We would like to thank J.-F. Müller for his continued interest in our paper. As shown in our paper, figure 6, our results agree with 3 previous studies of the formic acid yield from ozonolysis of alkenes as a function of RH. We are aware that Glausius et al., did not measure formic acid. As we stated in our original reply "Unfortunately there is no experimental data for formic acid yields" we are using the papers to show that there is a significant source of organic acids from both OH and O_3. However, it seems that Ozonolysis dominates. What is clear is that models currently underestimate formic acid in the atmosphere and that we have identified a possible significant source of formic acid. Indeed recent measurement by Veres et al., 2011 would seem to indicate that there is a strong photolytic source of formic acid. Clearly this could come from both OH and O_3. However, we are using these data within this paper to suggest that O_3 reactions would contribute, and on the basis of experimental evidence by Laursen relative to Glausius et al., it seems that ozonolysis is the dominant route.

There may be other routes to forming formic acid, and these may be possible for other systems (yet currently this is a hypothesis). However, what we are reporting in this paper is that, using our result, ozonolysis would contribute to the global budget in the atmosphere. Indeed, our field work (LeBreton et al., 2011) would suggest that even with this proposed new source, the formic acid measured in the atmosphere is still greater than can be predicted. So if we are out by a factor of two, the agreement is even worse. There is no doubt, as the reviewer is suggesting, that our estimate of HCOOH production from isoprene is an upper limit, where we assume that once formed, CH_2OO goes on to make HCOOH. The values of Neeb (0.3) and Hasson (~0.3) do suggest that about half of the CH_2OO produced decomposes.

It would seem that the rate coefficient of carbonyl oxide with water could vary between $1 \times 10^{-12} - 1 \times 10^{-17}$ cm^3 molecule^{-1} s^{-1}, although the smaller rate coefficients are less likely (e.g. 1×10^{-17} cm^3 molecule^{-1} s^{-1}). Given that $[H_2O]$ will vary from ~ $2 - 6 \times 10^{17}$ molecule cm^{-3} the loss rate with respect to water in the atmosphere would range from ~ 2 s^{-1} to 6 x 10^5 s^{-1}. Loss via unimolecular decomposition can vary from 0.3 – 250 s^{-1} (Fenske et al., 2000). It should also be noted that the experimental studies would find it impossible to differentiate between wall loss and unimolecular decomposition. Thus the estimates are a combination of both first order loss processes, possibly overestimating the loss. Thus it would seem that reaction with water could dominate in the atmosphere. Currently, there are no direct studies of the reaction of CB with water, in the literature. Our studies (Taatjes et al., 2008) have indicated that it is possible to measure CB and this would indicate that CB decomposition/wall loss does not dominate its loss.

Fenske et al., *J. Phys. Chem. A* **104***, 9921 (2000).

LeBreton et al., *Atmos. Meas. Tech. Discuss*. **4**, 5807 (2011).

Taatjes et al., *J. Am. Chem. Soc.* **130(36)**, 11883 (2008).

Veres et al., Geophys. Res. Lett. **38**, L17807,doi:10.1029/2011GL04820 (2011).