Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-420-RC1, 2021

Interactive comment on "Multi-year CO_2 budgets in South African semi-arid Karoo ecosystems under different grazing intensities" *by* Oksana Rybchak et al.

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Received and published: 4 January 2021

Synthesis

The paper presents CO2 budgets of semi-arid grasslands/shrubs ecosystems in Karoo, South Africa with a special focus on grazing. Two sites, one considered as submitted to heavy grazing, the other to a lenient grazing, were compared. CO2 fluxes were measured using the eddy covariance technique.

Semi-arid ecosystems and, more generally, African ecosystems are not enough investigated and poorly covered by flux measurements. In addition, as measures are sought

to mitigate climate change, especially in the livestock sector, it is of primary importance to better understand and quantify the impact of cattle management on ecosystem carbon balance. I think thus that the topic is of great interest. In addition, it perfectly fits the scope of the journal.

The methodology used to measure and treat the eddy covariance data seems correct and well implemented. I have however one restriction concerning the HG site that I will detail below.

The general paper shape (quality of writing, figure quality, reference list) is good.

I thus think that the measurements realised here have the potential to make a good paper that would deserve publication.

However, I have a strong reservation concerning the result presentation and discussion. The results have not been sufficiently addressed, being most of the time limited to a chronological presentation of the fluxes. The analysis is too superficial, premature and based on an over-interpretation of the results, I will detail this below. I thus think that the paper needs a complete reshape and, in view of the importance of the needed changes, I suggest rejection of the paper and I encourage the authors to resubmit it after a complete redesign of their result presentation and analysis.

General comments

The two main theses in this paper are that (i) site to site differences between carbon budgets is mainly determined by the current and past management and that (ii) their interannual variability is linked to soil humidity.

The first point is announced from the beginning and repeated several times in the paper but, unfortunately, never really supported by the measurements:

First, the budget over four years yields to 82.11 and -36.43 gCm-2 (corresponding to annual budgets of 20.5 and -9.1 gCm-2yr-1) for LG and HG, respectively. In view of the uncertainties due to the measurement technique and of the interannual variability, I doubt that these budgets are significantly different and, moreover, that each budget is significantly different from zero. The interpretation (which is central to the paper) that one site behaves as a source and the other as a sink is thus abusive. It is most probable that none of the site significantly depart from equilibrium.

Secondly, even if a significant site to site difference had to be found (maybe on the two first years), it could be attributed as well to present as to past management. Even if a possible impact of present management is evoked at some place, the reader is leaded to think that the past management is the most important factor. Indeed, in the material and method, a description of present management is missing and we have to wait for the result presentation (Figure 5) to realize that present management differed completely, not only between the two sites but also between the beginning and the end of measurement period in one site, the so-called "heavy grazed" site being not grazed at all during the two first years (!!). As a result the impact of presented grazing on the flux has been completely overlooked, which biases the conclusions and the discussion.

The analysis of the long term impact of overgrazing based on the differences between parcel species composition, with a greater abundance of unpalatable grasses in the HG site, is promising. Unfortunately, the analysis is only qualitative and does not rely on a quantitative inventory of species on each site.

Concerning the second point (response to humidity), it would be relevant to attribute the interannual variability to precipitation and soil moisture differences. This aspect is however insufficiently developed to my opinion and results presenting the responses of NEE, GPP and Reco to SWC should be deepened.

Finally, I have some reservations as regards the measurement methodology on the HG site for the grazed years: the authors announced that the site was divided in two paddocks and that the cattle were regularly moved from one paddock to another. I suppose thus that CO2 fluxes should differ if they come from the occupied or from the empty paddock due to cattle respiration but also to vegetation consumption. However,

no information about the paddock positioning with respect to the measurement tower or about cattle moving chronology was given; no flux discrimination between the empty and the occupied paddock was tempted. In these conditions, I suspect that the fluxes should be strongly affected by footprint variations. Discriminating fluxes between the paddocks, if possible, could provide information on the impact of present grazing on fluxes.

I recommend the authors reading papers by Jerôme et al., AEE 194, 7-16, 2014; Felber et al., BG 13, 2959-2969, 2016; Gourlez de la Motte et al., AFM 268, 156-168, 2019, and references herein to better address the difficult question of highlighting the grazing impact on carbon balance.

More specific comments

Abstract

L21: As already said above, in view of the uncertainties and of the interannual variability, I think that the difference between the two sites is not significant (and a source of 82 gC m-2 over 4 years is by no means "considerable" as the uncertainty on the annual sequestration is about 50 gC m-2yr-1). I found thus misleading to present one site as a source and the second as a sink. Both sites do probably not depart significantly from equilibrium. As this assertion is central to the paper, it questions all the discussion and conclusions.

Introduction

L78-80, Independently of the fact that I disagree with the sentence, an introduction should finish with a presentation of the objectives of the papers in the form of scientific questions, not with a result.

Sites description

A section describing the cattle management during the measurement period (stocking rate evolution, chronology, position of the paddocks) is missing. The fact that the so-called “heavy grazed” site is not grazed at all during the first two years should appear more clearly (and the name of the site should be changed as it is strongly misleading!).

What are the parcel sizes?

L100: The stocking density is generally expressed in ALU ha-1, not the inverse (furthermore it’s the definition you use implicitly when evoking a stoking density “double that of the recommended rate” on L105).

L100: Is the value you give (1/16 ALU ha-1) an annual mean or the peak value during the grazing periods? In the first case, what is the peak value?

L105 : I’m surprised by the order of magnitude of the stocking rate (but I’m not familiar with semi arid ecosystems). Do you confirm the number of 0.125 ALU ha-1 for the HG site? It seems very small to me, being rather familiar with stocking rates over 2 ALU ha-1 (but in a totally different ecosystem, of course).

Data processing

No specific comment on the flux computation procedure. It appears correct and well implemented. As mentioned above, I anyway have a problem with the HG site. As the cattle is confined in a paddock and regularly moved from one paddock to another, this would need a specific data treatment (see above).

Uncertainties

There is no standard way to estimate the uncertainties but I think that the procedure followed here takes the most important uncertainty factors into account. I’m thus OK with it.

L204: Shouldn’t RE be at the square in the sum ?

L206: Confusion between lower and upper case delta in in Eq 3 and in Eq 1 and L199 and maybe elsewhere. Please harmonize.

Results

Many figures and Tables are redundant. Besides, some key information (the annual budget for each site) does not appear in the Tables. I think that Figure 4 could be skipped as it does not bring more information than Figures 3. This is also the case for Figures 6, 7 and 8. All the information they bring is already given in Figure 5. In addition, I’m not convinced by the interest of giving min, mean and max values of Fc diurnal course (Table 2), daily (Table 3) or monthly (Table 4) cumulative fluxes as these values were not really discussed in the text. Besides this, a synthetic Table providing cumulated values of NEE, GPP and Reco for the different seasons and for the whole year (Something like Table 5 but completed with annual means and GPP and Reco data) would be expected.

The authours use different time scales to describe flux seasonal evolution: data are averaged sometimes on a monthly basis, sometimes on a four season basis and sometimes on a two season basis (dry and wet). This engenders confusion and is also maybe a cause of the table and figure redundancy. I suggest the authors to select only the most relevant time scale and to use it for all flux and meteorological variable descriptions.

The results present mainly chronological evolution of fluxes, which is a little bit scarce. The discussion could much more comprehensive if based on functional relationships (response of fluxes to driving variables) and on their site to site or inter-annual variability.

Flux numbers were often given with two decimals. This is useless and decimals have absolutely no meaning as the uncertainty is of the same order of magnitude than NEE itself. Rounding all flux numbers at the unit is probably more appropriate (and by the way, more readable).

The enumeration of numbers in the text makes its reading quite fastidious. It could most of the time be avoided and focus on the most striking and original points.

Figure 2: Presenting half hourly data of Tair, RH and PPFD makes the figure difficult to read and, especially, does not allow the reader to discern inter-annual differences. I suggest replacing half hourly measurements by day averages and, when relevant, extrema).

L261-285: The interest of the figure 3 is that it provides information on the inter-seasonal, inter-annual and site to site variability of this response. The text should rather emphasize these differences.

Figure 5a: I'm puzzled by the livestock period patterns in the LG: all grazing periods seem to correspond to periods where vegetation is not active. Again I'm not familiar with cattle management in semi arid regions but this appears illogical to me as, at these moments, vegetation should be absent or senescent and thus unpalatable for cattle.

Figure 5b: In the HG, the grazing pattern should highlight the alternation between the two paddocks.

Figures 5: By comparing the flux evolution with climate, it seems that the vegetation activity is not in phase with the solar radiation or with the temperature. This is maybe a specificity of semi arid sites (compared to temperate sites). As the paper is intended for an international audience that is not necessary familiar with South African climate, it could be nice to more clearly show the relation between meteorological variable and flux annual patterns.

L310-316: The text could be made more attractive by avoiding fastidious number enumerations and focusing on the most striking points. For example the lag between meteorological variables and fluxes evoked above could be better explained here. In addition, the inverted GPP and Reco peaks that appear during the growing season on several years (most clearly on Jan-Feb, Year II) are unusual and call for a comment. How do you explain them?

Figure 9: This figure is the second most relevant one as it clearly highlights the inter-

annual and site to site flux variability. It would be worth adding in each figure shaded areas indicating the cumulated uncertainties of each NEE.

L345-354: This paragraph contains the numbers, presented as the most important of the paper (as they are cited in the abstract). It is thus strange that they are presented in a text and not in a Table (while most of the Tables contain dispensable results). I would add that the number presentation is awkward (no sign difference between sources and sinks, order of the sites changed during the presentation) which makes difficult to the reader to build a budget by himself.

L353: What do represent the numbers after the +/- sign? Are there cumulated uncertainties on a 4 year budget, do they represent interannual variability or are there a mixing of both? This point should also be clarified in the Tables.

Figure 10: A good correlation between NDVI and GPP seasonal evolutions is the least than could be expected and is not very informative. It would be much more interesting to test the ability of NDVI measurements to reproduce inter-site GPP differences (maybe on the two first years) or inter-annual GPP differences (maybe by comparing Years III and IV).

Discussion

I have some difficulties to comment this discussion as I disagree on the point of departure, as said before. To my opinion, the introducing paragraph (L275-278) is incorrect in view of the carbon budgets and the uncertainty analysis presented before. Consequently, I also consider that all the sentences that are based on this assertion are also incorrect. I will not enumerate all of them.

The discussion on the development of unpalatable grasses in the HG site due to past overgrazing and its impact on ecosystem response to drought could anyway be interesting. Unfortunately it does not rely on a quantification of the parcel species composition and, in addition, does not lead to significant flux differences. This point could maybe be deepened by looking more closely the flux response to SWC and the way it differs between HG and LG during the drought periods (after getting rid of present grazing impact).

L390-393: In what the fact that grasses are stronger competitors for water implies that the studied sites were net sources? The link is not clear at all.

L422: I think that this is an over-interpretation. Several observations challenge it:(i) on the HG site, the NDVI was the lowest on Year I while there was no grazing; (ii) the decrease in NDVI peaks is also observed on the LG site; (iii) a lower plant development on Year IV compared to Year III could also be explained by the drought conditions. I think that, in view of the variability of meteorological conditions and the shortness of the grazing (two years) it is not possible to determine any long term impact of the grazing on the basis of the present measurements.

L424-430: See my comment above on the relation between NDVI and GPP.

L424-430: In addition, this paragraph looks like a catch-all in which several different points of discussion are put without development and without guiding thread.

L448-465: This paragraph is more or less a repetition of observations already made before. It is only descriptive and chronological. Example of questions (among others) I would have liked to see discussed: Precipitations are known to affect both GPP and TER (but at different time scales). How does it happen at your site and does it provoke an increase or a decrease of NEE in the end? As the two sites vegetation compositions are different, can you compare the impact on GPP and Reco dynamics of rain or drought? Are they different?

L473-478: I'm not convinced by the relevance to compare local carbon budgets with a global estimate that could encompass very different ecosystem types.

Conclusions

See my general comments above.

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