"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

senf@tropos.de

Response to Reviewer #2:

The manuscript by Senf et al. investigates the radiative forcing and stratospheric circulation response to the extreme 2019-2020 Australian wildfires in nudged and free-running climate model simulations. The topic is of interest to ACP readership and the paper is clearly written. My main criticism is that the current manuscript lacks a comparison of aerosol optical properties between the different simulations and the observations. Without such a comparison, it is difficult for the reader to make an opinion on the realism of the different scenarios and to place the estimated forcings in the context of the literature. There are other limitations inherent in the study, and although most of them are briefly alluded to in the paper, the authors should consider expanding those points in the discussion.

For these reasons, I recommend that the paper be reconsidered after major revisions. My concerns are detailed below.

Major comments

1) It would be helpful to include a description of the evolution of the aerosol field in the different simulations and a comparison with the (already published) observations to assess how realistic the different runs are. CALIOP, SAGE and OMPS-LP are all suitable instruments and were used to characterize the Australian wildfire plume. This validation is necessary since the representation of the smoke plume in the large-scale model misses important processes which strongly affect dispersion, such as the confinement of the plume within vortices.

> Thank you for this remark!
>
> We assume that we have misleadingly not made clear that the present work builds directly on the studies of Heinold et al. (2022). The simulations used (in our nomenclature FIRE0 and FIRE1) are identical to the simulations of Heinold et al. (2022). This gives us the opportunity to rely on the evaluations conducted in Heinold et al. (2022) with observational data. There, simulation data were extensively compared with ground-based remote sensing (AERONET, Polly Lidar) and satellite-based remote sensing (AVHRR, CALIOP). It could be shown that our simulation approach reproduces the observations within the uncertainties.
>
> As a compromise, we have devoted an introductory results subsection 3.1 to simulated AOT and comparison with observations. However, we must emphasize that further detailed observation-simulation comparisons here do not fit the thematic focus of our paper.

2) An important amount of water vapor was injected together with the wildfire aerosols. A significant reduction in stratospheric ozone was also reported (e.g., Yu et al., 2021). As both water vapor and ozone are radiatively active gases in the stratosphere, they will impact the circulation response and stratospheric adjustment. Lines 432-434 of the manuscript, it is conceded that those effects are not well-represented in the simulations. However, I disagree

with the authors when they write that such perturbations “mainly occur in the second half of 2020” (lines 432-433). A number of papers (for instance, Kablick et al., 2020 ; Khaykin et al., 2020 ; Schwartz et al., 2020) documented that perturbations in composition were already present a few days after the injection. The authors could at least comment further on the impact that neglecting those perturbations may have on their results.

> To make clear that we do not emit additional water vapor together with the wildfires, we added the following explanations in Sect. 2.2.1:
>
> *"The GFAS emission data are input into the model as external data and mapped onto source descriptions of several aerosol species such as sulfate, dimethyl sulfide (DMS), OC and BC. Please note, however, that our modeled fire emissions do not represent a potential water vapor source and corresponding resulting effects such as propagation of a water vapor anomaly (Schwartz et al., 2020) may be inadequately represented by our model data."*
>
> In the discussion we wanted to express that we do not describe stratospheric chemistry and thus rather focus on adjustments on shorter time scales. To make that more clear, we reordered and rephrased to corresponding paragraph:
>
> *"... Yu et al. (2020) estimated the contribution of the latter mechanism to be about 50 % with respect to the total ozone reduction. However, since we do not describe stratospheric chemistry with our current model setup, and since ozone changes become more important in the second half of 2020, we only conducted simulation experiments up to and including March 2020. Therefore, we are only able to draw conclusions on the changes and underlying mechanisms during the first three months after the extreme Australian wildfire event. Furthermore, a substantially moister lower stratosphere was attributed to the effects of the Australian fires by Diallo et al. (2022), possibly due to both locally increased water vapor emissions from the extreme fires (Schwartz et al., 2020) and changes in water transport due to adjustments in global circulation."*

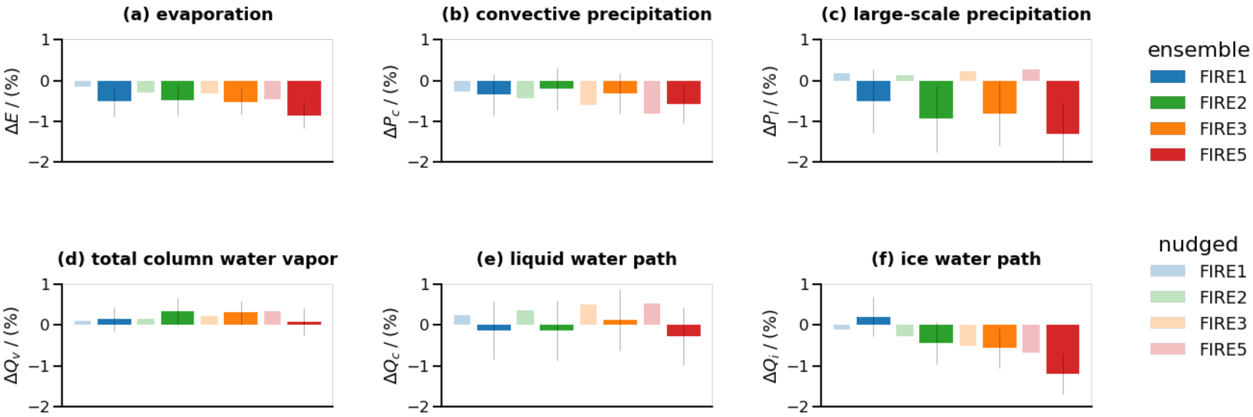
3) The treatment of the aerosols in the model should be presented in more detail in Sect. 2, in particular their interaction with chemistry and their radiative properties etc.

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

4) The precipitation response (Sect 3.3) does not seem very robust to me. Figure 10 and 11 show that, among the different simulations, it is not a monotonic function of the injected amount of black carbon aerosols. Is it really significant ? Furthermore, the mechanisms behind this response are not clearly explained. I would recommend either providing more information (and a mechanism) to support this hypothesis or shortening that point.

> Yes, indeed the precipitation response is very uncertain. When averaged over February and March, the following average results are obtained for the hydrological

variables:



Combining responses for the different fire strengths using rescaling as discussed in Sect. 2.2.1, a relative reduction of convective precipitation of -0.17 +- 0.2 % and a relative reduction of large-scale precipitation of -0.38 +- 0.32 % is found. I.e. the change in convective precip. is not significant, however the change in large-scale precip. is likely negative, however the magnitude is very uncertain. Both together have in fact a slightly reduced uncertainty: we find that total precipitation is reduced by -0.24 +- 0.13 %.

A reduction in precipitation is also in line with other studies by Samset and Myhre (2015) and Haywood et al. (2022), hence it is at least plausible that such responses may have happened also due to Australian smoke effects.

As a suggestion for improvements, we have shortened the section somewhat and emphasized the uncertainty in the precipitation response in several places.

Other comments

l4: 'as high as the stratosphere': Could you be more specific and provide an altitude range ?

With reference to Ohneiser et al. (2020), we changed the sentence as follows:

"...but also due to smoke aerosol released up to an altitude of 17 km."

L 13: 'in the Southern hemisphere..' :'averaged over the Southern hemisphere..'

Corrected.

l 18-19: consider being more quantitative here

We added the numbers from the re-scaled results in the abstract:

"... Subsequently, increased temperatures were also obtained in the upper troposphere,

causing a global decrease in relative humidity, cirrus amount, and the ice water path of about 0.2\,\%. As a result, surface precipitation also decreased by a similar amount, which was accompanied by a weakening of the tropospheric circulation due to the given energetic constraints. ..."

l 73 : Actually none of the cited papers explain the dynamical mechanisms behind the formation and maintenance of smoke vortices. They just describe the phenomenon in reanalysis or model simulations. A more insightful paper in that respect may be Lestrelin et al (ACP, 2021), which could be cited, although it does not provide a fully satisfactory explanation from a dynamical point of view either.

We added the suggested reference, but like to point out that Allen et al. (2020) provides an in-depth analysis of the vortex dynamical characteristics.

However, we also agree that scientific understanding for such complex dynamical phenomena must be constantly improved and thus changed the wording such as:

"... mechanisms ... appear to be well studied"

L 101-104: Could you include more detail (part of the table) ?

We added a few more sentences in the model description. Furthermore, the new section 3.1 also adds more details for describing the aerosol simulations.

L 121-125: Please recall the level of injection (the reader should go to Heinold et al. only for the details) and add a reference for the aerosol mass here. Also, the end of the sentence seems to be missing a verb.

We included the suggested additions.

L 133: I don't understand this sentence. Why can one not compare the different experiments when the response is not linear ? Would all the experiments be useful if the response were linear (would 2 not be enough)?

Yes, you are right! The considered sentence is indeed misleading.

We think that the applied sensitivity experiments that scale the perturbation strength are very helpful to understand the system's responses. What we actually meant was that the response scales with the perturbation strength and this may help attribute certain response pathways to the perturbation. 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."

L 143-144: I am skeptical that the nudging of wind only does not affect the energy budget, since wind is directly related to temperature through geostrophic/thermal wind balance as mentioned later in the paper. The Zhang et al reference might not be sufficient here, since those authors did not consider a large stratospheric aerosol injection but rather the background aerosol state.

We completely agree with your comment. What we actually liked to say was that the nudging tendencies do not explicitly appear as heat sources.

We rephrased:

"...such that no explicit temperature tendencies appear due to nudging."

L 157: 'order 10^-6' : '10^-4 %' would make clearer that it is the relative variation which is meant

Corrected.

l 174: I would remove 'very popular'

Corrected.

Figure 1: MATM could be defined in the main text as well as in the caption.

Following your suggestion, we defined MATM as well in the main text (first paragraph of results section).

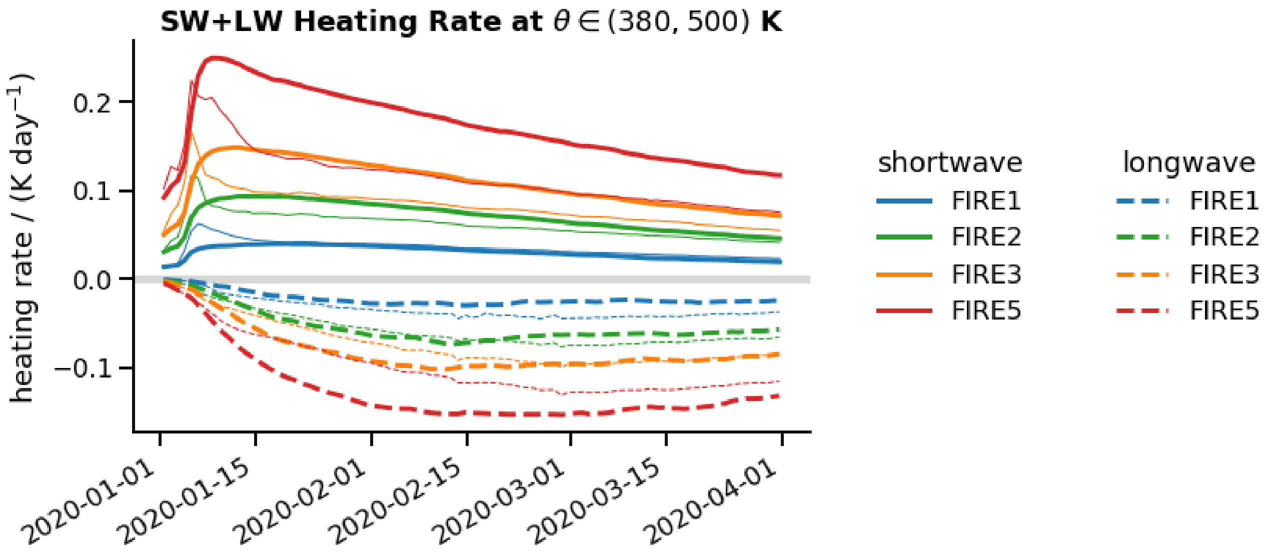
Line 276-277: "The nudged simulations seem to reach higher heating maxima for smaller fire strengths": do you have any idea why ?

This is a question that we cannot answer with definitive certainty and still requires further investigation by us. However, we believe that it may be important for the development of the nudged runs that dynamic structures from the observations were imposed onto the simulations in such a way that the smoke remain less diluted and therefore could be found in higher concentrations for a longer time. This would lead to higher heating maxima and also greater lofting rates as discussed together with Fig. 5.

L 280 : This longer decay time in the free running simulation seems at odds with Fig. 1 (which suggest a longer lasting SW perturbation for the nudged simulation) and the expectation that the plume is more diluted in free-running simulations. Again, it would helpful to have a comparison of the aerosol field between the two sets of runs.

Two aspect play a role here:

1. we show net heating i.e. a faster decay of net heating can be rather caused by a stronger longwave response and
2. we only consider a certain height range between 380 and 500 K in the lower stratosphere such that for stronger shortwave heating stronger self-lofting occurs and and the heating perturbation leaves the analyzed area in an upward direction.



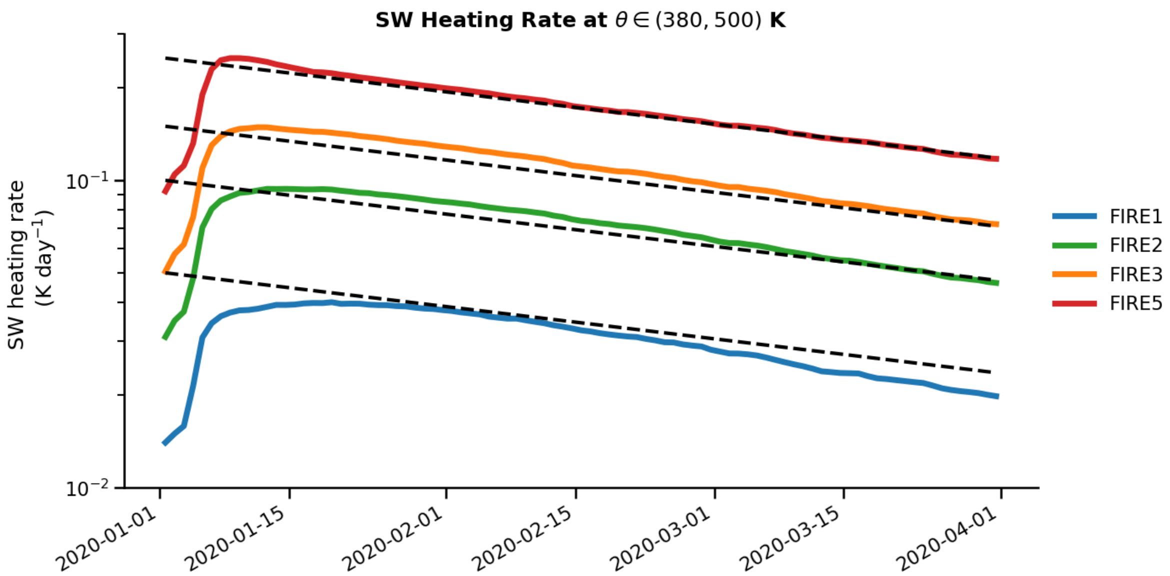
The net heating rate which is the sum of the individual components of the radiation flux convergence. The split in shortwave and longwave heating rates is show below (material is also provided for download as part of the notebooks here: https://doi.org/10.5281/zenodo.7575809).

The following statement was added:

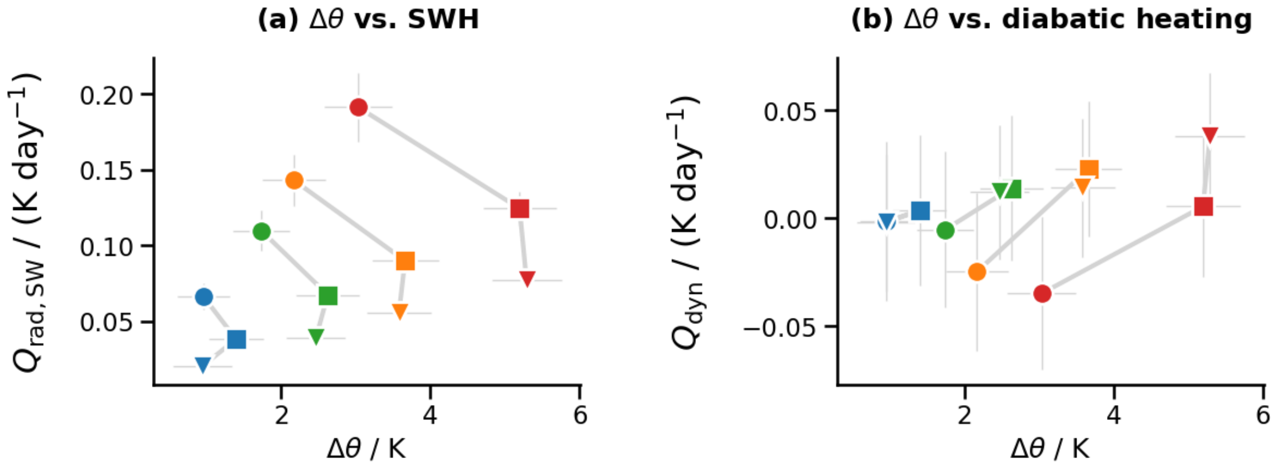
"The nudged simulations appear to fall off faster as some of the smoke-induced shortwave perturbation leaves the lower stratosphere due to stronger self-lofting upward (see next paragraph) and as the longwave response also appears to be stronger."

line 340: what is the lifetime of the black carbon aerosols in the upper troposphere in the model ? Do you confirm that they are refilled by sedimentation from above ?

Burden of stratospheric BC and resulting SW heating behave very similar with regard to the decay characteristics. We find e-folding times of around half a year for both. For illustration, the SW heating is shown as log-linear plot below. The dashed lines are for guiding the eye and represent an e-folding time of 4 months.



Moreover, our model data provide the indication that temperature perturbation in the UTLS moves slightly downward with time. However, we do not confirm that is effect is caused by sedimenting smoke particles. Indeed, the picture is more complex because the temperature increases despite the fact that SW heating due to absorbing aerosol is decreasing [see left panel below for quantities averaged over the SH at ~180 hPa; (circle, square, triangle) = (Jan; Feb, March) connected by a gray line]. We think that dynamic heating due to changes in large-scale circulation can not be neglected here.



The following sentence was added:

" The fact that the average temperature increases at 150 hPa and below despite the decreasing heating rates (see Fig. 10a) is an indication that effects due to changes in dynamics rather than sedimenting aerosol play an important role here. "

Line 365: See major comment 4

Please see our response there.

Line 402: You might consider citing De Laat et al (2012) regarding self-lofting. It is one of the first papers to mention this effect.

Thank you for the hint! We included the mentioned reference.

Figure 10: Please reproduce the legend here, so that the reader does not have to go to a different figure to interpret this one

Done!

l 395: data : model ?

Changed accordingly!

l 457-460: this sentence might rather belong in the introduction

We agree with you, but also do not see that the closing remarks lose quality because of this sentence.

l 481: remove 'as' ?

We made the part even a bit shorter.

References

de Laat, A. T. J., Zweers, D. C. S., Boers, R., & Tuinder, O. N. E. (2012). A solar escalator: Observational evidence of the self-lifting of smoke and aerosols by absorption of solar radiation in the February 2009 Australian Black Saturday plume. Journal of Geophysical Research, 117, D04204. https://doi.org/10.1029/2011jd017016

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