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Comment on acp-2022-440

Anonymous Referee #2

Referee comment on "High frequency of new particle formation events driven by summer monsoon in the central Tibetan Plateau, China" by Lizi Tang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-440-RC1, 2022

This manuscript investigates atmospheric new particle formation (NPF) taking place in the central Tibetan Plateau. To my knowledge, there are no prior publications of NPF in this location, so the obtained results can be considered worth publishing. The paper itself if well organized and the conducted analysis appears to be scientifically sound. There are, however, two major issues that require further consideration.

First, the measurements periods are rather short, about 4 weeks for the pre-monsoon season and less than 2 weeks for the monsoon season. As a result, it remains unclear how representative the obtained results are for this location during these two seasons.

Second, many of the conclusions made in the paper rely on SO2 and VOC concentrations. Unfortunately, there are very limited measurements on these 2 trace gases (only VOCs during the monsoon season), instead their concentrations were estimated from large-scale model simulations. The simulated concentrations may have large uncertainties, which are not quantified by any means in the paper.

These two issues, representativeness of the measurements and uncertainties related to SO2 and VOC concentrations, need to acknowledged much better when discussing the results and when making conclusions about NPF mechanisms etc.

Other important scientific comments

The description on how CCN concentrations were calculated (section 2.4) is incomplete. Apparently, the authors used equations 4 and 5 to determine the critical diameters corresponding to different supersaturations, and from these critical diameters one then gets the number of CCN using measured particle number size distributions. However, this calculation cannot be done without knowing the hygroscopicity parameter cappa. Did the authors simply assumed a fixed value for cappa or did they estimate it from some chemical data?

The discussion on the role of condensation sink (CS) in favoring/disfavoring NPF is not logical. The authors first say that their result differs from those found in earlier studies (lines 243-244), but then mention a few studies which actually agree with their findings (lines 245-247). Please reformulate this part of the text, as it causes confusion in its present form.

The discussion on the role of VOCs (lines 266-277) could be improved as well. First, considering the typical variability of VOC concentrations in ambient measurements, I would think a 20% higher VOC concentration is slightly rather than noticeably higher (line 268). It is also confusing that for the pre-monsuun season the VOC concentration difference is given as % while for the monsoon season it is given as an absolute value (ppb).

Wind direction is a very local quantity, and does not necessary tell correctly air pollutant sources or transport pathways. I wonder whether the authors have information on air mass trajectories which would provide more direct support for their statements on lines 283-295.

The enhancements of CCN concentrations due to NPF is reported in 3 different ways in section 3.4: 1) using an enhancement factor EF, 2) using percentage increases, and 3) stating that something is N times higher than... This is confusing. I highly recommend the authors to unify this discussion.

Minor comments:

line 49: I suppose that the authors mean quantities like the particle formation and growth rate as referring to parameters of NPF. I do not feel that parameter is a good wording here, rather suggesting something as characteristics of NPF.

lines 127-128: Classification of a NPF event seems untypical. Has the performance of this classification method tested and has it been used in other studies besides Fang et al. (2020)? The word obviously does not fit into this context.

lines 225-233: The authors list a number of things that potentially affect the occurrence of NPF. The list misses one highly relevant quantity: the intensity of solar radiation. This quantity should be mentioned here.

Unlike the Aitken mode, the nucleation mode is usually written using a lower-case letter (lines 326, 359, 383)

In several places (lines 225, 254, 258, 261, 284, 296, 299, 340), the use of tense is somewhat wrong, or at least uncommon. Please reconsider which tense to use in these places.