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Dr. Michael Boufadel, Dr. Francois Galgani, Dr. Gui-Peng Yang

Co-Editors-in-Chief

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We are pleased to resubmit our manuscript, “Initial estuarine response to the nutrient-rich Piney Point release into Tampa Bay, Florida” to be considered as an original research article in Marine Pollution Bulletin.

An earlier draft of this paper was submitted in December of last year and we received comments from three reviewers. Although the initial decision was to reject the manuscript, we contacted Dr. Boufadel about the quality of one of the reviews and we were encouraged to revise the draft for resubmission. We have carefully considered comments from all three reviewers of the original submission and have made substantial revisions in response to these comments. Overall, we have made many edits to improve clarity of the text and have shortened the content in many locations. The point-by-point responses are detailed below and we have included a version of the revised manuscript with track changes to aid in the review. We are confident these changes have addressed the concerns of our reviewers.

The impact of this event on Tampa Bay is important both regionally and nationally to raise awareness of how insufficient regulatory oversight and planning can lead to unintended environmental impacts. We stand by our conviction that these events need to be documented in the primary literature to advance the conversation on how legacy mining facilities can be safely and responsibly closed. We appreciate the opportunity to publish this important work in Marine Pollution Bulletin.

Sincerely,



Dr. Marcus W. Beck

Program Scientist

Tampa Bay Estuary Program

We sincerely thank the reviewers for providing comments on our manuscript. We provide a point-by-point response to these comments below. In general, most of the revisions address concerns raised by all three reviewers, including 1) numerous edits to improve the writing for clarity and brevity, and 2) additional information on sampling methods. We have also addressed concerns of Reviewer 2 about potential nutrient cycling by adding and expanding a section in the discussion and concerns of Reviewer 4 regarding additional detail on the simulation modelling.

All page and line numbers in the reviewer comments refer to the original submission. Page numbers noted in our response refer to the revised submission.

## Reviewer 1

The authors (a very long list of authors) have described the sequence of events that followed a phosphate mining spill. The paper is long and tedious- and in the end cannot conclude any strong relationships with red tide occurrence. I wish I could be more positive about this paper, but it has the feeling that authors were rushing to get something in press, rather than taking the time to write a well crafted manuscript. There are some very good writers on the list of authors- so I am a bit surprised that they all agreed to this final version.

* **Response**: Thank you for your comments. We have made revisions to improve the writing, by reducing length in many locations and making additional edits where noted. Many of the edits in response to the specific comments from the other reviewers are used to address some of the comments below.

graphic abstract- much too complicated; fonts too tiny, too much text

* **Response**: The graphical abstract was simplified and the font size increased.

highlights- the term Piney Point is meaningless except for locals

* **Response**: The first highlight was changed to “186 metric tons of total nitrogen from wastewater were added to Tampa Bay.”

abstract- poorly written

* **Response**: Abstract was edited.

Introduction- needs a complete rewrite- it takes more than 3 pages to get to the point that there was a spill. The general background is interesting but you need to grab the readers attention first.

* **Response**: Introduction was shortened and edits were made to improve clarity.

p. 5- inadequate explanation for why the focus on N. Why not both?

* **Response**: Please see our response to a similar comment from reviewer 4. Many studies, as well as the successful nutrient management paradigm for Tampa Bay, have demonstrated that Tampa Bay is nitrogen-limited. Phosphorus trends are also provided in the supplement.

Methods- the first paragraph on monitoring is all about modeling, not monitoring

* **Response**: We have added a “Modeling” sub-section at the beginning of the methods.

p. 7- how were the Karenia data quantified…they didn’t just appear in a database.

* **Response**: Text was added: “HAB sampling typically occurs in response to bloom events or fish kills with extensive quality control of cell counts conducted by FWC-FWRI (additional details in [Stumpf et al. 2022](#ref-Stumpf22)). HAB data were restricted to Tampa Bay boundaries and over 90% of the samples were collected within one meter of the surface.”

p. 7 - eyes on seagrass is meaningless (except for locals)

* **Response**: We have removed this from the text.

stats section- poorly written, confusing, wordy

* **Response**: Please see the responses to reviewer 2.

Results- rambling, disorganized and non-quantitative. Paragraphs don’t make sense (try using topic sentences and structuring concepts accordingly)

* **Response**: Please see the responses to reviewer 2, particularly regarding the first section of the results.

Discussion- I was left wondering what I learned. No conclusions drawn

* **Response**: Please see the responses to reviewer 2, particularly regarding the edits to the discussion sections.

In all, it seemed like an interesting exercise to describe all the patterns, but this does not make a quality manuscript that will stand the test of time.

* **Response**: We are confident that the edits made in response to your comments and those from the other reviewers have made this manuscript a more compelling and valuable contribution.

## Reviewer 2

Beck and colleagues report on the short- and mid-term effect of a very large nutrient-rich release from Piney Point. The authors focus on a few water quality indicators, but also include data for several additional responses (e.g., seagrass coverage). The results are largely what you would expect, initial rapid response of phytoplankton with cascading secondary responses.

* **Response**: We really appreciate your comments. Please see the detailed responses below.

First, a few of the things that make this a worthwhile contribution to MPB. The authors should be commended for their quick response to data collection and the large number of data streams that were brought to the study. It really is quite impressive. Further, compared to citizen monitoring, I expect the data is high quality and consistent. Second, the writing was quite good; there were some sections where flow or organization could be improved (beginning of results; last section of discussion); however, these considerations are very minor.

* **Response**: Thank you for pointing out the merits of this paper and providing suggestions where the writing can be improved. We have edited the sections you point out for brevity and clarity.

Second, a few things that could be improved or warrant some critique. What is the rationale for the lack of continuous line numbers? Journal suggestion? Reviewing is so much easier when line numbers line up with the text and continue throughout the document. I realize this is not something that should be fixed now, but it is bothersome.

* **Response**: Unfortunately, the line numbers are generated automatically by the journal submission system and we have no control over how they are presented. We appreciate your use of both page and line numbers in your comments to reference text locations.

That said, my one major critique of the manuscript, one that is difficult to reconcile with the available data, is the speculative nature of last section of the Results, titled Potential nutrient cycling. This is clearly a very important section and, in my opinion, warrants additional consideration. The title of this section is correct, yes; thus, I would like to see 1) this section moved to the discussion, 2) the MAJOR lines of evidence supporting this thesis outlined and discussed consecutively in one section, 3) major alternatives entertained and addressed. The authors might also think about what it would take to make the connections more explicit, if possible (stable isotopes, modeling, etc.).

* **Response**: Thank you for this comment. We agree this section is important, although it is definitely speculative without more quantitative results that definitively link the pieces. As such, we have moved the content to the discussion and expanded to text to include a more nuanced description of the trends, including what information may be needed to support the narrative. This includes information from the final paragraph of the discussion, which was shortened and combined with the prior paragraph. The new section begins on page 18.

MINOR COMMENTS/SPECIFIC SUGGESTIONS

P3/L56: Recommend removing “in other countries.” When I read that passage it seems to suggest a broader critique, one that is more political. That may not have been your intention, but that was how I read it.

* **Response**: Our point was to suggest that mining products in Florida are generally exported and have little benefit for the local communities. However, this may be a bit editorialized and was removed.

P4/L21-24: I am not sure if “unanticipated releases” captures the spirt in which these releases occurred. In fact, the title of the FWC report was “Response of estuarine nekton to the regulated discharge of treated phosphate-production process water.” This regulated discharge seemed to occur after extensive consultation with the EPA.

* **Response**: Agreed, we have removed “unanticipated.” We agree that the releases often occur after consultation or approval from regulatory bodies, but they are generally unanticipated in that long-term site management has not planned for these events, nor the potential impacts on the environment.

P4/L51: worth indicating that these concentrations, especially for TP, are several orders of magnitude greater than typical surface waters?

* **Response**: The sentence was edited: “Water quality of NGS-S measured in 2019 showed total phosphorus (160 mg/L) and total nitrogen (230 mg/L) were approximately three orders of magnitude higher than typical concentrations in Tampa Bay.”

P4/L56: deliberately “released”

* **Response**: Changed to “From March 30th to April 9th, approximately 814 million liters (215 million gallons) of stack water were released to lower Tampa Bay following an [emergency order](https://floridadep.gov/sites/default/files/21-0323.pdf) authorized by the Florida Department of Environmental Protection (FDEP).” A similar, redundant sentence earlier in the paragraph was removed.

P5/L4: I see the use of the term lower Tampa Bay; how does that compare to Figure 1, the areas of interest? Ultimately, lower Tampa Bay is used extensively, but I don’t see it defined. Also, is this region ecologically based or defined by currents?

* **Response**: The boundary between middle and lower Tampa Bay was added to Figure 1. These boundaries are used by the Tampa Bay Estuary Program to track annual attainment of water quality targets under a Reasonable Assurance plan for TMDL reporting to the Florida Department of Environmental Protection. We do not define the rationale for the boundaries in the main text because the regulatory delineations of the bay are not of interest to a broader audience, but we use the delineation as it is useful for our discussion of trends in the paper. Our area delineations (areas 1 - 3) were meant to provide a more relevant grouping for the paper.

P5/L22-27: I think a citation may be warranted here; while predictable, numerous studies ultimately supported the statement that nitrogen is limiting in Tampa Bay.

* **Response**: Agreed, we have revised this statement to include additional citations and explanation:
* “Numerous studies, as well as the successful nutrient management paradigm, have demonstrated nitrogen-limitation in Tampa Bay and the system is generally considered phosphorus enriched ([Wang, Martin, and Morrison 1999](#ref-Wang99); [Greening and Janicki 2006](#ref-Greening06); [Greening et al. 2014](#ref-Greening14)). As such, we focus on nitrogen in our analyses as the identified limiting nutrient for Tampa Bay and its potential to create water quality conditions unfavorable for seagrass growth due to enhanced algal production.”

P5/L29: interesting that discharge was to Port Manatee and not Bishop Harbor like the release discussed previously.

* **Response**: Yes, we believe the intent was to minimize impacts to sensitive back-bay habitats that were observed in previous releases, although we have no information to verify this claim and have not included it in the text.

P6/L24-39: that is an impressive and extensive list of agencies and partners; any potential drawbacks to so many data streams?

* **Response**: Data quality assurance and control was a primary concern with multiple partners collecting information. We have included additional information about the data collection procedures and how the Tampa Bay Estuary Program worked to facilitate quality of the data. Please see the additions starting on page 6.

P6/L54-65: is it routine to take water samples directly from the surface? Ultimately, were the authors worried about any potential stratification, particularly of Karenia brevis.

* **Response**: Most of the water quality and phytoplankton samples were surface collections following standard sampling methods used by our partners (see the text addition about sampling). Tampa Bay also has a very shallow average depth of just over 3m and is not strongly stratified for most of the year. So, we feel confident that the majority of samples used in our analyses were generally representative of conditions in the water column. Anecdotally, benthic fishes were observed during the fish kills (e.g., eels, rays), so it is likely that *K. brevis* was present throughout the water column, although we have very little quantitative data to describe the vertical distribution. Please see the addition of information on *K. brevis* sampling at the top of page 7 in response to reviewer 1.

P7/L32-37: why get data from Tampa Airport and the Airfield in St. Petersburg? There is an NWS location not far from Piney Point.

* **Response**: We have updated our weather data to include wind and precipitation from Albert Whitted Field in St. Petersburg and have excluded the precipitation data from Tampa International Airport. Our initial rationale for using data from Tampa was a longer period of record. However, we have revised the historical baseline in the plot to include only years 2006 to 2020, consistent with our comparison of the water quality data to historical observations. Also, the NWS location is in Ruskin, Florida and is farther from the bay proper than the location in St. Petersburg.

P9/L27-29: not sure what to make of the aggregating data to weeks or months; isn’t there a way to maintain the actual sampling regime?

* **Response**: The weekly aggregation was only done to allow plotting and comparison between variables on a common scale (e.g., Figure 6 and 8 in the revised text). None of the statistical tests used data with fixed sample dates. We removed the text because the statement is inaccurate and somewhat misleading.

P9/L49: This is a bit confusing; is this comparing “between” pre and post?

* **Response**: No, these tests were meant to compare all observations post-release to help identify periods of time when observations differed, e.g., when chlorophyll was at its maximum. The text was revised for clarity.
* “Differences in observations between months for April to September for water quality, seagrass, and macroalgae within each area (Figure 1a) were evaluated using a Kruskal-Wallis one-way analysis of variance (ANOVA) followed by multiple comparisons using 2-sided Mann-Whitney U tests ([Hollander, Wolfe, and Chicken 2013](#ref-Hollander13)). These tests were used to statistically characterize the temporal progression of changes in the bay following release from Piney Point, e.g., were July conditions significantly different from April?”

P11/L12-14: as personal preference, I see no reason to lead off the results with a general statement about the location of a figure summary. Instead, the authors might reference the figure when they describe, in general, that a bloom was observed ~2 weeks after the release began, red tide was observed within x weeks, the bloom peaks, and conditions become stable on xx.

* **Response**: The first section of the results describing the timeline was removed and text was placed in the appropriate sections that followed. The timeline is now referenced in the first paragraph of the discussion to provide a synthesis of the main results that were described.

P11/L20-: this first part of the results has a choppy structure

* **Response**: Please see the response to the previous comment.

P11/L31-34: the authors indicate the Tampa Bay influx was related to an ongoing coastal bloom; is this indicating the red tide bloom was ongoing prior to the Piney Point discharge? Also, not clear what “related” means in this context. This important statement may require additional context.

* **Response**: Yes, red tide was observed in the Gulf of Mexico prior to the occurrence in Tampa Bay. Content in the red tide section was edited to make this clear (page 15):
* “This first Tampa Bay influx originated from an ongoing coastal bloom in the Gulf of Mexico, as is common when red tide is observed in the bay ([Steidinger and Ingle 1972](#ref-Steidinger72); [Flaherty and Landsberg 2011](#ref-Flaherty11)).”
* Blooms in Tampa Bay originate from *K. brevis* that is transported in from the Gulf of Mexico. An important distinction that we make in this paper is that, although blooms in Tampa Bay are not uncommon, the severity of the 2021 bloom was very likely related to the favorable conditions in the bay prior to July. These favorable conditions were described in the paper as 1) nutrient availability from Piney Point, and 2) high salinity conditions from low rainfall. The passage of tropical storm Elsa was also a confounding factor. It is nearly impossible to identify a single factor causing the observed conditions, but we describe the considerations in the discussion (page 23).

P16/L26: not sure I see the rationale for analyzing the data using Braun Blanquet estimates, which, in my impression, was developed as a rapid assessment

* **Response**: Seagrass transect monitoring in Tampa Bay has occurred annually since the mid-1990s using similar methods as in this paper (i.e., [E. Sherwood et al. 2017](#ref-Sherwood17)). The primary difference for the annual monitoring is the transect length, which is typically much longer than the 50m used in the 2021 rapid design. This shorter distance was chosen to allow for quicker sampling times and greater coverage in response to the rapidly changing conditions in 2021. Thus, the rapid design provides data that are similar quality to the long-term data, with notable exceptions indicated in the manuscript (i.e., no seasonal estimates). Our rationale for evaluating Braun Blanquet coverage was to verify the results obtained evaluating frequency occurrence estimates. The text was modified to make this clear (page 15):
* “Tests using Braun Blanquet cover estimates confirmed the results from the frequency occurrence estimates (Tables S3, S4).”

P17/Potential nutrient cycling: Unfortunately, much of the latter portions of this section are speculative and better presented in the discussion. Further, it would have been nice to see something a bit more concrete linking the nutrient shift from photoplankton to macroalgae and release of macroalgae nutrients to K. brevis; stable isotopes perhaps; the nitrogen sources could have been highly enriched

* **Response**: This section was moved to the discussion, please see the response to your general comment above.

P18/L18: or a shift in the relative availability, N:P

* **Response**: Added text “…or changing availability of nutrient ratios creating favorable conditions for macroalgae growth ([Valiela et al. 1997](#ref-Valiela97); [Cohen and Fong 2006](#ref-Cohen06)).”

## Reviewer 4

This is an interesting study in that it utilizes a quasi-Lagrangian approach to quantify the influence of a nutrient-rich waste plume on an estuarine ecosystem and is the first that I am aware of that documents the impacts of the Piney Point episode. Nonetheless, there are some points that require clarification to give the reader confidence in the findings. In general, more details are needed on several aspects of the methodology, and I provide specific details below on this as well as other issues.

* **Response**: Thank you for your constructive comments. Please see the detailed responses below.

Page 1, Line 38 - “Elevated levels of phytoplankton” - needs to be clearer… is this referring to biomass or abundance?

* **Response**: This line was changed to “An initial phytoplankton bloom (non-harmful diatoms) was first observed….” This was based on chlorophyll measurements in the vicinity of Port Manatee and cell concentrations enumerated to taxa from laboratory analysis. The word limit for the abstract prevents these details, but they are presented in text (page 18).

Pages 6, Lines 4-22 - The sampling regime was largely dictated by output of a model of plume evolution. It would be useful to see more details on how the model is calibrated, and if/how its output is cross-verified. In other words, are field observations of T/S compared with model output to evaluate its performance in terms of accuracy of plume location? This seems like critical information for assuring that the sampling regime actually followed the plume.

* **Response**: TBCOM was previously tested for veracity against in situ observations (e.g., [Chen et al. 2018](#ref-Chen18)). A prior version upon which TBCOM was constructed was also veracity tested and used in several studies (Zhu et al., 2015[a](https://doi.org/10.1007/s12237-014-9815-4),[b](https://doi.org/10.1007/s12237-014-9793-6),[c](https://doi.org/10.1016/j.csr.2015.07.001), and [Chen et al. 2019](#ref-Chen19)) More systematic model/data comparisons were also reported in a recent PhD dissertation (J. Chen, Univ. of South Florida, 2021). Thus, prior to this application, TBCOM was found to perform well in simulating the circulation in the Tampa Bay, and TBCOM has provided publicly available, daily nowcast/forecasts (<http://ocgweb.marine.usf.edu/~tbm/index.html>) for the past four years.
* More detailed model description or analysis is beyond the scope this work. A dedicated manuscript is being prepared to document the TBCOM tracer model as a rapid response to the Piney Point event. Preliminary results can be found in CERF and AGU presentations:
* Liu, Y., Weisberg, R.H., Zheng, L., Sun, Y., Chen, J. (2021), Nowcast/Forecast of the Tampa Bay, Piney Point Effluent Plume: A Rapid Response, Abstract (OS35B-1036) presented at AGU Fall Meeting, New Orleans, Louisiana, December 2021.
* Liu, Y., Weisberg, R.H., Zheng, L., Sun, Y., Chen, J. (2021), Nowcast/Forecast of the Tampa Bay, Piney Point Effluent Plume: A Rapid Response, Coastal and Estuarine Research Federation (CERF) 2021 Virtual Meeting, November 2021.
* The main text was edited to include some of the above details (page 5): “The modeled plume evolution web product (<http://ocgweb.marine.usf.edu/~liu/Tracer/>) served as the principal guidance for coordinating the data collection during the event. Preliminary model results for Piney Point are reported in [Liu et al.](#ref-Liu21) ([2021](#ref-Liu21)) and previous model veracity testing was described in [Chen et al.](#ref-Chen18) ([2018](#ref-Chen18)) and [Chen et al.](#ref-Chen19) ([2019](#ref-Chen19)) (and references therein)”

Page 6, Lines 46-54 - More information is needed on laboratory procedures. In particular, with many different entities running samples and presumably utilizing different analytical approaches for each variable, it is important to know how comparable the data is between programs.

* **Response**: Our initial submission included supplement material that described the sample collection and processing in more detail. We have moved that content from the supplement to the main text, beginning on page 6:
* “Most samples were surface collections by boat, with sample frequency approximately biweekly for locations around Piney Point, although effort varied by monitoring group and was more consistent during the first three months after the release. Established laboratory and field sample protocols for all survey methods were based on an [Interagency Monitoring Project Plan](https://drive.google.com/drive/u/0/folders/1oBGvjdve-Gpo4Kn3Ovn8a8-yVoP25eec) maintained by the TBEP and those of the inter-agency partners. Data quality objectives followed guidelines outlined in the USEPA-approved TBEP Data Quality Management Plan ([E. T. Sherwood et al. 2020](#ref-tbep1620)). Many of the local partners also participate in the Southwest Florida [Regional Ambient Monitoring Program](https://tbep.org/our-work/boards-committees/technical-advisory-committee/#ramp) (RAMP) that ensures similar standards and protocols are followed in the collection and processing of monitoring data, including routine cross-reference of split samples between laboratories to check precision of measured values. Samples requiring laboratory analysis (e.g., nutrient assays) were obtained primarily from bottle collection at the surface, whereas in situ measurements were available for many parameters (e.g., dissolved oxygen, Secchi depth, etc.). In situ measurements were collected using common monitoring equipment, such as YSI sondes or Seabird CTD casts, depending on monitoring agency. Laboratory methods used to process samples were based on accepted procedures promoted through the Southwest Florida RAMP.”

Page 7, Line 22 - Were the breakpoints for K. brevis, or total phytoplankton abundance?

* **Response**: These were applied to K. brevis. The sentence was revised: “Bloom sizes for HAB species were described qualitatively as low/medium/high concentrations…”

Page 7, Line 37 - What about from ungauged flows?

* **Response**: Our methods for estimating inflows into Tampa Bay are based on those similarly presented in a technical report published by the Tampa Bay Estuary Program ([Janicki Environmental, Inc. 2012](#ref-Janicki12)). This report provides the basis for compliance assessment of our TMDL for reporting to the Florida Department of Environmental Protection and represents our best estimate at hydrologic load inputs to the bay. We have provided a citation to this report in the paper.

Page 11, Line 30 - Need to put Anna Maria Sound on the map. Likewise for Port Manatee.

* **Response**: Locations were added to the map.

Page 12-14, Water quality trends section - Very little attention is given to the role of the hurricane that hit the area in early July. Would be interesting to hear more about its effects on water quality in the study area.

* **Response**: As noted in the red tide section and discussion, Tropical Storm Elsa was a confounding factor when interpreting drivers of the red tide event. It is challenging to disentangle an isolated effect of the storm on the red tide and the same could be said for the broader suite of parameters that were used to assess water quality. For example, a clear chlorophyll spike in early July coincided with the red tide peak, which also coincided with passage of the storm. A lack of continuous monitoring in the bay also prevented a more comprehensive assessment of potential storm effects. However, water quality conditions were changing rapidly from the red tide, preceding the arrival of the storm by several days, and we consider the water quality conditions at the time of passage to be driven primarily by the *K. brevis* bloom in the bay. We have added some additional text describing these interpretations in response to your comment below about dissolved oxygen and fish kills.

Page 14, Macroalgae and seagrass trends section - This appears to be one of the weaker components of the project, although it is outside of my expertise. My biggest concern is that it is not clear to me how useful “% occurrence” is. As far as I can tell, this is not equivalent to biomass or areal coverage. Perhaps I am mistaken? Regardless, it would be useful to see a better justification for this metric. Also, how did the 2021 data compare to historic spatial-temporal trends?

* **Response**: Frequency occurrence is a routine reporting metric used by the Tampa Bay Estuary Program to summarize the long-term annual transect monitoring (see [Johansson 2016](#ref-Johansson16); [E. Sherwood et al. 2017](#ref-Sherwood17)). Additional text on page 10 was added to make this clear:
* “Frequency occurrence estimates were used to evaluate macroalgae and seagrasses as a standard metric used in previous analyses in Tampa Bay ([Johansson 2016](#ref-Johansson16); [E. Sherwood et al. 2017](#ref-Sherwood17)).”
* We realize that additional metrics could have been used to summarize the data and we presented trend assessments using the Braun-Blanquet abundance estimates as confirmation for the results from the frequency occurrence analyses (Tables S3 and S4). However, this does not provide an assessment of biomass or coverage. Weight data were collected for select macroalgae samples during the 2021 surveys and a preliminary analysis (see the Seagrass -> Biomass estimates tab [here](https://tbep-tech.github.io/piney-point-analysis/trend)) suggested similar trends as the frequency occurrence results. Because we did not have comparable weight data for seagrasses, the analysis was not included. It is also worth mentioning that coverage estimates are obtained every two years for Tampa Bay as an annual snapshot (noted at the top of page 21 in the discussion), but these are not of sufficient temporal resolution to support the analyses in this paper.
* There were limitations to our analysis that were noted in the original draft, primarily the lack of long-term historical data on seasonal trends for seagrasses and macroalgae. For example, page 22,line 52 in the original discussion: “Long-term monitoring data describing normal seasonal variation in macroalgae are unavailable and we cannot distinguish between seasonal and interannual changes and those in potential response to the Piney Point release.”

Page 17, Lines 17-20 - How did the 2021 fish kills compare to historical kill spatial-temporal trends?

* **Response**: We were also interested in this question and had included a long-term trend plot in an earlier draft, with results as early as 1995 (see [here](https://github.com/tbep-tech/piney-point-manu/blob/c45049af6885e1e7d29aeb414d161a0ce4bdeadb/figs/redtide.jpeg)). However, this was not included due to differences in reporting methods that have changed over time. Online reporting became available in recent years and we considered the results incomparable to the earlier data that were based on phone reporting methods.

Page 17, Lines 37-40 - Could there have been a role for low D.O. in the post-storm fish kills?

* **Response**: We included some text describing this issue in the discussion.
* “Water quality conditions before and after passage of tropical storm Elsa may have also contributed to fish kills by reducing bottom-water dissolved oxygen. [Stevens, Blewett, and Casey](#ref-Stevens06) ([2006](#ref-Stevens06)) documented impacts of a category 4 storm on fish resources in the Charlotte Harbor estuary, although tropical storm Elsa was much smaller and fish kills were documented prior to and after arrival of the storm. Lack of continuous monitoring data for bottom waters in Tampa Bay prevents a more detailed assessment of impacts of the storm on water quality.”

Discussion - In general, it would be useful if the team would spend some time establishing the prevalence of N-limitation in the system, either based on their own data or previous studies. The interpretation of plume impacts is predicated on the system being N-limited, so that needs to be established up front.

* **Response**: We agree that the nutrient limitation needs to be clearly stated as the basis for focusing primarily on nitrogen and not phosphorus. The second paragraph in the introduction was meant to provide this justification, but this was clearly insufficient as noted in the review. We include some additional text and citations in the final paragraph of the introduction as further justification - see the response to the similar critique from reviewer 2. Also note that phosphorus summary plots were previously included in the supplement.

Page 20, Line 22 - Ammonium, not ammonia. Or was pH such that ammonia would be the dominant form?

* **Response**: This is in reference to total ammonia nitrogen, as was provided by the water quality sampling. We have made a note in the methods section (top of page 6) that any mention of ammonia refers to total ammonia nitrogen.

Page 20, Line 39 - Might be worth having a separate paragraph that talks about the timing/duration of the K. brevis bloom in 2021 compared to historic conditions.

* **Response**: Previous content in the discussion below this paragraph provided some historical context on red tide events in the region. Specifically, on page 23: “Occurrence of this species has historically been spatially distinct, with blooms originating in subsurface water offshore on the West Florida Shelf…”
* We have added some text below the existing paragraph to provide more context: “Seasonal persistence in Gulf waters in southwest Florida can vary between years, with some blooms lasting as short as a few weeks, while others have been present for longer than a year (the 2018 bloom lasted sixteen months, [Skripnikov et al. 2021](#ref-Skripnikov21)). Severity in estuaries is typically less than Gulf waters as *K. brevis* is limited to higher salinity.”

Page 20 (last paragraph)-21 (first paragraph) - Without showing methods and/or data, I feel that it is inappropriate to introduce the nekton abundance/composition here.

* **Response**: Agreed, much of this content was removed.

Page 21, Lines 32-47 - Much of this text on seagrass seems speculative, and again, unclear about use of % occurrence as a metric.

* **Response**: We have replaced the beginning of this paragraph with more definitive text:
* “Previous research for Tampa Bay has identified water quality conditions that are likely to promote seagrass growth ([Greening and Janicki 2006](#ref-Greening06); [Greening et al. 2014](#ref-Greening14), and references therein). The results observed in 2021 suggested water quality conditions were not supportive of seagrass growth, although changes were not observed and the conditions likely did not persist long enough to impact seagrasses.”
* Also, please see our response above about frequency occurrence.

Page 24, Lines 17-52 - Speculative without showing methods of data collection or the actual data.

* **Response**: The paragraph was removed for brevity.

# References

Chen, Jing, Robert H Weisberg, Yonggang Liu, and Lianyuan Zheng. 2018. “The Tampa Bay Coastal Ocean Model Performance for Hurricane Irma.” *Marine Technology Society Journal* 52 (3): 33–42. <https://doi.org/10.4031/MTSJ.52.3.6>.

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