Response to Reviewers: “The role of the Indian Ocean Dipole in modulating the Austral Spring ENSO teleconnection into the Southern Hemisphere”

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We sincerely thank the reviewers for their valuable comments and insightful feedback, which have greatly contributed to improving the quality of the manuscript. In the following, the reviewers' comments are presented in black, while our responses are provided in blue.

Reviewer 1

The study analyzes how the IOD contributes to the ENSO-generated extratropical wave train to the Southern Hemisphere. They use observations and an ensemble of CFSv2 analysis to calculate the environmental anomalies generated by ENSO and IOD, in isolation and combined to each other, using composite and linear regressions. They find that positive IOD intensifies the wave train anomalies associated with El Nino in the Southern Hemisphere. On the other hand, no consistent anomalies in the wave train were found during negative IOD and their modulation of La Nina is not as clear, as it is dominated by noise.

The analyses in the manuscript are well thought and executed. The topic is especially difficult to address due to the high correlation between ENSO and IOD, leading to few publications in the area. I would recommend the manuscript to the journal after revisions. Please find my comments below.

Specific Comments:

Main points to be addressed:

Be careful when you mention the intensity of the Walker circulation throughout the text. I would mention the intensity of the anomalies in the fields you are analyzing and then argue that it is due to changes in the Walker circulation. If you want to attribute it directly to changes in the Walker circulation, then it would be better to use an analysis designed specifically for that (e.g. Vecchi et al. https://psl.noaa.gov/data/20thC_Rean/timeseries/monthly/Walker/, Kosovelj et al. https://www.mdpi.com/2073-4433/14/2/397, Sohn et al. https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2009JD013713).

Thank you for the comment: We acknowledge that our reference to the Walker circulation was simplistic and did not consider more appropriate variables or methodologies. Specifically, we mistakenly used "Walker circulation" to describe the vertical motions associated with ENSO and IOD at 200 hPa. As we mentioned in the manuscript, our aim was not to evaluate the ENSO-IOD interaction in equatorial and tropical latitudes, which is already well-documented in the literature. Therefore, we have revised the manuscript to avoid references to the Walker circulation and we focused on describing vertical motions instead.

I liked the analysis of temperature and precipitation. Have you checked if these responses are similar in the CFSv2 model?
Thank you for the comment. We have explored precipitation responses in the CFSv2 model but only in South America, which is the main focus of my PhD and is a region with few studies on this topic. We found that the model reproduces very well the rainfall patterns associated with pure and simultaneous ENSO and IOD events in South America. However, for brevity, we have not included this analysis in the manuscript.

Methodology:

Line 75: Nino 3.4 is mentioned throughout the text, but the link goes to ONI. As you are applying a running mean to DMI, ONI would be more similar to the method you are applying. Either way, please clarify the index used.
Thank you for the comment. We made the mistake of referring to the ONI as Niño 3.4. The index used is the ONI; we corrected it in the manuscript and figures.

Line 80: it was not clear to me if the DMI is calculated before or after extracting the linear trend. Was the linear trend filtered out from the IOW and IOE, or from DMI? There may be a difference between the methods due to the spatial pattern of the warming in the basin.
Thank you for the comment. Yes, the linear trend was filtered out from IOW and IOE and then we computed the differences between IOW and IOD. We have rewritten the sentence to make it clear.

The reviewer is correct about the potential differences in values of the DMI depending on the pre-processing steps followed to compute it. In this study we have decided to follow Saji et al. 2003a. However, we acknowledge that there are different methods and criteria used in previous studies to pre-process and compute IOD, as is outlined by Verdon-Kidd (2018).We have made some modifications to the manuscript to clarify this difference:

Where it reads:
“Before computing the DMI index, the monthly anomalies of the time series for IOW and IOE were calculated based on the 1940-2020 climatology, filtering out the linear trend and interdecadal anomalies defined as periodicities longer than 7 years, and applying a 3-month running mean.”

Now it reads:
“The monthly anomalies of the time series for IOW and IOE were calculated based on the 1940-2020 climatology, filtering out the linear trend and interdecadal anomalies defined as periodicities longer than 7 years, and applying a 3-month running mean. The difference between the IOW and IOE was computed after this pre-processing.”

Please add somewhere how many cases of IOD+, IOD-, ENSO+, ENSO-, and their combination exists in the observational record.
Done.

Results:
Line 130: add some references to papers showing the difference in strength between positive and negative IOD.
Done

Figure 1: Shouldn't the point around DMI ~ -1 and ENSO ~ 2 be classified as El Nino & IOD- too? I may be wrong, but there is another point classified as only El Nino that looks to have IOD- from this figure. I suggest double-checking the classification plot.
Thank you for the comment. Those events that do not qualify as either ENSO or IOD or only as one of them (e.g., ENSO, instead of ENSO-IOD) despite exceeding the magnitude threshold, do not meet the temporal criteria. These criteria are five overlapping quarters for ENSO (following CPC) and three overlapping quarters for IOD (following Saji and Yamagata 2003a). We note that this was not clear in the manuscript. In the case of ENSO events, although the criterion used is the same as the one operationally used by the CPC, we decided to add it to the manuscript to make it explicit. Also, we modified the description of the criterion used for the IOD for clarity:

Where it reads:
"We define an IOD event when the magnitude of the DMI exceeds half of its standard deviation (SD) in the average quarterly SON of each year."

Now it reads:
"Following Saji and Yamagata 2003a, we define an IOD event when the magnitude of the DMI exceeds half its standard deviation (SD) during the three overlapping quarters that make up the SON quarter of each year (i.e., when it exceeds 0.5*SD during the SON months after applying the 3-month running mean)."

In addition, the reason that certain events were not categorized as ENSO, IOD, or as only one of them, despite meeting the magnitude criteria, was clarified in the caption of Figure 1 and the first paragraph of the results section (they did not satisfy the temporal criteria).

Figure 2: Please clarify which color refers to divergence and which to convergence in the figure caption. Same for Fig. 5.
Done

Line 150: they are not negligible for the IO.
Thanks for the comment. We agree that the sentence was not clear as we meant to refer to the Pacific Ocean. We have redrafted it to make it explicit.

Where it reads:
"At 750hPa circulation anomalies depict almost negligible values equatowards of 20ºS for all cases (Fig. 4)."

Now it reads:
"For all cases, the circulation anomalies at 750 hPa represent almost insignificant values over the Pacific Ocean equatorwards of 20ºS.(Fig. 4)."

Line 160: I cannot see the weakening of the divergence in figure 2. Could you add more contours and/or add a supplementary figure with the divergence plotted in colors?
Thanks for the comment. In that sentence we made a mistake, we meant to refer to the velocity potential. It was corrected in the manuscript.

Line 175: I assume you mean the magnitude of the anomalies in the Walker circulation? Aren't the signs opposed?
Line 175: also, clarify you are talking about the anomalies being more intense, not the Walker circulation itself.
Thanks for the comments. We rewrote that paragraph to make it clearer by talking about 'the expected sign' if we assume that we expect to see a linear response of similar magnitude to that found in the regression but with opposite signs for each phase.

We also rewrote the parts in which we refer to 'Walker circulation', as mentioned above.

Where it reads:
"In general, the SST composites (Fig 5) resemble the regressed anomalies in spatial distribution and sign."

"Concerning the composites for divergence and velocity potential at 200hPa, they also resemble the corresponding regression fields in spatial distribution and sign. However, while the intensity of the Walker circulation is similar between both ENSO phases (Fig. 5a and b), it is much more intense for the positive IOD (Fig. 5c) compared to its negative phase (Fig. 5d). Accordingly, in the case of the simultaneous events, the Walker circulation is enhanced in El Niño - positive IOD (Fig. 5e) but not in La Niña - negative IOD (Fig. 5f)."

Now it reads:
"In general, the SST composites (Fig. 5} resemble the regressed anomalies in spatial distribution and the expected sign (similar sign for the positive phase and opposite for the negative phase)."

"Concerning the composites for divergence and velocity potential anomalies at 200hPa over the tropics, they also resemble the corresponding regression fields in spatial distribution and expected sign. However, while the intensity of the anomalous upward and downward anomalies are similar between both ENSO phases (Fig. 5a and b), they are more intense for the positive IOD (Fig. 5c) compared to its negative phase (Fig. 5d). Accordingly, in the case of the simultaneous events, these anomalous patterns are enhanced in El Niño - positive IOD (Fig. 5e) but not in La Niña - negative IOD (Fig. 5f)."

Line 186: what do you mean by "not very tidy"?
Thanks for the comment. Reviewer 2 also suggested replacing that sentence. We have modified the manuscript for clarity.

Where it reads:
"In addition, the WAF at 750hPa, are not very tidy for the pure positive IOD (Fig. 7c) but in conjunction with El Niño (Fig. 7e) clearly show for the combined events the energy dispersion associated with the merging of the IO wave train and the PSA pattern."

Now it reads:
"In addition, the WAF at 750hPa for the pure positive IOD (Fig. 7c) appear disorganized, lacking a clear and coherent wave dispersion direction. But, combined events of positive IODt and El Niño (Fig. 7e) show clear energy dispersion associated with the merging of the IO wave train and the PSA pattern."

Line 239: maybe rephrase this… "the opposite sign" implies that it would be opposite of what we expect. However, if I got this right, "the opposite sign" would be expected in this case.
Thanks for the comment. We rewrote the sentence to clarify it.

Where it reads:
"On the other hand, negative IOD composites from model outputs (Fig. 10d) resemble the regressed anomalies (Fig. 3d), but with the opposite sign. "

Now it reads:
"On the other hand, negative IOD composites from model outputs (Fig. 10d) resemble the regressed anomalies (Fig. 3d), but with the expected opposite sign. "

Technical Corrections:

- Line 80: Extra "a" in Saji and Yamagata reference
- Line 120 (last paragraph): were calculated? I think it would be worth rephrasing this.
- Be careful with the commas. There are a few commas separating subjects from the verbs throughout the text.
- Figure 1: the two tones of orange for El Nino and IOD+ and El Nino and IOD- are too similar.
- Line 150: equatorwards
- Line 159: rephrase this first sentence to be clear what responses you are comparing. Something like "The IO wave train is less intense and less significant for Nino34|DMI (Fig 3c) than in the Nino3.4 full regression (Fig. 3a), as evidenced by …"
- Line 182: remove the first "IO"
- Line 213: intensity
- Fig. 9: the titles of the panels are wrong.
- Link for CFSv2 is broken

We have corrected all the above listed issues.

Reviewer 2

General comments:
This manuscript investigates the interaction between the extratropical wave train forced by the Indian Ocean Dipole (IOD) and the wave train induced by the El Niño-Southern Oscillation (ENSO) during austral spring. The authors use a combination of linear regression and composite analysis of observational data and a model large ensemble to show that the positive phase of the IOD consistently reinforces the Southern Hemisphere circulation response associated with El Niño. In contrast, the circulation response induced by the negative IOD is weaker and exhibits large inter-event variability, showing a less consistent connection with La Niña. The authors have conducted a thorough analysis and I recommend the manuscript for publication after some revisions. Overall, I would like to see the authors improve the flow of the manuscript by reinforcing the main messages throughout, review and strengthen the interpretation of changes in the Walker circulation, and better link the temperature and rainfall analysis in Section 3.3 to the circulation analysis in Section 3.1. Thank you for your comments. We address all points and especially the three mentioned in the specific comments.

Specific Comments:
Lines 20-21: There have been some important studies examining ENSO and IOD teleconnections into the Southern Hemisphere. For instance, Cai et al. (2011, 2012), as reference in this manuscript, and McIntosh and Hendon (2018). Several studies have also attempted to disentangle the impacts of the IOD and ENSO on rainfall or temperature, particularly in Australia (e.g. Liguori et al. (2022) and references therein).
Thank you for the comment and the references. The reference to McIntosh and Hendon (2017) was added to the introduction. The reference to Liguori et al. (2022) was included in the impacts section.

Lines 51-52: It would be helpful to state the correlation between ENSO and the IOD earlier in the manuscript and include references.
Done

Lines 61-62: In this paragraph, please add more detail describing how you will build on earlier studies and increase understanding of IOD/ENSO teleconnections.
Thank you for the comment. We added more details in the manuscript on how we build on previous studies to advance the understanding of ENSO/IOD teleconnections.

Data and Methodology section: The organization of this section could be improved with some sub-headings e.g. “Observational data”…
Thank you for the comment. We have added a few sub-headings in the Data and Methodology section to improve the readability of the manuscript.

Lines 67-68: Add some references that show ENSO and IOD have the greatest influence on SH climate during SON.

Thank you for the comment. We realized that this sentence was not correct and did not express what we meant. The phrase was rewritten in the manuscript and the corresponding references were added.

Where it reads:
"We focused on the period 1940-2020 and in the austral spring, defined by the quarter SON, when both ENSO and IOD have the greatest influence on the SH"

Now it reads:
“We focused on the period 1940-2020 and the austral spring, defined by the SON quarter when the IOD peaks (Saji et al., 1999), has a stronger influence over the SH (Saji et al., 2005), and shows a stronger correlation with ENSO (Cai et al., 2011).”

Line 83-84: Is a threshold of half a standard deviation commonly used in other studies to identify IOD events? Do your results change substantially if only stronger events (1 standard deviation) are selected?
Yes, the 0.5 SD threshold has been used in previous publications (Saji and Yamagata 2003a, 2003b, Saji et al. 2005, Verdon and Franks 2005, Chan et al. 2008). However, as it is pointed out by Verdon-Kidd (2018) there is a lack of consensus in the literature on the criteria and threshold to define IOD events.

We did not test whether the results change when using the 1 SD threshold for DMI as we have very few events already. Raising the threshold would result in even fewer events, resulting in less robust compounds. However, if we had used the 1SD threshold, according to Figure 1 we would have obtained the same number of pure IOD events, and we would have missed some pure negative events and several simultaneous negative events.

Line 87: I like that you have used a large ensemble to help overcome the issue of small size in the observations. I’m wondering why you chose to use an ensemble of initialized predictions, rather than a single model initial condition large ensemble (SMILE), such as from CESM2.
Thanks for the comment. We follow Kumar et al. 2016 and Osman et al. 2022 who used initialized simulations of models that are widely used for seasonal-scale forecasting and therefore represent teleconnections well.

Line 92: Can you very briefly explain the reason behind the abrupt shift in the climatology in this model?
Kumar et al. 2012 mentioned that the shift in the climatology of the CFSv2 model around 1999 is related to changes in the Climate Forecast System Reanalysis (CFSR) of the ocean from which the initial conditions for the CFSv2 reforecasts are taken. These changes appear to be caused by the incorporation of Advanced Television and Infrared Observation Satellite Operational Vertical Sounder (ATOVS) data into the atmospheric data assimilation in 1999 (Xhang et al. 2012), which then affected the surface forcing for the ocean reanalysis used in the CFSR (Xue et al. 2011). It is

a non-stationary systematic error of the CFSv2 model well documented in Kumar et al. 2012 and Zhang et al. 2012, as mentioned in Saha et al. 2014. We consider it important to separate the climatology as recommended by Kumar et al. 2012 since the Equatorial Pacific is the region most sensitive to this issue, and this could affect the computation of the Niño 3.4 index in the model. We decided not to include this explanation in the manuscript because it is unrelated to the main objective of the research.

Lines 97-98: To be consistent with the model, the observational ENSO events should also be defined using the same standard deviation threshold.
Thank you for the comment. For the observed ENSO events, we considered it important to apply the most widely used operational threshold. As such, we adjusted the selection threshold for ENSO events in the CFSv2 model to align it with that of the observed events, i.e., ±0.5°C. Given that the previous threshold used , 0.5*SD = 0.59°C, is close to the new threshold, the results do not change significantly.
In Figures 13 and 14, the separation between moderate and strong ENSO events is now defined as follows: Moderate 0.5 - 1ºC and Strong > 1ºC.We have also modified the text in section 2 to clarify this point.

Line 128-130: It would be interesting to know when these two instances of positive IOD/La Niña and negative IOD/El Niño occurred.
We add the years of these events in the manuscript. We also added the number of events for each category (positive simultaneous events, pure El Niño, etc.).

Lines 136-138: A large part of the discussion of the circulation anomalies involves inferring changes in the Walker circulation from divergence and vertical velocity anomalies at 200 hPa. I am unsure about using 200 hPa divergence as an indicator of Walker Circulation strength. Commonly used approaches are outlined in Kosovelj and Zaplotnik (2023). Can you more clearly describe the link between these anomalies and the Walker circulation, and provide relevant references?
https://www.mdpi.com/2073-4433/14/2/397
Thank you for the comment: We acknowledge that our reference to the Walker circulation was simplistic and did not consider more appropriate variables or methodologies. Specifically, we mistakenly used "Walker circulation" to describe the vertical motions associated with ENSO and IOD at 200 hPa. As we mentioned in the manuscript, our aim was not to evaluate the ENSO-IOD interaction in equatorial and tropical latitudes, which is already well-documented in the literature. Therefore, we have revised the manuscript to avoid references to the Walker circulation and we focused on describing vertical motions instead.

Line 149: Showing surface-level anomalies, instead of at 750 hPa, would be more relevant to link with surface impacts in Section 3.3.
Thank you for the comment. The intention is to analyze low-level tropospheric circulation, but not to link it to surface impacts (see comments below about section 3.3).

Lines 180-181: What is meant by “less significant”? The centres of the extratropical anomalies are significant. The tropical response at 200 hPa is more widespread, as expected.
Thanks for the comment. We rewrote that sentence to make it clear that we are referring to the extent to which anomalies are significant.

Where it reads:
“At both levels, composites show a typical PSA-like pattern for pure El Niño (Fig. 6a and 7a), which is less significant at middle and high latitudes than for the tropics.”

Now it reads:
“At both levels, composites show a typical PSA-like pattern for pure El Niño (Fig. 6a and 7a), with significant anomalies covering smaller regions at middle and high latitudes than for the tropics.”

Lines 181-183: The significant parts of the positive IOD wave train are not really similar to the El Niño wave train though.
Thanks for the comment. We have rewritten the text to facilitate its interpretation:

Where it reads:
“On the other hand, composites for positive IOD (Fig. 6c and 7c) show an IO wave train propagating downstream from the IO to South America that is very similar at middle and high latitudes to that associated with El Niño.“

Now it reads:
“On the other hand, composites for positive IOD (Fig. 6c and 7c) show an IO wave train propagating downstream from the IO to South America, which bears some resemblance to the El Niño wave train at middle and high latitudes, though the significant parts of the positive IOD wave train differ from those associated with El Niño.”

In general: Throughout the text, perhaps at the end of a paragraph or section, it would help to include some sentences summarising the results and describing the implications, e.g. if the Walker circulation is enhanced, what does this mean for the teleconnections? When the composite does/does not resemble the regression, what does this say about the linearity of the response? While some of this discussion comes in the final section, incorporating it throughout the manuscript would improve the narrative.
Thanks for the comment. These sentences summarizing the results are already at the end of each section. However, we modified the manuscript as follows: Firstly, we added two sub-subsections in section 3.1 that separate the results obtained with partial regression from those with the composites. Secondly, we added sentences at the end of paragraphs in the following sub-sections to improve the narrative without making the text repetitive.

The added and/or amended sentences can be found :

- At the end of the first paragraph of section 3.1.1.
- At the end of section 3.1.1.
- At the end of the second paragraph of section 3.1.2.
- At the end of the fourth paragraph of section 3.1.2.

Lines 197-198: The large variability in the circulation response across negative IOD events is interesting. To strengthen this argument and demonstrate that negative IOD events have a more varied response than positive IOD events, you should also include a version of Figure S1 (and possibly Figure S2) for positive IOD events.
Given this point is mentioned in the abstract (lines 10-11), I think Figure S1 should be included in the main manuscript.
Thanks for the comment. We added the figures to the supplementary material for the positive phase of IOD. In addition, we rewrote the sentence in the abstract mentioning the intra-event variability of the negative IOD cases.

On including the figure S1 in the main text, there is only one paragraph in the whole manuscript that refers to the intra-event variability of the negative IOD and it is only a hypothesis. Therefore, we consider it more appropriate not to include this in the abstract.

Lines 199-200: The idea that the negative stationary wavenumber Ks has a role in explaining the large variability across negative IOD events is discussed, but what about the strength of negative events compared to positive events?
Thanks for the comment. Yes, the intensity of the events can make a difference, and we test this hypothesis with the CFSv2 model by separating the events by intensity. Unfortunately, we do not have many observed cases of pure negative IOD to assess this and make general statements about the nature of these events. We added a comment on this in the manuscript.

Lines: 233-236 in the revised manuscript
“However, given the small sample sizes available for the composites and the previously mentioned regression limitations, caution is necessary with these assertions. We must consider whether the observed response accurately reflects the nature of these events or is merely the result of sampling variability.”

Lines 217-220: Can/will these hypotheses be tested with the large ensemble, or is the purpose of the large ensemble simply to increase the sample size? Do you have plans to test these hypotheses?
Thanks for the comment. Besides increasing the sample size, we used the large ensemble to test

the hypothesis about the magnitude, as mentioned in the comment above. The other hypothesis arises from the discussion on Ks, and we plan to test it in the future with model experiments.

Line 240: Do you have an idea why the negative IOD composite in the model is so different to the observed composites? Could this be related to the weak SST anomalies in Figure 9d?
Thanks for the comment. Yes, it is a possibility. However, as we mentioned above, there is also the possibility that the observed sample is not representative of the nature of the negative IOD events and therefore we see a mismatch between observed and model composites.

Lines 246-247: The signal to noise figure is very informative!
Wouldn't we expect the negative IOD to have a lower signal to noise ratio than the positive IOD based on the discussion of Figure S1 (lines 197 onwards)? Including a version of Figure S1 for the positive IOD would be helpful here.
Thank you for the comment. We do not think that the expected S/N ratio for negative IOD has to be lower than that for the positive events. The observed sample size in both cases (positive and negative IODs) is too small to expect the S/N in the CFSv2 model to reflect the observed behavior.

Lines 262-263: The Indian Ocean wave train is still clear for strong negative IOD events in Figure 12g.
Thanks for the comment. We rewrote that sentence and the following one to give a more accurate description.

Where it reads:
"In agreement to the observed composites and regressions, the IO wave train is more evident in the pure positive IODs than in the pure negative ones for both moderate and strong events (middle row). In strong positive IODs, circulation anomalies are notably more intense than those in moderate events."

Now it reads:
"In agreement to the observed composites and regressions, the IO wave train for moderate events is more evident in the pure positive IODs than in the pure negative ones (Fig. 12g, h, i and j). However, for strong events (Fig. 12g and j), the IO wave train is evident for both phases and slightly more intense for the positive one."

Lines 266-270: I would argue that there's more of a difference between the moderate La Niña combinations with negative IOD than for the strong La Niña combinations, particularly over the Indian Ocean.
Thanks for the comment. We rewrote that paragraph to give a more accurate description of the results.

Where it reads:
“However, the combination of La Niña and negative IOD is not associated with a coherent change in
the circulation anomalies (Fig. 12, lower left panel). Composites associated with moderate and strong negative IODs combined with a moderate La Niña events are not notably different from that for the pure moderate La Niña events. On the other hand, the combination of strong La Niña with moderate
or strong IOD tends to amplify the anomalous circulation pattern over middle and high latitudes.”

Now it reads:
“However, the combination of La Niña and negative IOD is not associated with a coherent change in the circulation anomalies (Fig. 12k, l, n and o). Composites for moderate and strong negative IODs (Fig. 12h and g, respectively) combined with moderate La Niña events (Fig. 12n) are not notably different from that for pure moderate La Niña events (Fig. 12m) , in terms of the PSA-like pattern over the Pacific Ocean. However, the contribution of both moderate and strong negative IOD to the La Niña pattern is evident through the presence of the IO wave train (Fig. 12k and l, respectively). On the other hand, the combination of strong La Niña events (Fig. 12p) with moderate or strong IOD tends to amplify the circulation anomalies over middle and high latitudes (Fig. 12o and n, respectively). In moderate La Niña events, the IO wave pattern is also present but weaker, while in the strong La Niña events, it is embedded in the corresponding PSA-like pattern but with opposite sign over southwest Australia."

Line 295: Figure 15d --> Figure 15c? There is only one dashed line indicating significance. Please clarify how this temperature composite is consistent with the regression results.
Figures 15 and 16: The colour bars have changed from Figure 14, i.e., Figure 15 uses brown and green for temperature, and Figure 16 uses blue and red for rainfall.
Thanks for the comments. We have made a mistake in the captions of the figures. The figures are interchanged with respect to their captions, Fig. 15 goes with the captions of Fig. 16 and vice versa. The color palette never changed concerning the variables, i.e for precipitation it is always brown and green, whereas for temperature it is blue and red. We regret that this has led to a misinterpretation of some results.
The captions in figures 15 and 16 were corrected in the manuscript.

Lines 299-314: Are the rainfall results in Australia consistent with Cai et al. (2011, 2012)? Can the Africa results be compared with other studies?
Thanks for the comment. We added the references of Cai et al. 2011 and Cai et al. 2012.
For Africa, we have not found any work that analyzes the impact of ENSO-IOD in the southern tip of the continent during SON. In general, studies focus on tropical latitudes and East Africa, where the IOD has significant impacts.

Section 3.3: It would be great if you could link the changes in rainfall and temperature to the circulation changes in Section 3.1. E.g. if the wave train shifts, how does this affect regional climate?
Thanks for the comment. The goal of section 3.3 is to analyze the overall impact on temperature and precipitation over the main continental region, highlighting the notable differences between variables and between phases of ENSO, IOD, and their combinations. Performing a regional analysis of each continent is out of the scope of this paper. A regional analysis requires more detail, considering regional factors (e.g., topography) and other more appropriate variables (e.g., surface circulation, moisture convergence, outgoing longwave radiation). On the other hand, based on the mixed results obtained, this type of analysis should be even more exhaustive.That said, we added to section 3.3 links between surface impacts and circulation without losing sight of the more general approach.

Section 3.3: Do the rainfall and temperature regressions and composites in the model look similar to the observations? Perhaps the model results could be included in the supplementary material.
Thank you for the comment. We have explored temperature and precipitation responses in the CFSv2 model but only in South America, which is the main focus of my PhD and it is the region with less studies on this matter. However, for brevity, we have not included this analysis in this manuscript and we left it for an upcoming paper to be submitted soon.

Technical corrections:

Ensure all acronyms are defined throughout the manuscript (e.g., CFSv2 and PSA in the Abstract, and SST, z200, and CFSv2 in the main text).

Try to refer to specific panels in the figures as much as possible rather than using descriptions like “middle row” or “upper right panel”.

Line 122: “where” calculated --> “were” calculated.
Line 187: Replace “not very tidy” with another phrase.
Line 300: “territory” --> “country”
We have corrected all the above listed issues.

Reviewer 1 and 2:
Based on the comments from both reviewers, the following changes were made to the figures:

- Fig. 1 and 8. Darker color for pure positive IODs.
- Fig. 2 and 4. Change in the SST color palette and in the color of the divergence contours.
- Fig. 3, 5 and 6: Scale changes to avoid saturation of shading. Increasing the spacing, size and thickness of WAF vectors. Changed color of outlines from black to gray.
- Added labels to all graphics within each figure.

References:

Cai, W., Rensch, P., Cowan, T., and Hendon, H.: Teleconnection Pathways of ENSO and the IOD and the Mechanisms for Impacts on Australian Rainfall, Journal of Climate - J CLIMATE, 24, 3910–3923, https://doi.org/10.1175/2011JCLI4129.1, 2011.

Cai, W., Rensch, P., Cowan, T., and Hendon, H.: An Asymmetry in the IOD and ENSO Teleconnection Pathway and Its Impact on Australian Climate, Journal of Climate, 25, 6318–6329, https://doi.org/10.1175/JCLI-D-11-00501.1, 2012.

Chan, S., Behera, S., and Yamagata, T.: Indian Ocean Dipole influence on South American rainfall, Geophysical Research Letters, 35, https://doi.org/10.1029/2008GL034204, 2008.

Kumar, A. and Chen, M.: What is the variability in US west coast winter precipitation during strong El Niño events?, Climate Dynamics, 49, 1–14, https://doi.org/10.1007/s00382-016-3485-9, 2017.

Kumar, A., Chen, M., Zhang, L., Wang, W., Xue, Y., Wen, C., Marx, L., and Huang, B.: An Analysis of the Nonstationarity in the Bias of Sea Surface Temperature Forecasts for the NCEP Climate Forecast System (CFS) Version 2, Monthly Weather Review, 140, 3003 – 3016, https://doi.org/https://doi.org/10.1175/MWR-D-11-00335.1, 2012.

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