Hydrol. Earth Syst. Sci. Discuss., 6, C1572–C1579, 2009
www.hydrol-earth-syst-sci-discuss.net/6/C1572/2009/

Hydrology and Earth System Sciences Discussions

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

A. Flores (Referee)

lejoflores@boisestate.edu

Received and published: 17 July 2009

1 General Comments

The authors of this work approach a very important and difficult problem in contemporary hydrologic science and engineering: flood-frequency estimation under changing climate. They detail an approach in which they: (1) stochastically disaggregate monthly precipitation and temperature projections from a subset of the Coupled Model Intercomparison Project phase 3 (CMIP3) multi-model dataset to 6 hour time steps, (2) use the resulting disaggregated precipitation and temperature time series as the hydrometeorological forcings of an operational flood-forecasting model to produce simulated

discharges at the daily time scale, and (3) perform a flood-frequency analysis of these simulated discharges. The paper also deals to some extent with the methodological underpinnings of flood frequency analysis when the underlying process is non-stationary. Generally speaking, the paper is well written and the organization of the paper does a good job of conveying some concepts and methods that are technically involved.

In terms of the scientific contribution, I believe this work to be a fairly novel attempt to develop methodologies aimed at assessing flood risk under conditions of changing climate. I am unaware of other works that have, as yet, addressed this issue in this way. The potential advantage of the authors' approach lies in the relatively inexpensive computational framework laid out by the authors. Their work largely consists of taking two of the most critical climatological variables associated with flood generation (temperature and precipitation) output from a suite of climate models, and re-interpreting them within the context of a relatively simple, yet widely used, operational hydrological model. The method developed by the authors is substantially less computationally expensive than using nested and dynamically coupled ocean-land-atmosphere models to provide the forcings to a hydrological model, or the even-more expensive alternative of using a process hydrology model capable of simulating channel flows as the land surface scheme in one of these models. While progress is being made in these more computationally intense approaches, it is my opinion that this work represents an important effort to bring flood risk assessment under changing climate more to the forefront of discussion in the hydrologic science community. I recommend, therefore, that this paper be published, subject to minor revisions.

2 Specific Comments

Despite the potentially important contribution toward developing ways to assess the impact of climate change on flood risk that this paper represents, the authors' approach is not without important shortcomings that I believe need to be discussed more thoroughly.

The most important issue I see that needs some additional discussion relates to the use of model climate outputs to drive the Sacramento Soil Moisture Accounting model. I see here the possibility that potentially important and presently unaccounted for model errors are being introduced into 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 responses. 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. 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 associated 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 (e.g., as outlined by Koster et al., [2004]). 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 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 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 nonstationary. 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 explicitly treated the uncertainty in the disaggregation process, their work would seem to be well-posed as a 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 that 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 periods 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.

3 Technical Corrections

Page 2006, Line 26: "This assumes that every year is an independent sample from all possible years and that all years are equally likely." While this interpretation of the 100-year flood is prevalent in the community and practice, I suggest also including a more Bayesian interpretation that there is a 1% degree of belief that the magnitude of the "100-year flood" will not be equaled or exceeded in any given year. While it is a subtle contrast, I believe that including the Bayesian definition as well might help reduce some confusion about the concept of return periods under circumstances of changing climate. Moreover, I believe that this speaks to some of Prof. Sivapalan's concerns in his review comments.

Page 2007, Lines 15-16: "at the location that the user wishes to build the flood frequency." This line is a little confusing. I would suggest something along the lines of "at the location that the user wishes to determine the flood magnitude versus frequency relationship."

Page 2008, Lines 4-8: Beginning "Although the word non-stationarity is not used explic-

itly. . . " This is a rather long sentence that is difficult to understand. I suggest splitting it into two shorter sentences for clarity.

Page 2009, Line 10: "Milly et al. (2002) also showed increased changes. . . " This sentence is unclear and I suggest rewording. Is it an increase in flood risk or an acceleration of changes in flood risks? Please clarify.

Page 2009, Line 16: ". . . methods have been proposed on how a changing climate. . . " I suggest changing to "have been proposed to address how. . . "

Page 2009, Lines 20-23: This sentence is a little unclear. Do you mean to that the assumed consistency in statistical relationships would not capture changes in climatological variables such as the long-term average snow line and storm track, or individual meteorological events such as a rapid melt out or a shift in storm direction? I think you mean the former, but I suggest some clarification.

Page 2010, Line 15: Change "a" to "and"

Page 2010, Line 19-21: Why 10? Please add at least one sentence describing why 10 replicates were chosen.

Page 2011, Line 2: "The effect of a changing climate may alter geographically." Do you mean "may vary geographically?" Please reword.

Page 2011 and 2012: Please include some information about the prevalence/importance of snow in each of these basins, such as which basins are snow- versus rain-dominated.

Page 2012, Lines 24-26: See above discussion about this assumption. I suggest adding at least one sentence here about the validity/restrictiveness of this assumption.

Page 2012, Line 26: change "a" to "and" - it seems as though this sentence would be better placed in the previous subsection "2.1 Basin selection"

Page 2013, Paragraph 2: For the sake of completeness, I suggest briefly discussing dynamic downscaling and associated benefits/drawbacks.

Page 2015, line 5: Change “to create an ensemble distribution of projected climates” to “as an ensemble representation of projected climates”

Page 2015, Line 24: Since at this point you have introduced both climate (CMIP3) and hydrological (SAC-SMA) models, I suggest some additional care here and throughout the manuscript to distinguish between each.

Page 2016, Line 17: Change “time series temperature and precipitation” to “time series of temperature and precipitation”

Page 2019, Line 15: Please provide a couple of references describing the methods the NWS uses to calibrate the SAC-SMA model.

Page 2021, Line 22-24: I suggest more specificity here with regard to the use of the calibration set of the SAC-SMA model for evaluation of the weather generation scheme and a re-wording of this sentence. Include the specific years during which the weather generator evaluation was performed.

Page 2021, Line 26: Please be specific whether you mean 10 simulations of 6-hourly precipitation and temperature or discharge from the hydrologic model.

Page 2025, Line 24-25: The sentence beginning “Therefore, the design would be underestimating. . . ” – this is one of the most important sentences in the manuscript, but I suggest revising for clarity. Is it really the risk that would be underestimated, or the magnitude of the flood with a given risk?

Line 2026, Line 5: I suggest including here a couple of sentences discussing the potential sources of uncertainty in the authors’ approach associated with the rainfall-runoff behavior as captured by the land surface models in the CMIP3 output data and the previously calibrated SAC-SMA models used by the authors. In particular, how might changes in land cover and vegetation cover associated with climate change confound or introduce uncertainty into efforts to use conceptual models like the SAC-SMA model

for studies like the authors?

Page 2031 (Table 1), Row 1, Column 3: Correct spelling of “constraint”

Pages 2034-2035 (Figure 2): This graphic contains a lot of information. Perhaps the authors could blow up one of the subplots to provide a more conceptual discussion (either in the caption or text) about how the information is used in the stochastic disaggregation as an example.

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