LETTERS TO THE EDITOR

COMMENTS ON "ON THE ECONOMIC PROSPECTS OF NUCLEAR FUSION WITH TOKAMAKS"

Professor K. Pinkau, director of the Max-Planck-Institut für Plasmaphysik, wrote to you when he heard you intended to publish a paper by a member (D. Pfirsch) and an ex-member (K. H. Schmitter) of his institute, entitled "On the Economic Prospects of Nuclear Fusion with Magnetically Confined Plasmas."[1] As a result, you kindly invited us to prepare a rebuttal. We had already seen a draft version of the paper, because it was submitted to the Science and Technology Options Assessment, which was conducted by the European Parliament and formed part of their deliberations on the European Fusion Program, now approved. This draft used a number of inaccurate or unrealistic assumptions that implied poor prospects for pure tokamak fusion. In July 1988, we discussed these extensively with the authors but were unable to persuade them to our viewpoint,[2] which they extensively criticize in their paper. In the belief that the final version accepted for publication contains the same assumptions, we therefore want to outline our objections to their work so that you can make an informed judgment on whether to proceed with publication.

In the plasma physics area, the authors claim that fusion produces very low power densities. To justify this, they put together the poor combination of high plasma aspect ratio and low field, whereas, in reality, high aspect ratio A allows high on-axis fields B to be produced for the same peak field, and they assume a pessimistic space utilization a_{wall}/a inside the reaction chamber and low plasma reactivity f, while mistakenly only partially including the beneficial effects of plasma elongation k. More realistic assumptions (e.g., $A = 3.5$, $B = 5$ T, $a_{wall}/a = 1.1$, $f = 2$, and $k = 2$) lead to typical wall loads more than three times higher than those quoted, well within the realm of what is tolerable for reactor viability, even without improvements in plasma characteristics.

With regard to engineering assumptions and the ability to exploit these higher power densities, a "chocolate-block" type of first-wall construction suggested recently to the authors by G. Coast of PEC (and published in 1982 in the readily available International Tokamak Reactor reports[3]) would at least double the tolerable heat loads and hence halve the costs. Another alternative achieving a similar effect, and being pursued already in next-step device designs such as the Next European Torus and the International Thermonuclear Experimental Reactor, is for a thinner wall covered with tiles. Pfirsch and Schmitter's selection of austenitic steel is also pessimistic, as swelling will be a problem for such a material at quite low fluences. There is currently no intention to use it for reactors, and it is only considered for near-term devices because of its extensive data base. For martensitic steel, a more likely reactor candidate, a doubling of heat loads will be possible within the same thermomechanical limits, leading to a further halving of costs. Further optimization both of design and materials should be possible as more is learned of plasma/wall interactions in the future, leading to further cost reductions.

On the energy accounting issue, the authors are heavily critical of previous work performed by Bünde,[4] and of its inclusion in our report.[2] This is not the place to rebut criticisms point by point, but the following is worth noting. First, the claim that inappropriate definitions of payback time and harvesting factors are used is without substance, simply because these definitions are *not* applied. In fact, the Appendix to Ref. 4 shows how the large variety of justifiable definitions leads to widely differing results, even though energy output and expenditure remain the same. Thus, we characterize[4] the net energy balance by the net energy gain, i.e., the difference between energy output and energy expenditure, since this is decisive for any judgment of an energy source. Second, the input/output (I/O) method is acknowledged to be the best suited for accounting for the direct and indirect energy inputs to goods. As this uses money-energy relations valid for average goods in power plant engineering, the energy input will be overestimated for the more carefully manufactured items needed for a fission power plant. Nevertheless, this is used as a starting point for assessing the energy input for a fusion plant. Process chain analyses are only used for scaling up the results of the I/O accounting method from fission to fusion[4] and are done with extreme caution, taking into account all fusion reactor designs available at that time and using industrial information to the maximum extent possible. Unfortunately, the authors declare themselves incapable of checking these data, choosing only to check the input to high-alloyed steels and citing a value from the U.K. steel industry that is nearly twice that valid for the Federal Republic of Germany (FRG). This difference, due at the time to the use of a more modern steelmaking plant in the FRG, was explained in Ref. 4. Thus, what is meant to be a criticism is just a confirmation. Clearly, the direction in the future will be to reduce, not to increase, the figures used. Third, the authors completely disregard the exhaustive sensitivity analysis,[4] demonstrating the great robustness of the statement of the net energetic superiority of fusion. This analysis showed, for instance, that a fivefold construction energy increase and an additional tenfold increase in providing the fuel raw material only slightly reduced the potential net energy gain from fusion. Finally, the authors propose a massive energy requirement for total waste disposal in a fusion plant, equal to the energy required to supply all fuel to a fission plant. This incorrect conclusion arises from the assumption[5] that *all* fusion waste, irrespective of activity level, will be disposed of as low-level waste. This is to be achieved by using a uniform-sized container and including sufficient shielding to reduce the surface dose to acceptable levels, resulting in some cases in <1% of the container volume being available for waste. More relevant assumptions (e.g., a slightly larger container!) would result in a considerable reduction in the number of containers (preliminary estimates[6] indicate a 75% reduction) and a corresponding reduction in energy consumption. All these factors strengthen our belief that the energy gain for fusion can be greater than for fission.

Concerning availability, the low values from earlier studies of mirror machines[7] were a conclusion from a first cut at the problem, and the *same* report eventually concluded that it was quite reasonable to expect availability of ~80% to be achievable. When the authors take only the first-cut conclusion out of context to justify their ends, they therefore do the reader a disservice. The achievement of high availability is being made a key consideration in the design of components for next-step devices, and no *a priori* reason has yet been identified to show that sufficiently high availabilities cannot be achieved in a power reactor. Even among existing fission plants, for example pressurized water reactor (PWR) and Canada deuterium uranium types, there are considerable differences in availability, so clearly design can have a significant influence on the ultimate value.

On the authors' economic analysis, there is only room to mention our main criticisms here. First, their approach to capital costing based on power-density-related and fixed cost components is of questionable worth despite its simplicity. An approach to generation cost based on itemized costing of the whole plant,[8] as used in our study, must, despite some weaknesses in the data base, produce a more accurate result for the tokamak than scaling linearly from PWR costs. Second, their relative cost equation assumes that the power-density-dependent plant has a uniform unit cost, even though the power density ratio involved is 80 (we would say 20, based, for instance, on the above first-wall design arguments). This uniform cost is unrealistic, since with these high-quality components the bulk of the cost is in manpower, not in materials. Their costs would not increase in proportion to volume and a considerably less than linear proportionality (e.g., two-thirds) between power density and cost might be expected. Third, the authors plot their cost graphs over wide ranges of availability, times for scheduled first-wall replacements t_{rw}, and wall lifetimes t_w, most of which are not relevant. As explained above, unscheduled availability of 0.8 should be achievable in practice, and it is unlikely that any reactor construction would be undertaken unless t_{rw}/t_w were <0.1. These values lead to $K_{rel} \sim 0.9$ or greater. Putting these values in the modified formula, with $\alpha = 0.1$ and $\cap_{rel}$ of 0.9, gives a relative cost of ~2 compared to the PWR, more in line with our own analysis. Finally, the authors claim that fission fits their formula, and that we overlooked this. If we had used their formula, we would have obtained a relative cost factor of 3.3 between Magnox and PWR plants. This, however, is not "in very good agreement" with the factor of 2 we quoted for these relative costs. To model reality, it is necessary to include something like the two-thirds power law scaling suggested above.

The conclusions drawn by the authors resulting from putting together all these unrealistic assumptions inevitably paint pure fusion in a bad light, and provide an excuse for unsubstantiated statements on the potential for hybrid reactors. On the basis of our arguments explained here, we see no reason to modify the conclusions of our report, which indicate that there is a good chance that *pure* fusion can produce a viable future energy source.

Given all the above points, we are rather surprised that your reviewers have recommended this paper for publication. Perhaps you will reconsider this now in the light of our arguments. If, however, you decide to go ahead, we hope that you will find space to include our comments in the same issue of *Fusion Technology*. If you need further information, we are willing to provide it on request.

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RESPONSE TO "COMMENTS ON 'ON THE ECONOMIC PROSPECTS OF NUCLEAR FUSION WITH TOKAMAKS' "

In reply to the foregoing letter,[1] we would like to draw attention to a number of points in our paper[2] that seem to have been overlooked by Spears et al.

As far as plasma physics constraints are concerned, the claimed factor of 3 higher wall loading capability would not improve the situation, which we assume is governed only by the thermal wall load constraint. The factor of 3, however, is mainly due to their value of 2 for f. Concerning the reactivity f, we noted in Sec. III of our paper that one must also include negative dilution effects due to alpha particles and impurities, which should approximately compensate for the neglected positive profile effects.

In Sec. III, we selected $k = 2$ (as in the letter), but chose $a_{wall}/a = 1.2$, instead of 1.1, which means a decrease in the wall load of 10%. More optimistically than is assumed to be necessary for DEMO-DN (Ref. 3), we chose $A = 4$ instead of 3.5, which would result in a reduction factor of 1.31; on the other hand, we have in addition to $B = 5$ T also taken $B = 6$ T, which has a much stronger influence; i.e., it leads to an improvement by a factor of 2.1. We have discussed the seriousness of the beta problem, which certainly cannot be considered to be solved at present. Since, however, some improvements might be achieved in the future, and we mentioned possible ones, we noted at the end of Sec. III: "Since thermal wall load constraints alone, as discussed in Sec. IV.A.1, turn out to be almost as severe as present-day beta limitations, we base the following discussion solely on the thermal wall load constraints." We consider the thermal wall load problem to be more basic, but we do not exclude the possibility that beta might continue to be, as today, the more critical quantity.

We mentioned the "chocolate-block" type of first-wall construction suggested by G. Coast, especially in our conclusions, and also the 50% reduction of the nuclear boiler cost that he claims. At present, we are, however, not in a position to evaluate his proposal in sufficient detail. Spears et al.'s reference to the International Tokamak Reactor in this context is not relevant. It concerns procedures to sustain the removal of first-wall melt layers created by disruptions, a problem area not addressed in our study. Chocolate-block structures are not envisaged at present for any next-step device such as the International Thermonuclear Experimental Reactor (ITER).

The greater void formation resistance of martensitic steels was the main reason in the late 1970s for placing this alloy, though it is ferromagnetic, on the list of candidate tokamak first-wall materials. The data base available remained insufficient for a comprehensive assessment of the suitability of these materials for tokamaks. In our paper, we discuss the tokamak aspects on the basis of present-day technology; we therefore had to take austenitic steel as for ITER. We have optimistically omitted the problems of fatigue and neutron damage.

We mentioned and discussed in Sec. IV.A.1 the possibility of using tiles to protect the first wall. To our knowledge, there are presently no sound ideas on how this could be done in a commercial reactor.

The harvesting factor and payback time are very essential quantities. They govern whether it is possible to introduce a certain system for energy production. It is therefore very important to get the logic of these quantities correct. We refer again to Sec. II of our paper.

We do not agree with Spears et al.'s statement, "the claim that inappropriate definitions of payback time and harvesting factors are used is without substance, simply because these definitions are *not* applied." Their "energy-gain" in Table 2.4, p. 66, in Ref. 9 of our paper is just the harvesting factor, inappropriately defined of course. The values in question are correspondingly misleading: The tokamak to pressurized water reactor (PWR) harvesting factor ratio in the table is ~2:1, whereas, properly defined, it is ~1:2 if calculated from the same energy values. By the way, the "robustness" of statements based on inappropriately defined quantities is of no importance.

We also do not agree with Spears et al. that there are many definitions that are "justifiable." Justifiable definitions should lead essentially to the same results but not to "widely differing results."

In Sec. IV of our paper, we discussed Bünde's method of generating quasi-input/output (I/O) construction energy values of a tokamak reactor plant by scaling up uncheckable process chain analyses (PCA) values in Ref. 11 of our paper. Bünde uses the ratio of I/O-to-PCA construction energies gained for PWRs for scaling. We showed that the resulting energy values, contained among others in Table 2.4 of Ref. 9 of our paper, are unusable.

Concerning the energy input values for stainless steel, we used Japanese data[4] to confirm Roberts' values. These values are a factor of ~2 higher than those used by Bünde as PCA values. He referred to Altenpohl's book (Ref. 21 in our paper), which, however, does not contain this figure or data leading to it. Until now, Bünde has been unable to show us explicitly how he arrived at his results. We are therefore "incapable of checking these data."

Waste disposal, like fuel production in fission reactors, is only a minor point in our discussion. These processes influence quantities such as harvesting factors or payback times by only a few percent. It may be possible to do better than the groups at Toshiba and the Institute for Plasma Physics, Nagoya University, have done; this, however, would not change much. Of course, the large quantity of radioactive waste is a problem in itself.

Concerning availability, we very clearly state that we do not use the primary results of 2 to 3.4% obtained by Musicki and Maynard. In Sec. IV.A.3 we write: "To achieve a higher availability, they recommend, among other things, 'on-line