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

By F.A. Rains (Referee 1)

(referee comments in black, author responses in blue)

General Comments

First of all, I would like to say this was a very well put together study. Having performed wintertime respiration measurements myself, I know that it is not an easy task, kudos. Also, the system design seems robust, and accurate. Please take the below questions/comments with an open mind. Does the rate of flux affect the total quantity of CO2 released to the atmosphere? A bit of a rhetorical question, however this seems pertinent. It's clear that total C released is obviously a significant metric, but perhaps you could expand on how/why the rate of release is significant.

Thank you kindly. The referee's insightful and thoughtful comments will help clarify some key concepts throughout our study.

The question of whether the rate of flux affect the total quantity of CO2 released to the atmosphere is indeed important, and should not be overlooked. Though we mentioned in the Introduction the importance of organic C reserves in high latitude soils, we acknowledge that we neglected to make the connection to fluxes from soils and their contribution to atmospheric CO2 concentrations. This detail is added to the introduction, as suggested, along with corresponding references (e.g., Raich et al., 2002).

You mention a trend of thinning snowpack in North America over the last number of decades. Also mentioned is the insulating effect that a deeper snowpack plays in allowing microbes to exist/or allowing for microbial respiration. It would follow logically then that assuming no change in air temperature, a thinning snowpack would decrease microbial, wintertime respiration by allowing the soil to reach sub 0 Celsius, or whatever that lower threshold may be. This may be slightly off topic but it seems related and pertinent. This could perhaps be addressed by mentioning other long term meteorological trends in North American winters, such as average air temperature, etc…

Thank you for bringing this up. There are certainly some perceptible flaws in this logic, which can be clarified with the addition of detail regarding long-term meteorological trends in North American winters. One helpful assumption is that this thinning snowpack is a result of increasing air temperatures with the onset of anthropological climate change (rather than assuming no change in air temperature). Dyer and Mote (2006) help to address this, with their study indicating earlier onset of spring melt (associated with higher temperatures and variations in precipitation). The most important details are perhaps that there is still significant global snow coverage despite increasing global temperatures, and that soil respiration occurs beneath this snow. Thinning snowpacks are certainly prevalent on average, but air with the coldest temperatures have the lowest ability to hold water vapour—so more intense individual snow events are likely to occur with increasing air temperatures.

Content Comments

Section 1, lines 28-29. An example of "underestimating" winter contribution to atmospheric C would be supportive of your statement. It seems that assumptions are being made that current models assume that the wintertime contribution is nil. In fact some models may over estimate this variable. Again, an example of a widely used, modern model that excludes or under represents wintertime production of CO2 would be illustrative.

The referee raises a good point here. Rewording is necessary, as it has proven difficult to come up with a specific example of a widely used, modern model that underrepresents wintertime production. Instead, we can draw our attention to the fact that seasonal variation in soil CO2 fluxes is not always mentioned in meta-analyses of global soil carbon studies (Scharlemann et al., 2014). Though wintertime measurements may have been incorporated into individual studies, by neglecting this information in a meta-analysis, the reader is left wondering if overwinter CO_2 emissions were taken into account at all. Rather than imply that all current models assume that wintertime contribution is nil, we clarify in the manuscript that there is an existing abundance of growing season studies and a general lack of wintertime CO_2 soil knowledge, along with continued efforts to include non-growing season/overwinter soil CO_2 emission measurements in various models and inventories (Fahnestock et al. 1999, Raich and Potter, 1995).

Section 2.3 Model Development. Line 30. How did you calculate snow pack porosity, and tortuosity? Was snow pack density measured at different intervals or assumed homogeneous for the different "steps"? Also, Fick's 1st Law of Diffusion is adequate for explaining flux in a 1-dimensional, relatively homogeneous medium. However we know that a snowpack stratigraphy is highly variable in space and time. Furthermore, assuming the non-static/non-homogeneous nature of wind and how it affects the snowpack in a very localized manor, could lateral flux occur with the snow pack. Also, elaboration on the role of dense wind slabs, sun crusts, and other ice crusts/lenses within the snowpack would be enlightening. In addition, it seems plausible that Fick's 2nd Law of Diffusion could potentially be useful.

Several assumptions were made for the model and have not been previously stated clearly. As suggested, detail is added to the manuscript to further explain parameters such as snow pack porosity and tortuosity. Snow pack diffusivity values in the model were based off a range of acceptable values to encompass all possibilities in iterations of model runs (at step-change). The step-change snow diffusivity possibilities range from "dense" snow (close to soil diffusivity values) to "light" snow (close to values of CO_2 diffusivity in air). With this simplification, we were able to avoid the difficulty of estimating snow pack porosity and tortuosity, as snow pack diffusivity encompasses porosity and tortuosity measurements. Similarly, snowpack density values were not individually calculated or estimated, as snowpack diffusivity also encompasses snowpack density. It is important to note that we did not iterate through a range of snowpack diffusivities for pre-step-change conditions. Simply put, our initial conditions before the simulated advective event varied in snow depth and soil diffusivity, but not snow diffusivity. The referee brings up a critical point with our assumptions in terms of variation in snowpack density in space and time. Yes, our model does assume homogeneous density through vertical space, though "tests" a range of densities by working through a range of step-change snow diffusivities. It is certainly possible that lateral flux could occur within the snowpack, especially with wind slabs, sun crusts, and ice lenses. These physical features likely occurred at our field sites, but are unaccounted for in our modelling—as noted, modelling lateral CO_2 transport through a snowpack with this 1-D model is considered impossible. Once we breach the possibility of Fick's 2nd Law of Diffusion, we could be over-complicating the situation for what we were looking to do: understand and observe the differences in diffusive and advective transport through snowpacks, despite the challenges of wintertime measurement. A few studies in the past have used Fick's 2nd Law of Diffusion to model similar events (Solomon and Cerling, 1987), but the overwhelming majority of CO_2 diffusive studies use Fick's 1st Law. This is likely because Fick's 2nd Law of Diffusion reduces to Fick's 1st Law of Diffusion when it is simplified and applied to a steady state.

Conclusion. Why is total "accounting" via eddy covariance lacking in this regard? At the outset it would appear that eddy covariance can tell you not only the rate of flux, but the net production of CO_2 for a given footprint (accounting?), while eliminating margin for error i.e. snowpack variability. What other sources of CO_2 would be accounted for in addition to soil respiration that would not allow you assume all measured net wintertime CO_2 was in fact from the soil? A few more sentences explaining your statements/reasoning that in-situ CO_2 probes are superior would be enlightening.

Detail added, as suggested. We are not intending to give the impression that total "accounting" via eddy covariance is lacking in this regard. What we are trying to indicate here is that in-situ CO_2 probes are not superior to eddy covariance, but are typically cheaper, can be deployed more easily and more frequently, and can give us an indication of what is going on within the snowpack in terms of CO_2 transport.

Technical comments

Line numbering appears off, continues from abstract through first portion of introduction, and then switches back mid way. No other technical or grammatical errors were noted.

Thank you.

References

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