Earth Surface Dynamics Discussions

Earth Surf. Dynam. Discuss., referee comment RC2
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Comment on esurf-2021-33

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Referee 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-RC2, 2021

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.

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.

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.

(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.

(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.

(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