Response to Anonymous Referee #1

We thank the reviewer for the constructive comments and will modify the manuscript accordingly. Our response to the review is given below inserted after each comment. The reviewer's comments are in *cursive* and our response in regular letters.

The paper aims at presenting a novel rocket-borne instrument for sampling of Meteoric Smoke Particles (MSP) in mesosphere and justifying its aerodynamic properties. The authors present some results of aerodynamic simulations for neutral gas surrounding the instrument ying at supersonic velocities and for MSP-ow through this environment into the instrument. The paper is well structured and the results are clearly present. Nevertheless, there are some points which can be addressed in the frame of this study. Therefore, I recommend minor revision. Herewith I suggest some possible improvements to be considered for the revised version of the manuscript. Since Copernicus publisher will anyway make language corrections, I will ignore typos and some weird formulations.

1. Results of DSMC simulations of the environment (Fig. 1 or 3 & 4) may additionally include ow streamlines to make it more clear for the reader to understand the deflection of MSPs by the surrounding gas flow.

- The figures will be changed accordingly including streamlines.

2. The results demonstrated by Fig. 3 & 4 will be easier to understand if colorbars will be on the same scale (one color scale for all these plots).

- The figures will be changed accordingly.

3. Taking neutral temperature and density from MSISE-90 model as input for simulation is acceptable, however natural variability of these parameters must be taken into account. Thus, for instance, rocket-borne measurements at high northern latitudes [e.g., Lübken et al., 1999; Strelnikov et al., 2013] as well as e.g., lidar measurements in Antarctica [Lübken et al., 2015] show temperature variability of ~40-50K at altitudes of interest for the manuscript. Also the neutral density variability as can be seen in Fig. 9 of Strelnikov et al. [2013], makes $4\bullet10^{20}$ to $7\bullet10^{20}$ m^{-3} and approximately $1.5\bullet10^{20}$ to $3\bullet10^{20}$ m^{-3} at 80 and 85km altitude, respectively. This variability will contribute to uncertainties of the derived results (which is not addressed in the manuscript).

- Since the purpose of our investigation is to understand the conditions for dust collections in general, we are interested in finding a possible range of results, rather than the result for a certain atmospheric condition. We will point this out in the paper, and we will also discuss the atmospheric variability that – as the reviewer points out - will determine the uncertainty of our prediction.

4. Simulation of MSP flow (Sec. 3.2) must be described in more details. For instance, that you (probably) start particle tracking (solving Eq.1) outside the shock front and stop if some *conditions are met. This should further clarify, e.g., what happens to MSPs which do not hit the collecting surface (which I have not understood after reading the entire manuscript). You could also specify the grid used for these calculations, etc.*

- We will describe the calculations in more detail including the stopping conditions. We will also elaborate on what happens to the particles which do not hit the collecting surface.

5. Sec. 3.4 (Mass Estimate) contains two parameters: filling factor α and the collection efficiency σ, which must be explained.

- Explanations will be added in text

6. MSP trajectories in Fig. 5 & 6 are difficult to see (may be use of different colors can improve).

- Figure changes have been made

7. The manuscript makes an impression that only a single run of MSP flow simulation was made. This makes the results not quite reliable. Since these simulations have a probabilistic character, a certain number of trajectories must be calculated to gather an appropriate statistics. Thus, e.g., for assessment of MSP collection efficiency Asmus et al. [2017] simulated trajectories for 4000 particles.

- In contrast to the work by Asmus et al., we do not aim to derive a number density or flux from observed event rates, but we aim to obtain a reasonable estimate of the dust mass and dust sizes of particles that would be collected with MESS, hence 1% accuracy given in the above-mentioned paper is not the goal here. The largest uncertainties of the estimate that we make lies in the uncertainty of predicting the atmospheric parameters for the flight. We now point this out in the text.
- In addition, our main results refer to large primary particles for which other studies showed that they are not affected by Brownian motion and therefore we do not deem it necessary to use more trajectories for the primary particle estimates. The descriptions of the smaller particles shown are meant to serve more as an illustration of the flight and instrument condition. Phrasing in the text will be changed to point this out and we show more results on the smaller fragment trajectories.

8. The same is also true for the fragmentation study shown in Fig. 8; i.e. statistics and uncertainties are not shown.

- The uncertainties of the estimate that we make lie uncertainty of predicting the atmospheric parameters for the flight. We now point this out in the text.

9. The sentence in P.12 L.180: "The particles that are stopped will likely remain in the instrument, and could reach the collection area." Need some explanation. Why and how it happens? Why not blown away during payload precession?

- We expect that some particles could leave the detector, we did not see any cases in our calculation and given the conditions, we expect it is more likely that they remain in the instrument. We will mention this in the text.

10. In Sec. 4.3 (Estimate of collected mass) authors refer to a model study by Kiliani et al. [2015] in context of justification for their choice of MSP parameters, which is not appropriate. Please, refer to original measurements.

- Since we want to estimate conditions for the entry into the instrument, it does not seem plausible to us to use in such an estimate results from in-situ measurements that could be biased by similar effects. We will however consider MSP parameters obtained from model calculations in addition to estimates from NLC observations.

11. Also, in many places of the manuscript references are missing. E.g., P.7 L.104 (existence of NLC/PMSE conditions), P.9 L.135-136 (for MSP densities), P.11 L.171 (typical MSP size), and similar statements where a certain value (so-called typical) is assigned to some parameter.

- We will check and correct the references.

12. P.13 L.189 "traversed volume is..." units are missing.

- We will correct this.

13. p.13 L.193 I do not understand the statement "The secondary particle, or annular sampling area is A_f" and A_f is not defined.

- We will correct this.

14. Also in this discussion (Sec. 4.3) uncertainties are not addressed.

- We will discuss the uncertainties as outlined above.

15. "Heating of the particles" (e.g. P.13, L.191) is often mentioned in the manuscript, but never explained: why and how much (how fast) to expect.

- We will include calculations of the dust temperatures for typical cases and discuss the implications for the collection experiment.

16. P.13 L.209 must be Fig. 10

- We will revise this.

17. Discussion may address many uncertainties. E.g., angle of attack, which is somehow mentioned in the manuscript, but not quantified. Such values would help to define flight parameters needed for a judgment whether the instrument is suitable for a particular mission. For example, what is the critical angle of attack, what are velocity limits (rocket apogee) for presumably satisfactory MSP collection in the given altitude region. How the results are *sensitive to sizes of ice particles? Will any PMSE be enough for a successful MSP sampling or bigger particles (NLC) are needed.*

- We will expand the discussion part as suggested.

18. The conclusion inferred from simulations and often mentioned in the manuscript, that MSP collection is more efficient at 85km compared to 80km is already long time a well known result [e.g., Horanyi et al., 1999; Hedin et al., 2007; Strelnikova et al., 2009; Asmus et al., 2017].

- We agree this was found before for other instruments and now clarify that in the discussion.

19. Abbreviations MAGIC (instrument) and TEM (grids) in the beginning of the manuscript are not described.

- We will check and explain abbreviations used.

References:

Asmus, H., T. Staszak, B. Strelnikov, F.-J. Lubken, M. Friedrich, and M. Rapp, Estimate of size distribution of charged MSPs measured in situ in winter during the WADIS-2 sounding rocket campaign, *Ann. Geophys*., pp. 979-998, doi:10.5194/angeo-35-979-2017, 2017.

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Horanyi, M., J. Gumbel, G. Witt, and S. Robertson, Simulation of rocketborne particle measurements in the mesosphere, *Geophysical Research Letters - GEOPHYS RES LETT*, 26, 1537-1540, doi:10.1029/1999GL900298, 1999.

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Lübken, F.-J., J. H• o_ner, T. P. Viehl, E. Becker, R. Latteck, B. Kaier, D. Murphy, and R. J. Morris, Winter/summer transition in the Antarctic polar mesopause region, *J. Geophys. Res*., 120, 12, 394-12,409, doi: 10.1002/2015JD023928, 2015.

Strelnikov, B., M. Rapp, and F.-J. L• ubken, In-situ density measurements in the mesosphere/lower thermosphere region with the TOTAL and CONE instruments, in *An Introduction to Space Instrumentation*, pp. 1-11, TERRAPUB, doi:10.5047/aisi.001, 2013.

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