Review of "A new method to infer the size, number density, and charge of mesospheric dust from its in situ collection by the DUSTY probe" by Havnes et al.

This paper presents an extended analysis of DUSTY rocket measurements, that provides estimates of the ice particle radii and concentrations in NLCs. The manuscript presents new and appropriate results, is properly organized, and the choice of figures is sufficient. The paper is sometimes difficult to read and could use some attention to sentence structure. Most importantly there are a number of concerns described below that must be addressed before the paper can be accepted for publication.

General comments:

1. The DUSTY results are compared to nearby lidar measurements, and simultaneous photometer results. The DUSTY ice particle radii are systematically up to 2 times larger than indicated by the lidar, while the DUSTY ice particle concentrations are more than 10 times lower. These differences are not properly addressed in the paper, in part because there is no error analysis presented for the DUSTY results, and in part because the ice radii derived in the DUSTY method is not rigorously defined. The expressions appear to assume a mono-dispersed population, but the end quantity will inherently contain weighting by the particle, instrument, and analysis methods. Properly describing these effects will allow a greater understanding of the DUSTY results and will help understand the lidar comparisons.

2. The word "dust" is used in the paper to refer to a variety of aerosols including ice particles, meteoroids, and meteoric smoke, and this leads to unnecessary confusion. The term dust is typically used to indicate meteoroids (e.g., "cosmic dust"), or sometimes the meteoric smoke particles which result from meteoroid ablation products. NLC particles consist of water ice (crystalline H_2O), and should be referred to as "ice particles" or perhaps "NLC particles". It is therefore confusing that the authors refer to NLC particles as "dust" (which would indicate cosmic dust or meteor smoke by many or most people), and I strongly recommend that they correct this nomenclature throughout the manuscript.

Specific Comments:

1. Line 39: There are more up-to-date references than this.
2. Line 48: Updated references on meteor influx should include Carrillo-Sánchez et al. [2016], which is the state of the art result here.
3. Line 51: There are more up-to-date references concerning meteor smoke.
4. Line 84: "..mass analyze the…" is this the intended phrase?
5. Line 88: "…parameters such as…"
6. Line 91: please define the acronym G2
7. Figure 2: It would be customary to use height as the Y axis.
8. Line 169: Please give the height values (km) that define the upper and lower limits of the noctilucent cloud.
9. Line 173: by "cloud system" do mean "noctilucent cloud" ?
10. line 177: "dust" should be "ice particle"

11. Line 179: I do not understand why you are suddenly discussing iron particles of 100 nm radius. This seems irrelevant when the probe is encountering ice particles of much smaller radii.
12. Line 188: It would help to also define the angle in terms of the velocity vector and the cylindrical wire. Is it correct that $\theta = 90$ is the side of the wire and $\theta = 0$ it the top of the wire?
13. Line 206: 3% would be an upper limit, the range being 0.01 - 3 %.
14. Line 227: I think you are referring to the ice particle radius, not the dust particle.
15. Line 229: It is important to note here that this expression will yield the **average** ice particle size. As with most observations, the average will be weighted by some characteristic of the observed particles (volume, area, …) in conjunction with instrumental effects. In this case I think it might be the ice particle surface area, but I am not certain. Please discuss this property of the observations / interpretation, as it will pertain to how the results can be compared to other observations of ice particle size. It is also apparent that your expressions assume a mono-disperse (all one size) ice particle population, but this is not stated anywhere. As this is an unrealistic assumption, it would be relevant for you to adopt a model that includes the size distribution of ice particles. A study by Baumgarten et al. [2010] used lidar observations to show that NLC particles can be described by a Gaussian distribution with a functional dependence between the median radius and distribution width. This would be a good starting point for you to consider.
16. Line 230: The term dust density is ambiguous, When referring to particle density, one typically thinks of the particle mass per unit volume (e.g. ice density is 0.93 g cm^{-3}). I believe that with N_D you are referring to the concentration of ice particles, which would have units of # cm^{-3}.
17. Line 247: Again, the term "dust" is confusing. Do you mean the NLC ice particle, or perhaps the MSP particle that is embedded in the ice particle?
18. Lines 247-256: It seems like the approach could have multiple solutions (combinations of r_D and N_D) that explain one observation. Can you comment on this?
19. Line 254: By "dust density" I assume you mean "ice concentration". Please use either concentration or number density when you refer to the number of aerosols per unit volume of air.
20. Line 265: "cloud system" could be "NLC layer"
21. Line 275: By "dust cloud structure" do you mean that there was a layer of meteoric smoke particles detected above the main NLC layer, or perhaps a layer of small ice particles? This is an example of the confusion introduced by the inconsistent nomenclature.
22. Line 271: "panel 2" probably refers to Figure 3b, please clarify.
23. Figure 3: In panel b you use the color red for two different curves, please select a third color.
24. Lines 276 - 281: This discussion is hard to follow. It would help do properly define the panels in Figure 3 (as a, b, and c), and then refer to these when appropriate in the discussion. It is unclear what is meant by "weak structure", perhaps describing the phenomena according to the observation, like "enhanced signal", or "reduced oscillation", would help.
25. Line 282: by "dust" you must mean "ice particle", given the radii in Figure 4a.
26. Figure 4b: It would be customary to show the ice particle concentration as cm^{-3} (as you do in Figure 6b).

27. Line 305: Please give values for the uncertainties in r_D and N_D , for the typical results and also for the measurements that indicate radii of ~100 nm. These values could be stated in the text, or shown in Figure 6.
28. Figure 5: It would be useful to also show the lidar retrievals of ice particle radii and concentration, as time-height cross sections, since these are the quantities that you compare with the rocket results.
29. Line 338: Does the MISU photometer data interpretation include an assumed ice particle size distribution? Or perhaps they assume a mono-dispersed population as in the DUSTY analysis? Please clarify this as it pertains to the comparisons below.
30. Line 352: "…average sizes of the lidar measurements.." probably refers to the "…average ice particle sizes indicated by the lidar…"
31. Line 360: It seems like the calculated backscatter is nearly identical for the two refractive index assumptions, and you should comment on that.
32. Figure 6: The X-axis range in 6a could be reduced, and the photometer symbols could be larger.
33. Line 351: You compare DUSTY to the lidar results, which are given as the median radius of a Gaussian size distribution. In order to properly interpret these comparisons we need to understand what the DUSTY ice particle radius really means. Is it a surface area weighted mean? Or perhaps weighted to r^6 ? It is entirely possible that the DUSTY - lidar differences exist because you are comparing two different representations of the ice particle radius. Once you determine what the DUSTY ice particle radii really represent, then it will be a relatively simple matter to compute the same quantity from the lidar Gaussian size distributions. I strongly recommend that you use this approach for the comparisons.
34. Line 405: Consider comment above for Line 351.
35. Line 410: This could be somewhat addressed by looking at the complete range of radii and concentrations from the lidar, e.g., for the time period in Fig 6.
36. Line 421: The lidar - DUSTY differences in backscatter are more than a factor of 2, which indicates room for improvements / changes.
37. Line 432: Why do you consider a factor of 10 reduction here? Is this motivated by your understanding of the instrument performance? It is curious that the factor of 2 reduction in radii and factor of 3 increase in concentration bring the results closer to the lidar. Nevertheless, we still do not really understand what the DUSTY representation of particle radii means.
38. Line 438: By "dust density" I think you mean "ice particle concentration". This improper nomenclature appears in many places.
39. Line 446: By "dust" here, you are probably referring to MSPs. Please clarify.
40. Line 456: By "dust" I think you mean "ice particle" or "NLC particle".