Response to Referee Comments on “Dynamical Perturbation of the Stratosphere by a Pyrocumulonimbus Injection of Carbonaceous Aerosols”

We would like to thank all the Reviewers for their careful reading of the initial submission, and for the many constructive remarks and suggestions, which allowed us to improve the manuscript.

Following the reviewers’ recommendations, the reviewed manuscript now includes several comparisons between the observed and simulated SWIRLs.

Please find below our replies to the single Reviewers’ comments.

Note that the Reviewers’ comments are italicized, while the responses of the authors are in normal font and blue. The line numbers in authors’ responses refer to the line numbers of the revised manuscript unless specified otherwise.

Answers to Anonymous Referee #1

This paper describes a simulation of a large stratospheric smoke plume injected by an intense pyroCb event in Canada. Emphasis is placed on the successful simulation of a stratospheric anticyclone induced by diabatic heating within the plume. Radiative interactions of aerosols (coupling with meteorology) facilitate a realistic anticyclone that is comparable with observations in previous studies. This work represents a major advancement in aerosol modeling with radiative coupling, and is the most detailed simulation to date of the SWIRL (Smoke With Induced Rotation and Lofting) phenomenon. I therefore recommend publication after addressing the comments below.

General comments

While it is VERY impressive that GEOS was able to simulate a realistic SWIRL, I did have some difficulty reconciling the results with real-world observations. I became heavily dependent on looking at the work by Lestrelin et al. Readers will want a quick way to compare the timing, magnitude, and location of the simulated SWIRL with what actually happened. It’s okay to reference Lestrelin for some of this, but I think you need to have at least one direct comparison in the current manuscript. For example, can you add a second panel to Figure 2 highlighting the actual path and timing of the observed SWIRL? Is it the same path as Figure 6 in Lestrelin et al? How does the location and altitude of the observed SWIRL on 23 Aug compare with the simulation?

We thank the reviewer for this insightful comment.

According to Lestrelin et al, the vortex on the 23rd August is located at about 20° W 45°N with a potential temperature of 420 K. In the simulations, on the same day, the vortex is located at about 60°W 37°N and has a 470K potential temperature. Thus, in the simulation, the eastward transport of the vortex is less pronounced than in the real world, while the meridional displacement is larger. Also, the vortex is 50 K higher in the simulation than in the observations.

We have added a panel to Figure 2 showing the path and timing of the observed SWIRL (vortexes A and O of Figure 6 in Lestrelin et al) and added text about the comparison between the results of the simulation and real-word observations.

Additionally, we added a comparison of the potential temperature of the SWIRLs in observations and simulations in plot 9d. Several comments about the results of this comparison are reported in lines 273-281 of the revised paper.

This analysis focuses on the SWIRL at lower latitudes. Did the observed SWIRL stall for 7 days such as in the simulation? Do you know why the GEOS simulations were unable to identify the other SWIRLS identified by observations (e.g., Lestrelin et al)? Is it simply a function of differences in meteorology caused by the free running simulation? Some discussion of this is likely warranted.

The observed SWIRL did not stall over the Atlantic (see Figure 2, panel (b)).

More specifically, while in the real world the initial SWIRL (Vortex O, Lestrelin et al) reaches Europe around the 24th of August after an unimpeded trip across the Atlantic, the simulated SWIRL stalls for several days (until the 24th August) over the Atlantic just off the US East coast. The stalling of the SWIRL is caused by the action of the large scale circulation that eventually redirects it to the west.

Concerning the splitting of the plume and the formation of offspring SWIRLs, we did not find evidence of such an occurrence in the simulation. As shown by Lestrelin et al, the formation of offsprings occurs following the elongation and stretching of the main vortex, caused by its interaction with the ambient wind shear. In our simulation the most pronounced example of such an interaction occurs during the stalling of the vortex, where the lowermost part is stretched to the east, while the upper part remains compact and eventually starts to move towards the west. However, when our tracking algorithm is applied to the lowermost remains of the SWIRL proceeding towards Europe in the days following this event no offspring circulation is found.

The dynamics of the stretching appears to be crucial in the generation of offspring, and the ambient meteorology plays a major role in this process. Also, the magnitude of the stretching is also affecting directly the absorbing aerosol concentration, which is a key parameter in the generation and maintenance of a SWIRL.

Thus, the lack of offspring circulations (but in general, the differences in smoke transport) is given by the differences in meteorology between the free running simulation and the real world.

We have added several statements about this issue in the revised text (Lines 180-199).

Additionally, the discussion in Section 3.1 should include a bit more information on the limitations of using a free running simulation. Basically, provide a 1-2 sentence summary of what is in the Appendix. Perhaps make it clear that the replay mode is a good proxy for the observed plume, but you have to use free running methods here for reasons X, Y, Z.

We have added some more details about this issue in Lines 160-166.

In summary, I was a bit lost on how I could quickly compare the SWIRL simulation results with real-world observations. It's okay if the simulated plume takes a radically different path from the obs, but you'll need to explain that in the context of the free running simulation type used here. Basically, that subtle changes in ambient meteorology (e.g., relying forecast fields) can have a significant effect on plume transport. That's a limitation of doing a study like this, but it's the only way without adding much more complicated ensemble work or something similar. Some of this is already provided in certain parts of the manuscript, but a little reorganization would help, especially making these aspects more clear early on.

We thank the reviewer for this beneficial comment. In response, we have reorganized the text as suggested, addressing this issues in Lines 75-81.

The meteorology in a free running simulation is necessarily different from the observed one and thus a direct comparison can only be limited.

This is the price to be paid to the free running configuration of the simulation, but as the reviewer pointed out, this is the only way without doing a much more complicated work involving an ensemble of free running simulations.

We have added text that hopefully makes this clearer, also specifying that the aim is not to reproduce the observations faithfully, but to generate a realistic SWIRL following a PNE-like aerosol injection. From this point of view, the comparison with the observations must be intended as an instrument to evaluate whether the simulated SWIRL is realistic, but also to underline the importance of the background meteorology in the determination of the SWIRL evolution.

Specific Comments

Abstract line 1: Better to say that the PNE took place during the "evening and nighttime hours". The most intense pyroCbs developed before sunset.

Corrected as suggested.

Lines 33-35: Note: The PNE actually had 7 individual pyroCbs, including the 5 identified by Peterson et al 2018 and two more that occurred after sunset identified by Fromm et al 2021. The majority of the smoke likely reached the stratosphere via the 4 largest pyroCbs highlighted by Peterson et al 2018.

Fromm, M. D., G. P. Kablick, D. A. Peterson, R. A. Kahn, V. J. B. Flower, and C. J. Seftor (2021), Quantifying the Source Term and Uniqueness of the August 12, 2017 Pacific Northwest PyroCb Event, Journal of Geophysical Research: Atmospheres, 126(13), e2021JD034928, doi:https://doi.org/10.1029/2021JD034928.

Corrected and integrated as suggested.

Lines 120-121: double negative is confusing. Perhaps use something like: "These criteria constrain PV anomalies to those associated with the presence of high carbonaceous aerosol concentrations."

Corrected as suggested.

Figure 2: perhaps mark the PNE source region for reference?

Added the PNE source region in figure 2 as suggested.

Line 164: change "Caribbeans" to "Caribbean Sea"

Corrected as suggested.

Lines 251-254: This is a long and slightly confusing sentence. Please revise and/or clarify.

The text was integrated and revised (lines 273-281).

Fig. 9e and d are referenced before Fig. 9a and b?

The order of the plots has been changed according to the referencing order in the text.

Line 274: fix "(9f)"

Corrected as suggested.

Fig. 9c. Caption states that the three curves are averages, but the legend uses "TOT" and "AER HR". This is confusing. There is enough space to make a more descriptive legend here.

The legend has been expanded.

Line 300: confusing sentence

Revised. (lines 347-349)

Line ~305 to the end: This ending text might be better suited for a separate section called something like "discussion and limitations". The preceding text covers the actual conclusions I think. You could then end with a "big picture" paragraph highlighting the advance this study makes in plume modeling and future directions.

Revised.

Answers to Anonymous Referee #2

Review of "Dynamical Perturbation of the stratosphere by a pyrocumulonimbus injection of carbonaceous aerosols" by Goglioni et al., 2022, submitted to ACP

Doglioni et al. successfully modeled the anticyclone (SWIRL) in a pyrocb event using GEOS CCM. This study finds that the diabatic heating from the smoke aerosol is critical to maintain the SWIRL structure. In general, I find this study very interesting and important, and the author team has done a very impressive job by simulating the SWIRL in a climate model. I suggest publication with minor revision.

Can the authors comment on the resolution of a model needed to reproduce the SWIRL? This study uses 50 km, will 100-km or 200-km models will fail?

The issue of which resolution is the most appropriate to detect and resolve features of a SWIRL is a very interesting one. To properly answer this question, a sensitivity analysis would be needed, exploring different resolutions and comparing the resulting outcome, which would constitute a paper by itself. We predict that a finer resolution would have a strong impact at injection and shortly afterwards, as the aerosol concentrations in one gridbox would be larger and therefore also the heating rates. On the other hand, a coarser resolution would probably decrease the impact of the aerosol on the meteorology and possibly the maintenance of the swirl. A study that figures out the resolution threshold needed for the swirl would be indeed very interesting, but it does require additional ensembles of simulations, too much to be included in this paper.

Just curious, since the author have successfully modeled the SWIRL, it is very interesting (at least to me) to know how much smoke mass is associated with the SWIRL vs. how much smoke are out of SWIRL. Will that be different between the 2017 PNE and 2019 ANY events? Note, I am not asking for more runs, some comments are helpful here if the authors know that.

To the authors' knowledge no published results are available in the literature providing an estimate of SWIRL aerosol mass from observations, therefore we do not have any comparison between the 2017 PNE and the 2019/2020 ANY.

We can provide figures for the smoke inside and outside (i.e. in the stratosphere) of the simulated PNE SWIRL on the 23rd of August: outside 225 ktn, inside 5 ktn.

I agree with the authors that seems the vertical resolution of a climate (usually 1 km near tropopause) can be limiting in simulating the SWIRL.

Yes, that is a limitation that needs to be addressed in future studies in order to explore in-depth the vertical structure of the SWIRL.

I found there is limited comparison between model and observations. I understand it is free running mode, I still think comparison like the size of SWIRL; smoke masses; altitudes/durations are helpful to provide readers knowledge of what was happening in the real world.

We thank the reviewer for the useful remark. It is indeed important to put in evidence the comparison between the model and the observations even if the simulated SWIRL had a drastically different stratospheric life than the observed one. This direct comparison can show how realistic the SWIRL we simulated is with respect to the observed one, so it is important to us.

We have added a direct comparison between the path/altitude/duration of the observed and simulated SWIRLs (figure 2-figure 8). Also, some additional remarks focusing on the comparison between real world and simulated SWIRLs have been made.