

Interactive comment on “Spatial variability of CO₂ uptake in polygonal tundra – large overestimations by the conventional eddy covariance method” by Norbert Pirk et al.

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The authors measured net CO₂ fluxes over polygonal tundra in the high arctic on Svalbard. They present an interesting and well written manuscript, but seem to interpret some aspects differently than this reviewer. I think it is important to exchange ideas and opinions between authors and reviewers and that it is positive to have new ideas presented even if not everyone agrees. However, my main points given below is that the language should reflect this in a somewhat clearer way so that the uninitiated reader does not misinterpret the universality of some statements.

Having said this, I must admit that I learned a lot reading this manuscript and fully support its publication after careful revisions.

Besides an important methodological aspect of how to compute defensible annual flux sums, a key statement of the paper is that all the detailed image analyses starting with pictures taken in 1948 cannot confirm a rapid degradation of this polygonal tundra, but rather support the view that this landscape has been quite stable over the last seven decades.

Main critique

1. Your introduction completely misguided me in the wrong direction as your paper starts with the phrase “Carbon-rich Arctic tundra soils are often covered with polygonal ground patterns created by sub-surface ice wedges.” – (1) Your paper is **not** addressing carbon-rich Arctic tundra soils! (2) Absence and presence of polygonal ground patterns is not directly related to carbon-richness of the soil (see e.g. Davis 2001). In fact, the non-orthogonal polygonal tundra patterns are mostly found on homogenous silty or sandy grounds, whereas the carbon-rich surfaces in my experience mostly show orthogonal polygonal patterns that differ from your site.

In fact, this all does not matter, it is simply a problematic first phrase (the one scientific writing is all about). Please rephrase and start your story in the direction where you actually go. In fact, only on page 12, line 8 my initial suspicion was resolved as you wrote “with its typically shallow organic horizon in the soil”.

2. Abstract. I was confused by the your flux numbers. In principle, a negative sink is a source (page 1, line 8), but as an expert I guessed that you use the negative sign for net uptake and thus a sink of minus something is still a sink (not a source). OK, my recommendation is to put the number in parentheses to avoid the interpretation that it is a source. But the most confusing statement follows in the last line of the abstract: the text in lines 6 to 8 reads like: conventional calculation gives -46 gC m^{-2} , improved ogive optimization gives -82 gC m^{-2} which is a **strengthening** of net uptake, but your

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text on line 14 calls this “a weakening of the CO₂ sink” . . . I assume you wanted the reader to read the abstract differently. Please reword and clarify. Maybe also define your sign convention in the abstract.

3. You strongly vote for the ogive optimization method. I am not perfectly in agreement with your argumentation, though. As I mentioned initially, it is good to lead the discussion, but some more critical assessment of this method is required, which should be reflected in revised wordings at several places.

Your example in Fig. 2b clearly shows gravity waves seen with the bands of lenticular clouds. Under such conditions it is challenging to filter out the waves (which should not be considered fluxes). In principle, such conditions should fail any stationarity test and one could thus think of other methods to filter out such conditions.

In the example given in Fig. 2a you basically truncate the turbulence cospectrum at 1/25 Hz, thus arguing that 25 seconds of measurements is enough to determine a half-hourly flux. This is in stark contrast to other concepts such as large eddy simulations where the generally accepted knowledge is used that it is the larger eddies—not the small ones—that are relevant for the turbulent fluxes between the surface and the atmosphere.

Long ago I had to deal with a similar issue with my first measurements over lakes (Eugster et al. 2003) and there I used the direction of the momentum flux as a filter criterion. However, some software compute the momentum flux in a way that loses the directional sign so that it is unclear in which direction the momentum flux actually pointed. In principle the momentum flux (averaged over 30 minutes) should point towards the ground surface, but my experience is that in cases as you show in Fig. 2 there might be an upward momentum flux in the high frequencies which would question your interpretation that these high frequencies are better to estimate the local CO₂ flux. This only holds if your momentum flux in the high frequency range is clearly downwards. To accept your interpretation I would need to see the cospectrum or ogive of the

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horizontal windspeed time trace and the vertical windspeed time trace. The horizontal direction must be aligned with the flow so that $\overline{v'} = 0$ and $\overline{u} > 0 \text{ m s}^{-1}$. Otherwise, if the turbulent momentum flux is in the wrong direction then your argument that the corresponding CO_2 flux must be from the local surface would be incorrect.

Maybe you have also measured a wind profile? If the peak wind speed near the surface is **below** your eddy covariance (EC) measurement height, then this would be a condition where the momentum flux measured by EC is upwards, not towards the ground. I must admit that filtering with momentum flux direction is very rigorous and in many cases may be overly picky, but I hope I could explain you why I am not really of the same opinion as you (page 13, lines 12–18): if momentum flux is upwards, then your EC system sees the inversion interface between the cold air on the surface and the warm air aloft (which is present if you have clouds as those shown in Fig. 2), not the ground interface. You may overcome this with a more critical rewording; your text on lines 13–14 does not really provide a realistic “speculation”.

4. The limitations you list on page 2, lines 9–10 do not include the factor of self-heating if an open-path instrument is used. Later we see that you used a Licor 7200, but since your introduction is more general I recommend adding a statement here (many use open-path instruments in the Arctic due to power constraints). This is a factor that Baldocchi (2003) was not aware of, thus you should mention this after the citation.

5. There is confusion about your argument why you focus on 2014/2015 data and less on 2013: on page 3, line 3 you write: “were only recorded as wet molar densities and without the cell pressure necessary to convert them to dry mixing ratios”. Is this a typo or did I misunderstand this statement? It is the H_2O density measurement that is needed to convert from wet to dry mixing ratios. Temperature and cell pressure are only necessary to convert from densities to mixing ratios. A similar confusion is found on page 8, lines 1–2: “since they were wet rather than dry molar densities”. Before you argued because of the mixing ratios, here one wonders why the ogive method should not work with wet molar densities if it would work with dry molar densities?

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Please clarify these things. In principle you could use the Webb-Pearman-Leuning correction for your 2013 fluxes. Why did you not use this method to better profit from your interesting dataset?

Detailed technical remarks

1/10: use K instead of °C for temperature differences

3/1: add “flux” in EC CO₂ flux measurements

4/21: use “s” not “sec”

5/10: correlations between time series depend on the measurement interval; an $r > 0.9$ definitely does not hold for 10 Hz data, but may be seen with monthly data. Either specify which aggregation level you talk about, or simply remove this statement in parentheses. Giving the distance is an objective information that should be sufficient.

6/11–12: wording reflects some inconsistency in your statistical testing. I assume you used a t-test, but if you write “on average 10 cm larger thaw depth” then this implies a one-sided t-tests (testing for “greater than”). The wording on the line below (“this difference is not statistically significant”) however is the wording for a two-sided test. Please rectify.

7/9: do not specify “ $p < 10^{-12}$ ” since statistical models are not supposed to be accurate down to $p < 10^{-12}$. Normally for low values it is sufficient to indicate something on the order of $p < 0.0001$ or so.

8/13: use K instead of °C for temperature differences

References

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