TC-2020-183

RESPONSE TO KEVIN HAMMONDS

We are thankful to Kevin Hammonds for the thorough review of our manuscript and its constructive comments.
We have copied his comments below in light blue, and provided our answers in black below them.
Modifications to the text of the manuscript are proposed in green.

Note that we have discovered two numerical errors in the previous version of the manuscript that we we will correct in the new version:
- **L343**: The average gradient in the air is 79.00 K/m (and not 77.57 K/m as previously stated).
- **Table 1** and **Fig 7**: The effective diffusion coefficients for the Melt Forms sampled have been underestimated by 33%.

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.

The study of Krol and Loewe (2016) experimentally studied isothermal and thermal gradient metamorphism, and discuss whether a diffusion-limited hypothesis (roughly corresponding to a high α in our article) or a kinetics-limited hypothesis (low α) is more consistent with their observations. Note that they do not study the macroscopic vapor flux but the interface velocities (which are related but not equivalent to one another). They observe that isothermal crystal growth is slightly better represented by the kinetics-limited case while gradient metamorphism is well represented by diffusion-limited kinetics and assuming water vapor saturation at the interface ice/pore. Adding the effect of curvature in their derivations improve the quality of their fit to their temperature gradient metamorphism data.

While we fully agree that curvature effects might play a role for the growth of ice crystals, we think that the impact on the macroscopic flux is negligible. Indeed, for curvature effects to drive a net macroscopic flux, a macroscopic gradient of curvature should be present in the snow layer, which is unlikely to be the case in homogeneous snow layers. We are currently not able to test this hypothesis, but future work could be to add a curvature term in the saturated water vapor concentration and see the impact of the macroscopic flux.

We will add details on why we neglect curvature effects, **L155**:
"In the presence of a large enough thermal gradient, the dependence of the saturation concentration to the local curvature of the ice surface can be neglected compared to its dependence on temperature (Colbeck, 1993). Under this condition, we can expect c_{sat} to becomes a function of temperature only. Moreover, even if curvature effects were not negligible at the microscopic level it appears unlikely for them to results in a net macroscopic vapor flux. Indeed, in an homogeneous snow layer curvature differences are distributed isotropically within the microstructure, and thus do not result in a net movement of water vapor."

We will also refer to the study of Krol and Loewe (2016) and their experimental observations in the discussion, **L425**:
“ […] we observe that D_{eff} / (phi D_0) is almost always greater than unity, which supports the notion of fast rather than slow kinetics. This is consistent with the study of Krol and Löwe (2016) that report that fast kinetics is consistent with their microtomography-based observation of the temperature gradient metamorphism of a snow sample.”

The study of Krol and Loewe (2018) propose equations for the geometrical evolution of the averaged SSA and curvatures (assuming that the interface velocity is known in the microstructure). They do not treat the vapor flux in the pores, and therefore we do not see how this study can be compared to our.

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.

We agree and will remove the mention of convection in the abstract. We will reformulate the discussion on convection to **L437**:
“Indeed, the importance of convective mass transport in subarctic snowpacks has notably been pointed out by Trabant and Benson (1972) and Sturm and Johnson (1991), and thus appears as a good candidate to explain the high vapor movement in subarctic snowpacks.”

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.

Our understanding is that the work Hammonds and Baker 2016 quantifies the effect of latent heat on the macroscopic thermal gradient (the thermal gradient of snow at the layer scale), and not the temperature gradient within the pores themselves. On the other hand, the effective diffusion coefficient of vapor is rather governed by the ratio of thermal gradient in the pore space (governing the concentration gradient in the pores) over the macroscopic thermal gradient (governing the macroscopic concentration gradient).

At the pore scale, water vapor transports heat from the warm sublimation surfaces towards the cold deposition surfaces. In a sense, it acts as a heat transport mechanism in the pore space that occurs in parallel of heat conduction. Taking latent heat into account at the microscopic scale thus can be viewed as artificially increasing the heat conduction of air. This reduces the thermal contrast between the ice and pore spaces, and decreases the thermal gradient in the low-conducting pore space (all this working at constant macroscopic thermal gradient). Decreasing the thermal gradient in the pore space decreases the vapor concentration gradient in the pore space, resulting in a lower macroscopic vapor flux and a lower effective diffusion coefficient.

At the microscopic scale, the release and absorption of latent heat appears as a discontinuity of the heat conduction flux at the ice/pore interface (Calonne et al., 2014). From our understanding latent heat thus warms/cools both the ice and the air in the vicinity of the interface. The degree to which the surrounding air and ice are impacted depends on the ability of the heat to be conducted away in the ice or in the air spaces though their thermal conductivities.

The discussion of Libbrecht and Rickbery (2013) and the “bread-loafing” observations of Hammonds and Baker (2016) focus on whether the deposition zones are heated enough to disrupt the anisotropic crystallization of water, and to influence the crystal habits. In our case we are only concerned with the vapor flux in the pores, and do not treat crystal habits. We therefore do not see how to integrate such observations in our treatment, apart from the fact that they are consistent with the notion of warming deposition surfaces and cooling sublimation surfaces.

To quantify this effect we have performed an additional FEM simulation. In the case of a fully vapor saturated pore space (corresponding to the infinitely fast kinetics in our case), Yosida et al. (1955) argue that latent heat effects at the microscopic scale can be treated by increasing the thermal conductivity of air by $dc_{sat} / dT * D_0 * L$, where L is the latent heat of sublimation of ice. We integrated this in a FEM simulation of the DF snow sample, and found that the effective diffusion coefficient dropped from 0.982 to 0.980. The results of this double diffusion problem simulation, indicate that consistently with our understanding, latent heat effects reduce the macroscopic vapor flux at constant macroscopic thermal gradient and thus diminishes the effective diffusion coefficient of vapor. Moreover, the overall impact appears to be quantitatively small.

We will add to the text **L291**:
“At the microscopic level, adding latent heat effects would act as an additional mechanism transporting heat from the warm sublimating surfaces towards the cold deposition surfaces. It would cool the sublimation surfaces and warms the deposition surfaces, decreasing the thermal gradient in the pore space. Therefore, taking latent heat effects into account would not increase the effective vapor diffusion coefficient.”

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.

We are not sure to fully understand the comment of the reviewer. The cases of ice grains just below or just above the boundary can be treated exactly as ice grains far away from it: the ice appearing or disappearing in the volume control correspond to water molecules already present in the volume under the gaseous form. Therefore, no mass transport is associated with the deposition and sublimation of water molecules.

We think that showing ice grains just below and above the boundary would give the wrong impression that the gain of ice of the grain just above the boundary should be understood as compensated by the loss of mass of the grain just below it.

5) Is it possible to be more specific about the separation of scales (line 105)? Is Lmicro « Lmacro sufficient? Molecular attachment, for instance, occurs on a length scale even smaller than Lmicro. Please comment.

Our goal in this article is to see what can be said about the macroscopic vapor flux starting from equations directly at the pore scale. It is true that the microscopic equations are themselves “homogenized” from the atomic scale, for instance by encompassing all the processes of water molecules attachment onto ice within a single α parameter.

However, as we start from the pore scale, it is not necessary for us to explicitly treat the atomic scale (even though a good understanding of the atomic scale physics is helpful to develop a good comprehension of the pore scale physics). Similarly to Calonne et al. (2014), the validity of the pore scale equations are taken as a given in the article.

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".

The measurements of Libbrecht and Rickerby 2013 are based on the growth of faceted crystals, which only present a limited number of ideal surface types. In real snowpacks however, the variety of crystal surface types is much greater, with non-flat and vicinal surfaces, such as sublimation surfaces that tend to be rounded. Such surfaces behave differently from facets as deposition is facilitated on them, due to the greater density of steps. Moreover, we assume in the article that deposition and sublimation physics are symmetric, which might however not be the case. Therefore, we cannot guarantee that the simple non-linear law used in this article applies to the entirety of a snow sample. The quantitative use of non-linear kinetics would first require to properly characterize the crystallographic state of the different surfaces within a snow sample, something we cannot do at the moment.

We will add more justifications on why we think that while non-linear surface kinetics is a promising topic for snowpack physics, our results in this study should be viewed as purely qualitative and should not be over interpreted. For this we will add **L355**:
"[…] and might not properly apply for the entirety of ice surfaces in snowpacks. Indeed, this law has been derived using deposition measurement, and might not apply for sublimation surfaces (Beckmann and Lacmann, 1982). Moreover, the presence of vicinal surfaces in snowpacks, where the proposed law does not apply, is likely (Legagneux and Domine, 2005). Therefore, the point of using such a law is to qualitatively study the potential impact of a dependence of α to the local vapor saturation, rather than to produce quantitative results."
We will also justify our choice not to use the non-linear kinetics law on all of our snow sample **L402**:
"We also did not compute D_{eff} with non-linear surface kinetics (i.e. when α is not constant), as we are not confident in the validity of the chosen non-linear law for snow modeling."

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.

We will add a table with the numerical values of constants and appropriate reference in the Appendix, and refer to it in the main part of the manuscript.

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).

We will remove the word phase throughout the manuscript when it is not appropriate.

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.

We will change the "inferior" and "superior" to "less than" and "greater than" throughout the manuscript.

SPECIFIC COMMENTS:

Line 52: The cans filled with snow were "weighed" (not "weighted")
We will correct the spelling.

Line 109: I am not sure what is meant by "solicitations" in this context.
Here by solicitations we refer to the external physical conditions imposed to the sample, might it be the thermal and vapor gradients or imposed thermal and vapor fluxes. The employed term is usually "excitation", as in Calonne et al. (2014) for instance.

We will rephrase **L109** to:
"[…] L_{macro} is the length-scale characterizing variations of the snowpack or of the external forcing applied at the macroscopic scale, for instance the change between different snow layers or changes in thermal and vapor gradients"

Line 111: Add a "t" . . .but not so large that it spans several. . .
We will correct the typo.

Line 118: switch order of "time unit" to "unit time"
We will rephrase accordingly.

Line 133: Can you provide a citation for the use of an Effective Diffusion Coefficient?
We will provide references, within and outside of the snow science community, namely Colbeck (1993), Calonne et al., (2014), and Bourbatache et al. (2020).

Line 146: Instead of "submitted to", perhaps "placed under" or "held under" would be more appropriate in this context.
We will rephrase to:
"Let us consider a volume of snow (Figure 2 a), subjected to vertical macroscopic temperature and vapor gradients at its boundaries."

Line 200: Instead of "1/phi factor", should be "a factor of 1/phi"
We will rephrase this sentence accordingly, as well as in the sentence **L253**.

Line 255: To be more technically correct, replace "tomography scanning" with "X-ray computed microtomography (micro-CT)" or similar. (also in line 377)
We will use the term of "X-ray computed microtomography" in the article.

Line 293: Here and throughout the article, could you provide references for the values chosen?
We will provide the references within a new Appendix presenting all the physical constants used.

Line 335: "act as a blockage"
We will rephrase accordingly.

Line 336: “go around” not “goes around”
We will correct the typo.

Line 382: Replace “zoom” with ”focused view” or similar. . .
We will rephrase to:
“A close-up view showing the vapor stream lines [...]”

Table 1: Please change the label “Inf. Fast Kinetics” to “Deff”, as this more accurately describes these quantities.
The label Inf. Fast Kinetics refers to the value of α used for the simulations (in this case α → infinity). We will redo the table to clearly indicate that all numerical values in the right part of the table are all D_{eff} values.

Line 437: Remove “?” from Domine citation
There should be a citation of Sturm and Benson (1997) instead of the “?”, but we did not realize that the citation link was broken. This will be fixed in the new version.

Line 438: “snowpacks” not “snowpack” in this context
We will rephrase accordingly.