

Interactive comment on “Numerical experiments on isotopic diffusion in polar snow and firn using a multi-layer energy balance model” by Alexandra Touzeau et al. A. Kerkweg kerkweg@uni-bonn.de Received and published: 7 November 2017

Dear authors, In my role as Executive editor of GMD, I would like to bring to your attention our Editorial version 1.1: <http://www.geosci-model-dev.net/8/3487/2015/gmd-8-3487-2015.html> This highlights some requirements of papers published in GMD, which is also available on the GMD website in the ‘Manuscript Types’ section: http://www.geoscientific-model-development.net/submission/manuscript_types.html.

In particular, please note that for your paper, the following requirements have not been met in the Discussions paper:

- "The main paper must give the model name and version number (or other unique identifier) in the title."
- "If the model development relates to a single model then the model name and the version number must be included in the title of the paper. If the main intention of an article is to make a general (i.e. model independent) statement about the usefulness of a new development, but the usefulness is shown with the help of one specific model, the model name and version number must be stated in the title. The title could have a form such as, "Title outlining amazing generic advance: a case study with Model XXX (version Y)".
- "All papers must include a section, at the end of the paper, entitled 'Code availability'. Here, either instructions for obtaining the code, or the reasons why the code is not available should be clearly stated. It is preferred for the code to be uploaded as a supplement or to be made available at a data repository with an associated DOI (digital object identifier) for the exact model version described in the paper. Alternatively, for established models, there may be an existing means of accessing the code through a particular system. In this case, there must exist a means of permanently accessing the precise model version described in the paper. In some cases, authors may prefer to put models on their own website, or to act as a point of contact for obtaining the code. Given the impermanence of websites and email addresses, this is not encouraged, and authors should consider improving the availability with a more permanent arrangement. After the paper is accepted the model archive should be updated to include a link to the GMD paper."

Thus please add the models name (SURFEX/Crocus ?) and the version number to the title of your article. Additionally, it would be good if the explicit version described in this article would be archived in a permanent archive providing a DOI (e.g. Zenodo). Yours, Astrid Kerkweg

We apologize for not including the model references in the article title. We will add the relevant information to the title:

1. 1: 'Numerical experiments on vapor diffusion in polar snow and firn and its impact on isotopes using a multi-layer energy balance model (SURFEX-Crocus, version V8, revision c9d521da)'

We will also update our code availability section. The model SURFEX is open-source and available online after free registration through the platform cnrm-game-meteo.fr. Therefore it is not necessary to provide a copy on Zenodo.

We have updated the code availability section:

I. 762: 'The code used in the manuscript is a **development of the open source code for SURFEX/ISBA Crocus model, version V8, hosted on an open git repository at CNRM/GAME (https://opensource.cnrm-game-meteo.fr/projects/surfex_git2)**. Before downloading the code, you must register as a user at <https://opensource.umr-cnrm.fr/>. You can then obtain the code used in the present study by downloading the revision 'c9d521da' of the branch `touzeau_dev` (last access: January 2018). The climate forcing required to perform the runs is available as a supplement.'

***** Interactive comment on Geosci. Model Dev. Discuss.,
<https://doi.org/10.5194/gmd-2017-217>, 2017.

Interactive comment on “Numerical experiments on isotopic diffusion in polar snow and firn using a multi-layer energy balance model” by Alexandra Touzeau et al. Anonymous Referee #1 Received and published: 30 November 2017

Touzeau et al. presents a detailed study in implementing isotopes into a semi-complex one-dimensional snow pack model. Unfortunately it is my opinion that the authors still need a little bit more work to allow this publication to become a significant contribution to the community. I am though positive that the manuscript will be publishable after my major comments have been taken into account.

Major comments:

(The following list of comments are not ordered in accordance with importance as they are more or less equally important)

- The use of parentheses throughout the manuscript is not in accordance with good practice. It makes reading the manuscript difficult. Please rewrite relevant sentences.

Most of the parentheses will be removed in the revised manuscript.

- The term ‘oriented vapor transport’ seems to complicate the reading. The model has already been defined as 1D and hence no need to include the word ‘oriented’. Please remove throughout paper.

We used the term ‘oriented vapor transport’ to stress that vapor diffusion was not only driven by isotope gradients but also by temperature gradients. Diffusion induced by temperature gradient do not lead to homogeneous repartition of isotopes in contrast to the diffusion along isotopic gradient and this was the reason why we chose the term “oriented”. We agree that this may not be obvious, and we have thus replaced ‘oriented vapor transport’ by ‘thermally induced vapor transport’ in the manuscript.

- ‘Vapor density gradients’. Please change to ‘vapor pressure gradients’ throughout the paper. The use of vapor pressure is the normal term used i.e Merlivat and Jouzel 1979 and Jouzel and Merlivat 1984 etc.

“Density” was indeed probably not the best term (see also reviewer 3 comments). Because the unit of this term is kg.m^{-3} , we have chosen to use the term “concentration” as suggested by reviewer 3.

- If a sentence is longer than 2 lines, it is most likely too long. Please refrain from using extremely long sentence that complicates the understanding of the manuscript. This is seen at several instances through out the manuscript, but my favorite example is section 2.1 L106-109 where I really have no idea what is being described.

We have rephrased the introduction of Section 2.1. using shorter sentences.

l. 110-114: **'Here we describe first processes leading only to attenuation of the original amplitude (Sect. 2.1.1.). Then we describe processes which lead to other types of signal modifications (Sect. 2.1.2.). These modifications include transporting and accumulating isotopes in some layers without consideration of the original isotopic signal. They also comprise processes taking isotopes away from the snow, and therefore shifting the mean $\delta^{18}\text{O}$ value of the snow deposited.'**

- Rephrase 'mean local pluriannual value' or describe what you mean.

Here we define 'mean local pluriannual value' as the average isotopic composition in the precipitation taken over several years (~10 years). This value averages seasonal variations and synoptic variations in the precipitation. It may be different from the average value in the snow layers that corresponds to the same period of time due to post-deposition processes.

- Rephrase 'oriented processes' or describe what you mean

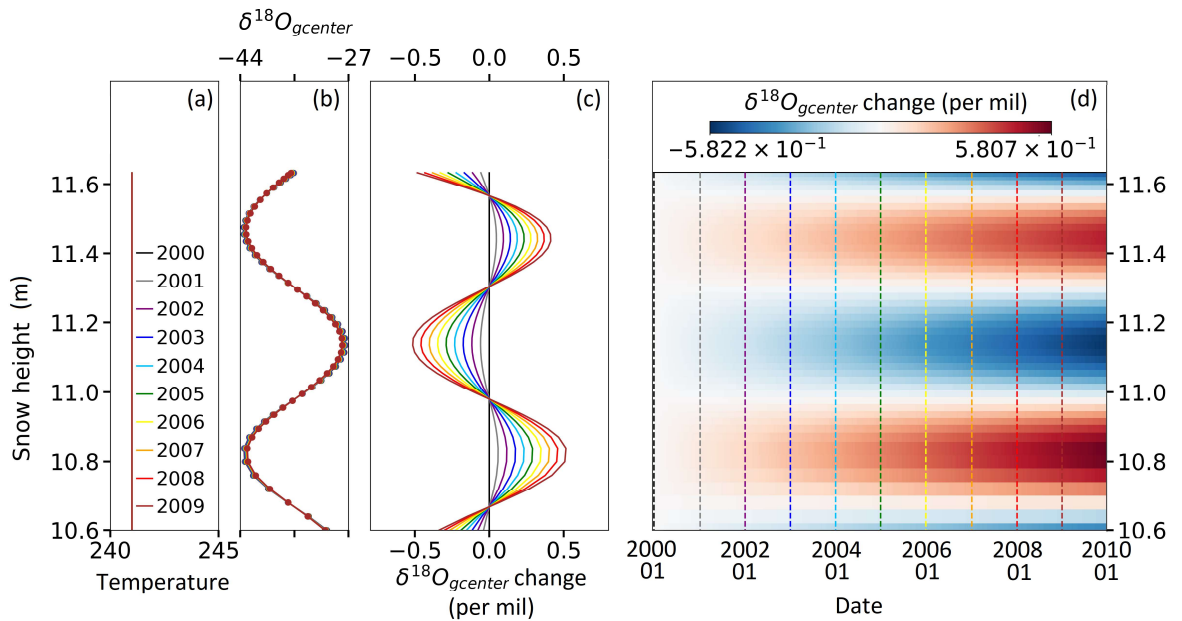
Here we mean dynamical processes of vapor transport that are forced by atmospheric pressure or temperature variations. We used the term 'oriented' in opposition to 'random', in the sense 'forced' or 'pushed' or 'driven'. Maybe we should have said instead 'orienting' processes, as it is the vapor molecules which get 'oriented', not the processes themselves.

We propose to keep the term 'oriented' for the water molecules themselves, and to replace 'oriented processes' by 'processes leading to oriented vapor transport'. We also add a line in the text to stress that 'oriented' is used in opposition to 'random agitation', and not in the sense of 'unidimensional' or 'vertical'.

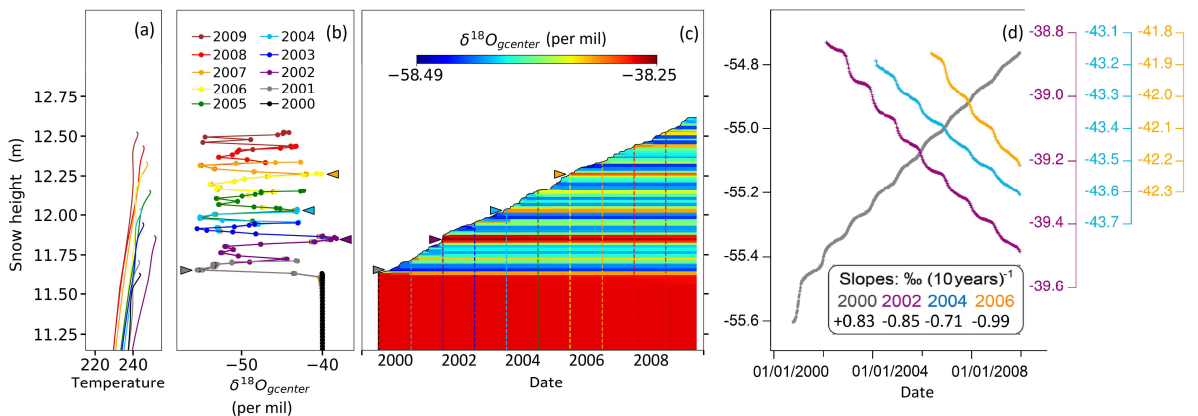
l. 137-139: **'We use the term 'oriented' here to describe an overall movement of water molecules that is different from their molecular agitation, and externally forced.'**

- In L113 you write "Indeed, higher temperatures correspond to higher vapor densities, and also higher diffusivities in the vapor and the solid phase". This is correct, but then you line 260 define the vapor diffusivity in air to be a constant despite that it is depending on both temperature and pressure. This needs to be corrected. You need to allow for a temperature and pressure dependence on the diffusivity.

The reviewer is perfectly right. We have run the two main simulations again with varying D_{v0} (function of atmospheric air pressure and snow temperature using the formula of Johnsen et al., 2000), and found some differences in the attenuation compared to the initial simulations. For the 10 years simulation at Dome C, the attenuation increases by 2-5%, and for the 10 years simulation at GRIP (with fixed temperature) it increases by 9-16%. Therefore we will replace the corresponding figures in the manuscript by the new ones, and modify the values of attenuation given in the text.



New Figure 2. Simulation 1: 10 years at GRIP with fixed temperature (240 K), with D_{v0} function of the temperature.



New Figure 11: Simulation 6: 10 years at Dome C with precipitation with varying $\delta^{18}\text{O}$; with temperature evolution throughout the year; with D_{v0} function of temperature.

- I have a problem with your first sentence in the introduction “Ice is a key archive for past climate reconstruction, which preserves . . . indications relevant to the temperature of formation of the snow precipitation. . . variations of the isotopic ratio of oxygen and deuterium”. This sentence is problematic because you have co-authors who have published papers documenting in both Greenland and Antarctica how the isotopic composition of the deposited precipitation is changed through exchange with the atmospheric water vapor isotopes. You cite 8 publications to document your statement, but they are between 10 and 30 years old. You thereby disregard published research for the last five years. Please update.

We do not see a contradiction here, as a climatic signal may persist even after post-deposition

processes have occurred. Therefore information regarding temperature may still be present, even if exchange with vapor isotope has taken place. Nevertheless, we will update the bibliography and soften these statements.

I. 25: 'The isotopic ratios of oxygen or deuterium measured in ice cores have been used for a long time to reconstruct the evolution of temperature over the Quaternary (EPICA comm. members, 2004; Johnsen et al., 1995; Jouzel et al., 2007; Kawamura et al., 2007; Uemura et al., 2012; Lorius et al., 1985; Petit et al., 1999; Schneider et al., 2006; Stenni et al., 2004; WAIS-Divide members, 2013; Stenni et al., 2011). They are however subject to alteration during post-deposition through various processes. As a consequence, even if the link between temperature and isotopic composition of the precipitations is quantitatively determined from measurements and modelling studies (Stenni et al., 2016; Goursaud et al., TCD, 2017), it cannot faithfully be applied to reconstruction of past temperature.'

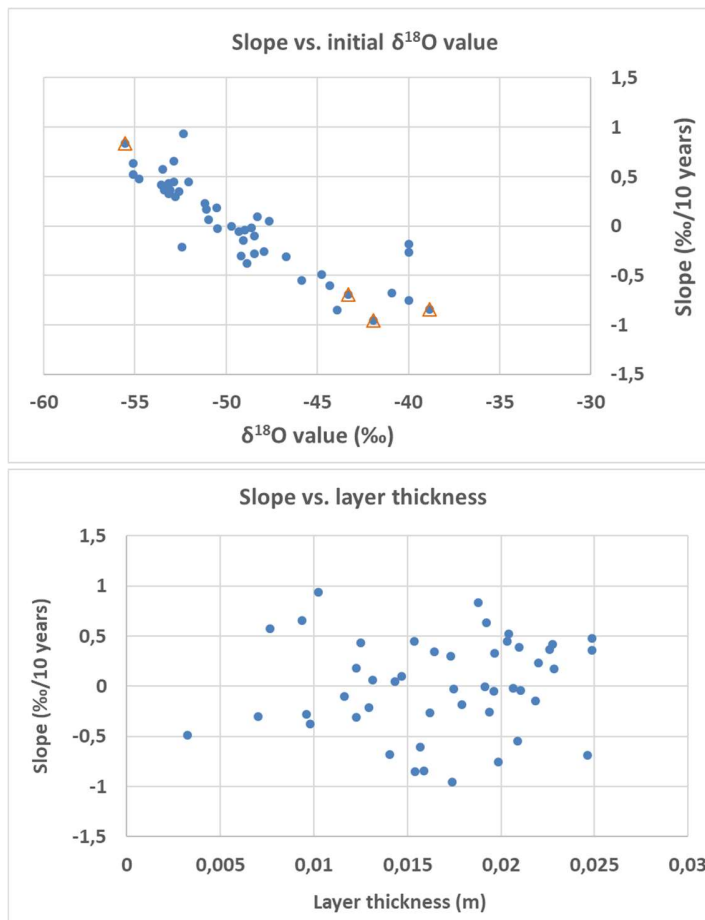
- In L 17: Why not study the influence of temperature and not only temperature gradients? What is the difference between "compaction" and "Wind compaction"? Do you study the effect of amount of precipitation or the isotopes of the precipitation?

- Physically, higher temperatures lead to increased diffusion through increased molecular agitation and also through increased vapor content in the air. In the first case, the control is a power function, while in the second case the control is exponential. Thus, we considered in a first approximation that molecular agitation was of second order and could be neglected. Still, in this new version, we will also consider the direct influence of temperature since the dependency of diffusivity on temperature is added.

- There are two possible types of compaction implemented in the model (see Vionnet et al., 2012, for more details):

- 1) Compaction caused by the weight of overlying layers ("compaction"),
- 2) Compaction caused by wind reworking of the snow, which leads to increased density in the top layers (« wind compaction »).

- We did not study specifically the effect of precipitation amount, as we used only one set of precipitation data coming from ERA-Interim. We did not vary this parameter to see how diffusion would be modified but it would be easy for future users to make such a study with the available code. Still, over the course of 10 years, variability of the precipitation amounts did occur. We followed 48 layers which were maintained for one year at least, and up to 10 years. For these layers, the thickness was ranging from 3 mm to 2.5 cm, and the slope was ranging from -0.137 to +0.133 ‰/10 years. Based on these layers, the slope does not seem to be related to the layer thickness. However, it appears that the slope is related to the original $\delta^{18}\text{O}$ value in the layer.



- Regarding the isotopic composition in the precipitation we have run a zero-simulation with constant $\delta^{18}\text{O}$ in the precipitation. We wanted to see how vapor transport could possibly generate $\delta^{18}\text{O}$ variations, based on temperature gradients, in the absence of initial signal (Figure 6 and Figure 8). For the first layer, the $\delta^{18}\text{O}$ changes by about 1‰ in one year, whereas for the deeper layers, the change is about 0.1‰ during the same period. We then used the air temperature to compute $\delta^{18}\text{O}$ variations in the precipitation, to evaluate attenuation based on a realistic $\delta^{18}\text{O}$ signal (Figure 10 and Figure 11).

- L 52: Use another word than “Mechanical shuffling”

We replaced this term by “mechanical reworking”.

- L119: You write that the annual cycles generally disappear at sites with accumulation lower than 200 kg/m²/year – but does that not depend on time scales – please be more precise.

It is true that thinning will also have an effect on the disappearance of annual cycles at deep depths. We will thus modify the statement saying that annual cycles disappeared at shallower depths (100 m deep) for sites with accumulation lower than 200 kg/m²/year.

I. 128: ‘In Greenland, Johnsen et al. (1977) indicate that annual cycles generally disappear at depths shallower than 100 m for sites with accumulation lower than 200 kg m⁻² yr⁻¹.’

- L120: You write that the diffusion is more intense in the upper layers – but don't the diffusion depend on the isotopic gradient and would you not expect that to be larger further down in the snow? Please be precise! Also the word 'intense' might not be the best to use in this case

Indeed, theoretically, if diffusion was initially very low, and no other processes were active, the effect of compaction could increase $\delta^{18}\text{O}$ gradients downward by reducing layer thicknesses. In that case, the diffusion based on isotopic gradients would indeed increase downward.

Our model is indeed able to study such effect. It may be the purpose of a future application through a much longer run of the model than those presented here. Our aim here was to take the diffusion effect from the beginning, i.e. from the upper layer where porosity is large and temperature gradient huge hence enabling a strong diffusion. This will be clearly written in the revised version since it was not clear enough here.

- Section 3.1.2: Describe why the new vapor transport subroutine is inserted after module 5 but before module 6? What are the thoughts behind this?

The steps of the model first describe changes in the snow structure and microstructure (new layers, densification, metamorphism, wind drift) and later the energy exchanges. Because vapor diffusion is closely associated with metamorphism, and lead to changes in the layer density, it seems natural to put it within this first series of modules that describe snow structure. Furthermore, its effect on the temperature profile is probably limited.

- L251: “. . . is the effective diffusivity of water vapor in the snow at the interface”. Do you mean effective diffusivity of water vapor in the air between the snow grains?

There is a first step where we indeed compute “effective diffusivity” for each layer (from diffusivity in air and taking into account the size of the porosity, Equation (5)).

$$\frac{D_{eff}(t,n)}{D_v} = \frac{3}{2} \left(1 - \frac{\rho_{sn}(t,n)}{\rho_{ice}} \right) - \frac{1}{2} \quad (5)$$

Then, what we name “interfacial diffusivity” ($D_{eff}(t,n \rightarrow n+1)$) is the average of two “effective diffusivities” from two adjacent layers ($D_{eff}(t,n)$ and $D_{eff}(t,n+1)$, Equation (6)). The “interface” here is the limit between the two layers.

$$D_{eff}(t, n \rightarrow n + 1) = \frac{1}{\frac{1}{D_{eff}(t,n)} + \frac{1}{D_{eff}(t,n+1)}} \quad (6)$$

This explanation is already present in the text.

l. 247: “flux of vapor at the interface between two layers”

l. 254: “The effective diffusivity at the interface is obtained in two steps: first the effective diffusivities ($D_{eff}(t,n)$ and $D_{eff}(t,n+1)$) in each layer are calculated (Eq. (5)), second, the interfacial diffusivity is computed as their harmonic mean (Eq. (6)). ”

To facilitate reading, we will add an indication line 252:

“and $D_{eff}(t, n \rightarrow n+1)$ ($\text{m}^2 \text{ s}^{-1}$) is the effective diffusivity of water vapor in the snow at the interface **between layers (see below).**”

- Equation 6: I am not sure, but isn't a layer thickness missing from this formula as you might not have the same layer thickness in layer n and n+1?

Assessing interfacial effective transport properties in the case where layer thicknesses are different is a classical, yet, critical issue (e. g. D'Amboise et al., 2017 GMD), especially if the contrast in layer thickness is too large. Here we ensure that the contrast in layer thickness remains as small as possible to limit the impact of this effect, and under such a situation we make the simplifying assumption that the interfacial diffusivity depends equally on the values of the two layers concerned.

- Equation 7: Why do you use an analytical approximation of Clausius-Clapeyron around zero and not a more precise empirical formula?

We are not aware that this formulation would provide worse results than empirical formulae.

- L 313 : "Long time" – what do you mean – please be precise

Original text:

'Equilibrium fractionation is a hypothesis that is correct in layers where the air has been standing still for a long time in the porosity and where vapor has reached equilibrium with ice grains, physically and chemically.'

We implied here that the equilibrium fractionation hypothesis was a reasonable hypothesis in our case. Indeed, the equilibrium situation is limited by the water vapor - snow mass transfer whose associated speed is of the order of 0.09 m.s^{-1} (Albert and McGilvary, 1992). In our case, we are dealing with centimetric scale layers thickness and recalculate the isotopic composition every second so that we consider that the speed of the mass transfer is not limiting the equilibrium situation at the water vapor - snow interface.

We have thus reformulated the text accordingly:

I. 329: 'Equilibrium fractionation is a hypothesis that is correct in layers where vapor has reached equilibrium with ice grains, physically and chemically. **This process is limited by the water vapor - snow mass transfer whose associated speed is of the order of 0.09 m.s^{-1} (Albert and McGilvary, 1992). In our case, we are dealing with centimetric scale layers thickness and recalculate the isotopic composition every second so that we consider that the speed of the mass transfer is not limiting the equilibrium situation at the water vapor - snow interface.'**

- L334: What vapor are you referencing to? H₂O in general or H₂¹⁶O?

Here we refer to H₂O. We propose to add this precision in the text:

I. 353: '**When the vapor concentration is the same in two adjacent layers, the total flux of vapor is null. But we still have isotopic diffusion because of the isotopic concentration gradients (Eq. (13)), as long as they are non-zero.'**

- L335: I believe you meant to write "we will still have diffusion of heavy water isotopes during conditions where the water isotopic gradient is non-zero.

This is very close to our meaning yes. We forgot to mention that in that case, diffusion is driven by isotopic gradients, only if they themselves are non-zero. Thanks for this precision. However, both

heavy and light isotopes will diffuse. Therefore, we propose this correction:

l. 356: “But we still have isotopic diffusion because of the isotopic concentration gradients (Eq. (13)), as long as they are non-zero.”

- L335-336: The sentence is very convoluted. I believe you could also have zero flux of H216O but a flux of H218O in one direction and HD16O in another direction.

We will remove this sentence, to simplify the reading.

- L353: “Here the condensation of excess vapor occurs without additional fractionation”. Why do you make this assumption? Whenever you have a phase change due to condensation you will have isotopic fractionation. I think this is something that needs to be updated in your code.

We take this fractionation into account **earlier** in the model. We define our interstitial vapor as being **at equilibrium** with the solid phase (all the time) due to permanent sublimation/condensation in the porosity. This is why we write “without any additional fractionation”. We do not want to apply this fractionation twice.

Kinetic fractionation due to supersaturation is also taken into account during the diffusion of the different isotopes, each with their associated diffusion coefficient.

Still, we understand that this aspect was not very clear in the initial manuscript and propose the following revision:

l.370: “Here the condensation of excess vapor occurs without additional fractionation **because (1) there is a permanent isotopic equilibrium between surface snow and interstitial vapor (each first step of the sub-routine) and (2) kinetic fractionation associated with diffusion is taken into account during diffusion of the different isotopic species along the isotopic gradients**”

- L356: “The transfer of isotopes takes place from the grain surface toward the vapor without fractionation” If you assume this then the interstitial vapor will not be in isotopic equilibrium with the snow surface. This would then correct itself. Hence I think that your code needs to be set-up such that the interstitial vapor is in isotopic equilibrium with the snow surface at all time.

Yes, temporarily, after this sublimation the vapor is no longer at equilibrium with the solid phase. But this is corrected immediately, as both are merged again before the next step (each step has a duration of one second). At the beginning of the next step, vapor isotopic composition is defined again at equilibrium with snow surface.

It is mathematically difficult to predict the composition of the sublimated vapor needed to have equilibrium in the end, and much easier to merge the two compartments and recreate later an equilibrium.

- Please note that you throughout the paper are mixing up GRIP and Summit. They are two different geographical places in Greenland albeit being close to each other.

We are sorry for this mixing, this will be corrected. Still, the climatic characteristics of these neighbour two sites are very similar so that this does not affect the results presented here.

- I am surprised to read that there are no density measurements for neither GRIP nor Summit

and that you therefore use NGRIP. Please double-check this.

Indeed, GRIP density measurements are available as listed in Bréant et al. (2017) and reference therein (http://gcmd.nasa.gov/r/d/LSSU_PSU_Firn_data and Schwander et al., 1997; Iizuka et al., 2008). The density profile is close to the NGRIP profile. We ran the model with the correct density profile and found that the new profile did not change the results. Still, the new version will include the correct data.

- You do not give a relationship for the isotope-temperature relationship for GRIP. Please correct.

This is because the simulations at GRIP do not include precipitation, so the isotopic composition in the precipitation (and its relationship to temperature) is not useful here.

We have added a sentence in the text to clarify this point:

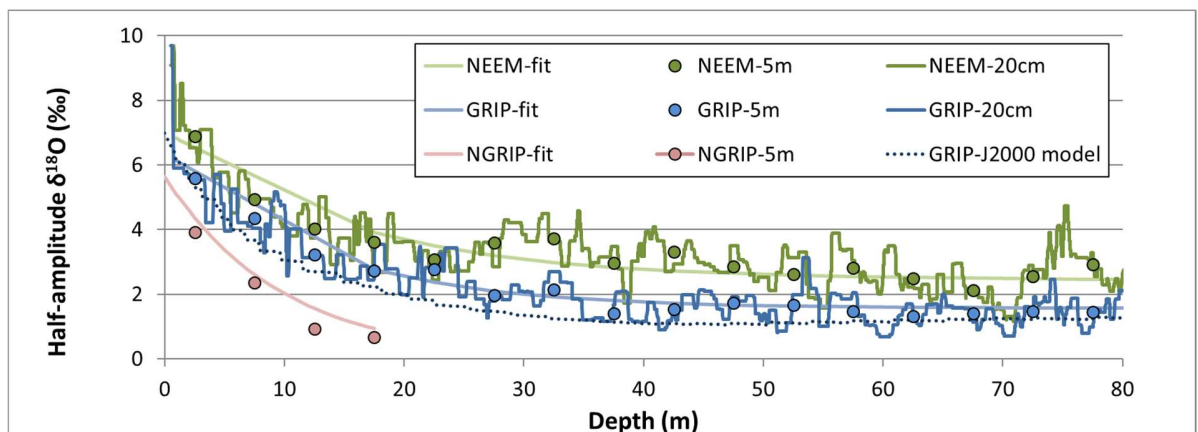
L. 466: "following Eq. (15) to link $\delta^{18}\text{O}$ in the snowfall to the local temperature (T_{air} , in K):

$$\delta^{18}\text{O}_{sf} = 0.45 \times (T_{air} - 273.15) - 31.5$$

(15)

We do not provide an equivalent expression for GRIP, Greenland, because the simulations run here (see Sect. 3.1.1) do not include precipitation."

- Figure 2: You should include a comparison with the model of Johnsen et al. 2000



New figure 4 with the model of Johnsen et al. (2000).

We have added a curve (GRIP-J2000 model) on this figure corresponding to the model of Johnsen et al. (2000) for GRIP. We have used their equation 4 (amplitude as a function of *diffusion length* σ and *wavelength* λ) as well as Figure 2 for the evolution of diffusion length with depth. We then obtained the wavelength evolution with depth on the Eurocore data by detection of maxima and minima.

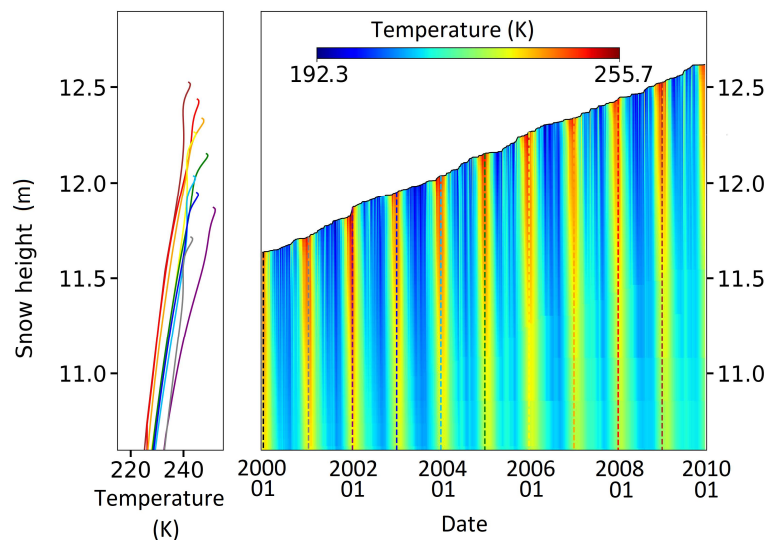
- Figure 3: You write in the manuscript that the temperature is varying but on the figure you only show temperatures for the summer. Does this mean that you only use summer temperatures? I would expect you would use varying temperatures through the whole year.

The temperature indeed varies the whole year but we have chosen to show only one temperature profile per year, to limit the number of curves on the graph. We chose January, because we considered that this month was one of the warmest, and likely to produce strong temperature gradients

and strong vapor diffusion.

We will include a figure showing weekly temperature evolution in the Supplement. We will also add a note in the Figure 3 caption to clarify this point.

I. 1102: “(a) Vertical temperature profile for each summer; (b) $\delta^{18}\text{O}_{\text{gcenter}}$ profile for each summer; (c) Deviation of the $\delta^{18}\text{O}$ relative to the original profile, for each summer; (d) Evolution of the deviation to the original profile of $\delta^{18}\text{O}_{\text{gcenter}}$. **Note that temperature varies during the whole year (see Figure TT in the Supplement).**”



- I am surprised to find that your model does not show an influence of temperature gradients at GRIP as you would normally assume that temperature gradients would force vapor to be transported between layers due to the vapor pressure gradient?

There is indeed a small effect of temperature gradient at GRIP. This can be seen on the two figures 2 and 3. When temperature gradients are active, attenuation is stronger in upper layers, while under constant temperature, the attenuation is the same at 15 cm depth and at 70 cm depth. Quantitatively there is also an increase of attenuation in Figure 3 (from $5.021 \cdot 10^{-1}$ to $7.567 \cdot 10^{-1}$). Thus temperature gradients enhance diffusion at this site. However, this increase is small, and does not bridge the gap with the data.

We state:

I. 495: “In conclusion, at GRIP, the diffusion of vapor as a result of temperature gradients **has only a limited impact** on isotopic compositions, and most of the simulated attenuation can be attributed to diffusion against isotopic gradients.”

- L503: Is the attenuation at GRIP significant larger than NEEM? 86% and 90% seems very similar.

The reviewer is right, we will replace “greater” by “**slightly higher**” in the text.

- L511: Why don't you calculate the attenuation using Johnsen at GRIP such that you can compare with Bolzan and Pohjola?

A comparison with the Johnsen model will be included in the revised version (cf. comment above)

- L526: It is unclear how Denux in 1996 can indicate that a study by Johnsen et al. in 2000 overestimates the attenuation. Time travel hasn't really been possible yet. You might write that “A study by Denux (1996). . .”

Sorry for that, of course he was referring to the study published by Johnsen in 1977, and dealing with the same model. We have corrected the error:

I. 561: ‘Denux (1996) and van der Wel et al. (2015) indicate that the model **developed by Johnsen (1977) and used in Johnsen et al. (2000)** overestimates the attenuation compared to observed values. For Denux (1996), the model of **Johnsen (1977)** should take into account the presence of ice crusts and the temperature gradients in the surface snow to get...’

- L528: You write that Johnsen et al. should take into considerations temperature gradients in order to not overestimate the attenuation. But would you not expect that temperature gradients would increase the attenuation due the vapor transport driven by vapor pressure gradients?

It is not clear yet if including temperature gradients would indeed increase the attenuation of the isotopic signal. This process might move the signal downward or upward without altering it much. It could also produce local isotope accumulation originally not present in the signal (see Figure 6). By creating these local isotope maxima the original signal could in the end ‘gain’ variability, instead of being smoothed.

However, the presence of ice crusts proposed in Denux (1996) is a more straightforward explanation, and should be tested first.

It is also possible that the discrepancy come from the ‘isotopic diffusivity’ used by Johnsen et al. (2000), which oversimplify a series of processes into one single equation. Introducing temperature gradients would necessarily imply a rewriting of this equation which might be the occasion to make the model more detailed and accurate.

We will slightly modify our sentence to enlighten which explanation is the most likely:

I. 564: ‘For Denux (1996), the model of Johnsen (1977) should take into account the presence of ice crusts, **and maybe also** the temperature gradients in the **surface snow**, to get closer to the real attenuation at remote Antarctic sites.’

- I strongly suggest that you set up an experiment with Crocus that allow you compare as closely as possible the simulated attenuation with the calculated attenuation using the model of

Johnsen et al. 2000.

This was exactly the aim of section 3.3.1 where indeed, temperature gradient were removed. We have added the comparison of the attenuation from Johnsen model in the figure (see above).

- Section 4.2.1: I suggest to remove the detailed description of simulation of density at Dome C to a supplementary material as it influences the flow of the manuscript which should be focusing on the evolution of isotopes in the snow pack.

OK, this will be moved.

- L 604: You suggest that the higher diffusion at GRIP compared to Dome C could be explained by higher temperatures – but in line 260 you assume that the diffusivity is constant and not influenced by temperature.

This will be corrected in the revised version (see comments above on the dependency of the diffusivity on temperature and pressure).

- In general for all the figures you need to adjust the values for the color bar such that you don't have too many digits. For example in Figure 2 the color bar should go from -0.6 to 0.6 and in figure 3 it should be -1.9 to 0.8.

The limits are computed automatically as the maximum and minimum values of the variable over the first 60 layers. These values are then used in the text as a point of comparison between the different simulations. If we choose/ascribe the limits, this comparison will not be possible anymore.

- Figure S1: Why not combine panel b, c, and d

We are not sure what the reviewer expects here. We can of course remove the blank spaces. However, if the reviewer was meaning to use only one window, then we prefer not to make the modification. With just one window, we will not be able to show all the information, because of the differences in horizontal scales. Especially, the very small shift caused by compaction on panel (c) would not be visible anymore.

Minor comments

L14 “The isotopes . . . resolution” should not be in abstract

OK

L16 “condensation is realized” – what does this mean

This sentence means that the vapor density is brought back to its initial value by condensing excess vapor or sublimating snow. This step thus corresponds to solid/vapor exchanges, after vapor transport. We propose the following correction:

“2) kinetic fractionation is applied during transport, and 3) vapor is condensed or snow is sublimated to compensate deviation to vapor pressure at saturation.”

L21: “model underestimates” -> modeled attenuation due to diffusion is underestimated, or that other processes, such as ventilation influences attenuation

We have modified the text according to the reviewer suggestion.

L24-25: should be moved to conclusion

OK

L42: Randomness in the core stratigraphy -> stratigraphic noise

We have modified the text according to the reviewer suggestion.

L45: series of snow pits -> series of records from snowpits

OK

L53: ice microstructure at solid state -> snow grains due to solid diffusion

OK

L58-61: Cite Ebner et al. 2016 and 2017

OK

L87 Missing parenthesis after Brun et al. 2011

OK

L99: Quick survey-> brief overview

OK

L118: Wavelength of what?

It was the wavelength of the seasonally periodic isotopic signal. However, the text has been modified and wavelength no longer appear.

L178: What do you mean by “Permanent cycles”

We mean that the snow grain is never fully stable, and always undergoes sublimation and condensation at its borders. Depending on the balance of these two processes, its size may increase or decrease. When the two effects are balanced its size is constant. However, even in that case, its isotopic composition is still subject to evolution as sublimation and condensation are both active.

The term “cycle” does not convey our meaning correctly, as both processes are active at the same time. We propose the following correction:

L.193: “Indeed each grain experiences continuous recycling through sublimation/condensation”

L184: to get an -> to obtain an

OK

L185: Remove the content of the parenthesis.

OK

L224: What does this mean: “and taken to compensate yearly accumulation

Sorry for this complicated formulation. When we apply compaction we decrease the height of the firm column, while keeping its mass constant. Its total density is thus increased. We do this to make space for the deposition of a new snow layer at the top while keeping the surface level constant.

Using an accumulation at Dome C of 0.001 kg m^{-2} per 15 min, and considering that total snow column (over 12 meters) weights about 4461 kg, the compaction rate is: $2.2 \cdot 10^{-7}$ per 15 min. For a layer of 330 kg m^{-3} , the density increase is: $+7.4 \cdot 10^{-5} \text{ kg m}^{-3}$ per 15 min. Per year, the total accumulation would be 35 kg m^{-2} and the density change, for the selected layer would be $+2.59 \text{ kg m}^{-3}$.

L240: What about the influence of absorption of radiation energy in layers below the surface layer?

It increases the heat of the layer, and therefore its temperature.

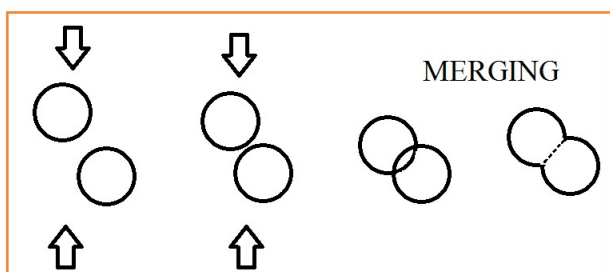
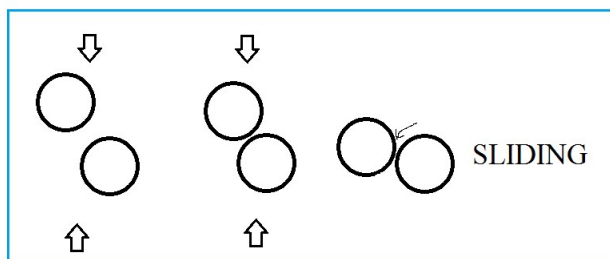
L254: “Interface”: Please be more precise on defining what interface you are referring to

We have added a sentence to define the interface between two layers.

I. 262: ‘In this section, the term ‘interface’ is used for the horizontal surface of exchange between two consecutive layers. The flux of vapor at the interface between two layers is obtained using the Fick’s law of diffusion (Eq. (4)):

L258: “interpenetrate”: What do you mean?

When two grains are strongly pressed one against the other, the boundary between them becomes flat, and the two grains are merged together to make only one grain. ‘Interpenetration’ is the step when their limits cross each other during the merging. If the pressure is not strong enough, the shape of the grain is not modified; they slide one upon another without merging.



L296: “that are” -> being

OK

L304: Have you defined kinetic fractionation previously?

No. We have added a sentence to define kinetic fractionation in the Introduction.

I. 151: 'It becomes the main process of vapor transport when air is stagnant in the porosity.

During diffusion, lighter molecules move more quickly in the porosity, leading to kinetic fractionation of the various isotopologues (Barkan and Luz, 2007).'

EQ 12: typo in D_{eff_n}

Thanks, we have replaced the notation $D_{eff,n \& n+1}$ by the symbol used before $D_{eff}(t, n \rightarrow n + 1)$, in order to keep homogeneous notations.

L486: "Amplitude decrease by -1.3 o/oo" – do you mean amplitude increase by 1.3 o/oo

No, we mean decrease (the amplitude is reduced because of attenuation).

We have corrected the text:

I. 515: 'Over 10 years (2000-2009), the amplitude decreases **by 1.3 ‰** which corresponds to a 8 % variation.'

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2017-217>, 2017.

Interactive comment on “Numerical experiments on isotopic diffusion in polar snow and firn using a multi-layer energy balance model” by Alexandra Touzeau et al. Anonymous Referee #2 Received and published: 1 December 2017

The post-depositional modification is an important but poorly understood part of the "isotopic paleo-thermometer". After the solid precipitation is deposited on the top of the polar ice sheet snow surface, its isotopic content is changed drastically due to the water and mass exchange with the atmospheric water vapor and due to molecular diffusion in snow. These processes disturb or even completely erase the initial climatic signal recorded in the isotopic content of the precipitation. To solve this problem, different approaches are applied including modeling of the snow pack evolution during snow metamorphism. This manuscript is an attempt to simulate the snow isotopic content of the polar snow in the course of the post-depositional processes. For the first time the snow-pack Crocus model is applied for this purpose. The authors clearly understand that this work is a small step towards the full description of the isotopic post-depositional modifications. A lot of efforts still has to be done. However, this attempt deserves to be published as a separate paper in "Geoscientific Model Development" journal.

The manuscript is nicely structured and provides a good review of literature on the formation of the climatic signal in the snow isotopic composition. The authors make a nice attempt to describe in simple way a rather complicated process of the isotopic modifications in the snow thickness. I do not have major corrections, only a few minor comments or questions:

In your model you do not take into account the mechanical snow mixing by wind. This mixing erases the initial climatic signal (shorter than few years) in central Antarctica, and makes the vertical isotopic profiles in the upper part of snow thickness similar to white noise. Recent study by Thomas Laepple (<https://www.the-cryospherediscuss.net/tc-2017-199/>) showed that the filtering of this noise by isotopic diffusion can create false cycles in the isotopic profiles. So, I suggest that in the further versions of your model you introduce random component of the initial isotopic composition of the precipitation (or of the upper snow layer if you wish) in parallel to the regular component given by precipitation events. You might mention this in section 4.4. and conclusion.

We agree with the reviewer that wind mixing should be included in the model somehow as it is an important process in Antarctica.

Libois et al. (2014) already paved the way to do it. They proposed to run the Crocus model in parallel (50 snow columns for the same site) and to exchange snow between these columns. This method is called 'stochastic snow redistribution scheme'. However, because the vapor transport scheme is run at a 1 s time-step, it is slowing down the model. Thus, running 50 simulations in parallel might be very time-consuming in our case.

Other solutions could be proposed, such as taking the first centimeters of snow away (wind ablation), store it temporarily in an atmospheric reservoir and letting it fall again. If several layers are

eroded, they could fall down in a different order, or maybe be mixed together while still in the atmosphere. We will also consider the reviewer suggestion, to add a random component to the signal in precipitation. This could be the simplest way to simulate this process.

We have modified the text in two places to stress the importance of this process to be taken into account in future work:

I. 689: 'The next step for Crocus-iso development is thus to implement ventilation. **Third, we are also aware that in Antarctic central regions, the wind reworking of the snow has a strong effect in shaping the isotopic signal. A combination of stratigraphic noise and diffusion could indeed be responsible for creating isotopic cycles of non-climatic origin in the firn (Laepplé et al., 2017). Wind reworking may also contribute to attenuation, by mixing together several layers deposited during different seasons.'**

L. 722: 'Second, in low accumulation sites like Dome C, wind scouring has probably an important effect on the evolution of the $\delta^{18}\text{O}$ signal in depth through a reworking of the top snow layers (Libois et al., 2014). This effect has not been considered here **but could be implemented in the model in the next years.'**

Other minor comments:

line 33 - better to write "1950s"

OK

lines 352-353 - why condensation is without additional fractionation?

See above (same comment by the first reviewer).

lines 502-503: the values (86% and 90%) are the remaining amplitudes, right?

Yes, we made a mistake. After the correction, the new sentence is:

I. 536: **The 2.5 m attenuation is slightly higher at GRIP (remaining amplitude: 86 %) than at NEEM (remaining amplitude: 90 %, Fig. 4):**

section 4.2.3: how much snowfall have you added to the snow thickness in this simulation?

The total cumulated precipitation was 37 kg/m² for year 2001 (11 cm of fresh snow, 4 cm i.e.). In average for the period 2000-2010, the annual total of precipitation was 29 kg/m²/year. 2001 is the year with the highest accumulation.

lines 582-583: this gives 10 cm / year, but above you said that the accumulation rate at DC is 8 cm / year (snow equiv.).

Sorry for this discrepancy. The '8 cm' value comes from glaciological analysis, and corresponds to long time scales, whereas the '10 cm' value is the one measured for recent years (see Landais et al., 2017). Our forcing data has an accumulation of 29 kg/year, corresponding to 9.6 cm of fresh snow, and therefore coherent with measured accumulation for the last 10 years.

We will modify the text to remove this ambiguity.

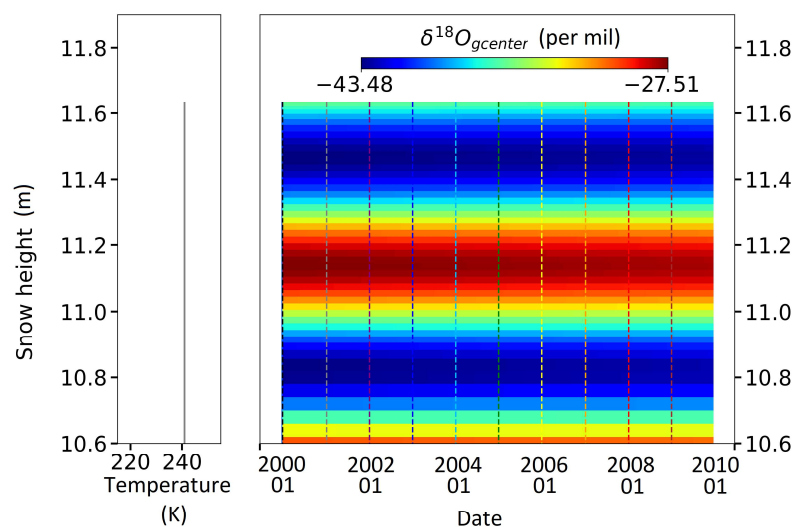
I. 432: 'About **10 cm of fresh snow** are deposited every year (Genthon et al., 2016; Landais et al.,

2017), which implies that in order to keep seasonal information, at least one point every 4 cm is required in the first meter...'

Figure 2d is a bit misleading. From the first glance a reader may think that the seasonal amplitude is increasing with time. Then, it becomes clear that it is actually $\delta^{18}\text{O}$ change that is increasing with time. It would be nicer to show here the $\delta^{18}\text{O}$ values themselves (instead of $\delta^{18}\text{O}$ changes), so that the colors would nicely illustrate the fading isotopic variability. The same comment is for Figures 3 and 12.

We are aware that our figure is not easy to understand at first glance and we apologize for that.

We hesitated between this 'difference' figure and the original one with the true values of $\delta^{18}\text{O}$ (see below). However, the attenuation is not much easier to see in the original figure (shown below).



An attenuation by 0.5‰ over a half-amplitude of 8 ‰ is barely visible for the maxima (dark red becoming lighter) and no at all for the minima (shades of blue difficult to distinguish).

We therefore prefer to keep the 'difference' figure in the manuscript. Moreover, since the caption clearly states that we are plotting the deviation to the original profile, we do not see our figure as misleading.

line 1055: December 2001.

OK. Thanks.

Interactive comment on Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2017-217>, 2017.

Interactive comment on “Numerical experiments on isotopic diffusion in polar snow and firn using a multi-layer energy balance model” by Alexandra Touzeau et al. Anonymous Referee #3 Received and published: 4 December 2017

1 Overview

Firn isotope diffusion is a process that affects the $\delta^{18}\text{O}$ signal of polar snow from the time of deposition until pore close-off. Taking place in the vapor phase within the porous medium of the firn and driven by the apparent seasonal, annual and multiannual isotopic gradients it results in an attenuation of the $\delta^{18}\text{O}$ signal, often obliterating its annual component.

Assuming a good estimate of the diffusive rates in firn is obtained, a “reverse calculation” of diffusion can be possible that allows the (almost) complete reconstruction of the initial signal. Additionally, knowledge of the diffusive rates offers valuable information on past firn temperatures and as a result can be used as a paleothermometry tool if ice core data of sufficient resolution and precision are available.

Previous studies have looked into the description and characterisation of these effects and part of these studies suggests that post depositional processes different to purely fickian diffusion of water isotopes can also be at play acting supplementary to the signal attenuation affects or even introducing biases (Town et al., 2008). These processes are mostly of advective nature caused by the bulk movement of air and vapor in the snow, driven by pressure and temperature variations.

In this work titled “Numerical Experiments on isotopic diffusion in polar snow and firn using a multi-layer balance model”, Touzeau et al attempt to build and test a water isotope module on top of the Crocus snowpack model. In particular, the authors focus on trying to simulate post-depositional effects that cause changes of the initial $\delta^{18}\text{O}$ signal in polar snow and firn. Processes related to snow/firn isotope diffusion as well as diffusive vapor transport due to temperature gradients in the firn are modelled assuming various scenarios. The study focuses on two different regimes that are representative of conditions typical for deep ice coring sites on Greenland and East Antarctica. Ice core data sets are also used in order to evaluate the performance of the model and the results are also compared to existing firn isotope diffusion modelling approaches. This is a very welcome contribution and it most certainly points to the correct direction with respect to future modelling efforts. The study also fits very well the description and scope of the GMD journal and the overall quality of the research conducted is of high level. Thus I would recommend it for publication in GMD after the following points are carefully considered by the authors.

2 General comments

1. Unfortunately the language of the manuscript requires a significant revision. In particular there are examples in the text where technical/physical terms are used wrongly and many definitions appear to be loose. This is particularly problematic for a manuscript of this type, where

modelling approaches and physical processes are described. The most notable example is the description of the transport mechanisms in snow in sections 2.1.1 and 2.1.2. Diffusion is a very well defined process and unfortunately the term is used falsely several times in sections 2.1.1 and 2.1.2 (and elsewhere in the manuscript). After reading these two sections I feel confused about the meaning of many of the terms used here and as a result about the kind of methods followed and the assumptions made in this study.

In this paper, we distinguish 3 types of diffusion:

- solid diffusion (limited effect in the first meters of snow, because it is very slow);
- diffusion in vapor phase caused by isotopic gradients (gradients present originally in the solid phase, but transmitted to the vapor);
- diffusion in vapor phase caused by temperature gradients (which produce vapor pressure gradients in the porosity).

The first two processes are not **oriented** (=externally forced), they result from random movement of molecules in vapor phase or of ions in solid phase. The last process is **oriented** (=externally forced) as it is forced by an external variable which is the atmospheric temperature. While the two first types of diffusion can only attenuate the original signal, the last type can add 'noise' to the original signal. When diffusion due to temperature gradients is active, original information is not only damped, but even replaced. Thus contrary to the first two processes, it will not be possible to 'reverse' the phenomenon in that case.

Thus we class the 'solid diffusion' and 'diffusion caused by isotopic gradients' within the category of 'processes' leading **only to signal smoothing** in an homogeneous way (Section 2.1.1). And we class 'diffusion caused by temperature gradients' as a process leading to **oriented vapor transport** (Section 2.1.2.), and therefore much more difficult to deconvolute. In this Section 2.1.2., we present other processes also leading to **oriented vapor transport** such as convection and ventilation. Note that we have not included convective processes (convection, and advection due to the vertical gradient of air pressure) in the model yet. Only diffusive processes are present.

The processes we describe are the same as described by others, and the only novelty here is in the way we split the processes. The classical way to do it is to separate convective processes (ventilation and convection) from diffusive processes (the three types of diffusion described). Here, we prefer to split them based on their effect on the original isotopic signal. Is it possible to deconvolute the signal stored in ice cores, because only smoothing is active? Or is the isotopic composition modified more strongly, in particular through the accumulation of heavy isotopes in a specific layer?

We are aware that this splitting may surprise people, as it is not based on the physics of the process, but on its effects. However, it has interest for people who are working at the interface between physical description of processes and interpretation of 'noisy' geochemical data. Moreover, we did not invent this splitting. It was first proposed by Ekaykin et al. in 2009.

In order to help the reader to follow our line of thinking, we have modified the introduction to this section 2.1:

I. 107: 'Several studies address the evolution of the isotopic compositions in the snow column after deposition. Here we describe first processes leading only to attenuation of the original amplitude (Sect. 2.1.1.). Then we describe processes which lead to other types of signal modifications (Sect. 2.1.2.). These modifications include transporting and accumulating isotopes in some layers without

consideration of the original isotopic signal. They also comprise processes taking isotopes away from the snow, and therefore shifting the mean $\delta^{18}\text{O}$ value of the snow deposited.'

Also in relation with a comment made by reviewer 1, we have replaced the term 'oriented processes', which was too vague, by the more precise expression '**processes of oriented vapor transport**' all other the section.

And we define these 'processes of oriented vapor transport' at the beginning of Section 2.1.2:

I. 134: 'We consider here the **oriented movement** of water molecules forced by external variables such as temperature or pressure. We use the term '**oriented**' here to describe an overall movement of water molecules that is different from their molecular agitation, and externally forced.'

What is an "oriented process" for example? In page7line73 the sentence "We focus on the impact of oriented vapor transport caused by vapor density gradients in the snow..." is very untechnical and unfortunately creates a lot of confusion about what the authors have done.

'Oriented process': See above.

Line 73: The sentence was modified based on this comment as well as on the first reviewer comment. I. 73: 'We focus on the impact of **thermally induced** vapor transport caused by **vapor pressure gradients** in the snow and recrystallization (i.e. 'dry metamorphism') and of diffusion against isotopic gradients.'

Here we focus on two out of the three diffusion processes presented above:

- a) the diffusion in vapor phase *caused by isotopic gradients*;
- b) and the diffusion in vapor phase *caused by temperature gradients*.

If the term "vapor density" indeed refers to "vapor (molar?) concentration" as I assume then the process described here is a vapor diffusion process.

It is not a molar concentration, but a **massic concentration (its unit is kg/m^3)**. And yes, the concentration gradients drive the diffusion in our model (Fick's law).

We agree that this term leads to confusion. The first reviewer suggested to replace this term by '**vapor pressure**' which is more commonly used. However, this is not coherent with our unit, and therefore not applicable for when we are describing the symbols in equations. Using the term '**concentration**' is probably a better option in these cases.

Therefore, we have replaced 'vapor density gradients' by '**vapor concentration gradients**' as much as possible in the paper. For equations, we have also used the term '**massic concentration**' which is more accurate.

After having read the text several times and tried to infer what the authors try to describe in sections 2.1.1 and 2.1.2 I conclude that they split the processes under consideration in two kinds.

Thanks for trying to understand this section, despite our particular way of splitting processes. We apologize for not being clear enough. We hope that after adjustments, this section of the manuscript will be easier to follow for the reader.

○ The first, what they call “signal attenuation on a vertical profile”, is a the combination of two processes, (a) solid isotope diffusion and (b) firn isotope diffusion in the vapor phase.

(a) The first is extremely slow and can easily be neglected in this study. I find it important that the authors point out in the text that solid diffusion affects all isotopes equally.

We have added a sentence in the text as a reminder:

l. 133: ‘**Note that in the solid phase, all isotopes have the same diffusion coefficient.**’

(b) The second is a diffusive process taking place in the porous medium of the firn driven by the isotopic gradients.

Both processes introduced here follow the same physical principle ie transport of mass due to concentration gradients of a substance. The transport occurs along (or down) the concentration gradients and not “against” as often described in the text.

As suggested, we have replaced ‘against’ by ‘along’ in the manuscript.

○ The second category of processes outlined in section 2.1.2 and termed as “oriented processes”. My interpretation of the text is that this type processes are “bulk motion” processes either due to pressure or temperature gradients.

The reviewer is right. The processes described in this second section indeed correspond to an overall movement of vapor molecules resulting from temperature or pressure gradients. Thus, they are not limited to ventilation and convection.

The first case is a typical example of advective transport.

NB: The first case (l. 145) was wind-pumping, which we agree to be convective by nature.

The second is a bit more complicated however the term diffusion used by the authors is incomplete. Temperature gradients in the snow will eventually cause vapor concentration gradients. The latter, will drive a diffusion process for the vapor as a whole. However this cannot be seen as an isotope diffusion process due to the fact that the diffusive transport of vapor has nothing to do with isotopic gradients. Eventually of course the diffusive transport of water vapor will very likely bring vapor molecules in layers of the snow with different isotopic composition where subsequently an isotope diffusion process will occur locally.

NB2: The second case (l. 147) was thermally induced vapor diffusion.

There has never been any confusion in our mind between diffusion driven by isotopic gradients and diffusion driven by temperature gradients. Indeed, we have separated strictly diffusion in vapor phase caused **by isotopic gradients** in the first section, from **thermal diffusion of vapor** in the second section. We agree with the reviewer that the second one affects isotopes only indirectly.

Although thermal induced diffusion is certainly a diffusion process, it is indeed a shortcut to call it an ‘isotope diffusion’ process, and a better expression would be ‘vapor diffusion process **with** consequences for isotopes’.

We will modify instances in the text where ‘isotope diffusion’ or ‘isotopic diffusion’ were appearing. We will clarify every time if we were talking about *diffusion along isotopic gradients* or about *thermally induced diffusion of vapor... and its consequences on isotopes*.

L. 1: 'Numerical experiments on **vapor diffusion** in polar snow and firn **and its impact on isotopes** using a multi-layer energy balance model'

L. 19: 'We also run complete simulations of **vapor diffusion along isotopic gradients** and **of vapor diffusion driven by temperature gradients** at GRIP, Greenland and at Dome C, Antarctica over'

L. 356: '... the total flux of vapor is null. But we still have **diffusion along isotopic gradients** (Eq. (13)), as long as they are non-zero.'

L. 715: 'The main process implemented here to explain post-deposition isotopic variations is diffusion. We have implemented **two types of diffusion in vapor phase: 1) water vapor diffusion along isotopic gradients, and 2) thermally induced vapor diffusion. The vapor diffusion between layers was realized** at the centimetric scale. **The consequences of the two vapor diffusion processes on isotopes in the solid phase were investigated. The solid phase was modelled as snow grains divided in two sub-compartments: a grain surface sub-compartment in equilibrium with interstitial water vapor and an inner grain only exchanging slowly with the surface compartment. We parameterized the'**

This was only an example of how the loose use of technical terms and faulty language creates unnecessary confusion to the reader already from the introduction, leading possibly to confusion and misunderstandings of the methods and principles used in this study. I find it essential that the authors look into the manuscript carefully and revise the text accordingly. In the "Specific Comments" section I include more of these examples as they appear in the text.

We have made our best to modify the terms in order to make the sentences and our meaning clearer. We will check the use of the term 'diffusion' and remove the term 'isotope diffusion' as stated above.

2. There is an unclear situation regarding the vapor diffusivity parametrization and value used in this study. It is not exactly clear if there is a temperature dependency of the effective diffusivity D_{eff} to temperature. Based on equation 5 in the manuscript and the comment on the value of D_v I conclude that the value of D_{eff} is taken constant and reflects a temperature of 263 K. If this is indeed the case I would be inclined to question the validity of many of the statements found in the manuscript that concern the comparison of this model with other models of diffusion or results from ice core data. The diffusivity coefficient is heavily dependent on temperature and thus a constant value is an oversimplification for such a study. I would strongly prefer a version of the manuscript where the diffusivity is allowed to depend on temperature. However, if the authors indeed choose to follow the approach of constant diffusivity they will need to stress out very clearly in the manuscript that the comparisons presented here are essentially between different things. This should be even more prominent for the case of the Dome C modelling experiments due to the very large difference between the site temperature and the temperature used for the diffusivity coefficient value (almost 40K).

Indeed, this has to be addressed, see answer to this question in the first review.

3. Despite my belief that the work performed by the authors is of high quality I need to point out that several elements of the manuscript feel opaque not allowing the reader to judge for herself on the quality of the work and the significance of the results. I find this a fundamental weakness of the manuscript that needs to be addressed. In particular:

• The authors claim that the model is evaluated for the top 10 m of snow. However only the top 50-60 cm are presented.

We do not present results downwards simply because the layers are too thick (~20 cm). Therefore no seasonal pattern is visible in these layers and the attenuation by diffusion is impossible to evaluate. But we can easily print a window of the whole snowpack for those interested.

• The authors do not provide any information about neither the ice core data used nor the method used to calculate peak-amplitudes. The latter is not a straight forward procedure and can have a significant influence on the result of such model-data comparisons for diffusion. Information about the depth interval the data originate from, the temperature, accumulation and pressure conditions of the sites as well as the resolution of the data are pieces of information a thorough reader needs to have access to. Present the ice core data.

We are particularly sensitive to this remark, as we encountered exactly the same problem while looking at previous publications where these ice core data were published and used for diffusion study. While the attenuation (%) was given, we were unable to find the original signal nor the methodology used to calculate the attenuation.

We propose to give in the appendix of the revised manuscript the methodology that we followed to compute the attenuation.

1) We define 'half-amplitude' as: $\text{abs}(\delta^{18}\text{O} - \text{mean}(\delta^{18}\text{O}))$. Thus, we first compute the mean $\delta^{18}\text{O}$ in the core, and then for each depth compute the absolute difference to the mean. Following a suggestion by Reviewer 3, we will replace 'half-amplitude' by the more common term 'semi-amplitude' in the manuscript.

2) We then look for maximas in this series of half-amplitudes. In the first version of the manuscript, we used 20-cm windows at this step. However, this is not well adapted for NEEM. In the revised version we will present results obtained with a 30-cm window for the first 10 meters of the core. Indeed, in this shallow part of the firn the density is about 400 kg m^{-3} . Using accumulation rates of 0.23 m i.e. at GRIP, and 0.22 m i.e. at NEEM, the expected length for the cycles is 52 and 55 cm respectively. Since we are looking at half-cycles, a window of 30 cm should allow to get all the maximas present in the record. Deeper in the firn we will use **a window of only 20 cm** coherent with higher snow density downward.

This has been clarified in the text:

l. 528: 'For NEEM the values of the four cores are taken together. For NEEM and GRIP, the **semi-amplitude** is computed along the core. **In the first 10 meters, the maximum value every 30 cm is retained, and deeper in the firn, the maximum value every 20 cm is retained (Fig. 4).** Maximum'

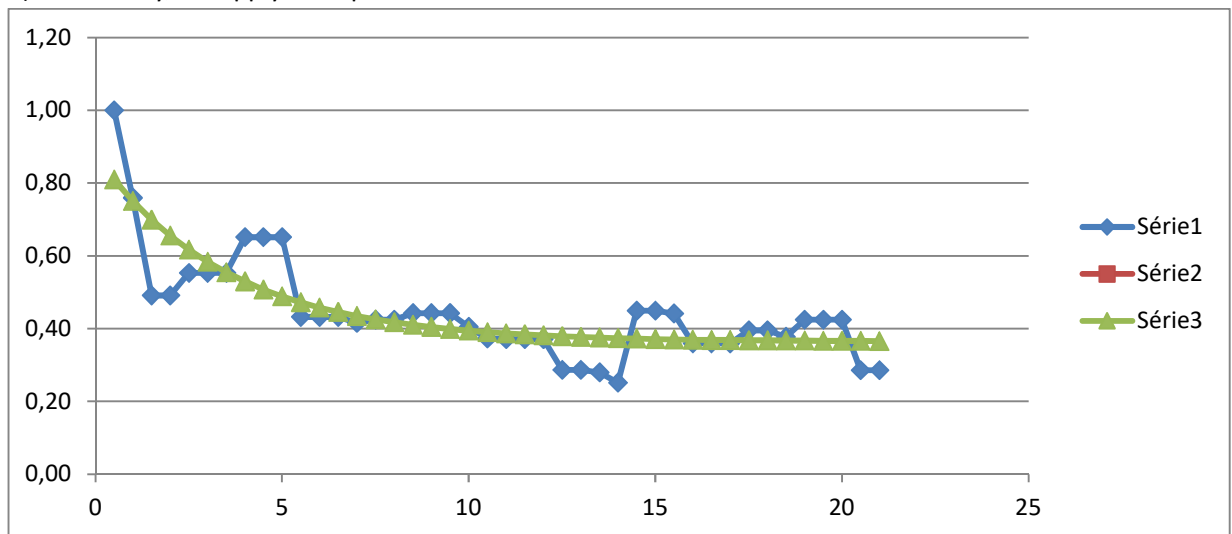
3) Then over this series of maxima, we keep the maximum value for **each meter of the core** (see Figure below). We use this larger window for the fitting, because we prefer to evaluate attenuation based on the larger (well-defined) maxima. We use the value obtained in the first meter as our '**initial half-amplitude**'. All other maximas are expressed relative to this first meter "maxima of maximas", even if the maximas downward happened to be larger.

We add two sentences in the text to clarify this step:

l. 528: '**From this first series of maxima, a second series of maxima is computed, with a larger window of 1 meter. The 'remaining amplitude' is then defined as the ratio between any of these 1-meter maxima and the initial 1-meter maxima.** Maximum semi-amplitudes every 5 m are also computed

and displayed on Figure 4.'

4) Lastly, we apply an exponential fit to these values:



- The initial $\delta^{18}\text{O}$ profile as well as snapshots of some layers should be plotted. The difference plots with the plethora of colors do not add anything neither for the case of density nor for the case of $\delta^{18}\text{O}$. The colormaps of these plots are unfortunately very ambiguous to read and despite having the max and min values it looks to me that some of these colormaps are non linear. In combination with the very small difference values for both the density and the $\delta^{18}\text{O}$ these color plots leave me guessing. There is very little valuable information I can extract from them.

The omni-present 2D colored graph is the most common way to present Crocus outputs. The script to make this graph is indeed delivered with the model.

However, we are aware of its limitations, which is why we decided to add **on the side of most figures** a series of 1D profiles, that are often more explicit than the traditional 2D Figure. We plot in particular **the original profile of $\delta^{18}\text{O}$ on Figure 2, 3 and 11**. We also plot the deviation to the original **as 1D profiles (depth) on Figure 2 and 3 and as 1D (time) profiles on Figure 11** for a selection of layers.

Although these 1D profiles are often more explicit, we also keep the traditional 2D figure in the article, because people might be interested to see the whole picture and not only what we select.

For instance, we hesitated to insert a 2D figure of temperature evolution when submitting this manuscript. We decided against, as we had already a lot of figures to display. This was an error: because we extracted only summer vertical profiles, the first reviewer asked for profiles in winter which were not apparent. So even if they are difficult to read, the 2D plots remain necessary.

Regarding the color bar, the reviewer is right in thinking that it is non-linear. We have two different (linear) scales for positive and negative change. Positive change is in level of red and negative change in levels of blue. Thus, when negative change is ten times smaller than positive change, it is still visible in the graph. Of course, white color corresponds to zero change. This color convention (red-white-blue, two linear scales both sides of zero) is used everywhere in the paper (except for Figure 11, which will be modified).

I. 449: 'The white color corresponds to an absence of change of the variable.'

We hope our explanation will make the figure easier to understand.

- The study considers all three isotopologues of water ($\delta^{18}\text{O}$, $\delta^{17}\text{O}$ and δD) however the authors choose to present only the results for $\delta^{18}\text{O}$. Based on (Johnsen et al., 2000) the diffusive attenuation is expected to be stronger for $\delta^{18}\text{O}$ compared to δD . Can the model produce this differential signal? This is a very simple test.

This is a good question. We did not look at the attenuation of δD signal simply because this variable never appear (we use $\delta^{18}\text{O}$, dex and ^{17}Oex). However δD can of course be deduced from the other parameters.

For winter 2000, the slope for δD is $-5.64\text{‰}/10\text{years}$. Divided by 8, this would correspond to 0.71‰ attenuation per 10 years in $\delta^{18}\text{O}$ which is indeed less than the value of $0.82\text{‰}/10\text{years}$ obtained for the $\delta^{18}\text{O}$ slope. We thus indeed find that attenuation is larger for $\delta^{18}\text{O}$ than for δD and will insert this remark in the new version.

We propose the following modification:

L. 617: "For the layer deposited during winter 2000, there is an increase in $\delta^{18}\text{O}$ values of about $+0.8\text{‰}$ over ten years. The slope is irregular, with the strongest increases occurring during summers (Nov.-Feb.) when vapor transport is maximal. The slope is also stronger when the layer is still close to the surface, probably because of the stronger temperature gradients in the first centimeters of snow (Fig. 11a; Sect. 4.3.1.). **The slope for δD , over the same period, is $+5.6\text{‰}$. If we bring this value to a $\delta^{18}\text{O}$ scale we get an attenuation of only 0.71‰ , so δD is apparently less sensitive than $\delta^{18}\text{O}$ to attenuation as indicated by Johnsen et al. (2000).** For the layers deposited during the summers, the evolution of $\delta^{18}\text{O}$ values is symmetric to the one observed for winter 2000."

4. The discussion about the comparison with GRIP data feels incomplete and not thorough. The actual data set is never shown in the manuscript while there is very little information about how diffusion is estimated for this data set. Measuring peak-to-peak amplitudes on ice core $\delta^{18}\text{O}$ data can be very misleading as the initial $\delta^{18}\text{O}$ value is unknown and most likely it has been variable through the time.

We have already answered above about our methods. We are acutely aware of the lack of a reliable method to estimate attenuation from an ice core dataset.

One technically correct way to estimate diffusion on data is to look into the spectral domain and estimate diffusion length values. Either way the reader had practically no access to information about how diffusion is estimated from the GRIP data.

Looking at the spectral domain was not possible in our case because the longest simulation was run for 10 years and this is much too short for this approach. For such short record, looking at the spectral domain (and at the diffusion length) would be efficient only if the ice record was perfect (no wind, no stratigraphic noise). **Then (in that case only)** we can expect periodicity and amplitude to decrease jointly in a predictable way.

Additionally, it should be noted that the GRIP data set, originating from a certain depth interval in the ice core (that is not given in the manuscript) it may have experienced a combination of

temperature and accumulation different from the modern one. Does the comparison presented here take this into account?

We are presenting a figure (3) that goes **down to 80 meters from the surface**. Thus, the core considered comes from **a depth interval that is 0 to 80 meters**, hence directly comparable to modern conditions. We will precise it in the text since it appears to be confusing.

We have added two sentences to clarify this point:

I. 520-521: in Masson-Delmotte et al., 2015, and at GRIP using one shallow core (1989-S1), published in White et al., 1997. **For the GRIP core, only the first 80 meters are considered. Therefore, the data presented corresponds to deposition and densification conditions similar to the modern ones.**

In particular if the CROCUS model only uses a fixed diffusivity value for 263 K, there is no doubt that there will be a discrepancy with the data deduced diffusion. These are very important elements of such a study and are notably absent in the manuscript.

We made a mistake in keeping the diffusivity constant with temperature. However, as indicated above, the direct effect of temperature (molecular agitation) is much less than its indirect effect through vapor concentration and vapor concentration gradients.

When using the formula provided by Johnsen et al. for temperature control on diffusivity, we find that the main difference compared to diffusivity in our simulation **comes not** from the temperature component but from the **atmospheric pressure** component. Indeed, we used air diffusivity at 263 K and 1 atm before and therefore just adding the pressure component (1/0.650) almost double the diffusivity because air is less dense and molecules have more space to move. Again compared to this effect, the temperature effect is limited.

5. Plots and captions need to be reworked. There are several stylistic inconsistencies that should not be allowed for a publication of this quality. A mixture of different font types, missing measurement units from axes, different approaches in presenting measurement units (using either parentheses or a / sign) and a % sign presented in two ways. I think that many of the captions are too long while in the same time they miss one important piece of information that is the number of the experiment and maybe the ice core site under consideration. I do not think it is the job of a reviewer to go through every single detail and problem with the plots thus I will trust that the authors are certainly able to carefully go through the presented plots and make the necessary changes.

We will take these remarks into consideration for improving the figures in the revised version of the manuscript.

6. Regarding the references given, I think that for the introduction section there is probably an overwhelming number of works cited and a small clean-up is possible. More importantly though, some of the works cited are not peer reviewed belonging to the "Discussions" versions of some of the Copenicus publications journals. I believe that the authors should consider these cases and preferably either omit them or update their references list in case some of the papers in question have reached a post peer-review status.

This will be taken into account in the revised version.

3 Specific comments

Here some more specific comments for the authors.

P2L45 "and then only stacking...". As one looks in higher depths in a core this is less of an issue. Yes.

P3L57 Make sure the reader understands this is vapor-solid exchange in the porous medium of the firn.

We have added a precision.

I. 54: Second, within the porosity, the vapor isotopic composition can change due to diffusion **along** isotopic gradients (in gaseous state), **thermally induced** vapor transport caused by **vapor pressure gradients**, ventilation (also in gaseous state), or exchanges between the gas phase and the solid phase (sublimation, condensation). **In the porosity**, the combination of diffusion **along** isotopic gradients in the vapor and of exchange between vapor and the solid phase has been suggested to be the main explanation to the smoothing of the isotopic signal in the solid phase (Johnsen et al., 2000; Gkinis et al., 2014; Ebner et al., 2016, 2017).

P3L67 Diffusion length mentioned here but no definition given.

In Johnsen et al. (1977) the diffusion length is defined as the **mean displacement of a water molecule during its time of presence in the porosity**. More precisely:

$$(\Delta L_f)^2 = 2 D \tau_v \left(\frac{2}{\pi}\right)^2$$

With D the diffusivity of water molecules, τ_v , the residence time of vapor in the porosity and ΔL_f the diffusion length.

We have added this definition in the text.

L. 66: **have been able to simulate and deconvolute the influence of diffusion along isotopic gradients in the vapor at GRIP and NGRIP using a numerical model. To do this, they define a quantity named 'diffusion length' which is the mean displacement of a water molecule during its residence time in the porosity. Using a deformation model and an equation of diffusivity of the water isotopes in snow, they compute this diffusion length as a function of depth. It is then used to compute the attenuation ratio (A/Ao), and in the end retrieve the original amplitude (Ao).**

P3L74 "...and of diffusion against isotopic gradients" Is this vapor firn diffusion or solid?

We are focusing on vapor diffusion, because solid diffusion plays only a minor role.

We propose the following modification to the manuscript:

I. 73: **'We focus on the impact of thermally induced vapor transport caused by vapor pressure gradients in the snow and recrystallization (i.e. 'dry metamorphism') and of diffusion along isotopic gradients in the vapor phase. Because the'**

P5L117 For an informative plot on the matter see Gkinis et al 2014. Higher accumulation rates also result in increased densification rates and therefore reduced diffusivities.

Thanks for this complement of information. We will add it to the text:

l.126: 'high accumulation rates ensure a greater separation between seasonal $\delta^{18}\text{O}$ peaks (Ekaykin et al., 2009; Johnsen et al., 1977) thereby limiting the impact of diffusion. **They also result in increased densification rates, and therefore reduced diffusivities (Gkinis et al., 2014).** Because sites with high accumulation'

P5L120 Diffusion indeed takes place in the ice column but with rates orders of magnitude lower than that of firn. You want to be more specific about it in the text as you often mix the terms vapor and solid diffusion without being specific about the process taking place in the porous of the firn or in the solid ice.

Again, our aim here was to separate "smoothing" processes from "building/shifting" processes. Diffusion **along isotopic gradients**, in the vapor phase **AND** in the solid phase will only lead to smoothing. This is why we do not distinguish between the two processes in this early section of the manuscript: they have the same effect.

But we are aware that they act at different time scales and at different depths. We are also aware that the solid diffusion is much slower (as indicated in section 2.2.1.).

We will modify the last sentence to clarify our meaning:

l. 132: 'Diffusion along isotopic gradients exists throughout the entire snow/ice column. **It occurs mainly in the vapor phase in the firn, especially in the upper layers with larger porosities. After pore closure, it takes place mostly in the solid phase, at a much slower rate.**'

P6L143 It would be helpful to add even one sentence where you explain why and how the spherical ice elements approach is too simplistic (is it?).

Approximating snow microstructure by a monodisperse collection of spherical ice elements has been carried out in several studies in the past (Legagneux and Domine, 2005, Flanner and Zender, 2006). This makes it possible to perform explicit calculations, for a medium featuring the same surface area/volume ratio, without accounting for the complex microstructure of snow. Several limitations arise, related to the requirement to better account for the full distribution of curvature of the ice/air interface, which is critical for snow metamorphism (Flin and Brzoska, 2008). Furthermore, the ice sphere geometry modifies the distribution of ice chord distances, i.e. the mean ice path which is relevant for ice diffusion. Such effects would better be accounted for using a more comprehensive description of the snow microstructure, although the level of complexity would make it untractable using the current generation of multilayer snowpack models.

P7L161 "...the transfer of molecules from the grain boundary towards the center of the grain is very slow" Solid diffusion at the temperatures we are talking about is indeed slow. However this sentence gives a false impression that there is a 1-way motion from boundary to center. This is wrong for two reasons. Firstly, any diffusion process would not result in a 1-way motion of molecules. Secondly and more important, solid diffusion in ice seems to be a self-diffusion process following a vacancy mechanism. This means that there is no isotope effect and diffusion affects all isotopologues equally or in other words molecular transport does not take place along and due to the isotopic gradients in ice (therefore there is no index denoting isotopic species in Eq. 1 - ice diffusivity concerns water molecules in the solid phase regardless of their isotopic composition). As a result the model

used here of an isotopically heterogeneous material with internal and external layers does not cause any isotope diffusion in the solid phase due to the radial gradients. In a perfectly homogeneous material you should be expecting the same magnitude of diffusive mixing in the solid phase as in the heterogeneous material assumed in the text. It would be good to correct these errors in section 2.2.2 and clarify the presence of the self-diffusion mechanism. The calculations of characteristic times in this section look correct and are relevant though. Just make sure that you clearly explain that this characteristic time concerns not only movement of the different isotopologues along a specific path (surface to center of grain) but of ALL water molecules towards all directions in the grain and across the grains.

We are sorry for this mistake and will correct the text.

l. 179: 'The grain center isotopic composition may change either as a result of crystal growth/sublimation or as a result of solid diffusion within the grain. **For solid diffusion, water molecules move in the crystal lattice through a vacancy mechanism, in a process of self-diffusion that has no particular direction, and that is very slow.** The diffusivity of water molecules in solid ice...'

l. 188: 'Therefore the solid diffusion ~~between the surface of the grain and the inner part of~~ **within** the grain, at the time'

section 3.1.1 I was wondering if it would be possible to outline the components of the Crocus model in a summarising table and shorten this section significantly?

We feel that, while possible, it is not necessary to shorten this section, given that this manuscript is a model description paper submitted to GMD. It is preferable that the manuscript is a little long, at place, rather than cutting apparently unnecessary details which may hamper the comprehension of some readers. We suggest to keep this part as it is, given that it does not include confusing or misleading information, and rather describes how the isotopic modules are incorporated in the overall Crocus structure.

P10L221 There does not seem to be any dependence of the densification rate to temperature or accumulation rate. Neither is there a two or three stage densification process as done usually in some other densification models. Can you elaborate on this? Would this model be suitable for modelling the full firn profile from surface to firn-ice transition?

The parametrization that we use for densification is very simplified, and would not work for a deeper snowpack or the entire firn column. Indeed, we 'compensate' the annual accumulation falling on top of the snow by densifying only the first 10 meters in order to keep the level of the surface steady. And we apply homogeneous compaction, not differentiating between layers with small or large crystal grains or made of resistant hoar.

The original scheme present in Crocus is much better by any regard, and should lead to density predictions much closer to reality. However, it will lead to stable surface level only if the entire snowpack is present within the model. Since we decided to study only the first meters of the snowpack, the original scheme was leading to a permanent increase in the surface level (the compaction below ten meters was absent, as were these layers). We therefore decided to modify the scheme to remove this side-effect.

We will add this precision to the text:

I. 240: **'Layer thickness decreases, and layer density increases** under the burden of the overlying layers and resulting from metamorphism. **In the original module,** snow viscosity is parameterized using the layer density and also using information on the presence of hoar or liquid water. However, this parameterization of the viscosity was designed for alpine snowpack (Vionnet et al., 2012) and may not be adapted to polar snow packs. **Moreover, since we are considering only the first 12 m of the snowpack in the present simulations, the compaction in the considered layers does not compensate the yearly accumulation, leading to rising snow level with time. To maintain a stable surface level in our simulations, we used a simplified'**

Eq.4 I think the right term for the quantity p_v should be mass concentration instead of vapor density (this term is wrongly used in more places in the manuscript). Density refers to the ratio of mass to volume of the same substance whereas what you use here is the mass of vapor divided by the volume of air in the open porosity of the layer under consideration. Accordingly I think you should change the symbol from p_v to C_v or similar.

We have replaced 'vapor density' by '**vapor concentration**' or by '**vapor massic concentration**' everywhere in the manuscript.

I may be missing something but if I use Fick's first law and a forward difference differentiation scheme I do not get the factor of 2 as in Eq. 4. Can you elaborate please?

We are looking at the diffusion between the middle of the lower layer and the middle of the upper layer. Therefore, the water molecules travel along a total distance that is $dz_{\text{low}}/2 + dz_{\text{up}}/2$. Half the thickness of the lower layer and half the thickness of the upper layer. So this factor comes from the denominator.

P11L260 D_v is a function of temperature and pressure. How significant is the fact that you are using a fixed value?

See above.

Eq.5 The fact that the diffusivity used here is independent of temperature and site pressure seems problematic to me. Can you comment on this and add a line in the manuscript about the effect of this approach?

See above.

Eq. 7 Again strictly speaking the quantity you need here is a concentration and not a density. Change the symbols as well.

We have modified the text according to the reviewer suggestion. Vapor density was replaced by **vapor concentration** and p_v by **C_v** , in the text, equations and tables. We also modified the definition of $c_{vap\ ini}^x$, to stress that it is not the same type of concentration as C_v . C_v has a unit (kg m^{-3}) whereas $c_{vap\ ini}^x$ is a ratio of mass and has no unit.

I. 1042:

$c_{vap\ ini}^x$ Ratio between the mass of a given isotopologue in the initial vapor (x is ^{18}O , ^{17}O , ^{16}O , ^1H or D) and the total mass of vapor (no unit). The mass balance is made separately and independently for H and O (i.e.: $c_{vap\ ini}^{18} + c_{vap\ ini}^{17} + c_{vap\ ini}^{16} = 1$ and $c_{vap\ ini}^{1H} + c_{vap\ ini}^D = 1$).

P14L312 Consider using the term rare isotope instead of heavy isotope. Also using an index i is more appropriate than a $*$ sign as later on in Eq. 8 and 9 you use “17”, “18” and “D” in the position of the $*$ sign.

For the isotopes considered here, ‘heavy’ and ‘rare’ are interchangeable, without harm, but it is not necessarily the case for other isotopes. We will add a note on this matter in the manuscript.

We will also replace $*$ by i in the equations as suggested by the reviewer.

Eq. 8 and 9

The term C_{vap}^i needs to be clarified both here and in Table 1. What I understand is that C_{vap}^i refers to isotope concentration as

$$C_{vap}^{16} = \frac{[\text{H}_2^{16}\text{O}]}{[\text{H}_2^{18}\text{O}] + [\text{H}_2^{17}\text{O}] + [\text{H}_2^{16}\text{O}]} = \frac{{}^{16}R}{{}^{18}R + {}^{17}R + {}^{16}R} = \frac{1}{{}^{18}R + {}^{17}R + 1} \quad (1)$$

(again (Mook , 2000) is a good source for definitions).

However later in Eq. 11 you seem to be using the same quantity for something slightly different, this time the masses ratio and not the abundancies ratio. Can you comment on that and make sure the definitions are clear to the reader? If needed add a definition equation in Table 1.

It is true that we make an approximation here. In the traditional equation to define the ratio of two isotopologues, molar concentrations are used. Here we approximate this ratio of molar concentrations by a ratio of masses. We thus neglect the molecular mass term (g mol^{-1}).

$$R^{18} = \frac{[\text{H}_2^{18}\text{O}]}{[\text{H}_2^{16}\text{O}]} \approx \frac{m_{vap}^{18}}{m_{vap}^{16}} = \frac{c_{vap\ ini}^{18} m_{vap}}{c_{vap\ ini}^{16} m_{vap}}$$

Our $c_{vap\ ini}^{18}$ is therefore also a mass ratio. It is the mass of the studied isotopologue in the porosity relative to the total mass of vapor in the porosity. We will modify the definition in Table 1 to make this clearer.

$c_{vap\ ini}^x$ Ratio between the mass of a given isotopologue in the initial vapor (x is ^{18}O , ^{17}O , ^{16}O , ^1H or D) and the total mass of vapor (no unit). The mass balance is made separately and independently for H and O (i.e.: $c_{vap\ ini}^{18} + c_{vap\ ini}^{17} + c_{vap\ ini}^{16} = 1$ and $c_{vap\ ini}^{1H} + c_{vap\ ini}^D = 1$).

We will also add a remark in the text about this approximation:

I. 339: ‘The equilibrium fractionation coefficients (α_{subi}) are obtained using the temperature-based parameterization from Ellehoj et al. (2013). **Note that we make a slight approximation here, by replacing molar concentrations by massic concentrations in our mass balance formulas (see Table 1 for symbol definitions).**’

P14L319 I would be very interested to know why you have used the fractionation factors from (Ellehoj et al., 2013)

We used them because they are recent.

P14I329 This note concerns the use of the term kinetic fractionation throughout the whole manuscript. Kinetic effects refer to anything that is non-equilibrium. And indeed fractionation due to the different diffusivity coefficients for the different isotopologues is a type of kinetic fractionation. Though it is an overstatement to claim that you have included all possible kinetic fractionation processes by only using the ratio of the diffusivities. Fractionation effects related to different binding energies of the molecules for example can also be affected by a non-equilibrium/kinetic regime and this is something that is not addressed by the D^*/D term. I would suggest that you go through the manuscript and clarify this (term kinetic is used in pages 1, 13, 14, 16, 30, 42 and 43). I would also refer the authors to the sections 3.1 to 3.5 in vol. 1 of (Mook, 2000). Even though some of these definitions sound trivial I think the manuscript can benefit greatly by getting these small details right, thus avoiding misconceptions.

We will check the manuscript to make sure that this term is used correctly.

When only the ratio of diffusivity is taken into account, we will use the term 'a' before kinetic fractionation, to underline the fact that it is only one aspect of kinetic fractionation. Alternatively we will precise kinetic fractionation 'during diffusion' or 'during transport', to distinguish this from the kinetic fractionation associated for instance to binding.

Eq. 12 See previous comment on Eq. 4

The factor 2 comes from the distance between the center of the two considered layers (see above).

P15I353 "Here the condensation of excess vapor occurs without additional fractionation". Is this not unphysical. Can you comment?

Please see above.

P18I407 Rephrase the sentence. The term "oriented processes" (also used in 2.1.2) is not a technical term. From what I understand your use of the term "oriented processes" refers to advection-based processes that bias the isotopic signal. Diffusion is not such a process, it attenuates the isotopic signal and is driven by isotopic composition gradients as opposed to for example ventilation that is driven by a bulk motion of air in the open porosity. Additionally diffusion takes place for much longer than 12 m (depending on close-off depth) whereas the extent to which ventilation is apparent in polar firn can be debated.

We apologize for being unclear.

However, we **do** include diffusion driven by temperature gradients in the '**processes of oriented vapor transport**', which are **not necessarily** driven by advection. Here, diffusion is driven by the gradients of vapor concentration, and ultimately by the temperature gradients.

As already indicated above, 'oriented vapor transport' means that it is **forced by an external variable** (temperature, pressure) and not resulting from random molecular agitation.

Because temperature gradients are particularly strong in the upper part of the snowpack, and because the porosity is also larger at shallow depths, this **thermally induced diffusion** is mostly effective in the top meters of the snow.

We will amend the sentence to precise that we are talking here about thermally induced diffusion, and not diffusion along isotopic gradients.

I. 426: Typically, **processes of oriented vapor transport** such as **thermally induced diffusion** and ventilation occur mainly in the first meters of snow so that the model starts with an initial snowpack of about 12 m.

P18I414 "Thus the diffusion process can only be studied in the first 2 m of the model snowpack" Can you elaborate on this? Is it a computation time issue that does not allow for thinner layers below the top 2 m. How do the calculations look like below this depth?

Yes, we have set an upper limit to the number of layers (100) to limit computational time. However, splitting 12 meters of snow into equal pieces would have led to layers of 12 cm. These thick layers would not have been very useful to quantify attenuation of annual cycles which have shorter wavelength.

The various modules are still active in the next 10 meters, and vapor transport occurs, but the density changes and isotopic changes are much reduced because of the low temperature gradients, larger distances, and larger masses of the layers.

P19I432 Stick to one name for GRIP/Summit throughout the manuscript.

See above.

P19I445 Citing a published work (Bréant et al., 2017) dealing with the density studies at Dome C and GRIP is of course acceptable though the density profile here is of great importance for the diffusion calculations, therefore giving some more information and possibly figures would be appreciated.

See above.

Additionnally you give the density as a function of n and t where t (the model time) is an independent variable to z. Can you explain this a little bit better? How do Eq. 16 and 17 give you an evolution of the column density and the densification rates? Please also update the reference to the one past the review process, published in the Climate of the Past

- In the Crocus model, t and z are orthogonal (as observed on the various 2D graphs in the manuscript). 't' corresponds to time evolution forward, over a few months or years. 'z' corresponds to the depth of a given layer (layer number is n). Of course, the depth 'z' of a given layer n will change with time because of compaction and deposition of new snow layers. Therefore, z depends on both n and t.

- The **initial density profile** is defined as a function of depth, but only at a given time $t=0$. So, in the equations (17) and (18), the density varies only with depth z, and depth varies only with layer number n, as t is fixed. We will amend the equations to make this clearer:

The initial density profile in the snowpack is obtained from fitting density measurements from Greenland and Antarctica (Bréant et al., 2016). Over the first 12 m of snow, we obtain the following evolution (**Eq. (17)** and **Eq. (18)**) for GRIP and Dome C respectively:

$$\rho_{sn}(t = 0, n) = 17.2 \cdot z(t = 0, n) + 310. \quad (N=22; R^2=0.95) \quad (17)$$

$$\rho_{sn}(t = 0, n) = 12.41 \times z(t = 0, n) + 311.28 \quad (N=293; R^2=0.50) \quad (18)$$

- Equations (17) and (18) are not used elsewhere in the model (only at model initiation). For $t > 0$, densification occurs based on Equations (3) and (4).

- NB: The NGRIP profile will be replaced by the GRIP profile in the revised version of the manuscript.

- We have updated the reference for Bréant et al., 2017.

P19I450 Earlier in the manuscript you mentioned that all diffusivity values are for a temperature of 263K. Does the isothermal profile at 241 K affect this and if yes how?

Please see above.

P20I457 In Table 2 you refer to a different work for the value of accumulation at GRIP. Be consistent and use only one reference.

We will keep the value of Dahl-Jensen et al. (1993) which was the original reference.

P20I457 You can be a bit more specific and call it “peak to peak amplitude”.

OK.

P20I461 Please refer to general comment nr. 1 with respect to the difference between “isotope firn diffusion” due to isotopic gradients in the snow/firn and signal attenuation/alteration because of air or vapor “bulk motion” driven by pressure or temperature gradients in the snow.

We have rephrased the sentence, and replaced ‘isotopic diffusion’ by ‘transport of isotopes’. Isotopes are indeed transported in both cases, either through diffusion along isotopic gradients or through the overall movement of water vapor forced by temperature gradients.

I. 483: ‘The second simulation is run with evolving temperature in the snowpack (computed by the model, using meteorological forcing from ERA-Interim, see Table 4). In that case, the **transport of isotopes** in the vapor phase results both from **diffusion along isotopic gradients** and from **vapor concentration gradients**. The initial snowpack’

P20I471 What does the term “densities” refer to here? Vapor densities (use term water vapor concentration instead) or firn densities. If it refers to firn densities can you be more specific about how your densification rates depend on temperature?

We are talking about the snow densities.

As already indicated above the compaction scheme is very simple here and taken to compensate yearly accumulation. It does not depend at all on the temperature.

However, the temperature and temperature gradients control the intensity of water vapor diffusion in the snowpack. Thus, layers with higher temperature will lose water and therefore density, because their thicknesses will not be modified during water vapor transport. Oppositely, colder layers will receive more water vapor and their density will increase. This is really a result of the vapor transport module, and has nothing to do with compaction. It is not exactly a densification process, since layers can gain or lose mass.

We have modified the text, to remove the ambiguity:

l. 496: 'In this series of simulations, the **snow densities** and $\delta^{18}\text{O}$ values thus evolve as a result of diverging and/or alternating vapor fluxes.'

Section 3.3.3 In my view this section is unnecessary and its sole sentence can be included in the previous section.

OK. We moved the sentence to line 475, right after the section title.

P21I486 Is this peak to peak amplitude?? Also writing that maxima and minima are reduced sounds inaccurate. Attenuation would result in reduced maxima and increased minima, or in the difference between the two being lower. Lack of visual examples makes this type of language errors quite critical as they can be very confusing for the reader.

The reviewer is right, our sentence was imprecise. What we meant to say was that the amplitude was reduced, with the maximas decreasing and the minimas increasing. This attenuation is visible on panel (b) of the Figure 2.

This is the revised sentence:

l. 509: 'As expected **the peak to peak amplitude of $\delta^{18}\text{O}$ cycles is reduced** as a result of diffusion.'

P21I490 The description of the model in the previous sections suggests that the diffusivity coefficient is independent of temperature. It is not clear though if there is some dependancy of the diffusivity to temperature for your model experiments. One possible cause of the increased depletion for the upper few cm could also be that the firn appears to be quite warmer, something that would result in enhanced diffusion rates for these few cm of the firn column thus attenuating this part more compared to the layers below. I also miss some info on the density profile here and specifically the surface density.

- The uppermost millimeters of the snowpack are indeed warm, but a little colder than the layers around 2 cm depth. The fact that the temperature is high implies large vapor concentration and therefore effective vapor transport and reinforced attenuation. So, we agree with the reviewer that this elevated temperature facilitates vapor transport and attenuation. The reverse temperature gradient that we describe will act on top of the previous phenomenon, by moving vapor preferentially upwards, and therefore bringing also preferentially light isotopes to the very first layer. Both mechanisms could produce the depletion observed, and therefore, we consider the two explanations valid.

- The initial density profile is the same as in the previous simulation. See text:

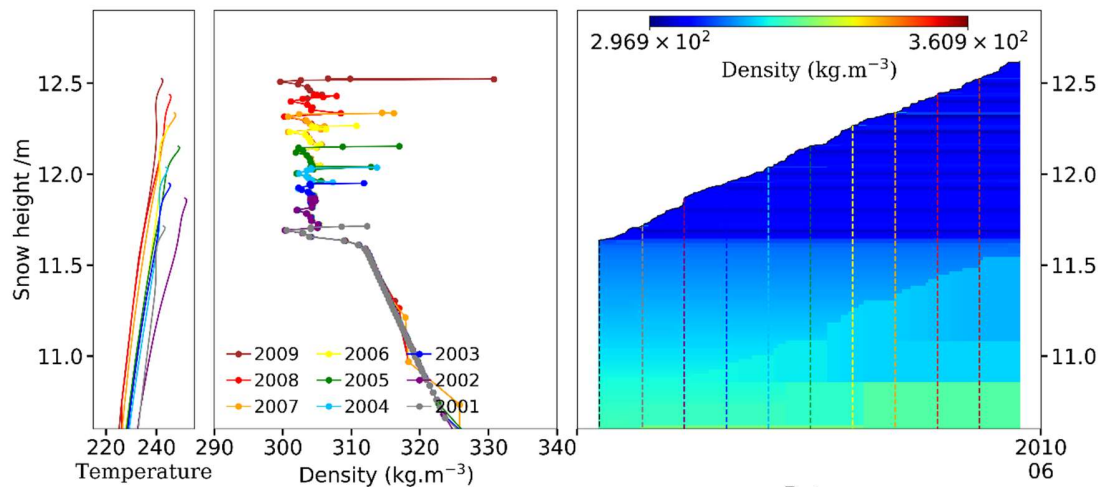
l. 436: The initial density profiles are defined for each site specifically (see Sect. 3.2.).

I. 464: **The initial density** profile in the snowpack is obtained from fitting density measurements from Greenland and Antarctica (Bréant et al., 2017). Over the first 12 m of snow, we obtain the following evolution (Eq. (17) and Eq. (18)) for GRIP and Dome C respectively:

$$\rho_{sn}(t=0, n) = 17.2 \cdot z(t=0, n) + 310.3 \quad (N=22; R^2=0.95) \quad (17)$$

$$\rho_{sn}(t=0, n) = 12.41 \times z(t=0, n) + 311.28 \quad (N=293; R^2=0.50) \quad (18)$$

The first layer has therefore initially a density of 310.3 kg m⁻³ (or very close, depending on its thickness).



Supp. Figure: Evolution of snow density with time (Dome C, **Simulation 6**, D_{v0} varies). Initial density profile linear (black line). New layers deposited with a density of 304 kg m⁻³. They gain mass due to vapor transfer as long as they are exposed. After burial, their density decreases again, as they are subject to alternating vapor fluxes (and the overall density is still close to 304 kg m⁻³).

Section 4.1.2 This section lacks a proper description of the methods used in order to estimate the amplitude of the isotopic signal for the cores presented. In Johnsen et al. the amplitude of the annual signal is computed using a rather sophisticated modification of the Maximum Entropy Method where the annual signal spectral peak is integrated to give a value in permile. This of course is an estimate dependent on the initial isotopic signature (some years have a greater amplitude than others) and for this reason 5m intervals are considered in Johnsen et al. How is this analysis performed here? Can a 20cm interval produce satisfactory results when the layer thickness for these depths at NEEM is in the order of 50-60 cm? Also the term half-amplitude should be peak-amplitude or semi-amplitude.

The reviewer is right, using a 20-cm interval is not adapted at NEEM. For layers below 10 meters at this site, using this window is ok, because then the cycles have a period smaller than 40 cm, therefore the half-period between a given maxima and the following minima is smaller than 20 cm. For layers shallower than 10 meters a window of 30 cm would be better. We will update our methodology and

Figure n°4, to correct this mistake: the window will be changed to 30 cm for layers above 10 meters, at NEEM and at GRIP.

I. 528: 'For NEEM the values of the four cores are taken together. For NEEM and GRIP, the semi-amplitude is computed along the core. **In the first 10 meters, the maximum value every 30 cm is retained, and deeper in the firn, the maximum value every 20 cm is retained (Fig. 4).** Maximum semi-amplitudes every 5 m are also computed.'

Note nonetheless that the 'remaining amplitude' was computed based on maximas obtained with a 1-meter window. Thus, updating the size of the smaller window should not modify our results and conclusions.

We will replace the term half-amplitude by semi-amplitude as suggested.

P22I513 GRIP is also slightly colder.

OK. But the temperature difference is quite small.

P22I520 This is a very good point. Temperature has a strong impact on the diffusivity coefficient. It is certainly relevant to consider various other processes that can be the cause to these discrepancies though a very simple test you could do here is to apply the Johnsen et al diffusivity parametrization in Crocus and compare the results.

This has been done, following the reviewer suggestion, and the results are presented above and will be integrated into the manuscript. The main impact on the diffusivity is not through the temperature, but through the pressure.

I am puzzled by the values that are given here for the firn diffusivity. These are much closer to air diffu values. Firn diffusivity values for $p = 350\text{kgm}^{-3}$ around the temperature of 241 K are orders of magnitude lower. I attach a plot of the Johnsen et al diffusivity for a range of temperatures. What is the reason for such a large difference? Fig. 1. Diffusivity in firn for O18 at $p = 350\text{kgm}^{-3}$

Unfortunately, we cannot see the Figure 1 mentioned.

However, we suppose that the reviewer is comparing our diffusivity in vapor phase D_{eff} to Johnsen et al.'s *isotopic* diffusivity in the ensemble {vapor + solid}, Ω_{fi} . The values are indeed very different, because the ensemble considered is not the same. It is much easier to move molecules around in the porosity, and homogenize it, than to move molecules around in the crystal lattice and homogenize it. Johnsen's diffusivity integrates not only the diffusivity in vapor phase (which is very close to ours), but also an exchange step between vapor and solid, and evaluates in the end the timing of snow homogenization, not of vapor transport.

P23I530 It should be mentioned here that the van der Wel (Van der Wel et al., 2015) study is made by spraying a layer of isotopically spiked artificial snow on top of the natural Summit snow. Such experiments are extremely challenging and the approach of using artificial snow can potentially introduce artifacts with respect to the diffusion processes.

This is right. As stated by the authors themselves, the artificial snow obtained by snow gun may have different diffusivity properties compared to natural snow. However, the spike layer was only 2 cm

thick and the diffusion process later continued into the natural snowpack, both downward and upward. So, at some distance from the artificial layer, the diffusion properties are probably back to natural.

The authors discuss this at length in the paper, and conclude that the discrepancy between their data and the Johnsen's et al. model prediction cannot be explained by reduced diffusion in the artificial layer. Indeed, they systematically remove the data from this layer from their computations. And while looking at the time evolution of diffusion length, the discrepancy increases, even if the layers considered are further and further away from the artificial layer region. So, it is not the artificial layer that is responsible for the discrepancy.

We will however add a note in the manuscript regarding this question, as recommended by the reviewer.

I. 554: 'Denux (1996) and van der Wel et al. (2015) indicate that the model by Johnsen et al. (2000) overestimates the attenuation compared to observed values. For Denux (1996), the model by Johnsen et al. (2000) should take into account the presence of ice crusts and the temperature gradients in the surface snow to get closer to the real attenuation at remote Antarctic sites. **Van der Wel et al. (2015) have compared the model results to a spike-layer experiment realized at Summit. Because an artificial snow layer cannot be representative of natural diffusion, they took care to evaluate diffusion based only on the natural layers present above and below the artificial layer.** For van der Wel et al. (2015), the discrepancy between Johnsen et al.'s'

P23I548 Perhaps you can slightly rephrase as “..are required to observe significant change in densities due to vapor transport at the seasonal cycle”.

We have modified the text according to the reviewer suggestion.

P24-25 The numbers of the experiments should be stated for clarity in the subsection titles or very soon after in the main body of each subsection.

OK.

P25I585 It is a little bit unclear here why and how the precipitation intermittency results in a biasing of the isotopic signal (from -53.2 ‰ to -49.8 ‰). I can see how the winter precipitation events are biased towards warmer temperatures and more enriched $\delta^{18}\text{O}$ values but cannot understand how this creates an additional bias in the isotopic composition of the snow.

We were imprecise here. What we call (wrongly) initial signal in the precipitation is the expected values in the precipitation if precipitation was falling every day at the same rate. It is based on the temperature data, and not on the actual precipitation amount.

This is what we meant by 'constant precipitation throughout the year', I. 622.

We will correct the sentence to clarify our meaning:

I. 617: '**Based on the atmospheric temperature variations only, the isotopic composition in the precipitation should vary around an average value of -53.2 ‰, with a semi-amplitude of 8.6 ‰. There is thus a decrease in amplitude and a positive shift in $\delta^{18}\text{O}$ values in the snow deposited compared to the signal expected for equally distributed precipitation. In detail, we find that the values recorded for summer layers (above -45 ‰) reflect the average summer temperatures, whereas the values recorded for winter layers (below -50 ‰) are biased toward warm events. Thus, taking into account precipitation**

amounts leads to an increase by +6 ‰ of the average $\delta^{18}\text{O}$ compared to values expected only from the temperature signal. The vertical resolution chosen for the model of 2.5 cm may also contribute to the decrease of the semi-amplitude. Indeed, light snowfall events do not result in the production of a new surface layer, but are integrated into the old surface layer.'

P25I590 "As expected the maxima and the minima of $\delta^{18}\text{O}$ are further reduced as a result..."
A more precise and careful writing would be very much appreciated. What does this sentence mean? Is this a decrease of the whole $\delta^{18}\text{O}$ signal, a decrease in the peak to peak amplitude of the signal or a decrease in only the minimum and the maximum of the signal?

As above, what we meant was that the amplitude decreases, because minimas are increasing and maximas are decreasing. We will correct the sentence to clarify our meaning.

l. 626: 'As expected, the **peak to peak amplitude of $\delta^{18}\text{O}$ variations** is further reduced...'

Additionally (see also general comments) you can technically not have isotopic diffusion because of temperature gradients. The latter can indeed create water vapor concentration gradients that will result in diffusive transport of all water vapor molecules. This is though not the same process as isotope diffusion.

Here, both vapor diffusion **caused by temperature gradients** and vapor diffusion **caused by isotopic gradients** are active. While diffusion along isotopic gradients will certainly lead to attenuation, the influence of the second process is more difficult to predict. It is possible that this process contributes to the observed attenuation, at least partially, as is the case for Greenland. The global effect of the two vapor diffusion processes seems to be an attenuation of the original signal.

We will modify the sentence to remove ambiguity here:

l. 634: 'As expected, the **peak to peak amplitude of $\delta^{18}\text{O}$ variations** is further reduced as a result of the **two vapor diffusion processes and of associated vapor/solid exchanges.**'

P26I600 It is very difficult for the reader to follow the discussion of this paragraph when no access is given to the $\delta^{18}\text{O}$ profiles pre and after diffusion. The approach of using contour plots or tracking single layers does not give a good picture of the initial conditions and the evolution of the simulation experiments. Even when those plots are presented they only cover the top 40-50 cm of the studied snow-firn column. As a result, referring to gradients of for example 24 ‰/m feels as an irrelevant piece of information.

It is not clear to us what should be modified here.

1) The initial $\delta^{18}\text{O}$ profile at the beginning of the simulation is without interest, since the initial snowpack has an homogeneous $\delta^{18}\text{O}$ value of -40 ‰.

2) Moreover, vertical profiles of $\delta^{18}\text{O}$ are presented at the beginning of each new year on panel (b). By comparing these profiles together, it is already apparent that the maximas are reduced from one year to the next (and that the minimas are increased). Therefore, we ARE presenting profiles before and after diffusion. We also think that including the panel (d) help to better visualize this attenuation.

3) Another possibility would be to present profiles that follow a diagonal on the 2D graph. For instance, we could present a diagonal profile for the isotopic composition over the first week after

precipitation, for all the layers, to present 'original' isotopic composition, and then a second one, for the layer composition after say 5 years of existence. Is this what the reviewer is aiming at?

4) Again, presenting what is happening downwards (below 40 cm) has no interest since the layers are isotopically homogeneous (so attenuation is unlikely).

5) We can indeed convert the gradient into another unit, such as ‰/cm, to be more coherent with our layer thicknesses.

I. 642: 'low vertical gradients of $\delta^{18}\text{O}$ of the order of 0.24 ‰ cm^{-1} , much smaller than the typical $\delta^{18}\text{O}$ gradients at Dome C (1.10 ‰ cm^{-1}).'

As stated above we will add in the Supplement a 2D plot of the temperature over the same period. This could help the reader to understand the conditions of the simulation.

P26I605 Indeed lower temperatures will slow down diffusive fluxes. This though can only be modelled if the diffusivity coefficient is temperature dependent something that is not the case for this study. Can you comment on this?

As explained above, the temperature also acts on diffusion by increasing the amount of water vapor available, and not only through diffusivity.

P26I607 Which other parameters are loosely estimated? When the term "large uncertainty" is used it is only logical for the reader to ask how large is the uncertainty.

The two parameters that are loosely estimated are τ and $\Delta t_{\text{surf/center}}$. We will clarify this in the text.

In this section we explore a range of possible values for both parameters, to evaluate how they affect the results and especially the attenuation. It was an error to state in advance that these parameters bring large uncertainty, because the situation is different for Dome C and GRIP, and because for GRIP the attenuation is increased only by one third. Thus, we will remove this assertion from the text, and simply say that these parameters lead to uncertainty on the simulation result. The rest of the section brings answers on this uncertainty.

I. 646: 'In parallel, the parameters of the model associated to grain renewal (τ and $\Delta t_{\text{gsurf/center}}$), could only loosely be estimated leading to uncertainty in the attenuation modeling.'

P28I644 Replace badly with poorly.

OK.

P28I645 Being able to implement more processes in a model sounds in principle as a step forward. However I think that a discussion on improving on the knowledge, assumptions and parameters used in the more dominating processes of diffusion is missing here. Integration of more processes that are poorly implemented can be misleading and give the false impression of an improved approach for the description of the problem. With this in mind I think that a comment on proposed improvements, measurements and proper tests with real data would be most welcome in this manuscript especially if it focuses on the more dominating processes of the problem.

Before testing them, it is difficult to decide which aspect of the problem is dominant. It may indeed be diffusion but we cannot be sure of that, especially since the model simulation falls short of

reproducing attenuation observed in the data. Therefore, listing other processes potentially active (and maybe even dominant) seems logical here.

The present article aimed only at presenting the model modifications and its possible applications. But comparisons to real data is of course a necessary follow-up to the present paper. For instance, it would be nice to compare model results to on-site experiments on diffusion, such as the one by van der Wel et al. (2015).

We will add a remark on this in section 4.4, stating that the model should be improved not only by adding more processes but also by better constraining diffusion with real data.

I. 683: 'output for GRIP. **In order to improve the model compatibility with data, two kinds of approaches are possible. On the one hand, it would be useful to realize simulations adapted to on-site experiments such as the one by van der Wel et al. (2015). This would allow to verify how diffusion can be improved in the model. For instance,** previous studies have suggested that water vapor diffusivity within the snow porosity may be underestimated by a factor of 5 (Colbeck, 1983), but this is debated (Calonne et al., 2014). **On the other hand, we also believe that other processes...**'

P28I660 The top 10 m of snow may have been modelled in this study but results only the top 0.5 m are presented here. Thus I think this sentence should be rephrased in order to reflect the actual results presented in the study.

We will modify the sentence:

I. 704: 'Water vapor transport and water isotopes have been implemented in the Crocus snow model enabling depicting the temporal $\delta^{18}\text{O}$ variations in the **top 50 cm** of the snow in response to new precipitation, evolution of temperature gradient in the snow and densification.'

P29I675 Refer to my general comments on the GRIP case.

See above.

4 Comments on figures

Figures of experiments results

The experiment number should be included in the captions and titles of all relevant figures.

We added the information in the captions. Our figures do not have a title, only axes titles, which will not be modified.

Color maps of figures The color maps of the density and $\delta^{18}\text{O}$ change plots can become more readable if there is also some information about where the zero value is. I assume it is the white but cannot tell with certainty.

Yes, this is right. We have added a line to clarify this point.

I. 442: 'The variations of the considered variable are displayed as color levels. **The white color corresponds to an absence of change of the variable. As**'

Density and O18 change plots I find these plots confusing and not intuitive. The meaning of the term "density change" and " $\delta^{18}\text{O}$ change" appears only in the caption of fig. 7 and 8. It is very hard

for the reader to understand what this change refers to. My impression until I reached figure 7 and 8 was that these were rates ie change per time. Please clarify in the main text and on the legends of the figures.

For Figure 2 and 3, the term ' $\delta^{18}\text{O}$ change' is already defined in the caption: it is the [deviation to the original profile of \$\delta^{18}\text{O}\$](#) .

For Figure 5: We indicated that the density change was 'cumulative'. This may not be very explicit for the reader. We will complete the explanation with the following sentence:

l. 1111: '[Here, 'density change' stands for the difference between density at t and at the beginning of the simulation for the selected layer.](#)'

We do the same for $\delta^{18}\text{O}$ on Figure 6:

l. 1121: '[Here, ' \$\delta^{18}\text{O}\$ change' stands for the difference between \$\delta^{18}\text{O}\$ at t and at the beginning of the simulation for the selected layer.](#)'

For Figure 7 to 10, the caption already contained the necessary information.

Figure 11: The original figure was indeed difficult to apprehend. Here, the initial profile was homogeneous, with $\delta^{18}\text{O}$ values of -40 ‰ at all depths. We had decided to use this value as reference and plot ' $\delta^{18}\text{O}$ change' simply as the simulated value of $\delta^{18}\text{O}$ minus -40 ‰. Thus, the values of -18 and -2 present on the colorbar corresponded to $\delta^{18}\text{O}$ values of -58 ‰ and -42 ‰.

To make this figure easier to understand, we will remove the term ' $\delta^{18}\text{O}$ change' and replace it by ' $\delta^{18}\text{O}$ '. We will therefore change the values to -58 ‰ and -42 ‰ on the colorbar. We will also precise in the legend that for this figure (only) the white color corresponds to a $\delta^{18}\text{O}$ value of -50 ‰. This value is nearly identical to the average $\delta^{18}\text{O}$ in the snow deposited: -49.8 ‰.

The caption will be modified accordingly:

l. 1150: '[Figure 11. **Simulation 6:** Evolution of \$\delta^{18}\text{O}_{\text{gcenter}}\$ values as a result of snowfall and vapor transport over 10 years \(compaction is inactive; merging between layers is allowed but limited\). \(a\) Temperature profiles at mid-January for each year. \(b\) \$\delta^{18}\text{O}_{\text{gcenter}}\$ profile at mid-January for each year. \(c\) Repartition of \$\delta^{18}\text{O}_{\text{gcenter}}\$ values as a function of time and depth. **For this figure, the white color corresponds to \$\delta^{18}\text{O}_{\text{gcenter}} = -50\$ ‰, close to the average isotopic composition in the new snow layers.** d\) Evolution of \$\delta^{18}\text{O}_{\text{gcenter}}\$](#) '

C20 Figure 11 It is odd that while the slope for the 2000 winter layer is opposite to the other summer layers and you choose to comment on this, the scale of the axis for these data is inverted thus visually "masking" the event. I would really not mind if the lines end up crossing each other if all axes are plotted in the same way.

OK, we will modify the Figure 11 as suggested.

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