Responses to the Reviewer 1:

► Comment 1 ***(General comment)***

This study proposed a method to predict sediment yields of soil erosion and sedimentation in South Korea. The authors first developed an empirical prediction model of specific degradation based on multiple regression analysis and tree data mining. In addition the also predicted gross erosion based on RUSLE and derived sediment delivery ratios by dividing gross erosion prediction estimated from RUSLE by sediment yield estimated with the empirical prediction model (Eq. 12) in order to validate the results of the empirical model.

I found the paper very difficult to read. To my opinion, it is mainly due to the fact that the objective of the paper is not clearly stated. As it stands, this paper gives the feeling of a confused compilation of the previous papers (Kang et al., 2019 and 2021) without the added value of this new paper being really defined and discussed. This feeling is reinforced by the fact that more than half of the discussion focuses on the effect of spatial resolution on the RUSLE results, although this is not a central objective of the paper.

Another major concern is the use of SDR to validate the empirical model (cf. chapter 3.3 Model validation using the sediment delivery ratio) when the authors do not have measured SDR data (nor gross erosion references). To my opinion, the use of SDR data from the literature does not allow any conclusion to be drawn on the validation of the sediment yield prediction (or specific degradation) model, especially as these SDR values are known to be strongly linked to hydro-sedimentary connectivity of the considered watershed, and therefore very site-dependent (see for instance De Vente et al., 2007, DOI: 10.1177/0309133307076485). As a consequence, there is little justification for the paper's general approach of articulating an empirical catchment model and a RUSLE approach to derive SDR values...

In addition, the discussion has to be completely rewritten in relation to the objective of the paper.

Response ◄

We really appreciate the editor's and reviewer's effort in evaluating our manuscript. Your comments were extremely helpful in improving our paper. Following the editor's and reviewer's suggestions, we will conduct thorough revision, and the point-by-point responses to each comment and suggestion are addressed below.

As you suggested, the revised version will provide a clear description about the objective of our study. Indeed, this paper is different from the previous papers (Kang et al., 2019 and 2021). Specifically, the previous study (Kang et al., 2019) discussed the model through a multiple regression analysis with the estimated degradation from the watershed located in alluvial plain river. Moreover, another paper (Kang et al., 2021) focuses on the geospatial analysis of the proposed model using data mining. In this study, additional field measurement data were obtained from mountainous watershed and developed the model using multiple regression analysis and data mining technique. Surprisingly, this study also generated models with similar dependent variables compared to previous models. However, we consider that the present study provided meaningful parameters affecting the erosion and sedimentation processes, and new methodology for evaluating the proposed models. I partly agree with your opinion that this paper is difficult to discern. This is because of the lack of some important information in the manuscript as they were presented in the previous papers. Considering your opinion that the objective of the paper is not clearly stated, the objectives of this study are described as follows:

1. Developing models for the estimated 62 specific degradation. **(**It is one of major differences with the previous studies)**.**
2. Evaluating the proposed model using various methods. Specifically, a geospatial analysis considering the resolution effects and various factor calculation methods (for L and S factors) are conducted for meaningful parameters. Additionally, the proposed models are validated with the gross erosion and sediment delivery ratio.

For the other major concerns (using SDR for validation), we completely agree with your opinion that SDR values are extremely site dependent. Therefore, there is little justification to support our opinion that the model using multiple regression analysis provides better results. However, we strongly consider that Fig. 8 validates the proposed model with the SDR from other studies. For your last suggestion, we will provide additional description in the revised paper.

As the 14 ungauged watersheds were also located in the main five watersheds (Fig. 1 (b)), they do not have significantly different watershed characteristics (i.e., they exhibit similar hydro-sedimentary connectivity of the considered watersheds). However, the suggested SDR with the predicted specific degradation using data mining represents a large difference with the SDR from the gauged river and stream. River managers and geomorphologists should use an efficient and simple method for predicting sediment load at ungauged watershed. This methodology could be considered as an evaluation method of the empirical catchment model for the ungauged watershed.

We have revised the manuscript considering these aspects.

► Comment 2 *(Specific Comment-1)*

Line 20-21. You use "percent water" and "percent wetland and water" as distinct parameters of the empirical regression analysis. What about the collinearity of these 2 parameters?

Response ◄

Thank you for your comment. In the abstract, we mentioned two parameters "percent water" and "percent wetland and water". Indeed, they are in collinearity condition. However, each parameter was used for different models. “Percent water" was a significant parameter in the model using multiple regression analysis, and “percent wetland and water” was used for the model using data mining. As they were used independently, we did not consider multi-collinearity between two parameters. We have revised the abstract by separating parameters for each model to avoid the confusion.

► Comment 3 *(Specific Comment-2)*

Line 22. Please clarify the sentence "Additionally, erosion maps from the revised universal soil loss equation (RUSLE) were generated to validate model variables". Which model variables are supposed to be validated by the RUSLE approach?

Response ◄

Thank you for your comment. In this study, the RUSLE was applied to validate the variables for all suggested models using multiple regression analysis and data mining technique. Specifically, the validation of variables was primarily focused on parameters related to land cover (i.e., anthropogenic factors). “Percent water” for data mining model and “percent wetland and water” for regression model, and “percent of urban” for model were primarily discussed through geospatial analysis.

Additionally, the geospatial analysis supports the proposed result of this study that sediment regimes where erosion occurred were primarily upstream, and sedimentation occurred in the downstream reservoirs and flood plains. To avoid confusion and impart clarity, we have revised it.

► Comment 4 ***(Specific Comment-3)***

Lines 92-93. Finally which values of the trap efficiencies were used in your studies? Why don't you use Heinemann’s formula or a similar formula to estimate them (Heinemann, H. G. 1981. A new sediment trap efficiency curve for small reservoirs. Water Resources Bulletin, 17, 825-830.)

Response ◄

Thank you for your comment. As mentioned in line 92, the reservoir trap efficiencies for multi-purpose dams in South Korea are typically > 96%. Other studies (MOC, 1992) indicated the trap efficiencies of 0.96, which is generally used for various reservoirs with multi-purpose dams in South Korea. Therefore, we used 0.96 as trap efficiencies based on Brune-curve.

As you suggested, using a Heinemann’s formula or other similar formula could be interesting research. However, the formula represents a conceptual design model for predicting sediment trapping performance of small impoundments. We are unsure about applying this formula for the trap efficiencies of reservoir with multi-purpose dams. However, this interesting approach will be tested in future research with additional agricultural reservoir data. Thank you for your valuable idea.

► Comment 5 *(Specific Comment-4)*

Line 99-102. A more detailed presentation of the SD assessment procedure is required in this paper. For future works, I particularly suggest to provide a evaluation of uncertainties in your SD estimations, because this evaluation is necessary if you want to show that your new empirical model is better than the previous ones. In a scarce-data context as yours, it can be assumed that the uncertainties on the SD values will be large (as seen in Fig. 2a for the SY/discharge relationship) and that therefore a variety of models can give similar results when considering the uncertainties in the calibration/validation datasets.

Response ◄

Thank you for your comment. In fact, South Korea lacks abundant sediment data. However, this study is a comprehensive study of the sediment yield based on recent and reliable sediment measurement data. We totally agree with you that the uncertainties in estimated SD should be considered for the new empirical model. The following methods were performed to consider uncertainties.

Mentioned in line 108

1) Specific degradation from low sample numbers (sediment measurements $\leq$ 15) are discarded while developing the model.
2) Unreasonable specific degradation compared to nearby watersheds are removed before developing the model

Mentioned in line 95

3) The specific degradation of the reservoir from the unreliable data (providing the result as design sediment deposition data) are discarded

Mentioned in 126

4) The confidence intervals for the developed model are suggested (additional information is available in Kang et al., 2019)

As an objective of this study,

5) the proposed models are evaluated through geospatial analysis and SDR
6) the suggested models are validated to existing model with additional specific degradation from other studies

In terms of methodologies (MEP or sediment data),

an appropriate reference for uncertainties of MEP and data could be available in the following reference.

- Yang, C.Y., Julien, P.Y., 2019. The ratio of measured to total sediment discharge. Int. J.Sedim. Res. 34 (3), 262–269.

The following reference could provide details about errors on total sediment load with same data.

- Kang, W. (2019). Geospatial Analysis of Specific Degradation in SOUTH KOREA. 234 p. Dissertation

The results for extreme specific degradation could be generated from low sediment measurements. However, this problem cannot be solved immediately (all available sediment data is used). Furthermore, we considered the conservative methods for estimating the specific degradation to compare with previous empirical models.

► Comment 6 *(Specific Comment-5)*

Line 109. I am a bit surprised that some gauging stations were removed from this study whereas many of them were used in the previous ones... To what extent could this partly explain the improved quality of the regression?

Response ◄

Thank you for your comment. The proposed model developed with same data. There are some mistakes in Table 2, and we will revise them.

In the previous studies, the data was used as below.

1) Kang et al., 2019 → 28/35 specific degradation in river (H3, N6, N12, G5, S1, S2, and S4 were discarded)
2) Kang et a., 2021 → 34/35 specific degradation in river (H3 are discarded)
3) In this study,
   Same specific degradation in river
   18/28 specific degradation in streams (additional data)

Compared to Kang et al., 2021, in the previous study, the multiple regression analysis was not considered. Additionally, the previous study used the entire data including low sediment data. Hence, we are uncertain whether it could improve the quality of the regression.

► Comment 7 ***(Specific Comment-6)***

Line 111 (section 2.2). Please consider dividing this section in several sub-sections to make this section easier to read and understand (At least on sub-section for the empirical approach and another for the RUSLE approach). Please also consider to provide more details on the regression model procedure and the parameters tested both for the regression model and the RUSLE approach.

Response ◄

Thank you for your suggestion. Following your suggestion, we have divided the Section 2.2. into several sub-sections to improve the readability of the section

- sub-section for the empirical approach
- sub-section for the RUSLE approach.

Additionally, we will provide more details on the regression model procedure.

► Comment 8 ***(Specific Comment-7)***

Line 121. What is the signification of SWAT-K here ?

Response ◄

Thank you for your comment. Soil type influences soil erosion and sedimentation process. To estimate the percentage of soil type and K factor, the detailed soil map from National Institute of Agriculture Sciences is used in this study. This detailed soil map contains information about 390 soil series. The specific information about the percentage of soil and rock is extracted from soil database from SWAT-K developed from the Korea Institute of Construction Technology.

Overall, SWAT-K provides comprehensive information about soil properties. We will add this information in the revised version.

► Comment 9 ***(Specific Comment-8)***

Line 124. Please clarify what you mean by "based on the RUSLE structure" in a more explicit way. The link is not obvious as the RUSLE structure was developed on a plot based scale and the regression model on a watershed-based scale.

Response ◄

Thank you for your valuable comment. As you mentioned, Wischmeier and Smith (1965) used annual data from 10,000 test plots from the agricultural areas in the U.S to develop the Universal Soil Loss Equation (USLE). The Revised Universal Soil Loss Equation (RUSLE) upgraded the USLE by focusing on better parameter estimation (Renard et al., 1997). Additionally, various researchers have attempted obtaining more reasonable watershed-based scale results of gross erosion for sediment yield.

In this study, the terminology "based on the RUSLE structure" implies the result of existing model.

$$\mathrm{SD} = 2.45 \times 10{-}7A\ {-}0.04P\ 1.94U\ 0.61W{-}0.64Sa1.51Hyp1.84$$

We considered that this result has similar structure as the RUSLE structure, implying that the meaningful parameters have a relationship with each factor of the RUSLE

$$\text{Gross erosion} = RKLSCP$$

- A (watershed area) – average soil loss
- P (mean annual precipitation) –R factor
- Sa (percentage of sand at effective soil depths of 0–10 cm) - K factor
- U and W (related to land use) – C and P
- Hyp (slope of the hypsometric curve) – L and S factor

Some terminologies of "RUSLE structure" are misrepresented in the manuscript, and we will revise it.

► Comment 10

(Specific Comment-9)

Line 200. What is the signification of "W" in eq. 13 ? idem for "Sa" and "Hyp"...

Response ◄

Thank you for your comment.
Existing model explained in line 200 was developed in the previous study.

The parameters can be explained as follows: the percentage of sand in the soil (Sa), percentage area covered by wetlands (W), and slope of the converted hypsometric curve (Hyp).

We will add this information in the revised manuscript. Additional details are available in Kang et al., 2019.

► Comment 11 *(Specific Comment-10)*

Lines 200-201. As far as I can see, the main difference between the previous model (eq. 13) and the proposed new model (eq. 14) lies in the value of the exponent associated with the Hyps parameter (positive in Eq. 13 and negative in Eq. 14). How do you interpret this difference ?

Response ◄

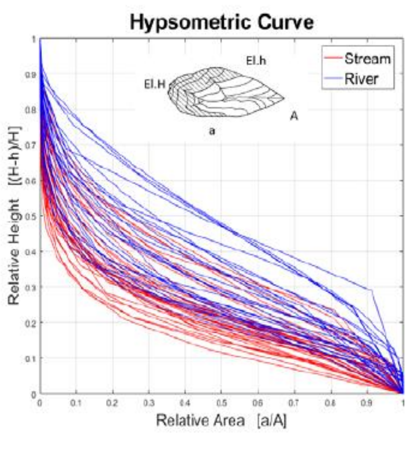
Thank you for your insightful comment. First, we would like to apologize for the lack of explanation. "Hyps (in Eq. 14)" represents the slope of the hypsometric curve between 0.2 and 0.8 of relative area (Below Fig. a).

However, the "Hyp (in Eq. 13)" represents the slope of the logarithmic hypsometric curve. The equation for hypsometric curve is similar with the equation of relative concentration with reference elevation as derived by Rouse (1937). In the previous study, similar conversion of suspended sediment concentration profile was conducted for hypsometric curve and slope of generated results is exported, and the slope of generated results was exported (Fig. b).

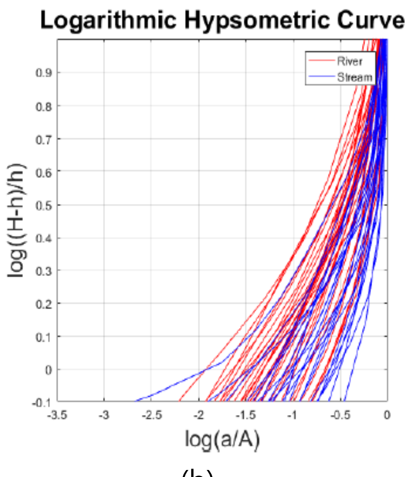
As shown in Fig. (c) and (d), the green line represents a reverse relationship between "Hyp" and "Hyps".

Additional details about this are provided in Kang et al. (2019) and Kang (2019).

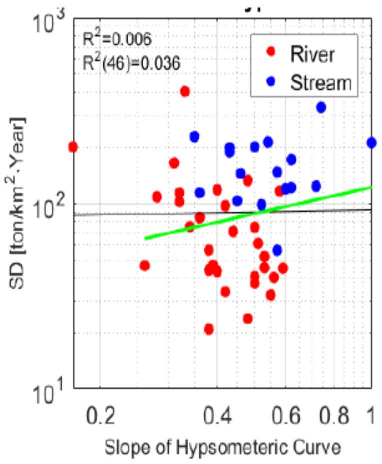
We will revise the manuscript to avoid confusion between "Hyp" and "Hyps".



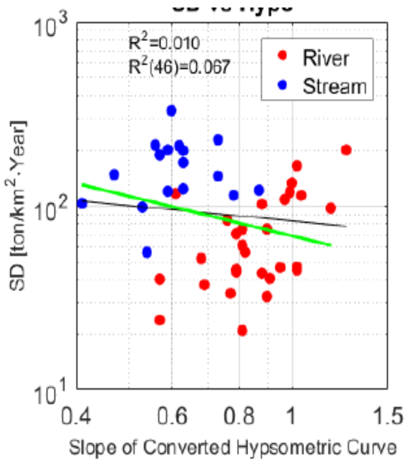
(a)



(b)



(c)



(d)

► Comment 12 *(Specific Comment-11)*

Line 216. What is the signification of SA010 in Eq. 16 ?

Response ◄

Thank you for this comment. It represents the percentage of sand at effective soil depths of 0–10 cm. (line 224)

► Comment 13 *(Specific Comment-12)*

Lines 248-249. "Thus, the results suggest that stream watersheds carry more sediment, and alluvial rivers provided more opportunities for deposition." Do you think this sentence provides useful new information ?

Response ◄

Thank you for your valuable comment. The results that the stream watershed in mountainous region could provide more soil particles and carry more sediment, and the alluvial rivers have more possibilities for deposition (wetland and water) are well supported with field measurement data and geospatial analysis (erosion map using the RUSLE, satellite images, and aerial photos).

Although the proposed results seem general facts, we consider that this is useful information. We believe that these results make a significant contribution to the literature because the general fact was well verified with field measurements and geospatial analysis, which has not been conducted yet.

► Comment 14 ***(Specific Comment-13)***

Line 254. Madiment, 1993 (or Maidment ?) has to be added in the final reference list.

Response ◄

We will add this reference. Thank you for your attentive review.

► Comment 15 ***(Specific Comment-14)***

Line 255. In the sentence "...as well as results of other studies", please provide the references of these other studies and discussed how their context is similar, or not, to yours...

Response ◄

Thank you for your suggestion. We will add the reference for SDR range.

► Comment 16 ***(Specific Comment-15)***

Page 12. Please consider enlearging Fig. 1 as it is difficult to read as it stands .

Response ◄

Thank you for your valuable suggestion. We will enlarge the figure.

► Comment 17 ***(Specific Comment-16)***

Page 15. In legend of Fig.7, your aerial images (in 7 b-d) looks to be terrestrial (or ground) images

Response ◄

Thank you for your valuable comment. The legend of Fig. 7 will be revised.

► Comment 18 ***(Specific Comment-17)***

Page 16. Change ungagued into ungauged in Fig. 8 and explain what means WS, MR and MT

Response ◄

Thank you for your attentive review. “WS” represents watershed, “MR” represents multiple regression, and “MT” represents model tree.

We will revise it to avoid confusion. Additionally, we will change “ungagued” to “ungauged” in Fig. 8.

► Comment 19 ***(Specific Comment-18)***

Page 18. Please consider adding a legend to Figure 11, or removing Figure 11.

Response ◄

Thank you for your comment. We will add a legend for Fig. 11.

► Comment 20 ***(Specific Comment-19)***

Page 19. In Table 1, Have you an explanation for the very high dry mass density 1 for SR1 (value of 2.1).

Response ◄

Thank you for your constructive comment. It is field measurement data, and the value was based on the survey report. We considered the site measurement data, and high dry mass density was obtained from the watershed or reservoir characteristics.

► Comment 21 ***(Specific Comment-20)***

Page 20. In Table 2, please explain why the SD values are not similar from those shown in your previous paper (e.g., Kang et al., 2019). And why some of the stations (e.g., NU4 and NU5) are marked as "discarded and/or unreasonable results) but still considered in Table 7.

Response ◄

Thank you for your valuable comment. In this study, we double checked the discharge data (compared with other daily discharge data set and H–Q relationship). When the discharge was not reliable, they were removed.
In other references, every SD values are consistent (dissertation, Kang et al., 2021, and this paper have same value).
Additionally, the SD from low sediment samples or unreasonable SD (compared to nearby watershed) were discarded. As replied to comment 6, there are some mistakes in Table 2. The proposed model was based on same data.

► Comment 22 *(Specific Comment-21)*

Page 21. Several variables in Table 3 are not defined. What is the significance of "Line:total", "Main" ... What is the difference between "Middle relative height at middle relative area", and "elevation at middle relative area", and "middle elevation" ?

Response ◄

Thank you for your detailed comment. As the information of 51 variables represents considerable data, we only defined the variables that are used in the proposed model. If you suggest to add all variables, we will consider adding it.

Linear watershed characteristics include parameters that describe stream network. The stream length was analyzed using the Korea Reach File (KRF) version 3 provided by the Ministry of Environments (ME). It was classified into three parts: (1) mainstream length (Main); (2) tributary length (Tri); and (3) total stream length (Total).

In terms of relief aspects,
"Relative height at middle relative area" vs ""Elevation at middle relative area"

- Hypsometric analysis is the distribution of surface area with respect to elevation. It has been typically used for calculating hydrologic information because the basin hypsometry is related to flood response, soil erosion, and sedimentation process. The hypsometric curve can represent the hypsometric analysis, and it typically explains the normalized cumulative area and normalized height from the outlet of the watershed outlet.
  As shown in Fig. 10 (a), the "relative height at middle relative area" is the normalized height at middle relative area (0.5). "Elevation at middle relative area" represents elevation (not normalized) at middle relative area (0.5)

"Middle elevation" is just median value of watershed elevation.

Additional specific information is available in the following reference:

- Kang, W. (2019). Geospatial Analysis of Specific Degradation in SOUTH KOREA. 234 p. Dissertation

Moreover, “middle relative height at middle relative area” will revised as, "relative height at middle relative area"

► Comment 23 ***(Specific Comment-22)***

Page 22. Please explain why the conservation practice factors (P-value) for each land use type increase with slopes (Table 5). In general, the conservation practice factors do not have a direct systematic link with slopes. Morover, the influence of slopes in RUSLE is already taken into account through the LS factor.

Response ◄

Thank you for your comment. There are several methods for estimating practice factors.

In this study, we attempted using official notification “Notification on investigation of soil erosion status” from the Ministry of Environment for using “RUSLE”.

The notification is proposed considering South Korea conditions and field survey.