Review of revised manuscript “Observed impacts of aerosol concentration on maritime tropical convection within constrained environments using airborne radiometer, radar, lidar, and dropsondes” by Amiot et al.

Recommendation: accept after revisions

Overall evaluation: I think the paper has improved. Some of my comments on the observational uncertainty and interpretation of the observations (e.g., correlation versus causation) are now addressed (at least as far as I can tell). However, I claim that the introduction and motivation for the study presents an unclear (or even misleading) picture of the invigoration conundrum. In my opinion, the introduction is written from the perspective of a person who believes that the convective invigoration in polluted environments (all other factor being equal) is a proven effect. On physical grounds, the pollution-induced invigoration has little merit, especially the so-called cold invigoration that strongly depends on details of the freezing and condensate off-loading aloft (see section 2 in Grabowski and Morrison *JAS* 2020 and discussions in Igel and van den Heever *GRL* 2021 and Varble et al. *ACP* 2023). Below I provide several specific comments that the authors should address before the manuscript is accepted.

Specific comments on the introduction.

The introduction of the pollution-induced invigoration in the third paragraph of the opening section should start with two seminal papers that initiated the discussion: Andreae et al. (Science 2004, not listed in the manuscript) and Rosenfeld et al. (Science 2008). The brief review of studies trying to prove and disprove the impact in observations and modeling should follow. However, the physical basis of the invigoration should be also brought into the picture following the discussion in the papers listed above. It would be appropriate to point out that the warm-phase invigoration depends on the finite supersaturation within cloud updrafts (because reducing supersaturation in polluted clouds increases buoyancy), with the condensation rate depending only on the updraft velocity as long as the supersaturation is equal to the quasi-equilibrium supersaturation (see section 2b in Grabowski and Morrison *JAS* 2020). The cold-phase invigoration critically depends on the details of the frozen precipitation off-loading aloft because the latent heating due to freezing approximately balances the weight of the liquid water carried across the melting level (see section 2a in Grabowski and Morrison as well as Igel and van den Heever and Varble et al. papers). I feel a correct motivation for the study under review is important for providing a proper context. It also reflects in my view rather mixed results discussed in the paper, perhaps because details of the physical mechanisms involved (finite supersaturations, freezing and off-loading cloud condensate aloft) are practically impossible to document in observations. However, I feel the study is an important contribution to the problem, but a proper perspective of the past research is important.

Specific detailed comments.

1. L. 44: What do you mean by “environmental contexts”. Please explain.

2. L. 65. The warm-phase invigoration is not defined. L. 91: the same for cold-phase invigoration.

3. When you bring Fan et al. Science paper, it would be appropriate to bring the rebuttal of their findings in Öktem et al. (*JAS* 2023). Just to show that the science is not as obvious as Fan et al. imply.

4. L. 74: "Numerous other modeling studies…". L. 92: "…despite numerous studies supporting the idea…". Such statements make me believe that the authors do believe in the invigoration, and do not seriously consider those who base their convictions on physical processes and object simple explanations of the "observed" impact of pollution on convection. What about studies that provide picture consistent with theoretical considerations as in Grabowski and Morrison papers or apply more careful analysis of observations (e.g., Varble *JAS* 2018, Öktem et al. *JAS* 2023)?

5. K-index, LR, and many other acronyms. All those should be defined once first used in the text. The reference to Table 1 that explains those acronyms in more detail should be included when these are introduced. I have to admit that, for someone not familiar with airborne instrumentation, the number of acronyms used in the paper is frustrating. As an example, QC for "quality control" is used just 4 times, and I do not think the acronym is needed.

6. L. 136: The reference to Kretschmer et al. is not needed. That paper is from a different field and not really explaining the difference between correlation and causality. The fact that correlation does not imply causality should be obvious in its own right, and it is recognized by some papers the authors cite (e.g., see the abstract in Lin et al. *JGR* 2006 already cited in the manuscript).

7. Text references to entries in the Table 1 can be improved. For instance, L.288: I suggest "…AVAPS parameters marked as "environmental" in Table 1 were employed…". L. 297: "…marked as "aerosol" in Table 1".

8. L. 329: "…result was unexpected…". Why? See my comments above.

References not cited in the manuscript:

Andreae, M. O., Rosenfeld, D., Artaxo, P., Costa, A. A., Frank, G. P., Longo, K. M., and Silva-Dias, M. A.: Smoking rain clouds over the Amazon, Science, 303, 1337–1342, https://doi.org/10.1126/science.1092779, 2004.

Öktem, R., D. M. Romps, and A. C. Varble, 2023: No Warm-Phase Invigoration of Convection Detected during GoAmazon. J. Atmos. Sci., 80, 2345–2364, https://doi.org/10.1175/JAS-D-22-0241.1.

Varble, A., 2018: Erroneous Attribution of Deep Convective Invigoration to Aerosol Concentration. J. Atmos. Sci., 75, 1351–1368, https://doi.org/10.1175/JAS-D-17-0217.1.

Signed: W. Grabowski.