Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., author comment AC2
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Reply on RC2

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-AC2, 2021

Reply to review by Stefan Hergarten. The replies are shown below the comments in *italics.*

Dear Elco Luijendijk;

first, I would like to mention that I enjoyed reading your paper about drainage density. Maybe I am even a bit biased because I really like this king of modeling -- playing with the equations, simplifying them in such a way that they become tractable, and then looking what we can get out of them. I noticed that mention the limitations in the sense of some oversimplification at several times. In this case, I do not worry so much about the simplicity of the involved processes. At some points, even more simplifications would have been possible, and a further reduction of the variety of processes could even improve our understanding. Anyway, this is my personal point of view and not a criticism on your work as a reviewer.

Nevertheless, I am still somewhat critical concerning the results and the depth of the discussion. Your model is quite specific. So I see the main merit not in providing a general tool, but in addressing the question for the principal dependencies of drainage density. Playing devil's advocate, I could suggest to assume a given across-slope profile (maybe not uncorrelated random numbers, rather something like a random walk) and apply your steady-state groundwater flow model with constant recharge. If the transmissivity increases, the gradients in hydraulic head decrease, which reduces the number of points where the groundwater level reaches the surface. This would exactly be a decrease in drainage density with increasing transmissivity. And then I would argue that this is already the main result of your paper and that you could omit the rest of the model.

Reply: That is true in principle, i.e., topography, recharge rate and transmissivity alone would determine drainage density. So in principle a dynamic model like the one presented in the manuscript would not be necessary to estimate transmissivity and one could use a static model like the model the reviewer proposes to test what drainage density would be for different sets of hydrogeological parameters. This is actually more or less what has been done in several studies cited in the introduction (Luo et al., 2010; Luo and Pederson, 2012; Bresciani et al., 2016), where existing drainage networks and topography have been used to constrain transmissivity. However, the drainage density that one would *obtain from such a static model depends strongly on the topography and the degree of incision. All else being equal, a topography with a high relief would result in a much lower number of active streams than if one would analyze a topographic cross-section with a low relief. And relief is in itself of course affected by the number of streams, because this determines how much water each stream discharges and how much incision power these streams have. So there is a feedback here between topography, groundwater flow and drainage density that I tried to explore using the model presented in this manuscript. The main research question that i tried to answer was if a process can be identified that governs the relation between transmissivity and drainage density.*

While this is a exaggerated, my feeling is indeed that you do not get the maximum out of your model. After following the theoretical part, I was a bit disappointed by the results and discussion sections.

There is one central point where I cannot assess how powerful the model is. You perform simulations over a given time span. Some of these (highest transmissivity) already arrive at one single stream over the entire slope (so 0.1 streams per km at 10 km domain size). I would like to know whether the model is able to predict the existence of more than one stream over very long time (so in some kind of steady state) or whether it always arrives at a single stream if we just wait long enough. If the latter was the case, this would reduce the merit of the model seriously.

Reply: Thats a very good point. To test this I included a model run in the revised version of the manuscript that show the evolution of drainage density over a longer timescale of 1 million years. The results show that these runs do reach a steady-state with more than one stream. The reason is that larger streams that incise the fastest at the start at some stage lose their advantage because the higher incision rate results in a lower slope of the stream in the out of plane direction. This lowers the incision and allows smaller streams to catch up. So this is a negative feedback that provides a limit on drainage density. I have added a more elaborate discussion of this effect to the revised manuscript. This process is obviously very dependent on the evolution of the regional baselevel. In case where the baselevel does not limit stream slope and incision, the system does evolve to one stream. However, the model results in the revised manuscript use a new more realistic value of sediment transport coefficient that results in much faster incision and a steady-state with > 1 streams within ~2000 years.

In the following, I discuss a few more specific points about the results, in particular about the parameter study.

(1) Figure 13(a) shows some kind of power-law dependency of the drainage density on both the transmissivity and and the initial slope. It looks as if the exponent was -1, so the drainage density seems to be proportional to 1/transmissivity and to 1/slope. This would be the chance to leave the purely qualitative level of the discussion and to get a least some quantitative results. I would urge you to investigate these dependencies and to try to explain these directly from the model equations.

Reply: Thanks for the suggestion. I would however like to reserve a more quantitative analysis of the results for a follow up manuscript. In particular the steady-state drainage density may be predicted by analytical equations that i am currently developing, but that will take some time to finalize.

(2) The consideration of erosion with a fixed base level seems to be a bit unfortunate to

me. Practically, you mix the buildup of across-slope topography by incision with the decay of slope gradient. As a rough estimate: 5000 m downstream length, 5e-3 initial slope is 25 m difference in topography. Incision is some meters, so not so strong that the decay of overall slope is already dominant, but in turn not negligible. It would be better to separate both effects.

Reply: Thanks for the remark. I have included a base level change in the model runs in the revised manuscript. For the area in the Netherlands that this model is loosely based the rate of baselevel change is very low (20 mm/kyr). However, the sensivity analyses in the revised manuscript explore the effect of higher baselevel decrease rates, which do have an important effect drainage density.

(3) You consider a variation of the slope exponent n. In the context of these models of fluvial incision and sediment transport, it makes no sense to change the exponent of the power-law relation and to keep the factor of proportionality constant. Beyond this, the ratio of the discharge exponent m and the slope exponent n is constrained much better than the value of n itself, so changing n alone really makes no sense. So you should keep m and n constant and consider the sediment transport kf as a variable parameter in your sensitivity analysis.

Reply: Thanks for the comment. I have repeated the sensitivity analysis with kf as a variable parameter instead of n and have added these results to the revised manuscript. I have calculated the variability of the kf using compiled sediment discharge data by Lammers and Bledsoe (2018, https://doi.org/10.1002/esp.4237). In the process I also discovered that the value used in the previous version of the manuscript was much too low for transport-limited streams, and have now used a more realistic and higher value. As a result streams incise faster and the stream network reaches a steady-state in within ~2000 years in the new set of model runs used in the revised manuscript.

(4) Just as an option for future work: periodic boundary conditions may be simpler here.

In summary, I feel that you developed a very nice model, but earning the fruits of your work is somewhat weak. Nevertheless, I would not argue against publishing it if you were running out of time. In this case, point (3) would be the point where I would definitely request a revision.

I hope you find my suggestions helpful.

Best regards,

Stefan

Reply: Many thanks for the very helpful review