Clim. Past Discuss.,
doi:10.5194/cp-2017-17-RC2, 2017

Climate of the Past Discussions

Interactive comment on "Atmospheric circulation and hydroclimate impacts of alternative warming scenarios for the Eocene" *by* Henrik Carlson and Rodrigo Caballero

Anonymous Referee #2

Received and published: 3 April 2017

This study uses a global climate model to try to answer the very interesting question of how warm worlds due to CO2 are different from warm worlds due to shortwave forcing (represented here by reduced cloud albedo), and how we might be able to see the difference in the geologic record. It is a fascinating and wide-ranging paper with a lot of insight.

However, on one key point (how to leverage the paleovegetation record) I think it is really missing the forest for the trees, so to speak. Namely, it focuses on regional water-induced vegetation differences between the two scenarios using a metric that the authors admit may not have much relevance. Yet direct CO2-induced vegetation differences would probably be much more one-sided, larger and easier to detect (and

by my reading of the literature would be likely to overwhelm the water-induced differences, calling their highlighting here into question.) Thus I recommend major revisions before full publication. I also have quite a few more minor science suggestions and writing suggestions under Minor afterward.

Major issue:

p2 li22, p9 li7-10, etc: The vegetation patterns might indeed carry some signature of the different hydrological cycle or regime, but an even stronger vegetation signature of LCTC vs. high-CO2 would be the direct CO2 effect on the vegetation itself! One would expect a very-high-CO2 world to be much greener than today's, with plants surviving in hydroclimate regimes that today cannot support them (because of the much lower transpiration losses in order to fix a given amount of CO2, i.e. the increased water-use efficiency.) In contrast, an LCTC world would not be expected to be particularly greener or browner than today's, except where induced by regional hydroclimate change. So the global vegetation extent and pattern could be a key *direct* indicator of the mix of forcings, independent of the forcings' effect on the water cycle. Certainly the vegetation pattern you prescribe (from the figure in Sewall et al 2000) is much greener than today's, with no unvegetated or desert-vegetation areas and very extensive forests, so you are almost implicitly assuming a major role for CO2 (or at least Sewall did.)

Similarly at end of page 3 and beginning of page 4 (and again p9 li12-13), yes the high CO2 might change the stomatal conductance and thus the terrestrial hydrology, but even more importantly it would dramatically change the vegetation itself (which is the thing you want to observe in the geologic record as you say on p9 li8-9), even before you get to the hydrology (!) You allude to this at p9 li13-14 when you cite the Roderick paper, but the way you put it is quite an understatement. Far from just "imperfect", the P/PET seems to have essentially nothing to do with the vegetation response when CO2 changes are involved. . . the CO2 utterly dominates. At least in models. See the plot in the Scheff manuscript, it is pretty stark (and largely backed up by the paleo vegetation data as they discuss.)

So I would strongly suggest getting rid of the whole PET-based analysis and instead looking at the land model's direct photosynthesis and/or leaf-area-index output if you want something you can qualitatively compare to paleovegetation records. Again, the contrast between the high-CO2 (fertilized) and the LCTC (unfertilized) case should be dramatic to say the least, unless the land model you are using is so old that it doesn't include CO2 effects. If you do this, you should add "and global vegetation" after "regional climates" at p11 li14.

I see that you are aware of this to some degree, and trying to get at this with the caveats and narrowing of the scope at p9 li21-24 and p10 li3-7 & li26-29. But it would seem cleaner and more of a clear demonstration to just plot the simulated changes in vegetation quantities like photosynthesis or LAI, instead of qualitatively fudging together the P/PET index changes with a vague expectation of additional greening/browning. It would also better highlight the most useful way of distinguishing high-CO2 from LCTC in the paleovegetation record - via the *direct* CO2 effects which have potentially global scope, rather than via the water effects which are much more regional and iffy as you point out.

Note, you can still keep all the precipitation and circulation stuff, since precipitation will still be relevant for understanding changes in P-E which will manifest themselves in proxies like paleo rivers and lakes, signatures of soil infiltration rates, etc. Precipitation also directly drives some of the paleomagnetic proxies. So I'm just talking about replacing sections 6 and 2.2 here, not touching the bulk of the paper which is much more relevant.

If you are really after differences in "eco-hydrological regime" (p9 li15) rather than vegetation per se, then I'd strongly recommend using P/Rnet (as suggested by the Milly and Dunne paper) rather than P/PET, to be conservative. But I'm not sure this is the best approach either, since there is no paleoproxy for eco-hydrological "regime" whatever that is. Instead there are proxies for specific tangible things like vegetation, rivers, lakes, infiltration rates, water isotopes, paleomag, etc. that all respond quite differently

to global climate change. In particular, vegetation responds very differently from P/Rnet or P/PET when CO2 is involved (as far as we can tell.)

Another way to see this is that satellite data shows the Earth is greening in recent decades (Zhu et al. 2016 Nature Climate Change) even while a P/PET calculation would tell you it has been drying (some of Fu's papers which analyzed historical data). This implies P/PET is not just "imperfect" for vegetation change under CO2/climate change, but grossly misleading.

Minor: (Note, some of these are on the PET parts, so they will not be relevant if you decide to replace the PET analysis as suggested above.)

p1 li10: Do you mean 11% greater absolute precip, or 11% larger precip *response* to warming? This should be clarified. See p4 li26-29 comment below.

pi li24-25: Can you at least include a few words about why one might think the Eocene would have fewer aerosols than the present? I.e., does this hypothesis just come out of nowhere or is there some qualitative speculation/cartoon behind it? Given how vegetated and warm the Eocene was, I would a priori think it would have more aerosols than present, via much-increased biogenic VOC emissions as well as more fires (more fuel.) Though, I suppose it would also have less dust due to the increased vegetation cover. So it could go either way.

p2 li30-31: The atmosphere and ocean models are specified here, but not the land model. What land model was used?

p2 li32: By prescribing a seasonally-varying q-flux but *also* using a slab mixed-layer ocean, you may be double-counting the seasonal storage and release of heat in the mixed layer. . . be careful here. What is the depth of the slab ocean? If you are going to use a seasonally-varying q-flux, your slab ocean should probably have minimal depth, since the seasonal cycle of the q-flux is *already* dominated by the fluxes in and out of the ocean every year due to seasonal warming and cooling of the mixed layer. (Does this make sense?) In this case, you should also insert “and seasonal storage” after “transport” on line 31.

p3 li25: For the reader unfamiliar with hydrology and PET, you should also mention that capital Delta is the slope des/dT of the es curve (instead of just giving its numerical formula out of context.)

p3 li29-32: Scheff and Frierson (2015, the follow up to the 2014 paper) checked the monthly assumption more closely. They found that it's not bad at all for changes in PET (probably because the day-night warming difference isn't actually that big in most places) though it can throw off the absolute PET values by double-digit percentages. However, they only tested greenhouse-driven warming, so I don't know if the result would be valid for LCTC/shortwave warming. Presumably the LCTC world warms much more during the day than at night, so this time-resolution issue could actually become important.

In fact, the diurnal cycle of temperature could be another major constraint detectable in the paleoecological record - plant species composition may respond quite differently to warming with much-increased diurnal cycle (LCTC) than warming with unchanged or somewhat reduced diurnal cycle (CO2) because different species have different tolerances to min and max temperature. Again this would be independent of any water-cycle-driven vegetation change. Thus, it would be interesting for you to quantify the difference in diurnal Tmax and/or diurnal Tmin between your two Eocene end-members, not just the difference in Tmean. Either of these may be quite dramatic, even though the Tmean doesn't differ much. All of this may additionally hold for seasonal cycle (winter and summer temperatures rather than annual-mean temperature.) We have a good idea of winter temperatures from some of the proxies like crocodiles and palm trees surviving in the Arctic. Some SST proxies are also particularly sensitive to summer temperature so they could be useful here.

General: The first mention of Figure 1 right now (p4 li12) comes after the first men-

tion of Figure 2 (p4 li5). So these two figures should be reversed and re-numbered accordingly. I.e. you need to re-number the current Figure 2 as Figure 1, and vice versa.

p4 li26-29: Not only is it a substantial increase due to this reasoning, it's also a substantial increase because it's on the same order or larger than what you would get by differencing LCTC with a Holocene run (or differencing high-CO2 with a Holocene run.) In general it might be more useful to contrast the differences of high-CO2 and LCTC with a common Holocene-like (i.e. low-CO2, normal-cloud) control, instead of contrasting the absolute precip values. I.e. by what factor or percentage is the LCTC-Holocene difference larger than the highCO2-Holocene difference. It will be a lot more than 11%. And it's that difference from present that we tend to notice when we think about Earth system change over time, not the absolute value.

p4 li29-34: This needs to cite Fig 2d.

p5 li3-4: No it doesn't “only” prescribe change in cloud droplet size - it also prescribes a big change in CO2 (of course.) The effects of the CO2 difference could also be important for the big cloud cover changes you see. Again, differencing both simulations with a common low-CO2 normal-cloud control would help in disentangling this.

p5 li10: Not to sound like a broken record here, but does a “modern” (Holocene-like) control simulation with this model also have this Pacific meridional tilt? You may just be seeing a general bias of the model, not a particular effect of the Eocene climate.

Exact same issue with “than observed in the modern climate” at li14-15, too. If Eocene model is different from modern observation, it's hard to tell whether that difference is due to “Eocene vs. modern” or whether it's due to “model vs. observation” (without further information or citations.)

p6 li1: Strictly speaking this equation will give a negative value for P, since LH as you just defined it will be a negative number (LH is upward, not downward) while Lv is positive. So you may want a minus sign in this equation to be technically correct.

p6 li8 and Fig 3: It's not immediately clear what bars in Figure 3 to look at to see "Atmospheric heating". It seems for SW and LW it's the black (TOA-surface) bar, but for LH and SH it's actually the orange (surface) bar. So this would be easier to read if you made black bars for LH and SH as well, and explicitly pointed out in the text or the figure caption that the black (TOA-surface) is the (net) atmospheric heating in all cases. In this case you would also flip all of the orange bars to opposite sign, to be consistent with your definition of negative up, positive down (see previous comment.) Then, the coloring would be consistent across all four heat transfer mechanisms.

Alternatively you could keep your opposite-sign convention for the orange bars, but then you should rename the black bars to "atmospheric heating" rather than TOA minus surface, since strictly speaking they are TOA plus surface in the current graphical setup.

Also p6 li8: it's hardly "dominated" by the LH component, the SW component is actually almost as big! Rather you could just say that LH is the largest component (a lot more defensible.) "Dominated" means much larger than the others, not just larger.

p8 li4: I assume this is supposed to be "westerly anomaly" (i.e. east-pointing.)

p9 li17-18: This should have a citation (e.g. to Middleton and Thomas) so the reader can know who is doing the classifying.

p9 li19-20: Low relative humidity is also very important for this, I think (you could check this.)

p10 li27: As alluded in the major comment, "eco-hydrological differences" is too vague because plants, rivers, soils, precip, etc. all respond to CO2-driven climate change in such a different manner from one another. If you mean vegetation, just say vegetation. (If you mean something else, say something else.)

Figures 7a and 2c: It's hard to see the precipitation patterns over most of the planet, because most of the map is in the very light colors which don't distinguish from each

other easily. You may want to have your color scales saturate to dark at lower precipitation values (e.g. 9 instead of 18) to make the patterns easier to see. More than 9 mm/day is very wet by anyone's measure.

Figures 7a and 7c: Color values still seem to be plotted over the ocean, even though you are not trying to plot ocean points. This is a little confusing, and for 7a it worsens the above problem. Make sure the oceans are actually white here (or some other neutral color.)

Typos:

p2 li30: "research" needs to be capitalized in the name of NCAR.

p4 li22: should be "subtropical anticyclones" rather than "subtropical cyclones" (I presume?)

p5 li9: "zonal winds are in the Northern Hemisphere… are concentrated" should be "zonal winds in the Northern Hemisphere… are concentrated"

p8 li14: extra word "may" near the beginning of this line

Interactive comment on Clim. Past Discuss., doi:10.5194/cp-2017-17, 2017.