Climate
of the Past
Discussions

CPD

Interactive
comment

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.

Jodie Pall et al.

jodierae.pall@gmail.com

Received and published: 11 November 2017

Manuscript Title: Arc volcanism, carbonate platform evolution and palaeo-atmospheric CO2: Components and interactions in the deep carbon cycle

Manuscript Number: cp-2017-112

General Comments by Anonymous Referee #1:

Referee comment: 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

Printer-friendly version

Discussion paper

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.

Author response: Autocorrelation calculates the correlation of a signal with a delayed copy of itself. In other words, it analyses periodicity in signals in a single time series and cannot be applied to compare two time series like those in our study. The equivalent of autocorrelation for two time series is cross-correlation, which is the time-domain equivalent of cross-spectral analysis in the Fourier domain. However, neither cross-spectral correlation nor cross-correlation can characterise correlation as a function of time or scale, unlike in wavelet analysis. Wavelet analysis, which is similar to cross-spectral correlation, is fundamentally different as it does not use sine or cosine waves to characterise a signal, but (as in our case) Morlet wavelets. It then deconvolves the signal into constituent Morlet wavelets to attempt to find similarities between two time series, irrespective of whether periodicities exist in signals. In this way, wavelet transforms are not only used for detecting periodicities in signals but to detect any time- or space-dependent correlations in signals. For clarity, justification of the wavelet analysis method were added on pg. 3, line 17-24 and pg. 4, line 4-6.

Referee comment: 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).

Author response: The referee is correct. However, in our analysis there were no periodicities < ∼32 Myr that were found to be significant and meaningful. Instead, only short-term in-phase behaviour has been investigated in the discussion which links a peak in carbonate-intersecting arc (CIA) lengths to a peak in palaeo-atmospheric CO2. In agreement with the referee's comments, the possible mechanisms for arc-related periodicities have been indicated in the introduction (see pg. 3, line 12-15), however the

authors believe that further explanation of the mechanisms is not warranted given the results, and is beyond the scope of the paper.

Referee comment: 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?

Author response: Following on from response #1, wavelet analysis was applied to find regions of correlation and not necessarily periodicities. However, it can be applied to uncover periodicities if there are any. Hence, the objective of this paper is to find correlations and periodicities that have not yet been explored. Lee et al. (2013) identified the possibility of periodicity which is a key assumption in the paper. Inclusion of this assumption has been added to the Introduction on pg. 3 line 14-17. Patterns of CO_2 storage occur when island arcs dominate continental arcs and CO_2 liberation when continental arcs dominate, which are linked to the periodic assembly and dispersal of continents. Earlier work by Lendardic et al. (2011) propose a mantle thermal mixing mechanism to explain supercontinent cycles that alter the nature of subductions in ways that effect CO_2 release. Our investigation assumed that mechanisms such as those explored by Lenardic et al. (2011) and Lenardic (2016) and hypothesised by Lee et al. (2013) exist and cause periodic linked behaviour between arc activity and atmospheric CO_2. However, we do not focus on the mechanisms as it would be beyond the scope of the paper. Secondly, the use of the cross-wavelet spectrum with wavelet coherence is applied to make sure that signals found in the data are real and significantly different from simple noise.

Referee comment: 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?

Author response: The result the referee highlights is because an Accumulation Model

of carbonate platforms was used which assumes that crustal carbonate reservoirs grow in thickness and spatial extent through time. Unless there is a dramatic change in arc regimes (i.e. from continental to intraoceanic), CIA lengths will only increase. The limitations of this assumption are discussed in section 2.2.

Referee comment: 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 signal5. 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.

Author response: It is true that the Kiessling et al. (2003) compilation of global, palaeo-distribution of carbonate platform maps extend to the Ordovician, however incorporating these now would require an extensive amount of work. The reason why no platforms earlier than the Devonian were mapped was because initially an 'Accumulation Model' and an 'Active Carbonate Platform Model' were tested and compared. The Active Carbonate Platform model assumed that crustal carbonate reservoirs could be depleted over tens of millions of years, and entailed that platform existed within certain windows. For the results to be comparable, carbonate platform evolution started at the same time. Given that our plate model only records plate motions from 410 Ma, both models were only implemented from the Devonian. However, modelling carbonate platforms from the earliest Phanerozoic and Precambrian is part of future work. In any case, the Cone of Influence (COI) means that the earliest and latest part of the time series are affected by distortion, such that our wavelet analysis excludes characterising the Devonian (and late Cretaceous to present day). As such, we believe that adding the Ordovician and Silurian platforms would not significantly impact wavelet analysis, which is the focus of our study.

Referee comment: 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 âĹij400 Myr.

Specific Comments by Anonymous Referee #1: Referee comment: 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.

Author response:We can only map out plate boundaries and, hence, subduction zone lengths using our analysis. This is why we used subduction zones as a surrogate for continental arc lengths. However, to avoid confusion, adjustments have been made in Section 2.3 (pg. 7, line 27; pg. 8 line 1) and Section 2.4 (pg. 8, line 10) and the caption of Figure 4 (pg. 16).

Referee comment: Fig 2/3: If filtering has removed any signal < âĹij5Myr, that portion of the results should be removed (or at least indicated).

Author response: Results referring to short wavelength (<5 Myr) periodicity have been removed from: pg. 13, line 4-5; pg. 14, line 17-18; pg. 15, line 17-18; and pg. 16, line 1.

Referee comment: Perhaps refer to Figure S1 (showing proxy CO2 data) when those data are mentioned (pg 9, line 24 and pg 10, line 10).

Author response: The referee's suggestion has been implemented with corrections on pg. 9, line 22 and pg. 10, line 12.

Referee comment: Additionally - how is noise/uncertainty in the proxy data accounted for? How is the sparseness of data older than 220 Ma addressed?

Author response: Noise and uncertainty in the proxy data cannot be accounted for using our approach, nor can our plate model, as one working hypothesis, account for uncertainty. However the authors recognise the importance of quantifying error and uncertainty, and have highlighted this as an area of future work. A paragraph that addresses error quantification has been added to the discussion (pg. 24, line 8-15).

Technical corrections: Referee comment: Pg 7, line 11: 'temporally limited to the Devonian' is unclear (it sounds like only the Devonian is being analyzed). Consider specifying that the maxi-mum time considered in the analysis is the Devonian10.

Author response:'Temporally limited to the Devonian' was changed to 'in the Phanerozoic from the Devonian to present day' on pg. 7, line 10.

Referee comment: 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...'

Author response: The referee's suggestion to change 'by which' to 'for interactions of carbonate platforms. . .' was accepted on pg. 8, line 27; pg. 9, line 1.

Referee comment: Pg 10, line 22: Wording: 'modelled data' is unclear12.

Author response: 'Modelled data' was changed to 'estimates from the Accumulation model' on pg. 10, line 21.

Author comments:

Additional corrections were made in the text, which are described below: 1. The colour of the subduction zone in Figure 1 has been updated to reflect actual colours of the figure, and text has been updated on the figure. 2. 'One-to-one' changed to general on pg. 4, line 18. 3. 'Continuously' removed from pg. 5, line 5. 4. 'Colors' changed to 'colours' for consistency with British English in Figure 2. 5. A short paragraph was moved based on order of figures (pg. 11, line 19-21). 6. Text has been removed that is not considered to add anything to analysis from pg. 11, line 10-12 and pg. 16, line

18-26. 7. 'E.g.' changed to 'e.g. on pg. 25, line 5

Printer-friendly version

Discussion paper