"How the extreme 2019–2020 Australian wildfires affected global circulation and adjustments”

by Senf, F. et al.

Author's Response to the Editor and Reviewers of the manuscript

Dear Editor and Reviewers,

We thank the Editor and the Reviewers for their comments on our manuscript. Based on your very valuable comments, the following general changes were performed.

- we expanded the description of the aerosol simulations
- we included a new subsection to discuss the simulated aerosol optical properties and how they compare with available observations
- we put more emphasize on the fact that our simulated precipitation response is still highly uncertain

In order to separate the reviewer's comments and the author's response we have used the following color coding and formatting. The reviewer's comments are printed in black, the authors responses are printed in blue and text parts taken from the updated manuscript are printed in green.

For the reviewer's convenience, we provide two additional files where changes between the original and updated manuscript are highlighted. First, the *diff.pdf* file contains the added and removed parts, the *diff-add.pdf* only contains the added content. The two files are compiled with degraded figure quality with *latexdiff* (unfortunately some artifacts remain) for fast difference checking.

Sincerely, on behalf of the authors,

Fabian Senf

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Response to Reviewer #1:

In this study, the authors use a climate model to simulate the large Australian wildfires of December 2019 and January 2020. They build on the study of Heinold et al. (2022), which looked at injection height and aerosol properties, to focus instead on rapid adjustments in the stratosphere and troposphere. They find that these adjustments are substantial and modulated by underlying clouds. Dynamical adjustments in the stratosphere are driven by different responses in the south and north branch of stratospheric circulation. In the troposphere, adjustments impact the water budget and may ultimately impact precipitation.

The paper is well written, and the description of the mechanisms of the response is generally convincing. Some aspects of the simulations and the discussion could however be clarified, as commented below. The revisions to address those comments should not require additional analyses, so should be minor revisions.

Main comments:

The aerosol simulations are not described. It is difficult to determine whether adjustments are local or not (e.g., line 227) when not knowing where aerosols are located in the simulations. A figure showing the extinction and absorption aerosol optical depth, or related variables, would be useful to follow the reasoning. In addition, it would be good to state the absorbing properties of BC and OC aerosols in the version of ECHAM6.3-HAM2.3 used in this study early on, rather than waiting for the discussion (lines 412-414).

> Thank you for this comment! We have firstly extended the description of the aerosol simulations in Section 2.1, and secondly, as suggested, we have added a results subsection in Section 3 showing the distribution of the simulated wildfire aerosol and discussing its optical properties.

The use of a combination of nudged and free-running simulations to isolate rapid adjustments should be presented more clearly in Section 2.2. My expectation was that dynamical adjustments are suppressed in nudged simulations, but not thermodynamical (temperature-driven) adjustments because temperature is not nudged. The comparison to free-running simulations therefore isolates dynamical adjustments. But the reasoning presented on lines 218-220 and 226-228 does not follow my expectations. Is it because dispersion is also different in the two sets of simulations? Overall, what does it say when both nudged and free-running simulations show similar responses?

> Nudging acts as an additional force that influences momentum balances and movements in the system. However, the nudging is weak enough so that important equilibria, e.g. geostrophy, are not significantly disturbed. The situation is different, for example, for ageostrophic wind components. There the nudging becomes visible already in first order which also means that nudging impacts the response of the global residual circulation.

Nudging changes the way the system adjusts and what new perturbed state it finds. However, it does not lead to the fact that no dynamic adjustments happen at all. It is too weak for that and a stronger nudging would possibly have negative effects on the whole model performance. The close coupling between thermodynamics and dynamics makes it difficult or impossible to separate the two aspects.

The following statement was added:

"Nudging acts only as a small force on the dynamics. Important equilibria, such as geostrophy, are well preserved despite nudging. Dynamic adjustments of the system are possible even with nudging, but may be distorted by the effect of nudging. For this reason, it is not possible to isolate dynamic adjustments by comparing the nudged simulations with the free-running ensemble simulations presented later. However, an agreement of both simulation approaches indicates a robust response of the system."

The simulated changes in stratospheric temperature and cirrus cloud cover are probably large enough to be observable. Are there observational studies that would support these findings?

We only partially agree with the statement above. Changes in stratospheric temperatures have been observed and attributed to the effects of the Australian wildfire by e.g. Rieger et al., (2021); Stocker et al., (2021) & Yu et al., (2021); see also Liu et al., (2022) for a synthesis. We mention these observational results in Sect. 3.3 & 5.

From our experience, however, we would argue that it is extremely difficult to identify such small changes in the cirrus cloud cover and very unlikely to causally link these changes to the effects of wildfires.

Other comments:

Line 41: The first instance of “radiative forcing” is ambiguous. Do you mean effective radiative forcing, or the radiative effect of the adjustments? From context I would say the latter, but that is not clear.

Thank you for this hint. We rephrased the sentence as following:

"...absorbing aerosol contributed positively to the effective radiative forcing with a similar magnitude as the instantaneous radiative forcing..."

Line 53: Is pyrocumulonimbus formation always happening when wildfire aerosols are injected high in the atmosphere?

From a cause-effect standpoint, we would turn the statement around to say that pyro-convection is able to lift wildfire aerosol to higher altitudes.

It is not clear for us, which part of the referenced statement led to confusion. We made it slightly weaker:

"...potentially injecting a fraction of the smoke aerosols as high as the lower stratosphere."

Line 76: I do not understand the use of “for” in this sentence. Isn’t it the other way around, that radiative coupling between troposphere and stratosphere allows the chain of effect to happen?

In the case of Australian smoke, we see that dynamical and radiative effects are intertwined. As described later, Stratospheric circulation is affected by smoke heating. This likely causes also dynamical perturbations in the troposphere. Moreover, we also see that smoke partially heats the upper troposphere directly.

To make it clearer, we slightly rewrote the sentence:

"... chain of joint radiative and dynamical effects could represent an important mechanism for the downward coupling..."

Lines 135-136 and 241-242: I am not sure why the FIRE experiments need to be rescaled and averaged. Aren’t the ensemble simulations enough to deal with statistical significance? And then check from the scaled FIRE experiments whether perturbations are indeed linear functions of the aerosol injection amounts?

Indeed, we have the problem that 1. ensemble runs in general have their weaknesses (see discussion) and 2. considerably more ensemble members are needed to improve the signal-to-noise ratio (x 100 for an order of magnitude better accuracy). The scaling applied can help increase confidence in the various simulation approaches by providing another third way in addition to statistical significance of the respective ensembles and in addition to the direct comparison of nudged and free runs.

In general, we think that the applied sensitivity experiments that scale the perturbation strength are very helpful to understand the system's responses. If the response R of the system is small than

$$R = S\lambda + \epsilon$$

where S is a tangent linear model to describe the system's sensitivity, λ is the perturbation and ϵ describes the inherent noise of system that does not go away due to limited average / ensemble size capabilities and also knowledge.

Rescaling by perturbation strength leads to

$$\hat{R} = S + \hat{\epsilon}$$

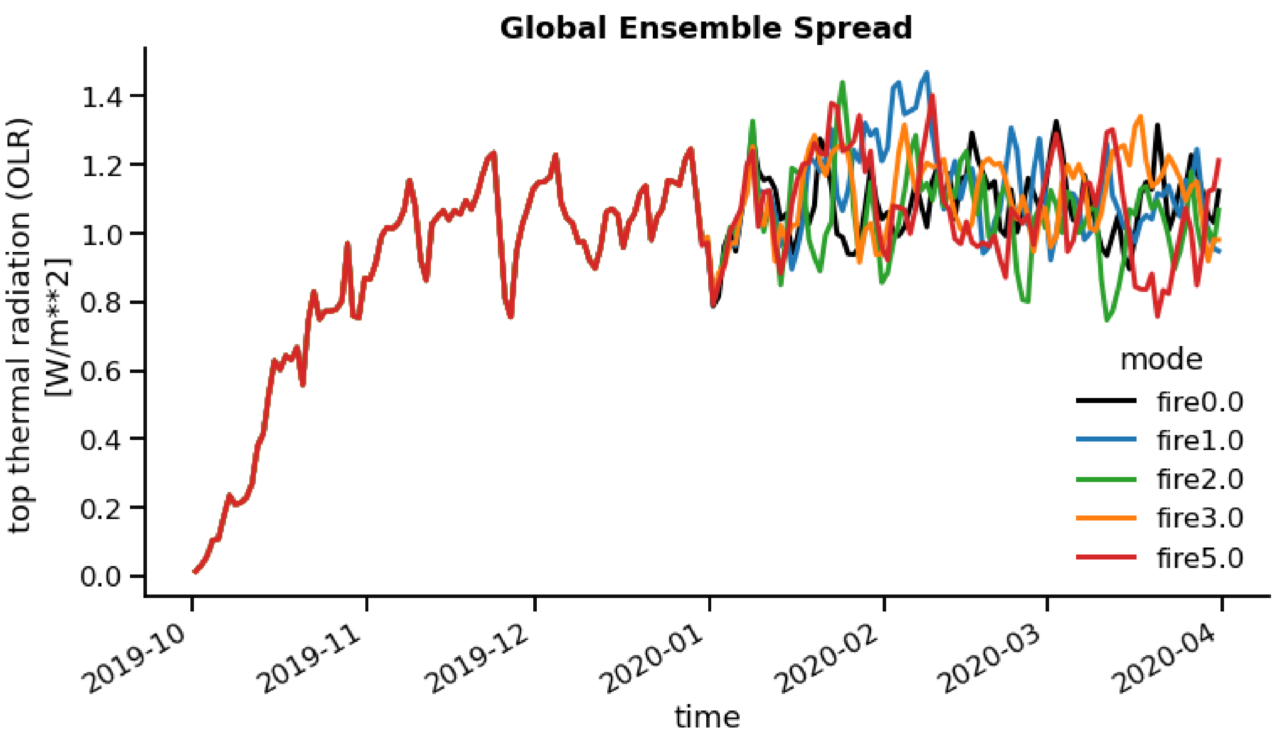
where the rescaled response $\hat{R} = R/\lambda$ is impacted by lower noise $\hat{\epsilon} = \epsilon/\lambda$. Thus, combining simulations experiments with different forcing scalings can be more beneficial than just expanding the ensemble size by the same amount of model runs because the signal of the response may become clearer for the larger perturbation even even if there is a risk that non-linearities will influence the adjustments.

The following reformulation is proposed:

"It seems helpful to compare experiments with different scaling factors even if there is the risk that non-linearities influence or even distort the adjustments for higher fire strengths. A linear behavior becomes visible when the response of the system grows equally with the strength of the forcing. When rescaling is applied, in which fire-induced perturbations are divided by the fire scaling factor, e.g. by two for FIRE2 vs. FIRE0, all rescaled responses should be of similar size under the condition of small perturbations and linearity. If all rescaled experiments are subsequently averaged into a composite value, the lower noise of rescaled responses from the runs with the larger perturbations allow increase statistical confidence."

Lines 157-160: This is an unusual ensemble initialization technique. Does that risk making the ensemble too narrow by applying only a small perturbation?

Ensemble spread needs to build up with our perturbation method because we start with the same initial conditions. As shown in the Figure below, where we use standard deviation of global-mean OLR across all ensemble member as measure of spread, it takes not more than a month until the model spread is fully developed. This is because we run an atmosphere-only configuration with fixed SST and because of the fast error growth in atmospheric dynamics.



Lines 269-270: I understand how the mere presence of clouds modulate the SW forcing, but how does that work in terms of LW adjustments?

In the following we try to find simple words to describe the core of the mechanism for effects of clouds in the LW adjustments. However, we would like to emphasize that accurate radiative transfer calculations are necessary to quantify the effect exactly.

1. the absorbing aerosol heats a layer in the lower stratosphere which is located above clouds
2. LW radiation from below enters the heated layer, is partially absorbed and re-emitted there
3. thus a fraction of the LW radiation from below is replaced by radiation originating from the heated layer
4. effects of clouds:
    - clearsky radiation fluxes from below the layer would be rather high (warm emitter) whereas cloud-affected radiation fluxes would be lower (cold cloud top temperatures)
    - thus, the portion of radiation that is replaced by the heated layer (due to absorption and re-emission) is larger if clouds are present

A slight reformulation is proposed to make this aspect clearer:

"In the tropics, the cloud effects significantly contribute to negative longwave adjustments because the cold cloud-top temperatures of the abundant cirrus increase the contrast of the warm perturbation in the lower stratosphere. "

Lines 275-276: Just to confirm, that heating is directly due to absorption by the injected aerosols?

Figure 4 shows the pertubation of the net heating rate, i.e. the difference in net heating rate between e.g. FIRE1 and FIRE0. It includes shortwave and longwave radiative flux convergences, thus slightly more information than the absorption by injected aerosol goes in here. However, especially during the first weeks after injection, the net heating rate is dominated by smoke absorption. See also our answer below!

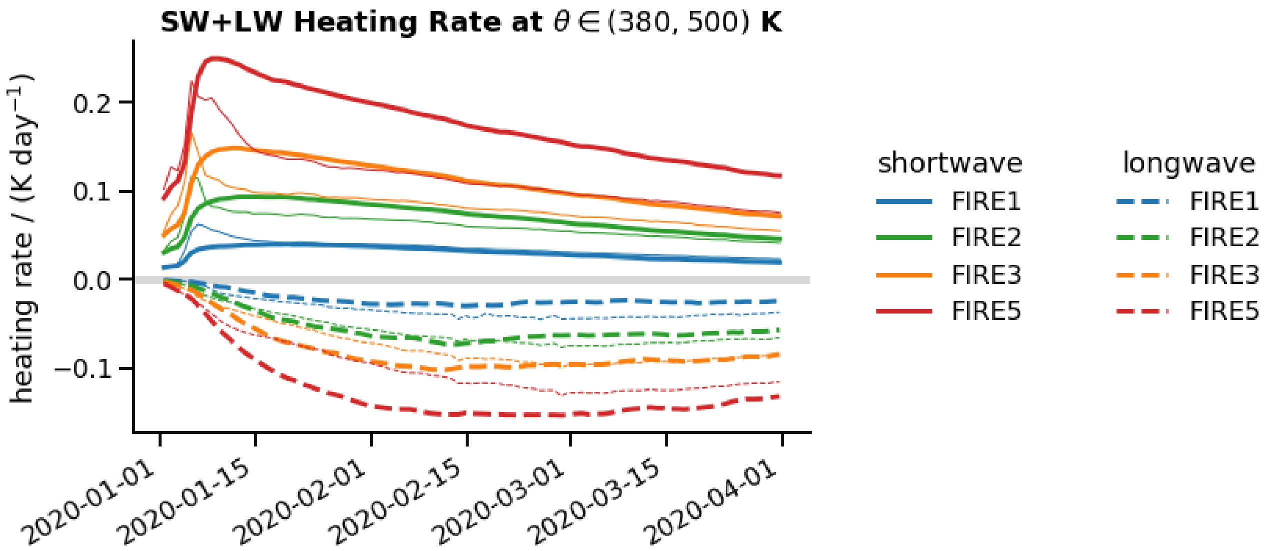
To make it clearer, the following re-formulation is supposed:

"To illustrate the consequences for the energy balance, Fig. 5 shows the net heating rate,

i.e. radiative heating due to convergence of shortwave and longwave fluxes, for the globally averaged lower stratosphere. A maximum ..."

Lines 279-281: And the heating decay is driven by the decrease in aerosol mass (or absorption)?

As stated above, Figure 4 shows the net heating rate which is the sum of the individual components of the radiation flux convergence. The split in shortwave and longwave heating rates is show below (material is also provided for download as part of the notebooks here: https://doi.org/10.5281/zenodo.7575809).



It becomes apparent that the decay of shortwave heating that is related to the decrease in aerosol mass is much slower.

Line 340: What range of pressures do you call the “upper troposphere” here?

We added:

"...but also the upper troposphere at around 200~hPa."

Lines 357-358: I do not understand what that sentence is saying, and what it implies for the results that were just presented.

To clarify the statement, the following re-formulation is proposed:

"Over land, mixed-phase and ice-phase cloud microphysical processes play an extremely important role in precipitation formation (Muelmenstaedt et al., 2015). It is therefore not implausible that a reduction in cirrus cover is partially accompanied by a reduction in precipitation."

Line 371: I would be good to specify which variability modes are referred to here.

The reference paper by Haywood and colleagues studied the impact of absorbing aerosol in the stratosphere and found reduction in surface precipitation as wellas changes in several climate variabilities modes such as the Quasi-Biennial Oscillation and North Atlantic Oscillation.

We like to connect to these findings with our study with the special emphasis on the reduction of precip. In order not to confuse the reader, we have deleted the second part of the statements.

Technical comments

Line 51: in several kilometers -> several kilometers

Corrected!

Line 59: smoke -> of smoke

Corrected!

Line 129: impact -> the impact on

Corrected!

Line 132: analysis -> analyse

Corrected!