**We wish to thank both reviewers for such helpful comments. Given the extensive nature of the revisions, we are not submitting a copy with track changes.** Our responses are listed below in black, while the reviewer’s comments are marked in blue. The line numbers are based on the tracked-change version of the manuscript word document.

**Reply to Reviewer #1**

I was looking forward to reading the manuscript as the topic very much interests me, but unfortunately I struggled. The writing was not as strong as it could be and that made it difficult to not only understand the methods but also evaluate them. For instance, there were a lot of compound sentences, which are hard to read as there is a lot of information being thrown at the reader, quickly. So it took several reads to make sure I understood everything, and sometimes even then I was not sure. Also, the organisation seemed off at times. For instance, sometimes I felt like the information was given to me backwards. That is, when I would get later on in a section, I would read a sentence that would make me go: "Oh! I wish I was able to read this sentence earlier." This extends to how some of the sections were broken up. Specifically, I do not understand why section 3.0 was used. I think it would be much easier to read through with all of the information in Sec. 3.0 moved to Sec. 2.2. I really encourage the authors to go over this with very critical eyes and ears to improve the writing. I tried to provide some specific suggestions in the comments below but it got harder to be very through as I continued, so re-writing should extend beyond these specific comments.

We have substantially rewritten and re-organized the paper, using your suggestions. In particular, we have moved Section 3.0 to Section 2.2.

By the time I finished my first read, I had the impression that the manuscript was rushed into review. I especially got this after reading the acknowledgements and seeing the project numbers identified with "xxxxxxx". I was a bit surprised and frustrated seeing this. I was left with the impression that the effort to ensure the document was a completed draft upon submission was not made.

The manuscript was hardly rushed into review, and we apologize if you got that impression. The NOAA project numbers were listed on lines 517-518. Contribution numbers were *purposely listed as XXXX* as contribution numbers from UMCES and NOAA are assigned only after the manuscript is accepted for publication. They remain xxxx in this document; typically that correction is made at the proof stage.

I have a bit of a disagreement with the interpretation of the results. I understand the metric of "Total Accuracy" was used to select the best model, but there are some risks with that. Just using one metric that is. There is a growing literature behind the need to consider multiple metrics in decision making. Maybe not in machine learning, but in other aspects of ocean research. Looking at all three of the BCV results, I would argue that SVM-RVM - not SVM - preformed the best. I go into more detail about this below in specific comments. I am not saying that the authors need to change their results because of course this is all up for debate. But there should be something about in the discussion.

In addition to accuracy, we have now added three other statistical metrics to assess the performance of the machine learning classifiers: recall, precision and F1 score. Please see lines 301-320.

Speaking of the discussion, I do not think it gave enough. Some parts seemed more appropriate for the introduction. For example, after the introduction I was under the impression this type of work has not been pursued before for the study area, so I was surprised when similar examples were brought up in the discussion.

We have moved some texts from the discussion section to the introduction section. The discussion has also been expanded and largely rewritten. We have revised the introduction section to describe previous applications of machine learning algorithms to HABs prediction. Please see lines 121-142.

So, I encourage the authors to spend more time with the discussion to determine the information that is important to bring up to the reader earlier (introduction), and build up the discussion a bit more based of what was learned from this study.

We have added three new paragraphs to the discussion section. Please see lines 422-455. We have also added a new paragraph to summarize the main findings of this study. Please see lines 500-512.

Also, I do not recall reading anything about problems / challenges with the methodology of the work. For example, using one metric to determine a best model. Or, weaknesses of the modelling approaches. It is really important for models to be open and transparent about modelling weaknesses / issues / problems / etc., or else it appears like we have something to hide. So, it is important - necessary even - to incorporate some honest criticisms of the methodology. This of course opens the door to discuss improvements for the future.   
  
We have added discussions on the challenges in various places and a more complete discussion of this in the discussion. Please see lines 389-391, 400-405, 416-419 and 445-455.

Some Specific Comments:  
  
The title reads as if machine learning can confidently predict blooms. Although the highest total accuracy being 62% is not bad, I do not consider this to be confident. The title should be re-worked to focus on what was done. For example: Machine Learning Classification Algorithms For Predicting Karenia brevis Blooms on the West Florida Shelf.

The title is revised as suggested. Please see line 4.

A graphical abstract should interpretable upon the first glance, and I am not sure what I should be taking in from it. Evan after reading the paper, the graphical abstract does not tell me what was done with machine learning that the focus was red tides. The graphical abstract needs to be re-worked or removed if it is not required.

The graphic abstract has been revised to include label identifying nutrient pathways, wind and upwelling, the various aspects that were tested.

Highlights: In the first bullet, it would be better if you just stick to the facts and state the rate of accuracy and remove the text "high accuracy". As it is currently written it gives me the impression that the results are being spinned up. In the discussion you can then argue how the prediction strength is relatively high (i.e., compare to other studies).

Highlights have been revised. We do not understand your comment about results being “spinned up”

Abstract; Lacking a bit, should be more references to the methodology. Where did the data come from? How were models evaluated? Etc.

We have revised the abstract and added text on the methodology. Please see lines 27-34.

pg. 1, line 21: I believe you need to add a comma after "magnitude".

Done. Please see line 23.

pg. 1, lines 25-28; \*\* A lot is happening in this sentence. break it down.

Rewrote the sentence. Please see lines 27-31.

pg. 1, line 30; the SVM model was used, just the SVM model?

We now use RVM, SVM and NB to conduct the sensitivity analysis to the river flow and riverine TN and TP loads. Please see the revised Figure 5 and see lines 384-391 and 400-405.

pg. 1, line 30; "to show that" makes it sound like this was already previously theorized. Maybe just used "showed".

Reworded the sentence. Please see lines 34-36.

pg. 1, abstract; \*\* the last 2/3 of the abstract have a lot of compound sentences. Break things down. It would be much easier to the reader the follow and reduce the chance of confusion.

Rewrote the sentences. Please see lines 31-40.

pg. 2, line 43; \*\* Break down this first sentence - too much information crammed into one sentence.

Dr. Glibert, please help. I DON’T KNOW WHAT SENTENCE IS BEING REFERRED TO HERE- is it one we rewrote? – It’s the first sentence of the introduction. Should we change this?

pg. 2, line 48; you could use the acronym WFS for West Florida Shelf throughout the rest of the manuscript to reduce a little text.

Done as suggested.

pg. 2, lines 52-53; delete "a region about the length of the state of New Jersey" - it is not really necessary as you say the actual value in the following text, and also this sentence will not mean anything to someone who is not familiar with the relative sizes of the US states.

Deleted the words. Please see line 76.

pg. 2, line 62; comma after "pathways"

Added. Please see line 88.

pg. 3, line 66; replace "grazing, suggesting" with "grazing - suggesting" except using an em dash was used rather than a hyphen

Edited as suggested. Please see line 92.

pg. 3, lines 69-70; \*\* re-work the sentence

Edited the sentence. Please lines 95-96.

pg. 3, line 74; replace "recent" with "2017-2019". It helps the reader to be very specific even if it seems repetitive.

Replaced as suggested. Please line 100.

pg. 3, lines 74-77; \*\* rework sentence.

Edited the sentence. Please lines 100-103.

pg. 3, line 86; why is it necessary to model long term trends?

This comment is confusing- we think we have justified why models are useful

pg. 3, line 86; Start a new paragraph when discussing what is being done for this research. It helps the reader.

Done as suggested. Please see line 143.

pg. 4, line 96; "the database" is confusing - what database? Does it have a name? Or should it just be "the Florida Fish and Wildlife Conservation Commission database"?

Edited the sentence as suggested. Please see lines 153-154.

pg. 4, Section 2.1.1; Is there a reason why this data is not mapped on Fig. 1? Are there restrictions on how to present the data, like no sharing the coordinate data? One way I got around that is by presenting the density distributions.

It is not clear what the review is asking here. Is the reviewer asking for all of the long term data to be plotted on the figure?

pg. 4, line 95; when you present data you should immediately state the units. What are the density units in the dataset you used?

Added the unit. Please see line 152.

pg. 4, line 97; insert "water" in front of samples. The more little details just help the reader.

Added “water”. Please see line 154.

pg 4, lines 99-100; In my experience it is common and often recommended to round to the hundreds place. I suggest rounding the values to 25.85 and 29.14.

Rounded the digits. Please see line 155.

pg. 4; line 100; You should make it clear you made restrictions to the data. It may be obvious to you, but it just helps the reader. Why did you make latitude restrictions? Why cut off at off at 9 km?

Added a sentence for the justification. Please see lines 155-157.

pg. 4, line 101; I do not think this should be a new paragraph. I think the whole section is one paragraph.

Combined into one paragraph. Please see line 161.  
  
pg. 4, line 106; insert "the" in between "in" and "major"

Added the word. Please see line 167.

Section 2.1.2; Fig. 1 is not referenced in the first paragraph and it should be as USGS data are indicated on Fig. 1.

Added the reference to Figure 1. Please see line 171.

pg. 4, line 101; insert a comma after "(USGS 2298830)" and "and".

Added the comma. Please see line 170.

pg. 5, line 111; you should say exactly what "nutrient data" you got.

Added the nutrient info. Please see lines 171-172.

pg. 5, line 113; The sentence "and were combined with USGS streamflow data to estimate ..." is going to be really confusing to readers unfamiliar with this methodology. You estimated something? How? What method? Is this common practice?

Edited a sentence. Please see lines 174-175.

pg. 5, line 115; replace "over" with "across the"

Changed as suggested. Please see line 177.

pg. 5, line 116; Did you use the website to calculate the weekly averages? What is the method used? References (other than a website)?

No, we obtained hourly wind data from NDBC. We then used these wind data to calculate the weekly averaged wind speeds using a vector average. Please see lines 177-178.

Fig. 1; The figure description can be improved. It should be readable independent from the text, so I would redefine the acronyms in the figure at least. And you only highlight the region where the intense bloom was but you have bloom data from other years. Did that span the same region? Different? It is easy for a non-familiar reader to get confused here.

We have edited the caption for Figure 1.

pg. 5, line 121; The three algorithms should be specified right away in this section. For instance: "To hindcast K. brevis cell density and test the strength of various explanatory variables, three machine learning algorithms were used: i) Support Vector Machine (SVM), ii) Naive Bayes (NB), and iii) Artificial Neural Network (ANN)."

Rewrote the sentence as suggested. Please see lines 197-200.

pg. 5, line 127; I do not think this should be a new paragraph.

This paragraph is now placed after the description of the four machine learning algorithms. Please see lines 282-285.

pg. 5, line 133; is there a reason you used k = 10? This is not a criticism - just curious if you had justification or just needed to select a number. In either case, you should specify just in case the reader has a similar question.

Add a sentence. Please see line 293.

pg. 5, Section 2.2; Why is there a section 3.0? It would make more sense to move everything in section 3.0 to 2.2.

Done as suggested. Thank you for the suggestion!

pg. 6, line 143; I was anticipating a reference for block cross-validation after reading "the data herein were further validated by block cross-validation".

Reference added. Please see lines 296-297.

Section 2.3; Why do both methods of cross-validation when time series data violates the k-fold cross-validation method?

Both the *K*-fold and block cross-validation methods are widely used for testing the predictive skills of machine learning algorithms. The block cross-validation method may provide a more strict test on time series which may demonstrate autocorrelation. We have written the text and would like to show results from both cross-validation analyses. Please see lines 287-300 and Table 1 as well as a discussion on lines 444-455.

pg. 6, line 156; The acronym SVM has yet to be defined in-text. It has been defined in the abstract but it needs to be defined again in-text.

Moved the descriptions of machine learning algorithms to section 2.2. SVM is defined by now.

pg. 6, line 156; Why SVM? At this point the reader does not know that SVM gave some of the better results so it is confusing. Be general.

Replaced SVM with the machine learning algorithm. Please see line 328.

pg. 6, beginning of section 2.4; I do not know anything about Platt Scaling Analysis so I am missing some sort of brief yet organised description about what it is and what it is doing, and it should be one of the first things done in the section - before the description of the calculation.

We have reorganized and rewrote the sentences. Please see lines 325-329.

pg. 7, line 162; what about sea surface height? Isnt that one you are using also? (pg. 5, line 118).

We have added the subsection 3.4 to discuss the role of sea surface height. Please see lines 411-419.  
  
Equations; equation #s should be right justified - easier for the reader to find equations

Done. Thank you for the suggestion.

Equations; Some equations lacking proper formula descriptions. After a formula, all the parameters and variables need to be properly described. For example, there is nothing following equations (2) and (3).

We have carefully gone through the equations and rewritten the descriptions of the machine learning algorithms. Please see Section 2.2a-d and lines 197-281.

pg. 8, line 195; so isnt there technically 4, not 3, models? Arnt SVM and SVM-RVM two different models?

Yes, we now describe all 4 models.

pg. 9, lines 201 and 202; Two different spellings. You need to be consistent throughout the document.

Corrected the spelling. Please see lines 245 and 246.

pg. 10, line 236; error in equation references.

Corrected.

pg. 11, line 240; Again, four different models results are reported - not three.

Corrected. Please see line 341.

pg. 11, section 4.1; I disagree with the interpretation of the results in that it appears SVM-RVM gives the more robust results - not SVM. Yes, SVM has the best non-HAB accuracy but its HAB accuracy is only 0.38. Thus, nearly 60% of the time SVM will fail to predict a HAB. Isn't predicting a HAB the point? Do we not want to predict HAB events in order to be proactive? On that note, one could argue that NB gave the better results in predicting HABs, but its non-HAB prediction is 0.47. So nearly 50% of the time it incorrectly predicts a HAB event. Meanwhile, SVM-RVM is pretty evenly matched between predicting HABs and non-HABs. I understand that the "Total Accuracy" metric was used to determine the better model, but SVM, SVM-RVM, and ANN all gave really close Total Accuracy metrics. I think this all highlights the risks in making decisions with a single metric. More thought is required and decisions need to be based on multiple metrics/indicators - not just one.

Excellent point and valid criticism! We agree that the most important metric for the HABs prediction is the recall value. We now found RVM and NB have the highest recall values. We now look at four metrics: accuracy, recall, precision and F1 score. Please see lines 301-323, 341-357 and Table 1.

pg. 11, line 243; Fig. 3a shows results from SVM-RVM - not SVM - according to the figure.

RVM.

Section 4.1; you only discuss BCV results so, again, why present k-fold CV results?

The *k-*fold cross validation is widely used in testing machine learning algorithms while the block cross validation is not as well known. We believe it is important to report the results from both cross validation analyses and compare them, particularly for the environmental science community. Please see lines 353-357 and Table 1.

Section 4.1; I have the urge to look at the results for all the models, not just what is presented in Fig. 3. Is it possible to have an appendix with additional results so the reader can view those as well?

We have added a supplemental figure (Figure S1).

pg. 12, line 270; "discharge was varied by 1-2 ...", shouldnt this be in methods?

Good point. We have added a sentence in the methods section. Please see lines 332-334.

Fig. 5; I do not remember reading why Suwanee would not be included. Did I miss it or was the text missing?

Nutrient concentration was not measured/available to use in the Suwanee River. We have added a sentence. Please see lines 173-174.  
  
pg. 13, line 284-286; I think this would be a useful sentence in the methods - not results- to provide clarity to the reader.

We edited the sentence to fit the results section better. Please see lines 392-394.

Fig. 6; why are there only these rivers shown? It would be more compelling if more river-to-river comparisons are made. With just the two I almost get the impression that something is being masked. I am not saying that is what the authors are doing - just being honest about the impression I am being given.

The thirteen panels in Figure 5 show how the HABs probability varies with flows and TN and TP loadings at all rivers. Figure 6 is simply a linear combination from two of these rivers. We use these rivers to make some points. Nothing was masked. It would be duplicative to produce 12 contour plots for all combinations.

pg. 14, line 316; These studies should have been mentioned in the introduction. I was under the impression that this has not been done before.

We have now moved the descriptions of these previous studies in the introduction section. Please see lines 121-141.

pg. 14, line 326; "the model" - which one, be specific. The reader needs help keeping the information straight.

Added the RVM model. Please see line 414.

Acknowledgements; Acronyms need to be defined and grant numbers need to be added.   
  
Grant numbers were already listed in the acknowledgements. The contribution numbers will be added once the manuscript is accepted for publication- see comment above.

**Reply to Reviewer #2**

We thank the reviewer for the helpful comments. Our responses are listed below in black, while the reviewer’s comments are marked in blue. The line numbers are based on the tracked-change version of the manuscript word document.

Summary and relevance of the study  
  
This manuscript optimizes and compares four classification models (Support Vector Machine, a Relevance Vector Machine extension of the SVM, Naïve Bayes classifier, and a feed-forward Artificial Neural Network) for the prediction of toxic blooms of the dinoflagellate Karenia brevis in the West Florida Shelf. The classification models use temperature, wind velocity and direction, and river nutrient discharge as predictor variables, and in-situ K. brevis cell density measurements from the period between 1998 and 2018 as the target variable. The classification model with the highest accuracy for bloom prediction (Support Vector Machine) was used to assess the role of wind velocity and direction on the recurrence frequency of K. brevis blooms, and the role of river nutrient discharge on the maintenance of bloom conditions in the West Florida Shelf using a sensitivity analysis. Strong northerly and westerly winds were found to increase bloom occurrence probability. The mechanism by which wind direction affects bloom occurrence probability is different for northerly and westerly winds. Strong northerly winds result in upwelling in the West Florida Shelf, which transports K. brevis inshore from lower layers of the offshore water column, and westerly winds hold the bloom inshore, once the bloom has reached the shelf. The river nutrient discharge from various rivers is able to sustain K. brevis bloom conditions inshore.   
  
The occurrence frequency of harmful algal blooms has increased both on a global and local scale due to higher nutrient availability (Brand and Compton, 2007; James et al., 2010), which in the context of climate change the increasing trend in occurrence frequency of extreme precipitation around the Gulf of Mexico (Risser and Wehner, 2017; Emanuel, 2017; Oldenborgh et al., 2017) will likely lead to a further increased nutrient availability and phytoplankton activity as demonstrated for other regions (e.g. the Great Barrier Reef; Parker et al., 2017). Thus, the development of an accurate and locally optimized prediction model for toxic blooms in the West Florida Shelf can be seen as a case study or proof of concept for the future implementation of prediction models in other regions and possibly on a global scale. In addition, a study that determines the drivers of toxic K. brevis blooms is worthwhile, as their identification is relevant for informing local policy makers for the implementation of future mitigation and prevention strategies.  
  
General assessment  
  
Based on my review, I recommend the manuscript to go through major revisions. This is because of issues in the motivation and novelty of the study. Additionally, I found issues in the manuscript structure, study design and chosen methodology, which impact the readability, results, and the interpretation of results. The main issue I found throughout the manuscript is the missing motivation behind the development and implementation of a new prediction model. The development of a new prediction model should emerge from the limitations of current existing models. However, the study fails to quantify or assess the (dis)advantages of developing a new prediction model using machine learning algorithms compared to current models, and does not compare the prediction accuracy to current models. Without the comparison to existing models and motivation for development, this study represents only an application of four well established classification models to in-situ measurements instead of a novel, quantifiable, and statistically robust approach. Thus, I encourage the authors to make the reasoning behind the development of the new model a focal point of the study and to highlight the novelty of this approach with its advantages and disadvantages. The issues concerning the structure of the manuscript, study design and methodology are addressed in detail below.

We have rewritten the introduction section to explain the motivation for this study. Please see lines 104-120 and 129-148 in the introduction. Please also see lines 461-475 in the discussion section.

Comments on the structure of the manuscript  
  
The structure and ow of the current manuscript is overall confusing, and it needs to be revised. In the following sections, issues encountered in each section are presented.  
  
Introduction:  
For the introduction, there is no transition or connection between paragraphs, and following the story line was difficult to follow. I propose the authors to restructure the introduction and guide the reader from the general aspects of harmful algal blooms towards the need for a new improved model. I propose the following structure:

-       General, global description of the term harmful algal blooms.  
-       Introduction of global changes of HABs in recent history.  
-       Description of study region and its relevance.  
-       Description of drivers of K. brevis bloom life-cycle and seasonality in the West Florida Shelf.  
-       Description of recent bloom events in the West Florida Shelf in relation to climate change and population growth.

Thank you for this great suggestion! We have reorganized the introduction as suggested. Please see lines 60-148.

-       Description or review of current modeling approaches and their (dis)advantages.

Added the sentences. Please see lines 104-142.

-       Summarized reasoning behind the study and aim of the study.

We belive this is now adequately addressed in the introduction as now written.

Methods:  
The description of the methods is split across two separate sections, whose purpose is missing or stated in vague terms. The study aims to compare the predictive power of different models, and thus a link or comparison between the models should be shown. The reasoning behind the choices in the different algorithms, thresholds, predictor variables, data transformation and aggregation should be motivated or discussed in a quantitative way. Additionally, section 3.0 (Calculation) does not seem appropriate, since it only contains the description of established classification models and does not represent a practical development from a theoretical basis" (see Guide for Authors). As for the introduction, I propose the authors to restructure the methods section and provide the reader with crucial information on the motivations behind the chosen algorithms and methods. I propose the following structure:  
1.      Description of biological data,  
2.      Description of physical/biogeochemical data,  
3.      Preparation of target and predictor variables,  
4.      General description of classification algorithms, and specific description for:  
(a)     Support Vector Machine (SVM),  
(b)     Relevance Vector Machine (RVM),  
(c)     Naïve Bayes classifier (NB),  
(d)     Artificial Neural Network (ANN).  
5.      K-fold cross validation scheme,  
6.      Evaluation criteria with contingency tables (accuracy, recall, precision),  
7.      Sensitivity analysis.

We have added texts to explain why we explored the four machine learning algorithms. Please see lines 197-203. We have moved Section 3 into the Method section 2.2, and have shortened/rewritten the descriptions of these algorithms. We have reorganized the method section as suggested. Please see lines 150-337.

Concerning the reference style, the source of each data product should be indicated using a regular citation and not just an URL. The R packages and version used should be mentioned and referenced. The references for models, equations, and other methods presented are missing. The explanation and equations of the models should be consistent with those presented in the references provided (e.g. equation (4) and its description in the manuscript does not match the equation in Cortes and Vapnik (1995).

Good point. We added the references for the data products and R packages and versions. However, the E.U. Copernicus Marine Environment Monitoring Service license agreement did not require a formal reference, i.e., “In case of any publication, the Licensee will ensure credit the Copernicus Marine Service in the following manner: This study has been conducted using E.U. Copernicus Marine Service Information” (<http://marine.copernicus.eu/faq/cite-cmems-products-cmems-credit/>). Credit for this data source is now given in the acknowledgements.

Results:  
The results section does not provide results for all models used. The study was presented as an inter-comparison of different prediction algorithms, and thus the reader expects for all models to be inter-compared with respect to well-defined and quantitative evaluation criteria. Otherwise, the title, abstract, introduction, and methods should be changed and a section detailing the criteria chosen to identify the optimal model should be in the methods section. Additionally, some sentences are misplaced, since they describe methods (e.g. l.257-259, l.270-271) or discuss the results (e.g. l.261- 263, l.265, l.282, l.290-297).

Table 1 provides the cross-validation analyses for all the four machine learning classifiers. We have added the sensitivity analysis from the NB and SVM to Figure 5 and explained why we did use the ANN for the sensitivity analysis (see lines 389-341). We have added Supplemental Figure 1 to show the time series comparison from the NB, SVM and ANN. It would be repetitive to present Figures 3-6 (12 additional figures) for all the algorithms.

Discussion:  
The current discussion appears to be an extended introduction rather than a discussion of results. There are results mentioned in the discussion that were not presented previously (e.g. l.325-327). The current discussion presents new concepts (e.g. l.316-328) that should have been mentioned in the introduction. In the discussion section, the accuracy, sensitivity, and applicability of the new prediction models should be compared to current existing ones, and the limitations of the new model should be explored. Without such a comparison, the merits of the study cannot be recognized and the reader is unable to perceive the advantages of the new prediction models over current ones. The main motivating factors or assumption for the study depicted in the introduction should re-emerge and either be confirmed or rejected.

As far as we know, previous models were developed for different purposes and cannot be directly compared with the machine learning algorithms developed in this paper. In the discussion section, we have added two new paragraphs comparing the results from different machine learning algorithms. Please see lines 432-455 and lines 397-407. Please also see lines 418-422.

Conclusion:  
The conclusion section is missing. This section should present the main take-home message of the study and provide an outlook for future research, which would likely improve the current limitations of this and other studies.

We have added a concluding paragraph. Please see lines 500-512. The discussion of the limitations is now woven into the discussion.

Comments on methodological flaws  
  
Overall, I found major flaws in the methods presented in this study. These flaws represent a source of bias in the results and their interpretation. The main issue I identified in the study design is that the reasoning for testing the role of wind speed, wind direction, river discharge, and river nutrient supply on the occurrence frequency of K. brevis appears circular in nature. Since 32 out of 34 predictor variables of the models consisted of the aforementioned variables, any change in these variables will by design result in an effect of the predicted occurrence frequency of blooms. Thus, if a model was fitted to only a couple of predictor variables, a change in the prediction due to changes in the predictor variables cannot be interpreted to mean that they are a driver.

We disagree with this assessment and believe this was a misunderstanding. The machine learning models were fitted to many predictor variables during the sensitivity analysis. When doing the sensitivity analysis to a particular variable such as wind speed, we varied this predictor variable while keeping the other predictors at their respective annual mean values. Please see lines 332-334.

I suggest the authors to provide a robust reasoning for their choice in predictor variables, to select predictor variables that cover different aspects that may influence bloom conditions, and to test the effect of single or combinations of single predictor variables during the training of the model.

Please see lines 82-103 and lines 129-142 for discussions how these factors might affect the *K. brevis* blooms. Please see lines 325-334 for the rationale to discern the effect of each individual factor in the sensitivity analysis.

Presentation and preparation of the target variable:

The presentation and preparation of the target variable appears to be awed. In section 2.1.1 the authors aggregate the data into weekly means for the entire study region, based on the top five cell counts. However, as stated in the manuscript, the study region is characterized by a high spatial and temporal heterogeneity, and thus such an aggregation can potentially lead to bloom conditions despite the fact that only five measurements in the study region are above the selected threshold. I suggest the use of a robust measure of aggregation, such as the median and interquartile range, or to use the top 5, 25, 50, 75, and 95% of the cell counts to calculate the weekly means/medians and perform the training and validation with each of these values. Such an approach can provide a confidence interval for the model predictions, which is currently missing and needs to be included.

Our approach for the spatial aggregation of cell density data is either identical or similar to those used in the previous studies (e.g. Liu et al., 2016; Maze et al., 2015). In the HABs prediction it does not make much sense to consider the cell count distribution such as the median and interquartile range. What is more important is whether a certain threshold in cell density is crossed. Moreover, the *K. brevis* was undersampled under low cell density conditions. Please see lines 153-165 for an explanation of our approach.

Presentation and preparation of predictor variables:  
The spatial and temporal resolution of the predictor variables are not compatible with the aggregated resolution of the target variable. In section 2.1.2 the physical and biogeochemical data is mentioned, but information on the spatio-temporal resolution and units of the data is missing. Such information is crucial, since the study aims to assess the role of different physical/biogeochemical variables on phytoplankton, which are known to exhibit rapid response times to perturbations (Kavanaugh et al., 2016) and are short-lived (Padisák, 1994). Given that the resolution of predictor variables do not match the resolution of the target variable, those predictors with the highest variability will likely dominate the results of the prediction, i.e. one single process on relatively local scale dominates the outcome of the prediction for the entire study region. The spatial resolution of the predictor variables should match the spatial resolution of the target variable. Since the spatial variability of the biological data in the study regions is smoothed into a weekly mean, I suggest to change the predictor variables accordingly to match the smoothed target variable, e.g. use the weekly mean total nutrient input as a single predictor variable instead of the nutrient input of individual rivers, or the resulting vector average wind speed and direction over the study area instead of the individual measurements in different stations divided into the U and V components.

All the predictor variables were preprocessed to produce weekly averages. Thus, they have exactly the same temporal resolution as the target variables (*K. brevis* cell density). In terms of the spatial aggregation, there are only a few weather buoys measuring wind speeds on the West Florida Shelf (WFS). The wind speeds have strong spatial variations, particularly in the cross-shelf direction. The regionally averaged wind speed vector would have little connections to the actual wind field over WFS. Similarly, adding all riverine nutrient loads together ignores different phasing in the seasonal variations of river flows. Moreover, averaging all rivers negates our ability to ask questions of individual rivers.

Optimization of hyperparameters:  
The optimization of the hyperparameters is awed and might have resulted in an overfitted model. In section 2.3 the authors test the classification models and optimize the corresponding hyperparameters simultaneously on the validation set in each iteration of the k-fold cross validation. However, this approach has been shown to result in overfitting to the training data (Cawley and Talbot, 2010), and thus diminishes the applicability of the model for subsequent predictions. To avoid overfitting, I suggest to use a nested k-fold cross validation approach (Schratz et al., 2019), which optimizes the hyperparameters on the training set in an inner loop, and tests the model on the validation set in an outer loop. After the optimization, the hyperparameters should be reported.

The R-library we used to find the hyperparameters already implemented the nested *K*-fold cross-validation approach.

Overcoming class imbalance:  
The implementation of the oversampling approach is awed. For the oversampling approach of the minority class (bloom), the study by (Fernandez et al., 2018) was referenced, which suggests as a best practice for constructing minority class observations to select a random point in the minority class and its k-nearest neighbours to calculate a synthetic minority observation. However, the implementation (see code cross validation.R line 94) is a simple resampling of observations of the minority class, which has been shown to result in overfitting (Galar et al., 2012). As was the case for the hyperparameter optimization, this diminishes the applicability of the model for subsequent predictions.

Thank you for the great suggestion! We have used SMOTE to generate synthetic data for the minority class and have redone the calculations. Please see lines 187-194 and Table 1. As you can see from Table 1, the results from random oversampling and SMOTE are similar.

Chosen evaluation criteria:

The chosen evaluation criteria (accuracy) is not an adequate metric to evaluate imbalanced classification problems (Sun et al., 2009). As an example using the provided dataset, there are 318 instances of the positive class (bloom) and 755 instances of the negative class (no bloom). A dummy model that predicts the negative class for all possible combination of predictor variables reaches an accuracy (see formula for accuracy in Sun et al., 2009) of roughly 70% since there are zero true positives (TP), zero false positives (FP), 318 false negatives (FN), and 755 true negatives (TN). I recommend to use precision and recall as evaluation criteria instead, as these focus on the positive class. The evaluation metric should be calculated for the entire dataset and not separated into weeks with blooms and weeks without blooms. For each optimized model the resulting contingency tables with true/false positives/negatives should be reported together with the number of observations in the majority/minority class.

Thank you for the great suggestion! We have added precision, recall and F1 score into the model evaluation metrics. Please see lines 301-320.

Implementation of k-fold cross validation:  
The implementation of the k-fold cross validation does not appear to be correct. As implemented in cross validation.R on line 13, ten missing values in the dataset were transformed to the majority class (no bloom). Missing values should always be treated as missing values. During the training and tuning of the four different models in cross validation.R between line 25-29 the data is prepared and normalized for the subsequent training of the models, however, on line 31 this data is overwritten by data stored in alldata.csv, which is not normalized, and does not contain the target variable. Thus, features with the highest magnitude will likely dominate over all other features in the training phase. Additionally, on line 114-121 during the training of the ANN, the predictor variables are specified differently from those for the SVM, RVM, and NB. Since the study compares different models for the prediction of algal blooms, the same predictors should be used in all models, or the differences in predictor variables in different models should be reported and explained.

We have removed the ten missing values and redone the calculations. Some of the files stored in the github were not updated, and we have uploaded the latest files. All data were normalized before being used for training the machine learning classifiers. The ANN has exactly the same predictor variables as other algorithms.

Description of results:  
The description of results lacks quantitative meaning. Statistically significant differences were mentioned throughout the manuscript (l.150, l.287, l.324). However, there was no quantification for the statistical significance of the results, and the methodology used to quantify statistically significantly differences was not provided. The results were reported without standard deviation or confidence interval. Reporting a confidence interval is crucial, as it informs the reader on the robustness of the prediction. A possible way to calculate a confidence interval of the prediction would be to implement a prediction ensemble model (e.g. Araújo and New, 2007; Bouska et al., 2014; Righetti et al., 2019) instead of individual models.

Table 1 presents the four commonly used statistical metrics for evaluating the performance of machine learning classifiers. An ensemble model is beyond the scope for this paper.

Graphical abstract, highlights and minor comments  
  
The graphical abstract does not summarize the findings. It should rather show the different effects of wind, starting from the inshore transport of the cells and the maintenance by westerly winds and nutrient input, which would then summarize how blooms initiated offshore are transported inshore and maintained. The second highlight is misleading, since the study mainly considered wind, river discharge, and nutrient input as predictor variables are only two out of 34 variables were not in this category.

The graphic abstract is meant to be a simple representation of the study. We think our abstract does that, although we have modified it now to label wind, upwelling and N&P. The point of the abstract –and the point of the paper is not to model how blooms are initiated, transported and maintained, but to show how wind, nutrients and upwelling vary the strength or probability of blooms.

Overall, with respect to the language used in the manuscript, I suggest to refrain from using non-standard units of measurement (e.g. "bloom covered a region about the length of the state of New Jersey" instead of quantitative measure in SI-units, l.52-53), avoid using vague (e.g. "in earlier years" instead of giving the exact year/period, l.55; "prolonged duration" instead of providing the duration in days/months, l.56), subjective descriptions (e.g. "clearly" instead of providing evidence or a reference, l.70, l.252), and emotive expressions (e.g. "unrelenting wet weather", l.74-75). These are only a few examples and I encourage the authors to have the language fully examined prior to publication.  
  
We have edited the sentences as suggested.

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Many of these papers have been added to our reference list. Thank you!