Anonymous Referee #3

We thank Referee 3 for his/her positive and constructive comments. Our responses for the revision of the manuscript are provided below in bold.

This is an interesting manuscript providing some basic information on how to couple human and natural systems towards an assessment of the most "beneficial" strategy for flood protection investments. This work is interdisciplinary in nature and presents a roadmap for future developments. I have to admit I have never worked on coupled human and natural systems and the material in the manuscript was rather informative. I think this is a well-written manuscript and that this journal represents a good venue. As detailed below, I only have few comments. The major comment I have is related to the premise of assuming stationarity. While this is a convenient working assumption, there is growing evidence that data related to the recent past may not be representative of the future. This is not just due to climate change but also to human modifications of the watersheds. I would like to see more discussion on how to potentially incorporate non-stationarity in the proposed framework.

We are expecting more variable extremes under climate change and greater uncertainty about the future. We would suggest that the stochastic stationary model that we have adopted has the capacity to simulate apparent nonstationary effects (plots to be included to demonstrate this) by choosing parameters close to the stationarity boundary.

It is increasingly recognised that the natural variability of the climate has resulted in decadal fluctuations in climate and extremes (e.g. Ntegeka and Willems, 2008 [cited in the manuscript]), that GCMs may not reproduce this variability (e.g. Johnson et al., 2011), and that alternative approaches to climate risk assessment are needed (Brown and Wilby, 2012). There is therefore great uncertainty in predicting how extremes will evolve in the future, and our approach is a first attempt at a climate risk assessment of adaptation strategies.

We agree that watershed/catchment changes are a source of nonstationarities, and will include a discussion of these (see response to Dr G Di Baldassarre above).

REFERENCES TO BE ADDED

Brown, C. and Wilby, R. L.: An alternate approach to assessing climate risks, Eos Trans. AGU,93(41), 401, doi:10.1029/2012EO410001, 2012.

Johnson, F., Westra, S., Sharma, A. and Pitman A. J.: An assessment of GCM skill in simulating persistence across multiple time scales. Journal of Climate, 24, 3609–3623, 2011.

- Pg. 8281, line 4: please explain the acronym "GCM/RCM". See also "OST" on pg. 8284 (line 15).

In the revised manuscript the acronyms will be expanded.

- Pg. 8283, line 21: I would add the reference to Barredo (2009; "Normalised flood losses in Europe: 1970–2006", HESS)

The reference will be added to the revised manuscript.

- Pg. 8291, line 24: should it be "However, there is evidence"?

The grammatical error will be fixed.

- Pg. 8282, first paragraph: I would reference Mauget et al. (2003a, b; Journal of Climate; "Multidecadal Regime Shifts in U.S. Streamflow, Precipitation, and Temperature at the End of the Twentieth Century" and "Intra- to Multidecadal Climate Variability over the Continental United States: 1932–99").

The references will be added to the revised manuscript.

- Pg. 8292, second paragraph: I would reference the work by Villarini et al. (2013; IJC, "On the temporal clustering of US floods and its relationship to climate teleconnection patterns") related to temporal clustering and climate controls.

The reference will be included in the revised manuscript.

- Section 4.2: could the authors please show us some exemplary time series of the flood peaks for these locations based on the model described in this section? It would be good to have some results for the three "persistence" levels.

We will include example time series plots in the revised manuscript.

- One of the premises of the work is the establishments of cost and damage functions (Figure 2). As a reader, I was left wondering how different the results would be for different combinations of cost-damage functions. Can the authors provide some sort of sensitivity of the results in Figure 3 to the choice of the cost-damage functions?

- The authors assume a 3.5% discount rate. I would like to see the sensitivity of the results to setting a higher and lower value.

The results will be dependent on the cost-benefit functions, as well as the assumed discount rate. We will discuss these sensitivities, which we plan to explore in future work.

Based on my read of Section 6, it appears that this modeling framework is still not ready to be fully coupled, and the focus is still mostly on the "hazard" side. As discussed in Section 7, I hope that future work will be able to couple the natural and human systems.

We will address the coupling fully in future work.