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Comment on acp-2021-924

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Community comment on "Observation Based Budget and Lifetime of Excess Atmospheric Carbon Dioxide" by Stephen E. Schwartz, Atmos. Chem. Phys. Discuss.,
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▪ **MacCracken Review Comments on**

"Observation Based Budget and Lifetime of Excess Atmospheric Carbon Dioxide"

by Stephen Schwartz

February 21, 2022

General Comments (drawn out of the specific comments below):

- The first thing that would be helpful to have clearly documented is how the baseline redistributions of emitted carbon are split into the various reservoirs over time. So, what is the airborne fraction, what fraction is going into the ocean and what fraction (net of uptake minus deforestation source) is going into the terrestrial troposphere. The Global Carbon Project prepares its analysis each year based on a range of data that they gather. And then, once done for the present, does the model represent the integrals over the historic period, etc. It is nice to base various fluxes on observed information, but the real test is whether, when everything is put together, the fluxes redistributing carbon into the three closely coupled reservoirs (the atmosphere, ocean mixed layer, and living terrestrial biosphere). It would also be helpful to have a comparison with how the MAGICC does this redistribution in that it has been calibrated to existing models—this is not to suggest that the detailed models that attempt to build up the carbon cycle from fundamental processes as much as possible are correct, but to understand what differences do exist and what that might show.
- The results of the paper are, as is acknowledged in the paper, dependent on how the transfers of carbon between the mixed layer and deep ocean. In that this relationship is determined by the circulation of the ocean, it would seem essential that the model for the interaction represents the ocean circulation and its effects. It is well-established through climate simulations that the representation used in the Schwartz model simply does not lead to the observed temperature distribution of the ocean, namely a

relatively warm upper ocean and a cold deep ocean. The observed distribution is a result of accounting for the downwelling of cold water in polar regions and its gradual upwelling over much of the area of the lower- and mid-latitudes. And it is also the case that simply representing the upper ocean as a wind-mixed layer without accounting for transport along isopycnals from near the surface in mid-latitudes down to several hundred meters in the lower latitudes is not likely to be adequate. These ocean influences can all be represented with a quite simple parameterization without having to include a globally finely resolved ocean representation. The proposed ocean model basically ignores the well-established understanding of the ocean circulation and instead includes only a one box deep ocean. Were this the approach used in a climate model, the equilibrium result would be a deep ocean and mixed layer having the same temperature—this is not only not the case in the real world in terms of the actual temperature, but even if trying to model only the perturbed temperature, one would not expect the change in the upper and lower temperature to be the same. And since CO₂ is transported in the ocean by the same ocean circulation as the heat, one would not expect that the CO₂ is being correctly being moved around in the ocean.

Despite this situation, the author goes to great ends to defend his one box approximation for the deep ocean, citing papers not really relating to the type of use he is putting the model, basically saying global models are too uncertain. Rather than what has been done in simplifying down climate models, which calibrate themselves to the complex models, this author basically claims to be sufficiently correct. Given all the time that has been put into this paper, it would have been quite easy for the author to actually investigate if his approximation and a slightly more complex model would give the same result. He could readily put in a one-dimensional ocean model with a polar downwelling pump as is done in the MAGICC climate model (and has been done in many other simple climate models). Another useful test that could be done is to test the dependence of the results on the assumed depth of the ocean. Detailed models and observations both suggest that downward mixing of heat in the ocean goes only as deep as near the depth of the thermocline (so maybe down to 750 meters total or so, including the mixed layer) and cannot go deeper because of the upward ocean movement in low latitudes as a result of the polar downwelling.

With radiation transport not a process in the ocean, it is the circulation that carries the heat around—and so could also be used to carry the CO₂ around. If such a big claim is going to be made about the quite short lifetime of a CO₂ perturbation if emissions are suddenly halted, there is just no excuse for not really doing the extra modeling to prove his choice is justified.

- Unless one reads very carefully, it is hard to find out that really what is happening after emissions stop is a redistribution of the total amount of injected fossil-fuel CO₂ among the various reservoirs, leaving the atmosphere with a percentage of its peak excess concentration persisting essentially indefinitely—the atmospheric perturbation due to fossil fuels would not naturally go back to zero until all the emitted CO₂ is taken up in ocean sediments or permanent land carbon. The IPCC has a simple representation of the decay in the atmosphere reservoir that involves, I think it is, five (or maybe four) separate exponentials, each one accounting for the time it takes for the carbon to mix into each reservoir and the coefficient being the share that would remain in each reservoir out to quite long times. The paper does not present the IPCC approximation nor explain how its results compare for each reservoir, and it would be informative to do this.
- It is strange in Figure 7 that the amount of carbon in the terrestrial biosphere looks to remain constant at the amount that is in these reservoirs at the time of the peak CO₂ concentration, such that, apparently, a reduction in the atmospheric CO₂ concentration does not lead to a reduction in the terrestrial carbon reservoir. This seems very strange given that the equilibration is quite rapid as the CO₂ concentration is increasing (FACE

experiments seem to show that equilibration for shrubs and grasses is within a few years; trees would take longer). This needs to be checked. Normally, it is thought that as the CO₂ concentration drops, CO₂ will be, in essence, exhaled by the biosphere back into the atmosphere. It would also be worth looking at the transfer from the living biosphere to the long-lived biosphere—this is normally thought to be a pretty slow process. Perhaps I missed it, but it would be interesting to have indications of how much carbon end up in each of the model's 5 reservoirs in the decades and then centuries after the ending of emissions.

- It is also not clear if there should be a limit on how much the terrestrial carbon can build up in the terrestrial biosphere given that the areas of growth are finite, the supply of nutrients is finite, trees have genetic limits on how large they can become, and one cannot just squeeze in more and more trees—density is limited.
- It would be interesting to see model results compared for more cases than simply a sudden cutoff of peak emissions to zero. So, what about a gradual reduction in emissions? What about if emissions continue? This is important because emissions are not going to suddenly go to zero, but will decrease slowly and so there will be very little adjustment right at the zero point as the adjustments have been going on all along as the emission levels drop.
- As near as I can tell, the model looks at the perturbation of the carbon cycle due to humans. It would be interesting to know if one simply put the amount of total carbon in the various reservoirs in the preindustrial period if it would distribute to the reservoirs as observed and if this would be a steady state condition. And then, again from baseline amounts, it would be interesting to know what the distribution might be for some larger amount of carbon. Given that the end point of the study described in the paper is the redistribution among reservoirs, is the model set up so the observed distribution would result were the preindustrial total loading of C being the initial condition—right now it appears that the preindustrial distribution is prescribed rather than used as a test of the model representation.
- Note—I have not reviewed the appendices nor examined all the equations in detail, generating my comments based on the text that is presented; and this was all done pretty quickly.

Specific Comments and Thoughts/Suggestions (noted as they come up, and sometimes repeated if points arise again):

Lines 23-24 (and elsewhere): My understanding of the current approach is that, in representing the decay time in one equation as opposed to a model with each process separately represented, there are five or so time-decaying exponentials, so separately into the biosphere, surface ocean, deep ocean, sediments, soils, etc. and so characterizing the time as if it is one number is just not a correct characterization of how the number is currently viewed. That is, there will early on be a short time constant for emissions into the atmosphere to redistribute some into the mixed layer and living biosphere, but that once this occurs, the time constant will be much longer for the redistribution into the deep ocean and long-term terrestrial biosphere.

Lines 28-29: A bit strange to be quoting Ramanathan when it was Revelle, I think it was, who talked about this as a 'great geophysical experiment.' I don't know reference for this, but it might be the 1965 report of the President's Science Advisory Committee (PSAC) in the chapter (or annex) on climate change that Revelle chaired or in his paper with Harmon Craig, I think it was, on ocean uptake of C-14 and/or other species.

Line 34: I don't really like the idea of saying just "anthropogenic CO₂" as if the actual molecules are different than the CO₂ in the air. Actually, the adjustment that is occurring is based on all of the CO₂/carbon that is in each reservoir, and one cannot just do a difference of the anthropogenic CO₂ amounts. I think this is important to be careful of because, on the other hand, the C-14 generated by the nuclear testing is so dominant in amount that it is, as I recall, most of the C-14 in the atmosphere, and there is also radioactive decay going on of the amounts over time.

Lines 40-41: Are there really estimates that suggest the lifetime of the perturbation is only a few years or a few tens of years? Those seem very, very low if one is talking about the lifetime of the full increase in the anthropogenic loading. I do understand that some experts do say that there would be a fast initial drawdown, but this would not take out anywhere near all of the overall anthropogenic loading, as I understand things. So, is this a comparison of apples and oranges? I see paper does cover this somewhat in the following sentences, but it seems to me confusing to be giving a time constant based on an initial slope (based I would imagine on how the airborne fraction allocation is going on) that does not lead to all of the perturbation being removed.

Line 54: CO₂ does not really "decay"—it appears as part of different compounds or in different forms, but does not really disappear the way C-14 does. Strange word choice.

Line 64 (regarding Figure 1): It appears that the approach does not account for the increasing time it takes to mix into the deep ocean and then into the sediments, so that the atmosphere gets to its adjustment time quite quickly. This seems incomplete because the time constants for spreading through the longer-term reservoirs do not seem to show up as does appear in other model estimates. This seems a bit strange.

Lines 83-84: On the relatively rapid response of surface temperature, that is really because of the relative magnitudes of the heat capacity of the various reservoirs. So, the atmospheric heat capacity is equivalent to less than three meters of ocean water, and so with the upper ocean being roughly 100 meters deep, the atmospheric temperature tends to follow the upper ocean temperature. The deep ocean is of order four kilometers deep and the flush through time of the upper ocean is of order 25 years, but for the deep ocean is more like 1500-2000 years. And then there is the fact that the IR emission at the surface of the ocean will shorten the temperature adjustment time as well, so not surprising. It is not clear to me that this can serve as an analog for the carbon cycle adjustment time in that the relative amounts of substances in the various reservoirs is more even and there is no real loss term the way there is an IR loss term for the energy.

Line 103: As I think I have said before on this approach, I think the two-layer approach to representing the upper and deep ocean has been shown to be fatally flawed in representing the ocean—basically, over time, the two boxes would tend to have, for example, the same temperature as opposed to a warm ocean on top and a cold ocean below. That is why the way the simple models are set up is with a one column ocean with multiple layers and slowly rising waters, and then a pipe from the upper ocean directly to the deep ocean to represent polar downwelling of cold dense waters (see the explanation in the documentation of the MAGICC model). And for carbon in the main column with its multiple layers, one would have to represent carbon going down a bit by biospheric action and then dissolving on the way down and being carried back up. I also wonder how you are (or are not) representing the compensation depth (so the depth at which CO₂ tends to dissolve from sediments, and that is being affected by ocean acidification). So, just to note, I am already now very suspicious of the approach. Given that such an additional representation would not add substantial time or complexity to your model, I'm surprised that you have not done this.

Lines 115-117: Just to note that virtually all of the observations to be used for

constraining the model are for a situation where the CO₂ is increasing, and assuming reversibility seems to be a rather significant assumption (e.g., waters are thermally stratified—and advection is not really reversible, etc.). So, if you had an upwelling-diffusion model (so downward polar pipe and upwelling column), there would not be the same reversibility that the model now seems to have. And, I might ask, is the living biosphere flux reversible—will a lower CO₂ level lead to a lower amount of biomass, as one would expect?

Lines 163-165: I agree there is much confusion, especially about the difference between the lifetime of a particular molecule of a substance and the lifetime of the perturbation to a concentration in situations where molecules of the substance are going in both directions across an interface. There is also difficulty when the substance is spread among multiple reservoirs that each have particular and quite different exchange times (hence the IPCC's five or so decaying exponentials).

Lines 172-178: Well, that is a start at the problem—there are then multiple components and varying types of transfer processes, not just diffusion, etc.

Lines 226-227: But is the labile exchange time the same in both directions? It is quite fast if one has new plants growing, but once created, decay can take time and I'd think there can be rather long lag terms if one grows actual trees—which might keep growing even though the CO₂ is down a bit. I'm just not sure that I agree there is an equivalent exchange in both directions—yes, for leaves, etc., but I'd not think that the case for new wood that is created, which could have a hundred-year return time.

Line 272: I'm just not up to trying to work through all the equations, so will be responding to the text. I'd just note that the labile cycle of uptake as leaves and wood would be different, as would the times of decay—I'm just not at all convinced such a simple model will be sufficient (so a caveat I am holding, waiting to hear about the tests being run). What is normally done, as for MAGICC, is to calibrate the simple model versus complex models. While I understand you want to calibrate directly, I think there is really the need to explain why the difference with the model is occurring and whether the assumptions made in going to the simple model capture this.

Lines 280-285 or so: The problem with using a half-life or 1/e times is that even small amounts of the perturbation affect the climate—there really is not a tolerance level.

Line 291: Given how much discussion you have here I'm surprised that you don't present the IPCC decay function, so the sum of five (or so) exponentials for comparison—does your model agree with the decay times the IPCC has for each of the terms and for the fraction going into that reservoir?

Line 317/Line 3010: Just a note on Figure 2 that the number 120 for F_{a1} has the 1 sort of lost due to some sort of cropping so looks like 20.

Line 360: Ah, interesting, and good to hear of the separation.

Lines 423-424: Treating everything below the mixed layer as the deep ocean seems to me to be a serious oversimplification, given how the isopycnals really control downward transport in middle and low latitudes, allowing a good bit of horizontal mixing from surface ocean to depths down to several hundred meters, but not down below 750 meters or so due to the upward motion that is balancing polar downwelling.

Line 446: I agree this is a curious anomaly. I started drafting a note for AGU/EOS many years ago but never got to completion. One of the interesting thoughts I heard about it had to do with possibly the spreading of weeds, etc. over croplands as a result of so many

men from farms being pulled into the military (as I recall, the flattening started early in the war years), and then perhaps a change after World War II as weeds were cleared and then in the amounts of cropping of C3 versus C4 plants and relative carbon uptake. This all got to be more than I felt that I could get into and so I sort of abandoned the paper—but it seems to me finding an understanding of this pause (which did not seem to be in emissions) might be very insightful. Perhaps with your model, that could be something to look into. [And I should note that I think there is a paper or more looking at this in detail, perhaps by an Australian author or two.]

Lines 467-468: I really don't like this notion of a piston velocity to represent the links between the mixed layer and deep ocean. I'd really suggest trying a different representation of the ocean and seeing if your conclusion holds up.

Line 476: On the flushing time of the deep ocean, 650 years seems lower than what Broecker, etc. have talked about, which I think is more like 1000- 1500 years or so. And I'm not sure that flushing time is the right way to think about it instead of as more like a pipe that it takes 1000-1500 years or so to pass through, so the increased uptake of CO₂ does not become available to the atmosphere until after that much time [though isopycnal mixing does lead to some higher amount of CO₂ down several hundred meters (or even more) in the lower latitudes]. Again, I think that the ocean component of the model needs to be upgraded. And I'd also note that I don't think ocean processes are simply reversible—time constants on that will apply.

Line 486: So, if you change the ocean to a deep ocean with a pipe going down from the surface in high latitudes and then the water spreading out, this provides the basis for slow upwelling to occur as new amounts of downwelling water push underneath and lift it up. So, the downwelling flow is a result of dense, cold water sinking and the CO₂ amount is based on how much CO₂ can be held at equilibrium by the cold water with the increased atmospheric concentration. This downward transport of CO₂ would then increase only slowly if the overturning circulation stays the same, even with the atmospheric concentration going up. And the water coming up into bottom of the thermocline layer (at 750 meters) would be staying at about the same loading as preindustrial due to the long circulation time. This would all be very different than how things work with a piston type approach to representing the downward flow. An additional advantage of actually representing the over-turning circulation would be that you could then do experiments changing the amount of the ocean overturning (with some suggestions that this overturning amount would be caused by Atlantic surface warming and reductions in the amount of rejected dense brine water as sea ice freezes—and some indications this change is already occurring). Were the overturning circulation to stop, the net uptake of CO₂ from the atmosphere and downward transport would necessarily go down a lot (also less upwelling of nutrients into the mixed layer to feed the biospheric pump of C to the deep and intermediate ocean). With your piston approach, there would not be such a reduction in the flux—it would just keep going (wouldn't it?). I guess what I would recommend is to apply your model framework to the energy in the system and see if the resulting vertical temperature distribution would result—basically, the transport of CO₂ and heat occurs in the ocean in the same way (there is no radiation term to have energy jump from one layer to a much different layer), so whatever structure you have should work for both—and getting the deep ocean to be cold won't occur with a piston velocity.

Lines 520-523: Here I totally disagree (or misunderstand—perhaps you are just referring to the amount of wind-driven exchange that is going on—and not the net transfer). The transfer from the atmosphere to the mixed layer depends on the gradient in the CO₂ concentrations and I don't understand how you can say this is constant given the annual increase in the CO₂ concentration has changed over time due to changes in emissions (is not what you are doing assuming instant equilibration of ocean and atmosphere instead of allowing a gradient to form and drive the flux into the ocean?). Also, with the colder

(saturated) water rising up in lower latitudes, it emits CO₂ as it warms and then in high latitudes, as the ocean waters cool, they take up CO₂. So, I just don't understand how you can say the gradient will be constant over time. And as the overturning circulation changes, there will be a change in the flux—so how can you make an assumption about it being constant? On the mixed layer to deep ocean fluxes, etc., there are the downward flux flows (in the downwelling waters and the biological pump) and then the upward flux flow in the slowly rising waters that went down long ago and are now rising with the CO₂ burden of the past. I think each of these fluxes needs to be kept track of separately. Also, given that the biological activity is dependent on the amount of nutrients that are carried upward—if the overturning circulation changes, then the flux of nutrients change. I get the sense that your model would work reversibly whereas with a more complete representation, this would just not be the case—things are not just instantly reversible.

Lines 586-587: Nice to use a transfer coefficient for heat, but just to note that your ocean would not lead to a cold deep ocean, and so it would really be inappropriate to be using heat influx for transfer into the deep ocean—you really need to do a better representation of the ocean.

Line 655: On the issue of CO₂ fertilization, there are also limits imposed by the supply of nutrients and of water, and so there would seem to be a need to be very careful about this, especially as the area of the relatively dry subtropics is growing.

Lines 688-691: So, have you run your model from preindustrial concentrations into the future assuming no emissions, such that the model holds the CO₂ concentration constant? I would think that this would be something to explain at the start of this subsection as proof that the model is stable in the absence of emissions. Again, I'm not really clear on how you are calculating the fluxes into the ocean and biosphere. So, are these fluxes being driven by the gradient created each year by the emissions—if so, then if emissions go to zero, there would no longer be a gradient, and so how would the flux continue to be the same? Or is this turnover time you calculate based on the current flux rate driven by the annual emissions. I just don't see how this number of 44 years accounts for the fact that as emissions go to zero, the gradient driving the flux would go down and so then would the flux? So, shouldn't the net flux from the atmosphere to the mixed layer go down exponentially over time, and do so quite rapidly as you are assuming a quick adjustment time of the atmosphere and mixed layer? Now, it might be that in your model the mixed layer to deep ocean flux would stay the same and so this would keep pulling down the mixed layer concentration and so then sustain the atmosphere to mixed layer flux. If so, then, again, making sure that you have the ocean exchanges properly represented is critical, and as I've said, I don't like the ocean circulation that you have. Basically, what will happen over a thousand years or so is the amount of C would re-equilibrate so that the total fossil fuel burden is spread through the upper and deep ocean (and as that process pulls down the CO₂, it will pull CO₂ back out of the labile biosphere reservoir). So, on your rate, is that an initial rate? I don't see how that can persist as the atmospheric concentration is pulled down as the time constant for mixed layer and deep ocean is so long.

In this regard, it would also be interesting to know what sort of steady equilibrium occurs with different total amounts of C in the combined set of reservoirs. So, for the amount in the preindustrial world, the equilibrium is 278 ppm (say), so would your model come to that equilibrium at that level with the total amounts of C in the non-sediment reservoirs? What about with 50% more? Or are your equations all based on the departure of the system from the 278 ppm base level, so you will inevitably come out at that if the distribution among reservoirs is not started as it was observed to be (so, what would happen if you put all the C in the deep ocean at t=0 and then ran to equilibrium; what about if started with all C in the atmosphere—would the preindustrial CO₂ level result?

Lines 700-701: Okay, so one exponential is the time for the atmosphere and mixed layer to equilibrate. This does not get rid of the overall perturbation.

Line 708: Missing word—should be “it is useful”

Line 726: Typo—should be “the situation”

Line 736: Just to note that the water coming up can be supersaturated in CO₂ as a result of the dissolution that occurs as the biological pump is working so that CO₂ keeps getting recycled up. Presumably, as the CO₂ concentration goes down, this will be affected as well. Again, I think there is a real problem in an overly simplified ocean model.

Lines 750-751: Does this large flux back and forth allow for the seasonal build-up and release of C by the TB as is indicated by the annual cycle of the CO₂ concentration as seen in the Mauna Loa record? Might that variation suggest you need to subdivide the LB box?

Line 756: This just can't be the case for trees growing? The FACE experiments might suggest there is a few-year equilibrium for weeds and grasses, but I don't see how this could be the case for wood as it would take much longer times for a full forest to come to equilibrium. So, again, perhaps the LB needs to be subdivided.

Line 806-808: So, you have no removal to the sediments, and so there is no ultimate sink of the C, all there will be is a redistribution among reservoirs—is that correct? If so, you will never get back to preindustrial if you have added C emissions. So, then, you might have a decay rate, but the level will never get back to preindustrial? Is that correct?

Is there any limit on how much the obdurate reservoir can build up? Basically, there is only so much land for buildup to occur unless you have a sink to peat and eventually back to fossil fuels. So, how much can really build up (are there limits due to nutrients, etc.? Should you have a return term based on wildfires? etc.?)?

And, using the piston approach, the vertical distribution of the excess CO₂ will be wrong (well, in fact, the single box does not have a vertical distribution—but, in essence, any amount taken up will instantly be creating a back flux, which is just not how the ocean works.

Line 816: So, in setting a $S_{a,eq}$, does this mean this is a perturbation model and is not based on gross amounts in a reservoir, such that you will return to the preindustrial value even if you through emissions add a large amount of C to the system that would, one would think, end up as distributed among the various reservoirs? I also don't understand why the difference would be with respect to the equilibrium value and the difference term in the fourth term in the equation—again, why is the difference done with respect to the concentration in the previous time step (so the emissions increase the atmospheric concentration and then this increases the mixed layer loading, etc.—what is the equilibrium value and why is it used?).

Line 817: Why is not the flux simply based on the gradient between the atmospheric and the mixed layer values—what are these equilibrium values?

Line 823: Something is missing—“in” what?

Line 903/line 3066: In Figure 7a, I do not understand how the amounts in the TB stay identical once the emissions stop. Over time, C will redistribute to the deep ocean, and this will lower the atmospheric concentration, and this will pull the labile C down pretty quickly and eventually the obdurate C—how is it that these stay identical over time?

Line 1135 or so: Have you also run comparisons of the models into the future with emissions continuing to see how you match or don't? This would seem interesting to see as well.

Lines 1186-1188: So, this is not really clear from the earlier discussion, namely that the atmospheric level, for example does not decay to zero, but to 16-22% of its peak value. That would have ongoing climatic effects and so the temperature would not return to preindustrial. Basically, there is a distribution of the fossil fuel carbon among the reservoirs, which is just how IPCC represents things in its five-exponential (I think it is) equation. I'd like to see how your model results for each reservoir compare to the term in the IPCC equation for that box. I'd guess the ultimate distribution might be similar—well, except I just don't like how your ocean is represented.

Just a note here that I would like to see how your model performs versus carbon cycle models assuming that global emissions go down over several decades to zero (so say, to net-zero by 2050). Are the results similar or different?

Lines 1347-1349***: Given this result, it would seem that the interactions and links between the mixed layer and deep ocean need to be done in a much more representative way than is done in your 5-box model. We know, for example, that such a simple model would not explain the cold deep ocean and warm surface—and that this feature is a result of how the ocean circulation is represented (there is not radiation transport, which along with convection is needed to explain the atmospheric structure). In that CO₂ is carried along by the circulation (save for the biospheric pump), I just do not think it appropriate to use a formulation that just does not appropriately represent the ocean circulation. There is just too much of a chance that the result you get, being different than the more complete models, is a result of the inadequate representation of the ocean circulation to accept your results as a serious challenge to the models.

Lines 1457-1459 and following: Might this difference be because your model leaves the amounts in the terrestrial biosphere constant in time, which just seems wrong. Might there be a term missing to reduce those over time as the atmospheric concentration declines? Going back to Figure 7, your model seems to have the TB indefinitely holding a very large fractional increase in amount as a result of the fossil fuel emissions—and the fraction seems so large it is just not clear there is enough land for this—or perhaps all trees have to grow as tall as redwoods or something. A persistent increase of the amount shown just does not seem plausible to me. Maybe what your biosphere model needs is to have the age of trees limited to some number and then those trees decay and new trees grow up with a lower CO₂ concentration; one just cannot keep having such high values (so is your algorithm based on the net mass of the trees or the fact that there is an ongoing exchange going on all the time, with average tree life perhaps, say, 40 years or so as many trees that try to grow die off as others succeed, etc.)?

Line 1590: EXACTLY.

Line 1594: But note that MAGICC has a much better representation of the ocean!!

Line 1601: But you are going much further out toward equilibrium, so not sure a C-14 result is really applicable. With gradients, most will surely be in the ocean, so what is so hard to figure out about that.

Line 1614-1615: I don't recall the models in the papers cited to know how they dealt with the oceans. Very clearly, one cannot get the baseline ocean temperature distribution with the way you have done a two-box model, and it would be simply incorrect to think that the end equilibrium temperature distribution doing it with just the perturbed amount of heat to think that the temperature anomalies in the mixed layer and deep ocean would be

the same. I just don't think you can justify this model for the time period you are talking about. It seems to me you just can't claim this—you really need to justify this by putting in a better representation to get results and then show that the simpler model is adequate—just saying so and citing a few papers with models being used for a different substance is just not compelling or convincing to me. It would not be hard to put in an ocean representation that is better (could likely be done in a time a lot shorter than reading your quite long paper)—so I think you should just do it. At the very least, make your deep ocean box only 650 or so meters depth so it only includes down to the thermocline.

Lines 1618-1622: It is one thing to start with a complex model and work to simpler model, checking as you go that the results match. It is quite different to start with a simple model and just sort of claim it works.

Lines 1627-1628: You have chosen a model form that is known not to yield a good representation of the temperature distribution or of the perturbation to temperature that the models get. Even though there are uncertainties, your ocean model is just known to not represent the effects of the ocean circulation, which is what is essential to be representing.

Lines 1657-1665: I'm sorry, but given the ocean representation you've chosen which does not adequately represent ocean circulation, this is just not convincing to me (and there seems to be some problems with the terrestrial biosphere representation as well).

Line 1706: At this point, I'm going to pass on going over the appendices—this paper is really quite long, especially Section 8, and quite a challenge to get through.