Response to Reviewer 1

The reviewer's comments are in black. Our responses are in blue text. The modifications and additions to the text are highlighted in yellow in the revised manuscript PDF file. However, to see what was deleted, please see the annotated original manuscript PDF file.

General comments: The paper describes the new development of the coupled weather and atmospheric composition system based on the Environment and Climate Change Canada's (ECCC's) operational Ensemble Kalman Filter (EnKF). While the paper describes this new configuration as an important advance for the ECCC system it misses important points to provide an accurate and complete description that such system should deserves. The first major point that needs to be addressed is that the paper advertises in several places that it is a greenhouse gases (GHG) atmospheric data assimilation and surface flux inversion system. However only CO atmospheric data assimilation is showcased. I would strongly recommend that the authors remove all claims that a flux inversion GHG system has been setup and then use a different terminology such as simply "atmospheric composition data assimilation" or "atmospheric carbon data assimilation" as in the title. The study uses synthetic observation to evaluate the system. Therefore, why the authors did not simulate the HYPNET CO2 and CH4 observations and perform the assimilation of such to at least justify the GHG component of the system? It seems that the added value of the paper is the extension of the ECCC operational system to atmospheric composition using CO assimilation as a proof of concept. While the focus is on CO assimilation, very little importance is given to the meteorology assimilation evaluation in such configuration. How does this compare to the actual operational ECCC system? Almost no references are given to reader to refer to the NWP system and its evaluation. I would recommend the authors to give a short summary on the meteorological data assimilation rather than ascertaining that the meteorological data assimilation is working as expected. The overall presentation of the paper requires strong efforts to improve clarity. Almost all parts of the paper lack clarity. Some parts are over emphasising some aspects that are not relevant for the evolution of the system while other parts that are important are covered very briefly. To give few examples:

- Very little is explained about the simulation of MOPITT synthetic observations, averaging kernels and their errors. It seems that a paragraph is maybe missing.
- Extensive description of the meteorological setup is given but very little is described and showed about the actual meteorological data assimilation results.
- Some of the terminology used is not really common for atmospheric data assimilation, I would encourage the author to revise this throughout the text.
- Several misleading statements about data assimilation and atmospheric composition need to be corrected.

Please refer to the specific comments for details.

Response: We are grateful to the Reviewer for their careful reading of the manuscript and for helpful suggestions. Our original intention was to present our work as the first of a long series of steps to reach our final goal of a greenhouse gas and flux estimation system using an operational weather forecast model. However, both Reviewers felt that the presentation did not sufficiently distinguish the completed work from the context of the desired future work. This led both Reviewers to conclude that the organization of the manuscript was confusing and possibly misleading. We appreciate this feedback and have rewritten the manuscript to focus on only the CO state estimation work which was completed. The overall context and goals of our project are now limited to only the first paragraph in the Introduction, and to a discussion of future work in the Conclusions sections. The term "greenhouse gas" also does not appear anywhere except those two mentioned locations where goals or future work is described. Also, as suggested by the Reviewer, we have provided much more detail about the meteorological assimilation system, both the pre-existing, operational system and our modifications to it in the revised section 2.3. A new supplemental section shows comparisons of our EC-CAS meteorological estimation with that of the original system (Figures S1-S6) as well as a Table S1 of the types of model perturbations used. Also, the behaviour of meteorological fields in the simulated observation context is now shown with 3 extra panels in Figure 5. In addition, as requested by the Reviewer, the data assimilation terminology was modified as suggested, and all of the specific comments were addressed. Finally, more information about the generation of simulated MOPITT observations was added (line 268-281 in original manuscript and lines 279-297 in the revised manuscript). Overall, we feel that the revised manuscript has greatly benefitted from feedback of both Reviewers. Below, we respond point-by-point to each of the specific comments made by this Reviewer.

Specific comments:

Line 38: Be consistent, so maybe replace by air quality. Or explain that air quality is partly driven by weather.
Response: Good point. "weather" has been changed to "air quality" (line 38 → line 45 in revised manuscript).

Lines 39-41: This sentence has some shortcomings that could mislead the reader. Be consistent with the previous sentence and please develop this statement in more precise information. Air quality is a bit different from tropospheric pollution. Tropospheric atmospheric composition prediction is essential to air quality prediction which is looking at surface levels of pollutants. Tropospheric pollution prediction relates to longer time scales than 5 days, especially for CO. Air quality is driven by emissions variations and synoptic variations of weather regimes.
Response: Thanks for the clarification. "Tropospheric pollution" has been replaced by "Tropospheric atmospheric composition prediction" in this sentence (line 39 → line 45).

Line 41: Which data assimilation systems are we talking about here?
Response: The word "those" has been deleted for clarity (line 41 → line 47).

Line 63 and line 65: Swap years to chronological order
Response: The introduction was rewritten and the paragraph containing these lines was deleted.

Line 78: This system now can estimate emissions using state augmentation as described in Gaubert et al., 2020 (Gaubert, B., Emmons, L. K., Raeder, K., Tilmes, S., Miyazaki, K., Arellano Jr., A. F., Elguindi, N., Granier, C., Tang, W., Barré, J., Worden, H. M., Buchholz, R. R., Edwards, D. P., Franke, P., Anderson, J. L., Saunois, M., Schroeder, J., Woo, J.-H., Simpson, I. J., Blake, D. R., Meinardi, S., Wennberg, P. O., Crounse, J., Teng, A., Kim, M., Dickerson, R. R., He, H., and Ren, X.: Correcting model biases of CO in East Asia: impact on oxidant distributions during KORUS-AQ, Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2020-599, in review, 2020.)
Response: Thanks for the update. The statement on lines 79-80 has been deleted. This reference now appears in the revised section 3.2.

Lines 81-82: Maybe this is a bit misleading as the paper seems to focus on CO (even if CO is important for GHG estimations). Also, the term "estimate GHGs" is a bit vague in my opinion. Maybe replace to something more specific such as "estimate CO atmospheric distribution".
Response: "GHGs" has been changed to "CO atmospheric distribution" (line 82 → line 65).

Lines 88-91: This paragraph is not necessary here as some of it should be moved to the introduction.
Response: This paragraph was deleted.

Line 88: "Trial fields" is quite uncommon data assimilation terminology. Maybe replace by forecast, background, prior or first guess fields depending on what you are meaning by trial here.
Response: "Trial fields" used to be quite common in atmospheric data assimilation. The problem with "background" is that inverse modellers (especially those dealing with CO_2) reserve that term for the large global mean over which local spatial perturbations exist. In addition, inverse modellers use the term "prior" to refer to the flux priors and applying this to the CO state could be confusing. We replaced "trial fields" with "forecast fields" throughout the article. Similarly "trial ensemble" has been replaced by "forecast ensemble" throughout the article.

Lines 88-90: The first and second stage are not explicitly mentioned. I would rewrite those two general sentences with a more traditional way to introduce the general concepts of data assimilation.
Response: As noted above, this paragraph was deleted.

Lines 94-95: The sentence "The model is initialized: : :" is confusing please rephrase.
Response: The statement has been changed to: "A number (N=64) of 6 h model forecasts are simultaneously integrated from N meteorological and CO initial conditions with forcing from N perturbed CO surface fluxes." (lines 94-95 → 73-75).

Line 95: Please "trial fields" replace with appropriate traditional data assimilation terminology throughout the text.
Response: As indicated in the response to comments for Line 88 above, we have replaced "trial fields" throughout the manuscript with "forecast fields". This paragraph (lines 93-101) has been rewritten for clarity (lines 73-83 in revised manuscript).

Line 97: "Blending" is not really the correct word for the data assimilation procedure. I would recommend the author to use the appropriate vocabulary for data assimilation in the literature that tackles atmospheric data assimilation.
Response: As noted above lines 93-101 have been rewritten.

Lines 99-101: You do not really need to specify what will be the sections to come here. Consider removing.
Response: We have deleted the sentence in lines 99-101.

Line 108: I do not think that "lib" is the appropriate terminology here. Please again replace with, for example: "... 80 vertical levels from the surface to 0.1 hPa."
Response: Done. (line 108 → line 89).

Line 109: what type of hybrid coordinate? There are several of them.

Response: It is a log hybrid pressure and sigma coordinate that is commonly used (with slight variations) in operational weather forecast models. The provided reference (Girard et al., 2014) describes it in detail.

Line 114-155: Please be more specific and add diffusivity in this sentence.
Response: Done. (line 114 → line 95-96).

Line 120: Not correctly written. The atmospheric chemistry scheme is not removed for CO2. You remove the reactive chemistry in a model to increase its performance. Please rephrase.
Response: The sentence was changed to: "In contrast, GEM-MACH-GHG uses a simple parameterized chemistry for CH_4 and CO while CO_2 is treated as a passive tracer." (line 120 → lines 101-102).

Line 130: Start a new paragraph here as you now write about CH4 surface fluxes.
Response: Done. (line 130 → line 112).

Line 135: Start a new paragraph here as you now write about CO emissions.
Response: Done. (line 136 → line 119).

Line 148: Please define xf and xa here. xf and xa are commonly called the prior and posterior state respectively in the EnKF terminology. Alternatively, you could call them forecast (hence the superscript f) and analysis (hence the superscript a). Please consider using the commonly used atmospheric data assimilation vocabulary throughout the text for more clarity.
Response: xf and xa have been defined as the forecast and analysis states. (line 148 → line 145).

Line 148: Consider directly defining the other elements of the equation 1 before going into explanations.
Response: Lines 153-154 were moved to lines 146-147 in the revised manuscript.

Line 152-153: The sentence "Pf is the forecast error: : :" is a bit vague, please be more specific in the definitions.
Response: We have introduced equations 2 and 3 which define P^fH^T and HP^fH^T.

Line 196: I think there are more relevant papers for this statement. In Inness et al., 2015 the system used was a CTM configuration where the meteorological fields are forced by external meteorological fields. In that sense the DA system could not drive any constrain on the meteorology. Please cite instead Barré et al., 2015 and/or Gaubert et al., 2016 and/or Kang et al., 2012 and so on... Those papers are using EnKF with this variable localisation between atmospheric composition and meteorological variables.
Response: Actually, Inness et al. 2015 does refer to a coupled system with chemistry modules embedded in the meteorological model. The older MACC system (Inness et al. 2013) was coupled in an offline way to the IFS, but Inness et al. 2015 point out that the earlier approach was not computationally efficient and that chemical tendencies were held fixed for 1 hour and this caused problems at the day/night boundary (see their Introduction). This was the motivation for a fully online chemistry model (called C-IFS). Furthermore, Inness et al. (2015) state on p3 (section 2.2) that "the error covariance matrix between chemical species or between chemical species and dynamics fields is diagonal". Thus variable localization was done by them. However, it is true that other references could be added here. We added Barré et al., 2015 and Gaubert et al. 2016. (line 196 → line 226).

Line 200-201: The sentence "The spatial correlation: : :" seems to have no link with the previous ones. Please remove or develop in a new paragraph.
Response: The statement was deleted.

Lines 209-210: The sentence "To simulate model : : :" is unclear. Please rephrase and possibly add a reference for this error representation method.
Response: The discussion of the meteorological system was moved to the section 2.3 and rewritten. This section now pertains to only the additional changes needed for CO data assimilation.

Lines 214-215: But this paper is not doing flux estimation. Maybe consider changing to atmospheric composition data assimilation and change to the appropriate references.
Response: As noted in our response above, this section was rewritten and moved to section 2.3. There is no mention of studies related to flux estimation in the revised section 2.3 and section 2.4.

Lines 218-219: "In EC-CAS, for the meteorological assimilation, the same scheme is used, but for GHGs, no such additive error is present." Is this the configuration used in this paper? If yes, why then bother going though all these details above?
Response: This paragraph was rewritten as noted in our last 2 responses. We moved all discussion of the EnKF configuration for meteorology to section 2.3. This then simplifies and clarifies the additional changes needed for CO assimilation.

Line 224: Then why not using synthetic GHG observation of CO2 and CH4 (amongst other GHGs)?
Response: Although the model does include CO_2 and CH_4 as well as CO, and our ultimate goal is to assimilate all 3 constituents, it is a major undertaking to test and validate the system for each of the three species. Each species is quite different in nature and in fact has a completely different literature. Thus the team involved in the validation exercise for a given species would be different. As an example of the differences, CO_2 has a lifetime of ~200 years and a very large background with primary surface fluxes from the terrestrial biosphere, ocean, fossil fuel emissions, fires, and land use change. However, CH_4 primary surface fluxes include agriculture, wetlands, ocean, anthropogenic emissions, fires and an atmospheric sink and it does not have such a large background value. Thus CO_2 has large positive and negative surface sources whereas CH_4 has mainly positive surface sources. Thus the best way to simulate surface flux uncertainty in the two cases will differ. Also, because CO_2 has a huge background and we are interested in variations of 1-10%, the type of forecast error variance inflation needed will differ from CH_4. CO differs again in having a larger dynamic range in mole fractions than either CO_2 or CH_4 and the shortest lifetime of the three species. Thus, timescales for forecast error variance saturation will differ. Then we come to the observing networks, which are quite different for all 3 species. This is just to name a few of the differences. We do plan to study the assimilation of each species separately, in good time.

Line 236: change to "the use of a surface flux". I would recommend the authors to be consistent with this terminology as fluxes are not necessarily at the surface in the atmosphere.
Response: Good point. We now refer to surface fluxes here and elsewhere in the paper.

Lines 269-270: ": : : its retrieved profiles are sensitive to CO in the lower troposphere where : : :" MOPITT retrievals are sensitive throughout the entire troposphere. The multispectral retrievals allow an enhanced sensitivity towards the surface over land only when the conditions are favourable. Please correct and amend the text accordingly.
Response: We have revised the sentence to read "retrieved profiles are sensitive to CO in the lower troposphere during daytime over land, where the flux signal from surface emissions is most readily detected." (line 269-270 -→ line 279-281)

Line 271: What are those data assimilation systems? This statement is not true. Number of air quality DAS only assimilate surface stations. Please be more accurate here.
Response: We have changed the sentence to read **"**As a result of this sensitivity to lower tropospheric CO, and the long observational record, MOPITT data are widely used for inverse modelling of CO emissions and for air quality studies**"** . We have added references to indicate some of the specific data assimilation systems for which this statement is true. "(e.g. Arelleno and Hess 2006; Fortems et al. 2011; Barré et al. 2015; Jiang et al. 2015b; Yin et al. 2015; Mizzi et al. 2016; Inness et al. 2019; Gaubert et al. 2020;l Miyazaki et al. 2020). (line 271 → lines 281-283).

Line 273-274: This statement is misleading. You do not use the averaging kernel to construct the observation operator. You feed the observation operator with the averaging kernel to sample the first guess.
Response: We have modified the text to better explain the need to account for the averaging kernel in the observation operator. (Please see lines 288-298 in the revised manuscript).

Line 276: This is unclear. Does this mean you discard all observations that have a retrieval surface pressure below 1000 hPa? I do hope you are not doing this. Please clarify the sentence.
Response: The sentence we wrote does not accurately reflect what we did. The 10 levels are a fixed grid. There are no actual observations below the surface. The lowest retrieved level corresponds to the surface level, which may lie at lower pressures than 1000 hPa. We have deleted this sentence and modified line 275 by replacing "1000 hPa" by "surface". (line 275 → line 286).

Lines 277-278: This is not the proper definition of the averaging kernel matrix. Please use the common definition given by Rodgers 2000. Inverse Methods for Atmospheric Sounding. Theory and Practice. https://doi.org/10.1142/3171 | July 2000. Pages: 256. By (author):; Clive D Rodgers (Oxford).
Response: Please see the modified text from lines 285-298.

Line 278: H is not a forward operator but only an observation operator in the Kalman filter as it does not need to generate a forward model prediction to get a model equivalent quantity. It is true in for example the 4D Var formulation. Please correct.
Response: We have replaced "forward operator" by "observation operator". Please see lines 293-294 in the revised manuscript.

Line 281: The authors do not use the same system as in Jiang et al., 2015a. If they do this needs to be clearer earlier in the paper. If not, please recall a bit more of the methodology or use the appropriate reference to the system used in this paper.
Response: The assimilation systems are different, but those differences are not relevant here. We have removed this sentence since explaining the methodology of the Jiang et al. study would not be helpful for the discussion here.

Line 285: "varied between 10-16%" is this the value that the authors use to set up the observation errors. It seems that few sentences are missing to explain the setup on MOPITT observation errors.
Response: We have used 10% to set up observations errors. We cite Deeter to justify this value. Please see lines 302-304 in the revised manuscript.

Lines 289-290: This sentence is hard to understand. Please rewrite.
Response: We have rewritten these lines.. Please see lines 305-311 in the revised manuscript.

Line 290: "other issues" Please be specific of what other issues.
Response: Please see lines 305-311 in the revised manuscript.

Lines 293-299: So why do the authors bother simulating observations then? Why not testing the DAS in real conditions? Please justify more clearly the choices here and certainly earlier in the paper.
Response: An important stage in the development of any data assimilation algorithm is to prove that it works. We know from data assimilation theory, that in the absence of bias in observations and models and with plentiful, and accurate observations, the system should work. By simulating observations, we can satisfy the constraints of unbiased observations. We have tried to achieve a balance between a highly idealized setup and reality by allowing the transport model to have imperfections and by using (simulated observations from) real networks like ECCC and MOPITT. Adding different observation networks gives us a further qualitative sense of whether the system is behaving properly since we can guess how using more realistic networks with data gaps will behave relative to the uniformly dense network. Assimilating simulated observations helps us to build confidence in the system we have built. This is only the first step. We will be assimilating real observations. Please see lines 305-311 in the revised manuscript.

Lines 308-309: The statement "An ensemble of forecasts: : :" is incomplete as is, I would remove it as this would need couple sentence to make this point clear and this paragraph is not the place for that.
Response: These 2 statements were deleted. We have explained the role of state dependent correlation in spreading observational information in other sections.

Lines 311-312: This was already mentioned earlier. Remove.
Response: Done

Line 320: Regarding the reference to Pires et al., 1996, I think numerical weather prediction and predictability ranges have evolved since the mid-90's. Please use a more recent reference. Also, the time of the DAS RMSE stabilisation is not due to weather predictability but mostly due to the DAS setup, i.e. background error, observation density, type and error and so on... Please rewrite the related statements.
Response: The statement was deleted.

Lines 321-322: The authors could add winds, surface pressure and RH (or another NWP variable of your choice) in a four-panel plot to make your point stronger and avoid such statement.
Response: Figure 5 was expanded to include other meteorological variables.

Line 328: What is the "additive model error term"? Is it inflation? Please refer to the section where it is defined and explained? If not define here and/or add the appropriate reference.
Response: The statement has been rewritten for clarity. It now reads: "The spread in the CO ensemble at any grid point is due the perturbations in the flux and those in winds". (lines 328 → 337-338).

Lines 332-333: This is statistically expected considering Gaussianity and the truth being drawn from the prior distribution itself. Please modify the statement accordingly.
Response: Though one can make sure that at initial time that the Gaussianity is respected (by drawing from Gaussian flux distribution and initial conditions), one cannot control the extent to which forecast distributions are Gaussian. This is due to the nonlinearities in transport model. Therefore Gaussianity is an assumption that can be violated based on the state (time and location).

Line 337: change "establishes" by "is"
Response: Done (line 337 → line 346).

Line 338-339: The authors should stop recalling what would be the next sub-sections at the end of each sub-sections.
Response: These lines were deleted.

Line 341: Please use more common vocabulary; "trial" is not used in atmospheric data assimilation.
Response: As previously noted, "trial" was replaced by "forecast" throughout the manuscript.

Line 347: what is the matrix inverse. Is it the inverse of P?,H?, R? Or K? Please be more specific.
Response: We have rewritten this section to better focus on the illustration of state dependent correlation and localization.

Lines 351-352: The sentence "The scaling factor: : :" is hard to understand. What is the scaling factor of the innovation? Please define.
Response: We have rewritten this section. The scaling factor is (HP^fH+R). We have dropped this sentence since it is not central to the illustration of distance dependent localization.

Lines 356-357: The statement "In theory a given: : :" is misleading statement. In the theoretical case of a perfect ensemble with an infinite number of members, the spurious correlation would not exist, and you would not need to localise. Hence you would apply the filter globally. Please remove or change accordingly.
Response: "In theory" was meant to indicate exactly this situation of an ensemble with infinite members. However, this is made more explicit in the revised text. (line 356-357 –> 361).

Line 359: The statement "small correlations cannot be trusted" is misleading. A GC localisation is not applied to remove small correlations but spurious correlations that are far from the observation location. Small correlations are not necessarily spurious. This also depends on the ensemble size and nature of the state (e.g. lifetime and transport). Please remove or change accordingly.
Response: We agree with the reviewer that both small and large sample correlations can be spurious. We have removed the sentence. However, small correlations are harder to estimate. The sample correlation coefficient has an uncertainty associated with it. The correlation coefficient can be viewed as an estimator. The pdf of this estimator is complex and depends on the sample size and the true correlation (see attached pages from Hoel's Introduction to Mathematical Statistics, Wiley, 1974). For example, with our sample size of 64, for a true correlation of 0.9, the sample correlation coefficient (r) will be estimated as lying with the range $0.84 < r < 0.94$ with 95% confidence. However for the same sample size of 64, a true correlation of 0.1 will be estimated as lying between $-0.15 < r < 0.34$ to 95% confidence. Note that the uncertainty range for a high correlation is smaller than that for a low correlation (here a range of 0.1 versus 0.49). So, it is indeed harder to correctly estimate small correlations for a given sample size.

Line 363: What the meteorological cut off values? Please detail and/or provide reference.
Response: A reference to the values used is included in section 2.3. Please see lines 186-188 in the revised manuscript.

Line 365: Change "has a peak" to "has its maximum".
Response: Done. (line 365 → 366).

Line 407: Change "blob" to "area".
Response: Done. (line 407 → 411).

Lines 409-410: This is incomplete. The transport of corrected concentration plays a major role as well. I would say this is the combination of both in your case. Please update the text accordingly.
Response: The correlations are a result of the flow-dependent transport. This clarification has been made to the text. We also note the role of downstream transport during the forecasts. Please see lines 414-415 in the revised manuscript.

Line 411: If the surface only concentrations and not the 0-5km column were displayed different results might appear as the observation network is at the surface. Also, it is hard to tell in figure 6 that the RMSE is much lower in Western Canada as this is at the edge of the colour scale. The authors should zoom and adjust the plot to make the point clearer.
Response: This is a very good suggestion. We have included two more panels in figure 10. Panel c shows the RMSE and panel d shows the benefit averaged over 0-1 km. The height of the ECCC stations varies from 5 to 707 meters.

Lines 441-442: Again, this is not only the EnKF but also the transport of corrected concentration by the model itself that improves the RMSE. Please correct the text.
Response: The EnKF includes the model forecasts as part of the algorithm. However, we believe the Reviewer's point is to distinguish between the analysis step and the forecast step, and indeed both are important for transporting information of observations downstream. (Lines 441-442 → lines 447-448).

Line 471: I would disagree with that statement. The vertical information content in the MOPITT retrieval as opposed to HYPNET is not precisely located but spreads across the vertical. So, this is not because the degrees of freedom on the vertical are comparable that the vertical information is similar. Please correct the statement.
Response: The reviewer is correct in noting that the MOPITT information is distributed in the vertical. We have modified the text to state that "HYPNET has information at three vertical levels while MOPITT has an information content with one to two degrees of freedom (Deeter et al., 2012) so that limited vertical information is provided by the two networks." (Lines 471 → lines 464-466).

Line 473: The authors do not show this as a not directly GHG gas has been assimilated. I would suggest removing GHG but change to something as "atmospheric composition" as only CO assimilation has been demonstrated in the paper.
Response: We have rewritten the manuscript to avoid discussing GHGs except to mention our future work. We changed "GHG" to "atmospheric composition". (line 473 → 468).

Lines 484: This is true, but this needs to be reformulated correctly. Please mention atmospheric transport.
Response: We have modified the statement to include transport. (Line 484 → 480-481).

Lines 494-495: I am not convinced this is a conclusion from Miyazaki et al., 2012. CO surface flux errors can be correlated with other fields if you consider the co-emission of different species through a given sector. Remove or change the statement accordingly.
Response: We have deleted this paragraph since it deals with flux estimation and is therefore more relevant to a paper which deals with flux estimation.

Line 506: The authors did not show anything about flux inversions. Please remove this statement.
Response: We have removed this statement.

Line 508: Please define smoother. Add a reference. Use the book from Bocquet et al., 2016 for definitions of the smoother.
Response: We have defined a smoother. We have added two references – Liebelt for the definition of smoother and Bocquet, 2016 for the formulation of the smoother we want to develop. See lines 494-495 in the revised manuscript.

Line 509: Future observations? That do not exist unfortunately... Please remove or correct.
Response: The statement has been clarified to indicate that a smoother uses observations later than the time of the analysis as well as those from earlier than the analysis time. See line 494-495.

Figure 7: What do the numbers in the x-axis indicate? Please clarify or change

Response: The x-axis labels refer to the date from 28 Dec. 2014 to 28 Feb 2015. The figure has been revised. Figure 5 has been similarly revised.