

Summary

This manuscript reports on numerical calculations of fast ion confinement and consequent peak wall heat fluxes for a specific flat top plasma scenario in the planned STEP device. The main loss mechanisms investigated were toroidal field ripple, resonant magnetic perturbations for ELM control, resistive wall modes, and toroidal Alfvén eigenmodes. In each case, a fixed equilibrium and set of plasma profiles were assumed, and a 1D scan was performed on some quantity relevant to the loss mechanism. The authors conclude that in realistic operating conditions, the heat fluxes are at an acceptable level.

Overall, there are useful quantitative results that can serve as baseline estimates for the anticipated heat fluxes due to fast ion losses. However, the generality of the conclusions is limited by the apparent lack of varying the equilibrium or plasma scenario, which could have a substantial effect on the resulting calculations. Throughout the manuscript, some descriptions of the methods and codes are overly brief, as detailed below. Aside from this, the language is clear throughout, and the manuscript is organized in a logical fashion.

I would like to see the following comments addressed before this manuscript is considered for publication in Nuclear Fusion.

Major comments (roughly chronological, not ranked in terms of importance)

1. Please include the plasma profiles that are being used in section 2 (plasma density, temperature, spatial distribution of alpha particles, q profile, etc). Likewise, it would be helpful to include some additional quantitative information about STEP in the introduction, such as magnetic field strength, major radius, and other basic parameters that provide useful context for this work. If these parameters have still not been finalized in the design, it then becomes even more essential to be explicit about the assumptions that are being made for the nominal configuration being used in your simulations. Some of this was mentioned in section 3, but it would make more sense to present it up front, as I would not consider the design parameters of STEP to be common knowledge.
2. I don't think the methods of Ref 20 are ones that most readers would be familiar with (kernel density estimation, leave-one-out cross-validation, and bootstrap resampling). The paper would be improved by giving a description of these methods. Especially since it seems that essentially all of the results and figures in section 2 rely on the calculation of the flux with these methods, these seem like nontrivial details that should be explained.
3. It was not clear to me if there were simulations performed that included all of the different field perturbing effects (TF ripple, ELM suppression, RWM feedback, and AEs) simultaneously. For instance, when the phase shift was being scanned for ELM suppression in Fig 3, did this use an otherwise purely axisymmetric field, or did it also include the toroidal field ripple with the nominal design parameters? If no simulation combined all of the effects, in my mind it is essential to do so, as it is not obvious that the alpha losses due to these different mechanisms would add linearly.
4. I found figure 5 difficult to read for two reasons. First, the reference point for the theta coordinate is not defined in the text. It seems that $\theta = 0$ corresponds to $Z = 0$ on the high field side, and then theta increases in a counter-clockwise sense based on matching up the left

and right plots, but this should be stated explicitly. Second, due to the very large changes in heat fluxes as θ is varied, the color scale does not really stand out on the left plot (almost the entire contour is dark blue, except for some isolated points). I would recommend remaking both panels on a log scale (the color scale on the left and the y axis scale on the right). Also regarding Fig 5, can any insight be gained about why these isolated locations have such dramatically higher fluxes than other nearby regions? The single point in the outer leg of the lower diverter that has very high flux seems odd to be since the points to the left and right of it have essentially zero flux. Why is it so intensely focused on this point such that there is not even a finite width to this peak like there is for the other ones on this plot?

5. Since the peak heat fluxes are so poloidally localized, does this mean that there are characteristic loss orbits for the alpha particles that prefer those specific poloidal locations on the wall? If so, it would be instructive if these characteristic orbit trajectories could be plotted on the inside of the cross section in the left panel of figure 5.
6. Similar to other comments, it would be worthwhile to give a brief description of the MARS-F code beyond saying that it solves an eigenvalue problem. E.g. what model does it use for the plasma, does it include the contribution from fast ions, etc. Some elaboration would be valuable to give the reading additional context beyond the existing description that it is a code that calculates the RWM eigenstructure. Likewise for HAGIS and HALO in section 3. Are these initial value codes, spectral codes, gyrokinetic codes, hybrid codes, what are their physics models, etc?
7. I didn't quite understand why TAEs were singled out as a representative fast ion instability. Compared to higher frequency gap modes like EAEs, etc, it makes sense to first consider TAEs. But were RSAEs explicitly excluded from the eigenspectrum calculation? If the fast ions are very super-Alfvénic as described in the beginning of section 3, I would naively expect that there would be a very dense spectrum of unstable modes that could be excited, maybe even including EPMs or other low frequency modes.
8. Do I understand correctly that only a single equilibrium and set of plasma profiles were used for this study? If so, I would think that all of the results would be sensitive to variations in the chosen equilibrium, but especially the RWM eigenstructure and TAE growth/damping rates. For instance, there is at least one paper I'm aware of that found a very strong sensitivity of alpha-driven AEs to small variations in the q profile for ITER-like plasmas: <http://dx.doi.org/10.1088/0029-5515/56/11/112006>. See for instance Fig 3. While I saw that varying the q profile was very briefly mentioned in the last paragraph as a possible avenue for future work, using a single equilibrium is in my mind one of the main limitations of this study, and so should be stated up front when the approach is described. All the better if an additional section could be added that includes some sensitivity studies.
9. Something that was not addressed in this work was fast ion losses due to TF coil misalignment, which was found to be impactful on fast ion transport in a similar study of fast ion confinement in SPARC: <https://doi.org/10.1017/S0022377820001087>. There is also a well-known, thorough study of fast ion confinement projections for ITER which came to a somewhat different conclusion than this STEP manuscript with respect to AE-induced transport: <https://doi.org/10.1063/1.4908551>. These differences could certainly be due to the different configurations in ITER vs STEP, but it could be interesting to comment on. In general, the

manuscript could be strengthened by comparing and contrasting its conclusions to similar studies of classical and anomalous confinement of fast ions in other reactor designs, if they are available.

Less significant comments

1. For all of the plots in the manuscript that show power fluxes, it could be useful to overlay horizontal lines that show the relevant acceptable threshold that was given in the introduction.
2. In section 2.2.2, can you please define $|b^1_{res}|$? Is it simply $\Delta B_{\perp} / B_{\perp}$, or something else?
3. In section 3, it is mentioned that the birth velocity of alpha particles is “nearly an order of magnitude higher than the typical values of cA ” in STEP. Would you please be more quantitative and quote a range of v/cA that is expected?

Very minor comments

1. Page 2, typo “Section2”
2. Page 5, type “toroidal is mode large enough”
3. The font size on some of the figure labels is fairly small. I would recommend increasing these font sizes to improve legibility.