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Comment on acp-2021-795

Anonymous Referee #3

Referee comment on "The ion–ion recombination coefficient α: comparison of temperature- and pressure-dependent parameterisations for the troposphere and stratosphere" by Marcel Zauner-Wieczorek et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-795-RC3, 2021

The manuscript authored by Zauner-Wieczorek et al. presents a good review of the historical theory development on ion-ion recombination under relevant conditions of the troposphere and lower stratosphere. The authors then made a simple sensitivity study on the limiting sphere theories and compared the different parameterisations of the theories to measurement data from a few laboratory and field as well as model results. The content of the work, especially the review part, is valuable. The comparison studies are a bit flimsy, without discussions on why some parametrisations worked poorly and there was no insights given for corrections or improvements. The clarity of the manuscript needs to be improved and the manuscript needs somewhat a major revision.

Comments:

When talk about ion-ion recombination, could you please first of all provide the definition of ion? Do you also consider the recombination of charged aerosol particles as ion-ion recombination?

You compared the different parameterisations on ion-ion recombination to a few laboratory, field and model results and demonstrated that some models clearly have poor

performance but did not discuss the potential causes. Could you please elaborate on this and provide insights into how they may be corrected or further improved?

Based on the comparisons with laboratory, field and model data, you suggested Brasseur and Chatel 1983 over other parameterisations. Given the fact that it has the semi-empirical nature, it is expected to agree better with measurement data. The measurement data (whether it is Rosen&Hofmann, Gringel et al., Morita or Franchin et al.) are based on probing air ion concentrations. Air conductivity is intrinsically dependent on ion concentration. Then the uncertainty from measurement loss inside the instrumentation or the system cannot be avoided. This was not discussed in the manuscript when making suggestions on the choice of theory.

You did not recommend Tamadate 2020 due to its resulting in large deviation from measurement data. It seems however that the authors did not perform a MD simulation as described in Tamadate et al. 2020, instead the authors used the formula listed in Table 2 and referred that as Tamadate 2020. However, this functional form is merely Filippov's approach, which is similar to Fuchs model, as described in Tamadate 2020.

I also find the manuscript was not very carefully prepared. The notations are especially confusing. For example, the mathematical symbol of prime should be used instead of ’ (e.g. p6 L137). Also d have several definitions through the manuscript, which is confusing. v_+ and v_- were not defined where they appear first and definitions of U_+ and U_- in eq 8 were missing. It is also unclear what is x on p5 L128. A few different notations were used for the same property, e.g. e and e_T for collision probability, d and d_3 for three-body trapping distance, etc. It is also sometimes difficult to distinguish between similar symbols like a and a and M for molar mass and [M] for number density of air molecules. Please revise the manuscript carefully and drop off the repeated notions and use symbols that can be better distinguished.

p7 L160-161. It is confusing that you talk about 'collision probability becomes almost 0' and then 'collision is governed by the collision cross section'. Could you please elaborate what you mean here? How do you distinguish 'collision probability' and 'collision cross

section'? To my understanding, the CCS is just a different way to quantify the probability of successful collisions.

p7 L177. normal value? what is not normal?

P13 L367. what do you mean by 'ion current'?

p18 L472. what do you mean by 'trapping sphere'? Is it different from limiting sphere?

p24 L587. Ta20 yields α values which are one order of magnitude too low (2.7 · 10–6 cm3 s–1 at ground level). Is it true? 2.7e-6 cm3s-1 does not seem too low.

Fig.1 caption. please consider using open circle instead of white point.

Fig.3c The color for Tamadate et al. 2020 in legend is different from that in the plot.

Table 1. please define the symbols in the caption. what is r_0?

I also suggest that you consider restructuring some parts of the text. I find organisation of section 2 in the current manuscript does not render a smooth textflow, especially concerning the definition of *d*. Because *d* appears earlier in the text already but its definition comes quite late. Also in section 4, there is a sudden jump to ion-aerosol attachment without preparing the readers with the purpose.