

Interactive comment on “Carbon dioxide fluxes and carbon balance of an agricultural grassland in southern Finland” by Laura Heimsch et al.

Anonymous Referee #2

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The ms introduces a new data set on eddy fluxes of CO₂ and H₂O observed above a managed grassland in southern Finland. Given the huge number of eddy covariance CO₂ studies, and especially of multi-year and multi-site analyses based on networks and infrastructures such as Fluxnet, ICOS, NEON and others, one could ask whether it is relevant to publish an additional study covering only one site over two years. However, apart from the fact that this ms is very well and thoroughly written, it deals with an ecosystem type for which data are still scarce. The majority of eddy towers is located in natural ecosystems, whereas agricultural sites in general are underrepresented. It is necessary to focus more on these sites because the driving factors of soil-vegetation-atmosphere exchange are much less well understood, and because the magnitude of the fluxes is strongly influenced by management activities. This makes the results from

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such case studies highly relevant not only for basic scientific analyses and for monitoring adaptation to climate change, but also for developing mitigation concepts. The data seem to be of high quality, and most of the conclusions are correctly drawn. It was a pleasure to read the ms, finally yet importantly because it is written in such a way as to make the study fully reproducible, which is clearly an advantage, from the readers' perspective, in comparison to papers that refer to a chain of other publications when describing the methods. Therefore, I recommend that the ms be published in Biogeosciences, provided the authors submit a moderately revised version that considers the following specific points.

Lines 83-84: Clover is not a grass – please revise the sentence.

Line 109: Was only the zero point of the CO₂ channel of the LI-7200 calibrated? No calibration of the span and/or the H₂O measurements? This is the only point in the Methods section that worries me a little bit, because from my experience I would say that this type of analyser requires a complete calibration about once a year.

Line 144: I had expected that the data coverage would have been described in more detail here: Was it spread evenly across seasons and/or times of the day? The temporal distribution of the gaps can have a considerable effect on the reliability of the gap filling and potential biases. Later I realised some comments on this in the appendix, but I would recommend adding a sentence or two about this in the main text.

Line 266: Regarding the relation to 'mean soil moisture' it is important which soil depth is referred to. Please add this information to the ms text.

Line 322 ff: I am not really sure about the definitions of the expressions of CO₂ exchange used here and in the subsequent paragraphs (this comment refers also to the 'uptake' mentioned in lines 338 and 340 – is this NEE or GPP or something else?). How is for instance the 'total carbon balance' defined, when for example comparing it to the nomenclature used by Kutsch et al. (2010) in AGEE 139, 336-345: Is it net biome production NBP? Or full field balance or farm gate balance? Please clarify.

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Line 345: Strictly speaking, the enhanced root system wouldn't directly enhance carbon uptake, but enhance water availability and avoid drought stress, thereby indirectly enable a higher GPP (but also enhance respiration?!). Please explain this effect more accurately.

Line 353: I think the reference to Pirasteh et al. is a very odd choice given that this effect has been well known and understood for many decades.

Lines 361-363: With respect to the WUE analysis, I strongly recommend an additional approach to disentangle transpiration from total ET in order to make the statements on WUE more meaningful. I understand that the ET components were not measured separately, but assuming that soil evaporation can be neglected in a grassland with complete canopy cover, the data set could for example be split into rainy and rainless days, and the rainless days be analysed separately. This would enable an alternative calculation of WUE from GPP and T if it is further assumed that, on rainless days, interception evaporation can be neglected, too.

Line 395: The last "the" has to be deleted.

Line 396: See above: Which processes or flux components are exactly included in the expression "management flux"? I think the answer to research question no. 3 on carbon sequestration and offsetting carbon emissions depends strongly on the definition of the boundary of the system under consideration (ecosystem – field – farm gate?).

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