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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Microphysical simulations of new particle formation in the upper troposphere and lower stratosphere" *by* J. M. English et al.

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The study by English et al. contributes to the understanding of new particle formation in the UT/LS. These processes are still not well understood and thus studies as the present one are important to improve our understanding. However, some parts of the paper are not well written and could be improved.

First of all, their major finding is not that the aerosol distribution for particles greater than 10 nm is controlled by coagulation and not nucleation. It is generally known that nucleation does only affect the small size bins. Further, it is also generally

known that coagulation is only dependent on the size of the particle and not on the composition. Thus, it is clear that coagulation is independent of the nucleation process and gets as stronger as more particles are newly produced (e.g. after a volcanic eruption like Pinatubo one will find the strongest effect of coagulation on the particle distribution). This is a result one gets as soon as microphysical model simulations considering nucleation and coagulation are performed (e.g. Khosrawi and Konopka, 2003; Khosrawi et al., 2010). However, it is generally not stated that clearly in previous publications as it is done by English et al. There is nothing wrong with this statement, but they should include some references since this finding is not really new.

The major findings of English et al. are that (1) IMN is quite important in the UT/LS which is in agreement with the findings by Lee et al. (2003) and (2) that one gets very different results dependent on which BHN scheme is used. However, here it would be nice if they could give a recommendation which BHN scheme is in better agreement with measurements. For the evaluation of their model they only perform a comparison with one of the schemes, but it would also be interesting to know how the other BHN scheme performs compared to measurements.

Another general point of criticism is that too many figures are included compared to the length of the text. Especially, since most of the figures concern the evaluation of the model which as is not the main focus of this paper. I would thus suggest that the authors check if some of the figures can be omitted.

Some specific comments:
p12442: It would be interesting to know which altitudes were considered. Which altitudes are considered here as UT/LS?

p12442, l4: Here you write "tropical" UT/LS, but in section 4.2 also the UT/LS of the midlatitudes and polar regions is considered.

p12442, l10: As you write it here it is quite confusing and misleading. With only reading the abstract it sounds like that nucleation is not important in the upper troposphere. The problem is that you first describe that you apply three nucleation schemes and when suddenly describe one of your results concerning the further development of the particle distribution without describing the results of the comparison of the nucleation schemes. Further, it is also not stated what you mean with atmospherically relevant processes. Thus, I would suggest to add some more details and move this text part further down.

p12442, l18: Similar as above. Your main focus is the comparison of the nucleation schemes, but in the abstracts nothing concerning the results on the performance of the nucleation schemes is mentioned, but it is discussed what happens with the particle distribution after the particles have been formed. In general, I would say that the abstract needs some more structure and a clear line what has done and what are the results of this study.

p12442, l21: Why are now the results of the upper stratosphere discussed? The paper focuses on the UT/LS, why then mention aerosol properties at 30 km in the abstract?

p12442, l25: I doubt that particles that are produced in the UT/LS will make the way down to the boundary layer. Even vice versa only a minority of particles succeeds to reach up to the UT/LS (the ones that e.g. do not serve as CCN). Further, especially sulphuric acid/water particles will melt on their way down due to the increasing temperatures.

p12242, l25: Check the structure of the sentence, something went wrong here.

p12443, l1: I would suggest to include “on the surface of Polar Stratospheric Clouds (PSC) particles” after “heterogeneous chemistry”.

p12446, l8-10: One should differentiate here between pure organics as found in the boundary layer and sulphate-organics as found in the upper tropical troposphere (Froyd et al., 2009). Further, some references should be added and differences in UT compositions at different latitudes should be discussed. Further, the kind of aerosols that are found in the LS is still unclear since there are not so many measurements characterising aerosols in this region.

p12448, l26-27: Somewhat earlier you write that this nucleation scheme gives unrealistic results in the middle and upper stratosphere, but now it is stated above the tropopause. If these values are really not reliable in the lower stratosphere why then using this scheme for a UT/LS study? If the problems really occurs at the altitude regions considered here it should be more clearly stated.

p12449, l10: same comment as for p12448, l26-27.

p12449, l13: Measurements are usually “validated”, but models are usually “evaluated”.

p12449, l14: From what you write about the other schemes, it feels a bit like cheating now. For the evaluation the scheme without any restrictions is used, but how the results will be affected by the schemes with restrictions is not further discussed.

p12451, l1: Why do the sulphate aerosols evaporate? This should be explained a bit more.

p12455, l10-13: Is that a really good solution? In general the problems with the unrealistic nucleation rates occurs in the middle and upper stratosphere, but here the lower stratosphere is considered, thus the effect on the present results should be low. It should be more clearly stated why these nucleation schemes are applied for the lower stratosphere though they were developed for the troposphere, what the errors are and how this affects the present results.

p12456, l1: Which time period has been considered?

p12456, l2-4: These results could be expected and are in agreement with the theory. Some references are definitely missing here.

p12459, l2-5: These results are not really new and also here some references are missing. Further, I would suggest to move the text starting in line 9 to line 5, so that the discussion on the effects of coagulation on the particle distribution is not splitted into two different parts.

Finally, a study which might be of interest for the present study presented here is the comparison of different nucleation models by Korhonen et al. (2003).

References:

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