Dear Editor!

Thank you for your comments to further clarify several aspects.

Our answers in the step-by-step reply in BLUE.

Thank you for your response to my comments. It is much clearer now what was actually observed with the lidar, but on going through the paper I realise that the rest of the text needs to be brought in line with the interpretation that there was a thick smoke layer up to around 12 km and aerosol above that whose microphysical properties could not be determined, and is likely to contain at least some volcanic aerosol.

Thank you very much for your priceless support by careful reading and checking the entire manuscript again as an expert!

I have made a number of comments in the attached pdf which I think need to be addressed to make the paper self-consistent.

We followed all instructions and most of them are highlighted in BOLD in the revised version.

I would also ask the authors to look critically at section 5, since there is now no overlap between the height range where smoke was measured and that where ozone was depleted. At the very least that point needs to be clearly emphasized.

We worked carefully again on Section 5.

We improved Figure 18a and b. Now smoke and sulfate influences are clearly indicated.

We carefully checked the text of Section 5 for 'smoke' and substituted 'smoke' by 'aerosol' or 'smoke and sulfate'.

We use the paper of Inness et al. (Figure 18b is based on this paper) in the discussion. This Innes et al paper is based on observations (CAMS high quality re-analysis data).

We do not agree that ozone loss was only given from about 18 to 22 km height (where almost total ozone depletion, a real hole, occurred). We think, aggressive chlorine was activated everywhere above the tropopause. And when the sunlight was back, ozone became destroyed even at 10-12 km height, i.e., in the smoke layer.

This is quite an open discussion (Section 5) and to a considerable extent just speculative. However, we feel we MUST present this Section 5! Otherwise, others are just waiting to take that nice topic as their own original topic. But we want to be able to give reference to our own paper(s) later on when we, e.g., will focus on Australian smoke and the record ozone loss in September 2020 over Antarctica.

We added a new (final) paragraph at the end of Section 5 to provide an impression (some numbers) of the potential impact of aerosol on ozone loss (just by chlorine activation, as a function of the surface area concentration).

We updated the conclusions (ozone hole paragraph, to be in line with the updated Section 5).