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Comment on acp-2022-175

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Referee comment on "Fire–climate interactions through the aerosol radiative effect in a global chemistry–climate–vegetation model" by Chenguang Tian et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-175-RC1, 2022

The authors present an interesting and very valuable study of the radiative forcing from wildfire emissions, using a version of the GISS-E2 model coupled with a vegetation model and the Pechony and Shindell (2009) fire parameterisation. The authors rightly point out that there have been relatively few studies to investigate the radiative impact of fire emissions in climate models, and there is considerable model diversity among the few studies that have looked at this. This study is therefore significant by adding another very useful data point to help constrain an otherwise poorly-known quantity. The authors' Table 2 (comparing their values with the handful of previous studies, which used different models), is particularly useful in this regard as a reference to anyone wanting to know the state-of-the-art of our current estimates of fire radiative forcing. Another selling point is that uniquely (to my current knowledge) the authors used a fire parameterisation to interactively diagnose fire emissions in their climate model, rather than prescribing them.

The paper is well-written and presented, with good quality figures. I have some specific concerns around how the study is described – which I feel is currently a little misleading – which I have outlined below. However, subject to addressing this by slightly re-framing some of the description of the study, I think this is a useful contribution which will be very appropriate for publication in Atmospheric Chemistry and Physics.

Main comments:

- The main issue with the current description of the study, is that it is framed throughout as diagnosing two-way interactions between climate and fire emissions (for example, but not limited to: the title ("Fire-climate interactions…"), the abstract (e.g. L17-L19), the introduction (e.g. L82, and especially L91-L93 "The main objectives are… to quantify the feedback of fire-induced climate effects to fire emissions and air pollutants", and the conclusion (e.g. L327-328)). However, this is *not* what the study actually does, because the authors use atmosphere-only simulations where sea-surface temperatures are prescribed, and therefore the modelled climate is not free to respond to the radiative forcing from fire emissions. The authors acknowledge this in L195-L197 ("Given that the model is driven by prescribed SST and SIC, only the rapid adjustments of atmospheric variables are taken into account"), but this contrasts sharply with the impression given repeatedly throughout the rest of the manuscript that the emissions -> climate response -> emissions feedback is being investigated. Rapid adjustments are not at all comparable to the full climate response (indeed, this is why fixed-SST simulations are used to diagnose effective radiative forcing!). This is especially apparent in the very small climate responses that the authors diagnose – for instance, a global mean surface temperature response of -0.06 K, which is an order of magnitude smaller than the full surface temperature response you would expect from a -0.6 Wm^{-2} radiative forcing. In a fully-coupled atmosphere-ocean simulation, the climate responses would therefore be vastly different, both in magnitude and probably spatial pattern. What the authors have primarily done, is presented a (very useful!!) analysis of aerosol effective radiative forcing due to interactive wildfires in the GISS-E2 model. Whilst the analysis of the fire responses to rapid adjustments is interesting, it should not be claimed as diagnosing the fire-climate feedback on fire emissions. I recommend the authors therefore reframe the sections of the paper mentioned above, to emphasise that the study primarily investigates radiative forcing from fires, and make it much clearer from the outset that the only feedbacks included are those due to rapid adjustments.

- Section 2.4: "Within each group, two runs are performed with (YF) or without (NF) fire emissions. For YF simulations, fire-induced aerosols are dynamically calculated based on fire emissions and atmospheric transport". It's a little unclear whether only the aerosol emissions were changed between the YF and NF runs. The results only ever refer to aerosol effects, however the description of the fire parameterisation in section 2.3 describes trace gas emissions from fire (NO_x, CO, CH_4) being simulated as well. Were these trace gas fire emissions also disabled in the NF simulations? In which case, the comparison of YF-NF does not solely include radiative perturbations from aerosols. However, if fire trace gas emissions were kept on and it was only the aerosol emissions that change, then it should be made clearer that these other fire emissions were held constant throughout.

- L319-L326: The consideration of whether the different aerosol radiative effects and rapid adjustments are additive or not is potentially very interesting. However, it is really hard to know how significant this effect really is, since no indication of uncertainty ranges are given for the global mean values. E.g. the authors note that the temperature rapid adjustments from the individual processes sum to -0.037K, whereas the total temperature rapid adjustment was -0.061. These are both very small numbers and the difference between them could easily be due to internal variability rather than because of a non-linear response. Similarly (probably even more so) for the precipitation changes. Could the authors estimate a measure of uncertainty (e.g. from the internal variability across the 20 individual years of their simulations?) to help establish whether these values are robustly different from each other, or indeed from zero?

Minor comments:

- L92-L93 "to quantify the feedback… to air pollutants" – as far as I can see, there isn't really any discussion or analysis regarding the effects on air quality, i.e. no figure showing changes in PM, aerosol concentrations, or any similar metric. So I would also remove the mention of feedbacks on air pollutants, as it seems to be something else which isn't truly in the scope of the paper

- L221: "78% of the total effects" – maybe change this to "78% of the total TOA radiative effect" or something similar, since this number only refers to the TOA quantity and not to the other radiative or other effects of AIE (for instance, in the surface RF, AIE appears to account for less than the ADE)

- L312-L315: the authors mention that a limitation of their study is that SSTs are prescribed, whereas in a fully coupled model "air-ocean interactions may cause complex feedbacks to aerosol radiative effects". This may be true, but there is no mention/discussion of the far more significant distinction: that in a model with a fully coupled ocean, the magnitude (and probably spatial pattern) of the climate response will be vastly different. (C.f. my first main comment above – this is another example where the authors seem to gloss over whether or not these atmopshere-only simulations truly diagnose the climate response to fire emissions, and subsequent feedbacks).

- Section 4 – another potential limitation that could be very briefly mentioned here, is that climate conditions (SSTs, sea ice) were used from a single year (2000) which was repeated. Fire emissions can be somewhat variable from year-to-year, for instance in El Niño years some regions can experience significant changes in annual fire emissions (e.g. Burton et al. 2020, https://doi.org/10.3389/feart.2020.00199), so I guess it's possible that the fire emissions and resulting ERF could differ over certain regions if the model were driven by SSTs from a different year?

- Supplementary figures S3, S4, S5: "Positive values represent the increase of downward radiation" – these figures show sensible and latent heat fluxes which are not radiative fluxes, so strictly this phrase doesn't make sense. Suggest changing it to "positive values represent a downward heat flux" or something similar

- Given that the primary (and very useful!) result of the paper is the analysis of the different aerosol radiative contributions to the total fire radiative forcing, it would be worthwhile also including a plot of the atmospheric absorption component. I realised this can be deduced by differencing Fig 1 and Fig 2, but would be great to see it included as a figure as well, rather than having to eyeball it