Review of the manuscript "On the ice-nucleating potential of warm hydrometeors in mixed-phase clouds" by Krayer et al.

Overview:
The paper presents the results of flow simulations of free falling particles focusing on calculations of high supersaturation formed in the particles' wake for the case when the environmental and particle temperatures are different. High supersaturation regions may result in the nucleation of INPs, which normally do not nucleate at low supersaturation. Under certain conditions, this process may result in the enhanced production of ice particles and explain one of the prevailing problems of secondary ice production. This study is a continuation of the work started in Chouippe et al. (2019). One of the important findings of this work is that regions with persistent high supersaturation, attained in the wake of warm hydrometeors, may extend up to 50 diameters downstream. The diagram in Fig.9 provides a clear assessment of the significance of this SIP mechanism at different temperatures. In my opinion, this is an important study further contributing in our understanding of the roles of different SIP mechanisms in ice initiation in clouds. The style, conciseness, and clarity of explanation left a very good impression. In my view, the paper should be published after addressing few comments listed below.

Recommendation:
Accept after minor revision.

Comments:

1. Since it was not specified in the text, it appears that Eq.12 assumes that the lifetime of all hydrometeors is the same. This assumption would work well for riming particles falling through a mixed phase environment. However, the condition T_p=0°C will be limited by the freezing time of drops and should be accounted for in Eq.12. Since freezing time for large and small drops may be different by few orders of magnitude (e.g. Murray and List, 1972), the effect of small droplets on the supersaturated volume may be lower than shown in Fig.6. The following results obtained in this work will also be affected. The effect of freezing time for the case of freezing drops requires clarification.
2. The supersaturation calculation was considered for a particle free falling in still air, i.e. in a non-turbulent environment (ε = 0). Could you speculate on a qualitative level, how ε > 0 may affect your results? Would it increase or decrease the global ice enhancement factor?
3. What is the role of air pressure P on the results obtained in this paper (specifically Fig.9)? Since particle fall speed, viscosity and thermal conductivity depend on P, it may have a noticeable effect on the mixing rate, supersaturated volume, amplitude of result supersaturation and the persistence of supersaturated regions. It is worth indicating what P was used in this study.
4. An important element not discussed in this study is the conceptual consideration of how this SIP process is related to natural clouds and identification of environmental conditions when it becomes significant. There are a few statements regarding this matter scattered throughout the manuscript. However, it leaves the reader with an impression of incompleteness of this paper. For example, as discussed in section 2, the condition T_p=0°C can be satisfied for riming ice particle or freezing drops. For the first case, the accretion of cloud droplets on the ice surface should reach the wet growth regime, i.e. when LWC reach the Ludlam limit. At -30C, for a free-falling hailstone, the Ludlam limit exceeds 5g/m3 (the exact number needs to be checked). Such high LWC at -30C

does not seem to be feasible. Regarding the second case, there are very few reports on observations of precipitation size drops (D>100um) at -30C. Therefore, this option also appears to be uncommon in clouds. In addition to the discussion on page 15 related to Fig.9, it is worth expanding the discussion about the feasibility and significance of this SIP mechanism in temperatures warmer than -30C. Mentioning that convective clouds are the most likely candidates for this type of SIP to occur would be also relevant.

5. Eq.10: What $D_{j,min}$ and $D_{j,max}$ were used in this study? It is worth indicating the in the text.
6. Definition of $\tilde{s}_i^*$ is worth introducing in the text prior to Fig.4.
7. Line 201: Should it be $\tilde{s}_i^*$?
8. Eq.12. Definitions of τ and Ω should be provided in the text.
9. Line 206: "… as a function of the threshold". I guess you employed $\tilde{s}_i^*$=0.1 threshold. This should be indicated in the text.
10. Lines 24 and 358: Field et al. 2016 should be 2017.

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