

Interactive comment on “Online dynamical downscaling of temperature and precipitation within the iLOVECLIM model (version 1.1)” by Aurélien Quiquet et al.

Dr. Fyke (Referee)

fyke@lanl.gov

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This manuscript describes a method to downscale temperature and precipitation from a coarse EMIC grid to a higher resolution grid, such as (for example) that typical for ice sheet models, but also other aspects (e.g. mountainous environments). This is a very pertinent, needed, and surprisingly difficult topic, and it's great the authors are tackling this in iLOVECLIM.

I think some work is still required before this can be published, so suggest a number of revisions. Some general themes that require improvement in my opinion, and that are reflected in my more detailed comments, are:

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-Given the 'EMIC' nature of the model and simplifications within the downscaling scheme, I think greater emphasis on caveats associated with EMIC-embedded downscaling should be more clearly described, so that potential users are clearly aware of what aspects of their scientific simulations are a result of model simplifications, versus real processes.

-A clearer high-level, but technical overview of the scheme would be really useful in the Introduction, so the readers enter the details with a pre-existing, rough, mental construct of what to expect. E.g. 'Briefly, the downscaling procedure reimplements the original ECBILT equations at 11 vertical levels, followed by... and...' Also, repeat this overview at the end.

-Several important equations are presented without justification (e.g. 'we used this formulation because...'). Even if Haarsma et al. (1997) is cited, it would still be useful to provide a physical reasoning for the equation form.

-Given the advertised capability of the downscaling, to enable science to be done at sub grid scales (e.g. GrIS ablation zones...) it seems necessary to show some 'zoomed-in' plots of particular regions, for example, GrIS. As it stands, the reader has to squint to try and see the downscaled fields, as they are represented across the whole domain.

-Some evaluation concerns: >there appears there is a non-monotonic saw-tooth or wave pattern in precipitation across Greenland, generated by the precipitation downscaling. This would be very problematic for actual SMB fields used for science. These features, and other potentially similar 'native grid artifact' features over non-ice-sheet regions (which I am less familiar with) need to be explained in more detail. ->the authors note that downscaling appears to have a significant effect on the continental-scale distribution of precipitation at the native grid scale but don't clearly discuss why. Deeper discussion on this topic would be excellent to see.

Comments:

P1L20: “approximative representation of land surface climatic variables that are affected by variability at high spatial resolution.” Confusing, suggest more clearly indicating that by variability the authors mean spatial variability that is lost at low resolution.

P1L24: “EMICs are unable by design to reproduce correctly meso-scale atmospheric processes induced by sub-grid topography”: suggest just saying “relatively fine-scale topographic features such as mountain ranges”

P2L2: “physical description require a high spatial gridding.”: suggest: “large-scale physical behavior depends highly on processes occurring at small spatial scales”

P2L8: “superimposed to” -> “added to”.

P2L9: “Such a strategy implicitly assumes that the model biases remain unchanged through time, independently from the imposed external forcings.” Also, that biases remain unchanged as ice sheet geometry changes significantly.

P2L14: “Fyke et al. (2011) go a step further as not only temperature but also precipitation”. Technically, we didn’t actually go the step farther: only temperature is downscaled in Fyke et al (2011). However, this is used in the energy balance and precipitation calculations, rain/snow decision, and relative humidity, which impacts the decision on whether to precipitate or not. But, precipitation is not downscaled directly, *per se*.

P2L20: Importantly, I don’t think the method is downscaling heat. It is downscaling temperature - which is not the same as heat.

P2L25: “closed water budget” -> “closed global water budget”

P2L29: The scheme could also be important for dynamic vegetation models (i.e. by resolving sub gridded elevation-dependent vegetation distribution envelopes)

P3L23: Review the justification for basing the linear temperature profile on the log of pressure (even if already stated in Haarsma et al (1997) and/or basic knowledge)

P4L1: why is the interpolated T500 used to obtain the near-surface temperature, in-

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stead of T650 (which is presumably closer to the surface)?

P4L1: Explain why different treatments are applied to near-surface temperature, versus T(p). Does this result in a discontinuity in temperatures, when comparing near-surface temperatures to surface temperatures? Perhaps I'm just confused here.

P4L9: "Due to orography, the atmospheric isotherms are shifted upwards": is there a citation explaining this physical phenomenon? Given its role in motivating implementation of the f_s factor, it seems important to explain.

Equations 3 and 4: nearly duplicative. Could the authors just present Equation 4, then say "f_s=1, reduces to the original equation of Haarsma et al. (1997)"?

Equations 3 and 4: why is T* over-barred in Eq 3, but not in Eq 4?

P4L13: For completeness, perhaps explicitly state these energy balance terms, indicating which ones use as input the downscaled near-surface air temperature.

P4L19: "In ECBilt, . . ." -> "In the idealized ECBilt representation of the atmosphere, . . ."

P4L20: What are the actual z_h heights? I don't actually see them specified anywhere in the manuscript...

Equation 5: could g be taken out of the integral (also, in the original Haarsma equation)?

General: suggest including web link for Haarsma et al. (1997) in reference, as it is not available by, e.g. DOI. As it is it takes some brief Google searching to find a PDF copy.

General: how does scheme work for elevations greater than 5500m (500 hPa)? Since mountainous regions are of specific interest, and many of these regions have elevations greater than 5500m, this would seem important to note.

Equation 6: it's not immediately clear why the authors need to calculate surface pressure as a function of surface temperature? Can't the authors apply a more direct pres-

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sure/elevation relationship? If I'm wrong (quite possibly) - perhaps a clearer description of why this equation form is used, could be useful.

General: the switching between use of values at the 650 hPa and 500 hPa levels is somewhat confusing. Can motivation/clarification be given on why this switching occurs?

Equation 7: is $s_a(k)$ 'surface' or 'surface area'?

Equation 7: I'm not sure the initial $1/k_{\max}$ term is correct here.

Equation 7: the authors could just say 'the surface elevation of the native grid comprises the area-weighted average of all k sub-grid points.', could they not?

Equation 8: I'm not sure it's necessary to write out the equation for linear interpolation here. I'd be OK with just saying 'linearly interpolate from the bounding vertical levels' or something similar.

General: some devil's advocate points that might be worth addressing: why not just compute T^* , T_s , and q_{\max} at each sub grid point, using the equations described earlier? What is the advantage of first calculating these at specified levels, then vertically interpolating? Related: others (e.g. in Fyke et al. (2011) didn't use 11 globally constant levels, but rather calculated the range of levels at each point, based on the high resolution topography within each native grid cell. This significantly reduced computation #'s (for example, over large flat regions) and also allowed for finer vertical resolution in the sub gridded levels. Was this approach considered here?

P5L17: how computationally expensive would it be, really, to bin sub grid points by aspect (in relation to wind direction), especially in the context of the full coupled model cost?

P5L20: so, for leeward slopes, this means that the lowest leeward slopes receive more than high leeward slopes (and leeward slopes get the same precip as same-elevation windward slopes). These should be explicitly discussed, as a representative 'caveat'

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of the scheme.

P5L20: also, in my understanding, it is 'mid-level' windward slopes that often get the most precipitation. Whereas the described scheme bias precipitation to low levels. Another caveat to emphasize.

Section 2.3.2: 'Dynamic precipitation' is an unknown term to me (a non-meteorologist). If it isn't a common meteorological term, perhaps re-name, or explicitly define.

P5L26: if the local topography exists above 500 hPa, does it receive any precipitation at all in the model?

P5L27: "expended"->"expanded"

Equation 12: it's not clear what the physical justification is for the form of this equation. Please describe in greater detail, with citations.

General: which of the 3 contributors to precipitation (2x dynamic, and convective) is generally the dominant term? This would be useful for readers to know.

P7L2: It's not clear to me how the convective precipitation scheme works. For example, given the repeated use of Eq. 13, where does the assessment of stability come in? I think a clearer description is needed here.

P8L2: by 40%, for the whole coupled model? Or just the atmospheric component?

Section 3.1.1/3.2: note the caveat that the authors are comparing a preindustrial simulation to recent historical climatologies (or describe why this isn't a caveat, e.g., why recent historical climatology is close enough to preindustrial climatology for the fields in question, to warrant direct comparison)

P7L16: given 'continentality' is usually associated with sub-annual ranges in temperatures, what does it mean to interpret 'continentality' over Siberia, when using annual-mean fields? Furthermore, it is unclear how this relates to other regions (as it is written, it seems to indicate that increased Siberian continentality causes biases elsewhere)?

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P7L18: “does not imply important changes in surface temperature”: relative to the default CTRL case? Perhaps reword for clarity.

P7L30: Given the ‘ijk’ indexing is hardly used in the manuscript (as figures mostly show the results only from one ensemble), I’m wondering how useful it is to describe this indexing scheme? It would become more useful, if plots of results as a function of parameter space, were shown...

Figure 2/4/5/6: why was DOWN020 chosen as the representative plot? How much do the different ensemble members look, and why/why not?

Figure 2, ‘DOWN NH40’ panel: it is surprising to see remnants of the native grid in many places, though I suspect I know why. I think a description of why this remaining imprinting occurs should be clearly explained to readers.

Figure 3: The green/blue shading is quite confusing to parse, visually. Could shaded ‘clouds’ be more visually accessible?

P8L26: “...to correct the model bias...” -> “...to correct broader region model biases that are unrelated to topographic forcing...”

P8L27: “...horizontal gradients...”->”...horizontal temperature gradients...”

General: given the advertised ability of the scheme to downscale high-resolution map-view T/P fields, and the intention of the authors that the downscaling will improve ‘regional’ studies, it would be useful to see regional subset plots of Figure 2, ERA-interim and DOWN NH40. For example, given my background, I’d like a closer look at GrIS precipitation!

Figure 4: A better description of the Taylor diagram scheme would be good. For example, is ‘standard deviation’ the standard deviation in mean annual values, for the long-term climatology (or is it describing the standard deviation in temporal variability?). Similarly for the correlation axis.

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P9L8: describe why the lack of impact on native-grid model performance is a good/bad thing.

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General: a more robust description of the analysis of the full 50-member analysis is warranted. For example, which varied parameter makes the most difference? Is it possible to identify parameter combinations that are optimal, for particular locations?

Figure 5: contrary to the text, it looks to me like DOWN NH40 *does* better resolve the Norwegian/W. North America high-precipitation bands...

General: A stronger justification is needed that downscaling does indeed produce 'scientifically useful' high-resolution precipitation fields. As it is, the reader is somewhat left to their own devices to piece together the various impacts of precipitation downscaling into a coherent story on how well the precipitation downscaling scheme (perhaps the most important but tenuous aspect of the whole procedure) performs, and whether it would help/hinder their scientific simulations.

Figure 6: The GrIS cross section highlights some concerning 'saw-tooth/wave' behavior, whereby the downscaled precipitation field changes across the boundaries of the native grid. To be honest, we have struggled with a similar (?) thing in CESM, and I ended up putting together this Google-based schematic that tries to explain our particular problem: https://docs.google.com/presentation/d/1gyaIZ5ypZ3XWxf2VTThuBmkpL9Qf18Qg5w_qpbzPm6s/edit#slide=id.p Non-monotonic SMB fields that could certainly overwhelm the positive impacts of downscaling over GrIS, and preclude use of the downscaling scheme for science, where the GrIS-wide SMB field is important. Please comment on why the 'saw-tooth/wave' pattern occurs in the downscaling, and why the authors consider it acceptable for subsequent science using downscaled SMB fields.

Figure 7: as with figure 4: a better description and interpretation of the Taylor diagram would seem important. Also, it's quite hard to pick out salient differences and understand them in a broader context, given the selection of what seems like an arbitrary

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subsample the whole ensemble.

Figure 7: it's not clear now the 'spatial correlation is greatly improved', via inspection of the Taylor diagrams. Which dots should the reader compare, to see this impact? Perhaps this is just my eternal personal struggle with Taylor diagrams, though :).

Figure 4: why not use the same set of ensemble members, as in Figure 7 (for consistency, and perhaps just to show that temperature downscaling is not as parametrically sensitive as precipitation downscaling)?

P10L20: Yes, it is notable how downscaling significantly impacts precipitation on the native grid (e.g. CTRL T21 vs. DOWN T21). For example, it appears North America as a whole receives quite a bit more precipitation when downscaling is utilized. Yet this important aspect is not clearly mechanistically explained. A physical reasoning behind this non-negligible impact should be described in the manuscript.

Section 4: I think noting some of the caveats of the scheme would be appropriate to clearly reiterate in this section.

P11L13: Given the advertised importance of downscaling for iLOVECLIM simulations of Greenland, again it would seem appropriate to show 'zoomed-in' downscaled T/P (or better yet, actual SMB?) over Greenland.

P11L13: Is the iLOVECLIM SMB energy-balance-based? It seems all the ingredients are available, at least on the 'l' levels. If PDD-based, conversely, I'm not sure the authors can claim anymore to be globally conservative, since the empirical nature of the energy flux calculations in PDD schemes does not track conservation (unlike EBM models, which are premised on a balance of actual energy fluxes)

General: I think it would be very useful to technically contrast this subgridding scheme with other previously published schemes, to allow readers some greater contrast on similarities/differences.

General: provide a brief overview of the technical stages of the scheme, either at the

start, or the end, of the manuscript. As it stands it's rather easy to get lost in the details.

General: I think some plots of characteristic T/P/precipitation vertical profiles would be very useful for the reader, to see the equations 'put into action' for some representative cases. As it is, I had to spend some time at the whiteboard to get a sense of what the equations actually produced, in terms of actual profiles.

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