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Interactive comment on "On the ice-nucleating potential of warm hydrometeors in mixed-phase clouds" *by* Michael Krayer et al.

Anonymous Referee #2

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Summary:

This study explores the possibility of the nucleation of ice crystals in the wake of hydrometeors undergoing wet growth through numerical simulation. The governing equations were non-dimensionalized using appropriate scales and solved using a spectral-element method. The control parameters are the Reynolds number and the temperature difference between the hydrometeor and the ambient. The spatially resolved simulation showed that the volume of the wake that is supersaturated is significantly greater than what was previously reported in the literature (Fukuta and Lee 1986). This is an interesting result. The authors transform the supersaturation in the wake to an ice nuclei concentration using a power law. Later, they average the ice nuclei concentration over the entire cloud volume and conclude that the increase in ice crystal

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Discussion paper

concentration is only marginal. The paper is fairly well written and organized. The volume analysis of the supersaturated wake is an important result and would be of interest to the mixed-phase community. However, the analysis on the ice enhancement in clouds is fairly weak. The main issues are the transformation of supersaturation to ice particle concentration using a power law, the choice of environmental parameters for wet growth and the estimation of the fractional cloud volume subjected to enhanced supersaturation due to the hydrometeors. This work should be considered for publication, but only after the comments listed below are addressed satisfactorily. The reviewer would like to revisit the manuscript after the comments are addressed.
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Major comments:

1) In the simulation, the surface temperature of the hydrometeor was fixed at 0 C. The ambient temperature was varied between -20 C and -40 C. Can the authors justify the choice of ambient temperatures for this study? Can the authors cite observations that detect wet growth at such low temperatures? There is a comprehensive experimental study by Greenan and List (JAS, 1995) on the surface temperature of hydrometeors at different conditions. It is unlikely that wet growth would occur at such low temperatures.

2) In section 3.3 the authors define a parameter called ice enhancement factor to quantify the effects of enhanced supersaturation. This parameter is justified, but the expression used for finding NIN is not. This expression is used in Baker 1991, but none of the recent work on ice nucleation use this expression (to the best of reviewer's knowledge). Such a power law relationship between the number concentration of ice nuclei and supersaturation seems physically inconsistent. For example, barring the effects of wettability/chemical composition, as the supersaturation is increased, the size of the aerosols that is activated is reduced. For ice nucleation, the size of the nucleus is an important parameter, and as the size of the nucleus is reduced, its ice nucleating efficiency is also reduced. So, the number concentration of ice nuclei may not increase with supersaturation like a power law with such high exponents as mentioned in this

paper. Furthermore, such a power law may not even be applicable to CCN concentrations when the supersaturation is quite high (Q. Ji and G. Shaw 1998 GRL). So, the applicability of such a power law to ice nuclei concentration is highly questionable. Can the authors comment/justify the applicability of the expression for NIN, as the whole of section 3.3 and the most important conclusion in the paper is based on this expression? This comment needs to be addressed in detail to support the conclusion. If this issue cannot be addressed satisfactorily, the authors can consider presenting their arguments based on fractional cloud volume (like in section 3.2) that is exposed to the enhanced supersaturation due to the falling wet hydrometeors.

3) The analysis in section 3.3 can be recast as the cloud volume that is exposed to very high supersaturation in the wake. This analysis concludes that the fraction of the cloud volume exposed to the high supersaturation in the wake is insignificant. There is a similar study published recently (Prabhakaran et al 2020 (GRL)). Their analysis concluded that a significant fraction of the cloud volume can be exposed to the high wake supersaturation during the lifetime of the cloud. Can the authors comment about the difference between these two analyses?

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Comments to improve the quality of the work:

1) In lines 108-109, the authors state that buoyancy contributions to momentum due to the variations in temperature and water vapor is negligible. Can the authors justify this statement briefly (a few lines) by quoting the value of the relevant parameter, e.g. Richardson number, along with the reference to Chouippe et al 2019? Would it be insignificant when the temperature difference between the ambient and the drop is 40 C? Similarly, in lines 118-119, can the authors justify briefly why the variations in the vertical velocity is not important in the present context?

2) In a deep convective cloud, the hydrometeors are falling through a turbulent environment. Can the authors comment about the role of turbulent fluctuations in the ambient?

How would the volume of the supersaturated region change with turbulence intensity in the ambient? There are some heat transfer studies from a heated sphere in a turbulent environment (Bagchi and Kottam 2008, Phys of Fluids). Can this be extended to the current study? It might be worthwhile to briefly discuss this as a part of future work.

3) Can the authors comment on how the supersaturated volume would be affected in the presence of cloud droplets and ice particles in the ambient?

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Minor comment:

Excess supersaturation - notation difference between Eq. 15 and Fig 4 caption. Fig 4 caption has a "*" on top of "s".

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