We have carefully reviewed the comments and suggestions and agree with nearly all of them. We detail below how we addressed them. Each comment is addressed individually with the original comment shown in regular text and the response shown in indented red text. Line numbers are given for the more extensive alterations in the ms and refer to lines in the updated version.

Associate Editor Comments

1. The introduction notes four hypotheses to explain the lack of post-spill responses at the population level. The paper then focuses on the last two. This is appropriate, but I wondered if the authors should be sure to come back to these other two hypotheses in the Discussion and Conclusions, just to avoid any chance that readers assume the first two are being discounted.

We have mentioned the first two hypotheses again on line 525 of the conclusions.

2. Methods/Supplementary Material:

a. Please ensure consistency among the text, Table 1, and Table S1. Starting on line 204, the text refers to wading birds but I believe they are “Marsh Birds” in Table 1. Skimmers are mentioned, but it is not clear until later that they are included in the “Gulls & Terns” group in Table 1. Gross conversion efficiency is called GE in Table 1 but GCE in the supplement.

The inconsistent nomenclature was an oversight on our part, thank you for noting it. We have changed “marsh birds” in table 1 to “wading birds.” We have specified that “gulls, terns, and skimmers” is abbreviated as “gulls and terns” earlier, on line 208. We have changed “GCE” in the supplement to “GE” to be consistent with table 1.

b. Line 212 – where are survival rates used in the model? Should those be included in Table 1 with sources?

Because the PB ratio is equivalent to the total mortality rate at mass balance, the survival rate is simply 1-PB. We have added 1-PB in parenthesis next to survival.

c. Lines 219-221 – can more information be given on how or why biomass of these leading stanzas were increased? Where did the supporting information come from? How reliable is this approach?

The biomass data were based on a local monitoring program that is likely to underestimate the biomass and abundance of the species collected. This did not pose a problem in De Mutsert et al. 2017 since all biomass data were based on the same program. With the updated data on dolphin biomass in the area included in this model, the biomass underestimates became problematic since the prey biomass in the model does not meet the increased predation pressure that comes with the updated marine mammal information. This issue is summarized beginning on line 218, and is not an uncommon problem in ecosystem models when many disparate data sources are combined into a single model that must also display realistic dynamics. After much adjustment of diets within a reasonable range, these particular groups were still unbalanced (that is, total predation on the group > total production). Our best recourse at this point was to increase prey biomass of unbalanced groups in the model that was underreported by the biological monitoring program. Ecopath can actually be used in this way to indicate missing prey biomass in the model.

d. Lines 239-240 – I think the P/B ratio is sometimes used to represent total mortality in Ecopath models. Is that what was used here? Why would rates be normalized to P times B? Why is “different natural productivity” used as a justification?

This is correct, Ecopath models assume that the system is at mass balance so all production and mortality must cancel out, and thus the P/B ratio is equivalent to the proportion of the population that dies (total mortality). The single variable PB *is* the ratio of production over biomass, not the product. We acknowledge two letters for one variable is not great notation, but it is unfortunately fairly standard. Where we had said the rates were standardized by each group’s productivity, we replaced “productivity” with “production to biomass ratio” in hopes of making it more clear PB is a single parameter. We also specified that this standardization procedure produces the fraction stated farther down because PB = total mortality rate at mass balance.

e. Bi and Bj should probably be defined right away in the supplement. The supplement refers to Table 1 in two places, but I think Table 2 is intended. Should the supplement give the equation for calculation of M0,i (last sentence of par 2)?

Thank you for the correction, we have changed references to table 1 to refer instead to table 2. We defined Bi and Bj in the supplement. We have also more clearly specified how M0 is calculated.

3. Discussion – the authors do a nice job of relating their findings to other results from the Gulf of Mexico, and include some discussion of considerations for use of the mass balance modeling approach. For ESCO, results need to be placed into a more wide-ranging context to be of widest interest to the readership. I don’t think a lot is needed, but the authors should include another paragraph or two giving wider context to their findings.

We have added a paragraph to the discussion about how fish populations and fisheries have had less clear cut responses to oil spills globally compared to other components of the ecosystem. It begins on line 473.

Reviewer #1: General comments

This manuscript describes a modeling study of two potential mechanisms responsible for the relative stability of fish populations following the Deepwater Horizon spill. Overall, this is a very well-written manuscript and I only have a few comments.

The approach of using an ecosystem-based model to explore direct and indirect effects does indeed seem like a logical means of exploring the questions posed in this study. One issue that seems central is that each predator group was interpreted independently upon a given taxon yet all fishing take was aggregated together. If I've understood this correctly, it seems likely that aggregating all fishing pressure will bias findings toward an interpretation that fishing is more important than the reduction in predation pressure. It seems that aggregating across all predators to create an integrated predation pressure would provide a better comparison to an integrated fishing pressure metric.

This is an interesting point we had not thought about. We think the way figure 3 is presented makes it easy to see that combining all reddish colors in the bars would not allow a combined predator group to “overtake” fishing in the three cases where fishing was dominant. For the GEM analysis, instead of adding the results for the three predator groups we ran an analysis similar to the one for fishing (i.e., allowing for a simultaneous and proportional change in a rate across several groups), this time applied to the three predator per capita production rates (described in methods on line 291, results presented starting on line 336). The new figure has been added to the supplement. This additional figure provided further support for our conclusions.

Similarly, I noticed that there are some predators such as juvenile sharks that have relatively large biomasses (an order of magnitude greater than all bird predators combined), identical TPs and high PB ratios were not included in the milieu of predators considered here. Including such predators and quantifying their top-down effects seems logical but perhaps there is a good reason not to do so? If this is the case, it would be helpful for the authors to note somewhere in the ms why they excluded several other key top predators from their reporting.

This is a great point. We chose the predator groups specifically because there was evidence of significant bird and marine mammal mortality following the oil spill, whereas this is not the case for sharks. So although we agree sharks are energetically important predators, they are not a likely cause of the persistence of fish populations following the spill. We have specified this on line 239.

I also wonder how well the model captures sustenance & recreational fishing pressure? I honestly don't know if there was a moratorium or at least public advisory against these forms of fishing following the disaster. If there was a change in recreational fishing pressure, then this might be expected to have an outsized impact on small sciaenids (among the focal groups here) - particularly for shore-based and sustenance fishing that tend to be the hardest to quantify. Can the authors comment on the role of recreational/sustenance fishing pressure in their model and whether or not they expect that this could affect their results (assuming that it is not in the model)?

To the best of our knowledge, all fishing in Barataria Bay ceased for at least some period of time following the spill (e.g., see 10.1073/pnas.1108886109). The fishing mortality rates attempt to capture all fishing, but we agree they may be underestimates given the difficulty of tracking small-scale and recreational fishing effort. We did discuss this issue and its implications in reference to the shrimp fishery. We have added that it likely applies more broadly on line 410.

Specific comments

Line 125 - please make 'represent' plural

This is fixed

Line 153-154 - please provide Latin names for Sciaenids

Respectfully, we think including latin names for the sciaenids would necessitate also including latin names for all other species in the sentence, and we are unsure why the one functional group was singled out. All latin names (including those for Atlantic croaker, spot, and silver perch) are listed in table S1. We have elected not to use latin names for any of the species in the sentence; however, if providing latin names for all species in the main text is preferred, we would be happy to make the switch.

Line 197 - this assumption seems unnecessary. There are many fitted L-W relationships published, including many on Fishbase.org, that could be used for individual species. At the very least, a taxonomically and morphologically similar species could be used if one doesn't exist (e.g., Atlantic menhaden for Gulf menhaden).

While it’s true that more specific L-W parameters are available for many of the fish species, the use of L^3 is unlikely to be an important source of uncertainty relative to the other assumptions in this part of the model (e.g., stable age distribution) or the uncertainty in biomasses used here. Use of L^3 to estimate weight is also standard accepted practice for Ecopath/Rpath models (i.e., all published Ecopath models that used multi-staza groups have estimated weights in this way) and it would require alterations to the source code to use a different function here.

Lines 219-221 - by how much were each of these functional groups increased? Was it a constant proportion, a constant amount, or did it vary across taxa?

We specified in the text that the amount needed to balance the model varied by group.

Lines 317-318 - this is an interesting finding and I wonder if it is due to a density-dependent response? Are such dynamics explicitly factored into this model or do they arise solely from populations responding to external forces?

We believe the most likely cause is a mesopredator release, especially since many of these are juvenile groups. However, density-dependence is technically possible since the model does include density-dependence in the “other” mortality term. Later in the paragraph we specified that the juveniles (most of the groups were juveniles) experienced high predation pressure from harvested mesopredators.

Reviewer #2: Manuscript Review: ESCO-D-22-00215

Overview:

This paper uses a previously developed food web model and a newly developed generalized equilibrium model to assess whether fisheries closures, predation mortality or both contributed to the previously reported stability of nekton populations after the Deepwater Horizon oil spill. The food web model was based on De Mutsert et al. 2017, built in the Ecopath with Ecosim and Ecospace modeling software, but was re-built in Rpath, a version of the same modeling algorithms, but built in R coding languages. Some updated parameterization was used in develop this version of the model as newer data were available. The paper is addressing an interesting question, of to what degree did the loss of predators in the system contribute to food web resilience when considering and NOT considering the fishing moratorium. I think this paper would be of value to ESCO, but before publication, a significant revision must be completed to make the paper widely approachable to the interdisciplinary

audience of this journal. The authors leave out a myriad of details about the models used in this study and assumed the reader has a detailed understanding of these methods. This assumption leaves many questions about important aspects of ecological modeling like temporal scales and data sources. The authors essentially do not define the generalized equilibrium model and why it is the appropriate tool to use for this study. Many statements in the paper are vague and not backed with citations or more specific details that would help the reader see the innovation presented here. After making the revisions suggested below the paper should be reviewed again to ensure the clarity of the content is sufficient for the broader readership of this journal.

We appreciate the reviewer’s critique of the paper and have tried to thread the needle between providing enough methodological detail that our approach can be understood by most readers, while not making the manuscript overly long. We note that much of the requested information was in table 1 of the main document, as well as the supplementary materials, and that the level of detail we provide here is similar to that in other ecosystem modeling papers.

INTRODUCTION

Line 73: Suggest adding Lewis et al. 2021 here as well

This has been added

Line 99: unclear coreferent. You cannot say 'this' or 'these' without something abundantly clear to which 'this' refers. Restate as a full noun phrase

We have clarified “these initial estimates”

Lines 114 - 116: The sentence here has a lot of "qualifiers" like 'typically' and 'potentially,' and without a citation providing evidence of this type of control, it is not appropriate for a scientific paper. It may be that simply better linking this sentence with the next would get at this concern.

We have added a citation to this sentence. The qualifiers were intentional, and we emphasize that evidence for a phenomenon is not a binary yes/no.

Line 125: Ecosystem modeling represents…..

We corrected this

Line 126: "…pathways as they and allow us….."

We have rewritten this as “modeling allows us”

Lines 144 - 148: The authors say they are addressing two questions, but don't actually state the objectives as questions. The "questions/objectives" are written as phrases that make the interpretation difficult to understand. Suggest the authors re-write for clarity

We have replaced “examine” with “ask” and “explore” with “answer” to emphasize there is a question. We only stated one question, with two approaches to answering it, so have made sure to use “question” singular.

METHODS

Line 157 - 168: The authors fail to provide a clear description of the generalized equilibrium model, and while they provide the citation, for reference and a longer description in the SUP materials, their description lacks a basic and general description of the model, which is problematic, since this journal is not a modeling journal specifically. The authors should take the time here or later in the paper to lay out WHY this tool was used and clearly and specifically say what outputs or inputs of the model were used between the Rpath and the GEM.

We have stated starting on line 164 that the GEM allows us to account for indirect food web interactions in quantifying the impact of changing predation and fishing rates, while avoiding fully projecting the food web through time.

In addition, the phrase "within the general neighborhood of equilibrium" is not clearly defined. I can envision that the authors may discuss this topic among themselves using this language, but in a scientific paper, it is provided without definition and context, leaving the reader having to imagine what is meant by it. If this phrase is going to be used in the paper, the authors should take time to explain why it is used and what it means in terms of the models being used.

In this same section, the questions posed do not need to be put into quotes.

We no longer refer to the neighborhood of equilibrium. We removed the quotes.

Line 171: general comments about the mass-balanced model

While the authors provide a brief and appropriate overview of Ecopath and Rpath, there are many descriptions lacking that would provide context for this specific study. The authors did a good job of describing how the Rpath model differed from the model developed in De Mutsert et al. 2017 in terms of new functional groups, but authors should also make explicit statements about the other input parameters in the model to let the reader know that there either was no difference in the P/B, C/B, etc in the Rpath version. This information should not be left to the reader to wonder. For the folks not familiar with EwE or Rpath, more information is warranted here.

The sources for each input parameter (Biomass, PB, QB, EE, removals) are available via superscripts on table 1 in both the previous version of the manuscript and the current revised version. Repeating this information in the text does not seem like a good use of limited manuscript space - however we added a brief statement on line 201 noting that this additional information is contained in table 1.

1. There is no time scale provided for what period the models represent, esp since newer data were added to the Rpath, it would be important to say what time period those new data are from, in addition to explicitly stating the time period for all the data in the model. The De Mutsert model was calibrated on data that were BEFORE the oil spill, so if only the mass-balanced model was used, these data do not represent the time after the oil spill, making the entire analysis and manuscript problematic. This is a severe oversight and needs to be addressed before publication

There are two potential issues related to time scale: First, the rates are all annual as the models are based on an annual time step, this is stated on line 187. Though note that both Rpath and the GEM are equilibrium-based approaches, so there is no time scale in that sense. Second, we intentionally chose a model parameterized to represent the food web before the oil spill - a point that we have emphasized in the text on line 202. We actually contend that using a model parameterized after the spill would be the more problematic choice. Namely, to understand how the system responds to the perturbation of the oil spill, it should be parameterized to represent conditions prior to the spill and then, for comparison, modified to represent known or hypothesized oil spill impacts. This is what we have done.

2. De Mutsert et al. 2017, used Ecosim as a calibration and analyses were performed in Ecospace. There was no mention that the Rpath model (the static mass-balanced module) was the only one used in this application. Was Ecosim used? It is not clear if there was a temporal aspect in this study, meaning if the models were run through time—this issue is connected with the lack of temporal clarity from earlier comments.

We did not simulate the model through time (e.g., in Ecosim), but instead compared the pre-spill mass balance to post-spill impacts. We have clarified this on line 266. Without consistent pre and post spill time series data for most species, a dynamic Ecosim model cannot be adequately tied to empirical data and our simpler approach is more parsimonious.

3. Line2 212 - 213: these rates should be cited or referenced if they were provided in an appendix or supplementary materials. All sources of information/parameters should be cited in the SUP materials.

All citations for these rates are cited in table 1 via the superscripts. We have added a reference to table 1 at the referenced location.

4. There is no mention of how diet matrix was created and the sources of the diet data. Was it exactly how the De Mutsert model represented trophic interactions? Likely not, because new functional groups were added so new trophic interactions were thus also added. Where did those diet data come from for the new Rpath version. Given that primary objectives in the model address predation and predation mortality, it is of utmost importance to provide the diet matrix that is driving these interactions and the sources of those data. This is a substantial oversight from the authors and will need to be addressed before publication.

We have added citations regarding diets to table S1.

5. Where do the fishing effort data from and what time period do they represent? Since these data are central to one of the objectives of the study, not stating the source and timeframe is a large oversight.

Fishing effort sources were cited in Table 1. They are mainly drawn from De Mutsert et al. 2017, so represent the time period shortly before the oil spill, as was intentional.

6. Did the authors use anything like PreBal (Link 2010) to determine if the Rpath model was within reasonable parameterization before moving onto next phases of analysis. I think this procedure would strengthen the manuscript.

We did not use PreBal explicitly. Many of the diagnostics are meant for large marine ecosystems, and not estuaries like the one modeled here. However, we did check various diagnostics and they generally meet recommendations. For example, there is a downward trend in the log(QB) and log(PB) with trophic level, the former particularly when ignoring homeotherms (i.e., birds and dolphins).

Line 243: Generalized equilibrium model. As mentioned above, the authors must provide describe what a GEM is and provide context as to why it is used in this study. This aspect of the study also suffers from a lack of clarity in terms of temporal scale. Are these models run over time? If so, what time period. If not, this aspect needs to be stated much more clearly. Also, what specific outputs from the Rpath model are used in the GEM or visa versa? Also, the whole idea of "in the neighborhood of equilibrium" is far to colloquial for a scientific paper. What exactly, quantitatively, does this mean and why is it important here? Even though the authors cite the paper in which the methods are derived, because this is not a modeling journal a much more specific explanation of these methods is warranted for the ESCO readership. I understand there are supplementary materials for this model, which is helpful for the algorithms that are used, but not helpful as to why this tool

was chosen specifically. It should be stated in a way that a non-modeler could understand.

We have revised this subsection, particularly its first paragraph. Per the editor’s recommendation, we have used more consistent notation between the main text and supplement, to help in clarifying which parameters in the GEM are taken from the Rpath model. We have adjusted the language and removed use of “neighborhood of equilibrium.”

Line 246: add comma before word "which"

We have added a comma

Line 263: unclear coreferent. You cannot say 'this' or 'these' without something abundantly clear to which 'this' refers. Please restate as a full noun phrase

We have specified “this simplification”

Line 276: This is the first mention of a socioecological system, if this term is going to be used in the methods, it should be clearly stated and defined in the introduction, that fishing is the "socio" part of the SES.

In the introduction we had referred to a “linked human-natural system” on line 90. We have changed the language to “linked socioecological system.”

Line 278: unclear coreferent. You cannot say 'this' or 'these' without something abundantly clear to which 'this' refers. Please restate as a full noun phrase

We have specified “this Monte Carlo procedure.”

RESULTS

Lines 286 - 288: Without an understanding of if the models were run through time and without knowledge of where the fishing effort data came from and the amount of fishing effort data, the results are less compelling because the reader has no way of knowing how the input data are driving the output.

The input data are cited in table 1.

Lines 295 -296: How do we know if the data are insufficient if we do not know the data sources?

The input data are cited in table 1.

Line 297: "Seabird predation most impacted menhaden biomass most out of the five…."

We have deleted the first “most”

Line 303: what do the authors mean by "more integrative" GEM model? As compared to what? Please clarify

We have clarified that it integrates across all food web interactions.

Lines 305 -306: can the "large positive responses" be quantified here? Large as compared to what? Without values, at least comparing functional groups to each other, then how does one know if the responses are large if there are no quantitative values assigned with it. I understand that statistical significance can not likely be used, but percent change would be a viable option here as well.

We have added the actual percent changes parenthetically

Line 312: unclear coreferent. You cannot say 'this' without something abundantly clear to which 'this' refers. Restate as a full noun phrase

We have specified this response to dolphins

Line 312: "…on direct mortality of Menhaden….."

The period between “direct mortality” and “menhaden” is intentional.

Lines 312 - 314: This entire sentence reads awkwardly and it is difficult to interpret the author's meaning here.

We have slightly rephrased some of the text in this paragraph so that it is more clear that we present a set of GEM results, then compare them to the Rpath-only analysis. Then move onto a new set of GEM results. We see how this organization was not clear in the original version.

Lines 314 - 315: It is unclear what the authors are referring to with the "50% intervals did not cross zero" ….this entire sentence assumes that the reader knows more than we do. These figures need to be better explained in the methods or first directed how to interpret them here, before providing the results.

Instead of referring to 50% simulation intervals we refer to the middle 50% of simulations. We refer the reviewer to the figure captions for further description. We have lightly edited the figure captions for clarity.

Lines 316 - 317: It is not clear what is meant by "uncertainty in functional responses." Since this is a topic sentence of a paragraph, it must be made clear what is meant by uncertainty. Also, the phrase, "..high uncertainty in how groups less impacted by direct mortality would respond….." needs to be rephrased for clarity.

Line 320: "….experienced less direct fishing effort pressure…."

We have changed “effort” to “pressure”

Lines 324: Any time the phrase "due to" is used in the results, it should be moved to the discussion because it starts to explain why we are seeing the results we are seeing.

Respectfully, we feel this is a stylistic preference.

Line 325: What function groups are the authors talking about here? It is prudent to remind the reader since there is a lot of moving parts in the results.

We have specified that the functional groups combined were juveniles and adults. This is on line 352.

Line 328: "….,the 50% simulation interval includes resulted in positive responses in biomass."

We have edited this sentence and clarified the responses were in biomass.

Lines 328 - 330: The sentence beginning here should be limited to presenting the results, not explaining why the results occurred—please remove the phrase that belongs in the discussion.

We have eliminated the final phrase of the sentence altogether (“in response to increases in seabird mortality”).

Line 333: The authors use modeling jargon that will not allow the broader readership to interpret results. What does "80% simulation intervals" mean and what is the significance of overlapping with zero. Also, the "slight positive responses" should be quantified again, as compared to what? Without values reported here, we are "just taking the authors word for it."

We adjusted the language to refer to the middle 80% of simulations. We have eliminated the word “slight.” Instead of overlapping with zero we refer to the middle 80% of simulated responses containing only positive values or containing both positive and negative values.

Lines 342: This topic sentence is vague and the supporting sentences do not elaborate on the vague statement. The authors should provide specific functional groups or species that they are referring to and describe what is meant by the term "influential," as that does not provide any context to the reader. Increases? Decreases? Changes? Please be specific or remove this paragraph all together.

We have revised the paragraph (which begins on line 371 in the revised version) for clarity. Specific functional groups are mentioned on lines 374-5, where we also define “influential.” We replaced the second instance of “influential” in the topic sentence with “led to responses of a greater magnitude” which is further expanded upon in the final sentence of the paragraph.

DISCUSSION

Lines 358 - 360: It remains difficult to draw conclusions as stated here when the data sources are not provided for most of the model parameterization.

The data sources are documented in table 1.

Line 361: The biomass of what species is more sensitive to small perturbations? Please be specific. Also, the source of the fishing effort data would also help provide context for the results and discussion.

We are speaking generally in this case, not about particular species. The data sources are documented in table 1.

Line 363: Please define what is meant by functional responses. In Ecosim, functional response and response curve could sometimes be used interchangeably, but it is not clear what is meant in this context…please define this concept early in the paper so it carries through the entire narrative.

On line 264 we have defined a functional response as “how predator diets respond to changing prey abundances.”

Lines 367 - 368: The rationale that "fishing likely" played a stronger role is a bit weak without more quantitative references to the findings and without the context of the data that parameterized the models in the first place.

The data sources are documented in table 1.

Line 369: Saying that the Gulf Shrimp fishery has a strong impact on target and bycatch populations is correct, but no data are provided to quantify this, nor was any citation referenced. A theme through out this manuscript is that the authors fail to provide sources of data or references to the data on which all their modeling data are built. This oversight calls into question the entire paper as the reader has no idea about how the model is parameterized.

The data sources are documented in table 1. This specific sentence is followed by a paragraph of support for the statement, all of which is well-cited.

Lines 375 - 377: This sentence should be rephrased for clarity

Line 378: Are these rates reflected in the input data of the models? It is not known to the readers since those information were not provided.

The data sources are documented in table 1.

Line 383: Do the input data capture the benthos data referenced here? It is not known to the readers since those information were not provided.

The data sources are documented in table 1.

Lines 384 - 385: This statement doesn't really provide an explanation of the responses, because the specific responses are not discussed or referenced here, it appears to be all speculative, which can be sufficient, but since we do not have the data sources for the models, it makes drawing these conclusions much more problematic.

The data sources are documented in table 1.

Lines 390/393: repetitive use of the phrase, "In our model…."

We deleted its first use.

Line 398: is predatory biomass, dolphin biomass only? Please be specific

This is a pervasive issue for all predators, the more general word was intentional.

Lines 399 - 400: The sentence beginning with, "Thus, any attempts…." Is vague and needs more clarity

We replaced “do so” with “estimate mortality”

Line 400: unclear coreferent. You cannot say 'this' without something abundantly clear to which 'this' refers. Restate as a full noun phrase

We have joined this sentence with the previous one to make this more clear.

Line 404: Is this predator population the dolphin population, please be specific

We intended predator population, not dolphin population. We note here the parenthetic mention of seabirds in the first half of the sentence.

Lines 412 - 418: It would be important to reference the source of the diet data for the bird populations in the model and the diet matrix provided in the Supplementary Materials.

We refer to our previous responses to this critique. We have now cited the diet matrix.

Line 428: unclear coreferent. You cannot say 'this' without something abundantly clear to which 'this' refers. Restate as a full noun phrase

We have specified this redundancy.

Line 449 - 450: Why do the authors make the assumptions that the marsh loss would not impact the importance of predation and fishing? More explanation is needed.

We have emphasized that it would be expected to impact the absolute responses, but not the relative importance of predation versus fishing, and explained this is because neither the predators nor the fishery rely directly on marshes.

Line 453: Believe is not generally a word we use in scientific writing, suggest changing to "contend"

We have made the recommended change

Line 456: "quite wide" compared to what? No context given is problematic

We have specified “the simulated ranges of responses for the generalized equilibrium model were so wide that even plotting the range of the middle 80% completely visually obscured the center of the distribution.”

Line 457: unclear coreferent. You cannot say 'this' without something abundantly clear to which 'this' refers. Restate as a full noun phrase

We have specified this wide range

Line 458: This section is the first mention of "saving models" so the reader has no idea what the authors are talking about if they are not familiar with how the modeling process works, which is not explained in the main text.

We have avoided the language of “saving models” as we acknowledge it can be alarming without previous mention. We instead refer to the set of models with responses in the middle 95%. We also refer the readers to the cited figure.

Lines 452 - 463: The issue of not knowing the time period of the data comes up in this section again—as conclusions are made, but I can't help what wonder what the time period is of the data were used to parameterize the model

The data sources are documented in table 1.

Line 466: If the authors are going to mention socioecological system in the discussion, and please be sure to define it in the context of this paper in the introduction

In the introduction we had referred to a “linked human-natural system” on line 90. We have changed the language to “linked socioecological system.”

Lines 474 -475: This conclusion is not novel, as even pointed out by the authors in other locations in the manuscript, so it should be cited.

We have added a citation to Van der Ham and De Mutsert, 2014.

Line 480 - 481: It is not clear what is meant by the phrase, "….once all food web linkages were accounted for." Please clarify.

We have deleted this phrase.