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Reply

Reply to the Letter to the Editor by Carol Stewart, David E Damby, Ines Tomašek and Claire J Horwell "Experimental design and data relevance in a volcanic ash-leachate health study re. Barone et al. (2021) 'Surface reactivity of Etna volcanic ash and evaluation of health risks' (STOTEN-143248)"

In their letter, Steward and coauthors manifest an overall disagreement on our paper. We will try to answer to their criticisms. First, we want remark here that the letter allowed us to find some transcription errors in our Table 2 and relative figures, and we acknowledge them for this. Also, they have suggested referring to the papers from Cangemi et al. (2017) and D'Addabbo et al. (2015), that are relevant for the discussion of our data on Etna ash, and we accept that not taking into consideration these papers was an error in our published manuscript. Thus, these tables, figures, and related captions and explanations have been corrected in the main text and resubmitted to STOTEN in the form of a formal author's correction with our apologies.

Steward et al. consider "the title to be misleading" in the sense that the word risk is used in it.

We still consider that the title "Surface reactivity of Etna volcanic ash and evaluation of health risks" is appropriate for the conducted experimental work. In our paper, we do not refer our work as a "risk analysis" or "hazard assessment". We perform a set of measurements, including BET, and check the reactivity of materials composing volcanic ash. Indeed, we do not claim that we have evaluated the health risk for the Etna area, but we provide elements for this. In our paper, "risk assessment" is cited one time (once) in the keywords. Moreover, we mention "hazard assessment" at page 2 citing Stewart et al. (2013a) "In line with the aims of the research, the protocol for analysis of volcanic ash samples for assessment of hazard", and also in referring Barsotti et al. (2010). We do not mention risk assessment in our discussion and conclusion. For this these reasons, we consider that the title is appropriate for our STOTEN research paper.

To be honest, for people native-spoken Italian or Spanish languages, there is always some discomfort in the English use of "hazard". Native Italian or Spanish speakers often feel somewhat uncomfortable using the word "hazard". The reason is that "azzardo" (in Italian) and "azar" (in Spanish) is related to gambling or betting, and we prefer always "risk". This is because this word comes from the Arabic, then passed to Spanish and Italian with this original meaning, then to the ancient French, and from there to the English (according to the Oxford Dictionary explanation) reaching a significance somewhat misleading for us. Thus, we can always feel as bearing a blame in our use of risk in English, but otherwise we read frequently in English the use of "risk" closest to our connotation and this comforts us. For instance, in the Mueller et al. (2020) paper quoted as a reference for Steward et al., many times, the world "risk" is used in the same way that we do it. However, we agree with Steward et al. that it could be inappropriate to include "risk assessment" in the keywords, whereas it might be better to use "risk evaluation".

As asserted in the paper, the Gamble's solution was built according to the protocol, details are provided in the Supplementary materials. Moreover, the paragraph 2.3 fully explains the trace element analysis, the use of international standards, details on experimental errors, etc. Also in this case, the reader can independently check the quality of our data.

Steward et al. remark that future research might consider the new IVHHN leaching protocol instead of the older one dating from 2013, and we agree with it. However, by just checking the date of submission of the paper and the arrival date of the new IVHHN in 2020, any observer can understand that the research and most of the writing of the paper were done previously. Also, we hope that Stewart et al. will not reject a priori any research carried out before their new protocol or following other ones.

Fig. 1 clearly represents a simplification of the several procedures that can be found in bibliography, a fact that has been explained in the text. It is a graphic introduction to the subject, and as mentioned in Chapter 1, it does not represent the methodology developed in the article (therefore, it is not cited in chapter 2), as any reader can easily deduce.

Steward et al. disagree with the ultrapure water leaching approach, the chosen granulometry for each leaching experiment, and the use of drinking water guidelines. Also, they use this letter to criticize the papers by Cangemi et al. (2017) and D'Addabbo et al. (2015), and to promote the Bosshnar-Stadlin et al. (2017) model. However, we are not going to comment on this point, since it is a not understandable criticism to third parties.

Just to refer to our paper, Steward et al. do a series of calculations assuming a 1:100 ratio in our leaching experiments, when in our text it is clearly explained that the solid/liquid ratio was 1 g:25 ml. We cannot find a meaning for this, that they used to introduce their Table 1, and we cannot understand why the invoked raw data of Cangemi et al. (2017) and D'Addabbo et al. (2015) are not used to compare the leaching results. In any case, we feel that these data from other authors are interesting and we have incorporated them in some way in our correction for STOTEN.

While choosing the appropriate granulometry, we took into account the Mt. Etna social and geographical context, that is very different than the Hudson one, where there are tenths of thousands of cows in extensive pasture sites. Instead, we can expect people drinking water affected by leachate arrivals, and perhaps some people eating vegetables (lettuce, cauliflower, spinach, grapes, etc.) covered with a thin film of fine-sized ash and not adequately washed and rinsed. In that context, we felt that was more useful to inhabitants in the region to proceed with finer ash. In Sicily, nobody would consent that their livestock directly eat vegetables with sand-sized volcanic ash, and no one would let their young children play on a ground covered with ash.

We agree that F is an element of health interest, being it easily soluble yet rapidly precipitable in a suitable hydrogeochemical context, for example as fluorite. Therefore, even if it is currently not possible for us

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to analyze it, we hope to do it in the future in a monographic study focused on the hydrogeochemical equilibria with bicarbonate-rich waters typical of the Mediterranean basin. Drinking water guidelines are commonly used in leachate-related studies, and we simply used it.

Finally, providing basis for future policy decisions is far beyond the purposes of this work. It is not our business. We remark that the readers can independently check the quality of our data and eventually use them for further analysis, and therefore we cannot understand the statement of Steward et al. in this sense.

Declaration of competing interest

The authors declare that they have no known competing financial interests or personal relationships that could have appeared to influence the work reported in this paper.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.scitotenv.2021.150077.

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