Geoscientific
Model Development
Discussions

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Interactive comment on "CE-DYNAM (v1), a spatially explicit, process-based carbon erosion scheme for the use in Earth system models" *by* Victoria Naipal et al.

Anonymous Referee #2

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General comments:

The manuscript presents a very interesting approach to model the effects of soil erosion and deposition by water on the C balance of a mesoscale catchment. As it combines techniques from LSMs with an newly developed erosion/deposition model it prepares the ground for large scale modelling. Therefore, I think the paper fits perfectly into the scope of Geoscientific Model Development. However, especially for a model development and testing paper which is building the ground for future environmental / climate analysis I see some major point which need to be addressed. Hence, I suggest major revision for the following reasons:

1. First of all the paper lacks clear aims (or research questions). In Line 68 to 83 the authors give an overview of the contents of the paper, but I think the entire paper would improve substantially if clear aims would be given here. For example, (i) introduce a coupled soil erosion and C turnover model with an LSM model which is applicable on regional scales. (ii) Rigidly test the model for the Rhine Catchment against other modelling results and regionally available data. (iii) Analyze the sensitivity/uncertainty of the model results due to weak input data and a priori model assumptions. (regarding (iii) see comments below.

2. Taking the temporal and spatial scale into account which should be later on analyzed with the model I think the authors found a good balance between model complexity and simplicity. However, the model is full of a priory assumptions, which will fundamentally affect the modelling results, so I personally do not think any model results can be interpreted without some estimates of at least the sensitivity of the model against these assumptions. The most important assumptions which could be tested easily are: C input via plants especially crops depending on erosion status, C enrichment during erosion and depletion during deposition, reduced C turnover in alluvial soils due to wetter conditions, etc. Overall, it is one of the major shortcomings of the paper that the modeling results in section 3.2 are presented single values (e.g. for 159 Tg C for C removal by erosion) and also conclusions based on this single model results are presented. I strongly suggest performing a a sensitivity analyses (including as far as possible effects of a priory assumptions) and giving results with a reasonable range. I am fully aware that it would be hardly possible to do a full uncertainty analysis and even an sensitivity analysis might be quite ambitious given the catchment size and the complexity of the involved models. However, it is not enough just stating in the discussion some important processes are not taken into account.

3. From my understanding of the paper and accounting for the scope of the journal testing such new model against data is essential. The authors did try doing so but here a lot of improvement is easily possible: (i) Include a section under methods explain

which data are used to test the model and also explain in some detail how this is done. For example, the comparison with other models as given in Fig. 3 and 5 is not clear, as the following information is missing: (a) Were the data from the more high resolution models aggregated to the raster cell size of CE-DYNAM to do a raster-by-raster comparison? (b) If a CE-DYNAM raster cell consist of erosional and depositional sites, which are not resolved in the raster cell, how to compare with gross erosion of a high resolution model (e.g. Panagos et al. 2015) which might have different proportions of erosional and depositional raster cells in this large 8 x 8 km^2 raster cell. (c) It is not clear at all what is compared as all model results from literature do not focus on the time span from 1850 to 2005. These details are essential for the reader to understand your model validation. (ii) From the figures I have some doubts that the different models fit very well (why not giving statistical goodness-of-fit-parameters?). So the question is how good the other models are (please see e.g. the scientific debate regarding the Panagos et al. (2015) map. So, at least in the discussion this model to model comparison needs to be stressed. (iii) Generally, the erosion (partly deposition) validation of CE-DYNAM is mostly done against other models also using USLE technology (USLE factors might be even derived from same data sources), so an extended discussion if this is meaning full is needed.

Specific comments:

Line 68-83: see general comment.

Line 89: be more explicit regarding 'low number of parameters'

Line 118 ff: I do not agree that not taking the L factor into account is a reasonable decision. I agree that it is somewhat difficult to estimate (for the German part of the Rhine catchment there are some estimates) but if you are interested in land use change it is an essential factor if you kick it out the entire basis of the USLE is set into question. (The P factor is simpler as it is set to 1 in most studies).

Line 130: The statement "...has been calibrated and validated for the Rhine

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catchment..." is confusing here? If calibration and validation was already done why doing it again? If the model has changed you need a new validation (but what about calibration? Are you using parameters in CE-DYNAM which were calibrated before it is necessary to indicate this in detail).

Line 113: Alluvial soils are indicated in German soil maps, so the statement is not correct for the largest part of the Rhine catchment.

Line 141 / Eq. 2: Generally I think it would be good being more precise with the equation. For example in case of Eq. 2, I would expect a reference to the different raster cells (Aft(i) = Lstream(i) x Wstream(i); whereas i is the raster cell.) as for other equations e.g. Eq. 4a it was not clear if this refers to the entire catchment is calculated for each raster cell.

Line 148: If alpha and b are constants it means that the upstream area necessary to result in a stream is always the same. I understand that in case of a large scale model simplifications are necessary but this assumptions is for sure not true for the Rhine catchment (see papers from hydrology of maps of the stream system (which by the way would be available for the entire Rhine catchment).

Line 159: '... at 8 km resolution... " I guess this means 8 km x 8 km raster cells. Should be changed throughout the text (also with other resolution given).

Line 163: I do not think the the assumptions of reduced hydrological and geomorphological connectivity in arable landscapes (compared to forest) is correct. From the recent studies dealing with flash floods it is obvious that it is a main problem that this landscapes have a very high connectivity as so many ditches, drainages etc. were built over the last century to get rid of any surplus of water on arable land. So, your assumptions for the range of the parameter f in different landscapes must be underlined by reasonable data.

Line 187: Does a multiple flow algorithm makes sense in case of a resolution of 8 km

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x 8 km?

Line 230 ff: In general this is a reasonable assumption for the crop residues. However, studies in small catchment clearly indicate that residue management is a key factor of SOC, so this a priori assumption has potentially a huge effect on the produced results. So, its importance must be analyzed with the model!

Line 238-240: The given equation are a fundamental problem with modelling the effect of soil erosion on SOC turnover. For example, using standard SOC pool residence times for all landscape positions is of tremendous importance for the entire C balance effect of erosion. So, again it would be very important to know how sensitive the results are against this assumptions. At least give some estimates / measurements at different landscape positions in the discussion and comment of the potential effect in modelling results.

Line 265: "... The next soil layer contains less C and therefore at the following time-step les C will be eroded under the same erosion rate..." If this would be always true one would expect a continuous decline in SOC in soils. However, assuming a long-term forest use on a slope you will found the soil in an equilibrium between new C input via plants and small amount of erosion. So, in this case the eroded material will have a more or less constant C content.

Line 277: Calculating a daily erosion fraction is a reasonable approach. However, if taking the episodic nature of erosion and deposition into account the C balance will be different compared to a small continuous process (see literature). Might be also discussed.

Line 291 ff: The assumption that there is no C selectivity (enrichment in eroded material and depletion at erosional sites) is taken in many modelling approaches. However, if there would be no enrichment of fines in the sediments transported in river systems, one would find e.g. sand in suspended sediments of larger rivers. Which is e.g. in case of the lowland Rhine not the case. Discuss this in the context of the scale of your

paper. Also important regarding the loss of C to the ocean.

Line 341: Where do the data regarding afforestation during the last two decades come from. To my knowledge this is a process already started in the late 1959th (please give reference)

Line 379: (see also general comments). I wonder why you did not use other more specific and potentially profound national data. E.g. for Germany there are several maps for potential erosion which are much more elaborated than the map of Panagos et al. (2015). Moreover, I wonder why you did not use the sediment delivery data of the Rhine which are freely available - I guess since the 1950th - which would be a good and reliable additional data set for validation.

Line 397-401: I suggest omitting these sentences and Fig. 4, because I do not see any additional value of this here. It is obvious from the model structure of all USLE based models (and all other erosion models) that an increase of erosivity and slope directly leads to an increase in erosion. Moreover, there is a coincidence in the catchment that highest erosivity and highest slopes occur at the same alpine area, but this is not any proof for the model. Hence, hence I think this is weakening your validation more that it would strengthen it. By the way: Erosivity and slope might explain 70% erosion if very different rainfall regimes and slopes (mountain areas and lowlands) are compared, but with a catchment like the Rhine (where except for the alpine part) the differences in slope and erosivity are relatively small soil cover (C factor) is getting much more important (erosion rates between grassland and arable land vary by a factor of 10-20).

Line 402: As I modeler I expect a goodness-of-fit parameter with this statement. (See also general comments regarding model to model comparison of different USLE implementations).

Line 424 ff: The comparison with the data from Hoffman et al. (2013) underlines a deficit in all your comparisons. It is at no time clear what is compared exactly. Mean of 7500 years against 1850-2005?

Line 438: Does the outflux fit to measured data? Would be easy to test even if this is not essential as only a very small amount will be delivered into the sea. (could be tested at several subcatchment, as data are available).

Line 451-452: This is a clear contradiction to your statement that differences in erosivity are very important for spatial differences in erosion.

Line 453: The close link between C erosion and soil erosion is obvious from your modelling structure but not necessarily correct (C enrichment depending on event size?)

Line 434 ff: See also general comments

Line 446: I suggest not to over interpret the modeling results from the alpine area of the catchment as the modelling and the data are weakest there. (i) Increase in measured precipitation most uncertain; (ii) calculated R factor very uncertain in all USLE approaches; (iii) alpine USLE factors not very well underlined by data (compared to arable and grassland),

Line 497ff. See comments regarding connectivity above. Moreover, even if the connectivity is high under forest (which I doubt), forest will produce not a lot of sediment and hence are not so important for building up alluvial soils at all.

Line 493ff. I do not see from the results the CO_2 fertilization plays an important role for an increase in dynamic replacement. I guess that the increase in yields due to changes in management are much more important (as reduced yields are not taken into account at erosional sites) as they boost dynamic replacement of eroded soils.

Line 501ff: It is obvious from first order kinetics that colluvial soils must have higher CO_2 effluxes as they contain more C. So, this is not a very new finding.

Line 506-507: Question: Is the modelled increase in respiration from floodplains resulting from an temperature increase or from an increase in depositional material, which would also result in an increase of respiration? Comment: Under real conditions the increase in respiration from floodplains is also a result of decreasing groundwater levels,

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diking etc. which are associated with river management. This is shown in local studies but to my knowledge not taken into account in any modelling.

Line 542-561: This is a nice collection of model deficits. However, for a modelling paper I would expect a bit more (see general comments).

Line 576-588: I think this conclusions are not fully supported by the results as the modelled C fluxes might be affected by a priori assumptions and model parameters which are not tested enough (see general comment regarding senistiviy analysis).

Table 1: I guess the spatial resolution is always given in raster cells, e.g. 0.25° x 0.25°

Table 2: As the resolution of the data sets are different how to make sure that the comparison fits, e.g. the higher resolution data set might exclude the river network from the SOC calculation while the lower resolution data set might include this areas into the SOC stock calculation. Give somewhat more details.

Fig. 3 and 5: What are the 10 classes given on the X-Axis?

Fig. 4. Omit this figure (see comment above)

Fig. 5c/d: What does it mean if CE-DYNAM has erosion rates which are up to a factor smaller than the Lugato model and C deposition rates which are more or less the same? Is it a result of different areas affected by erosion and deposition? Should be explained / discussed.

Fig. 7. Just a comment of an handicapped. About 4-8% of the male population are to a certain extend color blind (especially red/green is problematic), so if you do not what to lose these proportion of your readers you should adapted your color in your figures. There are color blind friendly color ranges available in most software packages. If the dashed lines range between min and max the outliers cannot be above or below the lines. So, I guess the lines represent something else.

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