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Earth Surface Dynamics Discussions

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.

Anonymous Referee #2

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This study aims to provide a basis for importance of accounting transient vegetation changes in landscape evolution models. By demonstrating the ability of a DVM to simulate vegetation at compatible scales with landscape evolution models, authors' objective is to highlight the potential for coupling vegetation and landscape modelling.

The study is well executed and well written in general. However, I could only recommend the publication of the manuscript after addressing some major points. The overall quality and the meticulousness of the paper suggest that such revisions are within the reach of the authors. Please see the comments below.

GENERAL COMMENTS

Neither adaption of a DVM for a particular study system nor simulating the past transient vegetation dynamics with a DVM is newsworthy anymore, unless novel methods are introduced in their application. Which brings us to the novelty of this paper: the coupling (or rather, the preparation towards coupling) of a DVM to a landscape evolution model. However, the manuscript fails to describe the steps that makes this coupling possible and discuss the approach with sufficient detail.

For example, in the final paragraph authors claim "In summary, we suggest that coupling state-of-art dynamic vegetation modelling with landscape evolution models has great potential for improving our understanding of the evolution of landforms" whereas this is not the essence of the current text. The text currently merely reports the simulated vegetation composition and cover over the last 21K years in fairness to the second part of its title. However, as I mentioned (although maybe not for Coastal Cordillera of Chile) this has been done multiple times by now. What distinguishes this study from such previous studies in terms of its potential to improve landscape evolution models and estimates of denudation rates?

Is it the improved ability of a regionally parameterized DVM to reproduce regional vegetation? Which is, by the way, only evaluated qualitatively and only through visual comparison, whereas more quantitative approaches are available in the literature. Then, comparison of results with a globally parameterized version is also necessary. Is it the importance of using a model that explicitly simulates the hydrological cycle and outputs runoff, evaporation, evapotranspiration directly, say, instead of indirect calculations of these variables from simulated vegetation cover? Then, comparison with such indirect calculations and their evaluation against data is necessary. Is it the introduction of landforms in a DVM and getting the topography as close as possible (P.8, L.5)? Then, the version with landforms should be tested against a version without, at least at the four sites. Besides, in my opinion, this novelty itself is not sufficiently explained, please see specific comments below.

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.

SPECIFIC COMMENTS

Introduction

P2., L.21: Could you provide examples of vegetation processes influencing erosive processes on comparable temporal scales?

P2., L.24-25: Please provide citation for the 120 ppm CO2 compensation point.

Background

This is a good place to include another short section to inform the reader about climate-vegetation interplay on erosive processes in Chile so that they can follow interpretation of results later. What does high precipitation-high vegetation cover or low precipitation-low vegetation cover lead to? Are types of vegetation rooting strategies relevant? Basically, guide readers to pay attention to certain aspects in the coming sections.

Methods

Eqn (1) is not referenced in the methods, and “n” and “A” are not mentioned.

Landform classification

If I understand correctly, the landforms are affecting simulations via temperature, radiation and soil depth, right? And the temperature difference is calculated with a fixed lapse rate (P.5, L.15)? Whether this is a value authors calculated or obtained from literature is not clear. How were the adjustments to the radiation received by a landform made using the slope and aspect (P.5, L.16)? There is no further explanation/equation. Ideally, a script could be provided for reproducibility of this section. Could you elaborate why no adjustment was applied to the precipitation? Could you also report how many simulation entities (grid cells/landforms) you started and ended up with after landform classification, and how much it would be different if you were to statistically downscale all the grid cells to obtain the same spatial scale? The contrast might help highlight the strength of this approach.

Table 1: Please provide what subscripts (e.g. i-t-m) stand for here as well.

P.6, L.7: almost repeating information with P.5, L.27-28.

P.6, L.17: no further information is provided about how the downscaling and bias-correction was performed. If the authors followed a previous study, please cite. Otherwise, please provide sufficient information or scripts for its reproduction.

P.8, L.24-26 and Figure 7: Authors use statements like general / strong correlation, but do not report any metric like correlation coefficient. Please provide numerical comparisons. Are there statistically significant differences in these relationships between periods or between biomes?

P.8, L.35: A low hanging fruit for authors would be to compare transient vegetation dynamics for a single landform to an averaged grid cell version (as opposed to re-running simulations without landforms to test the extent of improvement provided by landform approach), and discuss the importance of resulting differences for erosive processes.

Discussion

P.10, L.3: Comparison of model simulations to observational PD vegetation should have come by now. Ideally, right after section 4.1.

Most of the section 5.1 can be moved to results.

P.10, L.30-34: Seems like something to tackle with landforms. I.e. Why not apply a correction for precipitation?

Section 5.2: Although it is good that authors provide a comprehensive comparison of past vegetation to proxy data, this discussion is again not linked back to the big picture of why this is important for a potential coupling of vegetation-landscape modeling. For instance, authors could cite some palaeohydrological study and contribute its interpretations with their findings.

Or they could discuss their findings in relation to landscape processes, such as (P.9, L21) “Despite pronounced changes in vegetation composition, FPC only increases from approx. 51% (LGM) to 59% (PD)”, (P.8, L24-25) “While the general correlation of FPC to precipitation can also observed for LGM, the variability in mesic and xeric woodlands appears to be larger.” How could these translate to erosive processes? Could other simplified vegetation representations provide similar information or are these where advantages of DVMs come into play?

In the discussion, authors could further discuss what we have learned over or built upon Collins et al. (2004) and Istanbulluoglu and Bras (2005) as these studies were mentioned in the introduction (P.2, L.6)

Conclusion

P.13, L.16-17: How can we know? There was not a single comparison to such studies with simplified vegetation representation in the discussion.

P.13, L.22: Could authors elaborate on what their planned next steps are?

Could the authors summarize their findings into a brief roadmap/checklist for the community? Say, if I have a DVM that I would like to couple with a landscape model, what advice should I follow in the light of this study?

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