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Interactive comment on "Explaining CO_2 fluctuations observed in snowpacks" *by* Laura Graham and David Risk

Anonymous Referee #2

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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

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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?

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.

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

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.

P4 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.

P2 L13-31 Somewhere in this paragraph it needs to be made clear that the assumption is that CO2 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 CO2 production can be studied.

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.

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

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

P3 L32-37 I'm confused by this paragraph. In the preceding paragragph, it says that at each of the two stations there were CO2 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.

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?

P4 L9-11 What does "ideal" mean?

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

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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 CO2 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 CO2, but there needs to be a better justification and description of the approach.

Is there a different way to do this calculation with fewer assumptions? For example, could you calculate the R2 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)?

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 CO2 gradient before time zero and then there is a step change in the diffusivity and the CO2 gradient adjusts quickly. Is there a fixed emission of CO2 from the soil and then the combinations of the diffusion/depth parameters determine the CO2 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?

P5 L24-25 What does "the rate at which modelled CO2 responded to an induced wind event" mean? What is the rate? Is an induced wind event the same as an advective

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wind event in line 15?

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

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 CO2 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.

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.

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

P6 L34 How can you get a concentration of 151 ppm CO2 in the snowpack? Is CO2 being consumed or is it some kind of measurement error? Similarly, why is the atmospheric CO2 512 ppm (P7 L5)? Is this a calibration error.

P7 L5-7 Either test for a relationship between wind speed and CO2 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?

P7 L18, L23 What is equilibrium mean? Based on Figure 2 it seems like the model has some step change in parameter and then the CO2 gradient adjusts instantaneously. Similarly, I don’t know what a scenario is.

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

P8 L6-13 There are two different phenomena described in this paragraph. One is that there is a monotonic decrease in CO2 from the soil surface to the snow surface if the only source of CO2 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 CO2 concentration and wind speed. The authors have chosen to look over time to see if

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

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 CO2 and wind speed. These two paragraphs don't have much quantitative analysis in them.

P8 L27-35 Can you give a clearer description of how these processes that occur at different time scales would affect the CO2 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.

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 CO2 concentration which is what I would have thought disequilibrium would be.

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

P9 L16-27 Why not try to match the model conditions exactly to the field conditions to start at least in terms of CO2 concentration

P10 L6-7 Just like the beginning of the introduction, this sentence seems like an over-

reach with no connection to the rest of the text.

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

P10 L-15-21 Alternative measures of CO2 flux need to be discussed earlier in the manuscript.

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.

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 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.

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.

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

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

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caption.

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?

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

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.

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