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Interactive comment on "Technical challenges and solutions in representing lakes when using WRF in downscaling applications" *by* M. S. Mallard et al.

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The authors appreciate the constructive feedback provided by Reviewer #1. We address their comments point-by-point below. The reviewer's original comments appear in quotations and our responses follow. All page numbers referring to the manuscript in our responses below are valid for the revised manuscript, which is provided as a supplement with track-changes used to show revisions.

"This paper contains a good discussion of various approaches to simulating lakes within a regional climate model and the results of these approaches. It has strong value as a review paper, in addition to having a modest amount of original results. My comments are minor in nature, but I encourage the authors to heed them."

Specific comments:

1. “P. 7123 uses ‘interpolation’ on line 9, but then contradicts on line 17 by saying ‘no interpolation’. Clarify by possibly using other terms, like ‘spatial analysis.”’

The first use of “interpolation” in the sentence has been deleted (p. 2, line 55). The meaning of the sentence is clear without it.

2. “P. 7123, lines 9-15–This seems to be trying to compromise between accuracy and simplicity. I suggest just describing the situation that prevails in areas of interest, and the method that ends up being used there.”

We agree that the paragraph is detailed in its description of some interpolation methods with only a somewhat vague explanation of the others, which is what we believe the reviewer is describing as a “compromise.” The paragraph has been amended (p. 2, line 58-60) to direct interested readers to where a full description of all interpolation methods is available. A detailed description of all interpolation methods WPS utilizes is outside the scope of our study, as most are not relevant in our area of interest. However, we feel it is important to note that various methods are attempted using several points from the driving fields or just one point, and it is only after a series of methods have been attempted that the search method is employed. Otherwise, a reader could infer that the search method is often used, which may not be the case when the WRF Preprocessing System is used for other applications.

3. “P. 7124, line 25–It seems strange to refer to a reanalysis dataset as a proxy for a GCM. It’s just a dataset that can be used to drive an RCM for different purposes than would be achieved using GCM data.”

The sentence has been reworded (p. 4, line 102) to clarify why we refer to this dataset as a “proxy.” It is clearly stated that the R2 is a reanalysis dataset. The term “proxy for a GCM” is applied because it describes the purpose of driving with a coarse GCM which, as you mentioned above, is the historical evaluation of a downscaling methodology.

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Reminding readers of why the R2 is used is important, because the problems we focus on with the initialization of lakes could easily be solved by driving our runs with a higher resolution analysis. However, this would not serve the purpose of the retrospective downscaled runs, which is to establish a credible methodology for downscaling GCMs.

4. "This citation likely belongs in this page: Lofgren, B. M., 2004: A model for the simulation of climate and hydrology in the Great Lakes basin. J. Geophys. Res., 109, doi:10.1029/2004JD004602"

We are unsure for which page the reviewer is suggesting this citation. While Lofgren (2004) does discuss an approach applied within a RAMS-based model which is similar to WRF's "search" methodology, the present work is specifically focused on the representation of lakes within WRF, and it is beyond the scope of our study to describe how lake surface variables are set in RAMS or any other RCM. We do briefly discuss general problems in NARCCAP RCMs on p. 5 (bottom paragraph), but only to bring to the attention of the reader that WRF is not the only RCM that tends to represent lakes poorly in a downscaling application.

5. "P. 7129, line 24 implies that there is a correction that was applied in version 3.5.1. What is it?"

The table suggested below by the reviewer has been added to the manuscript and a reference to it is inserted to further clarify this sentence (p. 8, line 240). As stated in the introduction (p. 4, bottom paragraph), the default water surface temperature at which WRF determines ice is formed was lowered from 271 K to 100 K as of version 3.5.1.

6. "There are a whole lot of references to features of different versions of WRF spread across the manuscript. I suggest summarizing the relevant updates in a table."

A new table (Table 1 in the manuscript) has been inserted into the manuscript and several references to the table have been added to the text in order to aid the reader to noting the changes between versions more easily.

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7. “P. 7131, lines 7-14 overstate the uniqueness of ice as a limiting factor in evaporation. An ice-free lake can have cool water overlaid by warm and moist air, resulting in very limited evaporation.”

The sentence has been revised to say that its stated conclusions about the link between ice and precipitation are valid during the lake unstable season (p. 10, line 282) which is characterized by warm lake temperatures and relatively cool air masses. The role of temperatures, as well as ice, in suppressing evaporation is discussed more generally in Section 2.0 (p. 6-7). The studies being reviewed in this paragraph (Burnett et al., 2003; Kunkel et al., 2009; and Gula and Peltier, 2012) discuss changes in ice cover and its effects on precipitation and evaporation during the lake unstable season.

8. “P. 7134, last paragraph discusses how well FLake performs during historical periods. Are there any thoughts on how well it might do at climate change scenarios in which lake thermal structure and stability may well change, e.g. the situation illustrated by Austin and Colman (2007)?”

FLake’s ability to simulate the thermal structure of the lake can be limited by its inability to simulate 2- and 3-D processes and its assumption that the temperature profile consists of a homogeneous mixed layer and stable layer (thermocline) which extends to lake bottom. However, the shape (and therefore, stability) of its thermal profile can vary due to a variety of physical processes, including convective mixing and radiative heating of the water column. We expect that its performance over historical periods is indicative of its accuracy under a climate change scenario. However, it does not presently account for changes in lake depths which can be anticipated to change over the coming decades and this could negatively affect its performance in making future projections at multi-decadal time scales. This limitation is mentioned on page 15 (lines 443-446) of the present manuscript.

9. “What is the horizontal structure of both FLake and CLM? Are you using a horizontal array of non-interacting 1-d columns? The caption of Fig. 4 seems to imply yes for

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CLM, but unless I missed it, this should be stated more explicitly."

The reviewer's understanding is correct. In the case of both models, there is no horizontal interaction between columns. When the CLM lake model is first introduced we state that it is one-dimensional (p. 11, lines 324-325). The text describing the FLake model has been modified to state explicitly that it is a 1-D column model (p.12, line 350).

10. "P. 7135, lines 10-15–This is probably the most troubling part of this manuscript for me. The time series in Fig. 3 is showing strong evidence of numerical instability–highly unrealistic oscillation between high and low values over very short periods of time. This shouldn't happen even during a spin-up period. Without knowing the details of the code, it seems miraculous, first, that this instability continues for so long without crashing the model run, and second, that it suddenly stops and remains stable thereafter."

We agree that the results are highly unrealistic, which is why we choose to highlight the spin-up issues with these models so that users would be aware of the potential ill effects of running lake models without accounting for needed spin-up time. In the case of FLake, it should be noted that such values only occur during the first annual cycle of the ten cycles which are used to complete spin-up and these early results are sensitive to the model's initial state. The offline FLake run is initialized with surface temperatures uniformly set with a user-defined default (274.15 K) and a mixed layer depth which defaults to zero. As noted in the text, initial LSTs in the CLM simulation shown in Fig. 4 are taken from the interpolation from the R2, which also provides a poor initial lake state. We would expect that these initial conditions would negatively impact both models' short-term results, which is why we emphasize the need for adequate spin-up time to allow the simulated lake state to achieve equilibrium with the driving conditions and to become insensitive to its initial state.

To put these results in further context, the caption of Fig. 3 has been amended to in-

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clude more information about the initialized lake state for the offline FLake simulation and to note that the time series also shows ice temperatures as well. Some drops in surface temperature are expected as ice forms and ice-top temperatures cool. However, this abrupt cooling below 200 K is, as we note in the text, highly unrealistic. Since providing adequate spin-up time resulted in realistic values in our FLake simulations, we have not looked further into this aspect of the model's behavior. Being less experienced with the CLM lake model, we cannot comment on the potential causes of its behavior.

11. "P. 7135, lines 16-17–To reiterate and clarify the comment by Anne Clites, the dataset described in Wang et al. (2012b) is not simply the NIC analysis, but is a value-added dataset, with additional quality checks and a gridded format."

The text (p. 14, lines 399-401) has been modified to properly cite Wang et al. (2012b) without misattributing the work to the NIC. In addition, we have added a sentence to the Acknowledgements to state that this data was obtained from the Great Lakes Environmental Research Laboratory.

Technical corrections:

1. "P. 7123, line 1–insert 'spatial' before 'interpolation.'"

The suggested revision has been made (p. 2, line 47).

2. "P. 7128, line 26–'result from a downscaled simulation' seems clearer than 'result in...'"

This correction has been made (p. 7, line 214).

3. "P. 7130, line 28–Especially because winter itself is a limited time period, it seems to make sense to replace 'at a later time period' with simply 'later.'"

The wording has been changed, as suggested (p. 9, line 272).

4. "P. 7131, line 1–'...open (and free of ice)' is redundant. I suggest just 'free of ice.'"

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This revision has been made (p. 9, line 273).

5. “Check a style guide on hyphen usage: Remove hyphens from p. 7135 line 5 ‘spin up’, p. 7135 line 12 ‘time series’, and p. 7136 line 11 ‘spun up’. Add one to p. 7136 line 3 ‘110-year’.”

Most of the suggested revisions have been made (p. 13, line 395; p. 14, line 416). The use of “spin-up” was vetted by a technical editor when manuscript proofs were made.

Please also note the supplement to this comment:
http://www.geosci-model-dev-discuss.net/7/C3029/2015/gmdd-7-C3029-2015-supplement.pdf

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