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Discussions

Interactive comment on "Identifying robust bias adjustment methods for extreme precipitation in a pseudo-reality setting" *by* Torben Schmith et al.

Torben Schmith et al.

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Author reply to:

Referee comment #2 on "Identifying robust bias adjustment methods for extreme precipitation in a pseudo-reality setting" by Torben Schmith et al.

We will start by thanking the referee for a fair and thorough review. We will comment (marked with »> . . . «<) on each review items below.

General comments

In their contribution, Schmith et al. (2020) discuss the robustness of different bias-adjusting methods for (sub)daily rainfall extremes. This yields interesting results and

strong links with the context of convection-permitting models and emergent constraints. Yet, there are some aspects about whom I'd like a deeper discussion.

»> we appreciate this positive overall judgement of our manuscript and are positive towards adding more discussion to it.«<

The first aspect is the practical use of this study. This is foremost linked with the choice of bias-adjusting methods. Although the use of return periods is perfectly justified from a hydrological point of view, I've seen few studies that actually use bias adjustment directly on the return periods. As such, I'd like to see a larger discussion on the choice of bias-adjusting methods.

»> Our aim has been to evaluate basic adjustment methods, used in hydrological applications. The simple climate factor approach has been applied in numerous hydrological applications, such as in (Sunyer et al. 2015; DeGaetano and Castellano 2017) and others. We also wanted to test quantile-mapping approaches, which in extreme value theory takes the form of a parametric transfer function. This we have applied in two flavours in the spirit of (Räty et al. 2014). Finally, we wanted to benchmark against gainst the 'canonical' benchmark methods, (observations and raw model output). We will discuss this in a revised manuscript.«<

Given a well-justified choice, I understand the use of these simple methods, yet I'd like to see more discussion on how this relates with more complicated, but related bias-adjustment methods, such as e.g. CDF-t (Michelangeli et al., 2009), standard QM, QDM (Cannon et al., 2015), : : : Would it be possible to discuss possible consequences for the use of these methods for the adjustment of subdaily precipitation extremes? This could fit in the second paragraph of Section 5.1, which seems rather limited and abrupt at this point.

»> We will add some discussion of the more elaborate quantile mapping methods. Also discuss expected consequences for the adjustment, if possible. We will, however, emphasize that these methods build on alternative, but not necessarily more correct, assumptions. It would be interesting to test these methods in our framework, but we reserve this to future publications. In this connection, not that our investigation do not generally find that the more advanced methods (quantile mapping) outperform the simpler climate factor approach.«<

A last point related to the practical use is that I missed a more thorough explanation of why the observations perform well, why this version of quantile mapping performs poorly. Although this is discussedcslightly in Section 4.3, I wonder if more details or, if possible, practical guidelines could be given in the discussion.

»>A thorough reveal of causes for some models performing well would require quite some extra analysis which cannot be accommodated within this manuscript. We may speculate that the cause of observations performing so well as projection is related to the poor signal-to-noise ratio, as seen in Fig. 4. The relatively poor performance of the quantile-matching methods could be caused by the many extreme value distributions to be estimated, each of which are very uncertain.«<

A second aspect is that some concepts in the Introduction seem to be accepted as-is, whereas they could deserve a deeper discussion. A first example of this is the discussion of stationarity in the introduction. The references are limited in time, whereas more recent papers expanded this subject, such as Kerkhoff et al. (2014) and Van Schaeybroeck and Vannitsem (2016) on the type of bias relationship and Chen et al. (2015), Velázquez et al. (2015), Wang et al. (2018) and Hui et al. (2019), who discussed the uncertainty introduced by bias nonstationarity. As the stationarity of the bias is an important part of the discussion, I think the paper could benefit from these perspectives.

»> We were not aware of the above references, forwarding interesting aspect of stationarity. It is relevant to discuss this in the introduction, and we will do so in a modified manuscript.«<

A second, smaller example is the use of a delta change based method. While the

method isn't completely discredited, there has been some discussion whether it's use for climate change is not too dependent on the assumption that the temporal structure of the time series will not change from present to future (e.g. Johnson and Sharma (2011), Kerkhoff et al. (2014)). It would thus be interesting to read a deeper discussion on the limitations of the methods

»>We are aware of the assumption about unchanged temporal structure of time series in the delta change approach. Even if so, delta change methods were included in the studies (Räty et al. 2014; Räisänen and Räty 2013), and therefore we choose to include such methods as well. «<

Specific comments

L. 37: 'quantile-mapping' is used here, whereas in the remainder of the abstract (and the paper) 'quantile-matching' is used. I'd suggest to edit this for coherence, but to also use 'quantile mapping' throughout the paper, as it has been the most used term for this type of bias adjustment during the last few years.

»> Certainly, the nomenclature should be consistent throughout. We well take care of that, and follow your advice, adhering to the term 'quantile mapping' «<

L. 75-82: this paragraph is very scarce on references. Although some of the necessary references are given in the discussion, I think it would be good to also have the reference to the papers about CPMs in this paragraph.

»> Ok, we will do so.«<

L. 84-91: The terminology in this paragraph could be reconsidered. Although it is debatable whether or not to consider delta change as a bias adjustment approach (the latest textbook, Maraun and Widmann (2018), is on the edge), it feels very strange to read 'bias correction' as a subset of 'bias adjustment' approaches. The use of 'bias adjustment' as a replacement of 'bias correction' has been rising during the last few years, as it is clearer that the methods are statistical and cannot correct all climate model biases. Thus, I would withhold from the use of ‘bias correction’. Better terminology seems MOS, with delta change and bias adjustment as possible subcategories, or bias adjustment with delta change and bias adjustment s.s., although the exact choice is personal.

»> Yes, exactly this has also been on our minds! We think that your suggestion of using the generic term ‘MOS’ would be a choice to apply.«<

L. 253- 286: Although the method described here is indeed based on the same principles as XCDF-t as used by Kallache et al. (2011) and Laflamme et al. (2016), it’s not entirely clear how the new method is created by adapting the former. I think the link between both methods should be more detailed, so users can retrace it more easily and infer the strengths and limitations. Especially as it is specifically mentioned that the method ‘will be adapted to our needs below’, the adaptation seems rather limited.

»> We will make the connection with XCDF-t more clear in the revised manuscript.

L. 448-453: the explanation of the use of the index by Maurer et al. (2013) should be expanded. Firstly, it’s unclear to me where the terminology ‘measure of relative spread’ is derived from, as it is not named as such in the original paper. Secondly, the interpretation of the R-values is not discussed, although this is quite important: values < 1 indicate that the difference in biases is smaller than the mean bias of both periods, whereas values >1 indicate that the difference in biases is larger, which could have a potentially large impact. As both values are quite far < 1, the bias seems quite stationary, but in your discussion you state that the 24h duration is ‘less stationary’. Without giving this numerical explanation, this statement is hard to interpret correctly.

»> We will expand the explanation of R, and its interpretation, as suggested. Certainly, both R-values are below 1. However, R=0 is a sign of a stationary bias factor and this is the basis of our interpretation«<

L. 504-505: This last sentence does not seem to fit with the rest of the paragraph. I

think that, with some rewriting, this could become clearer.

»>We will rephrase this sentence.«<

Technical comments

»> we will adhere to the technical comments given below«<

L. 48: ‘Global climate models (GCMs) is : : :’ -> are

L. 110-111: ‘Only a few examples has : : :’ -> have

L. 112-113: ‘: : : applying bias adjustment improve projections’ -> improves

L. 142: the section marker should be corrected

L. 194: I can’t find the source of this problem, should not be referenced with co-authors. The official webpage by Springer (https://link.springer.com/book/10.1007%2F978-1-4471-3675-0#about) only mentions one author (Stuart Coles) and there is no mention of other authors elsewhere in the book. So unless I’m missing something, I think the more correct reference is Coles (2001).

L. 232-243: ‘Hosking and Wallis (1987) : : : warns : : : . Instead, he recommends : : :’. Shouldn’t these sentences be plural, or are you referring to ‘the paper’ in these sentences instead of ‘the authors’?

L. 254: ‘Kallache et al. (2011) and Laflamme et al. (2016) applies’ -> apply, as this verb is referring to multiple papers and authors.

L. 265: ‘ths’ -> ’the’

Figure 6 and Figure 8: Would it be possible to remove the underscores from the plot titles?

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