Hydrology and Earth System Sciences Discussions

Hydrol. Earth Syst. Sci. Discuss., author comment AC3
https://doi.org/10.5194/hess-2021-457-AC3, 2021

Reply on RC2

Timo A. Räsänen et al.

Author 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-AC3, 2021

Dear Pedro Batista,

Thank you for your thoughtful and constructive comments (EC). They made us to rethink our analyses and they provided a great learning opportunity. Most of all, they help to improve the manuscript. Altogether, the comments from the editor and the two referees prompted major revisions, and we have revised the manuscript. The major revisions are:

- The country scale results on actual erosion and erosion management index were removed from the revised manuscript due to limitations in the used C-factor data, which was pointed out by Pedro Batista (Referee #2). Correction of the C factor data requires considerable research work and decision was made to leave it as future work to be published in another publication.
- The LS factor and consequent erosion data were recalculated to account for field borders.
- The results on RUSLE evaluation, erosion susceptibility and susceptibility near water bodies were slightly restructured to accommodate the changes caused by removal of the two parts in mentioned in the previous point.
- New sensitivity analyses were added that provide estimates on
  - the propagation of uncertainties from RUSLE factors to erosion estimates
  - effects of location specific cropping and management practices and temporal rainfall erosivity distribution on C- factor values and the consequent erosion estimates
- The terms "potential erosion risk" and "actual erosion risk" of the original manuscript were replaced by new terms, "erosion susceptibility" and "actual erosion" as proposed by the referee to avoid the misuse of the term "risk".

Thus, the new findings of the revised manuscript are:

- New high-resolution (two-meter) country scale erosion susceptibility data for all agricultural land in Finland
- New evaluation of RUSLE and its performance in boreal conditions, which considers also different spatial scales and issues relate to upscaling from field parcel to larger spatial scales
- Improved scientific understanding of agricultural erosion and its spatial distribution

These findings provide new opportunities for research and erosion management. In the following we provide our comments and responses (AC) to the referee comments (RC) point by point.

Comments:

RC1: I enjoyed reading your manuscript "Improving the agricultural erosion management in Finland through high-resolution data". I appreciated the use of the high-resolution DEM and the field-parcel data for model parameterisation. However, there are some issues, which, in my opinion, need to be addressed before the manuscript can be considered for publication.
AC1: Happy to hear that you found our manuscript interesting. Thank you.

RC2: First, you state that the goal of your study is to produce 'erosion risk data for agricultural lands in Finland'. However, what you produced are soil loss maps, which do not translate into erosion risk assessments. I understand this is a common misconception in erosion modelling research, but the manuscript should not add to the confusion. For instance, you assume risk is the modelled erosion value for a given location, without stating the assets at risk, what negative consequences erosion could bring to these assets, and what are the probabilities of these consequences occurring.
AC2: We agree. This is an unfortunate blunder in terminology from our side. We have removed the word "risk" from the revised manuscript and used terms "erosion susceptibility" and "actual erosion".

RC3: Second, there are serious problems with methodology used for calibrating the C factor for the RUSLE. From what I understood, your approach considerably deviates from the original USLE or RUSLE methodology, neglects the influence of rainfall erosivity and crop stages, and relies solely on a deterministic parameter optimisation procedure, without considering the uncertainty in the input data and the calibration methodology. These issues are described in detail below, and please correct me if I am wrong. Third, there is no uncertainty analysis. Although you dedicate a large amount of text to pointing out the uncertainties in the model, you did not attempt to quantify them. In my opinion, if you wish public policy to be guided by your results, you should at least provide a forward error assessment to quantify the uncertainty associated to the model parameterisation. For instance, you state that your study provides a generalisation of the effects of management practices on erosion. Do you believe it is sound to provide such a generalisation, based on a limited number of observations, without a measure of uncertainty?
AC3: We agree with the comments here, and we have revised to the manuscript accordingly. In the revised manuscript we use the original definition of C from Renard et al. (1997), perform sensitivity analysis of forward propagation of uncertainties, and make a preliminary analysis on the effects of location specific cropping and management practices and temporal distribution of rainfall erosivity on C factor values and erosion estimates. We exclude the country scale estimation of actual erosion (A=RKLSCP) from the revised manuscript given the spatial variability C and aim to publish it later in the future, as explained in the beginning of our comment. These revisions are explained also in more detail in the responses to your specific comments.

RC4: As pointed out by Christian Stamm, I also have some concerns regarding how tile drainage and field borders were incorporated (or not) into the modelling.
AC4: We have justified the incorporation of subsurface drainage in a following way in the revised manuscript. According to Renard et al. (1997) subsurface drainage is considered in the P factor. Bengtson and Sabbagh (1990) suggested an average P factor value of 0.6, which was also recognised by Renard et al. (1997). However, the research on how the subsurface drainage should be considered in the RUSLE is limited, and to our understanding there is no commonly accepted approach for this. Except, RUSLE2 (USDA,

2013) incorporates a method for this, which considers the changes in K factor due to the subsurface drainage, but in our case the data was too limited to consider this. Therefore, we chose to follow the original suggestions by Renard et al. (1997) and Bengtson and Sabbagh (1990). According to our literature review the subsurface drainage reduces erosion 8-90% (on average 38%) (Bengtson et al., 1988, 1984; Bengtson and Sabbagh, 1990; Formanek et al., 1987; Gilliam et al., 1999; Grazhdani et al., 1996; Istok et al., 1985; Maalim and Melesse, 2013; Skaggs et al., 1982), which results to average P value of 0.62. We used the P value of P 0.6 as suggested and used earlier in Finland by Lilja et al. (2017a). Also, the research in Finland showed that substituting of old drainage pipes and drain trenches with new ones reduced erosion up to 15% on a clay soil (Turtola and Paajanen, 1995). The use of sum of surface and subsurface sediment is justified also by research. In Finland, it is observed that up to 50-90 % of the erosion loading at clay soils occurs via subsurface drainage (Finnish Environment Institute, 2019; Turtola et al., 2007; Turunen et al., 2017; Warsta et al., 2014, 2013) and that in clay soils erosion material in subsurface drainage flow seems to originate mainly from the surface soil (Uusitalo et al., 2001). In this Finnish research the origin of the erosion material was determined by an analysis of Cesium-137 contents of soil layers and eroded soil material in the subsurface flow. The modelling studies in Finland also support this finding (Turunen et al., 2017). A study from Norway also reports that soil material in the drain flow originated most likely from the plough layer and, and the soil material was transported to subsurface drains via cracks and macropores in the soil (Øygarden et al., 1997). However, we do acknowledge that the consideration of subsurface drainage in the P factor lumps a complex and poorly understood process into single value and has therefore limitations and is a considerable source of uncertainty. In the revised manuscript we perform a sensitivity analysis which also includes the uncertainty in the P value. This justification was added to the revised manuscript. Note also that our modelling approach tests the above assumptions against empirical data and the resulting performance can be considered reasonable.

Regarding the field borders, the LS factor was recalculated in the revised manuscript to account for the field parcel borders. We explain this in revised manuscript in a following way: "The LS factor was calculated from a two-meter resolution LiDAR-based digital elevation model (DEM) of Finland (National Land Survey of Finland, 2020), and by using the SAGA-GIS Module LS Factor (Conrad, 2013) and the method of Desmet and Govers (1996) with default settings. The LS calculation was performed in two-meter resolution for agricultural lands in 301 hydrological units that consisted of river basins, sub-basin groups (Finnish Environment Institute, 2010) and groups of islands (Fig. S1). The agricultural lands were defined according to the field parcel data from Finnish Food Authority, which contains over one million vectorized field parcels and it accounts almost all agricultural land in Finland. The use of vectorized field parcels treated each field parcel as an isolated hydrological unit (in terms of overland flow) in the LS calculation to account for the effects of varying landcover on surface runoff, as recommended by Desmet and Govers (1996). The approach is considered adequate as in Finland the fields are well drained and commonly surrounded by ditches, which advocates for the hydrological isolation of the field parcels. However, adjacent field parcels that shared the same parcel border were treated in the calculation as a single field parcel, since it is common that uniform field areas are divided into separate field parcels, as reported in the data of Finnish Food Authority, for annual cropping and management purposes." The evaluation of RUSLE at the seven field sites was not affected by the recalculation of the LS. Their LS factors were originally calculated using field borders.

RC5: L26: I did not understand what you meant with "the key process causing erosion is hydrological".
AC5: This is just poor language from our side. The purpose was to say that soil erosion by water is a hydrologically driven process. We have revised this in the revised manuscript.

RC6: L34: Do you mean erosion is affected by the short growing period?

AC6: The word “process” was removed. Thank you for noting.

RC7: L36-37: I think superscripts are missing here.
AC7: Corrected in the revised manuscript.

RC8: L64: In my opinion, acquiring spatial data for parameterisation and calibration is more of a challenge than computational power.
AC8: We have added the issue of parameterisation into the discussion.

RC9: L68: Could you also state some of the limitations of the USLE-family models here? For instance, you cite our paper to corroborate the ability of the USLE to simulate annual loads – I imagine you mean at the erosion plot scale. However, our review also shows how spatially distributed erosion rates compare poorly to independent measurements.
AC9: We have clarified that the sentence refers to the plot scale and we added the following sentences to the revised manuscript: ”The USLE type models are also commonly used over large spatial scales with varying spatial resolutions, but the success of these applications varies more than on plot scale. It is however, observed that the spatially distributed approaches are often able to rank erosion-prone fields if high quality data are available for parameterization, but the actual erosion rates compare poorly to independent measurements (Batista et al., 2019)”

RC10: L81: Wouldn’t the RUSLE require longer time series for estimating the R factor?
AC10: The short data period can indeed cause uncertainties in the R factor, since in short timeseries for example exceptional events can have a greater effect on the average R estimates. This may lead to uncertainties in the magnitude and the spatial distribution of the R. Following sentence was added to the discussion section of the revised manuscript: “The R factor was based on European data, which in the case of Finland was based on measurement data from 60 stations from the period 2007-2013. This data period is relatively short and may therefore cause inaccuracies in the average level and spatial patterns of rainfall erosivity.” However, the used data is currently the best available, and improvements can be made by estimating new R factor data, but this is beyond the scope of the manuscript.

RC11: L82-87: How are you defining risk? Can risk be expressed in mass area-1 time-1? It seems to me you are calculating erosion rates, which of course can be a threat to multiple assets (e.g. the soil itself, downstream infrastructure, etc). However, threats, assets, and potential consequences need to be identified in order to produce an actual risk assessment. This is a common misconception in model-based erosion risk assessments, in my opinion.
AC11: We have removed the word “risk” in the revised manuscript and as explained earlier. We will use terms “erosion susceptibility” and “actual erosion”.

RC12: L96-97: With a 2 m resolution, couldn’t you assess risk at field-block scale?
AC12: Yes. The developed two-meter resolution erosion data, which will be publicly available, allows field parcel and within-field parcel scale analyses. In the manuscript we however, considered that it is more useful to introduce the developed data and the spatial patterns of erosion primarily on a broader scale, since the development of country scale data is the key novelty of the research.

RC13: L112: These are not the sub-factors defined in the RUSLE (see Renard et al. 1997), correct? If so, please make it clearer you are using an adaptation.
AC13: In the revised manuscript we use the original definition of C by Renard et al. (1997).

RC14: L128-130: What is the ICECREAM model? In general I could not understand how the K factor was calculated. Are you taking single K factor values for mapping units in a

soil map? This can introduce large errors to model outputs (see Van Rompaey and Govers, 2002).
AC14: ICECREAM is a version of CREAMS model that has been adapted to boreal winter conditions (Rekolainen and Posch, 1993). The spatial K factor data was not calculated in this study, and it was developed earlier by Lilja et al. (2017a, 2017b) , by using the K values that have been estimated and evaluated in national research (e.g., Rekolainen and Posch, 1993; Bärlund and Tattari, 2001; Bärlund et al., 2009; Huttunen et al., 2016; Rankinen et al., 2001). The soil class specific K factor values are shown in the supplement. The Van Rompaey and Govers (2002) was very interesting reading, and we have taken a note on this and also included it in the discussion section of the manuscript. However, we did not follow their idea of simplifying K factor data, given that their findings are application and location specific and therefore they don't recommend direct extrapolation of their findings. However, as a sensitivity analysis such simplification would be an interesting, but we decided to leave it for future work. Also, the simplification of K factor data would have resulted in loss of known soil regions in Finland that affect the regional distribution of erosion. These soil regions include, for example clay soils areas in Southern Finland, and the highly erodible soils in the river valleys in the western coast. We have clarified the nature of the K factor data in the revised manuscript.

RC15: L134-135: This is an interesting point about the sink filling. Have you made any tests with and without it?
AC15: Unfortunately, we have not made tests on this, and it is a potential future work.

RC16: L150: I have some questions about this calibration. Usually, we would calculate soil loss ratios for different crop management systems/crop rotations by use of erosion plot data and/or plant, soil, and residue measurements. These ratios would then be weighted with rainfall erosivity to calculate the C factors for specific locations. However, here you are using an optimisation approach – could you explain why? Moreover, did you perform any kind of split-off test, in which part of the data is used for calibration and another for testing? Would you agree that parameter calibration is necessarily conditional and that different parameter values can produce acceptable model responses? If not, why? If so, shouldn't you use a range of behavioural parameter values to estimate the uncertainty in your model outputs? Moreover, it seems like you calibrated the sub-factor Ccrop, not the C factor. Did I understand this correctly?
AC16: The optimisation approach was used simply because of the lack of measurement data that would allow calculation of soil loss ratios. The optimisation was intended to provide rough estimates of the general magnitude of the C factors, which can be improved later by future measurements. Note that the approach has been applied previously by Lilja et al. (2017a). The erosion measurement data was also limited, and it did not allow splitting of the data to calibration and validation sets. We are also very aware that different parameter combinations can provide similar outcomes, which is common feature in modelling. However, we did our best to parameterise the RUSLE with realistic factor values, given the data limitations. In the revised manuscript the C factor is defined according to the Renard et al. (1997), and the optimisation was done for the C factor of each cropping and management practice separately. Subfactor thinking of Panagos et al. (2015) is not included in the manuscript anymore.

RC17: L175-176: I agree model evaluation is difficult. However, particularly when models are being used to influence public policy or to guide decision-making, model testing is a necessary step. Our point in the paper you are citing was not to say it is okay not to evaluate models because it is difficult and rarely done. Instead, we wanted to incentivise the erosion modelling community to improve how we perform model testing and uncertainty analysis.
AC17: We have removed this sentence to avoid the misinterpretation.

RC18: L180-187: I agree, and I appreciate how you are open about the limitations of your

testing data.
AC18: In this case, we considered it is important to be very specific on the nature of the analysis to avoid misinterpretations.

RC19: L210-221: Why was rainfall erosivity not considered in any of the C factor calculations? This is crucial in USLE-type models, since erosion rates will vary largely for a same crop if the time in which the soil is exposed coincides with the time in which there is greater rainfall erosivitiy.
AC19: In the revised manuscript we have made an analysis of the effects of location specific cropping and management practices and temporal distribution on rainfall erosivity on the C factor values and erosion estimates using the seven field sites as analysis locations. This was done by estimating the average annual C values at the seven field sites on monthly basis according to Renard (1997). The monthly R data was taken from Ballabio et al. (2017) and two sets of crop sequence specific soil loss ratio values were taken from Wischmeier and Smith (1978) as there were no measurement based soil loss ratio data available from Finland. The selected soil loss ratios were intended to parameterise cereals with normal autumn ploughing and spring cereals with reduced autumn tillage, and the soil loss ratio values were adapted to Finnish location specific cropping schedules. The main difference between the Southern, Northern and Eastern field sites is that in the two latter the spring field preparation and sowing occurs on average two weeks later. The aim of this preliminary analysis was to provide indication of the variability C values in Finland and its effect on erosion estimates, and it was not aimed to provide accurate estimates of the C factors in Finland, as described in more detail in the revised manuscript. The outcomes of the analysis provided interesting insights into the variation of C and erosion estimate by location, and to our knowledge the findings are novel in Finland.

RC20: L235-242: Have you considered using a sediment routing model, which would allow you to deal with these issues?
AC20: At the current development stage of RUSLE in Finland, the inclusion of routing model is too early, but we consider it as an important future research direction.

RC21: L267: "Reasonable, with limitations" seems subjective. I suggest sticking to the numbers at this point of the results.
AC21: Agreed and these remarks were removed.

RC22: L270: The mean error shows you if the model is biased or not, but this metric is affected by cancelation. Could you also provide the RMSE, NSE, or such? Moreover, are you comparing the average soil loss from the entire period or individual annual losses?
AC22: We have added RMSE value to the revised manuscript.

RC23: L287-290: Would you not expect the (mean?) estimated erosion rates per catchment to deviate from the TSS measurements anyhow? These are two different things. I agree that if there is a correlation between them, there is an indication that the model might be consistently identifying the catchments where there is greater sediment production. However, since the RUSLE does not quantify sediment delivery to water courses and only represents rill and interrill erosion, do you believe there is any chance this correlation does not amount to causation?
AC23: Yes. Erosion estimates of RUSLE and measured TSS from streams and rivers are different aspects of the erosion, transport, and deposition process, and represent different things. We have tried to be very open on the nature of this analysis and its limitations in the manuscript, and we do acknowledge that the relationship of correlation and causation may contain uncertainties in this case. Our thinking was that it still better to include this analysis to the manuscript than to exclude it, since the information provides some indication of the performance of spatially distributed RUSLE, given their known uncertainties (Batista et al., 2019).

RC24: L290-298: Do you believe mean values per catchment are good descriptors of your data here?
AC24: We assume that you are referring to the slope values. The presented slope ranges describe how the average slope of field parcel varies by sub-catchment. We are not performing any quantitative analyses based on these values, and they are only for illustrating the differences of the two catchments. Therefore, we think average slopes are adequate for this purpose. We clarified in the revised manuscript that these values refer to the average slopes of field parcels by subcatchments.

RC25: L313-315: Do you mean highly erodible soils?
AC25: Yes, thank you for noting this. Corrected in the revised manuscript.

RC26: Fig.4: By looking at your R factor map, I imagine the spatial variability of rainfall erosivity to have a large influence on the C factors for croplands.
AC26: This was analysed in the revised manuscript. See author comment (AC19)

RC27: L324-325: There seems to be a concern here regarding connectivity and sediment delivery to water bodies. Do you think the RUSLE is an appropriate model for looking at these issues?
AC27: RUSLE does not incorporate sediment transport, nor did we account for connectivity, but we do think that the RUSLE provides useful information on the variability of erosion near water bodies, both from the scientific and management point of view. However, RUSLE is not able to indicate which of the identified high erosion areas near water bodies will lead to sediment delivery to rivers and streams, and at this point such evaluation from the developed data requires judgement from the data user.

RC28: L390-395: If you are using the same data for calibration and testing, considering average annual soil losses, and with a +- 50% limit of acceptability, how rigorous do you believe your model evaluation procedure to be?
AC28: This model evaluation was not designed in our manuscript, but it was used by Lilja et al. (2017a). Our intention was to make our results comparable within the evaluation framework of Lilja et al. (2017a). We have clarified this in the revised manuscript. Note that typically the error was lower, as explained in Section 3.2. of the manuscript.

RC29: Moreover, this methodology for incorporating the effects of sub-surface drainage into the C factor needs more explanation, in my opinion. Is (tile?) drainage affecting rill and interrill erosion or the sediment export from agricultural plots?
AC29: We have justified the incorporation of subsurface drainage in the P factor in the revised manuscript in a following way. According to Renard et al. (1997) subsurface drainage is considered in the P factor. Bengtson and Sabbagh (1990) suggested an average P factor value of 0.6, which was also recognised by Renard et al. (1997). However, the research on how the subsurface drainage should be considered in the RUSLE is limited, and to our understanding there is no commonly accepted approach for this. Except, RUSLE2 (USDA, 2013) incorporates a method for this, which considers the changes in K factor due to the subsurface drainage, but in our case the data was too limited to consider this. Therefore, we chose to follow the original suggestions by Renard et al. (1997) and Bengtson and Sabbagh (1990). According to our literature review the subsurface drainage reduce erosion 8-90% and on average 38% (Bengtson et al., 1988, 1984; Bengtson and Sabbagh, 1990; Formanek et al., 1987; Gilliam et al., 1999; Grazhdani et al., 1996; Istok et al., 1985; Maalim and Melesse, 2013; Skaggs et al., 1982), which results to average P value of 0.62. We used the P value of P 0.6 as suggested and used earlier in Finland by Lilja et al. (2017a). Also, the research in Finland showed that substituting of old drainage pipes with new ones reduced erosion up to 15% on a clay soil (Turtola and Paajanen, 1995). According to the research cited above, the main mechanisms how subsurface drainage reduces rill and inter-rill erosion is via reduced surface runoff, increased soil permeability and increased crop yield.

The use of sum of surface and subsurface sediment is justified also by research. In Finland, it is observed that up to 50-90 % of the erosion loading occurs via subsurface drainage (Finnish Environment Institute, 2019; Turtola et al., 2007; Turunen et al., 2017; Warsta et al., 2014, 2013) and that erosion material in subsurface drainage flow originates from the surface soil (Uusitalo et al., 2001). The origin of the erosion material was determined by an analysis of Cesium-137 contents of soil layers and eroded soil material, and the findings are in agreement with modelling results (Turunen et al., 2017). A study from Norway also reports that soil material in the drain flow originated most likely from the plough layer and, and the soil material was transported to subsurface drains via cracks and macropores in the soil (Øygarden et al., 1997). However, we do acknowledge that the consideration of subsurface drainage in the P factor lumps a complex and poorly understood process into single value and has therefore limitations and is a considerable source of uncertainty. In the revised manuscript we perform a sensitivity analysis which includes also the uncertainty in the P value.

References

Ballabio, C., Borrelli, P., Spinoni, J., Meusburger, K., Michaelides, S., Beguería, S., Klik, A., Petan, S., Janeček, M., Olsen, P., Aalto, J., Lakatos, M., Rymszewicz, A., Dumitrescu, A., Tadić, M.P., Diodato, N., Kostalova, J., Rousseva, S., Banasik, K., Alewell, C., Panagos, P., 2017. Mapping monthly rainfall erosivity in Europe. Science of The Total Environment 579, 1298–1315. https://doi.org/10.1016/j.scitotenv.2016.11.123

Bärlund, I., Tattari, S., 2001. Ranking of parameters on the basis of their contribution to model uncertainty. Ecological Modelling 142, 11–23. https://doi.org/10.1016/S0304-3800(01)00246-0

Bärlund, I., Tattari, S., Puustinen, M., 2009. Soil parameter variability affecting simulated fieldscale water balance, erosion and phosphorus losses. Agricultural and Food Science 18, 402–416. https://doi.org/10.23986/afsci.5949

Batista, P.V.G., Davies, J., Silva, M.L.N., Quinton, J.N., 2019. On the evaluation of soil erosion models: Are we doing enough? Earth-Science Reviews 197, 102898. https://doi.org/10.1016/j.earscirev.2019.102898

Bengtson, R.L., Carter, C.E., Morris, H.F., Bartkiewicz, S.A., 1988. The Influence of Subsurface Drainage Practiceson Nitrogen and Phosphorus Losses in a Warm, Humid Climate. Transactions of the ASAE 31, 0729–0733. https://doi.org/10.13031/2013.30775

Bengtson, R.L., Carter, C.E., Morris, H.F., Kowalczuk, J.G., 1984. Reducing Water Pollution with Subsurface Drainage. Transactions of the ASAE 27, 0080–0083. https://doi.org/10.13031/2013.32739

Bengtson, R.L., Sabbagh, G., 1990. USLE P factors for subsurface drainage on low slopes in a hot, humid climate. Journal of Soil and Water Conservation 45, 480–482.

Finnish Environment Institute, 2019. Sediment and nutrient loading to surface waters in 3 different scales [WWW Document]. Finnish Environment Institute (SYKE). URL https://metasiirto.ymparisto.fi:8443/geoportal/catalog/search/resource/details.page?uuid=%7B15893DD0-0193-40AD-9E21-452D271DB791%7D (accessed 1.25.21).

Formanek, G.E., ROSS, E., Istok, J., 1987. Subsurface drainage for erosion reduction on croplands in northwestern Oregon. In: Irrigation Systems for the 21st Century, in: Proceedings of the Irrigation and Drainage Division Special Conference. American Society of Civil Engineers, New York, New York, pp. 25–31.

Gilliam, J. w., Baker, J. l., Reddy, K. r., 1999. Water Quality Effects of Drainage in Humid Regions, in: Agricultural Drainage. John Wiley & Sons, Ltd, pp. 801–830. https://doi.org/10.2134/agronmonogr38.c24

Grazhdani, S., Jacquin, F., Sulçe, S., 1996. Effect of subsurface drainage on nutrient pollution of surface waters in south eastern Albania. Science of The Total Environment 191, 15–21. https://doi.org/10.1016/0048-9697(96)05168-6

Huttunen, I., Huttunen, M., Piirainen, V., Korppoo, M., Lepistö, A., Räike, A., Tattari, S., Vehviläinen, B., 2016. A National-Scale Nutrient Loading Model for Finnish Watersheds—VEMALA. Environ Model Assess 21, 83–109. https://doi.org/10.1007/s10666-015-9470-6

Istok, J.D., Boersma, L., Kling, G.F., 1985. Subsurface drainage: An erosion control practice for Western Oregon (No. 729), Special report. Agricultural Experiment Station, Oregon State University, Cornvallis.

Lilja, H., Hyväluoma, J., Puustinen, M., Uusi-Kämppä, J., Turtola, E., 2017a. Evaluation of RUSLE2015 erosion model for boreal conditions. Geoderma Regional 10, 77–84. https://doi.org/10.1016/j.geodrs.2017.05.003

Lilja, H., Puustinen, M., Turtola, E., Hyväluoma, J., 2017b. Suomen peltojen karttapohjainen eroosioluokitus (Map-based classificication of erosion in agricultural lands of Finland). Natural Resources Institute Finland (Luke) 36.

Maalim, F.K., Melesse, A.M., 2013. Modelling the impacts of subsurface drainage on surface runoff and sediment yield in the Le Sueur Watershed, Minnesota, USA. Hydrological Sciences Journal 58, 570–586. https://doi.org/10.1080/02626667.2013.774088

Øygarden, L., Kværner, J., Jenssen, P.D., 1997. Soil erosion via preferential flow to drainage systems in clay soils. Geoderma 76, 65–86. https://doi.org/10.1016/S0016-7061(96)00099-7

Panagos, P., Borrelli, P., Meusburger, K., Alewell, C., Lugato, E., Montanarella, L., 2015. Estimating the soil erosion cover-management factor at the European scale. Land Use Policy 48, 38–50. https://doi.org/10.1016/j.landusepol.2015.05.021

Rankinen, K., Tattari, S., Rekolainen, S., 2001. Modelling of vegetative filter strips in catchment scale erosion control. Agricultural and Food Science 10, 99–112. https://doi.org/10.23986/afsci.5684

Rekolainen, S., Posch, M., 1993. Adapting the CREAMS Model for Finnish Conditions. Hydrology Research 24, 309–322. https://doi.org/10.2166/nh.1993.10

Renard, K.G., Foster, G.R., Weesies, G.A., McCool, D.K., Yoder, D.C., 1997. Predicting Soil Erosion by Water: A Guide to Conservation Planning with the Revised Universal Soil Loss Equation (RUSLE). Agricultural Handbook 703. US Department of Agriculture, Washington, DC, pp. 404.

Rompaey, A.J.J.V., Govers, G., 2002. Data quality and model complexity for regional scale soil erosion prediction. International Journal of Geographical Information Science 16, 663–680. https://doi.org/10.1080/13658810210148561

Skaggs, R.W., Nassehzadeh-Tabrizi, A., Foster, G.R., 1982. Subsurface drainage effects on erosion. Journal of Soil and Water Conservation 37, 167–172.

Turtola, E., Alakukku, L., Uusitalo, R., 2007. Surface runoff, subsurface drainflow and soil erosion as affected by tillage in a clayey Finnish soil. AFSci 16, 332–351. https://doi.org/10.2137/145960607784125429

Turtola, E., Paajanen, A., 1995. Influence of improved subsurface drainage on phosphorus losses and nitrogen leaching from a heavy clay soil. Agricultural Water Management 28, 295–310. https://doi.org/10.1016/0378-3774(95)01180-3

Turunen, M., Warsta, L., Paasonen-Kivekäs, M., Koivusalo, H., 2017. Computational assessment of sediment balance and suspended sediment transport pathways in subsurface drained clayey soils. Soil and Tillage Research 174, 58–69. https://doi.org/10.1016/j.still.2017.06.002

USDA, 2013. Revised Universal Soil Loss Equation Version 2 (RUSLE2), Science documentation. USDA-Agricultural Research Service, Washington, D.C.

Uusitalo, R., Turtola, E., Kauppila, T., Lilja, T., 2001. Particulate Phosphorus and Sediment in Surface Runoff and Drainflow from Clayey Soils. Journal of Environmental Quality 30, 589–595. https://doi.org/10.2134/jeq2001.302589x

Warsta, L., Taskinen, A., Koivusalo, H., Paasonen-Kivekäs, M., Karvonen, T., 2013. Modelling soil erosion in a clayey, subsurface-drained agricultural field with a three-dimensional FLUSH model. Journal of Hydrology 498, 132–143. https://doi.org/10.1016/j.jhydrol.2013.06.020

Warsta, L., Taskinen, A., Paasonen-Kivekäs, M., Karvonen, T., Koivusalo, H., 2014. Spatially distributed simulation of water balance and sediment transport in an agricultural field. Soil and Tillage Research 143, 26–37. https://doi.org/10.1016/j.still.2014.05.008

Wischmeier, W., Smith, D., 1978. Predicting rainfall erosion losses: a guide to conservation planning. Agricultural Handbook No. 537. Washington DC, USA: U.S. Department of Agriculture.