We would like to thank J. Lampel for his careful reading of the paper. We will certainly consider to apply the proposed technical tests.

The manuscript *Algorithm Theoretical Baseline for formaldehyde retrievals from S5P TROPOMI and from the QA4ECV project* shows nicely the state of the TROPOMI HCHO product and relates it to other approaches. I have a few short comments on technical aspects of the paper.

1. [Chance and Orphal, 2011] used the cross-section by [Meller and Moortgat, 2000] to rescale the higher resolved cross-section by [Cantrell et al., 1990], basically combining the advantages of both publications. Is there a specific reason why to use [Meller and Moortgat, 2000] alone?

The Chance and Orphal is based on the Cantrell et al., 1990 cross-sections, rescaled to match the Meller and Moortgat, 2000 cross-section. Cantrell et al. offers a better spectral resolution (R = 0.011nm), but its absolute values are biased. With the 0.5 nm resolution of OMI and TROPOMI, we have chosen to use Meller and Moorgat (R = 0.025 nm) avoiding any handmade modification. The two datasets (Chance et Orphal and Meller and Moorgat) result in very consistent slant columns. As Chance and Orphal is the official HITRAN database, we will consider to switch the cross-sections, but this will not affect the results.

2. At the lower end of the fitting interval O4 absorption might play a role as reported in e.g. [Salow and Steiner, 1936] and shown for MAX-DOAS in [Lampel et al., 2017b]. How large is the impact on the retrieved HCHO SCDs? In this context: Maybe a typical (not smallest) residual could be included in figure 4 as an overlay in all subfigures to illustrate for a certain instrument the relation between noise and fitted structures, maybe using a comparable instrument or scaling the residual appropriately.

We have not tested O4 absorption cross-section dataset including a band around 328 nm. However, according to Lampel et al 2017b, the maximum of this band is about 5 times smaller than the band at 342 nm, which itself is significantly weaker than the absorption at 360nm. We do not expect a strong impact on satellite observations.

3. Figure 4 is missing a 'SCD' (if you want to call it like that) for the Ring.

Thanks. The legend has been modified; 8% of inelastic scattering has been considered.

4. At which temperature was the Ring spectrum calculated? For air-borne DOAS measurements the temperature dependence of the Ring effect can be significant (see [Volkamer et al., 2015]) and for ground-based measurements also two temperatures for the Ring spectrum were necessary (e.g. [Lampel et al., 2017a]) in order to avoid systematic structures of up to 5×10^{-4}. For ground-based measurements of HCHO, e.g. in the marine boundary layer, this is significant. The correction of this effect reduced also the HCHO/BrO cross-interferences for ground-based observations. I attached a plot for the absolute and relative difference of the Ring spectrum for a temperature difference of 30K (Ring(243K) - Ring(273K)), see item 4. In Figure 4 you show that the OD attributed to the Ring is of the order of 4 %. A change of 2% of this signal is then of comparable magnitude as the OD attributed to HCHO.

We have taken 350K. Thank you for the suggestion. We will surely test the impact for satellite retrievals of HCHO.

5. Just curious: While vibrational Raman scattering (VRS) of liquid water plays up to 360nm no role (see [Peters et al., 2014] and references therein), does VRS of N2 play a role in this spectral range

(compare Figure 1)? For ground based measurements it is relatively hard to detect, but as residual spectra from satellite based retrievals keep looking better and better ... has this been tried? The largest part of it (the constant one in intensity space) will be compensated by the offset-polynomial, but then there is still the differential part, which should to some extend correlate with the overall signal of inelastic atmospheric scattering. Is this observed?

This has not been tested.