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Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Tephra hazard assessment at Mt. Etna (Italy)" *by* S. Scollo et al.

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We would like to thank Dr. Folch for his valuable comments that were included in the revised version. Herein, we report the specific Reviewer comments and our answers.

General comments -The authors use wind profiles from soundings at Trapani, at more than 200 km away from Mt. Etna. An obvious question is how these wind fields differ from those downwind Etna, very especially for low columns (WLL cases) and winds blowing from W (i.e. from Trapani, which seems to be the predominant direction). A mountain exceeding 3 km height does certainly affect lower troposphere wind direction and speed downwind. Is there any particular reason for using IAF soundings at Trapani and not other meteorological datasets (e.g. more local profiles extracted from ERA-Interim reanalysis)? Some comparison would be worth since this could affect somehow the results, particularly for WLL.

We prefer to use the radio-sounding data respect to meteorological datasets. Recent comparisons among data obtained by radio-sounding balloons in Trapani and in proximity of Etna have shown that, for the troposphere, the differences among these two datasets are < 35° with a mean value of 15° for the wind direction and of 10 m/s with a mean value of 4 m/s for the wind speed. A sentence was added to the revised version of the paper.

-Some model input data for the scenarios is missing. A Table summarizing the main inputs for all scenarios/eruptions would be useful.

The reviewer is right and a new table including the eruption source parameters was added.

-I partly disagree with section 4. The authors use two eruptions (1998 and 2001 for SSL and WLL respectively) to "calibrate" some model inputs trough best-fit, including the diffusion coefficient. This can be very misleading because the diffusion coefficient depends on the particular meteorological conditions during the days or the eruption. It is true that the values obtained (200 m2/s for SSL and 1800 m2/s for WLL) are consistent with turbulent diffusion being higher in the lower atmosphere, but the uncertainty is large. This should be at least mentioned. Also, TGSD from the "calibration" cases seems to be very different from those of the scenarios (see Table above). Given that particle size strongly affects the residence time, can this also affect the "effective diffusion"?

In TEPHRA, the Diffusion Coefficient (K) accounts for both the atmospheric diffusion and gravitation spreading of the volcanic cloud. Although the diffusion coefficient partially depends on the meteorological condition that may be different during the days of the eruption, in the Etna case, the contribution of the gravitation spreading of volcanic clouds should be predominant, being towns and cities located near the volcanic vent. These considerations were added to the discussion chapter.

-The authors consider thresholds for roof collapse (100, 200, 300 kg/m2) and damage

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to vegetation (10 kg/m2). However, all results except one are shown for roof collapse only (Figures 10, 11 and 14). Why? It would be more interesting to show same results (e.g. 300, 100 and 10) for the 4 cases.

We don't show the roof collapse case for OES-SSL1 because the threshold is exceeded only those areas without towns. This explanation was included in the text.

-In section 3 nothing is said about topography. Is this also an input for TEPHRA? If not, the impact on the results of assuming a flat topography for Etna should be commented, particularly for WLL.

We added a sentence about the topography to the discussion chapter.

-Finally, aggregation processes are not even mentioned. I know the complexity of accounting for aggregation in the hazard assessment, but this limitation should be at least mentioned. How could affect the results?

The effect of aggregation processes is included in the discussion chapter.

Minor comments -Pg 2, line 22. "distance from" → "distance to"

Corrected as requested.

-Pg 3, line 11. Actually, none of the references cited here, except Barberi el tal. (1990) use numerical models but analytical/semi-analytical. There are several other examples in literature for fully numeric tephra hazard assessment.

The term "numerical" was deleted.

-Pg 3, line 12. Reference Costa et al. (2006) does not deal with hazard assessment.

The reference was deleted.

-Pg 3, line 15. "numerical models of. . ." → "models of. . ."

Corrected as requested.

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-Pg 3, line 20 onwards. Last paragraph describes violent Strombolian activity, but nothing is said regarding subplinian, although both types have been introduced in line 20-22.

Considerations about sub-plinian eruptions were also included in the text.

-Pg 4, line 1. "airports in Catania and Reggio..." → "the airports of Catania and Reggio..."

Corrected as requested.

-Pg 4, line 28. "hazard assessment has been evaluated..." → "hazard has been evaluated..."

Corrected as requested.

-Pg 5, line 1. "The model was implemented to include..."→ "The mode was configured to include..."

Corrected as requested.

-Pg 6, line 2. "These kinds of" →"This kind of..." ??

"Kind" was deleted.

- Pg 6, line 17. Computing techniques?

"Computing" was deleted.

-Pg 6, line 18. MPI is a library, not a series on commands.

Corrected as requested.

-Pg 7, line 15. What do you mean by "small fall time"? Unclear. I would better say "For particles having a fall time lower than FTT".

We replaced "small fall time" with "if the particle fall time is small".

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-Pg 7, line 16. !!" is not defined.

Corrected as requested.

-Pg 8, line 2. The parameter C cannot have units of m2/s given the power 2.5 in eq. (2).

Corrected as requested.

-Pg 8, line 13. “at the time zero” →”at time zero”

Corrected as requested.

-Pg 8, line 20. “implemented” → “modified” or “configured”

Corrected as requested.

-Pg 9, line 20. “events to occur” →”events occurred”

Corrected as requested.

-Pg 10, line 3. “where it began to blow” →”where it rotated” Corrected as requested.

-Pg 10, lines 13-14. “equals to” →”equal to”

Corrected as requested.

-Pg 10, line 24. “of altitude from a vent” →”above a vent”

Corrected as requested.

-Pg 11,line 3. “changed between” →”varied between”

Corrected as requested.

-Pg 11, line 5. “10800s, PR” →”10800s, and PR. . .”

Corrected as requested.

-Pg 11, line 10. “data taken from” →”data from”

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Corrected as requested.

-Pg 12, line 21. The deposit in 1990 covered the WNW flank, implying predominant wind direction of 315° (according to the author's wind direction criterion). This seems to be a very anomalous situation according to Fig 7b and 7c. . .

In agreement to Figure 7, the wind direction toward WNW flank is rare but possible.

-Pg 13, line 10. It is a bit surprising that granulometry for 1990 is much coarser than for other weak eruptions. . .

This is not really surprising because other eruptions of Etna have the total grain-size distribution peaked at 0 phi such as for example the eruption of 4-5 September 2007 (Andronico et al., 2007).

-Pg 13, section 6.2. Granulometry for SSL2 is not given. Is assumed equal to that of 1990?

The granulometry was assumed equal to the 1998 Etna eruption and now it is included in the text.

-Pg 14, line 2. I would not give this volume with 3 decimal digits. . .

The volume was replaced with the total mass and only 2 decimal digits are now reported.

-Pg 14, line 15. "Tall eruption columns. . ."→ "Eruption columns. . ."

Corrected as requested.

-Pg 14, line 17. Falls?

In agreement with the reviewer 1, "falls" was replaced with "fell".

-Pg 15, line 21. "makes a comprehensive. . ."→"compiles a comprehensive. . ."

Corrected as requested.

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-Pg 15, line 22. “reliability” →”accuracy”?

Corrected as requested.

-Pg 16, line 2. I do not understand why these references here, all from other hazards. . .

Neri et al. (2007) was deleted.

-Pg 16, line 4-5. No. Parallel programming has nothing to do with “improving of physical models”. At most, it allows to run more complex models efficiently. . .

We are agree with the reviewer and delete the sentence.

-Pg 16, line 7-8. Unclear sentence.

The sentence was changed.

-Pg 16, lines 11 to 15. This paragraph is true only in part. Even if the quasi-steady approach is used, semi-analytical models still have the limitation of homogeneous wind field, and therefore can not be used for long-range transport (fine ash).

The reviewer has right but in our case TEPHRA is not used for long-range transport.

-Pg 16, line 16. “It should be noted, for SSL..” → “It should be noted that, for SSL..”

Corrected as requested.

-Pg 16, lines 25-26. Disagree. See general comments on this.

See the answer to general comments.

- Pg 17, line 12. “have shown” → “show that”

Corrected as requested.

-Pg 17,line 15. “us” →”as”

Corrected as requested.

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-Pg 17, line 22. Please explain better. . .Why OES-SSL1 affects only agriculture?

The reviewer is right and the sentence was modified.

-How is the big/small is the mass in this scenario compared with WLL (values missing in the table above)?

A table including the eruption source parameters was added to the paper.

-Pg 18, line 19. I do not think that Eulerian/Lagrangian is relevant here. . .

Corrected as requested.

-Figure 6. Do you have 4 soundings per day at Trapani?

Yes and it is written in the caption.

-Figure 6. The wind direction criterion used throughout this paper is not the standard one used in meteorology (0° wind coming from N, rotation clockwise).

Yes, this is true but we used the same method reported in Bonadonna et al. (2005).

-Figures 9,10,11 and 14. Difficult to see. Should be zoomed around the area of interest (where contours exist). All contours should be labeled.

Corrected as requested.

-Figures 12 and 13. For the ERS, the sampling strategy seems to be purely random, which is not the best choice in this case, with too little sampling. For example, why the peak in Fig 13?

The plume height and duration were sampled using a logarithmic distribution and therefore they peak at low values. This information is included in the text.

Reference

Andronico D, Cristaldi A, Scollo S (2008) The 4–5 September 2007 lava fountain at South-East Crater of Mt Etna, Italy. J Volcanol Geoth Res 173:325–328.

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