## Responses to Reviewer 1

We thank Reviewer 1 for the thoughtful comments. We have responded to each comment below. We believe that these modifications will significantly improve the manuscript.

***Some parts of the introduction seem to go very much into detail. For example, the event is discussed with its detailed properties, but the region as such is only introduced in section 2. The introduction could be shorter and more general. All specific information could be moved to subsequent sections. For example, the concept of "pseudo-global warming" does not become clear from what is written in the introduction anyhow. The structure could be improved elsewhere. For example, climate change is the topic of section 2.3 as well as section 4. I suggest integrating section 2.3 into section 4.***

We agree with the Reviewer's comments. The details of the 2007 event will be moved to Section 2 – Data and Methods.
The sentences that provide details about the "pseudo-global warming" method will be shifted to Section 2.3 – Climate Change Simulations
In this way, the introduction will be shorter and more general. It will also be clearer that we want to emphasize the new tool, as opposed to focusing on this case study.

We have decided not to integrate section 2.3 into 4. The reason is that section 2.3 is in the "Methods" section, where we go into the details of the PGW methodology. However, section 4 is part of results, so we think it would be confusing for the reader. We will change the titles of each section to clarify each section:

1 Introduction
2 Data and Methods
- 2.1 Data: Observations
- 2.2 Methods: Models
- 2.3 Methods: Climate Change simulations
  - 'Delta Method' for Model Simulations goes here but is not labeled as a separate section.

3 Results: Historical Simulations
4 Results: Climate Change Simulations
5 Conclusions

***The third section considers the simulation of the actual event, or the model calibration, as I would name it. However, it remains a bit unclear how well the overall model fits the observed data. The fit of some submodels (for precipitation, discharge flows) seems to vary a lot by time, location and so on (e.g. Figure 5). Regarding the economic submodel, detailed economic losses seem to be unknown (p. 9, top), so I believe that HAZUS and the input-output model were not in fact "calibrated" to the event. The authors could be clearer about this. Most***

***importantly, one would expect a summary regarding the authors' judgement of the OVERALL model performance in replicating the historical data.***

Yes, this is true. In short, we have confidence that the models are capturing the dominant physical mechanisms and are generally realistic. However, it is clear that there are issues with the representation of precipitation and hydrologic response. For this reason, we decided NOT to use the raw model output when doing the climate change simulations. The basic idea is that the models are not good enough to give us precise spatiotemporal values of the different variables (precipitation, streamflow etc.) However, we believe their representation of the dominant processes is good, and we trust that they are able to capture the CHANGES between the past and future – this is the reasoning behind the "Delta Method". We will explain this better.

The referee is right that the HAZUS model is not "calibrated" to the event in the sense that we can use an actual value of economic losses as a counterfactual to compare to the model results. The only data we can use to evaluate the model performance is the Department of Commerce estimated losses for the states of Washington and Oregon combined for this flooding event, which were approximately $1 billion dollars. In addition, the official building and inventory damages in Lewis county were estimated at $166 million. These are very close to our economic model results. In the revised manuscript, we will clearly state that the economic model is not calibrated and verified in the way that the physical models are.

We will incorporate a clearer statement regarding our assessment of the overall model.

***I can comment mainly on the economic aspects. The general idea of calculating direct losses first and then using an input-output model to calculate indirect or induced losses is plausible. The assumption that reconstruction is done by companies outside the affected area is also common. Regarding the obtained economic figures for the effect of climate change, they seem rather inconclusive. For example, what does it imply that physical damages of the considered event increase between 9 and 171% in Lewis County? The most relevant economic figure (for households, policy makers, insurance companies) would most likely be the expected annual losses and how these are affected by climate change. In particular, the probability of occurrence of the December 2007 event under present and future climate would be relevant in that regard. If we are talking about a 500-year event (as indicated on p. 2), future changes in this particular event would probably not be too relevant. Therefore, I wonder whether it would be possible to calculate hypothetical losses for, e.g. 20-, 50- and 100-year events. The meteorological records should provide the corresponding amounts of precipitation for these events and the economic losses could be obtained by using the model with the calibration for the December 2007 event. The PGW approach (as far as I understand it) would be applicable to those more frequent events analogously. Eventually, the expected annual losses (now and under climate change) could be calculated (see e.g. Velasco, 2015 for a simple approach).***

It is important to clarify that this is not a 500-year event for the entire basin. It was estimated to be a 500-year event only for the Doty stream gauge (we will make this clearer in the text).

The reviewer is correct, by focusing on such a large event, the future changes are probably not that relevant. In theory, repeating entire the methodology for a 20, 50, and 100-year event is possible – but it would take us a tremendous amount of time to complete the simulations that the reviewer is requesting (on the order of a year). In particular, obtaining the PGW simulations for the different return periods, and processing them through both hydrologic models and hydraulic model would take a very long time. However, the reviewer raises an important question, so we have attempted to address it using an alternative method, described below.

We started by analyzing the streamflow record for the Porter gauge (12031000, or gauge # 10 in the map). This gauge is downstream of the basin, and can capture the response of the full watershed. The gauge has a long record of data (70 years) so we can construct the flow duration curve and calculate the streamflow for different return periods (Figure 1). We then fit a log-normal distribution (black line) to the observed data. This is done to extrapolate to the 100-year return period (because we only have 70 years of data. We plot only the fitted values for the historical period in Figure 2, blue line.

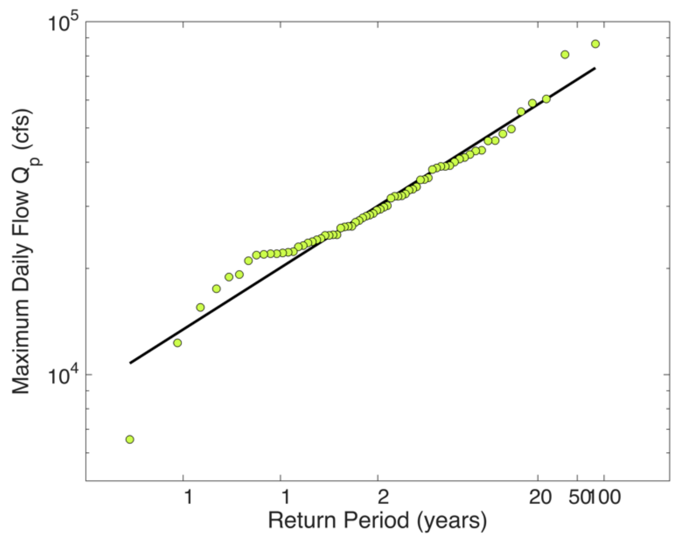


*Figure 1 Historical Annual Maximum Streamflow data for Porter gauge (green dots) and a fitted log-normal distribution (black lines).*

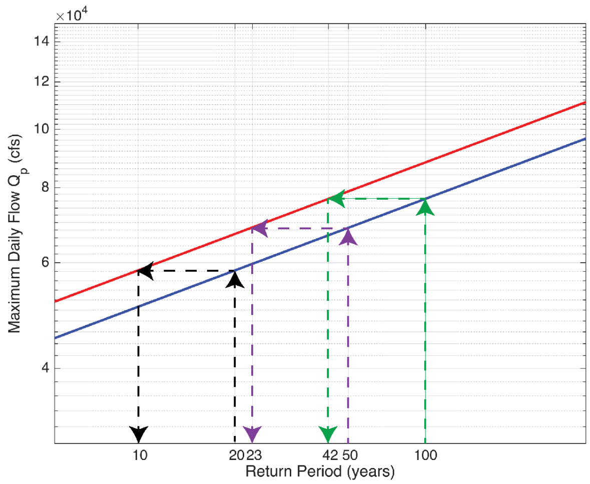


*Figure 2 Fitted streamflow for different return periods for the historical period (blue) and the future (red). The changes in streamflow in the future are calculated by assuming a 15% increase in streamflow in the future. We then calculate the changes in return period.*

Our goal now is to evaluate how the streamflow would change in the future, without going through the exercise of the PGW-HEC-HMS/DHSVM-HEC-RAS. To do this, we begin by evaluating the change in streamflow that we calculated for the December 2007 event, which was between -1% to about 30% (Figure 12a of our manuscript). Hamlet and Lettenmaier (2007) evaluate how streamflow intensity has changed in the historical period. In their Figure 10 (copied below as Figure 3), they show how increased cool season precipitation variability results in flood risk for a 20-year, 50-year and 100-year return period over the Western US. Over our area of analysis, the changes have been on the order of 10% increase to 20% increase – and this holds for the three return periods. **Given these results, we believe that an increase in streamflow of around 15% in the future is a reasonable approximation.**

We then repeat the exercise of fitting the log-normal distribution, **but assume that the streamflow values in the future are 15% larger**. We then obtain a new log-normal fit for the future values of streamflow (Figure 2, red line). **So, while the 100-year event in the historical period was approximately 7700 cfs, this would correspond to an event with a 42-year return period in the future (see green dashed curve). The 50-year return period event will be a 23-year return period event in the future (dashed purple), while the 20-year event will be a 10-year event in the future (dashed black).**

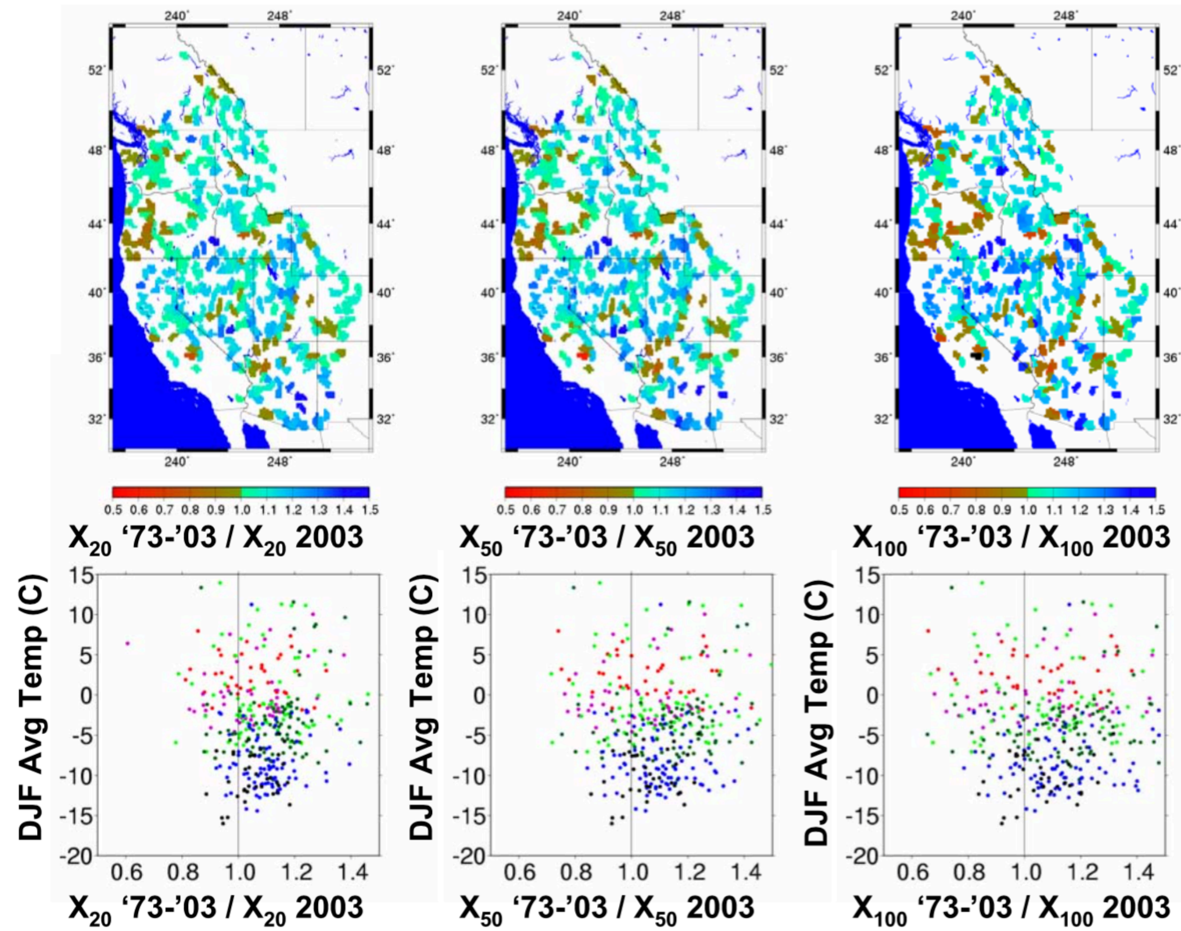


**Figure 10.** Same as for Figure 7 except showing composites based on all years from 1973 to 2003 for the 20-year (left panels), 50-year (center panels), and 100-year (right panels) return intervals compared to the same values for the unconditional probability distributions (all years from pivot 2003 simulations).

*Figure 3 Figure from Hamlet and Lettenmaier 2007.*

Using these changes in return periods, we use a method similar to Velasco et al. 2015 to evaluate the losses for the historical and future events. Using HAZUS, we simulate flood events for 20-, 50- and 100-return periods, based on the default dataset from the software. We can then calculate the total economic losses for the three counties (Grays Harbor, Lewis and Thurston) using the same economic methodology as before:

| **Return Period** | **Losses in Millions of Dollars** |
|---|---|
| **20yr** | **$(14.75)** |
| **50yr** | **$(17.12)** |
| **100yr** | **$(23.42)** |

Then, we calculate, the total losses for the historical period as the integral under the curve in Figure 4 ($920,000) and the total losses for the future as the integral under the orange curve ($1.2 million) for a total increase in losses of 23%.

| Return Period | 10 | 20 | 23 | 42 | 50 | 100 |
|---|---|---|---|---|---|---|
| Current | | $15 | | | $17 | $23 |
| Future | $15 | | $17 | $23 | | |

| | | | | | | |
|---|---|---|---|---|---|---|
| Probability of Exceedance | 0.1 | 0.05 | 0.043 | 0.024 | 0.02 | 0.01 |

| | | |
|---|---|---|
| Current | $0.92 | Average Annualized Losses (FEMA, Eq. 14-9) |
| Future | $1.12 | Average Annualized Losses (FEMA, Eq. 14-9) |
| **Change** | 23% | |

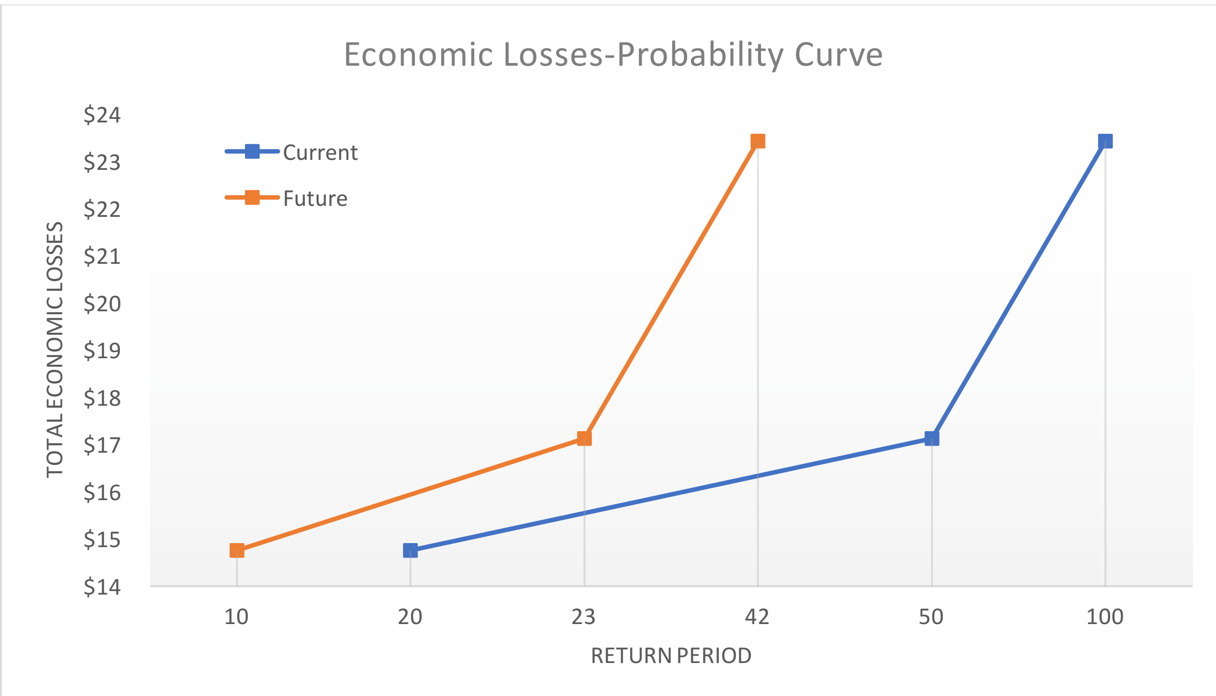


*Figure 4 Economic Loss-Probability Curve for the Current and Future period*

We will incorporate these results in the conclusions section, as a method to extend the current results to other events of different return period. We realize this is a simplification, in the sense that we are assuming a 15% increase in streamflow for all return periods. However, this example nicely illustrates the methodology that one would follow to calculate the expected losses for different return periods. We would indeed like to do this in a future analysis, by simulating a 20-year and 50-year return period event through the entire modeling system. Thank you for the interesting comment.

***Conclusion:***
***The overall quality of the paper is good and the suggested revision is somewhere between major and minor. The topic of the paper is relevant and the development of a coupled hydrologic, hydraulic and economic model is plausibly presented. The structure of the paper could be still improved and the implications of the results should be presented more clearly.***

We believe that the modifications to the manuscript that will be done to address your comments will significantly improve the structure of the paper and the implications of the results.

***Detailed aspects:***
***The abstract is very long (250 words). I would suggest leaving out the first three sentences, and starting the abstract with "In this work. . .".***

We understand the reviewer's concern and agree that that deleting these sentences would make the abstract more "to-the-point". However, after much deliberation (and consultation with other colleagues), we have decided to leave the first three sentences. The reason is that this manuscript is geared toward a wide audience (from atmospheric scientists to stakeholders) so it is important to put our work into context and motivate the research.