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Comment on **angeo-2021-10**

Anonymous Referee #2

Referee comment on "Io's auroral emissions via global hybrid plasma simulations" by Štěpán Štverák et al., Ann. Geophys. Discuss.,
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The paper "Io's auroral emissions via global hybrid plasma simulations" by Štverák et al. used hybrid simulations to investigate the aurora morphology of Jupiter moon Io. Unfortunately, there are numerous shortcomings and misconceptions as well as a lack of quantitative results in my view. Furthermore, I doubt the use of the approach of hybrid simulations (with an isothermal electron fluid and treating ions as particles) to study the issue of Io's aurora. The aurora is excited by collisions between electrons and neutrals in atmospheric regions and hardly/not affected by ion kinetic effects.

I will discuss some of the shortcoming below but these are not all. (I do not intend to be overly critical and try to keep it to the facts.)

The treatment of the electrons with two populations of old (upstream torus) and new (from ionization of atmosphere) electrons is oversimplifying the complex issue of the exchange of energy between the electrons. The study distinguishes between two cases where these two populations exchange energy instantaneously or not at all. In reality, the electron heat conductivity along the magnetic field in the plasma (like the electric conductivity) is almost infinity except where the neutral density becomes comparable (Banks, P.M., Kockarts, G., 1973. *Aeronomy*, volume A. Academic Press, San Diego CA). This means that the electron energy distribution around Io will be complex and one also needs to consider that electrons far away can exchange energy with the close electrons as long as they are on one magnetic field line. The simplified treatment leads to wrong distribution of the electron energy around Io and thus wrong aurora morphology.

It is unclear how the scaling of the simulated system in the hybrid model is set up. In their previous studies (Sebek et al. 2016, 2019), Io was downscaled by a factor of 10 in order to make the hybrid approach possible. How is it treated here and how does it work to

compare aurora images on real scales?

(And what did you assume for the atmospheric scale height (L119)?)

There are significant general mismatches between the simulated aurora presented here and the observations. The authors yet cherry-pick specific features and make hand-waiving arguments (with strange confusion of longitude and latitude!) about similarities and even conclude about presence of induction in an ocean.

A few examples for mismatches:

The simulated results maximize in the region upstream of Io. (Which the authors also state, e.g., in line 2017: "that most of the emissions are produced in the upstream hemisphere"). In the observations the emissions on the upstream central hemisphere are, however, very faint. There are a few FUV observations with a useful geometry for illustrate this. For example, in Figure 3 of Roth et al. 2014, bottom panel first row, the OI13565 A image marked with "38" in the lower left corner. This view shows the sub-jovian hemisphere and upstream is to the right (similar but vertically flipped view as VIEW-B in this paper). Here it is obvious that there are hardly any detectable emissions upstream of the moon. In the simulation here, in contrast, (VIEW-B in Figure 2 here) the upstream emissions is much much brighter than everything else. (Only the beam-simulated wake feature in the simulation reaches somewhat similar brightness). The shape of the flank emissions on the anti-Jovian side is very extended in the simulation, but is just as confined as on the sub-jovian flank in the observations. The authors mention this, but they do not discuss this. The overall mismatch definitely prevents conclusions on details like the role of induction in the simulation.

The paper completely ignores our work published in Roth et al. 2014 on the aurora observations. The Roth et al. 2014 study used all existing FUV HST observations of the aurora for the first time and presented simple mathematical equations that describe the aurora distribution in all these images with one model.

The Roth et al. 2014 study thus exactly provides high-level data in a format to be used in studies numerical simulations of Io's interaction like the one presented!

One issue related to the negligence of the Roth et al. (2014) results is the assumption that Io's FUV aurora contains a feature in the wake, denoted here and before "wake emission". In the Roth et al. 2014 study, it is explained why none of the emissions in the data can unambiguously assigned to be located in the wake and that, in contrary, features that appear to be in the wake in the 2d images originate from the flanks of Io.

The assumption of aurora by an electron beam is obsolete. There is no FUV wake feature.

The literature review in the introduction and entire study is incomplete and outdated, missing some key findings and papers. This becomes already evident by the years of the cited publication, all of which date more than 6 years back although there were relevant papers in more recent years.

One example in L36: The discussion of the volcanic vs sublimation supply totally ignores the latest literature and findings: Tsang et al. 2016 J. Geophys. Res. Planets 121, and de Kleer et al. 2019 Icarus 317, to name only two important new results.

Also, there is no reference at all to results of the Juno mission which is now almost half a decade at Jupiter. For example, they authors cite Bonfond et al. 2008 on the Io footprint morphology but there have been high-resolution images by Juno which revealed numerous new features in the footprint (Mura et al. 2020). In addition, there are several follow-up papers by Bonfond in Io's footprint morphology, which should be mentioned.

The study of Roth et al. 2017 is described in a misleading and actually wrong way, both regarding the results and the method.

It is a data analysis study where new observations of the aurora are presented and then analyzed with various methods considering different interior effects. One of the methods include MHD simulations to generally investigate the behaviors of tangential magnetic field lines. This does not make it an "MHD study", as claimed in several places.

The result is indeed that a magma ocean is inconsistent with the data, but also that induction in Io's core is well consistent. So the sentence "They showed that magnetic field without contribution of induced dipole field corresponds better to assumed locations of auroral equatorial spots." is just wrong.

Most importantly, the Roth et al. 2017 study uses the time-variability of the aurora as one of the two main diagnostics for magnetic induction. The lack of a *temporal phase shift* is the main argument against the presence of induction near the surface. This is completely ignored here.

Some line-by-line comments:

L173: "we note that local rate of any typical emission observed from Io ... would have similar structure and would scale according to magnitude of efficiency of given excitation reaction."

That is not correct. Some of the emissions related to allowed electronic transitions in the atoms and can be optically thick and proper radiative transfer modeling would be needed. Therefore, many studies use the forbidden transitions, e.g., OI1356 Å.

L262: "...are to certain extent in fair agreement with the real observations "

I disagree and think it requires any kind of quantitative comparison (not only visual, although I think even the visual comparison shows mostly obvious disagreements) to claim any agreement.

L284-L286: The spot latitudes derived in Retherford et al. 2000 are off for some images due to observational limitations and unfortunate conditions/geometries. The Roth et al. 2017 study used targeted new observations in 2013 and 2014 intentionally set up to optimize the geometries in order to properly address the issue of the spot locations. It is really a sneaky (and wrong) argument to use these old results (from basically same authors actually as in Roth et al. 2017) to spread doubts about the Roth et al. 2017 results.

And, btw, Retherford et al. 2000 is here cited with "e.g.". What are the other studies *quantifying* scattered spots?

L297: "are here located at rather low latitudes while inclusion of the induced dipole moves the spots to $\phi \approx -30^\circ$!"

Here the authors confuse latitude and longitude. The angle phi derived in Retherford et al. 2000 is a LONGITUDE.

