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Comment

***Interactive comment on* “Controls on winter ecosystem respiration at mid- and high-latitudes” by T. Wang et al.**

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Controls on winter ecosystem respiration at mid- and high-latitudes

T. Wang et al. Biogeosciences Discuss.

I am very much in line with the very detailed comments put forward previously by Werner Eugster and will not repeat all the points.

Overall: The topic of winter time CO₂ exchange is in my opinion one of the most interesting unknowns of the high northern latitudes, which is certainly not fully understood at this point. The ms use the La Thuile dataset from the fluxnet database with 57 sites in the Arctic, Boreal and Temperate region, covered with eddy covariance data to analyse winter-time exchange of CO₂, which seems like an obviously good approach

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to illustrate this problem. Unfortunate, the manuscript in my opinion suffers from a number of weaknesses in the analysis, which together with awkward definitions of key constrains, makes the manuscript unsuited for publication at the present stage. The ms uses the classical Arrhenius type equations in the interpretation of the ecosystem respiration during the winter, which seem as a natural point of departure, but does not really add to the present state knowledge, dating back to Lloyd and Taylor from 1994. Major Points: Abstract: Respiratory fluxes during winter of more than 10 g C m⁻² per day is simply not realistic and shows a lack of experience within this field of research. One must assume that the long list of very experienced never saw this part of the ms and I would recommend that the first author involve co-authors more actively in a new version of the manuscript.

Introduction: In order to compare the winter fluxes across a wide range of latitudes one needs a proper definition a winter season as a term, I'll agree to that. However, it seems to me that the latitudinal range of 34° and a temperature gradient of 24° C may be too much to illustrate the issue of cold season respiration, in a proper way, from just a few environmental parameters. This could be one of the reasons why the correspondence between RECO and e.g. temperatures in most cases is not better than the original version of the Lloyd and Taylor equation. I would suggest that a narrower range in both temperature and latitude was used to investigate this issue, as well as focus on the northernmost end of the latitudes to distinguish this study from the original Lloyd and Taylor work and many of those who followed. I do realize that this will decrease the number of data, but would assume that a more focused approach could also give more qualified explanations.

Materials and Methods: 2.1.1) I assume that the dataset used here are not gap-filled- it doesn't say so anywhere. Using a criteria of less than 30% gaps in the annual dataset could still allow substantial gaps during winter, and I would suggest that the 30% limited was applied to the winter season only. 2.1.2) Modis LAI data might be the best choice, but there is a scaling issue which is not dealt with, as it should be. I would

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suppose that significant variability in LAI exist a most sites and a spatial resolution of 1 km² cannot resolve that, at the eddy covariance scale, this should be discussed . 2.3) I find the use of two different definitions of winter confusing, especially since none of them really takes into account that winter last longer at higher latitudes, than at lower. Results: 3) The use of the 10 times (though it says 0.1 in the table) multiplication in table 2 for daily values is confusing, and seems to confuse yourself as it leads to the erratic values in the daily RECO in the abstract. Use of the term p-ration is confusing, because it is easily mixes up with the p-value used in statistics. Further, it is not clear to me exactly what you want to illustrate with this ration, please explain more thoroughly. On page 7007 you argue that Arctic wet tundra comes out with the lowest RECO, due to the low soil temperatures at the sites. I do agree that this is part of the reason, but more importantly the high standing water table of wetlands makes most of the decomposition anaerobic , and thus less efficient. Fig . 3 though trend lines are presumably significant, the explained variances are all very weak, and can in my opinion on not make basis for any conclusions. On Page 7011 the authors conclude that the sensitivity of RECO to temperature is different when looking at spatial gradient from interannual variability. Basically, I don't see any evidence of that in the material presented here or in the discussion just above this statement. 4.3) I do not see the logic behind the statements made, that you do not find any relation between soil carbon content (concentration) but still choose to use LAI as a proxy for substrate. Please, explain the hypothesis behind those apparently contradicting statements. Further, the differences in explained variance going from temperatures alone to equations (2) and (3) seem to be only marginally better than for temperatures alone.

Technical issues: The authors comments (p7008) on the influence of the “Burba correction”, which is not a fully resolved issue at this point, but do not distinguish between observations from open versus closed path instruments or whether the correction was applied or not. In my opinion this could be done and may potentially explain some of unexplained variance found in the data.

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