

Interactive comment on “A framework for assessing flood frequency based on climate projection information” by D. A. Raff et al.

D. A. Raff et al.

draff@usbr.gov

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Response to Individual Comments: Original Reviewer comments indicated by number. Responses to comments immediately follow.

Individual Comments - Reviewer #1 (Sivapalan)

(1) The description of the downscaling from monthly climate projections down to 6-hourly rainfall sequences raises concerns. First of all, does this downscaling generate intermittent rainfall events? That is the true nature of rainfall intensities, i.e., intermittency. There was no comment on this aspect.

Authors can make minor edits to language in manuscript to clarify; intermittency is re-

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tained. Note in the original manuscript on page 2016, line 27 denotes that “The general approach was to scale a monthly set of observed daily values . . .” This more accurately should say to scale a monthly set of observed six-hourly values. This paragraph goes on to describe this process in detail with an example onto page 2017. Therefore to alleviate some of reviewer Sivapalan’s concerns any intermittency that has been observed in the data set is maintained through the temporal downscaling methodology.

(2) Also, how do we know this disaggregation is done right? The authors could have done this using past records, and demonstrated the disaggregation worked well. Was this done? If so, can they demonstrate that this was satisfactory? This is important since floods are normally caused by rainfall events, although in these examples

The authors are assuming that this is not a question of whether the actual mathematical procedure was executed as described on pages 2016 and 2017 but rather whether the procedure itself is adequate. To that end the authors point the reviewer to the methods description on pages 2017 and 2018, and the results section 3.1 (weather generation for evaluating flood potential) on pages 2021 and 2022. The methods were not developed for this study but adopted from earlier work (Wood et al. 2002). The technique has been used in other hydrologic impacts studies under climate change where monthly climate projections were temporally disaggregated to develop sub-monthly weather forcings for hydrologic analysis (e.g., Payne et al. 2004, Christensen and Lettenmaier 2007, Maurer 2007). The actual ability of the temporal disaggregation (i.e. weather generation) procedure is best evaluated by its ability to reproduce the calibration data set empirical distribution function. This was done by the authors and described on the pages identified above and presented graphically in Figure 4. We point the reviewer to evaluate Figure 4 in which the disaggregation scheme ultimately produces distribution functions that in the authors’ opinion do a more than adequate job of encompassing the calibration set.

(3) I found the description of retrospective and lookahead flood frequency analysis very confusing. It is possible that these procedures reflect what is routinely done in USBR,

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but to most others these are potentially confusing. The authors have a responsibility to explain these well. I think the paper requires revision to make these procedures more clear to readers.

Reviewer Sivapalan's confusion is noted with respect to the retrospective and lookahead flood frequency analysis. The two methodologies are presented beginning on page 2019 in the methods section. In that section it can be identified that "The expanding retrospective is the current paradigm for flood frequency . . ." In contrast the lookahead flood frequency approach is not the current paradigm and is presented by this manuscript as an alternative approach. The authors feel that with minor revision to the language in the manuscript we can make this section more clear to readers. This feeling is further supported by reviewer Flores' comment that " In comparing what the authors term an "expanding retrospective" to a "lookahead period" approach the authors do a good job of illustrating the dangers of estimating flood quantiles using data with climate regimes that are known to be non stationary."

(4) Because I did not understand the methods used, I have major fundamental concerns about the results of their flood frequency analysis. [concerns about using flood frequency in a non-stationary system] . . . I do not understand how they can continue to use the notion of the return period. . .

Reviewer Sivapalan questions the continued usage of the return period given that the system is identified as non-stationary. The authors agree that the concept of the return period is drawn into question in a non-stationary system, or at a minimum needs to be defined specifically. The authors present their definition on page 2007 in which the return period simply represents the probability of a given event in any given year. In a non-stationary climate the 100-year flood or flood with a 0.01 (as originally presented, the authors actually prefer the definition put forth by reviewer Flores and would like to incorporate this offered definition during the revision process) can actually change in magnitude as the system moves as well. Therefore the authors do not feel that, given this definition, the concept of the return period is in conflict with the non-stationarity

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concepts presented in our manuscript. We further present, if this previous explanation does not satisfy the reviewer, that we are not out to change the paradigm and semantics of how flood hazards are communicated. This is not the point of the manuscript. The semantics of what a return period flood is can be discussed in a different forum, one that has influence over the terminology utilized by practitioners.

(5) Assuming (and hoping) that the computed flood frequency curves are for stationary climates only (i.e. different scenarios), then there is one final step of combining these and carry out flood risk assessment, perhaps assuming that each of these scenarios are equally likely, or using some other probabilistic assessment.

For the lookahead analysis the time period for characterization is assumed to be short enough for a stationarity assumption and long enough to get quality statistical parameters. As reviewer Sivapalan identifies it could be possible to assign probability to each scenario and arrive at some final risk number. This approach is in direct conflict with procedures used for climate projection selection as described in section 2.4 beginning on page 2015. The approach presented is attempting to identify the envelope of the risk by using 9 projections that span the gambit of warm-wet to cool-dry. Because the climate projections themselves do not come from any probability distribution it is very difficult to identify a supportable methodology to assign probabilities after the fact. During the revision process we could identify this conflict at this point in the manuscript, which would clarify this point. In that section it is identified that projections are selected to encompass the relative variability in the projections and to get an idea of the envelope of risk that may be apparent. The actual decision process that may be employed based off of the envelop of variability has yet to be identified in practice or in the literature and the authors feel that this can be the focus of another manuscript specifically geared towards that discussion.

Individual Comments - Reviewer #2 (Flores)

(1) . . . use of model climate outputs to drive the Sacramento Soil Moisture Accounting

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model. I see here the possibility that potentially important and presently unaccounted for model errors are being introduced in the authors analysis. This is because the precipitation and temperature data from which the forcings for the hydrology model are derived are themselves outputs from any one of several climate models. These climate models explicitly model physics at the land surface and are therefore associated with particular rainfall-runoff sequences. Moreover, the land surface parameterizations in these climate models are explicitly coupled to the atmosphere through latent heat flux that is soil moisture dependent. Correspondingly, the temperature and precipitation regimes that emerge from these climate models are associated with particular rainfall-runoff regimes that are a result of the particular representation of the physics in each climate model. The authors have treated the hydrological model used in this study as being independent of the parameterization schemes embedded within the climate models from which the hydrologic model forcings are derived and have not reported on any efforts to verify that the hydrologic model used for the flood frequency analysis yields runoff volumes that are consistent with the runoff volumes produced by the climate models.

For clarification, the climate models do not produce runoff sequences as part of their computations. They do, as reviewer Flores points out, have parameterized land surface schemes that are not carried over or accounted for in the hydrology model. We could during revision reiterate that our approach is first order in that we don't account for developing model physics and parameters. However, our approach is well supported in the literature. We could point to the body of literature that assess hydrologic impacts are assessed in offline hydrology models rather than hydrology modules embedded in climate models (e.g., Miller et al. 2003, Mauer 2007, Christensen and Lettenmaier 2007, Purkey et al. 2007)

If it is a question about whether the algorithm presented in the manuscript is able to reproduce the antecedent period and calibration period of the Sacramento model it is shown (Figure 4) that peak values are well reproduced. Reviewer Flores identifies that

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volumes may be an issue as well. Here please see Supplemental Figure 1 that show that, like the peaks, volumes are also well reproduced. This figure was not included in the initial manuscript over the concern with density of information and length of the document.

(2) Moreover, because the temperature and precipitation outputs used to derive the forcings for the hydrological model are obtained from a range of climate models that together span a broad range of parameterization schemes, it is likely that there is a correspondingly broad range of rainfall-runoff regimes that emerges from these climate models. The authors have used calibrated parameters for the hydrologic model, which remain fixed throughout the analysis. Setting aside for a moment the debate as to whether the changes in terrestrial vegetation associate with climate change would alter the rainfall-runoff characteristics in a way that would require adjustment of the model parameters, it seems that the variation between the physics of the climate models alone, and the corresponding variation in rainfall-runoff regimes these physics impart would necessitate the explicit treatment of uncertainty in the parameters and/or structure of the hydrological model. An explicit treatment of the uncertainty in the hydrologic model would, in principle, ensure that the rainfall-runoff regimes simulated by the hydrological model in response to forcings derived from a suite of climate models spans the range in rainfall-runoff regimes associated with those climate models. I see this as being a potentially important inconsistency in the authors' framework that would be particularly important in areas where the coupling between precipitation and soil moisture are thought to be strong. I suggest that the authors include a little more justification for their choice to neglect potential disparities in physics between the climate and hydrological models throughout the flood frequency analysis, and a discussion of the potential magnitude (if known) of discrepancies between the rainfall-runoff partitioning of the climate versus hydrological models.

The fact that the climate models have different land parameterization schemes than the hydrology model is a known potential limitation that the authors can identify through a

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minor revision. As our previous response indicates, we can acknowledge this limitation in methodology and cite the body of literature that reiterates its applicability. Further, we would like to reiterate that the climate models are operating at a spatial scale that is inconsistent with the generation of flood flows. We have relied upon the climate models for representations of temperature and precipitation. We then rely upon a spatial downscaling methodology (Wood et al. 2002) as summarized in section 2.3 that includes a bias-correction scheme to enforce statistical consistency between the climate models results and observations during a common historical overlap period. Wood et al. 2004 and Salathe et al. 2007 have shown this bias-correction and downscaling technique to be a reasonable approach to get temperature and precipitation at a spatial scale necessary to evaluate hydrologic impacts, including basin-scale flood events as we have analyzed in this study. Figure 4 within the original manuscript as well as Supplemental Figure 1 further supports the ability of the methods.

(3) The issue of the methodology through which the magnitude of floods with a given recurrence interval are estimated when the underlying process is nonstationary is of critical importance in assessing the potential impacts of climate change on infrastructure. I would just note here, that while the authors necessarily dealt with this issue in their work, there is by no means consensus in the community about how to approach flood magnitude estimation under the inherently nonstationary conditions that climate change presents. Given the lack of consensus in this area, I would argue that the authors be afforded latitude in their methodology. In comparing what the authors term an “expanding retrospective” to a “lookahead period” approach the authors do a good job of illustrating the dangers of estimating flood quantiles using data with climate regimes that are known to be non stationary. While the lookahead approach seems to be more appropriate for estimating the magnitude of extreme events by limiting the flood frequency analysis to periods with relatively consistent climatic behavior, the clear downside is the loss of additional data records with which to estimate flood magnitudes. Since the overall approach of the authors is to use the outputs of climate models, properly disaggregated in time (to force the hydrological model) and since the authors have

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explicitly treated the uncertainty in the disaggregation process, their work would seem to be well-posed as potential data assimilation or data fusion problem. In this case the predicted “observations” are the annual maximum series of each 30-year lookahead period (or characteristics of the annual maximum series). Clearly, in some applications, it would be helpful if uncertainties in the future flood frequency relationships of a basin are reduced as the advent of a particular lookahead period approaches. Are there surrogate data that the authors envision to be on the horizon that would better inform their predictions of these predicted annual maximum series? I would like to see the authors discuss how flood-risk projections estimated via through the lookahead tactic could be potentially constrained to observations in the future, and what those observations might be.

Reviewer Flores also notes that the problem of non-stationarity and flood risk estimation is well posed as a data assimilation problem and whether the authors have considered how to constrain projections as information becomes available. The authors do see this as a very beneficial addition to the work presented. The ability to constrain projections will be helpful to reduce uncertainty in the future and will consider how that might be accomplished in future work. That being said, however, this manuscript is intended as a characterization of how climate information may be incorporated into flood risk assessment currently. It can also be noted that there will be a set of next generation climate model outputs coming out as CMIP5 to support IPCC assessment report 5 in the coming years. This set of information may also reduce uncertainty in climate projections based flood hazard estimation.

General Comments:

Reviewer Flores also had a number of editorial suggestions. Without going through these specifically we feel that they would all be relatively easily accounted for through some very minor revisions. Reviewer Sivapalan identified a piece of literature that should be included in the introduction and we agree.

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References: Christensen, N.S., Lettenmaier, D.P.: A multimodel ensemble approach to assessment of climate change impacts on the hydrology and water resources of the Colorado River Basin, *Hydrol. Earth Syst. Sci.*, 11, 1417-1434, 2007. Maurer, E.P.: Uncertainty in hydrologic impacts of climate change in the Sierra Nevada, California under two emissions scenarios. *Climatic Change* 82: 309-325, 2007. Miller, N.L., Blashford, K., Strem, E.: Potential Climate Change Impacts on California Hydrology, *Journal of the American Water Resources Association*, 39, 771-784, 2003. Payne, J.R., Wood, A.W., Hamlet, A.F., Palmer, R.N., Lettenmaier, D.P.: Mitigating the effects of climate change on the water resources of the Columbia River Basin, 62, 233-256, 2004. Purkey, D.R., Huber-Lee, A., Yates, D.N., Hanemann, M., Herrod-Jones, S.: Integrating a climate change assessment tool into stakeholder-driven water management decision-making processes in California, *Water Resources Management*, 21, 315-329, 2007. Salathe, E.P., Mote, P.W., Wiley, M.W., Review of scenario selection and downscaling methods for the assessment of climate change impacts on hydrology in the United States Pacific Northwest, *International Journal of Climatology*, 27, 1611 - 1621, 2007. Wood, A.W., Maurer, E.P., Kumar, A., Lettenmaier, D.P.: Long Range Experimental Hydrologic Forecasting for the Eastern U.S. *J. Geophys. Res.* 107(D20), 4429, 2002. Wood A. W., Leung, L.R., Sridhar, V., Lettenmaier, D.P.: Hydrologic implications of dynamical and statistical approaches to downscaling climate model outputs, *Climatic Change* 62, 189-216, 2004.

Supplemental Figure 1: Performance of methodology to reproduce antecedent runoff performance. Boise River basin above Lucky Peak Dam 7-day annual maximum average flows (7-day volume is 7 x Flow Rate). Blue line represents empirical distribution function (ECDF) for the calibration set 1967 – 1997 for the SAC-SMA model. Grey lines represent ensemble of projections for the same 1967 – 1997 period.

Interactive comment on *Hydrol. Earth Syst. Sci. Discuss.*, 6, 2005, 2009.

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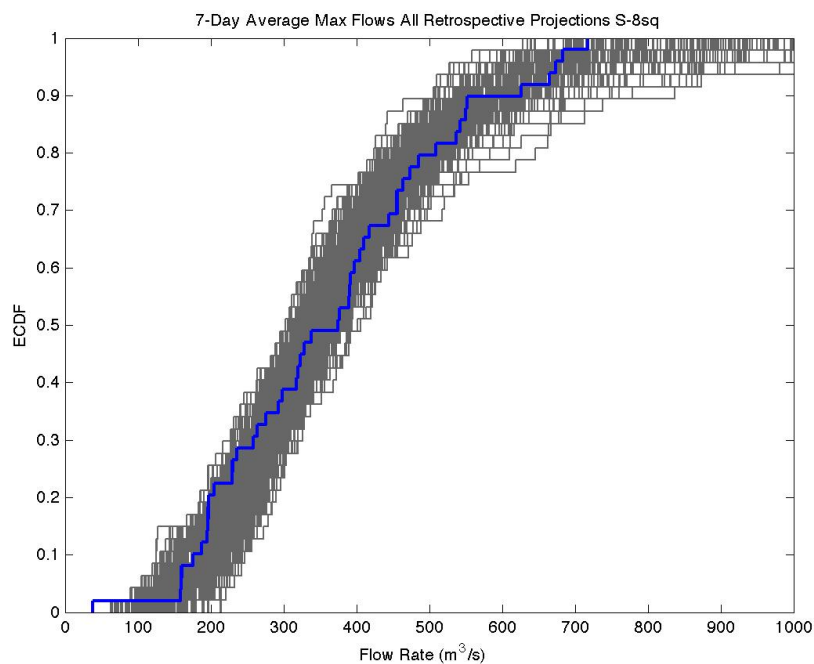


Fig. 1. Supplemental Figure 1: Full Caption in Text

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