

Minor comments:

(1) I could not find a map of the grid cells designated as fire prone. This should be provided to give the reader a better feel for the realism of the spatial distribution. It would also be good to have the distribution of burned area within 27 degrees of the equator against the remaining areas compared to the GFED4 data, in order to better judge the sensitivity study presented in Fig. 9.

(2) p 15187, l5: there should be separate citations for the emissions and for the burned area. Burned area studies cited should be from observations rather from models, and emissions at least from studies based on observed burned area. Some of the papers cited are fully prognostic models, and their estimates of burned area differ far too much from (still uncertain of course!) observations to be citable here.

(3) Further down in the same paragraph, Pechony and Shindell only simulate number of fires, while fire frequency is often defined as fractional burned area.

(4) Next paragraph mentions "climate-fire feedbacks". The studies cited before do not address feedbacks, and it is not clear which feedbacks you mean. Apart from that, see my major comment (1) and suggestions to re-structure the introduction. The term "climate-fire feedbacks" could actually be dropped altogether as it is not directly addressed here.

(5) p15188, 1st paragraph: as explained above, a set-up with an offline terrestrial model can very well account for fire-induced CO<sub>2</sub> fertilization if the effect is for example treated as a perturbation around a mean state. At best you could state that it would be more difficult and lack the same level of consistency, even though there are always other trade-offs like parameterisability and validity of the model.

(6) Same page, last sentence: I suggest that the introduction start with this sentence, include a more detailed description of the effects, then goes on with the histrocal run-down and continues to criticize previously used approaches.

(7) p15189, l12-16: here again the use of the word "emissions" is misleading, as what you mean is the process of emissions (i.e. fire in a combustion chamber vs. wildland fire), not the emission itself (the effect of emission could be e.g. the injection height).

(8) p15194, l8: "found into the ocean", typo?

(9) same page, "These two features illustrate a fundamental distinction between fossil fuel and fire: fossil fuel emissions represent a near-permanent addition of CO<sub>2</sub> to the active (i.e., non-geological) carbon cycling pools, whereas fire pulses temporarily reshuffle the carbon already existing in these pools." This statement, by being rather obvious not only for specialists, rather belongs in the introduction, if it is at all needed. Here, it sounds overly pedagogical.

(10) p15195, l21: but note that the recovery time is longer than in the studies cited.

(11) next page, l1-7: please use more objective and neutral language than "much more similar", "yet at a closer look", and "not actually equal". This sounds like a personal account of a researcher. Please leave room for a different impression created in the reader of the manuscript.

(12) same page, l8: "Based on CO<sub>2</sub> alone" is misleading, because it sounds like as if it implies no albedo effect.

(13) Same page, l19-20: Note again that fossil-fuel (burning!) emissions also come from fire, so the statement does not make sense as it is. It is also not the emission that makes the difference, but the fact that different things are combusted.

(14) Next page, l2-5: I am wondering who would be interested in cumulative gross emissions, or fluxes in general? I would suggest dropping these arguments, cumulative gross fluxes are more or less an oxymoron. It also contributes to the impression of over-selling the results.

(15) p15198, l25: again, it is not the CO<sub>2</sub> emitted that makes the difference.

(16) next page, l7: "wildland fire", not fire. The sentence is rather trivial, because a vegetation burning fire of course has a much more direct impact on land carbon than the indirect effect of CO<sub>2</sub>. We are here talking about effects at vastly different scales.

(17) p15200: "These fundamental differences imply that fire impacts cannot be accurately estimated from simulations of fossil fuel emissions in climate models." This statement is too general and one would ask who would have the idea to do this. Rather, the manuscript should specifically criticise concrete examples of previous publication and then state that such and such approximation has been found to lead to unacceptable results.

(18) p15201: please don't use purely prognostic simulations as a source for global emissions (see above comment).

(19) p15204, l10-11: I am not sure why I should expect anything but gross emissions to continue? Please explain what is new and unexpected here, or drop the statement.