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

Anonymous Referee #3

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This paper describes the updates of pyrE, the fire component of GISS ModelE. The discussion of the paper is written very well, demonstrating the comprehensive grasp of fire modeling by the authors. If this were the first paper describing pyrE, I would firmly support its publication. However, 10 years after the papers by Pechony and Shindell, the current updates of pyrE do not appear to have sufficient improvements to justify its publication. My guess is that the updates of pyrE proved to be more difficult than the researchers anticipated. As an important component of the venerable ModelE, pryE updates obviously need to be documented. I wonder if it would be better to include pryE updates as part of another ModelE paper rather than a standalone paper.

In model description, sections 2.1, 2.2, 2.4, and probably some of 2.5 are essentially the same as Pechony and Shindell (2009). For a journal paper, these sections should be summarized in 1-2 paragraphs and the authors can refer readers to Pechony and Shindell (2009).

Section 2.5 is new. However, burned area (BA) modeling is odd in this paper. The purpose of developing it is unclear. The introduction section is 7 pages long, but it did not include discussion on why modelE needs to simulate BA when fire emissions are calculated using fire counts. This is not a review paper. The introduction section is too verbose and should be shortened substantially. The BA modeling is based heavily on Li et al. (2012) and the biases compared to GFED4s are quite large (Figure 8). It seems to be worse than the other global fire models cited in the paper. If BA modeling does not serve a useful purpose for modelE, it should be removed.

I am alarmed by section 2.7. Line 368 states lightning ignition is scaled down by a factor of 10, but line 380 states that fire counts are scaled up by a factor of 30. These statements do not inspire confidence in modelE simulations. pyrE is supposed to be a physical model. Does this large a scaling factor imply that the model is not really physical? How is 30 determined? With such a large arbitrary scaling factor as a tuning knob, why is the scaling factor not tuned such that the simulated global fire counts agree with satellite observations (rather than a 32-42% low bias)? One can go a step further to tune this factor differently for each region. For modelE, it seems that a statistical fire model would work better than the current setup.

Line 381 states that BA is scaled up by a factor of 250. After discounting the factor of 30 in fire count scaling, BA is still scaled up a factor 8. Li et al. (2012) did not have to do this large scaling in their model. What went wrong here?

Sections 2.3 and 5.1.1 are new improvements to pyrE. Discuss how Eq. (6) was derived. Is it through some kind of linear regression?

The evaluation in section 2.6 is inadequate. Fire emissions are what matter to modelE simulations. Regional emission biases of pyrE OA and BC relative to GFED4s should be discussed. The column load comparison in Table 2 is not informative and should be removed. If fire emission is underestimated but the column load is not, it only shows that fire emissions are unimportant (and pyrE is not needed). Fire emissions may not

significantly change global mean column loads but they strongly affect the atmosphere, which the authors described in the introduction.

In the abstract, line 40-42 states “Using pyrE, we examine fire behavior, regional fire suppression, burned area, fire emissions, and how it all affects atmospheric composition.” In the current setup, fire behavior and burned area do not affect atmospheric composition in modelE.

In the abstract, the last sentence, “Yet, in terms of AOD, a simulation with interactive fire emissions performs just as well as simulation with prescribed fire emissions”, is misleading. It sounds as if global fire emission biases as large as 40% do not matter for modelE simulations, which I think is not what the authors meant. The pyrE and GFED4s simulations of AOD are very different in regions where fire emissions are present and large.

Figure 12 shows that fire AOD effect is only about 10% (line 676-677). The AOD evaluation in this paper should be about fire emissions not modelE AOD simulation. Figures 9 should compare fire-only AOD between pyrE and GFED4s simulations. The differences are large. Figure 11 is not useful because the non-fire AOD model biases are much more apparent than fire AOD.

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