Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., author comment AC1
https://doi.org/10.5194/esurf-2021-33-AC1, 2021

Reply on RC1

Elco Luijendijk

Author comment on "Transmissivity and groundwater flow exert a strong influence on drainage density" by Elco Luijendijk, Earth Surf. Dynam. Discuss., https://doi.org/10.5194/esurf-2021-33-AC1, 2021

Reply to review by Marijn van der Meij. The replies are shown in italics below each comment.

The author presents a detailed and well-documented model named GOEMod that simulates the effect of stream incision on landscape evolution through groundwater and overland flow. The model simulates a set of hydrological and geomorphological processes that affect the groundwater level, stream incision and drainage density. He identified a process he calls groundwater capture, where streams with high incision rates draw the water table below neighbouring streams. With a sensitivity analysis the author identifies the soil transmissivity as main parameter controlling the drainage density.

The manuscript is well written and the model is very well documented. As the author mentions in his Introduction, groundwater flow is often simplified or not at all included in current landscape evolution models. This work shows a potentially important control of groundwater flow on landscape evolution.

However, I think that the manuscript can improve on three points and would benefit greatly from additional discussion of the model and its results. The three points are the balance of the manuscript, the selected set of parameters and a comparison of model results with field evidence. I address these points in more detail in the general comments below. Next to that, I added several specific comments and technical corrections to the attached document. I look forward to seeing the revised manuscript.

With best regards,

Marijn van der Meij

1 Balance of the manuscript

In the title and abstract, the manuscript is presented as a study to identify the relation between groundwater flow and drainage density. For this purpose, the model GOEMod was developed. The manuscript itself however leans very much towards the model development and description of model behaviour and only shortly addresses the validity of

the model and lacks a comparison with field evidence to support the model findings. I suggest to address the other comments below to improve the balance between model description and interpretation of the findings in the manuscript.

Reply: I agree and have added a section that discusses the comparison between the model and field evidence for changes in drainage density and its correlation with transmissivity. This discussion is based on the references that were mentioned in the 2nd paragraph of the introduction in the previous version of the manuscript. However I would like to reserve a more elaborate discussion for follow up work. Compilations of transmissivity data are very scarce which makes a comparison with drainage density difficult. However, I am also about to submit a manuscript with a new large-scale transmissivity dataset that may be very helpful there.

2 Selected parameters

The setting of the model is a humid climate zone and is roughly based on The Netherlands. The model builds further on stream density studies from the Netherlands (lines 40-41 & 50-52). Also, data from The Netherlands are used to parametrize the model. This data includes rainfall data, porosity, transmissivity and stream slope. However, there are several parameter choices in the model that deviate from this setting to simplify calculations or achieve a better model result:

The transmissivity, porosity and slope parameters are adjusted (lines 284-289 & 398-400);
The runtime of the model is set to 100.000 years;
The elevation downstream of the model domain does not change over time (lines 225-226) and precipitation is also constant (lines 114-116).

Over the long simulation span of 100.000 years, downstream elevation cannot be assumed constant due to fluctuating sea water levels. Also, rainfall cannot be assumed stable over this time period. Fluctuations in these parameters will likely have a larger influence on stream incision than groundwater capture over these geological time scales.

This make me wonder if groundwater capture as resulting from the model is a realistic process in real landscapes, or whether it can only be simulated in modelling studies due to the unlikely conditions when under which it seems to occur. I would like to see some discussion in the manuscript on how realistic the parameter choices are and how likely it is that groundwater capture plays a role in the evolution of real landscapes.

Reply: The previous version manuscript unfortunately used a value of sediment transport coefficient that was much too low for transport-limited systems. An analysis of published sediment discharge datasets yielded a new more realistic and much higher sediment transport coefficient that results in a much faster incision and a much faster adjustment of stream networks. The stream network now reaches a dynamic steady-state over a timespan of ~2000 years. Because of this the revised manuscript now shows the results for model runs of 10,000 years as a standard, which is a timespan that sea level and climate can be expected to be relatively stable. The transmissivity and slope parameters in the revised manuscript now also closely follow the average values for the Netherlands reported by de Vries (1994) and others.

3 Field evidence

Following from the previous comment, I'm also missing a comparison of model results with field data to further support the concept of groundwater capture and the effect of transmissivity on drainage density. These points are briefly mentioned in the Introduction (lines 20-21 & 28-32), but are not discussed in the light of the model results. I would like to see such a comparison in the paper to further support the model results, for example in the form of a case study or as a meta-analysis of areas with different transmissivity and drainage densities.

Reply: Agreed. I have added a new section in the revised manuscript that compares the model results and published estimates of drainage density and transmissivity or permeability

Reply to additional review comments in the attached pdf:

Comment on Line 38: I'm curious about your ideas how to improve the hydrological modelling in landscape evolution models, for example using your approach. Could you elaborate on that in the discussion?

Reply: I have briefly commented on ways to improve the hydro(geo)logy in landscape models in the revised manuscript.

Comment L. 58: This focus requires a bit more introduction.

Reply: Agreed, this choice is now introduced in the last paragraph of the introduction.

Comment line 74: Is there a specific landscape where this initial relief is based on?

Reply: This admittedly is purely a guess. I have not been able to find data on typical relief for undissected landscapes (without streams).

Comment line 75: Can you switch sections 2.3 and 2.4? Section 2.3 already addresses the precipitation that is described in 2.4

Reply: Agreed, i have switched the sections.

Comment line 76: Clever way to determine the partitioning of precipitation. Can you elaborate a bit on why you have to determine this per individual event?

Reply: The reason is that erosion by overland flow is also calculated per rainfall event. And therefore an calculation of overland flow volume per event is necessary.

Comment line 91: How is the ET determined in the model?

Reply: The ET is a fixed value as is now mentioned more clearly in the revised manuscript.

Comment Fig. 3: Can you change this to 1? A fraction of 1.05 doesn't make sense

Reply: Thanks for spotting this. The colorbar scale has been updated.

Comment Fig. 5: Can you clarify that the colours of the traingles refer to the base flow of the streams?

Reply: I have added a clarification to the figure caption.

Comment line 240: What is the effect of this and the other simplifications/assumptions on your model results? And is it possible to improve this description in (a later version of) the model?

Reply: It would perhaps be possible to relax this assumption. The resulting equation for a more realistic distribution of erosion is possible, but the integrand may not be solvable, and at the very least would reserve quite a bit of dedicated time and a lot of coffee. I would like to reserve a more thorough exploration of this and other simplifications for a future manuscript.

Comment line 275: If I understand correctly from the manuscript, the overland flow and groundwater recharge are calculated on an event basis, while hillslope erosion and stream incision are calculated on a yearly basis. Is this correct? If yes, could you elaborate how you bridge these time scales? If not, could you clarify how you dealt with the different time scales of the processes?

Reply: That is correct. Overland flow and recharge are calculated at an event basis. Recharge is summed over a year to yield an average recharge rate. However, overland flow and erosion are calculated on an event basis, and are then summed over a year to yield an average overland flow erosion rate. I have added this clarification to the revised manuscript.

Comment Table 2: How many values did you select for each parameter? And did you also vary multiple parameters in one run to study their interactions?

Reply: I have added the number of values to the table. The sensivitiy analysis used a relatively simple one at a time sensitivity analysis. It would be interesting to explore changes in multiple parameters and their interactions, but I would like to reserve this for future work

Comment Fig. 13: In the text you name this the drainage density instead of the stream density

Reply: Thanks for noticing, i have corrected this.

Comment line 370: Can you elaborate on how these simplifications affected the model results and how they might change with more complex descriptions in the model?

Reply: A brief assesment of the robustness of the conclusions is already contained in this section. A more elaborate discussion & more complex model physics are planned for follow up work.

Comment line 399: Aren't these parameters typical for the Netherlands, where you based your simulations on?

Reply: Not really, at least for the parts of the Netherlands that are above sea level, where tranmsissivity tends to be higher (approx. 1-5 10^-2 m2/s, see for instance Bogaart et al. 2003). And the average topographic gradient is somewhat higher too with 1/2500 m/m.

Comment line 404: Is GOEMod also able to simulate this process?

Reply: It is. The revised manuscript contains a new set of model simulations that explores the persistence of drainage networks and that includes model runs where base level or precipitation increases, which results in the reactivation of abandoned streams.

Reply: Many thanks for the thorough and very helpful review.