REFEREE #2

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 is 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. 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.

We are conscious that the aims and the reasons why we investigated the relationships among K, aggregation and erosion estimates were not clearly expressed in the former version (as remarked also by rev#1). We would like to underline that our aim was not to substitute K computation with estimates from aggregate stability, even if aggregate stability measurement is a relatively simple lab test, but only to investigate the relationships between K and aggregation. At the same time, A erosion rate can only be taken as a comparative estimate as we do not provide field validation, which was not the purpose of this research.

Therefore we reformulated the aim of the work (and consequently the abstract and conclusions), clarified the statistical approach with a dedicated paragraph in methods, moved part of the discussion to results, presenting the plots of the correlations that were not present in the initial manuscript.

SPECIFIC COMMENTS

Page 186 (Abstract)

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

We completely reorganized the abstract according to both reviewers suggestions (see also referee #1 comments). The new abstract includes a more detailed methods description, with a better introduction of the residuals issue. Moreover, we emphasized (see your general comment also) that our aim is not to use aggregate stability in alternative to erodibility, but just to investigate the reciprocal relatonships. We hope the present abstract is clearer

New abstract is: *"Erosion is a relevant soil degradation factor in mountain agrosilvopastoral ecosystems, and can be enhanced by the abandonment of agricultural land and pastures, then left to natural evolution. The on-site and off-site consequences of soil erosion at the catchment and landscape scale are particularly relevant and may affect settlements at the interface with mountain ecosystems. RUSLE (Revised Universal Soil Loss Equation) estimates of soil erosion consider, among others, the soil erodibility factor (K), which depends on properties involved in structure and aggregation. A relationship between soil erodibility and aggregation should therefore be expected. On the other handerosion may limit the development of soil structure, hence aggregates should not only be related to erodibility but also partially mirror soil erosion rates. The aim of the research was to evaluate the agreement between aggregate stability and erosion-related variables and to discuss the possible reasons for discrepancies in the two kinds of land use. Topsoil horizons were sampled in a mountain catchment under two vegetation covers (pasture vs. forest) and analyzed for total organic carbon, total extractable carbon, pH, texture. Soil erodibilty was computed, RUSLE erosion rate was estimated, and aggregate stability was determined by wet sieving. Aggregation and RUSLE-related parameters for the two vegetation covers were investigated through statistical tests such as ANOVA, correlation, and regression.*

Soil erodibility was in agreement with the aggregate stability parameters, i.e. the most erodible soils in terms of K values also displayed weaker aggregation. Despite this general observation, when estimating K from aggregate losses, the ANOVA conducted on the regression residuals showed land use dependent trends (negative average residuals for forest soils, positive for pastures). Therefore, soil aggregation seemed to mirror the actual topsoil conditions better than soil erodibility. Several hypotheses for this behavior were discussed. A relevant effect of the physical protection of the organic matter by the aggregates that cannot be considered in K computation was finally hypothesized in the case of pastures, while in forests soil erodibility seemed to keep trace of past erosion and depletion of finer particles. A good relationship between RUSLE soil erosion rates and aggregate stability occurred in pastures, while no relationship was visible in forests. Therefore, soil aggregation seemed to capture aspects of actual vulnerability that are not visible through the erodibility estimate. Considering the relevance and extension of agrosilvopastoral ecosystems partly left to natural colonization, further studies on litter and humus protective action might improve the understanding of the relationship among erosion, erodibility and structure."

Page 188

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

The sentence was actually confusing. We mainly referred to the size and grade of aggregates. We indicated this in the text, now changed into *"A good development of the structure of topsoil mineral horizons in terms of size and grade (i.e. well developed and resistant aggregates) is therefore seen as fundamental in limiting erodibility, i.e. the combination of intrinsic properties affecting soil erosion."*

We refered to the strucure classification used by Wishmeier (see also comment page 193 LL 1-7).

Page 189

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

We reformulated as follows: *"We studied the relationships between soil aggregate stability (wet sieving test), and both erodibility (RUSLE K factor) and erosion rates (A, RUSLE estimate) in a mountain catchment with two different vegetation covers (pasture and forest). The aim was to evaluate the agreement between aggregate stability and erosion-related variables and to discuss the possible reasons for discrepancies in the two kinds of land use."*

Page 190

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

Done, we deleted the first part of the sentence, as we are not speaking of river erosion but rather sheet and rill erosion on slopes. Therefore, the sentence was somewhat confusing.

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

The LUT map has been inserted in figure 1 together with additional columns in table 2 with LUT characteristics and LUT IDs (i.e. intersection among vegetation, geology and slope classes, see comment 12 reviewer #1). Unfortunately, the soil map of Piemonte region is only available for the lowland agricultural areas.

Line 17: Please, replace "an overlay" with "intersection". **done**

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

We sampled only the mineral soil as the litter layer can not be considered for K computation with the adopted formula. We inserted *"Twenty-five topsoils (i.e. always within A horizons, discarding the organic layers) were sampled at 0-10 cm (n=25, of which 9 were represented by pasture, 16 by forest)."*

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.

Tables 1 and 2 were reorganized as follows: new Table 1 (only soil properties) and new Table 2 (RUSLE inputs and LUT-associated characteristics).

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

We inserted in the methods some lines and specified that for each LUT class we sampled a number of points proportional to the total LUT area (as requested also by rev#1). The sampling sites were selected to be representative of the LUT, after preliminary field surveys. "*The number of samples per LUT class was proportional to the LUT type abundance and considered the internal homogeneity of the LUT types."* **The site characteristics of the sampling points are now summarized in Table 2.**

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

We inserted the information as "*Soils were sampled in summer 2012".*

Line 22: please replace "shape" with "type". **Changed**

Lines 23 and 26: This reference is old. The last version of the "soil survey manual" is
the 3.0 (2012). **We left the older as it was the one actuallt used after sampling (year 2012).**

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

Yes, as now specified in the text. As written in comments to reviewer #1, too, in Bazzoffi (2007) a set of 6 equations used to compute R for all Italian municipalities is presented.

We computed the R yearly value using all six equations on a time series of 30 y which was available for the closest meteo station, based on monthly data. Then we averaged the results obtaining a standard deviation of ca 576, which is quite high but takes into account the different performance of the available equations.

This uncertainty affects the erosion rate estimates but is now taken into account to discuss the different behaviour of forest and pasture.

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.

We changed C (for carbon) into "carbon" in text throughout the manuscript in order to avoid confusion with the vegetation cover factor of RUSLE.

Page 193

Line 1: "s" in italic. **Changed. Same was done for the porosity coefficient"p".**

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.

We rewrote the part about structure and porosity codes indicating explicitly that s code varies from 1 to 4 and p code varies from 1 to 6, then listing the description of each class value.

"s is the structure code, varying from 1 to 4, based on aggregate shape and size assessed in the field during soil survey:
(1) very fine or particulate < 1 mm,
(2) fine granular and fine crumb, 1-2 mm,
(3) granular and medium crumb, 2-5 mm, and coarse granular (5-10 mm) and
(4) very coarse granular or prismatic, columnar, blocky, platy or massive, > 10 mm.
The coefficient p is the profile permeability code: , varying from 1 to 6 as follows
(1) rapid, i.e. >130 mm h^{-1},
(2) moderate to rapid, i.e. 60-130 mm h^{-1},
(3) moderate, i.e. 20-60 mm h^{-1},
(4) moderate to slow, i.e. 5-20 mm h^{-1},
(5) slow (1-5 mm h^{-1}),
(6) very slow (<1 mm h^{-1})."

Line 20: another symbol "C" used as "grid size" meaning. I should use a different symbol, "X" for example? **We adopted X as you suggested.**

Page 184. 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" **Done**

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

We rephrased and now the sentence starts with *"With higher carbon contents aggregates were globally more stable ."*

Page 195

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

Yes we now provided the graphs in the new figure 3 containing the following plots:, a)K vs a+b ; b)K vs c; c)Kvs sand aggregation index; d)K vs TOC. This figure includes therefore the plot previously identified as figure 3 (now 3a).

Lines 12-15: see comments on Lines 2-7**We added the plots now provided as additional material (annex A2)**

Lines 25-29: see comments on Lines 2-7. **We added the plot as additional material (Annex A3)**

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

Page 196

Lines 1-2: This sentence is poor clear. **We rephrased as follows** *"Figure 5 shows the plot of RUSLE erosion rates against total aggregate loss. Considering forests and pasture points two different trends were visible."*

Page 197

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

This figure is now provided (see comment page 195 LL 2-7)

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.

In this section we are discussing the original data with further elaborations. The data used in the discussion are now shown in table 2 (Ks values used for K calculations have been added). Kcont is not showed but can be easiliy computed from the data shown. Kcont is not reported in tha table in order to avoid confusion in the reader, as the computation was done only to excude bias.

Lines 18-21. I agree with this hypothesis.

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

We changed the sentence supporting it with citations ". *"In facts, as reported by Tisdall and Oades (1982), in coarse sand-sized aggregates, organic matter acts as a relevant binding agent for aggregates. Moreover, CaCO3 in the studied environment is of primary origin and not pedogenic, thus is not expected to act as cementing agent because of scarce reactivity and large grain dimensions (Le Bissonais, 1996). In addition, iron oxides are not relevant cementing agents for poorly developed soils (Bronik and Lal, 2005). In facts, as reported byTisdall and Oades (1982), in coarse sand-sized aggregates, organic matter acts as a relevant binding agent for aggregates. Moreover, $CaCO_3$ in the studied environment is of primary origin and not pedogenic, thus is not expected to act as cementing agent because of scarce reactivity and large grain dimensions(Le Bissonais, 1996)."*

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 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.

From the statistical point of view most of the variance of the aggregate stability was actually explained by SOM in our dataset. The sentence was rephrased more cautiously and is now supported with literature citation.
"In our dataset, the determination coefficients of the regressions between organic matter and aggregate stability (Fig. 2) suggested a relevant role of organic matter in aggregation (up to 93% of variance explained), as found also by Zanini et al. (1998) during wet sieving experiments in similarly poorly developed soils. "
Page 198
Lines 5-9: I don't see any ratio TEC/TOC in table 1. **We added a column in table 1**
Lines 9-12 Once again Authors are discussing about data not shown and introducing new results.
We inserted the plots as Annex A1 and introduced it in the results, too.
Lines 12-17: Have been these statements demonstrated in some part of the manuscript? I suggest to delete this part.
The results about TEC were not well presented, therefore we moved them to the results section and then commented in the discussion.
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 where not considered in the experiment?
This was a concern also of the previous comments. We underlined the role of organic carbon ad the main cementing agent in the considered environment, supporting it also with new citations. (see comments page 197 (21-24) and (24-26).However we set the sentence in a more hypothetical way. "*Trying to investigate the degree of transformation of organic matter in relation to erodibility (in terms of TEC and K) we did not observe any relevant correlation, which seems to suggest that the total amount of organic matter is more helpful for the purpose of erodibility studies. In facts, variations in SOM contents do not correspond to linear variations in K values, as clearly visible from the original Wischmeier's nomograph (Wischmeier and Smith 1978), thus the relationship disappearance may be caused by restricting the range of organic matter values."*
Page 199
Line 11: I didn't find any sign of this correlation in the manuscript. **This was actually reported at L 25 page 195**
Line 12: lack of dependence of what? **The sentence was rephrased as it was confusing.** *"As the LS was well correlated to A in both land uses, the lack of dependence of A from aggregates loss observed in forests points could be due to a high heterogeneity in the actual effect of forest vegetation in mitigating erosion."*
Lines 13-18: Did Authors try to correlate litter thickness and aggregate stability?
Actually we did not permform this analysis in present work. We agree with the reviewer on this point, and we inserted further insights for research, to address these weaknesses.
We therefore rephrased with a warning on litter effects (then recalled in the conclusions): *"As the LS was well correlated to A in both land uses, the lack of dependence of A from aggregates loss observed in forests points could be due to a high heterogeneity in the actual effect of forest vegetation in mitigating erosion. In forests, the variability in litter quality and thickness is expected to be high, as indeed C stocks in the humic episolum of northwestern Italian forest soils range from less than 3 to about 10 kg m-2 (Bonifacio et al., 2011), and could not be fully accounted by the range of C factor provided by the RUSLE tabular values, corresponding to rather wide vegetation densities ranges. As a consequence, aggregates may develop differently depending on the presence of organic layers giving rise to a large variability in the erosion amounts."*
Page 200 (Conclusions)
I didn't find any conclusion about the relationship between A factor and aggregate stability.
Conclusions were reformulated according to all reviewers' suggestions as follows:
"The soil aggregate stability in a mountain catchment was assessed with a laboratory wet sieving test and the results were compared with the erodibility factor K and the estimated erosion rate (RUSLE model). The K factor was positively correlated with the aggregate loss (wet sieving test), i.e. the most erodible soils (higher K) also displayed higher aggregates losses and quicker breakdown. Land use dependent trends were however observed in the estimate of K from aggregates loss. In facts, the residuals for forest soils were lower in absolute value and with average negative value, while the opposite behavior was found in pastures. Therefore, soil aggregate stability seemed to reflect better the actual vulnerability of topsoils to physical degradation. Several reasons for this behavior were discussed, and a relevant effect of the physical protection of organic matter by aggregates that cannot be considered in the traditional K formulation was hypothesized for pastures. In forests, soil erodibility seemed to keep trace of past erosion and depletion of fine particles. Moreover, while the RUSLE erosion rate could be satisfactorily predicted from aggregates loss for pastures, this was not possible for forests. In forests, erosion estimate seemed particularly problematic also because of a high spatial variability of litter properties. The protecting role of the forest floor in terms of richness and diversity, and not only of cover, in the RUSLE C factor definition, would need further investigation in order to better understand the mechanisms that determine the relationship between soil erosion and structure for the different land uses."
Table 1.

Missing values of carbonates, which were determined by gas-volumetric method. **We added the column with the carbonates content**

Table 2.

Sample IDs are different from table 1. **Now corrected**

Figure 1.

I would make visible the catchment in a further zoom window, not only the geographic location. Readers expect to find the catchment. **The figure has been modified according to the comments of referees #1 and #2. The new caption of Figure 1 will be: Fig.1 Digital elevation model of the study area (left-up); catchment location (right-up); Google Earth picture of the area (left-down); LUT map (right-down).**

Figure 2.

In the caption, please, replace “relationships” with “correlations”. Add the term “stability” after the word aggregation in the same line. **Done**

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?

Yes, this is how it was generated. We will insert this sentence in the manuscript (methods section). *”Then the K factor values calculated from sampling points were assigned to each LUT polygon.”.*

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.

Yes, actually it might be deleted but we prefer to keep it as it gives an idea of the spatial variability of RUSLE parameters and results.