

Interactive comment on “The response to pulse-like perturbations in atmospheric carbon and carbon isotopes” by Aurich Jeltsch-Thömmes and Fortunat Joos

Anonymous Referee #2

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The authors explore the interactive responses of carbon and carbon isotopes in the land-ocean-atmosphere system to a suite of atmospheric perturbations on timescales spanning from decades to 100,000 years. By considering feedbacks from CaCO₃ compensation and weathering as well as short term feedbacks from the land and ocean in an Earth system modeling framework, this study provides an important insight regarding changes in the global carbon cycle on a wide range of timescales. Another novel aspect of this study is that the authors developed and used analytical expressions to better understand the different responses in atmospheric CO₂ and the d¹³C of CO₂, simulated from the numerical model. This study is worth being published in *Climate of the Past* after clarifying the following few points:

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1. One of the main findings is that atmospheric CO₂ perturbation is relatively gradually removed over a 100,000-year timescale, due to non-negligible effects of carbonate compensation and rock weathering feedbacks on atmospheric CO₂. On the other hand, the atmospheric d¹³C-CO₂ perturbation is relatively rapidly removed because the oceanic biological pump and the subsequent burial into sediments play a dominant role in removing atmospheric perturbation (as stated in Abstract). It is interesting to see that the biological pump and the burial into sediments can effectively remove the atmospheric d¹³C-CO₂ perturbation on an e-folding timescale of 6 years (Table 2), while oceanic d¹³C-DIC is removed on much longer timescales (Figure 2b). What are the mechanisms by which the oceanic d¹³C-DIC is removed? If the burial of organic carbon from the biological pump is responsible for the long-term removal from the land-ocean-atmosphere system, I feel that the sedimentation model deserves a more detailed description than just pointing to a previous study. I also feel that the results are too briefly discussed in the manuscript. For example, how much organic carbon is buried in time with which d¹³C signatures?

2. It is also possible that the dissolution and weathering fluxes of CaCO₃ and the subsequent mixing with seawater might be responsible for the long-term removal of the d¹³C-DIC perturbation from the ocean. Furthermore, weathering fluxes to the ocean, with a d¹³C signature of -9.2 permil (as stated in the introduction, if I understood it correctly), might be important for the oceanic d¹³C-DIC budget, although this may not be directly important for the atmospheric d¹³C-CO₂. How does the contribution from weathering fluxes to the d¹³C budget compare with the contribution from the burial fluxes to marine sediments? How does the weathering fluxes in the model change in time and why? It would help if the authors discuss it in more detail. In any case, it is interesting that these slow, yet persistent, carbon burial or weathering fluxes over 100,000 years can remove about 80% of the total d¹³C perturbation in 100,000 years. A related question is the carbon isotopes budgets for the simulations SED and CLO (i.e., Figure 2 for SED and CLO simulations)?

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3. Another major finding from this study is that the burial flux of d13C is influenced by the spatial (vertical?) d13C gradients. It would be convincing if the authors show the temporal changes in the burial fluxes of d13C along with an index of the spatial d13C gradient for the suite of simulations.

4. The analytical expressions for the impulse responses indeed help to better understand the different behavior for CO₂ and d13C-CO₂, yet require some clarifications. The authors assumed 2 ocean-atmosphere boxes for the equation related to CO₂ while assuming 3 ocean-atmosphere-land boxes for the equation related to d13C-CO₂. Is it because for CO₂ budget the land carbon storage can be neglected? I can follow the derivation except (A1). From (A1), I can infer that atmospheric carbon concentration is equal to DIC₀ at time = 0. I think that a scaling factor might be missing here, as only 1 mol of CO₂ is equilibrated with every 200 moles of DIC. The scaling factor might be included in the authors' definition for h_a , and would be canceled out eventually, resulting in the same analytical expression as equation (5). Yet it should be clarified. The scaling factor depends on the perturbation P when P is large, hence we would end up arriving at an equation (5) that depends on P when P is large, which might be a reason that we see a different response for the p5000 simulation in Figure 1a (?).

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