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Interactive comment

Interactive comment on "Mercury's Sodium Exosphere: An *ab initio* Calculation to Interpret MASCS/UVVS Observations from MESSENGER" *by* Diana Gamborino et al.

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Reply to Anna Milillo's review: Mercury's subsolar Sodium Exosphere: An ab initio Calculation to Interpret MASCS/UVS Observations from MESSENGER

Gamborino, Vorburger and Wurz Ann. Geophys., 2018

Dear Editor, We thank Dr. Anna Milillo for her comprehensive review of our manuscript. Her comments and advice helped us to improve it. We have addressed all the points mentioned and revised the manuscript accordingly. In the text below you will find the reply to each comment. Your sincerely, Diana Gamborino.

General Comments: This paper reports the results of the MC simulation of Na exosphere at a specific position along the Mercury's orbit and compare it to the MESSENGER /MASCS observations. The authors conclude that close to the surface the Na atoms released by thermal desorption are the main constituents, whereas the main mechanisms able to transport Na at higher altitudes is the micro-meteorite impact vaporization. The results are interesting and original, and also the summary figures at the end are a nice schematization. Nevertheless there are some lacks in the explanations and in the description. The model is specifically computed at TAA 160 âŮę , that is, quite close to apohelion (low radiation pressure), and it is limited to equatorial region for comparing it to the MESSENGER observations. This is not clear in the title, in the abstract and in the first part of the paper, while it is an important point since different release mechanisms can act at different surface regions (local time, and latitudes). For instance, the title should be "Mercury's subsolar Sodium exosphere:. . ." Generally the paper does not consider adequately the recent relevant literature on the subject, especially in the introduction. I invite the author to update the introduction with more recent and relevant papers. Detailed comments are reported here below. Reply: We have changed the title.

Specific comments: page 1 line18: Oxygen is not the main issue here, anyway, if the authors want to mention it, the Mariner 10 detection was an upper limit. Reply: We agree and we have removed this sentence.

page 1 line 21: here the references are not adequate. There are many important observations from different telescopes, especially here the observations from the THEMIS solar telescope and from the McMath-Pierce telescope cannot be neglected. Reply: We have properly added these references to the same line.

page 1 line 23: if "these" refers to MESSENGER, it is not true. If the ground based observations are the observations showing high latitude enhance the references again are lacking of relevant literature. Reply: We have corrected this in the same line.

page 2 line 10: before Leblanc and Johnson, 2010, Sarantos et al 2009 for the Moon and Mura et al. 2009 for Mercury suggested that the release processes influence to each other. Reply: We have properly added these references to the same line.

page 3 line 9-11: this is a repetition Reply: Indeed this sentence is repeated. We have removed it.

page 3 line 15: Also here the references are not adequate: the Na short scale time variability has been analyzed by Massetti et al. 2017, this reference must be included here. Reply: We have properly added these references to the same line.

page 3 line 20: this last sentence should be moved with some more discussion in the conclusions section. Reply: We have moved this sentence to the conclusions (previous to last paragraph), and elaborated on it.

page 3 line 24: “amid”, there is a typo. Reply: We decided to remove the whole paragraph because is rather unnecessary.

page 4 line 3: “fr”, again here there are typos, Reply: We have removed the typo.

section 3.1: it is not clear if the authors consider average conditions or TAA variability or surface position. Some clarification on possible dependence by these factors that could affect the conclusions should be given. Reply: We use the parameters from the given observation conditions, which are: TAA=158°, subsolar point (latitude=0°, noon). This parameters can be found on Table 1. A discussion regarding different observations conditions can be found in the last part of the Results and Discussion, as well as in the Conclusions.

page 5 line 7-9: this sentence is not clear. It needs further explanations Reply: Indeed, it was not clear. We have re-written this sentence.

page 5 line 9-10: please quantify the contribution of neutral component. The heavy ions components are also relevant especially during CME (Kallio et al. 2008) since the yield is much higher than for protons or neutral hydrogen Reply: We have added the neutral component upper limit reported by Collier et al. (2003). For regular SW conditions, as prevail during the observations, only protons and alpha particles are important.

page 6 line 25: I would not write that ion fluxes onto the surface and yields are low. Is it low with respect to what? Reply: We made this sentence clearer in the same line.

page 6 line 30: I would write that the MIV contribution is estimated as comparable to SP (in fact it is not known) Reply: We agree that the MIV and SP contributions to the exosphere are comparable when solar wind SP is active. We have made this sentence clearer in the same line.

page 7 line 15: the figures should be numbered in sequence. Reply: We have changed the order of the figures. We renamed Figure 2 to Figure 3 and vice versa. Figure 3 is now at the end of the section 4 with its reference text in last paragraph of this section.

section 4: The first part is a repetition, while it should be stated clearly in the text that the model is applied to a specific TAA and SZA, as is listed in the Table 1. So the result applies to this specific situation. Reply: We have corrected, specified the specific observation conditions, and avoided repetition.

page 8 line 9: the figures should be numbered in sequence. Reply: We have corrected this.

Figure 4 and 5 : I suggest to put these two figures together as left and right since are essentially the same, and a comparison would be easier. Reply: We have put these figures together.

page 13 lines 8 and 9: some typos Reply: We have corrected the typos.

page 13 eq 6 and 7: Here I am confused, I think that not all the available “free” Na is thermally desorbed. It depends by the temperature. So a probability weight should be applied to the ambient source. I would consider that the Na in the exosphere for TD release is source for TD=(TD+PSD+MIV+SP + Diff)* Prob=TD+ambient*chi where TD, PSD, MIV and SP are the return fluxes In fact the free Na is available also for other release processes. This complicates the discussion. Eq 6 should be true if Prob = 1 and return flux for PSD =0. These assumptions are tacit, while they are explained later. I think that the treatment should be done clearer, also because it is not valid everywhere. Reply: We agree with you that the free Na is available for other release processes and that it depends on the surface temperature, as you say. We find that for the temperature we use and observation geometry, TD is more than 10 orders of magnitude more efficient in releasing particles compared to PSD. This is explained in detail in section 5.2 where we invoke the vapor pressure calculation into the argument. We find that, even if TD would reduce its release to a factor of 100 over PSD, TD would still dominate and justify that PSD is not effective in this particular situation. This is why we ignore PSD in the returning flux. Having neglected PSD, the weighted probability factor is not included because we assume that all the Na that returns to the subsolar point will be thermally desorbed rapidly. We agree that this is not valid everywhere and not necessarily the case for other observation conditions, where PSD or other surface release mechanisms might be active and compete with each other. We have improved the text in the Conclusion section, where we dedicate a new paragraph (previous to the last) to make clear that our results apply only to the observation conditions of the data we analyzed.

page 14 line 3: delete “radial column density” here. Reply: correction made.

page 14 line 4: add “Where” before vth and move the sentence before “The radial column density...” Reply: The v_th has to be introduced after the definition of the surface density. We have added the “where” that was missing.

page 14 line 12: this sentence is not clear. Reply: We have changed the sentence and made it clearer.

page 14 line 22-23: not clear , please explain better what is the suggested mechanism. Reply: We have changed the sentence and made it clearer.

Figure 7 caption: correct "Taken from the results of our model" Reply: This has been corrected.

Please also note the supplement to this comment:
https://www.ann-geophys-discuss.net/angeo-2018-109/angeo-2018-109-AC2-supplement.pdf

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