition of new ideas as the program evolves would encourage ongoing innovation. The Virtual Technology Laboratory under C. Baker has recently recommended a similar approach.

11. Conclusions

This is a propitious time to pursue MTF fusion research. First, Congress and important policy makers have challenged those of us in the national fusion energy program to look carefully for innovative approaches to fusion which have potential for lower-cost development paths. Given the physical constraints of fusion, this is not an easy challenge. However, MTF does offer significant possibilities in precisely this regard as described in this document.

Pulsed-power technology and scientific understanding of plasma has advanced significantly in recent years. Work on imploding liner ideas in the 1970s predated the pulsed power advances by Defense Programs. Important advances in plasma theory, computer modeling, and experiments, especially compact toroid research as described in review articles by Tuszewski and Jarboe^{9,10} all occurred after the early work was terminated.

In addition, new light may be shed on some interesting astrophysical problems. Specifically, MTF research involves reconnection processes in a $\beta > 1$ plasma, as occurs in the sub-photospheric plasma in the Sun. It may also be possible to observe radiation-condensation instabilities in an isobaric medium because MTF generates such a high-beta plasma.

Thus we conclude that an altogether modest level of effort, representing perhaps three percent of US magnetic fusion energy activity, should be directed towards this exciting possibility. What other concept at such a modest funding level holds potential for addressing interesting science, an innovative and qualitatively different approach to fusion energy, and burning plasma physics within a few years?

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Appendix A. Letter to Jerry Navratil on the question of MTF's readiness for Proof-of-Principle.

Dear Jerry,

Thanks for clarifying the question you asked at the last FESAC meeting regarding Magnetized Target Fusion (MTF). Let me repeat the question as stated in your May 26 email for the benefit of the rest of FESAC:

Dick,

I guess I see it as quite a simple question. In our program logic for the "portfolio" one of the primary criteria for advancing a concept to the PoP stage is the level of scientific maturity of the concept. The FRC is presently classed as a Concept Exploration (CE) level program. There are outstanding questions about MHD stability and confinement that must be resolved for the FRC and LSX is exploring these. One pre-condition for a successful MTF test is that you need to make an FRC plasma, produce it at twice the density and 3 times the temperature than has ever been done before and then translate that plasma successfully while maintaining these record levels of density and temperature into the imploding liner. The imploding liner facility also needs to develop the capability of producing the kind and quality of implosion required. It just seems to me that a more sensible approach given our program logic is to fund 2 CE level projects: one to develop a high density, high temperature FRC suitable for compression, and the other a CE level liner development project. In 2 to 3 years when these programs have results, it would be appropriate to consider approval of a PoP test of MTF based on the established success of the two CE programs.

I'll illustrate this point with an example. If PPPL had proposed a PoP test of the ST prior to the START results based only on theory and the tokamak data base, they would have been turned down, and told that a CE level test was needed. Once those results were in hand, together with the very extensive tokamak data base, then a favorable PoP evaluation could be made. It would have made no more sense to "pre-approve" the ST in 1992 for PoP testing assuming a successful CE test on START, than it would to pre-approve a PoP test of MTF, until the CE level building blocks of FRC target and liner are completed successfully.

By the way, none of this suggests I don't think the DOE should fund your MTF program. I just am opposed to what I see as lowering of the scientific standards for judging the maturity of concepts at the PoP level, by pre-approving them before the required scientific data at the CE level is in hand.

Hope this makes things clear.

Regards,

Jerry

I agree with you most emphatically that we should not pre-approve concepts and thereby lower the scientific standards for judging their maturity. Therefore I have prepared the following material in hopes of convincing you that we are doing nothing of that sort with MTF. Of course the OFES peer review committee last year worried about this same question at some length and

concluded that MTF deserved to be advanced to PoP status. Some of the reasons for that conclusion are based on the discussions held during the review and are thus undocumented. This letter should improve the documentation. Also, while FESAC is certainly empowered to review the prior committee's work and overrule it if that is appropriate, I trust it would not do that without also considering the material that led to the previous conclusions.

The complicated situation for MTF is that maturity has been achieved over a considerable period of time through theory, experiments, and reactor studies that were generally uncoordinated and unfamiliar because of our fusion program's history. In some cases the significance of research results, say with respect to compact toroids, were not even appreciated at the time with respect to MTF. A good example is FRC energy confinement. The Los Alamos group found a scaling law for confinement based on lower-hybrid turbulence (McKenna et al, Phys Rev. Lett. **50** (1983), 1787), and the University of Washington group and other researchers have confirmed the scaling and even found examples of better confinement. Because the original studies were done with the motivation of achieving Lawson conditions at conventional density and magnetic field, the result seemed pessimistic. It is only somewhat better than Bohm scaling and implies very large systems to achieve Lawson. However, from an MTF perspective, which utilizes orders of magnitude higher plasma density, it is easy to show that the same observed confinement scaling is completely satisfactory. Of course, the observed scaling is used for the MTF design of a PoP experiment. Another example is the theoretical analysis by Dan Barnes in which he quantified the stabilization of FRCs by kinetic effects (Barnes et. al., Phys. Fluids **29** (1986), 2616). It is indeed an open question whether or not the stabilization is sufficient for conventional applications of the FRC. Possibly shear flow will improve the prospects in that regard. But again in an MTF context, the burn time can be short enough that the instability growth time is irrelevant, and kinetic stabilization appears to be completely satisfactory. Discussions in early June at a theory workshop on FRC stability at Princeton supported the view that the observed stable regime (s parameter relative to elongation) chosen for plasma conditions in the PoP experiment are reasonable.

You said:

There are outstanding questions about MHD stability and confinement that must be resolved for the FRC and LSX is exploring these.

I argue that the "must be resolved" part depends on what application of the FRC one has in mind. Indeed, for conventional application of the FRC what you say is true and researchers at the University of Washington are doing the "exploratory" level work in that regard. During the PoP peer review process, the committee sent questions to all the advocates before we met. In retrospect that was quite an effective vehicle for efficient interactions during the day-long review. One of the questions is similar to yours, and here in part is how we answered:

PoP Reviewer question 2.2 Q: As you explained, the MTF POP program plan depends critically on the physics of FRC confinement and stability. These subjects are not well understood. Within the fusion program, there already exists concept exploration experiments investigating FRC sustainment with rotating magnetic fields. What is the connection between MTF POP and FRC concept exploration?

Answer: The statement “FRC confinement and stability are not well understood” is certainly defensible, but it carries the same luggage as “Tokamak confinement is not well understood.” In both cases the issue is whether or not the understanding is adequate for moving forward usefully in a research program. Most agree that empirical scaling for tokamak confinement, while demanding further work, is adequate for a continued experimental program and even construction of larger machines. The confinement and stability of FRCs is an intriguing and complicated subject. In our view the understanding is not all that bad, and in any case adequate to justify the proposed research.

The FRC concept exploration work at the University of Washington is logically complementary to the MTF work proposed here (see the letter from A. Hoffman included in the briefing book.) The key issues addressed are quite different.

RMF current drive of an FRC (or rotomak) is relevant to steady-state, relatively low density (10^{15} cm^{-3}), and large-size FRCs, as considered in D-He3 conceptual studies, such as ARTEMIS (Momota et al., Fus. Technol. 21, 2307 (1992)). The RMF technique provides a new method of FRC formation and a promising method of steady-state sustainment. The rotomak, pioneered mainly by I. Jones in Australia, has typically worked with low density ($\sim 10^{12} \text{ cm}^{-3}$) and low temperature ($\sim 10 \text{ eV}$). Usually the rotating field was comparable to the axial field, which gives rise to questions about the nature of the equilibrium (closed or open field lines) and confinement. The present research plans at the University of Washington, which involve an unprecedented level of current drive power and plasma size for a rotomak, are very exciting but “exploratory” in nature given that the character of the equilibrium and confinement remain to be determined.

Please request a copy from me if you would like the complete MTF team responses to reviewer’s questions. In the letter mentioned above by Alan Hoffman, he endorses the MTF PoP program and offered the following comment:

“Although we have been concentrating our compact toroid/FRC efforts on the low density, steady-state approach to fusion, we have realized for a long time that confinement and stability scaling laws are greatly in favor of high density, and necessarily pulsed approaches.”

That letter along with endorsements from leaders of the other MTF team institutions (Baldwin GA, Thomassen LLNL, Harkness Westinghouse, and Hogge, AFRL chief scientist) were distributed to the review committee last year and are also available if you would like a copy.

You also said:

One pre-condition for a successful MTF test is that you need to make an FRC plasma, produce it at twice the density and 3 times the temperature than has ever been done before and then translate that plasma successfully while maintaining these record levels of density and temperature into the imploding liner.

I am not sure what past data you were thinking about, but in fact FRCs with more than adequate density and temperature were produced in early experiments. In the proposal we say that a major task is to produce an FRC plasma target acceptable for integrated liner-on-plasma experiments. We quantify that by saying the FRC should initially have:

$$\begin{aligned} \text{Density} &\sim 10^{17} \text{ cm}^{-3} \\ T &\sim 300 \text{ eV} \\ \tau_E &\sim 10 \text{ } \mu\text{s} \end{aligned}$$

It is important to realize that the precise numbers here are not at issue. We seek understanding more than particular performance measures at the present time. Still, we believe that unless significant heating to temperature in the range of 1-10 keV after compression, with $n\tau$ above $10^{13} \text{ cm}^{-3}\text{s}$ is achieved, then we have failed to “prove the principle” of the efficacy of pulsed compression. Certainly it is true that if plasma impurities are above a few percent, we will fail and the issue will become one of understanding how the wall material mixed with the plasma during implosion. Final temperature in the desired range requires approximately “adiabatic” response during compression (i.e., reasonably small losses), and an initial temperature in the range of 100-300 eV. Frankly stated, formation with the needed parameters is *not* much of an issue for an FRC formed with theta pinch techniques developed originally back in the 1960s. Furthermore, translation has been demonstrated in many experiments and that is also not an issue.

Let me elaborate. An excellent review of FRC research was written by Michel Tuszewski in 1988 (Nuc. Fusion **28** (1988), 2033). He characterized the early work (prior to 1975) as focused on heating, and the later work as focused on confinement. There is an additional somewhat subtle feature. In the 1960s theta pinch researchers, who discovered field reversed configurations, tended to work at high density, high temperature, and more magnetic compression than optimum for long lifetime. In the small coils used for that work, typical lifetimes were 10 to 20 microseconds. Starting with Kurtmullaev in the former Soviet Union, after about 1975 the focus shifted to larger coils, lower density, less magnetic compression, and longer confinement time. As we now understand the physics, it appears that the early work is consistent with present understanding, but high density and small coils imply short confinement times. In 1979 Los Alamos proposed to expand FRC studies based on the earlier work and the improved understanding provided at that time by Rulon Linford, Dan Barnes, and others. In a second attachment to this email is a .pdf version of an appendix to the original 1979 FRX-C proposal. In it is a table summarizing early FRC experiments based on 65 references from the US, England, and Germany. One finds that FRCs were formed with density as large as $2 \times 10^{17} \text{ cm}^{-3}$, temperature up to 2 keV, and lifetimes over 10 microseconds. These are well beyond what is needed for the MTF proof-of-principle experiment. Of course the earlier work was generally less well diagnosed than we would like by modern standards, but in some cases the results were clear enough. Specifically, one of the later and better-documented experiments is in a reference from NRL (McLean et al, *Proceedings of APS Topical Conf. On Pulsed High-Density Plasmas*, LA Scientific Laboratory Report LA-3770, paper A5), which reports an FRC plasma with density $1.4 \times 10^{17} \text{ cm}^{-3}$, $T_e \sim T_i = 300 \text{ eV}$, and lifetime of 24 microseconds. This is a clear example of adequate performance for the proposed PoP experiments, and this reference and others were discussed verbally with the peer review committee. The NRL reference can also be viewed as

pages 38-42 of a report available through the LANL library at lib-www.lanl.gov/lapubs/00359060.pdf.

Beyond this experimental evidence, there have been significant advances in the theory and modeling of FRC formation and translation as described in the Tuszewski review article. These include analytic models by Steinhauer, Tuszewski, and myself, and an excellent two-dimensional numerical model called MOQUI by Richard Milroy at the University of Washington. Translation is well documented by experiments at LANL, U. Washington, and Osaka University. To a reasonable approximation, moving an FRC into a liner involves conversion of thermal energy obtained at formation into kinetic energy during translation, and back to thermal energy when the FRC is trapped in the liner. As seen in experiments and models, this process proceeds with negligible energy losses even when the translation speed is comparable to the Alfvén speed. Regarding formation, theory shows that theta-pinch formation works in approximately a self-similar manner as long as the fill pressure and tube radius are adjusted to keep nr^2 constant, where n is initial fill density (or FRC plasma density after formation) and r is initial discharge-tube radius (or FRC plasma radius after formation). . In other words, for a given capacitor bank, if we want higher density we simply work with smaller dimensions.

You raised another concern:

The imploding liner facility also needs to develop the capability of producing the kind and quality of implosion required.

With the press of time at the last FESAC meeting I probably failed to convey the importance of the recent experimental data on liner implosions. You and others understandably have concerns about liner technology because it is unfamiliar and clearly challenging. We are proposing to move metal at extraordinary velocity with sub-millimeter dimensional accuracy. We plan to work in a regime where the magnetic forces used to accelerate the liner considerably exceed the yield strength of the material, and Raleigh-Taylor modes of instability set limits on the accuracy by which the liner can be imploded.

Fortunately we have on the MTF team truly world-class expertise on liner implosions. During last year's review, Jim Degnan and colleagues from the AFRL (Air Force Research Laboratory in Albuquerque), and Bob Reinovsky (Los Alamos' Program Manager for high-energy density hydrodynamics) and colleagues from Los Alamos, offered the opinion that there is almost no doubt that a liner can perform as required for the MTF proof-of-principle experiments. Bob Reinovsky gave a survey of over 100 liner implosion experiments in the last five years addressing various issues, and a copy of his viewgraphs are available upon request.

Nevertheless, at the time of the proposal and during the peer-review discussions one concern emerged. As stated in the proposal on page 20,

“Typical liners used for DP (Defense Programs) applications have near-unity length-to-diameter ratios. FRC compression requires a length-to-diameter ratio of approximately 3.”

In a typical DP hydro experiment the target of the liner is a metal cylinder in which shocks are launched by the liner impact. In such experiments there is no advantage and it would waste kinetic

energy to make the liner longer than approximately its diameter. Therefore the MTF program needs data for an imploding liner with 3:1 aspect ratio as required for an elongated FRC target (desired for stability). According to numerical models there should be no problem, but in truth the models are two-dimensional, and do not address, for example, the possibility of helical distortions that might arise with a longer liner. In addition the required 10:1 radial convergence ratio with acceptably small dimensional perturbations has not been very well documented in past liner experiments because it is not usually a parameter of interest. "Acceptably small dimensional perturbations" are not very well defined yet, but we think that means perturbations of less than about 10% of the final radius or 0.5 mm in the geometry we envision with an imploded radius of 5 mm.

When we planned what to do this year with the first \$1M of MTF funding, we concluded that a valuable first experiment would be to document the convergence ratio in a liner of the desired dimensions for MTF using the Shiva Star facility at AFRL. As I described briefly at the last FESAC meeting, the first test, conducted on May 12, 1999, was a total success. Details will be presented by Jim Degnan in an invited paper at the ICOPS meeting this month in Monterey, and at the fall APS meeting. We will also bring data to the Snowmass retreat. Preliminary analysis shows rms radial perturbations less than 0.2mm with a radial convergence ratio of at least 13:1 (final radius of ~4 mm), thereby demonstrating liner convergence and symmetry beyond what is required for PoP.

To require more evidence than this in the way of scientific maturity with respect to the liner seems unreasonable. For any new technology applied to a plasma configuration the main issue for a proposal should be whether or not the technology can be applied as needed to do the plasma experiment. We feel the evidence in this regard is completely satisfactory.

You also offered an example to support the idea that MTF would be better judged as a concept exploration effort:

If PPPL had proposed a PoP test of the ST prior to the START results based only on theory and the tokamak data base, they would have been turned down, and told that a CE level test was needed.

You seem to suggest that the maturity of MTF is about the same as the ST idea prior to START. That is hard to accept. A better argument, to which I subscribe, would be: Development steps should be no larger than necessary to address the appropriate issues for a concept. By that argument, the ST START experiment followed by NSTX makes sense, and the MTF PoP proposal also makes sense. The ST concept is mainly an exploration to an extreme of aspect ratio, a variable that had been largely ignored in the past. The appropriate issues before START involved basic properties of equilibrium and stability with near unity aspect ratio. Because there was almost no database in existence, the logic of starting with a small CE facility to examine those ST plasma properties seems compelling. In the case of MTF, the appropriate issue to be addressed for further development is whether or not fast compression in a liner works as predicted theoretically. This view evolved in the last few years from extensive discussions at MTF workshops and in the preparation of an MTF R&D roadmap for the most recent Innovative Confinement Concept workshop held at Princeton. As discussed above, the sizable database on

both liner implosion technology and FRC properties (and spheromaks for that matter) leaves little reasonable doubt that a liner implosion experiment can be done. While we cannot be certain that no surprises will arise in the FRC target preparation work and the FRC-compatible liner development, it seems unlikely. The really interesting work and likelihood for surprise begins when we integrate the two elements. Thus the MTF program that makes sense is a multi-institutional effort to achieve liner-on-plasma experiments as soon as possible.

You also stated:

In 2 to 3 years when these [CE] programs have results, it would be appropriate to consider approval of a PoP test of MTF based on the established success of the two CE programs.

The essence of our PoP proposal is to do an integrated test of liner-on-plasma. Throughout the history of MTF-related research this was the least unexplored element. The Sandia electron-beam-driven magnetized “phi-target” experiments and some very limited work by Kurtmullaev in Russia, both done in the late 1970s, are the only previous attempts to mate plasma formation with a liner. As already argued, the most interesting scientific and fusion-relevant issues occur in the integrated test. I suspect the effect of your approach would be to postpone this most important aspect for a considerable time. What process do you imagine would be used and how long would it take *to decide* and to identify funds for an MTF PoP test following success of individual CE programs on MTF? Remember that the current debate about funding for MTF has been going on in one form or another for three years already. It seems to me that the arguments pro and con for MTF are clear enough at this point and now is the time for a decision.

Also, Ron Blanken raised a good point for you to consider: He said: “Do you have a similar question about readiness of the compact stellarator to proceed with a POP level program recalling that no one has ever done a compact stellarator experiment at the CE level?” I would add to his question: Would it not by your logic be better to do engineering tests on compact stellarator coils that are compatible with the PBX-M facility as a CE activity before we proceed to PoP activity for stellarators? In other words, should we hold back each PoP initiative until every element has been examined individually? I surely hope not in the compact stellarator case, the MTF case, or any other case. Possibly you would say that coil tests are not necessary because we “know how to build stellarator coils if we choose to do it.” A large number of us believe “we know how to build the needed liner and FRC.” The issue is what will happen when the liner implodes a plasma!

Finally, to think further about your questions, I went back once again and read the FESAC Alternate Concepts subpanel report that defined Proof-of-Principle and discussed what metrics should be used to advance a concept to PoP level. In some detailed respects, MTF does not fit the mold of what that panel imagined. For example, they stated that 8 years should be considered a minimum time to complete a proof-of-principle program. I trust it will not be held against MTF that we believe it can be done faster than that. Most importantly, the general thrust of the FESAC panel report shows that MTF is perfectly matched to what was anticipated for Proof-of-Principle. In terms of issues raised in the report, I note the following. Using either the FRC or spheromak as a target, MTF is based on a reasonable understanding of equilibrium and stability with some initial estimates possible for confinement. There is little question that net energy gain is feasible. There is

good evidence that an attractive power plant can be based upon the MTF approach as I discussed at the last FESAC meeting. The next step for MTF logically involves a national program as opposed to individual principal-investigator-level activities that characterize concept exploration. Finally, to quote that report: “The decision to proceed from one stage to the next should be based on the maturity of the concept in order to be reasonably confident that: 1) the next stage of the program will be successful; and 2) the anticipated benefits of the next stage of the research justifies the increased level of effort.” Basing “maturity” on probability of success for the next step seems appropriate. I hope the above arguments give you greater confidence that the peer review committee last year was correct when they concluded that MTF is indeed mature enough to carry out a successful PoP test. With respect to anticipated benefits, I suppose beauty is in the eyes of the beholder. Surely you would agree that if MTF succeeds, it will illuminate a qualitatively different possibility for the pursuit of fusion energy and it will allow new discussion of the optimum pathway. That is not bad for a 3-year total investment of \$21 million.

Sincerely,
Dick

Which version of the drift-wave instability theory should be applied to MTF devices?

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In this note we compare the drift frequency with the ion-ion collision frequency in a high-beta MTF plasma. We find that, typically, the drift frequency for the most dangerous long-wavelength drift perturbations is much less than the collision frequency and, therefore, the plasma is strongly collisional with respect to such modes. In other words, the parameter domain of MTF should be described by *strongly collisional* version of the drift instability. According to results of extensive studies of drift instabilities (e.g., [1]), in such situations, drift instability, if present at all, leads to transport much slower than Bohm transport. We leave specific analyses of instabilities and evaluation of anomalous transport coefficients until further publications and limit ourselves here to a simple parametric study.

We will express our results in terms of the plasma energy U , plasma density n , and plasma beta, $\mathbf{b} \equiv 16\pi nT/B^2$ in the imploded state. We assume that the plasma temperature in this state is $T=10$ keV. The plasma radius will be denoted by a , and the plasma length L will be described in terms of the elongation E (the ratio of the length to the plasma diameter, $E \equiv L/2a$). One has:

$$U(\text{MJ}) \approx 0.3 \cdot 10^{-19} E a^3 (\text{cm}) n (\text{cm}^{-3}). \quad (1)$$

The drift frequency is equal to:

$$\mathbf{w}^* \equiv \frac{cT}{eBa^2} ka, \quad (2)$$

where k is the component of the wave vector in the direction of the diamagnetic drift. Using the definition of beta, one finds that

$$\mathbf{w}^* (s^{-1}) = \frac{10^{15} \sqrt{\mathbf{b}}}{[a(\text{cm})]^2 \sqrt{n(\text{cm}^{-3})}} ka. \quad (4)$$

Expressing a in terms of U and n by virtue of Eq. (1), one finds:

$$\mathbf{w}^* (s^{-1}) \approx \frac{100 \sqrt{\mathbf{b}} [n(\text{cm}^{-3})]^{1/6}}{[U(\text{MJ}) / E]^{2/3}} ka. \quad (5)$$

On the other hand, the ion-ion collision time in a 10-keV plasma is, according to Ref. [2],

$$\mathbf{t}_i (\text{s}) = 3.3 \cdot 10^{12} / n (\text{cm}^{-3}) \quad (6)$$

where we have assumed that the ions have a mass equal to 2.5 proton masses, and have taken Coulomb logarithm equal to 10.

Typically, the most dangerous perturbations are the ones with smallest possible wave number (compatible with the finite size of the system), $k \sim 1/a$. These perturbations may lead sometimes to cross-field transport coefficients of the order of cT/eB . The dimensionless product $\mathbf{w}^* \mathbf{t}_i$ that characterizes collisionality of a certain mode is equal to:

$$\mathbf{w}^* \mathbf{t}_i \equiv \Gamma \cdot (ka); \quad \Gamma \approx \frac{3.3 \cdot 10^{14} \sqrt{\mathbf{b}}}{[U(\text{MJ})/E]^{2/3} [n(\text{cm}^{-3})]^{5/6}} \quad (7)$$

If the coefficient \mathbf{G} is less than 1, then all the modes with $ka < 1/\mathbf{G}$ are collisional and the anomalous transport should be well below the Bohm value. If, on the other hand, \mathbf{G} is greater than 1, then the most dangerous modes with $k \sim 1/a$ are collisionless and transport can reach the Bohm value.

For typical parameters of MTF experiments, \mathbf{G} is small. For example, in the case of fast liners compressing centimeter-size targets considered in Ref. [3] ($U=0.3$ MJ, $\mathbf{b}=10$, $E=2$, $n=10^{21}$ cm⁻³), one has $\mathbf{G} \sim 1/100$; for the Linus project [4] ($U=2500$ MJ, $n=10^{18}$ cm⁻³, $E=8$, $\mathbf{b}=1$) $\mathbf{G} \sim 1/150$; for the adiabatically compressed spheromak [5] ($U=150$ MJ, $n=3.5 \cdot 10^{17}$ cm⁻³, $E=1$, $\mathbf{b}=1$) $\mathbf{G} \sim 1/40$. Therefore, the anomalous transport should be well below the Bohm level.

In an elegant paper by El Nadi and Rosenbluth [6] a collisionless version of the drift-wave instability has been considered in a “infinite-beta” plasma. By that the authors meant a plasma whose cross-field confinement is provided by a direct contact of the plasma with the material wall (in the spirit of the “wall confinement” suggested by Budker [7]). The role of the magnetic field is reduced to suppressing the electron (and, in some cases, the ion) thermal conductivity.

Assuming that the magnetic field is uniform, El Nadi and Rosenbluth considered the case where the temperature and the density, both depending on the coordinate x , (perpendicular to the magnetic field which is directed along z), vary in the opposite directions, so that $n(x)T(x)=\text{const}$. The magnetic field in this case can be as weak as one wants; the only constraint is that the ion gyro-radius r_H is still smaller than the length-scale L . Their conclusion was that the anomalous diffusion coefficient is a fraction of cT/eB . Our discussion above shows that, in an MTF plasma, the anomalous transport is significantly lower than that value.

It is also worth noting that strong collisionality will even more significantly affect a special class of drift modes driven by the presence of plasma impurities [8]. For the ions with $Z > 1$, the collision frequency scales as $Z^4/A^{1/2}$; even if Z_{eff} is as low as 1.5, the collision frequency between the heavy ions becomes significantly higher than the collision frequency of the main ion component. This means that, for MTF applications, the theory [8] should be reworked to include strong particle collisions. Most probably, the high collisionality will reduce cross-field transport.

In the rest of this note we present a comparison of the Bohm confinement time and the time required to reach a certain Q value in MTF systems. This issue has already been considered in Ref. [9]. A (minor) variation introduced by us is related to an explicit inclusion of the parametric dependencies on E and \mathbf{b} .

We define the Bohm confinement time as

$$\mathbf{t}_{\text{Bohm}} \equiv \frac{a^2 eB}{cT} \quad (8)$$

Note that we define the Bohm diffusion coefficient without the numerical factor 1/16 and, therefore, our estimates are on the pessimistic side. The thus-defined time is just the inverse value of the coefficient in front of ka in the r.h.s. of Eq. (5):

$$\mathbf{t}_{\text{Bohm}} (\text{s}) \approx \frac{[U(\text{MJ})/E]^{2/3}}{100 \sqrt{\mathbf{b}} [n(\text{cm}^{-3})]^{1/6}} \quad (9)$$

For a 10-keV plasma, the fusion gain is:

$$Q=1.5 \cdot 10^{-14} n(\text{cm}^{-3}) t(\text{s}), \quad (10)$$

where t is the confinement time. We specify the confinement time t in terms of Bohm confinement time:

$$t = \chi t_{\text{Bohm}} \quad (11)$$

The coefficient $\chi > 1$ shows by what factor the transport is weaker than the Bohm transport. Collecting Eqs. (8)-(11) together, one obtains the following relationship:

$$Q \approx 1.5 \cdot 10^{-16} \frac{\chi [U(\text{MJ}) / E]^{2/3} [n(\text{cm}^{-3})]^{5/6}}{\sqrt{b}} \quad (12)$$

Table 1 represents the Q values for the three systems mentioned above. One sees that, with a very plausible assumption that $\chi=10$, all three provide reasonably high values of Q . In fact, in some of these systems a more stringent constraint on Q (at $\chi=10$) is imposed by a final dwell-time of the liner in the imploded state (recall also that we define the Bohm diffusion coefficient without a standard numerical factor $1/16$).

TABLE 1

Values of Q for various systems

System	$\chi=1$	$\chi=10$
1	4.2	42
2	7	70
3	1.8	18

System 1: $U=0.3$ MJ, $n=10^{21}$ cm^{-3} , $E=2$, $b=10$.

System 2: $U=2500$ MJ, $n=10^{18}$ cm^{-3} , $E=8$, $b=1$.

System 3: $U=150$ MJ, $n=3.5 \cdot 10^{17}$ cm^{-3} , $E=1$, $b=1$.

To summarize our discussion: in essentially all versions of MTF, the cross-field thermal diffusivity is expected to be significantly lower than the Bohm diffusion coefficient. The corresponding constraints on the performance of the system are very weak. The work on the theory of drift instabilities in the MTF setting is in progress.

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