COMMENTS

In addition to a number of excellent regular papers, this issue of *Fusion Technology (FT)* contains an important "critical review" by W. R. Fundamenski and A. A. Harms on the evolution and current status of D-^{3}He fusion. This paper was encouraged, reviewed, and processed by the Critical Review Committee of the American Nuclear Society (ANS). Their program on critical reviews covers all areas of interest to ANS members, and a particular review paper is placed in the appropriate ANS journal.

While I did not instigate this review, those who know me realize my enthusiasm for D-^{3}He fusion, which dates back to my first serious consideration of advanced fuels as a natural part of the 1976 book *Fusion Energy Conversion*, published by ANS, which I wrote under a contract by the United States Atomic Energy Commission/U.S. Energy Research and Development Administration (ERDA)—the forerunner of the U.S. Department of Energy (DOE). ERDA wanted a book on fusion energy conversion but did not anticipate that my look at this field would lead me to an exposition of the potential and advantages of advanced fuels, such as D-^{3}He and p-^{11}B. I felt these fuels would be essential for the eventual commercial development of fusion power, not only to obtain charged fusion products to make direct energy conversion attractive, but also to reduce neutron damage and induced radioactivity and to increase environmental compatibility—while possibly allowing smaller power units! (Small is beautiful—or is it?) A little known fact is that ERDA delayed publication of this book for over a year because my view did not match theirs! As a "compromise," two chapters of the original book were eventually omitted, but enough about the promise of advanced fuel fusion was retained to make this book a force in the advanced fuel fusion movement, to the extent that this movement remains today in an austere budget situation that cannot even adequately support "mainline" topics. Of course, in my very personal view, D-^{3}He should be a mainline approach if we ever expect to fully exploit the advantages of fusion. The point, which I made in 1976, is that by the time we achieve fusion power, mankind will need and expect more than is offered by the current deuterium-tritium (D-T) magnetic fusion approaches. First, there is the ever-lengthening timescale for achieving a workable fusion power reactor. Now we talk about 50 years hence—in 1979, I told my wife it would take 20 years, following the conventional wisdom of fusion scientists and ERDA administrators of that day. (What poor judgment! My wife remembers and frequently reminds me about this prediction.)

Anyway, what vision do we share for a power plant in 2050? Should it employ D-T fusion? D-^{3}He fusion? If you vote for D-^{3}He, please consider the

question: How do we get there from here? You may guess my answer to this question, but that brings us back to the current critical review. I have been a personal friend and colleague of Archie Harms for a number of years, and I greatly admire his insight and many contributions to the broad field of nuclear energy. Indeed, in view of his recently announced retirement, it is especially appropriate to note his years of contributions to both research and education as Professor of Engineering Physics at McMaster University. Thus, it is with hesitation and regret that I must disagree with one aspect of his critical review. (This is not the first, nor will it likely be the last, time that I personally disagree with aspects of papers published in *FT*. However, my philosophy as your editor is that I am not an expert in all the topics covered in the journal, so I must rely on the reviewers' judgment. Thus, unlike a number of other editors today, I do not single-handedly reject publication of a paper in *FT* if it does not agree with my thinking. In my view, that simply is not my function or right.)

What is my concern about this critical review? Certainly with the intense scrutiny given it by the Critical Review Committee and the referees selected by them, this paper cannot be faulted technically. My concern is related more to the "attitude" that hangs over the conclusions. Archie basically concludes that D-^{3}He is not suited to conventional tokamaks, while better suited high-beta devices lack sufficient databases to adequately evaluate them. I cannot disagree with that conclusion; indeed, it is the mainline stance. That's the problem! I still have an *idealistic* conviction (hope? desire?) that high-beta D-^{3}He approaches can and *will* work. The database must be expanded; there will be problems and issues to overcome, but that is hardly new in fusion research. The community has shown a remarkable ability to solve problems, so I see no reason why high-beta D-^{3}He should be an exception. My view is that we must set a worthy goal and pursue it with great vigor. The question, then, is if D-^{3}He fusion is in fact a worthy goal. My response is simple: Look at the D-^{3}He field-reversed configuration reactor study done by Momota et al. (Ref. 95 of the critical review) and published in *FT* in 1992 (**21**, 2307). The results from this study stand out like a star among the numerous fusion reactor studies that have been reported over the years. If actually achieved, such a power unit would clearly be extremely competitive in all respects—from environmental, economic, and safety points of view.

From a more precise standpoint, what I advocate is a product-driven development of fusion power rather than an evolutionary-type development. Indeed, the 1990 review of the U.S. fusion program by the Office of Technology Analysis made the distinction between these approaches clear. They identified the present DOE program as being an evolutionary approach. That is perhaps not surprising because such a program represents relatively low risk in its early phases (but risks grow as the product stage is approached), and low risk is generally stressed in government projects financed by taxpayer dollars. The key question is, however, what percentage of large government research and development projects have succeeded over the years in the sense of leading to an attractive product? Further, will fusion be one of them? All of us in the fusion community need to consider this effort and, particularly now that the U.S. fusion program is being revamped, ask what should be done to optimize the situation. My personal view, again, is that the D-^{3}He goal must play a vital role in shaping the U.S. fusion development program. What do YOU think?

Again, our thanks to Archie Harms and Bill Fundamenski for bringing up this subject at this most critical moment in the life of the U.S. fusion program.

George Miley