

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

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Author's response to Reviewer #2 comments

Thank-you for your comments. The lead author will address your comments. Author responses reference the same page and line number as they appeared in the original manuscript. The current revision of the paper is attached as a supplement. Due to the detailed and sometimes overlapping changes requested by both reviewers we summarize all document changes as a PDF document that is appended to the manuscript.

General comments The discussion article entitled "A trace gas method of evaluating interstitial air advection and diffusion in snow" provides potentially useful data and anal-

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ysis 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).

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

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publication, given the relevance of the experiments carried out and their potential to be converted into useful scientific results.

Author's response to general comments:

The introduction and methods sections were re-written.

Specific comments

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

Author's response to Title:

I changed the title to include "wind-driven". This change addresses a similar critique by Reviewer #1. Also, we added a description of wind-driven pressure changes to the introduction.

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

Author's response to Page 1, line 9 : I removed the words "or validated relationships".

Page 1, line 14 : Besides "surface (chemical) reaction rates and interpretation of firm 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.

Author's response to Page 1, line 14 : Besides the reference to interstitial water vapor at page 1, line 21, I added references to water vapor and snow metamorphism in the introduction.

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.,

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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.

Author's response to Introduction:

I rewrote the introduction to address these deficiencies.

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 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.

Author's response to Methods:

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I reorganized the methods section. Figure 2 was replaced with an alternate sketch of the experimental setup.

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.

Author's response to neutral buoyancy: I rephrased this section.

- 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.

Author's response to safety: I rephrased the section on safety.

- page 2, line 17 : reference needed here.

Author's response to page 2, line 17 : I added a reference regarding a thin film of water on ice.

- 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.

Author's response to page 2, line 20 – 22 :

I moved this line to the conclusion as a recommendation.

- 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.

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Author's response to page 2, line 32 :

I moved case information to the results section.

- 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 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 a description of the deployment. This calls for a major reorganization of the Methods section.

Author's response to Sections 2.1 and 2.2: These sections were reorganized.

- 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".

Author's response to section 2.1, page 3, line 9:

I included more specific information from Table 1 in this section.

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

Author's response to section 2.1, page 3, line 13:

I replaced "low-profile snowpit, exposing a clean face " with "shallow trench, exposing a clean face of the snow layer".

- 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).

Author's response to section 2.1, page 3, line 20:

I removed the term "spongy snow" and moved this description to the results section.

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Section 2.3 Page 4, line 5: that CO sensors are sensitive to humidity and temperature is already mentioned above.

Author's response to Page 4, line 5:

This information is needed to rationalize the need to calibrate the CO sensors for different snow temperatures. I don't think the repetition, briefly stated, is a problem.

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.

Author's response to Page 4, lines 9 and 10 :

I added a picture of the calibration chamber in the lab and explanation of how each calibration measurement was acquired. I also added a sketch to delineate the experimental configuration.

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 ?

Author's response to Page 4, line 15 and 16 :

More description of the CO sensor calibration was added to the calibration section.

Section 3 Data analysis Page 4, lines 21 to 25: these statements are not consistent with the title of the section.

Author's response to Page 4, lines 21 to 25: These lines were moved to the results section.

Page 4, line 30 : "modeled" needs to be replaced with "simulated" is 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

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

Author's response to Page 4, line 30 : I changed "modeled" to "simulated" and rephrased this section.

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.

Author's response to Page 4, equations (1), (2) and (3) :

I defined the symbols in equations 1-3.

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.

Author's response to Page 4, line 10 :

I rephrased: "We calculated the diffusion coefficient for each sensor and subtracted these values from those given by Eq. (3) to derive a residual that is an approximation of wind-driven dispersion enhancement." as: "For each CO release, we measured CO concentration as a function of time and distance from the release point to find t_{MAX} for each sensor. Using Eq. (3) we then calculated the diffusivity for each sensor and subtracted these values from a mono-valued diffusivity of $2.56 \times 10^{-5} \text{ m}^2 \text{ s}^{-1}$ consistent with snow (Huwald et al., 2012) to derive a residual that is an approximation of wind-driven dispersion enhancement."

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).

Author's response to Section 3.1: I reorganized Section 3 into two sections.

Page 5, line 15 ; it is surprising that a reference to Riche and Schneebeli (2012) is given

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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.

Author's response to Page 5, line 15: Thank-you for your insight on this detail. I have rephrased this paragraph.

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

Author's response to Results – Page 5, line 23 : I changed "diffusion constant" to "diffusivity". I also replaced "diffusion coefficient" with "diffusivity" throughout the text for consistency.

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

Author's response to Page 6, line 10 :

I replaced the descriptions with values from Table 1.

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".

Author's response to Page 7, line 6 :

I replaced "standard deviation" with "mass-weighted RMS distance from the center of mass". "Minutely" was changed to "One-minute".

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

Author's response to Page 7, line 14:

I replaced "NaN values" with "NaN (Not a Number) results".

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

Author's response to Page 7, line 19 :

Rather than using a constant for vertical diffusivity in Equation (1), the vertical diffusivity was adjusted to decrease with depth to determine a relative influence of vertically varying diffusivity on plume evolution.

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.

Author's response to Page 7, line 23 :

I removed "higher permeability" and included the actual snow density from Huwald et al. (2012) and included the reference to Calonne et al. (2012). The snow density in the Huwald et al. (2012) paper was higher than for case 13.

Page 7, line 30 : "conspired" ?

Author's response to Page 7, line 30:

I changed "at which time snow heterogeneity and vertical dispersion degraded conspired to degrade" to "by which time snow heterogeneity and vertical dispersion degraded".

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

Author's response to Page 8, line 23:

I changed "measured diffusivity" to "diffusivity, \tilde{D}_{MAX} , calculated from Eq. (3)".

Page 9, line 2 : what is the "smallest measured effective diffusivity" ? Over all measure-

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ments across sites ? What is the value found ? How does it compare with literature values ? This is not clear.

Author's response to Page 9, line 2 :

I changed "smallest measured effective diffusivity" to "smallest calculated effective diffusivity". The value found for the calculated effective diffusivity is not relevant because it is less than the value for molecular diffusivity and therefore not physically possible.

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

Author's response to Page 9, line 3 : I renumbered subsequent equations and references to equation 3.

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".

Author's response to Page 9, line 21 :

I both rephrased this in terms of intrinsic permeability and added a paragraph that discusses permeability to the introduction.

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.

Author's response to Page 9, line 29 : I rephrased this sentence.

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

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course appropriate to mention them in the Acknowledgements).

Author's response to Page 10, line 10: I removed references to Z. Liu and R. Hochreutner from the Author contribution section.

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).

Author's response to Table 1:

I have modified the manuscript, starting with the title, to clarify that we are only concerned with the top layer of a snowpack. So stratigraphy information is not only irrelevant but also would be confusing to the reader. I added standard deviation of the wind speed to Table 1 using 20 Hz data that also slightly altered mean wind speed (relative to 1-minute averages).

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

Author's response to Table 2 : I merged Table 2 with Table 1 and inserted the degree symbol.

Figure 1 : Size scale missing.

Author's response to Figure 1 : I replaced Figure 1 with an annotated picture showing CO sensor locations.

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.

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Author's response to Figure 2 : Figure 2 was redesigned.

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

Author's response to Figure 3 : The release positions are now marked in 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.

Author's response to Figure 4 : These issues are clarified by a revamped Figure 1.

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 the red star ?) ? Again, a sketch describing this experiment would be more than useful.

Author's response to Figure 5 :

Figure 1 addresses the reviewer's confusion with CO sensor spacing. Descriptions of the release point and sensor locations are also given in the figure caption. The figure caption is already long so a description of the distance is given in the text of the manuscript.

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

Since there is adequate space in this figure caption I added a description of the distance to the caption. A revamped Figure 2 should clarify sensor positions in this and other figures.

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

Author's response to Figure 7 : The red line is a wind barb as indicated in the figure caption.

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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.

Author's response to Figure 8 : I removed the word "impedence".

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.

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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

Please also note the supplement to this comment:

<http://www.the-cryosphere-discuss.net/tc-2017-9/tc-2017-9-AC2-supplement.pdf>

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-9>, 2017.

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