Thank you very much for the kind words and constructive review. Below we address the open issues. We have formatted our responses in blue text to better distinguish them from the comments. The formatted PDF response file is provided as a Supplemental File, and plain text is provided here.

Yours sincerely,

Tony Wong and Alexander Bakker (for the author team)

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Comment #1

The submitted manuscript, "BRICK, a simple, accessible, and transparent model framework for climate and regional sea-level projections," presents a good example for other geophysical modelers to follow. The authors describe a modular and transparent framework for projecting changes in regional sea level under different uncertain future scenarios, and they also give an example of how the framework can be used to plug in other modules enabling decision support based on the climate model outputs. While the flood risk management example is simplistic, it is illustrative of the potential for the BRICK model to be leveraged in a variety of useful applications. Further, the authors make a nice case for the value and importance of open-source, transparent, and simple modeling.

I believe the paper is of high quality and nearly ready for publication, but I have included a number of suggested revisions or comments on certain elements as outlined below:

Reply

Thank you very much. We address the suggestions below.

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Comment #2

P2, line 25 – difficulty can be due to a variety of things: closed platforms, reliance on databases or other inputs that are less portable than source code, etc.

Reply

We agree that it is important to stress that good coding practice is not the sole requisite for reproducibility. In response, we added the statement of:

"Studies based on simple, mechanistically-motivated models have the potential to be transparent and reproducible when presented in open platforms and when the underlying data are readily available. Yet, although there ..."

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Comment #3

P3, line 26 – Is that intended to be conservative, rather than "underconfident"? Worth explaining underconfident about what, exactly.

Reply

Conservative estimates and underconfidence are certainly related. In our view conservative estimates are deliberately risk-adverse (e.g. by using wide uncertainty ranges) whereas underconfidence refers to the tendency to have more outcomes within the estimated probabilistic uncertainty range than expected.

We add the following short explanation: "…, e.g. by applying conservative estimates in the sense of being risk-averse"

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Comment #4/5

I am unqualified to comment on the fundamental dynamics described in section 3. However, this section provides what appears to be an appropriate level of detail, and the components are based in reputable sources and representations from other well vetted models.

P14, 1-5 – Ability to easily recalibrate model in future with new data and/or methods is a very nice feature that should provide more longevity to the model

Reply

That is what we aimed for. Yet, we hope that the accessibility and flexibility will help others and ourselves to test alternative model choices and assumptions, as well as data.

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Comment #6

P18, 21-22 – "With respect to dike heightening, the expected investments are a linearly increasing function": this is not strictly accurate, as written, and should be explained more clearly. Jonkman (2009) makes a reasonable assumption that construction costs are proportional to the length of levee being constructed or upgraded, but the resulting calculations appear to show that investment costs are linear with respect to the log of the return period of level of protection provided. This is also a bit different than what readers might reasonably interpret the highlighted phrase to mean. Raw material costs when upgrading levees scale with the square of the levee height, because when raising the height, the base must also be widened.

Reply

The focus of this manuscript is especially the transparency, the accessibility and flexibility of the BRICK framework. The simple approximation of Jonkman et al. (together with its extremely clear description) is designed to fit this purpose. The reviewer is, of course, absolutely correct that our description should be as clear as possible too. Besides, the description contained a small error. We rephrased it as follows:
"In this simplified model, the investment costs only depend on dike heightening and are approximated by linear interpolation between data points provided by Jonkman et al. (and linear extrapolation for dike heightenings outside this range)."

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Comment #7/8

P18, 27 – what does the "exponential flood frequency constant" represent? Is this related to the amount the probability of flooding is reduced per meter of increased dike height?

P18, 27-28 – What factors are rolled up into the net discount rate? Jonkman (2009) assumes a real interest rate, net of inflation, and then makes further reductions for economic growth and changes in the yearly probability of flooding due to sea level rise. This is perhaps a small point because the discount rate is treated as uncertain, but if the intent is to follow Jonkman, it should be noted that the flood probability due to SLR is now endogenized in the BRICK analysis, rather than being an exogenous factor for Jonkman.

Reply

This is a great point, and in the revised manuscript we elaborate on details of the parameters of the flood risk module. Specifically, we now have added two separate paragraphs that provide a more detailed explanation about the uncertain parameters, including their assumed sampling distributions. We point to specific textual examples in our response to Comment #9, as those changes address both points.

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Comment #9

P18, 30-31 – How were the plausible ranges for each of these parameters chosen? Some of the choices seem a bit odd, such as assuming that the top end of the range for the investment cost uncertainty is 1. Why was the particular mean probability of flooding chosen? I acknowledge that this is not particularly important, given the illustrative nature of this simplified example, but some additional explanation of the experimental setup would be helpful to put it on par with the level of thoroughness given to previous sections.

Reply

We have clarified the choices for these parameter ranges in the revised text. A summary of these motivations has been added to the revised manuscript's text, and is included below. This new text also includes a more thorough description of the flood risk parameters (addressing the reviewer's comments #7/8 above). We acknowledge that some of the ranges are somewhat ad hoc. They are meant, of course, to serve as a demonstration of model capability and not to inform on-the-ground decisions.

"The uncertain parameters considered in this cost-benefit analysis include the initial flood frequency with no heightening (y^{-1}); the exponential flood frequency constant (m^{-1}); the value of goods protected by the dike ring (billion US dollars); the net discount rate (%); the uncertainty in investment costs (a unitless multiplicative factor); and the land subsidence rate ($m\ y^{-1}$). The central estimates for the exponential flood frequency constant (alpha) and the initial flood frequency with 0 heightening (p0) are taken from Van Dantzig (1956). The exponential flood frequency constant relates the increase in flood probability that results from an increase in sea level relative to the dike height. We make the assumption that this factor should scale (to first order) relatively well from Dutch case considered by Van Dantzig (1956) to the test case of New Orleans considered presently. The initial flood frequency with 0 heightening (p0) may not translate directly between these two cases, but highlights our intent for this experiment to serve as an example of future applications of the BRICK model to inform decision analyses. The admittedly ad hoc distributions assumed for alpha and p0 were selected to sample tightly around the central estimates from Jonkman et al. (2009). A more detailed treatment of this risk management problem would include using methods from extreme value theory to address the risks posed by storm surges (Coles et al. 2001).

The investment uncertainty considered in the sensitivity tests of Jonkman et al. (2009) included a base case, 50% lower, and 100% higher than the base case. We use this range for the investment uncertainty, applied as a multiplicative factor ranging from 0.5 to 2. The range for the value of good protected by the dike ring is taken from Jonkman et al (2009), where the lower bound is the lowest estimate of value of goods protected by the three dike rings considered in that work (US$5 billion), and the upper bound is the estimated combined value protected by all three dike rings (US$30 billion). The net discount rate range is centered at 4%, the estimate from Jonkman et al (2009) accounting for inflation and interest rate. Those authors' net discount rate is decreased to 2% due to economic growth (1%) and increased flooding probability due to sea-level rise (1%). Our demonstrative example endogenizes the effects of sea-level rise and accounts for parametric uncertainty in the value of good protected by the dike ring. Hence, we center our range for the net discount rate at 4% but allow for +/-2% uncertain range. The rate of land subsidence is based on the estimates of Dixon et al. (2006), with mean 5.6 mm/y and standard deviation 2.5 mm/y. We transform this to a log-normal distribution to disallow negative rates of land subsidence.

We sample the uncertainty in these parameters via Latin hypercube, where the population size is given by the number of sea-level rise ensemble members that are present (573 for the control BRICK ensemble). …" (proceeds as in original manuscript)

Additional notes:

We also have revised the notation in Table 1 to more clearly convey how the I_{unc} factor translates to uncertainty in the investment costs for dike heightening. We changed the notation to I_{unc} in [0.5, 2], which is more precisely conveys 50% lower to 100% higher.

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Comment #10

4.4.2. – Given the local example, it would be nice to say something about the sea level rise encountered by Louisiana here. Otherwise, this section seems a bit out of place. In the previous section, it is stated that results (for the decision-analysis module) are presented for RCP 8.5, but then this section dives into sea level rise elsewhere in the world and also in RCPs 2.6 and 4.5. The authors may wish to consider i) removing this section, ii) making more clear that the sea level rise serves as an input into the flood risk module and integrating it better into the rest of the section 4.4 discussion, or iii) moving this section back to 4.3 or elsewhere, then mentioning the local sea level rise in Louisiana as part of 4.4.1, in relation to being an input to the flood risk module.

Reply

We have revised the first sentence of Section 4.4.2 in order to make clear how the maps of regional sea level changes are related to the flood risk experiment of Section 4.4:

"In order to link projections of sea-level rise to problems of local coastal adaptation, regional sea level is projected to 2100 under the climate change scenarios of RCP2.6, 4.5, and 8.5 (Fig. 4)."

We have also revised the transition from 4.4.2 to 4.4.3 by modifying first sentence of Section 4.4.3:

"We now focus on the regional sea-level projections for the gridcell containing New Orleans, Louisiana (29° 57′ N, 90° 4′ W) under RCP8.5 (Fig. 4c), to demonstrate the use of these sea-level projections in a common local flood risk management example."