

## ***Interactive comment on “Revisiting the statistical analysis of pyroclast density and porosity data” by B. Bernard et al.***

**B. Bernard et al.**

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Dear editor,

Please find attached the rebuttal letter and the revised manuscript. Most of the reviewer comments were helpful and were accepted. Some other comments are discussed in the following text. In general we think that the quality of the paper greatly improved through this process and hope it will now be suitable for publication. Best regards,

Benjamin Bernard

Reply to reviewer #1 (Jamie Farquharson)

General comments:

C662

#1: model formats: we provided a R code because of several advantages: 1) R is a freeware so the potential users of our code don't need to purchase a licence (unlike MatLab and Excel); 2) in order to use our code, the potential users don't need a deep knowledge of how R work, they just need to know how to prepare the files and the commands to run the code provided in the text; 3) some of the statistical analysis tools provided in this code such as the stability analysis are not possible to reproduce in a spreadsheet format. Nonetheless we think that the reviewer is right when he states that in order to promote a standardised analysis we should provide this tool for different platform. To answer the reviewer comment we propose to include an Excel spreadsheet with basic statistics and weighted statistics. We also translated the whole R code in MatLab format. In addition the R code has been commented adequately in order for potential users to understand better how it works.

#2: Section 2.5 – Graphical statistics: This is not a discussion section, it is a methodology section that make clear reference to the origin of the formulae. The formulae are not new but it is the first time that they are used for density/porosity data analysis. We agree that it might not be sufficiently clear in the text and propose some modification to it (see new text) but we think that the formulae are in the adequate section. The graphical statistics are only inferior for two parameters, median and mean, but for the other they provide a new important information (see section 3.3). Maybe the text is not clear enough on this point. We included a sentence to stress this.

#3: Discussion #3a we agree with the reviewer, this section has been modified including a deeper bibliographic investigation in order to illustrate the interest of our approach. The main issue is that there are too few published database to include a quantitative effect of previous models. Also we would like to keep the text short as it is a short communication. #3b the reviewer raise an interesting point. We think that in the future, when more raw data are available, if this analysis is used on different deposits it will help suggesting a minimum number of clasts for each kind of deposits. We thought this was clear in the text and made some modifications to the text to make it clearer.

C663

#4 Punctuation after equations: we agree with the reviewer and will make sure that sentences that end with a formula/equation will have the adequate punctuation

Specific and technical comments:

Most of the comments are relevant and have been used to improve the manuscript, see the annotated version of the manuscript.

In order to answer to the comments L61-64 and L86-88, an effort has been made to clarify what is an intensive properties (with reference ad hoc) and how the representativeness parameter is obtained. Therefore the equations has been changed a little (even if the solution is still the same) in the next paragraph. This also answer the comment #2 of the second reviewer.

For the figures we chose not to change the color of the arrows because in order to keep those more visible as the rest of the figures a in tones of grey.

Reply to reviewer#2 (Thomas Giachetti)

General comments:

#1: Symbol and definition table: we agree with the reviewer and included a table of symbols and definitions with links to the results of the numerical codes. The n number is clearly defined in the text L105. We don't feel necessary to include a sample set number as it would not be used in any calculation.

#2: equation form: to answer some of reviewer#1 comments the form of the equations has been changed, solving the issue raised by reviewer#2.

#3: stability analysis: this paragraph has been reworked to answer to the reviewer's comment. The figure 4 represent the slope below 5% of absolute error compared to the number of measurements. If the slope is high (>1 for example) it means that if we add some measurements the density average might change a lot. This is actually explained in the paragraph 3.2.

C664

#4: Graphical parameters: we agree with the reviewer. The sentence has been modified to explain better how the Folk and Ward parameters allow a better distinction of the deposits.

#5: Analytic error compared to fluctuation of the porosity mode: in general the relative error on density measurements using the Houghton and Wilson (1989) methodology is under 5% and similar for the Kueppers et al (2005) methodology. Nevertheless the fluctuation of the porosity mode cannot be compared to the analytical error in the case because it is based on the same data. The fluctuation is not an error, it is an artefact due to the use of frequency analysis.

Specific and technical comments in the pdf:

Most of the comments are relevant and have been used to improve the manuscript, see the annotated version of the manuscript.

For the figure 1 the shape of the cumulative curves 1B and 1D are similar and can be superposed through a rotation of 180 degrees and not a mirror as proposed by the reviewer. Therefore we did not changed the text.

Please also note the supplement to this comment:

<http://www.solid-earth-discuss.net/7/C662/2015/sed-7-C662-2015-supplement.pdf>

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Interactive comment on Solid Earth Discuss., 7, 1077, 2015.

C665