<u>Response to Reviewer Comments # 1</u>

Natural Hazards and Earth System Sciences (NHESS)

Estimation of the effects of climate change on flood triggered economic losses in Japan

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Thank you very much for your invaluable comments to improve the quality of the manuscript. We address each of your points as below.

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?

We agree with the reviewer, the linear relationship between projected increases in extreme precipitation and projected increases in losses is one of the novel findings of the study. This relationship is useful for stakeholders as a decision making tool. Anyway with the used data/mechanisms, the proposed relationship is applicable in Japan and it is limited to specific conditions such as steep geography and humid climate in Japan. Also as highlighted in the manuscript this research calculated flood damage in fine resolution of 1km resolution. Therefore it provides a high accurate spatial distribution of flood damage. Therefore stakeholders/authorities/decision makers can decide priority locations for adaptation/risk mitigation activities. Taking reviewer's comment, more discussions are added to the manuscript.

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.

Thanks for the reviewer's invaluable comment on the technique used for this study. We realized that the method we used is already used in the literature, specially above mentioned paper of Merz et al, 2010. Therefore we should modify the statement removing the term "novel", as the method we used to estimate the damages is a standard/common application. The manuscript is revised accordingly and the important paper Merz et al, 2010 is referred and added to the reference list.

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.

In this study, we compared three GCMs (MIROC3.2, CGCM2.3.2 and PCM) and three SRESS scenarios (A1B, A2 and B1). As number of result based figures are higher, we used some selected (representative) Figures. Fig 3 shows that variation among three GCMs. It is discussed in the paper as "Spatial averages of the downscaled precipitation data show that different GCMs predict different increases in future annual average precipitation in Japan. Fig 3 shows the rates of precipitation increase predicted by the three GCMs considered (MIROC3.2, CGCM2.3.2 and PCM) for A1B SRES scenario. Although the three models predict different increases in every decade, an overall increase in precipitation is predicted by all three'. The Figure shows that the annual average precipitation between 2000 and 2100 is predicted to be 1.08% to 1.12% higher. Based on these changes, we selected MIROC3.2 to show the comparison of the results for different scenarios. We can provide the Figures for all GCMs under all scenarios, but we selected the given Figures to represent all results. (If needed we can provide all Figures, but we assume it will be too much for the paper).

Anyway for the most important results (Relationship between the increase in extreme precipitation and the increase in potential economic loss) we have submitted Figures showing variation for different GCMs and variation for different SRES scenarios.

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.

Please note that Fig 3 shows the "rates of precipitation increase" not the absolute precipitation. For the CGCM2.3.2 model, precipitation increases slowly until 2060, but there is an increase, not a reduction..!! (increase rate should be negative for a reduction…!!). Therefore our statement "Although the three models predict different increases in every decade, and overall increase in precipitation is predicted by all three", is correct. All increase rates are >0. To avoid the misunderstanding of the Figure, we have modified the Figure 3 in the revised manuscript, which now shows the percentage increment for each decade (2040, 2050 etc) compare to 2000.

We used 24-hr extreme precipitation (for different return periods) for flood estimations. Therefore flood extend and flood damage discussed in page 1636 is correct and the statement "...These results clearly show that flood-related economic losses in Japan will increase in the future as a result of climate change" is correct. Fig 4 shows the 24hr extreme precipitation over Japan for 50 year return period. It clearly shows that 24hr extreme precipitation will be increased all over Japan. The extreme precipitation is the main cause for flood, not the average precipitation.

Hope this will help to clarify reviewer's comment. As mentioned in the previous comment, even though we didn't try to include all the graphs/figures for all scenarios/models in the manuscript due to large number of figures, we are happy to provide them if needed.

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.

A1B scenario shows the highest increase rate of precipitation and leads to predict the increasing economic loss whereas A2 scenario shows the highest absolute value. Fig 5 shows the absolute extreme precipitation and economic losses.

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?

Fig 6 is referring to the rate of increase in economic loss, not absolute loss. It clearly shows that potential economic loss is higher in the order B1, A2 and A1B. Yes the statement is correct. Considering the increasing order, economic loss increases in order of B2, A2 and A1B.

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?

This is related to the first comment. As highlighted in the manuscript this study calculated flood damage in fine resolution of 1km resolution. Therefore it provides a high accurate spatial distribution of flood damage, where stakeholders/authorities/decision makers can decide priority locations for adaptation/risk mitigation activities. In the modelling chain, all the parts are equally important, but linear relationship shows a straight forward (preliminary) idea for the stakeholders showing that flood related economic damage as a function of extreme rainfall with multiple return periods. Taking reviewer's comment, more discussions are added to the manuscript.

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.

We agree with the reviewer. In this study, we calculated flood depth and it is the main parameter used as the influencing factor for economic losses. Due to some limitations such as lack of data, we have to keep the other parameters (population, asset values) constant. We do agree that the effects of these factors have an impact on the economic loss, but this study mainly focused on climate factors only. We revised the manuscript adding these information/discussions into the paper.

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.

In this paper, we mainly use multi scenarios and different GCMs to evaluate the impacts of climate change. We agree with the reviewer (and literature) that the uncertainty is higher in flood risk assessment. Also we understand that uncertainty analysis would enhance the quality of the paper, but we need more data to conduct an uncertainty analysis. As this paper targets to compare the changes triggered by different GCMs and climate change scenarios, the uncertainty is affecting same way for all scenarios. (for example, if return period changes from 50yr to 100yr, potential losses increase about 15% overall).

Even though we did not analyse the uncertainty, we discussed the uncertainty in the discussion part of the revised paper.

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.

This paper combines with previously published paper (Kazama et al, 2009) and brings that information forward. Model calibration and validation is published in the previous paper. We mentioned that reference in the manuscript.

Refer: So KAZAMA, Ayumu Sato, Seiki Kawagoe, Evaluating the cost of flood damage based on changes in extreme rainfall in Japan, Sustainability Science, Vol.4, Iss.1, pp.61-69, 2009. DOI: 10.1007/s11625-008-0064-y

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.

Thank you for the reviewer comments. These changes are encountered in the revised manuscript.

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.

Damage of residential area is obtained as a sum of residential building damage and office building damage. This is defined in the flood control economy investigation manual (MLIT) and is used in many studies to estimate flood damage in Japan (Kazama et al, 2009).
We calculated flood damage in 1km resolution. The land use of the (1km * 1km) mesh is decided by the predominant (highest percentage) land use. Therefore the heterogeneity of residential areas in terms of high-density or low-density is not taken into account, but the average (or dominant) land use type for each 1km*1km cell is considered.

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).

Thank you for highlighting the misleading statement. Even though it appears correctly in the manuscript due to some reason it is mistyped in the NHESSD manuscript, which we did not notice during proof reading. Anyway the sentence is corrected in the revised manuscript.

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.

The traffic zone damages are similar to public facility damages. This is defined in the flood control economy investigation manual (MLIT) and is used in literature taking the conversion factor as 1.694 (Kazama et al, 2009). This section is explained well in the revised manuscript.

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?

For the flood inundation, we used 2D non-uniform river flow model So we did not need to consider groundwater and soil water storage. Runoff depends only on elevation (slope). As our study area (Japan) characterised with steep slopes, it generates a rapid flushed flood during extreme rainfall period. This model application in the literature proves a good estimation results during flood peak period even though the runoff model neglected evaporation and infiltration (Kawagoe, 2010; Kazama 2009).

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.

Results and Discussions section is revised taking reviewer comment into account

Small comments/corrections

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.

This revision is done in the revised manuscript

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.

This revision is done in the revised manuscript

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.

This revision is done in the revised manuscript

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.

No change is done for the equation.

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).

These return periods are selected as they typically use for hydrological design and develop management guidelines.

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?

Sorry there is a textual correction. It should be 2^0C.

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".

Thank you. This is corrected throughout the manuscript.

Textual corrections

Thank you for highlighting textual correction. We have corrected all the minor corrections and manuscript is revised accordingly.