

## ***Interactive comment on “Tracking an Atmospheric River in a Warmer Climate: from Water Vapor to Economic Impacts” by Francina Dominguez et al.***

**Anonymous Referee #1**

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The paper is mainly concerned with a heavy precipitation (atmospheric river) event which occurred around the Chehalis river basin in December 2007. The goal of the paper is to simulate how climate change might affect this kind of event, including economic impacts. In the introduction, the actual event and its properties are presented compactly. The need for research regarding possible changes of atmospheric rivers due to climate change is convincingly motivated. The idea is to develop a coupled hydrologic-hydraulic-economic simulation model. Climate change is represented by the concept of "pseudo-global warming". Section 2 discusses the employed data and models. Main models are WRF (ARW) for the atmosphere, HEC-HMS and DHSVM for hydrology, the HAZUS model for direct economic losses and IMPLAN input-output table for the indirect (or induced) losses. The third section considers the simulation of

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the actual event. The fourth section concerns the simulation of the effect of climate change on the considered event.

Comments:

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. 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.

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

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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).

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.

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. . .".

Literature:

Velasco, M. et al (2015): Flood damage assessment in urban areas. Application to the Raval district of Barcelona using synthetic depth damage curves.

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