Letters

Fusion Reactor Design Studies

In his Research News articles of 25 June (p. 1320) and 2 July (p. 38) William D. Metz discusses fusion research and draws a picture of the fusion program, particularly in the second article, based upon the results of recent fusion reactor conceptual design studies. In particular, he infers conclusions about the size of fusion reactors based in part on a series of studies we have carried out at the University of Wisconsin. We will discuss three key items: (i) the role of conceptual power reactor studies at an early stage in the development of a new power technology area like fusion; (ii) the inferences which can be drawn about the size and cost of fusion reactors, particularly tokamaks, at this stage; and (iii) the directions in which fusion technology is likely to go based on these early conceptual design studies.

Research on fusion technology problems began in earnest just 5 to 6 years ago. At that time, it was concluded that there was a strong need to define the many technological aspects of fusion reactors in order to provide some idea of which problems might be most important. The first approach was to attempt a relatively comprehensive conceptual design study of a future fusion reactor, and the UWMAK-I reactor study (*1*), referred to by Metz, was one of the earliest efforts of this type. While these and other studies have provided us with many fundamental insights, they cannot, and should not, be viewed as optimal or final, particularly with regard to size or cost. Indeed, the purpose of our subsequent studies, UWMAK-II (*2*) and UWMAK-III (*3*), was to consider technologically different systems to provide the basis for beginning optimization studies. Note that, not exactly knowing where we would end up in these studies, we were very conservative in a number of key design assumptions. Some of these determined the physical size of the system, especially in UWMAK-I and UWMAK-II.

It is hard to optimize a design when a quantitative technical base is not yet developed. Let us illustrate this and the impact of very conservative assumptions by pointing out the shape of some fast fission breeder reactor core designs in the mid-1960's. Large and conventionally shaped liquid metal cooled fast breeder reactor (LMFBR) cores typically have a positive central sodium void coefficient, and this is well known. In an effort to ensure negative feedback effects, core designs were developed in the mid-1960's that had large surface-to-volume ratios to increase neutron leakage. These pancake-shaped core designs did indeed have negative central sodium void coefficients, and such a design approach would be classified as highly conservative. They also produced a highly unoptimized reactor design. Nevertheless, this result did *not* mean that fast breeders were inherently too expensive or that it would be futile to pursue further development of such systems. Rather, the results pointed to the severe penalty of such a conservative approach and spurred the research and development needed to ensure a safe system design in a more optimum core shape. Today, fast reactor cores are designed as right circular cylinders, not as pancakes. The early fusion reactor conceptual designs must likewise be viewed as pointing out critical problem areas and not as the final word on the size and shape of things to come.

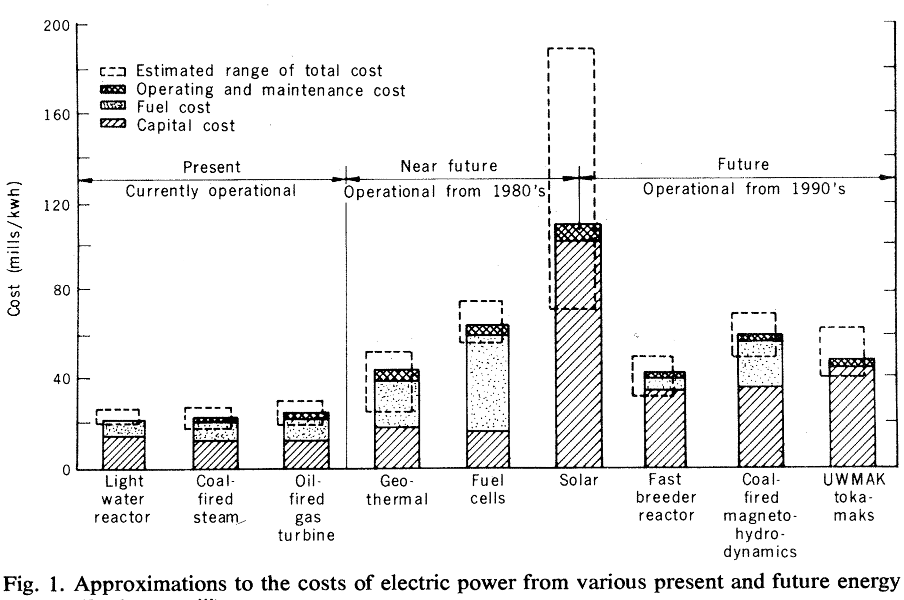

Fig. 1. Approximations to the costs of electric power from various present and future energy sources (*3*, chapter xiii).

The impression is given in Metz's article that tokamaks must generate large amounts of power and that they must be inherently large and therefore expensive. Our third study, UWMAK-III, considers a noncircular tokamak system where the plasma volume is one-third of that in UWMAK-I to generate the same thermal power output. Thus, the size of all the main reactor components like the magnets and the blanket and shield are substantially reduced. The fact that most conceptual designs are in the 1500 to 2000 megawatts electric range really reflects the fact that we assumed central station power units in the future would be somewhat larger than the approximately 100 Mwe units presently built. We did not intend to imply that smaller tokamaks (500 to 1000 Mwe) could not be built; they certainly can. Rather, power plants of 1500 to 2000 Mwe seemed more in tune with the early 21st century. Certainly, there is some economy of scale, as there is for most systems, but that does not imply that all tokamaks must be as large as the UWMAK-I design.

The cost of fusion power 25 or more years hence is difficult to state quantitatively at this time, but our preliminary studies (partly in conjunction with the Bechtel Corporation) show that it will probably be comparable with the cost of LMFBR's and cheaper than electricity from solar energy. Figure 1 summarizes the results of a self-consistent comparison of the cost of electricity in fourth-quarter 1975 dollars for present-day energy sources (most of which are temporary because of limited fuel supplies) and for several energy sources that are not fuel-limited and that will have to be exploited

in the long term. The details behind these numbers can be found in the UWMAK-III report (*3*, chapter xiii). Even the "unoptimized" early conceptual fusion designs show that they could be competitive with future fast breeder reactors, high-efficiency coal stations, geothermal stations, and fuel cells. All systems seem substantially less expensive than central station solar units. Thus, to suggest the cost of electricity from fusion will be several times that from advanced fission reactors is simply not consistent with our work. Any final choice among the available options (if such a choice must be made) will be made for a *combination* of reasons, including the long-term environmental impact (resource availability, storage of radioactive materials, land despoilment, and so forth) and the short-term societal impact (diversion of important materials, vulnerability to terrorist attacks, and so forth). We have found that fusion shows great promise in some of these areas, and that conclusion, combined with "reasonable" costs, encourages us to push on with fusion research to arrive at the final answers.

Undertaking the first generation of fusion reactor studies, conceptual though they are, has been a very healthy aspect of the fusion program. An effort is being made to understand the entire system and not to leave key aspects for 10 or 20 years hence or to spring surprises at that late date. We have discovered that certain engineering design approaches once thought most favorable are not particularly attractive. On the other hand, more optimized designs are being developed to take their place. We can now more clearly see the directions to go and we have a detailed basis to guide our analysis.

In his article, Metz notes that several people have called for a program that puts more emphasis on innovation and discovery, particularly referring to plasma confinement schemes. We would argue that this first generation of reactor studies has in fact laid the foundation for us to proceed ahead with innovation and imagination in the engineering design of these reactors to produce a more highly optimized system. Five or six years is only enough time to scratch the surface of an area like fusion reactor technology, and it should be clear that our most promising engineering discoveries still lie in the future.

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References

1. University of Wisconsin Fusion Feasibility Study Group, *UWMAK-I, A Wisconsin Toroidal Fusion Reactor Design* (Nuclear Engineering Department Report No. UWFDM-68, University of Wisconsin, Madison, 1974).
2. ——, *UWMAK-II, A Conceptual Tokamak Power Design* (Nuclear Engineering Department Report No. UWFDM-112, University of Wisconsin, Madison, 1974).
3. ——, *UWMAK-III, A Noncircular Tokamak Power Reactor Design* (Nuclear Engineering Department Report No. UWFDM-150, University of Wisconsin, Madison, 1976).

The Wisconsin reactor studies indeed show a progression in which the third design is slightly more compact than the first. But the reactor containment building for UWMAK-III would nevertheless be 65 meters high and 75 meters in diameter, with heat exchanger and turbine buildings almost as large. Even though the Wisconsin team has succeeded in boosting the power density of a fusion reactor from 0.7 to 2 Mw/m^3, there is a very long way to go before achieving the 100 Mw/m^3 power density that would allow a fusion reactor core to be as small as that of a light water fission reactor.

—W.D.M.

International Decade of Climatology

We were pleased to see from Willard F. Libby's letter of 28 May (p. 843) that an International Decade of Isotope Climatology Study is proposed, but we are concerned that only isotopic studies have been singled out. The research referred to by Libby is primarily marine in nature; the program, therefore, suffers from the exclusion of the great majority of the terrestrial record. Moreover, much of the marine sedimentary record cannot be interpreted with the degree of high resolution obtainable in the record on land where more rapid sedimentation allows annual or seasonal events to be resolved. This information is surely vital to questions of rapidity of past (and future) climatic change. In addition the marine record may be more climatically buffered than the land record.

Libby's reference to isotopic studies of the Greenland ice core raises doubts about the sole reliance on that approach for the polar regions, since alternative interpretations of the chronology of the glacial oxygen-18 record have been proposed and since complexities in correlations between ice cores have been noted (*1*). The ice core records do not provide adequate summer paleoclimatic data, and the tree-ring analyses are not applicable to the tundra regions. Thus very large arctic and antarctic areas which are sensitive to climatic change and were probably the first regions to experience