

This document is organised as follows: In italics are the original referee reports and in bold text is our response.

Referee 1

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.*

We have included a new figure (see Fig. 1 of the paper) that presents the plasma density, temperature, q profile, and nuclear fusion reaction rate. The nuclear fusion reaction rate specifically illustrates the birth profile of the alpha particles. Additionally, we have added a table (see Table 1 of the paper) that lists key parameters such as the magnetic field strength at the magnetic axis, the major radius of the magnetic axis, and other fundamental parameters. Furthermore, we have added a sentence to the paragraph preceding Fig. 1, emphasizing that STEP is currently in the design phase, and many of these key values are subject to change in the future.

- 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.*

We have added Appendix C to the paper, which provides a detailed explanation of the techniques used from Ref. 20.

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.*

We have added Section 2.2.4, which includes all 3D fields combined in a simulation and provides a detailed analysis of the distribution of the alpha particle energy flux.

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 theta 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?*

We have replaced Figure 5 with Figure 7. In the new figure, we have added a label indicating where $s_\theta = 0$, and in the preceding paragraph, we mention that it increases anticlockwise. We attempted to use a log scale for better visibility, but it did not yield the desired improvement. Instead, we have made the figure much larger, increased the line width, and adjusted the colours on the colour scale.

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.*

We felt that the figure would be too cluttered if orbits were overplotted. Instead, we have added labels to describe the characteristic behaviour of the two major hotspots. The top hotspot primarily arises from prompt losses, while the bottom hotspot is due to orbits that exit the confined plasma due to collisions. This explanation is provided in the text preceding the figure.

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?

We have added a more detailed description of MARS-F to the end of Section 2.1 and a more detailed description of HALO to Section 3.

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.*

Section 3, on the TAE stability calculations, has been largely rewritten to include new calculations for the equilibrium that was used in most of the alpha-particle loss simulations. Except for an insignificant region in the immediate vicinity of the magnetic axis, the q-profiles used in the paper are monotonic-increasing (see top right plot of Fig. 1 in the new version), and therefore reversed-shear Alfvén eigenmodes (RSAEs) cannot exist.

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.*

We have added an appendix (see Appendix B) which analyses the alpha-particle losses in a scenario with a slightly different plasma profile and background magnetic field.

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.*

We have added discussion of the SPARC paper to the discussion section of the paper. However, we have also commented here that the ex-vessel ELM suppression coils

being designed for STEP may alternatively be used as error field correction coils (see Fig. 2), which could be employed to mitigate the impact of TF coil misalignment on fast ion losses.

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.*

We believe the plots are already quite crowded and, moreover, the thresholds quoted in the introduction are just estimates, so have not added horizontal lines.

2. *In section 2.2.2, can you please define $|b^1_{res}|$? Is it simply $\Delta B_{perp} / B_{perp}$, or something else?*

We have added a sentence clarifying that b^1 is the component of the magnetic field perturbation perpendicular to the flux surfaces divided by the absolute value of the magnetic field at that location.

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?*

As mentioned above, Section 3 has been largely rewritten. It now includes a sentence quoting the average alpha-particle birth speed ($1.3 \times 10^7 \text{ms}^{-1}$) and a typical Alfvén speed in the plasma core ($2.4 \times 10^6 \text{ms}^{-1}$). These figures quantify the degree to which alpha-particles will be super-Alfvénic at birth in STEP.

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.*

We have fixed the typos and increased the font sizes of all the figures.

Referee 2

In this work, the authors evaluate the confinement (or lack thereof) of fusion-produced alpha particles for parameters characteristic of the STEP reactor design and predict the corresponding power load on the plasma facing components, along with a stability assessment regarding toroidicity-induced Alfvén eigenmodes (TAEs). The nature of the presented results is indeed important to guide and validate design decisions about the specific STEP project, but (in my opinion) they do not provide any significant novel advance in the field of magnetic-confinement fusion (e.g., a general finding or phenomena, a new technique or approach that can be useful in a wider context) in order to make them sufficiently relevant for the typical Nuclear Fusion readership. Therefore I recommend that the authors first correct the manuscript (see the list of comments below) and then submit it to a more appropriate journal, for instance Fusion Engineering and Design.

However, if the authors judge it otherwise and wish to consider it for publication in this journal, then I recommend the manuscript to be substantially revised and considerably expanded according to the comments listed below in order to bring it closer to the standards that are expected of Nuclear Fusion publications. In the resubmitted manuscript, the authors should strive to address the following issues:

1) clearly describe the scenarios being considered and display all relevant data (temperature, density, and safety factor profiles, distribution functions, etc) as is common practice in similar works;

We have added plots of temperature, density and safety factor profiles we use (see Figure 1) of the paper as well as table of common values (see Table 1).

2) clearly describe the numerical tools employed, their inputs and outputs, their adequacy to the intended purposes, and their eventual shortcomings;

We have extended the discussion on MARS-F and HALO as well as the collision operator used by LOCUST.

3) Consider also the three loss mechanisms (TF ripple, ELM and RWM control coils) simultaneously because the eventual synergies may add novelty to the work;

We have added Section 2.2.4 which does this.

4) Expand significantly the TAE stability section (a convincing stability assessment can hardly fit in just three short paragraphs and a picture), discussing the employed methodology and the achieved results against those described in previous publications, as the ones indicated in the comments below (mostly concerning ITER) and also the more recent Fusion Sci. Techn. 79, 528 regarding the EAST tokamak and Front. Phys. 11, 1267696 related with the JT60SA device.

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Major comments:

Page 1, line 55: "Here, we consider the contribution of alpha particles...". Which and how much is the contribution of other possible sources? Is the alpha-particle contribution addressed in the manuscript significant?

We have added some detail including a citation to another paper which discusses this further. We provide more quantitative details on how the alpha particle heat load on the PFCs compares to the other sources.

Page 2, lines 23--28: In this paragraph, the authors mention the input profiles they will be using but never display them. However, the information conveyed by the temperature, density, and safety-factor radial profiles is fundamental to understand much of the alpha-particle dynamics and TAE properties. Therefore, the authors should make an effort to display all the relevant data. In the following discussion, I will assume that the scenario under consideration is the one described in reference [3] and take the inputs listed there when needed.

We have added plots of temperature, density and safety factor profiles we use (see Figure 1) of the paper as well as table of common values (see Table 1).

Page 2, lines 55--56: "We model... of about 6%". Here, it is not clear which is the energy distribution being considered for the alpha particles. Is it a Maxwellian with temperature set to 3.5MeV or a slowing-down distribution?

We have added an equation (see Equation 1) to make it clear that the energy is sampled from a normal distribution.

Page 2, line 60: How are the friction forces computed and how do they relate with the collision operator being used? How are the corresponding collision frequencies computed and under which assumptions?

We have added a paragraph which goes into more detail about the friction forces and explains that the collision frequencies are derived under the assumption of a weakly-coupled plasma and Maxwellian velocity distribution.

Page 3, equation (2): Apparently, all markers hitting the wall are assumed to contribute the same amount to the total power loss. Shouldn't they be weighted by the Jacobian of the phase-space volume they represent? Of course, this will depend on how the phase-space is being discretised by the markers, which was not discussed by the authors. It should be clarified if more markers are being assigned to more populated areas of the phase-space (akin to a full-f scheme) or if the sampling process tries to be "homogeneous" to save computational resources (similar to delta-f schemes).

We have expanded the paragraph which discusses this to make it clear that we sample uniformly across the volume but then weight the markers according to the DT reaction rate.

Page 3, lines 57--58: "...with the numerical predictions of the ripple field...". Which numerical results are the authors referring to? Was the ripple field computed by some code? Which one? If a numerical and more accurate representation of the ripple field is available, why using the less accurate representation in equation (3)?

We have added an appendix (see Appendix A) that shows that the numerical result is very close to the analytical field and so we are justified in using the analytic approximation.

Page 8, line 60: The results presented in this section were achieved by taking into consideration each effect separately (toroidal field ripple, ELM suppression and RWM control schemes). Can synergies between them enhance the predicted losses if they are taken all at once?

We have added section 2.2.4 which includes multiple 3D fields superimposed in a single simulation.

Page 9, line 34: "This is nearly an order of magnitude higher than typical values of c_A ". Without proper information about the density profile under consideration, it is not possible for the reader to get the value of the Alfvén velocity c_A and judge about the validity of this statement. Following reference [3], the ion (1:1 DT mix) density $3.5 \times 10^{19} \text{ m}^{-3}$ produces $c_A = 4.2 \times 10^6 \text{ m/s}$ which is just 3 times smaller than the birth velocity, not one order of magnitude.

The core density in the scenarios considered here is actually about $2 \times 10^{20} \text{ m}^{-3}$ (see bottom left frame of Fig. 1 in the new version), meaning that the Alfvén speed is around $2.4 \times 10^6 \text{ ms}^{-1}$ (this is now quoted in Section 3) and the ratio of alpha-particle birth speed to Alfvén speed is ~ 5.4 . On a logarithmic scale it could be argued that this is indeed "nearly an order of magnitude". Nevertheless, we have dropped this statement from the new version of Section 3.

Page 9, line 40: "...high plasma beta device ... bulk thermal speed v_i will be comparable to c_A ". The plasma beta value seems to be particularly high from reference [3] data (thermal/magnetic energy ratio $\beta \sim 0.4$, although the authors do not give any clear hint about it), but this is mostly due to the electron pressure ($T_i < T_e$). Indeed, the ion thermal velocity seems to be quite modest ($v_i \sim 0.8 \times 10^6 \text{ m/s}$) and thus about 5 times smaller than c_A , not "comparable".

As shown in Figures 1 and 12 in the new version of the paper, the core values of T_i are in fact higher than those of T_e and correspond to a deuteron thermal speed of $1.45 \times 10^6 \text{ ms}^{-1}$, which is about 60% of the Alfvén speed in the same region of the plasma. The statement that the two speeds are comparable is thus justifiable.

Page 9, lines 42--44: "As a result ... strong Landau damping". If the calculations in the previous comments are correct (again following reference [3] as a source) the conclusion is precisely the opposite: the fundamental resonance is able to interact with sufficiently energetic alpha particles around 400KeV (and not 2 orders of magnitude below 3.5MeV as hinted by the authors), while thermal ions will interact mostly via the less efficient $c_A/5$ or $c_A/3$ side-band resonances. These velocity parameters (c_A and v_i) are similar to those in previously published stability assessments for the ITER case (e.g., Phys. Plasmas 22, 021807; Plasma Phys. Control. Fusion 57, 05401; Nucl. Fusion 55, 083003) where unstable TAEs were found. The author's results should be compared against these and any differences explained so that a broader knowledge about the stability mechanisms emerges from the exercise.

As mentioned in the above comments, the correct parameter values for these equilibria give a lower Alfvén speed and a higher ion thermal speed than those estimated by Referee 2, and so the ratio v_i/c_A is actually rather higher in STEP than it is in ITER and in other magnetically-confined fusion reactor designs. Moreover, the exponential sensitivity of the Landau damping rate to this ratio [see Eq. (9) in the new version of the paper] means that even a relatively modest increase in its value can result in the mode being suppressed. This is why TAEs are predicted to be stable in STEP despite being unstable in ITER. We agree with Referee 2 that the interaction of the modes with thermal ions occurs mainly via sideband resonances, but the sideband resonances in this regime are not weak. These points are made in the new paragraph in Section 3 starting with “The origin of the very strong mode suppression in Fig. 9 ... “.

Page 9, line 46: "... using the HAGIS and HALO codes". Why do the authors believe that these two codes are necessary and adequate for the proposed job? Their major characteristics should be described and their adequacy to the intended purpose clearly justified. In particular, it is not clear why the authors need a guiding-centre model for the nonlinear wave-particle interaction (HAGIS) together with a full-gyromotion one (HALO). Actually, it is stated in the manuscript that HAGIS is employed for the alpha particles (gyro-radius $\sim 10\text{cm}$) and HALO for the thermal ions (gyro-radius $\sim 1\text{cm}$). Why is that so? Since the authors are using a nonlinear model for the wave-particle interaction, have they explored the possibility of non-linear interaction between the several eigenmodes?

All of the TAE drive and damping calculations presented in the new version of the paper were obtained with HALO (rather than HAGIS), and both guiding-centre and full orbit versions of this code have been used. As the referee points out, the guiding-centre approach is less justifiable for alpha-particles than it is for thermal ions. Accordingly, we have calculated TAE drive rates using both approaches, and used the guiding-centre version of the code to model the thermal ions. The guiding-centre approximation should be sufficient for the bulk ions provided that their Larmor radii are small compared to the scale lengths of the modes, which is indeed the case [see Fig. 8 in the new version]. However, the full orbit code yields consistently higher intrinsic drive rates than the guiding-centre version [Fig. 9 in the new version]. The reasons for this are not yet known, but in any cases even the higher drive values are found to be smaller than the Landau damping rates in every case. We have so far only modelled single TAEs, and have thus not explored the possibility of nonlinear interactions between several modes. However, given that all such modes are linearly damped, it seems unlikely that nonlinear interactions play any role, at least in these scenarios.

Page 9, lines 48--50: "Growth rates of TAEs with a range of values of n ... are plotted in figure 7". How were these TAEs computed in the first place? With which codes and with which inputs? Figure 7 shows mostly one eigenmode per toroidal mode number ($n=2,4,6$ are exceptions, with three or two eigenmodes), but TAE gaps may produce multiple eigenmodes. Was the TAE gap systematically scanned in frequency to find all possible modes as described in previous stability assessments (e.g., Nucl. Fusion 55, 083003)? Why was the toroidal mode number limited to $n=18$ and why are no modes shown for $n=1$? Could the authors frame their toroidal mode number domain around the n value corresponding to the most efficiently driven TAE estimated following the methodology described in Phys. Plasmas 22, 021807?

The method used to compute the TAEs is explained in the new version of Section 3: first, the CSCAS code was employed to calculate the shear Alfvén continuum, and then TAEs lying in the continuum gaps were computed using the MISHKA MHD eigenvalue solver.

Page 9, lines 51 and 56: The reported growth/damping rates values (0.18 and -1.4) are not much smaller than 1 and therefore show that the MHD-perturbative approach employed by HAGIS and HALO is, most likely, not valid for that particular eigenmode.

The highest normalized bulk ion damping rate presented in the new version of the paper is slightly under -0.3 (rather than -1.4), and the *net* normalized damping rate of this mode (taking into account the drive in the full orbit case) is about -0.17, but the referee is nevertheless right to question the validity of the MHD-perturbative approach in these circumstances. On the other hand, the essential conclusion, that TAEs will not be excited in these circumstances, is not called into question here. Text making these points has been added to the end of Section 3. We agree that the linear response of the plasma will differ substantially from the MHD-perturbative limit insofar as it starts to become questionable whether TAEs still exist.

Page 10, figure 7: For the sake of completeness, figure 7 should be complemented with the corresponding damping rate for each eigenmode.

Figure 7 in the original draft has been removed. Figure 9 in the revised version shows damping rates as well as drive rates for each eigenmode, as requested.

Page 10, lines 52--53: "However, higher frequency compressional Alfvén eigenmodes are probably irrelevant to STEP...". In view of the large beta expected for STEP scenarios, have the authors considered the problem posed by lower-frequency (when comparing with the TAEs frequency at low beta) couplings of the acoustic and shear branches as described in the references Plasma Phys. Control. Fusion 49 B371 (beta induced acoustic Alfvén eigenmodes or BAAEs) and Nucl. Fusion 61 (2021) 096001 (high-order geodesic-acoustic couplings)?

This is a valid point, which we were aware of, but we have not so far performed any modelling of acoustic modes in STEP. Our focus has been on modes that we expect to couple to alpha-particles. Given that TAEs in STEP are so strongly affected by the bulk plasma, it is reasonable to suppose that this is even more the case at lower frequencies. We cannot yet rule out the possibility that modes in this range could be driven unstable by the thermal plasma, but this possibility is out of scope for a paper focusing on alpha-particle physics.

Text acknowledging the future need to investigate such modes (and their coupling to shear modes) has been added to the end of Section 4, where we also now mention global as well as compressional Alfvén eigenmodes.

Secondary comments:

Page 1, line 53: The acronym PFC is not defined.

Page 5, line 38: "...required to ensure... in Table1". This sentence seems in disagreement with the table caption

Page 8, line 42: The symbol "G" is not defined.

The acronym has now been defined, we have added a footnote to explain that G is for gauss and we have fixed text to align with the table caption.