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Interactive Comment

Interactive comment on "TopoSCALE: deriving surface fluxes from gridded climate data" *by* J. Fiddes and S. Gruber

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AUTHORS REPLY TO REFEREE #1

We would like to thank Anonymous Referee #1 for the effort in evaluating and commenting this manuscript. In the following text the referee's comments are marked "RC" and author comments "AC". We have subdivided comments alphabetically where appropriate. All RC references refer to original manuscript, whereas all AC references refer to the changes made in manuscript to be resubmitted.

As a general observation, we feel that the intended purpose of the described method has been misunderstood somewhat; we interpret this as a failing in our introduction and problem statement. We will therefore make changes in the introduction to insure we convey the following three core points, which summarise the problem we address:

(A) It is often difficult to apply surface models in remote areas due to lack of observational data with which to drive them.

(B) Gridded climate datasets can help fill this gap due to global coverage, but there is a mismatch of scale between the surface model and coarse climate model grids. Also many down-scaling techniques require local observation that may not be available in remote regions.

(C) There is information contained in vertical profiles of climate model datasets that can be used to help cross this scale-gap, especially in mountains.

SPECIFIC COMMENTS

RC1: The structure of the paper is confusing in terms of contents of Background, Methods, Data and Experiments. At present, the structure is very unprofessional and does not have a flow. A restructuring of the contents is absolutely necessary.

AC1: We have addressed this through the more concrete comments from Referee #2 (RC7).

RC2: (a) The 'TopoScale' method used in this paper simply interpolates temperature, relative humidity and wind at sub-grid scale (SUB) topography based on the given grid-scale (GRID) atmospheric forcing data at pressure levels (from 1000mb onwards). (b) First, the pressure level forcing data below the GRID topography should be masked for analysis. Then for SUB topography higher than the GRID, the interpolation may be done, but it is still a very very crude assumption. (c) The mountain valley circulations in these complex terrains generate their own diurnal cycle, which can be completely disconnected from the synoptic scale weather above the ridges, depending upon different weather conditions. Bottom line, the use of this method needs to be justified and discussed theoretically.

AC2: (a) We would argue that the method achieves, in addition, a scaling of LW radiation by utilising a novel emissivity based approach together with a topographic correction of both SW and LW radiation. Additionally, a subgrid treatment of precipitation is given, by a disaggregation approach. (b) There is a difference in quality in ERA-Interim data above and below grid-level (Section 2.2), as different methods are used in the ECMWF model, i.e. data is extrapolated below the model surface (grid-level). Full reference to how this is done is provided in the ECMWF model documentation (ECMWF 2011) and additionally now included in Section 2.2. However we have shown that TopoSCALE based on below grid-level data still produces useful results (Section 5.5) as compared to e.g. a simple lapse-rate approach. (c) Again, we agree, e.g. Section 6.3: "The surface boundary layer (as opposed to the atmospheric boundary layer) will have a residual effect upon surface measurements, which will not necessarily be present in pressure-levels representing the free atmosphere. For example, turbulent exchanges of sensible heat fluxes can be a significant contributor to energy exchange between surface and atmosphere (Cline, 1997; Helgason and Pomeroy, 2012). These effects will also likely affect diurnal cycles of observations." One can ask the question of how much better we can represent local conditions without having to resort to a full simulation of atmospheric dynamics (e.g., diurnal valley winds). Here, we show that some improvement is possible while remaining at the coarse scale for the atmosphere.

RC3: It is not clear, how and why the 'Reference method' described in Section 3.2 was chosen, the use of this reference method needs to be explained. Why not use some existing statistical schemes to compare?

AC3: This underscore our thoughts on needing to expand/ clarify the introduction. The problem we address is that of situations where statistical methods can not be used by definition e.g. (final sentence of Introduction): "The methods proposed here aim to provide an alternative to statistical methods when observations are not available and be complimentary to dynamical methods i.e. they could be used to further downscale RCM output to site scale. " We also state that these reference methods are not intended to be comprehensive but are primarily there to provide a reference-frame of familiar methods to contextualise the results, which are first and foremost validated against observations. The chosen methods are from publications which have Google Scholar citation scores of 17-128, which supports our assertion that these reference methods are commonly used in the literature to extrapolate meteorological forcings.

RC4: The focus of the result section is on the daily means. The 3hourly scaled data are again aggregated to daily means to compare with observation data (available at 10mins to hourly resolution). Diurnal cycle is essential for forcing LSM, so it would be more convincing if the statistical comparisons are presented on sub-daily time scales, rather than daily mean time scale where diurnal cycle is averaged out. Especially it is important for complex terrains which has series of ridges and valley. Section 6.3 alone is insufficient.

AC4: We agree that sub-daily values are important and crucial for driving the LSM, and as the referee points out (although deems insufficient) we have touched on this in Section 5.3. The purpose of this manuscript is to explore how well we are able to capture spatial patterns, hence the choice of a simplifying aggregation level of daily-means. This level of aggregation also broadens the range of work our results can be compared to as it is commonly used. Naturally, higher temporal resolutions would not perform as well statistically against observations but the message would likely be the same in terms of relative performance between methods.

OTHER COMMENTS

RC1: Page 3382, Ln 1-5: This sentence needs to be rephrased. 'Heterogeneous' or 'Complex' terrain? What kind of lateral variability? 'At the site or scale', please elaborate. Maybe breaking the sentence into two would be more appropriate.

AC1: Rephrased, shortened sentence: Page 3382, Ln 1-5: "Simulation of land surface processes is problematic in heterogeneous terrain due to the the high resolution required of model grids to capture strong lateral variability caused by e.g., topography , and the lack of accurate meteorological forcing data at the scale it is required." We

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feel that further elaboration is unnecessary in the Abstract as examples and references are provided in the Introduction, for example: “...strong lateral variability caused by e.g. topography, surface or sub-surface processes (e.g. Gubler et al., 2011; Riseborough et al., 2008; Arnold and Rees, 2009) ,...”

RC2: Page 3382, Ln 15: Expectations should be removed from the abstract, and focus should be on the content of the manuscript.

AC2: Rephrased sentence: Page 3382, Ln 15: “This method may be of use in improving inputs to numerical simulations in heterogeneous and/or remote terrain, especially when statistical methods are not possible, due to lack of observations i.e. remote areas or future periods. ”

RC3: Page 3382, Ln 8: What does it mean by good description of the atmospheric column?

AC3: Rephrased sentence: “...the well resolved description of the atmospheric column provided by climate models, ...”

RC4: Page 3382, Ln 20-25: Explain the problem with examples rather than just citing other works. AC4: We have expanded the section with examples as suggested.

RC5: Page 3383, Ln 26: Explain lumped model simulations.

AC5 : We have removed this as we see that in this context the term ’lumped model’ could be confusing. It is common to understand the distribution of a meteorological forcing in terms of space (i.e. xy coordinates) whereas a lumped model is likely to be described by other characteristics (e.g. surface type, elevation bands) and not explicitly in xy coordinates. This implies that 1-D, 2-D and lumped models are not directly comparable in the sense we intend.

RC6: Page 3385, Ln 4: ’Complex products ?’ Rephrase.

AC6: Rephrased to: “Reanalyses datasets are complex in that they combine output

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from a climate model with assimilated observations."

RC7: Page 3385, Ln 24: What do the authors mean by 'impact model' ?

AC7: We have now chosen to remove this term for clarity, rephrased sentence: "...in providing accurate driving fields to a land-surface model (Hagemann et al., 2011) ."

RC8: Page 3386, Section 2.3: Sub-grid scale issue is the most important issue in the context of the downscaling study presented in this paper. But, this section is poorly discussed. Especially, discussion on the scaling of soil moisture due to topography and its effect on the partition of surface energy fluxes is left out. This needs to be further addressed.

AC8: It is important to remember here that the focus of this scheme is to provide a forcing that is corrected for topography (elevation, slope, aspect, shading etc.) to a surface model. We do this in a manner that is more sophisticated than commonly used lapse rate methods and applicable in regions where there are few data for statistical methods. The surface model computes the surface energy balance and is completely separate from TopoSCALE. Therefore, we would say that issues such as scaling of soil moisture, while we recognise are critical to atmosphere-land surface interaction (e.g. Seneviratne et al. 2010) are not the core problem we address.

RC9: Page 3387, Section 2.4: Data at pressure levels below model surface should be masked.

AC9: This comment is addressed in Section 1, AC2(b).

RC10: Page 3388, Ln 2: Describe Tgrid and Rhgrid. And at what height Tsub, Rhsub and Wsub are calculated from the subgrid surface height ?

AC10: All formerly undescribed variables have now been incorporated into a modified Table 1. Tdgrid is the actual ERA-I field that Rhgrid is derived from. The definition of a measurement height or term 'screen level' is a concept bound to boundary layer dynamics, and relevant especially when turbulent fluxes are low with a stable boundary layer, when there can be distinct vertical stratification of the airmass even within short (0-2m) vertical distances above the surface. Therefore, we think that placing an arbitrary measurement height on data computed from pressure level data, which does not interface with the surface boundary layer, does not make sense.

RC11: Page 3388, Ln 15: Appendix B2: Explain the wind sub model with diagrams and example.

AC11: The wind sub-model is not novel work and implemented for completeness and additionally as an option, hence its position in the Appendix. Full description of the model is available in Liston and Elder (2006), as indicated in the text.

RC12: (a) Page 3388, Eq. 1, Explain pV and T notations, (b) grid suffix missing in emissivity? (c) Eq. 2, Explain Tgrid, is it the 2m air temperature here ?

AC12: (a) All Appendix B sections have been incorrectly referred to in the text, this has now been corrected. The explanation of pV is given in Appendix B3. (b) Eq.1 is intended to be applicable both at grid and sub therefore no suffix is given. T is also intended to be generically applicable, again why no suffix is given. We have modified the equation to clarify this by adding a combined sub/grid suffix. (c): See AC10.

RC13: Page 3389, Eq. 3, (a) Should be LW sub ?, (b) and what is Tsub, 2m temperature ?

AC13: (a) This has been corrected. (b) Please see AC10.

RC14: Page 3391, Section 3.1.3, (a) Appendix A should be Appendix B5 ? (b) Explain how this 'pf' factor is estimated. (c) Also explain the inversion of 'non-linear lapse rate of Liston and Elder (2006). (d) This section is not comprehensible. Explain with clarity.

AC14:(a) as AC12(a). (b) Factor 'pf' is estimated as the mean annual value of monthly values given in Liston and Elder (2006). This fact has been added to the text. (C) This has been given as an additional equation in Appendix and re-worded in text. (d) Additional equation added to section, text rewritten, nomenclature improved and Appendix

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equations expanded.

RC15: Page 3393, Eq. 14 is missing bracket.

AC15: Yes, added.

RC16: Page 3396, Ln 3: ’No missingness ?’

AC16: changed to ’no data-gaps were...’

RC17: Page 3399, Ln 17: Should be Figure 7b

AC17: Changed.

RC18: Page 3400, Ln 7: Figure 10 (Please explain the abbreviations in the Figure in the caption).

AC18: We have changed station abbreviations to the elevations they represent, which is the relevant attribute.

RC19: Page 3403, Ln 16- 27: Remove.

AC19: As the reviewer gives no specific reason for this comment we assume it is a disagreement in style and that the concluding numbered points are possibly too verbose. We have streamlined this part of the conclusion.

REFERENCES

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Seneviratne, S. I., Corti, T., Davin, E. L., Hirschi, M., Jaeger, E. B., Lehner, I., Orlowsky, B., and Teuling, A. J.: Investigating soil moisture–climate interactions in a changing climate: A review, Earth-Science Reviews, 99, 125–161, 2010.

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Interactive Comment

http://www.ecmwf.int/research/ifsdocs/CY31r1/index.html

Interactive comment on Geosci. Model Dev. Discuss., 6, 3381, 2013.