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Interactive comment on "FALL3D-8.0: a computational model for atmospheric transport and deposition of particles, aerosols and radionuclides – Part 2: model applications" *by* Andrew T. Prata et al.

Anonymous Referee #2

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Review of the article "FALL3D-8.0: a computational model for atmospheric transport and deposition of particles, aerosols and radionuclides – Part 2: model applications" by Prata et al.

The manuscripts presents applications and evaluation of the Eulerian transport model FALL3D-v8, alongside a companion paper (Part 1), which presents the model physics and some limited verification. The applications are of SO2 and volcanic ash transport using a data insertion scheme, evaluated using satellite imagery; the transport and deposition of volcanic ash evaluated using tephra samples; and the transport and de-

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Discussion paper

position of radionuclides evaluated using deposition measurements. This manuscript pertains to 'model evaluation', using the language of the journal. Quoting GMD's scope, they suggest that "where evaluation is very extensive, a separate paper focussed solely on this aspect may be submitted. . .typically, this comprises a comparison of the performance of different model configurations or parameterisations." In the case where the manuscript contains "substantial conclusions about geoscience rather than about models, and such papers are not suitable for submission to GMD." In its current form, in my opinion, the manuscript does not sufficiently evaluate the model to fit the scope of publication in GMD. Very little of the manuscript focuses on model evaluation – i.e. the model's ability to reproduce real world physics – with too much focus on the data used to evaluate. The paper has, however, passed the access review stage, suggesting that the topical editor has deemed the manuscript acceptable for GMD's scope. Therefore below I provide my suggestions for revisions to improve the manuscript, followed by technical comments.

1) The manuscript does not sufficiently evaluate the model physics. There are many ways that this could be done in harmony to the companion paper. For example, one evaluation could be through simulation of an ash cloud using the emissions terms (1)-(4) in Sect. 3.2.3 detailed in the companion paper. An additional important evaluation case is the effect of including the fourth-order Runge-Kutta scheme in the solving scheme. The superiority of the new aggregation scheme in v8 over that used in v7 should also be demonstrated.

2) The manuscript should include an example using emissions term (5) from Sect. 3.2.3 in the companion paper (i.e. resuspension). Desert dust would be a sensible choice if the authors wish to move the model away from being purely volcanological. This will better demonstrate dispersion from within the boundary layer.

3) A huge section of the paper is taken up by description of the satellite detection algorithms. This level of detail should not appear in the main text, which should focus on the model. No reasoning is given for using a bespoke satellite detection and retrieval al-

gorithm here. A previously published algorithm should be used using an available data source to improve transparency. For example from SACS (https://sacs.aeronomie.be/) or some similar openly available source. This point is emphasised by the manuscript stating that (Line 321) ‘it should be noted that the retrievals presented here are preliminary and require further cross-validation with other satellite retrievals’.

4) The choice of the 1986 Chernobyl accident seems an odd choice given the relative improvement in measurements during the Fukushima-Daiichi accident. This would also allow the authors to demonstrate the decay scheme for Strontium-90.

5) It is unclear why there is so much emphasis on data insertion. The paper generally reads as justification for using data insertion, which has already been shown in Wilkins et al. (2016). Either the volcanic ash or SO2 example should be dropped as a single example shows that the model is capable of data insertion.

Technical comments:

Abstract: Acronyms (i.e. SAL/FMS) should not be defined in the abstract.

Line 16: Change to ‘15+ year track record’

Section 2.1.: If keeping, a brief description of SEVIRI is needed.

Section 2.1.1.: I would strongly urge the authors to use an ‘off-the-shelf’ product, but if keeping then it needs to be made clear that this is a bespoke algorithm relevant to this test case.

Eq 1: The subscript ‘ash’ should not be italic and it would be clearer is Twc was replaced with -0.5K in the equation.

Eq 3: Place -2K in the equation

Section 2.1.2, Eq 5: What geometric thickness of the cloud is assumed here? Is this the same thickness that is used in the insertion scheme?

Eq 8: Put -2.5 K in equation

Line 172: Specify what you mean by ‘meteorological clouds’, i.e. water and ice clouds.

Line 178: “As mentioned above”, specify the section.

Line 186: Which gases?

Line 187: What ‘amounts’ are you referring to?

Line 203: Is this vertical distribution also the same slab as used in the satellite retrieval? The Puyehue-Cordón Caulle eruption was known to have complex multi-layered cloud structures. How has this been dealt with in the satellite retrieval and insertion?

Lines 212 and 216: FMS and SAL need defining in their first appearance in the main body of text.

Section 3: These validation metrics are only valid for the data insertion scheme. Please provide metrics for the other test cases. How S, A and L are combined into a single metric needs to be detailed.

Line 255: Are the ‘ash mass loading areas’ the areas of the satellite pixels or the meteorological/output resolution of the model? How are the alternate resolutions compared?

Sections 4.1 and 4.2: These section are evaluating the data insertion method, which has been evaluated in previous work, rather than the model itself.

Section 4.1.: What is the grain size distribution used in this case? Assuming it is the retrieved effective radius, how many bins are used etc?

Lines 259-263 and 264-269 can be cut. This information is superfluous to the model.

Line 299: Change ‘meteorological clouds’ to relevant cloud type.

Lines 206-310 can be cut.

Line 370: It is not clear to me why ARW was run first. Why was this initial step needed?

Line 393: ‘factor 3 error band’ should just be ‘a factor of 3’.

Line 399: ‘nuclear accidents’

Line 4.4: In contract to the lengthy explanation of the observations in the other test cases, nowhere in this section does it specify what was actually measured. This section also needs discussion on how general this set up is. For example, the FALL3Dv8 would be unable to be used to model the recent 2017 release of ruthenium-106 in Europe nor iodine-135/xenon-135 during Fukushima.

Line 416: The supplementary material contains nothing on how it accounts for diffusion, deposition nor decay.

Section 5: Many of the conclusions are about the satellite detection scheme and applications of the model, rather than evaluation of the model itself.

Line 456-459: Model performance has not been discussed anywhere else in the manuscript and therefore does not serve as a conclusion/future work. This should be removed unless performance is explicitly detailed elsewhere in the manuscript.

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