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Interactive comment on "Deepening roots can enhance carbonate weathering" *by* Hang Wen et al.

Anonymous Referee #3

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This manuscript deals with a topic of paramount importance in the present context of soil C research: the potential consequences of land-use changes (in this case, wood encroachment in grassland areas) in the soil inorganic C dynamics. It offers a comprehensive perspective and succeeds in providing some simulated data not only on the potential consequences of such change on soil CO2 and water flows, but also on their relative importance as drivers of carbonates weathering in the study area. In this sense, it offers, within the limitations of the modelling approach considered and the available data, a quantitative view of the potential consequences of the mid-term changes in soil pCO2 and water infiltration when vegetation changes in a soil with calcite accumulation at depth.

In my view, the manuscript is well written, describes well a complicated numerical

simulation (the materials in SI are very helpful), and contains interesting information, and a territory-based approach that makes it worth of interest.

The contribution is therefore meaningful and adds to a field where more information is needed. The quality of Figures is very high, both in terms of graphical design and the information they provide.

There are however some points that were not completely clear to me while reading the manuscript, and therefore I consider that they could be complemented for a clearer understanding. I cannot add much to the thermodynamical considerations in the construction of the model, but, as a soil scientist, there are some questions that came to my mind while revising the manuscript.

- In relation to the soil profile description and the gradient in calcite concentration depicted in Figure 1 ("The gray color gradient reflects the calcite abundance with more calcite in depth") and Figure 3.b, it is not clear for the reader which was the actual calcite distribution with depth. Only in the supplementary information (Table S1) it is said that the calcite volumetric concentration (m3/m3) was 0 in the upper soil horizon (0-54 cm). In line 324, the calcite-no calcite interface is set at 55 cm. If this is correct, this makes that in the model representing the grassland situation, where 95% of the flow was considered to "exit the soil column to the stream" at 50 cm (line 166-167), there would be almost no contact between soil water and calcite accumulated in the soil profile below 54 cm. Indeed (line 359), in the HF simulations, it is said that water "bypassed the calcite zone". I understand this can represent the actual situation in the study site, but, if this was the case, I believe this should be more clearly stated in the description of the site and the model: deeper rooting with wood encroachment would actually take soil water enriched in CO2 to the calcite accumulation areas in the soil profile, where otherwise (grassland), only a very limited amount of it would reach. I also wonder how realistic this can be if the actual distribution of rainfall along the year would be considered (for instance, intense storm episodes or wet vs. dry season rainfall patterns).

- In this sense, there is some confusion about the type of materials water enters in contact with at depth. While the abstract talks about “carbonate rocks” (line. 27), suggesting the parent material, the description of the soil profile (Table S1) denotes the horizon at 146-188 cm as “B” and that at 99-145 cm as “AB”. This would mean that these would be accumulation horizons of materials leached from the upper “A” horizon (0-54 cm). If I understood well this table, their mineral composition according to this table indicates that calcite was not the dominant mineral in any of them.

- In relation to this, a point that is also not clear to me in the manuscript, is to what point re-precipitation of dissolved calcite at depth could determine some of the consequences of increased calcite dissolution with deeper woodland root systems. This would affect both the final fate of Ca and DIC in groundwater, and the assumptions done on the changes in permeability due to increased porosity with calcite depletion (lines 385-394). It is known that in this type of soils, carbonates can re-precipitate for instance around root channels, where water concentration can decrease rapidly compared to the bulk soil matrix.

- In relation to the annual average $CO_2(g)$ and CO_2 (aq) concentrations used in the numerical simulations (Table 1), and the idea explained in l.310-315 that some “impositions” on time and depth distribution of soil CO_2 had to be done to the model to capture the variation in alkalinity and Ca data and different horizons, I wonder to what extent this can be considered in the discussion about the use of annual averages to explain processes that can be very dependent of monthly (or even daily) fluctuations (in addition to spatial heterogeneity).

- Finally, a small comment on the term “shallow soil”. It is used repeatedly in the manuscript to refer in fact to the model where grassland roots and subsurface waterflow are considered (in contrast to woodland roots and waterflow). For example, in l. 319 (“soil CO_2 production rate was the highest in the shallow soil”). In my understanding, the soil considered in the model has the same depth in all cases (Figure 1). This makes the term “shallow” (vs. deep?) misleading. I’d suggest to revise this point and

either change the term “shallow” and/or to use it to qualify the upper soil layer or the waterflow, not the soil.

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