Junfeng Wang et al. investigated soil respiration in an alpine meadow of the Qinghai-Tibet Plateau (QTP) over almost two complete years. This investigation produced an impressive data-set of CO_2 soil respiration fluxes, which is, especially for winter-time, unique for this region and of high importance for permafrost regions in general. This data-set was used to study the impact of freeze-thaw processes on the soil respiration fluxes and the different fluxes during different freeze-thaw stages are shown. Furthermore, the regulation of soil respiration by other parameters is shown. To address the relevant scientific questions within the scope of the journal, the manuscript needs major revision with regards to description of the methods, the discussion of the results and maybe to the usage of this impressive data-set.

Major comments

In general, the title seems to be a bit misleading as the impact of freeze-thaw processes on soil respiration fluxes is not as obvious to me in the manuscript as stated in the title. Of course, it makes sense to partition the year-round measurements into different freeze-thaw stages (and for sure, there are flux differences between the freeze and thaw stages) and also it's worth to discuss the role of freeze-thaw processes on the fluxes. However, the main driver of these fluxes is still the soil temperature, which is already widely known. If the authors want to point out the significant role of freeze-thaw processes, they should state this more clear throughout the manuscript and bring more (statistical) evidence of it's impact on the fluxes. So far, the authors have shown an increase of soil respiration in the ZC substage, which might be attributed to the freezing process. However, the amount of outgassed CO_2 during this period make up much less than 5% to the annual budget and the data points during this period seem to be really sparse. During the WC, SW and ST stage the freezing and thawing processes seem to be of minor importance to the soil respiration, even though an impact during the SW stage is discussed but evidence for this impact is missing in the manuscript. Therefore, the authors might shift the focus of the manuscript towards a model-based budget of soil respiration on an annual basis (see next paragraph) or they bring evidence on a statistical basis on the regulation of freeze and thaw processes on Rs fluxes.

The flux data-set is impressive, especially as it is really difficult to conduct these chamber-based measurements during winter-time and regularly over a two-year period. Can the authors state something about similar data-sets in such areas (alpine, permafrost-affected)? Especially the winter-time soil respiration fluxes would be of interest to the reader here. How high/low are these fluxes compared to other regions and why? Aren't there other soil respiration fluxes from such areas from this pedon-scale? Then the authors should point out this uniqueness of the data-set as the winter-time Rs fluxes make up about 30% of the annual fluxes. A modeling of the Rs fluxes has been done, but from the text it remains unclear which model is used to calculate the budget (model from equation 1 or interpolation of average Rs flux rate(described at line 170)) and where the (modeled?) fluxes shown in figure 4 come from. However, to calculate an annual budget, an interpolation of average Rs fluxes seems to be not sufficient, while temperature-based respiration models are widely used to calculate flux budgets. Furthermore, the interannual-variability of the Rs fluxes between the two years might be worth to look at. Are there differences in the budgets and if so, why (e.g. it seems like the Rs fluxes from the SW stage are significantly higher in the second year)?

Some more information on soil and vegetation composition of the chamber set-up would be helpful to the reader (especially when the fluxes are compared to those from other regions). What soils are generally found in this area? Are they organic-rich/poor? What is the active layer depth? How deep are the main rooting zones of the vascular plants? If the roots mainly reach e.g. about 20cm into the soil, the insertion depth of the PVC collar might be too low as lateral roots still reach into the chamber collar and may alter the measured respiration flux. Furthermore, the closure time of the chamber is of interest. Where they similar during winter and summer-time? If the plants inside the collars were removed just one day before the measurements started, there might be some artefacts due to this disturbance (Diaz-Pines et al., 2010) that need to be taken into account. In general, a critical review of the clipping method should get more attention and it should be stated why this method was applied instead of other less disturbing methods (Subke et al., 2006). Furthermore, the reader needs to know something about the flux calculation procedure? Was a linear or an exponential model used to calculate the fluxes? Based on which quality criteria (check Görres et al., 2014)?

Two tables are missing in the manuscript. As they seem to contain a lot of information on flux details, they may already answer some of the question that are stated in this review.

Minor comments

In the abstract some abbreviations are used without an introduction, which needs to be changed.
If the authors shift the focus of the manuscript, the abstract should be changed accordingly.

Line 40: At least one citation is needed here.

Line 114: To compare the fluxes from this region with other regions it would be good to say something about the soils (carbon contents, C/N, etc) beside a detailed vegetation description.

Line 119: What about the soil moisture probes at different depths? Why were they inserted as in the end just the SWC at 5cm was used?

Line 144: Are there no differences in vegetation cover, soils, etc. so that one measurement plot in the six 5x5m measurement plots can serve as replicates? If not, there might be a chance of discussing other impacts such as carbon content, vegetation cover and more on the Rs fluxes. Anyway, a detailed description of soils and vegetation is needed here.

Line 148: What have the authors done with re-growth of plants during the measurement period. For sure, there have been some.

Line 159: Unfortunately, it remains unclear which model was used for calculating the contributions of Rs from each freeze-thaw stage to the annual budget. This must be stated clearly. So far it reads, that the resulting fluxes from equ.1 were used to describe the dependency of Rs on T, while for the budget calculation interpolated average fluxes were used. If a model exist, why interpolated averages were used then? May it would make more sense to use a temperature-based model and, as Q10 was also used in the manuscript and it is shown that there are differences between the different stages, to also include Q10 into a model (e.g. Eckhardt et al., 2019).

Line 179: ANOVA is described here but not referred to later in the text.

Line 228: Yes, there are freezing and thawing processes in the active layer, but the suggestion that they strongly regulate the Rs fluxes seem to be a bit speculative as the authors don't bring any evidence here (again, some statistics would be helpful), that there is a regulation of Rs fluxes by these processes (and should therefore be part of the discussion and not of the results). The only argument is that the freeze-thaw processes are taking place at the same time when the Rs fluxes are starting to rise (which might be simply due to rising temperature).

Line 308: Can the autotrophic respiration act as reason for the differences in Q10 here? Due to the clipping of the vegetation in the chamber plots, there shouldn't be any, right?

Line 375: As there is no clear evidence for a regulation of the Rs fluxes, the authors should be more carefully use the term 'significantly' to describe this relationship (or refer to ANOVA?). For sure, there are significant differences between the Rs fluxes from the different freeze and thaw stages, but are they really driven by the actual freezing and thawing processes or just driven by different soil temperatures of the stages?

Figure 1: Additionally, the authors should include the freeze and thaw stages in the graph

Figure 2: The authors should use a consistent date string (compared to figure 1). Furthermore, drawed lines in the graph would give a better readability to see which Rs fluxes belong to which stage.

Figure 3: Which year are those flux contributions from? Why not for both years? May a mean value would be better practice?

Figure 4: From which model are these Rs fluxes shown here? Are the SWC values relevant (if so, why aren't they included in a model?; if not, why are they shown?)?

Literature

Diaz-Pines, E., Schindlbacher, A., Pfeffer, M., Jandl, R., Zechmeister-Boltenstern, S., and Rubio, A.: Root trenching: a useful tool to estimate autotrophic soil respiration? A case study in an Austrian mountain forest, Eur. J. For. Res., 129, 101–109, https://doi.org/10.1007/s10342-008-0250-6, 2010.

Subke, J.-A., Inglima, I., and Cotrufo, M. F.: Trends and methodological impacts in soil CO2 efflux partitioning: A metaanalytical review, Glob. Change Biol., 12, 921–943, https://doi.org/10.1111/j.1365-2486.2006.01117.x, 2006.

Görres, C. M., Kutzbach, L., and Elsgaard, L.: Comparative modeling of annual CO2 flux of temperate peat soils under permanent grassland management, Agr. Ecosyst. Environ., 186, 64–76, https://doi.org/10.1016/j.agee.2014.01.014, 2014.

Eckhardt, T., Knoblauch, C., Kutzbach, L., Holl, D., Simpson, G., Abakumov, E., and Pfeiffer, E.-M.: Partitioning net ecosystem exchange of CO2 on the pedon scale in the Lena River Delta, Siberia, Biogeosciences, 16, 1543–1562, https://doi.org/10.5194/bg-16-1543-2019, 2019.