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COMMENTS TO THE AUTHOR(S)

The authors present a comparison between local (LPC) and global (GPC) current-free boundary conditions applied to an axisymmetric, hybrid simulation of a Hall effect thruster. They apply these boundary conditions to three separate system configurations: one with an external cathode (EC) inside the magnetic field separatrix (MS), one an EC outside the MS, and one with an internally mounted cathode, using four separate domain sizes, i.e., 12 different cases. There are several issues noted for this manuscript to be considered for PSST in its current form:

Major Points:

1) The authors should address how the simulations are validated against experimental data. It is indeed an interesting study from the perspective of Hall thruster simulations, but the paper reads more like a “sensitivity study,” without detailed discussions of the physics. So, what is the novelty in the physics that the authors discovered or investigated with this new computational model? There are some mentions of experimental measurements which agree with the cathode location study, but there are no direct numerical comparisons to any experimental results (e.g., thruster performance, oscillation amplitude or frequency, etc.).

2) The authors do not discuss the details of the hybrid code, HYPHEN. It can be understood that the authors did not want to make the paper any longer (already 16 pages), but it is critical to explain the basics of the code. The major comments include the following. (i) The E-module is perhaps the most important. Please add some detailed explanation for readers who are not familiar with hybrid models in Hall thrusters, e.g., what kinds of equations are solved and how are the equations solved? In particular, the quasineutral drift diffusion model solves an elliptic PDE for the potential, so how one can “force” the current condition via the electron equations is of great interest and importance (e.g., eq (1) perhaps determines the balance between the electric field and pressure gradient). (ii) The sheath (S) module might also play an important role in the investigation. Please elaborate on “The local sheath potential fall is also determined” and “retains non-Maxwellian features of the electron velocity distribution function (VDF)”. (iii) Please include particle statistics in the report or demonstrate particle convergence. How many macroparticles per cell were used in this study? Are the presented results instantaneous or time-averaged? (iv) Can you include any comments about the computational cost of the model?

3) The anomalous scattering frequency is assumed to be a function of axial position only. Are the same values used in the same magnetic field lines or radial direction? Past studies (cf, Hall2De) have shown that an anomalous scattering frequency is also needed for the cathode-to-plume coupling.  It would seem to me that in a paper investigating the cathode boundary condition these details would be important and should at the very least be mentioned. [Page 3, Paragraph 4]

4) Figure 2 is interesting, but what is the meaning of showing all four cases? There are only 2 paragraphs explaining the results. It seems like P1 is different when the domain is truncated but P2-P4 looks pretty similar, i.e., the near-field plume is not affected, and the far-field plume shows a circulation of electrons at far field. The paper could be significantly shortened by modifying this discussion. [Page 3, Section B, Paragraph 2]

5) It is hard to see “The main differences among cases are found in the outwards flow of electrons, especially in the lateral part of the plume.” Also, I am confused by this sentence: “the surface-averaged values over P of B and the effective Hall parameter, for plume sizes P1 to P4, decrease from 7.4 to 1.8 G, and from 212.2 to 68.1 G, respectively.” What is the significance of this statement? This just comes from the smaller domain? [p. 6]

6) Figure 4 seems to be a very busy figure but is least effective in addressing the scientific messages. It is perhaps more effective to just show the comparison of the 2D figures of C1, C2, and C3 with GP3 and LP3, which are shown in Figs. 6 and 7. To make the paper more concise, the authors may make a figure comparing LP3/GP3C1, C2, and C3 as one figure. Comparing LP3/GP3 vs LP4/GP4 seems less scientifically interesting. If the authors feel otherwise, then the recommendation would be to add more scientific discussions about what “physics” is captured and is important to be captured with the larger domain. The scientific messages of Fig 4 can be done without it and what the authors want to talk about in Fig 5 can be better captured.

7) Figure 5 seems interesting, but it should be clearly mentioned that all results assume the same anomalous mobility profile. The cathode location may significantly change the near-field electron transport profile.

8) The calculation of the thrust is non-trivial. Please elaborate on whether the thrust was found to be constant beyond a certain control volume and on how the control volume / surface to evaluate the thrust is chosen.

9) Table II is interesting (especially, ϕ\_∞) but more details are needed. (i) Why do we not see the convergence of results? For instance, LP2C1 = 13.22 V; LP3C2 = 14.15 V; LP4C1 = 10.97 V. (ii) The small thrust with smaller domain is attributed to magnetic force. (iii) Please provide more clarification and physics discussions. The authors mention that the results are “converging”, but the main interesting physics is what is the correct value of ϕ\_∞? For instance, if the cathode potential is 0 V, the far-field potential should be Vcc, which is a positive value with respect to the cathode potential? (v) Finally, while it is appreciated that the authors provide all values thoroughly, many of the values seem to be less interesting. For instance, all discharge power, current, efficiencies are not that sensitive to the far-field boundaries.

10) It is perhaps important to mention the difference between ϕ\_P vs ϕ\_∞. The sheath theory in Eq 4 should only be applicable for ion-attracting electron-repelling sheath. Do the authors apply Eq 4 for ion-repelling ion-attracting sheath as well?

11) Section IV mainly discusses the observations in Tables III and IV. In the text, the decrease and increase of certain values are shown, but it seems to lack a concrete scientific discussion. If there is no scientific discussion, one could consider moving this to the appendix or significantly shortening the discussions. Specifically, the increase in ϕ\_∞ in Table III (when increasing the domain size) is counterintuitive.

Minor points:

12) The authors mention “zero current condition at the plume downstream”. It would be better to briefly explain that current-free plasma at the far field plume or the assumption that the accelerated ion beam is neutralized at the far field plume.

13) The explanation of the electron energy flux should be made more carefully (cf. eq (2)). If one considers a half-Maxwellian, it can be considered that c = 2. 5/2 comes from the 3/2 p + p in the energy equation (but without any q). So, mathematically where do 9/2 and 13/2 come from?

14) More discussion is needed on the modeling details. (i) In the introduction, there is no mention about whether ions = kinetic and electrons = fluid. Also, state-of-the-art models use mainly drift diffusion approximation but it is not specified that the authors use QDD until later. The use of QDD is important in how the local vs global current-free conditions are applied. (ii) The model is described as an axisymmetric r-z simulation but the drift-diffusion model will only work in Cartesian (i.e., Coriolis/centrifugal forces will not be captured)? Please elaborate.

15) This sentence is appreciated: “Eq. (3) is an implicit equation for ϕ\_∞ which is solved iteratively [by linearizing Eq. (4)].” However, this is why the equations for the electron fluid model are recommended. It would be much clearer that ϕ is solved at the “n-th” timestep, but ion current density comes from n-1 th timestep. Then, the interior cells can use the drift-diffusion formulation to solve for ϕ at n-th time step but the boundary condition requires the evaluation of  j\_neP at the n-th timestep, which may (or may not) need the implicitness for ϕ. The model for ϕ\_∞ is very interesting (i.e., novel), but it is recommended to be more specific about the meaning of this, i.e., the global potential value, ϕ\_∞, is chosen such that the global current is maintained to be zero.

16) “[T]urbulent cross-field transport due to high-frequency electron instabilities” should more precisely be “turbulent cross-field transport due to high-frequency plasma waves driven by kinetic and fluid instabilities.”

17) Figure 5 appears to have some stray red dots.

18) What was the justification of choosing GP3 as the reference case for comparison between cathode placement results? Wouldn’t it be better to use the larger domain case? Could this be elaborated upon in the text?

19) Could the authors elaborate on the selection of locations for estimating Vcc (i.e., plume boundary at channel mid-radius vs crossing point between the channel midline and the CML vs crossing point between the channel midline and the MS).

20) There seems to be consistently a red “radial” line near the anode (e.g., Fig 2). Does this mean that there is a strip of very large current ? This isn’t the focus of the paper but seems strange. Is this a meshing artifact (see Fig 1b)? [Page 5, Figure 2]

21) Figures 5(e) and 5(f) are never discussed or references in text.

22) What is meant by “electron momentum flux is transferred to ion momentum flux”? Are the authors trying to imply collisional momentum transfer or referring to the decrease in electron contribution to the net thrust with increasing domain size? Is this just meant to be a description of the magnitude of the ion vs electron current? [Page 7, paragraph 4]

23) The net zero-current condition implies that the cathode current equals the anode current at steady state. This means that there is no leakage current. If this is the case, it would be interesting to explicitly mention that.