Atmospheric Chemistry and Physics Discussions

Atmos. Chem. Phys. Discuss., referee comment RC2
https://doi.org/10.5194/acp-2022-175-RC2, 2022

Comment on acp-2022-175

Anonymous Referee #2

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-RC2, 2022

Overall I think this paper will be suitable for publication in Atmospheric Chemistry and Physics, after revisions I've described below. I'm calling these major revisions, since one of my main concerns is that insufficient detail on the key components of the modelling system are provided in the submitted manuscript – as a result, I am unable to review the methodologies used within the model in the manuscript's current state (and will need to do so in a second look, once that information is provided). Some additional caveats to the conclusions should also be provided in the revised draft.

Having said that, I think that authors results (once they have provided that additional background) are quite interesting and worthy of publication: the potential negative feedback between forest fire smoke presence and fire starting conditions is an example, as is the use of a vegetative feedback model. The caveats associated with these models need to be clarified, see my detailed comments below.

My comments are divided into main and minor issues, and are usually referenced by line number in the current manuscript.

Main Issues:

A general issue regarding the manuscript Figures: the use of dots to show regions where the p value is less than 0.1 does not work. The dots are difficult to see, and obscure the values below, and the reader is unable to determine the boundaries of the $p<0.1$ region in most cases. The dots are difficult to distinguish from features such as the coastal boundaries in the maps, etc. Instead, the authors should (1) remove the dots from the existing Figure panels, and (2) create additional panels which show just the p value < 0.1 regions as colour-filled areas. These new panels should be placed in either inrevised Figures, side-by side (original panel minus the dots on the left hand column of panels, and

the corresponding p<0.1 region panel on the right hand column of panels), or as additional Figures in the main body of the manuscript, with a p<0.1 Figure following each of the original Figures (redone without the dots). As it stands, it's too difficult for the reader to distinguish the p<0.1 areas with the corresponding underlying regions on the map.

Lines 36-37: the idea that a reduction in precipitation could lead to a decrease in fire emissions is counterintuitive, and should be approached with caution and more caveats than provided by the authors in the current draft, since these effects will depend critically both on how well the flammability index employed simulates the fire risk, and how well the modelling system simulates the inputs for that risk.

There are several areas where there is insufficient description of the model and model components used:

Section 2.2.

- To say that the methods are "well established" without providing further information is insufficient for publication. Needs more details (which could appear in a summary Table); e.g. number of gas-phase species and reactions, what secondary particle formation processes are included (and the methodology used – e.g. how is secondary organic aerosol formed?), what aerosol species are included in the model, how the aerosol handles aerosol mixing state and aerosol size distribution, how the radiative transfer routine makes use of the model's aerosol speciation and size distribution (e.g. Mie scattering with a homogeneous mixture? Some other form of mixing state and a heterogeneous mixture assumption?). In its current form, for example, the reader can't tell whether the model includes any other aerosol species aside from OC and BC, while forest fires emit gases such as SO2, NH3, and NOx, as well as some base cations in particulate form, all of which may result in the formation of secondary organic aerosol.
- The issue of time-scale-to-effect needs to be discussed. The impression I have from the manuscript in its current state is that meteorological effects like the precipitation change go into the vegetation model, which then predicts a new LAI value for the given vegetation type, which is then used in the flammability calculation and hence in forest fire emissions. However, if this is the case, the changes in LAI are assumed to be instantaneous in the authors' model. An analogy: a "normal" springtime where the normal amount of foliage is added to the trees is followed by an extreme drought summer. While the vegetation may "dry up", the amount of LAI may not change, and the biomass available for combustion may not change – and the dryness of the vegetation may result in a greater fire risk, not a lower one. The authors describe the vegetation model as operating "dynamically", but not what that means in sufficient detail for the reader to understand whether changes are instantaneous or have an inherent time scale.
- The height to which forest fire plumes reach in the atmosphere has a critical impact on their downwind distribution, their radiative transfer impacts, etc. The paper has no description of the manner in which plume rise of the forest fire plumes is calculated, or how the emitted mass is added to the model grid. This description needs to be added

to the manuscript. If plume rise is not used to redistribute the emitted mass in the model, this is potentially a significant limitation on the accuracy of the model results, and should be acknowledged as such in the text.

- The equation for flammability (eqn 1, line 142) seems rather simplified (for context, I'm more used to regional air-quality model smoke parameterizations, based on fire weather index). The authors should contrast this with some of the other indexes available for predicting fire conditions on a forecasting basis (e.g. FWI, Wagner et al, 1987 : https://cfs.nrcan.gc.ca/publications/download-pdf/19927). Note also that the units of precipitation (line 141) seem to be reversed – shouldn't that be mm day^{-1}, not day mm^{-1}?
- Line 156: The equations for natural and anthropogenic ignition sources are presented without discussion on the basis for the parameterization or how IN is used. Is there some threshold value of IN below which ignition is not assumed to occur, for example? Some more discussion on the basis of the parameterization and how it is used should be added here. Similarly, what's the physical basis for equation 6 (line 159)?
- Line 163, equation (7): This seems very simplified. The use of one set of coefficients implies that fire suppression activities are the same everywhere - they are not; different political jurisdictions within the same country can have different policies (e.g. no suppression unless the fire is within some distance of a population centre, versus "all fires are suppressed"). The authors need to explain why these coefficients are applicable over the entire globe, and the assumptions that were used in their creation. Note that what I am after here and in the above points is more description of the physical basis and the potential limitations they might have on the results: I'm aware of the need for simplifications in a climate modelling parameterization context – what's missing here is enough information on what's been done and the caveats that might affect the model results.
- Lines 173 to 175: So, really, this is a way of allocating GFED emissions. Unclear (needs a few more sentences of explanation): how is the temporal allocation of fire emissions simulated? Are the GFED emissions available as a function of monthly total, and these are divided up over each day within the month based on the daily average of the met inputs to the equations? Also, how are the emissions distributed in the vertical? No mention has been made of plume rise calculations. This has a crucial effect on the dispersion of the pollutants downwind, and their climate impacts. Or is the vertical distribution provided by GFED?
- Section 2.2 also needs a description of the methodology used for each of the 3 aerosol effects (ADE, AIE, AAE); how they were parameterized in the model, perhaps as an additional table. How the aerosol speciation, mixing state, and size distribution was utilized by these parameterizations should be included in that description.

Lines 183-185: exactly what is meant by dynamically allocated needs to be described in a few more sentences (note timescale-to-effect issue noted above).

Lines 204-205: How is it possible that the model underestimates boreal fire emissions relative to GFED? The impression I had from section 2.2 was that the fire location procedure redistributes the GFED emitted mass, so should be equal. A few sentences explaining possible causes for the discrepancy are needed.

Line 258: I question the AAE impact a bit: there is a question of the duration of time over which the layer of deposited particles exists at the surface before processes like additional precipitation remove the layer or cover it over. Deposited forest fire smoke

does not last forever - how do the authors deal with, e.g., the deposited particles being removed by subsequent precipitation, covering by new vegetation, etc.? If the smoke layer deposition is assumed to never change post-deposition, then the albedo change impacts may be overestimated.

Lines 268 to 273: Please explain the reasoning here in more detail; this is unclear. The methodology apparently assumes an instantaneous change in the LAI as a result of the fires (if this is not the case, please describe timescale in the 2.2 methodology). However, a more likely outcome of drought is that the LAI in the short term will remain unchanged, while the foliage will become drier. The issue of time scale of the change in LAI to take place needs to be discussed in more detail, as well as the underlying foundation for the LAI dependence. I think this is a potential confounding factor to the work.

Lines 278-279: This is not correct: the number of factors contributing does not take into account their magnitude or the potential for non-linear interactions. Relative magnitudes could potentially be compared in maps, but the number of factors is meaningless, when the magnitude of each of their effects may be quite different. Figure 5d should be removed, in favour of, e.g., maps of relative contributions of the four factors to the total change as an additional multi-panel figure in the SI.

Minor issues:

Line 26: the change in average land precipitation should be stated as a relative change in percent as well as the absolute value, to give the reader an idea of the significance of a 0.018 mm month^{-1} change in the average.

Lines 30-31 vs lines 28 to 29: the last and second to last sentence in the abstract apparently contradict each other, the 2nd to last line implies less fires due to feedbacks, the last implies more fires. Please clarify this in the abstract.

Line 71, line 88: IPCC should be capitalized.

Line 105: Reference is population dataset, but how was it used to get human-induced ignition approximation? Needs a couple of sentences describing the methodology used.

Line 123: “dynamically” should be “that dynamically”

Line 141: units on precipitation are apparently inverted?

Line 153: "ignitions determine" should be "ignition determines"

Line 170: the abbreviation PFT is used here without definition, also SIC line 191.

Lines 193-194: "are analyzed." I wasn't sure whether you meant "were calculated" or if there was some additional analysis being done.

Lines 205-206: Also, forest fires release large amounts of NH3 and NOx (the latter makes nitric acid, which can combine with the NH3 to make particle nitrate). Also some SO2 which can react to make sulphate. Also some minerals/dust-like material. How are these included (are they included?) in the model?

Line 213: "enhance surface aerosols" : What about aerosols aloft? Wondering about the model's vertical distribution algorithm for smoke emissions.

Line 226: Maybe this should read "net shortwave radiation reaching the surface"?

Line 234: I found the result for Australia counter-intuitive: is the albedo of Australia starting off high due to high albedo land surface (not much snow and ice in Australia)? There may be an underlying assumption here of the smoke particle deposition not being disturbed, post-deposition. Lots of wind-blown dust in Australia; a surface layer of deposited smoke particles would be expected to get mixed with the local dust over time, reducing the impact of the AAE. Or is that sort of effect included in the model? Same question on line 258: how long would a surface layer of smoke particles last, given processes like erosion, new vegetation growth, mixing with wind-blown dust (potential for coagulation there), etc?

Lines 240 – 241: Change in shortwave is not balanced by change in longwave if I've understood the numbers correctly; rather, 1.23 W/m2 reduction in downward shortwave, and reduction of 0.83 W/m2 in upward longwave, which implies a net decrease in energy and cooling. Please comment on the significance of this level of change (e.g. with respect to the standard IPCC global average "contributions of different aspects of the radiative balance to global radiation budget"): how significant are the changes relative to global net radiative transfer.

Figure 5d: Note that in Figure 5, the caption reads, "Only grids with fire OC larger than $1x10^{-21}$ kg s^{-1} m^{-2} are shown in (d)." Why choose this particular number, as opposed to, for example, showing the grids where $p<0.1$?