ITER, fusion and the power reactor

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To:  John Marburger, OSTP Director,
Dr.  Mike Holland, SPA OSTP
or
TO WHOM IT MAY CONCERN:
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Dear Sir or Madam,

For the fusion program, which I work in since 1971 in Kurchatov Inst. (Moscow) and since 1992 in PPPL (Princeton), the present times are transitional to the power reactor development. The imminent decision on launching the construction of ITER will have an impact on reshaping and boosting the fusion research in the World. It also should affect the US approach to the reactor.

1 ITER and the basic reactor issues

In considering the readiness of the present fusion program for such a challenge as a power reactor, it is necessary to realize that the ITER project, although vital for keeping fusion research afloat, contributes rather little to the reactor development. In fact, ITER represents the dead end of a development which started so well 35 years ago (if counted from the 1968 Novosibirsk Conference when tokamaks were recognized as a way to fusion reactors). It was 25 years ago when PPPL amazed the World by generating the reactor range of plasma temperatures in a well confined plasma configuration on the PLT machine. This success, complimented by many others in all fusion related areas, launched an ambitious TFTR (PPPL) program resulted in 10 MW of fusion power in 1994, which later was matched and surpassed (by a factor of *one and half*) on the JET machine (Abingdon,UK).

Despite these achievements in plasma parameters and its understanding, fusion as a program appeared to be not ready for presenting a clear vision for a power reactor development. The problem became evident in the late 80s when soon after initiation of the ITER project, the plasma physicists canceled the attainment of the most important ITER target parameter, i.e., the $\simeq$ 10 MW year/m^2 level of neutron fluence (necessary for development and testing the first wall, which is the most crucial element of the fusion reactor). After this, (in early 90s) ignition was canceled as another target of the project. As a result, the ITER team was left struggling with implementation of a doubtful plasma physics concept into a project, which would, at least, appear to be a step to the reactor.

The two above mentioned cancellations converted the entire project into the next step in "fusion science" rather than a step toward a reactor. While providing for physics a unique opportunity of studying the

plasmas at reactor temperatures and alpha particle heating, ITER has twice bigger volume than the reactor and an order of magnitude lower fusion power density and neutron flux to the wall than the fusion reactor. As a result, even without nuclear components (for power extraction and tritium breeding) ITER costs almost twice a reasonable power reactor unit.

If compared with the space program, the ITER project is a large "space center" built around a "steam" engine (low pressure plasma), with neither rocket engine (i.e., high power density plasma regime) nor the rocket itself (i.e., the first wall and the tritium cycle systems) ever developed. Such a "space center" with its experience (expected in future) in operating the large scale fusion facility is useful (and should be supported) if there is (a) confidence in the possibility of acceptable fusion power reactor and (b) another way of its development.

Independent of ITER *decision*, a separate program should address the crucial triple goal objectives of the reactor physics and technology, i.e., high power density plasma regime, the first wall, and the tritium cycle. In fact, the situation and necessity of a complimentary program is well understood by the ITER people.

2 FES and new approaches to the reactor

In present, we live under the so called "Fusion Energy Science" mentality which after a failure to address the major power reactor issues substituted targeting the power reactor by "excellence" in science about the decades old plasma concept with its illusory relevance to the power reactor. On the surface it was complimented by a special technology program for "reactor studies" which results were interpreted exclusively to justify the existing plasma physics approach. After cancellation of the TFTR program the mentality of FES was elevated to the rank of the official name of the US fusion program.

But good science is not yet progress. Papers and reports, generated in a production mode, are not yet the critical knowledge providing the moment*um* for development. For more than 3 decades plasma physicists have struggled with understanding the confinement with no progress in making it relevant to the reactor requirements. The solution came not from the plasma physics community with its, highly advertised, supercomputer simulations of turbulence (which was certainly a great scientific achievement). It came from a modest Russian experiment on the T-11 tokamak (Troitsk) which demonstrated the unexhaustible capacity of lithium covered walls in pumping plasma particles. While on the eve of losing the TFTR program the management of PPPL was busy counting TFTR neutrons versus number of memory cells in micro-chips, Russians realized (at that time intuitively) that the "kitchen" effect of lithium conditioning (necessary for all TFTR high performance shots) is the essence of TFTR contribution to fusion. Then, in 1998 it was understood that large pumping capacity unavoidably leads to high plasma edge temperature and to elimination of turbulence as the major energy loss mechanism. If utilized properly, lithium covered walls open the way to any confinement level necessary for a reactor.

For the same 3 decades plasma physicists struggle with stability issues and low beta (ratio of the plasma and magnetic pressures). Nothing makes the MHD plasma model (adequate for strong magnetic fields) relevant to the reactor. The solution came not from their side but from the modest technology Lab at the University of Urbana (Illinois), which performed crucial measurements confirming low lithium sputtering. These data make lithium covered walls consistent with the high temperature plasma edge and allow for something unthinkable for the plasma physicists, namely, placing a stabilizing wall right at the plasma boundary and, thus, stabilizing the most restrictive so-called "free boundary" modes.

It is the story of more than two decades that the plasma physicists investigate the power extraction schemes from a plasma with reactor scale power density. The presently adopted concept of divertor (used successfully for killing the TFTR program) with its concentrated power deposition falls short by more than an order of magnitude with respect to reactor requirements. Again, the conceptual solution came from outside of the FES "street light spot" when propulsion of lithium was invented as potentially capable of extracting any necessary amount of energy from the reactor plasma.

Trapped in their 35 old concept the FES plasma physicists missed noticing such a minor thing that

insufficient tritium is available for development of the first wall for their "power" reactors, either tokamaks or stellarators.

Since January of 1999, when the concept of LiWalls was presented to the fusion community, there was a dual reaction from the technology and plasma physics sides. For technologists, at least a substantial part of them, this was a new opportunity. As a result, many crucial data were obtained on lithium and other liquid metals. New approaches to critical problems, not addressed properly so far, were formulated, such as "yacht sail", wire based first wall designs, liquid flibe blanket, etc. They opened expectations on potentially wide use of low-Z materials in the neutron zone and, thus, on low activation (in their proper sense) fusion reactors as well as on potential consistency of the first wall with the high energy fusion neutrons.

The reaction of some influential plasma physicists was a little bit different. First, they tried to ridicule the very notion of the high temperature of the plasma (about 10-15 keV) near the material wall, which, in fact is the natural and relaxed state of the plasma. A chairman of some innovation assessment committee of APS qualified it as nonsense by referring to his knowledge of the solar plasma and its heat fluxes. He, probably, did not recognize the difference between a well confined tokamak plasmas with a very limited heat flux (if not intentionally concentrated) and the solar flares. Other misleading stories were distributed about rivers of (highly flammable) liquid lithium and their enormous speed (200 m/sec) necessary to stabilize the plasma. Just for calibration purposes note that, e.g., painting of 1/30th of the ITER wall surface with 0.1 mm lithium layer (or 0.5 Gal) would provide particle pumping for 2 hours (with a full control of tritium). Problems with helium pumping (which is the exhaust of fusion) were raised as unresolvable, and so on.

In order to secure its fidelity to the 35 year marriage with the low edge temperature plasma and eliminate possibilities for "alien" developments, FES managers have destroyed (or sold) a series of machines (old PLT, PBX, TFTR in PPPL, TEXT in University of Texas, Austin). With modest modifications they would constitute a perfect set which could provide systematic, scaled studies of the new plasma regimes and technology development, probably beyond break-even. Instead of using a unique opportunity given by the US government in Sept. 2002 (by R.Orbach) the 2003 development plan was introduced to extend for the next 35 years the unperturbed life of the present approach. Its authors were too modest in their time request for "configuration optimization" as a centerpiece of the plan. Taking into account three decades of permuting three parameters of an axisymmetric plasma (elongation, triangularity and the aspect ratio), the innovative (in fact, 50 years old) 3-D plasma referenced in the plan provides opportunities for, at least, a century for its optimization. The promises of electricity in the grid in 35 years (with no ignition or addressing the real reactor issues planned), were simply ungrounded.

With all apparent nonsense, FES is a natural state of an old, externally subsidized program. Its behavior and evolution does not dependent very much on individuals involved. In business, a similar state is known as an "activity trap" (probably, not relevant to today market which drives infected companies bankrupt). In physics such a state was described long time ago as a thermostatic or a "thermal (or entropy) death" state, which any multi-parameter system will reach if there is no drive synchronizing its parts.

With equipartition distribution this is the most comfortable state for the components of the system. Having no free energy to produce anything coherent, the state has countless degrees of freedom to preserve its near maximum entropy. Only thermal (allowable) perturbations are possible, while anything disturbing disappears as a temporary hot spot with no effect on the system.

At the macroscopic level, FES is undistinguishable from the thermostat. Independent of the quality of individual scientists or their specific research, the impotence of FES is similar to absence of free energy in a thermostat even with a substantial thermal energy. Naive attempts to hide the real problems of fusion management while introducing elements of structuring with sub-programs and "centers of excellence" does not affect the growth of entropy and fragmentation. The leadership, capable of making the long range correlations, and competent enough to see and solve the interface problems is not only absent in the program, but intentionally is made impossible.

According to a FES official "We are long past the point where a single individual can simply assert a solution and expect to have everyone immediately change programs and directions." Instead of making an

environment encouraging leadership, numerous committees are established to "oversee" the illusory development of fusion and prevent any large fluctuations. It is hard to believe that there are people who may expect from these committees (the servant of the thermostat) anything except advises on equipartition distribution and re-re-formulations of how great is the failing program. Also the disorder with the sense of the time, which is characteristic for the thermostatic state, is evident. On one hand, six years passed since new ideas in fusion were presented are considered too short to evaluate them by FES experts (highly degraded already) and make a decision, while on the other hand, 35 years of a development plan, which piles all the unresolved problems at the reactor phase, are considered reasonable for promising electricity from fusion.

A general rule of a good bureaucrat is to be in phase with the second law of thermodynamics while trying to restrain the unavoidable growth of entropy in the system and delay the final state. Fusion is already in this state for, at least, a decade. The above statement *(from a DoE official)* just confirms this observation. There is nothing to be proud about in suppressing individuals and assisting the second law of thermodynamics. This law works by itself in eliminating the prominent fluctuations and never requires assistance.

3 Ignited Spherical Tokamak (IST) based program for reactor development

It is not possible to expect the development of the reactor from FES adopted approach. It is also not possible to expect for new ideas to distract it from perfecting the understanding of the 35 old plasma with the same low edge temperature. Validity of the ideas does not matter in the existing situation.

It is also risky for anybody to make crucial changes in the thermostatic system. Its accumulated thermal energy, while useless for the progress, may be released as a free energy against anybody who tries to destroy the comfort of the system or to make significant changes.

A separate program is necessary for the power reactor R&D which would be based on new regimes with high edge plasma temperature and wall stabilization in high beta configurations of the spherical tokamaks. Ignition and the high fusion power density, both necessary for reactor development, seem to be possible based on present understanding of confinement, stability and lithium interaction with the plasma.

1. In terms of confinement, these new regimes rely on implementation of the core plasma fueling and lithium covered pumping walls. Absorption of all particles (including impurities) by the walls leads to high edge plasma temperature, elimination of thermo-conduction, which at present is the major channel of energy loss. Then, only particle diffusion determines the energy losses from the core of the plasma.

   In contrast to conventional plasmas where the temperature gradient drives the turbulent thermo-conduction energy losses (essentially unlimited), the particle losses are always determined, and limited, by the best confined component (either electrons or ions).

   Thus, the new program will target the best possible situation for energy confinement, possible only with lithium covered walls.

2. In terms of stability such a program will develop the plasma stabilization with the walls situated right at the plasma edge. Properties of lithium are consistent with this idea. Also, existing experiments on spherical tokamaks (US, UK) already shown the feasibility of beta close to necessary 40 %.

   In contrast to conventional plasma, which is always detached from the walls, the proximity of the wall surface (compatible with the plasma edge) creates the best possible situation for the plasma stability and may further improve the plasma beta.

3. In terms of energy extraction the program will develop distributed energy deposition to the plasma facing surfaces, the only approach compatible with the reactor requirement. The experience from

several tokamaks around the world indicates the possibility of the precise shape control of the plasma even at the scale of the ion larmor radius.

With the larmor radius enhanced by the high temperature and by moderate magnetic fields of spherical tokamak configuration the possibility of distributed power deposition is realistic.

There are many other important aspects which define the reactor development program as distinct from the current FES. These three, the most fundamental ones, are sufficient to show that in all basic aspects the reactor development program is orthogonal to the current FES.

A University scale machine (similar to TEXT), capable of making 2-3 keV plasma, would be necessary for the long term scientific research of the plasma lithium wall interactions with some University students involved. The focus of the program would be on a series of 2-3 spherical tokamaks (10-20 m^3 plasma) with gradual development and implementation of the low recycling regime, core fueling, non-inductive start-up, high-beta stationary, bootstrap current sustained regime (with a limited RF current drive assistance) as the deuterium-deuterium phase. Finally, the ignition in DT and a stationary operational regime will be implemented in IST (30 m^3), specially designed for development of the first wall and the tritium cycle. A crucial milestone would be a success with the core fueling. Other issues seem to be technical and resolvable on the way.

It is important that new technology and design approaches are the indispensable part of the program from the very beginning. With the plasma physics issues much less challenging than in the presently dominant approach, the fusion technology will eventually take over for addressing the currently "untouchable"power reactor issues such as power extraction, survival of the first wall in the high energy neutron flux, and activation. Regretful to say, but there are no essential needs in fusion technology for FES, where the technology position was recently substantially undermined.

Experiments on spherical tokamaks made us confident in the possibility of accessing the high plasma pressure without use of very large magnetic fields but with high betas at modest fields. The technology data on lithium are sufficient to be confident in possibility of new regimes. If designed for utilizing new opportunities, spherical tokamaks can achieve ignition in about 30 m^3 of plasma volume (20 times smaller than ITER, see Fig.) with the fusion power exceeding 0.5 GW (larger than in ITER).

Being not a prototype of the power reactor, IST are crucial (and the only one known) for development of the high power density regime of the reactor, its first wall and the a tritium cycle. Because of limited tritium availability there is no chance to develop them based on large volume configurations. Thus, IST would be generically a necessary parallel to ITER program, focused on material issues and high power density. At the same time, among existing compact concepts there is nothing yet approaching the fusion performance of tokamaks and their modifications.

Six years of existence and negligence of such a self-consistent reactor development concept as LiWalls by the current "leaders", pretending of developing the fusion energy source, indicate that the government intervention, decisiveness and wisdom are required at this stage.

Most probably, some restructuring is already in vision. At this time, launching the new program (which could be called, e.g., as the Fusion Technology Program), can be arranged in a smooth way as a reaction to the incoming ITER decision. In this regard, it can be considered as a natural separation between a reactor focused development and the broad scientific fundamental plasma physics research. Accordingly, the fusion technology will find a proper place for realization of its potential and creativity.

In its turn, FES will be able to concentrate even more on fundamental physics and computer simulation of fusion, which have also unlimited opportunities for creativity.

In restructuring it is important to recognize and not to mix the unmixable and give a reasonably clean start for the new development.

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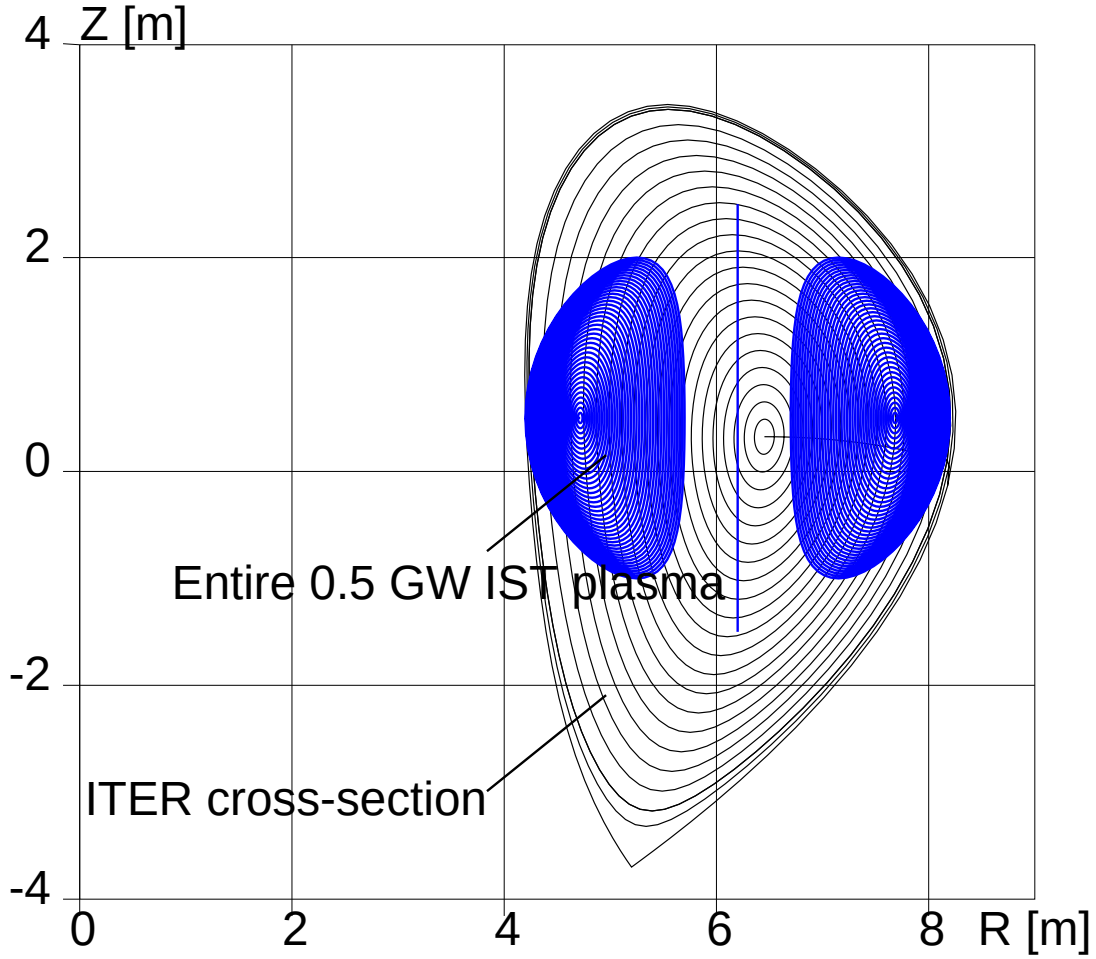

Fig. The relative sizes of the IST and ITER plasmas