White Paper on Magnetic Fusion Energy Priorities

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It is important to make ITER work (surpass fusion break-even) and it is equally important for ITER to reveal what the next steps should be. I feel DOE is showing excessive concern at the present time about materials and plasma-wall interactions for the following reasons:

1. The ITER design is pretty much frozen so any research on plasma-wall interactions or materials between now and the time ITER starts is unlikely to have a big effect on how ITER initially works.
2. We do not know precisely whether there will be problems with plasma-wall interactions or materials. There are many possible types of problems or there may be no problems at all. The only way to learn with certainty what the problems are is to operate ITER and find out. By prematurely devoting substantial resources to problems that cannot be precisely identified, it is likely that much of the effort will be misdirected and so will not address the real problem which is unknown at this time. It is better to wait to find out what the problems are before attempting to address them.

Once ITER is operating and identifies what the problems are, it will make sense to focus resources on overcoming these problems.

It is important to make ITER work because this will presumably make governments and the public consider fusion to be a more serious option than they do now. However, even if ITER works as promised, governments, the public, and utilities will remain concerned about the high cost of ITER and will ask if there is a way to achieve fusion in a more economical way. It would be well advised to be able to answer this question affirmatively. The obvious way would be to refer to ongoing research into configurations that would produce fusion more economically. Unfortunately, the USDOE has decided to decimate support for research into so-called innovative concepts and has put all its eggs into the basket labeled "tokamak". This neglect of alternates further hurts the ultimate prospects for fusion energy because alternate concept research provides excellent training the next generation of fusion physicists. This is because the exploratory nature of alternate concepts provides a broader introduction to fusion science than does restricting attention to the very well established properties of tokamaks. In essence, alternate concepts provide excitement to the field, help recruit new blood, generate new ideas, and help answer the question that is sure to be asked on how to make fusion less expensive. It is no coincidence that several private fusion ventures have been established to exploit various alternate concept ideas. These ventures are likely ahead of their time and will probably not produce fusion before ITER, but their very existence reflects the widespread popular support for trying out new ideas which is in contrast to the current DOE policy that new fusion configuration ideas are not needed.

My recommendation is that if there is any additional funding, it should not all go into plasma-wall interactions and materials, but instead a substantial fraction should go into alternate (innovative) concepts as these provide a potential answer to the question "how to make fusion cheaper" and they provide one of the main training grounds for the next generation of fusion physicists.