ANSWERS TO REVIEWER 2

We thank the reviewer for the provided feedback and useful comments, which have allowed improving the quality of the manuscript. Please find below the point-by-point replies to the raised questions and comments.

This well-written and timely article numerically studies the plasma response of magnetically shielded Hall thrusters with conducting walls, electrical configuration, and current density. It is motivated by several papers from JPL and the University of Michigan on thrusters of this type, making these numerical results quite timely in interpreting the results from the experiments.

I've attached a hand-marked copy of the manuscript that includes several minor suggestions for word choice, grammar, etc, that the author can integrate at their discretion.

We thank the reviewer for these suggestions They have been implemented in the revised version of the manuscript.

I also provide detailed comments that are numbered in the scanned copy. Except for items 2 and 5, a response/revision is needed.

1. The description of the code is great and the citation to your other work is noted. Your results would hold considerably higher credibility if you could comment on how the code outputs were previously validated. Can you state how accurate the code is to predicting global properties (thrust, current)? Can you comment on any 1:1 comparisons with internal or near-field plasma properties of the plasma (Te, ion velocity, plasma potential, etc), and, in particular, how such comparisons effect how the anomalous electron mobility profile was selected? These descriptions of validation are every bit as important as how an experimentalist might be held to task about quantifying the uncertainty of their measurements. I encourage you to spend some time here being candid about what you do, and do not know, about the uncertainty of the code outputs.

Here, we may refer to the HT5k and HT20k studies, where we compare the simulation tool to experimental values. We cannot show any published 1:1 comparison of internal or near field properties. But we can discuss the small sensitivity analysis of pera22b and mention the 1D profiles published by SITAEL. We can also discuss the sensitivity analysis that the author suggests for this article, if we end up doing it.

2. Your observation that the B field topology of the thruster you study and the H9 are different, especially with the magnetic null, is an opportunity to comment on whether one configuration may have advantages over the other. Do we want more or less current to be collected the walls? Since the ion energies are all low, does it matter? Would it be better to collect more current at the outer wall versus the inner? Your response here is optional.

The response to this question should be based on the one to Referee 1, in the part concerning the differences between the magnetic null configuration and the one without it. With respect to the discussion of the distribution of currents, I think this should be a separate question, since that is not primarily determined by the existence of a null point. For the first question: we could talk about the path to the radiative surface (already mentioned in the article). For the second question: we can point towards the recent work of Hofer, which looks for innovative ways of heat dissipation when operating at high power densities (also already present in the article). To the last question, we can address the discussion talking again about the optimal paths to the radiative surfaces. And in second place, we can also note that such a redistribution could be done with minimal changes in the potential thanks to the presence of the magnetic null point.

3. MS-HETs and conducting walls did not make high current density operation possible. See, for example, operation of the unshielded H6 Hall thruster up to currents of 40 A previously in the work by Jameson (Jameson, K. K., "Investigation of Hollow Cathode Effects on Total Thruster Efficiency in a 6 kW Hall Thruster," Ph.D. Dissertation, Aerospace Engineering, University of California, Los Angeles, Los Angeles, 2008.) and especially Reid (Reid, B. M., "The Influence of Neutral Flow Rate in the Operation of Hall Thrusters," Ph.D. Dissertation, Aerospace Engineering, University of Michigan, Ann Arbor, MI, 2009.). My issue here is the phrase, "previously unattainable." There are examples in the Russian literature of devices operating at much much higher current densities than H9 Muscle. See for example, Grishin SD, Erofeev VS, Zharinov AV, Naumkin VP, Safronov IN (1978) Characteristics of a two-stage ion accelerator with an anode layer. J Appl Math Tech Phys 2:28-36 Zubkov IP, Kislov AY, Morozov AI (1971) Experimental study of a two-lens accelerator. Soviet Phys - Tech Phys 15(11):1796-1800 Morozov AI (2003) The conceptual development of stationary plasma thrusters. Plasma Phys Rep 29(3):235-250

I suggest you change this phrase to something more benign such as, "significantly higher than the state-of-the-art."

Thanks for the clarification and the supporting references. We have implemented the suggested modification and incorporated the references in the new version of the article.

4. I've marked "4" here on page 8 and on page 10. Keeping these parameters constant is not well supported by available experimental data, especially data we have on the current density dependence. Most importantly, see the PhD thesis of Reid (Reid, B. M., "The Influence of Neutral Flow Rate in the Operation of Hall Thrusters," Ph.D. Dissertation, Aerospace Engineering, University of Michigan, Ann Arbor, MI, 2009.). Reid studied the internal plasma properties of the unshielded H6 up to 40 A. The effect of the high neutral flow is to essentially "push" the discharge downstream, which most certainly must effect the e- transport. You can address this criticism by either 1) including a sensitivity study that looks at variations in the location of the anomalous electron transport profile, or 2) acknowledging this limitation and that it could effect the accuracy of your results. Mikellides and Lopez Ortega (JPL) has studied the effects of the profile extensively and could possibly be referenced here to help estimating the error of keeping the values constant. Mikellides, I. G. and Lopez Ortega, A., "Challenges in the Development and Verification of First-Principles Models in Hall-Effect Thruster Simulations That Are Based on Anomalous Resistivity and Generalized Ohm's Law," Plasma Sources Science and Technology 28, 1 (2019). https://doi.org/10.1088/1361-6595/aae63b

This question could be addressed by performing the sensitivity study suggested by the referee. If that were done, this objection would be solved. The sensitivity study could be applied to case $f = 2$ and $f = 4$, moving the transition zone downwards, checking that the efficiency trends are kept. Furthermore, we can cite the article proposed.

5. The effects of high current density are to increase the ion energy in the plume (due to the much higher double ion content) and to increase the plume divergence. These trends will 1) increase pole erosion and 2) make it harder to integrate the thruster on a spacecraft. The authors should take the opportunity to comment on these trends. Pole erosion, which is now the life limiting wear out failure mode of MS thrusters, could simply be referenced to the numerous works of Lopez Ortega. S/C integration has been largely unaddressed in the literature thus far, so commenting on how current density will challenge this activity is timely. Of particular note that change in

plume divergence angle, which will mean more the s/c must be out of the "cone of the death" and that the higher ion energy will be especially problematic for sensitive s/c surfaces (including adjacent thrusters!). Your response here is optional.

This question can be replied by: firstly confirming the trends indicated by the author, which align well with the ones observed in our simulations. Then, we can add the comments about spacecraft integration and pole erosion, in the manuscript, adding the reference.