Biogeosciences Discussions

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-46, 2016

"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 #1

In this manuscript, the Authors investigate the carbon (C) exchange dynamics and C balance of cattle-grazed pastures in New Zealand using the eddy covariance (EC) technique, and a technique to estimate C uptake and loss by cattle. As is the case with many grasslands and pastures globally, a mostly-ungrazed pasture was net C neutral during the one-year study period, whereas a pasture that experienced irrigation, fertilization and periodic grazing was a net C sink. Yet, the C sink dynamics of this intensively-managed pasture hinged on whether or not C uptake and loss by cattle was accounted for in the ecosystem C balance.

This manuscript is well-written, the C measurement methodology is technically-sound, and I have only nominal contextual comments. However, I am concerned that this study does not present an advancement in understanding of pasture C dynamics, nor is it up to par with other, similar studies. Specifically, I am concerned that the Authors attempt to draw conclusions on ecosystem C balance using a single year of measurements, especially because there is no true control ecosystem nor is there any pretreatment comparison. Yet, for this OZFlux special issue, I think this manuscript may ultimately be considered for publication because it focuses specifically on management issues in New Zealand. I will be happy to recommend this article for publication once my main concerns are addressed.

Reply: Thank you for the generally positive comments. This is the first study of irrigated pasture, which is indeed a management practice of particular relevance to New Zealand, and therefore we felt indeed that this OzFlux Special Issue would be an ideal forum to present our results. We are glad that the reviewer is in support of our judgment in this regard.

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. On the basis of one year's data, we did not expect to provide "advancement in understanding of pasture C dynamics". Instead, our manuscript has a strong focus on describing the methodology and ascertaining its robustness. This is because NECB was expected to be a small difference of large inputs and outputs and therefore quite sensitive to errors in these.

We are fully aware that there is no "control ecosystem" or pre-treatment comparison available. These are very difficult to both define conceptually (as different management factors always confound each other) and to find in the reality of commercial farming (where farm managers' decisions are driven by many factors but do not include suitability for, or continuity of, a research programme). We still believe that our study is valuable, in showing how and with what uncertainty the NECB of a commercial, irrigated farm operation can be obtained. We also believe that we have exercised care not to overstate the interpretation of our results.

1) The Authors need to better frame their findings in the context of existing literature on pasture/rangeland C dynamics and C balance, and also the C balance of grasslands in general. I believe that this study is best presented as a supplement to the larger body of literature that exists on this subject. It may also be useful to focus more on the weekly and seasonal dynamics of C

exchanges as a way to differentiate this manuscript from longer-term studies.Potential sources include:

Felber et al. 2016. Agricultural and Forest Meteorology

McGinn et al. 2014. Journal of Environmental Quality

Oates and Jackson. 2014. Rangeland Ecology and Management

Reply: Thank you – these are all interesting studies covering different aspects. We will consider whether they add useful context to our Discussion (Section 5.3). We believe that we have already presented our study as a "supplement to the larger body of literature" by discussing where our results would fit compared with the data from 21 sites collated by Rutledge et al. (2015).

Please note that seasonal dynamics are relatively small at our site, compared to other temperate climates. The overwhelming dynamic for the IFR pasture is the repeated decimation of biomass by grazing, from grass heights of order 20 cm to about 5 cm. (This occurs on a time scale closer to monthly than weekly.) The dominant dynamic at the UUW pasture is the summer drought, which occurs in the majority of years in this region (and is a major reason for farmers to convert to irrigation). Both these dynamics are illustrated in Figs. 5 and 6 and discussed in Section 5.2.

2) Because the UUW pasture is not a true control, the Authors may wish to truncate their study period to Aug 2012 – May 2013 (so grazing did not occur in the UUW pasture during the study period). Alternatively, it may be appropriate to cite literature on the neutral C balance of other ungrazed grasslands in NZ, which would support the Authors' determination that the grassland in their study is an acceptable control.

Reply: We do not claim anywhere that the UUW pasture is considered a "control". It was part of the commercial farm under investigation and of some limited use to assess the tendency of what happens to the C budget of a barely-managed pasture in the same climate. We may unintentionally have caused the impression of UUW as a "control" site, with some readers, by stating near the end of the Introduction that we aimed to "determine how the annual NECB… differed…". We will reformulate this.

We do not see any value in truncating the study period for the UUW site since that would not make it any more suitable as a "control". It can be seen in Fig. 6 that cumulative NEP from August to May was positive (ca. 80 g m^{-2}), but such a figure is of limited use without including what would happen over the three winter months (most likely, a reduction of cumulative NEP because respiration tends to exceed photosynthesis in winter). The winter-grazing that occurred in May was part of the management, and by reducing biomass combined with the effects of trampling, the grazing event probably amplified the net C losses from May to August 2013 that would have occurred without grazing. The observations of annual NEP being slightly positive and NECB slightly negative appear thus very reasonable and we do not understand why this information should be suppressed or truncated. In turn, we also consider it appropriate to compare the UUW site to extensively-managed pastures elsewhere, rather than to ungrazed grassland.

3) On page 16, line 15, the Authors state that the UUW pasture and IFR pasture had different grazing histories prior to the study period. Without a pretreatment comparison, I am greatly concerned that these pastures aren't comparable.

Reply: As stated before, we do not claim that the UUW pasture is a "control" for the IFR pasture. The two pastures were part of one farming system and served different purposes within that. The first half-paragraph of Section 5.2 (which contains the mentioned statement) serves to explain the different management aspects and interpret what their effects were.

Regarding the longer-term history, please note that before the conversion to dairying the two sites were within the same paddock. The conversion caused the same initial disturbance, cultivation, fertiliser application and seeding at both sites. Following conversion, the different managements labelled as IFR and UUW had then been applied for four years prior to our measurements.

In-text comments:

I recommend that the Authors use active voice throughout the manuscript.

Reply: This is a style question. The reviewer expresses his/her preference but we do not share this preference.

I recommend using negative NEE values to indicate a carbon sink or uptake by the ecosystem, and positive NEE values to indicate a carbon source or loss.

Reply: Again, this is the reviewer's personal preference. We considered the pros and cons of using NEE or NEP and preferred the latter because our main interest was C gains or losses of the pasture ecosystem, not of the atmosphere. NEP follows the same sign convention as NECB, while NEE has the opposite sign (Equation 1). Therefore it appeared more logical to use NEP.

Page 1

Line 11: Remove sentence beginning, "Primary terms..."

Reply: We disagree. This sentence provides the logical connection between the previous and the following sentence.

Line 17: Differences in GPP and RE are both very large. I recommend simply stating these differences as a result.

Reply: We decided to give absolute numbers for GPP and RE and their relative differences. The reviewer seems to suggest giving absolute differences instead, but we do not see why that would be superior. Finding a factor 2 in GPP between the intensively-managed site and the site with little management is, in our view, quite instructive information.

Line 18: Efficiency measured as what metric or variable?

Reply: The details are given in the text (end of Section 4.2) and Table 1. We would have thought that "total water input" and "to produce biomass" in the queried sentence are sufficiently clear, but will consider reformulating.

Line 19: Need a stronger conclusion than this. What new information was obtained in this study?

Reply: This is, to our knowledge, the first study reporting NECB of irrigated pasture. It happens to be a significantly positive value, but since we have one year at one site we cannot generalise that finding. We believe the queried sentence puts our results into adequate context.

Line 24: Intensification of grasslands needs changed.

Reply: Agreed. We will reformulate.

Line 26: Needs citation.

Reply: The reviewer probably means we need to add a citation for the global intensification trend. We are happy to do that: Thornton, P. K., Livestock production: recent trends, future prospects, Phil. Trans. Roy. Soc. B 365, 2853-2867, 2010.

Line 29: Remove “for pasture”

Reply: Agreed, that would fit better into the second half of the sentence.

Page 2

Line 1-4: Remove this sentence.

Reply: We will consider that suggestion.

Line 7: This sentence is unclear.

Reply: OK, we will reformulate.

Line 31: Because of the coarse temporal resolution of what?

Reply: Of the SOC sampling. This is typically done several years apart, and in the meantime management practices may have changed a number of times.

Line 31: Remove this sentence.

Reply: We will consider that.

Page 3

Line 1-7: Consider moving this to Methods or removing.

Reply: This passage describes in which ways the EC methods provides valuable information to both construct annual C budgets and to interpret them (partitioning, link to ET). It is, thus, motivation for the choice of methodology and therefore serves a purpose in the Introduction.

Move the entire “C budget of a pasture ecosystem” to Methods

Reply: With due respect, we disagree. This section sets out the general concept of the study, not methods. Since we construct the C budget differently to most other pasture studies, by excluding the animals, it is important to have this clarified prominently and early in the paper.

Make a separate Site section in the manuscript.

Reply: Again, this is a matter of personal preference. It is a very common practice though to include the site description under Methods. For example, all three references cited by this reviewer in Comment 1), above, provide the site description as the first subsection under Methods. We follow the same practice here.

Consider moving a good portion of the Methodology to an Appendix.

Reply: We have considered that, but we have three appendices already. We provide the methodology quite detailed because we see that as a logical prerequisite for the detailed uncertainty analysis that follows. Readers who are less interested in the details can easily skim or skip across subsections (which are clearly labelled by the three-tiered subheadings).

Page 10

Please add p-values or other values of statistical significance to the Results.

Reply: Firstly we are not sure which of the results on this page the reviewer refers to. For the C budget terms in Section 4.2, it is clearly stated in the first sentence that the uncertainties are given in the following section. And for the environmental parameters in Section 4.1, there is hardly a need to do so, as they provide context only. Secondly, testing "statistical significance" means assessing whether an observed difference between the means of two variables has a high or low probability for having occurred randomly. Such a test requires replicated sampling of each variable. The annual C budget terms given here have no replications and thus cannot be subjected to such a test.

Line 11: Remove sentence beginning "This amount..."

Reply: No, this sentence provides the logical connection between the previous and the following sentence.

Line 17: This occurred in both pastures?

Reply: No, UUW only, and we believe that is sufficiently clear from the temporal sequence indicated by "From mid-March…" and "After that".

Again, I suggest that differences in GPP and RE are actually both large, just that GPP differences > RE differences.

Reply: See reply to earlier comment: we cannot see anything wrong with giving absolute numbers for GPP and RE, as well as their relative differences.

Page 11

Line 21: Consider changing mmol C and mol H2O to grams and mm. Also, need a unit of time.

Reply: We disagree: mmol/mol is an adequate unit for water-use efficiency and is in common use. Time drops out when forming the ratio, so no time unit is required.

Why are the "Uncertainty analysis" and "Non-CO2" sections in Results? Please separate results from methodology in these, and put them in the correct sections of the manuscript.

Reply: The uncertainty analysis is a stated objective of the analysis and therefore an integral and important part of the Results section. The non-CO_2 budget terms are as important for determining NECB as the NEP term. They required their own methodologies, which are described in Sections 3.5 and 3.6 under "Methods". Section 4.4 gives the results for these terms and is thus in the "correct" place.

Page 15

Line 3: Remove “warranting the rigorous...”

Reply: OK, this half-sentence is not essential, though it is a point for discussion.

Entire “C budget uncertainty” section is not appropriate for the Discussion.

Reply: We disagree. Uncertainty evaluation was a stated objective of this study, and having a good estimate of uncertainty for the C budget terms is important in order to interpret them correctly, therefore it is appropriate to discuss the uncertainty results. The contribution of the turbulence-threshold choice to NEP uncertainty deserves particular attention, as our treatment of this contains some novel aspects (Fig. 8).

Page 16

Line 17: Please rewrite and clarify this sentence.

Reply: OK, we will.

Line 21: “Efficiently used” needs to be better explained.

Reply: The following half-sentence states precisely which ratio we consider as a measure of efficiency (GPP/WI).

Line 23: Remove sentence beginning “It is instructive”

Reply: The reviewer probably means to criticise that this short sentence does not convey any factual information. However, it still has a function in the text, flagging a change of focus (away from water-use, towards partitioning). Whether to retain or remove the sentence thus appears to be a question of style or taste.

Line 32: change “C balance” to “C neutral”

Reply: We will reformulate this.

Page 17

Line 5: Why did this maximize GPP? Need a citation.

Reply: To maximise GPP is the goal of the farm manager. We do not mean to say here that the observed GPP was equal to an actual (physically or biologically constrained) optimum. We will consider how to reformulate this.

In Section 5.3, need to include additional sources.

Line 25: Need citations

Reply: No, the “slightly less than 200 g C m^{-2} yr^{-1}“ is not based on the literature but based on the estimation described in the preceding sentence: 50 % of grazed biomass-C from the IFR pasture equals 215 g C m^{-2} yr^{-1}, which needs to be subtracted from the NEP excluding cows

(408 g C m^{-2} yr^{-1}) in order to obtain a redefined NEP that includes the cows' respiration. Perhaps we should insert parentheses with these values to make the computation explicit.

Line 27: Explain the difference between this study and Rutledge 2015 more substantially.

Reply: The main difference regarding the C budgets is the treatment of animal respiration, precisely what is explained in the earlier parts of this paragraph. In the first sentence of this paragraph it is also stated clearly that what we are comparing to is the collection of literature data by Rutledge et al. and not just their own experiment.

Line 33: Information about management activities is important, but it is impossible to separate the influence of irrigation versus that of fertilizer in your study. It's probably best to pay this some attention, and suggest the value of better understanding the influence of these variables.

Reply: We agree that it is impossible to separate the effects of irrigation and fertiliser application, not only in our study but in most commercial farm settings, since both practices go hand in hand to produce more grass. One might try small-plot studies, probably best with chamber techniques, to separate out the influences of the different management factors. However, there are many potential sources for artefacts with such studies which may limit the applicability to real-world farm management. It appears more useful, for paddock-scale studies like ours, to assign observed effects to the management as a whole. For this, the management needs to be described well, and we merely point out that for the historical data in long-term C stock studies such description is not always available.

Table 3: I recommend briefly explaining why some data were not available, and why FCH4 is the same for both pastures.

Reply: Both recommendations concern very minor terms in the table. Regarding the first, we state in Section 4.4 that DOC leaching under irrigation was estimated based on data from a nearby experiment with a similar soil. Data without irrigation were not available. By "nd" we indicate that this term was not determined, and we do not think that an explanation why would add value to the table. Regarding the CH_4 emissions, the numbers are derived from the companion paper by Laubach et al. (2016). In that paper, the underlying measurements are presented, and two approaches are explored how to combine the results from two complementary micrometeorological methods. It is further explained why for CH_4 we opted in favour of the "merged daily" approach, which by pure happenstance led to identical annual sums for the two pastures. Had we opted for the "means combined" approach instead, or for using only one of the two methods, then the results for the two sites would have been different from each other (see Table 3 of Laubach et al.). We understand that the exact equality of the given numbers invites misinterpretations, so we will consider adding a footnote to explain this.

Figure 1: Not a typical map, but it works.

Reply: We should have called it "schematic", not "map".

Figure 3: Consider moving to Appendix.

Reply: With all due respect, we disagree. We feel that we need to provide some justification to the reader why, for the determination of a low-turbulence threshold, we did not follow the widely-adopted

practice of using u_star as the threshold variable but used sigma_w instead. This figure is part of that justification.

Figure 8: Consider moving to Appendix.

Reply: We considered this but have decided not to move Fig. 8. We are not aware of any published graph combining sigma_w- and u_star-thresholds in this fashion. We found it intriguing that the proportion of gaps appears to be a parameter collapsing these onto the same line, and this was not expected. The figure presents a novel finding and therefore we would like to retain it in the main body of the paper.