Dear Dr Gromov

many thanks for the new version of your manuscript. The revised discussion of the contamination correction is basically acceptable now. However, you derive it in terms of number densities, which are pressure-dependent, but then use a parameterization in terms of mole fractions, which are not. You justify this as being "for convenience", but this appears to be a fundamental difference of physical quantities because the terms on the two sides of equation (A4) depend linearly on pressure ({CO}) and quadratically on pressure ($\{O_3\}$^2). The corresponding parameterization in terms of mole fractions is not subject to these differences. Could you please address this in a further revision?

You had a number of questions regarding my other comments, which I am happy to clarify below (in pink).

I am looking forward to receiving your suitably revised manuscript in due course.

Best regards,

Jan Kaiser

99: "in NH tropospheric emissions" (otherwise this would be a tautology)

We see no tautology here. The CO variations result from mixing of the little varying stratospheric [CO] and largely varying tropospheric [CO]. It is the result of mixing we discuss here. Besides, variations in tropospheric [CO] are *by far* more strongly determined by the presence of hydroxyl radical that by the variations in emissions.

> Would you please clarify how far below 400 nmol/mol this observation holds.

102: "in C1 and C2 [CO], for [O3] > 400 nmol/mol the C1 CO mixing ratios [...]"

This comment is unclear to us. We describe continuous changes in [CO] with increasing [O_3], this will change the meaning of the sentence to something we do not intend to state.

> Your response is also unclear to me. If you don't refer to [O_3] mole fractions > 400 nmol/mol, perhaps you could clarify what you mean.

104: "In the 580-600 nmol/mol [O3] bin"

This comment is unclear to us. We describe to what [CO] in C1 one observes in particular bin (around 580 nmol/mol of [O_3]), this will change the meaning of the sentence to something we do not intend to state.

> Your response is also unclear to me. If you don't refer to the 580-600 nmol/mol bin, perhaps you could clarify what you mean because the data in Fig. 1b (the one you are referring to) are presented in binned format.

105: "accommodates and extra 14 nmol/mol"

Here we meant that this [CO] contains extra 15 nmol/mol as compared to average C2 value. We adjust the statement accordingly.

> Thank you for changing "accommodates" to "contains", but the difference between 39.7 and 25.6 is 14.1, which is not "some extra 15". I suggest you change this to "contains an extra 14 nmol/mol".

160-162: This sentence duplicates the message of the previous one and can be deleted.

Please explain. The statements "[CO] from WAS and *in situ* measurements correlate well" and "anomalies in both [CO] and $\delta^{18}O(CO)$ manifest functions of [O_3]" do not appear duplicate to us.

> . I referred to the sentences "However, both anomalies in [CO] and $\delta^{18}O(CO)$ manifest clear but complex functions of the concomitant [O_3]." and "That is, the C1 *in situ* and WAS data very likely evidence artefacts pertaining to the O_3-driven effect of the same nature.".

223: Please delete "Practically" and change "resort" to "use". The Keeling plot itself does _not_ require an estimate of [CO]_c; however, your data selection criterion (for delta_true) does. Please change this sentence accordingly.

Perhaps, the Editor has misunderstood the message of the sentence. Here we emphasise that we can employ the MM using solely the estimate of the contamination strength (*i.e.*, the amount of molecules admixed to the reservoir with some initial composition). Furthermore, do you imply that using the Keeling plot one does not require to know the amount of molecules admixed into a reservoir with known starting composition? (It obviously would be nonsense, of course, perhaps we did not understand your comment?)

> Indeed, the Keeling plot does not require an estimate of $[CO]_c$.

253: The symbol 13delta_c has not been defined. For consistency, this should be delta13C_c(CO), or, following conventional symbol and index notation, delta_c(13C, CO).

The Editor contradicts himself here. In the previous version of the manuscript we used a consistent notation using indices to distinguish δ_c for ^{13}C and ^{18}O, which the Editor requested to remove (see the comment on l. 227 of the previous version). Since distinguishing different δ_c, δ_a and δ_t variables is obviously necessary we return to the previous notation, *e.g.* $^{13}\delta_c$ and $^{18}\delta_c$.

> There is no contradiction. The distinguishing indices are not necessary for the equations. However, your adopted delta notation for specific isotope deltas is $\delta^{13}C$, not $^{13}\delta$. There does not appear to be any reason to adopt different notations in the same manuscript, so I suggest to use $\delta^{13}C_c(CO)$.

348: Add "in combination with an empirical parameterisation of the [CO] artefact in terms of the O3 mixing ratio" after brackets, followed by "to single out ..."

We would like to keep the current formulation, as we already make a statement above (ll. 345−346) on the quantification of the artefact CO production.

> A simple Keeling plot does not require a data selection criterion. The mixing effects are presumably the reason a Keeling plot without data selection fails, so the sentence does not some qualifying statement.

Fig. 6: The x-axis label should be "MM", not MMA. The legend labels should be delta18O_c(O3) and delta13C_c(O3); also in the caption.

We change the labels to $^{18}\delta_c$ and $^{13}\delta_c$, respectively, that are clearly associated with calculations with the MM. This also allows to avoid somewhat confusing $\delta^{13}C_c(O_3)$ (the carbon isotope ratio from O_3 makes no sense here).

> Please be consistent with your notation and use $\delta^{13}C_c(CO)$ and $\delta^{18}O_c(CO)$. The suggestion to use O_3 was a mistake.