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Interactive comment on "The response of precipitation characteristics to global warming from global and regional climate projections" *by* Filippo Giorgi et al.

Anonymous Referee #1

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The manuscript by Giorgi et al analyze climate model simulations – mostly global, but also two regional models – describing and analyzing sundry characteristics of the hydrologic cycle and how they are projected to respond to warming. They include a short comparison with regional climate simulations, and then go on to discuss some consequences of the changes: a measure of hydroclimate stress, interannual variability, and potential predictability change.

While the response of precipitation to warming is certainly an important question within the scientific scope of the journal, the majority of this study presents concepts and ideas that are very much established in the existing literature, but through analysis that

is less robust or comprehensive than other studies, and meanwhile the study does not give credit or acknowledgement to many of these existing studies. As a result, the original contribution of this work is not clear. It is stated in the introduction that the intention is not to "provide a review of the extensive literature on this topic," and also in the conclusion that "some of the results . . . are consistent with previous analyses." But statements are made which contradict existing work, and analysis presented here falls short of the standards established by this other other work. In some cases, sections of the paper are undermined when considered in light of the existing, unacknowledged literature. Overall, it seems to me that the degree to which existing literature is neglected and to which the conclusions lack originality is problematic. The only part of the study that is both novel (to my knowledge) and convincing is section 3.2, on potential predictability, but this part of the study is more rough than others, and gets only a passing mention in the conclusions.

Specific comments:

Line (L) 57-58: It seems that using a threshold of 1 mm/d applied to climate models could be problematic. This threshold makes sense at a rain gauge, but it means something different averaged over a model grid cell – and what it means will vary with the resolution of the model (see e.g. Chen and Dai 2018).

L91: There is a substantial literature that explores robust responses of the hydrologic cycle, and restricting analysis to just 1 or 10 of the models is an outdated approach.

L121-57: The "rich get richer" aka "wet get wetter" paradigm has been shown not to hold over land by Byrne and O'Gorman (2015), which would point toward it being somewhat irrelevant for the consequences discussed in section 3.

L132-135: These descriptions of the changes in ITCZ and monsoons do not reflect the current state of understanding. See, for example, a review by Byrne et al., (2018) that includes changes in ITCZ strength, and Biasutti et al., (2018) on changing monsoon strength.

L170, 226-227, Other studies have examined the changing distribution of precipitation and include uncertainty estimates and also compare across models. Pendergrass and Hartmann (2014) examine over 20 models for RCP8.5 and CO2 increase scenarios, and show that the MPI family of models has a very different behavior in terms of its heavy precipitation response to warming, mostly in the tropics, compared to other climate models, which calls into question how robust the results shown here would be across models.

L228-238: Recent work by Thackeray et al., (2018) shows compensation between extreme and non-extreme events across CMIP5 RCP8.5 simulations from different models. This highlights the role of the energy budget in affecting the distribution of precipitation – but regional climate models don't capture these energetic feedbacks.

Figure 7: This figure is difficult to understand. Some attempt should be made to label what the boxes mean on the figure, visually, and/or axis labels should be included. The lack of labeling of the figure combined with the way the index labels are included is confusing. What is the purpose of the red arrows? Doesn't R95 relate to only some of the boxes (the bigger ones), rather than all of them, like SDII does? Doesn't HY-INT relate to all of the precipitation events, rather than just a few?

L296, L329-332, L339: Extreme precipitation responds not just to changes in moisture, but also to changes in circulation. In some models the increases in the most extreme precipitation are substantially larger than Clausius-Clapeyron, and in some regions they are much smaller. The regional variation in extremes is documented by Pfahl et al., (2017). The variation in the warming response to percentile definitions, including the range across models extending well above CC, is shown in Pendergrass and Hartmann (2014).

Figure 8 and 9: These maps seem to show the sum of the cumulative stress from wet and dry extremes that are shown in Fig 2 of GCR18, though this is not explicitly stated. In that figure of GCR18, wet extremes drive positive stress (ERSY), while dry

extremes sometimes drive positive stress and other times drive negative stress. In locations where dry extremes drive increasing stress, adding ESRY from wet and dry extremes results in cancellation, which is effectively an assumption that wet extremes will mitigate dry extremes. This cancellation does not seem justified; a short timescale heavy precipitation event could still cause flooding in situations where there has been a deficit of precipitation on a longer timescale, depending on the characteristics of the surface where it falls. It is worrisome that this section consists of an incremental and not-well-justified advance on the author's recent work.

L407, Figure 9 caption: The figure caption says that the text includes more detail on how population is incorporated, but the text only seems to say "population scenarios are also accounted for." Please describe how population is accounted for. Furthermore, GCR18 was not the first study to include population weighting of trends in the hydrologic cycle, but none of the previous work is acknowledged here or in GCR18 – one example is Sedláček and Knutti (2014).

L426-9: "the latest generation of GCM projections does not provide strong indications concerning changes in the frequency or intensity of such modes [ENSO and NAO are explicitly stated]." There is substantial literature documenting the effects of changes in ENSO, in particular, on interannual variability of precipitation. One example is Power et al., (2013).

L424-467: The effect of increasing variability in the CMIP5 ensemble on a variety of timescales including daily, interannual, and across multiple years, was documented by Pendergrass et al., (2017).

L521-543: As mentioned in the general comments, it seems that the only novel conclusion arrived at here may be the potential predictability. âĂÍ

L562-578: I certainly agree with the statement that conclusions based on coarse resolution models will have to be revisited and potentially modified as our ability to simulate precipitation advances. But as mentioned in my comment regarding lines 228-238, recent work by Thackeray et al., (2018) shows the key role that global energy constraints play in determining how the distribution of precipitation responds to warming – a factor that is not accounted for in regional climate models. As written, this paragraph can be interpreted as implying that high resolution regional climate models are a solution, whereas the findings of Thackeray et al., (2018) provide evidence to the contrary. This drawback of regional climate modeling should also be acknowledged.

Typos and minor comments

L260: MDSL is not defined.

L355: "stress associated wet" should be "stress associated with wet"

L575: "strognly" should be "strongly"

Additional references

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