

Geosci. Model Dev. Discuss., author comment AC2  
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## Reply on RC1

Stefan Hergarten and Jörg Robl

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Author comment on "The linear feedback precipitation model (LFPM) - a simple and efficient model for orographic precipitation in the context of landform evolution modeling" by Stefan Hergarten and Jörg Robl, Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-179-AC2>, 2021

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Dear Kyungrock Paik,

thank you very much for your review! Unfortunately, we did not see your very recent paper in HESS and will of course refer to your study in the discussion of our examples in Section 7.

The focus of our paper is indeed on the new model approach for orographic precipitation -- but of course only on a minimum level required for modeling the co-evolution of topography and climate. We started from the model of Smith & Barstad (2004) (SB model in the following) and tried to improve it in detail first, e.g., by lateral dispersion. However, it turned out that the SB model is very limited, although a good approach at the time. As discussed in Section 6 of our manuscript, the SB model cannot capture large-scale patterns. Smith & Barstad already mentioned the respective scales in their paper (Eq. 7), and the modified source term introduced in their Section 3 does not change this behavior fundamentally. So about your comment "L364-365: If this is correct, it can be a strong reason to develop an alternative model.": Of course, it is, and designing a new model from the scratch turned out to be simpler than extending the SB model accordingly.

Since the SB model is apparently often implemented as some kind of black box (which is tempting because the Fourier representation is formally simple), it makes sense to extend the comparison of our model and the SB model a bit. Maybe we can use the scenario with the plateau from Sect. 3(c) considered by Smith & Barstad (2004). There, the limitation of the SB model becomes immediately visible.

But anyway, would you really say that the SB model was validated thoroughly? The original paper ended at a somehow realistic precipitation pattern, but to be honest, it was not much more than a stronger precipitation at the windward side compared to the leeward side. The subsequent study (Barstad & Smith 2005, J. Hydromet.) mainly revealed that a validation is difficult. Even in the later paper by Barstad & Schüller (2011, J. Atm. Sci.), the only real-world example was not very convincing at the leeward side. Right in the beginning, we did some comparison with TRMM data at the Himalayas, where our model was able to nicely reproduce the large-scale precipitation patterns. However, in nature there is more than one moisture source / wind direction, and precipitation is not only controlled by orography -- so the match was not perfect. Hence, we did not find the

comparison very useful and then decided to focus on the comparison to the previous models and to show some potential applications where the properties of the model are relevant rather on a qualitative level.

About the technical comments:

(1) L39: This concept, i.e., in reality the flow discharge, instead of the drainage area, controls the erosion is not new. For example, it was stated in Paik (2012 ref below) as "While the above equation expresses the erosion rate as a function of the drainage area, it should be the flow that contributes to the bedrock erosion in reality. In the formulation of empirical equations, the drainage area has often been chosen as a surrogate of the flow discharge due to the difficulty of measuring flow discharge. However, there is no need to use the drainage area instead of flow in the numerical modeling."

It was not our intention to claim that the idea was new. At least the studies cited in line 38 of our manuscript used this concept. The models can, of course, easily be written in terms of discharge. However, many concepts in the field of landscape evolution and tectonic geomorphology (e.g., erodibility, steepness index) refer to catchment size. Therefore, the models usually involve an actual precipitation and a reference precipitation, while we prefer the concept of the catchment-size equivalent described at the end of the paragraph (Eq. 2). To be frank, we have no idea what to do with the comment.

(2) Some notations are not defined in the text, e.g.,  $u_v/c$  in equation (3).

We thought that defining  $u_v$  and  $u_c$  and speaking of the "respective" property, it should be clear that  $u_v/c$  can be either  $u_v$  or  $u_c$ . Anyway, we can mention this explicitly if you find it helpful. And please let us know whether there are any other undefined notation.

(3) L140: I personally had also been tempted to use this approach. But I have had the following peer comment on this idea some time ago: ".. the temperature change with mean elevation change is not likely to represent a reasonable assumption. Yes, the atmosphere is cooler over mountains - but the air source for the precipitation is over an ocean and topography is not going to force the air to be cooler upwind of it." I still advocate this equation but you would need some supporting argument for its use.

What did your colleague tell you? The decrease of temperature with increasing altitude is the basis of all models in this context. Also of the SB model that you used in your study. If the air is blown uphill, it is of course cooled down already by adiabatic expansion. So we either missed the point or your colleague told strange things.

(4) L364-365: If this is correct, it can be a strong reason to develop an alternative model. But you should demonstrate it in comparison with real observation and Smith and Barstad (2004) model to convince it.

Of course, it is and this was exactly the reason developing this novel approach. Please see comment above.

(5) Section 7.1: The simulation domain here is only about a few hundred km. How can these simulations capture the 'continentality'?

We see continentality just in the sense that the moisture input from the ocean decreases with increasing distance from the ocean. This decrease is controlled by the parameter  $L_l$  in our model. Since we wanted to use basically the same mountain range geometry in Sections 7.1 and 7.2, we tested a "realistic" value of  $L_l$  where the effect is rather weak over the scale of the mountain range and an artificially low value of  $L_l$  where "continentality" is even visible at this scale.

Best regards,

Stefan and Jörg