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Istituto Nazionale di Geofisica e Vulcanologia

Catania, 30 November 2015

Dear **Anonymous Referee 3** ,

Herewith, we report our answers to revisions regarding the manuscript "**PM_{10} measurements in urban settlements after lava fountain episodes at Mt Etna, Italy: Pilot test to assess volcanic ash hazard on human health**" by Daniele Andronico and Paola Del Carlo for possible publication in **NHESS.**
The manuscript now comprises 20 double-spaced pages (submission item: "REVISION") and includes 10 figures and 1 table.

We made the revisions as requested modifying the text and figures 4 and 6, inverting figures 2 and 3, and adding the new figure 9 (with the previous 9 becoming now figure 10). In this letter we explain how and where the reviewer comments have been incorporated in the manuscript.

In our opinion, the main focus of this work is to prove the formation of PM10 during and immediately after the tephra fallout deposition in urban areas produced by Etna explosive activity. On this basis, our contribution would help guide future studies on the potential risk related to the exposure of PM10 particles in this area. Thus, in the revised version, we have stressed this point in order to convince the reviewers and the Editor that our results can represent a starting point to stimulate new studies about this much undervalued problem in the Etnean territory, as is clearly proved in a recent medical study that acute respiratory and cardiovascular diseases, and ocular disturbances, have significantly increased during the ash exposure caused by the 2002 Etna eruption.

We indicated the changes in an annotated version of the revised manuscript file (submission item "Revision_AndronicoDelCarlo_nhess-2015-145").

I certify that the Co-Author Paola Del Carlo is aware of this revision.

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Yours sincerely,
Daniele Andronico

Answer to Anonymous Referee 3

General Comments Many thanks for asking me to review Andronico & Del Carlo's manuscript on PM10 measurements near Mt Etna. The need for this sort of research and analysis is clear but this particular manuscript does not deliver results in a scientifically robust way so it is hard to draw any conclusions from this study.
It is the second time that I have seen this manuscript, given that it was submitted to another journal previously, and it is clear that the authors have not been able to address the major issues with this paper, except to imply in the Discussion that this is seen as a pilot study. The authors attempt to correlate increases in airborne particulate with the November 2011 eruption of Etna. The premise for the manuscript is sound but the short duration of air quality measurements using the TSI DustTrak make the manuscript of poor scientific quality.
We understand your overall evaluation of the work and appreciate the criticisms. We would however like to stress that this is a preliminary research with further work planned in the future. Despite some shortfalls it seeks to make some very much needed headway into this important field, especially in terms of the real situation here on Mt.Etna
We have added a few sentences which describe the main effective limits of our procedure (duration of the measurements and height of the DustTrak) in the "Methods" chapter (3.1 DustTrak measurements) at new lines 175-183. Some other comments (i.e. that related to the counting of the cars) are in our opinion perhaps a little exacting at this juncture; nonetheless, in this letter we answered to them specifically.

Specific Comments

1) The authors only measured PM10 for between 10-16 minutes at each of three locations (during two time periods). With this duration, it is not possible to draw statistically meaningful results from the data. Ten minutes of measurements result in only ten data points as the DustTrak was set to record only once per minute. The authors should have measured for 24 hours at each location, in order to compare with EU standards. If this was not possible, then at least four hours of measurement is necessary in order to collect sufficient data to smooth out the effects of factors affecting background concentrations (e.g., variations in traffic flow). It is also not clear why the authors were not consistent in the duration of their measurement (ranging between 10 and 16 minutes).
During the ordinary monitoring activities of tephra fallout we usually "follow" the fallout deposit to map it and collect representative samples. At that time, we decided to test the presence of PM10 in the air due to 1) fallout and/or 2) passage of the cars on the deposit itself. So, the three measurements were carried out in 3 sites selected due to a) their suitable location with respect to the car traffic, b) dispersal axis of the fallout and c) possibility of properly measuring the PM10 by DustTrak. This is the reason why the measurement tests have different durations (i.e., the time necessary to collect the samples at the specific site).

2) For unknown reasons, the authors chose to take measurements using the DustTrak at varying distances above ground level (1m, 80cm, 40cm) at the different locations. The instrument should have been set up at a standard level, using a tripod, so that data were directly comparable amongst sites. In addition, the DustTrak should have been set up at a height relevant to human exposure e.g., 1.5m, which is considered the 'breathing zone' for adults. Instead, all heights used are relevant to children/toddlers (1m and 80 cm) or are so low as to not be relevant to human exposure (40 cm). The amount of material suspended by vehicles changes rapidly with height above ground (Horwell et al. 2003) so the use of different heights unfortunately renders the data incomparable amongst sites. At heights for adult exposures, one would expect the readings to have been substantially lower than those observed here.
For the same reasons previously described, also the height of the DustTrak varied during or measurements. We have the tripod and the case provided by the company to measure the PM10 outdoors but we did not

have time to do it because our first purpose was to complete our mission, i.e. the monitoring activity for sampling and mapping the fallout deposit before the night conditions could prevent this.

3) The authors do not present data on vehicle counts during this time so it is not possible to smooth the data according to factors affecting the background concentrations (vehicles would have been generating PM10). Also, collection of these data would have allowed quantification of traffic levels rather than generalised statements regarding traffic.
It would have been better to count the number of cars instead of posing a qualitative observation, indeed, as we had already indicated in the text (now at new lines 148-151), we followed the campaign method reported by Moore et al. (2002), pioneers of PM10 outdoor measurement related to volcanic ash fallout, who at Montserrat did tests of 15 min in different locations which they classified based on the qualitative observations of "human activities", including the car traffic. However, on the basis of the volcanological literature we experienced, we never found the counting of cars (at least in volcanic areas) for correlating high PM10 levels with the car traffic; probably this investigation is routinely carried out in non-volcanic areas.

4) As the authors only had one instrument, data were not collected simultaneously amongst sites and, therefore, comparison is further complicated due to the likely changes in particulate concentrations as the day progressed.
For the future, we have planned to conduct a survey for PM10 monitoring with more instruments and available people in order to collect simultaneous measurements.

5) Measurements in November and December were made on different week days (Tuesday and Thursday). Could local variables affect traffic counts on these days?
Based on the perception we had in the road while measuring PM10, the car traffic without question was very similar. We also know these locations and the surrounding areas well in order to report that close by and around them there are no particular activities (for example, outdoor markets) which can influence the increase of car traffic.

Due to these factors, the Discussion contains several statements which are greatly overstated given the data. For example: ": : : our study shows that the presence of fine grained tephra in the ground may be highly hazardous in terms of PM10 concentrations in the air when not quickly removed."
We think that our data suggest extreme caution and precaution should be adopted in case of tephra fallout from Etna to safeguard the population, as well supported by the independent research from Lombardo et al. (2013) showing that acute health effects have significantly increased during the ash exposure caused by the 2002 Etna eruption. This is the crucial message we would like to get over in our paper. Anyway, in the revised version we tried to report it better, for example, at new lines 266-271: "Measurements carried out shortly after the 15 November 2011 Etna lava fountain have shown that the values of PM_{10} are higher than those measured a month after at the same sites. This survey has documented that tephra fallouts on the ground may pollute the air quality, with PM_{10} levels (at least during the short-duration tests) far exceeding the recommended limit for a 24 hour exposure.", and at new lines 286-289: "Sites usually characterised by low PM_{10} values proved very sensitive after the fallout from lava fountains, because relatively low quantities of tephra deposit per square meter (from ~500 g m^{-2} to >1500 g m^{-2}) were able to produce significant concentrations of particulate matter in the air.", while in the cited phrase we replaced "shows" with "suggests" slightly modifying and moving the whole phrase into the Conclusions chapter ("Our study, in particular, suggests that the presence of fine-grained tephra on the ground may be hazardous in terms of PM_{10} concentrations in the air when not quickly removed, and matches well with a medical study on acute health effects due to volcanic ash exposure during the 2002 Etna eruption."; new lines 368-371). At new lines 318-320 we also give a real quantification of the potential effects of the passage of cars and the related tephra grinding and particulate matter remobilization in terms of PM10 particle resuspension measured by the DustTrak: "In other terms, for each 100 g m^{-2} the grinding produced ~2.5 g m^{-2} of PM_{10}

particles in the ground deposit. This is indeed a high percentage of potentially dangerous particles for human exposure directly produced by passing cars."

The final sentence of the abstract also greatly exaggerates the interpretation of the findings; just because there is PM10 in the air does not mean that there will be health problems. Ash clearance from the ground is expensive and disruptive so further evidence of health impacts would be required before this recommendation should be made. I suggest toning down this part of the abstract, especially as the authors only measured PM10 and not smaller fractions which are more likely to be hazardous.
In the final phrase we did not report the equation "PM10 in the air=health problems", but now we more clearly and simply have written that the impact from tephra fallout should receive more attention in order "to avoid potential health risks" (new line 392). In any case, the new medical research we cited does unequivocally show that there is a clear relationship between ash fallout and health effects.

Unfortunately, the submitted manuscript does not present scientifically important results. The authors only measured PM10 for between 10-16 minutes at each of three locations – a total of less than 1.5 hours' work - from which it is impossible to draw meaningful conclusions. The data give an indication that PM10 was significantly raised on 11 November, but I suggest that the authors take these data and use them to plan a further study during the next ashfall event. If they have only one DustTrak, I suggest they use just one location but set up the DustTrak as soon as possible during/after the event and leave it running for several days to show the inevitable decay of airborne dust concentrations over time (and how this varies according to weather conditions promoting dampening or re-suspension of deposits).
We disagree with this comment because 1) it is the first time that these measurements have been performed on Etna; 2) our study is widely supported also by a medical research (Lombardo et al., 2013) that demonstrates an evident association between ash fall and acute health effects in exposed local communities; 3) we believe we have the responsibility as researchers to bring this issue to the attention of the community. This is the reason that led us to describe our research as a "pilot test".

Technical Corrections

1) Page 3927, line 5 – The definition of PM4 and PM10 is incorrect (coarse particles of 4-10 _m diameter). PM10 is all particles sub-10 _m diameter and PM4 is all particles sub-4 _m diameter.
You are right, the words "i.e. PM10 and PM4" erroneously remained in the brackets. We deleted these at new lines 45.

2) Page 3927, line 22 – please can the authors define what they mean by light, relatively heavy and serious fallout?
The range of intensity of these paroxysms is very wide, so that at new lines 68-74 we modified the commented sentence and added the following phrase: "Most of these paroxysms are typically associated with the production of tephra (from ash to lapilli to bombs) injected into the atmosphere and dispersed all around the volcano and further, where they may cause light to relatively heavy and serious fallout. In urbanised areas, in fact, paroxysms characterised by low mass eruption rate (i.e. values of ~2×10^2 kg s^{-1}; 16 November 2006; Andronico et al., 2009) produce deposits prevalently made of ash, while paroxysms with high mass eruption rate (i.e. $4.5 \pm 3.6 \times 10^5$ kg s^{-1}; 23 November 2013; Andronico et al., 2015) produce lapilli fallout deposits, differing also from a hazard point of view. (Dani, change in text too)

3) P3928, line 5 – please replace morphoscopic with morphological.
Done (new line 103).

4) P3930, line 24 – the sampling name convention is confusing, using PM1, 2 and 3, which have been previously described in the text to mean 'particulate matter sub-x _m diameter'. This is very confusing.

You are right. We changed the name of the samples with the acronym of the eruptive vent followed by the number (new line 188).

In the next line, please replace 'weight for m2' with 'mass per m2' to correct the English.
Done (new line 189).

5) SEM data are not presented in the Results but are mentioned in the Methods.
We have reported in the Results section the information provided by the SEM analyses (new lines 240-242-252).

6) For health, the aspect ratio of the particles could have been measured to give quantitative data on particle shape (and therefore the presence, or otherwise, of fibrous particulate).
The comment is pertinent. We do not believe that the particle shape could influence the formation of particulate matter (PM10), because we have no particles with such fine dimensions in the ground material. So given this, do you perhaps mean that if the particles are elongated the possibility that their mechanical breakage (as inferred by our data) could be enhanced? However, due to the fact that grain-size analyses have been carried out by CAMSIZER (Lo Castro and Andronico, 2008), we have automatically measured the shape parameters. In particular, we select the parameters aspect ratio and sphericity. They do not show any particular indication about the elongation of the particles, or rather, their values don't allow assuming them as spheres or as elongated particles. We added a sentence at new lines 243-245 showing the values of these parameters for the 2 analyzed samples.

7) P3934, line 4. The heading here is confusing – what pilot test are they referring to? It is possible they are meaning that the entire study was a pilot but this is not clear and the discussion refers to the December readings too, not just the November ones.
Ok, we deleted the word "pilot" in the heading of the new sub-chapter 5.1 (new line 258).

8) P3934, line 6 – replace 'tephra in' with 'tephra on'.
Done (new line 368 moved in the Conclusions chapter.

9) P3934, line 12 - Data from December are described as being 'significantly different' from November. Were any statistical tests conducted? They are not presented anywhere. Again, I don't believe that these data would be meaningful anyway after 10 minutes of measurement. They should not use this phrase.
At new lines 267 we replaced the commented words "significantly different" with a more simple "higher" and deleted the word "significantly" before "polluted" in the following at new line 269, then modifying the original phrase into "Measurements carried out shortly after the 15 November 2011 Etna lava fountain have shown that the values of PM_{10} are higher than those measured a month after at the same sites. This survey has documented that tephra fallouts on the ground may pollute the air quality" at new lines 266-269.

10) P3936, line 25 – Conclusions. Why might there be a quasi-permanent cloud at 2-3m from ground? This is not based on any data and is not discussed in the earlier sections so should be removed.
You are right, so we have added this information in the Discussion chapter too at new lines 344-347: "Notably, both during the removal of the accumulated tephra and the remobilization by car traffic, a high re-suspension of ash affected the roads causing the formation of a PM10 cloud, thus increasing the ash exposure and inevitable inhalation for several days (Fig. 10)".

References Horwell CJ, Sparks RSJ, Brewer TS, Llewellin EW, Williamson BJ (2003) The characterisation of respirable volcanic ash from the Soufriere Hills Volcano, Montserrat, with implications for health hazard. Bulletin of Volcanology 65:346-362.
Reviewer 3 reported this reference at the end of his revision. We have already cited it in the Introduction chapter.