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Biogeosciences Discussions

Interactive comment on "Carbon balance of a grazed savanna grassland ecosystem in South Africa" *by* Matti Räsänen et al.

Anonymous Referee #1

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The paper by Räsänen et al. explores carbon dioxide fluxes measured with the eddy covariance method for three years at a grazed savanna grassland in Welgegund, South Africa. The material is appropriate for a scientific study and the data obtained appear to be high-quality. It is relevant for many African ecosystems to focus on CO2 fluxes response to environmental drivers in order to better predict fluxes patterns in the context of climate change. Therefore, the work is interesting and worthy of publication in Biogeosciences Journal because of the lack of knowledge regarding the carbon cycle for Africa continent. However, I have a number of issues with the paper which lead me to suggest that it requires major revisions before it becomes acceptable for publication in BG.

General Comments:

1) Firstly, while the study site is located on a savanna grassland which is grazed by cattle and sheep, authors did not provide any information on the average stocking rate and the management of the site during the studied period. Is the site grazed intensively or not? What was the stocking rate? How the grassland was managed? What is the slope of the field? At the measurement height what is the fetch? Was the fetch adequate to characterize the carbon dioxide and water vapor fluxes of the vegetation type? These are important for understanding and interpreting the results.

2) It is also well known (see references below) that grazing affects a range of ecological and biogeochemical processes and properties, including plan community composition, soil physical properties, soil C and nitrogen content and the magnitude of C and carbon dioxide exchanges which in turn influence soil organic carbon storage. This study could have been more attractive if the impact of grazing on carbon dioxide exchange had been investigated. This probably would help to better assess for example the relation between the total ecosystem respiration and environmental drivers.

3) Authors used the Kaimal cospectra in the computation of the correction factors that are used to correct the high frequency losses (L129 – 130). However, recent studies (Mamadou et al., 2016) showed that Kaimal cospectra can be significantly different from sensible heat cospectra, and the high-frequency loss correction for CO2 using these different cospectra resulted in the large difference in CO2 flux calculations, i.e., using Kaimal cospectra can result in an overestimation of CO2 fluxes even if the site could not be considered as difficult (i.e., fairly flat, homogeneous, low vegetation, sufficient measurement height). Especially, at their studied site, authors found that the choice of Kaimal rather than sensible heat cospectra reversed the annual carbon balance from being a net C sink to being a weak C source. Did the authors verify if their kaimal cospectra differ or not from sensible heat cospectra before chosen them as idealized cospectra?

4) Most of results presented in the section 3.4 are too much qualitative, superficial and descriptive and should be supplemented with additional statistical analyses in order to

provide more quantitative rigor.

5) The uncertainties associated to the annual carbon dioxide balance estimation are not evaluated. This remains a great lack for the study. The authors also clearly mentioned in their introduction that environmental drivers for the inter-annual variation in NEE are poorly understood. Unfortunately no progress regarding this point has been made within the present study.

Specific comments

L18-19: What about the dependence, at monthly scale, of the nighttime respiration on soil moisture or soil temperature? L24-25: by increasing autotrophic respiration? L32: The seasonal cycle of what? Please clarify. L32-33: The alternation of "wet and dry seasons" cannot in my view be generalized for the "whole Africa". In other regions of Africa, the dry and wet seasons are separated for example by two transitional seasons... L67, in site description section: Please, give values of the roughness length, zero-displacement height and site's slope. L102-103: Specify the sampling rate of the meteorological variables. L113: Specify the type of the gas analyzer. L115-118: What are the characteristics of the sampling tube (inner diameter etc.), the pump and the gas used for the zero and span? L127: Give an indication of the magnitude of low frequency correction factors. L129-130: Provide an illustration of kaimal and the sensible heat cospectra according atmospheric stability to attest that both cospectra match. L133: Replace the calculated fluxes by "the corrected fluxes". L133: What was the fraction of data excluded this way? L133-136: Do you only use u* filtering criteria to discard bad data? if Yes, explain why. L181: Complete "air" with temperature. L182: You never indicated how water vapor data have been treated. What is the cut-off frequency for H2O fluxes? How these data have been corrected for low and high frequency losses? Which criteria have been used for the filtering of bad data? L183: Explain how high evapotranspiration rate were due to higher precipitation and transpiration rate during the rainy season? What about soil evaporation? L206: air or soil temperature? L211-215: The low (high) values of the correlation coefficients cannot only be used to attest

the robustness of dependences. These must be accompanied with the p-values. L225-226: showed how? L232: I cannot get this conclusion... L223–L233: Why is there so much interpretation in the results? L301: most or must? L305-310: I am afraid that because of the difference of their climate, the Dahra site and cannot be easily compared to the Welgegund site. You should mention this in your discussion. L315, L317: Write Nalohou not Nolohou… L475: Figure 1 and also in the title: "air" or "soil" temperature? L500: Figure 3: Is it necessary to show evapotranspiration curve? L522: Figure 4: bin averaged for how many data?

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