Dear Dr. Wu,

Thank you for giving us the opportunity to revise our manuscript for PLOS Climate. My co-authors and I have addressed all of the reviewers’ comments, as well as the notes from the editorial team regarding the journal’s requirements. You can find our responses below.

We think the reviewers’ suggestions were constructive for clarifying our study approach, methodology and results, and we accepted most of them. We note, however, that we were less inclined to remove/re-organize text pertaining to the forecasting component of the paper, as suggested by reviewer #2. The forecasting component is a novel development of our modeling approach and therefore we think it’s appropriate to include most of the existing text. We believe this information is valuable for readers who are interested in applying a similar approach to their own study systems.

Sincerely,

Mary Hunsicker (on behalf of all co-authors)

**Reviewer #1:** Climate-induced community responses are highly concerned as the urgent need of ecosystem-based and climate-ready fisheries management. Especially, marine heatwaves such extreme events have largely affected marine organisms and further the communities, and will lead to community reorganizations and the so-called regime shifts, results in great economic and social effects. This study focuses on the California Current Ecosystem (CCE), a famous upwelling ecosystem, using the newly developed Bayesian Dynamic Factor Analysis (DFA) to track the community’s response to climatic drivers and to forecast future changes in the community state. There are some insightful results to benefit understanding the biological responses to the unparalleled marine heatwave in 2014-2016 in the CCE. The forecasting ability and its effectiveness of this methods are proved by this study. Hence the methods can be extended to other areas in future works. The manuscript is well-structured and well-writing. I only have some minor comments for the authors to make corresponding revisions.

Response: Thank you for reviewing our manuscript and providing us with constructive feedback.

\*Please note: All line numbers in our responses below refer to the revised manuscript with tracked changes.

Minor comments

1. Lines 62. A simple definition on marine heatwave is recommended.

Response: We added a definition to the introductory paragraph of the manuscript (lines 63-64).

*These effects may be exacerbated when changes in ocean conditions are more extreme, such as during marine heatwaves (prolonged events of anomalously warm ocean waters).*

1. Lines 74-77. It is better to clarify that the thermal shifts that from a cold to a warm regime happened in the northeast Pacific.

Response: We added this information to the text (line 76)

1. Lines 79-85. Providing the exact numbers of SST or heat content anomalies would be helpful for readers to understanding the unparalleled marine heatwave.

Response: We added this information to the text (line 82-84).

*However, between 2014 and 2016 these ecosystems experienced a marine heatwave that involved the warmest sea surface temperature (SST) and heat content anomalies that had ever been observed over large areas of the North Pacific, with SST anomalies over 6oC (Bond et al. 2015; Walsh et al. 2018).*

1. Lines 120-129. The authors describe the DFA and the Bayesian DFA that both are useful replacements of the traditional PCA. However, the superiority of the Bayesian DFA to the DFA should be further clarified.

Response: Thanks, we edited this sentence to indicate that the Bayesian approach offers added flexibility (lines 135-140).

*Ward et al. (2019) recently developed a Bayesian implementation of DFA that offers added flexibility in model aspects over conventional approaches; examples include allowing for extreme “black swan” events (rare and difficult to predict events; Anderson et al. 2017), and trend processes that don’t follow a random walk. Output from these Bayesian DFA models can also be used to estimate the probability of extreme events occurring or switches among contrasting system states.*

1. Lines 151-152. I wonder why the authors use the same climate variables from two regions. Are they distinguished from each other in the long-term scale?

Response: We used the climate variables from the two regions because the regions differ in terms of their biogeography. There is a well-documented biogeographical boundary at Point Conception, California and we wanted to account for this in our analysis. We added this information to the manuscript on lines 190-193.

In the alongshore direction, we calculated averages for two regions with a division at Point Conception, California, separating the southern portion of the CCE (31–34.5˚N) from the central region (34.5-40.5˚N, Fig. 1). *This is in response to the recognition of Point Conception as a major biogeographic boundary for the California Current System, with differing wind and current patterns north and south of that feature (Checkley and Barth 2009, Gottscho 2016).*

Our results indicate that the time series from both regions are in agreement with respect to their loading on a single climate trend.

1. Lines 166-169 There should be literature to support that the ENSO is the dominant mode of interannual variability influencing the CCE. In addition, why do not the authors consider the effects of decadal-scale climatic variability modes such as the PDO (as mentioned in the introduction that the PDO is important for the northeast Pacific) and use winter averages?

Response: We now cite Jacox et al. 2015 to support our statement about ENSO being the dominant mode of interannual variability influencing the CCE. The decadal variability (e.g., PDO) is captured in the variables we use, but we did not include the PDO index as a predictor in our analysis because we wanted to focus on the proximate drivers of ecosystem change. Additionally, using ROMS outputs will enable us to use ROMS forecasts to then forecast biological changes in the CCE.

Jacox MG, Fietcher J, Moore AM, Edwards CA. ENSO and the California Current coastal upwelling response. J. Geophys. Res. Oceans 2015.

1. Lines 190-191. How do the authors deal with the unequal time span in biological variables? Will it cause or not cause problems in the Bayesian DFA?

Response: DFA can handle unequal time spans so this is not a problem for our biology model. Although unequal time span may increase uncertainty in the latent trend during years with less overlap in time series because there is less data. The uncertainty associated with the greater paucity of data in the early part of the biology time series included in our study is more explicitly discussed on lines 471-474 and 638-648.

1. Lines 249-250. Similar to question 5, as biological variables are pooled together, is there any need to use climate variables with spatial differences (e.g. SST in the central and southern regions)?

Response: The biological variables are pooled together in the biology DFA models; however, because they are sampled across a biogeographical boundary (Point Conception, California; see response to #5) we felt that we should include climate time series from both regions (south and central).

1. Lines 253-257. Are they the same between LOO-CV and k-fold cross validation in which the k is equal to the number of years?

Response: k is equal to the total number of years for the LOO-CV, which was used for regime detections in DFA trends. However, k is equal to 10 years (last 10 years of the dataset) for the LFO-CV, which was used to identify the best climate, biology, and biology-climate DFA models.

1. Lines 253-277. The LOO-CV, k-fold cross validation and LFO-CV confuse me. To my knowledge, the authors use LFO-CV to firstly identify parameters of (1), (2), (3) and (4), secondly identify the most appropriate error structure for the climate dataset. If it is, the authors should describe more on the LFO-CV rather than introduce other types of information criteria in this part.

Response: Yes, this is mostly correct. We only used LOO-CV to identify regimes/alternating community states in the DFA trends (3). Here we describe the LOO-CV because it is a commonly used method and we want to distinguish this method from the LFO-CV method. We also use the LOO-CV to identify regimes in the DFA trends later in the methods. We cite the Burkner et al. paper for more details on the LFO-CV method.

1. Lines 291-293. It should be the number of the regimes instead of trends.

Response: Thanks for catching this. We corrected this typo.

1. Lines 306. The biology-climate model has only one climate covariate which I think may be incomplete. In my opinion, it would be better to include all the climate variable into the model and then conduct a variable selection procedure to identify the best model, or just use the climate trend (from the climate Bayesian DFA) as the climate covariate as it represents the shared variability of all the climate variable.

Response: Thank you for this comment. We are planning to update our analysis annually to provide an indicator of ecosystem state for the California Current Integrated Ecosystem Assessment and Ecosystem Status Report. We intend to expand our modeling framework and conduct a variable selection procedure as you described. We mention this and other extensions of our study in the discussion section starting at line 853. We prefer the variable selection approach using the ROMS output over using the climate trend because using ROMS outputs will enable us to use ROMS forecasts to then forecast biological changes in the CCE.

1. Line 353. It should be S2 Fig.

Response: Thank you for catching this. We made this change.

1. Lines 383-386. According to the “90%” criterion, Pacific sardine is not or weakly associated with the trend. Therefore, there is less need to focus on these species that were not or weakly captured by the trend as their dynamics are not well reflected by the trend.

Response: All species shown in figure 4b loaded strongly on the trend (probability > 0.9), including juv/adult Pacific sardine (see lines 450-459). Species that were included in our analysis but did not load strongly on this trend (probability < 0.9) are not shown in figure 4b and are not mentioned in the results or the discussion.

1. Lines 395-398. Similar to question 7, as different span of time series will cause uncertainty, why do not the authors unify spans of all the time series.

Response: We used the full length of the available time series for the biology DFA model because we wanted to produce a historical record, dating as far back as possible, of the community state in the California Current Ecosystem. Despite the large uncertainty around the trend earlier in the period (e.g. 1950-1970), the trend values during this time are still useful for evaluating changes in community state over time and evaluating whether they are within the range of normal variability or not. We note that for the biology-climate covariate models, we unified the time spans of the climate and biology time series.

1. Lines 405-409. I wonder if there is any similarity in these species as they show synchronous increases.

Response: Good question. Three of these species are cool water associated mesopelagic fishes, which were sampled in the southern California Current. We added this information to lines 478-482.

*This shift coincides with a strong increase in the abundance of a few species during that period, including eared blacksmelt (Lipolagus ochotensis), slender blacksmelt (Bathylagus pacificus), northern lampfish (Stenobrachius leucopsarus), which are cool water associated mesopelagic species, as well as a rise in northern anchovy (Engraulis mordax) abundance prior to the shift (S1 Fig.).*

1. Line 412. It should be S3 Fig.

Response: Thank you for catching this. Done.

1. Line 429. The term B matrix is better to appear in the materials and methods.

Response: We removed the B matrix term from this sentence (line xx)

1. Line 437 It should be S4 Fig.

Response: Thank you for catching this. Done.

1. Lines 436-437. The result of biology-CUTI model is similar to the biology-BEUTI model, it may indicate the two covariate is correlated. Therefore, a correlation analysis among climate variables can help readers understand your results better.

Response: Yes, this is correct. These variables are correlated, and we mention this in the discussion on lines 853-858.

1. Line 564. The “Suryan et al., 2021” is not found in the reference list.

Response: Thank you for catching this. We added this citation to the reference list.

1. Lines 710-731. The discussion about how we can use the results from this study to benefit management in the CCE is highly recommended.

Response: The final three paragraphs of our discussion are devoted to explaining how our study is applicable to ecosystem-based management in the CCE (lines 959-1024). We added a new heading 'Management application’ at line 959 and new text at lines 965-967.

*Our intention is to continually update our analyses when new data become available to provide the most up-to-date information on the CCE community state for scientists, managers, and stakeholders.*

We feel that these paragraphs are sufficient for linking our work to management, however, we will provide additional text if the Editor thinks it’s necessary.

**Reviewer #2:** Comments on “Tracking and forecasting community responses to climate perturbations in the California Current Ecosystem”

General comments:

The manuscript implemented a Bayesian Dynamic Factor Analysis method to track the response of California Current Ecosystem to climate perturbations and to forecast future changes in community state. Their results demonstrated strong relationships between community state and multiple climate variables, and identified that nitrate flux through the base of the mixed layer had the strongest correspondence with individual species and the community trend.

Overall, I think the paper is well written and logically presented. The analyses are conducted with solid statistical methods and supporting data. My concerns lie on the organization of the results and discussions, all of which are connected to the objectives of this study. The study included a rich results, however, a large of texts usually make the readers difficult to follow the center idea of the paper. The author may consider to focus on the most impressive results to make it concise, by removing the less ones. This is a following result of the unclarified objectives in the introduction, where the authors attempt to include a range of goals in this study.

\*Please note: All line numbers in our responses below refer to the revised manuscript with tracked changes.

Response: Thank you for your constructive comments. We are aware that there is a lot of information presented in this manuscript. We have clarified our goals and objectives in the final paragraph of the introduction. We also present the reader with a road map of our work flow in the *Modeling* sub-section of the methods section and we indicate that the four steps of our work flow map on to the four study objectives outlined in the introduction (see lines 244-252).

The discussion is tedious. I appreciate the authors’ efforts to organize the sweeping discussion, where as it should be pointed out that no all of them are necessary. I would suggest the authors to focus on the most important findings in the discussion, and if possible, try to bring new insight by summering the underlying patter and driving factors. A list of many relevant facts makes little help towards in-depth discussions.

Response: Thank you for your suggestions. We trimmed parts of our discussion based on the reviewer’s comments. We also introduced section headers within the discussion to help the reader follow the organization and break up the paragraphs. We note, however, that we were less inclined to remove/re-organize text pertaining to the forecasting component of the paper. The forecasting component of the DFA is a novel development of our modeling approach and therefore we think it’s appropriate to include most of the existing text. It may seem tedious to some but we think it is valuable for readers who are interested in applying a similar approach to their own study systems.

When novel community states were considered, I wonder if it would be a issue that the very strong signals in the past may conceal any signs of changes in recent years, resulting in insignificant results, which however does not necessarily mean the lack of alternate community state. The point here is the statistical significance may influence each other if the extent of changes differ substantially among the community states, which is dependent on the variables measured in the surveys.

Response: This is a really great point, and something we’re actively working on to improve with these models. The type of model you’re describing is one with non-stationary variability -- e.g. the magnitude of the variation is changing in some way, either randomly, or over time. If the non-stationary process is estimable, then output from that model would allow the meaning of each state to also change over time (so that strong signals in the past wouldn’t necessarily impact inference of change in recent years). We discuss the issue of non-stationarity in our discussion on lines 953-957).

Specific comments:

1. Line 42-49: the objectives may be summarized or stated in brief in the introduction.

Response: The specific objectives of the study are listed in the final paragraph of the introduction.

1. Line 77: The texts after “Since then” may be organized in the following paragraph.

Response: Done.

1. Line 95: it seems arbitrary to attribute all the events mentioned to the consequence of heatwaves.

Response: We respectfully disagree. We feel that it’s appropriate to acknowledge the previous studies and findings that motivated this study. While it may be possible that the conclusions of some of these studies are uncertain, the consistency among them with respect to the ecosystem-wide consequences of this event are robust. Readers are, of course, free to read the original reports and arrive at their own conclusions.

1. Line 105: it is not quite clear how the early detection is viable here.

Response: The idea here is to provide the earliest possible detection that the response of the broader ecological community to a climate perturbation is outside the observed range of normal variability and therefore may be a signal of a wider ecosystem shift and/or an indication to increase ecosystem monitoring and take a more precautionary approach to management.

1. Line 110: No doubt there are promising progress in community modelling. Even so, I can hardly agree that “the time is ripe” for near-term forecasts, regarding the uncertainty in current modelling frameworks and the limited knowledge in community dynamics.

Response: We changed the text (line 115) to “*as climate models and forecasts of ocean conditions continue to improve, there are burgeoning opportunities to develop and test methods that could provide near-term forecasts of community state…”*

1. Line 114: “asynchrony among time series” may be explained in more details.

Response: Here we mean that the time series data are unevenly or irregularly spaced.

1. Line 117: PCA and MDS represent a small partition of the method used in the research field of community ecology, and a more comprehensive review is recommended on this topic.

Response: This comment refers to “tools such as Principal Components Analysis (PCA) or nonmetric multidimensional scaling have often been used for identifying leading patterns of variability in multivariate datasets (e.g., Koslow et al. 2002, 2013)”. Our text acknowledges that these are merely examples. A review of the different methods and the strengths/weaknesses of each is beyond the scope of this paper. Zurr et al. 2003, which we cite in this paragraph, also addressed this point so it’s not new. Our point here is simply that DFA is better suited for time series analysis than some of the multivariate approaches that have been commonly applied to marine ecosystems.We provided additional information on why it’s a better method in response to reviewer 1 comment above (#4):

*Ward et al. (2019) recently developed a Bayesian implementation of DFA that offers added flexibility in model aspects over conventional approaches; examples include allowing for extreme “black swan” events (rare and difficult to predict events; Anderson et al. 2017), and trend processes that don’t follow a random walk. Output from these Bayesian DFA models can also be used to estimate the probability of extreme events occurring or switches among contrasting system states.*

1. Line 140: the goals may be revised and reorganized to be concise, distinguishing between approaches and objectives.

Response: We changed some of the text in the final paragraph of the introduction to better distinguish the goals of the paper from the specific objectives of our study (lines xx-xx)

1. Line 169: In this case, it would be critical how the ROMS was calibrated and validated in the ecosystem. Some supporting information are recommended.

Response: We inserted the following text at lines 199-203:

*This ocean model is constrained by available satellite and in situ observations to improve its fidelity to nature, and has been validated against independent in situ observations (Neveu et al. 2016, Schroeder et al. 2014). Output from this model has been widely used to characterize CCS oceanography, its relation to large scale climate variability, and its influence over the marine ecosystem from phytoplankton to top predators (see Discussion).*

1. Line 187: how is the threshold of 15 years selected?

Response: For our analysis, we wanted to include long time series that spanned as many climate perturbations as possible and also have enough biological time series to develop an informative indicator of community state. The threshold of 15 years allowed us to meet both of those criteria. In addition, 15 years is a threshold that has been previously used to define "long oceanographic time series" in the California Current. For example, McClatchie et al. 2014 wrote "Here, we define long time series in fisheries oceanography as surveys longer than 15 years that sample either juvenile fish or ichthyoplankton (fish eggs and larvae) combined with fields of oceanographic properties."

We added the following text to lines 221-226:

*A threshold of 15 years allowed us to include long time series that spanned as many climate perturbations as possible and also have enough biological time series to develop an informative indicator of community state. In addition, 15 years is a threshold that has been previously used to define "long oceanographic time series" in the California Current (McClatchie et al. 2014).*

McClatchie, S., Duffy-Anderson, J., Field, J.C., Goericke, R., Griffith, D., Hanisko, D.S., Hare, J.A., Lyczkowski-Shultz, J., Peterson, W.T., Watson, W. and Weber, E.D., 2014. Long time series in US fisheries oceanography. Oceanography, 27(4), pp.48-67.

1. Line 187: It should be noted that index standardization is usually used for fishery-dependent surveys in which data are usually aggregated in resource-rich areas, but not for properly designed scientific surveys.

Response: There is some interannual variability in the sampling effort (time and space) of the scientific surveys associated with our study and so it was appropriate to standardize the data for those surveys. We also would highlight that index standardization is routinely used for fishery-independent surveys around the world — these surveys are ‘properly designed’ but the samples are spatially random year to year, and not at specific stations. For example, see the following papers:

Maunder, M.N. and Punt, A.E., 2004. Standardizing catch and effort data: a review of recent approaches. Fisheries research, 70(2-3), pp.141-159.

Potts, S.E. and Rose, K.A., 2018. Evaluation of GLM and GAM for estimating population indices from fishery independent surveys. Fisheries Research, 208, pp.167-178.

Thorson, J.T., Maunder, M.N. and Punt, E., 2020. The development of spatio-temporal models of fishery catch-per-unit-effort data to derive indices of relative abundance. Fisheries Research 23: 105611

1. Line 197: it seems questionable to change all zeros to NAs, as zeros may be observations rather than missing values in surveys.

Response: We had considered other approaches, such as adding a very small number to the response before log-transforming. However, this is known to cause issues and be sensitive to the choice of arbitrary constant. By changing the values to NAs, we’re assuming that the species was within the survey region and available to be detected, but was in such small quantities that it was not. The choice of approach is a question of professional judgement, but one that the results are generally robust to.

1. Line 199: the influences of log transformation on the DFA may be noted here.

Response: The log-transformation is common in fisheries applications of DFA, because the objective is to create time series that have approximately normal observation and process errors. We have included additional distributional families in the R package ‘bayesdfa’, but have not yet tested these — future modeling work will ideally include these to work with raw distributions that vary by time series.

1. Line 234: could nonlinear relationships be handled in the modelling framework?

Response: Yes, non-linear modelling relationships are simple to include. We have some examples demonstrating this in the ‘bayesdfa’ package — these can be done with smooth splines, like a GAM, or Gaussian processes, etc.

1. Line 249: should the climate datasets and biological datasets be used in the same time span?

Response: The time spans don’t need to be the same to estimate separate biology and climate trends. We wanted to create a historical record of the community state dating as far back as possible. However, we did truncate the biology time series to match the time span of the climate times for the biology-climate covariate models.

1. Line 259: more details may be needed about the LFO-CV before explaining the target of the method.

Response: We think this is clear as written — additional technical details can be found in the cited Burkner et al. paper.

1. Line 270: may denote it as T-1. Also, could the prediction be considered for a scope more than one year?

Response: We changed the text at line 313 to: *​​For each model formulation, we applied the LFO-CV method by first fitting the model to all years of data prior to year T (i.e., training data, years 1, 2, …, (T-1)) and then using the fitted model to predict the trend value in year T (i.e., test data).*

We could extend the modeling approach to make predictions more than one year in advance; however, we chose to start with one year as that would provide the best test for identifying whether we have any forecast skill. If we don’t have much skill forecasting the biological response one year ahead then we’re not likely to have much skill forecasting the response a few years in advance. Also, one-year-ahead forecasts are appropriate for timing of management decision making in the CCE, which happens on an annual basis.

1. Line 271: “repeated this process for a time series dataset of 10 years” or something like this may be clearer.

Response: We changed to ‘repeated this process for 10 years of the dataset’.

1. Line 274-277: show them in an earlier contexts. A subtitle of “model structure optimization” may be added to this section somewhere.

Response: We added the subtitle ‘Model structure optimization’ to line 290.

1. Line 291: How does this LOOIC relate to the LFO-CV mentioned earlier?

Response: LFO-CV was used for model selection of the climate, biology, and biology-covariate DFA models because these models are intended to be used in a forecasting context. LOOIC should have been written as LOO-CV (sorry for any confusion). LOO-CV is the Leave-One-Out Cross Validation information criterion used for model selection of the Hidden Markov Models that we used to estimate the presence of regimes in the state indices.

1. Line 298: could have more texts about the “community states and raw time series” here.

Response: We clarified our meaning by adding text to lines 339-342.

*While a wide variety of multivariate or univariate time series methods could be applied to our observed time series to generate forecasts, our objectives were to develop simultaneous estimates of both the community state (i.e., the biology trend value) and the raw time series (i.e., individual time series summarized by the biology DFA model).*

1. Line 311: the “individual species parameters and the community state” are not clear here. The last sentence of this paragraph may be revised.

Response: We clarified our meaning with the following text (lines 353-371):

*Once the best-supported biology-covariate model was identified, we used that model to make predictions of the community state (i.e., DFA trend value) in 2018 using climate data from that same year and the raw time series of the individual species (i.e., the biology time series summarized by the DFA model). We evaluated forecast skill based on the prediction errors of individual species time series and by comparing the forecasts for 2008–2018 to the 2008–2018 trend values estimated from the biology-covariate model that only included data prior to the forecast year.*

1. Line 327: A description of trend may be placed before the correlation with climate time series.

Response: We switched the order of the text here (lines 375-389).

1. Line 359: not quite clear how the number of states were determined here.

Response: The number of states was determined based on the Hidden Markov model with the lowest LOO-CV value. We now mention this in the method on line 334-336.

1. Line 382: may mark the occurrence of the marine heatwave in the figure.

Response: We made a note of the marine heatwave years in the figure 4 caption.

1. Line 386: supplementary figures may be needed to illustrate the associations with the heatwave, as Figure 4 only showed the correlation with the single trend.

Response: We’re unsure about what information the reviewer is suggesting we present in supplementary figures. This text provides an ecological interpretation of the loadings with respect to the biology trend. The discussion provides additional context for our interpretation of biological / community response to the marine heatwave.

1. Line 397-401: some of them can be moved to the discussion or SI, regarding the rich texts in the results section.

Response: We removed this text (original line numbers 398-401) because this information is also presented in the discussion. Otherwise, the text is directly relevant to the main findings of the paper.

1. Line 424-473: consider cut short the texts in this section. Some results may be shown in the SI if they are not closely related the topic of this study.

Response: The text in this paragraph is directly relevant to the topic of this study. We moved Fig. S8 (now Figure 7) to the main text to emphasize that these results are a key finding of our paper. We note that an older version of Fig. S8 was included in the original version of this manuscript by mistake. We now include the correct version of this figure, which is slightly different but does affect the results in any way.

1. Line 480: explanation of prediction uncertainty may be placed in the discussion.

Response: Given that the DFA model forecasts are a novel component of our modeling framework, we think it’s important to include this explanation within the results section.

1. Line 508-548: the discussion may focus on the potential insights that the study can provide, instead of listing all the relevant facts per se. otherwise, the discussion can be largely condensed.

Response: We trimmed and re-organized this paragraph based on the reviewer’s comments.

1. Line 558: “Suryan et al. (2021)” was not properly included in the references.

Response: Thank you for catching this. We have included this citation in the references section.

1. Line 596: the discussions could be more focused on the interpretation of identified DFA trends in the biological and ecological sense.

Response: Under the new subheading ‘Long-term changes in community state’. we provide a good interpretation of the ecology and potential mechanisms driving the trends and loadings and covariate effects. Also, because our study is an attempt to synthesize a broad suite of community indicators and their response to climate perturbations our intent is not to get too detailed in our interpretation of each indicator included here. Instead, we point readers to more detailed investigations into the drivers and mechanistic understanding of the indicators.

1. Line 650-669: this paragraph may be removed as it is not exactly relevant to the analyses of this study.

Response: We respectfully disagree with this comment. An important goal of this paper is to test our ability to develop near-time forecasts of community state using CCE ROMS output and forecasts. This paragraph provides support for using CCE ROMS to forecast biological processes in the California Current. This paragraph also addresses this reviewer’s comment #9.

1. Line 696-709: This is also not quite a proper discussion for this study, which is not designed for the purpose of methodological development

Response: Again, we respectfully disagree as this paragraph provides further information on the forecasting approach and how it might be modified to address different but related questions. Moreover, the forecasting component of the DFA is a novel methodological development.

**Journal Requirements:**

1. Please provide separate figure files in .tif or .eps format only, and remove any figures embedded in your manuscript file. If you are using LaTeX, you do not need to remove embedded figures.

Response: We converted all figures for the main text from .pdf files or .tif files. The figures are also available as .eps files, if needed,

2. In the online submission form you indicate that your data is not available for proprietary reasons and have provided a contact point for accessing this data. Please note that your current contact point is a co-author on this manuscript. According to our Data Policy, the contact point must not be an author on the manuscript and must be a third party. Please revise your data statement to a non-author institutional point of contact, such as a data access or ethics committee, and send this to us via return email. Please also include contact information for the third party organization, and please include the full citation of where the data can be found.

Response: We did not include authors as contact points to provide expert knowledge on the surveys and biological collections should readers have any questions. We removed this information and instead only include direct links to the databases/dashboards where data can be accessed and/or a non-author institutional point of contact for accessing data.

3. Please provide us with a direct link to the base layer of the map used in Figure 1 and ensure this location is also included in the figure legend.

Response: We updated the map in Figure 1 using a base layer from NOAA that is in the public domain. We provide the following text to the Figure 1 caption.

*The base map layer was sourced from* [*NOAA National Geophysical Data Center (2009) ETOPO1 1 Arc-Minute Global Relief Model.*](https://www.ncei.noaa.gov/access/metadata/landing-page/bin/iso?id=gov.noaa.ngdc.mgg.dem:316) *NOAA National Centers for Environmental Information (accessed: 19 April 2013, Amante, C & BW Eakins 2009).*

Amante, C & BW Eakins (2009) ETOPO1 1 Arc-Minute Global Relief Model: Procedures, Data Sources and Analysis. NESDIS NGDC-24, National Geophysical Data Center, United States Department of Commerce, Boulder, CO, March 2009. 19 p. Accessed: 19 April 2013. DOI: 10.7289/V5C8276M

4. Please amend your detailed Financial Disclosure statement. This is published with the article, therefore should be completed in full sentences and contain the exact wording you wish to be published.

i). State the initials, alongside each funding source, of each author to receive each grant.

ii). State what role the funders took in the study. If the funders had no role in your study, please state: “The funders had no role in study design, data collection and analysis, decision to publish, or preparation of the manuscript.”

Response: We amended the statement to read:

*Funding for this project came from NOAA’s Fisheries and the Environment (FATE) program (project 16-01) awarded to M.E.H, E.J.W., M.A.L. and C.J.H. and NOAA’s California Current Integrated Ecosystem Assessment program (C.J.H.). The funders had no role in study design, data collection and analysis, decision to publish, or preparation of the manuscript.*

5. Changes to the reference list should be mentioned in the rebuttal letter.

We added the following new citations to our reference list.

1. Amante C, Eakins BW. ETOPO1 1 Arc-Minute Global Relief Model: Procedures, Data Sources and Analysis. NESDIS NGDC-24, National Geophysical Data Center, United States Department of Commerce, Boulder, CO, March 2009. 19 p. DOI: 10.7289/V5C8276M
2. Checkley DM Jr, Barth JA. 2009. Patterns and processes in the California Current System. Prog. Ocean. 2009; 83, 49-64.
3. Gottscho AD. Zoogeography of the San Andreas Fault system: Great Pacific Fracture Zones correspond with spatially concordant phylogeographic boundaries in western North America. Biological Reviews 2016; 91: 235-254.
4. Jacox MG, Fietcher J, Moore AM, Edwards CA. ENSO and the California Current coastal upwelling response. J. Geophys. Res. 2015. doi:10.1002/2014JC010650
5. McClatchie S, Duffy-Anderson J, Field JC, Goericke R, Griffith D, Hanisko DS, Hare JA, Lyczkowski-Shultz J, Peterson WT, Watson W, Weber ED. Long time series in US fisheries oceanography. Oceanography 2014; 27: 48-67.

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