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Comment on acp-2020-1315

Anonymous Referee #2

Referee comment on "New methodology shows short atmospheric lifetimes of oxidized sulfur and nitrogen due to dry deposition" by Katherine Hayden et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-1315-RC2, 2021

This paper presents an estimate of dry deposition fluxes using aircraft observations. Such "regional" estimate provide a very useful constraint that can be used to improve the representation of dry deposition in global models with implications for both air quality and ecosystems. The observation-based estimate is compared with the deposition velocity calculated by the GEM-Mach model (TON and TOS) and by a suite of dry deposition algorithms (TOS only). The authors conclude that Vd is significantly underestimated and show that revisions to the representation of Ra, Rb, and Rc could reduce the model bias. The study is interesting and fits very well within ACP.
However, I do have concerns regarding the robustness of some of some of the results (especially for NOy) and I am unable to recommend this study for publication in ACP without significant clarification.

Comments

1) line 111. What is the sensitivity to organic nitrogen?
If the sensitivity is low, how does it affect the conclusions of the study?

2) line 191. Is this also a minor loss for NOy?

3) eq (2). If I am not mistaken, the authors assume that X_SO2 = - X_pSO4, if so this should be made clear. I would also suggest to write equation (4) as
Delta T_TOS = Delta T_SO2 + Delta T_pSO2 = -D_SO2 - D_pSO4.

4) The authors mention that the region is very dusty. This suggests that some SO2 (and much HNO3) could react on dust. Since coarse SO4/NO3 are not measured by the AMS, such flux could be mistakenly counted as dry deposition. The authors need to clarify how

this is accounted for.

5) line 275 and line 453
It would be helpful to summarize the differences between the different dry deposition algorithms listed here (Table 1 of Wu et al. (2018), for instance).
Without such information, it is very difficult to understand the impact and validity of the changes in Ra and Rb recommended by the authors in the GEM-MACH model.

6) line 351. Deposition velocities vary a lot across the different members of the NOy family.
Differences in NO emissions between model and observations (Tables 1 and 2) could lead to biases in the ratio of NO to NO2 or the conversion rate of NOy to HNO3, which would impact the simulated Vd(NOy).
Careful evaluation of the O3 and NOy simulation are needed to support the authors' conclusions regarding Vd(NOy).

7) I assume that changing Ra (and Rb?) will not only impact the removal of chemical species but also the heat, moisture, and momentum fluxes in GEM-Mach. Could the authors discuss the magnitude of these changes?

Minor comments:

1) line 20
"Dry deposition fluxes decreased exponentially with distance" This statement is unclear. Distance from where?

2) line 250
GEM-MACH has not been introduced yet. Delete or define earlier.

3) line 294
Introduce notation F7 as flight 7 (F7)

4) fig. 1 Flight 20 shows two plumes for TON but 1 plume for TOS. Could the authors comment on this difference?