Review of the manuscript "Radiative impacts of the Australian bushfires 2019-2020 - Part 2: Large-scale and in-vortex radiative heating"

Dear Editor, dear anonymous Reviewers,

Many thanks for your constructive criticism and the very useful comments. Based on your comments, we have thoroughly revised our manuscript, which has now e.g. a much more detailed description of the CALIOP observations, a more detailed description of the RT calculations inputs and a clearer indication of how the HR translate to lofting rates for both the hemispheric-scale and the vortex-scale plumes, among other minor corrections and clarifications. We also extended the reference list considerably. Please find more detail and a point-by-point reply to all of your specific comments in the following (Your comments **in black** and our replies in blue). When specific manuscript lines or lines intervals are mentioned (Lxxx-yyy), the line numbers are for the revised manuscript version. We think that, thanks to your comments and suggestions, the present version of the manuscript is greatly improved with respect to the previous version.

Thank you very much,

Pasquale Sellitto on behalf of all co-authors

Reviewer #1

General comments:

On the positive side, the overall organization of the manuscript is good, and the sentence level flow is also very good, making the paper a pleasant read. The intended purpose of the study is mostly clear and well defined. The topic is an important one with a lot of recent literature, and the heating rate calculations will be of interest to readers of the journal.

On the negative side, however, the paper seems rushed with many holes where important details are left out, and the methodology is therefore often not clear at all.

We would like to thank the Anonymous Reviewer #1 for her/his kind words and the very useful comments. Please find our replies to the five individual general comments (GC) that we could identify, i.e. with a reference to the inherent Specific Comments (SC).

GC1) For instance, there's some emphasis on clouds in the introduction but they are not mentioned again and there's no information about what underlying albedo was used to calculate the heating rates. This may be a major flaw, if the clouds were really not taken into account despite the need for it being already emphasized in the authors' Part 1 paper.

See specific comment SC4.

GC2) Next, a "background" condition is mentioned in the methods but is not shown in figures or equations, so I can't tell whether it was really used or not.

See specific comments SC11, 12, 26 and 41.

GC3) The section on use of the lidar data (which is new to this paper compared to Part 1) is quite superficial and leaves a lot of questions unanswered.

See specific comments SC16 and 17.

GC4) Secondary conclusions about the heating rates being "consistent" with quantified plume rise rates are not supported at all.

See specific comments SC3 and 46.

GS5) Finally, the effort to compare with other similar research concerning radiative effects for the same smoke event, both in the introduction (to give perspective on this paper's unique contribution) or in the discussion (to compare/contrast results) is quite limited. It's a very popular topic and various easy Google searches turn up many apparently relevant papers that are not referenced here. Also, for the the lidar methodology section in particular, there are no lidar references at all to explain or support the authors' methods.

See specific comments SC6, 7, 16 and 17.

Specific comments:

(Abstract)

SC1) L20 Add "for February 2020", because this result is specific to that month, correct?

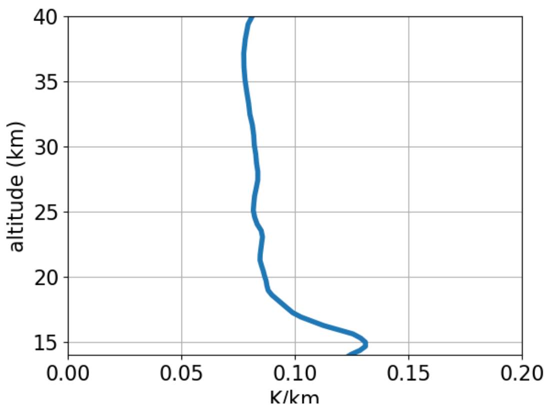
Yes, correct: the information is added in the Abstract.

SC2) L21 The logical link between optical properties and aging is not established in this paper.

It is true, the explicit link between aging and the variability of optical properties is not a specific target of this paper but this is discussed in the "Part 1" (S22) paper. This is further stated at L47-49 in the Introduction and elsewhere in the Discussion (see e.g. L264-280), where S22 (and references therein) is cited.

SC3) L24 The quantitative consistency between the heating rates and the lofting rates is not established in this paper.

A typical value of the lofting rate in the stratosphere is 100 m/d for a HR of 1 K/d. This is easily obtained by representing HR in terms of the potential temperature and then comparing with the vertical gradient of the potential temperature in the stratosphere. Some simple estimations of this conversion are shown in the figure on the right. We now cite this typical value in the revised manuscript (L312- 313).



(Introduction)

SC4) L46 I'm confused by the discussion of clouds. In Part 1, some calculations were made indicating that there could be a very significant impact of surface albedo and it was indicated that clouds should certainly be taken into account in further work. However, this introductory summary of Part 1 is the only time that clouds are mentioned in the current

work. There's no information about what surface albedo was used in the current calculations. Were clouds ignored again?

Part 1 paper (S22) is about TOA and surface RF in the SW range, which are affected by cloud cover (especially through a reduction of the insolation, e.g. term S in Eq. 1 in S22) and underlying SW surface reflectivity (term R_S in Eq. 1 in S22). For the vertically localised effects discussed in the present Part 2 paper, namely the radiative heating in terms of the HR, the possible presence of clouds would be overwhelming in given specific vertical profile, if present at the same time as relatively thin aerosol layers, like the ones discussed here (see e.g. Liou 2002). Thus, we see a limited interest in discussing the effect of clouds in the present manuscript. We mention this aspect, and the fact that there are cloud-free conditions, in the revised manuscript.

SC5) L64 "to cover both the radiative effects mentioned previously" isn't clear. Is this referring to the two factors at line 46? But it doesn't actually seem to cover the 2nd factor (the presence of clouds) at all.

No, this refers to the hemispheric and the in-vortex spatial scales. We changed the sentence, which now reads as follows: "…which complements the RF estimates of S22, to cover both the effects at the hemispheric and in-vortex spatial scale mentioned previously".

SC6) L62 At least one other paper has heating rate calculations for the same event. This should probably also be included in the discussion of the context for the current paper. Wu, D., X. Niu, Z. Chen, Y. Chen, Y. Xing, X. Cao, J. Liu, X. Wang, and W. Pu (2022), Causes and Effects of the Long-Range Dispersion of Carbonaceous Aerosols From the 2019–2020 Australian Wildfires, Geophys Res Lett, 49(18), e2022GL099840. https://doi.org/10.1029/2022GL099840.

Wu et al., 2022 only discuss (clear-sky) shortwave heating rates (so not including the longwave spectral region, which we demonstrate to be very important) due to smoke aerosols from the Australian fires, for the first days of January 2020. Thus, the comparison with our results is not simple (Wu et al.: 1) SW-only, 2) time series in beginning January, 3) reanalyses; The present work: 1) SW+LW, 2) monthly means in January-April, 3) observations and offline RT calculations). Nevertheless, we added a mention of these results at L297-298.

SC7) There are many other papers addressing the same event. It would be helpful to readers to include a broader discussion of these and how the current manuscript fits in to this extensive literature, and to provide more insight on whether this manuscript is or isn't consistent with hypotheses and findings of other researchers. For instance, at least one paper also uses CALIPSO data to calculate radiative effects: Papanikolaou, C.-A., P. Kokkalis, O. Soupiona, S. Solomos, A. Papayannis, M. Mylonaki, D. Anagnou, R. Foskinis, and M. Gidarakou (2022), Australian Bushfires (2019–2020): Aerosol Optical Properties and Radiative Forcing, Atmosphere, 13(6), 867.

Papanikolaou et al. (2022) present results of RF, not of HR. Thus, this would have rather been a reference for the "Part 1" paper and not the present "Part 2. We haven't found other papers addressing HR for this event but please feel free to suggest more, if you have any other.

SC8) L64-65 Is the inclusion of longwave calculations new for this paper compared to Part 1? It seems like an important addition. It would be good to mention it here.

The difference between the “Part 1” and “Part 2” papers is the fact that the RF is estimated in Part 1 and the HR is estimated in Part 2, two very different radiative parameters with completely different implications (RF: climatic effects and energy balance; HR: localized heating or cooling and vertical dynamics). The LW is included in “Part 2” paper because this spectral region is much more important for HR than RF for smoke aerosols. The inclusion and importance of the LW is already mentioned at L67-68.

SC9) L70-72 The description of the sections is cut-and-pasted from the Part 1 paper, and does not properly reflect this paper.

This is just a quick presentation of the paper’s structure, which is not dissimilar from “Part 1”. We have slightly modified so that it reflects more the present “Part 2” paper’s structure (we had mistakenly used “RF” instead of “HR”).

(Data and Methods, radiative transfer)

SC10) L83 In Figure 1 or in the discussion of it, it would be helpful to specify what ALL of the required inputs for the radiative transfer model are. That is, spell out which "non-measured aerosol optical properties" are needed. Also, is the extinction an altitude-dependent profile of extinction, or a layer-integrated AOD? Also, are the layer height and depth needed? What about surface albedo? Solar geometry?

Most of the information requested by the Reviewer is already discussed in the manuscript. The non-measured optical properties used in our calculations are the SSA (single scattering albedo) and g (asymmetry parameter, representative of the scattering phase function). This is already discussed in detail at L95-118, for the hemispheric-scale runs, and is recalled at L128-131, for the in-vortex run. As discussed especially at L95-118 but also at L278-280 in reference to Eq. 3 (formerly Eq. 1) and in S22 for the RF, these are the only aerosol optical parameters needed to estimate radiative effects, besides the aerosol extinction. For the extinction itself, it is stated in the text: “…using monthly average OMPS-LP aerosols extinction coefficient PROFILES at 675 nm”, for the hemispheric-scale runs (L97), and “we use HIGH VERTICAL RESOLUTION aerosol extinction PROFILE observations with the satellite-borne LiDAR CALIPSO-CALIOP” (we added “profiles”), for the in-vortex runs (L126-127). Thus, 1) they are profiles and 2) layer height/depth is then explicitly included through them. Surface albedo (SW) and emissivity (LW) was not mentioned in the previous manuscript version and this information is now added at L136-137. Radiative calculations are realized at different solar elevations and equinox-equivalent daily average HR are then calculated, as stated at L137-139.

SC11) L120 This discussion of the "clean background" is confusing in multiple ways. First, there would be some biomass burning in the same months in any year, so this should not be called "clean background". In fact, the authors discuss this in part 1, but that discussion isn't repeated in part 2, so the confusion occurs all over again. Better to use a different phrase than "clean background". In fact, the authors aren't attempting to calculate a forcing with respect to an aerosol free atmosphere, only an "anamolous forcing" due to this particular event, correct?

Yes, this is correct – we just removed “clean” in the revised manuscript. As it is probably not optimal to duplicate in “Part 2” paper the discussion of “Part 1”, we just added a mention to this latter paper.

SC12) L120 (continued) But it's also confusing that there's no further mention of the "background" conditions in section 4. Why doesn't it appear in the figures or equations? Is the background even relevant to this paper?

It is. As it is mentioned at that text line ("The plume's HR is obtained by subtracting the HR results of a background atmosphere from the HR outputs of the fire-perturbed scenario…"), the HR shown in Figs. 4-6 and Tab. 2, as well all results in the paper, are obtained by subtracting out the HR results of a background atmosphere from the HR outputs of the fire-perturbed scenario. Please also note that an example of background extinction profile is shown in Fig. 2c (dashed line, see also figure's caption: "…and February 2019 (background profiles, dashed lines…)").

SC13) L129 The CALIPSO products lidar ratio, color ratio and depolarization ratio don't really give quantitative constraints on SSA and g. I suggest rewording this to reflect the qualitative nature of the relationship, perhaps something like "Assumptions on SSA and g are aided by inferences of aerosol composition supported by CALIPSO products lidar ratio, colour ratio, and depolarisation ratio".

Yes, thank you, we modified the text accordingly.

SC14) L139 somewhere in this section I would expect to see a discussion of what underlying albedo is used in the calculations.

Please see our reply to comment SC10.

(Data and Methods, limb measurements)

SC15) L152 "spatial coverage". Do the authors mean "spatial resolution"? That is, I think SAGE III covers the same latitude bands (or else it would not be able to provide a useful Angstrom exponent) but the resolution is too coarse to be useful for the study of details.

We mean the number and frequency of solar occultation (SAGEIII) versus limb measurements (OMPS-LP). In effect, the best is probably to use "spatial sampling" (changed in the revised text to "…with a much sparser spatial sampling").

(Data and Methods, CALIPSO)

SC16) L159-162 This description of the aerosol optical depth and lidar ratio from CALIPSO is very superficial and I'm left with many, many questions about the assumptions and uncertainties, whether it's done correctly, and even why an alternate AOD and lidar ratio calculation is done at all. Adding lidar measurements is the primary new component of the methodology compared to Part 1, so it's strange that the section is so abbreviated. The authors should fill in this section with enough information about the methodology to allow for reproducibility.

Agreed and done. An additional and detailed description of the method is included in the revised manuscript. It provides all specific equations used to process LiDAR signals for calculating the AOD and LiDAR ratios. This method is a classical retrieval approach using Platt's equation (1973) for the specific case of aerosols layers bounded by altitudes without aerosol and clouds. The equations used in the method are simple straight-forward manipulations of the classic LiDAR equation (Fernald 1984) as done by Platt (1973). We also acknowledge previous work using the same transmittance constraint (Young, 1995; Omar et al., 2010; Cook et al., 1972; Prata et al., 2017). The advantage of this method is that it does not require any assumption of the aerosol optical properties, particularly the lidar ratio. This is particularly suited in our case, where the analysed aerosol layers are very specific ones (aerosols emitted by fires and reaching exceptionally high atmospheric altitudes after a long period of long-range transport) and pretty much unknown. Moreover, it does not rely on an automatic detection of the aerosol layer boundaries or the particle type.

SC17) CALIPSO standard products include layer AOD and extinction (see the CALIPSO Data Product Description https://www-calipso.larc.nasa.gov/resources/calipso_users_guide/data_desc/cal_lid_l2_layer_v4-51_desc.php), as well as type identification and lidar ratio. The algorithms for producing AOD, extinction, and lidar ratio include a constrained transmittance retrieval similar to the one described here, for specific cases when the aerosol layers have the required aerosol-free air above and below them, and otherwise uses lidar ratios modeled for the inferred aerosol type (Young, S. A., and M. A. Vaughan (2009), The Retrieval of Profiles of Particulate Extinction from Cloud-Aerosol Lidar Infrared Pathfinder Satellite Observations (CALIPSO) Data: Algorithm Description, J Atmos Ocean Tech, 26(6), 1105-1119. doi: doi:10.1175/2008JTECHA1221.1.). The manuscript does not say very clearly whether the CALIPSO standard products are used, but it seems not. Why isn't the standard CALIPSO AOD product used? The motivation for discarding this and using an alternative algorithm should be made clear. Also, if the authors have made their own L2 retrieval for whatever reason, then this section should include the full methodology including equations or a prior published article that uses the same methodology. Other papers that have used similar calculations should probably also be acknowledged (e.g. Prata, A. T., S. A. Young, S. T. Siems, and M. J. Manton (2017), Lidar ratios of stratospheric volcanic ash and sulfate aerosols retrieved from CALIOP measurements, Atmos. Chem. Phys., 17(13), 8599-8618. doi: 10.5194/acp-17-8599-2017). More specifically, are you using Platt's equation? (Platt, C. M. R. (1973), Lidar and Radiometric Observations of Cirrus Clouds, Journal of Atmospheric Sciences, 30(6), 1191-1204. doi: https://doi.org/10.1175/1520-0469(1973)030<1191:LAROOC>2.0.CO;2.) How do you subtract the molecular backscattering, and what assumptions are involved in that? Do you account for multiple scattering?

Clarified. All these aspects are clarified in the revised manuscript. We only use the standard L1 CALIPSO product of attenuated backscatter profile and we do process it ourselves using the standard two-way transmittance methods (as similarly done by Prata et al., 2017; Young, 1995; Omar et al., 2010; Cook et al., 1972). We have acknowledged this previous work. The reason for not using the operational standard product is to choose ourselves the aerosol-free altitudes and avoid relying on automatic detection; as well as avoiding the assumption of lidar ratios based on aerosol type detections. The case analyzed in the paper is quite specific, of high altitude dense aerosol layers of smoke aerosols after significant long-range transport. For example, the standard CALIPSO operational product v4.51 often classified these particle layer as ice clouds, as for the example of Figure 3 in the paper.

(The plume and the vortex)

SC18) L208, Consider writing in which direction the vortex is traveling.

Information added.

SC19) L208, I don't understand "the frontal structure". Can this be explained a bit more?

I think the confusion originates from the fact that we did not specify that the "front" is to be considered in terms of the vertical distribution of the ascending aerosols. We corrected this in the revised manuscript. Both the higher-altitude frontal shape and the leaking lower section are extensively described by Podglajen et al. (2023) (the reference is updated in the revised manuscript; we added the preprint DOI and web link).

SC20) L218. The lidar results quoted for smoke are for smoke in the troposphere. There is ample literature of lidar observations of smoke in the stratosphere to suggest that it is very commonly much more depolarizing. Consider the following references:

Burton, S. P., et al. (2015), Observations of the spectral dependence of linear particle depolarization ratio of aerosols using NASA Langley airborne High Spectral Resolution Lidar, Atmos. Chem. Phys., 15(23), 13453-13473. doi: 10.5194/acp-15-13453-2015.

Haarig, M., A. Ansmann, H. Baars, C. Jimenez, I. Veselovskii, R. Engelmann, and D. Althausen (2018), Depolarization and lidar ratios at 355, 532, and 1064 nm and microphysical properties of aged tropospheric and stratospheric Canadian wildfire smoke, Atmos. Chem. Phys., 18(16), 11847-11861. doi: 10.5194/acp-18-11847-2018.

Hu, Q., et al. (2019), Long-range-transported Canadian smoke plumes in the lower stratosphere over northern France, Atmos. Chem. Phys., 19(2), 1173-1193. doi: 10.5194/acp-19-1173-2019.

Sicard, M., Granados-Muñoz, M. J., Alados-Arboledas, L., Barragán, R., Bedoya-Velásquez, A. E., Benavent-Oltra, J. A., Bortoli, D., Comerón, A., Córdoba-Jabonero, C., Costa, M. J., del Águila, A., Fernández, A. J., Guerrero-Rascado, J. L., Jorba, O., Molero, F., Muñoz-Porcar, C., Ortiz-Amezcua, P., Papagiannopoulos, N., Potes, M., Pujadas, M., Rocadenbosch, F., Rodríguez-Gómez, A., Román, R., Salgado, R., Salgueiro, V., Sola, Y., and Yela, M.: Ground/space, passive/active remote sensing observations coupled with particle dispersion modelling to understand the inter-continental transport of wildfire smoke plumes, Remote Sens Environ, 232, 111294, https://doi.org/10.1016/j.rse.2019.111294, 2019.

The basic idea is that aged smoke plumes are expected to be less depolarising than fresh plumes, which is true in both the troposphere and stratosphere, as discussed in e.g. Haarig et al. 2018 (mentioned in the comment). This is due to the fact that fresh plumes contain more irregularly shaped dry particles, while aged plumes contain more spherical particles due to progressive hydration and mixing with secondary (liquid) carbonaceous or sulphate aerosols (see also discussion in S22). In Haaring et al. (2018) and Hu et al (2019) (now cited in the text), plumes from Canadian wildfires in 2017 are observed about 1-2 weeks after the main pyroCb injection events, while the hemispheric dispersion of the Australian fires plumes described in our paper occurs over several months. At the timescales of the transport of the papers cited above, we are inclined to consider the observed plumes as relatively "fresh", thus with larger depolarization ratios, especially at the dry conditions of the stratosphere. The Australian fires 2019-2020 themselves emitted much more water vapour than the Canadian fires 2017, thus facilitating hydration of the smoke particles and formation of secondary aerosols. In any case, it is clear that ageing is slower in the stratosphere than the troposphere, as shown by Sicard et al. (2019) and this aspect is mentioned in the revised text.

SC21) L220 Note that color ratio for attenuated aerosol backscatter is not the same quantity as the color ratio of (unattenuated) aerosol backscatter. In particular, if there is a strong

spectral dependence of the absorption, 532 nm will be more strongly attenuated than 1064 nm and that will impact the color ratio. (Liu, Z., et al. (2019), Discriminating between clouds and aerosols in the CALIOP version 4.1 data products, Atmos. Meas. Tech., 12(1), 703-734. doi: 10.5194/amt-12-703-2019)

Agreed and clarified. We have clarified this aspect, indicated it is the attenuated colour ratio we estimate in our work.

SC22) L221-223 This explanation for the large depolarization ratio is different from other published hypotheses of why stratospheric smoke aerosol tends to be depolarizing, see especially Haarig et al. 2018 and Sicard et al. 2019 given above, as well as Papanikolaou et al. 2022. There should be additional discussion about the various hypotheses to explain why the authors prefer the ash hypothesis.

As discussed in SC20, the plumes shown by Haarig et al. (2018) and Sicard et al. (2019) are not to be considered, in our opinion, "aged" smoke plume, being measured only 1-2 weeks since the pyroCb event for Canadian fires 2017. We feel that it is not easy to exclude, for those observations, that irregularly shaped particles are still present in the stratosphere (wrt e.g. Fig. 3e versus 2e in Haarig et al.) We also appreciate the attempt to explain this behaviour of the depolarization ratio of Haarig et al. (see their Sect. 4.1) and the fact that the authors keep the question open for further future investigation through e.g. RT models including polarisation information and a LiDAR model: we support this idea and encourage future works in this direction. Based on these considerations, we don't feel that any hypothesis should be excluded, and we don't have the tools to sort this out in the present study. We added a mention to the hypothesis of Haarig et al. in the revised manuscript.

SC23) L231 (Figure 3 caption) What altitude range is represented by the ECMWF-IFS images?

We added the information in the caption "…(at 464 K isentropic level, about 18 km altitude)…".

SC24) Table 1. Define the colour ratio. I think you are using the opposite ratio to the CALIPSO product definition, which is fine, but it's not stated. Is it 532/1064 nm or 1064/532 nm? Also specify, is it colour ratio of particulate backscatter or attenuated backscatter?

Information added in Sect. 2.3.

SC25) L253-254. This comment that it is not known how the HR depends on the optical properties is a bit odd with no followup discussion, considering the authors immediately go into a description of how they calculate the heating rate from the optical properties. What parts are uncertain? Are there aspects of the calculations used here that are uncertain? What assumptions and uncertainties are associated with those calculations?

There simply were no such sensitivity analyses as the ones we do in the present paper, so the need for our study. We discuss the HR dependence on the SW and LW optical properties, in the specific case of wildfire aerosols, in the following sections (discussion of Figs. 4-6: L257-285 for Fig. 4, L282-316 for Fig. 5, L337-352 for Fig. 6 i.e. three scenarios of possible optical properties).

SC26) L256. Earlier a "background" condition was mentioned. How does this fit into the equation?

Please refer to SC12. In our RT model, HR are estimated for both fire-perturbed and background conditions, then the background HR is subtracted out from the fire-perturbed runs. As the Eq. 3 (formerly Eq. 1) is only illustrative of the different dependences, we are inclined to not modify the text to express this again in this part of the manuscript but please let us know if you prefer that we mention this here.

SC27) L259. What value is used for the underlying albedo? The full CALIPSO curtain (which is shown here only clipped) shows clouds at various altitudes below the plume. In the Part 1 paper by the same authors, they demonstrate that bright clouds underlying the aerosol layer can have a very large impact on the radiative calculations, so how are the clouds accounted for?

See SC4 and SC10.

SC28) L267. The quoted number is specific to SSA = 0.8, so I suggest replacing "for smaller SSA... absorbing aerosols" with "for the most absorbing aerosols with SSA = 0.8"

OK, done.

SC29) L269. Can the authors give any further insight relating to the simultaneous heating around 35 km?

At this stage, we don't have any hypothesis to give for this heating at higher altitudes.

SC30) L273. Clarify in the text what altitude range the quoted values refer to.

This information was added in the revised manuscript.

SC31) L275. Instead of averaging over SSA and g, it would be better to give the minimum and maximum results. Since the different SSA and g inputs are not a statistical representation the aerosol but rather a grid of possibilities, an average doesn't have a lot of scientific meaning. A range of input was used because the values are unknown and a range better represents the state of knowledge. The same should go for the output.

The variability of the HR as function of the different hypotheses is clearly visible in Fig. 4.

SC32) L276. "The radiative heating profiles are averaged in the altitude range 12-25 km". This is from the Figure 5 caption text. Please put it in the main text also.

Added.

SC33) L278. Again I don't think it's a good idea to average over the different SSA and g values. (To clarify, I have no concern about averaging over the altitude range or the weighted average used to combine the latitude bands. It's only the average over the assumed SSA and g that is problematic, because the average removes important information about the range of results obtained and is too easily mistaken for a "best estimate").

The range of daily average HR results as a function of SSA and g assumptions (which are all plausible) is clearly visible in Fig. 5, so we don't see a need to discuss this further in the text – this would just make the paper lengthier and, possibly, less clear.

SC34) L280. Similarly, when the results are given quantitatively (here and in the conclusions and abstract), please give the min and max instead of the standard deviation. The standard deviation is not a good representation of the distribution of possible results, since the 4 or

8 results are clearly not normally distributed, and the inputs were not any kind of statistical representation either. The min and max will unambigously and correctly describe your findings. Also, in the figures, please make the vertical bars represent the min and max.

As mentioned in comment SC33 and discussed in the text, all the SSA and g assumptions are plausible and, in addition, the variability of the HR with the different SSA and g assumption are clearly visible in Fig. 5. Thus, we think that providing an average has a marked interest for the readers. This has been done for S22 and we also think it informative to keep the RF and the HR estimations consistent, in terms of the aggregation of the results for different optical hypotheses. Changing this would remove consistency between "Part 1" and "Part 2" papers. We agree that the variability is also important, and it is actually mentioned it in the revised manuscript (see SC37). We added this information in the Abstract and Conclusions as well.

SC35) L282. "see the error bars". The error bar for the LW is so small it can't be seen. Please include in the text a quantitative description of the spread in the LW, to distinguish it from zero.

Information added into the revised manuscript text.

SC37) L284. "variability... between 0.02 K/d (SSA=0.95)... and 0.12 K/d (SSA=0.8)". Actually the variability is larger than that, and these values of heating rate are not the values for the specific SSAs given in parentheses. The full range of variability should be used here, with the actual values for the specified SSAs, not the standard deviation, which has very little meaning in this case.

That's true, we have corrected the text here.

SC38) L285. "The net SW+LW HR is consistent with ... [a plume rise] of a few km in 4 months (see Fig 2a... of Yu et al. 2021). I'm not following how this consistency is established. I see at least the weaker conclusion that the authors have successfully found a combination resulting in a positive heating rate which they argue is required to explain any amount of plume rise. The reference to the Yu et al. figure seems to be only to show that there is observational evidence that the plume did rise by that amount (which is shown in Figure 2 in this paper anyway), but I don't see that it shows a relationship between plume rise rate and heating rate that can be used to establish any kind of claim of quantitative consistency. If I'm missing something, please explain it in more detail in the text. Otherwise, please soften the statement to remove any implication that a quantitative consistency has been demonstrated. (Here and in the conclusion and abstract).

See SC3.

SC39) L287. "consistent ... with Heingold et al. (2022) even if slightly larger". Please explain this apparent contradictory statement in more detail. What are the specific values from this work and from Heingold et al. that are being compared, and if there is a discrepancy, what's the likely explanation for it that makes them nevertheless consistent?

We have added in the text of the revised manuscript the specific section in Heinold at al. where these results are shown and discussed.

SC40) L287. Also how do the calculations in this study compare to heating rates given by Wu et al. (2022)?

See SC6.

SC41) Figure 4. If the background heating rate is used, it should be shown in Figure 4 also.

We don't see any reason to show the background HR, especially in such a figure (which is already carrying a lot of information – adding the background HR would just make the figure less readable). In Fig. 4, the specific HR due to the Australian fires aerosol (so with the background HR subtracted out from the January-April 2020 HR) is shown, which is all we need to answer the scientific questions addressed in the manuscript.

(Vortex calculations)

SC42) L309. I don't understand the motivation for having different input models in section 4.1 and 4.2. That is, why does 4.2 use 3 prespecified models of SW and LW SSA and g, wherease 4.1 performed a full sensitivity study using all of the combinations? Can this be clarified in the text, please?

There are at least three reasons to carry out a full sensitivity analysis for the 4-months dispersion of the overall plume at the hemispheric scale only. A) The inherent phenomenology of the hemispheric plume is very different with respect to the isolated vortices. First, the temporal scales involved are very different (4 months for the hemispheric dispersion of the overall plume and from a few days to a couple of weeks for the main vortex). Second, the smoke aerosols are isolated from the ambient air when within the anticyclonic vortices. Longer timescales and the interaction with ambient air make the large-scale plume more free to evolve in terms of optical properties. B) The study of the overall plume at the hemispheric scale appears before in the manuscript then the study of the vortex. Once the variability of the HR in terms of the (evolving) optical properties of the smoke aerosols is established (Sect. 4.1) we can just focus on extreme situations (very absorbing and small particles versus less absorbing and larger particles, e.g., as done in Sects. 4.2) without losing generality. C) The results obtained in Sect. 4.2 demonstrate that only one specific aerosol optical scenario is consistent with the observed vortex lofting (and with lofting in general) so, in our opinion, there is no need to include the whole possible SSA and g variability in Sect. 4.2 studies, as done in Sect. 4.1. As a matter of fact, the study for the isolated vortex is made with a sufficient subset of the hypotheses made for the overall large-scale plume. We think that the reasons behind this choice (especially reason C) are already clear from the text and we avoid, at this stage, to add more discussion, which we think redundant. Please don't hesitate to ask us to expand this in the text in case you think it necessary.

SC43) L311. The definitions of the aerosol models should be included in the text, not just the figure caption, please

We are really committed to a short, synthetic paper – even if complete of all details, of course. The three aerosol scenarios are specified in the caption and also il S22, as clearly stated in the text. We feel that including this in the text is redundant.

SC44) L326 and L327. Ranges are given for two of the inputs (SW g for black carbon and SW SSA for brown carbon) but that conflicts with there being only a single line for SW heating rate for each aerosol model. The specific values that are used in the test should be the ones given in the caption.

In case multiple values of optical properties are used, single lines in Fig. 6 are averages of the HR profiles with these optical properties. Please see S22 for details.

(Conclusions)

SC45) L351. See earlier comment about "consistency" between heating rates and observed plume rise rates.

See comment SC3.

Technical comments:

TC1) L17 suggest reversing the two features in the sentence, so you're isolating the vortex from the dominant plume.

TC2) L21 and L266 suggest replacing "and then" with "and therefore" or "and thereby". (To avoid the ambiguity of "then" referring to a time-based relationship)

TC3) L45 suggest replacing "reconciliate" with "reconcile"

TC4) L56 suggest replacing "maintain" with "persistence"

TC5) L56 "attributes" should be replaced with "attributed"

TC6) L92 suggest deleting or replacing "at the basis", since it's a vague phrase that doesn't really convey anything (also in this particular case, it's ambiguously similar to "at the bases" which is not what the authors meant but would be valid grammatically, so it's especially confusing). "diabatic heating of the compact anticyclonic vortices" seems sufficient. Or if not, then I suggest replacing it with "due to".

TC7) L128 "discusses" should be "discussed"

TC8) L129 "constrains" should be "constraints"

TC9) L173 and L192 suggest replacing "double" with "dual"

TC10) L174 "compacts" should be "compact"

TC11) L193 suggest replacing "mutual" with "relative"

TC12) L226 suggest replacing "these latter" with "the vortices" for maximum clarity.

TC13) L231 I don't understand what the authors mean by "individuated" here (Figure caption). Please replace with another word or phrase.

TC14) L233 Table 1 caption. suggest replacing "individuated" with "specified".

TC15) L237 Suggest spelling out "heating rates" in the section title, for the convenience of readers who may read the section headers before reading all the text in detail.

TC16) L244 Suggest "we represent cooling rates as negative HR" to replace the phrase beginning "we use the idea of..."

TC17) L281 suggest replacing "the evidence discussed" with what it refers to, perhaps "Reflecting the lack of variability in SSA in the LW and the lack of sensitivity to changes in g inferred from Mie studies as discussed above"

TC18) Figure 4. The caption suggests that there are both dashed and dotted lines, but I can only see one line pattern.

All TCs have been addressed.

Reviewer #2

General comments

In this new study, Sellitto et al. present OMPS and CALIOP aerosol satellite observations and associated short- and longwave radiative transfer calculations to estimate the hemispheric and in-vortex heating rates of the 2019-2020 southeastern Australian bushfires. The study finds a strong dependence of the radiative transfer calculations and heating rate estimates on aerosol type and microphysical properties (single scattering albedo and asymmetry parameter). Heating rates estimated for small-sized, strongly absorbing black carbon particles are found to be consistent with the observed self-lofting of the wildfire plume on global and local scale. The important role of longwave emissions on the heating rate estimates is particularly emphasized.

Overall, the paper is well-written, clear and concise. I found it interesting to read and think it fits well within the scope of the journal. The study is scientifically sound and the results seem plausible to me. I would recommend that the paper be considered for publication, subject to a few minor comments as listed below.

We would like to thank the Anonymous Reviewer #2 for her/his kind words and the very useful comments. Please find our replies to the Specific Comments (SC) in the following.

Specific comments

SC1) l62-68: In the introduction (and/or the conclusions), it would be good to discuss the broader implications and relevance of the study a bit more. Specific estimates of heating rates for the 2019-2020 Australian event are provided, but are they relevant overall? How do they compare with other events? Are the results relevant to chemistry transport or climate modeling?

We have added some elements in this direction in the Conclusions (see L369-370 and L383-388).

SC2) l167-170: It might be good to add another 1-2 sentences regarding the ECMWF-IFS derived data sets for vortex tracking. Was this a specially generated data product/simulation or is it based on common IFS forecasts/operational analysis?

It is a standard IFS analysis, as described in detail in Khaykin et al., 2020. We mention this paper as a reference for IFS more clearly, in the revised manuscript.

SC3) Figures 4 and 5: For these figures, I have trouble relating the individual curves to the individual aerosol properties listed in the caption. The caption refers to dotted lines, but I don't see any dotted lines in the plots? Also, I see only one sky blue line and three dark blue/black lines, but no medium blue line in my printout. The orange line in Fig. 5 is barely visible, and different types of orange dots are not found.

For the dotted lines, please note that they are mostly superposed to the dashed lines, reflecting the very weak dependence of the HR on g (information added in the caption of Fig. 4). We slightly modified colours (orange is now darker and medium blue is now clearer) so that they are more visible.

SC4) l334: The text refers to the impact of the plume on the UTLS region, but it seems that the radiative effects are mostly confined to the lower stratosphere (18-23 km altitude), but not to the upper troposphere?

This is only true for the in-vortex impacts (e.g. Fig. 3). The large-scale plume effects extend down to 11 km (see Fig. 4).

Technical corrections

TC1) l40: about than ten times -> about ten times

TC2) l50-51: it seems the units (W/m^2) for the radiative forcing are missing?

TC3) l243: can in turns -> can in turn

TC4) l243: the vertical dynamics _of the_ lofting or sinking

All TCs have been addressed.