Response to reviewer #2

Dear Anonymous Referee #2,

We want to thank referee #2 for the review and the opportunity to improve our paper. We hope we have adequately answered all the reviewer's comments. Reviewer comments are addressed in the following with a point-by-point response in *italic*. Sentences that we suggest for addition or modification to the revised version of the manuscript are indicated in *italic blue*.

Best regards,

Dikraa Khedhaouiria on the behalf of all co-authors

1. Reviewer general comments to the authors

This paper is about the use of an hybrid covariance data assimilation scheme within the Canadian Precipitation Analysis. The paper is of very good quality, very well written, nice to read, with a very rigorous presentation of the elements of the study, the observations, the model, the scores used, the results etc. Despite using a methodology that is quite familiar now, an hybrid covariance scheme, the paper is quite innovative as it presents, to my knowledge, one of the first application of an hybrid scheme to precipitation forecasts. The authors provide a very convincing demonstration of the preeminence of the hybrid scheme over an Optimal Interpolation scheme only. This demonstration is based on different scores like the normalized root mean square error or the scores derived from a contingency table for binary events like for example the equitable threat score or the false alarm ratio and also with the comparison against ST4 data. In particular they show that the hybrid performs better than the OI only during both winter and summer seasons and in particular confirm previous findings from [Wang et al., 2008] of the effectiveness of the hybrid scheme with sparse observations networks.
All in all I had a hard time finding anything relevant to say about that article just because it is so well written. That said, if I was to say one flaw is that the authors do not emphasize enough the fact that the hybrid scheme not only performs better than the full static case but also better than the full dynamic case. They actually talk about it only in the conclusion. I imagine that the authors are interested in the improvement of their data assimilation system compared to the OI version of it, but they must understand that the fact that the hybrid performs also better than the EnKF for precipitation forecasting can be of great interest for the rest of the community. I would suggest the authors to complete their analysis in both sections 7.1 and 7.2 by commenting further about the full dynamic case, and also to complete the figures 3 and 4 by adding the line of the case $\beta = 1$. I sincerely believe that it would help improving the paper and that it would not require too much work from the authors.

- *Thank you very much for this positive evaluation of the manuscript. We agree that adding information about the impact on scores when using β=1.0 would give a better overall picture of the methodology. The fact that reviewer #1 also raised this concern prompts us to propose changes in this direction to our manuscripts. The last comment in Section 2 below provides a more thorough response to this concern.*

2. Specific comments

Page 3, line 76: "β is comprised between 0 and 1, ensuring that the total background error covariances are conserved". This is true if the matrices $\mathbf{P}^{\mathrm{b}}_{\mathrm{OI}}$ and $\mathbf{P}^{\mathrm{b}}_{\mathrm{d}}$ provide "independent estimations of the true background error covariance matrix", [Ménétrier and Auligné, 2015]. So, I would be grateful to the authors if they could go a little bit more through that point, and explain why they think that the matrix $\mathbf{P}^{\mathrm{b}}_{\mathrm{OI}}$ they build represents an estimation of the true background error covariance matrix.

- *We thank the reviewer for this interesting comment. Modeling the error covariance matrices (background and observations) is a crucial step in data assimilation and is a continuous research area (Cheng et al., 2019).*

  Even though they do not necessarily agree on this assertion, Ménétrier and Auligné (2015) explained that parameters used to weight the ensemble-based (P_e) and the static covariance matrices (P_c) sum to one (which is our case: $\beta + [1 - \beta] = 1$) because of the assumption that P_e and P_c provide independent estimates of the true background error covariance matrix. If we understand the comment well, reviewer #2 has no problem assuming that P_e is a good estimate of the forecast error covariance as it is very well documented (see, for example, Lorenc 2003). It is indeed one of the major hypotheses in the Ensemble Kalman Filter. Reviewer #2 is therefore questioning how *our P_c matrix, i.e., $\mathbf{P}^b_{OI}$, is also an estimate of the true background error covariance matrix. The answer to the latter point is that based on several assumptions and past experiments, the elements of $\mathbf{P}^b_{OI}$ matrix were supposed to be isotropic, homogeneous and follow and exponential decay (see Equation 3). This error covariance matrix structure has been successful for CaPA (see Fortin et al., 2018 for an extensive review) and other studies conducted on different variables and using such an approach (Mitchell et al. 1990, Garand and and Grassotti 1995, Brasnett 1999; among others). Although $\mathbf{P}^b_{OI}$ could be improved by relaxing some of the assumptions, this work is beyond the scope of this study. It is also interesting to note that when comparing the experiments with $\beta = 0.0$ to $beta = 1.0$ (which could be considered somehow as an EnKF configuration), results showed that $\mathbf{P}^b_{OI}$ are not completely different and displayed similarities for some precipitation thresholds. All this point, therefore, suggests that $\mathbf{P}^b_{OI}$ provides relevant information regarding the horizontal correlations of the background errors and represents an estimation of the true background error covariance matrix.*

Page 4, eq. (4): what do the notations z_i and z_j stand for? Is that the value of the innovations at locations i and j? Please, add the definition of z_i and z_j after eq. (4).

- *In line 89, page 3, we introduced both the locations i and j and the variable Z such as: "[...] the Euclidean distance between locations i and j and the correlation length. These parameters are estimated using variographic analysis of the innovations, $Z = d - Hx_f$. The d, x_f and H correspond respectively to the measurements [...] ", suggesting that z_i and z_j would be the value of the innovations at locations i and j. However, to add clarity, we suggest to replace line 95:*
  - *"where σ_o^2 corresponds to variance errors of the observation."*
  - *by "where z_i, z_j and σ_o^2 correspond, respectively to the innovations at location i and j and to variance errors of the observation."*

Page 4, line 95: I am not familiar with variographic analysis, in my understanding, you use eq. (4) to fit it on the empirical semivariogram and determine an optimal value of σ^2_{OI}. If I am correct, please can you add a sentence clarifying that point here (even though this is also specified page 5, line 119), otherwise it is unclear why you introduce this function here.

- *Yes indeed, the variographic analysis is done to estimate the parameter σ^2_{OI}, but also σ^2_o and l_{OI} and is realized before the analysis as such. We mentioned in line 89, page 3, that when referring to σ^2_{OI} and l_{OI} "[...]. These parameters are estimated using variographic analysis of the innovations, $Z = d - Hx_f$.[...] ". In light of the reviewer's comment and to add further clarification, we will modify this sentence as follows: "In CaPA algorithm, parameters σ^2_{OI}, l_{OI} but also σ^2_o, which is the variance errors of the observation needed to build the observation error covariance matrix (explained further below), are estimated before the analysis as such and by using a variographic analysis of the innovations. The innovations are classicaly defined as $Z = d - Hx_f$, where d, x_f and H correspond respectively to the measurements the forecasts and the observation operator, which is here the nearest neighbour interpolation (Fortin et al., 2015). [...]".*

Page 4, lines 105-106: I am aware that this comment is obvious but in order to speed-up the computation you could also perform the analysis for each grid-cell in parallel. I guess it would not require a lot of modifications to the existing version of the code. I have no idea how much the computation efficiency is critical in this case though.

- *The reviewer is quite right. Reducing computation time by parallel modelling in this part of the CaPA code has already been discussed internally. However, as the reviewer guessed, computation time is not a limiting factor for CaPA, at least for the moment. Some actions are already in place in* *the code to reduce computation time. For example, the covariance matrices have a maximum size of 16×16 per observation type, which leads to a maximum of 32×32, when using both surface stations and radar QPEs (see lines 105-109 of the manuscript). This size limitation allows to speed up the resolution of the matrix systems. Finally, changing this part of the code would require a non-negligible amount of work and an intensive internal process on the part of the ECCC to ensure that CaPA, as an operational system, will not suffer any degradation due to these changes. For these reasons, there are no plans to implement this change.*

Page 7, line 185, eq. (13): that criteria for rejecting observations is baffling to me, I feel like I missed something. Eq. (13) basically means that if the absolute difference between the Box-Cox of the observation and that of CaPA is smaller than a specific threshold then the observation is rejected. While you would like to reject observations that are too "far" from the model to avoid too strong updates. Can the authors correct that point? Or just let me know if I missed something.

- *We thank the reviewer for pointing out this; the manuscript has an error when describing equation 13. This quality control aims to reject observations from stations that appear very different from the closest ones (Lespinas et al. 2015). We take advantage of this comment to also highlight that we intend to simplify the superscripts in equation 13. Therefore, we intend to correct lines 184-185 currently as:*
  - *"For this purpose, an analysis is estimated at a site s_k using neighboring stations in a LOO approach. The observation is rejected or, is said invalid, if:*

    $$\left| x_{s_k}^{(OBS)} - x_{s_k}^{(CaPA)} \right| < tol \cdot \sqrt{(\sigma_o^2 + \sigma_a^2)}, \tag{13}$$

    "
  - *for: "For this purpose, an analysis is estimated at a site s_k ($x_{s_k}^{(a)}$) using neighboring stations in a LOO approach. The observation at the same site ($x_{s_k}^{(o)}$) is rejected or, is said invalid, if the following is not fulfilled:*

    $$\left| x_{s_k}^{(o)} - x_{s_k}^{(a)} \right| < tol \cdot \sqrt{(\sigma_o^2 + \sigma_a^2)}, \tag{13}$$

    "

Page 7, lines 190-191: if I am not mistaken, I have counted so far 3 quality checks, maybe it could be an idea to summarize them in a table.

- *We thank the reviewer for this good suggestion. In reality, it exists several other quality control steps of input datasets before their assimilation in CaPA that were not described in the manuscript. For example, precipitation at radar pixels is cleaned using cloud cover from GOES (Geostationary Operational Environmental Satellite) images, among many other quality control checks. Detailing all the QC steps is beyond the scope of this manuscript; some of them are already well documented in Lespinas et al. (2015) and Fortin et al. (2015), as mentioned in the manuscript on page 6, line 178. Therefore, adding a table with the three quality checks (QC) might mislead the reader into inferring that the table is an exhaustive QC list. The reviewer's comments made us think that we need to reformulate the way we list the QC steps in the manuscript and emphasize that other quality control steps exist. To do so:*
  - *we suggest to change:*
    - *"A first temporal QC is performed" (p.6, L. 178) for "A temporal QC is performed"*

    * *"A second quality control" (p.7, L. 182) for "A different quality control"*

  - *we also intend to add: "Several other QC are applied to precipitation input datasets, but their extensive description is beyond the scope of this manuscript (see Lespinas et al. 2015 and Fortin et al. 2015 for further information)."*

Page 7, lines 199: "seamless precipitation fields", I do not know here if this is my english that is at fault or my limited knowledge of precipitations, but I do not know what is a "seamless precipitation field", can you precise it between parenthesis maybe, or add a reference if necessary?

- *The term "seamless precipitation fields" refers to a field without discontinuities as opposed to scattered precipitation values observed at surface stations. In the manuscript, the term seamless was chosen to emphasize that Stage IV data is continuous in space and would allow different types of verification. "Seamless" is a pretty common adjective when describing spatially continuous precipitations fields, as shown by a Google search of the term "seamless precipitation fields". We suggest to change "seamless precipitation fields" to "spatially-continuous precipitation fields".*

Page 9, lines 256-263: based only on the shape on the curves it seems that the hybrid approach brings potentially a dramatic improvement compared to the OI only based approach. Though, a quick calculation shows that the relative reduction of NRMSE of the hybrid approach for the optimal value of β is rather limited with around 3.4%, 2.3%, and 7% reduction of NRMSE, respectively for fig. 2-(a), 2-(b), and 2-(c) (though I must say that in the case of winter 7% is quite good). I would then recommend the authors to go a little bit more through that in that paragraph.

- *We thank the reviewer for this suggestion. It is true that quantifying the improvements and degrations in NRMSE values relative to the reference experiment would provide a better perspective of the results. We suggest to modify lines 258-265:*

  - *"NRMSE values for β ranging from 0.0 to 0.4 were indeed very similar for the summer experiment assimilating radar QPEs (Fig 2.b), with a minimum obtained at 0.4. On the other hand, the experiment without radar QPEs showed larger variability in NRMSEs for the different βs with a minimum obtained at 0.5 (Fig 2.a). These results suggest that when the density of assimilated observations is lower, the hybrid approach brings more added value and is thus consistent with the literature (Hamill and Snyder, 2000; Wang et al., 2008a). During the summer, the use of $\beta >$ 0.6 (0.9) with (without) radar assimilation deteriorated the NRMSE values compared to the reference analysis ($\beta = 0.0$).*
    Interestingly, the winter experiments illustrated a different pattern. According to the NRMSEs (Fig 2.c), the analysis improved when the β increased and reached a minimum at $\beta = 0.7$."

  - *for "NRMSE values for β ranging from 0.0 to 0.4 were very similar for the summer experiment assimilating radar QPEs (Fig 2.b), where the optimal β equal to 0.4 corresponds to an NRMSE reduction of 2.3% compared to $\beta = 0.0$. On the other hand, the experiment without radar QPEs showed more significant variability in NRMSEs for the different βs. The optimal value of 0.5 reduces the NRMSEs by 3.4% when compared to the experiment using β=0.0. These results suggest that when the density of assimilated observations is lower, the hybrid approach brings more added value and is thus consistent with the literature (Hamill and Snyder, 2000; Wang et al., 2008a). During the summer, the use of $\beta >$ 0.6 (0.9) with (without) radar assimilation reduced the NRMSE values and therefore deteriorated the analyses compared to the reference analysis ($\beta = 0.0$).*
    Interestingly, the winter experiments illustrated a different pattern. According to the NRMSEs (Fig 2. c), the analysis improved when βs increased and reached a minimum at $\beta = 0.7$, leading to a 7% reduction in the NRMSE values."

Also, the authors have missed an opportunity here to deepen their analysis and show the benefits one could retrieve from the use of an hybrid scheme, not only compared to the full static case, $\beta = 0$, but also compared to the full dynamic case, $\beta = 1$. Indeed, the authors do not mention that case while at the same time they show that the hybrid performs better than the EnKF only. What I mean is that if the hybrid was performing better than the static case only but no better than the dynamic case it would be of no interest. So, despite the reference case of the authors being $\beta = 0$, I would highly recommend that they treat the case of the standalone EnKF only for the reason aforementioned and that they complete that paragraph accordingly.

- *We thank the reviewer for this very important suggestion. We propose to add after the sentence in line 271: "Summer 2019, with and without radar QPEs, and winter 2020 have optimal β that is equal to 0.5, 0.4, and 0.7, respectively" the following: "For all three experiments, the hybrid approach showed its relevance as it overcame both the static ($\beta = 0.0$) and the dynamic configuration ($\beta = 1.0$).". We will also further discuss the impact of using $\beta = 1.0$ in Section 7 (see details in the following comment).*

Sections 7.1 and 7.2: the authors definitely have to talk more about the case $\beta = 1$. The authors could complete the figures 3 and 4 by adding the curve for $\beta = 1$ and then complete their analysis by emphasizing the fact that the hybrid also improves the results compared to the full dynamic case. I do believe that it would not require too much work from the authors while improving the quality of the paper.

- *This comment was also raised by reviewer #1, and as we agree that taking it into account would benefit the manuscript, we will add more insights on results when $\beta = 1$. The followings provide a list of the modifications we propose:*
  - *Figures 3 and 4 will additionally display the metric (FBI-I, ETS, POD, FAR) values when $\beta = 1$ (grey curve). The new version of these figures and their captions are provided at the end of this document (pages 10-11). We want to draw the reviewer's attention to Figure 3.d. An error occurred while merging different figures. In the current manuscript version, Figure 3.d is the same as Figure 4.b, which is wrong. We will correct Figure 3 accordingly as shown on page 10 of this document. The results are much more consistent with the obtained NRMSE values (Figure 2.b). Indeed, using $\beta = 0.3$ and $\beta = 0.4$ during summer, with the assimilation of radar QPEs, lead to similar verification metrics.*
  - *In Section 7.1, we intend to add the following sentence: "For all three experiments, the hybrid approach showed its relevance as it overcame both the static ($\beta = 0.0$) and the dynamic configuration ($\beta = 1.0$)."*
  - *We suggest to modify in Section 7.2 lines 281-286:*
    * *" Figure 3.a illustrates the metrics for the summer without the radar QPEs for the optimal $\beta = 0.5$ compared to $\beta = 0.0$, where filled markers indicate no significant differences at the 95% confidence level between the two experiments for a given threshold. The 6-h precipitation analysis displayed a significant increase of skill (at the 95% confidence level) as shown by the ETS and a decrease of the false alarms (FAR) for all the selected thresholds. The POD was slightly deteriorated, especially when looking at the small precipitation events. As illustrated by the FBI-1, the selection of $\beta = 0.5$ led to generally lower precipitation amounts than with the use of $\beta = 0.0$. The impact was positive for small precipitation events (thresholds of 0.2 and 1.0 mm), but it tended to smooth out higher intensity events."*
    * *for "Figure 3.a illustrates the metrics for the summer without the radar QPEs for the optimal $\beta = 0.5$ and $\beta = 1.0$, both compared to $\beta = 0.0$. Filled markers indicate no significant differences at the 95% confidence level between the two experiments for a given*

threshold. The 6-h precipitation analysis with $\beta = 0.5$, displayed a significant increase in skill (at the 95% confidence level) as shown by the ETS and a decrease of the false alarms (FAR) for all the selected thresholds. The POD was slightly deteriorated, especially for small precipitation events. As illustrated by the FBI-1, the selection of $\beta = 0.5$ led to generally lower precipitation amounts than with $\beta = 0.0$. The impact was positive for small precipitation events (thresholds of 0.2 and 1.0 mm), but it tended to smooth out higher intensity events. Interestingly, using a completely dynamic configuration, $\beta = 1.0$, showed improved performances compared to $\beta = 0.0$ and $\beta = 0.5$, but only for precipitation events above 0.2 and 1.0 mm and except POD, which degraded for all thresholds. However, looking at precipitation events of higher intensity with $\beta = 1.0$, e.g., 5 and 10 mm, did not necessarily degrade FBI-1 or the ETS scores compared to $\beta = 0.0$ but were not as well represented as when using $\beta = 0.5$."

- *We also propose to add right after the sentence "the POD slightly deteriorated, and the FBI-1 reduced for small precipitation events but increased for events of medium to high intensity." (line 296-297) the followings: "The dynamical approach, $\beta = 1.0$, showed a different pattern when assimilating radar datasets. Almost all scores and thresholds were significantly degraded compared to $\beta = 0.0$ and $\beta = 0.4$. The only improvement regards precipitation events above 0.2 mm, but the degradation for other thresholds is too substantial."*
- *To comment of the winter results, we will change:*
  * *lines 300-306: "Finally, Figure 4.a illustrates the same metrics during the winter and compares the $\beta = 0.7$ to the reference experiment. The ETS, was significantly improved, and the false alarms at the 95% confidence level were reduced. Fewer precipitation events were generated for all selected thresholds with the analysis using $\beta = 0.7$ than with $\beta = 0.0$. Again, this improves the performance for 6-hour precipitation greater than 0.2 and 1.0 mm, but not for accumulations greater than 2.0 mm. However, the degradation of FBI-1 for high-intensity precipitation was much less pronounced than in summer, especially for such a high β value. The probability of detecting events (POD) greater than 0.2 mm was significantly reduced, but was increased for heavy events precipitation ($>$ 10 mm)."*
  * *for "Finally, Figure 4.a illustrates the same metrics during the winter and compares the $\beta = 0.7$ and 1.0 to the reference experiment. With $\beta = 0.7$, the ETS was significantly improved, and the false alarms at the 95% confidence level were reduced. Fewer precipitation events were generated for all selected thresholds with the analysis using $\beta = 0.7$ than with $\beta = 0.0$. Again, this improves the performance for 6-hour precipitation greater than 0.2 and 1.0 mm, but not for accumulations greater than 2.0 mm. However, the degradation of FBI-1 for high-intensity precipitation was much less pronounced than in summer, especially for such a high β value. The probability of detecting events (POD) greater than 0.2 mm was significantly reduced, but was increased for heavy events precipitation ($>$ 10 mm). In between performances were obtained for experiment with $\beta = 1.0$, the latter did no better that when using $\beta = 0.7$ but were not that different for several thresholds and scores (e.g., FBI-1 for precipitation above 0.2 mm (Fig 3.a)."*
- *Finally, we intend to change lines 313-314 "In light of these results, it appears that the use of the optimal β value identified through the use of NRMSE did indeed show an improvement in skills and a reduction in false alarm rates for both summer and winter experiments." for "In light of these results, the optimal β value identified through the use of NRMSE showed improved performances compared to static ($\beta = 0.0$) and dynamic ($\beta = 1.0$) configurations. Improved skills and reduced false alarm rates were indeed obtained when using the optimal β for both summer and winter experiments."*

3. Technical corrections

Page 3, line 73: repetition: "the the background field".

- *Thanks you, we will correct this typo.*

Page 4, line 96: $\mathrm{P}^a{}_{\mathrm{OI}}$, is it an error in the notation? Should not it be $\mathrm{P}^b{}_{\mathrm{OI}}$?

- *We thank the reviewer for this comment. Indeed, we did not introduce $P^a{}_{OI}$ matrix, which corresponds to the covariance matrix of the analysis error when the analysis is solely based on OI approaches. Even if, by design, the time-varying elements of $P^b{}_{OI}$ matrix lead to a time-varying $P^a{}_{OI}$ matrix (see equation 8), we will correct $P^a{}_{OI}$ for $P^b{}_{OI}$ as raised by the reviewer. Introducing a new matrix and its definition will burden the manuscript without adding necessary information.*

Page 5, line 126: I would recommend not to write "(1) minus..." but "1 minus". The notation (1) is misleading and can make think about the numerotation of an equation.

- We will take into account this recommendation by writing *"1 minus"*.

Page 6, line 155: the acronym SYNOP is not defined, does it stand for synoptic?

- *The reviewer is totally right, SYNOP stands for synoptic stations and the acronym was not defined. We intend to modify line 155 p. 6 "For more reliability, the NRMSE is computed only at SYNOP and manual SYNOP stations during the summer and [...]" for "For more reliability, the NRMSE is computed only at surface synoptic observations (hereafter SYNOP) and manual SYNOP stations during the summer and [...]". In addition, as suggested by the other reviewer, we will add an Appendix that lists all the acronyms (including SYNOP).*

Page 9, eq. (16): it seems that there are a few mistakes in the writing of eq. (16), I guess eq. (16) writes:

$$\mathrm{FSS} = 1 - \frac{\frac{1}{N_y}\sum_{i=1}^{N_y}\left(\mathrm{f}_a(i) - \mathrm{f}_o(i)\right)^2}{\frac{1}{N_y}\left[\sum_{i=1}^{N_y}\mathrm{f}_a^2(i) + \sum_{i=1}^{N_y}\mathrm{f}_o^2(i)\right]}$$

- *We thank the reviewer for catching the errors in equation 16. There are indeed one parenthesis that is not right ($f_a(i)$ should have been $f_{a(i)}$) and n in the sum component of the denominator should have been N_y. For the other suggestions provided by reviewer #2 (mainly regarding the parenthesis not being in the subscripts), we prefer to follow the same notation as several other articles (see for example equations (2) and (3) in Schwartz et al. 2009). Therefore, we suggest to modify:*

$$FSS = 1 - \frac{\frac{1}{N_y}\sum_{i=1}^{N_y}\left(f_a(i) - f_{o(i)}\right)^2}{\frac{1}{N_y}\left[\sum_{i=1}^{n}f_{a(i)}^2 + \sum_{i=1}^{n}f_{o(i)}^2\right]} \quad (16)$$

for

$$FSS = 1 - \frac{\frac{1}{N_y}\sum_{i=1}^{N_y}\left(f_{a(i)} - f_{o(i)}\right)^2}{\frac{1}{N_y}\left[\sum_{i=1}^{N_y}f_{a(i)}^2 + \sum_{i=1}^{N_y}f_{o(i)}^2\right]} \quad (16)$$

However, to add more transparency regarding the subscript i definition in equation (16), we will reformulate the following sentence (lines 242-243): "Then, the fractions of grid-cells above the threshold (probabilities) in a pre-selected neighborhood (for example, a square of 30 km) are calculated for CaPA (f_a) and ST4 (f_o) respectively" for "Then, the fractional values at the ith grid cells (probability of precipitation above a selected threshold) in a preselected neighborhood (e.g., 30 km square) are estimated in CaPA ($f_{a(i)}$) and ST4 ($f_{o(i)}$), respectively."

Page 11, line 296: repetition: "the POD slightly was slightly deteriorated".

- *Thank you, we will rectify that sentence as "the POD slightly deteriorated".*

Page 14, line 425: repetition: "repetition: "for the observation density observations"".

- *Thank you, we will remove the repetition so that the sentence can read "for the observation density".*

4. References

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Schwartz, C. S., Kain, J. S., Weiss, S. J., Xue, M., Bright, D. R., Kong, F., Thomas, K. W., Levit, J. J., and Coniglio, M. C. (2009): Next-Day Convection-Allowing WRF Model Guidance: A Second Look at 2-km versus 4-km Grid Spacing, Monthly Weather Review, 137, 3351 – 3372

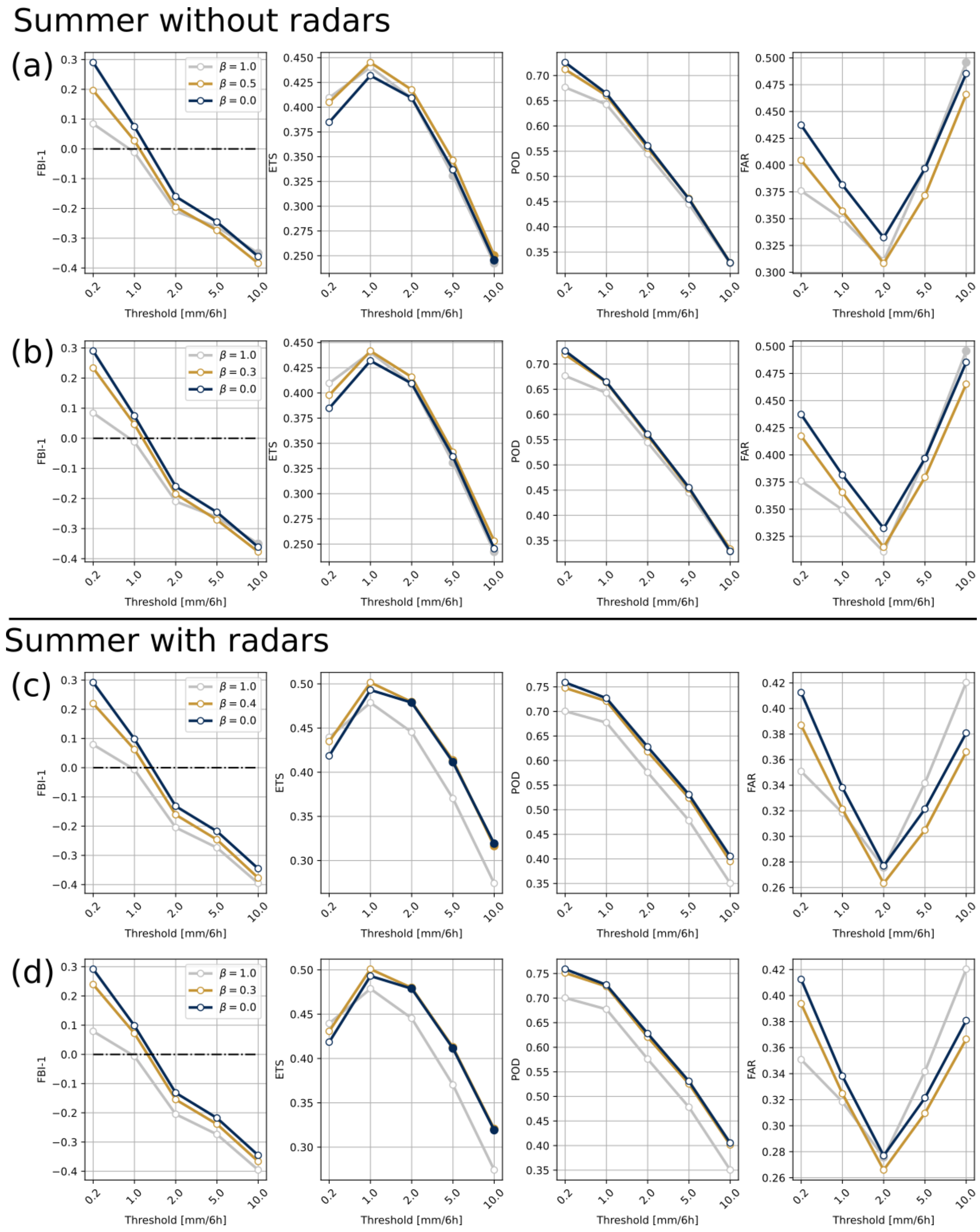

Figure 3. FBI-1, ETS, POD and FAR across the whole domain for summer experiment without radar QPEs for precipitation analysis with $\beta = 0.0$ (dark blue line), $\beta = 1.0$ (grey line), $\beta = 0.5$ (yellow line in a) and $\beta = 0.3$ (yellow line in b). Same figures but for the summer experiment with the assimilation radar QPEs with $\beta = 1.0$ (grey line), $\beta = 0.4$ (yellow line c) and $\beta = 0.3$ (yellow line d) all three compared to the reference experiment when $\beta = 0.0$ (blue line). Filled markers indicate no significant differences at the 95% confidence level between the reference experiment $\beta = 0.0$ and $\beta = 1.0$, $\beta = 0.5$, $\beta = 0.4$ or $\beta = 0.3$ experiments.

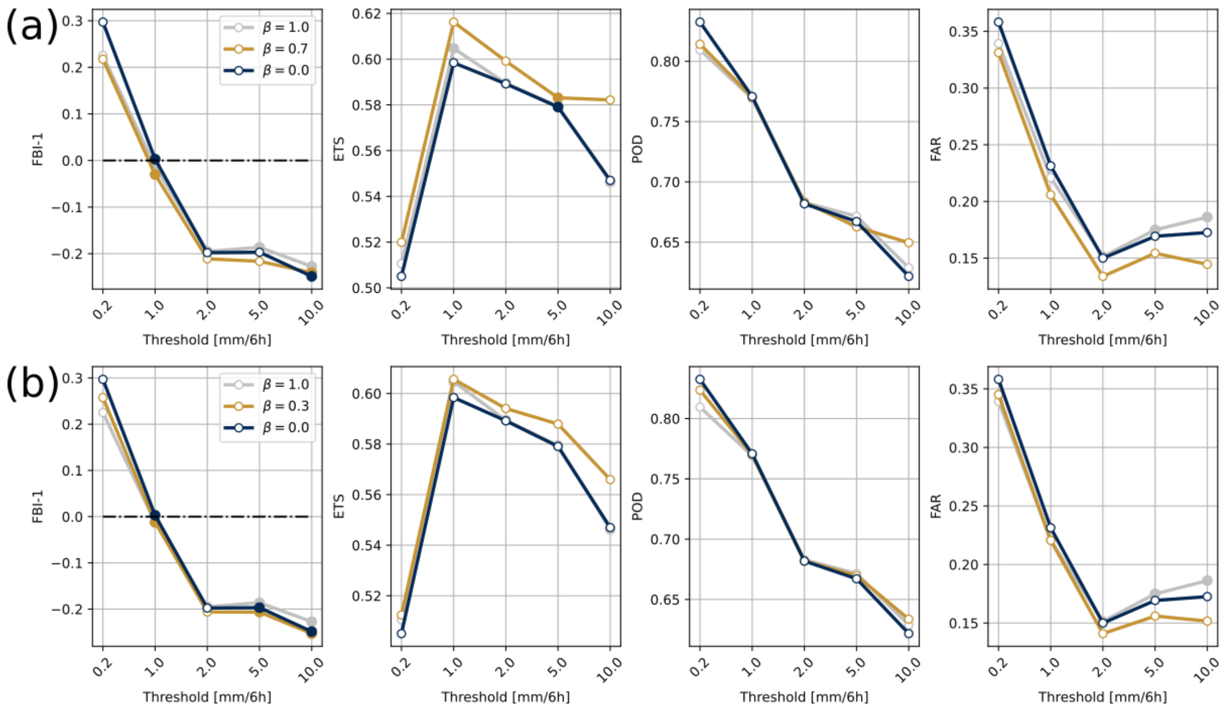

Figure 4. Same as Fig. 3 but for the winter experiment and with $\beta = 0.7$ (top panel), $\beta = 0.3$ (bottom panel) and $\beta = 1.0$ (two panels), all three compared to the reference experiment when $\beta = 0.0$.