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Climate of the Past
Discussions

Interactive comment on "Arc volcanism, carbonate platform evolution and palaeo-atmospheric CO_2: Components and interactions in the deep carbon cycle" *by* Jodie Pall et al.

Anonymous Referee #2

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This is an interesting study that explores links between arc activity and the long-term CO2 budget. This is not a new effort, as the authors acknowledge, but the authors apply a statistically rigorous method that most reliably tests the relationships between two signals, subduction zone length and atmospheric CO2. This manuscript is also very well written and constructed. That said, I do have a few concerns on the data compilation, methodology, and the interpretations. I do not have a good grasp of wavelet analyses used in this paper, so I would not comment on the methodology. However, I understand that the conclusions are based on the reconstruction and data compilations.

The study is partially inspired by the hypothesis in Lee et al. papers. I notice that "continental arc" in Lee et al. is not equivalent to "CIA" in this study, because thickened crusts lead to evolved magma that is more efficient in wall rock decarbonation when intruding.

It is good that the authors did not extract CO2 signals from models like GEOCARB, so did not mix model results with the data extracted from natural samples. In addition to the uncertainty issues noted by the authors, GEOCARB scales tectonic input of CO2 to the spreading rate of mid-ocean ridges, so does not explicitly account for magmatic-metamorphic outgassing at arcs. Although faster MOR spreading in general corresponds to more vigorous global tectonics, arc activities also depend on the configuration of plates. Thus, it is logically inconsistent to compare arc lengths with GEOCARB model results. The authors apply a filter to remove high-frequency noises (note: "noise" might not perfectly appropriate in this context), but I doubt whether the resolution and uncertainties in ages of Park and Royer data are within a few million years. I suggest that the authors briefly summarize and discuss both uncertainties and temporal resolution of the data compilation of Park and Royer (2011).

I have a major concern on the accumulation model used in this study. Early Paleozoic and pre-Cambrian carbonate platform deposits are entirely ignored in the reconstruction, so the CIA curve (Fig. 4) starts at 0 km at 410 MA, which is unrealistic. The CIA lengths almost monotonically increase, and the present CIA length is about 6 times of Permian and 1.5-1.2 times of Cretaceous. This curve alone, without any wavelet analyses, would falsify the hypothesis of Lee et al., misleading the readers to conclude that the contribution of arc activity to Earth's long-term climate is minimal. As the authors have noted, the accumulation model provides an upper bound because it is assumed that the platform carbonate was not depleted in geologic events (erosion, subduction etc.). It is only fair to compare the arc lengths with CO2 proxies if the authors also provide a lower bound estimate.

The CIA and NCIA lengths are derived from the GPlates reconstruction and 24 palaeographic maps. It is not clear to me how the authors differentiate arc lengths from lengths of convergent margins, or subduction versus collision zones. Again, it will help the readers to assess the quality of reconstruction if uncertainties are discussed in addition to the model assumptions and limitations. In the current version it sounds a bit like almost no error! The authors need to justify that the data and model reach a 1-Myr resolution (Line 7, Page 26). The present global length of CIA in Figure 4 is about 35,000 km, more than double of the lengths of continental arcs measured from geologic maps (15,000 km; Fig. 4d in Cao et al., 2017 EPSL). This large discrepancy makes me worry about the GPlates reconstruction in this study. Where does the extra CIA length account for? It is essential to address this discrepancy in the revised manuscript for the readers to understand the meaning of CIA defined in this study.

I am extremely curious to know how wavelet analysis using the compilation of Cao et al. (2017) or McKenzie et al. (2017) and the CO2 record (Park and Royer, 2011) would turn out.

It is not clear to me how arcs on subducted fossil plates are constructed in GPlates or whether this portion of fossil arc (subduction zone) is added in Fig. 4. This is not directly relevant to the conclusion of this study, but the authors should state the assumptions and protect their model results from being misinterpreted.

I don't think it is a good idea to mix all decarbonation processes at convergent margins in Section 5.3. These are not the limitations of THIS model that addresses the relationship between arcs and long-term CO2. Instead, the authors might focus on a series of assumptions and limitations in the reconstruction and data compilation (comments above).

To address these concerns, it potentially requires substantial work of model development and data compilation and it seems that this will take long. That is why I suggest rejection but with strong encouragement to resubmit. I hope the authors are willing to perform a major revision, as it will significantly strengthen their arguments. I would like

to say that I have great respect for the work that has been done in this project and for this research group in general, but I cannot be positive at this time. I very much hope that my comments help to improve the manuscript.

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