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## Comment on gmd-2022-121

Holger Metzler (Referee)

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Referee comment on "Matrix representation of lateral soil movements: scaling and calibrating CE-DYNAM (v2) at a continental level" by Arthur Nicolaus Fendrich et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-121-RC2>, 2022

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The authors provide a spatially upscaled version of an erosion, transport and deposition model (CE-DYNAM) to European scale, while sticking to a high spatial resolution. This is an important scientific contribution because lateral C transport has largely been ignored so far in process-based models because usually models consider different grid cells as independent. Such lateral transports can have a significant effect and should be investigated because nonlinear effects could potentially lead to drastically new insights and better understanding compared to existing models.

The spatial upscaling was computationally feasible only because the authors emulated the original model by a matrix approach. This allows the application of sparse matrix models as well as an improved application of parallel computation methods. Another advantage of matrix models is that they allow a rigorous mathematical analysis, something the authors did not do in this manuscript (it was not their goal) but can be done in the future based on the matrix reformulation. This was not possible with the original implementation. Furthermore, I appreciate all the effort the authors showed in reimplementing an existing model with a matrix approach, emulating the original simulation results very well. In this regard I like also the explanation of the matrix shape in Section 3.3. Nevertheless, I wished I could have "seen" a matrix, at least as a schematic block matrix after the authors speaking so much about matrices.

The presentation and the writing is clear, explaining the model calibration and simulation results as well as model limitations and future opportunities very well. Sometimes though the text appears too lengthy in my opinion. This starts with the abstract and continues with quite some overlap in the sections about calibration, results, simulations and limitations. Furthermore, in particular in later sections I was overwhelmed by an extensive use of potentially unnecessary numbers.

I furthermore do have some issues about an easy reproducibility of the method, because to me it seems that in Section 2 some formulas are incorrect and notation is not precise.

Well, either the formulas are incorrect or I understood them wrong, neither option is preferable. In particular I consider the statement (p. 3, l. 71): "We expect our mathematical development of CE-DYNAM to facilitate its reproduction and incorporation in LSMs such as ORCHIDEE, DayCent, and others." to be pretty bold. Under this point of view formulas and notation should show no flaws.

This starts with Table 1, which is in general very nice, but it is incomplete, some symbols that are used later on do not appear here. This was sometimes annoying for me while reading. For example  $I^*$  was never properly introduced. On p.5, l. 132 it is stated that inputs are disturbed along  $D$ , which I do not understand.

**Here my issues with notation and formulas, which should be thoroughly checked:**

p. 8. l. 1: Should it be  $\Delta_i$ ? Is it used to compute  $d_0=0$ ,  $d_1=d_0+\Delta_0$  and so forth?

p. 8. l. 183:  $k_e$  is missing in Table 1. The "Total mass of soil" under the bracket refers to total mass of soil in hill slopes? Why is it decreasing with  $h$ ? To me this looks like we have an infinite erosion if we do not have any hill slopes, isn't this going in the wrong direction?

p. 10, l. 202. If there is going to be a new input flux, should there also be a new output flux? The input must come from somewhere. Please also indicate where the new input flux goes, I do not want to guess here.

p. 10, l. 213. The way  $k_r$  (missing in Table 1) is defined two lines later, it seems wrong to me to call it a flux. It is a rate (dimension 1/time). It will only become a flux once it is multiplied with a pool content. This leads to major confusions for me later on.

p. 11, l. 235: Please use other notation than  $(a, b)$  for the indexing of the sum, the two letters are already taken. It becomes very confusing this way. Again, I think that if  $k_r$  is a rate rather than a flux, then  $k_s$  will be as well because all the other factors in Eq. 11 are dimensionless.

p. 10, l. 241 "all PFTs" should rather be "all PFTs but  $p_0$ ", right?

p.12, l. 247: "Such input flux". I disagree again, same problem. It is not yet a flux because it is not yet multiplied with a carbon stock, which should be  $S_{[(a, b), \dots]}$  here?

p. 12, l. 257: The "P" here looks different from the ones introduced on p. 10, l. 221. Furthermore, I am not sure whether it belongs here in the first place, well the absolute value is unnecessary in any case. But why multiply by the number of non-zero PFTs in cell (a, b)? Shouldn't this be already included in  $k_\tau$  already? Could you write it down explicitly for yourself without the P but a second sum instead and check whether it is correct this way?

p. 12, l. 260: In  $k_s$  I think that source and target are confused.

p.13, l. 265: What is  $k_t^*$ ?

p. 13. l. 266. Should  $k_s$  with source (a, b) be multiplied with some stock indexed by (a, b) instead (x, y).

I obviously do have some confusions about the first-order description, where sources and targets seem not to match, at least in my head. So would like to encourage the authors to carefully check the notation and the formulas again, along with their implementation.

### Small issues:

- In general units are sometimes italic and sometimes not, sometimes with a space between the number and the unit, sometimes without.
- The use of singular and plural gets mixed up quite often.
- p. 3, l. 86: "approach" --> "an approach"?
- p. 3, l. 88: Table 1 --> (Table 1)?
- Table 1:
  - - Description sometimes ends with a period, sometimes not.
  - - The adimensional respiration rates of carbon  $k_r$  actually do have a dimension: 1/day.
  - -  $I^{[c_j, c_j]}$ : One of the j's should be an i. Probably the first one, then please also adapt the description, to make it consistent with  $k_t$ .
  - - Then it is a little unfortunate to use  $\omega$  and  $w$  for the depth to bedrock and the flow accumulation, respectively.
- p. 7, l. 145: I am not sure if such a procedure necessarily converges to a pullback attractor (given there is one in the first place). The pullback attractor is reached when starting the simulation earlier and earlier, basically moving toward an infinite simulation history.
- p.7, l. 152: Why now change the notation from S, I, and k to x, B, and A?
- p. 10, l. 243: "soi"
- p.14, l. 280: The seventh-largest what?
- p. 15, l. 308:  $u_i$  instead of  $p_i$ ?
- p. 15, l. 312: "P is Panagos", what does this mean?
- p.15. unnamed formula: what is  $r(y, m)$ ?
- p. 18, l. 398, 399: What are C-factor and "R factor"? Please also note the different way

of writing them.