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Interactive comment on “The interactive global fire module pyrE” *by* Keren Mezuman et al.

Anonymous Referee #1

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General comments In this manuscript, the authors describe pyrE, a new fire module for the ModelE Earth System Model. pyrE builds upon work by previous authors but includes some novel elements that could be of interest to the broader community of global fire modelers. The authors present not only the direct outputs of pyrE (which performs acceptably, especially considering the huge variation in performance seen in global fire models), but also evaluate its impact on ModelE's representation of atmospheric chemistry, specifically with regard to aerosol optical depth. There is nothing especially groundbreaking presented here, but the manuscript represents a well-written and (mostly) thorough documentation of an important part of an Earth system model—something exactly appropriate for publication in this journal. However, the authors need to be much clearer about the choices they had to make because of limitations of their vegetation model, and better place these choices into the context of previously published fire models.

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My main criticism has to do with the authors' contextualization of their decision to tie emissions to fire count. This begins in the Abstract:

"Fire emissions are generated from the actual flaming phase in pyrE (fire count), not the scar left behind (burned area), as is commonly done in other interactive fire modules."

Continues at the end of the Introduction:

"pyrE uses fire count to derive emissions, and is therefore more directly connected to the actual fires, in contrast to other fire models that use BA, a measure more indicative of fire's effect on the landscape."

And shows up again at the beginning of Section 5.3:

"Due to the intricate processes involved in burned area spread, most fire models struggle to reproduce the observed trend [Andela et al., 2017] and seasonality [Hantson et al., 2017a] of burned area. A more direct approach would be to use fire count, similar to the approach of Pechony and Shindell (2009, 2010) and Pechony et al. (2013)."

In the real world, fire emissions are a product of (a) how much area burns, and (b) how much fuel is combusted per square meter of burned area. Burned area is not just some side product that vegetation models need to look at to count how many trees die; it's the only way to know exactly how much fuel could possibly be combusted in a fire, and fuel combustion is what creates emissions. Thus, it is flatly incorrect to assert that bypassing burned area makes pyrE "more directly connected to the actual fires." The burned area IS the "actual fire."

It is unclear from this manuscript whether Ent, the part of ModelE that represents vegetation, actually simulates biomass in any meaningful way. Presumably it does not, which makes the use of fire count-based emissions factors acceptable—modelers must sometimes do what is possible given existing structures. (If it DOES simulate biomass, I would say that pyrE would need to be completely reworked and this manuscript rejected.) However, THAT is how this decision should be framed—as the best that can be done given an extremely basic representation of the terrestrial biosphere. The authors should not attempt to position their method as something superior to what is done in nearly every other global fire model, which are integrated with seemingly superior vegetation models. (It's possible I'm reading too much into what the authors have written, and that they're not actually trying to position it that way, but my overall point remains: The authors need to explain that what they are doing is a kludge to work around a deficit in their vegetation model.)

This is especially galling considering the authors' complete omission of SPITFIRE-based fire models (three presented in Rabin et al., 2017 and supplement, which describes the models participating in the Fire Model Intercomparison Project [FireMIP]) from the Discussion. These and other models not only calculate process-based fire counts and burned area, but also process-based fireline intensity and fuel consumption. THOSE models are "directly connected to the actual fires." But no, the only text hinting at those models' capabilities is the second-to-last sentence of the Conclusions: "Almost no fire models include fire energy." Three of the eleven fire models presented in the FireMIP protocol paper is hardly "almost no models"!

It seems that the authors have chosen fire counts rather than burned area because, as they point out in the Results, pyrE performs better for fire count than for burned area. But did the authors ever actually test whether parameterizing emissions factors based on fire counts actually gives better results than parameterizing based on burned area? This should be tested and presented, at least in a Supplement. In general, the authors need to present much more information about their parameterization methods and results. All we see about the parameterization of emissions factors is the input data and a reference to multivariate curve fitting—much more information is needed in the interest of reproducibility.

Dancing around this deficiency in their vegetation model, rather than addressing it head-on, seems to pop up in other parts of the manuscript. For example, the authors do not really present any evidence for the idea that anthropogenic fire suppression in the Middle East is as strong as it is in the United States. This does not really make sense, given that, as the authors point out, there is so little biomass across much of the Middle East. Instead, the issue is more likely that pyrE does not consider fuel availability at all in calculating fire counts. Again, a kludge to deal with this is something that is justifiable, and the authors need to address it directly. This also touches on the need for more transparency about parameterization. The authors need to present literature evidence for the elimination of suppression in Africa, as well as for the new parameterization for the US. The first paragraph of Section 2.3 relies heavily on broad statements that are not backed up by any citations.

This is unrelated but a potentially large issue: The authors are not interested in cropland fires, which makes sense given their model system's limitations, but why then do they not filter out MODIS hotspots and GFED emissions based on which were detected on cropland? One of the MODIS products is a land cover map (three, actually) that has been used in previous work to filter out hotspots detected on cropland, either for the purpose of discarding them or analyzing them on their own.

It's also worth pointing out that the authors commit an all-too-common mistake in conflating MODIS hotspot detections with "fire counts." One large fire might have a fireline long enough for multiple hot pixels to be detected; a slow-moving fire might result in pixels that are counted multiple times as hotspots despite being part of one fire. The authors' use of the hotspot data itself is not necessarily flawed—it's fine for the "fire counts" parameterization to target hotspots since it's all ideally going to get worked out in the emissions factors—but the authors should revise the manuscript to clarify exactly what it is the remote sensing data show. A true "fire counts" product is something more like the Global Fire Atlas (https://www.earth-syst-sci-data.net/11/529/2019/).

Minor comments

- Some parts of the manuscript are too detailed and/or technical. For example, the authors spend over a page in the Introduction discussing the ways that people use fire to manage land, and exactly when those land management fires occur. For a process that's not even represented in the model, that seems like a pretty big waste of space. I would prefer to see relevant information along these lines presented instead in the Results and/or Discussion, to provide context for poor performance in some regions. (The authors do a bit of this, but more would be an improvement.) It is also unnecessary to get into the technical details of remote sensing, such as the sensor channels used to detect fire counts or the reflectance characteristics used in calculating burned area.

- It seems that burned area is only used as an input to flammability in subsequent time steps. This should probably be made clearer, given that burned area is a primary product of most existing fire models. More importantly, though: How long does it take for that effect to fade? Is it just how much of the grid cell has burned EVER? Surely not, but the authors don't specify how this works.

- Why do the white (zero) areas on the right (model-simulated) side of Fig. 8 not match up exactly?

- Figures throughout could use more labeling. It makes it hard to interpret figures when the reader needs to keep going back to the caption to figure out what's in the right vs. left column, top vs. bottom row, etc.

- I understand what the authors are getting at here given the context (at the end of the presentation of results of the simulation at times of day equivalent to the MODIS overpasses) but this sentence makes little sense and should be reworked. It sounds like "Even though A=B, A>B.": "The implications of these findings are that even though the simulated monthly mean fire count is in the range of Terra and Aqua (Fig. 4, A1), the simulated fire count is in fact higher than MODIS retrievals." [Lines 541–543]

- These related sentences were extremely confusing until eventually I remembered about how previously-burned area affects flammability; this should be clarified: - "Nevertheless, even with this large correction factor, burned area has a very minor impact on fire count and fire emissions as it accounts for a small fraction of the grid cell that is

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able to burn." (lines 382–384) - "burned area itself has a minor impact on fires due to its small percentage in a grid cell" (lines 577–578)

- I first noticed this at Line 588 and following, but it may have occurred earlier: The authors seem to sometimes incorrectly refer to the NHAF region as "sub-Saharan Africa." Sub-Saharan Africa in fact refers to ALL of Africa south of the Sahara, not just the northern-hemisphere portion.

Technical corrections

- "Bias-high" and "bias-low" throughout should be "biased high" and "biased low".

- Line 618: Should be cold- and drought-deciduous

- Line 663: Extra period

- Line 670: Missing word

- Line 677: Incorrect capitalization of pyrE

- Line 684: Extra comma

- Line 699: Missing comma at end of line

- Line 700: Should be Middle EAST, presumably

- Fig. A1 BONA: Truncated "1" at top of Y axis

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