

Response to Referees (CC1, CC2, RC1, RC2, RC3) for "ESD Ideas: Reliable Adaptation Policies to Sea-Level Rise Require Incorporating Complexity in Economic Models"

We thank all referees for their constructive and critical comments, which will substantially improve the clarity, rigor, and impact of our Ideas piece.

CC1 (Paul Pukite).

CC1 comment 1. "What is first needed is a useful model of mean-sea-level variation as measured at the various coastal stations listed at PSMSL.org. This is the link between the relative simplicity of conventional daily tidal analysis and the large scale impact of ENSO. Once this is done, the discrimination and isolation of the increasing trend can be observed.

<https://github.com/pukpr/GEM-LTE>":

Response: Thank you for highlighting the importance of empirical tidal analysis and ENSO-scale variability as a foundation for detecting SLR trends. We agree that robust geophysical baselines are essential, and our proposed complex systems based approaches are designed to integrate and analyse the economic effects of empirical SLR time series and improved physical models. We will improve the manuscript with your suggestion and if possible cite the repository within the limitations suggested by the Idea format.

CC2 (Judy Lawrence).

CC2 comment 1. "This ESD Idea is appealing and reflects the use of Agent Based modeling in a SLR context elsewhere. For example. there is a body of work in New Zealand in this context to reflect the concerns with current use of CBA in conditions of uncertainty and changing climate risk. Further research on agent based modeling in tandem with neural networks is underway. If the authors also have examples of how these approaches have been used it would enhance the utility of the idea. A collaboration would be nice.

Allison AEF, Lawrence JH, Stephens SA, Kwakkel JH, Singh SK, Blackett P and Stroombergen A (2024) Planning for wastewater infrastructure adaptation under deep uncertainty. *Front. Clim.* 6:1355446. doi: 10.3389/fclim.2024.1355446

Allison, A., Stephens, A., Blackett, P., Lawrence, J., Dickson, M., Matthews, Y. 2023. Simulating the Impacts of an Applied Dynamic Adaptive Pathways Plan Using an Agent-Based Model: A Tauranga City, New Zealand, Case Study. *J. Mar. Sci. Eng.* 2023, 11, x. <https://doi.org/10.3390/xxxx>

Stroombergen, A and Lawrence, J (2022) A novel illustration of real options analysis to address the problem of probabilities under deep uncertainty and changing climate risk. *Climate Risk Management* 38, 2022, 100458 <https://doi.org/10.1016/j.crm.2022.100458>

Response: Thanks for finding the idea appealing and suggesting articles and collaborations, we surely will try finding a way to cite these interesting articles within the limitations of the idea format and follow-up for discussing collaborations: some of us also have research on agent based modeling in tandem with methods similar and evolving from neural networks, an online workshop could be a way to collaborate. Furthermore we shall reflect this suggestion in the context of the piece, because the state-of-the-art boundary outlined is alive and the piece revision could be improved by reflecting the generous suggestions received.

CC2 comment 2. “Two improvements in this paper would help

1. The title could be improved . Suggest "Reliable sea level rise adaptation policies require complexity to be incorporated into economic models" or something shorter.”

Response: Thank you; we will revise the title to "Reliable Sea-Level Rise Adaptation Policies Require Complexity to Be Incorporated into Economic Models." as suggested.

CC2 comment 3. “2. I like the Sankey diagram but the graphics in diagram B needs each community to be more clearly denoted. The Figure Panel descriptions need to be set out better as its a long read. Could this perhaps be incorporated into text in the main paper?”

Response: Thanks. We will clarify in the text how communities are indicated and what are the signs indicating belonging to each community. We will also try incorporating some elements of the Figure Panel into the text of the main paper.

RC1 (Robert Kopp).

RC1 comment 1. “This format is challenging to review. The article reads like an condensed excerpt from the motivation section of a funding proposal.”

Response: Thanks. While we can confirm this piece is original and it was created with this sole purpose, we will restructure the manuscript away from a proposal-like flow and into an Ideas piece flow.

RC1 comment 2. “The first paragraph provides general background on sea-level rise; the second paragraph (uncited) presents well-known problems with classical theory-based climate impact economic models; the third and fourth paragraph critique classical approaches, particularly CGE models; the 5th-7th paragraph present ABMs as solutions; the 8th paragraph and the Figure 1 throw in a whole bunch of other solutions that are not going to

be intelligible to someone not already versed in the literature; and a final sentence concludes.”

Response: Thanks for the summary. We think that new ideas (here ABMs, network science, and other combinations of methods as discussed solutions) do not automatically reach all audiences, and that can cause some authors not to be familiar with the solutions we suggest. While the reference list includes a few key papers for the curious reader to follow-up, we will further enrich this list with selected references as suggested, for which we express our gratitude.

RC1 comment 3. “The discussion of more classical economic approaches does not touch on approaches more innovative than traditional CGE (e.g., spatial integrated-assessment approaches reviewed by Desmet and Rossi-Hansberg, 2026, <https://doi.org/10.1093/jeg/lbaf049>).”

Response: We would like to express our gratitude because it is a valuable reference for a number of reasons:

- 1) it’s the state-of-the-art in the field of innovative CGEs,
- 2) it highlights the value of including spatially-explicitness and physical capital into a global IAM, which are some preliminary steps that signal towards our contribution, and
- 3) it could help the economics reader to find the coordinates of our Ideas piece in the current landscape, thus surely we will include it.

We will explicitly cite the paper as an important step forward in spatial IAMs, and then clarify that our proposed complexity approach goes beyond CGE entirely by capturing out-of-equilibrium dynamics and agent heterogeneity, thereby complementing their advances.

RC1 comment 4. “Agent-based modeling in climate policy analysis is not new (see Savin et al., 2023, <https://doi.org/10.1002/wcc.811>, for a review). Nor is it new to the sea-level rise context; a Web of Science search for "sea level" AND "agent-based" finds 54 papers.”

Response: Thanks, surely our contribution extends beyond the idea of ABM for policy. Now we further clarify our idea in the paper so that the reader sees value in coupling the complex dynamics we suggest, which clearly go beyond ABM for policy.

Furthermore, we will scope those "sea level" AND "agent-based" papers to try finding innovations that could refine the boundary of the state-of-the-art and include them in the paper. In any case, it’s still clear that the gap we identified and the intellectual space for our Idea contribution remains substantial and deserving attention.

RC1 comment 5. “The paper would thus benefit from a greater focus on what novel approach to ABM the authors are proposing, and how their proposed approach overcomes the barriers that have limited ABM uptake in the economic and policy analysis communities. I

recognize that this is challenging to do in the space provided, but such discussion would greatly increase the value of the contribution.”

Response: Thanks. We fully agree that refining our contribution towards novel approaches beyond barriers in existing ABMs is important to improve this paper, and are preparing a new draft accordingly.

While we recognise the barriers that have limited ABM uptake, the limitations of this format are a substantial challenge for fully expanding into the topic, so we will compile some key arguments into a limited number of sentences. Thanks for the suggestion.

RC2 (Anonymous Referee).

RC2 comment 1. “This manuscript addresses an important topic: whether prevailing economic approaches to sea-level-rise adaptation omit key dynamics relevant to real decisions. The paper’s diagnostic critique is often persuasive. For example, it argues that existing approaches have important limitations regarding “bounded rationality, learning, risk perception,” “inequality and distributional impacts,” “indirect socio-economic losses,” and “adaptation processes over time.”

Response: Thanks for your generous summary, and for acknowledging that the manuscript addresses an important topic.

RC2 comment 2. “However, the manuscript is substantially stronger in diagnosing weaknesses in existing approaches than in substantiating the proposed alternatives. The abstract makes a very strong claim, stating that “complexity science applied to sea-level rise uniquely enhances adaptation policies.” Later, the paper concludes that “capturing interconnected nonlinear dynamics across domains and scales will improve assessments” while “facilitating reliable decision-making.” Those are consequential claims, but the body of the paper does not provide a comparative argument, empirical demonstration, or implementation-oriented assessment sufficient to support them at that level of confidence.”

Response: Thanks. These claims emerge from the logic detailed below in our answer to *RC2 Comment 10*, which can be summarised as: If complexity science can uniquely help produce more reliable cost estimates, and reliable estimates can guide reliable policies, then complexity science can uniquely enhance reliable policies.

As mentioned below in our answer to the comments generously provided by the anonymous referee (see *RC2 Comment 5*), this is an Idea format requiring us to present innovative and well-founded scientific ideas in a concise way that have not been comprehensively explored (see https://www.earth-system-dynamics.net/about/manuscript_types.html).

On the one hand, an empirical demonstration, or implementation-oriented assessment, as suggested, could fall outside the scope of the Idea format as described above. On the other

hand, while a comparative argument as suggested would be a useful way to approach this piece, our logic for presenting this Idea is clear and provides solid grounds for it, and includes a substantial number of comparative arguments between existing approaches and innovative approaches (see *RC2 comment 10*).

We are grateful for the input, after considering it, the logic is still clear and the Idea presented stands up to these criticisms, please be invited to see it in our answers below:

RC2 comment 3. “Major comments

1. The paper’s prescriptive case is underdeveloped relative to its diagnostic case.”

Response: Thanks. The paper suggests applying complexity science to economic assessments of the impacts of sea-level rise and elaborates on what that means for the science-policy interface. The idea presented is larger in scope than a single static recipe for a single case; please note that coming up with a prescription for every possible relevant implementation would have its own problems, the interest can be instead in rather specific mechanisms that in specific contexts make specific patterns relevant (cf. Bonabeau,2002). Thus, our prescriptions are limited to an idea that can be further developed into key specifics developed from our perhaps broader prescription. Detailed explanations appear below in relation to each of the comments, for which the authors are very grateful.

RC2 comment 4. “The manuscript gives a compact but credible account of limitations in current CBA- and CGE-based approaches. It states, for example, that CBA can “omit the tail risk and the effect of extreme events,” and that CGE models may lead to “substantial underestimation of SLR damages and losses.” It also notes that local studies often ignore indirect damages, while macroeconomic assessments treat them only in aggregate. These are all legitimate concerns.”

Response: Thanks for acknowledging that the concerns expressed are legitimate.

RC2 comment 5. “But when the manuscript turns to solutions, the argument becomes much thinner. The transition sentence is telling: “Here we present solutions for enhancing models of the economics of SLR, accompanied with visuals reflecting our opinion.” That phrasing signals that what follows is primarily a perspective, not a demonstrated comparative assessment. The manuscript then presents ABM, network analysis, and adaptive pathways as promising directions, but mostly at the level of possibility rather than demonstrated decision advantage.”

Response: Thanks. This is an Idea format, which requires us to present innovative and well-founded scientific ideas in a concise way that have not been comprehensively explored (see https://www.earth-system-dynamics.net/about/manuscript_types.html). This format can be seen primarily as a perspective, as you mention.

A demonstrated comparative assessment as suggested would fit another format. Some such demonstrated comparative assessments already exist in the literature (see Hommes & Poledna, 2026).

We agree that improving the idea presented beyond a possibility is important, and will substantiate the solutions explained with a more balanced, comparative argument linking specific model features to decision advantages.

RC2 comment 6. “That gap matters because the relevant question is not whether richer models can represent more mechanisms in principle. It is whether they improve decision support in practice, and under what conditions. The paper does not adequately engage with why simpler frameworks remain dominant in applied policy settings, including tractability, auditability, communicability, calibration burdens, and institutional familiarity. As written, the manuscript argues more for the promise of complexity methods than for their demonstrated superiority in adaptation decision-making.”

Response: Thanks. We agree that an analysis for exploring tractability, auditability, communicability, calibration burdens, and institutional familiarity is a relevant idea, and agree that a full analysis could help comparing a range of methods in adaptation decision-making.

While we cannot perform a full analysis on tractability, auditability, communicability, calibration burdens, and institutional familiarity because it would go beyond our present focus and the current format, the matter is highly relevant. We will acknowledge that simpler frameworks remain dominant due to tractability and institutional familiarity, while clarifying that our proposed approach addresses this by modularizing complex models (e.g., ABM with standardized input data, mechanisms, and output formats) to preserve transparency and facilitate stepwise calibration. We will further reflect on what practices led complexity-based models to win over other approaches in other fields despite issues with tractability, auditability, communicability, calibration burdens, and institutional familiarity (e.g., see for Covid-19 the paper by Elsenbroich & Badham, 2023).

RC2 comment 7. “2. Figure 1 is presented with the visual language of formal analysis but without a documented method.”

Response: Thanks. We will surely add a methodological depiction sufficient to reproduce the figure while constrained within the Ideas format. Detailed explanations appear below in relation to specific comments.

RC2 comment 8. “This is, in my view, the clearest weakness of the manuscript. The paper says the visuals are “reflecting our opinion,” yet Figure 1 and its caption go well beyond a simple conceptual schematic. The caption states that “some issues are highly connected,” that “the size of nodes indicates the centrality,” and that node colours indicate “membership to communities of strongly interlinked items.” It also interprets substantive communities and identifies certain nodes as especially central.”

Response: Thanks. In line with other comments on this relevant matter (see *RC3 Comment 2*), we will further clarify the figure adding a method description fully sufficient to make the figure reproducible and to understand the aspects highlighted.

RC2 comment 9. “That presentation strongly implies a formal analytic procedure. But the manuscript does not explain how issues and solutions were identified, how links were defined, whether weights were binary or continuous, how “centrality” was measured, or how “communities” were detected. Without that information, the figure is not reproducible and cannot be meaningfully evaluated. In its current form, it risks reading less like evidence and more like prior beliefs translated into the aesthetics of network analysis. At minimum, the paper would need to specify the source material, coding rules, graph construction procedure, weighting scheme, and community-detection method, or else present the figure more plainly as a qualitative conceptual diagram.”

Response: Thanks. We will add a brief method description that makes the figure fully reproducible and clarifies the formal analytic procedure as requested, detailing the requested source material, coding rules, graph construction procedure, weighting scheme, and community-detection method.

RC2 comment 10. “3. The manuscript does not establish that model complexity is the binding constraint on better adaptation policy.”

Response: Thank you for this suggestion, which will greatly improve the value of the manuscript. The logic that establishes that model complexity is a major binding constraint will be clarified in the paper along the lines of these premises:

If complexity science provides advantages that yield more reliable cost estimates, and such reliable estimates can help guide sound adaptation policies, then complexity science can meaningfully guide reliable sea-level rise adaptation policies.

Furthermore, detailed explanations appear below in relation to specific comments.

RC2 comment 11. “The paper argues that current models “might misinform responses,” that some existing methods underestimate risk, and that more complex approaches would mitigate “risk underestimation and maladaptation.” That is plausible. But the key causal step is not demonstrated: the manuscript does not show that additional model complexity is the factor most likely to improve real policy outcomes, as opposed to other binding constraints such as institutional capacity, financing, governance fragmentation, or limited local data.”

Response: Thanks for acknowledging the plausibility of some of the premises in the manuscript. Barriers to adaptation emerging from local governance capacity, finance for adaptation or other aspects, and the causality and relative importance of them in improving policy outcomes is a very interesting research direction, and we would like to refer the reviewer to other contributions from some of the authors (e.g., Thomas et al., 2021). With

this paper in perspective, it seems clear that demonstrating the causal step or selecting the most likely factor that the reviewer is requesting would require other formats and methods.

While such detailed analysis would lead to another type of paper, we see how important the suggestion is and how it relates to our focus, and we will acknowledge these real-world barriers to uptake and reflect on how they interact with key elements within our focus.

RC2 comment 12. “Relatedly, the paper moves too quickly from greater representational richness to greater policy usefulness. Those are not the same thing. In real decision contexts, transparency, interpretability, and communicability are often central to uptake. More complex models may improve realism on some dimensions while reducing usability or legitimacy on others. A paper advocating complexity-based approaches for policy support should address that tradeoff directly. As written, the manuscript assumes rather than demonstrates that more realistic models necessarily yield more reliable adaptation decisions.”

Response: Thanks. We agree that greater representational richness and greater policy usefulness are not the same thing, indeed the article has no claim implying both are the same.

Our point is a bit more humble and limited to the logic stated above: If complexity science can help produce more reliable cost estimates, and reliable estimates can guide reliable policies, then complexity science can guide reliable policies.

We will further clarify the text so that the reader do not see greater representational richness and greater policy usefulness being implicitly referred as the same.

Investigating policy uptake from complexity science applied to the science policy interface, would require a completely different set of methods and formats. We take this idea seriously for a future contribution because our present focus is on economic assessment.

We agree that consideration of the mentioned trade-off is important and we will explicitly address the tradeoff between model realism and usability.

RC2 comment 13. “Conclusion

This manuscript raises worthwhile concerns and could become a useful perspective piece. But in its current form it does not support its strongest claims. Its critique of existing frameworks is more convincing than its case for the superiority or practical policy relevance of the alternatives it recommends. Figure 1 is especially problematic because it adopts the appearance of formal analysis while the manuscript describes the visuals as “reflecting our opinion” and does not document the method needed to interpret the network structure it reports. As an Ideas piece, the manuscript need not provide a full comparative assessment. However, it does need to offer a well-founded and proportionate argument for why the proposed complexity-based approaches would improve adaptation decision-making in

practice. In its current form, that case remains suggestive rather than adequately substantiated. For that reason, I do not recommend publication in its present form.”

Response: We are very grateful for the detailed analysis, thanks for concluding that the manuscript could become a useful perspective piece.

Above in the responses to *RC2 comments* 1 to 13 we attempted our best to provide substantial responses to each point stated.

We will improve the explanation of how complexity-based approaches can improve adaptation decision-making in practice.

RC3 (Judy Lawrence).

RC3 comment 1 “The paper postulates that economic complexity is required in models to provide reliable policies for SLR decisions. While I don't doubt this as a proposition, the paper only goes so far as to traverse the problem space but doesn't address how this might be done except superficially. While it suggests Agent Based models can be augmented to reflect economic costs and benefits this is not a new idea. I am familiar with at least a few research outputs that do this in published journals such as.

Allison, A., Stephens, A., Blackett, P., Lawrence, J., Dickson, M., Matthews, Y. 2023. Simulating the Impacts of an Applied Dynamic Adaptive Pathways Plan Using an Agent-Based Model: A Tauranga City, New Zealand, Case Study. *J. Mar. Sci. Eng.* 2023, 11, x. <https://doi.org/10.3390/xxxxx>

Stroombergen, A and Lawrence, J (2022) A novel illustration of real options analysis to address the problem of probabilities under deep uncertainty and changing climate risk. *Climate Risk Management* 38, 2022, 100458 <https://doi.org/10.1016/j.crm.2022.100458>

Accordingly the paper is an unfinished idea that needs more depth of analysis and wider literature sourced.”

Response: Thanks, we will look for new "sea level" AND "agent-based" papers that could help refine the piece towards a deeper level of analysis, and provide more details about how precisely incorporating complexity in economic models can provide reliable policies for SLR decisions.

We will incorporate these indicated sources and furthermore explore more literature to substantiate these points, as mentioned above in *RC1 Comment 4*.

RC3 comment 2

“The figures also need further consideration as communicable graphics. The second panel in Figure 1 is very hard to follow and could form the basis of a simplified version of it with fewer interconnecting lines.”

Response: Thanks. We will improve the figure's communicability by emphasizing the signs and colors that indicate each community and by reducing visual noise: the links will be given lower prominence using a nuanced gray scale. To further reduce visual clutter, we will introduce incremental transparency, making the network much more readable. Additionally, we will change the font, revise the legend, make the colors more distinct, and add the communities to the legend.

RC3 comment 3

“Accordingly I reject it in its current form.”

Response: Thanks for your consideration, we respectfully request the opportunity to revise the manuscript according to these and the suggestions of other referees.

We hope the improvements we will make in the figure and manuscript, which will include a renewed scoping of the literature and improvements in how the figures communicate to the audience in graphics and text, provide more positive grounds.

References.

- Bonabeau, E. (2002). Agent-based modeling: Methods and techniques for simulating human systems. *Proceedings of the national academy of sciences*, 99(suppl_3), 7280-7287.
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- Hommel, C., & Poledna, S. (2026). Forecasting economic crises: The great recession, the sovereign debt crisis, and covid-19 in the euro area. *Economic Modelling*, 107497.
- Thomas, A., Theokritoff, E., Lesnikowski, A., Reckien, D., Jagannathan, K., ... & Bowen, K. (2021). Global evidence of constraints and limits to human adaptation. *Regional environmental change*, 21(3), 85.