

Comments on the manuscript NF-106989 'Confinement of fusion alpha-particles and Alfven eigenmode stability in STEP', by A.P.K Prokopszyn et al.

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

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?

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.

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?

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?

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).

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)?

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?

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.

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 ($T_i \ll T_e$). 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".

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 400 KeV (and not 2 orders of magnitude below 3.5 MeV 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.

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?

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?

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.

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

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)?

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.