Biogeosciences Discussions

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“Carbon budgets for an irrigated intensively-grazed dairy pasture and an unirrigated winter-grazed pasture” **by J. E. Hunt et al.**

Author Response to Anonymous Referee #3

This study reports important numbers about net carbon budgets at two differently managed pastures and concludes that 1) net ecosystem carbon budget results show agreements with other previous studies done over grazed grassland 2) this finding is inconsistent with long-term carbon stock studies of other New Zealand pastures. The methodologies used in this study are well described and based on the latest procedures of eddy-covariance method. Inversely, this implies that this study puts more focus on methods and numbers rather than efforts to understand underlying biogeochemical processes. Please check comments below and hope that these comments help to improve this manuscript for better readability and contributions to communities.

Reply: Thank you for the comments approving of our methodology. Indeed our manuscript has a strong focus on describing this in detail. This is because NECB was expected to be a small difference of large inputs and outputs and therefore quite sensitive to errors in these. Given that this is the first study of irrigated pasture, we felt high priority for ascertaining the robustness of our C budget results, rather than interpreting them with a view of the underlying processes.

Overall comments:

◦ More extensive comparison with previous studies is needed. This study tries to compare their results with values reported by previous studies but remains to be superficial, especially to NEP. Particularly, in 5. Discussion, the authors briefly mention that studies of impacts on soil moisture and temperature on GPP and ER are needed. But I believe that this analysis can be done directly because GPP and ER are already computed with meteorological and soil variables. Do not stay in simple speculation only from NEP and go forward further with separate analysis with GPP and ER.

Reply: Firstly, we are unable to find the passage that we supposedly “briefly mention”. We do not state that studies of impacts of soil moisture and temperature are needed, as a large number of these already exist. For the pasture system studied here, irrigation is employed to keep the soil-moisture range small, and the other management aspects of repeated intensive grazing and fertiliser application are the strongest drivers of carbon fluxes. What we do state is that “the processes linking the distribution of soil water and nutrients with respiration need to be studied”. The key word here is “distribution”, which requires small-scale studies that were not undertaken here.

◦ The authors wrote that annual GPP of the managed pasture was twice that of the latter but ER showed about 68

Reply: this comment seems to be incomplete.

◦ The authors argue that respiration by cattle is originated from carbon in the grazing term in Eq. (3), cattle respiration should be excluded from the carbon budget equation to avoid double-counting. But I am not quite sure if this argument should go to the term of excreta, if considering that carbon uptake by cattle through grazing is conserved as the sum of respiration (metabolism) and excreta while they stay in pasture. Table 3 shows that difference between excreta and grazing is about 303 and 30 gC m-2 year-1 at the managed and unmanaged pastures, respectively and I wonder how to deal with this issue properly. In addition, if considering typical values of CO2 respiration by cows per day and grazing period of about 10 days per year, these differences seems to be related to cattle respiration. This is also important in comparing this study with previous papers because previous studies did consider the cattle respiration in the carbon budget equations. More clear description is needed.

Reply: We have dealt with this "issue" properly. Two facts are crucial to understand our approach to the C budgeting of the IFR pasture. The first is that our "pasture ecosystem" is in practice one paddock, where the NEP measurements are made, biomass removal determined, grazing events recorded etc. (We assume that this paddock is representative for all paddocks of the farm that are included in the rotational-grazing operations.) The second key fact is the short duration of cattle presences: each grazing event lasted only 1 to 2 d and was followed by a cattle-free period of 20 to 30 d. Therefore, the cattle are not part of the "ecosystem" considered by us, because most of the time they are elsewhere (in other paddocks, at milking, or off-farm in winter). Regarding the C budget of the pasture ecosystem: by determining the biomass grazed we have accounted for the C exported by the cattle. The C conversions within the animals (Appendix C) are, with one exception, irrelevant to this budget because any C that is respired, converted to CH_4, used for milk production or kept as liveweight gain is not returned to the pasture. The exception is the excreta, which are returned to the pasture almost in their entirety (mostly by deposition during grazing, and in part as effluent collected during milking). The excreta, upon deposition, become part of the pasture ecosystem. The C returned with them must be counted as an input. From then on, any CO_2 emissions from the excreta are legitimately included in NEP. – The budget approach for the UUW site is identical, except that there was only one grazing event, and hence the amounts of biomass removed and excreta returned are one magnitude smaller than at the IFR site.

◦ Several sentences are redundant in method, results and discussion sections.

Reply: Without specific suggestions which sentences, this is difficult to reply to. We will re-read carefully. Some repetitions may have been intentional, to remind the reader of a relevant fact stated in a different section before.

Specific comments:

◦ Figure 4 (b): Volumetric soil moisture sometimes exceeded 0.6 and I wonder if this large number is related to calibration issues of TDR in soils having significant clay.

Reply: Soil VWC reached 0.6 once (in June, after intensive rain) but did not exceed this value. The total porosity of the soil layer from 0 to 10 cm depth was 0.66 (±0.07), as determined from three pits (S. Carrick, pers. comm.); a VWC value of 0.6 measured at 5 cm is therefore possible. (For deeper layers, the porosity was substantially smaller.) Our soil moisture sensors (Delta-T SM300) were not TDR instruments.

◦ page 6 line12: If considering 1 m tube length and 5.8 mm and 3.9 mm inner diameter, transit time of 0.36 s and 0.28 s seems to be pretty long. Please can you explain how it could be decided?

Reply: Thank you for querying this. This was incorrectly expressed by us. The given values represent the total time lags, which include not only the travel time along the tube but also the time to exchange the gas analyser's cell volume, of 0.016 L (half the volume of the original tube), and a fixed processing delay of 0.13 s (see LI-COR manuals). We will clarify this in our revision.

◦ page 11 line 5: It seems to me that ER increased after grazing events.

Reply: No, this was not the case. Presumably, the reviewer inferred this from Fig. 5? In this figure, grazing events are located immediately above the upper edges of dark-blue areas (sharp drop in photosynthesis due to reduction in leaf area). The effect of these events on respiration would need to be gauged from subtle colour changes during night-time hours (left and right of the blue-edge areas). If the reviewer was right, there should be transitions from lighter (orange) to darker (red). Such changes do not seem to be a general pattern, though. We illustrate this with the following figure which shows NEP across two grazing events. The events are shown as shaded bars, and the NEP data inside these are gap-filled (thus, to be ignored here). Comparing the diurnal NEP minima before and after grazing events, there is no indication in this figure that respiration increased after grazing. In general, changes in NEP minima from one day to the next appear to be mainly driven by meteorology (temperature).

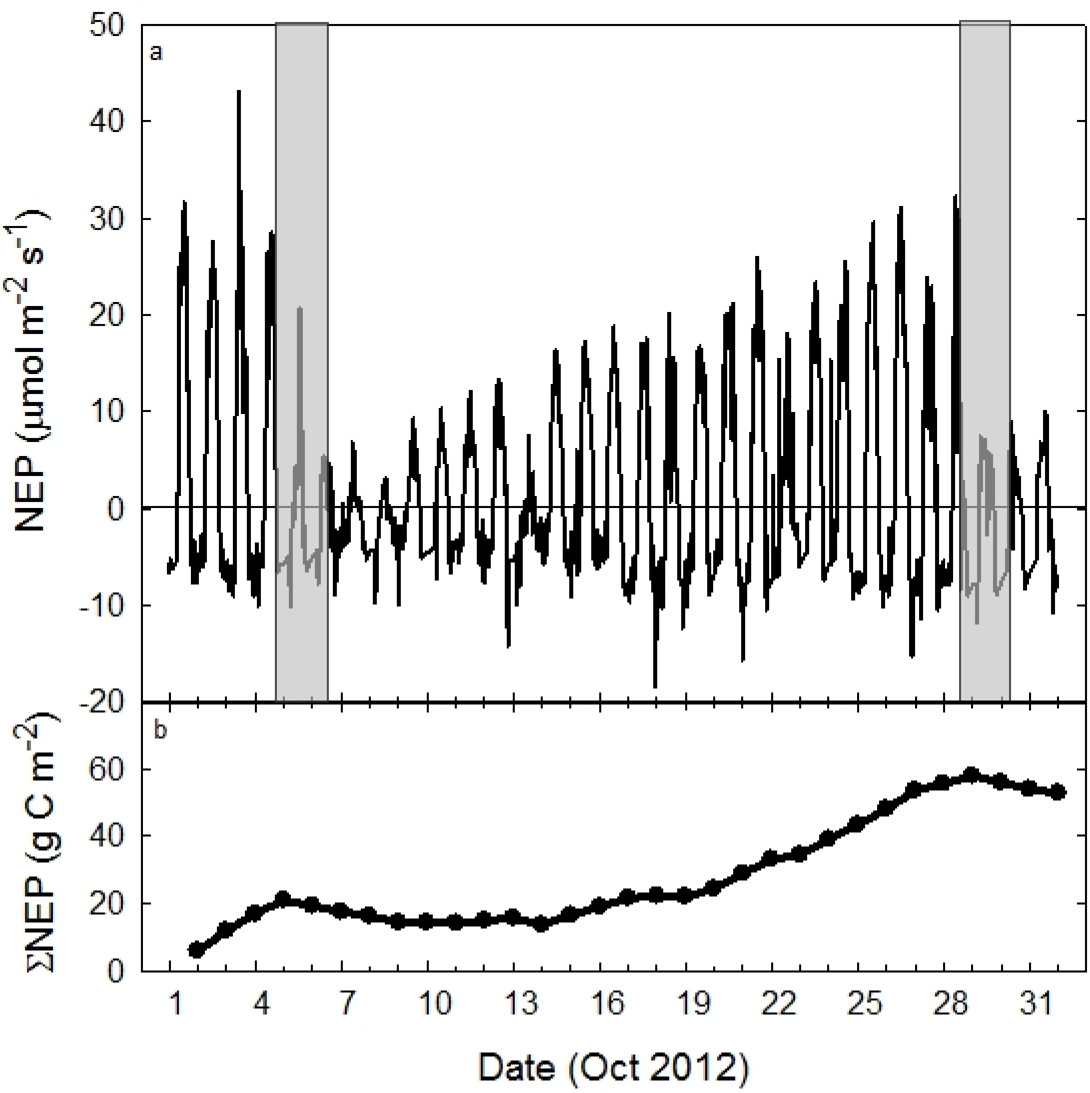

Figure R1. Evolution of (a) half-hourly CO_2 flux density (NEP) and (b) cumulative CO_2 uptake over one month, including two grazing events (shaded) at the IFR paddock.

◦ page 12 line 26: How can we know that it is not reasonable?

Reply: Thresholds outside the ranges considered in Fig. 8 differ by 50 % or more from the thresholds obtained with MPT or CPD algorithms. As Fig. 3 illustrates, if a threshold was chosen too small then it would fall into the range where ER steeply rises with the turbulence-indicating variable (which is the artefact one wishes to exclude). Conversely, if a threshold was chosen too large, then a large number of runs with fully-developed turbulence would be excluded and the results be biased towards certain weather patterns with stronger winds.

◦ page 13 line3-4: How can we know that it is not reasonable?

Reply: Presumably, the reviewer meant to ask “How can we know that this is reasonable?” (referring to 3 % uncertainty for EC and footprint)? These uncertainties are discussed for NEP in the second and third paragraph of Section 4.3. (p.11-12), and results applied here to GPP and ER in the same proportion.

◦ page 13 line 12: How can we know that it is not reasonable?

Reply: Presumably, the reviewer meant to ask “How can we know that this is reasonable?” (referring to the inherent robustness of neural-network gap-filling for ET)? Patterns of ET are well-studied and reasonably predictable: near-zero at night, and driven primarily by available energy and water vapour deficit, with modifications due to vegetation effects (represented in the algorithm by NDVI as a driver). The gap-filling algorithm reproduces the ET patterns in response to these drivers well.

◦ 5. Discussion: It will be much better if there are figures to show GPP and ER separately and extensive analysis on GPP and ER with atmospheric drivers, soil temperature, and soil moisture.

Reply: The objectives of this paper, as stated at the end of the Introduction, are to present a methodology to obtain NECB for an intensively-grazed pasture system, to carefully evaluate its uncertainty, and to identify the effects of the farm management practices. The effects of meteorological drivers have been studied many times before, and we do not expect to add new knowledge in this respect. The strongest drivers in the studied system are not the meteorological ones, but the amount of biomass and its rapid decimation with each grazing event.

◦ page 15 line 23: How can we know that it is not reasonable?

Reply: Presumably, the reviewer meant to ask “How can we know that this is reasonable?” (referring to “other sources of NEP uncertainty were relatively minor”)? As is detailed in Section 4.3 and summarised in Table 2.

◦ page 17 line 12: How can this study consider cattle respiration in comparing with other studies.

Reply: This is explained in p.17 L.22-25: the cows would have respired about half of the grazed biomass-C, so by correcting for this amount we get a rough estimate of NEP for the pasture ecosystem including the cows.

◦ Figure 5. It seems to me that ecosystem respiration increased shortly after grazing events (Fig. 5). Can you explain why?

Reply: See reply to same question above, including Figure R1.