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Atmospheric Measurement Techniques Discussions

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.

Anonymous Referee #1

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Review of "The Impact of MISR-derived Injection Height Initialization on Wildfire and Volcanic Plume Dispersion in the HYSPLIT Model" by Vernon, Bolt, Canty, and Kahn for publication in Atmospheric Measurement Techniques.

The paper presents a series of case studies of simulated transport of plumes from three volcanic and three wildfire events. For each case the NOAA HYSPLIT trajectory model is used to simulate the plume dispersion. Two different configurations of the model are presented in each case, a "nominal" case in which plume injections are handled in a default prescriptive way similar to the HYSPLIT operational configurations, and a configuration in which the plume injection altitude is prescribed based on the satellite-

derived plume height estimates from the MISR MINX product. For the cases of wildfires investigated the "nominal" case vertical profile of smoke injections is derived from an internal plume rise model that uses information of the observed fires. For the cases of volcanic eruptions the "nominal" case injection profiles are from the surface to the VAAC reported plume top. For both fires and volcanoes the alternative MINX-based approach specifies injections from the surface to the maximal observed plume height from the stereo-imagery. The results of the pairs of simulations show that, to varying degrees, you get a different estimated plume transport depending on which of the injection parameters are used. A qualitative comparison is made to MODIS imagery which generally shows a reasonable plume transport in either case. The implication of the study is that, where available, satellite imagery of plume height may be a useful method to prescribe aerosol injection altitudes for transport studies.

The paper is straightforward enough but would benefit from some clarification (see suggestions below), and I recommend publication after revisions.

I do take issue with the conclusion that "MINX can help improve simulated plume dispersion." This is pretty weakly stated, and so a relatively benign conclusion, but on the other hand I don't find much convincing in what is actually presented that leads me to reach that conclusion. I don't think the MINX product would be worse than the nominal assumptions, and I can understand in principle why it should be better (somewhat more definitive than the VAAC reports, observationally based versus parameterized in the case of fires). I'm just not convinced it was shown to be the case. The significant differences that I see are mainly in the wildfire cases, and I would speculate here that the plume rise model is the culprit here and is the strongest case to make that MINX makes a positive impact. But since you cannot really quantitatively compare the simulated trajectory clouds to, e.g., AOD products it is a tough sell to me that it is clearly better.

Some clarifications would be very useful. Regarding MINX, and mainly in Sections 1.2 and 2.1, it is not at all clear what the wind speeds being diagnosed in MINX means.

Do you mean that the time to derive the full set of observations of the same point from all nine cameras allows enough of a time differential to determine the wind speed of the plume? Is this what is meant by “proper motion of plume elements?” What is that time difference? And how does the diagnosed wind relate to the zero-wind and wind-corrected plume heights shown? Are you bringing in wind from a meteorological analysis to make the wind-corrected plume height determination? Also, both red and blue bands are stated to be used in the height determination, but I have no idea how these are used together, or which one is definitive, or even how different they are.

I’m also curious, and this is not clearly stated that I can see, but is the “maximum” height of the MINX observation of the plume truly representative of the injection altitude? In thick plumes I imagine there could be significant plume rise from radiative heating in the aerosol layer, and so the plume may continue to rise above the true injection altitude. On the other hand, diabatic heating the plume rise is probably not part of the HYSPLIT simulations, so maybe the intent is to capture as best as possible the short term plume rise and represent in the injection term. Please say something to explain this line of thought.

Related: page 6, line 3: it is probably fair to say that MINX retrievals are definitively *not* available for events where MISR has no coverage. Suggest you rephrase this sentence.

Regarding the HYSPLIT configuration, mainly sections 1.3 and 2.2, it is also not clear what configuration is used here, although I infer it is either the Eulerian or hybrid method as opposed to the Lagrangian configuration. It might just be better up front to also state the particular configuration, such as what the output grid spacing is (and why is it shown in hexagons?) and the relevant processes in *this* study. (I see later that the wildfire outputs are 0.25 degree grid, but have no idea why the output grid differs for each volcanic simulation. What does this have to do with WMO documentation?) It seems that at least particle settling is considered, given the information on particle size distributions, but are other dry and wet removal processes considered?

I gather the wildfire emissions vary in time throughout the run based on the BlueSky model. Can you expand a bit more on the plume rise model mechanism? Suggest you strike reference to HYSPLIT “MESSAGE” file, which means nothing to me. It’s a model output.

Are the volcanic emissions a single pulse, or distributed over some time?

I think the clarity could be improved by consolidating the MINX descriptions (Section 1.2 and 2.1) and the HYSPLIT descriptions (1.3 and 2.2) into a single subsection for each. I would organize the paper this way: 1) Introduction - first paragraph that is there presently, and then a second paragraph that describes what the study is actually going to show 2) Methods section that describes the tools used 2.1) modeling tools that consolidates the HYSPLIT description 2.2) satellite tools that consolidates the MODIS/MISR/MINX stuff 2.3) Injection parameters that explains the two approaches used to specific injections 3) results (Section 2.3 in current probably belongs in here) 4) conclusions

Page 24, line 16: should reference Figure 9 Page 24, line 19: capitalize “Figure” Page 25, line 5: capitalize “Figure”

The figures are generally quite dense throughout the paper, although Figures 1 and 3 (and similar later) are relatively clear. Figure 4 is impossible to read even blown up on my monitor; I think the source figure is just of poor quality. The supplementary material is copious, but not clearly referenced in the main text. Supplemental Figure 1 is referenced a couple of times, and there is mention of the daily snapshots for the case studies, although the figures are not enumerated in the supplemental section at all. I did not see where in the text the 15 pages of height/wind speed profile plots were referenced.

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