Response to CC1

We thank Dr. Barron for their comments. We have provided responses to each comment below. Community comments are in black and author comments are in gray.

The article does good job of summarizing research on the Holocene evolution of the California Current and its effect on the climate of California. Graphics suggesting the driving forces behind this change, such as a plot of insolation change during the Holocene, would be beneficial.

As a researcher cited, I'd like to make some observations. Our studies (Barron et al., 2003, 2019; Addison et al., 2018) suggest the cool-water, upwelling regime of the California Current narrowed during the late Holocene (~3 ka) off northern and central California, possibly in response to intensification of the North Pacific Gyre Oscillation (NPGO). Testing of this hypothesis requires multiple core transects across the California Current, particularly south of Monterey where detailed Holocene records are lacking, as well as independent means of documenting the Holocene evolution of the NPGO.
We appreciate the comments of Dr. Barron and we will include additional language about gaps in available marine data.

Studies to the south are needed, particularly in the region south of Monterey and north of Point Conception. The Santa Barbara Basin is shoreward of California Current and appears to feel the intensification of springtime upwelling at about 4 ka. Further south, off Baja California, warming of surface waters at ~4 ka has been tied to intensification of ENSO in MV99-GC41/PC14 by Marchitto et al. 2010; Science , 330). Increased opal MAR in that core also occurs at ~4 ka (Arellano-Torres et al., 2019, doi:10.1029/2018PA003479), seems to confirm that warmer SSTs, a likely indicator of enhanced ENSO expression, and increased upwelling are closely linked within the California Current. At the same time, this ~4 ka step in the intensification of the California Current likely signals a major decline in the influence of the North American Monsoon (NAM) in southern California. The initiation of NAM decline at ~ 8 ka is proposed by Barron et al., 2012 (doi:10.1029/2011PA002235).
We appreciate this perspective. We will include suggested citations as well as the discussion of the linkages between the NAM and marine processes as suggested here.

It is also important to note that seasonal bias of various proxies as well as the physical setting of a given core within the California Current can make comparison of Holocene records complicated. The seasonal biases of various surface water proxies tend to become more apparent during the late Holocene, as modern seasonal variation coupled with a narrowing California Current becomes more pronounced. For example, diatoms typically increase their flux to the sediments during the spring-summer upwelling season; however, *Fragilariopsis doliolus*, a subtropical diatom associated with the North Pacific Gyre, increases in relative abundance during September and October, during a period of reduced upwelling. Expression of this late Holocene change in diatom an assemblage off northern California is proposed by (Barron et al., 2003), but this hypothesis needs further testing.
We will further underscore the seasonal bias of proxies as an important factor in interpretation of proxy records.

Alkenones are thought to record average seasonal SSTs near the California coast vs. winter SSTs in gyre settings (Herbert in Barron et al., 2003). Therefore, comparison of diatom and alkenone SST proxies in different settings likely will differ. Similarly, as planktic foraminiferal habitats extend deeper in the water column, various SST proxies (assemblages, oxygen isotopes, Mg/Ca ratios) are likely to vary in different settings and over the course of a given Holocene record.
Our paper attempts to synthesize the interpretations of multiple papers from multiple proxy types. As above, we will clarify the importance of the seasonality of multiple marine proxies.

Response to RC1

We thank the reviewer for their comments. We have provided responses to each comment below. Reviewer comments are in black and author comments are in gray.

The paper by Palmer et al. compiles 101 papers (although the abstract says 100 plus 50?) from the western US to summarize millennial scale (EH, MH, and LH), spatial patterns of climatic, ecologic, pyrogenic, and oceanic changes. Overall, I found the paper interesting and applaud its titanic effort. Summarizing data is not easy. The authors do a good job covering the literature with very few exceptions (see below and attached PDF). The methods are sound, the criteria for inclusion reasonable, and the final spatial and temporal interpretations within the bounds of the available science. Note: as a summary paper, I trust that the interpretations by the authors follow those by the primary authors, so I did not double the cited literature.

We thank the reviewer for their comments.

I provide suggestions/edits/comments below in order of the text, not importance.

1) line 48: under-sampled because of a lack of temperature-sensitive terrestrial records...I think this qualifier should be mentioned. Most WNA records are more water sensitive.

We will add the suggested qualifier.

2) line 57: summer insolation was at a Holocene maximum...winter insolation was at a Holocene minimum. add Kaufmann et al., 2020, and Routson et al., 2019. up-date with Swain et al., 2018; Goss et al., 2020 - specific to W US and CA.

We will add language clarifying insolation following the comment above and we will add the recommended citations.

3) line 85: need to mention Wise's dipole work since it is the dominant feature of interannual hydroclimate in the W US...also, Dettinger and Cayan...also worth adding something about atmospheric rivers in this section since they are THE source of major hydrologic change - feast or famine W US climate.

We will add suggested citations and language around the dipole and atmospheric rivers.

4) line 125 - not sure if this Kirby paper actually deals with plant or animal communities? Maybe Kirby et al. (2018) show a strong coupling between hydroclimate and vegetation over 32,000 years at Lake Elsinore? Or, Dingemans et al. (2014)?

We will add suggested citations.

5) line 141: because of diverse age control issues between and within the 100 comparison sites, you might qualify this first question to reflect millennial-scale patterns since less than millennial is unlikely without significant age control assumptions across sites?

We will update the question to read: what are the millennial-scale patterns and climatic phases during the Holocene for the Western United States? This change also addresses RC1 and RC2 comments regarding age control.

6) Figure 1: I would prefer a labeling scheme for each site so that the reader can go back and forth from the table to the figure to find the sites...e.g., 1, 2, 3, etc.

We will update the labeling convention in Figure 1 to link individual studies to sites on the map.

7) Figure 1: draw the boundaries as defined by your spatial criteria...PNW, SW, etc...

We will add a visual boundary on Figure 1 indicating the regions discussed in the paper.

8) line 163: I think there should be an age control criterion...what is the minimum number of Holocene ages required to make millennial-scale statements??

For inclusion in step two of the review (coded results through time, Figures 2,3), studies must report climate reconstruction for at least "3000 years of the Holocene, and in which the authors must have identified and described a clear climatic pattern or patterns for an entire Holocene interval." [Line 180-183]. Due to the variability in the types of age models used by original authors, we did not elect to require a minimum number of radiocarbon ages or other age types. Rather, as discussed in the methodology and in response to RC1 - 9 below, we maintain the original interpretations of the authors. To ensure clarity of age models from the original papers for readers, we will add two columns to Appendix 1: type of chronology used and number of points in the age model.

9) line 163: are you updating the age models? In many of these types of summary papers, the age models are outdated and likely obsolete. Most review papers begin with fresh age models to make sure that papers published 20 years ago are correctly compared to papers published yesterday.

In this review, we maintain the original interpretations of the authors including both the age models and data interpretations. We attempt to include a diverse set of previously published studies and a variety of proxy types. As such, we maintain the original authors' interpretation of the proxy data as well as the original age model. Our work intentionally utilized the early, mid, and late Holocene as broad time bins to accommodate some age uncertainty and interpreted climate trends on millennial timescales. In our review, we will not recalibrate all age models, but throughout the text we will clarify the role of age uncertainty.

As the variability due to age control was highlighted by both RC1 and RC2 we will make two important changes. We will add statements throughout the paper highlighting the role of age uncertainty in interpretations. Additionally, we will add two columns to Appendix 1: type of chronology used and number of points in the age model. This will clarify the age model data for reviewers and readers.

Further, interpretations from the second step of the review (coded results through time, Figures 2,3) are on millennial timescales, any exceptions to this will be noted in the updated manuscript. Importantly, the sections on the Medieval Climate Anomaly, Little Ice Age, and Era of Colonization are exceptions to our millennial-scale interpretations. We will clarify this in text.

10) line 210: Add Leidelmeijer et al. (2021) - Barley Lake early Holocene.

We will add the suggested citation.

11) line 230-234: in the SW (west of AZ), a wetter early Holocene was a product of more intense winter ppt associated with low winter insolation...the monsoon plays little if any significant role in the annual hydrologic budget west of AZ...see Kirby et al. (2005, 2007, 2012) and (Bird et al., 2010). It is reasonable that the monsoon boost may have "helped" maintain lakes where playas exist today, but without the increase in early Holocene winter ppt caused by lower winter insolation and its likely impact of storm tracks, the SW (west of AZ) would have been dry.

We will rework the section on the role of the monsoon. Per comment 11, 14, and 16, we will include a discussion of the role of winter precipitation in the wet early Holocene. We will remove language that attributes the wet early Holocene to the monsoon alone and we will include discussion of both winter precipitation and the monsoon. We will include some of the existing citations on the monsoon but clarify

that the monsoon rarely reaches west of the Mojave Desert and that the wet Holocene could not have been possible with monsoonal rain alone. We will add suggested citations.

12) Figure 2: add numbers so that the reader can cross-reference sites to the table.
We will update the labeling convention in Figure 1 to link individual studies to sites on the map.

13) line 240: Leidelmeijer et al., 2021 agrees with a dry early Holocene from Nor Cal.
We will add the suggested citation.

14) line 266: see earlier comments...the monsoon provided a hydrologic buffer, but it cannot explain the general increase in moisture...winter ppt must be the answer because the climate of the SW (west of AZ) is unimodal and dominated by winter ppt. All the monsoonal rain in the "world [hyperbole]" could not make the SW (west of AZ) wet without ample winter ppt... enhanced by lower winter insolation and its likely modulation of winter season storm tracks over the SW during the early Holocene.
See response to RC1 #11 above.

15) line 306: add Barber, Donald C., A. Dyke, Claude Hillaire-Marcel, Anne E. Jennings, John T. Andrews, Maclean W. Kerwin, Guy Bilodeau et al. "Forcing of the cold event of 8,200 years ago by catastrophic drainage of Laurentide lakes." *Nature* 400, no. 6742 (1999): 344-348.
We will add the suggested citation.

16) line 415 and elsewhere: I think you are overplaying the significance of the monsoon the CA annual hydrologic budget. CA is characterized by a unimodal hydroclimate in terms of what matters for its annual hydrologic budget...and what matters is simply winter ppt amount and to a lesser extent, summer evaporation.
See response to RC1 #11 above.

17) line 417: most of CA receives no significant monsoonal ppt ever...except Mojave and east (see Hereford, Richard, Robert H. Webb, and Claire I. Longpre. *Precipitation history of the Mojave Desert region, 1893-2001*. No. 117-03. 2004.).
We will add the suggested citation.

18) Section 3.2.5 and other human sections: check out...Grenda, Donn R., and Alex V.Benitez. *Continuity and change: 8,500 years of lacustrine adaptation on the shores of Lake Elsinore*. Statistical Research, 1997.
We will add the suggested citation.

19) line 628: the Late Holocene Dry Period is reserved for the published LHDP by Mensing et al. 2013...I think you misinterpreted Lund and Platzman's data and LHDP age range...at Zaca Lake, ALL 3 papers show an LHDP period between 2500 and 2000 cal yr BP. The MCA is also present as well as the LIA...but nothing comes close in duration or magnitude as the LHDP.
We will update the paper following the reviewer comment on the LHDP.

20) section 3.5: add Crawford, Jeffrey N., Scott A. Mensing, Frank K. Lake, and Susan RH Zimmerman. "Late Holocene fire and vegetation reconstruction from the western Klamath Mountains, California, USA: A multi-disciplinary approach for examining potential human land-use impacts." *The Holocene* 25, no. 8 (2015): 1341-1357.
We will add the suggested citation.

21) line 838: really should consistently point out that the early Holocene was characterized by both higher summer insolation and lower winter insolation...BOTH played a role in the millennial-scale Holocene changes you discuss in this paper.

In all areas when insolation is discussed we will include the role of both the summer and winter insolation.

22) line 862: AND, age control issues!!!, proxy sensitivity issues, and differences in the proxies used from site to site.

We will add a discussion of age control, proxy sensitivity issues, and differences in the proxies used from site to site to this section. Per RC2, we will highlight the importance of age control issues in multiple sections in the paper.

Please also note the supplement to this comment:
https://cp.copernicus.org/preprints/cp-2021-109/cp-2021-109-RC1-supplement.pdf

We will make all line edits included in the RC1 Supplement.

Response to RC2

We thank the reviewer for their comments. We have provided responses to each comment below. Reviewer comments are in black and author comments are in gray.

Before I get to my review, I need to offer my sincerest apologies to the authors for taking so long to review this paper. A series of unfortunate errors on my part led to this paper falling through the cracks for a timely review. The underlying science of the manuscript didn't warrant such a slow response, and I owe the authors a significant apology for this error.

We appreciate the acknowledgement of the time delay.

The manuscript prepared by Palmer et al. is a literature review that incorporates data from 50-100 published studies of marine & terrestrial paleoclimate records from across the US West Coast and western US. The paper is arranged according to early, middle, and late Holocene time intervals, with subdivisions for each time period devoted to regional synthesis, terrestrial climate (including fire reconstructions), marine conditions (mostly SST & upwelling intensity), paleoecology (largely pollen-based as well as some limited consideration of marine diatom & foram fauna), human-environment interactions (e.g., archaeology), and/or specific climate events (e.g., Little Ice Age, 8.2 ka event, European colonization, etc.). Of particular significance is the inclusion of a series of maps that correspond to these different Holocene time intervals and climate interpretations of the underlying reviewed studies.

While the subject matter is of great interest to the field of paleoclimatology generally, and the US West Coast specifically, I take exception to this work on the basis of 4 reasons:

- Many of the subsections listed above are superficial treatments of the subject matter, particularly the archaeology subsections. In several cases, these subsections are based entirely on only 1-2 studies. Some of the human-environment interaction sections are so short, I wondered why the authors even considered writing them (e.g., Sect. 3.1.6, 3.2.5). Why the focus on the Channel Islands? There are thousands of archaeological sites in the western US, and if you really used only the search term "archaeology" as the basis for inclusion in your review, then there should be a LOT more information contained in your review! My suggestion is to either drop the archaeology sections since they are pretty tangential to the main climate thrust of the manuscript, or else improve the thoroughness of the archaeological review sections.

The reviewer brings up several important points. We address each here.

We will update the language in the methods section to indicate that the human-environment sections are not intended to be a comprehensive review, rather the goal of the paper was to conduct a comprehensive review of climate and to compare this to human history over time. In our revisions we will explicitly state that we intentionally choose to include human history as complementary to climate data, that data from human history are presented as snapshots falling within broader climate intervals that we examine, and that we utilize snapshots of human history to understand if human occupation/migration/behavior patterns aligned with climate interpretations (e.g., heightened conflict during drought periods). Further, human history, including Indigenous human history and colonization, is an important part of climate history. We will clarify the language around these sections and add additional studies and citations as suggested by RC2.

We focused on the Channel Islands because of the abundance of data available from these sites. The Channel Islands have excellent preservation: the longest and most continuous archaeological data come from here (addressed further below). We will add clarifying language around the Channel Islands and highlight that the focus on the Channel Islands is due to preservation alone, not its relative importance to climate history. The destruction of other archaeological archives throughout the West complicates reconstruction of human history over the entire area studied here.

- There is no consideration by the authors of the importance of age control regarding any of the records considered in this synthesis. I know this isn't the most fun subject to deal with, but you can't just ignore it. For example, several marine sediment records mentioned (e.g., Gardner et al., 1988; Barron and Bukry, 2007; Barron et al., 2017; McGann, 2015) either contain only 1 or 2 dates to pin down the entire Holocene, or are based on benthic forams that have huge reservoir corrections. I'd argue this fact may be a key reason to describe the lack of synchronicity in adjacent climate records that is mentioned in Line 856, or at least as important as the impacts of local vs regional "factors".

In this review, we maintain the original interpretations of the authors including both the age models and data interpretations. We attempt to include a diverse set of previously published studies and a variety of proxy types. As such, we maintain the original authors' interpretation of the proxy data as well as the original age model. Our work intentionally utilized the early, mid, and late Holocene as broad time bins to accommodate some age uncertainty. In our review, we will not recalibrate all age models, but throughout the text we will clarify the role of age uncertainty.

As the variability due to age control was highlighted by both RC1 and RC2 we will make two important changes. We will add statements throughout the paper highlighting the role of age uncertainty in interpretations. Additionally, we will add two columns to Appendix 1: type of chronology used and number of points in the age model. This will clarify the age model data for reviewers and readers.

Further, all interpretations from the second step of the review (coded results through time, Figures 2,3) are on millennial timescales, any exceptions to this will be noted in the updated manuscript. Importantly, the sections on the Medieval Climate Anomaly, Little Ice Age, and Era of Colonization are exceptions to our millennial-scale interpretations. We will clarify this in text.

- In the Methods section, you also highlight that you will "prioritize records with high temporal resolution, continuous records…" [Line 204] = you should state objectively what this high-resolution data threshold is. Also, archaeological midden piles are not continuous records, which again brings into question why the authors opted to discuss these papers in the context of this review.

As RC2 noted, there is variability in the development of age models. We will incorporate additional language about this variability in our revised manuscript and update the appendix (see above). To this specific point, for inclusion in step two of the review, studies must report reconstruction for at least "3000 years of the Holocene, and in which the authors must have identified and described a clear climatic pattern or patterns for an entire Holocene interval." [Line 180-183]. We will clarify in the text that the records we included are records of long temporal duration. We will change the language of this statement to state "prioritize continuous records."

Archeological midden sites provide snapshots of human history and due to the continuous deposition of material in midden sites, they can provide records through time, although these records are a collection of snapshots, rather than a continuous record. In addition to midden records, we also include pollen and fire records as they relate to human history in this area. See above for further explanation of inclusion of

archaeological data. We will add language to the paper to clarify the nature of midden (and other archeological data) as snapshots of human history, rather than a continuous record. In our revisions we will explicitly state that we intentionally choose to include human history as complementary to climate data, that data from human history are presented as snapshots falling within broader climate intervals that we examine, and that we utilize snapshots of human history to understand if human occupation/migration/behavior patterns aligned with climate interpretations (e.g., heightened conflict during drought periods).

- This manuscript requires significant restructuring. The research questions (hypotheses) are not introduced until Line 141, which is far too late in the introduction section. I read Sections 1.1 & 1.2 and got confused as to where this paper was going, as it rambled and lost focus until the hypotheses were introduced. Both of these sections can be culled by 50%. There is also no Discussion section that explicitly addresses the research questions using the results of the review, particularly Questions #2 and #3 (e.g., Lines 142-143).

The restructuring RC2 identifies is focused on the introduction and discussion. Regarding the introduction, we will improve the flow of the introduction by reducing the length of the section (per RC2), by incorporating line edits (provided by RC1), and making suggested changes to research questions (per RC1). The reviewer states that there is no discussion section that addresses the research questions 2 and 3. We chose to include a combined Results and Discussion section (as per the Climate of the Past protocol). In this section there are clearly labeled sections that discuss question 3 (ecological implications and human environment interactions). We will update the name of the Regional Synthesis sections to include language that indicates that these sections address the marine-terrestrial connections (question 2).

Minor issues

- There are many grammatical & style issues to address throughout the manuscript, particularly in the introductory paragraphs. Because I recommended you cull 50% of this section, I'm not going to go through that section in detail. However, the authors do need to pay attention to these issues in the rest of the manuscript. For instance, small typos such as in Line 198 […(Figs.s 2,3)…] or Line 335 [add a comma after "of northern California"] require very detailed attention to catch, which the authors clearly need to do.

RC1 has kindly provided line edits that we will address. In addition to the line edits and restructuring of the introduction (see above), we will also take a close look at the grammar and style of the manuscript.

- The very first sentence of the manuscript's abstract begins with a prepositional phrase, which is considered bad form in scientific writing, so please re-write. The occasional prepositional phrase is okay, but generally you should avoid using them.

We will rewrite the first sentence of the abstract.

- The authors are inconsistent in their use of capitalization of directions to describe the western United States and the Northeast Pacific Ocean. There are specific rules for how to apply directional adjectives, check out https://editorsmanual.com/articles/capitalizing-directions/ for examples.

We will update all capitalization.

- For the initial identification of potential studies using key words, which database(s) were used? Many bibliographical databases have known shortcomings (such as exclusion of key research papers that are older than a decade or two), so it is worth reporting this detail and defending its selection.

We will include further discussion of databases in the methodology section.

- Line 340: What is total carbon? Do you mean total ORGANIC carbon, or carbonate bound carbon? "Total carbon" is kind of a meaningless proxy, if that is indeed what you are reporting, so please clarify.

Total carbon was included by the original author. We will update the text to state "total carbon (organic carbon and carbonate bound carbon)."

- Fig 4b = Y-axis label is wrong, as there are 2 different proxies plotted (opal + sedimentary $d^{15}N$). Also, what is the color coding supposed to mean on all of these similar figures (e.g., Figs 4, 5)?

We will update the y-axis label on Figure 4b. We will clarify the use of colors in the figure caption.

- Appendix A = you have duplicated Columns 1 & 2, please clean it up.

We will update the titles of columns 1 and 2. These columns in fact are both needed as some papers include multiple sites that we include here.

In conclusion, I urge the authors to address these issues and re-submit the manuscript. I don't think any of these complaints are deadly to the manuscript, but some of them will require some time and careful effort to address. I hope the authors choose to pursue these modifications, as a Holocene-focused paleoclimatological synthesis of the US West Coast is of great interest to many scientists.

We appreciate this assessment and agree with the reviewer's assessment that this paper will be of value to the scientific community.