

The paper by Hakala et al. (2019) describes the occurrence of new particle formation (NPF) at a rural background site in Saudi Arabia, Hada Al Sham, using a two-year long dataset. This study is of high interest, as it reports observations from a still poorly documented region / environment, where anthropogenic emissions are likely to play a significant role in atmospheric processes. More broadly, such long timeseries are needed for a better understanding of NPF, and in turn better description of the related effects on climate in global models. Moreover, the paper is well written, very pleasant to read, and figures are clear. I would however place a caveat on this analysis, since the investigation of a specific aspect of the observed events (DMD) strongly contributes to the interest / novelty of this work, and is unfortunately not complete to leave room for a companion study. Nonetheless, I recommend the publication of this study after some minor revisions which are listed below; they concern the main text, but abstract and conclusion should be modified accordingly.

P8, L11-12, L16: “NPF event frequency has been shown to be affected by at least: solar radiation, SO<sub>2</sub> concentration...”, “which is widely regarded as the driving compound of atmospheric new particle formation”. I would suggest to slightly balance these statements which are too strong in my opinion, since SO<sub>2</sub> (and in turn H<sub>2</sub>SO<sub>4</sub>) has not been shown to be a limiting/determinant precursor in all environments, as well stressed on P2, L23-26.

P9, L16-17: The reported results strongly point toward a significant / dominant role of anthropogenic precursors, but, again, this assessment (“implies that”) is in my opinion too strong. Indeed, I think that a positive influence of marine conditions on NPF, even if minor, cannot be excluded based on the available measurements, since the Red Sea sector / coastal area is also a signature of the air mass back trajectories on event days. One may for instance hypothesize that marine conditions could affect NPF:

- 1) with some specific precursors;
- 2) but also because they might present favourable conditions for NPF to be triggered, such as for instance lower CS compared to “pure continental” air masses;
- 3) or because they most likely display increased RH compared to inland air masses, which might contribute to higher NPF frequencies / particle formation and growth rates observed in western air masses, as discussed later in the paper.

P9, L31-32: Could the authors comment more on the results they report for CS? In specific, what could be the reason for higher nocturnal values on event days? Could this observation suggest an enhanced accumulation of the precursors during the same nights, thus facilitating the occurrence of the process on the next morning? This would be consistent with the fact that the sources and sinks of the driving precursors share the same origin, as suggested on P13.

L34: “did not have a significant effect on these results”: Can the authors give an average of the CS increase observed when including APS measurements in the calculation?

P10, L2-10: I would not restrict the conclusions to the PM<sub>10</sub> related observations, and more broadly suggest the lack of precursors, not only anthropogenic, in the inland, in particular because it is clearly mentioned later (P13, L19-21) that the enhancing effect of mineral dust has been previously reported in conditions (timescale) which differ from that of the present study.

P10, L33 - P11, L1-4: The different characteristics of the DMD events are discussed throughout the paper, and I think that the reader would sometimes benefit from some clear links between the observations. For instance, the seasonal variation of the DMD frequency is reported on P8, L1-2, but is not further commented in this section. The analysis of the environmental conditions together with the timing of the events provided in the next section points toward an effect of temperature on the occurrence of DMD. This observation should afterward be used to further discuss the seasonal variation of the DMD frequency, which supports such an effect of temperature, since the maximum of the DMD frequency in summer coincides with highest temperatures.

Same type of comment also applies to the CS (P9, L31-32; P13, L24-30).

P12, L4-7: J and GR show a seasonal cycle in Hada Al Sham; in contrast, the NPF frequency does not, which is not “common”, as, for instance, Nieminen et al. (2018), report a seasonal cycle of the NPF frequency with a maximum during local spring / summer for 30 stations out of 36. This observation suggests that in Hada Al Sham, the occurrence and strength of the NPF process are somewhat disconnected, or not driven by the same “factors”.

The fact that J and GR have the same seasonal cycle is also interesting, and, again contrasts with the results recently reported by Nieminen and co-authors. Indeed, they report instead similar cycles for the NPF frequency and J, while GR usually displays slightly different variations, which are, at least to a certain extent, attributed to the involvement of different vapours in the successive stages of the NPF process.

Could the authors comment on these aspects?

P12, L28-33: Is RH on average lower on non-event days? I would expect so given the inland origin of the air masses on these specific days, but RH (and related effects on NPF occurrence) is surprisingly not discussed in Sections 3.1 and/or 3.2, despite being shown on Fig. 6.

P13, L7-10: I am not sure if the correlation between J and ABL height and, more specifically, the fact that events are observed earlier with respect to ABL development during summer time is related to the ABL height itself only. Based on Fig. 8.a, emissions from energy production are increased during summer. Assuming that these emissions directly affect the amount of vapours relevant to NPF, we may thus assume that there is a larger pool of these precursors available already before sunrise in summer, and that in turn NPF is mainly limited by photochemistry. This would be consistent with events triggered shortly after sunrise, and consequently also earlier during ABL development. During other seasons, NPF might in contrast be more vapour-limited, and thus started later, both with respect to sunrise and ABL development, when there is a sufficient amount of precursors. In addition, would it be reasonable to assume that during summertime radiation is stronger already in early morning, thus leading to more “efficient” photochemistry also contributing to earlier occurrence of the process?

P13, L24-30: These observations are very similar to those reported from several high-altitude stations, where NPF is thought to be triggered from precursors originating from lower altitude and transported at the sites together with their sink. The authors could actually draw a parallel with this situation (eg: Manninen et al. 2010; Boulon et al., 2010). The fact that the sources and sinks of the precursors share the same origin also most likely explains (at least to some extent) why the CS is on average lower on non-event days compared to event days (P3, L31-32).

Technical / minor comments:

Title: Even if it was convenient to keep the title short, I have been afterward a bit surprised that the word “shrinkage” is used in the title, as the authors clearly explain on page P3 L2-4 why they decided to “avoid” it in the paper!

P2, L21-22: “These species are likely ... or anthropogenic VOCs”: could the author reformulate this sentence for clarity?

P3, L12 and P4, L4: the dates reported for the start/end of the campaign are slightly different.

P5, L25: “principle” instead of “principal”.

P6, L7: “due to collision and coalescence”.

P12, L23: What does the “event-time” correspond to? Is it between NPF start and NPF end, or between NPF start and end of NPF event?

Fig. 1: could the authors change the colour of the red vertical line, which is not easy to distinguish from the background?