

Comment on hess-2021-457

Christian Stamm (Editor)

Editor comment on "High-resolution erosion susceptibility data for agricultural lands of Finland" by Timo A. Räsänen et al., Hydrol. Earth Syst. Sci. Discuss.,
<https://doi.org/10.5194/hess-2021-457-EC1>, 2021

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Comments hess-2021-457\\
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Dear authors,\\
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I list a number of more general and detailed issues. \\
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General aspects:\\
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\item[Language:] In general, the text is easy to read. Still, sometimes there are issues  
with the grammar such as missing articles (e.g., L. 19, 34 - 35).
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\item[Typos:] There are are few instances with misspellings (e.g., L. 38, 218).
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Detailed comments:\\
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\item[L. 48:] Strange sentence. What does \textit{implementation through natural  
constraints} mean?
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\item[L. 132 - 133:] How was the LS factor linked to the actual parcels? How do upslope  
fields influence downslope parcels?
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\item[L. 133:] What is the empirical basis for the claim that sink filling increases the  
errors? Sinks in a DEM can be real (and should be accounted for) or can be artifacts. Why  
did you not distinguish between the two situations?
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\item[L. 162:] According to my knowledge, the RUSLE model does conceptually not  
account for sub-surface transport through tile-drains. Nevertheless, you compare RUSLE  
simulations to empirical data of the sum of surface and subsurface sediment transport.  
Should that not be reflected in a conceptual modification of the RUSLE model including a  
model parameter accounting for the split between surface and subsurface transport?
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Additionally, the subsurface flow can induce mobilisation of soil particles also within the  
soil profile, especially in the vicinity of subsurface drains because of the disturbances of  
the soil profile due to the installation of the drains. How is this accounted for?
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\item[L. 274 (Tab. 4):] Please provide the number of observation years and the standard deviation of the measured erosion.

\item[L. 303 - 304:] Are these novel findings?

\item[L. 318 - 319 (Fig. 4):] The high resolution DEM only affects the the LS factor, doesn't it? Hence, only this map should make any difference to previous estimates, shouldn't it?

\item[L. 349 (Fig. 6):] To which degree are these findings novel?

\item[L. 353:] Replace \textit{high field area} by \textit{areas with a large fraction of arable land} (or similar).

\item[L. 357 - 358:] Is that statement not trivial given the definition of the EMI index?

\item[L. 445 - 446:] Where is the evidence that it was indeed the lack of high resolution risk maps that prevented the implemented of targeted measures?

\item[L. 448 - 452:] The four bullet points seem rather similar to me. Can you more precisely explain what the differences are?

\item[L. 454 - 456:] Is this a novel result?

\item[L. 458 - 460:] This seems to be quite standard knowledge, or am I wrong?

\item[L. 471 - 472:] Given that you have access to actually crop management data, it should be straight forward to assess the effects such modification in practice, shouldn't it?

\item[L. 477 - 478:] Where is the evidence for that? It is a frequently used arguments by natural scientists that improved model will enhance management, but which evidence demonstrates the validity of the claim?

\item[L. 481:] The previous erosion risk estimates were rather similar (see L. 384 - 389). So in which sense has the understanding of erosion risk considerably been improved?

\item[L. 487 - 488:] What do you mean by considering erosion risk across multiple scales? What does it mean from a scientific point of view, what does it mean in practice?

\item[L. 489 - 490:] Which aspect provides new opportunities for analysing the P- and C cycle given the similarity of previous erosion estimates?

\item[L. 491 - 492:] Where can one see this demonstration? The manuscript does not compare how policies or planning has changed due to the new erosion risk map.

\end{description}

Sincerely \\

Dr. Christian Stamm

Editor HESS

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