

The road to fusion

On the occasion of this special issue on fusion, I would like to speak once again in favor of a policy that I believe will prove crucial to the eventual success of our fusion effort—the need to maintain a broad-based program that supports a variety of approaches. (I had earlier made similar remarks in an address given at the APS Meeting in Colorado Springs in November.)

Fortunately, this need has achieved recognition in both the US and other countries. Most recently, in the US a prestigious ad hoc group of scientists and engineers convened to review fusion research under the chairmanship of John Foster. They came out squarely for the importance of maintaining a broad-based approach to fusion. To quote their report:

“The strategy which we recommend for the next several years is to pursue fusion on a broad front: broad in the sense of vigorous support to several different conceptual physics approaches, and broad in the sense of an intensive physics/engineering analysis, tradeoffs and experiments to identify and resolve problems which could stand in the way of a practical fusion reactor.”

I believe that there are many reasons why it is important to maintain breadth. Not only do we not yet know enough about the plasma state to feel cocksure about it, but also we have to recognize clearly the possible differences between devices that may produce scientific successes in the course of the research and those systems that can finally be successful from an engineering and an economical standpoint, and can at the same time best satisfy environmental and safety requirements. The best magnetic confinement system might turn out to be the least desirable system as judged against these criteria. It might turn out in fusion as it says in the Bible—“And the last shall be first.” Let’s face it, we are still learning our way around in fusion, despite our many successes.

If we think back on some of the approaches that were once very much in vogue and are now extinct we can see what might have transpired if, at the time a particular approach was in vogue, it had been singled out as “the chosen path” and all other approaches had been shelved. We would indeed have been in big trouble today. The gist of the argument for narrowing down is very familiar. It goes something like this: “If you will just concentrate your energies single-mindedly on one approach you will not only more clearly define just what it is you have to accomplish, but you will at the same time gain the approval of those outside the program who have been waiting for you to make up your mind and tell them the one true way to fusion power.” I claim that such arguments are both simplistic and dangerously fallacious, not only now but probably for the foreseeable future.

Some may be concerned about the extra expense involved in supporting research in a number of parallel approaches. I believe this concern misses the point: engineering development costs of any single approach can be expected to outweigh the total cost of a well-balanced, multiple-approach research program. To be as certain as possible about our final selection in the context of economic and environmental requirements, it is only prudent to spend the extra money needed during the research phase.

However, lest my remarks on the need for program breadth be misinterpreted, please understand that I am not espousing an ideologically egalitarian approach to the support of fusion research. Such a policy would inevitably lead to a “too little for too many” syndrome, a game in which nobody wins. There are, there should be, and there always will be “front runners” on the road to fusion, but the fusion marathon is still to be completed. In this context, then, it is my belief that the best strategy for fusion research should adhere to the following guidelines:

- ▶ Maintain a broadly-based program, one that includes both a spectrum of different approaches to fusion and a concern for fundamental plasma physics issues.
- ▶ Promote the development of fusion-relevant technologies, for example high-intensity particle beams and high-field magnets. We need to do this both to speed the pace of the research and to undergird future engineering requirements.
- ▶ Insure a proper balance between the research effort expended on “conservative” approaches, such as the Tokamak, and that expended on more speculative approaches, for example the Field Reversed Mirror. As we attempt to maintain this balance we need to take seriously the lesson of history that “fads” come and go in fusion research, just as they do in other pursuits.
- ▶ Encourage a search for innovative approaches, particularly those that lead to simpler or more compact fusion-power systems. At the same time, we should use the already considerable body of knowledge concerning plasma physics and plasma engineering to screen out the clearly unworkable ideas from the promising ones in assessing these innovative approaches.

I realize that many of these points have been emphasized by others, and that some reflect personal biases of my own. Nevertheless I firmly believe that they represent guidelines that define the surest and shortest path to fusion power—a truly inexhaustible source of energy for all mankind.

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