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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Toward a more physical representation of precipitation scavenging in global chemistry models: cloud overlap and ice physics and their impact on tropospheric ozone" *by* J. L. Neu and M. J. Prather

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This manuscript describes a substantial improvement in the physical representation of the process of scavenging by "large scale" precipitation (i.e., not by deep convection) in global atmospheric chemistry models, which has been and will remain a challenge in such models due to the large difference in scales between the resolved grid and the microphysical processes being represented. Although the manuscript is quite technical in nature, and perhaps equally appropriate for GMD, it is made appropriate for ACP through the careful analysis of several sensitivity studies, as well as the comparison

with observations; unfortunately the currently available observations are not sufficient to distinguish between the new and previous parameterizations, though they do provide clear evidence that at least some amount of scavenging of HNO3 by ice is occurring, in agreement with the previous study of von Kuhlmann and Lawrence (2006). Despite the technical nature, the manuscript is clearly-written and the details will be useful for others in the community who want to adopt the algorithm or compare alternate formulations to it. I recommend publication after attending to a few minor comments, listed below.

1) Several assumptions are made in the algorithm which do not appear to be justified in the manuscript (if they are already there and I overlooked them, then the justifications need to be moved to somewhere more prominent; I would suggest summarizing all such assumptions and justifications in a table somewhere). Examples include: the assumption of maximum overlap of clouds in connected precipitating layers (this is one extreme – random overlap is another possibility and the reality likely lies between these); the retention coefficient of 0.5 for HNO3 (normally it is found to be closer to 1 in laboratory experiments); the choice of a 25%/km evaporation rate; and the imposed 10% cloud fraction (this is not an exhaustive list; generally the manuscript should be scanned by the authors and justifications added anywhere that a semi-arbitrary choice is made).

2) abstract: it could be a bit confusing to most readership that HNO3 is first indicated to be a "critical" part of the tropospheric O3 budget, but then later in the same sentence it says that HNO3 has "little impact on. . .O3" (here it simply should be added that this implies the NOx and O3 concentrations or mixing ratios, not their budgets); furthermore, in the next sentence it is stated that "the O3 budget is much more sensitive to the lifetime of HNO4", even though HNO3 is already considered "critical" for the O3 budget.

3) the first paragraph of the introduction should be moved into the end of the abstract (the next paragraph, starting "Recent research. . ." is much more appropriate for an

introduction)

4) Karcher is spelled with two dots over the “a” (change throughout the manuscript)

5) p. 24419 line 13, “models that have. . .description*s*” (add "s")

6) p. 24421, line 17+: it is unclear what is meant with the reference to Lawrence and Crutzen (1998), which did show that cloud ice sedimentation in and of itself has a large impact on the HNO3 distribution (in addition to precipitation scavenging); please elaborate.

7) p. 24428, line 13: “spanning a similar range” – should this be “smaller range", since it is comparing a 50-70% change with a >200% change?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 24413, 2011.