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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Smoke-charged vortices in the stratosphere generated by wildfires and their behaviour in both hemispheres: comparing Australia 2020 to Canada 2017" *by* Hugo Lestrelin et al.

Hugo Lestrelin et al.

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Answer to reviewer #1

General comments

This paper details the dynamics of a newly-discovered phenomenon associated with large pyroCb smoke plumes in the stratosphere, namely the self-organized anticyclonic vortices that are formed due to absorption of solar radiation by black carbon within the plumes. A comparison is made between the 2019-2020 Australian plumes and several plumes associated with large Canadian fires in 2017. Detailed analysis of the Canadian plumes use the Lait Potential Vorticity (PV) from ERA5 to track the vortex evolution and to examine the composite dynamical structures of the vortex. ERA5 ozone is also used to successfully track the vortex locations. Composites of PV, temperature, and ozone help to further understand these features. The paper also details how these features are maintained in the analyses by assimilation of temperature and ozone data, and it examines various dynamical indices to test whether the vortices are in balance and/or inertially unstable. This paper provides an excellent addition to the study of smoke-induced dynamics.

We appreciate the evaluation of the reviewer and his numerous detailed comments.

Specific comments

- *Line 10: You use the term "low absolute potential vorticity" here. Just to be clear, does this mean low magnitude (i.e., absolute value) of the potential vorticity?*

  We mean small absolute vorticity and small potential vorticity as well.

- *Line 83: I don't think "g is the free-fall acceleration" is necessary, since "g" isn't in Eq. (1).*

We had omitted the required constant $-g$ factor in the definition. It is now restored.

- *Lines 112-114: Quoting from the paper, "we used both Π and the ozone anomaly defined as the deviation with respect to the mean at the same latitude and altitude." So are both Π and ozone defined using the anomaly with respect to the zonal mean, or is just ozone calculated as the anomaly, while Π is the raw value from Eq. (2)? This sentence could be read either way.*

  Only ozone is defined using the anomaly with respect to the zonal mean. The sentence has been modified.

- *Lines 112-114: As mentioned by the authors, the commonly used PV has a disadvantage of large background vertical gradient. While Khaykin et al. (2020) used relative vorticity, Kablick et al. (2020) used the "regular" PV to analyze the 2019-2020 Australian smoke plumes. They used the PV anomaly relative to the zonal mean in units of percent of the absolute value of the zonal mean PV. This alternate approach also reduces the influence of the large background gradient.*

  We agree there are several ways in order to remove the background gradient in the display of the vertical structure. The Lait PV has the advantage to remain an adiabatic invariant of motion.

- *Line 139-142: You talk about the "kernel of almost zero PV and low ozone...." To visualize this statement, it would be interesting to see latitude/altitude cross-sections of PV and ozone along the CALIPSO track to compare with Figures 1 and 2.*

  The latitude/altitude and longitude/altitude sections are displayed in the animation of vortex O in the supplement and are indeed very instructive. They are centered on the IFS vortex to provide continuity. Doing the same with vortices A, B1 and B2 brings less additional information. Figure 1 and 2 show the location of the vortex

center according to the PV and ozone tracking. As these centers are mimima in the 3D domain, surrounding contours can be plotted but this would totally obscure the aerosol pattern on many panels.

- *Line 147 (also Lines 158 and 394) : I am unfamiliar with the term "thalweg". Are you referring to a trough? Could you provide some dynamical field on the maps to indicate where this is occurring to help visualize the point you're making?*

  We mean trough and this has been corrected. Montgomery potential contours have been added to all the isentropic charts of the animations.

- *Line 194-195: Is the statement "it differs from the 2020 case where such effect is not observed for any of the three vortices" referring to results in Khaykin et al. (2020)?*

  Yes. The sentence has been improved.

- *Section 3.5. This section provides a helpful reference of other papers that have studied this event. The PV anomaly associated with smoke during the Canadian event was also examined in a recent paper by Allen et al. (2020). While that paper focused primarily on the "Koobor" vortex, they also included a PV anomaly map for 28 August 2017 associated with what this paper calls "Vortex A" (see Figure 16 of the following reference). Allen, D. R., M. D. Fromm, G. P. Kablick III, and G. E. Nedoluha, 2020: Smoke With Induced Rotation and Lofting (SWIRL) in the Stratosphere, J. Atmos. Sci., 77, 4297-4316, https://doi.org/10.1175/JAS-D-20-0131.1.*

  We missed this paper which was published at a time close to our submission. It is now referred and commented. Indeed the structure shown in the figure 16 of this work displays the vortex A on 28 August but the structure provided by MERRA-2 is apparently much less compact than in the ERA5, perhaps due to a lower spatial resolution in the vertical and horizontal directions. The elongated vortex O is also visible in several panels of this figure.

- *Line 243-245: It is interesting that the 2017 case doesn't show the temperature dipole. Is that simply due to the contours chosen (i.e., the warm anomaly is really there, but it is less than 1.0 K)?*

  There is indeed a warm anomaly of weaker amplitude which is actually responsible for the cooling pattern. A new version of the figure which shows the warm anomaly contours is now provided.

- *Line 252: It is unclear exactly how you calculate the horizontal length scale L_h. The text says it is defined as "the diameter of the ring of wind speed maximum". Do you calculate the diameter from the wind speed explicitly for each case? If so, does the wind speed calculation involve removing the background wind in order to focus on the wind associated with the anticyclone?*

  We estimate the length explicitly for each case along the East-West direction (i.e. between local extrema of the meridional velocity). The background wind is not removed in the calculation, since by definition its variations at the scale of the vortex are small so that they do not affect local extrema.

- *Line 256: Similarly, L_z is "the vertical extent of the vorticity contour at maximum wind speed". Is this calculated explicitly? Would it be possible to include some more details on this calculation?*

  Yes. This is now explained with more details.

- *Line 257: You say "the 2017 vortex A being about 8 times smaller in volume than its gigantic 2020 counterpart". This difference seems too large. If the cloud is considered as a cylinder, then volume is $V = \pi(L_h/2)^2 L_z$. Using numbers from Table 1 we get $V_A = \pi(686/2)^2 3.5 = 1.3 \times 10^6$km^3, and $V_{Koobor} = \pi(784/2)^2 6.1 = 2.9 \times 10^6$km^3. So Koobor is only 2.9/1.3=2.2 times larger in volume than vortex A. Are these estimates correct, or do you use another method to estimate volume?*

  You are correct, thank you for pointing this out. We had previously estimated the volume of the vortices using the same value of vorticity threshold for both cases, but it is more consistent to adapt the value to each case. This has now been corrected.

- *Line 263: You may want to define the condition for inertial instability here.*

  The criterion of vanishing PV is actually formally related to symmetric instability (Hoskins, 1974) but, here, we see that the absolute vorticity (not shown) also vanishes, therefore the symmetric instability is an inertial instability.

- *Line 328-330: It looks like the dates used for the Australian vortex are 2-27 February in Figure 8 (mean structure), Figure 9 (heating rate, temperature and vorticity tendencies), and Figure 11 (ozone tendencies), but for Figure 10 (PV increments) the dates are 7-19 January. It there a particular reason that different dates are chosen for PV? Are the mean composites and increments of PV much different if you calculate them for the different periods? Also, are the green lines on Figure 10 from 7-19 January or 2-27 February?*

  There was a mistake here, thank you for spotting it. We have replaced the figure in the main body of the paper by the correct time period, while the 7-19 January period figure has been moved to the appendix. Note that the green contours always correspond to the vorticity in the time period indicated.

  The reason for showing the January periods is to emphasize that the LPV increment dipole does not tilt along the direction of the background wind (which corresponds to the drift of the vortex) but rather along the direction of the wind shear, a property which might be found counter intuitive. We follow the reviewers advice and now show this feature in the appendix.

  The composite anomaly and increment structures are qualitatively consistent between different periods, but there can be quantitative differences, some of those related to the composite are reported in Tab. 1 of the paper. Regarding the increments, there are differences associated for instance with changes in the ascent rate of the vortex in altitude coordinate which is larger, for Koobor, in January (250 m/day) than in February (150 m/day). This is mentioned in the revised paper together with the new figure in the appendix. The choice of the February time period fr the main body of the paper is motivated by the relative steadiness of the structure during that month (see Fig. 7b of Khaykin et al. (2020), which shows a rather constant relative vorticity maximum).

- *Line 335: The northwest-southeast tilt of the PV increments for the Koobor vortex, shown in Figure 10a, is interesting. In the recent analysis by Allen et al. (2020), they examined how Koobor tilts with height and found a NW-SE tilt of the vortex in January. They used a dynamical argument to show how this tilt may develop from internal vortex dynamics in a shear flow. The PV increments shown in this paper appear to support this observed dynamical structure. Also, would the same argument you make in Appendix B apply to the ozone structure seen in Figure 11?*

  The argument of Allen et al. (2020) concerns the structure of the vortex and does not apply to the increments, although they are both related to the vertical shear. We now mention their explanation. Vacillations of vortices in a shear flow constitute a fairly standard topic which has been studied a lot, included by the authors in 2D (Legras et al., 2001) and, in 3D, by David Dritschel and collaborators (e.g., Tsang and Dritschel, 2015). There are also a large number of works in the oceanographic literature. Therefore Koobor and its siblings were expected to follow the general laws and to undergo vacillation, erosion and splitting when submitted to a shear. Such vacillations are obvious in Fig. 6b of Khaykin et al. (2020). Koobor went through a first splitting by 25 February 2020, which was actually well predicted by the ECMWF opertaional model a few days in advance, and a second one by 30 March. For the Canadian case, the deformation of vortex

  O in the trough led to its splitting in at least three parts that we followed.

  Our reasoning in appendix B applies to the ozone increment (or the increment or any quantity organized as a monopole).

- *Line 344: Does "low absolute PV" mean low magnitude (i.e., low absolute value of PV)?*

  Yes. The model provides PV and absolute vorticity values close to 0 or even slightly negative. The contrast is such with the ambient PV that the practical situation is that of a ball of zero PV immersed into the stratosphere. Such contrast is made possible by the vertical motion. The familiar formation of cut-off lows or cut-off highs by horizontal isentropic motion never reaches such a situation.

- *Line 408: May want to define terms explicitly in the text here, particularly W and Λ. I assume these are vertical wind and vertical shear of the zonal wind. They are indirectly defined in the Table B1, but not in the text.*

  Done.

- *Line 416: How is the wind shear estimate calculated here (i.e., what time range is used)?*

  The time range of the composite, this is now specified.

- *Figure 3 caption: Could you include in the caption what time of day was used for the PV analyses?*

  Done

- *Figure S4: Is this calculated with normal PV or with the Lait PV? Also, as a reference, it would be useful to include the zonal mean PV on this plot. This should become increasingly negative with time as the parcel ascends. Do the Canadian plumes show a similar behavior?*

This is the standard PV. The anomaly is defined with respect to the zonal mean PV at same altitude. Therefore this later is the difference between the blue and the orange curves. The initial stage of vortex O exhibits a similar behaviour in 2017. After its split in 3 parts, the offspring exhibit non zero absolute vorticity at the core. As such events usually generate only a small amount of in-mixing, it is unclear whether the effect is real or due to the limitation of assimilation.

- *I enjoyed the animations in the supplementary material. I assume the PV used in the animations is the normal PV, not the Lait PV, right?*

  Yes

- *Lastly, there are quite a few different names used in this paper for different aspects of this new phenomenon. For example the terms "smoke-charged vortex", "smoke charged pancake vortex", "smoke vortex", "smoke plume", "smoke bubble", and just "bubble" could possibly be condensed into fewer descriptions. The Australian plumes are called "Koobor", "2nd Vortex", and "3rd Vortex", while the Canadian plumes are "Vortex O" (also called "mother vortex"), "Vortex A", "Vortex B1", and "Vortex B2". Different terms are also used for "Koobor", such as "main vortex", and "major vortex". Given this is such a new discovery, to avoid potential confusion, terms could be consolidated and defined (e.g., how does the term "bubble" differ from the terms "plume" and "vortex"). Looking forward, do you have any recommendations for a general scheme as to how these events can be categorized, in order to separate them from stratospheric smoke plumes that do not show a dynamical signature? Allen et al. (2020) coined a new term for this phenomenon, "Smoke With Induced Rotation and Lofting (SWIRL)", an acronym that accounts for the aerosol source as well as for two of the obvious dynamical aspects of the phenomenon.*

  We agree that too much gratuitous variations is undesirable and should be blamed. We have reduced them in the revised version. However, we still find useful to have different words to name the smoke bubble or the unshaped smoke plume as seen purely by CALIOP and to name the vortex identified from the reanalysis. We are not very fond of SWIRL as it suggests that heating can generate by itself mean PV or rotation, which is wrong. Swirl is also used already in the fluid dynamics literature, often associated with the word vortex (see, e.g. Mitrofanova et al., 2013), which might be confusing, and has unpleasant meanings in slang. We offer our own acronym, ASTuS (Ascending Smoke Turbo in the Stratosphere) where turbo is a latin word for vortex, which does not have these drawbacks.

Noticeable technical corrections

- *Line 286: What is "beta drift"?*

  The beta-drift is a old concept of vortex dynamics on a rotating planet which is standard in the literature on tropical cyclones (see, e.g., Wang and Li, 1992). The beta-drift occurs for a vortex immersed within a planetary PV gradient and results, for a cyclone, into a poleward and westward motion with respect to the mean flow. This is changed to an equatorward motion for an anticyclone. The ingredient of the beta-drift is the wrapping of planetary vorticity around the vortex (Sai-Lap Lam and Dritschel, 2001). This effect is visible in Fig.10 of Allen et al. (2020) but the beta-drift is ignored by these authors who privilege the effect of the vertical tilt.

- *Figure 5 Caption: I think that "orange" and "green" in the figure caption aren't consistent with the lines on the figure.*

  There are actually two curves on each panel of the figure but because the PV tracking and the ozone tracking are very close, they are most often superimposed.

- All other corrections have been applied

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Answer to reviewer #2

Scientific points

1. *Line 90 Müller & Günther uses Π_g for $\epsilon = -4$ and Π_L for $\epsilon = 9/2$. Maybe the authors could use the same convention, and add a comment to explain why they use different values of ϵ.*

   We have added a comment regarding the choice of ϵ, which depends on the background temperature profile. However, we prefer to leave the notation as is to avoid introducing unnecessary new symbols, since the precise value of ϵ is not central to our argumentation and mainly included so that the analysis is reproducible.

2. *Line 114, the authors state 'mean at the same latitude and altitude'. Do they mean a zonal average or a time average?*

   It is a zonal mean.

3. *Can the volume integrated PV be determined for each vortex from the available data? If yes, can anything meaningful be discussed, in particular during the vortex evolution and the splitting events? Alternatively, does the nature of the way PV is obtained make such an analysis irrelevant?*

   This is an interesting diagnostic that we have not yet developed and which could be combined with the aerosol sections provided by CALIOP. We would like to delay this development to another work in which we will study the life cycle of the vortices.

Minor wording points

1. *Line 30, sentence 'It is a natural..'. Possibly rephrase to read 'Investigating... is a*

natural extension to [ADD REF(S)]

Done

2. *Line 37, maybe insert ‘Australian’ between ‘2020’ and ‘case’*

   Done

3. *Line 116, if the steps $n-1$, n and $n+1$ refer to times, it may be worth mentioning is explicitly.*

   Done

4. *Line 133, ‘to dissociate/dissociating’: the verb/term ‘to split/splitting’ is the most often used when discussing vortex breaking.*

   Agreed and corrected

5. *Line 147, Please check the use of the word ‘thalweg’.*

   It has been replaced by the more common "trough".

6. *Lin 153, insert ‘a’ between ‘month’ and ‘half’.*

   Done

7. *Line 175, fix the reference to the figure*

   Fixed

8. *Line 201 ‘formation’ may be better than ‘birth’; ‘decay’ or destruction’ may be better than ‘loss’ (also line 118)*

   Birth has been changed into formation but loss has been kept as we cannot say how long the vortices survived after we could not track them anymore. We have added a sentence mentioning this point.

9. *Line 279, NH is not explicitly defined. Although line 331 suggests the authors refer to Northern Hemisphere.*

   The few instances of NH and SH have been expanded.

10. *Line 313, SW is not explicitly defined.*

    Expanded to shortwave

11. *Overall revise the punctuation. Some sentences are long and could be split into several shorter sentences. Additional commas could also help readability.*

    We tried, however, to do our best in this respect. We have cut a few long sentences and added some commas.

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Answer to reviewer #3

Major comment

My strongest criticism, is related to the explanation how the reanalysis data, like ERA5 does work (sections 2.2.1 and 2.2.2). A more careful explanations would help to understand better this paper, especially if you assume that not every reader is an expert in the assimilation procedure. Because either ECMWF operational analysis nor the ERA5 reanalysis does assimilate the aerosol observations (the only pure observational evidence from CALIOP) it is difficult to imagine that ECMWF/EAR5 data does contain any smoke-related information at all. However, you show that in the PV/ozone fields (Figure 3/7) there are clear signatures of such smoke clouds. Thus, if these structures are reproduced by the reanalysis, the respective assimilation increments should be small...?

On the other hand, you also show that the assimilation increments within such structures (Figure 9) are really large. Is it true only within such "undetected clouds"? Maybe a separate figure (like Figure 7) but only for the assimilation increments would also help to follow the cloud? In any case I would recommend to explain better the applied method, especially the apparent contradiction between the "resolved" clouds in ERA5 data and unresolved properties manifesting in the "large" assimilation increments.

It is clearly beyond the scope of our work to provide a tutorial on assimilation which is a whole field by itself but we have tried to add some sentences to help the reader who is not familiar. And yes it is a wonder that the model reproduces a smoke vortex even if it does not contain smoke. The "miracle" is due to the fact that the vortex exhibits a strong thermal signature which is well detected by the satellites. This pattern is forced into the model by the assimilation and through the principle that the motion is balanced (McIntyre, 2015), the whole vortex is reconstructed with some accuracy. Over the continental regions the assimilation also uses ground based radio-soundings that contain wind information. Therefore there is no contradiction between resolved and unresolved structures. The missing smoke heating rate is replaced by the assimilation increment, hence the amplitude of this later. In the supplement of Khaykin et al. (2020), it was shown how the deviation of the observations, with respect to the a priori simulated by the model, is reduced by the assimilation, and therefore how the information is used to guide the model. In this study, we are looking at the assimilation from the point of view of the model world and we diagnose the effect of the assimilation on the temperature and PV field. The delicate point is that the temperature assimilation increment cannot be simply interpreted as a heating rate because it diagnoses the final equilibrated state where momentum and temperature are in balance. Assimilation is an iterative procedure where the model is disturbed to get closer to the observation but it is done in such a way that the modification does not add transient riddles of gravity waves. It was shown in Khaykin et al. (2020), that if suddenly the assimilation procedure of new observations is stopped, which is what is done to produce a weather forecast, the vortex does not rise anymore but stays on the same isentropic level and its amplitude decays in about one week. We show in this study that this decay is mostly due to the longwave radiative exchanges that damp the thermal dipolar structure. This is done mostly by carbon dioxide and water vapour. In the real vortex, the longwave radiative effect of the aerosols can only accelerate this damping.

Minor comment

- *L103-106 difficult to understand...please reformulate (see my main point)*

  Several additions have been made to this section to improve clarity.

- *Figure 10 You mentioned in section 2.2.1 that you do not use the ERA5 PV but calculate your own PV from eq. (1). How do you proceed for the assimilation increments of PV discussed in section 4.2.2*

  We use our own calculation to get PV at the full vertical resolution of the model

  and not on a selected number of levels as provided by ECMWF. PV is calculated in the same way for the a priori state resulting from the forecast and for the new analysis. The increment is defined as the difference between these two quantities exactly as for the temperature.

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Answer to Albert Ansmann

We thank Albert Ansmann for his appraisal of our work and for providing a complementary list of references. We were actually aware of these references but one. They were not included as our purpose was not to review the whole literature on smoke clouds, in particular from ground lidars, and the works that focus on the determination of the optical properties or the mass distribution of aerosols, two topics that are outside of our work. We quoted Ansmann et al. (2018), as this study shows results directly related to our work. We have, however decided to quote Torres et al. (2020) and Baars et al. (2019), as they both contain informations that we can comment from our work. In particular Torres et al. (2020) provides a detailed description of the early stage based on CALIOP and the images of the EPIC camera at Lagrange point. We also quote de Laat et al. (2012) for its precursor ideas. Boers et al. (2010) is only concerned by smoke patches in the troposphere, Hu et al. (2019) is almost entirely dedicated to the optical properties of the aerosols and we are not discussing any observational results on the 2020 Australian case which is the focus of Ohneiser et al. (2020).

We stress that none of these previous works recognizes the importance of confinement by the vortical structure generated by the immersion of low PV in the stratosphere, the dipole thermal structure of the resulting equilibrated anomaly, the strong induced long wave relaxation (which is not a cooling) and, in general, the implications of an equilibrated response to heating.

We have also concerns about a number of large estimates of rising/heating rates that we do not recover in Khaykin et al. (2020) or in our work. It is easy to generate overestimates of rising rates from a structure moving over a fixed observatory. For instance, Ohneiser et al. (2020), and yourself in your comment claim an ascent rate of 1km/day when Koobor was near the tip of South America which is about three time the ascent rate found by Khaykin et al. (2020) (corroborated by Allen et al. (2020)) at the same time on the trajectory. The ascent of Koobor was basically smooth for three months, especially in potential temperature, except during two episodes of vertical split and it rose from about 16 km to 35 km, which is a considerable way against the Brewer-Dobson circulation but it did not reach the ionosphere. Our view is that only careful Lagrangian tracking of the structures can reliably assess the ascent rate. As we intent to develop this point in a future work, and review the existing literature in this respect, we do not wish to include an incomplete discussion in the present work.

Baars et al. (2019) show interesting evidence of aerosol patches over Europe and the Mediterranean area reaching 23 km by mid September and again in mid December. Although unstated, they do not expect the second patches to be the remnant of the first due to the mean descending circulation. They invoke a circuit identified by Kloss et al. (2019), where smoke patches were injected into the tropics by early September and rose then slowly to higher levels and came back to the mid latitudes carried by the Brewer-Dobson meridional circulation. There is, however, a hole in Baars et al. (2019) reasoning which is that the tropical rise is reported to reach 21 km by March 2018 while the aerosols supposedly blown away by the Brewer-Dobson are found at 23 km over Europe in December 2017. It is now clear that the missing piece of the puzzle is provided by vortex A which reached 23 km by late September leaving a tail along its path.

As for the culmination of the smoke at 23 km in 2017, we see that the center of the vortex A reached 23 km at the end of our tracking which means that the top was at 24-25 km. Besides this we cannot say that the vortex disappeared and stopped rising after we lost its track with the ERA5 potential vorticity. **?** mention that the return to the extratropics was caped at 23 km by the properties of the Brewer-Dobson circulation. This hypothesis is plausible but is not demonstrated due to the fact that the fast ascent of vortex A also culminated at 23 km.

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