

Reply to Reviewer 1's Comments:

GENERAL COMMENTS:

I deeply appreciate for author's efforts. After revisions, the manuscripts become clearer and the scientific significance is much more improved. Especially, I can feel that analysis and discussion regarding the PDO impact become way better than before. While I'm now agreeing with most of author's results, there are some additional points which need to be clarified. After the minor revisions, I'm happy to do reviewing again for publication.

Issues related to detrending

Now I can clearly see that high-frequency noise is removed via the 5-day moving average filter. However, still, I don't know whether the data is detrended or not. You mentioned that Z500 is subtracted by global-mean, but I think it is extraction of pattern effect rather than the detrending. Since global warming entails some tropical SST pattern and related Z anomalies, I cannot agree that removing global-mean is same with detrending of forced response. In addition, since you include the IVT for the circulation anomalies, I think it inevitably has the effect of the global warming. Indeed, in L406-408 and L416-419, you attribute difference between past and recent PDO impact to the global warming. So, I think your manuscript itself admit that there are forced responses which are not detrended.

Since the analysis regarding the forced responses (such as L406-408 / L416-419) is very interesting, I think it would be great to keep current method. However, since it is different from previous circulation analogue studies, I suggest authors to explicitly mention that the anomalies are not detrended and include the forced responses by global warming.

Thank you very much for this comment! We further explain this in Lines 98-105 (note that all line numbers hereafter refer to the track-changes version of the revised draft):

"To account for the thermal expansion of the warming atmosphere, we subtract the daily global area-weighted mean Z500 from the daily Z500 data at each grid point so that there is no linear trend in the Z500 data due to warming (Christidis and Stott, 2015; Siler et al., 2019; Zhuang et al., 2021b); however, this procedure does not remove any anthropogenically forced changes in atmospheric circulation patterns. Other studies (e.g., Sippel et al. 2019) also use linear detrending or high-pass filtering to preprocess Z500 data at each grid point, assuming that the forced circulation response was smooth and additive; we do not employ these approaches due to our short observational record and they could remove some decadal circulation trend due to internal variability (Zhuang et al. 2021b)."

Analogue part in SOMA methodology

First, it become much easier to read and understand the methodology, which is especially important for this study with new techniques. I have some minor comment with the part 4) in page 7 (L229-231). After getting analogue days from each BMU (SOM) nodes, the authors get regression coefficients by regressing precipitation anomalies directly onto the circulation anomalies. This is definitely different with previous circulation analogue such as Lehner et al. 2018, which get the linear coefficients by finding optimal combinations of analogue-day circulation anomalies to reconstruct target day circulation anomalies. The method in this study (regressing precipitation directly onto the circulation anomalies) would inherently maximize the contribution from the moist circulation, and I thought this would be problematic. However, the high R2 values in Figs. 5,6 would be only possible if the circulation anomalies are indeed important as their assumption, so I'm now agreeing with your regression methods. However, I think you need to

explicitly mention that “solving regression problem for P” in this study is different from the circulation analogue in previous studies (Deser et al. 2016; Lehner et al. 2018) for clarity.

Thank you for the suggestion. We added this statement to Lines 223-226:

“Therefore, P'_{SOMA} and Z' can be considered linearly dependent as well, allowing us to treat the calculation of P'_{SOMA} as a regression problem, which is different from the circulation analogue in previous studies (e.g., Deser et al. 2016; Lehner et al. 2018). Detailed steps are outlined below.”

Other minor comments:

L11-12: I think the content of “and 62~68% of the amplitude of the mean precipitation anomalies” are quite redundant for abstract. I think this is estimation from beta in Fig. 5b, and similar to results in Fig. 5a, which is already mentioned as 54-61%. I suggest the removal of this phrase.

Done!

L18-19: It seems the conjunction is not appropriate here. If I were you, I will use

“However, these circulation-induced changes are not totally related to the PDO phase shift (mostly less than half) since internal variability or anthropogenically induced changes in circulation can also be potential contributors.”

Done!

L35-37: After revision, you decided to use “moist circulation vs. residual” rather than “dynamic vs. thermodynamic”. But here, still, you have dynamic vs. thermodynamic perspective. It would be better to change the terms “dynamic vs. thermodynamic” to “moist circulation vs. residual” in all possible places in manuscript for the consistency.

Thank you for the comment. Here, we deleted “(dynamics)” and “(thermodynamics)” as you suggested (Lines 36-37). However, we keep the use of “dynamics” and “thermodynamics” in the literature review section (Lines 52-78) as these are the terms used in the articles referenced.

Following that, we explain the use of “moist circulation vs. residual” instead of “dynamic vs. thermodynamic” in Lines 200-207: “It's worth noting that attempting to separate the actual dynamic and thermodynamic components solely through a circulation clustering approach like SOM can be challenging. Each type of circulation, as represented by SOM nodes, inherently encompasses thermodynamic responses. Therefore, the distinction between “dynamic” and “thermodynamic” components can be ambiguous when using these terms. Consequently, we prefer to refer to the precipitation anomaly influenced by moist circulation patterns involving Z500 and IVT as P'_{SOMA} instead of P'_{dyn} , and the residual part that cannot be explained by circulation as P'_{RES} instead of P'_{the} . This emphasizes that our results regarding circulation contributions are contingent on our chosen set of circulation variables.”

L38-39: I think this study does not attribute the recent trend to anthropogenic warming. This sentence is now adding the confusion.

By assuming that circulation changes are totally due to natural variability, we can infer the anthropogenic warming's contribution to the recent trend (or its lower bound, as in Zhuang et al. 2021b). However, since this is not the focus of this study, and we agree these statements could add more confusion, we have deleted Lines 38-44 in the introduction.

L49-51: I suggest “atmospheric circulation variability or the PDO-related circulation variability” -> “the PDO-related circulation variability”. Atmospheric circulation variability itself contains the effect from PDO-related circulation anomalies, and this paragraph seems to be more related to the PDO.

Revised.

L70-73: In Lehner et al. 2018, they use monthly data for circulation analogue, rather than daily data. You need other references which use daily data for circulation analogue. If there are no such study, you may need to mention the time-scale difference.

I've added a few more references that use constructed circulation analogue at daily scale at Lines 69-70:

"Flow analogue or dynamic adjustment (e.g., Deser et al., 2016; Horowitz et al., 2022; Jezequel et al., 2018; Lehner et al., 2018; Terray, 2021; Yiou et al., 2007) ...".

L80: "daily variability and amplitude of anomaly" -> "daily variability", since they are similar.

Revised.

L85: "precipitation anomalies, their variability" -> "daily variability". In abstract or introduction, giving both of them is adding confusions.

Revised.

L185-187: Could you explain why the fact that "flow analogue explains a larger portion of observed P'" is the reason for "the flow analogue is better estimate of circulation contribution compared to the C2007"? To me, "larger portion of observed P'" just means that circulation analogue has larger contribution from the circulation component. I agree with that flow analogue has advantage for the flexible precipitation anomalies to each circulation type, but it is hard to agree with this particular part.

We revised Lines 189-193:

"The constructed flow analogue method generally explains a larger portion of the observed P' variance and its estimate is not sensitive to SOM node number, so it has a higher accuracy in capturing the complex relationship between atmospheric circulation and precipitation anomalies and theoretically provides a better estimate of circulation contribution compared to the C2007 method."

L206-L217: In previous paragraph, author said that they will use the term P_{SOMA} instead of P_{dyn} . However, in this paragraph, authors are using P_{dyn} . It is now adding confusions.

We used " P'_{dyn} " in this paragraph because we were referring to the analogue precipitation anomaly calculated by the traditional constructed analogue approach, instead of SOMA. This time we have changed them all to P'_{SOMA} to avoid confusion in the revised manuscript.

L222: The subscript "i" is used for analogue day in L207 but suddenly used for BMU nodes. It would be better to use different one.

We have revised the use of subscripts in Lines 227-244 so now "i" represents days and "n" represents nodes.