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 $\delta^{18}O$ 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 Dv0 (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.

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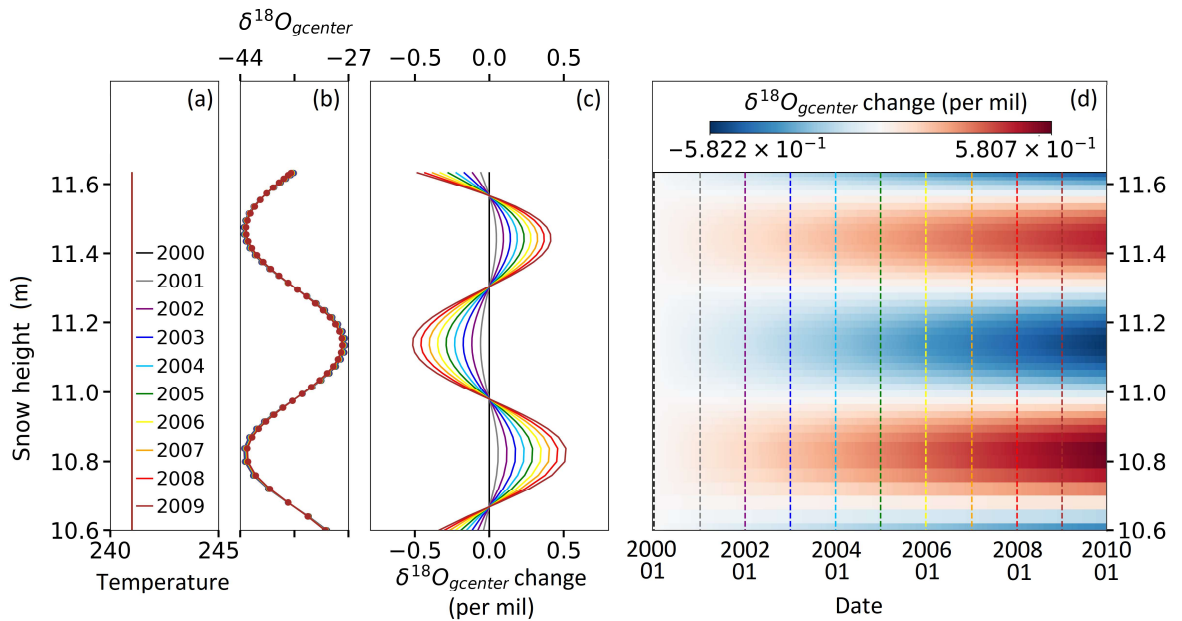
New Figure 2. Simulation 1: 10 years at GRIP with fixed temperature (240 K), with D$_{v0}$ function of the temperature.
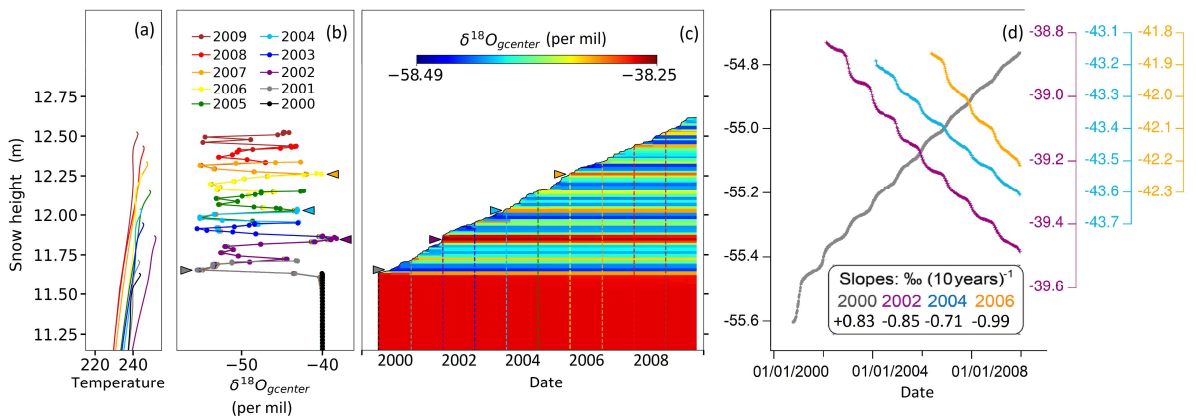
New Figure 11: Simulation 6: 10 years at Dome C with precipitation with varying δ^{18}O; with temperature evolution throughout the year; with D$_{v0}$ function of temperature.

- 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.

l. 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 $\delta^{18}O$ value in the layer.

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Slope vs. initial δ¹⁸O value

Slope vs. layer thickness
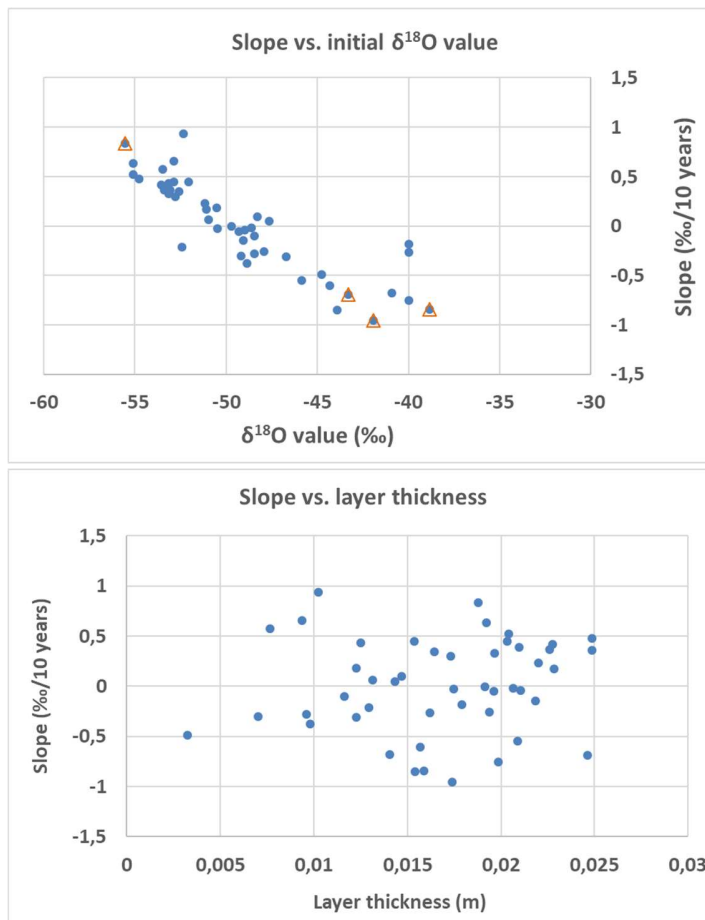
- Regarding the isotopic composition in the precipitation we have run a zero-simulation with constant $\delta^{18}O$ in the precipitation. We wanted to see how vapor transport could possibly generate $\delta^{18}O$ variations, based on temperature gradients, in the absence of initial signal (Figure 6 and Figure 8). For the first layer, the $\delta^{18}O$ 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 $\delta^{18}O$ variations in the precipitation, to evaluate attenuation based on a realistic $\delta^{18}O$ 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/m^2/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/m^2/year.
l. 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 $\delta^{18}O$ 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)).

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

Then, what we name "interfacial diffusivity" (Deff(t,n→n+1)) is the average of two "effective diffusivities" from two adjacent layers (Deff(t,n) and Deff(t,n+1), Equation (6)). The "interface" here is the limit between the two layers.

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

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 (*Deff*(*t,n*) and *Deff*(*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 *Deff*(*t, n*->*n*+1) (m2 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 that 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:

l. 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:

l. 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

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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 $\delta^{18}O$ in the snowfall to the local temperature (T_{air}, in K):

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

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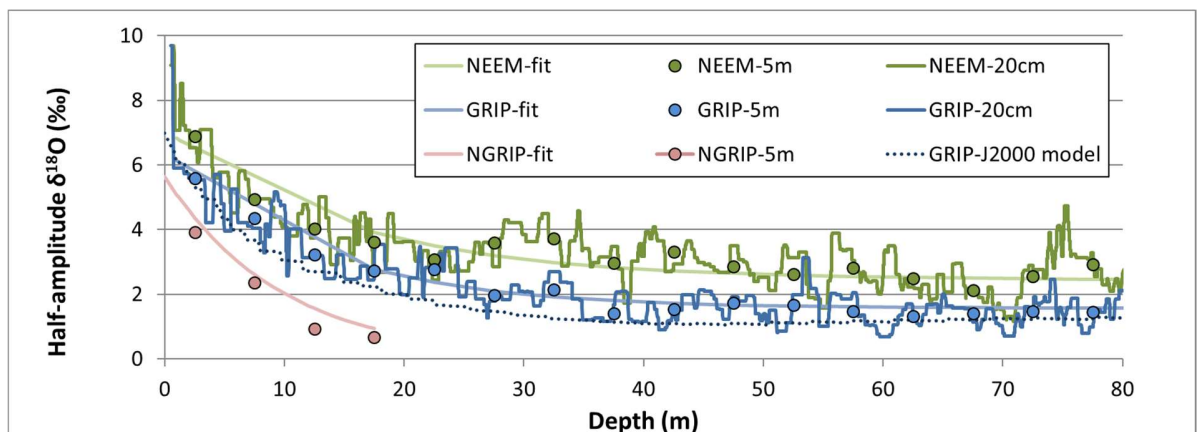
New figure 4 with the model of Johnsen et al. (2000).

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

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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.

l. 1102: "(a) Vertical temperature profile for each summer; (b) $\delta^{18}O_{gcenter}$ profile for each summer; (c) Deviation of the $\delta^{18}O$ relative to the original profile, for each summer; (d) Evolution of the deviation to the original profile of $\delta^{18}O_{gcenter}$. **Note that temperature varies during the whole year (see Figure TT in the Supplement)."**
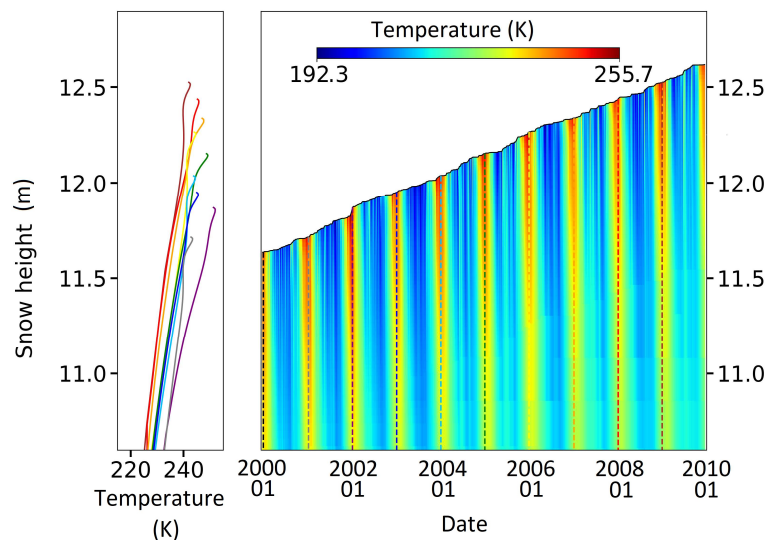
- **I am surprised to find that your model does not show an influence of temperature gradients at GRIP as you would normally assume that temperature gradients would force vapor to be transported between layers due to the vapor pressure gradient?**

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 10^{-1} to 7.567 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:

l. 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."

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- 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:

l. 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:

l. 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."**

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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

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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 firn 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 10^{-7} per 15 min. For a layer of 330 kg m^{-3}, the density increase is: +7.4 10-5 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 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.
l. 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.

SLIDING

MERGING
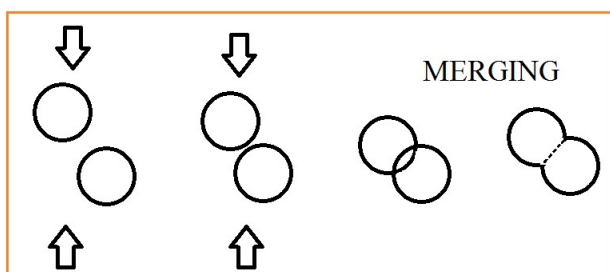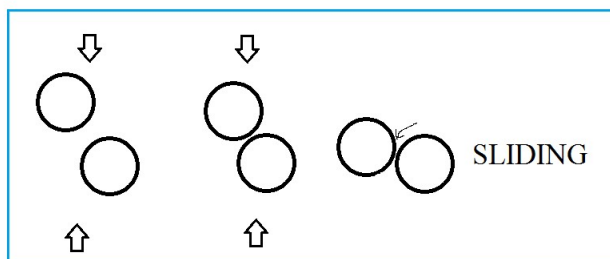
L296: "that are" -> being
OK

L304: Have you defined kinetic fractionation previously?

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

l. 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&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:

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