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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 redundancy' for major subsystems..." and "Assuming this possibility [for the purpose of achieving an availability necessary for base load power plants], remote though it be, we also made calculations with correspondingly high K_u values, but of course without taking into account the still unclarified increase in investment cost." An availability of 80%, as mentioned by Spears et al., was, however, not used in our study. Such an availability is achieved for only a few fission reactors after many years of operational experience with these comparatively simple systems.

Industrial experience shows that itemized costing of exotic installations tends to be extremely unreliable. Where the proponents did the itemized costing themselves, they usually finished with too low results (see Sec. IV.A of our paper).

Concerning the relation among Magnox, the advanced gas-cooled reactor, and the PWR, Spears et al. originally scaled up the cost by multiplying the PWR cost by the power density ratios, leading to completely wrong results, which they then used as an argument against our procedure. It is most gratifying to see that Spears et al. have now used our formula essentially correctly, obtaining a reasonable result. There are, however, a number of quantities, such as thermal efficiency and others, for which we have given a detailed discussion in the fusion case, but which were taken for the above application in their simplest forms. It is possible that the factor of 3.3 might even come closer to the empirical factor of 2 when more appropriate values are used for these quantities.

In conclusion, we think that our results are based on realistic, partly optimistic assumptions and could be used as a guide for further program definitions aiming at pure fusion, perhaps via hybrids.

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