

# ***Interactive comment on* “Examining cross-scale influences of forcing resolutions in a hillslope-resolving, integrated hydrologic model” by Miguel A. Aguayo et al.**

## **Anonymous Referee #2**

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In the manuscript “Examining cross-scale influences of forcing resolutions in a hillslope-resolving, integrated hydrologic model” the authors present a numerical study of the impact of spatial scale of both forcing data and surface discretization on two distributed hydrological model states – soil moisture and snow water equivalent.

My broad concerns with this manuscript revolve around a) the limited details regarding the process representations used; and b) the limited mechanistic description of why the results are as they are. Despite what is clearly a lot of effort, the direct implications of the study are well known and not particularly novel. Certainly, the impact of atmospheric forcing resolution (Materia et al, 2010; Rasmussen, et al 2010; Bonekamp, et

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al, 2018; Vionnet, et al. 2020), DEM resolution (Schlögl, et al 2016; Metcalfe, et al. 2015; Rasmussen, et al 2010), forcing errors (Raleigh, et al. 2015), and process scale (Klemeš, 1983) on both snowcover and hydrology are well known (to cite but a few examples). I will elaborate on these points below.

I have a major concern with this work viz a viz the (lack of) details regarding key-processes. The authors note that as the computational grid becomes more coarse, fine scale topography smooths out and thus impacts the surface energetics and subsequent SWE distributions. Certainly, this was to have been expected and especially the case with snow cover simulation in mountain terrain where smaller computational cells can better represent the heterogeneities in mass and energy (Dornes, et al, 2008; Schlögl, et al 2016; DeBeer, et al 2017; Vionnet, et al. 2020). However, there is seemingly no consideration of any of the small- to medium-scale (i.e., the 50 m to 250 m length scales considered) processes that might be affected by the scale perturbations, and no mechanistic discussion thereof to put the results into context. For example, there is no description of canopy interception of precipitation, nor if shortwave radiation to the slopes is cosine-corrected to take into account slope and aspect nor a discussion on shadowing, a key component in mountain terrain (Marsh, et al 2012). There is no discussion on the mechanical impact of terrain on wind fields, a key consideration in alpine terrain where overlapping terrain impacts makes simple spatial interpolation insufficient (Ryan, 1977). Specifically, wind accelerates over ridge crests and decelerates on the lee-slopes (Wood, et al, 2000). Near ridges, these high windspeeds can greatly increase sublimation (Mott et al, 2018) and cause blowing snow that further increase rates of snow sublimation. Mass lost to mid-winter sublimation can be as high as 10% to 30% (Mott et al, 2018), and is a critical component for evaluating total end of winter snow mass. The authors idly muse that “[. . .] might demonstrate that snow processes are not only dependent on orographic characteristics at that specific spatial scale, but also are highly dependent on the complex interaction between mass and energy inputs coming from the atmospheric processes and local topography.” This is such a trivial, well-known statement I am uncertain why they hedge it with ‘might’! Yet,

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such a limited analysis and an obvious statement does not help the reader fully understand the impacts of the various numerical experiments. The interplay of topographic scale and NWP resolution on such key processes was missing from the analysis. It is not clear if these processes are implicitly treated, ignored, or not considered. The above noted processes are required for realistic snowmelt modelling and at the very least in such an analysis should be mentioned. This is to say nothing of the topographic impacts on the hydrology impacting soil moisture, however I hope my point is clear.

Another concern I have is with respect to the meteorological downscaling. In fact, I do not think it was done? As far as I could tell, there is no indication as to what geopotential or atmospheric height the forcing data were extracted from the WRF simulation. If a 'surface' prognostic variable was taken this can be problematic as, depending on the coarseness of the NWP resolution, the NWP terrain might be below the higher-resolution CLM topography. Thus, there may be cold biases, longwave biases, et cetera that are not considered (e.g., Bonekamp, et al, 2018). Further, this is a major complication with how windflow is treated. As noted above, high winds on crests (not even considering blowing snow) can lead to large sublimation losses, and differences in these should be visible between a 50 m to 250 m terrain model especially when forced with a 1km and a 9 km WRF simulation. However, the results and description lead me to believe that winds and meteorological forcing were simply assigned from a Xkm WRF simulation to the CLM terrain. I struggle to reconcile a decision not to downscale any meteorological forcing with running a 30 m hydrological model in alpine terrain.

A key detail and piece of analysis that is not mentioned is if the same amount of mass is input from the 1 km WRF simulations versus the 9 km simulations. Specifically, there is no reason to expect that cumulative seasonal precipitation is the same between the 1 km and 9 km WRF simulations (e.g., Bonekamp, et al, 2018; Rasmussen, et al 2010). Thus, the difference in results between the baseline and the various spatial scale perturbations is going to be a combination of small-scale process representation and dif-

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ferences in mass input. I realize scope with these types of numerical experiments can quickly get out of hand with “what if!” type questions. However, I am disappointed there is no comparison of the downscaled (however it was done) WRF forcing data with observations. I think this would have helped gauge if a 9 km WRF simulation was even close to providing anything but large-scale synoptic conditions or that it may be totally unsuitable for any type of hydrological modelling. Does it provide a precipitation mass estimate that is remotely close to correct?

Lastly, I think a major short coming of the manuscript is a lack of statistical analysis of the errors. The authors postulate a north-south distribution of errors, however there is no rigour in this statement to truly quantify this. Nor the elevation dependence on RMSE. The author’s state “requires accurate and timely knowledge of runoff generated by snowmelt for water management” however no attention is given to melt timing, the impact of scale on this, nor if the melt timing produced by any of the combinations is close to observed melt timing.

As a note on the distributed RMSE, I wonder if the authors considered other ways of expressing the error metrics? The RMSE plots (Figure 8,9) have the WRF resolution artifact which is rather distracting. Distributions of RMSE or perhaps something like the Wasserstein metric may help the multi-scale analysis.

Regrettably, in the current form, I cannot recommend this manuscript for publication.

Specific points follow:

Figure 1 needs a legend

L1 I found the abstract quite long and should be tightened up

L5 “many issues” – a bunch of effort is spent making it seem liked distributed models are highly uncertain (which is absolutely a fair concern) and yet they are used herein. I think this should be tightened up, especially in the abstract

L13 “discrepancies” word choice

L33 “those” = what?

L34 “prove performance” debateable, lots of simple models completely fail in cold region, alpine terrain. Either way, citation needed.

L35 “watershed processes. . .properties” such as?

L38 “Because these models require. . .impossible to validate” I think I understand what you’re saying here but could be tightened up. By construction, any model is impossible to validate in an ungaged basin.

L39 “Inc, 2015” confirm Inc is right

L39 & L43 Are you using integrated and physically based synonymously?

L48 “often not representative” citation needed

L49 “radar-retrieved precipitation” can also note low elevation gauge bias and/or total lack of gauging in complex terrain.

L51 “these models” which models?

L54 “spatial scales” spatial and temporal?

L67 “fewer of these studies” which studies?

L75 “To our knowledge only one study. . .” I would suggest that is not correct. A few examples: Rasmussen, et al 2010; Ikeda, et al. 2010; Bonekamp, et al, 2018; Vionnet, et al. 2020

L93: “more integrated way” what does this mean? Process coupling? Process representation?

L98: Discussion should be lower-case d

L106: “facing” word choice

L125 How fine of a scale is ‘fine’ referring to?

L125 'Within this' what is this referring to?

L127 , missing after e.g.

L139 remove "also" after ParFlow

L143 Extra ( ) around Kollet citation

L144,145 double space after the e.g.,

L159 "forcing data" note what variables are being used

L161 "synthesizing" what does this mean in this context?

L165 I'm a bit surprised this was the only variable calibrated

L174 Personally I am not a fan of 'Nature run' or 'Truth'. Without any in situ observations we don't know what the 'truth' is (which is already a loaded term with models), and 'Nature run' makes me think 'naturalized flow'. I think 'reference' or 'baseline' would be more appropriate.

L188 "hydrological <state> variables"

L190 This entire section is missing variable units!

L212 How is net radiation calculated i.e., albedo parameterization? Key for how it interacts with your scale perturbations

L218 What z0 are you assuming for your turbulent heat fluxes? What stability parametrizations are being used??

L258 What does 'dampening the domain' mean in this context?

L260 "Afterwards", after what?

L270 "short-term simulation" what is the length of this?

L275-281 Reads like Results?

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L317 double space in front of 11

L368 “Decreases in terrain resolution may lead. . .” potentially, but it can also lead to increased uncertainty due increased number of parameters and process scale considerations. In cold regions smaller scale tends to result in better process representation due to capture small scale heterogeneities in mass and energy, however the rainfall-runoff literature tends to find increased uncertainty! This is a very nuanced point with a lot of literature supporting multiple sides that, in my opinion, deserves more than this one liner.

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