Interactive comment on "Assimilating Compact Phase Space Retrievals (CPSRs): Comparison with Independent Observations (MOZAIC in situ and IASI Retrievals) and Extension to Assimilation of Truncated Retrieval Profiles" *by* Arthur P. Mizzi et al.

Anonymous Referee #2

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General Comments

This paper provides an evaluation of the impact of the CPSR approach for full and truncated MOPITT CO retrieval profiles as compared to the assimilation of the original retrievals in vmr and log(vmr). In this application, the applied quasi-optimal retrievals (QOR) include a transform to 'diagonalize' the retrieval error covariance matrix E_m. The CPSR approach is differentiated from the QOR by including the intermediate step

of applying a 'compression' transform to the averaging kernel matrix A.

The background information in the introduction is quite limited as if this paper was to be taken an extension of Mizzi et al. (2016). It would benefit from additional information. For example, the introduction could include mention of examining the level of influence in the vertical by the different approaches and provide related background on this aspect based on earlier work. The introduction could mention use of QOR in addition to raw retrievals in addition to CPSR in assimilation for evaluation against independent observations. It is important to mention of the benefits and or similarities of CPSR identified in Mizzi et al. (2016) specifically alluding to the assimilation not only of raw retrievals but also QOR, especially since QOR is applied in this paper in addition to raw retrievals and the paper wishes to further validate the CPSP approach. This is currently done in the abstract and could alternatively be done in the introduction. The introduction could bring up papers where MOPITT CO assimilation was performed and indicate related results – which might then be relevant in the results section.

The need for additional information would extend to information on the IASI CO (e.g. vertical resolution and range, average kernels, any information accuracy) and a bit more on MOZAIC CO (e.g. vertical range and resolution, amount of data used, accuracy, precision).

The evaluation is based on a very limited period. While this is not ideal, results do show a sensitivity to the observations. The comparison to observations in the paper can lack rigour though such as the rather qualitative visual comparison of Figs. 4 and 5 as oppose to an absent statistical comparison and evaluation from the fields and data of these figures. The representation of singular vectors in most figures without having a quantitative sense of their relative influence (i.e. resultant influence with the singular values) may be adding some potential ambiguity in assessing their relative importance. As well, there is an absence of accompanying statements and comparisons (with references) to MOPITT CO assimilation results from other papers.

Other than reduced computational time referred in Mizzi et al. (2016) regarding the CPSR approach (and the related advantage of 'compressing' the averaging kernel), the main take away message gathered by this referee regarding the benefits of CPSR and QOR with full profiles is in not requiring to introduce non-diagonal observation error covariance matrices (assuming averaging kernels are also used) in the assimilation as may/would be required to obtain similar results from the raw retrievals (with a priori removed), this pertaining to the vertical distribution of observation information in the analysis.

The aspect of removing biased observation elements through truncated retrieval profiles with the CPSR approach retains the computational efficiency (even though not quantified in this paper) but also notably reduces the influence of the MOPITT CO measurements in the lower troposphere. On the other hand, the comparison to IASI in Figure 1, suggests that not truncating the retrieval profiles would provide better overall results for CPSR and QOR even with the biased data. Might this be a reflection of how the results in Figure 1 are generated and the what they represent? Are there are any other papers which would have used truncated MOPITT CO profiles?

The comparison to MOZAIC above 400-500 hPa (Fig. 1) is presented independently from the comparisons to both IASI and MOPITT at these levels (Fig. 6). Exploiting the similarity of IASI and MOZAIC in comparison to MOPITT would need to be done directly, such as when discussing Fig. 6, as oppose to the reader needing to make this link in relation the MOPITT CO bias.

Another take away message and concern is the stated conclusion that assimilation of MOPITT CO raw retrievals shows little impact. This is attributed, in the paper, to having applied diagonal observation error covariance matrix (line 1 of page 10) if not also the increased observation error variances for the observation with removed a priori. The mention and consideration of other papers and accompanying results on MOPITT CO assimilation, such as Miyazaki et al. (2015) which show notable impact, would be necessary.

There is the tendency to very frequently use ':' followed by (i), (ii)…. It might be worth verifying if this can be reduced. As well, ':' is not really necessary in these cases.

While the paper reads fairly well, an overall revision is recommended to polish up the text.

Specific Comments

Abstract:

P1L18: The choice of 'results confirm' suggests that a computational assessment is performed and included in this paper, which is not the case. It may be a matter of rephrasing and or expanding, in the introduction, on the computational benefit indicated in Mizzi et al. (2016) in use of CPSR.

P1L23-24: Point (ii) is not specifically shown in this paper.

Introduction: (see also General Comments)

P2L10: This line is a summary line of a result of Section 5.1. Might best be removed by referring to issues and concerns to be addressed in the paper and not the results themselves.

P2L12: "In the second part of the paper" refers to what section? As well it assumes a first part which has not been specified explicitly (this referring the P2L10 above). It is suggested to begin this sentence (if kept) instead with 'Therefore, we . . ..'

P2L13: "The rest of this paper" would best be replaced by "This paper" considering P2L12 above and that the results section is also alluded to below.

P2L14-18: Sections 2, 3, 4, and 5 instead of II, III, IV and V. This applies to one or two more places in the paper.

P1L16: '. . . and an extension of CPSRs' (added 'an')

P1L17: Might be worthwhile to refer here to the content of the two subsections in

Section 5.

Section 2:

P3L9, P3L10, P4L3, : '... the DART' (added 'the')

P4L12: 'is much finer' instead of 'is much greater'

P4L20: 'Miyazaki' instead of 'Miyazki'

Section 3: (see also General Comments)

P4L25: 'In the first part of the paper' is actually intended to refer to the first part of the results section. Best to re-phrase.

P4L23 and P5L1: Suggestion - 'independent observations from the IASI instrument and the MOZAIC project.' or something similar. (best to remove parentheses)

Section 4:

P5L17: '...difference are: (i) the ...' or '...differences are (i) the ... and (ii) the ...' or ...

P5L17: There is also the number if days (9 days instead of one month).

P5L18: Does (iii) actually refer to univariate CO assimilation as oppose to localization – unless this is what is meant here by localization (i.e. not be being coupled to MET assimilation in this case)?

P5L24: It might be worthwhile to mention whether CPSR and QOR use vmr or log(vmr) for 'y' in the actual assimilation application (especially since A is applied/provided for log(vmr)). If log(vmr) then equation (2) and (3) would be good as is as long as 'y' is defined accordingly.

P6L1-6: Phrasing could be improved and simplified

P6L3: Change Section V.A

C5

P6L16 and P6L18: The two lines referring to Gaussian/non-Gaussian distributed errors seem to contradict each other somewhat.

P6L20-21: Not clear on the value/meaning of this last sentence.

P6-7: Equation numbering not aligned (as would be from use of 'right-justified')

P6L18-19: Point (ii) could refer to Eq. (2) and QOR to make even clearer the relationship between QOR and CPSR.

P6L20-P7L1 and P7L4-P7L8 are somewhat repetitive. Maybe part P6L20-P7L1 could be removed with some changes for an introduction to what follows.

P8L20-24: As pointed out earlier, one could point to Eq. (2) and QOR for this part.

Section 5:

P9L13: How about the the MET and CO assimilation not being coupled (or being localized?) as per P5L18.

P9L14 and top of Figure 1: What are the units? Maybe unitless because both are referring to log(vmr)? Do these sum up the contributions from all vertical levels? Out of curiosity, how large are these values relative to the observation and background error standard deviations? This might be useful to compare with the RMSE.

P10L1: Due only to discarding the observation error cross-covariances and not also due (at least partly) in removing the a priori effect? Just wondering? A comparison to other papers also assimilating MOPITT CO might be pertinent here.

P10L4 and Figure 1: Would be better to split Fig. 1 in Fig. 1 (for upper panels) and Fig. 2 (for lower panels)

P10L4: Use of arrows might not be best.

P10L4-P10L23: Would some or much of this have been stated in Mizzi et al. (2016)? If so, might be best to reduce the text.

C6

P10L25-P11L8: There is mention of the increased bias with MOZAIC from CPSR and QOR, this supported also by IASI CO in Fig. 6 and related to the MOPITT bias (also displayed by compared MOPITT and IASI in Fig. 6 – if IASI has comparatively no or less bias?)

P11L13-15: Any CO assimilation papers showing or not some impact near/at the surface?

P11L12: 'little or no change' instead of 'little or no improvement' as whether or not there is any improvement is not shown here.

P11L19: The Fig. 2 blow-up histograms are not really needed. It's up to the authors. Might it be best to split the histogram and the lower panels into two separate figures?

P12L1-4 (and beyond): Could differences in the vertical of the CO background (forecast) error variances/covariances also be a contributing factor to some degree, this depending on the assimilation setup? Having some sense of the variation in the vertical of error variances (and error correlations) might be beneficial. Would differences in background error covariances in different papers contribute to explaining differences in results?

P12L12: Was any scaling really needed?

P12L7-22 (and beyond): See General Comments on the display of the singular vectors.

P12-P13: I only skimmed the text for the review on these pages.

P13L17-18: One might question the application of the scaling in the first place.

P13L18: e.g. '. . . that, when . . . is considered, the . . .' (while this is likely somewhat subjective, adding some commas here and or similarly elsewhere in the paper might be considered)

P13L20: '. . . and the first . . .' (added 'the')

P13L22: Might the validity of this assertion depend on the singular values?

P14L2-3: Please indicate actual references and elaborate on results where applicable.

P14L3: What is meant by 'do not adjust for the averaging kernel linear dependencies or for the observation error covariance"s"'. [Might the latter be in reference to not including error correlations (cross-covariances)?]

P14L11-13 (and remainder of the paragraph): While there is some level of consistency in the coastal regions, it is not that evident that one could say that the analysis and forecasts are 'generally consistent' with the observation. Maybe some re-phrasing would be needed. A quantitative evaluation might help.

P14L15-16: Has (i) been looked at to some degree?

P14L16:17: Has (ii) been verified?

P14L17: The changes in the analyses seem rather weak in the central U.S. or thereabouts in comparison what is needed to increase the analysis to levels fairly close to what is seen in Fig. 5. Might a quantitative evaluation help?

P14L20: Does this refer to the central U.S. or is an overall assertion? It is not so clear from the figures if for the central U.S.. Either way, a quantitative evaluation (by regions maybe) might be more meaningful to justify this assertion (and those above).

P14L25: Might it be worth to mention/discuss the level of similarity and differences between 'SS' and 'RS' profiles?

P15L3-6: 'for pressures less than about 500 hPa, the MOPITT CO assimilation with CPSR draws the forecast and analysis further away from IASI while the opposite occurs for larger pressures.'

P15L3-6: Could refer to the comparison to MOZAIC in Fig. 1 to support the comparison with IASI in the upper levels.

P15L11-21: An alternative would be for a version of this 'summary' to instead be in the 'Summary and Conclusions' section. It's up to the authors.

P15L14: It is not really that the 'phase space' observations error variances is reduced as oppose to the transformation allowing to account for the otherwise neglected 'retrieval space' error correlations.

P15L16-17: Part of (ii) is actually a repetition of (i). Some change in the sentence is needed.

P15L17: As part of (ii), has the statement 'linearly dependent portion of the transformed retrievals do not . . .' (repeated earlier as well) been verified, noting that background error covariances (and its non-zero error correlation coefficients) contribute to determining the distribution of information for strongly overlapping averaging kernels (likely requiring more computational effort though). Any other references for his part (e.g. Migliorini, 2008 and or 2012 or even Mizzi 2016)? If so, they should also be indicated earlier on in this paper.

P15L21-23: Have other assimilation studies shown this as well – that the resulting CO analyses and forecasts in the upper levels would be biased. This result would be expected considering the literature on the MOPITT CO data – assuming IASI and also MOZAIC CO is less biased. Might be good to indicate that this was not entirely un expected.

P15L23. This also applies to the comparison with MOZAIC CO.

P16L4: Section V.C to be changed.

P16L8: '. . .accounts for the error correlations of the observation error covariance matrix.'

P16L11: e.g. 'that, in the upper troposphere, the' (commas)

P16L17: e.g. 'troposphere, there'

P16L18: 'A comparison with'

P16L21-22: Could be re-phrased.

P17L1: Remove 'However', i.e., 'The forecast . . .'

P17L3: '. . . United States similar to, though weaker than, the CPSR experiment' or something similar

P17L5-7: e.g., 'The upper tropospheric impacts of Fig. 9 show even smaller changes for the CPSR-RJ3 experiment except for the reductions over the southeastern United States. The CPSR-RJ3 experiment therefore further demonstrates, in addition to Fig. 7, the reduction of bias in the upper troposphere through the removal of the biased observation profile elements, this though at the expense of reduced improvements in the lower troposphere.'

P17L9-11; This should explicitly refer to the upper right-hand side panel with the comparison to IASI CO.

P17L11: The improvement is rather small though as compared to CPSR (and QOR) in Fig. 1. This needs to be indicated. Is this related to how this diagnostic is generated, e.g. maybe because of a dominance of the lower tropospheric RMSE contributions (as compared to the upper layers)?

P18L5-6: This should refer to the levels with pressures below about 500 hPa. 'Significantly' seems to be an exaggeration based on the curve. I suggest removing 'significantly'.

P18L7-13 and Figure 10 (with Table 2): The bottom row of Fig. 10 (even in combination to Table 2) suggests that the 'reject middle three' may have least impact in assimilation. This would be contrary to just looking at the traces in Table 2 which, based on the earlier statement, indicate the 'reject bottom three' provide the least amount of info. Am I missing something? Any discussion or comments.

P19L9: Removing 'likely' seems appropriate as it seems pretty certain.

Summary and Conclusions:

P19L16: Instead of 'magnitude of the observation errors' is it more the omission of the 'observation error correlations in the assimilation' in comparison the CPSR and QOR effects?

P19L19: 'Truncated the observation errors' may not be the correct wording considering the above.

P19L22-23: Applies also to IASI CO. Even better would be to instead mention MOZAIC CO at P20L5.

P20L2: 'because, by accounting for . . . error correlations ,'

P20L21-P21L3: Different contradictory statements in this sentence related to the impact at the surface. Also, one could mention the approximate proportion of cases where surface impact may occur. One might also consider the background error variances in also contributing to the level of impact at near the surface (on top of the averaging kernels themselves (and obs error covariances))

P20L10: 'confirming the applicability of the CPSR . . .'

P20L13: 'Excluding the assimilation of some elements of the observation profiles can . . .'

P20L16: 'to address the reduced impact from not assimilating retrieval profile levels' ('reduced' instead of 'remote' and . . .)

Additional remarks on Tables and Figures:

Table 1: Might be better to follow the form of Table 1 in Mizzi et al. (2016)

Figures: Font sizes for panels with y-axis as pressure are on the edge of being too small or are too small. Please check.

Figures 6 and 7: For clarity, might be best to drop the 'SS' results (at least for Fig. 7 if not both). That is unless the one intends to mention and discuss in the text, for Fig. 6 for example, the level of similarity and differences between 'SS' and 'RS'.

Figure 10: 'except that this figure' (added 'that') Figure 7 (lower panels): Unless this is a visual clarity issue, it seems that the Met EX RS results near the surface differ between the CPSR panels and the L10VMMR panels, while they would be expected to be the same. Please check.

References: I did not check the format of the references.