

Interactive comment on “A trace gas method of evaluating interstitial air advection and diffusion in snow” by Stephen A. Drake et al.

S. Morin (Referee)

samuel.morin@meteo.fr

Received and published: 1 May 2017

The discussion article entitled "A trace gas method of evaluating interstitial air advection and diffusion in snow" provides potentially useful data and analysis in order to address the partitioning between the various processes responsible for trace gas movement in snow. This topic is fully consistent with the scope of The Cryosphere, and requires careful experimental investigations combined with modelling approaches, which are both employed in this study. Unfortunately, the manuscript lacks precision in many respects, in particular including the description of the field experiments and numerical modelling, the terminology used, the physical properties of snow, and the literature references. In general more quantification is needed in the text, which is often too vague and qualitative (although actual numbers can sometimes be found in the tables and figures, but unfortunately not used in the text).

C1

The only general comment I have refers to the description of the processes conducive to trace gas movements in snow. The title refers only to "advection" and "diffusion". It seems, from reading parts of the article, that the authors make a distinction between different advection processes (at least two : "quasi-static pressure gradients [...] in response to wind-induced pressure gradients" – page 1, line 19, "turbulently generated pressure fluctuations" – page 1, line 22). Given that the literature in this field is complex and there may be significant overlap between concepts used in the literature, it would be useful that this manuscript describes the processes very accurately and precisely, in order to avoid ambiguity in this central issue for this manuscript.

Besides this need for clarification, I do not have any major comment on the manuscript, having not detected any major flaw, but conversely it is often very challenging to precisely understand how the experiments (numerical and in the field) were designed, how the results were achieved, and what are their implications. I thus rather than major remarks have quite a long list of comments and points, which deserve attention from the authors in order to better convey their research to the readers. Not being myself a native English speaker, I refrain from any comments on the wording, although in several places the wording seems (perhaps) colloquial and ambiguous (I hope this can be addressed post-acceptance through the copy-editing step).

Nevertheless, I believe that a significantly improved manuscript could be accepted for publication, given the relevance of the experiments carried out and their potential to be converted into useful scientific results.

Specific comments

Title

I think it would be appropriate to mention in the title that the emphasis is placed here on "wind-driven" processes.

Abstract

C2

Page 1, line 9 : I recommend rephrasing the term "validated". It is very unclear what is meant here. "Validate" is a very strong verb.

Page 1, line 14 : Besides "surface (chemical) reaction rates and interpretation of firn and ice cores", I think the primary implications of this work apply to interstitial water vapor movement and snow metamorphism itself, see e.g. Calonne et al. (2015). This is unfortunately almost never mentioned in this manuscript.

Introduction

The introduction is short and does not provide a sufficiently broad status of the current knowledge in this field. For example, on the experimental work it ignores work carried out in order to measure fluxes of trace species in snow (e.g. Seok et al., 2009). On the snow microstructure side, it only vaguely alludes to some of the physical processes and properties involved (e.g. page 2, line 4 the word "permeability" is mentioned but the introduction lacks the establishment of the conceptual and physical framework where such variables are used). Without a proper introduction of the state of the literature and knowledge gaps, it is difficult to understand what added-value is brought by the current manuscript. I strongly recommend expanding the part from page 2, line 5 to 9 in order to better describe what previous knowledge gap is addressed by the current study, and how. At present, the introductory statement regarding the current study seems only to be a disclaimer on why only top-most snow layers are targeted, without even knowing what kind of measurements are dealt with and to serve what purpose.

Methods

In general the "Methods" section needs considerable improvements, to enhance the clarity of the description of the experimental goals, the instruments used for CO measurements but also ancillary conditions (not only wind, but also snow properties, meteorological conditions etc.) and the experimental sites. Figure 2 is very unclear, I recommend providing pictures of the set-up in the field and conceptual sketches illustrating the actual set-up for the various configurations used (apparently implementations vary

C3

from the various cases reported). Maybe such sketches should be provided along with every result graphically shown, in order to better understand the set-up and the corresponding data. I think the first sentence of the "Methods" belongs to the Introduction (in order to better explain the goals of the manuscript), and everything else should be placed in sub-sections with informative title. The status of the part from page 2, line 10 to page 3, line 5, is unclear and often repeated with the content of sections 2.1 and 2.2 (e.g. height of the wind sensor), which is illustrative of this confusion.

Specifically:

- neutral buoyancy : The paragraph from page 2, line 10 to line 22 is quite tortuous and mixes several issues together. Line 15 it is said that "it is nearly neutrally buoyant", then line 18 "neutral buoyancy is not strictly achieved for this experiment" then line 22 recommendations are made to make the experiment "essentially neutrally buoyant". It is very hard from this text what is really at stake here, given that no quantification is provided and the text is meandering around the issue of neutral buoyancy.
- safety : The same paragraph states line 16 that CO is "safely handled" then line 20 it can "cause unhealthy side effects". Here again, better clarity is required.
- page 2, line 17 : reference needed here.
- page 2, line 20 – 22 : I think such "recommendations" could be placed in the Discussion or Conclusions sections (along with a quantification of the related issues), not in the first paragraph of the Methods section.
- page 2, line 32 : "cases 12 through 14": at this point, the "cases" have not been introduced yet. This illustrates the need for a major reorganisation of the manuscript, in order to better streamline the description of the methods and experiments.
- Sections 2.1 and 2.2 do not seem logically organized. For example, in section 2.1, page 3, lines 13 to 20 describes the experimental approach employed to deploy the sensors and contains details relevant to the equilibration time, which could be referred

C4

to as "Deployment description", and not at all a description of the sites. Conversely, section 2.2 confuses data selection (first words of the section), quality control and operational constraints, which do not correspond to a description of the deployment. This calls for a major reorganization of the Methods section.

- section 2.1, page 3, line 9 : more information should be given on what is referred to the "broad range of wind forcing and snow permeability regimes".

- section 2.1, page 3, line 13: what is a "low-profile snowpit" ?

- section 2.1, page 3, line 20: what is "spongy snow" ? If this corresponds to a description of snow properties, this should be provided in the Results section, while the Methods section should describe the methods employed to characterize snow physical properties (currently missing, although this is critical for this study).

Section 2.3

Page 4, line 5: that CO sensors are sensitive to humidity and temperature is already mentioned above.

Page 4, lines 9 and 10 : perhaps a sketch and pictures could be useful to better understand the functioning of the calibration chamber. At present, this is very unclear and there is a large margin for interpretation of the sentences describing the apparatus.

Page 4, line 15 and 16 : what is the definition for "cold snow" and "warm snow" calibration ? How is this implemented in practice for field measurements ?

Section 3 Data analysis

Page 4, lines 21 to 25: these statements are not consistent with the title of the section.

Page 4, line 30 : "modeled" needs to be replaced with "simulated" as what is dealt with here is actual simulation results. In addition, given that the equation used (the model) includes advection and diffusion (line 31), how is it possible that deviations between observations and simulations correspond to the influence of "advection and

C5

snow heterogeneity" (line 30) ? This is very unclear and needs to be better described.

Page 4, equations (1), (2) and (3) : symbols need to be described in the text. What is D , r , t , C etc. ? What is t_{MAX} ? The absence of description of the symbols hampers the understanding of the rest of the manuscript, unfortunately.

Page 4, line 10 : I can't understand what is meant by "We calculated the diffusion coefficient for each sensor". The sensors measure the concentration of CO, I don't see how this solely can be used to compute the diffusion coefficient.

Section 3.1 (note that, if there is only one subsection at the end of section 3, this implies that the structure is not optimal; either add more subsections from the beginning of the section 3, or drop the section 3.1 and make simple paragraph).

Page 5, line 15 ; it is surprising that a reference to Riche and Schneebeli (2012) is given to support the fact that snow permeability could be anisotropic. First of all, anisotropy of effective thermal conductivity was demonstrated by Calonne et al. (2011), but, more importantly, Calonne et al. (2012) provide direct estimates of the anisotropy of the intrinsic permeability of snow for various snow types found in mid-latitude snow types. There is thus no need to speculate here. Given the existence of literature not accounted for in this paragraph, it probably needs full rephrasing.

Results

Page 5, line 23 : what is a "diffusion constant" ?

Page 6, line 10 : "moderate" : rather provide numbers. "mid-to-low density" : rather provide numbers.

Page 7, line 6 : what is the "plume standard deviation" ? Line 7 : what do "minutely" refer to ? Earlier in the manuscript this refers to "1-minute time resolution".

Page 7, line 14: "NaN" should be defined.

Page 7, line 19 : is there an analytical form for Equation (1), which accounts for non-

C6

homogeneous diffusion coefficients ?

Page 7, line 23 : "higher permeability (lower density)" : better provide numbers. Furthermore, as demonstrated in Calonne et al. (2012), not only density but also specific surface area, drive variations of snow permeability.

Page 7, line 30 : "conspired" ?

Page 8, line 23 : How was the diffusivity "measured" ? Up to this point in the manuscript, no apparatus measuring the diffusivity was described.

Page 9, line 2 : what is the "smallest measured effective diffusivity" ? Over all measurements across sites ? What is the value found ? How does it compare with literature values ? This is not clear.

Page 9, line 3 : note that equation (3) already exists in the manuscript (this should be equation (4)).

Conclusions

Page 9, line 21 : "invalidating the notion of a mono-valued diffusion coefficient over small areas" : this notion was never introduced before in the article. This statement requires that the literature review identifies the need to "invalidate" (or not) this "notion".

Page 9, line 29 : "gran-scale properties" : this is the first mention of microstructure-scale snow properties. This aspect deserves to be better introduced in the manuscript, on the basis of references suggested here as well as in the review provided by Reviewer #1. Otherwise, this manuscript will be inconsistent with current knowledge in snow physics.

Authors contribution:

Page 10, line 10 : Given that Z. Liu and R. Hochreutner are not authors of this manuscript, I don't understand why they are mentioned here (it is of course appropriate to mention them in the Acknowledgements).

C7

Tables and Figures:

Table 1 : More information on snow stratigraphy is needed, given that this concerns only 5 snow pits this should be doable in a condensed form. Also, in addition to mean wind-speed, its standard deviation would be useful to better assess the steadiness of the wind conditions. The content of the CO sensor depth column is not clear (and it has formatting issues, some number have a '.0', some not).

Table 2 : Could be merged with Table 1. "Degree" symbol missing.

Figure 1 : Size scale missing.

Figure 2 : Very unclear. I don't understand where is the CO cylinder and where the CO is injected into snow. The different arrangements of the "cases" could be better explained, maybe using a "3D" sketch (yet simple) which would better show how the apparatus was implemented in the field. Pictures could be used to better illustrate how the system was implemented.

Figure 3 : The CO release position should be indicated on panels a and c

Figure 4 : I could not understand why there are several lines with the same "distance" (e.g. twice 45 cm in subplot a). Only a sketch explaining the set-up of this particular experiment could help, I'm afraid. As such it is highly ambiguous.

Figure 5 : "Distances" in the labels of the axes in b, d and f should be better described (are these horizontal or vertical distances ? Where is the injection of CO (is is the red star ?) ? Again, a sketch describing this experiment would be more than useful.

Figure 6 : "Distances" in the labels of the axes in subplot a should be better described (see comments above).

Figure 7 : Same comments as for Figure 5 . Furthermore, what is the red line ?

Figure 8 : Same comments as for Figure 7. The caption refers to "impedence" which appears unique in the manuscript and is not referred to in the text.

C8

References (if not already quoted in the manuscript)

Calonne, N., Flin, F., Morin, S., Lesaffre, B., du Roscoat, S. R., and Geindreau, C.: Numerical and experimental investigations of the effective thermal conductivity of snow, *Geophys. Res. Lett.*, 38, L23501, doi: 10.1029/2011GL049234, 2011.

Calonne, N., C. Geindreau, F. Flin, S. Morin, B. Lesaffre, S. Rolland du Roscoat and P. Charrier, 2012. 3-D image-based numerical computations of snow permeability: links to specific surface area, density, and microstructural anisotropy, *The Cryosphere*, 6, 939-951, doi: 10.5194/tc-6-939-2012, 2012.

Calonne, N., C. Geindreau, F. Flin, 2015. Macroscopic modeling of heat and water vapor transfer with phase change in dry snow based on an upscaling method: Influence of air convection, *J. Geophys. Res.: Earth Surf.*, 120, 2476-2497, doi: 10.1002/2015JF003605, 2015.

Seok, Brian, Detlev Helmig, Mark Williams, Daniel Liptzin, Chowanski, K., Jacques Hueber: An automated system for continuous measurements of trace gas fluxes through snow: An evaluation of the gas diffusion method at a subalpine forest site, Niwot Ridge, Colorado. *Biogeochemistry*, 95(1): 95-113. doi: 10.1007/s10533-009-9302-3 , 2009

Interactive comment on *The Cryosphere Discuss.*, doi:10.5194/tc-2017-9, 2017.