Response to Reviewer

In general, the authors addressed my comments well and I see significant improvements in the manuscripts. I noticed some corrections and clarifications are needed before the manuscript can be accepted.

Thank you for your suggestions. We have modified the manuscript accordingly. Our responses to your comments are below. Additionally, we have made very minor modifications to the axis labels or captions of Figures 11, and A1-A3 for further clarification. We have added the following sentence to *Acknowledgements* as well.

Page 20 lines 309-310:
"… We also thank the two anonymous reviewers for their insightful comments and suggestions that allowed us to significantly improve the paper."

Moreover, we have added the following sentence in *Code and data availability* for further clarification;

Page 20 lines 299-300:
"Readers interested in the specific modifications made to the WRF-CHEM source code for this study can contact the corresponding author."

1. WRF model was indeed coupled with ocean models in some studies (such as COAWST: https://www.sciencedirect.com/science/article/pii/S1463500310001113?via%3Dihub. COAWST code is publicly accessible). So it is not accurate to say "WRF/WRF-CHEM does not have the capability of coupling with an ocean model".
Response: Thank you for pointing this out. We were unaware of the development. The text has been corrected as follows;

Page 7 lines 141-144:
"*~~Current WRF/WRF-CHEM does not have the capability of coupling with an ocean model, which would remain as one of their desirable future developments.~~ Some efforts have been already made to couple WRF/WRF-CHEM with an ocean model as in Warner et al. (2010) and Zhang et al. (2019). Over MC where the varying sea conditions can strongly influence convective activities, the use of such comprehensive models is more desirable and may lead to more realistic simulations.*"

Citation:

1. **Zhang, Y., Wang, K., Jena, C., Paton-Walsh, C., Guérette, É.-A., Utembe, S., Silver, J. D., and Keywood, M.: Multiscale Applications of Two Online-Coupled Meteorology-Chemistry Models during Recent Field Campaigns in Australia, Part II: Comparison of WRF/Chem and WRF/Chem-ROMS and Impacts of Air-Sea**

Interactions and Boundary Conditions, Atmosphere, 10, https://doi.org/10.3390/atmos10040210, 2019b.

2. **Warner, J. C., Armstrong, B., He, R., and Zambon, J. B.: Development of a Coupled Ocean–Atmosphere–Wave–Sediment Transport (COAWST) Modeling System, Ocean Modelling, 35, 230–244, https://doi.org/10.1016/j.ocemod.2010.07.010, 2010.**

2. Saying "latent heat release from droplet activation" is not correct. Droplet activation is an instant process and does not involve much water phase condensation. I think you meant the droplet condensational growth after the activation. Stronger condensation in the polluted case brings down the supersaturation in updrafts, leading to lower supersaturation shown in the figure in the response letter, which would eb a nice figure to be included in the paper (can be a supplementary figure).

Response: We used the expression of "latent heat release from droplet activation" in our previous response to the reviewer. We agree that this expression is inaccurate. In fact, what we really meant was "latent heat release following droplet activation". We have made sure that the latter expression is used in the manuscript. In addition, we have added the following figure of reduced supersaturation, which we have shown in our previous response to the reviewer, as Figure A4 in the revised manuscript along with its explanation in the main text.

Page 13 lines 213-214: "*Figure A3 shows the estimated amounts of maximum latent heat released ~~upon~~ following droplet activation and freezing.*"

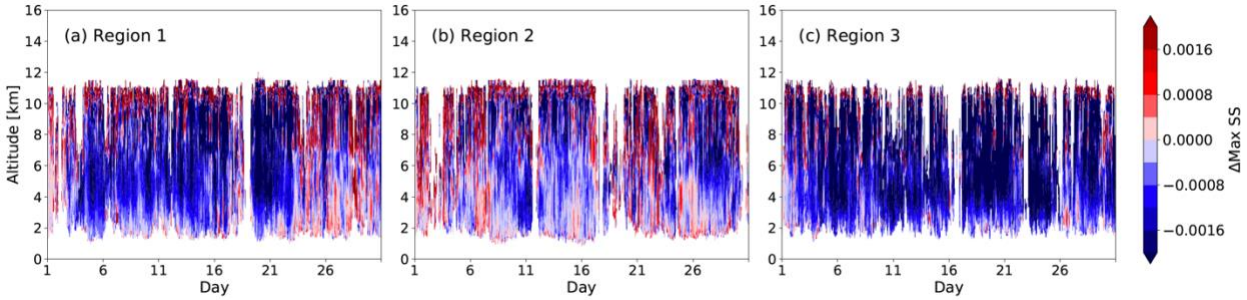

Figure A4: Differences (FIRE-NOFIRE) in maximum supersaturation S_{max} averaged within each region, only sampled where updraft ≥ 5 ms^{-1} and S_{max} > 0.

Page 13 lines 216-217: "*As a result of the enhanced condensation, the maximum supersaturation is lowered inside convection in FIRE (Figure A4).*"

3. The autoconversion rate in the microphysics scheme you used is parameterized as decreasing with increased droplet number. So looking at the autoconversion rate alone is not adequately to say something about warm rain in your case. The best way is to track warm and melted rain in the code. If you do not want to address this since it involves in rerunning model simulations, that is fine since it is not a major point of the paper.

Response: We agree with the reviewer that the autoconverstion rate alone is not sufficient as the representation of the entire warm rain processes. The rigorous separation of warm and cold rain indeed requires the modification of the source code and re-runs of the simulations, and therefore will remain as a future work.

4. The reference is wrong in this sentence "Such aerosol-induced changes in stratiform anvil clouds, namely their extended lifetime and heightened cloud top, have been reported in previous studies (e.g., Fan et al., 2018)". It should be Fan et al., 2013.

Response: The correction has been made in line 227 on Page 14.