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Interactive comment on "Fossil plant stomata indicate decreasing atmospheric CO_2 prior to the Eocene–Oligocene boundary" *by* M. Steinthorsdottir et al.

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Steinthorsdottir and colleagues present a record of CO2 from the Eocene and Oligocene based on the stomatal dimensions of a single fossil species from the beech family. Their results help shape our understanding of the climate evolution across the Eocene-Oligocene transition, a topic of interest to many in the paleoclimate community. A distinguishing feature of the manuscript is the tracking of a single species over a substantial amount of geologic time: this strategy helps to minimize any species-specific biases ("biotic effects"). Although the proxy chosen is only semi-quantitative (the stomatal ratio proxy), because the authors have focused on a single taxon it is likely that

interpreted trends in paleo-CO2 are correct, even if the absolute values are not.

I start with three larger concerns, followed by a list of lower-level concerns.

1. I am surprised that more space was not devoted to discussing the paleo-CO2 work of Roth-Nebelsick and colleagues; her group has reconstructed CO2 using a gas-exchange model from some of the same sites presented here and using the same species (!). It is odd to see the work first mentioned on p. 17 (and only in passing). In principle, the gas-exchange-based CO2 estimates should be more robust than the authors' stomatal ratio-based estimates, which are only semi-quantitative. This needs to be discussed at length.

Related to this point, the authors say that pore geometry could not be measured, but Roth-Nebelsick's group has made these measurements successfully on the same species for the sites that overlap with the current study's sites. Can the authors describe this discrepancy?

2. A major take-home message of the study is that CO2 during the late Eocene (~40-34 Ma) declined more dramatically than one might expect given the parallel changes in global temperature. I don't necessarily buy this argument. The change in deep-sea d18O from 40-34 Ma is at least as large as the change across the Eocene-Oligocene transition (and some of the latter change is due to ice-volume). Hansen (2013, Proc R Soc A) presents estimates of global mean surface temperature based on the deep-sea d18O and record and ice-volume inferences; this record would allow the authors to quantify the average Earth system sensitivity within the 40-34 Ma interval.

Related to this point, the authors also claim very little CO2 change across the Eocene-Oligocene transition. But the dating constraint on the authors' earliest Oligocene site is poor. If this site is slightly younger (by one or two million years, for example), then the lack of CO2 change relative to the latest Eocene may not be surprising given the global deep-sea temperature record. I encourage the authors to pull back on this point.

And a final related point: in several places the authors seem to argue that CO2 change preceded temperature change but CO2 and temperature are still mechanistically linked. But the timescale of this lag is on the order of many millions of years. As such, this is a dramatic statement that I find difficult to accept given our current understanding of the Earth system.

3. The authors speak at length about a bias towards low estimated CO2 with the stomatal methods relative to estimates from other proxies. This is not true, at least in any consistent way. Please see figure 1 in Beerling & Royer (2011).

minor comments:

p. 2, line 12: “hysteresis effect”. I think the authors are talking about tipping points here, not hystereses. A hysteresis describes how elements of the Earth system may not come back to the same state even if other elements of the Earth system do (for example, a rise in CO2 followed by a parallel drop in CO2 may not lead to temperature rebounding back to the pre-perturbation value).

p. 3, line 29; add “on” between “based climate”

p. 4, lines 3-4: Neither the Goldner paper nor the Inglis paper are appropriate for this statement. The Goldner paper is about the narrow interval straddling the E-O boundary, and their work supports the conventional view that a CO2 decline, not oceanic reorganization, is responsible for the sharp temperature decline at the boundary; the Inglis paper is about Eocene temperatures (no Oligocene temperatures) and supports the conventional view for a secular cooling across the Eocene. These papers do not support the statement the authors are trying to make here.

p. 4, lines 5-18: The E-O records of CO2 from Pagani (alkenones) and Pearson (boron) should be discussed somewhere in here.

p. 12, line 14: add “the” before “NLE”.

p. 14, line 23: Royer (2003) shows this for Ginkgo as well. (Royer, D.L., 2003, Esti-

mating latest Cretaceous and Tertiary atmospheric CO2 concentration from stomatal indices, in Wing, S.L., Gingerich, P.D., Schmitz, B., and Thomas, E., eds., Causes and Consequences of Globally Warm Climates in the Early Paleogene: Boulder, Colorado, Geological Society of America Special Paper 369, p. 79-93.)

Figure 4: Add errors bars for the temporal uncertainty in your CO2 estimates. Perhaps use dashed lines given that you know that the temporal ordering of the sites is correct (even if the absolute ages are fairly uncertain).

With regards, Dana Royer

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