

Interactive comment on “Quantifying uncertainty in urban flooding analysis caused by the combined effect of climate and land use change scenarios” by I.-W. Jung et al.

Anonymous Referee #2

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Jung et al. evaluate the impacts of potential land use change and climate change on urban flood frequency using the PRMS model for two watersheds with contrasting degrees of urbanization. Overall, this is a well-written paper. I have two main points:

1) The main focus of the paper needs to be consistently clarified throughout the text. The title recommends uncertainty caused by climate change and land use change. In the abstract and the rest of the paper, however, five uncertainty sources are investigated. Yet four of them might be classified into either climate change or land use change category, uncertainty in hydrologic model parameter is apparently a different

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category. In addition, the abstract highlights two research questions (the first sentence), while the introduction section claims three. Furthermore, I would suggest the authors frame the conclusions accordingly centered on the research questions proposed in the introduction section.

2) About the Latin Hypercube Sampling (LHS) technique, it is a sampling method rather than a parameter (uncertainty) estimation method (e.g., Tang et al., 2007 HESS, 11, 793-817). The procedure described in the paragraph from Page 5378 to Page 5379 sounds similar to the GLUE procedure (Beven and Binley, 1992, HP, 6(3), 279-298). The authors should clarify the parameter uncertainty analysis method utilized. Additionally, parameter uncertainty should be discussed in section 3.5 due to the facts of that I) NSE = 0.6 is a subjective threshold; using a different value likely alter the results significantly; and II) using a different parameter uncertainty analysis method would also likely lead to very different results. Last, combined with the comment in main point #1), I do not think the parameter uncertainty part supports the main conclusions of the paper. That part could be thus omitted without compromising the integrity of the study. However, I would leave the decision to the authors and the editor.

Specific comments include:

- 1) Lines 25-26, Page 5372: two “future scenarios” are used here
- 2) The title of section 2.1: “framework” overstates the following descriptions
- 3) The tense should be consistent in the text (e.g., Line 15, Page 5373 “this study considers”; Line 23, Page 5379 “this study applied”)
- 4) Lines 3-4, Page 5374: LHS is not a parameter estimation technique (refer to comment in main point #2))
- 5) Line 21, Page 5375: The NSE of April is smaller than 0.77
- 6) Line 1, Page 5378: the statement “parameters regarding ... and parameter uncertainty analysis” need to be clarified

- 7) Line 27, Page 5378: It should be feasible parameter range instead of PDFs which is divided into equal intervals
- 8) The title of section 2.6: peak FQ or PeakFQ
- 9) Section 3.1: needs to be clear what parameters are calibrated and how
- 10) Line 4, Page 5384: “two futures”
- 11) Line 5, Page 5382: “by”
- 12) Line 2 17-18, Page 5386: “land use ... changes more abruptly than climate change”, how to determine one changes more abruptly than the other? They are two different concepts
- 13) Section 3.5: discusses uncertainty sources other than which specified in the title; refer to main point #1)
- 14) Lines 23-24, Page 5387: there are other methods applied to address model structure uncertainty, including the BMA method and the DREAM method, among others.

Interactive comment on Hydrol. Earth Syst. Sci. Discuss., 7, 5369, 2010.

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