Earth Surf. Dynam. Discuss.,
https://doi.org/10.5194/esurf-2018-14-AC2, 2018

Interactive comment on "Effect of changing vegetation on denudation (part 1): Predicted vegetation composition and cover over the last 21 thousand years along the Coastal Cordillera of Chile" *by* Christian Werner et al.

Christian Werner et al.

christian.werner@senckenberg.de

Received and published: 30 June 2018

We thank the anonymous reviewer for the general support of this paper. Below we first reply to the general comments raised and follow with a detailed line-by-line response to the specific comments (detailed comments given as supplement for better readability).

Concerning the novelty of our application of the DVM we slightly disagree with the reviewer and would like to emphasize that: 1. The site-specific application of a DVM to understand vegetation changes from the LGM to present is novel. We know of few

other studies that have done a calibrated and transient application of this nature. 2. As mentioned in the text, this study covers the German Priority research program EarthShape study areas. Available proxy data for past vegetation in these study areas is limited, and the transient simulations presented here provide a backbone of paleo vegetation predictions that are essential for understanding other elements of EarthShape (e.g. soil formation, nutrient cycling, climate-vegetation interactions with surface processes). Thus, we anticipate this manuscript will very useful to EarthShape participants, and hopefully a broader audience, for a necessary step needed to understand present day observations of biota and surface processes.

Concerning the reviewers comment that the coupling preparation (i.e. the landforms approach) is not well described: we agree, and apologize for the confusion. We have modified the section and also added a more detailed explanation as Appendix C to explain this better. This study mainly provides technical achievements in terms of the transient simulations from LGM to present and downscaling of those results to spatial scales that can be linked to landscape evolution modeling. Our coupling at this point is very basic, and we quite simply use the changes in predicted vegetation cover produced from the DGVM as a guide for the amplitude of change we impose in the landscape evolution model. For example, in the companion paper by Schmid et al. (2018, this issue) we use the best, currently available, parameterizations for how vegetation influence hillslope and surface water flow processes. Currently, this parameterization requires knowledge of vegetation cover. While this is simplistic, there is no more detailed approach currently available, that can also be scaled to the millennial timescales considered. Our future work will aim towards improving this, but in this set of current papers we start with the simplest approach to evaluate if the vegetation cover changes predicted are even important for surface processes studies. Quite interestingly, these vegetation cover changes we predict are in fact important, and we'll build upon that in the future. The manuscript text has been updated to reflect the above comment. We hope this satisfies the reviewers concerns and helps to clarify any confusion we caused.

We thank the reviewer for highlighting these points, and we've modified the manuscript to address the concerns of the reviewer. In particular we (a) introduce a paragraph where we highlight again why the chosen approach is superior to existing methodologies (although a true evaluation of the effect of these differences can only be shown when the model actually is coupled and thus has to be addressed in future work), we (b) revisited the relevant sections to clearly state the aims of this paper: (i) develop and evaluate a regional DVM parametrization, (ii) introduce a new approach to efficiently simulated sub-grid heterogeneity and enable future coupling between models of different spatial resolutions, (iii) investigate the temporal succession of biome composition for the four ES focus sites and the resulting changes of vegetation cover and runoff from LGM to PD, (iv) investigate the effect of paleo climate model drivers and changes of atmospheric CO2 on the simulated vegetation and vegetation composition. And (c) we added a quantitative evaluation of simulated FPC (MAE between model results and MODIS data, Sect. 4.3). We address many of the points by introducing a section illustrating the effect of the new landform component on simulated vegetation cover (Appendix B, Fig. S4) and also explain the novelty of this approach in more detail (see also response to Reviewer 1). We extended the section explaining why the global DVM setup is insufficient (need for regional PFT adaptation) for this application and also checked all sections discussing the simulated runoff results. Even though the capability of LPJ-GUESS and other DVMs to simulated a hydrological cycle in detail and thus account for the effects of i.e. surface runoff and transpiration on denudation rates makes them in our view a great tool for this research, we did not try to fully investigate the simulated hydrological fluxes in this study as a) part II (Schmid et al., 2018) currently does not use surface runoff in the first simulations and b) little data is currently available to thoroughly check the results. For reasons of completeness and the interested readers we still report those numbers here, although the major focus if this manuscript rests on the simulation of vegetation cover. However, the future coupled model setup will include surface runoff effects on landscape evolution and we intent to then also include a full evaluation of those model results. To make this clearer to the

reader, we carefully checked all sections where runoff is reported and added cautious notes. We also added a section that illustrated the effect of the new landform mode on FPC for the four ES sites as requested (Section 5.1)

Although the questions are raised in the introduction, what makes a DVM useful over the simplified vegetation representations used so far in landscape evolution models, or a particular DVM more useful than others, for its coupling with landscape evolution models is left untested and unanswered in the paper. And some of the relevant bits of information (e.g. P.10, L.16-22) are buried deep. I invite authors to rethink about their last sentence "The current simulations are an important step towards applying such a coupled model to the study area of EarthShape" and their main conclusions listed few lines above that. None of their main conclusions is about or linked back to the importance or potentials of such coupling. This paper should clearly convey how much more we learn about vegetation from DVMs - or from your particular version of a DVM - that is crucial to know for improved predictions of landscape evolution, that otherwise we could not know." We did expand the discussion (5.1) to discuss the effects of landforms on FPC and how this is useful for a future coupling to a LEM. We also did change the mentioned section to the following:

(6) We consider the implementation of a landform classification a feasible tool to a) mediate between coarse DVM model resolutions and generally higher resolution LEM with little computational expense and b) to account for sub-grid variability of micro-climate conditions that are otherwise absent from DVM simulations at larger scales In summary, we suggest that coupling state-of-art dynamic vegetation modelling with LEMs has great potential for improving our understanding of the evolution of landforms as the DVM using the landform approach can approximate spatial heterogeneity observed in the field that otherwise is not represented by standard DVM implementations. The FPC linked to topography structure will likely result in varying denudation rates in the landscape and have thus the potential to influence landscape evolution. The regional model adaptation and illustrated model improvements are an important step

towards applying such a coupled model to the study area of EarthShape.

Please also note the supplement to this comment:
https://www.earth-surf-dynam-discuss.net/esurf-2018-14/esurf-2018-14-AC2-supplement.pdf

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