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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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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°, that is,

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Discussion paper

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.

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.

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.

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.

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.

page 3 line 9-11: this is a repetition

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.

page 3 line 20: this last sentence should be moved with some more discussion in the conclusions section

page 3 line 24: “amid”, there is a typo.

page 4 line 3: “fr”, again here there are typos,

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.

page 5 line 7-9: this sentence is not clear. It needs further explanations

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

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?

page 6 line 30: I would write that the MIV contribution is estimated as comparable to SP (in fact it is not known)

page 7 line 15: the figures should be numbered in sequence.

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.

page 8 line 9: the figures should be numbered in sequence.

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.

page 13 lines 8 and 9: some 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.

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

page 14 line 4: add “Where” before vth and move the sentence before “The radial column density...”

page 14 line 12: this sentence is not clear.

page 14 line 22-23: not clear , please explain better what is the suggested mechanism.

Figure 7 caption: correct “Taken from the results of our model”

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