Climate of the Past Discussions

Clim. Past Discuss., referee comment RC2
https://doi.org/10.5194/cp-2021-104-RC2, 2022

Comment on cp-2021-104

Anonymous Referee #2

Referee comment on "Clumped-isotope-derived climate trends leading up to the end-Cretaceous mass extinction in northwestern Europe" by Heidi E. O'Hora et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2021-104-RC2, 2022

Firstly, I must sincerely apologize for the lateness of this review. Despite being vaccinated and having received a booster, I still managed to get CoVid and was sick for weeks. I deeply apologize to the authors for this unreasonable delay in evaluating their paper.

The authors report on a very nice record of temperature change from sections in the Netherlands and Belgium following the eruption of the main phase of the Deccan traps. They find an apparent spike in temperatures around 8*C above background, followed by a slow decline over the following few hundred thousand years. This I s a relatively straightforward paper with what could be considered conservative interpretations of some really quite provocative data.

What these data indicate is that even though the temperature change due to the eruption of the Deccan traps was apparently significant (ca. 8*), it was also relatively short lived, having been mostly erased in 75-100kyr. This is consistent with the nature of the pCO_2 signal we see from other LIPs such as the central Atlantic magmatic province (CO_2 blips can result in a doubling, but are mostly erased over the subsequent 2-300kyr (schaller et al 2012)). These temperature estimates are also consistent with earlier estimates (eg wilf 2003, woelders et al 2017, zhang et al 2018), although the authors actually show a much cleaner signal than some of the previous work (In my opinion). Given that, the Deccan warming and the K-Pg extinction strata are pretty clearly unrelated events (as is the prevailing thinking). BUT it also makes me wonder, what are the true effects of this warming? It is very short, basically a blip with a duration of no more than 100kyr, because the samples at ~66.3 Ma are indistinguishable from background. In fact, it is possible it is much shorter than 100kyr because of the limits of the sampling resolution here. However, what I see in the data is a longer-term _cooling_ from 66.37 to 66.07 Ma. In fact, this is the most significant secular signal in the data – what does this mean? It's of the right timescale to be the predicted pCO_2/temperature decrease due to continental weathering following the emplacement of the Deccan traps (e.g., Desert et al 2001, 2003). I think the authors should highlight this!

There are always questions of timescale, so just so it's clear (and I probably missed it), but has the ejecta horizon/dust/Ir level etc. been positively identified in these N. European sections? Can we be certain that the horizons identified as coincident with the initial phase of volcanism are indeed correctly correlated? Bulk C-isotope data (Line 191) does not instill a lot of confidence… If I were to be critical of one point of this paper, it is the confidence with which the correlation is made between marine sections in Europe and the terrestrial record of Deccan volcanism that relies mostly on radiometric ages from India. This should be expanded upon and the tie points made more explicit (is the age model published?).

Can you add the timing (duration?) of Deccan volcanism to Fig. 5? I need some frame of reference for where I should be looking for the increase in temperature. The line at 66.4 is helpful but what is the duration?

It may just be my general skepticism of absolute sea level estimates in ancient rocks, but can the authors expand upon the goals of the sea level reconstruction here? To what end?