

Interactive comment on “The Impact of MISR-derived Injection Height Initialization on Wildfire and Volcanic Plume Dispersion in the HYSPLIT Model” by Charles J. Vernon et al.

Referee #1

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The quality and clarity of the paper is generally improved. I feel the feel that the last sentence of the abstract overstates the result of the paper (“...HYSPLIT model tends to represent plume evolution better...if the injection height in the model is constrained by hyper-spectral satellite retrievals.”). The final sentence of the conclusion section is more appropriate (“...the use of MINX injection heights, when available, can improve downwind dispersion forecasts...”). “Can” being the operative word. Of the three fire cases two were clearly improved by the use of the MINX data, and the authors clearly explain why, as well as why the Fraser Plateau case is not improved. Of the three volcanic plume cases, for Eyja and Etna I agree with the authors on the conclusion of the similar performance and why. For the third case, Chikurachki, I’m not sure I agree with the authors that the MINX-informed injection height results in a better simulation. My read of Figure 9d is that the ash plume travels in “v” shape from the volcano, with one arm of the “v” on the western side of the circled area and the other on the eastern side. The argument seems to be that the larger area of “high” particle concentration on the western side in the MINX-injection indicates the better performance. The region of “Nominal High” and “Nominal Slight” in Figure 9c is just over the eastern end of the circled area and seems to me to maybe represent that eastern arm of the plume, so I could argue that the nominal injection is better in that sense. I presume the two plume arms are at different altitudes. I find that case to be ambiguous. So my conclusion of the paper is using the MINX-injection “can” help, but often does no better than the nominal case. I don’t think this detracts from the utility of the approach, and the point about smoke injections in high shear environments is well made. So I suggest the paper is suitable for publication with consideration of my point about the interpretation of Chikurachki and a softening of the final sentence in the abstract.

We thank the reviewer for their review and helpful comments. We have softened the final sentence of the abstract and made our evaluation of the Chikurachki case more clear. We added a comparison of the eastern plume in both simulations and the sub-visible portion between the “v” shape of the plumes. We also added an explanation about the improvement in the western plume, overall plume shape, and trajectory for the MISR-initialized simulation. We did not place as much emphasis on the specific locations of ash concentration based on how small the scale is for this case and the limits of the resolution of global meteorological data, like the GDAS used here. The explanation before was intended to focus on the large-scale features like plume shape and trajectory, which has been clarified now in the text. We also touched upon the contouring between the levels of ash concentration as a possible explanation for the subvisible portion between the “v” shape of the plumes.

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