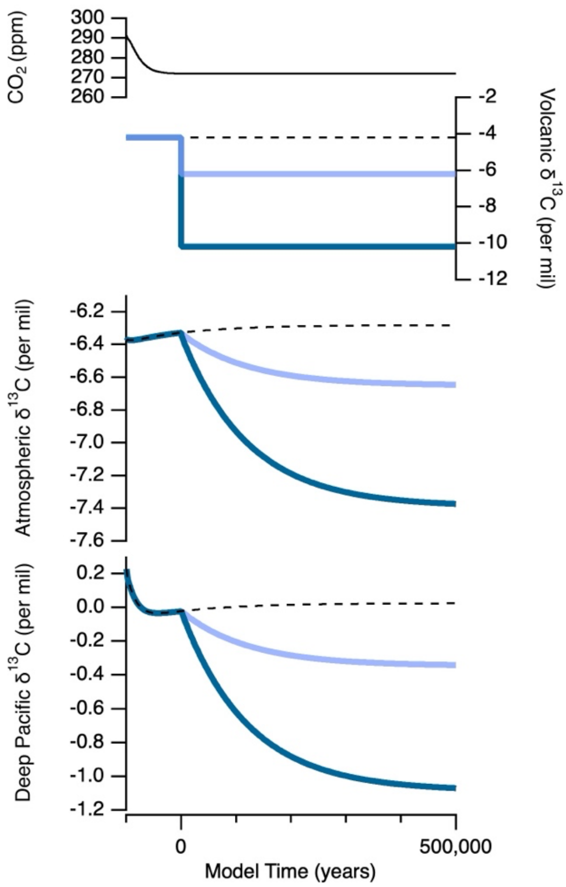
Review of Köhler, P., "Closing the Plio-Pleistocene ¹³C cycle in the 405-kyr periodicity by isotopic signatures of geological sources https://doi.org/10.5194/cp-2024-63 Preprint.

Summary

Köhler presents a series of modelling experiments that quantify the possible impact of changes in the isotopic signature of either carbonate weathering or volcanic emissions on the carbon cycle over orbital timescales. The analysis forms the basis of a novel hypothesis that the mysterious presence of 400,000-year cycles in the ^{13}C cycle are a product of changes in the isotopic composition of sources.

The paper will be an interesting result for the readers of Climate of the Past and particularly timely as it could inform new results from the soon to be extended ice core records.

The main result that isotopic composition of whole ocean-atmosphere-land biosphere system can change if you modify carbon isotope signature of the long-term carbon geologic fluxes (either volcanic or weathering) is not that surprising. What is surprising to me is the timescales over which these changes have the power to modify the whole ocean-atmosphere-biosphere system given the small fluxes (~0.1 PgC per year) into such a large system (~40,000 PgC) have enough leverage on the system (i.e. a ~400,000-year response time). See Figure 1 in this review where I spun a similar box model and then suddenly changed the isotopic composition of volcanic emissions. Yes, it changes the 13C of the whole system, but it takes a lot of time and input has to be large. Because of this weak lever, it means some of solutions presented in the analysis require one to really ratchet on the system by changing the isotopic composition of the source by a lot (sometimes up to 10 per mil in the study).



My primary criticism of the analysis is that I am left wondering how the isotopic composition of volcanic/weathering sources could shift (for long periods of time) by such wide ranges. I wasn't very convinced the mean isotopic composition could shift within the full range observed in either modern volcanic sources or the entire geologic record of carbonates. To shift the mean from one end of the distribution to the other, sometimes within a glacial cycle, seems extreme. Considering the volcanic hypothesis, it would suggest that at times in the past only small fraction of the modern-day volcanic regions are active, yet somehow as pumping out as much CO2 as today. Considering the weathering hypothesis, it would suggest that a relatively narrow geographic area might be contributing to the weathering flux. Overall, I would suggest some <u>more work needs to be done to quantify if the swings in the source isotopic composition are feasible</u>.

Major comments:

1. **Experimental design**

I struggled a bit to see the utility of the "prescribed/overwritten" experiments. I'm not sure they add much to the discussion. First, they are unphysical as they break conservation of mass and secondly, as the author acknowledges, are unrealistic as the swings in the isotopic composition are too large. From an illustration point of view including them in the figures blows up the y axes (even when the axes are truncated which is not ideal). I would try and remove the prescribed experiments from the main text completely. This would also help the reader see the more realistic nudged experiments. In the current version I had to zoom in to maximum extent to see the important variability (my printed copy couldn't resolve the figures)

Secondly, it was quite tricky for me to pick out from the figures and the text exactly how big the impact the nudged experiments had on d13C budgets. It's kind of in the figures as the presumably the difference between the "control" runs the prescribed runs, but this is tricky to visualize. Note one issues is that there is often a mean offset between the control and the nudged runs which I don't think is mentioned in the text. I would strongly suggest that somewhere the effect of the prescribed changes in isolation (i.e. just changing the external isotopic signatures) are shown.

2. **Why were changes in the mean isotopic composition of organic carbon not considered?**

By the logic that isotopic composition volcanic emissions or carbonate weathering is sufficiently wide to allow for major shifts back in time, it would follow that the changes in the weathering and/or burial of organic carbon, which also have a wide range of isotopic values could impact whole ocean-atmosphere-biosphere system. From Cartapanis et al., 2018: "We estimated the mean δ^{13}C of organic matter in the first 10 cm of the sediment as −22.2 ‰ with a standard deviation of 2.3 ‰, consistent with prior literature (Sundquist and Visser, 2003)." Note however, that doesn't include the possibility of long-term changes in the C3-to-C4 abundance which seem possibly over these timescales)

Is there a reason that scenarios involving organic carbon were not considered? From the main body and supplementary information, it was not clear to me if long-term weathering and burial of organic carbon was included (there is a mention of the supplemental figure of phosphate burial). I tested this by running my model with the same experiments as in Figure 1 both with (shown) and without (not shown) a long-term weathering and burial flux of organic carbon (~0.06 PgC per year). Without the long-term fluxes I found the impact of changing the isotopic signature of volcanic sources was slightly greater additional gross fluxes. Although a relatively minor effect, it would be good to know if they are included.

3. **Testing the plausibility of the scenarios**

One of the arguments for plausibility stems from the fact that the modelled changes in the source signature fall within the range observed distributions.

It would be more convincing if the changes in the isotopic signature of the weathering or volcanic fluxes were correlated with the modelled net fluxes. Is this the case? For example, it would make more sense to me if you had a scenario where volcanic flux increased after deglaciation and also changed the isotopic signature (perhaps reflecting more arc degassing and less mid-ocean ridge sources?).

On the weathering hypothesis, I can't think of any suggestions that are beyond the scope of this paper. I would suggest future work looking at the spatial pattern in the isotopic composition of riverine input to the ocean to see if obliquity-induced changes in the climate could cause major shifts in the delivery of ^{13}C to ocean.

4. **How are we going to test this hypothesis(es)?**

One of my major challenges with reading the paper was trying to figure out this this was a testable hypothesis. In some ways, it's a good challenge to have as it makes the reader think of new and interesting things to measure. However, I would have appreciated some more guidance. Moreover, it should be the case that when a new hypothesis is presented, we should set out ways to test it. Could the author please elaborate on the path forward in the conclusions?

Line by line comments:

Line Data 2.1. This section is too short and should either be folded in to below or more motivation for the compilations are needed. I suggest something to the effect of a list "To perform this analysis the following are needed:

Lines 77-98. Description of d18O and d13C decoupling conundrum. There's a lot of description the benthic d18O stack and its many features and thus a lot is packed in, including some very brief mentions of theories. Here I suggest a picture is worth a thousand words. Why not show the d18O stacks alongside the d13C (by combining Figure 1 with one the supplemental, possibly leaving the wavelet of d18O in the supplemental as it a better known).

Lines 125: Volcanic CO2 signature. *Mason et al., 2017* is a great up-to-date reference to use. Also, it may have ramifications for the analysis as the global emissions are estimated to range between at –3.8 to –4.6 per mil, slightly heavier than is typically assumed.

Mason et al., Remobilization of crustal carbon may dominate volcanic arc emissions. Science357,290-294(2017).DOI:10.1126/science.aan5049

Line 173. Please briefly note the conclusions of this study.

Line 178. "a lot bigger" = "larger/greater"

Table 1: The naming convention for the experiments is quite confusing as they don't contain any obvious information about whether they are nudged or prescribed. For examples, the letters -L and -P aren't clear to me. I would suggest a wholescale reset.

Line 290 "not a lot different" please rephrase with more precise language.

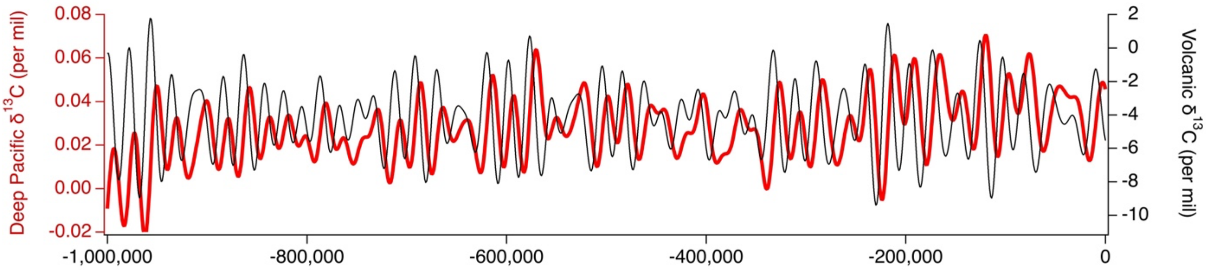
Line 312 ". Remember that in non-linear systems resulting frequencies might differ from those in the forcing (e.g. Rial et al., 2004)." I wasn't sure exactly what was being alluded to here. Can you elaborate more about the missing 400 ka cycle?

The premise of the analysis is that the 400ka cycle is difficult to explain and is missing from a modelled scenario, so I'm also a bit perplexed about why it does not then emerge from the analysis. Because you input (changes in rock or volcanic d13C) is passing through a massive damper, it would make sense that you would lose the higher frequency changes and those that are preserved would be heavily lagged. The fact that the results suggest the dominant power is around 40ka could suggest a number of things which I would appreciate some more discussion:

1) The control experiment (ie the SE) is missing crucial components in the 40 ka and thus the result is purely down to an inaccurate representation in the control.

2) The presence of the strong 40 ka cycle in prescribed/nudged experiments is a product of overfitting. If the model is trying to fit 400 ka signal with a very sluggish response time 100-400ka) there's a possibility that the model could overshoot and then undershoot if the tuning parameters are too sensitive. With the current analysis, I couldn't rule this out, but I believe the author could show with some simple experiments whether or not this is the case.

One possible way to help the reader understand what is going on would be to force the model is with synthetic timeseries of d13Cvolc/weathering with set periodicities and powers (e.g. an orbital curve with 400,100, 40, and 20 ka). Run that forcing through the model and see how the damper of the carbon cycle alters the resultant d13C in the atmosphere or deep Pacific. As example, here's a result using 65N summer insolation tied to large swings in the volcanic signature over the past 1 million year. This would also allow you to address how strong the lever is on the system. Alternatively, this could be done with the experiments I suggested above for isolating the effects of the prescribed isotope fluxes.



Line 390 "the ring of fire" please be more exact.

Figure 3. Colouring of data in panel C changes from red to grey and is confusing compared to other modelled scenarios also grey.

Sincerely,

Thomas Bauska

References:

Cartapanis, O., Galbraith, E. D., Bianchi, D., and Jaccard, S. L.: Carbon burial in deep-sea sediment and implications for oceanic inventories of carbon and alkalinity over the last glacial cycle, Clim. Past, 14, 1819–1850, https://doi.org/10.5194/cp-14-1819-2018, 2018.

Emily Mason et al., Remobilization of crustal carbon may dominate volcanic arc emissions.Science357,290-294(2017).DOI:10.1126/science.aan5049