Final Response, Ralf Sussmann, Karlsruhe Institute of Technology, Garmisch-Partenkirchen, Germany, 30 July 2012

It is a pleasure to thank both the referee Vanessa Sherlock and the Anonymous Referee for very sound and helpful comments which lead to significant improvements and interesting extensions of the paper, and we expressed this in the acknowledgement section. We thereafter present our point to point reply.

Review #1: GENERAL COMMENTS

"When Garmisch retrievals are corrected to a common a priori seasonally-varying differences in the NIR and MIR retrievals are reduced and there is a reasonable linear relationship between the corrected NIR and MIR retrievals over a 50 ppb range in XCH4. The correlation between corrected NIR and MIR retrievals at Wollongong is much poorer, but there is no discussion or supporting material (e.g. timeseries) given to understand why this might be so (in fact, no correlation analysis is performed)."

We agree with this point of criticism for the data initially shown in the discussion paper. We overcame this issue during the revision by two measures. i) We prepared updated time series with an increased amount of data for Wollongong, and ii) we found and eliminated a bug in the computation of the dry air column for MIR XCH4 which lead to a residual impact by varying water vapor and introduced additional scatter mainly to Wollongong data because Wollongong is a more humid site than Garmisch. As a result of i) and ii) the NIR-MIR correlations show up now comparably well for Wollongong and Garmisch. We added an analysis of correlation significance via a *t*-test showing for both Garmisch and Wollongong data a significant MIR-NIR correlation with >99 % probability (new Table). We also added a Figure of the (updated) Wollongong MIR and NIR time series.

„The authors derive an intercalibration scale factor based on a linear regression of the corrected MIR versus NIR retrievals assuming/forcing a zero y intercept. Thus the estimated scale factor (slope) reflects the mean x, y for the sample, not the within-sample slope (and the uncertainty estimates reflect the scatter of the MIR retrievals (y) about the sample mean). Consequently, these regression estimates may not be the most appropriate characterization of the intercalibration and the site-to-site consistency. I believe the intercalibration requires demonstration of a strong, common linear relation between the MIR and NIR retrievals over the observed range of XCH4 (1720-1800 ppb) at the two sites."

The NIR and MIR retrieval methods are predicted to be both linear and have no intercept. If we apply least squares fits allowing for nonzero intercepts to the revised Wollongong and Garmisch data sets (i.e., updated and with the bug related to the dry air column eliminated, see above), the fits yield intercepts that are zero within 2 sigma uncertainty. In this situation we think it would be valid to force a zero intercept as concluded earlier by Wunch et al. (Atmos. Meas. Tech, 3, 1351–1362, 2010) in an analogous case.

However, note that with the updated data sets of our revised paper (extended plus bug eliminated) we also do no longer find XCH4 intercalibration factors (i.e., slopes NIR/MIR) to differ significantly from 1 within 2 sigma uncertainty; this holds for both Garmisch and Wollongong (see update of Tables 2 and 3 in the revised paper). This implies that there is no significant site-to-site bias between Garmisch and Wollongong on 2 sigma confidence level. As a consequence we do no longer derive and recommend an intercalibration factor (neither overall nor per site) but just report the (insignificant) MIR/NIR slopes (scaling factors) for Garmisch and Wollongong, respectively, in the revised paper.

Review #1: SPECIFIC COMMENTS

(1) "The description of the MIR and NIR retrieval strategies is very limited for a reader who is not VERY familiar with both NDACC and TCCON retrievals."

We added a section with more details and explanations. See also answer to review #2, Ad 8.

(2) „The coincidence criteria for the MIR and NIR data is not explained at all. How do we go from NIR and MIR data in a ratio of 10:1 spectra to the monthly means in the figures? This must be described in some detail, from temporal coincidence for raw data through to definition/calculation of the monthly means."

We took the single NIR and MIR measurements from coincident days to calculate the monthly means. This information is added to the revised manuscript.

(3) "Section 3.1 is entitled 'Eliminating a priori impact', when in fact the correction only eliminates differences due to different retrieval a prioris. After correction to a common a priori x_c, there is still the smoothing term $(I-A)(x_c - x_{true})$ whose magnitude depends on the averaging kernel A of the given retrieval. The phrase 'eliminating a priori impact' is used repeatedly in the manuscript, and should be corrected."

We corrected the wording throughout the manuscript accordingly.

(4) "The correction described in Equation 1 holds for linear or linearised retrievals. The reader has no idea what the magnitude of the delta-x is in Equation 1, or whether the retrievals truely are linear over this change/these changes in x. The comments below assume Equation 1 does hold for the x in question."

To show the magnitude of delta-x we added a plot of the original MIR and NIR a priori profiles along with the ensembles of the ACTM profiles for both Garmisch and Wollongong. To show the amount of non-linearity we performed new retrievals of the full Garmisch MIR and NIR time series using the daily ACTM profile as prior. We show plots of the difference of the time series retrieved with ACTM as prior and the time series prepared using Equation 1 for introducing ACTM as prior (we did this also "using one of the two constant retrieval a prioris", see comment below). From this we learn that the nonlinearity introduces small mean biases in the order of <1.2 ppb in both MIR and NIR cases at Wollongong and Garmisch. Also the seasonality of these biases is negligible or small (stdv <1.2 ppb). We added related plots and information to the revised manuscript.

(5) „Similarly, given the intercalibration is dependent on the adjustment to a common a priori, I think some summary of the differences in the NIR and MIR averaging kernels as a function of zenith angle (and the x) should be given in graphical form in the paper."

We added figures of the MIR and NIR averaged kernels as a function of the zenith angle.

(6) „The exact application of the model x_M for the correction to a common a priori is not clear – is the model used 'pointwise' i.e. $x_M(t)$ is used to correct the XCH4;Y(t) (Y=NIR,MIR) or is an ensemble mean < x_M;Z > used for each site Z? I assume it is the former, but cannot be sure."

The model profiles for each site are given in three hours-intervals. Therefore, we interpolated the model profiles for each measurement time on the model pressure grid, and applied this interpolated profile. We added this information to the revised manuscript.

(7) „I also do not understand why one of the two constant retrieval a prioris was not selected as the common a priori. This would have assisted our understanding of the correction, as the

seasonal variation in the correction term would then only be driven by changes in the averaging kernels as a function of zenith angle.“

Our intention to use the 3-hourly-resolved (ACTM) model profiles for each site as common a priori was to get a (common) a priori that is as close as possible to the true methane profile at the site at the moment of observation, in order minimize the smoothing term (I-A)(x_c - x_{true}), which is different for MIR and NIR (because of the differing averaging kernels). This being said we also understand that there are also arguments for using "one of the two constant retrieval a prioris". Therefore we added to the revised paper additional retrievals of the Garmisch and Wollongong MIR and NIR time series based on the common MIR a priori and also time series base on the common NIR a priori. As expected by our smoothing argument above, the stdv of the MIR-NIR differences (with common a priori profile) are smaller for the ACTM cases than for the cases using "one of the two constant retrieval a prioris". The seasonal variation of the correction term is visualized by adding the residuals (retrieval with corrected a prirori minus retrieval with original a priori) to Figs. 1 for both the ACTM case and two new cases using either of the two constant (MIR or NIR) retrieval a prioris.

(8) „As noted in the general comments, the correlation of the corrected Wollongong retrievals is much poorer than for Garmisch, and requires comment/further analysis. There is a subset of 5 months where the MIR retrievals are significantly lower than expected from the intercalibration relation. The discrepancy is 10 ppb, comparable in magnitude with the amplitude of the seasonal cycle in XCH4 in the Southern Hemisphere midlatitudes (15 ppb peak-to-peak at Lauder), so one would want to understand the origin of the scatter in the Wollongong retrievals shown in Figure 4 if we are to have confidence in the intercalibration. For starters the Wollongong timeseries could easily be added in figures 2 and 3 (the monthly mean XCH4 at each site do not overlap significantly).“

See answer to corresponding general comment above: this issue has been resolved and the Wollongong time series been added to the manuscript.

(9) "Additionally, error estimates for NIR (and MIR) retrievals should be accounted for in regression analyses.“

We added error bars derived from the stdv of the NIR-MIR difference time series (which corresponds to the standard deviation of the least squares fit) as a proxy for the total (statistical and systematic) error for the individual data points of the Figures. This is then propagated to the final slope error.

(10) „Even with correction to a common a priori there is a suggestion of a residual seasonality in the Garmisch timeseries (maximum differences in the early part of 2008 and 2010). It would be interesting to see the raw data differences (~ coincident corrected MIR/NIR) as a function of time and zenith angle at both sites.“

The question is whether one can understand the March 2008 and March 2010 maxima of the corrected NIR-MIR differences to be due to a zenith-angle (airmass) dependency. As proposed, we prepared coincidences now on a 10-min scale (our initial coincidences had been same day) from the corrected MIR and NIR Garmisch series and plotted the resulting NIR-MIR differences month-by-month as a function of zenith angle. The resulting zenith-angle dependency of the NIR-MIR differences is about (-0.25) ppb/deg for the interval 25 – 60 deg and it is about (+0.1) ppb/deg for the interval 60 – 82 deg. From this together with the fact that the average SZA of the March data is about 60 deg on would conjecture that the NIR-MIR differences should show a minimum for March which contradicts our finding of a March maximum. This could be interpreted in a way that the observed small airmass dependency of the corrected NIR-MIR differences is not the dominant driver of their observed residual seasonality. From this we conclude that the dominant driver is the seasonality of the smoothing term, see answer to specific comment (7).

We add one related comment on potentially different SZA sampling for NIR and MIR with our original coincidence criterion, i.e., use of same-day measurements to construct the monthly means: Our NIR and MIR measurements are performed in alternating mode so the monthly-mean zenith angles are practically the same for the MIR and NIR data sets with NIR-MIR SZA differences of the monthly means of 2 deg on average with a stdv of 3 deg. Given the above mentioned SZA dependence the resulting impact on the NIR-MIR XCH4 differences is negligible. Just for confirmation we redid Fig. 3 also using the 10-min coincidence criterion - and found a slightly increased stdv of the residuals, rather than a reduced stdv which one would have expected if the SZA sampling of the NIR and MIR monthly means would have been significantly different using the same-day coincidence criterion. The reason for the smaller stdv of the residuals for the same-day criterion is that this data set contains more data which allows for better statistics. This is the reason why we keep the same-day coincidence criterion as a basis for the conclusions of our paper.

(11) "Figure 1 could be cut without any loss of clarity in the presentation"

We agree with the figure originally shown in the discussion paper. However, after adding the residuals (series run with corrected a priori minus series with original a priori) to the figure it shows new information, namely the seasonality of the correction term. We did this also in response to the related comment above for the new cases using "one of the two constant retrieval a prioris" .

(12) „Add Wollongong timeseries in Figures 2 and 3."

The according figures for Wollongong have been added to the revised manuscript.

(13) „An additional figure, summarising x(P; t) and A(P; χ) should be added."

To show the magnitude of delta-x we added a plot of the original MIR and NIR a priori profiles along with the ensembles of the ACTM profiles for both Garmisch and Wollongong.

We added figures of the MIR and NIR averaged kernels as a function of the zenith angle.

(14) „Introduction paragraph beginning l23: This paragraph could be more carefully worded. The contribution of local and remote sources and sinks on surface and column measurements depends on the time scales in question. Given the lifetime of CH4, information on atmospheric transport is required to interpret both types of measurement."

We agree to reword. We were thinking about local sources and the fact that information on vertical boundary layer transport is necessarily required for the inversion of sources and sinks from in situ measurements. We will add some comment about remote sources and information on horizontal transport needed for interpretation of both types of measurement.

Review #2: GENERAL COMMENTS

"...in my opinion Figure 3 shows still a seasonality. The authors should come up with a benchmark to quantify the seasonality."

We added an auto-correlation plot of the residuals of Fig. 3 to the revised manuscript which confirms that there is a seasonality. Note we also investigated the reason for this residual seasonality (dominated by the smoothing term, not by airmass dependency), see answers to review #1, specific comments (10) and (7).

“Furthermore, the authors do not argue why they can assume a zero intercept in Figure 4. By forcing a zero intercept, the linear fit is dominated by the artifical datapoint in (0/0), because the measurements group around 1700 - 1800 ppb.“

See first paragraph of our answer to review #1, 2nd general comment.

„...the authors never show a calibrated final dataset. It would be good to include a figure with a final dataset for Garmisch and Wollongong, calibrated with one global scaling factor. By now only the Garmisch dataset is shown, only calibrated with the site specific calibration factor.“

The final MIR+NIR data sets will be shown together in the revised paper for both Garmisch and Wollongong.

“It remains unclear to me how the standard NDACC and TCCON data products (retrieved with the standard a priori) can be intercalibrated with one global calibration factor, when the difference features a seasonality?”

First of all see second paragraph of our answer to review #1, 2nd general comment: For the updated and revised time series the calibration factor is 1 within its uncertainty for both Garmisch and Wollongong. In other words, both the corrected and uncorrected data sets can be used as are. It is clear, however, that the uncorrected time series can only be exploited for validation purposes when considering the mean bias (e.g., versus a satellite instrument) for one or more complete seasonal cycles. For validation of seasonal cycles and for validation studies restricted to less than one complete year of coincident data, of course, the differing a priori profiles of the ground FTS and the satellite instrument should be taken into account – in an analogous manner as we did between NIR and MIR ground FTS using Eq. 1.

“The Figures should be revised in regards of axis labeling (e.g. Does 2008 mean January 2008?) and errorbars should be applied. Additionally, the Wollongong dataset should be shown as well.“

Axis: see answer Ad 19 below; error bars: see answer to review #1, specific comment (9); Wollongong data will be shown.

Review #2: SPECIFIC COMMENTS

Ad 1: “page 1356, line 9: "shows a phase shift in XCH4". The authors mention a phase shift. In my opinion, Figure 2-4 show a time dependent bias, but not a phase shift. A phase shift would be a constant shift in time. The authors mention the phase shift only once. Throughout the text they speak about a time dependent bias. If the authors are convinced that it is a phase shift, it should be better explained or shown by a crosscorrelation.“

We performed a cross-correlation analysis which shows characteristics of a small (<1 month) phase shift. Certainly it is no pure phase shift in a mathematical sense, but the bias shows some seasonal periodicity. Therefore, we use the term “seasonal bias” throughout the revised paper, which is more general than “phase shift”, and less specific than the term “time-dependent bias” which would imply dominant non-periodic time dependencies.

Ad 2: “page 1356, line 14: "The difference time series [...] do not show a significant trend". This is a statement, which should be better shown and quantified.”

This statement is within the abstract. Obviously referee #2 has overlooked our derivation and explanation of this finding later in the manuscript (p 1363, l6): “Another finding from analyzing the difference time series XCH4(NIR)−XCH4(MIR) is that they do not show a significant trend. See Fig.

3 (upper trace) for the example of Garmisch data, and Table A1 for derived numbers on trend and uncertainty. The same result is found for Wollongong (Table A1).” In addition the technique to derive the trend will be explained, see answer to Ad 18.

Ad 3: “page 1357, line 24: "They are representative of a much wider area". It is unclear to me what is meant with "wider area". Additionally a citation should be given, e.g. Keppel-Aleks, G., P.O. Wennberg, and T. Schneider, (2011), "Sources of variations in total column carbon dioxide", Atmos. Chem. Phys, 10.“

We reformulated this sentence to: “They are representative of a larger geographical region while in-situ measurements can represent a specific location or biome.” This reference has been inserted.

Ad 4: “page 1357, line 25: sentence "In situ measurements are more directly traceable [...] while column measurements provide the same quantity as satellites [...]. Why do the authors list these characteristic in contradiction? It is unclear to me what they try to argue for or why they list these characteristics. If they want to list the advantages and disadvantages, it is not a complete description.“

These characteristics are neither supposed to be in contradiction, nor to be a complete description of the different measurement techniques. It is to be understood as continuation of the previous sentence. We hope that reworded this sentence will clarify its meaning: “Therefore in situ measurements are more directly traceable to calibration standards and ground-based column measurements are preferred for satellite validation since they provide the same quantity as satellites measure.”

Ad 5: “page 1360, line 11: Why do Garmisch and Wollongong measure with different path differences? Does the path difference have an influence on the results?“

The reasons for the different path differences are historical. There is certainly no significant influence on the column retrievals, since both Wollongong and Garmisch MIR optical path differences are larger than required for column retrievals (set to allow for profile retrievals, i.e., much higher than TCCON optical path differences).

Ad 6: “page 1360, line 11: Why does Wollongong average only 2 scans, but Garmisch 6 scans?“

Again, the reasons are historical.

Ad 7: “page 1360, line 22: Do the authors mean Wunch et. al. (2010) or Wunch et. al. (2011a)? I would suggest to cite the TCCON paper (Wunch et. al., 2011a).“

The citation is changed to Wunch et al. (2011a).

Ad 8: „page 1360, line 17-22: The paragraph about the retrieval strategies is rather short. Do the authors apply the TCCON calibration factors? Do they exclude data due to flagging? Additional information on the retrieval strategies should be given, at least basic differences of the retrieval strategies. SFIT and GFIT are totally different methods and the authors should line the differences out.“

As to the request of additional information on the retrieval strategies, may be referee #2 has overlooked the details given in Table 1 (we eliminated some typos as to the flagging in the revised manuscript). Additionally, the underlying references are cited there. We added the information to Table 1 that for CH_4 the calibration factor TCCON/aircraft (WMO) = 0.978 (Table 5 in Wunch et al., 2010) is applied, and added a short summarizing section on the retrieval strategies to the main text.

Ad 9: “page 1362, line 2: In my opinion the authors should show a figure of the averaging kernels, instead of citing other publications, because differences in the a priori estimates play a major role in this publication.“

We added a figure of the averaging kernels.

Ad 10: “page 1363, line 10: The authors state that the bias can be attributed to differing spectroscopy. This is a statement that should be argued. Why do they think so? Which spectroscopy? In the MIR or the NIR or both? If the bias between MIR and NIR is due to spectroscopy, why does the difference still show a seasonality? How can this be explained? The authors calibrate the MIR to the NIR, because the TCCON measurements are already calibrated to WMO standards. Are the calibrated NIR measurements shown, and if yes, which calibration factor was applied (Wunch et al., 2010 or Geibel et al., 2011)? If the NIR measurements are calibrated, is the bias then due to the MIR spectroscopy?“

We eliminated this statement about the bias because there is no longer a significant bias, see our answer to review #1, 2nd general comment. The question as to the seasonality of the difference has already been answered in response to review #1, specific comments (10) and (7). As mentioned in response to specific point Ad 8, we applied the CH4 calibration factor of Wunch et al. (2010).

Ad 11: “page 1363, line 13: The authors speak of "column uncertainty". It is more correct to speak of "absolute accuracy". Additionally, in my opinion the systematic biases in the spectroscopy are not the reason for the calibration campaigns, but a result of the calibration campaigns.“

We changed the “column uncertainty” to “absolute accuracy”. We accept the point as to the motivation for the TCCON aircraft calibration campaigns and altered the relevant passage to: “The TCCON measurements are calibrated against aircraft profile measurements which can be traced back to the the WMO in-situ trace gas measurement scale. An absolute accuracy of 1% for TCCON measurements was found and attributed to spectroscopic errors (Wunch et al., 2010).”

Ad 11, continued: “In the TCCON calibration campaigns, it could be shown that the a priori information do not add a bias to the data, and therewith the bias was attributed to spectroscopy. Furthermore, the authors should argue why in their method the a priori information add a bias, but not in the TCCON calibration. Does this mean that the MIR a priori information cause the bias?”

We understand we are talking about “overall” (i.e., seasonal mean) bias here. We think the statement that TCCON (overall) biases do not depend on a priori information may have hold in the cases investigated so far. However, it would be an overstatement to put this as a general rule: According to Equation 1 in principle any change in the shape of an a priori profile can introduce a bias in all real cases where the averaging kernel is not ideal, and this holds both for a profile scaling retrieval (GFIT, NIR) and a profiling retrieval (SFIT, MIR). It depends on the details of the kind of changes to the a priori profile and the atmospheric state and SZA-dependent kernels of a certain study, whether these biases become significant within their uncertainty. To quantify this effect for the cases of our paper we have prepared a Table of overall biases arising when exchanging the original NIR a priori by the ACTM a priori as well as by the MIR (WACCM) a priori for both Garmisch and Wollongong and also changing the original MIR a priori by ACTM or the NIR (GFIT) a priori. The resulting biases are given along with their 2-sigma uncertainties. From this we find that there arise small but significant biases for the cases using in the NIR retrievals the MIR (WACCM) a priori, as well as for using in the MIR retrievals the NIR a priori for both Garmisch and Wollongong. Furthermore, for Wollongong there arise significant biases for both MIR and NIR retrievals using the ACTM a priori. These (significant) a priori-dependent overall biases cover a range of 0.8-5.4 ppb (see details of revised manuscript).

In addition to such a priori-dependent overall bias, our paper demonstrates that changing the CH4 a priori profile generally leads to a seasonal correction, i.e., a seasonal bias. This is documented in terms of a seasonally vaying residual (retrieval with new a priori minus retrieval with original a priori) in Figs. 1 of the revised manuscript for a variety of cases (NIR and MIR for Garmisch and Wollongong using ACTM, WACCM, and GFIT a prioris).

Ad 12. "page 1363, line 20: Wunch et al. (2010) and Geibel et al. (2011) come up with two different calibration factors. The authors should at least discuss these differing results and should state which calibration factor they apply."

See answer to Ad 8. We think that stating that i) we use the generally accepted and used Wunch et al. (2010) calibration factor should be sufficient, together with ii) a brief statement that there is an alternative approach/factor by Geibel et al.. We feel, however, that a discussion of the differing results is out of the scope of our paper, not least due to the fact that Geibel et al. is still under review at the time being.

Ad 13: "page 1364, line 9: "Also [...]" For Garmisch the uncertainty increases, but for Wollongong it decreases. This should at least be mentioned."

And it will be mentioned.

Ad 14: "page 1363, line 22: The authors force a zero intercept in their calibration method. By this, they create an artificial data point and assume that nothing would be measured in the NIR in case nothing is measured in the MIR. This should be further discussed, especially because this datapoint (0/0) dominates the linear fitting, because the measurements group around 1700-1800 ppb."

See answer to review #1, 2nd general comment.

Ad 15: "page 1365, line 14: Why do the authors not apply this method now to further sites?"

This would be subject of future work.

Ad 16. "page 1378, Figure 3: In my opinion the seasonality is muted, but not eliminated, especially for 2008 and 2010. The authors should come up with a method to quantify the time dependent differences."

This question has already been answered in our response to the general comments.

Ad 17. "page 1373, Table 1,2, A1: Why do the authors show the calibration factor uncertainty with 3 sigma and the uncertainty of the trend NIR/MIR with 2 sigma. The methods and the error estimation should be better explained."

We will use 2 sigma consistently throughout the revised paper. The error estimation will be better explained in the revised manuscript. See also answer to review #1, specific comment (9), and Ad 18.

Ad 18. "page 1375, Table A1: What is the Garmisch trend for the same time period like in Wollongong? How was the trend estimated? The authors should explain their method."

The Garmisch trend for the same time period like in Wollongong and using the same months is also non-significant. However, we don´t see the reason for exploring a common time period as the issue here is not to compare absolute Garmisch versus Wollongong trends but to investigate whether there might be differences in the NIR versus MIR trends at either station. The trends were obtained by a direct linear fit to the monthly means. No intra-annual function is applied since we investigate (NIR-MIR) difference time series. This information will be added to the revision.

Ad 19: "Figure 1-3: Please change x-axis to at least half-year values. It is not clear what "2008" means. Is it January 2008?

We added a footnote explaining that, e.g., "2008" means "1 Jan 2008" and the minor tic is 1 July.

Ad 19, continued. Please apply errorbars to the data points. Why are the data in Figure 2 and 3 scaled with the Garmisch calibration factor? The authors argue that one single global calibration factor can be used, but they use the site specific value.“

We added error bars, see also answer to Ad 17 and to review #1, specific comment (9). We skipped the scaling factor in the figures which had been to ease visualization of the time dependencies – but this is no longer required, since the scaling factor of the revised data set is 1 within its uncertainty anyway (see our answers to the two general comments of review #1.

Ad 20: "Figure 1 shows the same information content like Figure 2/3. It could be replaced by a new figure, that shows an estimate of the seasonality in Figure2/3 (for example a cross-correlation).“

We added cross-correlations related to Fig. 2/3. As to Fig. 1 see answer to review #1, specific point (11).

Ad 21: "Figure 3. The residual shows the residual of the non-scaled values and not the residual of the shown data in the lower panel. This was confusing to me.“

We made this consistent in the revised manuscript.

Ad 22."The authors should show the same findings for the data in Wollongong. Do the Wollongong data show the same seasonality?"

The figures for Wollongong can be found in the revised manuscript. As can be seen there, Wollongong data show no significant seasonality.

Ad 23. "Figure 4: The authors should include errorbars, and should explain their method better. Why can they use a zero intercept? By this they invent an artificial datapoint (0/0) that has a big impact."

See answer to review #1, specific comment (9), Ad 14 , and Ad 17.

TECHNICAL CORRECTIONS

All agreed and done.

End of response.