Response

Reviewer 1

In general the paper can be considered as very good and valuable contribution to erosion research and to development of methods for quantitative assessment of soil erosion spatial distribution and rates because the authors are combining the use of fallout radionuclides and stable isotopes. This is a very new approach and this study can be understood as cutting edge science. Therefore I recommend this paper for publishing.

However, the information value of this study can be higher if some issues in data interpretation are explained and discussed more in detail. These items will be mentioned under following bullet points.

1. Page 3, lines 12-14: Authors mention that use of FRN methods is scarce in Korea. If any study exists it should be referred.
   The only existing study we found that involves the use of FRN for soil erosion assessment is from Menzel et al. 1987. The reference was not cited in the correct position of the sentence and thus was moved to the end of the sentence (line 52).
2. Page 5: The geographical and soil data of studied site should be rewritten in more systematic way. They are not well organized. In first few lines (3-9) the study site is characterized very briefly , than at line 9-10 it is written that more data should be find in earlier papers of Jeong et al (2012) and Jung et al (2012) and this makes an impression that more data will be not provided. However, further more data characterizing study site are provided, but in not well organized way. I suggest to mention the geographical and soil data more systematically, for example in this order: data on geology – rock as soil parent material, geomorphology – altitude, inclination of slopes, slope length, exposition, eventually some characterization of relief, climate – temperatures, precipitation amount and regime, intensities and kinetic energy of rainfall, portion of snow from total precipitations, soilscape- soil classification, texture, stoniness, thickness, pH, organic carbon content, carbonate content, vegetation cover and land use.
   We fully agree with the Reviewer 1, the site description was not well structured and thus rewritten (line 94-124). We first present the characteristics of the Haean basin followed by a description of our experimental site. Some of the requested data will, however, not be presented here because they either belong to the method or result section.
3. Page 6, line 4: it is mentioned that core samples were taken down to bedrock. Firstly it would be useful to record to which depth the particular cores were taken. Secondly, there would be useful to refer about stoniness. The core sampling is difficult or impossible in stony soils. If the cores were taken until bedrock, were there no problems with stoniness?
   Indeed, the soils were very stony and to get each profile 3-5 trials were needed. Because of the stoniness and the generally shallow soil cover, for two reference cores the samples depth was not sufficient to reach low ^{137}Cs activity values for the lowermost sample increment. We provide soil depth that we should better term sampling depth in Table 1. For the reference site we included the depth information in lines 128, 129.
4. Page 8, last paragraph: If the authors want to compare erosion rates estimated by FRN methods and calculated by USLE, it is very important to pay attention to input parameters. It would be useful to describe more in detail how the particular factors were calculated and

what are their typical values. What means for example Malvern Mastersizer 2000? If it is some method to calculate K-factor substituting the original Wischmeier´s K-factor equation and K-factor nomograph, it should be referred exactly and explained briefly.

We improved the description of the K-Factor calculation (lines 199-210).

5. Page 9, line 4: There is mentioned that data on bulk density are not shown. It would be better to show these data if they are available.

   We included information on bulk density in line 217.

6. Page 9, line 27-page 10, line 1: It is stated that deviation of correlation between δ 13C and C content is indicating erosion. This is in fact one of key point of this study, because this correlation deviation is used as indicator of soil erosion. Therefore it would be very important to explain why the erosion disturbs this correlation. As erosion is a process mechanically transporting soil particles to which both 13C and 12C are bind, it is not clear why their ratio should be changed by erosion?

   This is indeed a point that should be presented very clearly. We included additional information in this paragraph (lines 240-252)

7. Page10, second paragraph: The stable isotopes are very new phenomenon in erosion research and majority of erosion researchers are probably not familiar with their origin and behavior in soil. It should be explained more in detail in order to make this paper easier usable by wider community of erosion researchers and soil physicists. For example it is not well explained why there is difference in δ 15N between topsoil and subsoil, or between young and old surface horizons. Similarly, it is not clear what is the role of podsolization at reference site with respect to δ 15N. May be even the meaning of "δ 15N" and "δ 13C" should be explained.

   Additional information regarding the concept of the use of stable isotopes was included in paragraph (lines 252-272). The meaning of delta values is given in lines 159, 160.

8. Page 10, line 16: It is mentioned that figure of relation of δ 15N and erosion rates is not shown because there is no correlation. Despite of that it would be better to present the figure. Results should be presented systematically even in case of lacking correlations.

   The figure is presented (now Fig. 5).

9. Page 11, beginning of 2nd paragraph: It is not clear why those cores which did not reach the bedrock were excluded from inventory assessment. 137Cs is usually concentrated in certain layer from surface to certain depth. In most other studies there is done depth incremental sampling to identify the thickness of 137Cs contaminated layer and than the bulk sampling follows this depth but almost never the sampling is done down to bedrock, which is in most soils very deep, and especially the bedrock usually does not occur abruptly, but there is firstly transitional stony layer preventing core sampling above the bedrock.

   This also refers to point 3. Since the soils are relatively shallow and also relatively stony, we thought to have reached bedrock. For the two excluded cores we found even in the lowest depth increment some remaining ^{137}Cs. Therefore, we conclude that not the entire profile was sampled.

10. Page 11, lines 15-17: This study should be mentioned also in overview of literature in introduction chapter.

    Actually it is mentioned in line 51.

11. Page 11, lines 20-25: Here it would be good also to mention how many profiles were sampled at reference site selected for Suweon site.

The information was added in line 310.

12. Page 12, the whole discussion on comparison of 137Cs erosion rates and USLE erosion rates: The comparison of erosion rates estimated by FRN methods and calculated by USLE, should consider the differences in erosion processes and different time scales covered by these two methods. While FRN method cover all erosion (water, wind, tillage) and sedimentation processes occurring at a sampled point during approximately 50 years, USLE represents only water erosion during period of 12 years for which the R-factor is calculated (1999-2011). This should be mentioned and considered in data interpretation and discussion. Further the rain data used for calculation of rainfall erosivity may not represent the studied site exactly. As the rainfall station is 5 km from studied site the microclimatic difference may pay some role because in mountainous areas the orography may influence the microclimate very much.
    The point that ^{137}Cs and RUSLE derived erosion rates are only limited comparable is very important. We added some discussion on this point (see lines 331-340).
13. Page 12, first paragraph: Here the data on study sites are missing for interpretation. Erosion of 7 tons per hectare is relatively high for forest area. May be this is due to steep slope, great slope length or poor grass cover in the forest.
    We already mentioned the steep slopes and the very high rainfall intensity originating from monsoon events. Moreover, the mean values for the transects are within the range of erosion rates measured for other Korean forests (lines 344,345). Information on grass-cover was added in line 122.
14. Page 12, end of second paragraph: Again, more information about quantity of snow in the study area and its possible role in runoff and erosion is needed.
    Unfortunately, snow fall data is not available for this site. Moreover, the freeze-thaw erosion phenomenon described by Park and Woo (1989) is not directly related to the proportion of snowfall.
15. Page 12, lines 16-19: Why only microclimate reasons for differentiation is considered? Why differences in relief can not play some role? We know nothing about relief neither from characterization of site nor from this discussion.
    We only implied that the different exposition has an effect on the precipitation but didn't mention it clearly. Therefore, we rewrote this part of the discussion (lines 331-340). The relief information was already provided in Table 1. And now it is more obvious from Fig. 1.
16. Page 13th, last sentence of Conclusions: Combination of stable isotopes and FRNs is really a new achievement in erosion research methodology and this approach has great potential as it was shown in this paper. Therefore this achievement should be emphasized more strongly and clearly in Conclusions. For example short repetition can be made here telling how the differences in of δ 15N and δ 13C indicate eroded and stable areas and thus help to select the reference sites for FRNs.
    The sentence has been changed accordingly (lines 359-362).

Typing errors, language corrections:

17. Page 4, line 1: „thinks of carbon" is probably a typing error, probably there should be „sinks of carbon"
18. Page 11, lines 23-25: The last sentence of second paragraph is grammatically wrong. It should be rewritten, for example like this: „The 137Cs fallout value for this site (3424Bqm-2 measured in 1986 representing 1884 Bqm-2 after decay correction for 2010) is within the confidence interval of our estimated reference value."

The corrections were considered (line 68 and line 310).

Comments to figures:

Figure 1: The elevation model of the whole catchment is not needed at all as the study site represents only a very small area in its periphery. It would be much better to cut off this part of the catchment where the study site is situated and to magnify this area at the figure. The magnification of the study area should be done to such extent that the particular sampling profiles may be marked at the sampling transects.

Also authors should consider whether it would be not more demonstrative to replace black and white elevation model by contour lines.

If the authors would manage to amend the figure also its title should be changed accordingly to express the content of the figure. For example it should be titled: „Study site located in Haean catchment with sampling design."

Later in the text, tables and figures the transects and sampling point are numbered (reference site 1, reference site 2, TI, TII, sampling points 1, 2, 3, 4, 5 ...). This numbering should be expressed also at figure, otherwise the reader do not know which transect is T1 and which is T2, which reference site is 1 and which 2, which transect points are at ridge, slope and in valley.

The Figure 1 was improved according to the suggestions of the Reviewer 1.

Table 1: The title of the table can be amended as follows: „Soil redistribution rates (erosion represented by negative values and accumulation by positive values) estimated for sampled points of Transect I (TI) and Transect II (TII) by 137Cs method (****here the 137Cs conversion model used should be indicated****) and RUSLE.

Sampling points at transects which are only numbered from 1 to 5 should be characterized as geomorphological units, for example plateau or ridge top, convex upper slope, straight middle slope, concave lower slope, foot slope or valley bottom, etc.

It is difficult to find for each sample point a suitable geomorphological unit. However, we added in the title which sample number corresponds to the upper and lower slope location. Moreover, the geomorphological situation is visible from the slope length and slope angle.

The selection of parameters for the table should be reconsidered and rearranged. Different approaches of logical order of particular columns can be chosen. I propose this order: Input parameters for 137Cs method (there is 1 parameter only – 137Cs inventory), soil redistribution rate estimated by 137Cs method, input parameters for RUSLE (these are distance to hilltop, e.g. slope length and slope angle, soil redistribution rate estimated by RUSLE, and finally eventually other parameters. There are two parameters which are not used as input for any of used methods – slope depth and slope exposition. I am not sure, whether they are really needed in the table. May be at the end of the table the subtraction or ratio (or both parameters) of soil redistribution values calculated by both used methods can be presented.

The Table 1 was improved according to the suggestions of the Reviewer 1. The exposition was removed from the table and added in the figure. K-factor and LS-factor were introduced. We kept the soil depth in the Table, because of point 3 (above); however, we changed the title to sampling depth.

Figure 2: The title may be amended as follows: „ Vertical distribution of mean delta 13C for reference sites and sites affected by soil redistribution (error bars express the standard error)

Explanation of graphical signs at the figure should be „reference sites“ and „erosion sites“ instead of „reference“ and „erosion“.

This was changed accordingly.

Figure 3: The title may be amended as follows: „Relation of carbon content and delta 13C (n express the number of all depth incremental samples at particular transects and reference sites).“

Subtitles of particular graphs should be:

Reference site 1

Reference site 2

Transect 1

Transect 2

The four graphs at this figure are wrongly grouped. If they are grouped like this it looks that each reference site is attributed to particular erosion sites. This is however not true, because both transects are very close to one reference site and much farer from another reference site. It would be better if firstly both reference sites will be presented and than both transect sites.

This was changed based on the Reviewers 1 suggestion.

In fact it is not clear what represents the number „n“. I presume that these are all depth incremental samples at particular transects and reference sites. Or are there involved also bulk core samples of reference sites?

The bulk samples are not involved. Now, with the new proposed title of the Reviewer 1 it should be clear.

It seem strange that on first graph there n = 35 but point at graphs are only 18. Similarly at last graph n = 29 but bullets are 21.

Thanks! Accidently we cut off the x-axis. Now, all the points appear in the graph.

Figure 4: The title may be amended as follows: „ Vertical distribution of mean delta 15N for reference sites and sites affected by soil redistribution (error bars express the standard error)

Explanation of graphical signs at the figure should be „reference sites“ and „erosion sites“ instead of „reference“ and „erosion“.

This was changed accordingly.

Figure 5: There should be applied comments as for Figure 3 accordingly.

This was corrected accordingly.

Figure 6: The title may be amended as follows: „Average vertical distribution of ... with indicated trend line and standard errors (n = 10)

At this figure it is again not clear how many values are involved. The points are 6, but some of them are obviously averages. But why n = 10? If each average is calculated at least from three values than n must be at least 14. It would be good to clarify it.

This was an error in the title of the figure. The number of each depth incremental samples was 6, the total number of reference inventories was 10.

Conclusion:

All listed comments should serve to improve the paper. However, I prefer to allow the authors to consider themselves which comments may be implemented and which not in order to prevent too long delay caused by reworking the paper.

Dear Dr. Fulajtar,

Thank you very much for this detailed review and your helpful suggestions! Especially your feedback for the stable isotope part is very helpful for us and will help to improve the readability of our article.

Reviewer 2

1. General comments

This study shows the suitability of the methods, that was reported by Schaub and Alewell (2009), confirm reference sites for 137Cs-method using stable carbon and nitrogen isotope, although these are not necessarily suitable for quantitative assessment of soil erosion and sediment deposition by themselves. As authors refer, selection of reference site controls the result of 137Cs-method. Therefore, the fact that these methods can apply for the different environment would contribute to more accurate measurement of erosion/deposition rate of surface soil erosion. So I think this study is worthy of publication in this journal after some modification.

2. Specific comments

2.1. In this paper, authors showed that the correlations between nitrogen and carbon content and its corresponding <delta>15N and <delta>13C signatures indicate the disturbance of the site. I think authors should refer whether this indicator could assess the disturbance quantitatively or not.

This point was mentioned in the abstract (line 30) and was included in line 360.

2.2. P. 2571, Eq. 1; This equation describe the depth distribution of 137Cs in an undisturbed soil. I think the equation for estimation of erosion rate shold be added.

The equation was added (line 188).

3. technical corrections

3.1. P. 2574, L. 3; "accessed" should be "assessed". 3.2. P. 2571, Eq. 1; "ho" should be "h0" (subscript zero). 3.3. P. 2568, L. 1; "sources and thinks" should be "sources and sinks". 3.4. P.2584, Fig. 1; This elevation map gives us a little information on sampling site. I could not find which is the transect 1.

The corrections were considered and the Figure 1 improved.

Thanks to both reviewers! The comments given were helpful to improve our manuscript.