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Interactive comment on "Marine regime shifts in ocean biogeochemical models: a case study in the Gulf of Alaska" *by* C. Beaulieu et al.

Anonymous Referee #2

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I think this is a good paper that is publishable with relatively minor revisions, assuming that none of the things I flagged as insufficiently explained lead to discovery of major conceptual issues. I congratulate the authors on a generally well written paper.

Major points:

(1) A major conceptual issue is that the authors treat 'regime shifts' as being qualitatively different and distinct from 'red noise' (e.g., 14015/9-11, 14022/13-14), and I don't think there is a strong conceptual basis for this. North Pacific variability follows a red noise spectrum, and within such a spectrum will inevitably be found some brief periods of rapid change that can be interpreted as regime shifts. This is the key point of Rudnick and Davis. The point is precisely that regime shifts are part of the continuum of variability, not a priori evidence of shifts between discrete stable states, as this paper

seems to imply. The assertion that a "change in the slopes rather suggests a change in the relationship and thus, a nonlinear response" (14016/2-3) directly contradicts Rudnick and Davis who state that detection of regime shifts is "not evidence of nonlinear processes leading to bi-stable behavior".

(2) I also think the discussion of 'predicting' regime shifts with coupled models (top p. 14023) is vague and overoptimistic. Simulating such events with a forced ocean model (hindcast) and with a coupled model are very different propositions. Predicting them is much more difficult still. No results shown in this paper have any bearing on whether such predictability is possible. It may be that these authors are simply misusing the word 'predict' and don't actually mean this at all (the final sentence of this paragraph, discussing downscaling of climate projections, suggests that this is the case). But in any case I don't think this section is useful; it could be substantially reworded or deleted entirely. All that has been demonstrated here is that the ocean model is adequate to simulate the PDO mode in a hindcast, and therefore it is *possible* that the mode could be accurately simulated (in a statistical sense) in a coupled model. Projections with such a model could be usefully downscaled, but this does not in any meaningful sense constitute a 'prediction' of future regime shifts. I recommend the authors go through the MS searching on every instance of 'predict' or 'prediction' and consider carefully whether it is (a) accurate and (b) necessary. (I would do the same with "nonlinear" in accordance with point (1) above.)

(3) There are several implausible elements in the data shown in the graphics. In Figure 5 the black dashed lines are said to represent regression equations over the whole half century from 1957-2007. But these lines do not seem very plausible to me. In the case of chlorophyll, if we took this line and rotated it about 20-30 degrees clockwise it would be a much better fit. The residuals would be smaller and much more homogeneous. So it's hard to envision a procedure that would generate this line as the least-squares best fit to these data. This applies to the other panels and Figure 6 as well, although for 5b (PP) it's a bit hard to tell because there really do seem to be distinct regimes

before and after 1977 and any single relationship would be a poor fit.

Similarly I find it implausible that the zooplankton time series in Figure A1d is best fit by a constant value rather than a regime shift in 1979-1980 as is the case for CHL, PHY and DIN in this model. Whether or not the pre-1979 values are best fit with a constant or a slight downward slope, I find it very hard to believe that the least-squares fit would not improve if the post-1980 mean was reduced by about 0.01. Or perhaps a single long term downward trend (Model III) would be the best fit. But it isn't plausible that the model shown in the figure is the best one. This suggests that the method applied is not quite as general as presented and that some unstated assumptions may have been inadvertently coded into the statistical procedure.

(4) The principal components analysis is not explained in the Methods. The caption to Figure 4 implies that the PC's were calculated for regional averages, but the region of averaging is not stated. Such averaging is not necessary to calculate principal components: one could just combine all of the variables into one big state space and calculate EOFs for that space. But either way you need to specify the geographic domain, or the region of averaging if regional means were used. It also usual to normalize the different fields (e.g., z-scores) so that the different magnitudes of the variables (and therefore arbitrary choices of units) do not affect the results. I assume this was done but it is not stated. I also assume they used annual rather than monthly data, but again this is not specified in the text. There should be a short paragraph in the Methods describing exactly what was done here.

(5) I think the biogeochemical model descriptions could be clearer, particularly in the area of phytoplankton nutrient limitation. Some models are described as using multiplicative limitation and others as employing the "law of the minimum", but the description is vague with respect to which environmental factors these formulations apply to (e.g., 14010/24-25, 14011/12-13, 24). The usual practice is to use a minimum for multiple nutrient limitations (Blackman's rule), and then either a multiplicative or a minimum for nutrient vs light limitation (and occasionally temperature also although models that

use a min() function for temperature are rare). I don't know of any model that uses a multiplicative function for e.g. N and P limitations.

(6) The data presentation is uninspired. Figures 5-8 all show variations on the same thing, and show only a limited and arbitrary subset of the possibly configurations (4 biogeochemical fields vs SST for two models and vs MLD for two others). These plots are somewhat space-inefficient, and in principal these authors could show many more data in fewer figures e.g., by showing a 4x5 matrix of CHL/PP/PHY/ZOO (rows) vs all five models (columns) for SST (Figure 5) and MLD (Figure 6). Maybe they don't think it is necessary to show all of the data, but right now it seems like only an arbitrary subset are shown. Even if only the current set are shown, the four figures could be reduced to two as there is a lot of whitespace and redundant information.

Minor points:

The footnotes to Table 4 are confusing and unnecessary. c and d do not appear to be used at all. a and b are not necessary as they are redundant to information already in the Table. All that is needed is to add "Years in bold have a significant shift" to the caption. I am generally opposed to the practice of specifying significance levels as P=X rather than P<alpha, but in this table both are used. Choose one or the other. This applies to Table 6 as well, except in this case note c does appear a few times.

I don't think equations 2, 3, 5 or 6 are necessary. What information do they contain that is not already expressed in equations 1 and 4?

Table 2 add space after epsilon in first line

Multiple references within a parenthesis should ordered either alphabetically or chronologically. I don't know if this journal specifies which but it should be one or the other.

14011/19-20 I'm not sure what is meant by 'heterotrophs' here ("three zooplankton groups (heterotrophs, microzooplankton and mesozooplankton)"). Aren't all zooplankton heterotrophic?

14005/9 delete “itself”

14006/9 delete “number”

14007/3 change “distinguish” to “distinguishing”

14007/24 delete “Specifically”

14007/24-26 The interpretation of Polovina et al 1995 here strikes me as overly simplistic. If you look at their Figure 8C, whether mixed layer depth and zooplankton biomass increase or decrease depends on the season. I think it’s fair to say that the MLD shoaled after 1977 (their Figures 3 and 5C), at least in winter, and that the winter mixed layer depth probably drives the seasonal cycle of biological productivity. But what is stated here is not an accurate characterization of the data shown in that paper, and anyway the model used is rather archaic and maybe shouldn’t be taken too seriously.

14008/14 change “challenged” to “limited”

14008/29 change “described” to “ascribed”

14010/8 phosphorus misspelled

14014/22 Not clear what “length” means in this context.

14015/19 change “support” to “aid”

14018/4 add reference to Table 5 here; add “principal” before “component”

Interactive comment on Biogeosciences Discuss., 12, 14003, 2015.