Review of manuscript "Estimation of the effects of climate change on flood-triggered economic losses in Japan" by Tezuka et al.

In their manuscript, Tezuka et al estimate how flood losses may change in Japan in the future as a result of climate change. The authors find a linear relationship between increasing extreme precipitation and increasing losses: this finding is very interesting and in my mind forms the main novelty of the research. The paper is generally well written an easy to understand. However, in my mind there are several major issues that need to be addressed before the manuscript could be considered for publication. Firstly, it is difficult to judge all of the conclusions or findings, since results are only shown for a limited number of the model/scenario combinations used. Secondly, there appear to be a number of inconsistencies in the results that appear to have large implications for the main findings. Thirdly, the authors state that the paper is novel in the way it uses land use data to assess risk; however I find this part of the method rather standard (which is not a problem per se, but I do not understand why its novelty is repeatedly stressed). Next to this, I have several moderate and minor issues that the authors should address or respond to. Specific comments can be found in the paragraphs below.

Main comments

1. To me, the main novelty of the manuscript appears to be in the fact the findings of a linear relationship between projected increases is extreme precipitation and projected increases in losses. However, I feel that this interesting part is not discussed in much depth by the authors. If indeed this finding is correct, what are the implications of it for stakeholders? Does it mean we can avoid expensive and time consuming model runs? To what extent? Have such findings been seen elsewhere?

2. Related to the comment on novelty above, instead the authors repeatedly state that the paper is novel due to the method used to estimate damages, stating that "A novel technique based on the land use type of the flood area was employed to estimate economic losses...". However, I do not see why this is novel. To me, this part seems to be a rather standard application of the classical land use based approach to flood damage model using inundation maps, land use maps with associated asset values, and stage-damage functions. As can be seen in several reports/papers (e.g. Green et al., 2011; Merz et al., 2010), this is the most common and standard procedure for flood risk assessments. Whilst this is not a problem, I don't understand why it is being called novel here. Also, the authors state themselves (later) that the flood damage assessment method is the "method described by flood control economy investigation manual (MLIT, 2005)" for assessing costs of flood damage in Japan; hence, again, it does not seem novel.

3. Many of the results are only shown for either one GCM or one scenario. However, are the different combinations very different? In some cases, they appear to be so. For example, in Figure 3, until 2050, 1 of the models shows decreasing extreme precipitation, whilst the others show increasing extreme precipitation. However, in some of the further results, we only see those for MIROC3.2, which shows an increase.

4. There are also, as far as I can see, a number of important inaccuracies in the results and/or their interpretation. For example, on page 1634, line 7 onwards, the authors state that "Although the

three models predict different increases in every decade, and overall increase in precipitation is predicted by all three". However, reference to Figure 3 shows this not to be the case, as the precipitation appears to actually decrease for the CGCM2.3.2 model until 2060. This is not trivial, since if this model shows a decrease in extreme precipitation until 2060, then presumably the extreme discharge is also lower, and as a result the flood extent and flood damages. Hence, I assume that there are in fact also model/scenario combinations (for some time-periods, INCLUDING the 2050 time-frame discussed on page 1636), in which future damages are lower than present as a result of the projected changes in climate. If this is indeed the case, the implications for the results and conclusions of the paper are large. Namely, the statement that "...These results clearly show that flood-related economic losses in Japan will increase in the future as a result of climate change", is not fully supported by the findings, without further qualification. Moreover, the results are only shown for A1B, whereas I would like to see the result for all scenarios and models to be able to assess the results.

5. There are further inconsistencies in the results and discussion. On p 1635, line 4, it is stated that by 2050 the highest risk of flooding is under the A1B scenario. However, on p 1635, line 12, it is stated that the highest losses are predicted for the A2 scenario. Reference to figure 5 also suggests that the losses are largest for A2.

6. I am not sure of the accuracy of the statement on p 1636, line 1-2, that "The overall variation shows that the potential economic loss is greater for the SRES-B1, A2, and A1B scenarios...". In fact, is figure 6 not referring to the rate of increase in economic loss, rather than the absolute economic loss?

7. On page 1636, final paragraph, the authors state that the relationships that they found (between projected increases in precipitation and projected increases in losses) could be useful for various stakeholders. Whilst I agree, please specify here how they may be useful. Also, please reflect on the implications: e.g. could parts of the modelling chain be left out or carried out less intensively?

8. Whilst the paper specifically examines the effects of climate change on flood damages, there is no mention of other changes that may also influence damage. For example, a whole host of studies suggest that in many regions socioeconomic changes (e.g. changes in population and asset values) may have a larger impact on future flood damages than climate change (See for example the SREX report of IPCC (IPCC, 2012)). Whilst this may not have been studied in this paper, the authors should at least mention this in the discussion.

9. The paper does not have any treatment of uncertainty, whilst this is known to be large in flood risk assessment (see, e.g. Apel et al., 2008; De Moel et al., 2011; Merz et al., 2004). Has any uncertainty assessment/sensitivity assessment being carried out? If not, please at least discuss why not, and clearly state the limitations and implications of this.

10. As far as I can see, no parts of the model chain (hydrology, hydraulics, damages) have been validated against observed or reported data, or at least this is not discussed here. Please provide information on the validation carried out, and on the parts of the chain for which no/limited validation could be carried out due to lack of observed data.

11. Introduction: The introduction does not clearly state the aims or objectives: the last paragraph does say what is done, but reads more like a brief methodology. It would be useful to include a

paragraph clearly stating the aims and objectives (or research questions) of this study. Moreover, parts of the introduction seem out of place. For example, the there is a paragraph describing some generic problems of using GCM output in regional/local scale assessments. However, this would seem more suited to the methods section, where the use of GCMs is discussed – in its current position it detracts from the overall flow of the introduction.

12. The damage assessment method appears to contain just one sort of "residential area": is this correct? If so, please mention the potential problems of this (with reference to relevant literature), given the heterogeneity of residential areas, such as high-density residential areas in cities, low-density areas in cities, small towns, villages, etc.

13. Generally, whilst describing the various economic loss parts of the method, the authors state that losses are estimated "...as a function of the water..." (e.g. p. 1631, line 12, and elsewhere). However, what is exactly meant here? Do you mean that you use depth-damage curves to estimate damage as a function of inundation depth? i.e. the standard way to assess economic flood losses? If so, please make clearer, and mention that this is internationally considered a standard approach (e.g. Green et al., 2011; Merz et al., 2010).

14. The term "economic damage in traffic zones" is very vague: what is meant by it? Damage to roads? To vehicles? To assets located near roads? Moreover, it is also stated that a factor of 1.694 was used to estimated traffic zone damage compare to "general asset damage", with the reasoning that "MLIT defines 1.694 as the ratio between the cost of damage to public facilities and the cost of damage to general assets." However, if this factor is assumed to represent the factor difference between general assets and public facilities, I still do not understand why it is then applied to calculate "traffic zone damage". From this, it seems that we can conclude that "traffic zone damages" are equal to "public facility damages" - are there empirical data to support this? Please explain this section more clearly.

15. Also in the methods section, it is stated that extreme rainfall data are used to force the hydrological model. However, what initial conditions are used? Are these constant? Would the extent of flooding not depend greatly on initial conditions such as groundwater and soil water storage?

16. The "Results and Discussions" section is difficult to follow, mainly due to the absence of any subsections. Please include subsections (such as is done in the methods section), in order to add structure to this section.

Small comments

17. P1620, L22-23: "…are major natural disasters triggered by the effect of climate change". This is not quite correct. These disasters are not "triggered" by climate change, but by meteorological/climatological conditions". These conditions can be affected by climate change.

18. P1621, L12: here, the use of the word 'vulnerable' could be confusing, given its specific usage in the language of DRR (e.g. risk as a function of hazard, exposure, and vulnerability, UNISDR (2011)). Maybe consider something like "Japan is particularly prone to flood hazard" instead.

19. P1621, L13: It is stated that "Increase in the frequency and intensity of local heavy storms…have been recorded in Japan in recent years…", followed by a list of some storms and their date.

However, this does not per se indicate an increase in frequency or intensity, it is simply a list of recent events. Please re-phrase more carefully.

20. P1623, Eq 1: I am wondering why the chosen parameter symbols are used, e.g. Bd for fine resolution factor, Gf for monthly precipitation. There may be a reason, but I find did not find any logic.

21. The authors choose to examine extreme rainfall (and flood losses) for several return periods, namely 5yr, 10yr, 30yr, 50yr, 100 yr. What is the reason (if any) for selecting these return periods. Also, why did you choose only to look at damages for a number of return periods, and not also annual expected damage; these are highly influence by choice of return periods (e.g. Ward et al., 2011).

22. In the methods section, pg. 1626, around lines 11 onwards, the authors discuss the use of a threshold for precipitation in the form of rainfall/snow: it is stated that a threshold of 20C. Maybe I am misunderstanding what is meant here, but why is 0C not used?

23. In many parts of the text, the projections are referred to as "predictions". For example, on p. 1634, line 10, but also elsewhere. Long-term scenarios from climate models do not provide "predictions", but "projections".

Textual corrections

- P1620, L9: replace "shows" with "show"
- P1620, L9: replace "flood" with "flooding"
- P1621,L12: replace "…the steep geography…" with "...its steep geography…"
- P1622, L15: replace "into" with "to".
- P 1627, L21-22: replace "...by calibration the model..." with "...by calibrating the model..."
- P1634, L24: replace "...of 24h are obtained..." with "...of 24h were obtained..."
- P1634, L7: here, and also elsewhere, SRES is referred to as SRESS. Please correct to SRES in all places
- P1634, L7: replace "...for A1B SRES..." with "...for the A1B SRES..."
- P1634, L22: replace " ...with than..." with "...with more than..."
- P1636, line 22: replace "...to public..." with "...to the public..."

References

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