Peer Review of the Predictive Model Technical Reports:

Louisiana's Comprehensive Master Plan for a Sustainable Coast

December 14, 2012

THE WATER INSTITUTE
OF THE GULF

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Executive Summary

Background on the Models

An independent peer-review was coordinated for the predictive models used in Louisiana's Comprehensive Master Plan for a Sustainable Coast (herein the 2012 Coastal Master Plan or the master plan). The peer-review panel provided quality assurance and guidance for improving the technical documentation and future model development. The master plan was informed by one of the largest collective coastal modeling efforts to date in Louisiana. Funding was provided by the Coastal Protection and Restoration Authority (CPRA) of Louisiana to assemble a team of experts to carry out this work. The modeling approach involved over 60 topical experts who, through a coordinated endeavor, created a suite of interactive predictive models. The overall approach included seven 'primary' linked models (Figure 1) that predicted change in the nature of Louisiana's coastal system under both future conditions without additional restoration and risk reduction projects and as a result of project implementation. Some models provided input data for other models, whereas other models provided output data which were directly used to support estimations of how far individual projects could go toward achieving the master plan objectives.

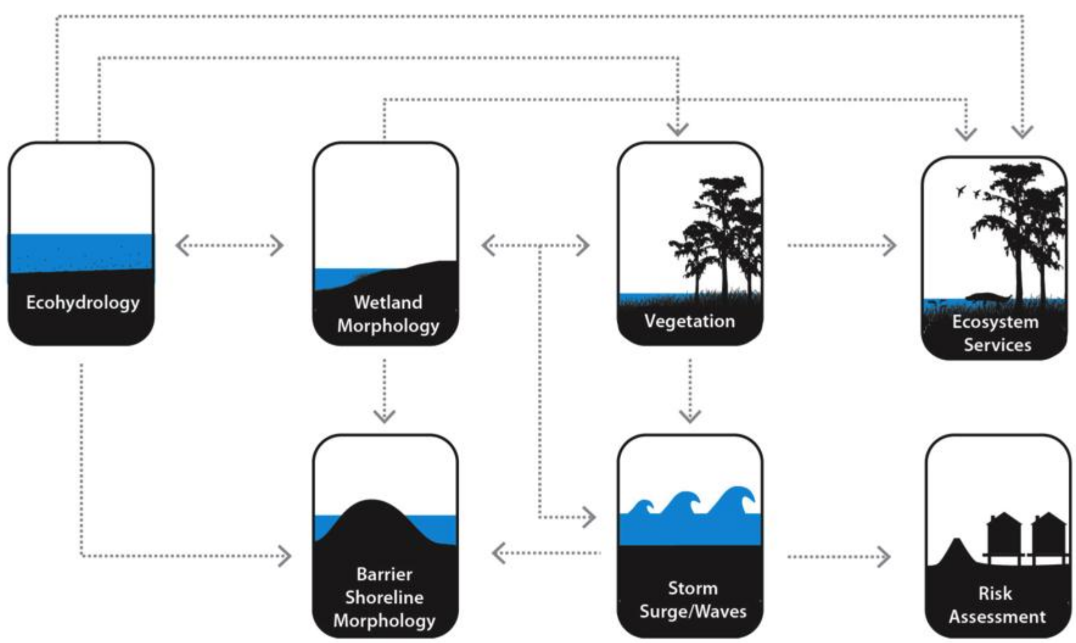

Figure 1. Modeling in a systems context as utilized by the Coastal Protection and Restoration Authority of Louisiana during the formulation of the 2012 Coastal Master Plan.

Simulations were carried out on a coast-wide scale, using a 50-year planning horizon, with projects assessed by the following models: Eco-Hydrology, Barrier Shoreline Morphology, Wetland Morphology, Vegetation, Ecosystem Services, Storm Surge and Wave, and Risk Assessment. Key model output was post-processed for input to a planning tool. CPRA used the model outputs and the products from the planning tool to inform the development of the master plan.

This report focuses on the independent peer review of the predictive model reports which was coordinated by The Water Institute of the Gulf (herein The Water Institute).
For more information on the technical aspects of the models or for information on how they were utilized during the master plan effort, please refer to the appendices of the 2012 Coastal Master Plan (online: http://www.coastalmasterplan.louisiana.gov/2012-master-plan/master-plan-appendices).

Why Peer Review?

The importance of peer review is widely recognized as a means of validating technical products by engaging expert peers, which in turn helps to build credibility. By enlisting topical experts to take a critical look at the modeling documentation, technical assumptions, and model limitations, the review process ensures the modeling teams receive guidance for formalizing and finalizing their technical documentation as well as advice on planning next steps for model development and improvement. Lastly, peer review demonstrates that CPRA has proactively sought out input and review guidance from national and international experts.

Peer Review Process

CPRA contracted with The Water Institute to coordinate this peer review process. As such, The Water Institute recruited and selected three anonymous peer reviewers and one peer review editor for the following predictive model technical reports:

- Eco-Hydrology
- Wetland Morphology
- Barrier Shoreline Morphology
- Vegetation
- Nitrogen Uptake (one component of the Ecosystem Services suite of models in Figure 1)
- Storm Surge and Waves
- Risk Assessment
- Model Uncertainty Analysis

The peer review was intended to ensure quality in technical reporting and documentation, as well as to inform and guide next steps for model development and/or improvements. Reviewers were asked to focus their reviews around the following general themes:

- Does the documentation clearly and adequately reflect the modeling process?
- Is this type of modeling appropriate for large-scale (entire Louisiana coast), long-term (50 years) coastal planning efforts?
- Are the technical assumptions / equations acceptable?
- Are there any fundamental flaws that should be revised for future coastal planning efforts?

Electronic comment templates were created by The Water Institute and provided to the reviewers to catalog their comments on a report line-by-line basis. All reviewer comments were reviewed by The Water Institute, and they were transferred to the modeling teams. In situations where it was deemed necessary, guidance from review editors was solicited and provided to the modeling teams together with the reviewer comments.

Modeling teams used the electronic review templates to log their responses to the reviewer comments, and they modified their reports as appropriate using track changes. The review editors received copies of the reviewer comments, model team responses, and the revised model reports. They were asked to review these documents and draft a summary of the overall theme of the reviewer comments, assess whether the report authors responded appropriately to reviewer comments, make notes of overall strong points about the modeling application, and recommend any modifications or additions that could enhance this type of modeling effort (i.e., modeling for coastal planning in Louisiana). Editors were not asked to provide technical comments on the report content per se, rather, they were asked to provide a high-level, broad summary of the reviewers' comments and author responses.

Peer Review Editors

Bilal Ayyub, University of Maryland

Jeff Cornwell, University of Maryland

Chris Craft, University of Indiana

Robert Dean, University of Florida

Casey Dietrich, University of Texas

Bill Nuttle, Eco-Hydrology

Steve Pennings, University of Houston

The following documentation is a compilation of summarized review editor reports; editor reports were revised to highlight the review process and recommended next steps for model development for use in coastal Louisiana planning efforts. These comments and recommendations have been documented and are being considered by CPRA as models are further refined and developed for use in the 2017 Master Plan.

Peer Reviewers

Brian Beckage, University of Vermont	Robert Muir-Wood, RMS-London
Laura Brandt, US Fish & Wildlife Service	Vincent Neary, Oak Ridge National Laboratory
Don Cahoon, US Geological Survey	Chris Paola, University of Minnesota
Jim Chen, Louisiana State University	Jay Ratcliff, US Army Corps of Engineers
Scott Hagen, University of Central Florida	Julie Rosati, Independent Reviewer
Patrick Inglett, University of Florida	Martha Sutula, Southern CA Coastal Water Research Project
Upmanu Lall, Columbia University	Jeremy Testa, University of Maryland
Ed Link, University of Maryland	Arnold van der Valk, Iowa State University
Igor Linkov, US Army Corps of Engineers	Mike Waldon, US Fish & Wildlife Service (ret.)
Daniel P. Loucks, Cornell University	Ty Wamsley, Independent Reviewer
Frank Marshall, Frank Marshall Engineering	Ping Wang, University of South Florida
Jim Morris, University of South Carolina	Jeff Wozniak, Sam Houston State University

Peer Review Schedule

- **May 29, 2012** The Water Institute began contacting potential reviewers and editors to solicit interest; scopes of work and copies of the model reports were provided; responses were requested by June 7.

- **June 11, 2012** The Water Institute delivered electronic copies of the model reports to the reviewers.

- **July 6, 2012** Reviewers submitted completed comment templates to The Water Institute.
  - The Water Institute reviewed all reviewer comments and enlisted the assistance of review editors where necessary.

- **July 13, 2012** The Water Institute submitted three reviewer comment templates and reports to each modeling-team lead.
  - Any initial guidance provided by editors was also transferred to the modeling-team lead.

- **August 8, 2012** Modeling-team leads submitted their responses to reviewer comments in the electronic comment templates, as well as their revised model reports to The Water Institute.
  - Reports were revised using 'track-changes.'

- **August 13, 2012** The Water Institute submitted three sets of reviewer comments, modeling-team lead responses, and revised model reports to the review editors.

- **September 7, 2012** Editors submitted summary reports to The Water Institute.

- **September 19, 2012** The Water Institute submitted a draft peer-review report to CPRA. Note that one report was received by The Water Institute several weeks later than the others and the Editor's Report was thus not received until early November.

- **November 14, 2012** The Water Institute finalized the Peer Review report and submitted it to CPRA for final review.

Eco-Hydrology

Eco-Hydrology - Editor Report

Introduction

Within the context of regional-scale management of the Louisiana coast, the Eco-Hydrology models support the evaluation of management actions based on projected effects on the evolution of the coastal landscape, including changes in the coastal ecosystem and the resources and services it provides. In particular, the Eco-Hydrology models provide an account of the material budgets for fresh water, sediment and nutrients. These directly affect the geomorphology of the coast (i.e., land loss and land building), the structure of wetland communities, and the level of ecosystem services associated with essential coastal habitats. Designed explicitly to support the formulation of the 2012 Coastal Master Plan, the models are used to project future conditions along the coast over a period of 50 years.

The development and application of the Eco-Hydrology models reflect the extraordinary demands of their intended use. Major constraints include limitations on data available to characterize both initial conditions and to support simulations over such an extended period of time and the need to keep runtimes low in order to maintain the utility of the models as a planning tool. In answer to these constraints, the modelers have implemented an aggregated approach that divides the coastal region into three more or less hydrologically distinct sub-regions and exploits experience with prior large-scale modeling efforts. The result is a set of three separate sub-regional models, each presenting different challenges in assembling supporting data, and two different solution schemes (different in the programing language used and in the approach to representing water flow over the landscape).

Based on the reviews summarized here, the Eco-Hydrology models can be judged as a success. The words of Reviewer #2 capture the overall assessment of the three reviewers and the review editor:

1. The documentation is adequately detailed, and […] will provide a firm basis for future work by the current team, or others.
2. The decision to use compartment or link-node modeling was well justified considering the spatial and temporal scales of the problem being addressed.
3. Technical assumptions and equations are well documented and are reasonable. While future work may revise these equations, they have been demonstrated through calibration and validation to represent the variables within the systems. While there is no standard of practice for wetland modeling, most of the modeling presented here uses approaches and equations which have been successfully applied elsewhere.
4. No fundamental flaws are apparent in the modeling scheme used here.

Some shortcomings of the Eco-Hydrology models identified in the peer review point to the need for more regionally-integrated thinking on the way that the models are conceptualized and used. The management solutions to the problems addressed in the master plan involve the reallocation of flows of fresh water, sediment and nutrients over very large spatial scales. Possibly, this is without precedent in the area of comprehensive coastal management, excepting activities narrowly focused on maintaining navigation throughout the region. However, the scale and complexity of the management interventions proposed in the master plan are comparable to what has been done in regional planning for water supply, flood control and, increasingly, the restoration of freshwater ecosystems, such as in the Florida Everglades and in northern California.

What is missing in the present application is the coastal analog to the concept of a watershed. This concept would express a unifying vision of how the Louisiana coastal region functions, or ought to function, as a single, integrated entity. With respect to the water, sediment and nutrient fluxes treated in the Eco-Hydrology models, one might describe the overall functions of the Louisiana coast as 1) maintaining estuarine function (fresh water inflow and resulting patterns of salinity variation), 2) capturing sediment (sedimentation and land building) and 3) reducing nutrient discharge into shallow coastal waters (nutrient uptake and transformation by wetlands). Strictly speaking, lack of an overarching vision for the Louisiana coast is not a deficiency of the Eco-Hydrology models. To the contrary, the continued refinement and application of these models will contribute to the process of articulating a shared vision of what the Louisiana coast is now and what it might become.

Summary of Reviewer Comments

Comments by the reviewers touch on three themes. A complete response to these requires more than simply revising the report document, and so these same themes are echoed in the recommended next steps.

Representation of the Sediment Budget - All three reviewers commented on the mixed use of total suspended solids (TSS) and suspended sediment concentration (SSC) as metrics in calculating sediment flux in the models. Looking beyond the specific points raised by the reviewers, the extent of the reviewer comments on this point recommends rethinking how the sediment flux calculations are currently implemented in the models and presented in the documentation.

Approach to Synthesizing Missing Data - Similarly, all three reviewers asked for clarification on approaches used to assemble the data needed to initialize the models and as input to the long-term simulation calculations. Reviewers requested that more information be given about adjustments made in the bathymetric data to calibrate the models and about procedures used to fill in missing data. More information is needed to justify the procedures used to assemble the data needed to implement 25-year-long simulations out of considerably shorter periods of observed data.

Reporting and Diagnosis of Model Errors - Comparison of model calculations against observed conditions in the section on calibration and validation of the models relies entirely on visual inspection of time-series data. There is little analysis and discussion of the probable causes of the errors. For the most part, model calculations appear to compare quite well with observations. However, the graphs exhibit a few instances in which the discrepancy is large. Reviewer comments requested better representation of the errors and more discussion and analysis of the cause for the instances of large errors.

Author Responses to Reviewer Comments

In addition to comments on the topics just mentioned, the reviews offered a number of comments and questions on points requiring clarification in the report. Authors' responses indicate changes were made to the report in response to these comments.

On the larger issues raised by reviewer's comments, many of these were anticipated by the authors, as indicated in a list of model improvements presented in Section 6 of the report. In particular, the authors anticipated the need to improve the representation of suspended sediment transport and deposition in the models and the need to unify assumptions, approach, and implementation of the models across the three sub-regions.

Recommendations for Future Model Development

Comments generated from the peer review of the Eco-Hydrology report indicate the need to address the following areas in the next phase of model development:

Representation of the Fresh Water, Sediment and Nutrient Budgets - Attention should be given to developing a coast-wide summary of the Eco-Hydrology model calculations. This summary would represent the major fluxes in the fresh water, sediment, and nutrient budgets with sufficient detail to illustrate the effect of large-scale river diversions and similar magnitude management interventions. These summaries might be presented graphically, as a schematic flow diagram, or in table form. The summary fresh water, sediment and nutrient budgets would serve two purposes. First, the summary budgets will represent the overall function of the Louisiana coastal region, as an integrated whole, in terms of the variables simulated by the Eco-Hydrology models. Second, compared with the baseline 'without-project' scenario, the summary budgets will provide a quick overview of the net effect of the management interventions implemented in each modelled scenario.

Approach to Synthesizing Missing Data - Queries for more information about how the input data were assembled point to the need to regard the task of assembling the data required to drive long-term simulations as a task that is distinct from the implementation of the Eco-Hydrology model calculations and deserving of some deliberate forethought. Undoubtedly, the modelers have approached this task with the appropriate deliberation. The principles used to guide the process of synthesizing the input data sets are an important part of the modeling process. However, information on this aspect of the Eco-Hydrology models is not complete in the current model documentation and must be more explicit in the future.

Reporting and Diagnosis of Model Errors - Expand the reporting of model errors to include summary error statistics and narrative discussion of the principal sources of error. The present report includes a section on "Sources of Model Uncertainty," but this is physically remote from the section on model calibration and verification where the model errors are presented (graphically) and no attempt is made to identify the probable cause or causes behind anomalous errors. In the future, it would also be useful to compare the errors achieved by the Eco-Hydrology models for coastal Louisiana with errors achieved for similar parameters in other large-scale coastal simulation modeling exercises, such as for Chesapeake Bay and San Francisco Bay. This comparison will be useful in setting goals and expectations for what can be achieved through continued refinement of the Eco-Hydrology models for coastal Louisiana.

Wetland Morphology

Wetland Morphology - Editor Report

Introduction

This model was developed as a part of the 2012 Coastal Master Plan through the Coastal Protection and Restoration Authority (CPRA) and is designed to compare and prioritize coastal wetland restoration and protection projects in the state. The Wetland Morphology predictive model (herein the Wetland Morphology model) is one of a series of linked models, including the Vegetation, Eco-Hydrology and Barrier Shoreline models, and it utilizes data from other models (for example, suspended sediment concentration is an input from the Eco-Hydrology model and plant community distribution comes from the Vegetation model). Additionally, the pre-compaction relative elevation sub-model (see Figure 6, and equation 1), within the Wetland Morphology model, was used to estimate soil carbon (C) storage under different restoration scenarios.

Three reviewers critiqued the model and submitted comments. The reviewers have extensive experience with modeling marsh sedimentation and vertical accretion. Their expertise varies but their approaches complement each other.

Summary of Reviewer Comments

The following is a summary of the reviewer's responses to the review questions. This is followed by a distillation of their comments to present the (I) Overall theme of the comments, (II) Responsiveness of the report author to make changes, (III) Strong/Weak points of the model and (IV) Recommended modifications to the model.

Does the documentation clearly reflect the modeling process?
The three reviewers agreed that the documentation does a good job of explaining and describing the model. Reviewer #3 said the report does an "excellent job of thoroughly documenting the model as formulated". The reviewer also commented on the "transparent and thorough presentation" such that "anyone wishing to evaluate the results will have access to the 'working parts' of the model." However, reviewer #3 also said that "it does not do as well at explaining the limitations of the model." Reviewer #2 said the documentation reflects the modeling process "reasonably well." Reviewer #2 commented on the writing style and said it needs "polishing in places." Reviewer #1 also offered some specific comments for clarifying the text but agreed with the other reviewers that "adequate information is provided to understand the modeling process."

Is this model appropriate for large-scale, long-term coastal planning?
The reviewers agreed that the model is appropriate for large-scale, long-term coastal planning. Reviewer #1 said "the authors make a strong case that this approach is appropriate." Reviewer #1 liked the use of hierarchical models and said "I can think of no better approach to address this problem." Reviewer #2 commented that "This is a very good start." However, reviewer #2 also said that that there are still a number of shortcomings that were spelled out in detail in their review. Reviewer #3 agreed "in general" that the model was appropriate subject to specific (and general) comments in the review. The *specific* comments of Reviewer #3 are presented under question 4 below.

Are the technical assumptions sound?
Generally, the reviewers felt that the technical assumptions of the model are sound. However, reviewers #2 and #3 pointed out several limitations to the current approach. Reviewer #3 had questions about the sediment redistribution model "whose physical basis is not clear from the report." Reviewer #3 questioned why no new(er) advances to understand spatial patterns of deltaic land building were incorporated into the model. Reviewer #3 suggested that it would be better to select several (competing) algorithms to test against each other. Finally, reviewer #3 commented that the many empirical relationships used in the model "may not be dependable outside the range of conditions for which they are calibrated", a comment that probably is already understood by the model developers.

Similar to reviewer #3, reviewer #2 commented that the model is based primarily on empirical relationships and, in some cases, the rationale for a particular relationship is questionable. The relationship between short- and long-term accretion on page 53 was mentioned by reviewer #2 as an example. The reviewer also questioned the relationships between soil bulk density and plant community type in table 8. In spite of these concerns, reviewer #2 said "the present model is a very good start". Reviewer #1 did not offer detailed comments regarding this question.

Are there any fundamental flaws that should be fixed for future planning efforts?
The reviewers noted several limitations to the model. Reviewer #3 thought the box model approach used is "detached from a broad spectrum of recent and ongoing advances in the way deltas and their wetlands grow and organize themselves." Reviewer #2 would like to see more mechanistic improvements to the soil processes as part of the model, "one that incorporates feedbacks between plants, sediments, tides and sediment transport." Reviewer #1 commented that the "impact of storms is under-represented in the model" and recommended incorporating the stochastic effects of storms.

Theme of Comments

The theme of the reviews was mostly upbeat. They felt that the model, as currently developed, "was a very good start" (Reviewer #2) and thought the documentation adequately and accurately described the modeling process. Reviewer #3 commented on the "transparent and thorough presentation" such that "anyone wishing to evaluate the results will have access to the 'working parts' of the model." Reviewer #3 also mentioned that a "strong feature of the work is its attention to the importance of biotic processes in influencing sediment and wetland dynamics."

The reviewers thought the model could be used for large-scale, long-term coastal long-range planning, subject to several caveats. (1) Reviewer #3 commented on the absence of large-scale processes (i.e. delta formation, pro-gradation, abandonment) in the model and went on to say that the model seems "somewhat detached and insular from a broad spectrum of recent and ongoing advances in understanding the way deltas and their wetlands grow and organize themselves." The reviewer mentioned the Wax Lake Outlet/Delta as an example of research that should be considered. Reviewer #3 also suggested developing a plan for incorporating research advances and high resolution models, as they become available, to the model framework and to communicate more with the worldwide delta research community to gain useful insights.

While commenting that the model was a very good start, Reviewer #2 felt that parts of the model were based on correlative data and might not operate outside of their calibrated boundaries. A case in point is the data describing the bulk density and soil organic C as a function of seven vegetation types (see

Table 8 on page 54). Likewise, the reviewer wanted to see an empirical, not correlative, basis to Figure 22 on page 57, short-term versus long-term accretion.

Author Responses to Reviewer Comments

In general, the author(s) of the report responded positively to the reviewer's suggestions and incorporated them into the revised document.

Reviewer #1 and reviewer #3 wanted to see more incorporation of storm effects. The author(s) agreed that the impact of storms on surface sediment deposition, erosion and surface elevation change is under-represented in the model and in the Eco-Hydrology model. The author(s) added text regarding limitations (pp. 26, 51 and elsewhere) and an approach to overcome the shortcoming (p. 68).

Reviewer #2 was concerned that belowground processes are not modeled. Rather, soil parameters (bulk density, percent soil organic matter) based on plant communities types were used as "threshold" or class variables. The reviewer mentioned this in several places and was concerned about the instantaneous shift in soil processes that will occur when a change in plant community composition (e.g. brackish to saline marsh) occurs. The author(s) explained their rationale for using this method but offer no specific improvements to the model. Reviewer also commented on the correlative nature (not process-driven) of figure 22, short- versus long-term accretion.

Reviewer #2 also was concerned that the LIDAR data used to measure surface elevation over large spatial scales over-estimates it when aboveground biomass is high. The author(s) do not justify/explain anything other than to say that an uncertainty analysis was conducted.

Reviewer #3 was concerned that what is missing from the model is the inclusion of large-scale, delta-based research literature to help guide model development. The author(s) disagreed that the model is detached from the delta-based research literature and offered up a vigorous defense of their work. At the end, the author(s) "admit that we could not incorporate recent morphodynamic deltaic research findings in to our efforts." This is probably out of the scope of the current model but should be pursued as the model evolves over time.

Reviewer #3 thought the documentation should include a broader discussion of limitations of the model. The author(s) state that they added some material to the revised report and to other ongoing manuscripts. A list of Capabilities and Limitations is shown on page 20 but there is no detailed discussion of them. The reviewer also discussed the merits of bringing information from studies of the Wax Lake Delta (an aggrading delta of 100 km^2 since 1980). Here, the author(s) add some text about this on p. 24.

Finally, reviewer #3 would like to see a plan for incorporating advances in high-resolution models into the model framework. The author(s) responded by saying that they are working on a series of spatially explicit models (hydrodynamic and sediment transport, process-driven vegetation productivity, soil nutrient biogeochemical, sediment cohort) to address this issue.

Strengths and Weaknesses of the Model

Strengths

- The use of hierarchical models and extensive spatial (input) data.
- Consideration of biotic processes influencing sediment and wetland dynamics.
- Extensive use of ground-based and remotely sensed data as model inputs.

Weaknesses

- Lack of explicit spatial and temporal consideration of extreme events (storms).
- Not enough incorporation of processed based models, especially for soils and sediments.
- Not enough consideration of larger-scale (deltaic) processes (and communication with this group).
- Not enough discussion of the limitations of the model.
- Lack of a plan for incorporating research advances and high-resolution models into the modeling framework.

Recommendations for Future Model Development

The reviewers offered a number of substantive suggestions to improve the model. For some suggestions, there was a consensus among the reviewers. For others, the suggestion was voiced by a single reviewer. Because of the diverse yet complementary backgrounds, these suggestions should receive serious consideration during future improvements to the model. Based on the reviews, three recommendations are offered to improve the model.

- The correlative data used to describe associations between vegetation/plant communities and soil processes (bulk density, soil organic matter) should be augmented with process-based algorithms. The author(s) are encouraged to review Jim Morris' Marsh Equilibrium Model (Morris 2002, 2012) that describes marsh accretion as a function of physical variables such as depth below mean high water, suspended sediment concentration and (aboveground) plant biomass. While it may not be possible to incorporate this work into the current version of the model, the author(s) should strive to incorporate such physical/mechanistic processes into the next generation.

- The author(s) should strive to better incorporate the effects of extreme events (storms, floods) into the model, especially the stochastic temporal effects as well as the spatial variability in sedimentation that occurs with it. The model incorporates long-term sediment deposition from storms (storms deliver, on average, 1000 g sediment/m^2/year; see p. 51) to the coastal wetlands. The authors can do better in terms of modeling spatial variability in sediment deposition for different coastal habitats. While it is more difficult, temporal variability in storms and floods can be better captured in the model as well, if not in this version, in the next generation.

- The author(s) need to consider larger-scale (deltaic) processes more than is currently done. Because of the large spatial scale needed to model the entire Louisiana coast, it may not be possible to employ a hydrodynamic model such as Delft 3D FLOW. Nevertheless, in addition to the Wax Lake Delta, the author(s) should explicitly consider the large body of literature on deltaic processes (progradation, abandonment) that may occur in some areas of the coast during the next 50-100 years.

Barrier Shoreline Morphology

Barrier Shoreline Morphology - Editor Report[1]

Introduction

The objective of this modeling effort was to "provide insight about how various subsystems of the coastal zone interact, and in particularly how they positively or negatively interact as potential natural and anthropogenic alteration to the coastal zone occurs." The three reviewers provided thorough and consistently critical comments of several aspects of the manuscript and additional work was required to accomplish the claimed capabilities of the models. All three reviewers considered the model to have "fundamental flaws" for application to restoration projects in the Louisiana barrier island system. It is important to recognize that the system being modeled is extremely complex and the efforts of the modeling team are appreciated in this context.

The modeling effort includes two models: The Barrier Morphological Model (BMM) and the Inlet Morphological Model (IMM), components of the 2012 Coastal Master Plan. Most of the following comments and recommendations pertain to the BMM.

Summary of Reviewer Comments

Claims made for model applicability are not justified. Details are provided in the individual reviews. For example, regarding the statement on Lines 18-20 of the report: "this model can help determine the response of a sandy coastline to long-term wave climate and relative sea-level rise forcing, and can provide the final barrier position at time scales of one year to several decades," it is difficult to accept this statement for, as pointed out by the Reviewers, the model has been calibrated but has not been verified as would be required to support this claim. Calibration does not guarantee predictability. See further comments on calibration below.

The report is dismissive, without justification, of the potential effects of features not included in the model. For example, Line 69 says "*The time-step of one year does not affect the results since the transport equation is not in deferential form*." (Note should be "differential"). The main point is how is this statement justified? A similar statement is found starting on Line 271: "*Limitations related to the theory and assumptions include, among others, ignoring local wave refraction and other nearshore processes. This will affect the way local waves are transformed, and result in errors in the prediction of the longshore sediment transport. These errors are not expected to be large – generally less than 10% - but could provide local reversals of transport in areas with nearshore coastal features such as nearshore bars, shallow ebb deltas, oblique isobaths etc.*" What is the basis for the 10% and is the 10% in terms of shoreline changes?

[1] *NOTE: The Barrier Shoreline Morphology model report review editor provided a synthesis of the reviewer comments and suggestions for report revisions prior to the authors revising their report. The report presented here describes the editor's comments following these revisions.*

The model was calibrated but not verified. Further, there is no concise and coherent description of the calibration process. It is stated that β is a calibration parameter representing the "presence of muds in the system." Was β held constant in the calibration process or was it allowed to vary by island, by cell, etc? Similarly for the $Q_{overwash}$ *and* $Q_{offshore}$ terms which are discussed in the report on Line 60-62 as: "*Resulting net long-shore transport (on an annual basis) was then balanced by pre-determined cross-shore transport rates, obtained during model calibration (using a 20 year period)*." Such information is essential for judging the robustness of the model Otherwise, the Reviewer or User has no basis for judgment and developing confidence. There are several places in the report where calibration is referred to as described in Section 2e. However, the title of Section 2e is "*Identification of formulas used in the model and proof that the computations are appropriate and done correctly.*" Were the calculated net or gross longshore sediment transport rates compared with published estimates? Were there reasons that a verification phase was not conducted? If so, this should be stated. The calibration is discussed in much greater detail later.

Lack of accounting for storms. As noted by Reviewer 3, the longshore sediment transport varies with wave height to the 2.5 power which can be significantly greater than the average wave height to the 2.5 power. Also storm surges are apparently completely neglected by the methodology. Barrier islands are usually breached by large storm surges and the accompanying waves.

Lack of a local wave model. It is clear that the wave model did not include effects of wave refraction and shoaling. Line 835 states "*The use of offshore waves rather than describing the refraction and energy changes as the waves shoaled (as described in Sections 1 b and f) could result in skewed outputs.*" What are the meaning and significance of "skewed"? Bretschneider and Reid (1954) have examined the effects of energy dissipation as waves propagate across the continental shelf and found that this can result in a significant effect on wave heights. It would have been useful to have conducted simple calculations demonstrating whether the lack of a local model is justified.

The effect of nourishment in inducing subsidence as investigated by Dr. Julie Rosati apparently has not been recognized.

Author Responses to Reviewer Comments

In general, the authors have made a sincere effort to respond to the concerns of the reviewers through modifying the material of concern or providing reasonable explanation why the suggestions were not incorporated. However, two main concerns with the report remain: lack of transparency and the dominance of the model predictions by the calibration terms. Some major details follow for the main issues.

In general, the authors have "toned down" the claims of the model capabilities.

Some of the related concerns expressed by the modelers have been addressed whereas others have not. For example, Line 286 states, in discussing wave effects: "These errors are not expected to be large – generally less than 10% - but could provide local reversals of transport in areas with nearshore coastal features such as nearshore bars, shallow ebb deltas, oblique isobaths etc." Although the basis for this statement was provided informally, no additional explanation is provided in the report.

The modeling effort has now added what is considered “verification.” However, the verification added to the report is not independent of the calibration. The calibration period encompassed the 20 year period from 1989 to 2009 and the verification period extended over the 13 year period 1996 to 2009. Thus, the verification phase is actually a subset (actually more than half) of the period encompassed in the calibration period (Figure 13). It is not clear why this was done as it is not genuinely responsive to the reviewers’ concerns.

Additional comments are in order relative to the calibration. The calibration terms are very large compared to the other terms (the other terms are the calculated shoreline changes due to the gradients of the longshore sediment transport). The calibration terms may be, on average, on the order of 10 to 20 times (estimated “by eye”) of the calculated terms (Figure 2). In the following discussion, consider that 90% of the shoreline changes are represented by the calibration terms. Computational models are only valid in cases in which the calibration represents a relatively small contribution to the total effect represented or in cases in which a “global” calibration has been conducted versus a “local” calibration, as done here. Otherwise the model becomes a projection of the effects represented in calibration over the period of application. This means simply that, without calibration, the model has very limited predictive capability, that the model predictions are dominantly the calibration terms and that the other terms (calculation of longshore sediment transport, inlet effects, etc) are secondary (say 10% contributors). The calibration includes a local adjustment of the source terms with some smoothing (Lines 178, 465, 556, 910) by which means it would be possible to obtain perfect agreement (and indeed the calibrated results below look quite good). However, this means that in applying this model, by and large, the predictions will simply be a projection of what has happened in the past. To illustrate this point further, suppose the longshore sediment transport and the barrier island modules were perfect in the model. The associated shoreline changes would be the blue lines in the upper panel of Figure 2, which are approximately 10% of the total change.

It is not clear that the modelers recognize this feature of their method. If they do, it should have been discussed in the report. To return to the issue of verification period, as noted, the associated shoreline changes are dominantly (say 90%) an application of the rates determined through the source terms for the longer period. This is of further concern as both the calibration and verification periods include the 2005 hurricane season in which Hurricanes Katrina and Rita impacted the area so strongly. Additionally, these source terms consist of cross-shore transport (overwash and offshore transport), which respond to relative sea level rise (RSLR) (especially overwash) and if accentuated rates of RSLR are considered for future scenarios, these source terms are expected to increase (if the calibration phase was representative). It is not evident from the report that this is recognized.

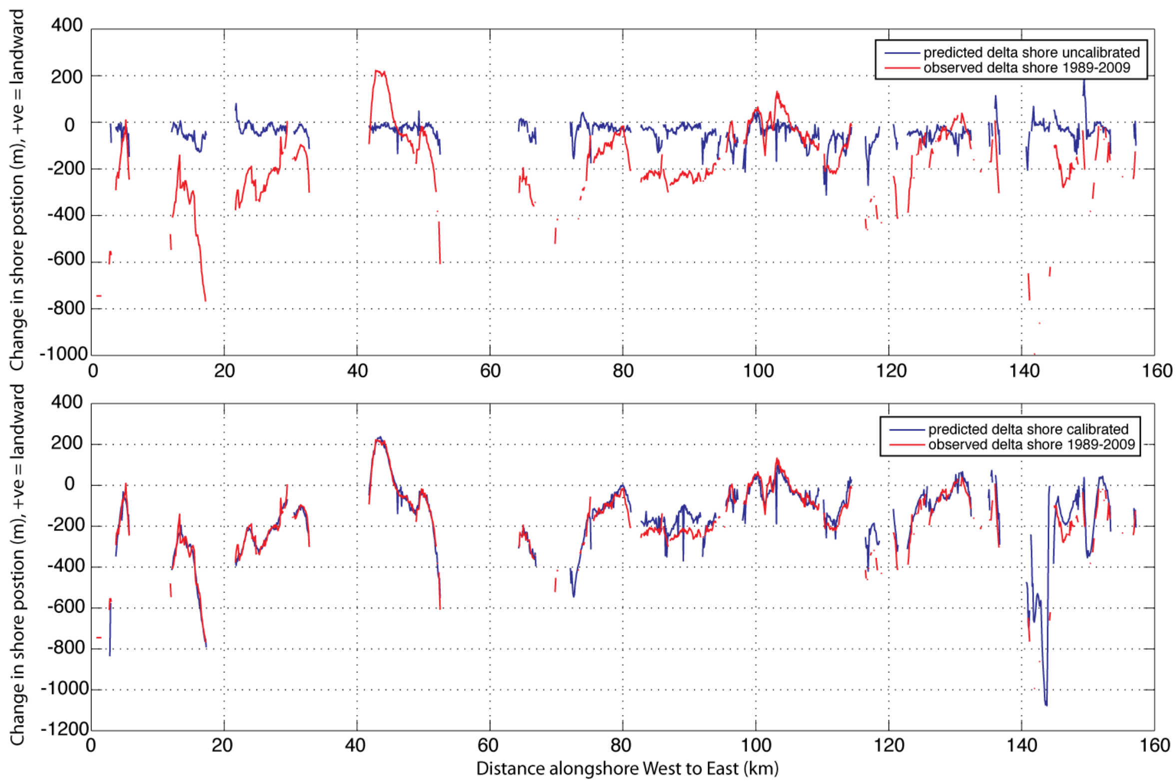

Figure 2. Model skill assessment during calibration without adjusting source terms (upper panel), and after adjusting source terms (lower panel; Figure 10 in Barrier Shoreling Morphology Model Technical Report).

Recommendations for Future Model Development

In order to provide perspective, the recommendations are presented in order of significance.

The authors should recognize and discuss that the model is, through calibration, a projection of past changes that occurred over the period 1989 through 2009 (Figure 2). This issue overshadows all of the others. As stated earlier, based on the contents of the report, it is not evident that the modelers recognize this characteristic of their model. Although it is appreciated that there is an understandable interest in developing a quantitative predictive model based on physics, the emphasis here should be on providing the best capability of representing future behavior of the system. Simply put, based on the report, the complexity of this natural and anthropogenically-influenced system precludes such a physics-based approach.

This has implications to the further development of this model as refinements would be modifying the 10% term unless some objective approach could be developed to represent the cross-shore transport which should be approached warily. It is strongly recommended that any future models adopt an entirely empirical approach (at present, it is 90% empirical) to the shoreline changes and develop factors which depend on RSLR and storminess to represent the changes in the mean and variance of the shoreline changes. This would be an ongoing effort in which the bases for the projections would be updated as more shoreline position information becomes available. It should be recognized what the calibration factors represent versus what is calculated by the model. The calibration factors represent empirically, the effects of storm surge (which is not included in the model), the effects of including yearly average wave characteristics (not included in the model), overwash and other components of cross-shore transport (would be included as one of the contributors of the completely empirical approach), all of the effects of beach slope characteristics, etc. Thus, a completely empirical approach would also address deficiencies in the longshore sediment transport model identified by the reviewers. The model could be applied for any future time, although as for any model, the uncertainty would increase with time into the future.

Relative to the issue above, the reviewer suggests the following : (1) Include a quantification of the calibration contribution by calculating the average absolute value of the calibration component (Figure 2) versus the average absolute value of the without calibration component (essentially the 10% contribution due to longshore sediment transport). As noted previously, this ratio should be on the order of 9:1, and (2) Carry out the calibration and verification phase efforts for two independent periods. Calibration: 1989 to 1996 and Verification: 1996 to 2009. These results would be quite informative as the first period was not impacted by Hurricanes Katrina, Rita, etc. and could assist in quantifying approximately effects of energetic periods.

As noted above, it is recommended that a completely empirical BMM model be developed (Reviewer 3 suggested this briefly). Thus, nature would provide the best estimate of the effect of all of the processes without any approximations. As noted, this would address almost all of the concerns of the three Reviewers. Additionally, for consistency, it is recommended that the IMM module be based on a parallel approach, that is, purely empirical. Such an approach would significantly improve transparency of the model. In developing future calibration coefficients as more data become available, in order to separate project effects from natural effects, it will be necessary to calculate and remove the effects of projects; however, at present, these effects are not included in the methodology. See below.

With the possible exception of anthropogenically-induced subsidence, the model, at its present stage, does not account for “anthropogenic alteration to the coastal zone” as stated under the objectives. Rather, the model relies on these effects to be calculated external to the model and then added to the model results. This appears to be a reasonable approach as there are a number of models (analytical and numerical) that can provide reasonable estimates of project evolution.

Additional longer-term considerations include:

- Applying wave and storm surge data on a more frequent basis (at least every three hours).
- Coupling shoreline and inlet models more frequently than 25 years.
- Illustrating the model capability with a number of idealized cases (e.g., perhaps two-dimensional in which the waves approach the shoreline normally.
- Examine the effects of averaged waves and water levels versus more frequent sampling.
- Other tests could include the effects of storm surge induced currents on tidal inlets).
- Being absolutely diligent regarding definition, description, and discussion of relevant variables.

Vegetation

Vegetation - Editor Report

Introduction

This summary report covers the Vegetation model, which is a component of the 2012 Coastal Master Plan. It covers the overall theme of the comments, the responsiveness of the report authors in making requested edits, overall strong/weak points of the modeling application, recommended modifications and enhancements, and briefly summarizes the editor comments.

The goal of the Vegetation model is to forecast landscape-level changes in Louisiana coastal plant communities in response to natural and anthropogenic changes in hydrology (lines 13-18). As such, the model can also be used to assess the potential outcomes of different restoration projects. The Vegetation model takes inputs from the Wetland Morphology model, and provides information to the Wetland Morphology, Ecosystem Services and Storm Surge/Waves models (Figure 1).

Given the multiple interacting models, there is an obvious potential for errors in one model to propagate into another. This point was not explicitly addressed by the reviewers. The issue of how the models interacted came up in some comments relating to model resolution and time scale, but for the most part the reviewers focused their comments on the Vegetation model itself.

Reasonable expectations for a model of this type depend in part on the funding and data available to the modelers, and in part on the nature of the other models that integrate with the Vegetation model. The reviewers and the editor were generally unaware of these constraints, and as a result some comments may be naïve. In the end, however, whatever the constraints, the model will either be useful as a planning tool or it will not. Thus, the single most important recommendation of my report is that the Vegetation model needs to be validated/tested with an independent data set in order to get some sense of how accurate its predictions are.

Summary of Reviewer Comments

Three reviewers evaluated the model documentation. In general, the reviewers praised the documentation, and felt that the modeling approach was appropriate for large-scale, long-term coastal planning. In terms of technical assumptions, the reviewers were less concerned about what was in the model than about what was left out. In particular, all three reviewers, in different ways, raised the same three major concerns about the model. These three concerns identify limitations in the way the existing model can be used in coastal planning, and identify issues that should be addressed to improve the model if additional funding becomes available.

Model Simplicity. Vegetation responses in the model are driven only by variation in salinity and sea level. Other potential drivers, such as eutrophication, rising CO_2 levels, and hurricane disturbance are ignored. Thus, the model assumes that salinity and sea level are the primary drivers of vegetation pattern, will remain so over the next 50 years, and that the responses of vegetation to salinity and sea level will not change with changes in eutrophication, CO_2 levels, hurricane disturbances, or other abiotic factors (Reviewer 1 comment 1; Reviewer 2 comment line 123, Figure 4, Figure 5; Reviewer 3 general comments).

No Time Lags. The model assumes that vegetation mortality and colonization patterns respond instantaneously to variation in salinity and sea level. In other words, the model assumes that there are no time lags in mortality or recruitment that would drive a gradual successional process at a given location in response to environmental change. This concern is particularly important for recruitment dynamics—the model assumes that species will recruit to areas far outside their current distribution if conditions become suitable, and assumes that large plants (e.g., mangroves) will arrive as adults within a single time step. Both of these assumptions over-simplify natural vegetation dynamics (Reviewer 1 comment 3; Reviewer 2 comment line 565, Section 5; Reviewer 3 general comments, comment line 579-593). The mortality terms in the model will generate some successional dynamics as conditions change, because mortality rates outside the optimum conditions for each species decline gradually to 100 percent, and so a species will die off gradually rather than instantaneously if conditions for that species decline by a modest amount. Moreover, if change is modest, there will not be large obstacles to recruitment by new species, which are likely to be growing nearby, or already present at low densities in an area. Thus, this concern is probably most relevant to vegetation dynamics following large disturbances (marsh die-back, hurricane disturbances) that might produce widespread mortality or strongly change abiotic conditions over large areas. Under these circumstances, the short-term dynamics of the model are likely to be incorrect because the model ignores a variety of processes that may create time lags in vegetation recovery or change.

No Validation. The model has not been validated against independent data sets. As a result, we have no idea how well it can forecast vegetation under any conditions other than those used to develop the model. (Reviewer 1 comment 2; Reviewer 2 comment lines 37-47, 656, 733, Section 5; Reviewer 3 general comments, comment line 719-763). In other words, even if we ignore the first and second concerns, we don't really know how accurate the predictions of the model are going to be. The model seems reasonable, but will it predict future vegetation states with an accuracy of 50% or 95%? We don't know. In addition, the reviewers made a variety of more minor comments, asking for clarification or corrections to the report.

Author Responses to Reviewer Comments

The report authors were highly responsive in making changes that involved edits to the text to improve clarity or precision. These changes involved rewriting moderately large sections of text, and adding new figures. In contrast, the authors did not substantively address the three major concerns identified above. Fully addressing these concerns would have been very challenging, in that they identified fundamental limitations of the modeling approach. It would have been unrealistic to expect the authors to radically change the model at this point. The authors should, however, be transparent about these major concerns in the text. In the first two cases they are, but more work needs to be done to fully address the third major concern. Below the three major concerns are described, and followed by brief description of a minor point regarding reviewer responses.

Model Simplicity. The authors responded that the information is not available to parameterize the model for many other factors, but that these other factors could be included in future versions of the model as new data become available (response to Reviewer 1 comment 1). The authors have the information to explore other variables derived from salinity (such as maximum salinity, or salinity variation) but lacked the time to explore this to date (response to Reviewer 2 comment on line 123).

The authors do a good job acknowledging this limitation of the model in the section "Statement on the capabilities and limitations of the model" (line 431 and following). In my opinion, this is an adequate and appropriate response to this concern at this point in the model development.

No Time Lags. The authors respond that they would like to incorporate dispersal into a future version of the model (response to Reviewer 1 comment 3). The lack of explicitly modeled dispersal mechanisms is clearly acknowledged in the section "Statement on the capabilities and limitations of the model" (line 431 and following). In my opinion, this is an adequate and appropriate response to this concern at this point in the model development.

No Validation. The authors agree that the model validation needs work, and mention one possibility for validating the model if more funds are available (response to Reviewer 2 comments on line 656, and section 3.d; response to Reviewer 3 comments on lines 719-763). The attempts to validate the model to date are described in the section "Description of process used to test and validate model" (lines 947-985), but the text does not explicitly state that these attempts were limited and qualitative, and that the model has not been quantitatively validated. It is important to recognize that the model has been parameterized using a large data set, but has not been validated in any formal way, and as a result there is no way to know how accurate the predictions are. In retrospect, it might have been a better approach to have used only a portion of the available data for parameterization, leaving the rest available for validation, but that is water under the bridge at this point.

Strengths and Weaknesses of the Model

Strengths

The model is conceptually quite simple, and its logic is easily understood. The documentation clearly reflects the modeling process. The model incorporates the two variables (salinity and flooding) that are likely to be most important in driving vegetation change in the study area. The general approach is appropriate for the project goal of predicting large-scale, long-term change under various global change and management scenarios. The model handles far more vegetation classes than a previous version, and handles vegetation mortality and community composition in a more nuanced way.

Weaknesses

The model includes only two driving variables, and ignores many others, including hurricane disturbance, eutrophication and CO_2 concentrations, that are known to also affect vegetation patterns. This concern suggests that the model may fail to predict vegetation patterns in areas that experience hurricane disturbance or increasing eutrophication, and that model predictions may increasingly diverge from actual vegetation patterns as CO_2 levels rise.

The model does not include dispersal/recruitment dynamics, and instead assumes that plants can recruit immediately to all available habitats. This concern suggests that, given rapid environmental change, actual vegetation patterns may change more slowly than predicted by the model. If so, the model may be useful in predicting long-term average conditions, but not in predicting short-term dynamics following extreme events.

The model has not been validated, and so we know nothing about how successful it will be in predicting future vegetation states. This concern means that we have to view the predictions of the model in an adaptive management context, as testable hypotheses rather than as predictions in which we are highly confident. If the model performs well, managers can assign more confidence in its predictions. If it performs poorly, it will be necessary to fix the issues that led to poor model performance before using the model further. Addressing this concern would help inform us about how serious the first two weaknesses are.

Recommendations for Future Model Development

The first priority for future work is to test/validate the model using a new data set in order to determine how well it predicts vegetation patterns under any circumstances. Until this is done, there is no way of assessing the reliability of the model predictions.

Based on the results of the validation, a second priority would be to incorporate additional processes into the model. Because data are probably lacking on how all 19 vegetation types respond to (say) eutrophication or CO_2 changes, including additional factors is likely to be increasingly speculative, or to require extensive (and expensive) experimental work to provide relevant data. There may be sufficient existing data, however, to incorporate vegetation responses to severe disturbances such as hurricanes or severe freezes, both of which are likely to strongly affect some vegetation classes while having little effect on others. There may also be sufficient data to incorporate some simple dispersal/recruitment mechanisms into the model. For example, there could be limitations on how far a species could disperse each year, how rapidly a species could expand in a given area within a current year, and how rapidly large woody species could grow in a single year. Although data for these functions are probably not available for all species, they must be available for at least one or two species, and could be included as constants across all species to set some upper bounds on what is realistic for dispersal/recruitment dynamics.

If the Eco-Hydrology model could be modified to predict turbidity, it is likely that turbidity could easily be incorporated into the Vegetation model to greatly improve the predictions of the presence of submerged aquatic vegetation (SAV).

The comments of the reviewers and responses of the model authors also raised two issues that need to be considered as part of the overall integrated modeling effort. First, the issue of model integration and error propagation need to be addressed it appears that it may be addressed elsewhere (lines 1125-6). Second, the model will be of the most benefit for planning in Louisiana and for use elsewhere in modified form if it is available in a form that is user friendly and that is well documented. In particular, it would benefit the broader scientific community if an annotated version of the model code were made publically available through a standard scientific data/code repository.

Nitrogen Uptake

Nitrogen Uptake - Editor Report

Introduction

The purpose of this editorial review of the Nitrogen Uptake Spatial Statistical Approach (SSA) model, a component of the 2012 Coastal Master Plan, is to describe the responsiveness of the author to make requested edits, overall strong/weak points of the modeling application, and any recommended modifications and enhancements for this type of modeling effort.

Summary of Reviewer Comments

Although the three reviewers identified some issues in model description, report organization, and lack of inclusion of some potentially controlling processes, all reviewers and this editor agree that this report presents a model that 1) advances the calculation of N removal in Louisiana aquatic environments and 2) is adequate for management scenarios. Environmental models are simplifications of complex systems and we can only model what we fully understand. Thus, SSA is a spatial extrapolation tool designed to use habitat-specific nitrogen removal rates to estimate N removal at large scales. This model appears appropriate for this purpose and given our present state of understanding of nitrogen cycling processes, is advanced as far as it can go without considerable effort in generating Louisiana-specific nitrogen cycling data. The improvements to the report address the key concerns of the reviewers, and its revised form adequately describes the model.

Author Responses to Reviewer Comments

The reviewers focused heavily on the first theme of the review (*was the documentation clear and adequate*). Reviewer 1 requested clarification of whether nitrate concentration was incorporated into the model (it was not). Furthermore, the reviewer pointed out that the model does not fully predict all N removal. It was unclear how the appropriated literature value/average for each unit was chosen – the authors responded that they used “medium” denitrification values. The reviewer requested more detail on vegetation categorization, and the authors responded that more detailed classification were not available and perhaps more importantly, that N cycling data were not available for a more detailed vegetation classification scheme. The question of anammox (i.e. nitrite plus ammonium yields di-nitrogen) was raised, but clearly it is beyond the model’s scope. Reviewer 2 found the section on model description and depiction confusing; in response, the section on model scope and model “modules” was improved via extensive modification. A number of clarifications were suggested (“different types of wetlands and open water bodies”, wrong association of G01 with moderate scenario). The major concern with documentation was concerned that the authors do a poor job presenting what they have done and how each component fits into the broader SSA model. The authors point out improvement in the model description as a response. The reviewer argues that assumptions are stated without explaining them fully. The authors suggest this will be included in a peer-reviewed publication. Reviewer 3 was concerned about the general organization of the model report and that a more traditional way to present the work was more appropriate. The response to a number of organizational/formatting questions was that the report format met the requirements of the sponsor. The revised version was still somewhat difficult to read without a considerable amount of work.

Reviewer 3 made a number of observations of imprecise, sometimes confusing wording, with many comments similar to the other two reviewers. Overall, the authors were responsive to the comments regarding documentation, with a response to comments regarding overall organization suggesting the sponsor desired the current organization of the report. The revised document is clearly more user friendly, easier to understand, and better organized (especially with better model description).

The second review theme was whether this type of modeling was appropriate to large-scale, coastal planning efforts. Reviewer 1 said "overall, I find the approach of the model sound and it should meet the requirements for planning purposes." The caveats (biogeochemical dynamics) will be addressed later in this document. Reviewer 2 suggested there were no fundamental flaws and that model revisions were not necessary. Reviewer 3 thought the model was a clear improvement over previous efforts and the modeling approach and technical assumptions were appropriate and appropriate for large scale on long term planning efforts. All three reviewers pointed out that the model was not mechanistic and that it could be limited for that reason. There is agreement the general affirmation of the value of the model; the model is clearly an advance considerably beyond previous efforts. Models are necessarily simplifications and cannot represent all processes in a spatially-explicit way. Perhaps the biggest concern is voiced by Reviewer 3 regarding the lack of true validation data. The model authors suggest that the model will serve as a basis for developing future assessment efforts.

The question of equations/technical assumptions has been addressed by all reviewers and in general, they appeared satisfied with the approach. Most of the questions were clarifications made to better explain the model and its assumptions. The authors suggest that many of the missing elements that show more detail will be included in a peer-reviewed publication.

Recommendations for Future Model Development

The final review theme is whether there are fundamental flaws or revisions needed for future planning efforts. The simple answer is no, this model has achieved its original goal and as such, advances the knowledge of N removal in Louisiana aquatic environments.

Each reviewer identified a number of important processes and environmental controls that are not represented in the model:

- Response to overlying water nitrate concentration;
- Effects of temperature on biogeochemical N processing;
- Coupled nitrification-denitrification is not included. While this may be quite reasonable in some freshwater wetlands and rivers, this process can be dominant in coastal sediment with algal input into the sediment;
- Water depth and residence time;
- Direct effects of plant communities on N cycling;
- Effects of benthic biota on denitrification (through bioirrigation, mixing).

The difficulty with including these processes comes from inadequate data on how all of these processes affect N removal, little direct information in Louisiana environments, and inadequate understanding of the spatial distribution of these processes (i.e. if oysters and other benthic animals are important, do we know their distribution and their effect on the N cycle). There may be no aquatic environments where all the controlling processes in the benthic N cycle have been effectively described. Faced with

inadequate data and model coefficients for mechanistic models, researchers and planners are thus faced with using spatial models such as SSA as our most effective management tools.
While literature values for denitrification are generally endorsed by the reviewers, some concerns remain about using such data. Given the idea that these rates are driven by nitrate, nitrate concentrations from each study would have been useful. There are a broad range of techniques used in the cited literature, and it is clear that different techniques can yield different rates. For this iteration of the SSA model, literature values are a good start to allow the authors to make reasoned calculations of N removal. The first step to improve this model involves making seasonal and spatial (i.e. different plant community) measurements of denitrification using modern techniques. Determining how denitrification changes with changing nitrate loading would also improve the model, likely making it more predictive for how nutrient management practices would impact denitrification.

Storm Surge and Wave

Storm Surge and Wave - Editor Report

Introduction

As part of the 2012 Coastal Master Plan, predictive models for hurricane waves and storm surge were employed for simulations along the Louisiana coastline. The ADvanced CIRCulation (ADCIRC) model was used to compute storm surge and currents, and it was coupled to the Simulating WAves Nearshore (SWAN) model to include the effect of wind-waves and swell. The SWAN+ADCIRC model has been validated extensively on high-resolution, unstructured meshes of the northern Gulf coastline for several recent storm events. A new mesh was developed and validated for the parameters of this study, and it was then used to investigate the system's response to synthetic storms under current and future conditions including changes in topography and sea level rise (SLR). This work is described in the Master Plan, Appendix D-24: Storm Surge and Wave Predictive Model Report.

The Storm Surge and Wave Predictive Model Report was reviewed by external experts, who offered comments and questions relating to the specific choices, methodology and reporting of the modeling effort. This document is a summary of the reviewer comments, author responses, and recommendations for future coastal planning efforts.

Summary of Reviewer Comments

The reviewers provided a thorough examination and response to the modeling application. Their comments did not reveal any high-level objections to or flaws with the present study; instead, they focused on specific details about the application and its description in the technical report. It was their consensus opinion that the modeling application was appropriate for the large geographic scale of the Louisiana coastline and the long time periods associated with sea level rise. Their comments, questions and suggestions, as well as the authors' responses, have led to an improved document that is an adequate reflection of the modeling process. The following sections summarize this review process.

Author Responses to Reviewer Comments

The reviewers' comments addressed the following themes: model description, mesh development, input parameterization, model validation, and presentation.

Model Description

A consistent theme in the reviewers' comments was the model description, especially the strengths and limitations of ADCIRC and SWAN with respect to the present study. In a broad sense, the authors addressed this theme through an expanded description of the purpose of the study, specifically that the model outputs (predictions of hurricane waves and storm surge) will be utilized in the assessment of risk, project planning and other efforts. The reviewers also commented on specific aspects of ADCIRC and SWAN.

The initial selection of ADCIRC was questioned, because other models exist to simulate the effects of storm events on nearshore circulation. The NOAA SLOSH model (http://www.nhc.noaa.gov/ssurge/ssurge_slosh.shtml) was presented as an alternative, although it was noted that SLOSH is not utilized typically with the high levels of resolution required by this study. The authors responded by noting that other models were considered initially, but ADCIRC was selected because its community has developed and validated high-resolution models of southern Louisiana that could be adapted by the project team. Other comments required clarification about the ADCIRC model's treatment of sea level rise, its mass conservation properties, and how the wave radiation stress gradients from SWAN are applied as forcing to ADCIRC. The authors revised the model text to clarify these points.

The SWAN model's initial development for structured meshes was noted as a potential limitation, because it is possible that the recent extension to unstructured meshes may not be robust in all applications. The reviewers also encouraged clarification on several aspects with respect to phase-averaged wave modeling with SWAN, especially how the length scales associated with the physical processes of wave generation, transformation and dissipation are compared to the length scales that are modeled in this study. The SWAN parameterizations of these processes are a potential limitation, because the optimal modeling of these processes must be done with phase-resolving models at much smaller scales. The authors clarified the description of SWAN and its parameterizations to address these comments.

Mesh Development
The reviewers also commented extensively on the mesh development. Hurricane waves and storm surge are simulated on an unstructured mesh with triangular finite elements. Instead of developing from scratch a mesh to describe the geographical features that impact waves and surge in southern Louisiana, the project team was able to leverage an existing mesh. The SL18 mesh has been developed and validated for ADCIRC and SWAN for several years. Critical features were retained for the present study, and the mesh was de-refined in other regions to improve the computational efficiency.

The reviewers asked for an expanded discussion of which features were retained and which regions were de-refined, especially as a summary early in the report. For example, it was noted that the areas behind the protection system (such as New Orleans and Lafourche) are not included in the mesh, because their flooding is the focus of another project team. There was also a request for clarification about the de-refinement process, specifically how the de-refined mesh was validated through comparison to results obtained on the original, high-resolution mesh. The authors noted that the comparison shown for Barataria Bay, in which the response values were nearly identical for several layers of refinement, was similar to comparisons in other regions that were not included in the report for brevity.

There was a request to clarify the sources for the bathymetric and topographic datasets applied to the unstructured mesh, including the dates of their collection. The authors noted that the topography has been re-applied from a DEM provided by the Wetland Morphology team, by using an elemental averaging from the DEM to the mesh. The bathymetric data and some of the raised features were carried forward from the original SL18 mesh, and the text contains references to reports describing these data sources. With respect to small-scale features, the text includes a discussion of their representation as either a line of mesh vertices or a sub-mesh weir. The authors included specific examples of these features at the request of the reviewers.

Input Parameterization

The reviewers suggested improvements to the discussion of the input parameters other than the mesh topography. With respect to bottom friction, both ADCIRC and SWAN can utilize Manning's *n* values that are assigned based on land-use classes. The reviewers noted that this method is the current state-of-the-art, but there were specific comments about the selected Manning's *n* values and their parameterization in the models. For example, it was noted that the selection of $n = 0.012$ for both the low- and high-intensity developed areas appears to be counter-intuitive, but the authors explained that, although these areas have different features, their characteristics in an average sense can produce similar effective roughnesses. The reviewers also asked for a discussion of how SWAN parameterizes the bottom friction, which the authors addressed with expanded text and references to the literature.

The wind stresses can also be reduced in overland regions through the use of directional effective roughness lengths, which account for the reduction in surface stress due to upwind roughness, and canopy coefficients, which eliminate the surface stress in regions with extensive tree cover. The reviewers noted that a binary system of assigning canopy coefficients is sub-optimal, especially for future studies, as the community will be moving toward a parameterization of roughness based on LiDAR data. Also, it was noted that there was no change in the canopy coverage between the current and future scenarios, because the same land-use dataset was used for both scenarios. This decision could be re-considered in future studies.

Model Validation

The reviewers questioned and commented significantly on the validation of SWAN and ADCIRC. With respect to the input wind fields, there was a request for information about the development and validation of the historical wind fields, including whether they are available to the public. The authors note that the winds were used for simulations in peer-reviewed studies of Hurricanes Gustav and Ike. It was also noted that the storm set for the risk assessment is reduced to 40 storms, without much discussion or justification, but the authors note that the storm set reduction is described in the report by the Risk Assessment team. A larger set of synthetic storms could be included in future studies. In addition, it was noted that the wind forcing was not changed in the future scenarios, to reflect the effects of climate change on the surface winds and characteristics, and the authors added this clarification in the text.

It was noted that the description of the QA/QC process could be expanded to include a discussion of specific regions in the unstructured mesh that were addressed and improved during the initial stages of the validation. If a region needed to be improved, either by adding resolution or changing its parameterization, then this could have implications for other regions during the SLR scenarios. The authors responded that very few adjustments were made during the validation process, because they started from high-resolution meshes that had been validated extensively for the same storms. It was also noted that maximum values for water levels and wave parameters were supplied to the Risk Assessment team. In geographic regions that were not impacted by any of the storms in the test suite, computed results were extrapolated from nearby regions, as described in the report by the Risk Assessment team. The authors added text to clarify these points.

The reviewers also commented specifically on aspects of the validation with respect to the computed water levels. It was noted that the discussion of the results with SLR should be expanded to indicate regions where the increase in water levels is larger or smaller than the SLR, because these regions would thus respond differently with respect to their wetland ecology. The authors added text to describe the nonlinear effects of SLR. Another reviewer requested an expanded discussion of the differences between the modeled and measured hydrographs, and the authors provided text to describe the potential sources of error in the modeled hydrographs, including topographic and bathymetric values, surface characteristics such as Manning's *n*, and forcing functions such as tidal constituents and river flux boundary conditions. It was also noted that studies with varying river flow rates would be valuable, especially for projects near the rivers.

With respect to the wave computations, it was noted that the solution from SWAN is not validated to measured data, such as the observations from the LSU WAVCIS stations or the gages deployed by Andrew Kennedy. The authors responded that the SWAN results were compared to high-resolution simulations that have been validated, and also indicated that more measurements are needed during high-energy events. Another reviewer commented on the apparent similarities in the wave height differences in the future scenarios. The authors noted that, while the overall magnitudes were similar, the pattern of the solution behavior was different in some specific regions. In addition, for the scenario with the larger SLR, the waves propagated farther inland.

Presentation

Finally, the reviewers recommended several ways to improve the presentation of the results. The computational requirements in Table 1 were expanded to include the number of cores used for each simulation. The authors were encouraged to define acronyms at their first appearance and add a table of acronyms, present the results consistently in the English unit system throughout the report, and label the vertical datum as NAVD88 (2004.65). Several figure captions were revised and clarified, and definitions of some terms were added.

Recommendations for Future Model Development

The reviewers' comments provided several areas in which the study can be improved for future coastal planning efforts.

The treatment of the bottom friction and surface wind stresses will benefit from the research community's continued efforts to improve these parameterizations. With respect to bottom friction, current efforts are focused on the development of surface roughness based on the vertical variability in the LiDAR surveys. These surveys should provide better spatial resolution than the land-use datasets, and the surface roughness will be tied to measurements at specific geographic locations, instead of the observed land-use categories. This improved parameterization of surface roughness could be shared between ADCIRC and SWAN. In addition, the latest release version of SWAN includes the capability to dissipate wave energy due to local vegetation. This capability is promising, but it will require extensive data collection about the vegetation in the Louisiana coastal environment, as well as rigorous validation in a modeling context.

The parameterization of the surface wind stress will also benefit from current research. The surface canopy coefficients are set currently to zero or unity, to indicate either the absence or presence of canopy. However, it is thought that the coefficient can be set to an intermediate value, to indicate a partial sheltering from the wind stresses, through a careful examination of the LiDAR survey. This method would allow for reduced wind stresses to force waves and surge in regions with light-to-medium canopies, such as along the edges of forests or within the bayous. These improved parameterizations could then be carried forward to the future scenarios, to simulate the effects of the changes in wetland ecology.

The model validation in future planning efforts would benefit from an increase in the measurements of wave parameters during extreme events, especially along the coastline and within the coastal floodplains. It was noted by a reviewer that few data sources exist for validation of hurricane wave simulations in southern Louisiana, and the authors added text to indicate that the results in the present study were found to be sufficiently similar to results on high-resolution meshes that have been validated to the available wave measurements. An increased emphasis on this data collection will improve the validation process for wave models in future studies.

With respect to the synthetic storms, it was noted by the reviewers and authors that the given set was limited in its effects on the storm surge in Louisiana. At some locations with limited responses, values were extrapolated from nearest neighbors within an identified hydraulic sub-unit. The authors describe this methodology within the report, but they agree with the reviewers that it would be better to expand the synthetic storm set for future coastal planning efforts.

Finally, although it was not mentioned explicitly by the reviewers, future coastal planning efforts will benefit from an increased commitment of computational resources. In the present study, the authors started with a high-resolution computational mesh of southern Louisiana that had been validated extensively for several recent storms. This high-resolution mesh was then de-refined by removing resolution in the far-field. The resulting mesh was shown to provide results of the same quality in the region of interest, while providing an increase in efficiency. However, the availability of additional computational resources would allow for higher levels of resolution to be applied, either by employing smaller mesh spacings in the same region of interest, or by expanding the region of interest with similar mesh spacings. It is expected that these higher levels of resolution would improve the models' accuracy and allow for simulation at even smaller physical scales.

Risk Assessment and Damage

Risk Assessment and Damage - Editor Report

Introduction

The 2012 Coastal Master Plan by the Coastal Protection and Restoration Authority (CPRA) includes restoration and protection projects approved by the Louisiana Legislature. CPRA developed these projects using several predictions and planning models that are undergoing external peer review. This report summarizes the outcomes of the peer review of the Risk Assessment predictive model by the three reviewers and the review editor.

Summary of Reviewer Comments

Documentation of the CLARA Model

According to the reviewers, the documentation adequately reflects most of the modeling effort performed in the study. All the models are extensively explained; however parts of the report are unclear requiring further information.

Report Structure - According to one of the reviewers, the report is too fragmented into sections and subsections and a graphical roadmap in the beginning would be desirable.

Hydrodynamic Model Equations - Providing the hydrodynamic model equations would enhance the understanding of how infrastructure and subsidence affect flooding.

Climate Change Scenarios - The reviewers requested a definition of the climate change scenarios studied and providing results by various scenarios.

Baseline Storm Rates - The reviewers requested a definition of baseline storm rates particularly for Category 3 or greater.

Terminology - One of the reviewers requested changing the description of some results from *flood risk* to *damage associated with floods*.

Visuals and Other Editorial Changes - A reviewer noted that the graphical representation of the outputs could be improved to produce risk maps and damage maps under different climate change scenarios and tropical cyclone scenarios.

References - The CLARA model is based on the FORTE approach as used by the IPET team. This should be clearly acknowledged in the future.

Appropriateness of the CLARA Model

The reviewers generally concurred on the appropriateness of the CLARA model for large scale, i.e., entire coast of Louisiana, and long term, i.e., 50 year, planning efforts; however raised several important limitations and concerns that prompted responses by the CLARA team as listed under separate headings in their order of importance.

Appropriateness of CLARA Input Conditions for a 50-year Planning Horizon - According to the reviewers, a planning horizon of 50 years requires a broad set of future conditions for characterizing potential loss and risk profiles that should not be confined to a set of scenarios governed by software program ADCIRC limitations.

Verification and Calibration - The reviewers pointed out the importance to contrast general results for the New Orleans polders with previous estimates of flood depths and expected losses, and to assess the source of any major differences based on the data, processes assumptions and computational approaches of the CLARA model and other studies, such as LACPR, IPET and the recent armoring study for New Orleans.

Meeting the Protection and Restoration Goals of the 2012 Master Plan - A reviewer raised a concern that the CLARA model cannot address key considerations in order to meet the goals of the master plan of protection and restoration by not addressing potential environmental damage and restoration, and not including metrics for resilience and sustainability requiring the explicit treatment of environmental and social impacts.

Technical Assumptions and Equations - The reviewers generally concurred on the appropriateness of the technical assumptions and equations for the intended use of the model; however raised several important limitations and concerns as described under separate headings.

- *Conservatism and Biases* **-** Some model assumptions could bias results towards overestimating flooding and damages, e.g., breach-water levels inside defenses would reach the levels of the maximum height of storm surges outside the defenses, in particular for the largest polders, and tying failures to peak surge rather than storm hydrographs of surge and waves. Other assumptions might lead to underestimating flooding levels, e.g., not modeling gates and associated probabilities of being left open during hurricane events, and as noted by reviewers modest increases in the frequencies of intense storms with time. The aggregate effect of these assumptions, with concurrence among reviewers and the team, is an overestimation of flooding and associated damages by the model.

- *Expected Annual Damage and Seemingly Large Damages* **-** The reviewers recommended to the team to perform a correlation study to at least the recent 500-yr and Expected Annual Damage (EAD) data resulting from the District's planning study for levee armoring.

- *Erosion Due to Overtopping* **-** According to one of the reviewers, the assumption that erosion occurs only after overtopping of surge (i.e., surge elevation exceeding levee elevation) does not consider the significant impact of overtopping waves in initiating back side erosion and breach as observed in Hurricane Katrina. This assumption may be more appropriate for the 2011-plus levees with robust materials or armoring, but likely represents stronger than realistic performance for existing structures in areas not impacted by the new Hurricane and Storm Damage Risk Reduction System (HSDRRS) structures.

- *Accounting for Transitions* **-** According to one of the reviewers, assigning the same fragility as the adjacent weakest structure to a transition is expedient, but it may not be representative in many cases. This assumption implies that the transition will fail by the same mechanisms or failure modes as the conventional structure and at the same water levels. Transitions were found to be particularly vulnerable in Katrina and may suggest a more rigorous assessment of their fragility than currently used in the CLARA model. The other concern raised by the reviewer is the approach used to handle floodwater volumes and depths in CLARA if a transition fails.

- *Number of Storms Used* **-** The reviewers indicated that the use of 40-storm JPM-OS-sample is less than IPET's 76-storm sample, but IPET's sensitivity analysis showed that the benefit of increasing the number of storms was insignificant. The use of 40 storms coupled with 700+ synthetic storms is deemed to be reasonable.

- *Anomalies* - A reviewer observed that Figure 7 shows at least three significant anomalies in surge prediction biases that warrant attention in order to enhance understanding particularly where the differences exceed three ft.

Author Responses to Reviewer Comments

Documentation of the CLARA Model

Report Structure - The team plans to address this concern, however, by publishing a separate, reorganized technical report in the third quarter of 2012. The new report will include much of the information presented here, but reorganized it into a more logical structure and including outcomes from the analysis the team conducted for the final master plan. In addition, the team is currently seeking other venues in the academic literature to communicate key outcomes from this analysis, which could follow the reviewer's suggestions regarding organization.

Hydrodynamic Model Equations - The team responded that the ADCIRC hydrodynamic model is described in Appendix D-24 of the Master Plan.

Climate Change Scenarios - The team stated that the master plan scenarios, including both hurricane frequency and intensity were identified in conjunction with State planners and the Master Plan Development Team, and the rationale for their selection is described in Appendix C of the master plan and its references. Although the team provided input to the scenario development process, in this instance the final decision was made by the State and Master Plan Development Team.

Baseline Storm Rates - The team clarified that the rates vary by degree of longitude, and are based on Figure 4-2 of IPET Vol. VIII, Appendix 8-2, as noted in the Flood Depth Exceedances subsection. The report was revised by adding the range of rates from this figure for ease of reference.

Non-overtopping Induced Breach Factors and their Spatial Variation - As requested by the reviewers, the team indicated that these factors as input data are discussed on Page 32 of the revised report. These data are used to estimate seepage, slope stability and overtopping failure probabilities, and vary across the system.

Terminology - The team concurred and reported to have made the change; however retained the overall description as flood risk in the context of the threat-vulnerability-consequences framework. The team inserted additional text to clarify this distinction and explicitly excluded other consequence components of flood risk, such as loss of life. In addition, the team adopted the reviewer's suggestion to describe the damage outputs as *damage associated with storm surge flooding*. The team noted that the term risk in the revised report is generally reserved for the conceptual framework, and is not applied in reference to specific outputs from the model.

Visuals and Other Editorial Changes - The team believes that a comparison across specifically the climate-related hurricane scenarios would be helpful and informative; however these uncertainties were combined with a series of other scenario-based assumptions regarding sea level rise, coastal subsidence, and other ecosystem uncertainties, e.g., Mississippi River discharge and nutrient concentrations, to create three planning scenarios used by all modeling teams. Regarding the visuals themselves, the tradeoff analysis was conducted by a separate team using damage output from CLARA in a separate tool, and the visuals and outputs produced by the CLARA team were intended to support the analysis in general rather than provide visuals to directly support tradeoff analysis.

The reviewers offered editorial changes and enhancements to visuals that were acknowledged and reportedly implemented by the team.

Appropriateness of the CLARA Model

Appropriateness of CLARA Input Conditions for a 50-year Planning Horizon - The team's response revealed the widespread and underlying impact of ADCIRC on the ecosystem models, including the Eco-Hydrology, Coastal Morphology, Vegetation, etc., which provide critical inputs to the surge and wave analysis and thus to CLARA by facing runtime constraints and delays. Ultimately, however, the State of Louisiana determined that this narrower range of uncertainty was acceptable for initial plan development because of the adaptive nature of the plan. The State of Louisiana has mandated the update of the master plan every five years with the next update planned for 2017. The next update will use many of the tools developed in the ongoing round with a broader exploration of uncertainty over a 50-year planning horizon.

Verification and Calibration - The team acknowledged the importance of verification and calibration to enhance confidence in the results, and attributed this deficiency to the challenging timeline of the project that resulted in the CLARA model. The team identified these comparisons as a critical next step to help verify and potentially calibrate CLARA for future analysis and planning.

Meeting the Protection and Restoration Goals of the 2012 Master Plan - The team clarified the scope of the CLARA model that excludes long-term environmental damage or other ecosystem outcomes since they are specifically addressed by other modeling teams by referring to Appendices D, E, F and I of the Master Plan.

Technical Assumptions and Equations - The reviewers generally concurred on the appropriateness of the technical assumptions and equations for the intended use of the model; however raised several important limitations and concerns that prompted responses by the CLARA team as described under separate headings.

- *Conservatism and Biases* **-** The team justifies the appropriateness of such conservative estimates on the basis of providing an upper bound that is suitable for planning purposes, and identifies some key assumptions for future consideration to potentially enhance the model. This item is discussed further under Hydrographs for Flooding Volumes.

- *Expected Annual Damage and Seemingly Large Damages* **-** The CLARA team updated Figure 5 of the report although noted it is still intended to be illustrative, and the economic valuation of damage differ from IPET and LACPR, for example, CLARA describe damage in terms of structure replacement value, while LACPR and IPET instead report depreciated structure value with assumed depreciation rates, leading to potentially substantial differences in the damage baseline. The team obtained copies of the levee armoring study results, but has not yet had the chance to compare the result sets.

- *Erosion Due to Overtopping* **-** The team clarified that the assumption "overtopping due only to wave action does not result in erosion on the protected side of the levee" simplifies the analysis and eases the model implementation. The team has insufficient data and lacks experience to estimate the general effect of wave action on erosion across the system. While this assumption somewhat overestimates the strength of the system, it should be noted that the probability of failure rises quickly as soon as the mean water height rises above the levee crest; so averaged over many storms, the difference in damages estimated as a result of applying the two approaches is likely to be small.

- *Accounting for Transitions* **-** The team acknowledged a need for a rigorous assessment of the failure mechanics of transitional structures; however the team believes that this assumption is valid in the context of the CLARA methodology employed, which extends 2-dimensional failure analysis to a 3-dimensional case, i.e., for all intents and purposes, transition structures are 2-D structures. The team notes, however, that the presence of a transition does increase the probability of failure of a reach as compared to structure without a transition. The team justifies this statement on the basis that each transition is included as an additional element of the system with a nonzero probability of failure (albeit inherited from its adjacent levee sections.). Transitions could be treated as separate elements with their own fragility curves. Often these transitions have greater elevations and different characteristics than adjacent elements of the system.

- *Number of Storms Used* **-** The team's sensitivity analysis particularly for the eastern portion of the coast, which compared surge elevations with 154 and 40 storms for one landscape and scenario, found that peak surge from the 40-storm output was within 0.5 to 1.0 ft. of the 154-storm estimates, and the 40-storm sample was most often more conservative, i.e., produced higher peak water elevations, for these areas.

- *Anomalies* - The team attributed these anomalies to results from an earlier version of the CLARA model associated with non-wetting storms, and the CLARA model has been updated to address this issue. This figure was not updated in the revised report based on new versions of CLARA.

Recommendations for Future Model Development

The CLARA model does not include some features or behaviors that are either essential or might be desirable.

Hydrographs for Flooding Volumes - Not using hydrographs to estimate floodwater volumes is a fundamental deficiency and a flaw in the CLARA model. The assumption that breach-water levels inside defenses would reach the levels of the maximum height of storm surges outside the defenses is unrealistic and goes beyond bounding the solution as believed by the team to the territory of producing meaningless results. Ayyub et al. (2009 a and b) examined this effect during the development of the FORTE model and as a result, developed the method of using hydrographs to compute floodwater volumes. The team acknowledged this deficiency and is planning to update the model to use hydrographs to estimate floodwater volumes in future work.

Breach, Erosion and Other Failure Modes - Not accounting for all failure modes is a fundamental deficiency. Although overtopping failures may dominate in practice; during Katrina in New Orleans, however, failures occurred at water levels significantly below overtopping. All other failure modes of significance should be added to the CLARA model. The team stated that a future version of CLARA will include the catastrophic failure resulting in a breach and associated flooding. The team will investigate the breach depth and width. The water volumes will be calculated using hydrographs; however the exact approach for implementation is not yet determined.

The USACE has developed sophisticated and complete data and relationships concerning overtopping and erosion as a part of an analysis for effectiveness of armoring. This analysis involved using the IPET risk analysis method to re-analyze the HSDRRS under two conditions with and without armoring. The team is planning to incorporate these and other results regarding erosion on the protected side as a result of overtopping and waves, and believes that the lookup table approach used in CLARA is crude and likely overestimates the potential for failure, especially for armored levees. It is advisable that all credible failure modes should be added to the model.

Gates and Other Openings - Not accounting for uncertainty in the states of gates and other opening is a fundamental deficiency. IPET model included these features and associated probabilities of gates left open, and the team may consider using a similar approach. Floodwater volumes from openings should be based on hydrographs.

Deterioration and Aging - The CLARA model does not account for time-variant deterioration and degradation of defenses, and changes in practices over a planning horizon. The study does not consider wind effects associated with the storms, interactions between wind, rainfall and surge, and events induced by extreme floods, e.g., ships may be blown into flood defenses in the midst of extreme hurricanes, causing very significant levels of property damage to buildings, infrastructure, and communication and power systems, and does not recognize the holistic nature of a combined wind and storm surge catastrophe in extreme storms. The team assumed some subsidence by lowering affected structures; however other potential ways in which the system could deteriorate are not considered. In the context of this modeling effort, taking into account such factors would be exceptionally difficult, and the team explicitly stated in the report some of the potential ways in which the system could deteriorate. The team notes that the CLARA model was developed with the goal of evaluating options to reduce storm surge flood damage for possible inclusion in the master plan. The CLARA model estimates damage directly resulting from storm surge and wave flooding. As a result, the CLARA model does not explicitly estimate wind damage which is consistent with the hazards considered by previous studies such as IPET and LACPR.

Unenclosed Barriers - Regarding the no-fragility assumption for unenclosed barriers, this assumption does not allow the CLARA model to differentiate performance by structure quality. The team intends to address this issue in a future version of the model. According to the team, nearly all of the unenclosed barriers considered in this analysis are newly-proposed alignments, e.g., Morganza to the Gulf, and as such the structure quality, e.g., fill type, must be addressed through planning assumptions. As a result, even with a model capable of considering fragility in semi-protected areas, comparisons of structure quality in this analysis would be limited by the engineering and construction assumptions made for proposed new alignments by state planners.

Wind Effects - Excluding wind effects, such as direct damages, damages due to directional wind with rain, impacts on lifelines and pump operation, etc. is a limitation that underestimate losses.

Verification and Calibration - The team obtained copies of the levee armoring study results, but has not yet had the chance to compare the result sets. A reviewer suggest a comparison of the results from that study and the CLARA model's results would be an important next step to benchmark and improve the model to support future analyses.

Report and Graphical Presentation of Results - The team plans to prepare a separate, reorganized technical report in the fall. This report will include much of the information, but reorganized into a more logical structure including outcomes from the analysis conducted for the final master plan. In addition, the team is currently seeking other venues in the academic literature to communicate key outcomes from this analysis.

The reviewers recommended to the team to show the damage results across climate-related hurricane scenarios. The team hopes to conduct a follow-on analysis with this focus in the near future.

Uncertainty Analysis

Uncertainty Analysis - Editor Report

Introduction

Within the context of regional-scale management of the Louisiana coast, the uncertainty analysis addresses the question of whether the set of actions recommended by 2012 Coastal Master Plan present a feasible path to achieving its goals. In particular, the uncertainty analysis tests the hypothesis that the uncertainty inherent in model predictions, used in formulating the plan, is too large to reliably evaluate the anticipated effects of proposed management actions. The results of the analysis must be taken as indicative, rather than definitive. Time and resources did not allow a complete uncertainty analysis to be performed. Indeed, a definitive analysis may not possible in this case, given the complexity of the modeled processes and the time span over which predictions are attempted. However, this study establishes a useful framework for addressing questions related to the feasibility and eventual implementation of the master plan.

In addition, the uncertainty analysis provides other results benefiting coastal management. These recommend extending and refining the analysis as an on-going activity. To implement the uncertainty analysis, investigators were required to specify a set of indicators (model outputs) that characterize overall conditions in the coastal ecosystem; these comprised a set of suitability indices, representing key ecosystem services, and land area. Narrowly defined, the uncertainty analysis consists of comparing the variation in these indicators resulting from management actions with the uncertainty inherent in their calculation by the models. A more general analysis of the patterns of variation, both temporal and spatial variation, based on these indicators can provide insight into the structure and functioning of the coastal ecosystem - that is, to the degree that the models accurately represent the mechanics of the ecosystem. The report provides two examples: first, in the plots in Figure 8 of the 2012 Coastal Master Plan showing the relative effects of different types of management actions on each ecosystem service; and second, in the plots in Figure 9 showing the interaction among the ecosystem services in the system's response to management actions.

Based on the reviews summarized here, the uncertainty analysis can be judged as a success. The words of Reviewer 3 capture the overall assessment:

- "The documentation clearly/adequately reflects the process that was used to assess uncertainty in the systems-models.
- This type of uncertainty analysis maybe overly ambitious [for an application of this complexity] …, but clearly much was accomplished and the information resulting from it should be helpful to those making investment and management decisions.
- Technical assumptions and equations to the extent shown are appropriate and reasonable.
- I don't see any fundamental flaws or otherwise that should be revised for future uncertainty analysis…[except for a shared need to continually refine these analyses.]"

Shortcomings of the uncertainty analysis identified in the peer review relate to limitations on the degree to which all sources of uncertainty can be explored fully, which are generally acknowledged, and also to the need for better documentation of predictive ability of the component models by comparison with relevant data on the structure and functioning of the coastal ecosystem. To a certain degree, the later need is addressed by documentation of the component models, which has been reviewed separately. However, being able to demonstrate the ability of the coupled systems-models to accurately represent patterns of behavior of the coastal ecosystem is critical to gaining acceptance for the master plan. This should be addressed in the next stage of work with these models, and the uncertainty analysis provides a framework that will be useful in this effort.

Summary of Reviewer Comments

Comments by the reviewers touch on two themes: comments by Reviewers 1 and 3 addressed the application of uncertainty analysis to coupled hydro-ecosystem models; and comments by Reviewer 2 addressed the need to use data to better ground the abstract analysis of model uncertainty in reality.

Reviewers 1 and 3 have direct experience with the development and use of predictive models for the management of large-scale wetland ecosystems similar to those of the Louisiana coast, e.g. the Everglades. The coupled system of models used to formulate the master plan represents a relatively novel application of uncertainty analysis. Comments by these reviewers addressed this application of uncertainty analysis as a largely unprecedented application of a useful analytical tool. Both recognized and acknowledged the difficulties and limitations inherent in this application. Each praised the work done, while offering comments and suggestions for improvement.

In contrast, Reviewer 2 chose to address his comments from the more philosophical perspective of the value of predictive models in guiding decisions. The reviewer's comments are generally supportive of the work, but he offers criticism on two points that can be answered here. The first is his implied criticism that the modeling exercise may be "nothing more than an expression of expert beliefs that are encoded in mathematical formalism." The second regards his invocation of global climate modeling as a comparable application of predictive models to inform decision-making.

On the first point, earlier in his remarks, the reviewer characterizes the difficulties underlying the coastal plan as a situation in which the decision-makers have limited ability to comprehend the details of the analyses and the analysts have limited ability to quantify the full range of [system behavior]. Given this as the starting point, it is a significant achievement to have assembled a comprehensive description of the coastal ecosystem, out of the fragmented knowledge of experts, and then coded this mathematically in the form of the coupled systems models. In this form, the collective understanding of the ecosystem can be subjected to objective analysis, one, to test its sufficiency, and second, to evaluate the efficacy of proposed management actions.

On the reviewer's second point, it is questionable whether the experience of modeling global climate change is entirely relevant to the application of predictive models to management of the coastal ecosystem in Louisiana. The questions that has motivated global climate change modeling – Are human activities altering climate? – is moot in the case of the coastal ecosystem. There is no question that human activities alter the structure and functioning of the coastal ecosystem, for flood control and navigation. Coastal managers and analysts are faced with a more complex problem - not only to calibrate the models so that they adequately predict the future behavior of the ecosystem, but it also to calibrate the type and scale of proposed actions so that they will achieve the goals for management.

But it is also easier in other respects, because recognizing the need to take action in the coast does not depend entirely on demonstrating the absolute accuracy of the model predictions.

Reviewer 2 is correct on a third point, which is that, ultimately, for the models to have utility in decision-making it is necessary to establish the credibility of their predictions. Credibility depends on bringing in real data to justify the selection of the median parameter values used in the uncertainty analysis.

Author Responses to Reviewer Comments

The author has responded to each of the reviewers' comments by making changes to the text, where this is possible, and in the recommendations for further work.

Recommendations for Future Efforts

The author concludes with a list of recommended steps to improve and extend the uncertainty analysis. Of these, it is worthwhile to emphasize the main point of the final recommendation – "[T]he main utility of the uncertainty analysis will always lie in its ability to support the decision-making process. …[T]he supporting role of the uncertainty analysis should go beyond the planning phase and continue into the adaptive management phases of the projects (Lall et al. 2002). Recognizing the uncertainty inherent in project development and design, the uncertainty analysis can inform decision makers and managers to make the necessary operational adjustments and changes during the lifetime of the projects. This effort would require monitoring and performance evaluations (using models and data) associated with iterative uncertainty analysis that updates the degree of confidence about the project predicted performance."

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