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

Stephen A. Drake et al.

sdrake@ceoas.oregonstate.edu

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Author's response to Reviewer #1 comments

The lead author will address your comments. Author responses reference the same page and line number as written in the original manuscript. Due to the detailed and sometimes overlapping changes requested by both reviewers we summarize all document changes as a PDF supplement.

General comments This paper demonstrates an interesting method to better understand the relevance of advective flow in a snowpack. This is demonstrated by several measurements. What was a bit surprising to the reviewer that the method is already described in much detail by Huwald et al., 2012. The paper demonstrates the application of the method in several field cases. The authors demonstrate that diffusion and advection of carbon monoxide is affected by wind speed. The main conclusion of the paper is that "atmospheric pressure gradients can induce subsurface advection". This is not an entirely new results. Unfortunately, the physical properties of the investigated snowpacks are not described to a degree that is state-of-art. Neither a highly resolved density profile, specific surface area measurements, measurements of spatial variability (using e.g. near-infrared photography, Tape et al. 2010) were applied. The use and progress of this paper for interpretation of advective flows in snow is therefore very limited beyond description. The presented model, assuming isotropic and non-layered properties of the snowpack, is very simplistic.

Author's response to general comments:

Thank-you for your thoughtful review of this manuscript. I'll address your general comments one at a time.

"What was a bit surprising to the reviewer that the method is already described in much detail by Huwald et al., 2012."

Author response: Excluding instrumentation repair that involved rewiring and replacing 6 CO sensors, the CO measurement instrumentation described in this manuscript is the same as described by Huwald et al., 2012. The deployment design of the snow pickets is different, however. In Huwald et al., 2012, snow pickets were placed vertically with the picket ends protruding from the snow surface, introducing experimental problems with which the authors of that paper needed to contend. For example, wind blowing over the tops of the pickets created a pressure gradient that exacerbated along-picket leakage. As stated in the methods section, we alternatively dug a shallow trench to expose a face of the surface snow layer and pushed the snow pickets horizontally and parallel to each other into this undisturbed surface snow layer. After placement, we backfilled the shallow trench to completely cover the exposed ends of the snow pickets. The revised version of the manuscript will delineate these points more clearly.

“The paper demonstrates the application of the method in several field cases.”

Author response: The number of deployments was purposefully limited to discrete snowfall events that were several times deeper than the snow picket diameter so that the snow picket would not unduly influence CO plume evolution. This experimental design choice limited the number of possible deployments.

“The main conclusion of the paper is that ‘atmospheric pressure gradients can induce subsurface advection’. This is not an entirely new result.”

Author response: The main conclusion of the manuscript is that the evolution of a trace gas plume in snow under windy conditions reveals an advective signature as well as the presence of preferential pathways. Previous work has inferred gaseous advection using temperature measurements (Albert and Hardy, 1995, Sokratov and Sato, 2000) and point-based measurements of tracer gas concentrations (Albert and Shultz, 2002). What is innovative here is that we have explicitly resolved advection in a plane (e.g. 2D) using tracer gas.

“Neither a highly resolved density profile, specific surface area measurements, measurements of spatial variability (using e.g. near-infrared photography, Tape et al. 2010) were applied.”

Author response: Indeed, the suggested field-based measurements of snow characteristics would improve context for the results. However, there is a scale mismatch between the high-resolution sampling suggested by the reviewer and the representative volume of the tracer gas measurements. For example, a highly resolved density profile of the 1m $\times$ 1m $\times$10cm domain using a 100 cc Hydro-tech sampler would have required 1000 measurements. Optimistically assuming that no samples were damaged, at 1 minute per sample this would have required 16.7 hours to complete and still would not describe the snow with fine enough resolution to model airflow for a particular snow state. Near-IR photography gives fine-scale resolution of a 2D section but not in 3D and not with the precision needed to represent snow topology accurately enough to model differences in airflow in the presence of weak and/or transient pressure gradients. The authors are not aware of a successful cast of a snow sample this large (approximately 1m $\times$ 1m $\times$ 20cm) by current techniques. The snow samples in Calonne et al. (2012) were cubes ranging from 2.51mm to 9.16mm per side. More than 105 samples of the 9.16mm per side cube would be needed to describe the deployment volume. In short, a method to image a volume of snow on the order of that used in this experiment with sufficient precision to simulate fine-scale airflow through natural snow has not yet been demonstrated.

“The use and progress of this paper for interpretation of advective flows in snow is therefore very limited beyond description.”

Author response:

Quantitative analysis is limited because we characterize air movement in two dimensions rather than three. A lack of concentration measurement in the vertical coordinate limited the quantitative results because we could not resolve vertical mass flux. Nevertheless, we do quantify how the normalized Peclet number changes between a windy and calm case. Technological improvements in imaging larger sections of snow structure in 3D would facilitate quantitative analysis and model simulations.

“The presented model, assuming isotropic and non-layered properties of the snowpack, is very simplistic.”

Author response:

The simplicity of an analytical advection-diffusion model is precisely what makes it an effective standard that, when compared with real-world experimental results, highlights the deviation of an experimental result from an idealized scenario. This technique allows us to discriminate deviations from isotropy that characterize real-world snowpack state.

Specific comments

Title: Snow is always porous and air is a constituent, and there is no closed porosity (the major difference to firn). So "interstitial air" is redundant. A better fitting title could be: "A trace gas method of evaluating macroscopic air advection and diffusion in snow"

Author's response to title:

Thank you for this suggested change. Our intention was to emphasize that the manuscript addresses air contained within the top layer of the snowpack. We agree with your assertion and changed the title.

page 2,line 4: More recent measurements show that basic properties of the snowpack do change often in a very complex way within one layer. The traditional method to characterize a snowpack requires usually cast samples (e.g. Arakawa et al.) or other recently developed quantitative techniques.

Author's response to page 2,line 4:

(This issue was addressed above in the general comments section.)

page 3, line 15: The backfilled snowpit (dimensions?) could be a major source of disturbance for the measurements, as the density (and consequently permeability) is easily increased by about 20%. Any checks or numerical simulations of this effect?

Author response to page 3, line 15:

The snow pit merely provided access by which to place the snow pickets horizontally into undisturbed snow. The CO measurements were acquired in undisturbed snow. The snow pit was backfilled to minimize surface roughness gradients that might generate a pressure field that could influence CO plume evolution. The snow pit wall was parallel to the prevailing wind direction so as to minimize the disturbance to in-snow advection. The snow pit depth was equal to the surface layer of snow and the width was approximately the 1-m length of the snow pickets. The manuscript was modified to clarify these points. For clarity, we changed the verbage from “snow pit” to “snow trench” in the manuscript.

Author’s response to page 2, line 15:

page 3, line 20: What is "relatively high-density, spongy snow"? Which method was used (beyond interpretation of the measurements) to assure that no leakage occurred?

Author’s response to page 2, line 20:

The average snow density of the snow layer in which the snow pickets were deployed is provided in column 5 of Table 1 for each case. The snow hardness is provided in Table 2 (since condensed to Table 1). We agree that the term “spongy snow” is vague and non-standard. We will instead use the density and hardness measurements to characterize the type of snow that seats snugly against the snow picket. Snow having a hardness of 1-finger seemed to form the best seal against the snow picket. Leakage around a picket was inferred by anomalously high propagation rate of the CO gas as detected by sensors along a single picket.

page 4, line 1: how was homogeneity measured? A single storm event can easily create several mm-thick denser layers.

Author’s response to page 4, line 1

We changed “homogenous” to “discrete”. Plume evolution of the trace gas releases reveal snow inhomogeneities. We revised the manuscript to clarify that we do not expect a snow layer to be homogenous.

page 5, line 15 ff: Riche and Schneebeli (2012) measured enhanced horizontal thermal conductivity in snow with little or no temperature gradient metamorphism. This would contradict the general statements about the snowpack in his paper. Clearly, anisotropy at several scales (mm to dm) is a key factor for diffusive processes. We rephrased this section. We concur that anisotropy is a key factor for diffusive processes and the difference in length scale between pore space dimension and the length of, for example, a fracture poses modeling challenges for permeable media.

Author's response to page 5, line 15:

page 7, line 28 (and other places) please define "low density snow" "high density snow" in quantitative terms.

Author's response to page 7, line 28:

We changed this sentence replacing "low density snow" and "moderate wind speed" with values given in Table 1. page 9, line 19: The conclusion drawn here is not well supported. The observed pattern (especially Fig. 8)= is in my view not at all conclusive (the point x=20,y=30 could be an outlier).

Author's response to page 9, line 19:

We agree that the observed pattern in Fig. 8 at point x=20, y=30 is an outlier and examine a probable cause for this anomaly on page 9, line 9. The conclusion is not based on the value at x=20, y=30 but rather on the larger normalized Peclet numbers throughout the domain in Fig 8a relative to Fig 8b.

Table 1: Definitions of the snowpack are insufficient for any comparison or application of the results. How was the density measured? What was the vertical spacing? What was the snow type (see International Classification) etc.

Author's response to Table 1:

We added a description of how density was measured in the methods section. The snow type is shown in Table 2, column 3.

Table 2: What was the snow temperature / temperature profile? The description of the snow seems to indicate that the snow stratigraphy was rather complex (guess ...)

Author's response to Table 2:

In the updated manuscript we clarify that measurements are acquired in a single, thick surface snow layer so a detailed stratigraphic description and temperature profile below the layer in question is not relevant.

Searching for papers about this topic, I found the following references, which seem to be relevant to the topic: Massman, W. J., and J. M. Frank (2006), Advective transport of CO 2 in permeable media induced by atmospheric pressure fluctuations: 2. Observational evidence under snowpacks, J. Geophys. Res., 111(G3), 1–11, doi:10.1029/2006JG000164. Ebner, P. P., M. Schneebeli, and A. Steinfeld (2015), Tomography-based monitoring of isothermal snow metamorphism under advective conditions, Cryosph., 9(4), 1363– 1371, doi:10.5194/tc-9-1363-2015. Massman, W. J. (2006), Advective transport of CO 2 in permeable media induced by atmospheric pressure fluctuations: 1. An analytical model, J. Geophys. Res., 111(G3), 1–14, doi:10.1029/2006JG000163. Ebner, P. P., M. Schneebeli, and A. Steinfeld (2016), Metamorphism during temperature gradient with undersaturated advective airflow in a snow sample, Cryosphere, 10(2), 791–797, doi:10.5194/tc-10-791-2016. Ebner, P. P., C. Andreoli, M. Schneebeli, and A. Steinfeld (2015), Tomography-based characterization of ice-air interface dynamics of temperature gradient snow metamorphism under advective conditions, J. Geophys. Res. Earth Surf., 120(12), 2437–2451, doi:10.1002/2015JF003648. Other references: Tape, K. D., N. Rutter, H.-P. Marshall, R. Essery, and M. Sturm C3 (2010), Recording microscale variations in snowpack layering using near-infrared pho- tography, J. Glaciol., 56(195), 75–80, doi:10.3189/002214310791190938. Arakawa, H., K. Izumi, K. Kawashima, and T. Kawamura (2009), Study on quantitative classification of seasonal snow using specific surface area and intrinsic permeability, Cold Reg. Sci. Technol., 59(2), 163–168, doi:10.1016/j.coldregions.2009.07.004. Calonne, N., M. Montagnat, M. Matzl, and M. Schneebeli (2017), The layered evolution of fabric and microstructure of snow at Point Barnola, Central East Antarctica, Earth Planet. Sci. Lett., 460, 293–301, doi:10.1016/j.epsl.2016.11.041.

Author’s response to references:

Thank-you for the list of references.

Please also note the supplement to this comment:
http://www.the-cryosphere-discuss.net/tc-2017-9/tc-2017-9-AC1-supplement.pdf

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