**Climate
of the Past**

Discussions

EGU

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 #1

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General comments:

This is a clear, well-written analysis of an important problem in paleoclimate: what is the connection between tectonic activity and the carbon cycle/climate? The hypothesis is clearly stated and the use of plate reconstructions for the analysis is a useful and appropriate way to approach the problem.

The wavelet analysis appears to be rigorous. However, I wonder why this method was chosen rather than, for example, a simpler autocorrelation? Autocorrelation would appear to address the same hypothesis without assuming periodic behavior. In fact, the existence of periodic signals on the order of 10s of millions of years is very surprising –

if those signals are real and significant, then the authors should explore possible mechanisms for generating such periodic changes in CO2 and/or arc activity (e.g. around pg 9, Line 17-18 and pg 11, section 4.1). It is not clear to me how or why periodic signals should appear from tectonic interactions with carbonate platform. Without a proposed mechanism, perhaps the signals are simply noise in the data?

I also find it very surprising that arcs interacting with carbonate platforms seem to have increased 5-fold (as shown in Fig. 4) from 250 to 50 Ma. Is this result perhaps an artifact of only mapping out Phanerozoic platforms from the Kiessling 2003 database? Why are Precambrian platform areas not included? There are many examples of known, extensive Precambrian carbonate platforms, and I suspect that adding them to the analysis would remove a significant portion of this signal. It seems unlikely that such a major change in magnitude could occur based on tectonic interaction with a depositional environment known to have existed since the Archean. If this result is robust, then the authors should advance some possibilities for why this dramatic change occurred.

If the above comments can be addressed, this study demonstrates a useful application of global plate reconstructions for examining Earth system behavior over the last ∼400 Myr.

Specific comments:

One aspect of the analysis was unclear to me: are the locations of past arcs mapped out in ancient plate reconstructions? This was the impression I received from the description of the plate reconstruction model, the mapping of carbonate platforms, and Fig. 5. However, this impression seemed to be contradicted by Fig. 4 caption and pg 4, lines 6-8, which describe using subduction zone lengths as a surrogate for volcanic arc lengths. Why is the latter necessary if the arcs and plate boundaries can be accurately mapped out? I hope the authors can clarify their methods.

Fig 2/3: If filtering has removed any signal < ∼5Myr, that portion of the results should

be removed (or at least indicated).

Perhaps refer to Figure S1 (showing proxy CO2 data) when those data are mentioned (pg 9, line 24 and pg 10, line 10). Additionally - how is noise/uncertainty in the proxy data accounted for? How is the sparseness of data older than 220 Ma addressed?

Technical corrections: Pg 7, line 11: 'temporally limited to the Devonian' is unclear (it sounds like only the Devonian is being analyzed). Consider specifying that the maximum time considered in the analysis is the Devonian. Pg 9, line 4: wording is awkward: 'corresponds to an upper limit by which carbonate platforms can interact...', consider changing 'by which' to 'for interactions of carbonate platforms...' Pg 10, line 22: Wording: 'modelled data' is unclear.