This study captures a Saharan mineral dust transport event to the Mediterranean on 20 April 2017 through airborne observations, investigating the effects of mineral dust on ice-nucleating particles and cloud condensation nuclei. The findings make a meaningful contribution to advancing the current state of knowledge. However, the methodological descriptions lack sufficient clarity, and both the figure presentation and the writing of the results section are difficult to follow. I would recommend this manuscript for publication in Atmospheric Chemistry and Physics after the authors address the following comments accordingly.

Major comments:

1. The authors have not included continuous line numbers in this version of the manuscript, which makes it difficult to pinpoint specific locations precisely. In the comments below, I will try to indicate the relevant section and sentence.
   In the Methods section "The Cloud Indicator Algorithm", the authors should briefly describe the computational approach underlying this algorithm, or alternatively, provide a detailed description in the supplement.
   Regarding the model inputs, it should be clarified whether the "size distribution" refers to the aerosol size distribution or the cloud droplet size distribution. A brief explanation of the distinction between liquid clouds, clouds in the Mixed-Phase Temperature Regime (MPTR), and cirrus clouds should also be provided. Furthermore, I believe that the calculation equation of the cloud-aerosol volume factor (fca) should be presented.

2. In Section 2.2.3 UNIVIE size distribution retrieval, I am not sure how to distinguish between hydrated sea salt (SS) and dry SS.

3. In Section 3.2, I suggest that the authors should first introduce the significance of using the ratio between N_D>30 μm and predicted INPs before presenting the results. Furthermore, it remains unclear why two schemes are employed for INP calculation, as no discussion of the differences between the two approaches is provided in the manuscript.

4. In Section 3.3, the two CCN activation events reported by the authors are separated by only approximately 30 seconds, with each event being of very short duration. Given that these measurements were obtained during flight, it is considering whether the observed differences may reflect spatial variability in the sampled air mass rather than two distinct CCN activation events.

Specific comments

1. In Section 2.1, it is questionable whether citing a manuscript currently listed as "Weinzierl and Coauthors, in prep." is appropriate, as unpublished work in preparation may not be accessible to readers.

2. I think the term "cloud indicator" does not appear to require italicization and should be formatted in regular text throughout the manuscript.

3. In Section 2.2.3, it is suggested that the authors use numbered subsection headings (e.g., 2.2.3.1, 2.2.3.2, etc.) rather than bold and italicized text to denote sub-sections.

4. Abbreviations for certain terms, such as sulfate and organics, should be introduced at their first occurrence in the manuscript. For instance, these terms appear earlier in the beginning of Section 2.2.3, yet their abbreviations are only introduced later in the manuscript.

5. Abbreviations such as D and N should be consistently formatted in italics throughout the manuscript. While italics are applied correctly in Section 2.2.3, they are not maintained in the Results section.

6. In Figure 4, the legend for each panel should be placed within or immediately adjacent to the corresponding panel, as the current layout is difficult to follow.

7. In Figures 1 and 5, it is recommended that the colors used to represent sulfate, nitrate, and sea salt be revised to align with the conventions commonly adopted in other literatures.

8. In Figure 6, the particle number concentration appears to reach values as low as 10^{-4} cm^{-3}. The authors should clarify the detection limit of the instrument or bias of algorithm used and discuss whether measurement/calculation uncertainties may be significant at concentration in this range.

9. The ice crystal images presented in Figure 7 do not appear to be discussed in the main text, and the authors should either provide a corresponding discussion or consider removing them from the manuscript.

10. Figure 8: The same comment as Figure 4.

11. The tables in the manuscript do not appear to follow standard academic formatting

conventions, and the authors are encouraged to revise them accordingly.

12. I think language quality of the manuscript should be improved. For instance, the sentence on line 15 of page 19 appears to contain a grammatical error, and its intended meaning is unclear.