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Dear Sir,

On behalf of my co-authors, it is with pleasure that I am submitting a revised version of the manuscript entitled "A 10km North American Precipitation and Land Surface Reanalysis Based on the GEM Atmospheric Model". This version has been significantly improved from the preprint version by taking into account all of the comments and suggestions made by two anonymous reviewers. In addition to changes made in response to these comments and suggestions, we have made minor typographic corrections, but also wish to introduce two more substantial changes to the manuscript.

Firstly, we would like to add Pr. Juliane Mai as a co-author on the manuscript. Pr. Mai was responsible for integrating the datasets RDRS-15 and RDRS-10 into the CaSPAr database and the operational dissemination of data to users. She also gathered feedback from many users, which provided useful information for organizing the manuscript and selecting what verification material to include in the paper. After a colleague of Pr. Mai pointed this out to me, all of the co-authors agreed that she should be included as a co-author.

Secondly, our thinking has evolved as to how to best deal with the bug identified in the parameterization of the snow density. In the preprint version of the paper, we affirmed the following:

> As explained previously, a bug was identified during the production of the 2000-2017 RDRS-10 reanalysis dataset. This bug impacts all operational configurations of the GEM model, as well as the RDRS-15 reanalysis dataset. It has since been corrected, and the production of the reanalysis for the years 1980-1999 will include the bugfix. This will cause a sudden shift in the bias and skill of the snow depth, density and water equivalent in January 2000. Based on the assessment of the impact of this bug over a one year period, modest changes are expected for snow depth, but snow density and water equivalent will be more impacted. For this reason, the snow density and water equivalent fields will not be distributed for years 2000-2017.

Instead of following this approach, we believe that it was best to re-process years 2000-2017 with the bugfix. The RDRS-10 dataset described in the paper (which includes the bug and is available for the period 2000-2017) will continue to be distributed as version 2 of the reanalysis (version 1 corresponds to RDRS-15 dataset, and will also continue to be distributed). Hence, version 2.1 will include the bugfix and will cover the whole period 1980-2018, not just 1980-1999. Using this approach, it will be possible to make available for download the snow density and snow water equivalent fields from version 2.1 over the whole 1980-2018 period. We thus propose to replace the above paragraph by the following:

> As explained previously, a bug was identified during the production of the 2000-2017 RDRS-10 reanalysis dataset. This bug is related to the maximum value of snow density and impacts all operational configurations of the GEM model, as well as the RDRS-15 reanalysis dataset.

> It has since been corrected, and the production of the reanalysis for the years 1980-2018 will be re-launched to include the bugfix. Version control will be used to distinguish between the different configurations of RDRS. RDRS-15, covering years 2010-2014, will be known as version 1, the RDRS-10 dataset presented in this paper, covering years 2000-2017, will be distributed as version 2.0. The RDRS-10 dataset with the bugfix will be distributed as version 2.1 and will cover years 1980-2018.

The following text was also added to the "data availability" section to provide more details on how to distinguish each product on the CaSPAr website:

> The RDRS-15 and RDRS-10 products are called "RDRS" and "RDRS_v2" respectively. Once its production is complete, the version of RDRS-10 covering years 1980-2018 and containing the bugfix for maximum snow density will be available under the name "RDRS_v2.1". More details on how to retrieve data from CaSPAr can be found at https://github.com/julemai/CaSPAr/wiki/How-to-get-started-and-download-your-first-data. The list of variables available can be found under https://github.com/julemai/CaSPAr/wiki/Available-products#list-of-available-variables-in-rdrs-v2.

A point-by-point reply to the comments from the reviewer follows. A version of the paper showing all the changes is included with the submission. It was created using the *latexdiff* PERL script, which unfortunately does not handle the references correctly. Please note that a number of missing references were added to the paper, as well as additional references suggested by the reviewers.

Kind regards,

Vincent Fortin

Reply to reviewer 1

General comments:

My major comment is about the choice of the final setup. It was decided to produce the entire period with RDRS-10 (the Regional Deterministic Reforecast System with a horizontal resolution of 10km). However, throughout the paper no explanation is given what this choice is based on. In contrast, at various places it is noticed that RDRS-15 performs actually better than RDRS-10. A clarification for the decision seems missing and the paper would benefit from the additional information.

As indicated in lines 123-125 of the manuscript, "a resolution of 10 km was chosen for the production of a 1980-2018 reanalysis, in order to match the current resolution of the Regional Deterministic Prediction System (RDPS) and of the Regional Ensemble Prediction System (REPS) currently in operation at CCMEP for short-term weather forecasting over North-America". Having the same resolution for the three systems (RDRS, RDPS and REPS) facilitates the computation of anomalies (by comparing RDPS or REPS to the RDRS climatology), and simplifies its application by end-users who make regular use of RDPS or REPS.

The long-term plan for the RDRS is to stay as much as possible in sync with the RDPS and REPS in terms of GEM model configuration and resolution, and re-launch the reanalysis whenever major changes are made to RDPS or REPS. Finally, although some degradations are seen when increasing the resolution from 15 km to 10 km, it is shown in the paper that these differences are small. In particular, the gains obtained in terms of precipitation skill through the optimal interpolation of precipitation observations largely compensate for this small degradation (see Figure 9).

During the early stage of the project, the originally targeted resolution and configuration of the system was 10 km for the very same reason as noted above. However, technical and computational resources reasons prevented from producing this preliminary 5 years sample (2010-2014) at a 10 km resolution (which was mostly dedicated to produce a proof of concept.)

We agree that this should be stated more clearly. The following text has been added in a new sub-section of the discussion:

> Despite the fact that not all variables were improved when resolution of the reanalysis was increased from 15 km to 10 km, a resolution of 10 km was nonetheless chosen for the production of the 1980-2018 reanalysis, in order to match the resolution of NWP systems currently in operation at CCMEP for short-term weather forecasting over North-America (RDPS and REPS). Having the same resolution for the three systems (RDRS, RDPS and REPS) facilitates the computation of anomalies (by comparing RDPS or REPS to the RDRS climatology), and simplifies its application by end-users who make regular use of RDPS or REPS. The long-term plan for the RDRS is to closely follow the RDPS in terms of GEM model configuration and resolution, and re-launch the reanalysis whenever major changes are made to RDPS. It should be emphasized that the degradations seen when increasing the resolution from 15 km to 10 km are small. Furthermore, the gains obtained in terms of precipitation skill through the optimal interpolation of precipitation observations largely compensate for this small degradation (see Fig. 9).

Specific comments:

Lines 33-37: The need for a higher spatial resolution is discussed. I suggest to add the resolution of discussed datasets, e.g. ERA5 and NARR, as well as the required resolution for land-surface and hydrological modelling applications.

The horizontal resolution of ERA5 and NARR (approximately 30 km) is now mentioned in the paper. References to Lobligeois et al. (2014) as well as Shrestha et al. (2002, 2006) have also been added to support the statement that higher resolution forcing data is required for hydrological prediction than what current reanalyses can provide. However, we suggest to also acknowledge the encouraging results reported by Tarek et al. (2020) using ERA5. To that end, the following text was added to the paper:

> For example, in regions of France showing high precipitation variability, Lobligeois et al. (2014) note a significant improvement in streamflow simulations when gridded rainfall is provided to a hydrological model at a resolution of 8 km or better. In studies conducted in China, Shreshtha et al. (2002, 2006) report better hydrological simulation results when the number of grid cells per watershed is at least 10, meaning that 30 km resolution would only be appropriate for watersheds of size 1000 km^2 or larger. Nevertheless, Tarek et al. (2020) report satisfactory results over North American when using ERA5 precipitation and temperature to drive a lumped hydrological model, even for watersheds of less than 1000 km^2 in size (although the skill does increase with watershed size).

Line 45: The surface reanalysis product MESCAN-SURFEX might be discussed in this section as well. For instance:

https://doi.org/10.3402/tellusa.v68.29879

http://www.uerra.eu/publications/deliverable-reports.html (deliverable 2.8)

The following two sentences were added at the end of the paragraph:

> Going one step further, optimal interpolation was used by Soci et al. (2016) to create reanalyses of temperature, humidity and precipitation over Europe at high spatial resolution (5.5 km) and over a short historical period (2007-2010) by combining short-term forecasts of an operational NWP model with in-situ observations using the Mescan analysis system. This approach is interesting

> but difficult to apply over a long historical period, since the outputs from the same NWP model would not be available.

Line 171: I am not familiar with the Yin-Yang grid. A reference would help the uninformed.

A reference to Qaddouri and Lee (2011) has been added.

Line 198: "whole period" is not correct. Actually, ERA-interim is available 1 January 1979 – 31 August 2019.

We replaced "whole" by "most of" to reflect the fact that the GDRS and RDRS products currently stop at the end of 2018.

Line 247: When is the first guess provided by GDRS? Figures 1 and 3 indicate that the first guess is based on RDRS only.

This is a typo. Although we did run CaPA with a first guess provided by GDRS as part of the project, we chose not to present these results in the paper, nor to distribute the outputs. The end of the sentence has been changed from "by either the GDRS or the RDRS" to "by RDRS".

Line 531: Here is one section showing that RDRS-15 compares better to observations than RDRS-10. However, at the end it was decided to produce the final dataset with RDRS-10. The reasons remain unclear.

Please see the response in the general comments section.

Lines 571-572: The given hours are hard to understand. I suggest to be explicit here:

"... are used for hours 6, 9, 12 and 15 UTC (resp. 18, 21, 0 and 3 UTC), with results shown..."

Thank you for the comment. We adjusted the text as suggested.

Line 582: Please rephrase the sentence.

"..., but the RDRS-10 bias value is always higher than that of the RDPS, which ...". The reader might understand RDRS-10 results are worse but that is not the case as explained afterwards. However, rephrasing this part would ease the readability.

The sentence was rephrased as follow: "but the RDRS-10 bias curve is always above the bias curve of the RDPS".

Line 704: How can the reanalysis product, which covers only past periods, be useful for hydrological prediction?

The following text was added to better explain this sentence:

> This analysis of simulated streamflow provides some evidence that the forcing data based on the RDRS can be useful for hydrological prediction, and in particular for transboundary watersheds of North America. This is true even if the reanalysis is only available for a historical period. Indeed, models relying on NWP outputs typically require to be calibrated in order to perform

optimally, partly due to the errors/biases and shortcomings of their input datasets. This is particularly true for surface and hydrological models. Such a calibration can only be performed based on historical datasets that ideally have the same climatology as the product used to drive such models in a forecasting context. However, archived datasets from operational NWP models tend to evolve in time and are usually not available for time periods long enough for such a calibration exercise. Another important point worth mentioning is that hydrological models are not only used to predict future flows. For example, they can be used to predict past flows at ungauged locations, to infill missing data at gauged locations, and to perform what-if scenarios to assess the impact of climate change, land-use changes and reservoir regulation changes.

Figure 2: The abbreviations used in the legend should be the same as in the remaining manuscript.

GEM Global should be changed to GDRS

GEM regional to RDRS

Thank you for the remark. The legend aims to provide additional information to what is already written in the figure. Indeed, GDRS is based on the global configuration of the GEM model, the RDRS is based on the regional configuration of the GEM model, and RSAS is based on CaPA and CaLDAS. This is now emphasized further in the text.

Technical corrections:

Figure 7, caption, last sentence: "Results are only based on ..."

Thank you. The correction has been made.

Reply to reviewer 2

General Comments:

Overall this is a good paper which should be published. The authors develop and document an approach to produce reanalyses related to the Canadian operation NWP prediction system with an aim towards water-driven applications. The approach developed and described herein is complex and involved and sometimes leaves one with a feeling of "chewing gum and baling wire (in that a sequence of inline and offline tools are strung together is several different ways, bootstrapping down-scales, to reach the objective)." This remark is not intended as a criticism or in a negative sense: another way to state this is that this is an inventive application of available tools, that leads to some useful insights and establishes a workable system to produce reanalyses over North America.

I found the organization of the paper to be reasonable and the writing was mostly clear. Likewise, I do not have any major technical criticisms of the paper either, and found myself more or less resonating philosophically with the authors decisions, and agreeing with their logic given their objectives and circumstances. A few suggestions to improve the paper in places are offered below.

Specific Comments:

On ∼line 230 + ... It is slightly disappointing, if understandable, why the authors did not employ radar data in this work. I hope that future work will allow the authors to explore the use radar data to improve precipitation analyses and assimilation. Likewise, perhaps future developments will allow them to simplify and streamline the overall process.

The radar data would certainly be beneficial for improving the quality and skill of precipitation analysis. They could be easily added to the offline 24 h analysis, albeit only for recent years thus affecting the time consistency of the precipitation analysis. Adding this supplementary input to the online analysis would however be much more time consuming (mostly due to technical reasons). Another possibility, even more attractive and easier to implement is to include IMERG satellite data into the final offline 24 h analysis. We are currently exploring this and it will probably be the next improvement in terms of precipitation observations assimilated. To this end, the following text was added to the "Future developments" section of the discussion:

> In particular, work has already started to improve the offline precipitation analysis by including additional in-situ observations not available at the time of production. It is also planned to take advantage of a recent innovation in the operational CaPA precipitation analysis, which now assimilates IMERG data (Integrated Multi-satellitE Retrievals for Global Precipitation Measurement, Huffman et al., 2020). This precipitation product based on remote sensing information has been shown to significantly improve the skill of the operational analysis in summer months, in particular in regions of North America not covered by ground radar (Boluwade et al., 2017). This is of particular interest since it would be technically challenging to include Canadian radar data prior to November 2014. IMERG products, on the other other hand, are readily available for assimilation in CaPA since June 2000 (Huffman et al., 2020)

I was also disappointed that cloud and radiation fields were not analyzed and evaluated herein (or at least they were not presented). These could have significant impacts on the water and energy budgets, and hence on the analysis and interpretation of the other hydro-meteorological fields and their application by others. This in fact may be the most serious technical deficiency in the paper though I do not think it should prevent publication.

While cloud and radiation related fields are definitely important for surface-atmosphere interactions, they were not evaluated and analysed here, due to clarity and brevity concerns and because the focus of the paper is to introduce the surface and precipitation reanalysis and evaluate its main aspects. Those fields should be analysed in detail in a subsequent study. To this end, the following text was added to the "Future developments" section of the discussion:

> Works also remains to be done in order to evaluate other forcing and state variables, such as cloud cover, incoming radiation and soil moisture. This is of particular importance given their impact on the water and energy budget. We expect to address this important issue in a future publication.

Lastly, in interpreting some of the biases in surface temperature and moisture in section 3, the authors may wish to consult a recent paper by Barlage et. al. in GRL (2020), which found both scale dependencies and a significant impact from coupling in ground water processes.

Thank you for the comment. Biases in surface temperature and humidity can come from many sources, including scale-dependent parameterizations and lack of critical physical processes in the land-surface model that is used. The current version of RDRS relies on a fairly old version of ISBA, which certainly lacks many important processes. A more in-depth understanding of the source of the biases at the surface is certainly needed in order to guide future development, but is out of scope for the present paper. ECCC is currently working on a more advanced land-surface scheme to replace ISBA, namely the SVS (Soil, Vegetation and Snow) land-surface scheme. In particular, SVS does represent sub-grid scale drainage processes and can be coupled to a simple unconfined aquifer model. The paper by Barlage et al. (2020) will be of interest in the context of the implementation and evaluation of SVS in the GEM model.

Technical Corrections:

To help readers better understand the author's work, the following suggestions are offered:

Line 278, incomplete sentence: "In order to produce a reanalysis of precipitation and land surface $< *** >$*, in addition..."* $< *** >$ *= a missing word: is it "land surface states" or "...fields" or "...parameters", etc.*

The sentence has been reworded as follow: “In order to produce a precipitation and land surface reanalysis”.

Line 313, “mosaicked from regional multi-sensor (radar+gauges) precipitation ***analyses*** *(MPEs) produced by the 12 River Forecast Centers” − > “analyses” should be “estimates”.*

This was a typo. It has been corrected.

Lines 442-443, in the sentence “...as well as observations ***across the atmosphere*** *than results from...”, it is not clear what is meant by “across the atmosphere”. Vertically? Geographically?.*

Vertically. The sentence has been corrected.

In Figure 9, the caption (at least) should define what the x-axis is in the figure (it is defined in the text, but help the reader out here...): “The x-axis is the precipitation threshold (in mm day-1).”

Thank you for the comment. The text of the caption has been corrected.

For Tables 4 & 5 (in the captions and where referenced in the text) present differences of RMSE, but the differences are never defined [is it ’X-Y’ or ’Y-X’?]. The reader can of course ’figure it out,’ but why not help the reader understand more quickly, and instead of saying (e.g.) “RDPS vs. RDRS-10”, say “RDPS - RDRS-10”.

Thank you for the comment. The change has been made.

Acronyms:

The authors should carefully review the use of acronyms, both to ensure that they are defined and/or are defined on first use. Some examples this reviewer found:

“ERA” (-5 and -Interim): first used on Lines 20/21; not properly defined.

Thank you for the remark. The definition has been added.

“ISBA”: used first on line ∼143 (not defined until later in document).

Thank you for the remark. The definition has been added when the acronym is first encountered.

“RSAS”: introduced in figure in section 2.1; defined later in section 2.2. “SYNOP, SWOB and METAR”: used on line 240; defined later in section 2.5.

Thank you for the remark. The list of network in section 2.2 has been removed, as it was a duplication of information found in 2.5.

Figure 2 has been moved later in the text after the definition of RSAS (it was not necessary to introduce it so early in the manuscript).

“COOP”: line 290, not defined.

Thank you for the remark. The acronym is now defined in the text.