Geoscientific Model Development Discussions

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Comment on gmd-2021-161

Anonymous Referee #1

Referee comment on "Assimilation of GPM-retrieved ocean surface meteorology data for two snowstorm events during ICE-POP 2018" by Xuanli Li et al., Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2021-161-RC1, 2021

Review of

Assimilation of GPM-retrieved Ocean Surface Meteorology Data for Two Snowstorm Events during ICE-POP 2018

by Li et al.

General comments

This manuscript intends to investigate the impact of the assimilation of the Global Precipitation Measurement (GPM) retrieved ocean surface observations on two winter storms during 2018 using GSI and WRF model. The discussions primarily focus on the analysis impacts in the observation space, and the precipitation predictions for each case. This study has potentials in improving our understanding of how to optimally utilize the GPM retrievals. But the unclear motivation and lack of in-depth discussions make the paper reading frustrating. I would not recommend publication of this work until substantial revisions have been made. The detailed comments are listed below.

Major comments/concerns:

- One major concern I have about this manuscript is the unclear goal. If this study is focusing on the assimilation of GPM retrievals as suggested by the title, then the introduction should provide more information about the GPM retrievals, such as how it was (or was not) previously applied/researched in the field of snowstorm predictions (or other similar fields), and what's the novelty of this current study (not just the technical specifics in section 2.1). Also, the results section should contain more discussions on how the corrections from this dataset are physically benefiting the storm predictions, not just showing there is a difference and the model is improved. This is a case study after all. Please consider re-organize the manuscript/title to better represent your point and outstand your novelty.
- The results section spends large portions of contents on how the background is

modified towards the observations for each snowstorm event. But the discussions on either A-B or O-B can just tell us that the DA is working properly since the verification is dependent. Any successful DA should make the analysis closer to observations than the background, this is from the mathematical nature of the minimization of the cost function. As indicated between L126-132, there should be plenty of other observations (e.g. D3R) available in addition to the retrievals. Why not use those independent observations to verify your analysis? It will objectively measure if the DA is really improving the analysis.

Minor but still important comments:

- L220: The cost function is not just to measure the difference between the model and observations (otherwise it should be just yobs-Hxb). It is the sum of weighted (B and R) differences between the analysis estimation xa and model background xb (Jb), and between xa and observations y (Jo). The goal of DA is to find an optimal estimation xa that minimizes this J.
- L225: xguess is xb, please use just one subscription for consistency. This y term is also commonly known as innovation in recent DA literatures.
- L240: This comment may be trivial, but can you provide more details on how you perform the cycling? There are two start mode in WRF, restart and cold start. In the restart mode, WRF is able to continue the model integration without interruption by using both the analysis at the current time step and the tendencies from the previous time step. However, our current DA methods usually only updates the analysis, not the tendencies. I'm wondering if you saw any discontinuity issue if you are using this mode. On the other hand, if you are using cold start mode (restart=.false. by default, which only uses the analysis at the current step), it will interrupt previous integrations and perform differently with your control experiments even without DA. You have to let the control exp stop at the same time for consistency.
- L261: Current Fig. 5 may not be the best way to show those info. I don't think it's possible to retrieve the percentile info directly from the figure.
- L261-271: What do these statistics mean? Some of the o-b standard deviations are almost comparable or smaller than the corresponding observation errors (e.g. specific humidity). Are you suggesting your model background is very good already?
- L291-293: Maybe just show the RMSD of O-A and O-B to better present the point if you are not going to physically explain how those improvements can benefit the storm analysis or predictions?
- L299-301: I'm confused by the motivation of the discussion here. Differences should be there as long as you changed the initial analysis. Of cause the differences will spread out as the model tries to balance the changes. But without observations in those areas, how do we know if this "spread of information" is correct or not?
- L308-315: What is your localization length scale? Are these reducing differences in the area of your observations and increasing differences in the remote area reasonable? What's the physical explanations or guesses?
- L324-329: How should we interpret this result from Fig. 8? Is it reasonable for a continuously cycled DA experiment to become more alike the NoDA experiment after several cycles? It appears to me that the impact of DA is fading after cycling. Is this true? Also, the increasing RMSD of wind speed at almost all levels from 21 UTC to 22 UTC (Fig. 8d) seems to be inconsistent with the reducing total RMSD in Fig. 8a. Why?
- Fig. 9 -14: Can you use something like ETS score to quantitively verify the precipitation predictions? Also, in your discussions with the precipitation patterns, can you physically relate them to your previous analysis impacts? E.g. how does a warmer temperature in

the analysis within certain area result in more precipitation predictions.

- L371-380 and others: Why does the accuracy of this IMERG matter? How is this related to the focus of this paper? It reads to me that the discussions on the IMERG is out of no where. If you think this is a important part of your project and is somehow related to the focus of this paper, please add more information in the intro.
- L407-410: Why does the specific humidity increase while the innovations are mostly negative?
- L444-445: What makes the Feb case less significant than the Mar case? Any hypothesis? It appears to me that the O-B differences in Feb case is larger than the Mar case (Fig. 6 v.s. Fig. 12). Could it be related to my previous comment on the specific humidity?