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Climate of the Past
Discussions

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

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Hi Aurich and Fortunat,

Great paper! In my opinion, this is already well-written and clearly presented. The work is relevant to anyone interested in the carbon cycle, especially to those of us interested in d13C.

I'm curious about two things:

(1) One conclusion of the paper is that you cannot get a 0.2 ‰ depletion in atmospheric d13C-CO2 and a 10 ppm rise in CO2 concentration from a terrestrial pulse,

as is suggested by Bauska 2018 as a possible cause of the variations observed at the onset of HS4. Your conclusion is based on your Figure 6, which shows that terrestrial pulses of carbon reduce d13C-CO2 only ~0.1 ‰ per 10 ppm increase in CO2. But you state in the caption that the results in Figure 6 are the means of the model output for the 30 years immediately following the perturbations. So the atmospheric data during the perturbations (i.e. while CO2 is increasing and d13C-CO2 decreasing) are not considered? In my own modeling experiments with the OSU 14-box model (model described in Bauska 2016), fitting d13C-CO2 v. CO2 output during a land carbon pulse, not just the recovery after it, significantly decreases the slope in the Keeling plot and makes a -0.2 ‰ change in d13C-CO2 per 10 ppm increase in CO2 due to land carbon seem more feasible. Do you think that the perturbations themselves are too fast to be recorded in ice cores? Surely this is true for extremely fast pulses, but the results in Figure 6 include perturbations up to 400 years duration, which I think should be captured by ice cores even with relatively large gas age distributions.

Put another way, I observe that land carbon pulses in our box model plot as quasi-ellipses in a Keeling plot with the slope of the d13C-CO2 v. CO2 being more negative on the way "up" versus on the way "down," even for multi-centennial carbon releases. So I guess the question is - to what extent is the "up" variability recorded in ice cores? - because it may argue for the Bauska 2018 interpretation. The conclusion in your paper seems particularly strong without elaborating more on this point.

(2) Another conclusion in the paper is that the PGM-LGM d13C-CO2 difference observed in the Schneider/ Schmitt/ Eggleston work cannot be due to changes in terrestrial carbon storage, or internal reorganizations in the marine carbon cycle without considering burial. But I had a difficult time understanding how the results of your experiments argue for these points. Regarding terrestrial carbon storage, is your point that the PGM-LGM land carbon difference would have been too large because the atmospheric imprint of any smaller land carbon transfer would have been attenuated too much to cause the observed PGM-LGM d13C-CO2 difference? If so, the connection

between the model results and the stated conclusion could be clearer. I thought it was also difficult to follow your point about the marine carbon cycle. Perhaps showing the marine and atmospheric d13C data for the PGM v. LGM would help, and stating more clearly exactly how your experimental results support the conclusions.

I'm very keen to hear your thoughts. Thanks, and good luck with the rest of the submission!

- Andy Menking

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