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Discussions

Interactive comment on "Combined use of stable isotopes and fallout radionuclides as soil erosion indicators in a forested mountain site, South Korea" *by* K. Meusburger et al.

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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. âĂć Page 3, lines 12-14: Authors mention that use of

FRN methods is scarce in Korea. If any study exists it should be referred. âĂć 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. âĂć 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? âĂć 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. âĂć 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. âĂć Page 9, line 27-page 10, line 1: It is stated that deviation of correlation between ïĄďïĂă13C 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? âĂć 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 ïĄďïĂă15N 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 ïĄďïĂă15N. May be even the meaning of "ïĄďïĂă15N" and "ïĄďïĂă13C" should be explained. âĂć Page 10, line 16: It is mentioned that figure of relation of ïĄďïĂă15N 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. âĂć 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. âĂć Page 11, lines 15-17: This study should be mentioned also in overview of literature in introduction chapter. âĂć 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. âĂć 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 ero-

sion 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. âĂć 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. âĂć 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. âĂć 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. âĂć 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 ïĄďïĂă15N andïĂăïĄďïĂă13C indicate eroded and stable areas and thus help to select the reference sites for FRNs. Typing errors, language corrections: âĂć Page 4, line 1: "thinks of carbon" is probably a typing error, probably there should be "sinks of carbon" âĂć 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."

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 sam-

pling 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. 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. 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. 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". 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. 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? 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. 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". Figure 5: There should be applied comments as for Figure 3 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. 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.

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