Atmos. Chem. Phys. Discuss., 14, C11379–C11382, 2015
www.atmos-chem-phys-discuss.net/14/C11379/2015/

Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Time-dependent freezing rate parcel model" *by* G. Vali and J. R. Snider

G. Vali and J. R. Snider

vali@uwyo.edu

Received and published: 20 January 2015

Responses to Reviewer #2

The reviewer's point about the difference in our level of understanding of the thermodynamics of cloud parcels and that of ice nucleation is clearly valid. In this paper, the simple cloud model is treated with well-established thermodynamic theory and ice nucleation is treated on the basis of laboratory observations which at this time have only scant theoretical underpinnings.

We disagree with the reviewer about the degree of reliability of the formulation of freezing nucleation that is used in the paper. Indeed, it is not based on a first principle theory, but it is backed by considerable empirical evidence and by a plausible physical

model. While further developments in the description of ice nucleation are certain to be forthcoming, there is convergence toward, if not full community agreement on (as the reviewer points out) approaches that combine the singular description with time-dependence. Individual assessments differ on how strong that convergence is; in our view it is significant enough to trust in its use now and to expect further reinforcements of it in the future.

In all, we thank the reviewer for his/her thoughtful comments. Specific points are listed below, with the reviewer's text in italics, and the responses in normal font.

"The parcel is labeled as adiabatic. Is that adiabatic, or pseudo-adiabatic? Changes in the liquid water content are explicitly calculated. Is the latent heat of condensation accounted for? Is the latent heat of freezing?"

We use "adiabatic" without a modifier because that is consistent with meteorological definition of a saturated ascent (http://glossary.ametsoc.org/wiki/Moist-adiabatic process). Our model is that of a reversible adiabatic parcel. We also note that the difference between reversible and pseudoadabatic is relatively minor and would have no perceptible impact on the results here presented. Latent heat of condensation is included, that of freezing is not because of the negligible mass of ice compared to liquid.

"The ice nucleation parameterization is based on measurements that are presented in terms of ice nucleating particles per unit mass of water. That is used to initialize the parcel. Is it adjusted as more water condenses? I don't think it is, but then why calculate changes in liquid water content (see previous question)? For a given ice nucleating particle, more water surrounding it (i.e. a bigger droplet) doesn't make a difference (once you have bulk water)."

The approach of calculating ice concentrations in proportion to liquid water content was stated in the first three paragraphs of Section 2. It follows from the use of $K(T)$

determined with samples of precipitation and cloud water as the model input. This is a limitation of the treatment, as was stated in the last paragraph of Conclusions. The model implies that ice nucleating particles enter into cloud droplets in proportion to the liquid water content as opposed to a pulse input at cloud base. Detailed treatments of aerosol to cloud transfer are avoided. More complete models can clarly be formulated but the goal here was to demonstrate the effects of the time history of the cloud parcel and lttle emphasis is placed on the absolute values of the derived ice concentrations.

"There are a few places in the manuscript which state that the stochastic description overestimates the ice concentration in the parcel. Overestimated in comparison to what? There's a difference between the stochastic description and the one you are using here, but you can't say one is correct and the other isn't because you don't have an observation of a parcel rising through the atmosphere with ice concentrations recorded as a function of time and/or height."

The statements are intended to call attention to the much larger ice concentrations that result from the stochastic treatment than from the TDFR model, as shown in Fig. 1 with the dash-dot lines and as explained in the text (last paragraph on pg. 29317). Neither are compared with actual observations in clouds for lack of suitable data. Again, the credibility of the TDFR model results versus the stochastic prediction lies in the reliability of the laboratory measurements on which the TDFR model is based.

"A comparison of the results presented here to those presented in Knopf and Alpert (2013) would be warranted. They present results from a stochastic based model that seem quite reasonable."

We have included that comparison in the revised text. The Knopf and Alpert (2013) measurements of cooling-rate dependence yielded results quite comparable to those of Vali (1994) giving added confidence in the way cooling-rate dependence is treated in the TDFR model.

Pg. 29308, line 9: "is" should be "are".

corrected

Herbert et al. (2014) is not in the list of references.

added

Pg. 29319, line 10: "effected" should "affected"

corrected

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 29305, 2014.