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Atmospheric Measurement Techniques Discussions

Interactive comment on "Photocurrent modelling and experimental confirmation for Meteor Smoke Particle Detectors onboard atmospheric sounding rockets" *by* Gabriel Giono et al.

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All changes implemented are marked in red in the revised article, which is attached as a supplement to this response.

Overall comments Comment #1: The referee pointed out the lack of quantitative conclusion on the photocurrent estimation. Response : The method was refined by allowing the model to adjust the photoelectric yield and the altitude of the molecular absorption and a much better fit was obtained on the measurements from Robertson et al. (2014). The residual showed a sinusoidal pattern versus height, with +/- 300 pA amplitude. This was set as the limit for which the method can be applied. Similar results are seen

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in the flight data from April 2018 (to be published separately).

Comment #2: The referee mentioned that the photoelectric yield is greatly dependent on the material preparation (surface, oxidation, alloy, contamination, etc..) and ask if these conditions were known for both the aluminium plate from Robertson et al. (2014) and the materials reported by Feuerbacher and Fitton (1972). Response: We fully agree with the comment, and agreed that such information was missing in our article. More information on how the material was prepared by Feuerbacher and Fitton (1972) was included, although some information is still missing (type of alloy, surface roughness). As for Robertson et al. (2014), no information could be found in their article. Overall, we tried to emphasis the importance of these properties and stress the lack of detailed knowledge about them.

Comment #3: The referee asked for some guidelines based on our results for building future instruments. Response: We added a part in the conclusion section suggesting pathways for instrument design and testing based on our results.

Minor comments Abstract, line 18: As opposed to the abbreviation MSPD, the abbreviation MSP has not been defined. → Added

Page 2, line 2: add "to" after "referred" → Added

Page 2, line 4: "subject to" instead of "subject of" → Corrected

Page3, line 4, and several other places in the manuscript: The authors refer to upcoming sounding rocket campaign PMWE during Spring 2018. By now, this campaign has taken place. I therefore recommend to update these text passages in the manuscript. (However, I would not ask the author to refer to any results from that campaign.) → More information added, stating the two flight from April 2018 and the two upcoming flights scheduled for 2019.

Page5, line 8: The authors state that molecular oxygen densities can vary by a factor 2 in the atmosphere. They should specify what altitudes they refer to. → This was a

mistake (at least below 150 km, which is our region of interest). The molecular oxygen density between 60 and 100 km varies by +/- 20% between the low and high solar and geomagnetic activities cases. This was corrected for in the text, and illustrated by a new figure as well.

Page 5, line 26: It is unclear what is meant by "the height of the corresponding atmospheric column...". The sentence becomes more correct by simply removing the words "the height". → Corrected

Page 6: The authors argue that atmospheric (molecular Rayleigh) scattering is not important for their study. This is correct. However, simply referring to the absorption and scattering cross section in figure 3 is not sufficient when making this argument. In principle, large amounts of (upwelling) scattered radiation from lower atmospheric altitudes (where the product of scattering cross section and number density is large) could contribute to producing the photocurrent. However, absorption by O2 (< 200nm) and O3 (> 200 nm) at lower altitudes prevents these contributions from becoming important. → We agree with this comment, and we thought our explanation in the text was clear enough to reflect this idea. We added more explanation by writting “absorption by O2 should dominate below 120 km, thus making the contribution from photons scattered below this altitude negligible”.

page 8, line 8: It is confusing that the authors talk about transmission when it comes to solid surfaces. I recommend to replace transmission by absorption. Hence Absorption = 1 - Reflectance, rather than Transmission = 1 - Reflectance. → Changed accordingly Discussion paper page 9, line 1: Define "quartic equation". → Added

page 9, Section 2.4: Is reflection of radiation from the grid wires included in the simulation? This should be explicitly stated. In that case, how important is reflection from the grid wires? If it is not important, this would be an important message to other instrument modelers: in that case only the shadowing effect of the grids would be important to consider in the radiative transfer. → We clarified this. The reflected rays are not

considered, mainly because they are unlikely in this design of the detector. Only rays hitting the inner side of the grids could be reflected towards the electrode, but the inner side of the grids is partially shadowed by the grids themselves.

page 13: In the absence of the contamination issue, the spectral flux of photons to the MSPD would be well defined based on the absolutely calibrated light source and the known transmission curve of the filter. There would be no need for the photodiode. Hence, the reason for using the photodiode is the contamination issue. Do I understand that correctly? This should be clarified in the text. → More explanation added. The photodiode was mainly here to monitor change of the light source intensity over time (e.g. due to temperature). It is true that if the light-source is perfectly calibration there is no need for a photodiode. However, it is always useful to monitor the flux, and was even more due to the contamination problem.

page 13, line 21: I would not call the wavelength responses as "empirical", rather "assumed" or "estimated". → Changed

figure 12: Define "PEEK" in the caption. → Added

page 16, line 9: the authors refer to "two orders of magnitude". Figure 13a only shows a factor 3. Also: The relative decline in photocurrent and photodiode signal is very different in Figure 13 and Figure 14. This does not seem to be consistent. More explanations are necessary. → This was a mistake. Factor of three is correct. The difference in decrease rate between the electrode photocurrent and the photodiode current was also clarified. Basically, the photocurrent is more sensitive to shorter wavelengths (due to the shape of the yield), whereas the photodiode current is more sensitive to longer wavelengths (due to its sensitivity response). The contamination on the filter was most likely affecting shortest wavelengths more, hence reducing the photocurrent more compared to the photodiode current.

Table 2: The notation "estimated electrode photocurrent" is confusing. Please clarify. → Change to Calculated photocurrent based on the model.

page 19, line 10: The authors ask for "modern and reliable" measurements of material properties. I do not like the implicit connection of these two words. 40 year old measurements are certainly not unreliable just because they are old. (And modern measurements made in 2017 or 2018 are not necessarily reliable.) Please change the wording. → We agree that this statement was not properly formulated. The “modern and reliable” was more referring to “open-access” or database compared to published curve without error bars. We changed it to “open-access accurate measurements”.

Please also note the supplement to this comment:
https://www.atmos-meas-tech-discuss.net/amt-2018-87/amt-2018-87-AC2-supplement.pdf

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