Manuscript # bg-2015-296: Response to referees

We thank the three referees for taking time to review our manuscript "***Marine regime shifts in ocean biogeochemical models: a case study in the Gulf of Alaska***" and for providing thoughtful and thorough comments. We agree with the referees' main suggestions, and have revised the manuscript to address those points.

Referees comments are in bold and our responses in plain text.

Response to anonymous referee 3:

This manuscript aims to apply a new method to the detection of regime shifts in time series of biological and physical variables. The Gulf of Alaska was selected as a test case for presentation of this method. I like the overall approach that the author has taken with his analysis, which presents a more rigorous method for identification of shifts in time series, including the filtering out of more red noise type events. Unfortunately I found too many grammatical errors, and odd word usage or confusing sentences which detracted from the enjoy-ability of this paper to suggest that it is ready to publish as is. I have tried to highlight an example of some of these in my specific comments but revision should be carefully checked for grammar. Additionally, I did not get the feeling that the author was overly familiar with the existing literature on regime shifts and ecosystem dynamics in the Gulf of Alaska as there was no discussion about either the Pacific Decadal Oscillation (PDO) or the ENSO which operates on a shorter time scale than the PDO but could be equally important in driving ecosystem dynamics in this region. If the manuscript was revised to address these concerns I think it could be a valuable contribution to the literature.
Specific comments: {} - odd word choice [] - suggested replacement

We highly appreciate the comments of this referee, especially for the time he/she took to make suggestions to improve the language, which make the manuscript clearer. There are native English speakers in the list of authors, who have carefully checked the revised version to correct grammar mistakes.

Furthermore, following the referee suggestion, we added a discussion on these drivers of the North Pacific climate, along with the North Pacific Gyre Oscillation (discussion and conclusions section, second paragraph). We further analyzed these indices, and stress previous results suggesting a significant shift in the PDO in the late 1970s.

Page 14004

L14 ..Our study demonstrates that ocean biogeochemical models are capable of simulating the late 1970s shift, {indicating} [manifested as] an abrupt increase in sea surface temperature forcing followed by an abrupt decrease in nutrients and biological productivity.

We have replaced "indicating" with "manifested as"

L20....the 1980s shift was {constrained} [driven by] by changes in mixed layer

We have replaced "constrained" with "driven by"

L22.....simulate regime shifts in the Gulf of Alaska region, {thereby} [These models can therefore be considered useful tools to enhance our] {providing a better} understanding..

We have modified the sentence as suggested by the referee.

Page 14005

L8. The regime shift can be. . .. This is a run-on/confusing sentence that needs rewording

We have split the sentence into two parts to avoid confusion.

L18. . .respond differently to [an ecosystem subject only to] natural disturbances

We modified this sentence as suggested.

L20 . It might be better to replace 'global-warming' with climate change.

We replaced "global-warming" with "climate change"

L22. . . shifts in bottom-up control in the food web via phytoplankton or zooplankton. This needs to be expanded on. Presumably you mean through temporal or spatial changes to the lower trophic level carbon production but this is not clear.

We have clarified the sentence and now specify " via changes affecting the abundance of phytoplankton or zooplankton"

L32 – This sentence sound repetitive of the one preceding it. I would suggest combining the two.

We are unsure which sentence the referee is referring to here as there is no L32 in page 14005.

Page 14006

L6 –A substantial part. . . This information doesn't seem to fit here. I would move this whole paragraph to the end of the introduction – just prior to Paragraph 5 where you describe organizational flow.

The paragraph was moved as suggested by the referee.

L6- Multivariate principle component analysis can tell us how the different components of the ecosystem are responding together. I think this is what you are doing but you should say as much to inform the reader not familiar with this type of analysis.

We apply principal component analysis to the z-scores of the physical and biological time series averaged over the Gulf of Alaska for each model to reduce the dimensions of all variables analysed into uncorrelated principal components. We also apply the change-point methodology to the first principal component (PC1) obtained for each model, which explains most of the variability, and test whether PC1 also exhibit a shift in the late 1970s. We then investigate which variables are contributing most to the late 1970s shift, by comparing their individual contributions to PC1 for each model. We added these details about principal component in the methodology section.

Page 14007

L1 {forcings} I would replace with [events]

We would rather stick with "forcings" here as it is the term also used in other studies we are referring to.

L3..and [able to] distinguish these. . .

We replaced "distinguish" with "distinguishing" instead

L7..and threw response {can be} [was] explored..

We replaced "can be " with "was" as suggested by the referee

L24.. [More} specifically[,] in the Gulf of Alaska, a modeling study..

We removed "Specifically" as suggested.

L27.Increases in spring zooplankton biomass were observed.. Over what time period?

We specify the increase was observed when comparing the periods of 1956-1962 and 1980-1989.

Page 14008

L2 – over what time period were the observed increases?

From the 1970s to the 1990s, as mentioned in the following sentence

L7 – Inconsistent use of tense. ..climate shift occurred. . .ecological response varie{s}[d]

Verb tense corrected as suggested by the referee.

L8 – Further south – Than what? Presumably the GOA but it is not clear in this paragraph.

Than the Gulf of Alaska – now specified.

L12..not a return to pre 1977.. So what were the changes ? would be good to inform the reader of this.

As suggested by another referee, discussion of the late 1980s shift has been removed from the manuscript and so was this sentence.

L22. It is unlikely that we will be able to have long term predictions of regime shifts that correspond to the timing of an actual regime shift. You would not expect the forward looking climate models that are used to driving the ocean models to capture the timing of the regime shifts. The may be able to predict regimes in a statistical sense in that they have the right number occurring over the right time frame i.e. decadal, but all models of this nature generally have limited success simulating the timescales of variability and the chaotic randomness (internal variations) found in the real world and should not be expected to align temporally with a model's internal variations (Walston et al., 2014).

We agree with the referee that the prediction of regime shifts with coupled models is overoptimistic and vague. To avoid any confusion, we decided to entirely delete this paragraph of the discussion section.

L29 to be {described} [assigned] only to their representation. .

We replaced described by ascribed instead, as suggested by referee 2.

Page 14010

L3 – {errors} due to the physics —I don't think it's correct to talk about errors- unless you are assessing which of a suite of physical models is better. I would just call them [differences]

We replaced "errors" with "differences" as suggested.

L9 ..dissolved oxygen [initial condition] fields. . .

We added initial condition.

Page 14012

L5 – Does the CORE forcing have a 'shift' in the forcing? Presumably it does and this is what is driving the shift in ocean temperature that you see. Is CORE a re-analysis product that assimilates temperature observations? If so we would expect it to reflect reality and the timing of the regime shift. If not, it would be quite surprising that it managed to simulate the correct timing for the shift.

Good point. Yes we would expect it to reflect reality and the timing of the regime shift if it comes from atmospheric forcing: "CORE2 provides observationally derived geographical fields of atmospheric properties (temperature, humidity, wind), as well as downwelling heat and freshwater fluxes." These additional details were added in the simulation description (section 2.2).

Page 14013

L17 For the models with [a detectable] shift..

We instead added "a" before shift

Page 14014

L5 – if a model with [a] shift.

We added "a" before shift as suggested.

L9 – {can be} [was] added. . .

We replaced "can be" with "was"

L11 – The explanation of the Monte Carlo simulation that starts on line 21 needs to be incorporated here. Otherwise it is not at all clear what you are talking about.

We believe we need to first introduce the concepts of decision rule and critical value to explain what we are estimating using Monte Carlo simulations. Thus, we kept the order as is.

Page 14016

Results are usually presented in past tense

We thoroughly checked that the tense we use to present the results is consistent.

Page 14017

L20 –Even though our analysis.. This sentence needs re-wording for clarity.

The sentence was reworded as: "Even though our analysis does not suggest a significant shift in MLD in the late 1970s, a subtle change is suggested by the cumulative sums smooth change of slope. Similarly, a slight change of slope in MLD is observed in the late 1980s."

Page 14018

L26 – There is one {possible} exception .. It is an exception, not a possible one.

We removed "possible".

L29 {forcing} [driver]

We replaced "forcing" with "driver"

Page 14019

L1 {Therefore} the forcing. . .

We removed "therefore"

L6. . .suggesting a linear [biological] response

We prefer not to add biological here as it is already mentioned in this sentence – we think it is obvious we refer to a biological response here.

Page 14019

In general there appeared to be quite a bit of repetition of the results here and less inference of what the results can tell us, why this may be significant for the Gulf of Alaska ecosystem and how this compares to others finding in this area.

We agree. We cut repetitions in the discussion section and added a comparison to the results of other modelling studies (e.g. Haigh et al., 2001; Capotondi et al., 2005; Alexander et al., 2008). Our results are in agreement with Haigh et al. (2001), who are suggesting a year-round deepening of the mixed layer depth in the Gulf of Alaska after 1976, which led to a decrease in nutrient, phytoplankton and zooplankton after 1976. Other studies instead suggest that the MLD shoaled after 1977 resulting in increased plankton production in the region. This is the case in the Polovina et al. (1995) study, which suggested that shoaling in the spring/winter MLD led to increased productivity in a plankton population dynamics model. Alexander et al. (2008) also simulate a shoaling in the winter mixed layer depth in the late 1970s, giving rise to a early-spring increase in primary production, phytoplankton and zooplankton biomass followed by a late-spring decline in both phytoplankton and zooplankton biomass. Despite the caveat that we are analysing annual mean time series it is important to point out the contradictory direction of change in mixed layer depth. Possibly reconciling this discrepancy, Capotondi et al. (2005) suggest a deepening trend in a broad band along the coast and shoaling in the central part of the Gulf of Alaska. Thus, the comparison of the various attempts to simulate the late 1970s regime shift of the Gulf of Alaska raises the possibility that the abrupt and spatially coherent ecosystem change is actually caused by a previously unappreciated heterogeneous set of environmental changes with distinct spatial pattern and timing in the annual cycle. Further analysis would be required to investigate changes at the seasonal scale and at a finer spatial resolution and is beyond the scope of this study.

L16 – would you expect this shift given the forcing you used to drive the models?

We would not necessarily expect this shift as sea surface temperature was not relaxed to match the observations but sea surface salinity was weakly relaxed (characteristic timescale of 180 days) towards observations to minimize drift. This has been added in the simulation description in section 2.2.

Page 14021

L14 – The fact that Polovina used observations and found a shallowing mixed

layer post regime in this region but the models are all predicting a deepening warrants a more in-depth discussion. Are the models all wrong? What use are they if they can simulate regime shifts but with the wrong response?

See response above for p.14019

<u>Page 14023</u>

L2- Predictability. . .See earlier comment about predictability of regime shifts

A mentioned earlier, we agree with the referee that the prediction of regime shifts is a different problem and removed any mention of it in the manuscript.

References:
Alexander, M., A. Capotondi, A. Miller, F. Chai, R. Brodeur and C. Deser, 2008: Decadal variability in the Northeast Pacific in a physical-ecosystem model: The role of mixed layer depth and trophic interactions. Journal of Geophysical Research - Oceans, 113, C02017, doi:10.1029/2007JC004359.

Capotondi, A., M. A. Alexander, C. Deser, and A. J. Miller (2005), Low frequency pycnocline variability in the northeast Pacific, J. Phys. Oceanogr., 35, 1403 – 1420.

Haigh, S. P., K. L. Denman, and W. W. Hsieh (2001), Simulation of the planktonic ecosystem response to pre- and post-1976 forcing in an isopycnic model of the North Pacific, Can. J. Fish. Aquat. Sci., 58, 703 – 722.