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Atmospheric Chemistry and Physics Discussions

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Interactive comment

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.

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Discussion paper

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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Discussion paper

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