

Geosci. Model Dev. Discuss., referee comment RC1
<https://doi.org/10.5194/gmd-2021-179-RC1>, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Comment on gmd-2021-179

Kyungrock Paik (Referee)

Referee comment on "The linear feedback precipitation model (LFPM 1.0) – 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-RC1>, 2021

Co-evolution of topography and local climate is a hot subject, and a numerical modeling approach as attempted in this paper is highly anticipated. I found this paper interesting and well-written. I see a great potential contribution of this paper to the community. Nevertheless, basic questions remain as follows, for the submitted manuscript.

I first am a bit uncertain about the 'main' focus of this study. Is this to propose a new orographic precipitation model? Or do authors put focus on co-evolution? I think this has to be clarified first. I have different comments depending on the choice. If the former is the focus (seemingly from the title of the manuscript), I feel that it requires model comparison and validation with observed data. Earlier orographic rainfall models (e.g., Smith and Barstad, 2004 cited in this work) have done this job thoroughly. By contrast, there is no single comparison with a real precipitation field. This is something missing and requires some work.

If the focus is the latter, I recommend authors to revise the title and importantly compare their results with earlier co-evolution studies. Many co-evolution modeling studies have been published recently, and most of them are not mentioned in this manuscript. I suggest authors first to check the following paper, just published in HESS, and references cited in that paper.

Paik, K. and Kim, W.: Simulating the evolution of the topography–climate coupled system, *Hydrol. Earth Syst. Sci.*, 25, 2459–2474, <https://doi.org/10.5194/hess-25-2459-2021>, 2021.

Even the focus is on co-evolution itself, some validation of a new orographic model is still desired. However, as long as the authors make good scientific contributions with their

modeling, the necessity for thorough validation is less important than the previous story. There have been some earlier studies that I remember which adopted very simple orographic models with little validation but were published due to their independent scientific contributions.

Below, I provide more technical comments.

* 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."

Paik, K.: Simulation of landscape evolution using a global flow path search method, *Environmental Modelling & Software*, 33, 35-47, <https://doi.org/10.1016/j.envsoft.2012.01.005>, 2012.

* Some notations are not defined in the text, e.g., u_v/c in equation (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.

* 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.

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

Paik

