Author response to comment on "Explaining CO_2 fluctuations observed in snowpacks" by Laura Graham and Dave Risk

By Anonymous (Referee 2)

(referee comments in black, author responses in blue)

This work presents both field data and a simple model to address a methodological problem with winter CO2 flux measurements. While both field and model data are presented it isn't really a model-data comparison as the field data isn't directly compared to the model data. As currently structured, there isn't a convincing narrative nor is it clear what is novel. While continuous CO2 datasets are not that abundant, the authors don't present that data and focus on a confusing method of comparing CO2 concentrations to wind speeds. They have adapted a soil diffusion model to the soil-snow system. I'm not sure if the model is too simplistic or if the text just needs greater clarity, but I couldn't follow how this model could help explain the field data. Figure 2 shows that it can create a step change in CO2 concentration gradient, but that isn't enough to be able to match the field data. The paragraph starting on P10 L8 describes how combining modeling and field data could be really powerful, but the temporal changes in the CO2 concentration gradient related to advection that we know happen based on the field data presented here and elsewhere weren't clearly shown. The writing is generally good; my main criticism is that the flow of the narrative, in particular the connections between paragraphs, could be improved. In addition, both the introduction and conclusion sections contain overly broad statements that aren't supported by the rest of the paper. There is certainly need for this type of work and perhaps with some modifications to the model and/or greater clarity of what was done in both the text and the figures could show how the model and field data can be compared, this paper would be acceptable for publication. Finally, I appreciate the authors presenting all the CO2 data. However, it is somewhat surprising to me that snowpack CO2 concentrations could be as low as 151 ppm on Page 6 or well under 400 in Figure 4 or that the atmospheric concentrations was 512 ppm on Page 7, about 100 ppm higher than what it should be. Could the authors justify these seemingly strange measurements?

Thank you kindly. Our goal is that the Biogeosciences community can easily understand this CO2 model-data work, and therefore we appreciate these thoughtful comments by the referee. Through this review process, we hope to clarify the text, improve flow, and solidify how this simple model can be used to help us understand the physical processes of CO2 transport through snowpacks—and not simply generate the CO2 concentrations observed in the field.

Variable concentrations are addressed below with the P6 L34 comment.

P1 L19-21 These lines are way too general for the rest of the paper. The paper is about making the winter flux techniques better not about the soil C and the global C cycle. Or at least it needs to be demonstrated how the results might directly affect the global C cycle.

Introduction altered, as suggested.

P2 L1-3 The authors haven't presented any evidence for yet about why rates might be underestimated

Referee 1 had a similar comment—refer to our reply to Referee 1 for details.

P2 L4-5 This paragraph is just about using the diffusion gradient in the snowpack method to measure soil flux. This method should be explained and its advantages and disadvantages to the other methods should be described.

Detail added, as suggested.

P2 L10-12 This is the key piece of knowledge that this paper is attempting to explain. There is a methodology for measuring CO2 flux that has known limitations. The net result is that it is difficult to separate variability from advection (an artifact) from microbial processes (the actual goal). It would be very helpful if there was a standard correction that could be used with the diffusion based method to account for advection.

We agree, a standard correction that could be used with the diffusion-based method to account for advection would be very helpful. However, at this point, we must first understand the physical processes of CO_2 transport through snowpacks before we can move toward pinpointing a standard correction.

P2 L13-31 Somewhere in this paragraph it needs to be made clear that the assumption is that CO_2 production is happening in snow-covered soils, but there are methodological limitations to how the production is quantified. This paper is not about the controls per se, but about how to overcome the methodological limitations so that the mechanisms of CO_2 production can be studied.

Thank you for pointing this out. Detail added, as suggested.

P2 L32-34 It would be helpful to talk about of the role of timescale in advective processes in the introduction to justify why you are looking at the hours to days timescale. There is a mention of it in the discussion, but is important here too.

Detail added, as suggested.

P3 L14 Delete the sentence starting with obviously. How do “variable meteorological conditions” affect snow depth?

Sentence deleted, as suggested. Detail added to explain how “variable meteorological conditions” affect snow depth, as suggested.

P3 L16 How long were the measurements made during winter 2014? (as well as winter 2015 in line 34).

Thank you for picking up on this oversight. Detail added, as suggested.

P3 L32-37 I’m confused by this paragraph. In the preceding paragraph, it says that at each of the two stations there were CO_2 measurements at 0, 50, and 125 cm, but now it says 25, 50, 75, and 100 cm for NM2. Similarly, in this paragraph, it says the data were collected hourly whereas above it was half hourly. Were the Eosense sensor in addition to Vaisala sensors at the depths above? These sensors need more description. You already said that snow depth and wind speed were measured at both stations in lines 29-31. Somewhere in the methods or results can you indicate what the timing of snow-cover and the maximum depth were.

Detail and clarification added, as suggested. There were some differences and design improvements between the stations for winters 2014 and 2015, which should certainly be made clearer in this paragraph.

P4 L5-7 You need to say why it is important for steady state conditions that snow depth had not changed. Is there a quantitative way that this was determined?

Detail added concerning why it is important that snow depth had not changed. Yes, there was a quantitative way to determine “no change in snow depth”. This detail will be added to the updated manuscript.

P4 L9-11 What does “ideal” mean?

“Ideal” in this sense refers to the best set of environmental conditions for which a strong negative correlation between CO_2 concentrations and wind speeds could be found.

P4 L18-22 I’m confused about the time period selection. Let’s say the snowpack didn’t change depth from January 1 to January 5th. Did you look at that whole time period with one regression? In the caption for figure 2 it says that the average number of 30 minute measurements in the filtered dataset was 29-50 at different heights/sensors. Does that mean the snow depth was changing every day? Or was there some other criteria besides snow depth changing that set the length of time. It seems like the time period selection was based on finding a change in CO_2 after an increase in wind speed. Is this assuming that advection is equally affecting the snowpack from the soil surface to the snow surface? Is that a valid assumption? I'm not convinced that this is the right approach to determine the effects of wind on snowpack CO_2, but there needs to be a better justification and description of the approach.

The referee's understanding is correct—if the snowpack did not change depth from January 1 to January 5, that entire time period was looked at with one regression. We believe the referee is referring to the caption for Table 2, not Figure 2. The average number of 30-minute measurements is 29-50 at different heights/sensors to effectively indicate "average sample size", and does not necessarily indicate that the snow depth was changing every day. This n indicates the mean number of values (each value is one half-hourly measurements) that was used for each group of regressions (for a given sensor height at each station). Since measurements were recorded half-hourly, we can see how the average duration of each time period ranges from 15 h to 25 h. A potential conclusion from this could be that the snow depth is changing every day—however, there are three distinct criteria that were used for data filtering, as indicated in section 2.2 and in the caption for Table 2. We recognize that this filtering biases our dataset towards having negative relationships between CO_2 concentration and wind speed, but this was necessary in order to pick out the advective events we were interested in investigating further.

Is there a different way to do this calculation with fewer assumptions? For example, could you calculate the R^2 for the snowpack concentration gradient every 30 minutes and plot that vs. wind speed as a test of whether the wind speed affects the predicted gradient with zero wind? Or compare the concentration gradient to the wind like Seok et al. (2009)? If you examine every time point there is the problem that the previous time points likely affect the relationships. Perhaps you could also try averaging different lengths of time (e.g. 30 minutes to 12 hours or longer)?

The referee presents an interesting suggestion here. However, given the setup of our experiment, it would not be possible to calculate the R^2 for the snowpack concentration gradient every 30 minutes. Though it is possible to have higher frequency measurements with the instrumentation we were using, we recorded one value for every 30-minute time interval in order to save battery power—a concern especially when attempting to collect continuous overwinter measurements (with limited solar power, while at a remote location). Evidently, a 1-point regression would not be possible. Though it may be possible to compare concentration gradient to the wind like Seok et al. (2009) with our 2014 data, we reserved this for our 2015 data (Figure 5) when we had more sample heights throughout the snow profile. Though we could try averaging different lengths of time, we are unsure of what purpose that would serve, as we were specifically looking for time periods within our dataset when we would find a negative correlation between wind speed and CO_2 concentration within the snowpack.

More detail can be added to this section, especially to indicate that we acknowledge a bias in our data filtering technique.

P5 L6-11 This paragraph needs to be clarified to describe what the model is doing. How are the initial conditions set? What are "step changes in transport rate?" I thought there were step changes in the parameters. Based on figure 2 there is some constant CO_2 gradient before time zero and then there is a step change in the diffusivity and the CO_2 gradient adjusts quickly. Is there a fixed emission of CO_2 from the soil and then the combinations of the diffusion/depth parameters determine the CO_2 concentration gradient? Is there a temporary step change and then it returns to the initial value? Or does Storage flux needs to be defined, perhaps even in the introduction. Why is the soil diffusivity included in the model? How is snow depth included? Is the model run with all possible permutations of the 3 parameters?

Detail can be added and this paragraph can be clarified, as suggested. For instance, initial conditions are mentioned in the last two sentences of that paragraph already, but perhaps not clearly stated as initial conditions: "Snow diffusivity before the step change was held constant at 8.06×10^{-6} m^2s^{-1}. Each model run began with the system in equilibrium state (with storage flux set to 1 μmol m^2s^{-1})." "Step changes in transport rate" refers to the step changes in the parameter "snow diffusivity at step change"—this can be easily clarified. The referee is correct: there is a fixed emission of CO_2 from the soil in the model, and the changes in parameters determine the CO_2 concentration gradient (and therefore calculated storage flux). A definition of storage flux can be added for further clarification. Soil diffusivity was included in the model to determine if CO_2 transportation in a diffusive model behaved as expected with an abrupt vertical switch in diffusivity. A description of how snow depth is included is in the paragraph immediately preceding the paragraph in question. Yes, the model was run with all possible permutations of the 3 parameters—this will be clarified both in the text and in the Table 2 caption.

P5 L24-25 What does "the rate at which modelled CO_2 responded to an induced wind event" mean? What is the rate? Is an induced wind event the same as an advective wind event in line 15?

The "rate at which modelled CO_2 responded to an induced wind event" refers to change in CO_2 over time (ppm/s) with the step change in snowpack CO_2 diffusivity. Graphically, it is the slope of the line of recovering modelled flux. Yes, an induced wind event is the same as an advective wind event in line 15. Clarification added, as suggested.

P5 L30-31 What is the "enhanced concentration profile experiment?" How were the data processed?

Clarification added, as suggested. The "enhanced concentration profile experiment" is the winter 2015 data. The data "processing" refers simply to how the rate of change of CO_2 per unit time after a noticeable wind event was calculated (indicated at the end of the sentence).

P6 L14-19. I'm confused by this example. I thought the ideal situation was when wind speed increased gradually and then abated. These figures show the opposite with wind speeds gradually decreasing and then increasing. Figure 3 seems like a good example, but I'm confused by Figure 4. The snow-atmosphere interface seems hard to measure. Is there a time period with a deeper snowpack that could be shown instead? Why are the CO_2 concentrations so much lower (360-390 ppm) than atmospheric (512 ppm)? There was a brief period when the concentration was around 420 ppm that seems to be driving the relationship in this case. If those few hours of data were removed, it doesn't look like there would be much of a relationship.

Clarified, as suggested. For instance, though wind speed increasing gradually and then abating is stated as an ideal situation, the opposite process could be considered similarly ideal—this edit has been made. Figure 3 shows the same time frame, but deeper in the snowpack. Showing these figures side-by-side allows for direct comparison of how the CO_2-wind relationship changes with depth into the snowpack. The CO_2 concentrations here are considerably lower than the average reported atmospheric concentration, yes. However, there is considerable variability in atmospheric CO_2 concentrations throughout the sampling period.

P6 L20-22 Wasn't data collected the whole winter? The data shown in figure 3 and 4 are not included in this time period. This should be clarified in the methods section.

Clarified, as suggested.

P6 L22 How come there is no data for the 0 depth on NM1?

Clarified, as suggested.

P6 L34 How can you get a concentration of 151 ppm CO_2 in the snowpack? Is CO_2 being consumed or is it some kind of measurement error? Similarly, why is the atmospheric CO_2 512 ppm (P7 L5)? Is this a calibration error.

Detail added, as suggested. Upon further inspection, some values were not filtered out before making the plot. The 151 ppm value is the typical reading when the sensor relay did not turn on. Other than the 151 ppm value, most values are > 290 ppm and sometimes low values can arise if water droplets freeze in the tubing and a suction creates subambient pressure in the sensor. We have filtered these out as causation is known, and this should have been done before. Thanks to the reviewer for catching this. Average atmospheric CO_2 over the sampling period in 2015 is calculated as 512 ppm. There is probably some calibration offset involved here, though sensors were calibrated immediately before the study, and checked afterwards. It is also important to recall that the sensors have an error of several percent. But lastly it is important to remember that those measurements are taken just above the surface and also include stable nighttime measurements when concentrations just above the surface often increase significantly. At some of our sites in Canada, we have observed night concentrations in excess of 700 ppm, as measured with a Picarro CRDS analyzer. For this study, our sites are productive systems with warm winter soils that never freeze because of early snowfall, and so healthy respiration in winter does have an important effect on concentrations just above ground level, and are in this case unbalanced by photosynthetic processes. Oscillations in atmospheric CO_2 concentrations with time will have interesting feedbacks to the snowpack concentration profile, which we have seen in modelling, but that's another study.

P7 L5-7 Either test for a relationship between wind speed and CO_2 or delete this sentence. It is surprising to me that so much of the first week or so there is no concentration gradient in the snowpack. That is on the 16th-17th and 19th-23rd the whole snowpack is essentially the same as the atmosphere. This seems somewhat surprising. It doesn't seem that much windier than the second week of the experiment. What is happening?

Clarification added, as suggested. For instance, "may have" is changed to "was". Based on the data we collected, it is unclear why the snowpack concentrations from the 16th-17th and 19th-23rd are essentially the same as the atmosphere. Possibilities include ice lenses, temperature changes, or changes in density. Referee 1 had a similar comment—refer to our reply to Referee 1 for details.

P7 L18, L23 What does equilibrium mean? Based on Figure 2 it seems like the model has some step change in parameter and then the CO_2 gradient adjusts instantaneously. Similarly, I don't know what a scenario is.

Detail added, as suggested. Equilibrium refers to no change in the modelled storage flux (storage flux at a constant 1 μmol $m2s^{-1}$). A scenario is a model run under a given set of parameters.

P7 L31-34 I'm not sure why soil diffusivity was included the model as a parameter.

Detail added, as suggested. Explanation also found with P5 L6-11 comment.

P8 L6-13 There are two different phenomena described in this paragraph. One is that there is a monotonic decrease in CO_2 from the soil surface to the snow surface if the only source of CO_2 production is the soil. I'm sure with ice lenses or if the density of the snowpack isn't constant that there are ways this couldn't be true, but it seems like this should generally be true. The more important question is whether there is a relationship between CO_2 concentration and wind speed. The authors have chosen to look over time to see if

Referee 1 also brought up the consideration of ice lenses and variations in density—refer to our response to Referee 1 for further detail. P8 L6-13 comment was left incomplete by Referee 2.

P8 L14-17 You should either calculate storage fluxes or remove this paragraph.

Storage fluxes could perhaps be calculated with this data. However, it is still important to look at concentration gradients before over-complicating our understanding of the physical processes.

P8 L18-26 I'm not sure why you can conclude that advective transport needs to be taken into account by the fact that during 33.6% of the time analyzed there is a relationship between CO_2 and wind speed. These two paragraphs don't have much quantitative analysis in them.

The 33.6% value refers to a simple calculation of percentage of wintertime measurements that satisfied all three conditions. This statistic is important especially since the filtering process biased the data we presented. This is clarified, as suggested.

P8 L27-35 Can you give a clearer description of how these processes that occur at different time scales would affect the CO_2 concentration gradients and the fluxes measured with Fick's Law? What are "a continuously enhanced friction velocity" and "an enhanced diffusive regime?" It seems like 54 days of measurements should be enough to capture some synoptic variability if you looked for it.

Detail and clarification added, as suggested.

P9 L9-12 I'm not sure I understand exactly what the model did or what equilibrium means. If I look at figure 2 it seems like an instantaneous change in diffusion led to an instantaneous chance in concentration gradient. I don't see any change over time in CO_2 concentration which is what I would have thought disequilibrium would be.

Clarification made, as suggested. Figure 2(b) does in fact indicate an instantaneous change in CO_2 concentration, along with the instantaneous change in concentration shown in Figure 2(a).

P9 L13-14 I don't understand these sentences.

Sentences clarified in the text. Generally speaking, these sentences are meant to indicate that the diffusive model used can be used to mimic advective events, and that this method is simpler than other models that use a diffusive-advective coupled approach.

P9 L16-27 Why not try to match the model conditions exactly to the field conditions to start at least in terms of CO_2 concentration

We tried to start with reasonably close conditions, but did not seek to match exactly because of course we had a broad portfolio of starting concentrations to begin with from the various sites and depletion events. Other studies have applied an iterative procedure to do something similar, and could be applied on a site by site basis if adapted to snow environments—as the referee suggests (e.g. Latimer and Risk, 2016). Though we could have attempted this, our primary goal was to properly understand and illustrate the underlying physics of CO_2 transport through snowpacks, and to test whether a simplified enhanced diffusion model could be used—because simplified techniques would make a better basis for iterative field-model fusion applications. As such, matching the model conditions exactly to the field conditions is unnecessary.

P10 L6-7 Just like the beginning of the introduction, this sentence seems like an overreach with no connection to the rest of the text.

Adjustment made, as suggested.

P10 L11-14 This seems like where model data synthesis could really move this field forward.

We agree. However, we believe that we must first have a thorough understanding of the most basic physical processes of CO_2 transport within and through the snowpack. There are many other more complicated ways of modelling CO_2 transport in various diffusive media—this is a simpler technique to get to the basics of the differences between diffusion and advection of CO_2 in snowpacks.

P10 L15-21 Alternative measures of CO_2 flux need to be discussed earlier in the manuscript.

Detailed added, as suggested. This is also mentioned in the comment referring to P2 L4-5.

P10 L22-27 This study show snow profile depletions, but I wouldn't say that it explains them. While I agree with the sentiments in the rest of the paragraph, they aren't direct conclusions of the work here.

Text altered, as suggested.

Figure 1. This figure can be deleted. Or it needs to be improved so that the labels match up to the icons and the depths are shown.

Figure improved. Depths are not shown, as the schematic represents two winters with slightly differing sensors depths.

Figure 2. Indicate that an instantaneous change in the diffusivity mimics advection. Storage flux needs to be defined in the text somewhere. Either call it storage flux or apparent storage flux. Indicate the depths on panel b.

Clarifications made, as suggested.

Figure 3. Use the data not the record number on the x axis. Would you expect there to be a hysteresis because of advection? That is, if you drew a line connecting all these points in time would the concentration be higher than average when the winds are decreasing and then lower than average as winds are increasing again? Or maybe vice versa? I realize it is not a crucial question for the model-data comparison, but it seems important for converting concentration gradients into fluxes.

Date used instead of record number on the x-axis, as suggested (applied also to Figure 4). If there is some sort of hysteresis due to advection, it would likely be very hard to distinguish in a time span of hours to days.

Figure 4. Why not pick a time to show when this sensor is really in the snowpack?

A corresponding time when the sensor is "really" in the snowpack is shown in Figure 3. By showing a sensor higher up in the snowpack (closer to the atmosphere) in this figure, we are able to demonstrate a difference in CO2-wind speed relationship with height within the snowpack.

Figure 5. The different colors/dashes are hard to see. It would be better if the wind speeds were in a separate panel. Atmospheric CO2 probably isn't necessary to snow either.

Clarification to the different colours/dashes has been done, as suggested. Atmospheric CO2 is removed, as suggested. We believe wind speeds on the same panel allow for easier direct comparison, even if the figure appears to be confusing at first glance. The wind speeds can be placed in a separate panel, if necessary.

Figure 6. The dashes are hard to distinguish. Why are there 4 cases for a and b but only 2 or 3 for c and d? Are the lines on top of each other? If so, make this clear in the caption.

Dashes altered, as suggested. There are 4 cases for a, b, and d, and only 2 cases for c. Lines are on top of each other in d. The 2 cases for c both refer to short-term storage flux—factor increase in CO2 flux for long-term storage flux is incalculable (0 divided by 0). These clarifications are added to the caption, as suggested.

Figure 7. I like the idea of a conceptual figure, but I'm not sure why lots of little arrows represent diffusion and one big arrow represents advection. Can you make something that shows how the concentration gradient and fluxes change over time in response to wind? Maybe some combination of the information in Figure 2 and Table 3 along with a calculation of the flux using Fick's law and a calculation of the storage flux over time?

A change to this conceptual figure with an "x-axis" demonstrating time will help clarify this confusion with different sized arrows representing diffusion and advection: small, constant movement of CO2 (diffusion) is represented with small arrows, whereas larger packages of CO_2 (advection) moves less frequently and is represented with a larger arrow.

Table 2 is a good summary, but it seems like you can get rid of n as duration is essentially n/2

Though duration is essentially n/2, it is important to show both n and duration. This is because they have different purposes: n gives an indication of the robustness of the R^2 measurements, whereas duration gives a more practical visualization of the length of the time periods.

Table 4 Can these events also be shown on figure 5? The measurement depth is in the caption and can be removed from the table.

Thanks for these two suggestions. As space allows, these events are added to figure 5 (there is a lot already going on in Figure 5, but we understand the importance of pointing out the data that was analyzed further). Measurement depth removed from table, as suggested.