

Interactive comment on “Temporal and spatial decoupling of CO₂ and N₂O soil emissions in a Mediterranean riparian forest” by Sílvia Poblador et al.

Anonymous Referee #2

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In this paper entitled “Temporal and spatial decoupling of CO₂ and N₂O soil emissions in a Mediterranean riparian forest” Poblador et al report annual and seasonal greenhouse gas (GHG) emissions from a Spanish riparian zone. The authors found that N₂O fluxes from denitrification were lower than in previous studies but that CO₂ flux was quite high. As expected, these fluxes were negatively correlated in space and time. The authors conduct a sound observational study with interesting results, but I was left wondering what the key findings were and how they advanced our understanding of the role of riparian zones as a terrestrial-atmospheric-aquatic interface. I outline a few general concerns below before providing line edits. 1. The manuscript focuses on GHG emissions, but the temporal and spatial scale and the methods of the study

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do not seem suited to answer this question. High CO₂ fluxes in themselves do not indicate whether an ecosystem is a carbon source or sink, since net ecosystem carbon balance is the relevant parameter. Furthermore, there is large inter-annual variability in both CO₂ and N₂O fluxes in Mediterranean ecosystems, and the magnitude of difference observed here does not seem strong enough to infer landscape functions. I think the study has many other interesting implications about the link between microbial, physicochemical, and hydrological variation, but I feel it does not shine when framed as an assessment of GHG budgets. 2. The introduction reports many interesting observations but the lack of a focused research question, hypothesis, and broader conceptual framework make it hard to identify the salient points. Revising the intro to focus on clear question (rather than just stating multiple times that little is known about GHG flux from Mediterranean riparian zones) would strengthen the paper substantially. 3. The discussion currently feels like a continuation or repetition of the results sections. Clearly summarizing the key findings and their implications at the beginning of the discussion would orient the reader to better appreciate the value of this study. Subheadings could be effective at organizing the content and an overall shortening is probably in order since the discussion is quite long for the amount of new material it presents. 4. A conceptual figure laying out the expected or observed functioning of the riparian zone in regards to respiration and denitrification would be useful and could help focus the paper. 5. There are many unnecessary acronyms that make the text unwieldy. Avoiding uncommon acronyms (e.g. GWL, SWC, NNM, NN, DNT, PLS, DEA, TCD) would make the paper more accessible. 6. The paper is generally well written but it has quite a few non-standard phrasings and English formulations. Asking for a proofread from a native speaker would be worthwhile.

If the paper can be restructured around a compelling question, it could be a valuable contribution to our understanding of riparian zones in the larger landscape context, but the current lack of focus limits the paper in its current state.

Line edits: 21: powerless is a strange word choice 38: Not clear what 70% of total

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emissions means. Also no need to put greenhouse in quotes 40: Not clear what this means? 44: contribute to increasing 59: may complicate upscaling (instead of may difficult to upscale) 72: alter instead of vary and measure instead of measured 88: compositions 111: with instead of by using 262: There is a large body of research on scaling riparian soil measurements. References: There are several inconsistently formatted references. 620: Figure 5 has a pretty low information content. I wonder if it could be included in the supplementary information.

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