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Author Comments - Response to Reviewers

Nicholas Depsky et al.

Author comment on "DSCIM-Coastal v1.1: an open-source modeling platform for global impacts of sea level rise" by Nicholas Depsky et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-198-AC1, 2022

Italicized Text: *Reviewer comments*

Plain Text: Author comments

The following discussion represents the authors' consideration and response to the comments provided by the two reviewers of our manuscript. Overall, we were pleased to find the feedback to be positive and helpful. The majority of comments pertained to clarifications in wording, descriptions of methods or structure of the article, with a number of suggestions for improved visualizations in our figures. We are implementing the vast majority of these suggestions in our final revised submission. We are also in agreement with reviewer comments pertaining to potential input data improvements and are working to incorporate these updates of relevant inputs where necessary. Our responses to each comment are laid out in order of reviewer, with our responses provided in line. For each comment, our responses consist of a conversational reply to the reviewer, as well as a reference to the updated text or material in the revised manuscript itself that reflects the changes we made in response. A small number of comments provided by the two reviewers dealt with similar topics, for which our response text is similar.

In addition to addressing the comments of the reviewers, several of the input data sources used in our paper have received minor or major version updates since we initially began the work. Due to the modular nature of the DSCIM-Coastal platform, these are easily ingested to generate updated results. In our revised manuscript and analytical outputs (the DSCIM-Coastal platform), we will incorporate the following updates:

- CoastalDEM has been updated from v1.1 to v2.1, which is discussed in several of the responses below (see Kulp and Strauss, 2021 for details on this update)
- SRTM15+ (a low resolution global DEM used to augment our elevation information in areas where CoastalDEM does not exist) has been updated from v2.3 to v2.4
- Global Mangrove Watch 2016 has been updated from v2 to v3
- The UN World Population Prospects has been updated from the 2019 to the 2022 version
- The Asian Development Bank Key Indicators dataset has been updated from the 2021 version to the 2022 version
- The GADM dataset has been updated from v3.6 to v4.1

- The OECD Regional Statistics dataset has been updated from the 2021 version to the 2022 version

R1 Comments and Responses

Moderate Comments

Following the FAIR principles (Line 144) is excellent, but it is not demonstrated that the DSCIM platform is FAIR. One possibility could be to explain how the criteria of Force11 have been implemented.

We have included an additional footnote in the manuscript to better itemize that ways in which the DSCIM-Coastal platform abides by the FAIR principles according to the criteria detailed on Force11.org. This footnote can be found in the updated manuscript in **Section 1.3**:

"Both components have been developed in accordance with FAIR Guiding Principles for scientific data management (Wilkinson et al., 2016) that are intended to improve the Findability, Accessibility, Interoperability, and Reuse of scientific data. [FOOTNOTE]:

These data and modeling components abide by the FAIR criterion as specified by The Future of Research Communications and e-Scholarship (FORCE11). Specifically, they are i) Findable via unique and persistent identifiers, with these identifiers specified in component metadata and indexed in a searchable resource (Zenodo, Github); ii) Accessible in that they are retrievable via these identifiers and are open, free and universally implementable; iii) Interpretable through the use of a formal, accessible, shared and broadly applicable language/vocabulary (manuscript and metadata in standard English and code in Python) and the inclusion of appropriate references to other data where necessary (e.g. input data sources); and iv) Reusable by specifying accurate and relevant attributes, applying an accessible data usage license and complying with coastal modeling community standards of language and data/code provision (Force11.org)."

It is not clear how flooding is modeled. Is this a bathtub approach? And in this case, given that the Bathtub approach generally highly overestimates the flooded area during events characterized by overflow, can this lead to overestimates of damage and adaptation costs? Similarly, how would the consideration of erosion and salinization of estuaries and coastal aquifers increase costs? I would not expect a detailed quantified value here but may be a note in the discussion.

Our localized sea level rise modeling approach accounts for the significant heterogeneity in sea level rise and extreme sea levels experienced across segments, such that it is not a bathtub approach at the global scale; however, within each coastal segment, flooding is indeed modeled using a bathtub approach. We agree with the reviewer that implementing a more sophisticated hydrodynamic flood model for each segment could mitigate potential high-bias in flooding extents under the bathtub approach by accounting for land-surface roughness and flood deceleration dynamics. However, given the detailed input data and computational resources such an approach would require at the global scale, such an undertaking was deemed out of scope for this study.

It is also important to point out that our main findings concern the magnitude of climate-induced SLR-related damages. This refers to the difference between damages incurred under the various future emissions scenarios under climate change (CC) and those incurred under the no-climate change (no-CC) baseline scenario, both of which employ the same flood modeling approach. Therefore, general bias associated with our flood modeling (e.g. overestimation of flood extents) would be present in both the CC and no-CC trajectories and thus would be somewhat corrected when the CC vs. no-CC difference is calculated for each future CC scenario.

To add clarity to this component of our methodology, we have added the following sentences in Sec 2.5.5 (Sea Level Rise):

“...each region of the world experiences the median projected RSLR for that scenario. In physical terms, this means that the inundation and flooding experienced by a given segment is represented as a simple bathtub model, conditional on that segment’s RSLR and ESL heights. However, it is important to note that, using the methods detailed in Kopp et al. 2014, 2017 and Muis et al. 2020, the heterogeneity of RSLR and ESL values across different segment locations is preserved in our approach.”

We have also included the following text to address the potential limitations in our flooding approach and with regard to the other processes highlighted in your review (e.g. erosion, salinization) in Sec 3.3 (Model Limitations and Planned Improvements):

“Additionally, improving our simplified "bathtub" modeling approach for estimating flood extents at each coastal segment could potentially yield better approximations of flooded areas from storm surge events. Bathtub flood models may overestimate flooding extents compared to hydrodynamic models due to their simplification of physical processes related to the deceleration of overland flood flows (Bootsma et al., 2022, Vousdoukas et al., 2016}. However, given the computational and input data requirements to construct and execute more complex hydrodynamic flood models at the global scale for each segment, time step and future scenario, such an approach was deemed out of scope for this study. Also, when isolating the climate change-induced coastal costs, we difference the costs of a no-climate change baseline scenario that uses the same local bathtub flood model. This differencing also serves as a bias correction step, partially mitigating any over-estimates of flooding damages potentially introduced by the bathtub approach, though some high or low bias may still be present in the final results. It should be noted that the raw, total cost values for scenarios that have not been differenced by the no-climate change baseline will reflect any bias associated with the bathtub flood model, which make the climate-change-only costs more robust to these potential errors.

Other geophysical dynamics associated with SLR inundation and related flooding, such as coastal erosion and salinization of aquifers and estuaries, are not currently included in our approach. Efforts to better predict such phenomena at a global scale and, crucially, value their economic impacts would permit their inclusion in future modeling efforts.”

The resolution of the coastal segments is obviously a key issue: there is a trade-off between the computation time and the ability to remain realistic in terms of coastal extreme sea levels modeling. The study demonstrates well that reducing the number of points was possible because some regions representing a very small fraction of the total aggregated costs are simplified (e.g. French Polynesia). However I wonder to what extent the aggregated numbers given for 50km segments can be realistic. Typically, for example, the wave setup contribution to extreme sea levels is lower in harbors than in adjacent beaches (e.g., Lambert et al., 2020). What can be the impact of this simplification on the final results?

We agree that finer-scale modeling of coastal wave setup and ESL dynamics would be advantageous where possible. However, our ability to represent such processes is limited by the granularity of the outputs of the global tide and surge model (CoDEC/GTSM) that generates our ESL values at each segment. These outputs are provided at 50km spacing (10km in Europe, but we upscale to 50km for global consistency), which is often not sufficient for capturing some of the finer-scale inlet/harbor related processes you cite here. The SLIIDERS framework is designed to be adaptable to future improvements in input data. Thus, increased resolution of global ESL estimates could easily be incorporated to better represent these processes in future iterations of the pyCIAM model. We have provided a sentence to this effect in Sec 3.3 (Model Limitations and Potential Improvements):

“Finer-scale wave setup and ESL behavior within complex coastlines at the sub-segment scale could also be useful to capture in future modeling. This would require estimates of ESLs at a much higher spatial resolution than is provided in the CoDEC/GTSM dataset and is therefore currently infeasible given available input data.”

When discussing future socio-economic development, it is unclear why the work of Merkens et al. (2016) who downscaled SSPs in coastal areas is not acknowledged or discussed.

We appreciate the suggestion and have reviewed the downscaled coastal SSPs in the Merkens et al. (2016) paper (M2016). We compared the values in this dataset at the admin-1 level to those used in our current analysis, which scales present-day population distributions (from LANDSCAN) with country-level growth projections. No consistent patterns emerged in the difference between our values and those underlying M2016. A core objective of developing SLIIDERS was to update underlying data sources used in earlier iterations of similar datasets (e.g. DIVA). Our present-day population distribution is derived from 2019 population observations in the LANDSCAN data product and thus contains more recent data than the Global Urban Rural Mapping Project (GRUMP) dataset used in M2016. For this reason, along with the uncertainty and challenges associated with forecasting future subnational migration patterns, we chose to use the LANDSCAN data layer, scaled over time by country-level population projections coming from the Institute for Applied Systems Analysis (IIASA), the official purveyor of the widely-used SSP database.

We believe that a benefit of creating our open-source platform is that it allows all interested future users to explore the differences in model results that would result from replacing our SSP population projections and distributions with those from M2016 or any other comparable data set. We now mention this possibility and cite M2016 in Sec 2.2.5 (Extreme Sea Level Capital Damage):

"However, should one wish to model within-country migration due to considerations such as SSP-consistent coastal urbanization and migration flows (e.g. Jones and O'Neill 2016, Merkens et al., 2016), such changes can be accommodated by updating the appropriate variables in the SLIIDERS input dataset."

Figure 2 displays non-zero costs in states or regions which are not connected to the sea: e.g., Arizona in the US, Auvergne-Rhône-Alpes in France. In addition, it displays apparently zero costs in regions known to be highly exposed to sea-level rise (e.g. Occitanie in Mediterranean France, characterized by low lying urbanized sandy lidos). Can this be explained?

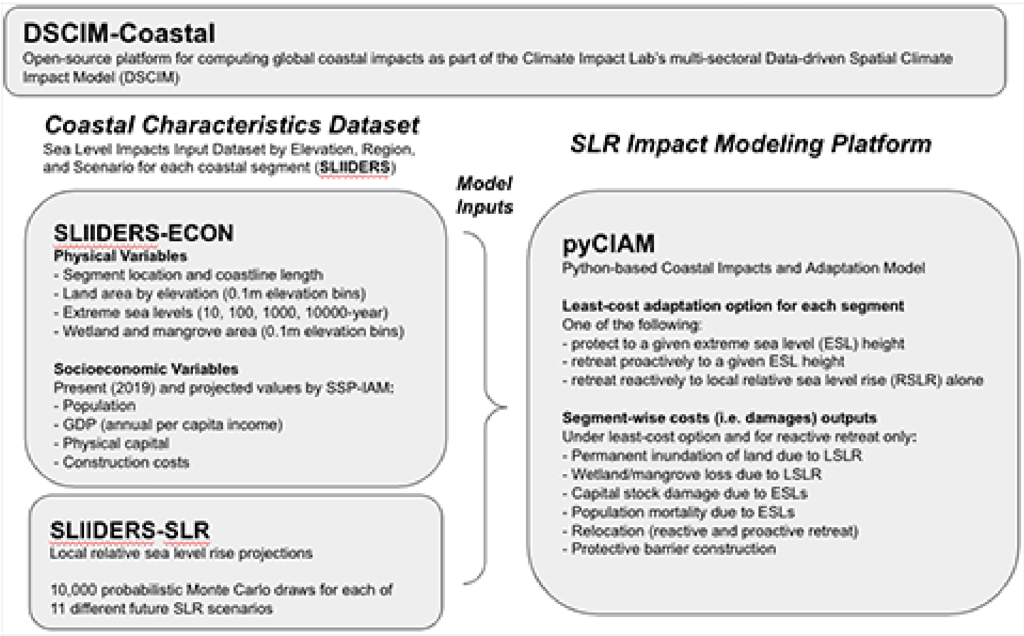
Non-zero costs in regions that are not connected to the sea occur because there are low-lying areas inland that are not hydraulically connected to the ocean. Under present modeling approaches, the "local bathtub" flooding approach used at each segment and described earlier in this response would flood these areas. We have used the HydroSHEDS dataset to classify endorheic basins and eliminate some of these regions. We have also incorporated the protection of levees in the U.S. and the Netherlands and have manually masked certain known low-lying inland areas. However, such a process is not complete and may miss some cases such as those you reference. We have now incorporated a hydraulic connectivity model to improve upon the bathtub flooding approach and mask areas that are not exposed to coastal extreme sea levels due to a lack of coastal connectivity. We have added this explanation to Section 2.5.3 - Elevation. We have also updated our topographic elevation dataset to utilize CoastalDEM v2.1 in place of v1.1 in order to integrate the significant improvements represented in that data update, as detailed in Kulp and Strauss (2021).

The presence of zero costs in regions known to be highly exposed to SLR was due to a plotting error and was not reflected in the numerical model outputs. This has been corrected in the manuscript's figures, and we are thankful for the careful review that uncovered this issue.

A diagram displaying the different components of the model and main principles would help the reader.

We agree and have included a new figure (FIG01) that replaces Table 1 at the end of Sec

1.3. We believe the figure better illustrates the hierarchy and structure of the various data and modeling components of the study:



Minor comments

Line 114: please note that the IPCC glossary gives slightly different definitions for these terms (e.g., costs are a measure of some impacts)

We have updated this line as follows:

“CIAM differentiated between six types of costs (i.e. “damages")...”

Line 205: maybe add ‘are characterized as follows…”

We have updated this line as follows:

“Costs estimated by pyCIAM are categorized in the same manner as described in (Diaz, 2016) and are characterized as follows:”

Line 239-250: this is a reminder of the Method of Diaz, 2016, but another method is used here. Is this section necessary here?

This section is used to describe two key issues with the original (Diaz 2016) implementation that motivate the different approach that we take. Thus, we feel that it is a relevant passage. However, to improve readability, we have now removed the detailed replication of equations presented in Diaz 2016 and replaced them with a reference to the equations in the original paper.

Line 256-257: I don't understand which population redistribution is meant here: is this due to SSPs or due to additional coastal migrations, e.g., in response to SLR?

This refers specifically to any non-SLR induced, within-segment redistribution across elevation slices. For example, if a segment's population was to migrate toward or away from low-lying regions of the segment, that would affect the segment's fractional losses incurred for a given extreme sea level. Such a case would be realized if we were to use the subnational SSP projections of Merkens 2016 or Jones and O'Neill 2016, as described earlier. We have updated this part of the text to add clarity as follows:

"However, should one wish to model within-country migration due to considerations such as SSP-consistent coastal urbanization and migration flows (e.g. Jones and O'Neill 2016, Merkens et al., 2016), such changes can be accommodated by updating the appropriate variables in the SLIIDERS input dataset."

Line 267-269: I understand that you have accounted for mortality as described in the Diaz paper, and not in its implementation. Is this correct? Eventually, the sentence could be made a bit clearer. Further, is this the population exposed to flooding (not ESL) which is considered to compute mortality?

Yes, that is correct. We corrected the storm (ESL-driven) mortality calculations to match the specification in the Diaz 2016 paper, and not the implementation discovered in the original CIAM code, which differed from the description in the paper. This mortality calculation pertains solely to mortality due to extreme sea levels (ESL) during storms and other high-water events. There is no mortality assumed with the permanent, gradual inundation due solely to local sea level rise. We have updated the text to add clarity:

“In the implementation of Diaz (2016) (i.e. the original CIAM code), both the mortality assumption and the depth-damage function appear to have been used in conjunction, although the text of the Diaz 2016 paper states that the depth-damage function should only be used in the estimation of capital stock damage, not mortality. We therefore corrected this discrepancy in our implementation of ESL-driven mortality estimates in pyCIAM.”

Line 294: It is excellent to model this issue, but should this issue be framed only in terms of benefits and costs, or could there be also a general aversion to relocation motivated e.g. by optimism biases and attachment to specific location?

We agree. Optimism-bias and emotional attachment to a specific location are some of the motivations for relocation aversion we are referring to when we say “non-market costs of relocation”. In line 284, when we state “non-pecuniary emotional consequences”, we are implying precisely these sorts of aversions or attachments that likely impede relocation relative to what would be expected if one was to only consider simple market-driven cost-benefit equilibria. Assigning dollar values (i.e. 5x annual local GDP per capita) to these factors in our model is simply a way of estimating this non-market relocation resistance in units of cost (dollars) that are harmonized with the other cost types being modeled. This implies that the costs we estimate reflect the total welfare impacts, inclusive of the monetized value of these preferences.

Line 326: I don’t understand what is meant by data fidelity here. Can you rephrase or explain?

We updated this line for clarity:

“We note that this approach is facilitated by the resolution of the input data represented in SLIIDERS and of the model outputs provided by pyCIAM.”

Line 340: Table 2 shows these times apply for an Apple MacBook Pro laptop with a 2.8 GHz Quad-Core Intel Core i7 processor and 16GB of RAM, but Table 2 is referred to much later. May be add it here to help the reader.

Agreed. Table 2 (now Table 1) should appear right after this sentence at the end of Sec. 2.4. This was a LaTeX compiling bug and has been corrected in the updated manuscript.

Line 358-359: the note on resolution between parenthesis is based on the resolution of the initial dataset, but it can be clearer by just giving the grid cell or segment size.

These 1:50m and 1:10m resolutions are indeed based on the Natural Earth coastline input dataset used, but those are also the same spatial vector layers that we use to represent our model's coastlines when performing geospatial computations, such as calculating coastline lengths for each segment. Therefore, those inputs were not altered or converted to raster grids, meaning their native resolution specifications still apply in our modeling context. The '1:50m' and '1:10m' labels indicate the scale of the physical vector layers. In other words, in the 1:10m scale product, variations in coastline contours up to 10m should be reflected by the vector polylines in this product. One can think of these scale values as a form of smoothing of coastline shapes, where the value indicates the maximum length of coastline across which smoothing can occur. As such, 1:50m coastlines are more smoothed (less granular) than the 1:10m product. We have added a footnote in the manuscript at this location clarifying coastline scale values, in Section 2.5.1:

"The '1:50m' and '1:10m' labels indicate the scale of the physical vector layers, which can also be thought of as the maximum length of coastline across which simplification of complex coastlines into straight line segments can occur. 1:50m coastlines are more smoothed (less granular) than the 1:10m product."

Section 2.5.3 can be misunderstood: as I understand it, the CoastalDEM is not independent of SRTM, but it improves SRTM by learning coastal features in the US where there is Lidar and improving in other regions. Can this be clarified?

We agree and have added some text to clarify this point as follows:

"In addition to higher resolution elevation estimates compared to the 30-arc-second GLOBE DEM used in Diaz (2016), CoastalDEM significantly reduces bias found in the widely used SRTM DEM. The impact of this bias reduction is substantial, implying that roughly three times the amount of present day population resides below projected high tide levels under low-emissions sea level rise scenarios by 2100 globally compared to previous SRTM-based estimates (Kulp and Strauss, 2019)."

Section 2.7.4 : what protection standard is assumed beyond the US and the Netherlands? Is this 1:100?

No protection standard is assumed beyond the US and Netherlands due to the lack of comprehensive data on such infrastructure globally. We are aware that this is a limitation and have thus discussed this in the Model Limitations and Planned Improvements section (Sec. 3.3).

In section 3.3 – Model limitations: I suggest to include here some aspects raised above, such as the 50km coastal segmentation and the flood modelling approach (bathtub?). In addition, the analysis is purely economic, but there are other aspects that can be included, such as the feasibility of adaptation measures in 2070/2100, when resources and energy availability and costs may be completely different than today (See SPM of the WGII). Another limitation could be the consideration of salinization and erosion, as rightly noted in the introduction.

Thank you for these suggestions. We have incorporated such discussion as detailed in our responses to the related items in the 'Moderate Comments' above. We added a sentence about the feasibility of adaptation measures as well, as follows:

"Additionally, the potential changing feasibility of both adaptation and accommodation measures in future decades, due to potential factors unrelated to climate change, like shifting supply chain and/or labor market dynamics, are not currently represented. These may prove to be relevant to society's capacity to effectively adapt in the future."

In Figure 5 and in the similar figures in annex, it is difficult to see the difference between the IAM and CIAM models because triangles, circles and squares are superimposed. Can this be improved? One possibility could be to display them on different column?

Yes, we agree with this comment and have implemented some slight x-wise jittering of the scatter plots in this and similar figures to improve interpretability of the different marker types.

Data and codes

I have visited the codes without trying to extract all data run them on my computers (I am limited in terms of computer performance, data storage and time to run models!). It seems to me that the dataset lacks a bit of explanations (why two files, the smallest one seems to contain a lot of files of 0kB). Furthermore, it is not completely clear from the abstract why the framework and the codes are in two different deposits.

Thank you for the feedback. The goal with providing code and data in multiple deposits was to (a) separate source code from data products, since different users may be interested in one or the other of these objects and (b) emphasize the separate, and modular nature of DSCIM-Coastal, which involves a data aggregation and harmonization effort to create a global dataset of coastal socioeconomic and physical characteristics (SLIIDERS) and a coastal sea level rise impacts model (pyCIAM) that ingests those datasets as input. However, for the purposes of someone wishing to access all of the code and data associated with the manuscript, we agree that navigating multiple Zenodo deposits can be unnecessarily confusing. Based on this feedback we will migrate all code and data to a single Zenodo deposit, organized accordingly to separate code from data and SLIIDERS from pyCIAM. This will represent a snapshot of the source code and resultant data at the time of manuscript publication; however, we will maintain the SLIIDERS source code and the pyCIAM source code as separate github repositories because they will receive separate, continued development after the manuscript is published.

R2 Comments and Responses

Major comments

First, I was trying to get the model running on my PC, but did not manage to install the model. I believe that a user description would be helpful (e.g. how to install the model; how to run the model e.g. the command one should use to start the model, how can updated data be incorporated/run by the model and so on). As the main advancement of the model is the transparent and open-source provision of the data and code, the authors should try to make it as easy as possible for other scientists to get the model running. Due to the difficulties in setting up the model – I am currently not able to evaluate the code and data provided within the manuscript. Further, I was wondering if the intention of the platform is also to regularly update data if new e.g. elevation data become available (Line 36/37 and Line 101/102). If so - how are the authors planning to update the data?

Thank you for the feedback. A user description is provided in the README files associated with constructing the SLIIDERS dataset (in the SLIIDERS repository) and running the pyCIAM model (in the pyCIAM repository), including step-by-step instructions. In the current version, the execution of the model occurs in a Jupyter notebook environment, rather than via a command line interface, with an example provided in the "run-pyCIAM-slrquantiles.ipynb" notebook. To make this clearer, we will (a) move these step-by-step instructions to the top of the README file and (b) provide additional comments to make the execution of the notebook more intuitive. In addition, we will provide a Docker file containing an environment with all necessary packages to execute the model and replicate our results.

We have designed the DSCIM-Coastal platform to encourage regular updating of both the input data sources used to create the SLIIDERS dataset and the coastal climate impact modeling software (pyCIAM). We hope for this to evolve into a community effort and, as such, are not intending to take sole ownership of these tasks. However, as detailed at the top of this response, we have ingested updates to numerous input data sources that have changed to date since we initially began development of this model. This was performed by updating the notebooks associated with data acquisition, cleaning, and processing within the SLIIDERS code repository. To better describe this process to community members that may wish to contribute and/or update SLIIDERS or pyCIAM, we will add instructions on how to do so in the associated READMEs.

Second, I wonder if it is always necessary to point out how Diaz (2016) calculates certain parameters. It may be less confusing for readers to focus mainly on the new model version. I think it is very important to mention what is new in the paper, but I find it a bit confusing and overwhelming in parts.

We agree with this concern and have removed some of the more convoluted and potentially misleading or overwhelming portions of the Diaz (2016) methods summaries. Specifically we removed equations (1) and (2) from 'Section 2.2.5 - Extreme Sea Level Capital Damage' that reflected Diaz (2016)'s implementation to improve readability. We instead simply provide a reference to the equations in the original paper.

Third, it is not clear from the manuscript how coastal flooding is calculated. I guess the authors used a bathtub method, but this should be clarified in the manuscript.

Yes, it is a bathtub model for each individual segment, with the flooding levels at those segments determined by our localized estimates of sea level rise and storm surge. This comment is similar to one provided by R1, for which we provided the following response and updated language in the text.

Our localized sea level rise modeling approach accounts for the significant heterogeneity in sea level rise and extreme sea levels experienced across segments, such that it is not a bathtub approach at the global scale; however, within each coastal segment, flooding is indeed modeled using a bathtub approach. We acknowledge that implementing a more sophisticated hydrodynamic flood model for each segment could mitigate potential high-bias in flooding extents under the bathtub approach by accounting for land-surface roughness and flood deceleration dynamics. However, given the detailed input data and computational resources such an approach would require at the global scale, such an undertaking was deemed out of scope for this study.

It is also important to point out that our main findings concern the magnitude of climate-induced SLR-related damages. This refers to the difference between damages incurred under the various future emissions scenarios under climate change (CC) and those incurred under the no-climate change (no-CC) baseline scenario, both of which employ the same flood modeling approach. Therefore, general bias associated with our flood modeling (e.g. overestimation of flood extents) would be present in both the CC and no-CC trajectories and thus would be somewhat corrected when the CC vs. no-CC difference is calculated for each future CC scenario.

To add clarity to this component of our methodology, we have added the following sentences in Sec 2.5.5 (Sea Level Rise):

“...each region of the world experiences the median projected RSLR for that scenario. In physical terms, this means that the inundation and flooding experienced by a given segment is represented as a simple bathtub model, conditional on that segment’s RSLR and ESL heights. However, it is important to note that, using the methods detailed in Kopp et al. 2014, 2017 and Muis et al. 2020, the heterogeneity of RSLR and ESL values across different segment locations is preserved in our approach.”

We have also included the following text to address the potential limitations in our flooding approach and with regard to the other processes highlighted in your review (e.g. erosion, salinization) in Sec 3.3 (Model Limitations and Planned Improvements):

“Additionally, improving our simplified "bathtub" modeling approach for estimating flood extents at each coastal segment could potentially yield better approximations of flooded areas from storm surge events. Bathtub flood models may overestimate flooding extents compared to hydrodynamic models due to their simplification of physical processes related to the deceleration of overland flood flows (Bootsma et al., 2022, Vousdoukas et al., 2016}. However, given the computational and input data requirements to construct and execute more complex hydrodynamic flood models at the global scale for each segment, time step and future scenario, such an approach was deemed out of scope for this study. Also, when isolating the climate change-induced coastal costs, we difference the costs of a no-climate change baseline scenario that uses the same local bathtub flood model. This differencing also serves as a bias correction step, partially mitigating any over-estimates of flooding damages potentially introduced by the bathtub approach, though some high or low bias may still be present in the final results. It should be noted that the raw, total cost values for scenarios that have not been differenced by the no-climate change baseline will reflect any bias associated with the bathtub flood model, which make the climate-change-only costs more robust to these potential errors."

Minor comments

It would be helpful to have a graph that gives an overview of the different modules within the model and input data.

We agree and have included a new figure (FIG01) that replaces Table 1 at the end of Sec 1.3. We believe the figure better illustrates the hierarchy and structure of the various data and modeling components of the study.

Line 72: What is meant by high-resolution here?

We altered this sentence to now read as:

"Several past studies employed global coastal impact models to estimate future damages from SLR and ESLs under various trajectories of global GHG emissions, socioeconomic scenarios, and adaptation pathways for thousands of sub-national coastline segments."

Line 86: The DIVA modelling framework is published under the INSERT LICENSE. The code and data are available in the (protected) DIVA repository: https://gitlab.com/daniel.lincke.globalclimateforum.org/diva

We are aware of this landing page but have found that as of 28 Oct 2022 there is not actually a license listed, instead just the placeholder 'INSERT LICENSE' text. We checked the provided gitlab link's functionality both during the writing of the original manuscript (12 Apr 2022) as well as during this revision process (28 Oct 2022) and have not found it to be functional. We communicated this issue with the repository's creator but did not receive a response. There is a similar (unprotected) repository named “diva_published” located at https://gitlab.com/daniel.lincke.globalclimateforum.org/diva_published, which contains a variety of csv files and shapefiles that look to be input data to the creation of the original DIVA dataset. However, it does not contain any of the code used to create the DIVA dataset.

Line 138: Subheading title: ‘The Data-driven Spatial Climate Impact Model Coastal Impacts Architecture’ Is there a little mistake? It reads a bit strange to me.

Agreed. We have renamed this subheading ‘This Study: The Data-driven Spatial Climate Impact Model - Coastal Impacts’

Line 153: A table of the datasets that are included in Sliders and the references would be helpful for readers

Two such tables are already included in the appendix as tables C1 (physical variables) and C2 (socioeconomic variables). We opted against including these in the main text due to their size.

Line 155: Would it make sense to also include the new IPCC SLR scenarios (AR6)?

We agree that including these latest SLR scenarios would be a worthwhile inclusion. The bulk of our model creation and manuscript writing was done prior to the release of the finalized AR6 scenarios, but now that they are available we are working to integrate them into this platform. We are striving to implement this in the manuscript prior to its final submission but if that proves infeasible, we will provide an updated release of the modeling platform with these scenarios included shortly thereafter. This ability to continuously update relevant input datasets as they are improved over time is one of the primary motivating factors for designing the SLIIDERS/pyCIAM platform in a modular, open-source fashion.

Line 188: How are people spatially relocated to an unaffected area within the model?

They are relocated to what is assumed to be a safe inland area not in danger of future sea level rise or coastal storm surge impacts - effectively removed from the model moving forward. We have added some clarity to this point in Section 2.1 as follows:

"Reactive Retreat: When a portion of land falls below MSL, all people and mobile capital are relocated to an unaffected, inland region away from the coast that is not in danger of future impacts from SLR or ESLs, and immobile capital is abandoned...

Proactive Retreat: All people and mobile capital below a certain retreat height are assumed to be relocated to a safe, inland region, and immobile capital below that height is abandoned."

Line 255: What are the baseline costs?

In this context, baseline costs refers to the initial cost of constructing the protection. We have updated this sentence to read as follows:

"As in Diaz (2016), maintenance costs are assumed to be 2% of the initial construction cost."

Line 265: 'VSL-valued' – What does VSL stand for?

We have altered the original text to provide clarity for this term, where VSL indicated 'value of a statistical life', as well as a citation for the paper from which our VSL framework was derived, the new sentence reads as follows:

"The expectation of annual costs of mortality occurring due to ESL events based where death equivalents are represented in dollar terms using a value of a statistical life (VSL) framework, as employed in Diaz (2016)."

Line 267: Small typo: double 'of'

Corrected.

Line 275: Some more detail on how the height of adaptation measures is influenced/calculated would be interesting here.

We slightly altered this sentence to add clarity as follows:

"The maximum heights of projected RSLR at each segment during a given planning period in turn influence the heights at which protect or retreat adaptation options are employed. For segments that just adapt via reactive retreat, the height of retreat would exactly match this projected RSLR, while segments employing 10, 100, 1000, 10000-year retreat or protect actions would consider the heights of these ESLs atop this projected RSLR baseline for that planning period."

Line 327: DIVA assumes a homogenous population per elevation increment and segment

We re-worded this sentence to more clearly articulate the shortcoming of DIVA being its assumption of uniform population and capital densities across entire segment areas, while our approach leverages improved input datasets to disaggregate these densities by segment-elevation bin:

"We note that this approach is facilitated by the resolution of the input data represented in SLIIDERS and of the model outputs provided by pyCIAM. The DIVA inputs used in Diaz (2016) assume that population and capital density are homogeneously distributed throughout each segment, and are non-varying by elevation."

Section '2.5 Physical Model Inputs in SLIDERS': It might be useful to have this section first to understand the data model/structure better.

While we appreciate this suggestion to improve readability of this section, we feel that the current structure is necessary to maintain in order to preserve the logical flow of our work. Given that our work is heavily based on a pre-existing modeling platform's (CIAM) structure, we feel it is important to provide the context of that model's structure early on in this section, as the characteristics and limitations of CIAM were largely what compelled the kinds of updates and improvements we chose to make to the physical and socioeconomic input data. Therefore we feel that detailing those updates subsequently to the model structure and related sections is the most sensible.

Line 350ff: Why did you use 50km (and not e.g. 20km)? What is the reasoning behind it? If one thinks about adaptation units, a homogeneous segmentation might not be the best solution as parameters as population density, coastal geomorphology changes quite rapidly. For a global application, one must make simplifications, but I wonder how the authors come up with a 50km segmentation.

We used 50km segmentation because that was the coastal spacing between the majority of the global CoDEC Global Tide and Surge Model (GTSM) dataset (Muis et al. 2020), from which we drew our surge height values. Given the lack of modeled surge height at higher spacing and the computational constraints associated with growing the total number of coastal segments to be optimized for adaptation action, we deemed the 50km value to be suitable for this study.

Section Elevation: Might be good to include several DEMs (in the long run) as the model results are very sensitive to variations in input data (e.g. see Hinkel et al. 2021)

We appreciate this comment and may consider this in future updates to the model moving forward. However, provisioning multiple versions of the SLIIDERS dataset and associated pyCIAM results using different DEM products was deemed out of the scope of the present study. This is partly due to the added complexity associated with this approach but mostly due to the fact that there is an increasing consensus that the CoastalDEM products represent the best data product for modeling coastal topography that is presently available. Along these lines, in this revision we will improve our DEM approach by replacing CoastalDEM v1.1 with the newly-developed CoastalDEM v2.1 (Kulp and Strauss 2021), which significantly improves upon the CoastalDEM v1.1 model. Other members of the coastal impacts research community may utilize this framework to explore the impact of using alternative DEMs (or other input data sources, such as population or physical capital). Given the large number of datasets used as inputs in the creation of SLIIDERS, assessing the sensitivity to alternative sources is outside the scope of this model description paper; however, it may be the subject of future analyses.

Section 2.74: the FLOPROS database by Scussolini et al., 2016 is a first collection of current protection levels. Might be good to mention here.

Thank you for pointing out this study. We were aware of this work and view it as a potentially compelling avenue for further research, and interested users could potentially integrate the modeled protection levels from FLOPROS in an altered configuration of SLIIDERS and pyCIAM. Given the fact that these protection levels were estimated via a prediction model as opposed to representing ground-truthed data, we opted to exclude them from the initial implementation of our modelling platform but acknowledge their potential utility in lines 678-680, as follows:

"Other studies develop statistical models to empirically ground such relationships (Scussolini et al., 2016), and these have been incorporated in other global coastal adaptation models (Tiggeloven et al., 2020) and could be evaluated for use in future versions of pyCIAM."

Line 545-545: This part is not clear to me. Could you please support the argument with references and explain your correction/improvement more precisely?

Thank you for this comment. There are no additional references aside from the USACE National Levee Database (NLDB) that are relevant but we do agree that some additional clarity as to the implications of this approach could be better articulated. We therefore provide some additional language in this section about the magnitude of protected areas in the US (~12-13% of coastal population and asset value are deemed protected from the NLDB excluded from the model). We also added similar discussion of the limitations of this assumption and potential ways to improve it in the future in Section 3.3 - Model Limitations and Potential Improvements.

Figure 6, lowest panel: What is shown by the grey color (local sea level)?

This indicates that the retreat height undertaken by segments that adopt only 'reactive retreat' as their adaptation strategy is equal simply to the local sea level at each segment. In other words, each of those segments retreats exactly in line with local sea level rise at that segment to avoid permanent inundation, but no higher.

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