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 Alfven 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.**

1. *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.**

1. *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.**

1. *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.**

1. *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 are pushed out due to collisions. This explanation is provided in the text preceding the figure.**

1. *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.**

1. *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-Alfvenic 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.*

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1. *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*](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 particles losses in a scenario with a slightly different plasma profile and background magnetic field.**

1. *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.*](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.*](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.**

*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, moreover, the threshold in the introduction are just estimates so have not added horizontal lines.**

1. *In section 2.2.2, can you please define |b^1\_res|? Is it simply delta Bperp / Bperp, 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.**

1. *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?*

**It’s difficult to quote a range as STEP is still very much in the design process.**

*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 Alfven 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, 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 Alfven velocity c\_A and judge about the validity of this statement. Following reference [3], the ion (1:1 DT mix) density 3.5e19 m^{-3} produces c\_A=4.2e6 m/s which is just 3 times smaller than the birth velocity, not one order of magnitude.*

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*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~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~0.8e6 m/s) and thus about 5 times smaller than c\_A, not "comparable".*

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*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 where 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.*

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*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 ~10cm) and HALO for the thermal ions (gyro-radius ~1cm). 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?*

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*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?*

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*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.*

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

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*Page 10, lines 52--53: "However, higher frequency compressional Alfven 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 Alfven eigenmodes or BAAEs) and Nucl. Fusion 61 (2021) 096001 (high-order geodesic-acoustic couplings)?*

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*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 not 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.**