**COMMENTS TO THE AUTHOR:**  
**Reviewer #1:**

The authors created a useful and informative model of coral fertilization and larval survival based on a meta-analysis/data compilation of published data on numerous environmental stressors. While their model will provide utility in this field, I have some concerns about the limited data used in their model and the way the authors described and interpreted their results. Specifically, the authors were too limiting in their search of the literature and missed studies from which additional data could have been used to generate a more robust model.

We agree that the data set is small and have completed a further search of the literature including more studies where possible. We also agree that out interpretation of the results may have been confusing and have made significant changes throughout the manuscript to correct this.

I am also concerned by the circular reasoning that seems to be employed at times in the Discussion in which the authors state that their model agrees with previous findings in the literature, yet the data in their model was extracted from those very same studies (based on Supplemental Table 1). To ameliorate this issue, the authors should focus more directly on the outcomes of their model, such as the relative consequences of one environmental factor versus another, rather than dwelling on whether their model does or does not reflect the findings of past studies.

We strongly agree with this concern and now recognise that this reasoning is not sound. Instead we have now chosen to focus on applications of the model we have created and used real life data to demonstrate this.

Introduction:

- Rather than solely summarizing the literature, it would be useful if the Introduction also discussed the use of meta-analyses in these endeavours and the potential for real-world application of their modeling.

We agree with the reviewer’s suggestion and have included a discussion on the use of meta-analyses within this kind of study as well as discussed its potential application using real-world scenarios.

Methods:

- I am concerned by the use of a limited set of search terms as the only way to identify papers for the meta-analysis. Given that the authors are targeting the effects of specific stressors on coral larvae, a more exhaustive search is possible and will not be particularly time-consuming. I identify two papers below on the effects of salinity on coral larval survivorship and I am certain there are papers on other stressors that could be included. Limiting meta-analyses to papers found by using specific search terms is often done to query consensuses in the literature, but this approach is too limiting for meta-analyses that seek to use as much data as possible to generate predictive models.

We agree with the reviewers concerns and have conducted a more rigorous search of the literature as well as including the papers you have suggested below.

- Line 137: An important component of seawater is the microbial fraction. Thus, to suggest that seawater in all the studies is ‘typical’ based on nutrients and abiotic characteristics alone ignores an extremely important characteristic of the seawater. More specifically, there likely were different levels of water filtration used in these studies and I suggest that the authors include the degree of water filtration (e.g., 45µm) as another variable identified from each study (perhaps as a column in Supplemental Table 1).

We agree that abiotic and nutrient variation may be a factor affecting the results within each study however the level of filtration within each experiment was not always given in each study. Instead we have discussed this limitation within the discussion and hope that this paper will be used as an example of the way water quality data can be incorporated.

Results:

- The effect of salinity on fertilization and larval survivorship is a primary result of the study (abstract, modeled by itself in the Results (line 203), and addressed extensively in the Discussion). Yet, the data modeled for salinity and fertilization is based on two papers and larval survivorship is based on one paper. This seems very limiting, especially given that there are other papers examining the effect of salinity on coral larval survivorship. I am fairly certain that the data reported in both the papers listed below meet the authors’ criteria for inclusion and thus should be incorporated for a more robust conclusion:

Vermeij, M. J. A., Fogarty, N. D., & Miller, M. W. (2006). Pelagic conditions affect larval behavior, survival, and settlement patterns in the Caribbean coral Montastraea faveolata. Marine Ecology Progress Series, 310(11).

Hartmann, A. C., Marhaver, K. L., Chamberland, V. F., Sandin, S. A., & Vermeij, M. J. (2013). Large birth size does not reduce negative latent effects of harsh environments across life stages in two coral species. Ecology, 94(9), 1966-1976.

We agree that our current understanding of salinity is limiting as it has come from just a few papers. We have therefore included the paper suggested above to give our analysis more rigour and to support our findings.

- It would be interesting to model the degree of water filtration or presence vs. absence of microbes as another variable. Water filtration could be modeled as a continuous variable with an explanation of which microbial components are lost as the degree of filtration increases. Alternatively, water filtration could be modeled as a binary variable based upon whether the level of filtration removes bacteria or not.

Discussion:

- Again, much like in the Introduction, the authors focus on the results of specific studies, rather than the results they achieved from their model by combining data from multiple studies. Because of this approach to the writing, the Intro and Discussion read much like a review with a model in the middle. The meta-analysis and modeling approach is interesting and useful and should be more of the focus of the Intro and Discussion. Specifically, it would be useful to consider the relative or hierarchical strength/degree of harm from each factor as identified by the model. For example, there could be sentences such as “per unit change in nutrient x there is 50% greater change in survivorship that a per unit change in nutrient y.” These sorts of comparative conclusions would be informative, especially when applied to real-world situations.

We agree with the Reviewer that our reasoning was confusing and circular as well as that we mainly review the literature rather than discuss our own findings or uses of our findings. For these reasons we have made extensive changes to the manuscript and focused our discussion on the applications of the model we have created.

- Line 280: The sentence starting in this line seems to be very circular. As I read it, the authors state that conclusions from the literature agree with their model, but their model is based on data from these same articles in the literature (based on one being listed in Supplemental Table 1). This isn’t very informative and may be misleading to a casual reader by suggesting external verification of the model.

We agree with the Reviewer that our circular explanation of our findings may be misleading and have therefore made extensive changes to the manuscript to allow our findings to be more clearly understood.

- Line 280: In addition to the circular reasoning issue that I identified above, this sentence is also concerning because only one paper (Scott) was used in the modeling, while the other (Richmond) was not, yet both are suggested to confirm the results of the model. Why was Richmond not included? How are the results of the two studies different? This use of the literature is imprecise and confusing.

We agree that the use of references in this section are confusing and have made changes to rectify this issue. This whole section has been extensively changed.

- Line 285: It is not surprising that the model would reflect the results of the Chua study (because it was included in the model) but not necessarily reflect the results of the Albright study (which was not included in the model). I certainly would hope that the model reflects the data that went into it. Given that one study was used in the model and the other was not, it is misleading to draw the conclusion in this sentence that Chua is supported and Albright was not.

We agree that our circular reasoning for supporting our model is both misleading and confusing and have therefore made extensive changes to the discussion section.

- I appreciate that the authors identify some of the limitations of their model. This is rarely done and was useful and refreshing to read. While I fully understand their inability to compare across species, perhaps they could at least include reproductive differences (brooding or broadcasting) or another higher-order life history character as an effect in the model.

We have taken on the Reviewers comment and did try to include reproductive differences within our model however all species used within out fertilisation model were broadcasting and only one species in our survivorship model was a brooder and therefore this type of variable could not be included.

- The authors of many studies like this one suggest that their models can be used to establish environmental pollution limits and conservation priorities. Such an application is a very useful and noble pursuit, and thus I commend the authors for this. Yet, as with many other studies, the details of how the models can be used are buried in the methodological details. This way of describing the model may be informative enough for casual readers, but won’t be all that useful for someone trying to generate a useful prediction with the model based on a real-world situation. To allow others to use the model I suggest that the authors include a brief yet detailed supplemental describing how someone with e.g., phosphorous measurements from an MPA reef, can use the model to estimate the impact of phosphorous on coral fertilization and larval survival.

We agree with the Reviewer and have created a supplemental that will allow others to utilise the model in a real way with the code for the model openly available on GITHUB as free access.

Minor comments:

- Line 56: “changes to alternative stable states” should be re-phrased.

We can see that this statement is poorly worded and have rephrased this sentence.   
- Line 305: Consider re-phrasing “our study is significant because…”

We can see that this statement is poorly worded and have rephrased this sentence.   
  
**Reviewer #2:**

Overall comment: The difficulty of what is being attempted here is great. As noted in the conclusions the number of usable studies found is small and hence I think the uncertainty in any conclusions found is large. Hence I find many of the conclusions as to degree of effect unconvincing especially after examining the strength of the statistics as well as the range of concentrations causing effects. Overall I think many more caveats are needed in interpreting the analysis and unfortunately these will weaken the value of the conclusions from the study.  
  
Other comments:  
1. Very annoying that "phosphorus" is generally miss-spelt throughout.

These spelling errors have been corrected

2. Line 49/50 - runoff MAY CAUSE eutrophication (a complex term to use anyhow) but not necessarily.

We agree with the Reviewer and have re-phrased this sentence.

3. Line 51/52. Separate "heavy metal" from "pollutants" - not automatically the same thing.

We agree and have re-phrased this sentence.

4. Line 52/53 - "higher trophic level" - higher than what?

We concur that this sentence is unclear and have made the appropriate changes.

5. Line 55 - increased nutrient status can cause issues to coral reefs even in the absence of decreased herbivory.

We agree with this comment and have included both within this sentence

6. Line 60. - Not "leading to" but perhaps "associated with"

This change has been made

7. Line 86 - nutrient "concentrations" not nutrient "load"

This change has been made.

8. Line 88/89 - The presence of copper and lead in ocean waters is not mainly the result of mining and manufacturing! Perhaps INCREASED concentrations above "natural" may be.

We agree that this sentence was unclear and have made changes to reflect this.

9. Line 93. Rewrite this line as it is inaccurate as well as not making sense.

We have re-written this line to make it more accurate and clear.

10. Line 95 - "increased" freshwater fluxes?

We can see how this idea is confusing and have made changes to clarify this sentence.

11. Line 123 - What does "phosphor(o)us" mean here?? I'm assuming maybe "phosphate" or DIP - absolutely need to use an accurate term for what the levels or concentrations were actually of. As written it could be anything from TP, PP, DIP, DOP, TDP or some combination of these. This is totally unacceptable.

We agree that we have been unclear when defining the term ‘phosphorus’ and have made changes throughout the text to clarify this.

12. Line 133 - Suspended sediment in experimental systems comes in many forms - with varying amounts of organic content, carbonate content, nutrient content etc and differing particle size. These will not all have the same toxic effect on the processes examined in this study.

While we agree with the Reviewer that suspended sediment can come in many forms, the studies used within our model did not specify the organic content, carbonate content or the nutrient content. While some the papers did suggest the particle size it was not consistently found across all papers used.

13. Line 124 - By "acidification" you mean pH I think and you should say so.

This change has been made.

14. Line 148 - Of the 18 studies how many included each of the parameters used in your analysis? i.e. how many studies included, for example, salinity as a test parameter. This is important to know.

We agree that including the number of papers used for each parameter would be useful and have created a table to show this information (supplementary material).

15. Line 213/214 - However the results also show the resilience of the processes to changes in the other parameters.

We agree with this comment that the model is affected by the other parameters and have made this clearer within the text.

16. Line 216 - elevated copper often related to shipping and anti-fouling in coastal marine environments rather than just industrial activities

We agree with this comment and have made changes within this section to highlight coppers use in anti-fouling.

17. Line 221 - No discussion here WRT the effects of "free" Cu ions versus organically bound copper in marine waters. Not a simple system.

We agree that this system is complicated and have made extensive changes to the discussion to better explain our model and the environment.

18. Line 228 - Presumably what is meant here is reducing copper pollution NOT reducing copper presence in natural unpolluted systems. Needs to be made clear.

We agree with the Reviewer – copper is naturally occurring in the marine environment so only reducing copper pollution. We have made this clearer within the text.

19. Line 230/231 - Incorrect - Cu is once again the main anti-foulant (in combination with herbicides) on new and current ships.

We agree and have clarified this within the text.

20. Line 231 - Most copper in marine environments DOES NOT originate from anti-fouling!!

We understand the confusion here is due to poor wording. We have clarified that this does not include naturally occurring within marine environments.

21. Line 236 - I don't believe lead is likely to be a more serious pollutant than copper in nearshore environments because of its higher "levels". I note the references used here are very old and pre-date the reduced use of lead in petrol and the increasing use of Cu again in anti-fouling due to cessation of use of TBT.

We agree with the Reviewer and have re-evaluated our references and wording to better reflect the importance of copper.

22. Line 250 - 100 mg/L is hardly a "low level" of suspended sediment!

We agree and have clarified this section to better reflect natural systems.

Line 267 - wetlands to "fix-nitrogen" - I assume you mean to remove nitrogen as biomass or dinitrogen. I don't see how this affects phosphorus however the subject of this sentence.

We agree that this information is unnecessary and therefore it has been removed and this section edited.

Line 276/277 - Why "especially in tropical waters"?

We agree that this statement was unclear and unnecessary and so it was been removed and this section rewritten.

Line Line 279 - There's no suggestion climate change will lead to the oceans or even coastal waters becoming GENERALLY less saline. Episodically perhaps in larger runoff events.

We agree that this section was unclear and have made sure to note that the changes will be episodic in nature.

Line 285/286 - pH effect depends on ranges of pHs tested in original studies. These are not shown so hard to comment.  
We agree that this statement was unclear and have made extensive changes within this section to better explain our model and findings.

Conclusions generally - More discussion of the effects of these parameters in episodic events would be good as this is when one gets the greatest range of many of the parameters tested.

We agree with the Reviewer and have now included three examples showing the use of the model and its parameters in episodic events.

Figure 2b - Huge concentrations of lead - unrealistic in the real world - amazing there is such a small effect given the toxicity of lead.  
Figure 2d - Not much affect until salinity is less than 25. i.e. all OK between 25 to 37.

Table 1. The references used for these "natural" concentrations in seawater are all inappropriate coming from aquarium sources etc. Please use well scientifically validated sources of which there are many. In particular assuming phosphorus means phosphate 0.446 (a strangely precise number!) uM is very high for "normal" tropical ocean waters.

We agree with the reviewer and have sourced where possible the values from scientifically validated sources.   
  
  
**Reviewer #3:**

Review of Wood's Environmental factors limiting fertilisation and larval success in corals  
This paper addresses for sure an interesting (i.e., publishable) topic, but several improvements are required before the ms (as a review paper) truly overviews the current state of the field the authors address in this ms:  
  
-     There are a lot of published papers missing from the analyses (e.g., the very famous review paper of Fabricius (2005)). Consequently, the paper simply reflects the "input", i.e., the limited number of papers used to generate the "model" rather than yielding new insights/ conclusions. Similarly, in the abstract the authors talk about the results of a combined "model", but after reading the ms, I am still not sure what was meant by this "combined model". Please clarify.

We agree with the Reviewer that our use of a combined model was confusing and have therefore completed more analysis and created a new model that better reflects it use in real-world scenarios. We have also gone back to the literature and included a number of new papers to boost our analysis. In the case of review papers – they were not able to be included within the analysis as they do not have original raw data within them. We did however read a number of review papers including Fabricius, 2005 and included the papers used within their review.

-     Carry-over effects whereby planktonic experiences (partly) determine benthic performance should in my opinion be included to realistically reflect how stressors affect the ultimate benthic performance of recruits, especially since the authors set out to come up with a model linking various pelagic life stages.

Unsure what this means?

-     In the Abstract the authors mention that their findings can be used to define "recommended targets for water quality", but these appear to be missing in the text itself.

We agree with the Reviewer that the use of an example would allow our study to be more useful and so have included location-specific examples. We will also make out model publicly available in an online repository on GITHUB to allow others to more easily use it.

-     I do not see how "a model" was constructed that includes all studies used. More so, it looks like the authors performed a meta-analysis of all datasets to draw conclusions on this larger dataset. This is confusing as one could interpret the use of the term "model" for some conceptual approach whereby all sorts of life-history aspects are combined to generate predictions on which species will be most successful in recruiting under what circumstances. This is not the case, and while the approach taken is not "wrong", I found it very confusing until later in the ms.

Unsure how to explain this?

-     The use of "alternative stable states" (56-57) is inappropriate as these "states" should be able to exist under the same conditions whereas the states the authors refer to here exist under very different environmental conditions (e.g., altered herbivory and nutrient regimes). Check the original ecological texts on the appropriate use of these terms (I realize coral reef ecologists use these terms frequently in this manner, but it is still inappropriate).

We agree that this may be inappropriate in other fields and have rewritten this sentence to better reflect the wide-spread use of the terms.

-     Clarify when you talk about larvae in the water column vs those crawling on the bottom… This will greatly aid to a better understanding of what factors affect what life stage.

We agree that the term larvae was unclear and have now corrected the manuscript to better explain larvae within the plankton.

-     "experimental treatments tended to be large" (138)… Please clarify

We agree that this is unclear and have clarified this within the text.

-     While statistics are not my field of expertise, I noticed that values in your graphs often cluster around one value (often "0") with a few data points outside this range. The observed trends thus result from a very uneven distribution of data points along each axis of interest. Are corrections required for such data-distributions? Please clarify/ explain.

Unsure how to explain this?

-     Please quantify "relative importance" (211-212). Putting a value on "relative importance" would greatly contribute to a better understanding of the negative effects of all "stressors" considered in this study.

We agree that out discussion was unclear and that terms used were confusing. We have therefore made extensive changes to the entire section to better explain out model and show its applications.

-     Throughout the Discussion terms like "affected" are used a lot. Please provide "direction" (negative or positive) and "magnitude" (e.g., 3 times more) to explain how and to what degree stress factors affected the larvae.

We agree that this is unclear and have made extensive changes to the discussion to better show the use and workings of our model.

-     The results of the combined fertilisation x survivorship (FxS) model consist of only one example. This section needs elaboration. It appears that the model is a simple multiplication of independent factors whereby the joint probability is not surprisingly "less" than that of each factor individually (299-301). This section is unclear and needs to be better explained throughout the ms.

We agree that our explanation of the combined model was unclear and more elaboration is required. We have further explain the model within the methods and have included a new analysis to show the use of the model in real-world scenarios.

-     Random factors were included in the model design, but results remain unreported. E.g., species choice could have affected the outcome of the analyses, but results are nowhere presented.

We agree this these should have been reported and have done so now.

-     Authors state that interaction effects or species choice did not affect the outcome of their analyses (309-319) but fail to support such claims by other studies.

We understand that this section was unclear and have made changes to better support our claims and methods.