Atmos. Chem. Phys. Discuss., 10, C10231–C10234, 2011
www.atmos-chem-phys-discuss.net/10/C10231/2011/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Leipzig Cloud Interaction Simulator (LACIS): operating principle and theoretical studies concerning homogeneous and heterogeneous ice nucleation" *by* S. Hartmann et al.

G. Vali (Referee)

vali@uwyo.edu

Received and published: 17 February 2011

MAJOR COMMENTS:

Only after getting to near the end of the paper did I realize what the authors meant by the title of the paper and that I misunderstood it until then. As it is, the title suggests that the paper will address the physical concept of the LACIS instrument and provide details about its realization. To some extent that is true but it is not quite reflective of what the paper is about. LACIS has been written about extensively before and the operating

principle is not detailed in this paper much beyond what has been already written elsewhere. In fact, the description given here relies on references to previous papers. This paper describes a numerical model of processes within the instrument and compares model predictions with measured ice crystal formation within the instrument.

If the paper really set the goal of proving the validity of the various aspects of the operating principle of LACIS one would have wanted to see empirical proof of instrument characteristics. Perhaps that was already given in earlier papers. Here, impressive precision is quoted for the temperature control but this is not translated into measured accuracies of air temperature as a function of time and how it may undergo transients changes. Flow rate, the thickness of the ice coating on the walls, deviations from laminar flow are some of the issues. What happens when vapor deposit on the ice walls grows in dendritic or other complex form?

The authors tackled the difficult task of modeling processes within the instrument, both with regard to conditions and with regard to evolution of the three phases of water. This is a major achievement. However, a limitation of the theoretical description isn't specifically stated (or I missed it). At this stage, the model is formulated for monodisperse uniform chemical aerosol and for a single (selectable) mode of ice nucleation. Neither the theoretical formulation nor the practical implementation are described for dealing with multiple processes acting at the same time. These may follow in the future, one can surmise.

In dealing with heterogeneous ice nucleation, the authors take the careful approach of considering both CNT and the singular description. The latter was shown by Niedermeier et al. (2010) to provide the better explanation of observations. Their formulation takes the form of an empirical fit to results obtained and is here used in analyzing the same instrument for the same type of aerosol. Thus, as far as I can see, the results here presented are really summed up in lines 8-10 of page 25600, while the sentence following that in lines 10-12 is not fully justified.

A fundamental issue of ice nucleation can also be raised. Equation (11) presents the singular model as a rate function, i.e. time dependent. This is in contradiction with the basic notion of the singular theory. A rate function can be applied here because temperature is changing at a fixed time rate, so f(T) can be substituted by g(t). However, a change in the transformation function T(t) would require the constants of (11) to be changed.

MINOR POINTS:

page/line

25579/0-8: Why are all-ice clouds excluded?

25579/18: Immersion nuclei do not have to be CCN; they can enter cloud droplets by (passive) scavenging.

25579/20: What is the importance of quoting Megahed (2007) here? The statement is generally accepted as is - what does the reference add to it?

25579/28: IN what sense are the IN "effective"?

25584/22: How certain are the authors about the efficacy of the water/ice discrimination? Couldn't the tail of the narrow distribution (assumed to be water) be due to ice? Couldn't some part of the broad distribution (ice) be due to water droplets? It would be useful to have some quantitative assessment of this potential overlap, especially if experiments are to be conducted at higher temperatures or with polydisperse/mixed composition aerosol . The mention of future improvements in this regards adds to the feeling that the current approach has definite limitations in acuity.

25585/27 and following text: How could contact nucleation be identified? What if ice crystals formed in the first place, were evaporated and formed a second time by other mechanisms?

25586/6: It seems redundant to talk about ice supersaturation when water supersatu-

ration is specified.

25593/17: Are two decimals justified?

25593/11: "Section" is used both for parts of the apparatus and for parts of the paper. Not a source of major confusion but if possible, it should be avoided. Perhaps 'segments' or 'stages' could be used for the apparatus.

25593/22: Again, is two decimal accuracy justified and needed?

25594/1: In what sense are the temperature profiles 'inhomogeneous'?

25594/11: This definition of the temperature error seems highly arbitrary.

25594/21: "... version b ..." is not used in section 3.2

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 25577, 2010.