Response to RC2

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.

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.

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.

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.”

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 CO_2 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.

Specific comments

P2., L.21: Could you provide examples of vegetation processes influencing erosive processes on comparable temporal scales?
We added a paragraph illustrating the relationship between vegetation and erosive processes.

"Acosta et al. (2015) showed that ^{10}Be-derived mean catchment denudation rates are lower for steeper but vegetated hillslopes in the Rwenzori Mountains and the Kenya Rift Flanks than the erosion rates for sparsely-vegetated, lower-gradient hillslopes within the Kenya Rift zone. Jeffery et al. (2014) highlighted that vegetation cover plays a major role in controlling Central Andean topography which links directly to the potential erosion in those areas. On a shorter timescale, Vanacker et. al. 2007 determined that the removal of natural vegetation due to land use change increases sediment yield from catchments significantly, while catchments with high vegetation-cover, natural or artificial, return to their natural benchmark erosion rates after reforestation."

Vanacker, V., von Blanckenburg, F., Govers, G., Molina, A., Poesen, J., Deckers, J., and Kubik, P: Restoring dense vegetation can slow mountain erosion to near natural benchmark levels. Geology, 35(4), 303–306, doi: doi.org/10.1130/G23109A.1, 2007.

P2., L.24-25: Please provide citation for the 120 ppm CO_2 compensation point.
This was a typo. The correct value should be 150 ppm. We also added the reference Lovelock and Whitfield (1982).

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.
We did include a brief transitional paragraph as suggested but, in light of the overall length of the manuscript, decided against a longer section. We did however try to expanded relevant parts of the introduction to cover the questions raised.

Methods
Eqn (1) is not referenced in the methods, and "n" and "A" are not mentioned.
Corrected

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.
We acknowledge that the original manuscript was lacking detail in this section and extended the explanation of how the landform-approach alters site conditions and thus vegetation development and cover. As some of these questions were already raised by RC1 we refer to our response given there. In short, the lapse rate for temperature correction for landform elevation differences was based on the International Standard Atmosphere (e.g., Vaughan, 2015).
Two key technical advantages of this approach are that a) we do not have to match the high-resolution of the landscape evolution model when the two models are coupled (which might be wasteful computationally as large sections of the sub-grid topography might share topographic conditions and thus would produce identical outputs). The landforms act as a mediator between the coarse resolution model drivers and help to aggregate topographic characteristics and then

disaggregate vegetation cover. Further, b) we can keep the general model infrastructure (i.e. model drivers and resolutions) as is and do not have to create a separate down-scaled driving dataset.

We added a new Appendix C that illustrates the concept and implementation of the landforms in LPJ-GUESS in detail.

Vaughan, W. W.: BASIC ATMOSPHERIC STRUCTURE AND CONCEPTS, Standard Atmosphere, 12-16, 2015.

Table 1: Please provide what subscripts (e.g. i-t-m) stand for here as well.
We added the extra abbreviation information in the caption as suggested.

P.6, L.7: almost repeating information with P.5, L.27-28.
We deleted the duplicate sentence on page 5.

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.
Following Hempel et al. (2013), we used an additive bias-correction for the temperature, and multiplicative corrections for the precipitation and downward shortwave radiation (the same technique was also used in e.g. O'ishi and Abe-Ouchi 2011). The reason why we use multiplicative instead of additive corrections for precipitation and radiation is due to the fact that these fields cannot be less than zero. After the bias-correction, the resulting anomalies were interpolated to the ERA-Interim grid using bilinear interpolation. We have clarified all this in the main text.

Hempel, S., Frieler, K., Warszawski, L., Schewe, J., and Piontek, F.: A trend-preserving bias correction - the ISI-MIP approach, Earth Syst. Dynam., 4, 219-236, doi: 10.5194/esd-4-219-2013, 2013.

O'ishi, R., and A. Abe-Ouchi (2011), Polar amplification in the mid-Holocene derived from dynamical vegetation change with a GCM, *Geophys. Res. Lett.*, 38, L14702, doi: 10.1029/2011GL048001.

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?
We agree with the reviewer that the claim was not justified by hard evidence. We change section and added an analysis of corellation coefficents between time-slices and selected 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.
This is indeed a good idea and we added this to the manuscript (Section 5.1, Fig. S4). See also our response to R1.

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.
This was indeed a problem in the original manuscript organization and also identified by Reviewer 1. We thus broke this section up and moved descriptions of the model results into the relevant result sections and merged the discussion paragraphs into the main discussion section.

P.10, L.30-34: Seems like something to tackle with landforms. I.e. Why not apply a correction for precipitation?
While a correction of precipitation by adding assumed fog precipitation could be used, the uncertainty in the occurrence and the small scale of this phenomena would add a large uncertainty to our simulations and would also be impossible to validate for past times. Furthermore, these effects are dependent on stratification of the lower atmosphere, the proximity to the coast (and sea surface temperature) and thus beyond reach for a general model parametrization that is intended to be applicable for larger areas and long time periods. We did add a paragraph discussing the omission of a precipitation correction in section 5.1.

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?
We added a clarification as for why we did a thorough comparison with paleo vegetation record (i.e. to demonstrate the capability of the model to simulated conditions of differencing climatic conditions and the resulting changes of vegetation composition and cover). However, one result of this study was that unfortunately the global paleo climate dataset TraCE-21ka seems to underrepresent substantial hygric changes observed by proxy records. Therefore, a detailed comparison of our runoff results to palaeohydrological studies is not helpful. We made sure that we mention this data limitation at the relevant sections in the text. We also extended the section by discussing the effects on erosive processes. In general, as mentioned in other responses, we checked that we provide a better link from our study part1 to the general topic of vegetation effects on erosion dynamics.

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)
We are a bit reluctant to add a discussion section about this as we do not have the actual model-coupling in place and thus cannot report findings of the effects (this is work in progress and will be evaluated in detail once we have a fully-coupled model).

However the mentioned papers treat vegetation cover as a cumulative value of total ground cover. Our study shows that for some model domain, even if the cumulative change in vegetation cover may be small, there exists a large change in PFT distribution which should be considered when thinking about the effectiveness of surface processes. Future studies should try to incorporate the individual effects that different PFT's (e.g. differences in LAI lead to different values of rainfall interception, different root densities and distributions lead to different values of soil cohesion etc.) exert on the land surface into the description of their landscape evolution models.
We did highlight advantages of a state-of-the-art DVM in the text over traditional landscape evolution model vegetation representations when appropriate (i.e. fire dynamics, response to changes in [CO_2] (section 5 and 5.4).

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.
We rephrased the sentence to: "The simulation also captures vegetation change drivers that are not explicitly represented in simplified vegetation representations used so far in landscape evolution models, such as plant-physiological effects of changing [CO_2], fire dynamics that varies greatly with PFT composition and interaction with soil water resources through different rooting strategies."

P.13, L.22: Could authors elaborate on what their planned next steps are?
We expanded the paragraph with some detailed next steps: "In future work we will implement a two-way coupling of the dynamic vegetation LPJ-GUESS to the landscape evolution model LandLab. LPJ-GUESS will be driven by climate data and produce a continuous dataset of vegetation cover and surface hydrology that is passed to LandLab. Landlab will use this vegetation cover and simulated denudation rates and, in turn, will provide updated topography (and after a landform classification updated areal cover of landforms) and the associated soil depth information to LPJ-GUESS."

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?
Unfortunately we do not see that a general roadmap can be provided, as DVM greatly differ in model composition and process representation. However, one general guidance would be that one needs to bridge the scales of these models. We believe that running DVMs at LEM resolutions is only feasible at the small catchment level. But even in these small-scale studies the duplication of vegetation properties for similar DEM cells and landscape positions seems wasteful and could be avoided by adopting the proposed landform approach allowing for a more efficient simulation.
We added this suggestion as an additional bullet point to the final conclusions: "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".