Detailed answers are stated below. Black text is the review, blue text our answers.

Main comments

1) To prove the validity of the model, it would be more convincing if the authors showed a comparison to previous model studies. Particularly, I am surprised about the sentence “The comparatively high number concentration of cloud droplets is due to our low water accomodation (sic! – note that an ‘m ’is missing) coefficient.” (l. 505/6). In line 210, it is mentioned that alpha = 1 is used, which is the upper limit for this coefficient. What was the drop number concentration as predicted by Jaruga and Pawlowska (2018) and which accommodation coefficient did they use? Please clarify.

We use r_alpha and r_beta as relaxation parameters (line 217), as derived in Fukuta & Walter (1970). In this reference, two coefficients are described, one for r_alpha and another one for r_beta, named the water accommodation coefficient, alpha_acc, and the condensation coefficient, beta_con.

Note that some authors (e.g., Mordy 1959, Kogan 1991) only use one relaxation parameter, mostly r_beta (or a modification). In those cases, beta_con is then called the accommodation coefficient alpha, instead of condensation coefficient beta_con, as done in the original reference. We believe that this inconsistency in the literature caused confusion. Moreover, we believe that our subscript ``acc´´ was misleading. We changed it to alpha_H2O and beta_H2O, where alpha_H2O is what is commonly being referred to as accommodation coefficient (but condensation coefficient in the original reference). This is now also clarified in ll. 218-223 of the revised manuscript.

We prescribed an alpha_H2O = 0.0415 for our simulations with Cminor, which resulted in a droplet concentration of 397cm^-3. For alpha_H2O = 1, the number concentration drops to 338cm^-3. Jaruga & Pawlowska (2018) used alpha_H2O=1, which resulted in a droplet concentration of 269 cm^-3. In the intercomparison study by Kreidensweis et al. (2003), the analyzed models predicted droplet concentrations between 240 and 380cm^-3 for alpha_H2O=1, and about 270 to 410cm^-3 for very low alpha_H2O. Thus, the results obtained with Cminor and by Jaruga & Pawlowska (2018) fall well within this range.

2) How does the new solver compare in terms of computing time to previous ones that have been used for the same chemical mechanisms used here? Is it comparable?

In the revised manuscript, we compare Cminor to KPP solving the Master Chemical Mechanism, as now detailed in ll. 530-541 and the new Fig. 6 of the revised manuscript. We compare the computation times needed for KPP and Cminor for runs executed on a workstation computer. Shortly: Cminor needs approximately 1.48 times the time KPP needs for the simulation for equal conditions and error tolerances. While KPP has been optimized for years and creates a whole source code perfectly fitted to one specific mechanism (and initial conditions), we are aware of optimization potential left in the basic linear algebra routines of Cminor, and potentially somewhere else. On the other hand, reading and symbolically decomposing the matrix takes 2.2 seconds for Cminor, but it takes 5 hours and 52 minutes for KPP. The KPP code had to be generated on a comparable Linux machine, because larger mechanisms cannot be handled by KPP on Mac systems (due to restricted stack size limit). Changing a reaction parameter, adding a reaction, etc., always needs this time to be incorporated.

The second comparison to other models is shown in Fig. 7, where our revised manuscript shows the values presented in Jaruga & Pawlowska (2018). Also, the droplet number concentration is discussed and compared shortly to the values of Jaruga & Pawlowska (2018) and the intercomparison study by Kreidenweis et al. (2003) (see ll. 546-552 in the revised manuscript).

3) The chemical systems addressed are highly idealized. While I understand that the current paper is a model development paper, some more perspectives should be given how to apply Cminor to current atmospheric chemical problems that deviate from the rather simple cases. This could be briefly mentioned in the conclusions as a perspective for future extensions and applications. They include, for example,

- chemical processes in/on aerosol particles (doi: 10.5194/acp-10-3673-2010)
- ionic strength effects: aqueous phase rate constants have been shown to be a function of the salt content of the aqueous phase
- phase partitioning of semivolatile compounds. Even though it is mentioned that CAPRAM4.0a can be used to predict SOA formation in the aqueous phase, it is not clear how Cminor treats gas-aqueous partitioning of formed aqSOA species that may not follow Henry's law since they form salts or partition according to their volatility which may not follow Henry's law when water content becomes small
- could an externally mixed aerosol or drop population be considered, i.e. particles or droplets of the same size but different chemical composition?

A sentence mentioning the missing processes was added to the end of the summary in the revised manuscript (ll. 613-614 in the revised manuscript). Externally and internally mixed aerosol compositions are already possible to arbitrary extent, which is now clarified in Section 2.3 (ll.124-126 of the revised manuscript).

4) l. 100: It is not clear why you single out peroxy radicals as being potentially constant and why they are summed up to a single entity (supplement l. 168), given that they may have very different reactivities. Please add a justification and appropriate reference

Peroxy radicals are not assumed to be constant. We clarified our description, indicating that there is the possibility to consider them as a ``FACTOR´´, not as a passive species. Some reactions are formulated in a way to incorporate any peroxy radical. In this case, instead of writing one reaction for every RO2 species, the user can specify ``FACTOR: $RO2´´, and the sum of the concentrations of all peroxy radicals is used as an educt for the reaction. This is reformulated in ll. 100 to 103 in the revised manuscript and elaborated in the revised supplement in ll. 168 to 169, and ll. 471 to 506.

5) According to listing 1 of the supplement, it seems that only one salt can be used per CCN, e.g. NaCL or NH4(SO4)2. Could the model be used for realistic initial aerosol composition such as 50% amm sulf and 50% organics?

We clarified that Cminor can use any aerosol composition and internal and external mixtures in Section 2.3 (ll. 124-126 in the revised manuscript).

6) Some equations are numbered. Others are not. Please use consistent numbering throughout the paper.

In the original manuscript, the numbering was limited to equations referenced in the manuscript. In the revised manuscript, all equations of significance have a number.

Minor/technical comments:
l. 35: Phase transition depends also on chemical composition itself
Changed (ll.34-36).

l. 182: Call it 'aqueous phase 'here because the following text only refers to water (not to liquid organic phases)
Changed everywhere "liquid phase" appeared.

l. 187: It should be upper case K
Throughout the manuscript, we consistently denote rate constants by lower case k.

Supplement:
l. 62 multiplied with…
Changed (l. 62)

l. 413: Avogadro number is 6.022 10^23 not ^22
Changed (l. 413)

Table 3: What is ‘accommodation coefficient ’here? Before you describe that the accommodation coefficient for each species can be set separately.
We clarified this in the revised Table 3.