

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 #1

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Review of "Effect of changing vegetation on denudation (part1) : Predicted vegetation composition and cover over the 21 thousand years along the Coastal Cordillera of Chile" by Werner et al., submitted to Earth Surf . Dynam. Discuss.

The main objective of this paper is to demonstrate that a dynamic vegetation model can be used to simulate vegetation at temporal and spatial scales consistent with those of landscape evolution models (as stated by the authors themselves at the beginning of the discussion section), towards a coupling of both types of models. The study focuses on the vegetation of the Coastal Cordillera in Chile and its evolution since the

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last glacial maximum. It uses the LPJ-GUESS dynamic vegetation model forced by climatic outputs of a transient simulation (TraCE-21ka) of the Community Climate System Model version 3 (CCSM3) from the last glacial maximum to the present. The results are analysed along the two main variables that are important for land denudation process in the landscape evolution model, i.e., vegetation cover fraction and surface runoff.

There are mainly two aspects which, to my knowledge, can be considered as novel in this paper: (1) the development of specific plant functional types for the Coastal Cordillera in Chile and their use in a dynamic vegetation model to study vegetation changes since the last glacial maximum, and (2) the development of landforms within a dynamic vegetation model. Thus, the paper deserves publication. It is moreover well written and relatively well organised. It can be published after a minor revision addressing the few comments summarized below.

Major comments

I have two main comments that the authors should address in their revised version:

(1) It is a bit strange to find the validation of the model near the end of the paper, in the Discussion section (Evaluation of predicted PNV), and furthermore this validation should be more quantitative. This section should come much earlier in the paper, probably at the beginning of the Results section. Other parts of the Discussion section could also be transferred to the Results section (but maybe near the end of the section): the sensitivity tests performed in subsections 5.3 and 5.4; these are results and not just discussion. More importantly, I feel that the validation of subsection 5.1 should be improved. As it is now, it is limited to a visual comparison of biome maps, as well of the foliage projection cover map predicted by the model with the vegetation cover map derived from MODIS data. You should provide at least some statistics for this comparison. Also, there is no validation of runoff, while it is reported as a very important variable for the landscape evolution model.

(2) It is not clear to me that this study fulfils the objective of demonstrating the ability

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of a dynamic vegetation model to produce results useful for the spatial and temporal scales of landscapes evolution models. The authors develop a landform sub-model, but they do not really test it. They just present some transient evolution for one landform in each of four given model pixels. However, we do not know how far the use of the landform sub-model improves model prediction. It would be useful to illustrate the landform results for a given pixel (in addition to the altitudinal profiles that I guess use the landforms). Also the results of simulations with the landforms should be compared to those obtained with a model without landforms. How far does it improve the comparison with observed vegetation, or with MODIS vegetation cover? How far the results are affected by the adjustment of radiation for slope and aspects, or by the change in soil depth from the valley to the mountain slope or ridge? Landform modelling is a novel aspect put forward by this paper. So, it is important to discuss it more fully.

Minor comments

Introduction

- p. 2 lines 15-20: this paragraph provides a review of the use of DGVMs for paleoclimatic applications. They, however, mostly refer to studies performed with LPG-GUESS. Please, please provide also examples of studies performed with other DGVMs.

Methods

- p. 4, line 25: “We approximate the fraction A of the land surface ...” instead of “We approximate the land surface ...” - p. 4, line 37: Field capacity looks strange here. This would mean that a bucket approach is used in both layers. However, since drainage is not possible below field capacity (this is its definition), it would mean that subsurface runoff and percolation rate through the second layer are always zero in your model. Please check. - p. 5, line 15: you use a constant average lapse rate of $6.5^{\circ}\text{C}/\text{km}$, whereas the lapse rate could significantly vary, especially in desert areas where it could tend towards the dry adiabatic value of $9.7^{\circ}\text{C}/\text{km}$. Moreover, other climate variables can change significantly with elevation in mountain areas, such as

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precipitation, cloudiness and air relative humidity. - p. 6, section 3.4: for PD, you use the 1960-1989 period. Does the atmospheric CO₂ for PD correspond to the mean CO₂ during this period? If so, it is significantly larger than the Holocene mean value and it is thus necessary to perform a pre-industrial simulation in addition to PD, in order to separate the CO₂ and the climate effect in the difference between PD and MH.

Results

- p. 7, line 20: “the Deciduous ‘Maule’ Forest occurs as total rainfall decreases and rainfall seasonality increases.” According to table 1, the ‘Maule’ Forest is a temperate forest made of temperate summergreen trees, not raingreen trees. So, we would expect that it is the seasonality of temperature and not rainfall that determines the occurrence of these trees. Please be more precise on the processes that link this forest to rainfall seasonality. - p. 8 lines 19-21 and table 3: FPC in the south is lower during MH than at PD. Why? According to Fig. 3, in the south, the climate is wetter and colder during MH. We would expect larger FPC. Is the difference due to CO₂, which is larger at PD? This needs to be commented. - p. 8 line 35: “. . . between PFTs that might otherwise be lost . . .”

Discussion

- p. 10, line 19: “The surface runoff simulated here was found to be consistent with expected patterns” – This is not really true, since no validation of runoff has been made. - p. 12, line 38: Hickler et al., 2015; Zhu et al., 2016 – Please refer to earlier literature, this has been discussed much earlier by many authors.

Table 1

- Please provide, as far as possible, example species for all PFTs

Table 2

- it might be interesting to also list in this table the PD biome areal extent from the observed map of figure 1, in order to compare them with the model

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Table 3

- according to the legend, the table lists PD absolute values, but relative changes (in % ??) with respect to PD for the LGM and MH. However, the title at the top of the table, runs over three columns, which is misleading, because it suggests that all values are % cover or mm yr-1, i.e., absolute changes. Please revise.

Figure 1

- it might be useful to provide a map of elevation next to the vegetation map

Figure 8

- Legend, line 11: “ dark grey” instead of “darkgrey”

Appendix A

- p. 32, line 8: “. . . completely different shapes . . .” instead of “. . . completely shapes. . .”

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