

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2021-179

Sebastian G. Mutz (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-RC2>, 2021

Assessment of the study's contribution

The manuscript represents a potentially very important contribution to model-based approaches in the field of tectonics-landscape-climate interactions. A common problem in landscape evolution modelling is the efficient inclusion of realistic orographic precipitation, since General Circulation Models (GCMs) have weaknesses in representing such precipitation, and non-hydrostatic regional climate models (RCMs), which are able to represent orographic precipitation much better, are equally complex and also have high computational requirements. An efficient orographic precipitation model, that is able to respond quickly to orographic changes produced by landscape evolution models (LEMs) or prescribed topography therefore bridges this gap in modelling. The authors address an important and widely recognised gap by presenting an alternative to the previous, simple orographic precipitation models, such as Smith and Barstad's model based on linear theory for orographic precipitation (LTOP). While the LTOP model has increased in complexity over time and represents a viable option for LEMs, the model presented here has some advantages over it, and model diversity in general increases the overall reliability and knowledge gain of the community's modelling efforts. The presented study therefore is, in my eyes, a very valuable contribution to the LEM and Earth system science community in particular.

General Comments

The manuscript is well written and generally easy to follow, as is appropriate for a manuscript that is of potential interest to different geoscientific communities. The authors present the readers with backgrounds on SPIMs, the need for simple orographic

precipitation models, and how the model presented here complements previous approaches. I believe this is appropriate given the contribution assessment above. The readers are talked through the governing equations and model in sufficient detail to develop a feeling for the model's potential applications and limitations. The demonstrations (section 7) are particularly useful for the LEM community. The conclusions are helpful for readers to determine the suitability of the model for their purposes. I do, however, have a few (mostly minor) concerns about this study. I believe these can be addressed fairly easily:

1. Title: Since the focus of this study - judging by introduction, examples and references - currently lies on presenting an orographic precipitation model specifically for LEM/geomorphology community, I think it is better for the title to reflect that when it is published in a journal that also sees publications of climate models "for climatologists". If the manuscript is intended to simply present an orographic precipitation model, the text would have to be adjusted to highlight how it fits into the realm of climatology/meteorology and its vast model landscape. Given that this type of model is likely most needed in the geomorphology/LEM community, I would simply adjust the title here rather than change focus of the manuscript.

2. The study's focus (only a potential concern): If the study's focus is the perceived one (described above), my only concern is the title. The model of course has potential applications beyond the geomorphological community. However, if the idea is to address a wider audience in this particular manuscript, I would expect much more discussion of its fit into the climate model landscape, as well as (performance and skill) comparisons to models that are well established in climatology for precipitation simulations in orogens (e.g. WRF), for example by application of the model presented here to a region already investigated with WRF and/or other models (ideally of varying complexity).

3. Model validation: The manuscript describes well the conceptual differences between this and comparable models (e.g. LTOP), and the model construction seems very reasonable. However, it is not clear what its prediction skill is compared to other models. There is no application of the presented model to a real setting, followed by a comparison to observational data or other comparable models. Esp. for scientists interested in applying the model outside a purely theoretical framework, this lack of validation is problematic and should be addressed.

4. The manuscript lacks discussion of the potential applications (and caveats) of the model outside the more theoretical realm/sensitivity experiments. The point above is one way to address this. Furthermore, I imagine that this model is of great interest to those investigating the co-evolution of orogens, climate and landscapes for real settings and times in the past. To do that, however, a number of additional steps need to be taken (see specific comment for L48-51). I think a discussion of this would increase this study's usefulness and also avoid ill-informed use of the presented model.

5. Equations (minor point): Each term in the equations, starting in the introduction or at least from the very beginning of section 2.1, should be given units explicitly. Partially, this suggestion may stem from the way I think of and follow/read equations (I find it more difficult to think them through without units in front of me), but it would enhance reproducibility and help avoid confusion regardless. I strongly suggest clearly stating the units for each of the terms in the equations throughout the entire manuscript, even if they are just the SI units the terms are usually expressed in. I also recommend going through all again carefully to catch possible oversights during write-up (see specific comments).

6. Code documentation: This point is not directly related to the manuscript, but important for potential users nevertheless. As someone who is actually interested in applying this model, I downloaded the code for openLEM from the link provided here. My go-to language for modelling (and most other things) is Fortran, but I usually don't have issues following C++ code if it is well commented and/or documented. However, it is difficult to locate the relevant code if I am interested in only the orographic precipitation model (decoupled from openLEM). Much of it seems to be in orogen.cpp, but much of the code lacks sufficient comments to navigate easily. I think a clean documentation, more comments and orographic precipitation model packaged as a separate model (decoupled from openLEM) will remove barriers for other scientists to use it. I appreciate the explicit offer of assistance in the "code and data availability" statement, but think an independence of the authors' assistance through documentation benefits everyone, including the authors.

Technical/Specific Comments

Below, I suggest a few small corrections that came to mind during reading.

L4: GCM coupling not only increases the complexity, but GCMs also have notable weaknesses in representing precipitation, esp. in mountainous regions. That is arguably the bigger problem of using GCMs. In case of RCMs like WRF, "only" the increased complexity and high computational demands remain a problem. I suggest a small adjustment to this statement in the abstract.

L18: "[...] the the geometry [...]", omit one "the"

L29: Maybe change to "[...]all particles are immediately excavated once detached from bedrock." for better readability.

L48-51: In addition, GCMs would not be suitable tools for predicting orographic precipitation [e.g. Meehl et al. 2007], esp. not at the catchment scale (see above comment). However, RCMs come with the same computational drawbacks the authors mention here. I suggest highlighting this point here. That said, once a study is upscaled for larger orogens in studies of how their evolution is linked to climate, landscape evolution and erosion, the changes in large scale surface uplift has significant impacts on regional and global climate [e.g. Takahashi and Battisti, 2007; Paeth et al., 2019], and thus on the boundary conditions (moisture availability, wind and therefore advection velocity , etc.) for RCMs or less complex orographic precipitation models like LTOP or the model presented here. This means that once larger changes are introduced to an orogen, there is no way around running GCMs, even if they then simply drive simpler orographic

precipitation models rather than RCMs. The same is true once we leave the realm of sensitivity experiments and look at an orogen in the geologic past, when palaeoenvironmental boundary conditions create a very different global climate and thus change the input fields for any RCM or simple orographic precipitation models [e.g. Mutz and Ehlers, 2019]. The need for GCMs for such upscaled experiments ought to be highlighted somewhere – here or (probably more fittingly) in a “caveats/warning” paragraph in conclusions, or both.

L79: I would describe it more accurately as “the goal of this study”; the goal of the paper is to present the study/model.

L97: I suggest giving discharge a different symbol in the introduction to avoid potential confusion altogether. If the authors think it is merited to referenece q in context of discharge anyway, this may be done by adding a side note a la “[new symbol] is discharge, often denoted as q in other manuscripts, [...]” in the introduction.

L135-144: Equation 15 does not follow 14 as it currently stands. However, the flaw seems to be in 14. If $\beta/\beta_0 = e^{-[a/(T_0-\Gamma H)]} / e^{-[a/T_0]}$, then 14 should read as $e^{-[a/(T_0-\Gamma H)-a/T_0]}$, i.e. the last term in the exponential should be subtracted if I’m not mistaken. 15 would then follow 14 again, so I think it’s simply a matter of getting a sign wrong during the write-up of the manuscript. For 16, it’s not clear from the text why the $-T_0\Gamma H$ term in the denominator is considered negligible.

L534 (Fig.9): The coloured dots next to uplift rates are somewhat difficult to make out. Furthermore, I suggest adjusting colours to take into consideration common forms of colour blindness. This is a general recommendation, but something I notice every time I see red next to green as here. If that has been considered when these particular shades were picked, please ignore my second comment.

L181 (Fig. 10): I suggest changing the colour scale to something other than the rainbow colours (e.g. a simple grey scale) (1) to make visualisation more accessible (consider colour blindness), and (2) because the rainbow scale has been demonstrated to be misleading due to the lack of clear perceptual ordering.

I hope my input here helps polish the manuscript somewhat and look forward to seeing a revised version.

Best wishes
Sebastian Mutz

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