White paper submitted to
The National Academy of Sciences
for their evaluation of the DOE magnetic-confinement burning-plasma research program

DOE should strongly support physics research and technology development for advanced-fuel FRCs

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July 2017

I. Introduction: the mainline approach

In 2009, DOE convened the Research Needs Workshop (*ReNeW*)[1] to provide "decisive influence" on research needed to accelerate progress on fusion energy during the "ITER era, roughly the next two decades." In the following 8 years, the ITER schedule slipped at least 8 years. ITER's tritium operations are now expected to commence after 2035,[2] three years later than asserted just last year.[3] An estimate of ITER's cost-to-build (CtB) is 44-71B$, (4.0-6.5B$/0.0909, the US share), gleaned from the May 2016 DOE report entitled *The US Participation in the ITER Project*.[3] This report states that ITER's CtB has grown a factor of five since 2003. Between the beginning of ITER's Conceptual Design Activity (CDA) in 1988 and the 2003 appraisal, ITER's CtB had grown by more than factor of 3. ITER's CtB has risen over a factor of 15 since its inception.

At the *ReNeW* workshop, experts concluded that tokamak-centric reactor development efforts would require at least two large devices after ITER before a credible commercial reactor could be designed, let alone be operated. These devices were: 1) a neutron-irradiation (materials-test) facility, to qualify the neutron resistance of meter-scale structures inside the reactor. The facility needs to provide a 14-MeV neutron fluence equivalent to that expected over a reactor's 40-year lifetime, an extremely costly, time-consuming, and technically demanding requirement; and 2) a DEMO[4] reactor with a duty factor in excess of 80%, operating for more than 10 years, and producing GW-levels of electrical power. The DEMO reactor would have the additional requirement of breeding its own tritium since ITER operations would exhaust **all of the world's** T reserves.[5] Breeding and efficiently recovering T in such large quantities and in as complex and inhospitable an environment as a fusion reactor have never been attempted.

Based on these experiences with the ITER project – over 50 years from proposal to expected completion of only the *baseline* goal – one can only assume similar schedules for these next two large and similarly complicated and ambitious proposed devices. Attempts to integrate tokamak fusion power into the electrical power grid in an economic way must be considered to be at least a century away. Implementing improvements into tokamak reactors, to improve their economic competitiveness, will be difficult because of their size, cost, radioactivity, and complexity. Add to this story the well-known physics problems, such as disruptions and high steady-state heat load,[5] tokamaks still face after 60 years of research.

DOE's *ReNeW* announcement acknowledged the importance of earlier DOE review activities, such as the *EPAct Report* (2006),[6] the so-called *Greenwald Report* (2007),[7] and the Toroidal Alternatives Panel (*TAP report*)[8] effort (2008). The *TAP report* promoted stellarator reactors to alleviate the disruption problem and ease some aspects of steady-state operations. However, stellarator-reactor studies[9] show them to be ~3x larger in major radius than tokamak-reactor designs. The cost would scale accordingly, to 150B$/prototype, hardly economical. Moreover, stellarators would not address the steady-state heat load problem, nor the tritium-breeding or the neutron-irradiation issues. That the NCSX,[10] a relatively small, non-radioactive US stellarator project, ran 3x over budget and was not completed – and its German cousin, Wendelstein 7-X,[11] took more than 9 years longer to build than planned – confirms the extreme difficulty that the stellarator's twisted geometry creates for engineers and gives little confidence in the stellarator approach as viable to fusion power.

The *TAP* workshop reviewed four classes of toroidal alternatives to the *standard* tokamak – the spherical tokamak, the stellarator, the reversed field pinch, and compact tori (CTs, namely spheromaks and field-reversed configurations, FRCs) – and set goals for their research. The first three approaches, all D-T burners, will suffer from many of the same maladies as do the mainline tokamaks, large size and cost, extensive neutron damage, demanding tritium breeding techniques, and high heat loads. Ever-expanding schedule delays would continue to be the norm. The 4th and least studied class, the CTs, has seen DOE slash their already inadequate funding and cancel entire programs.

II. FRCs: much progress

Yet over the past 5 years, 10 to 800 M$ in venture capital funding[12,13,14] has flowed into different private CT research projects! Why? There are four primary reasons: 1) CTs, as their names indicate, can be much smaller than tokamaks, at least a factor of three smaller in linear dimension. This translates into 30-fold volume and cost reductions and accelerated schedules. The compactness largely arises from the lack of magnets through the "hole" in the CT plasma donut and also *via* one aspect of the second reason now described. 2) *Aneutronic* (or advanced) fuels (AFs), *e.g.,* D-^{3}He or p-^{11}B, may be burned in CTs but not in tokamaks. AFs reduce radioactivity. AFs reduce the amount of shielding needed, reducing the reactor's size, and also remove the need to breed T in the vessel or to develop and test new neutron-resistant materials. 3) AFs require far higher ion energies than D-T, hence stronger magnetic fields. The peak magnetic fields (on the coils) necessary for tokamaks to burn AFs are above 30 T, not feasible with either ordinary conductors or superconductors. In contrast, CTs, particularly FRCs because of their near-unity β, 20x higher than a tokamak's, require magnetic fields below 10 T. Finally, 4) AFs release all their fusion energy in charged particles. This would amplify the tokamak's steady-state heat load a factor of 5 above its already intolerable level. CTs, because of their quasi-linear geometry, have simple ways to reduce the peak heat loads to tolerable levels. Energetic charged particles allow the possibility of high-efficiency direct energy conversion.

Why did DOE reduce FRC funding? The emergence of international tokamak collaborations and agreements, buoyed by strong tokamak progress in the 1980s and 1990s, placed political constraints on the domestic fusion program. That early momentum has supported tokamaks for over two decades. In periods of declining budgets, FRCs became easy targets. Historically, FRC physics has suffered from three criticisms, considered showstoppers. As we shall now describe, all three criticisms have been proven false in modern FRCs.

The first criticism was stability. FRCs were **predicted**[15] to be **unstable** to the internal tilt mode, whose growth rate is about an ion's transit time along the FRC's length, *ca.* 1 μs. FRCs in the 1970's and 80s did not provide definitive proof for or against this instability. Modern FRCs,[16,17,18,19] 1990's-present, consistently show results contrary to the pessimistic predictions. They have sustained collisionless high-β plasmas for 10-300 ms, limited by technical capabilities, not unfavorable physics. Kinetic effects, not included in the fluid model that predicted instability, explain this stability.

The second criticism was that FRCs needed to be large, to provide adequate **energy confinement** to achieve the Lawson ignition criterion. But FRCs can operate without ignition, in a driven mode where power is continuously provided to the plasma. This situation is actually very similar to that in tokamaks which need auxiliary power to sustain their MA currents. (For example, ITER plans to inject 50-93 MW of power to drive current.[20]) Moreover, FRCs, with $q = 0$, should have better effective energy confinement that $q \sim 3$ tokamaks whose neoclassical confinement[21] falls as $1/(1+q^2)$. Recent FRC experiments support this q dependence[22] and the proffered reasons[23] behind this excellent behavior, with β/χ , the Sheffield confinement-quality criterion, [24] about 10x better than in tokamaks having the same T_i.

Though the energy confinement criterion and the stability criterion have opposite dependences on machine size, as described later, there is a middle ground, a *Goldilocks* solution, where both are satisfied.

The third criticism is against the **advanced fuels**. Based on its extremely low neutron production compared to D-T, more than a factor of 10^5, p-^{11}B is the big winner. But critics correctly point out the difficulty of getting **net power production** from p-^{11}B fusion because of the low energy release (8.7 MeV *vs* 17.6 MeV for D-T), the high radiation losses at the higher temperatures required compared to D-T, and the low-density of ^{11}B in the plasma, due to its high nuclear charge, 5.[25] Indeed creative physics research is needed to address this weakness, a worthy challenge to research physicists and one that DOE should strongly support. The AF fuel D-^{3}He is criticized for a single reason: ^{3}He's **low terrestrial abundance**. Oddly, ^{3}He terrestrial reserves are of similar size to those of T! The ^{3}He reserves are sufficient for certain important and unique applications, *e.g.,* spacecraft propulsion for solar-system and exoplanet exploration[26] and planetary (earth) defense against asteroid impacts[27] and limited military use, though not for civilian power needs. Ample ^{3}He for civilian power could be bred *via* He-catalyzed, T-suppressed, D-D fusion.[28,29,30] Reactors of this type would be about 30x cleaner than D-T reactors while those of D-^{3}He would be 1000x cleaner. Some also affirm that the great abundance of ^{3}He on the moon is adequate response to the terrestrial scarcity.[31]

It should be noted that, though DOE funding for FRCs has been very low, less than 0.1% of the national fusion budget (*ca.* 400M$/yr), in the last 8 years, in large part due to venture capital, FRCs have come rather close to achieving the primary goal set for them in the *TAP Report*, "To demonstrate that a CT with simply connected vessel can achieve stable, sustained or long pulsed plasmas at kilovolt temperatures, with favorable confinement scaling to proceed to a pre-burning CT plasma experiment."

To advance the US's burning plasma program during the ITER era, DOE should substantially increase efforts on FRC research and development, to 50M$/year for the next decade, about 12% of the current fusion budget, ITER *plus* international *plus* domestic. At this level, a half-dozen *serious* FRCs could be built and operated – in universities, national labs, and private companies – providing a healthy, competitive research network, with facilities comparable in number but far lower in cost to that in the highly productive US tokamak program of 40 years ago. For this stage, technology development would be funded at about 1/5 the level of physics research. Technical and physics areas for this research are now described.

III. Research areas leading to burning plasma studies in FRCs

The plasma physics operational in small hot FRCs is remarkably different from that in tokamaks, in large part due to the major difference in the so-called s parameter, the ratio of the distance from the minor axis to the separatrix divided by the ion gyro-radius. In a small AF-burning FRC, $s < 10$, while for ITER, $s > 10^5$. The large s value in tokamaks means fluid models are often appropriate, though gyrokinetic models are now most often used for transport studies. In contrast, small hot FRCs require a full kinetic treatment. Even the 1st adiabatic invariant, a foundation stone of tokamak research, is not conserved in FRCs.[32,33] Large ion orbits may knit together the open and closed field line regions in FRCs, making useless the notions of a well-defined separatrix or of flux surfaces. A second major difference in the physics is that the plasma current in tokamaks is parallel to the magnetic field, **B**. In FRCs the current is perpendicular to **B**. Thus, the Taylor approach to stability analysis,[34] based on the Beltrami relation, $\nabla\times\boldsymbol{B} = \lambda\boldsymbol{B}$, is not valid for FRCs. Additionally, the strong field gradients and nulls, the lack of a toroidal field in FRCs and the low ratio of electron drift speed to ion thermal speed will have profound effects on whether micro-instabilities can grow.[35,36] Because of the differences between FRC and tokamak physics, new diagnostics and codes must be developed to perform this research properly; new control methods devised to handle the interacting functions of the many component systems.

The technology research and development needed for FRCs is on a far smaller scale than for D-T tokamaks because of the removal of the tritium-breeding requirement and the intense neutron flux. Most research will be aimed at qualifying and specifying existing materials for each function. For example, consider neutron shielding. Though the absorption properties of ^{10}B and ^{11}B are well known, with ^{10}B being far superior, one has choice whether to make the shielding pure B (and in which polymorph), or a compound, such as B_4C. The answer will depend on a variety of questions, such as whether the shielding serves several purposes, such as the vacuum vessel and/or a heat extraction component. Another example concerns which superconductor to use and at what temperature it should operate. The answer will depend on each superconductor's performance under neutron irradiation, time-varying magnetic fields created by the plasma, and the economics of manufacture and operation.

III.A. Physics areas of research

1. Plasma heating and current drive studies: Studies of different heating methods – beams, RF, and compression – are necessary, to provide diversity and control to the experiments. RF heating should allow FRCs to be smaller than beam heating because of the large plasmas needed to stop high energy heating beams. RF can be absorbed in small devices. The physics of RF heating in FRCs is quite different than in tokamaks, in large part due to the strong magnetic-field gradient and field nulls, both of which create stochastic orbits.[37] RF heating and current drive allow FRCs to be steady state, in contrast to the pulsed nature of compressional heating (which allows studies of

plasma dynamics). Steady state will avoid stress due to cyclic thermal and mechanical loads in reactors but is not essential at present. RF can be tuned to heat ions and/or electrons. Will non-thermal or non-equilibrium distributions evolve? How efficiently will RF be able to drive the required MA-level plasma currents in the desired locations within the FRC? Will ion-electron drag diminish current drive efficiency? RF antenna designs can alter energy coupling efficiency and energy confinement. Fully self-consistent models, both numerical and analytical, are needed of antenna operation, plasma heating, RF penetration, and current drive. Extensive experimental studies are needed, with new FRC-specific diagnostics.

2. Plasma transport: An FRC plasma comprises several regions, each with different transport processes dominating the physics. In the FRC's core, classical ion energy confinement is expected, in part because of the small size of fluctuations compared to gyro-radii. Will classical ion confinement be achieved? How about electron energy confinement there? More modern techniques should be applied, especially to studies of electron thermal transport. The ions in tokamaks have nearly neoclassical confinement but their electron transport is anomalous. Moreover, there is a loss cone in the FRC core, axis-encircling cyclotron orbits. How will these be affected by placing the FRC between two mirror coils whose loss cone is the opposite, excellent confinement for axis-encircling cyclotron orbits? The FRC device has numerous mirror regions. The relevant ones in this context are those with the weak part of the mirror field being located in the r-θ plane of the two X-point nulls and the strong field regions being at $z = 0$ and the two mirror-coil locations. Theoretical studies predict little energy loss through the X-point nulls[38] but particles losses can still occur. The effects of particle trapping and loss from the mirror regions may also be important for stability. New physics is expected from non-local cross-field heat and particle transport from the core into the SOL *via* fast-ion slowing down.[39] In D-^{3}He reactors this can have important beneficial consequence on T removal, T-suppression, and energy exhaust. Additionally, there will be a Fick's component to the transport across the (nominal) separatrix which must be considered. And, finally, transport in the SOL is expected to be dominated by axial, field-parallel flow and the SOL width would not be determined by Fick's law but by dynamics in the remote gas box divertor. The plasma in the SOL will consist of diverse populations, hot and cold ions, hot and cold electrons, and impurities. These expectations need detailed theoretical and experimental attention.
3. Radiation transport: In their midplane, FRCs have a magnetic field that increases with minor radius from the minor axis. Accordingly, synchrotron radiation emitted by the core (at the cyclotron frequency fundamental) will be of lower frequency than the plasma frequency towards the edge, hence not be transmitted. However, harmonics – such as generated by the field inhomogeneities – may be transmitted and therefore represent an important power loss mechanism. Additionally, synchrotron radiation propagating along the major axis will encounter decreasing fields, hence be readily transmitted. An excellent synchrotron radiation transport model is needed for the plasma. This must be joined with knowledge of the boundary conditions, especially the wall reflectivity.
4. Advanced fuels operational scenarios: With auxiliary heating, non-uniform, far-from-equilibrium ion and electron energy distributions are likely. These can have profound

effects on stability[40] and fusion rate. How the energy is shared between species is important. Additionally, the power-production stability of AF-burning plasmas and the effects on power production of excursions in density and temperature must be considered. In contrast to D-T tokamaks, AF high-β reactors operate near the peak of the fusion reactivity rather than on the rising slope. Excursions in density can raise the power above the desired operating point. Finally, fusion ash extraction methods must be tested experimentally.

5. Stability: The tilt mode has apparently been stabilized in present machines, but these have $s < 3$. One must not be complacent when extrapolating to larger-s fusion reactors. The effects of shape, such as axial discrete coils, can be an additional stabilizing factor, as well as the RF plasma heating method itself. One more macro-instability issue merits attention, the family of interchange modes. For small S*/E, less than 3,[41] Larmor radius stabilization may occur, as well as shear flow stabilization. Though Ioffe bars have stabilized interchange modes, loss of axial symmetry may have a detrimental effect on energy confinement. Early micro-instability studies revealed that the lower-hybrid drift instability mode (LHDI) would be the main culprit.[35] These studies also stated that the LHDI would be stabilized under FRC reactor conditions, an assessment that needs verification.

III.B. Technology research areas

1. Neutron shielding: Though AFs produce far fewer neutrons that D-T, the residual neutron production[42] will still require shielding to protect both equipment and personnel. ^{10}B is a superb shielding material because of its relatively large neutron absorption cross-section near 1 MeV, low density, and high electrical resistivity. One question is whether pure B can be fabricated into meter-scale structures that perform multiple functions, specifically heat removal, vacuum integrity, and mechanical-load support, while permitting RF penetration. If not, will B_4C provide a solution?
2. High-temperature superconductor performance: Though both high and low-temperature superconductors can provide the required magnetic field, *ca*. 3-10 T, they will experience a low-level of neutron irradiation and potentially time-varying magnetic fields. Studies are required of the resilience of superconductors, mounted in their support structures, to these environments.
3. High efficiency and reliability CW MW RF heating systems/components: Small clean FRC reactors have high circulating power. Dissipation of this power might cause inefficiencies so large as to be unacceptable. Development of highly efficient broad-band, tunable RF amplifiers, low-loss transmission lines and tank-circuits, and efficient antennas for coupling power to the plasma in steady state are necessary.
4. H, D, T, ^3He, and ^4He separation methods: The plasma exhaust stream of a small clean FRC reactor will contain a mixture of fuel atoms/ions and fusion products (ash). It is expected that the highly valuable ^3He and T constituents of the exhaust stream will be in low concentrations, as low as one part *per* 10^5. Extracting and separating these efficiently from the exhaust stream is important for developing T breeding methods that would allow He-catalyzed T-suppressed D-D fusion to make fuel for D-^3He reactors.[43] One suggested technique in permeation through multi-layer materials with implanted selective diffusion barriers

5. Thrust generation: Small clean fusion reactors are predicted to have unique capabilities for spacecraft propulsion. They would provide high specific power, high specific pulse, and moderate thrust, all which contribute to shortening the time for a mission and increasing the payload. These capabilities make them the only choice for certain missions, such as deflection of comets on earth impact trajectories. Testing of elements of these predictions, such as thrust augmentation and specific impulse control, is necessary.
6. Gas target divertors: An essential component of the small clean FRC reactor is the gas box divertor. It serves several functions. Firstly it cools the plasma exhaust stream so that sputter erosion is reduced and energy is distributed over a large area, mainly by the conversion of particle kinetic energy into low energy photons. Secondly, it provides a way to extract energy from the fusion products by sourcing the SOL with cool dense plasma of controllable thickness. Thirdly, power deposited in it can be converted into electricity, for providing power to the reactor (or spacecraft). Testing these concepts is critical.
7. Cooling and energy extraction systems: High efficiency systems must be designed built and tested for collecting the power lost from the reactor, both as photons and particles, and converting that into electricity.

IV. Summary

To avoid fusion power being delayed into the next century, increased research into and development of small, clean fusion reactors must occur. Clean, low-radioactivity fusion, only possible with advanced fuels, would eliminate the need to carry out costly and time-consuming neutron-irradiation and tritium-breeding studies, greatly accelerating the possibility of implementing fusion power. Reactors of scale 1-100 MW are suitable for a distributed power grid, amenable to incremental improvements in their technologies, and would provide unique capabilities in spacecraft propulsion. In the US, private capital is supporting this research at a level more than 100x greater than DOE.

Clean fusion is only possible in high-β devices. Advanced fuels are not feasible in tokamaks in large part because of excessive heat loads and nearly impossible magnetic field requirements. The FRC is the magnetic confinement device with the highest β, about 20x higher than a tokamak's. The FRC has a relatively simple linear geometry, greatly easing heat exhaust and allowing ready extraction of the fusion ash. These properties result is a 1000-fold reduction of neutron wall load for D-^{3}He fuel compared to D-T tokamaks. The p-^{11}B fuel mix would produce 100x even lower neutron levels, but physics studies are needed to explore whether net energy production is possible.

The plasma physics in FRCs is quite different than in tokamaks because of the kinetic nature of the plasma and field-normal direction of the current flow. Extensive efforts must be made to develop the proper theoretical and modeling tools, ones that treat the kinetic, possibly non-equilibrium, FRC plasma. Among the technical issues that need to be addressed are: T and ^{3}He (and other fusion ash) separation in the exhaust stream; manufacturing ^{10}B shielding that serves multiple purposes, *e.g.,* as a vacuum vessel and as a heat removal component; assessing the ability of high temperature superconductors to withstand extended neutron exposure and pulsed field loads; developing highly efficient RF heating systems; and methods to directly generate thrust from the fusion process.

The scale of the proposed research effort, 50 M$, is comparable to that of the 1970's US tokamak program, that is, about 6 mid-scale (10-20 cm radius) devices with kilogauss magnetic fields, each staffed by 10 PhD physicists and the appropriate level of support staff, *i.e.*, engineers, technicians, administrators, *etc*. An additional 12 M$ would be spent on the technology R&D.

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