Dear Cristina Prieto

We would like to thank the three reviewers for their feedback on our paper (nhess-2018-253) entitled “Low-hanging fruits in large-scale fluvial landscaping measures: trade-offs between flood hazard, costs, stakeholders and biodiversity”. Our paper combined multiple disciplines - hydrodynamics, ecology, and cost estimates - which were used for the determination of the effectiveness of landscaping measures in alluvial rivers. The positioning of the measures was driven by a stakeholder analysis in terms of land ownership and preferences for specific measures. The additional value of combining disciplines showed in the quantitative evaluation of measures, which were positioned and parameterized using *a priori* stakeholder preferences. This brought insights from these disciplines together in a single visualization and enabled the comparison of apples to oranges.

The reactions between the reviewers varied strongly. Our study resonated deeply with the experience of reviewer three (R3) in working with government agencies and R3 suggested a number of additional components that could be taken into account. R1 did not perceive a clear take home message and R2 preferred a more monodisciplinary approach. We believe we found a balance in addressing the different comments by reformulating the objectives, a thorough rewrite of the discussion, and a clarification of the take home message.

The revised manuscript has significantly improved from the suggestions and comments of the reviewers, whom we now thank in the acknowledgements. We hope that our manuscripts is now suitable for publication and look forward to further comments of the reviewers.

Sincerely,
Menno Straatsma, on behalf of all coauthors.

Specific comments to reviewer 1

R1: Manuscript compares a variety of options for flood hazard mitigation (in a given area) with respect to (i) hydraulic effectiveness, (ii) economic cost of the measure, (iii) number of people/stakeholders involved (as a measure of social impact and / or feasibility) and (iv) ecological impact. Modelling tools for individual components of the problem are standard; evaluated scenarios are relatively . Please, consider that I am not an expert in ecology; therefore, I cannot judge whether the biodiversity index from the model BIOSAFE provides a significant measurement of ecological value. In the following, I will take it as granted.

Reply: The summary of the evaluation parameters by R1 are correct and concisely represented. We disagree with the statement that the modelling tools are standard: the fast and detailed integration of a GIS with Delft3D Flexible Mesh has only been around for a year, and the large scale application is rarely carried out to our knowledge. The integration of models from various disciplines creates additional value for decision support that can not be derived from monodisciplinary studies.

R1: I read (and re-read) the manuscript with interest but, at the end, I could not find any conclusive message to be learnt. In fact:

1. From the hydraulic point of view results are quite obvious.
2. “Clear trade-offs were revealed between evaluation parameters, but no single measure represented the optimal combination on all aspects”: this is also not surprising.
3. Links among control variables (mitigation measures) and state variables are quite expected; accordingly, trade-offs are also quite expected.

4. Showing the value of (semi) automatic tools while dealing with complex problems involving spatial variables over large areas does not represent a relevant finding.
5. It is true that "The multidimensional evaluation space provides a frame for the cocreation of adaptation paths for climate-proofing deltas", as well as for other critical areas; however, authors terminate the narration just at the critical point of the story ("Our methodology suits the early stages of the planning process"): and now, how to proceed? So far authors conclude that "The results can help to argument in favor of establishing multi-stakeholder platforms": this is surely correct, but not enough as a conclusive value of the paper. I understand that, at present, this is the state of development of the process, so that the author cannot tell the reader much more than this, based on the reality of the field case (see the Discussion section). However, this makes the story only weakly instructive.

All together, my opinion is that there is some (much?) interesting experience in this field case, but the material is not organized / presented in such a way that the reader can take home significant conclusions. I therefore suggest rejection of the paper.

Reply: Based on this comment, we have revised the objective and updated the discussion to a large extent to bring forward the take-home message of our paper, which is that we quantitatively evaluated measures, which were positioned and parameterized using a priori stakeholder preferences.

We believe there is much interesting experience in our paper, as was also recognized by R3. This has now been brought forward more clearly. These findings will be surprising for many readers, and the application in subsequent stakeholder processes will likely show additional value. R1 did not provide any references to substantiate the statements that our findings are unsurprising, which has made revising our manuscript challenging, because it remained unclear where this information would have been available. Our request for references remains unanswered. We adapted in the following manner and feel confident that this provides a clear take-home message for the reader:

- We removed the emphasis from the trade-offs themselves and highlighted the quantification of the trade-offs in a standardized way. This compact visualization of a multidimensional feature space represents a new finding, which is based on a standardized method that can be applied elsewhere and should be of interest for a wide audience.
- We highlighted the a priori stakeholder preferences and the way we included this in the positioning of measures. We discuss the position our paper taken in decision support, but also of stakeholder modeling using agent based models and game theory
- Stakeholder processes result in different outcomes based on the presence and resources of the participants. We do not find any papers that documented this even though we reviewed the literature carefully. We argue that our results provides a common ground for any stakeholder meeting, which increases mutual understanding.

I have no suggestions on how to better shape this material with respect to my negative evaluation. On the opposite, I have a number of observations, which may be useful in the case the manuscript will be allowed to proceed along the editorial process and/or it will be submitted to different journals. Please, find them in the annotated pdf file.

Reply: We believe, we have brought the message forward clearly in the revised version of the paper. We are grateful for the annotations in the pdf. The paper was revised accordingly and where possible. A detailed list of adjustments can be made available if required.

Specific comments to reviewer 2

R2: The article is interesting but far from the goal that the authors promise in the preface. The automatic decision-making system is not adequately displayed or analyzed. In any case, the article is interesting, I suggest that the preface be adapted to the content.

Reply: We are grateful for R2's judgement that our article is interesting. R2 also suggests to change the introduction to lower the expectations of an 'automatic decision-making system'. In the introduction, we stated:

"To the best of our knowledge, none of the DSSs for fluvial flooding listed by Newman et al. (2017) contained an option for the automatic creation of measures at the spatial scale of the river reach. Coupling a DSS with automated measures could have additional value in the exploratory phase of planning new measures to provide all stakeholders with the efficiency of measures with respect to flood hazard reduction, costs, and biodiversity."

We changed these sentences to:

"To the best of our knowledge, none of the DSSs for fluvial flooding listed by Newman et al. (2017) enabled the semi-automatic planning of measures at the spatial scale of the river reach. A semi-automatic system was presented by Straatsma et al. (2018), who used a rule-based system for positioning and parameterization of measures. Coupling a DSS with semi-automated planning of mitigation measures could have additional value in the exploratory planning phase to provide all stakeholders with the efficiency of measures with respect to flood hazard reduction, costs, and biodiversity."

This wording shows that our method is not fully automatic; the rules for positioning still need to be provided.

Straatsma, M.W. and Kleinhans, M.G., 2018. Flood hazard reduction from automatically applied landscaping measures in RiverScape, a Python package coupled to a two-dimensional flow model. Environmental Modelling & Software, 101: 102-116.

R2: Although the article presents an extensive substance, which, due to insufficient space, is superficially treated. I suggest that the authors reorganize the article and devote themselves to a more specific topic of their choice. It is possible to process only stakeholders' questions.

Our paper presents the results of an interdisciplinary study that includes geocomputation, hydrodynamics, ecology, cost estimation, which are driven by insights from social sciences on stakeholder preferences. It is true that any of these aspects could be focussed on individually and treat them in more detail. In fact many of these details are studied by collegues and presented in specialized journals. We treat the various components superficially to limit the word count of the article, but we refer to other publications for more details for the ecological modeling, hydrodynamic modelling, and intervention positioning and parameterization. We highlighted the stakeholders in the main objective to: "Our objectives were to (1) quantify multi-faceted trade-offs between landscaping measures to adapt a large delta distributary to sea level rise and increased river discharge while honouring ecological value and societal stakes, and (2) include government complexity by positiong the measures in areas owned by the two largest stakeholders versus all stakeholders based on a priori preferences." The discussion was adjusted accordingly with the first two sections representing these two objectives and their take home message.

R2: A minor error, on page 2e, is referred to in Figure 3e, and should be 2e.

Reply: this was corrected.

R2: I fully agree with first referee.

Reply: See R1 for details on for the first referee.

Specific comments to reviewer 3

R3: General comments
My thanks to the authors and editors for the opportunity to review this manuscript. There are many aspects of the processes reported in this manuscript that resonate deeply with my experience working with government agencies and stakeholders in floodplains of large rivers of North America. I think it is a useful manuscript that will provide a good example for numerous other planning efforts.

Reply: We are grateful of the characterization of the paper as useful and that it will provide a good example for many planning efforts. We highly value this characterization, because R3 has personal experience with planning and government agencies.

R3: Specific comments
I would start with a request to define some terms to gain a broader understanding among an international audience. Different river-management communities and languages have different terms for river features, and it would help improve communication if the authors invested in a few words to define terms. For example: For many – but perhaps not all – North Americans would expect that "landscaping" means planting flowers and shrubs for aesthetic purposes. Landscaping in the context of this manuscript is reconfiguration of the channel-floodplain geomorphology, essentially terraforming. Similarly: groynes = wing dikes, or more generically, channel-training structures. Embankments = levees. Braid hedge = I have no clue. Etc. I do not recommend abandoning the European terms; I'm simply requesting a parenthetical definition to help in the translation.

Reply: We recognize the difference in terminology and we struggled at times reading the American terms. Parenthetical definitions were added to the revised version.

R3: I found the scale, scope, and approach were very useful in the context of regional planning. The value was readily apparent in the multiplication of scenarios as channel configurations, roughness, upstream hydrology, and sea-level rise scenarios were combined. Granted, the hydraulics are simplified, but I believe the modeling would be useful in other at a planning level to filter scenarios for efficiency and to educate stakeholders about the opportunities and constraints.

Reply: This is exactly the function of this type of model, and we are happy that its value as such is perceived.

R3: I was surprised that floodplain sedimentation was not a bigger issue in scenarios, especially with floodplain lowering. Toward the end of the manuscript the authors assert that the sediment load of the river is diminished due to upstream reservoirs, but presumably is not zero. Would lowering scenarios also require long-term maintenance to continue to clean out sediment? It would be beneficial for the authors to address sediment and sedimentation dynamics.

Reply: This is a certainly a relevant remark, but we saved this topic for a follow-up paper, limiting the scope of this paper to implementation of the measure. Temporal effects due to sedimentation were

considered out of scope. Morphological changes in the distal parts floodplain are small, around 0.1 mm of silt and clay for each day the floodplain conveys water. Increasing the inundation frequency to 50 days per year, the most extreme floodplain lowering scenario, only raises the distal part with 0.5 cm per year. The proximal parts of the floodplain and side channels behave quite differently. Depending on sediment availability and planform geometry, geomorphological changes have been observed of 20 cm per year. Unfortunately, the numerical modelling of the sediment entrainment and deposition with a range of grainsizes is still in its infancy. We know of a few PhDs that are working on this topic at the moment, but their results are not sufficiently conclusive to include in this paper. In addition, computation times would dramatically increase. A recent study by Van der Deijl et al. (2017) showed that sediment trapping just downstream of our model domain, sediment trapping is very low due to limited sediment concentration in the water.

Morphological modelling has been included as one of the current limitations of the research and as a potential future inclusion in the discussion.

van der Deijl, E.C., van der Perk, M. and Middelkoop, H., 2017. Factors controlling sediment trapping in two freshwater tidal wetlands in the Biesbosch area, The Netherlands. Journal of Soils and Sediments, 17(11): 2620-2636.

R3: The ecological scoring for floodplain vegetation seems limited, as the scenarios included only two treatments. North American floodplains left fallow are normally rapidly colonized by successional tree species which can have additional ecological value, especially as bird and mammal habitat. The tradeoff, of course, is that the woody communities can impart substantially greater flow resistance. The authors assert (p. 9) that stakeholders view flood safety as the main objective, but it is not clear that the tradeoff with a broader range of ecological value was adequately evaluated. It would also be useful to evaluate a "natural" roughness condition as a reference condition.

Reply: Vegetation succession after the measure implementation was indeed left out, assuming the vegetation is managed in such a way that the ecotope and trachytope distribution does not change over time. In the floodplains of the River Waal very strict rules apply for the maximum hydrodynamic roughness that is allowed, although spatial differentiation is taken into account in these rules. Exceeding the roughness prompts action from the ministry of infrastructure and water to reset the vegetation by mowing, or deforestation. Not included in the paper were try-outs with other vegetation types following a natural succession (meadows at the start, herbaceous vegetation after five year, shrubs at 10 and forest at 30 years), where herbaceous vegetation gives an increase in water level of around 25 cm, but shrubs dramatically increase water levels during design discharge with more than 1.5 m, leading to dike breaches. We did not want to extend the paper with additional methods and results due to the current length of ~10 000 words excluding references and chose to refer to Makaske et al. (2011) for water level lowering and provide an indication of the potential biodiversity increase. See the new discussion section below with list of limitations/extentions, as they are not limited to morphology and biodiversity.

Our methods are limited to the implementation of the measures and the effects on the peak water levels and several extensions would create additional value for decision support. Firstly, extending flood hazard to flood risk of the protected land would provide insight in the costs of the measures in relation to the avoided loss in case of a dike breaching flood. For this the failure probability of the embankment should be assessed (Marijnissen et al., 2018) as part of a full flood risk assessment (Vrijling, 2001). Secondly, the altered flow patterns from the measures will give a morphologic response over time in the floodplain and in the main channel. Increased floodplain inundation affects the sediment deposition with a mean sedimentation rate of 0.13 mm/day of inundation for the floodplains and 2 mm/day inundation at the entrance of fast aggrading secondary channels (Baptist

et al., 2004). Geerling et al. (2008) found a deposition rate of 3.7 cm $year^{-1}$ for a lowered floodplain next to main channel. The increasing elevation reduces the conveyance capacity and limits the longevity of the measure. For the main channel, opposite effects are projected: the Rhine delta has a reduced sediment supply due to the storage in upstream reservoirs for hydropower, which led to erosion of the main channel over the last decades (Frings et al., 2009). For the future, Sloff et al. (2014) predicted a main channel erosion of 0.25 m in the lower reach and 0.4 m sedimentation in the middle reach of the Waal, based on a 2D morphological study spanning the period 2015 to 2055. We assumed that the 1.8 m sea level rise translated into a 1.8 m rise of the downstream boundary condition and ignored the long-term morphological changes. Under natural conditions, the bathymetry would follow the rising sea level, but the results of Sloff et al. (2014) justify our assumption. Thirdly, vegetation management strongly affects the development of the hydrodynamic roughness. If the land is left fallow, vegetation succession will lead to herbaceous vegetation, shrubs and floodplain forest after 5, 10, and 30 years, respectively leading to a maximum increase in water level of 0.6 m for the IJssel distributary of the Rhine (Makaske et al., 2011). The succession positively affects the biodiversity with maximum increase of around 10 % after 30 years. BIOSAFE needs to be updated to include these succession stages, as no ecotope succession model is currently available and more detailed models (Asaeda et al., 2014;Sanjaya and Asaeda, 2017;Oorschot et al., 2016;Camporeale et al., 2013) can not yet be linked to BIOSAFE. Fourthly, compensation of land owners that have increased inundation of their land due to the removal of minor embankments could be included just like avoided damage from lower exposure to flood risk in a full cost-benenit analysis. See Mechler et al. (2015) and Di Baldassarre (2015) for further discussion on risk management. Finally, we assumed that all measures are implemented instantaneously, whereas the timing could be made dependent on updated sea level rise projections to optimize the measures under uncertainty and avoid unnecessary costs (Postek et al., 2018;Kind, 2014). These potential extensions were out of scope for this paper.

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Frings, R. M., Berbee, B. M., Erkens, G., Kleinhans, M. G., and Gouw, M. J. P.: Human-induced changes in bed shear stress and bed grain size in the River Waal (The Netherlands) during the past 900 years, Earth Surface Processes and Landforms, 34, 503-514, 10.1002/esp.1746, 2009.

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Mechler, R., and Bouwer, L. M.: Understanding trends and projections of disaster losses and climate change: is vulnerability the missing link?, Clim. Change, 133, 23-35, 10.1007/s10584-014-1141-0, 2015.
Oorschot, M. v., Kleinhans, M., Geerling, G., and Middelkoop, H.: Distinct patterns of interaction between vegetation and morphodynamics, Earth Surface Processes and Landforms, 41, 791-808, 10.1002/esp.3864, 2016.
Postek, K., den Hertog, D., Kind, J., and Pustjens, C.: Adjustable robust strategies for flood protection, Omega, https://doi.org/10.1016/j.omega.2017.12.009, 2018.
Sanjaya, K., and Asaeda, T.: Application and assessment of a dynamic riparian vegetation model to predict the spatial distribution of vegetation in two Japanese river systems, Journal of Hydro-environment Research, 16, 1-12, https://doi.org/10.1016/j.jher.2017.05.002, 2017.
Sloff, K., Van der Sligte, R., and Ottevanger, W.: Morfologische pakketsom Waal: morfologische effecten Ruimte-voor-de-Rivier maatregelen, Deltares, Delft1208454-000, 188, 2014.
Vrijling, J. K.: Probabilistic design of water defense systems in The Netherlands, Reliab. Eng. Syst. Saf., 74, 337-344, https://doi.org/10.1016/S0951-8320(01)00082-5, 2001.

R3: For context, it would be useful to add whether or not there is aggregate extraction (or other dredging) from the channel as well as the floodplain.

Reply: We now provide this information in section 2: “The main channel, 250 m wide, is fixed in place by groynes (spur dikes, wing dikes) for navigation and prevention of ice dams. It incises in its own deposits due to limited sediment supply from the catchment. Maintenance dredging in the insides of the bends is required to maintain the minimum navigable depth. The dredged material is dumped again in the deep parts of the outer bend. Excavation of floodplain sediments occurs mainly in combination with interventions for flood hazard reduction.”

R3: There is an apparent miss-citation of a figure on p. 6. Figure 3e should be figure 2e.

Reply: Correct, this has been changed now.

R3: On p. 8 the citation of 2.5% probability for the 1.8 m setup of downstream water level should be clarified. Is this an annual probability? What additional climate change assumptions are behind this?

Reply: The 1.8 m sea additional setup was assumed to be caused by sea level rise only and excludes the possible setup from wind storms on the North Sea. The probability mentioned is due to the scenario and model uncertainty and not the annual probability as it could be interpreted from the perspective of storms. If it were an annual probabilty, the probability of the co-occurrence of a river flood with a storm should be taken into account, which we did not do. We expect to clarify with the following adjustment to the manuscript: “Sea level rise (dh) was implemented as a 1.8 m additional setup of the downstream water level (dh1.8) for 2100. We did not take additional increase in water levels into account from storms on the North Sea. We chose a rise of 1.8 as a high-end projection based on two probabilistic studies that included scenario and model uncertainty: Le Bars et al. (2017) reported a median rise of 1.84 m (95% confidence interval = 2.92 m), which included the possibility of Antarctic ice sheet collapse (DeConto et al. 2016) and De Winter et al. (2017) reported a 2.5% exceedance probability for dh = 1.5 m for the North Sea.”

R3: This also brings up my final comment: in reality the objectives would probably be subjected to additional weighting, as is typical in multi-criteria structured decision making. Explicit weighting would change the scores and tradeoffs. If flood-risk reduction is the dominant objective, it would be weighted accordingly. In the US it is often the case that ecological objectives get weighted much higher than some of the socio-economic objectives because of special treatment for threatened or endangered species.

Reply: In practice, the results of our study could function as a reference for more detailed designs. The final weighting of of interventions would indeed be based on preferences, lobbying, and legal status of protected species etc. In the methods we stated: “No attempt was made to select a single optimal measure by means of minimizing an objective function, because such techniques require weighing factors for the four aspects and these are currently unknown. The weighing factors can also change quickly due to changing public opinions and political will.” Based on this comment though, we added the following to the discussion: “In the final stages of intervention planning, additional weighing of interventions is required in practice using a multi-criteria analysis. Changing the weights will alter the trade-offs between the evaluation parameters. For example, the single objective of flood hazard reduction would rank embankment raising, floodplain lowering, side channels and roughness smoothing as top priorities, whereas conversion to natural grassland would be favoured from the river restoration perspective of protecting threatened and endangered species.”