

Interactive comment on “Soil aggregation, erodibility and erosion rates in mountain soils (NW-Alps, Italy)” by S. Stanchi

Anonymous Referee #2

Received and published: 23 February 2015

GENERAL COMMENTS

The original idea to compare the soil erodibility factor estimated from RUSLE (K) and aggregate stability is pretty good, less the idea to compare also RUSLE A factor and aggregate stability. The RUSLE consider not only factors soil-related. Although a correlation of K factor and aggregate stability is expected in some environments, there are factors, such as rainfall erosivity, which are independent of aggregate stability. Moreover, in my opinion, the result obtained by applying RUSLE in this experiment is just a prediction. A correct evaluation of using aggregate stability as proxy parameter for soil loss assessment should be obtained comparing aggregate stability and real data of erosion in plot experiment.

I've found some weak points in the manuscript, see specific comments.

C50

Generally, most of discussion is founded on data not shown. I suggest to discuss only data of the manuscript, avoiding to support also hypothesis and statements not demonstrated or verified in the scientific literature. I would invite also Authors:

-to emphasize the usefulness of using aggregate stability instead of RUSLE K factor in assessing soil erodibility

-express clearly if and how aggregate stability measurement could be taken into consideration for calculating the RUSLE K factor, or eventually the annual soil loss rate instead of the RUSLE A factor.

SPECIFIC COMMENTS

Page 186 (Abstract)

It should be rewritten for the most after accepting comments and suggestions.

Page 188

Line 9: not clear what “good development of soil structure” means. Type? Grade? Size?

Page 189

Lines 5-6: I wouldn't follow this hypothesis, since effective erosion rate, estimated by applying RUSLE, depends on many other factors.

Page 190

Line 2: Is the “highly channeled riverbed” an exclusive sign of soil erosion? I would delete this.

Line 15: I should show the LUTs map in the paper. Was the soil map available and eventually used?

Line 17: Please, replace “an overlay” with “intersection”.

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Line 18: It seems that all 25 topsoils have 0-10 cm of thickness. I should say “were sampled at 0-10 cm”. Moreover is not clear if the O-horizon has been sampled or not.

Line 20: In the table 1 I can see elevation and land cover of the sample sites, and physical or chemical properties of the soil samples, therefore “characteristics” in my opinion is not correct. I wouldn't cite the table 1 here.

Line 21: Could Authors please specify which were the spatial sampling criteria? Random? Arbitrary? Representative of each LUT?

Line 21: In which year has been the soil sampled? Could please the Authors specify year and month (or season) in the manuscript?

Line 22: please replace “shape” with “type”.

Lines 23 and 26: This reference is old. The last version of the “soil survey manual” is the 3.0 (2012).

Page 192 Line 19: Is 1680 the result of the R calculation? Please, specify.

Line 27: the symbol C (Carbon) generate confusion for the readers, as “C” has been used for the factor “Cover” of the RUSLE. I suggest to use the term “carbon” or a different symbol (i.e. Corg). I've found the symbol C used for Carbon also in page 191 line 1, page 190 lines 26 and 28, etc.

Page 193

Line 1: “s” in italic.

Lines 1-7: It is poor clear if numbers between brackets are codes or input values for the calculation. Please rewrite this part in different form.

Line 20: another symbol “C” used as “grid size” meaning. I should use a different symbol, “X” for example?

Page 194

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Lines 18-20: Start the sentence with “Organic carbon content. . .” and move the citation of (Fig. 2) at the end of the sentence. Replace, please, “relationship” with “statistical correlations”

Lines 20-23: this sentence need to be revised. It sounds like carbon content as a temporal increase.

Page 195

Lines 2-7: Where are graphs of the models of these statistical correlations? Could, please, the Authors provide them?

Lines 12-15: see comments on Lines 2-7.

Line 17: Tab. 2 has been already cited in the previous line. Please delete this citation.

Lines 25-29: see comments on Lines 2-7.

Page 196

Lines 1-2: This sentence is poor clear.

Page 197

Lines 1-2: I did not find any figure showing the model of correlation between c parameter and soil erodibility.

Lines 11-18. I would avoid in discussing about data not shown, perhaps introducing new results, which must be shown in the specific section of the manuscript and obtained by methods that need to be described in own proper section too.

Lines 18-21. I agree with this hypothesis.

Lines 21-24: Please support this statement with some references.

Lines 24-26: I disagree with this statement. Model in fig. 2 shows good correlations between TOC and parameters of aggregate stability. This could not exclude statistically

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the existence of good correlations, even with higher coefficients, between aggregate stability parameters and content of others cementing substances in soils. I suggest to support this hypothesis showing data, results, and/or appropriate references.

Page 198

Lines 5-9: I don't see any ratio TEC/TOC in table 1.

Lines 9-12 Once again Authors are discussing about data not shown and introducing new results.

Lines 12-17: Have been these statements demonstrated in some part of the manuscript? I suggest to delete this part.

Lines 18-24: Is this the only possible hypothesis explaining the supposed underestimation of K values calculated by aggregate stability? What about the eventual effect of other cementing substances which were not considered in the experiment?

Page 199

Line 11: I didn't find any sign of this correlation in the manuscript.

Line 12: lack of dependence of what?

Lines 13-18: Did Authors try to correlate litter thickness and aggregate stability?

Page 200 (Conclusions)

I didn't find any conclusion about the relationship between A factor and aggregate stability.

Table 1.

Missing values of carbonates, which were determined by gas-volumetric method.

Table 2.

Sample IDs are different from table 1.

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Figure 1.

I would make visible the catchment in a further zoom window, not only the geographic location. Readers expect to find the catchment.

Figure 2.

In the caption, please, replace "relationships" with "correlations". Add the term "stability" after the word aggregation in the same line.

Figure 4.

Is not clear how the K map has been generated. I guess from LUTs map and then assigning to each polygon values calculated from sampling points? Or differently? Please explain this in the manuscript. However I found this figure not essential in the paper, since it deals with analysis of points.

Interactive comment on Solid Earth Discuss., 7, 185, 2015.

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