To the Authors and the EiC,

I have read and reviewed the article, “Macroscopic water vapor diffusion is not enhanced in snow”, submitted for publication in The Cryosphere by K Fourteau, F Domine, and P Hagenmuller. In this study, the authors investigate via theoretical considerations of diffusion and attachment kinetics combined with numerical simulations, whether or not water vapor diffusion in snow is enhanced on the macro-scale, when compared to water vapor diffusion in air. The authors frame the historical context of their study by providing a detailed overview of previous work on the topic, beginning with Yosida et al. (1955), that have led to the commonly held belief that water vapor diffusion *is* enhanced in snow at the macro-scale, due to “hand-to-hand” mechanisms of water vapor transport occurring at the micro-scale. The authors challenge this explanation by i) deriving a theoretical model from first principles that accounts for both attachment kinetics and diffusion within an inert or kinetically active porous medium and ii) performing numerical simulations on both idealized snow microstructures and actual snow microstructures obtained from micro-CT. With this approach, the authors show that although the diffusion of water vapor in the pore-space between ice grains can be greater than that of water vapor in air, this effect is more than countered by the overall tortuous nature of the ice matrix, such that diffusion alone cannot account for the sometimes large water vapor flux observed in snow at the macro-scale, even with infinitely fast and/or non-linear kinetics.

Substantively, my only comments relate to potential areas where the paper could be improved by further explaining or providing evidence for why certain terms/processes were either neglected or only speculated upon in their study. For instance, to explain field observations of a larger than expected water vapor flux in snow, the authors suggest that convection may also be occurring at the macro-scale, but present no evidence in support of this speculation. Furthermore, the effects of ice grain curvature on the overall vapor flux are neglected, also without explicit evidence or discussion. Last, the effects of latent heat are neglected as well, again without sufficient evidence. While I would generally agree that the effects of ice grain curvature and latent heating are not primary drivers of water vapor diffusion in snow when a strong temperature gradient is also present, it is my opinion that the manuscript could be strengthened and the broader impacts increased if further explanation were provided with regards to these and other topics.

Overall, I found the article to be convincing, well-written, and worthy of publication in The Cryosphere. Furthermore, it should be noted that the topic at hand has been one of debate, and it is not without due deliberation that I submit my review. I congratulate the authors on the merits of their work and acknowledge the respectful tone with which they address the historical significance of their findings. Additional comments below.

Recommendation: Minor Revisions

Best regards,

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General Comments:

1) Where Colbeck 1983 is cited for neglecting any contribution to the vapor flux from the local curvature of ice (lines 155-157), I think an opportunity is missed here to acknowledge and discuss some of the more recent work on the topic (Krol & Lowe 2016, 2018) that have also investigated local ice crystal growth rates as a function of curvature from micro-CT. Similarly, these authors also develop a treatment of vapor diffusion in snow that accounts for the full ice matrix at the pore scale, that they claim can be scaled to larger length scales, such that their work seems equally relevant in that regard as well.

2) Although it is mentioned in the Discussion appropriately and with references, I would recommend removing any mention of convection as a possible process by which water vapor transport is occurring in snow from the Abstract, as there is no evidence provided in support of this statement explicitly from this study.

3) Could you further explain your reasoning for neglecting the latent heat flux in your treatment (lines 290-292)? In Hammonds and Baker 2016 (Figure 7), it was estimated that the latent heat flux from deposition may have accounted for approximately 10% of the *increase* of the local temperature gradient (these calculations were based on Riche and Schneebeli 2013). Furthermore, whether or not the latent heat is expected to be absorbed into the ice matrix or released into the surrounding air upon phase change would also be of relevance for increasing or decreasing the local temperature gradient. Experimental SEM observations from Hammonds et al. 2015 also address this point, Last, Libbrecht and Rickerby 2013 (Section 2.3) also discuss the likelihood for latent heat flux to be released or absorbed as a function of ice crystal size.

4) Figure 1: This illustration and explanation (lines 87-90) could be improved by also showing the case of the "ice phase" (here, phase is used correctly) that is just above or just below the two cans, such that if one calculated the net mass flux for all three cases across this boundary, a zero net mass flux is observed.

5) Is it possible to be more specific about the separation of scales (line 105)? Is $L_{micro} << L_{macro}$ sufficient? Molecular attachment, for instance, occurs on a length scale even smaller than L_{micro}. Please comment.

6) What is different about ice crystal growth in a snowpack (Line 318 – 319)? For faceted ice crystal growth, such is presented in this study, the molecular attachment considerations presented in Libbrecht and Rickerby 2013 seem sufficient. Furthermore, theory would dictate that attachment from the vapor phase would be preferred on the primary prism face (Brumberg et al 2017). Recommend deleting "and might not properly apply to ice in snowpacks".

7) Please include an additional item in the Appendix that presents the numerical values used for each constant given in each equation, with units and a citation for each.

8) Throughout the manuscript, the word “phase” is used to represent what I think is meant to be “space”, as in “pore phase” or “air phase”. I would recommend using the word “space” instead of “phase”, and reserving “phase” only for referencing the thermodynamic state (i.e. solid, liquid, or gas).

9) Throughout the manuscript, the words “inferior” or “superior” are used to represent “less than” or “more than”, in terms of comparing two quantities. As the terms “inferior” and “superior” are often used in English with connotations of mediocrity or greatness, respectively, would recommend just using “less than” or “greater than” throughout the manuscript.

Specific Comments:

Line 52: The cans filled with snow were “weighed” (not “weighted”)

Line 109: I am not sure what is meant by “solicitations” in this context.

Line 111: Add a “t” …but **not** so large that **it** spans several…

Line 118: switch order of “time unit” to “unit time”

Line 133: Can you provide a citation for the use of an Effective Diffusion Coefficient?

Line 146: Instead of “submitted to”, perhaps “placed under” or “held under” would be more appropriate in this context.

Line 200: Instead of “1/phi factor”, should be “a factor of 1/phi”

Line 255: To be more technically correct, replace “tomography scanning” with “X-ray computed microtomography (micro-CT)” or similar. (also in line 377)

Line 293: Here and throughout the article, could you provide references for the values chosen?

Line 335: “act as ***a*** blockage”

Line 336: “go around” not “goes around”

Line 382: Replace “zoom” with ”focused view” or similar…

Table 1: Please change the label “Inf. Fast Kinetics” to “D_{eff}”, as this more accurately describes these quantities.

Line 437: Remove “?” from Domine citation

Line 438: “snowpacks” not “snowpack” in this context

References:

Brumberg, A., Hammonds, K., Baker, I., Backus, E. H., Bisson, P. J., Bonn, M., ... & Shultz, M. J. (2017). Single-crystal Ih ice surfaces unveil connection between macroscopic and molecular structure. *Proceedings of the National Academy of Sciences*, *114*(21), 5349-5354.

Krol, Q., & Löwe, H. (2016). Analysis of local ice crystal growth in snow. *Journal of Glaciology*, *62*(232), 378-390.

Krol, Q., & Löwe, H. (2018). Upscaling ice crystal growth dynamics in snow: Rigorous modeling and comparison to 4D X-ray tomography data. *Acta Materialia*, *151*, 478-487.

Hammonds, K., Lieb-Lappen, R., Baker, I., & Wang, X. (2015). Investigating the thermophysical properties of the ice–snow interface under a controlled temperature gradient: Part I: Experiments & Observations. *Cold Regions Science and Technology*, *120*, 157-167.

Hammonds, K., & Baker, I. (2016). Investigating the thermophysical properties of the ice–snow interface under a controlled temperature gradient Part II: Analysis. *Cold Regions Science and Technology*, *125*, 12-20.

Riche, F., & Schneebeli, M. (2013). Thermal conductivity of snow measured by three independent methods and anisotropy considerations. *The Cryosphere*, *7*(1), 217.