Web links to the author's journal account have been redacted from the decision letters as indicated to maintain confidentiality.

9th Feb 21

Dear Dr Paul,

Your manuscript titled "An Indo-Pacific see-saw wobbles the Earth at intraseasonal timescales" has now been seen by 2 reviewers, whose comments are appended below. You will see that they find your work of some potential interest. However, they have raised quite substantial concerns that must be addressed. In light of these comments, we cannot accept the manuscript for publication, but would be interested in considering a revised version that fully addresses these serious concerns.

We hope you will find the reviewers' comments useful as you decide how to proceed. Should additional work allow you to

• address these criticisms (that is, either to incorporate the suggestions or provide a compelling argument why the point made by the reviewer is not valid, or relevant to the editorial threshold as outlined below)

AND

• meet our editorial thresholds as outlined below,

then we would be happy to look at a substantially revised manuscript.

In the following, we list our main editorial concerns.

****

Editorial threshold 1: Demonstrate how your results show conclusively that the oceanic mass redistribution due to the MJO (i.e, the Indo-Pacific seesaw) is the main driver of the observed wobble of the earth at intraseasonal timescales.

Editorial threshold 2: In light of the literature pointed to by one of the reviewers, that these results provide new insights into the question of the oceanic excitation of earth's wobble via barotropic adjustment.

****

However, please bear in mind that we will be reluctant to approach the reviewers again in the absence of substantial revisions.

If the revision process takes significantly longer than three months, we will be happy to reconsider your paper at a later date, as long as nothing similar has been accepted for publication at Communications Earth & Environment or published elsewhere in the meantime.

We understand that due to the current global situation, the time required for revision may be longer than usual. We would appreciate it if you could keep us informed about an estimated timescale for resubmission, to facilitate our planning. Of course, if you are unable to estimate, we are happy to accommodate necessary extensions nevertheless.

We are committed to providing a fair and constructive peer-review process. Please do not hesitate to contact us if you wish to discuss the revision in more detail.

Please use the following link to submit your revised manuscript, point-by-point response to the referees' comments (which should be in a separate document to any cover letter) and any completed checklist:

[link redacted]

** This url links to your confidential home page and associated information about manuscripts you may have submitted or be reviewing for us. If you wish to forward this email to co-authors, please delete the link to your homepage first **

Please do not hesitate to contact me if you have any questions or would like to discuss the required revisions further. Thank you for the opportunity to review your work.

Best regards,

Joy Merwin Monteiro, PhD
Communications Earth & Environment
orcid.org/0000-0002-3932-3603

EDITORIAL POLICIES AND FORMAT

If you decide to resubmit your paper, please ensure that your manuscript complies with our editorial policies and complete and upload the checklist below as a Related Manuscript file type with the revised article:

Editorial Policy Policy requirements

For your information, you can find some guidance regarding format requirements summarized on the following checklist:(https://www.nature.com/documents/commsj-phys-style-formatting-checklist-article.pdf) and formatting guide (https://www.nature.com/documents/commsj-phys-style-formatting-guide-accept.pdf).

REVIEWER COMMENTS:

Reviewer #1 (Remarks to the Author):

Review of "An Indo-Pacific see-saw wobbles the Earth at intraseasonal timescales" by Afroosa et al.

This paper examines a barotropic ocean response to the Madden-Julian Oscillation that quickly moves mass between the tropical Indian and Pacific Oceans. This mass exchange is enough to cause measurable changes in Earth rotation that match observations. The authors demonstrate the MJO wind changes are the cause using a sensitivity experiment.

I found the results of this paper novel and certainly publishable [...]. While the paper is generally well-written, there are some aspects which were either confusing, contradictory, or written in a way to slow comprehension. I will point these out in specific comments below. I suggest a careful review by the authors and the editor to make the paper more readable.

I thought the experimental setup was done well and the results do support the conclusions that the MJO winds cause notable and regular fluctuations in the Earth rotation due to propagating barotropic signals.

I do have some questions about the analysis, however. Primarily, I think the authors need to discuss why they choose relatively small boxes for their index when the correlation patterns shown in Figure 2 are large-scale. I also see no mention of low-pass filtering the time series either in the manuscript or the supplemental material. However, the curves are clearly smoothed. Finally, I have some concerns about the quantitative analysis of the correlations, especially the significance. Was this smoothing accounted for in determining the degrees of freedom? I will discuss all of these below as well.

Overall, I feel this is a paper that could be published in the journal, since it is, to my knowledge, the first time this see-saw effect has been observed and linked to Earth rotation variations. However, I think some more work is needed to improve the readability and to full describe the analysis performed.

Improving readability
===============
1. There are a lot of acronyms used in this manuscript. Some, like MJO, are common so are easy to follow in the context of the sentence. Others (like TIO or EWDA) are not so common, and so when reading I had to remind myself what these were over and over again. I know scientific writing tends to be acronym heavy, but I would appreciate if the authors tried to minimize their usage. And for EWDA, why don't they just use bottom pressure anomalies (with a note they have been converted to equavialent water thickness via the hydrostatic equation).

2. I found many of the statements on lines 46-62 contradictory. For instance, "Most of those signals come from the variability in the angular momentum of the Earth’s core and the atmosphere" is followed a few sentences later by "As a consequence, though the ocean is believed to be a major driver of the polar motion...". I think a careful re-write of this paragraph is in order so the authors can make their major point: polar motion is caused by the motions in the core and the fluid envelope of the Earth (atmosphere and oceans). Studies have shown the ocean plays a large role in polar motions variations, but the exact mechanisms have not yet been determined.

3. I know authors commonly try to cram a lot of information into a single sentence using ()

statements, like the authors do on 86-89: "During a strong positive (negative) See-saw Index peak, ~1.5 Sv (2.6 Sv) [1 Sv =106 m3s-1] of water is gained (lost) in the Indian (Pacific) Ocean, which is equivalent to a spatially uniform basin rise (fall) of ~1.0 cm (0.8 cm) of EWD in the Indian (Pacific) Ocean." Reading such a sentence and gleaning the information in it from a single read is virtually impossible, however. I always have to re-read it multiple times to get the meaning. Is it really that hard to just break this into two separate sentences, one discussing the Positive peak, and the other the negative peak?

Comments on the Analysis
===================

1. Figure 2 is very nice. It also clearly shows the large-scale correlations in the bottom pressure. Why then do the authors use two small boxes equatorial boxes [((65°E:75°E, 5°S:5°N) and (155°W:165°W, 5°S:5°N))] for their calculation of the index? Wouldn't larger areas defined by the correlation patterns be more meaningful? Is there any significant difference between the two? The authors need to at least justify why they use the smaller area.

2. The see-saw index in Figure 1 has clearly been low-pass filtered in some manner. However, I see no discussion in either the main paper or supplemental material on how. Please indicate somewhere how the data were filtered. This is vital for replicability of results.

3. The comparison of the two bottom pressure recorders in figure 3a is not that convincing of the see-saw pattern, in my opinion. The authors do not state the correlation between the two, but I cannot believe it is that high. The correlations of the bottom pressure recorders with the single one in Figure 3b is more convincing. I don't really see that Figure 3a provides any better information than Figure 3b does. I suggest removing Figure 3a. Also, again, these BPR fluctuations have clearly been low-pass filtered. Describe how somewhere in the text or supplemental material.

4. It is stated that the correlations significance is 90%. Did the authors account for the smoothing when computing degrees of freedom for this calculation? Any smoothing of a time-series will reduce degrees of freedom because serial correlation is increased. Some discussion of this is needed.

5. The authors state: "Due to the Chandler wobble resonance that dominates the polar motion it is not possible to directly compare our model-derived estimates with polar motion observation. However, we can compute the excitation functions required to generate the observed polar motion during the strong see-saw of 2012-13."

This period is also affected by Chandler wobble, though. So this argument is not exactly correct. Really, the authors are looking at that period because the Index is so high that it suggests that the oceanic signal will be much larger and not potentially obscured by a Chandler Wobble, as would other periods. Please re-state the reasoning for looking at that one specific period more clearly.

Reviewer #2 (Remarks to the Author):

The paper describes interesting results regarding intraseasonal variability in ocean bottom pressure and associated circulation, mostly confined to the Indian and Pacific basins, which is claimed to “wobble the Earth”. The work expands earlier findings of large-scale bottom pressure signals in the

Indian Ocean related to MJO winds and is of wide interest for the oceanographic and geodetic communities. I enjoyed reading the paper but have a couple of issues with the interpretation of the results. In addition, in attempting to spice up their findings, the authors end up making unnecessary claims and overlooking previous works on oceanic excitation of polar motion and oceanic large-scale barotropic variability. Because of these issues, detailed below, I cannot recommend publication at this point.

My major point can be traced to fig 4. It seems clear from the plotted curves that the observed excitation (grey) is nearly out of phase with the estimated oceanic excitation (violet), over most of January-March period. Thus, the correct interpretation for this period would be that the atmospheric and hydrologic excitation (not shown) is considerably larger and nearly in phase with the observed excitation, and that the effect of the ocean is to essentially dampen that excitation.

Such interpretation is very different from the claim that the ocean is wobbling the Earth. Yes, fig 4 shows that the ocean is an important contributor to polar motion excitation for the 2013 winter, but seems like it is not the largest contributor nor the "real" driver. Even after cherry picking the year where the "seesaw" is strongest! Such results would be clearer if the atmospheric and hydrologic excitation was shown in the figure or in another supplementary plot. I think a more candid interpretation of fig 4 is needed and the flavor of the paper will most likely change.

Some aspects of fig 2 would also benefit from clarification. While it is true that the patterns in panels a, b are similar, as emphasized in the text (115-117), it is also important to note that the anticorrelations are considerably stronger and of broader scale in the case of wind forcing confined to the Maritime Continent. This suggests that intraseasonal variability in the Pacific has a strong component that is "locally" forced and not a part of the "seesaw" driven by winds over Maritime Continent. This is true for the whole Pacific, not just over the Arctic and North Pacific as stated (119-120).

The variance explained analysis in fig 2c is also consistent with the interpretation that the variability in the Pacific is much more than just that related to the "seesaw" highlighted by the authors. Ultimately this variability measurably affects the oceanic excitation (e.g., in January and February 2013 as indicated in fig 4). Results thus need to be framed in such light. It is not at all demonstrated that the "seesaw" will be the most relevant process for intraseasonal oceanic excitation aside from the specific event of 2013. And even for 2013, the story is not as linear as stated in the paper.

Some other points follow, sequentially listed by line/figure number.

26 "is the first signature from a climate mode that can be isolated into the Earth polar axis motion" Rewrite more clearly, including removing the reference to MJO as "climate mode". The MJO is normally referred to as a major intraseasonal mode of variability of the tropical atmosphere, and I have not seen it referred to as a "climate mode" like ENSO, PDO or AMO, obviously for reasons of its intraseasonal time scale.

31-32 Statement seems imprecise: it can suggest that the MJO continuously travels around the globe as a pure zonal wavenumber one disturbance, which is somewhat simplistic.

43-45 Not really a "discovery"! The barotropic response to MJO-like wind stress anomalies has been discussed, for example, by Ponte and Gutzler (1992, JGR), indicating the expected large-scale nature of the adjustment.

48 “near-global rotation of the ocean”? Not clear what is meant, please rephrase.

51-62 This discussion, set in most general terms, does not do justice to what we know about ocean angular momentum and its role in driving polar motion since the earlier work of Ponte et al. (1998, Nature) and the subsequent literature. At the end of this text, the authors seem to imply that their work adds “evidence of climate modes excitation of polar motion through ocean”, which takes the MJO, in my view inappropriately, to be a “climate mode”. (see also comments about line 26)

fig 1 Text should clarify if surface freshwater fluxes are included when estimating volume fluxes. This would be the case if the model uses a real freshwater flux boundary condition. Also perhaps you should refer in the caption to the basin boundaries in fig 2d, which are used to define the volume fluxs in fig 1a,b.

81-82 The winter of 2013 does not seem to show particularly strong MJO activity judging from fig 1c? In this regard, it might help to draw dashed lines encompassing the phases associated with Maritime Continent activity in fig 1c, which are noted in the caption.

87-89 Sentence is unreadable, preferable to have two separate sentences

97-98 This is nothing really new. It is well known that the ocean is capable of showing large scale bottom pressure signals at a variety of time scales, from tidal to much longer periods (e.g., Stepanov and Hughes, 2006, JGR).

fig 1+2 It would be useful for the reader to have more explanation about why some particular choices are made and whether results are sensitive to these choices. Why define the seesaw in terms of small boxes in middle of the basins, instead of larger basin-scale averages? Why not use simpler southern boundaries in fig 2d (e.g., straight lines along 30S)? Why pick the yellow star site for the correlations?

fig 2+3 The text should be explicit about how statistical significance is calculated for the correlations shown in these figures, particularly how the degrees of freedom are estimated. Also I am assuming that only significant correlation values are plotted (in color), but the caption should be more explicit.

121-126 I don’t see 30% values over large Pacific regions in fig 2c, probably only along the equator, where it is expected because the winds in the Maritime Continent box are capable of exciting the equatorial Pacific wave guide. Thus the results do not seem surprising.

143-144 How is the “4-6 cm peak-to-peak” change in the Indian Ocean consistent with the value of 2 cm that we infer from what is stated in line 89 for strong seesaw events and also from fig S3?

146-147 All BPRs shown in fig 3 in the Pacific seem to show anticorrelation, so I don’t understand where the 42% number comes from. Are you only showing in fig 3 the anticorrelated BPRs?

161-163 These two sentences seem to convey the same idea? Please remove the redundancy or clarify their different meanings.

166 Awkward and unclear sentence, please rewrite.

fig 4b I don’t find this schematic illuminating. It does not really add anything to the story and it would be best deleted. More interesting and useful would be to have effects of ocean bottom pressure and currents on chi_2 separated and discussed, so that the reader gets to know what is their relative contributions to the oceanic excitation.

198-209 This text needs to be rewritten to address some of the issues raised above, including comments on lines 26, 43-45, 51-62, 97-98.

REVIEWER COMMENTS:

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Review of "An Indo-Pacific see-saw wobbles the Earth at intraseasonal timescales" by Afroosa et al.

This paper examines a barotropic ocean response to the Madden-Julian Oscillation that quickly moves mass between the tropical Indian and Pacific Oceans. This mass exchange is enough to cause measurable changes in Earth rotation that match observations. The authors demonstrate the MJO wind changes are the cause using a sensitivity experiment.

I found the results of this paper novel and certainly publishable [...]. While the paper is generally well-written, there are some aspects which were either confusing, contradictory, or written in a way to slow comprehension. I will point these out in specific comments below. I suggest a careful review by the authors and the editor to make the paper more readable.

I thought the experimental setup was done well and the results do support the conclusions that the MJO winds cause notable and regular fluctuations in the Earth rotation due to propagating barotropic signals.

Comment 1 : I do have some questions about the analysis, however. Primarily, I think the authors need to discuss why they choose relatively small boxes for their index when the correlation patterns shown in Figure 2 are large-scale. I also see no mention of low-pass filtering the time series either in the manuscript or the supplemental material. However, the curves are clearly smoothed. Finally, I have some concerns about the quantitative analysis of the correlations, especially the significance. Was this smoothing accounted for in determining the degrees of freedom? I will discuss all of these below as well.

Overall, I feel this is a paper that could be published in the journal, since it is, to my knowledge, the first time this see-saw effect has been observed and linked to Earth rotation variations. However, I think some more work is needed to improve the readability and to full describe the analysis performed.

Response 1: We thank the Reviewer for the critical comments. We have responded against each comment made by the Reviewer. Additional references have been added. One new plot (Fig 4b), and the associated text has been added in the revised manuscript to present the relative contribution of ocean mass and ocean motion to the polar movement excitation function. One new section has been introduced in the Supplementary Methods to describe the estimation of the degrees of freedom. We hope that we have addressed all the concerns raised by the Reviewer.

Improving readability
==============

Comment 2. There are a lot of acronyms used in this manuscript. Some, like MJO, are common so are easy to follow in the context of the sentence. Others (like TIO or EWDA) are not so common, and so when reading I had to remind myself what these were over and over again. I know scientific writing tends to be acronym heavy, but I would appreciate if the authors tried to minimize their usage. And for EWDA, why don't they just use bottom pressure anomalies (with a note they have been converted to equavialent water thickness via the hydrostatic equation).

Response 2: We understand that the use of many acronyms can be difficult for the readers. We have therefore incorporated the suggestion of the Reviewer in the revised manuscript. We have discontinued the use of the following acronyms to improve the readability of the revised manuscript: TIO (tropical Indian Ocean), EWD (Equivalent water depth), EWDA (Equivalent water depth anomaly), BPR (bottom pressure recorder) and ITF (Indonesian ThroughFlow). We believe that it will improve the readability of the revised manuscript. Acronyms like MJO and NEMO are popular indeed and hence have been retained. There are acronyms like BPR-MC and BPR-PAC which designate specific bottom pressure recorders and have been retained. MC-EXP, which refers to a sensitivity experiment, is also retained. Identical changes are carried out in the revised Supplementary material as well. So, there are only 5 acronyms used in the revised manuscript, namely, MJO, NEMO, BPR-MC, BPR-PAC and MC-EXP.

Comment 3. I found many of the statements on lines 46-62 contradictory. For instance, "Most of those signals come from the variability in the angular momentum of the Earth's core and the atmosphere" is followed a few sentences later by "As a consequence, though the ocean is believed to be a major driver of the polar motion...". I think a careful re-write of this paragraph is in order so the authors can make their major point: polar motion is caused by the motions in the core and the fluid envelope of the Earth (atmosphere and oceans). Studies have shown the ocean plays a large role in polar motions variations, but the exact mechanisms have not yet been determined.

Response 3: We thank the Reviewer for pointing out the contradictions that have inadvertently creeped in the text. We have rewritten the major parts of this paragraph to eliminate contradictions. We believe that the new write up in the revised manuscript (line 53-72) is lucid and devoid of any contradictions. We have copied below the modified paragraph for easy reference.

"The see-saw generation involves large-scale mass redistribution and currents across and within the Pacific and Indian basins. The associated global-scale angular momentum arising both from a large-scale circulation in the ocean and a global-scale mass redistribution, is expected to leave its signature in the Earth's rotation(Ponte et al., 1998 ; Gross 2003 & Gross et al,, 2004). The Earth's rotation is not constant, and presents fluctuations in a broad range of frequencies(Lambeck 1980 & Gross 2007). Most of those signals come from the variability in the angular momentum of the Earth's core and the fluid envelope of the Earth (atmosphere and ocean) (Lambeck 1980). The signature from the ocean is relatively elusive, because the geometry of the ocean, interspersed with continental landmasses, is not always optimal to generate a strong global angular momentum fluctuation associated with an observable impact on the Earth's rotation. The polar motion, i.e. the wobble of the solid Earth around its rotation axis, has an even less favorable geometry: it can be excited by either a global current corresponding to a rotation around an axis at the equator or a global-scale mass anomaly with a 45°-tilted ellipsoid(Barnes et al., 1983). An ocean impact over polar motion can thus only be generated by strong global-scale dynamics, with a very particular geometry. As a consequence, there is hitherto only little evidence of prominent intraseasonal modes of excitation of the polar motion through the ocean, with previously reported estimates of the oceanic contribution typically two to three

times weaker than the atmospheric counterparts at intraseasonal periodicities (Ponte et.al, 1998; Natsula and Ponte, 1999; Zhou et al, 2005). Our study demonstrates that the MJO-induced see-saw, being a large-scale process, does leave oceanic footprints on the polar motion. The excitations induced by the oceans are of the same order of magnitude as the atmosphere. ”

Comment 4. I know authors commonly try to cram a lot of information into a single sentence using () statements, like the authors do on 86-89: "During a strong positive (negative) See-saw Index peak, ~1.5 Sv (2.6 Sv) [1 Sv =10^6 m^3s^{-1}] of water is gained (lost) in the Indian (Pacific) Ocean, which is equivalent to a spatially uniform basin rise (fall) of ~1.0 cm (0.8 cm) of EWD in the Indian (Pacific) Ocean." Reading such a sentence and gleaning the information in it from a single read is virtually impossible, however. I always have to re-read it multiple times to get the meaning. Is it really that hard to just break this into two separate sentences, one discussing the Positive peak, and the other the negative peak?

Response 4: **We have now broken that sentence into three sentences to improve the readability of the revised manuscript (line 98-103). We have also copied below these sentences for ready reference.**

“ During a strong positive See-saw Index peak, ~1.5 Sv [1 Sv = 10^6 m^3s^{-1}] of water is gained by the Indian Ocean whereas ~2.6 Sv of water is lost by the Pacific Ocean. This is equivalent to a spatially uniform basin rise of ~1.0 cm in the Indian Ocean and a concurrent fall of ~0.8 cm of equivalent water depth in the Pacific Ocean. This dynamics reverses during the negative peak of the See-saw Index (Supplementary Fig.S3).”

Comments on the Analysis

Comment 5. Figure 2 is very nice. It also clearly shows the large-scale correlations in the bottom pressure. Why then do the authors use two small boxes equatorial boxes [((65°E:75°E, 5°S:5°N) and (155°W:165°W, 5°S:5°N))] for their calculation of the index? Wouldn't larger areas defined by the correlation patterns be more meaningful? Is there any significant difference between the two? The authors need to at least justify why they use the smaller area.

Response 5: We thank the Reviewer for raising this issue. The location of the smaller boxes, that defined the See-saw Index in the earlier version of the manuscript, was chosen in the area that displayed the largest correlation (Fig. 2a). However, even if we choose boxes that encompass the entire two basins - Indian and Pacific - to define the See-saw Index, the qualitative feature of the time-series of See-saw Index remains unchanged. Below we share the comparison plot of See-saw Index estimated from the earlier small boxes (red line) and estimated from the entire two basins (black line) (Fig. R1).
We have now used the definition that involves the entire two basins in the revised manuscript (see also the revised Fig1a,b). The new definition in the revised manuscript (88-89) is as follows:

“We define a See-saw Index from the control run as the normalized difference of mean equivalent water depth anomaly between the Indian and Pacific basins.”

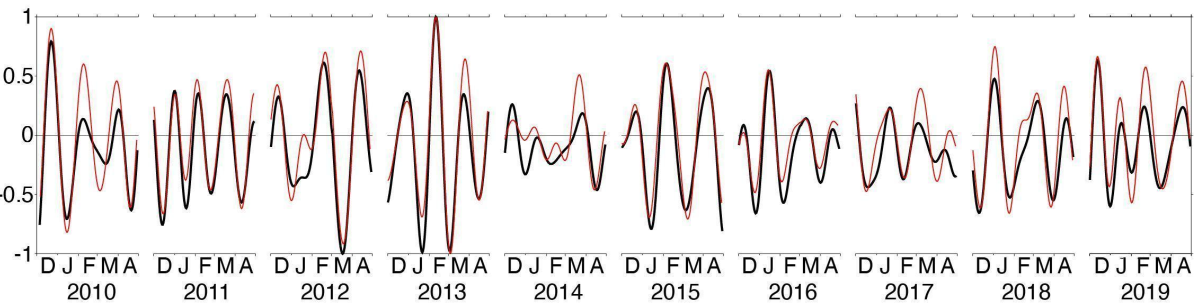

Fig R1 | Seesaw Index comparison : Black line represents the Seesaw Index estimated from the entire Indian and Pacific basin. Red line represents the Seesaw Index estimated from the small boxes in Indian (65° E:75° E, 5° S:5° N) and Pacific (155° W :165 °W, 5° S:5° N) basins.

Comment 6. The see-saw index in Figure 1 has clearly been low-pass filtered in some manner. However, I see no discussion in either the main paper or supplemental material on how. Please indicate somewhere how the data were filtered. This is vital for replicability of results.

Response 6: Our entire study is based on intraseasonal timescales (30-80 day band) during boreal winter months (Dec-April). All filtering (30-80 day band) across all variables was done using Lanczos filter. It was mentioned in the previously submitted Supplementary Materials that the bottom pressure records (both model and observation) were intraseasonally filtered using Lanczos filter (line 46-47 and line 85-86). However, for clarity, as suggested, we have now mentioned that in the main text of the revised manuscript (86-88). The following line is added:

"All quantities subsequently analyzed have been intraseasonally filtered using Lanczos filter to isolate the 30-80 days timescale unless otherwise mentioned."

Comment 7. The comparison of the two bottom pressure recorders in figure 3a is not that convincing of the see-saw pattern, in my opinion. The authors do not state the correlation between the two, but I cannot believe it is that high. The correlations of the bottom pressure recorders with the single one in Figure 3b is more convincing. I don't really see that Figure 3a provides any better information than Figure 3b does. I suggest removing Figure 3a. Also, again, these BPR fluctuations have clearly been low-pass filtered. Describe how somewhere in the text or supplemental material.

Response 7: **We thank the Reviewer for the suggestion to remove Fig 3a. But Fig 3a is necessary to indicate the amplitude of bottom pressure anomalies in the Pacific and Indian Ocean. BPR-MC and BPR-PAC exhibit strong anti-correlation during strong MJO years - 2011-12, 2012-13 and 2016-17. The correlation coefficient is -0.62 (>99 % significance). We have therefore decided to retain this figure in the revised manuscript to convey a sense of amplitude in bottom pressure fluctuations to the readers who may not be very familiar with the typical range of bottom pressure fluctuations. The BPR fluctuations were indeed filtered (30-80 day) which was also mentioned in the previously submitted supplementary (line 89-90). It was also mentioned in the caption of erstwhile Fig 3. The same is again mentioned in the revised manuscript (line 86-88).**

Comment 8. It is stated that the correlations significance is 90%. Did the authors account for the smoothing when computing degrees of freedom for this calculation? Any smoothing of a time-series will reduce degrees of freedom because serial correlation is increased. Some discussion of this is needed.

Response 8: We have indeed accounted for the smoothing while estimating the degree of freedom. We have used the following formula for estimating the degree of freedom (DOF) for a band-passed time series and subsequent significance:

$$DOF = 2N\left(\frac{\Delta T}{T_{c1}} - \frac{\Delta T}{T_{c2}}\right) - 2,$$

Where, $\Delta T$is the sample interval, T_{c1} and T_{c2} are the cutoff periods in the bandpass filtering ($T_{c1} < T_{c2}$)and N is the sample size. In this study for intraseasonal (30-80 days) band-pass filter, ΔT is taken as 1 day, T_{c1} and T_{c2} are 30 and 80 days respectively, and N is 1501 (10 years of daily data during December-April). Based on this equation, we estimated that the DOF for the winter months (December-April) of 2009-2019 is 60. Corresponding to this DOF, the correlation values greater than 0.21 and less than -0.21 are 90% significant in accordance to the Pearson correlation table.
We have included a section in the modified supplementary (line 92-102) to highlight that we have indeed taken care of smoothing while estimating the degree of freedom. We have also now referred it in the caption of Fig 2 and Fig 3 in the revised manuscript.

Reference :

1. **Bendat, J. S. & Piersol, A. G. *Random Data. Random Data: Analysis and Measurement Procedures: Fourth Edition* (John Wiley & Sons, Inc., 2010).**

Comment 9. The authors state: "Due to the Chandler wobble resonance that dominates the polar motion it is not possible to directly compare our model-derived estimates with polar motion observation. However, we can compute the excitation functions required to generate the observed polar motion during the strong see-saw of 2012-13."

This period is also affected by Chandler wobble, though. So this argument is not exactly correct. Really, the authors are looking at that period because the Index is so high that it suggests that the oceanic signal will be much larger and not potentially obscured by a Chandler Wobble, as would other periods. Please re-state the reasoning for looking at that one specific period more clearly.

Response 9: We thank the Reviewer for pointing this out. Considering the narrow-band filtering and the frequency range away from the Chandler Wobble band, we agree that we could work directly from the observed polar motion, rather than transforming the polar motion in terms of observation. On the other hand, this would mean to integrate the excitation from the models, which is unstable and dependent on the initial conditions (Chao et al., 2012), and then filter both polar motion observation and excitation to compare them. In addition, the comparison between observation and excitation of the polar motion is classically performed in the excitation domain (Lambert et al, 2006, Wilson & Haubrich 2007, among many others), and following this methodology helps the comparison with previous studies.

References:

1. **Chao, B. F. & Chung, W.-Y. Amplitude and phase variations of Earth's Chandler wobble under continual excitation. *J. Geodyn.* 62, 35–39 (2012).**
2. **Lambert, S. B., Bizouard, C. & Dehant, V. Rapid variations in polar motion during the 2005-2006 winter season. *Geophys. Res. Lett.* 33, 2–5 (2006).**
3. **Wilson, C. R. & Haubrich, R. A. Meteorological Excitation of the Earth's Wobble. *Geophys. J. R. Astron. Soc.* 46, 707–743 (2007).**

Reviewer #R2 (Remarks to the Author):

Comment 1: The paper describes interesting results regarding intraseasonal variability in ocean bottom pressure and associated circulation, mostly confined to the Indian and Pacific basins, which is claimed to "wobble the Earth". The work expands earlier findings of large-scale bottom pressure signals in the Indian Ocean related to MJO winds and is of wide interest for the oceanographic and geodetic communities. I enjoyed reading the paper but have a couple of issues with the interpretation of the results. In addition, in attempting to spice up their findings, the authors end up making unnecessary claims and overlooking previous works on oceanic excitation of polar motion and oceanic large-scale barotropic variability. Because of these issues, detailed below, I cannot recommend publication at this point.

Response 1: We thank the Reviewer for the constructive comments on the important aspects of the paper. We have rewritten the interpretation of our results and have also responded to the specific comments of the Reviewer. Additional references have been included. One new plot (Fig 4b) and associated text have been included in the revised manuscript to present the relative contribution of ocean mass and ocean motion to the polar movement excitation function. One new section has been introduced in the Supplementary Methods to describe the estimation of the degrees of freedom. We hope that we have addressed all the concerns raised by the Reviewer.

Comment 2: My major point can be traced to fig 4. It seems clear from the plotted curves that the observed excitation (grey) is nearly out of phase with the estimated oceanic excitation (violet), over most of January-March period. Thus, the correct interpretation for this period would be that the atmospheric and hydrologic excitation (not shown) is considerably larger and nearly in phase with the observed excitation, and that the effect of the ocean is to essentially dampen that excitation.

Such interpretation is very different from the claim that the ocean is wobbling the Earth. Yes, fig 4 shows that the ocean is an important contributor to polar motion excitation for the 2013 winter, but seems like it is not the largest contributor nor the "real" driver. Even after cherry picking the year where the "seesaw" is strongest! Such results would be clearer if the atmospheric and hydrologic excitation was shown in the figure or in another supplementary plot. I think a more candid interpretation of fig 4 is needed and the flavor of the paper will most likely change.

Response 2: We thank the Reviewer for pointing this out. Yes, indeed the peak atmospheric contribution dominates that of the ocean but not by a large margin. The atmospheric contribution to the excitation function is of the same order of magnitude as the oceanic contribution and the peak is larger by only ~30% (please see Fig 4a in the revised manuscript). In contrast, the contribution from hydrology at intraseasonal scales during boreal winters is inferior to that of both oceans and atmosphere by an order of magnitude. We have now replotted Fig 4a wherein we have also shown the excitation from the atmosphere and hydrology in addition to that of the ocean. We also agree with the Reviewer that the ocean is not always the driver of the polar motion. Instead, as the Reviewer mentioned, the polar motion is mostly driven by the atmosphere. However, there are occasions where the phase of the oceanic excitations lead the polar motion observation leading to speculations that the oceans may be a driver, at least at times. Most of the time though, the role of the oceans is to dampen/stabilize the polar motion of the Earth. Nevertheless, the signature of oceanic contribution to the polar motion of the Earth is detectable and quite apparent. We have therefore modified the interpretation of Fig 4a in the revised manuscript (line 204-234).

Comment 3: Some aspects of fig 2 would also benefit from clarification. While it is true that the patterns in panels a, b are similar, as emphasized in the text (115-117), it is also important to note that the anticorrelations are considerably stronger and of broader scale in the case of wind forcing confined to the Maritime Continent. This suggests that intraseasonal variability in the Pacific has a strong component that is "locally" forced and not a part of the "seesaw" driven by winds over Maritime Continent. This is true for the whole Pacific, not just over the Arctic and North Pacific as stated (119-120).

Response 3: We agree with the Reviewer. Local dynamics and/or remote dynamics not originating from the Maritime Continent indeed play a strong role in the entire Pacific Ocean. This is also apparent in Fig 2c where we show that only ~15-20% of the equivalent water depth variability in the Pacific Ocean is explained by the winds over the Maritime Continent. In contrast, the Arctic and the Atlantic Ocean are mostly not influenced by the winds over the Maritime Continent. We have now clarified this aspect in the revised manuscript (line 142-148).

Comment 4: The variance explained analysis in fig 2c is also consistent with the interpretation that the variability in the Pacific is much more than just that related to the "seesaw" highlighted by the authors. Ultimately this variability measurably affects the oceanic excitation (e.g., in January and February 2013 as indicated in fig 4). Results thus need to be framed in such light. It is not at all demonstrated that the "seesaw" will be the most relevant process for intraseasonal oceanic excitation aside from the specific event of 2013. And even for 2013, the story is not as linear as stated in the paper.

Response 4: We do agree that the local dynamics in the Pacific Ocean may considerably contribute to the oceanic excitation. However, the oceanic excitation due to winds over the Maritime Continent explains ~70% of the oceanic excitation due to winds over the entire globe during 2012-13. This clearly demonstrates that the see-saw, facilitated by the winds only over the Maritime Continent, casts a dominant influence on the polar motion of the Earth during the boreal winter of 2012-13. If we extend this analysis over the entire duration of our study (2009-2019), the oceanic excitation due to these winds explain ~35% of the global oceanic excitation. In contrast, if we consider two strong years of 2011-12 and 2012-13, this number rises to 60%. We have now introduced a Supplementary

Figure (Supplementary Fig 5) where we present the time series of χ_2 from MC-EXP and the control run to address concerns about the dominance of see-saw in exciting polar motions compared to local dynamics during strong MJO years. The same plot is also reproduced below for the convenience of the Reviewer (Fig. R2). This is largely because the mass and motion terms in the excitation function induced by the see-saw are synchronous and are of the same order of magnitude (see Fig. 4b and the associated text (line 223-234) in the revised manuscript). These terms add up and yield a detectable signal in the polar motion.

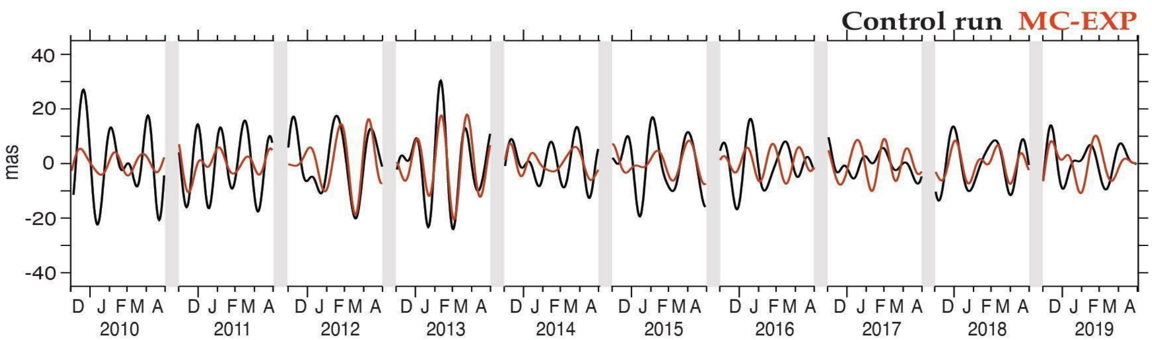

Fig R2 | Plot of time evolution of the intraseasonal χ_2 estimated from control run (black) and MC-EXP (red) during each December-April of 2009-2019.

Comment 5: Some other points follow, sequentially listed by line/figure number.

26 "is the first signature from a climate mode that can be isolated into the Earth polar axis motion" Rewrite more clearly, including removing the reference to MJO as "climate mode". The MJO is normally referred to as a major intraseasonal mode of variability of the tropical atmosphere, and I have not seen it referred to as a "climate mode" like ENSO, PDO or AMO, obviously for reasons of its intraseasonal time scale.

Response 5: We have discontinued referring to MJO as a climate mode in the revised manuscript (line 25-28). The modified sentence is as below:

"We also explain how this large-scale MJO-induced oceanic phenomenon is the first signature from a prominent intraseasonal mode that can be isolated into the Earth polar axis motion, in particular during the strong see-saw of early 2013."

Comment 6: 31-32 Statement seems imprecise: it can suggest that the MJO continuously travels around the globe as a pure zonal wavenumber one disturbance, which is somewhat simplistic.

Response 6: We thank the Reviewer for pointing this out. We have made appropriate changes in the revised manuscript (line 31-35). The modified description of MJO is as below:

"The Madden-Julian Oscillation (MJO) is the most energetic large-scale intraseasonal atmospheric disturbance (Madden & Julian 1971 & 1972). It originates in tropical Africa and travels eastward through Indian and Pacific basins as 1st and 2nd zonal wavenumbers of zonal wind, precipitation and convection (Wheeler and Kiladis 1999). Eventually these disturbances die out over the Atlantic Ocean and the African Continent. These disturbances are largely confined in the tropical belt."

Comment 7: 43-45 Not really a “discovery”! The barotropic response to MJO-like wind stress anomalies has been discussed, for example, by Ponte and Gutzler (1992, JGR), indicating the expected large-scale nature of the adjustment.

Response 7: We thank the Reviewer for bringing this to our notice. We have now cited Ponte and Gutzler (1991, JGR; we are not aware of any Ponte and Gutzler 1992 JGR paper) which showed the existence of barotropic waves in the tropical Pacific driven by MJO-like wind stress. We inserted the following reference to their findings to remove ambiguity (line 40-44):

"Past studies on the impact of MJO on the global ocean barotropic variability have been rare. One noticeable exception is the pioneering work by Ponte and Gutzler (1991), who concluded the possibility of a response of an idealized ocean basin (mimicking the tropical Pacific) to an idealized MJO-like atmospheric forcing".

However, it may be noted that our approach and conclusions are very different from theirs. Indeed, their work was based on a highly idealized framework (both in terms of ocean modeling and of its atmospheric forcing). Their results were not backed by any observations and their oceanic response was, as a matter of fact, one order of magnitude weaker than observed. In contrast, we considered a fully realistic modeling framework, namely a global tri-dimensional stratified ocean model with realistic geometry including the Indo-Pacific connection and the Southern Ocean, forced by realistic atmospheric fluxes, duly validated against the available observations. This allowed us to identify the prominent location of the MJO-associated wind stress that drives barotropic processes in the Indian and Pacific Ocean. We recall that, as shown by Rohith et al., (2019), the ocean stratification is indeed instrumental in the generation of the barotropic response of the Indian Ocean to MJO. Moreover, as explained in the present manuscript, both the Indo-Pacific connection and the Southern Ocean are essential ingredients of the quasi-global barotropic response to MJO we reveal. This justifies the use of the word "discovery" in our manuscript. This does not seem to us an over-statement. Still, for clarity, we reformulated our statement as follows (line 50-52):

"This challenges the earlier understanding of a response only via slow-propagating baroclinic waves thereby adjusting the density field of the basins in ~2-3 months (Zhang 2005 & Oliver and Thomson 2010)".

Reference :

1. **Ponte, R. M. & Gutzler, D. S. The Madden-Julian oscillation and the angular momentum balance in a barotropic ocean model. J. Geophys. Res. Ocean. 96, 835–842 (1991).**
2. **Rohith, B. *et al.* Basin-wide sea level coherency in the tropical Indian Ocean driven by Madden–Julian Oscillation. *Nat. Commun.* 10, 1257 (2019).**

Comment 8: 48 “near-global rotation of the ocean”? Not clear what is meant, please rephrase.

Response 8: Since the see-saw encompasses the Indian and Pacific basins which altogether constitute about 70% of the area of the global ocean, we had thought it fit to mention it as

near-global. In retrospect, we understand that it is confusing and have therefore made appropriate changes in the revised manuscript (line 54-56). The modified sentence is as below:

"The associated global-scale angular momentum, arising both from a large-scale circulation in the ocean and a global-scale mass redistribution, is expected to leave its signature in the Earth's rotation (Ponte et al., 1998 ; Gross 2003 & Gross et al,, 2004)."

Comment 9: 51-62 This discussion, set in most general terms, does not do justice to what we know about ocean angular momentum and its role in driving polar motion since the earlier work of Ponte et al. (1998, Nature) and the subsequent literature. At the end of this text, the authors seem to imply that their work adds "evidence of climate modes excitation of polar motion through ocean", which takes the MJO, in my view inappropriately, to be a "climate mode". (see also comments about line 26)

Response 9: We do agree that the earlier works highlighted the role of oceans in modulating polar motions across multiple time scales. There have also been hypotheses that proposed how oceans can modulate polar motion changes. However, the exact oceanic mechanism that drives mass and motion changes and thereby intraseasonal polar motion changes has not yet been demonstrated to the best of our knowledge. We understand that the formulation of sentences in the previous manuscript has given rise to some confusion. We have therefore reformulated the paragraph in the revised manuscript (lines 53-72). We also have included relevant citations to the previous works by Ponte et al., 1998, Natsula and Ponte 1999, Zhou et al., 2005 in line 69. We have also discontinued the use of MJO as climate mode in the revised manuscript.

Reference:

1. **Ponte, R. M., Stammer, D. & Marshall, J. Oceanic signals in observed motions of the Earth's pole of rotation. Nature (1998).**
2. **Nastula, J. & Ponte, R. M. Further evidence for oceanic excitation of polar motion. Geophys. J. Int. 139, 123–130 (1999).**
3. **Zhou, Y. H., Chen, J. L., Liao, X. H. & Wilson, C. R. Oceanic excitations on polar motion: a cross comparison among models. *Geophys. J. Int.* 162, 390–398 (2005).**

Comment 10: fig 1 Text should clarify if surface freshwater fluxes are included when estimating volume fluxes. This would be the case if the model uses a real freshwater flux boundary condition. Also perhaps you should refer in the caption to the basin boundaries in fig 2d, which are used to define the volume fluxs in fig 1a, b.

Response 10: Freshwater fluxes like rainfall and snow (monthly climatology) are included while estimating volume fluxes. Because of the use of the Z* formulation (Adcroft, A. and J.-M. Campin, 2004) in the simulations, the freshwater fluxes are considered as supplementary mass in the ocean and thus modifying the volume directly. We have added more details regarding the freshwater fluxes and volume conservation in the revised supplementary(lines 33-37). We have also defined the basin boundaries in the caption of Fig 2d in the revised manuscript.

Reference:

1. **Adcroft, A. & Campin, J. M. Rescaled height coordinates for accurate representation of free-surface flows in ocean circulation models. *Ocean Model, 7 (3-4), 269–284.* (2004).**

Comment 11: 81-82 The winter of 2013 does not seem to show particularly strong MJO activity judging from fig 1c? In this regard, it might help to draw dashed lines encompassing the phases associated with Maritime Continent activity in fig 1c, which are noted in the caption.

Response 11: We agree that the MJO indices do not reflect a strong MJO in 2012-13 compared to 2011-12. However, we assessed the spatial average of curl of the wind stress normalized by ocean depth (viz. the source term of barotropic forcing) over the forcing region (Maritime Continent), and it is comparable during both the years (see Fig R3 below). We have also drawn dashed lines encompassing the phases associated with Maritime Continent activity in Fig 1c in the revised manuscript.

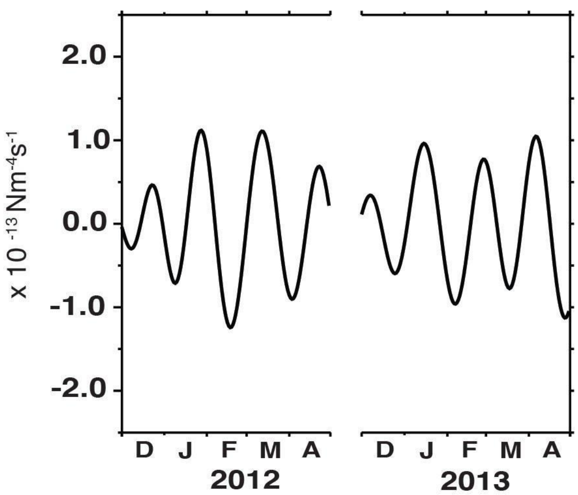

Fig R3 | Plot of time evolution of the source term of barotropic variability ($f\left(\nabla\times\frac{\tau}{H}\right)$) averaged over the Maritime continent (90˚E:140˚E, 32˚S:2˚N) during each December-April of 2009-2019 at intraseasonal timescales. Here, f is coriolis frequency, $\tau$is wind stress and H is ocean depth.

Comment 12: 87-89 Sentence is unreadable, preferable to have two separate sentences

Response 12: We have now broken the above sentence into three sentences to improve the readability of the revised manuscript (line 98-103). We have also copied below these sentences for easy reference for the Reviewers.

" During a strong positive See-saw Index peak, ~1.5 Sv [1 Sv = 10^6 m^3s^{-1}] of water is gained by the Indian Ocean whereas ~2.6 Sv of water is lost by the Pacific Ocean. This is equivalent to a spatially uniform basin rise of ~1.0 cm in the Indian Ocean and a concurrent fall of ~0.8 cm of equivalent water depth in the Pacific Ocean. This dynamics reverses during the negative peak of the See-saw Index (Supplementary Fig.S3)."

Comment 13: 97-98 This is nothing really new. It is well known that the ocean is capable of showing large scale bottom pressure signals at a variety of time scales, from tidal to much longer periods (e.g., Stepanov and Hughes, 2006, JGR).

Response 13: We agree that large scale bottom pressure signals were reported earlier in various research articles. However, we wanted to stress on the fact that an Indo-Pacific see-saw of the scale reported here, from both observational and numerical studies, has never been reported earlier. Stepanov and Hughes (2006) in particular concluded the secondary role of the Indian Ocean in inter-basins barotropic mass exchanges, at intraseasonal timescales. To make it clearer to the reader, we modified our statement as follows (line 111-113):

"It is striking to discover that a large-scale see-saw of oceanic mass encompasses the Indo-Pacific basin and extends further over the vast majority of the world ocean."

Reference:

1. **Stepanov, V. N. & Hughes, C. W. Propagation of signals in basin-scale ocean bottom pressure from a barotropic model. *J. Geophys. Res.* 111, C12002 (2006).**

Comment 14: fig 1+2 It would be useful for the reader to have more explanation about why some particular choices are made and whether results are sensitive to these choices. Why define the seesaw in terms of small boxes in the middle of the basins, instead of larger basin-scale averages? Why not use simpler southern boundaries in fig 2d (e.g., straight lines along 30S)? Why pick the yellow star site for the correlations?

Response 14: The location of the smaller boxes, that defined the See-saw Index, was chosen in the area that displayed the largest correlation (Fig. 2a). However, even if the entire two basins - Indian and Pacific - are chosen to define the See-saw Index, the qualitative feature of the time-series of See-saw Index (Fig 1a) remains unchanged (Please see the figure below; Fig R4). We have therefore used the definition of See-saw Index that involves the entire basin in the revised manuscript. The new definition in the revised manuscript (line 88-89) is

"We define a See-saw Index from the control run as the normalized difference of mean equivalent water depth anomaly between the Indian and Pacific basins."

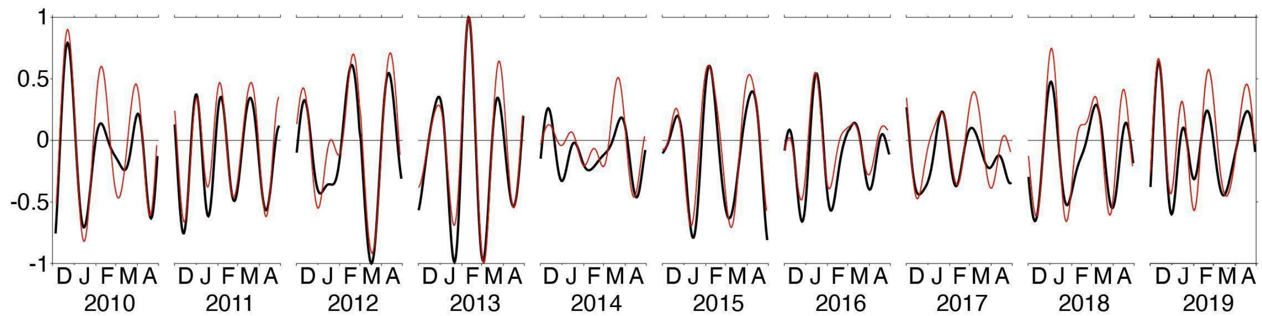

Fig R4 | See-saw Index comparison : Black line represents the See-saw Index estimated from the entire Indian and Pacific basin. Red line represents the See-saw Index estimated from the small boxes in Indian (65°E:75°E,5°S:5°N) and Pacific (155°W:165°W, 5°S:5°N) basins .

The spatial correlation pattern (fig. 2a & b) suggests that the oceanic region beyond 30°S in the Indian Ocean is affected by the barotropic processes mentioned in our manuscript. Our idea is thus to assess the volume of water across the southern edge of this high-correlation area, and to investigate how this flux is carried along to sustain the see-saw. We however do agree that taking a simpler southern boundary would produce the same inferences.

In order to draw a meaningful parallel with Fig 3 in revised manuscript, we have chosen the yellow star which represents the location where a long, reasonably uninterrupted time-series of observed bottom pressure data exists. Also, it lies within the boundary of the source region of the purported seesaw. Nevertheless, the conclusions of our work wouldn't change had we taken any other location within the region exhibiting seesaw.

Comment 15: fig 2+3 The text should be explicit about how statistical significance is calculated for the correlations shown in these figures, particularly how the degrees of freedom are estimated. Also I am assuming that only significant correlation values are plotted (in color), but the caption should be more explicit.

Response 15: **We have used the following formula for estimating the degree of freedom (DOF) for a band-passed time series and subsequent significance (Bendat and Piersol 2010):**

$$DOF = 2N\left(\frac{\Delta T}{T_{c1}} - \frac{\Delta T}{T_{c2}}\right) - 2,$$

Where, $\Delta T$is the sample interval, T_{c1} and T_{c2} are the cutoff periods in the bandpass filtering ($T_{c1} < T_{c2}$)and N is the sample size. In this study for intraseasonal (30-80 days) band-pass filter, ΔT is taken as 1 day, T_{c1} and T_{c2} are 30 and 80 days respectively, and N is 1501 (10 years of daily data during December-April). Based on this equation, we estimated that the DOF for the winter months (December-April) of 2009-2019 is 60. Corresponding to this DOF, the correlation values greater than 0.21 and less than -0.21 are 90% significant in accordance to the Pearson correlation table.

We have included a section in the modified Supplementary (line 92-102) to highlight that we have indeed accounted for the smoothing while estimating the degree of freedom. We have also now referred to it in the caption of Fig 2 and Fig 3 in the revised manuscript.

References :

1. **Bendat, J. S. & Piersol, A. G. *Random Data. Random Data: Analysis and Measurement Procedures: Fourth Edition* (John Wiley & Sons, Inc., 2010).**

Comment 16: 121-126 I don't see 30% values over large Pacific regions in fig 2c, probably only along the equator, where it is expected because the winds in the Maritime Continent box are capable of exciting the equatorial Pacific wave guide. Thus the results do not seem surprising.

Response 16: **We do agree that 30% values are mostly seen over the Equatorial Pacific. However, what we believe is surprising is that it is facilitated by a wind-forced barotropic wave that owes its origin to a small region in the Eastern Indian Ocean mostly. We have modified the percentage values from 15-30% to 15-20% (line 141, 147, 222).**

Comment 17: 143-144 How is the "4-6 cm peak-to-peak" change in the Indian Ocean consistent with the value of 2 cm that we infer from what is stated in line 89 for strong seesaw events and also from fig S3?

Response 17: The 2 cm (or Fig S3) refers to the model basin-averaged intraseasonal equivalent water depth anomaly variability in the Indian Ocean. In contrast, 4-6 cm peak to peak refers to the observed variability in intraseasonal equivalent water depth anomaly at a specific bottom pressure recorder location in the Eastern Indian Ocean - that too within the source region where the variability is expected to be more pronounced than elsewhere.

Comment 18:146-147 All BPRs shown in fig 3 in the Pacific seem to show anticorrelation, so I don't understand where the 42% number comes from. Are you only showing in fig 3 the anticorrelated BPRs?

Response 18: We are showing only those bottom pressure recorders in Fig 3 whose correlations are larger than 90% significance. All the bottom pressure recorders that pass this 90% significance test are anticorrelated in the Pacific. The rest of the BPRs do not pass the 90% significance test. All the bottom pressure recorders that are significantly anticorrelated constitute 42% of the total bottom pressure recorders installed in the Pacific basin. We have deleted the sentence that mentioned 42% because we think that it is not conveying any significant information and it may be confusing to the Readers. We have now revised the sentence to bring more clarity (line 167-173). The revised sentence is :

"Anomaly in equivalent water depth at BPR-MC was correlated with anomaly in equivalent water depth from all available bottom pressure recorders globally and all bottom pressure recorders whose significance (Methods) exceeds 90% are plotted in Fig.3b. 19 out of 45 (~42%) bottom pressure recorders in the Pacific Ocean exhibit significant correlation. All the Indian Ocean bottom pressure recorders synchronously oscillate, whereas all the bottom pressure recorders in the Pacific and in the Arctic Ocean are anti-correlated with the bottom pressure recorders in the Indian Ocean."

Comment 19:161-163 These two sentences seem to convey the same idea? Please remove the redundancy or clarify their different meanings.

Response 19: We have removed the sentence "*In addition, the North-South asymmetry of the global-scale mass distribution anomaly also impacts the polar motion*" in the revised manuscript.

Comment 20:166 Awkward and unclear sentence, please rewrite.

Response 20: We have rewritten the sentence in the revised manuscript (line 188-190). The modified sentence is :

"The excitation of the polar motion is classically estimated using excitation functions – χ_1 for rotation around an axis at the Greenwich meridian (x-axis) and χ_2 for rotation around an axis that passes through the Indian Ocean at 90º E (y-axis) (Methods)."

Comment 21: fig 4b I don't find this schematic illuminating. It does not really add anything to the story and it would be best deleted. More interesting and useful would be to have effects of ocean bottom pressure and currents on chi_2 separated and discussed, so that the reader gets to know what is their relative contributions to the oceanic excitation.

Response 21: We would like to retain the erstwhile Fig 4 (it is Fig 5 in the revised manuscript). We believe that it may help as a mnemonic to the readers, in particular the ones more familiar with oceanography than with geodesy. We would like to keep it to assist the readers (Fig 5 of the revised manuscript). We have included a new figure in the revised manuscript (Fig 4b) to show the mass and motion contributions to the polar motion from MC-EXP and control run during 2012-13 and have added additional statements (line 216-234) to describe their role and relative contributions to the polar motion. The relative contribution of mass and motion terms from MC-EXP for the entire duration of our study shows that these terms are of the same order of magnitude not only during 2012-13 but also during some other boreal winters. This figure (Fig R5) is reproduced below for easy reference for the Reviewer.

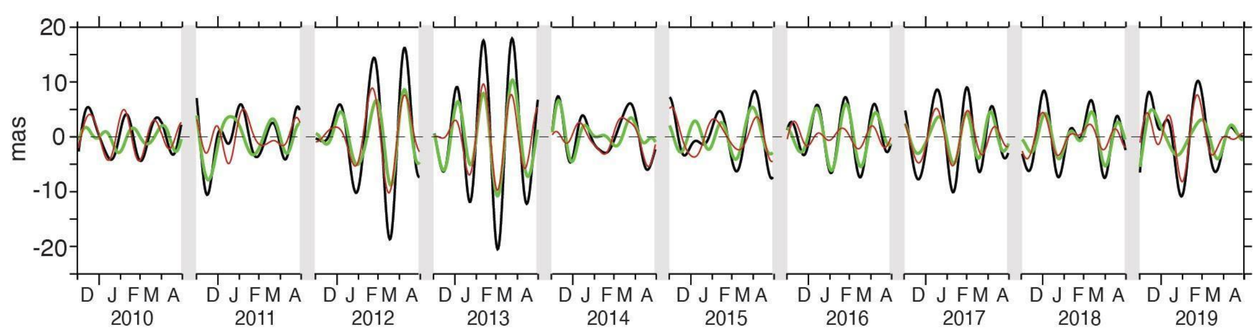

Fig R5 | Plot of the time evolution of intraseasonal χ_2 estimated from MC-EXP (black), ocean current (red) and ocean mass (green) contribution during each December-April of 2009-2019.

Comment 22: 198-209 This text needs to be rewritten to address some of the issues raised above, including comments on lines 26, 43-45, 51-62, 97-98.

Response 22: We have rewritten the first paragraph of the summary of the revised manuscript (line 244-258). It is as follows:

"The MJO winds, acting over ~4% of the Earth's surface, induce a global-scale ocean mass redistribution, which in turn significantly influences the Earth rotation. This entire phenomenon is schematically illustrated in Fig.5. The strong boreal winter MJO winds over the Maritime Continent elicit an intraseasonal large-scale barotropic response from the Indian and the Pacific Ocean whose extent, unlike previously known oceanic manifestations of the MJO, is not only limited to the tropics but also reaches the extratropics within a span of days compared to earlier estimates of months deduced from slow-moving baroclinic excitations. The winds induce a barotropic circulation around the Australian continent and its periodic reversal at intraseasonal timescales is manifested as a see-saw in the oceanic mass within the Indo-Pacific basin. The large-scale oceanic mass redistribution in the Indo-Pacific basin, accompanied by large-scale to-and-fro transports in the two basins associated with this see-saw, benefits from a favourable geometry to excite polar motions, allowing the strong 2013 MJO event to be the first intraseasonal disturbance detected in the polar motion signal through the ocean. The large oceanic excitation is comparable in magnitude, but out of phase, with the atmospheric excitation and stabilizes the large polar motion changes induced by the atmosphere to the solid Earth. "

6th May 21

Dear Dr Paul,

Your revised manuscript titled "An Indo-Pacific see-saw wobbles the Earth at intraseasonal timescales" has now been seen by our original reviewers, whose comments appear below. In light of their advice I am delighted to say that we are happy, in principle, to publish a suitably revised version in Communications Earth & Environment under the open access CC BY license (Creative Commons Attribution v4.0 International License).

We therefore invite you to revise your paper one last time to address the remaining concerns of our reviewers. Specifically, please ensure that your manuscript is appropriately worded, and as clear as possible, and carefully address the comments from reviewer 2.

At the same time we ask that you edit your manuscript to comply with our format requirements and to maximise the accessibility and therefore the impact of your work.

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We hope to hear from you within two weeks; please let us know if you need more time.

Best regards,

Joy Merwin Monteiro, PhD
Editorial Board, Communications Earth & Environment
orcid.org/0000-0002-3932-3603

Heike Langenberg, PhD
Chief Editor
Communications Earth and Environment

On Twitter: @CommsEarth

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I thank the authors for answering my major concerns in their revised manuscript. I now find it much easier to read and follow. I can now recommend it be published.

Reviewer #2 (Remarks to the Author):

The authors have addressed my concerns for the most part satisfactorily but a few issues remain with the text. These are listed below and should be dealt with before acceptance. Other minor editorial suggestions are also included.

1 The original manuscript was written under the “headline” that the ocean is wobbling the Earth. The authors agree that the results in Fig 3 had to be interpreted differently and have changed the

story somewhat, but the title still conveys the same original misleading message.

38 Clarify what “basin-scale signal of 4-6 cm, in the entire tropical Indian Ocean ” refers to. This amplitude seems to be only attained in some regions (i.e., at one of the BPR locations in fig. 3), with the basin-scale amplitudes being somewhat lower?

41-44 The description of ref. 5 is non-informative. It is preferable to just add ref. 5 at the end of the previous sentence, after “rare”.

53-67 The original submission was done mostly ignoring previous literature on ocean angular momentum and polar motion. The revised text provides more citations but is still poorly written. The authors refer to “rotation” and cite one paper on polar motion (line 56). But then things get very confused. In lines 59-62, the authors seem to be talking about just the rotation component about main axis (length-of-day)? Otherwise they should not claim that “signature from the ocean is relatively elusive” (line 59). That should be clarified in the text, but the mixing of length-of-day and polar motion is a confusing digression. The text should focus on polar motion, which is the subject of the paper. They do go on to polar motion and say that “polar motion… has even less favorable geometry” (lines 62-63), which is simply incorrect, apart from being badly phrased.

67-72 This text just continues to downplay past evidence of oceanic excitation of polar motion, “two or three times weaker than the atmospheric counterpart”, and claims that this paper has found a different result. That is not supported by the evidence in fig. 4: even for the 2012-2013 event, picked because it is the most favorable event for the “see-saw” story, the ocean excitation is still about half of the atmospheric counterpart.

58-59 “…core and its fluid envelope (atmosphere…”

69 Need to also cite ref. 8 here.

165-166 To be clear this should state “The variability in BPR-MC amounts to 4-6 cm peak-to-peak, that of the BPR-PAC is ~2-3 cm…”. This continues to be a source of confusion in the current text, as it was in the original.
169 “Nineteen out of…”

216 “…excitation of polar motion…”

218-223 On the issue of how important the “see-saw” is for polar motion excitation, the authors should state explicitly what the 70% and 50% values, given in lines 219 and 221, represent. Similarly for numbers given in lines 222 and 223. The latter numbers seem to be “variance explained”, as described in fig. 2c, but actually that quantity also does not seem to be explicitly formulated in the paper. The text in the paper needs to be clear on how these numbers are calculated.

222 More precisely “…captures only ~ 15-20% of equivalent water depth variability in some regions of the Pacific Ocean…”

228-231 Statement seems to imply that Fig. S5 shows information on matter and motion terms, but that is not the case.

232 “…demonstrates that the wind stress over the small region…”

248 Delete “unlike previously known oceanic manifestations of the MJO“. It is not an accurate statement.

255-256 As phrased, this is another inaccurate statement. The role of the ocean on intraseasonal excitation has been known for a while.

264 fig 5 If the authors insist in having this schematic, then there are at least two main issues. The winds are depicted as “cyclonic” based on what? I am not an expert on the MJO but I always thought the main surface signature in the tropics is in the zonal winds. The other issue is the depiction of equivalent water depth, which shows much lower anomalies all around the coasts, without a clear basis for it?

288 This reference needs to be corrected to include all authors.

REVIEWERS' COMMENTS:

Reviewer #1 (Remarks to the Author):

I thank the authors for answering my major concerns in their revised manuscript. I now find it much easier to read and follow. I can now recommend it be published.

Response : We thank the Reviewer for giving us very useful suggestions to improve the paper and for accepting the modifications in the revised version of the article.

Reviewer #2 (Remarks to the Author):

The authors have addressed my concerns for the most part satisfactorily but a few issues remain with the text. These are listed below and should be dealt with before acceptance. Other minor editorial suggestions are also included.

Comment 1: 1 The original manuscript was written under the "headline" that the ocean is wobbling the Earth. The authors agree that the results in Fig 3 had to be interpreted differently and have changed the story somewhat, but the title still conveys the same original misleading message.

Response 1: We thank the Reviewer for raising this concern. We agree with the Reviewer. As our earlier title suggests, the ocean does contribute to the wobbles of the earth, although it does so by counteracting the atmospheric component. Based on the new interpretation of the results, we have now changed the title of the paper as "Madden-Julian Oscillation winds excite an intraseasonal see-saw of ocean mass that affects Earth's polar motion."

Comment 2: 38 Clarify what "basin-scale signal of 4-6 cm, in the entire tropical Indian Ocean " refers to. This amplitude seems to be only attained in some regions (i.e., at one of the BPR locations in fig. 3), with the basin-scale amplitudes being somewhat lower?

Response 2: We thank the Reviewer for pointing this out. We have modified the statement in the revised manuscript (line 37-39). The modified statement reads as:

"It amounts to a basin-scale signal that reaches up to 4-6 cm in the tropical Indian Ocean, with the mass redistribution achieved through fast-propagating barotropic waves adjusting the tropical Indian Ocean in ~2-3 days[4]".

Comment 3: 41-44 The description of ref. 5 is non-informative. It is preferable to just add ref. 5 at the end of the previous sentence, after "rare".

Response 3: We have incorporated the suggestion made by the Reviewer in the revised manuscript. We have removed the sentence " One noticeable exception is the pioneering work by ref.5, who concluded the possibility of a response of an idealized ocean basin (mimicking the tropical Pacific) to an idealized MJO-like atmospheric forcing " in the revised manuscript.

Comment 4: 53-67 The original submission was done mostly ignoring previous literature on ocean angular momentum and polar motion. The revised text provides more citations but is still poorly written. The authors refer to "rotation" and cite one paper on polar motion (line 56). But then things get very confused. In lines 59-62, the authors seem to be talking about just the rotation component about main axis (length-of-day)? Otherwise they should not claim that "signature from the ocean is relatively elusive" (line 59). That should be clarified in the text, but the mixing of length-of-day and polar motion is a confusing digression. The text should focus on polar motion, which is the subject of the paper. They do go on to polar motion and say that "polar motion… has even less favorable geometry" (lines 62-63), which is simply incorrect, apart from being badly phrased.

Response 4: We thank the Reviewer for pointing this out. In this paragraph, we have emphasized that the rotation of the Earth about its three axes responds to the changes in the components of the ocean angular momentum. We have also stressed on the fact that the presence of continental landmasses, which hinder the flow of ocean currents, presents a natural obstruction to the ocean flow from realising large scale dynamics unlike the atmosphere. A large-scale ocean dynamics, strong enough to cast a signature on the polar motion, can only be realized under certain specific circumstances. We have rephrased parts of the paragraph and have made it more transparent in the revised manuscript (line 50-66). Excerpts from the revised text reads as:

"The Earth's rotation about its three axes is not constant, and presents fluctuations over a broad range of frequencies[8-13]. The rotation changes are classically separated into two parts: the changes in the angular velocity are described in terms of changes in the length-of-day, whereas the rotation of the solid Earth around its rotation axis corresponds to polar motion[11-13]. Most of those signals come from the exchange of angular momentum between the solid Earth and the fluid parts, namely the Earth's core and its fluid envelope (atmosphere and ocean)[11,13,14]. For geometry reasons, the atmosphere impacts dominate that from the ocean for the length-of-day, except at tidal frequencies[14]. The picture is more complex for polar motion, for which the relative domination of the ocean or atmosphere depends on the frequency band. In the intra-seasonal band of interest for our study, the polar motion is mostly forced by the atmosphere[8]. However, the contribution from the ocean is also significant[8,15,16] . The impact from the ocean on polar motion mostly comes from the spatial distribution of the oceanic mass, though, for some particular phenomena, the mass transport gives rise to an observable change[8,15,16]. The see-saw generation involves large-scale mass redistribution and currents across and within the Pacific and Indian basins. The associated

global-scale angular momentum, arising both from a large-scale circulation in the ocean and a global-scale mass redistribution, is expected to leave its signature in the Earth's rotation about its polar axis and the polar motion[8-10,13]"

Comment 5: 67-72 This text just continues to downplay past evidence of oceanic excitation of polar motion, "two or three times weaker than the atmospheric counterpart", and claims that this paper has found a different result. That is not supported by the evidence in fig. 4: even for the 2012-2013 event, picked because it is the most favorable event for the "see-saw" story, the ocean excitation is still about half of the atmospheric counterpart.

Response 5: We agree with the Reviewer, that on an average scenario ocean explains about half of the atmospheric counterpart. But, during late December to early February, the oceanic component explains more than half of the atmospheric counterpart (Figure 4a). Therefore, the 2012-13 event indicates that during a strong MJO event the ocean can contribute more than 50% of the atmospheric contribution. Also, we have not claimed anywhere in the manuscript that the oceanic contribution exceeds half of the atmospheric contribution. Nevertheless, we have revised the statement (line 68-69). The revised statement reads as:

"The excitations induced by the oceans, though generally minor, are at times of the same order of magnitude as those induced by the atmosphere."

Comment 6: 58-59 "…core and its fluid envelope (atmosphere…"

Response 6: We have modified the sentence in the revised manuscript (line 55) as suggested.

Comment 7: 69 Need to also cite ref. 8 here.

Response 7: Based on comment 5, we have removed line 69 of the previous manuscript.

Comment 8: 165-166 To be clear this should state "The variability in BPR-MC amounts to 4-6 cm peak-to-peak, that of the BPR-PAC is ~2-3 cm…". This continues to be a source of confusion in the current text, as it was in the original.

Response 8: We have modified the sentence in the revised manuscript as suggested (line 145-147).

Comment 9: 169 "Nineteen out of…"

Response 9: We have modified the sentence in the revised manuscript as suggested (line 149-150).

Comment 10: 216 "…excitation of polar motion…"

Response 10: We have modified the sentence in the revised manuscript as suggested (line 186).

Comment 11: 218-223 On the issue of how important the "see-saw" is for polar motion excitation, the authors should state explicitly what the 70% and 50% values, given in lines 219 and 221, represent. Similarly for numbers given in lines 222 and 223. The latter numbers seem to be "variance explained", as described in fig. 2c, but actually that quantity also does not seem to be explicitly formulated in the paper. The text in the paper needs to be clear on how these numbers are calculated.

Response 11: We have rewritten the sentences in the revised manuscript (line 188-194) for more clarity. The revised sentences are

"In contrast, during the 2012-2013 strong MJO event, the ocean angular momentum from the MC-EXP (Fig 4a; cyan curve) captures ~70% of the variance of the oceanic signal from the control run. In addition, MC-EXP is in phase with the residual geodetic excitation function (Fig 4a; black curve) and the oceanic excitation computed from MC-EXP captures ~50% of the variance of the residual geodetic excitation. This is surprising because MC-EXP captures ~15-20% of the variance of equivalent water depth in some regions of the Pacific Ocean, and 70% in the Indian Ocean from control run. "

Based on the Reviewer comment, we have incorporated the expression used for the calculation of "variance explained" in the caption of Fig 2 (line 524-526). The statement is as follows:

"The variance captured is calculated using the equation, $1-\left(\frac{\mathrm{variance}(A-B)}{\mathrm{variance}(\ A)}\right)$, where A represents the control run and B denotes MC-EXP."

Comment 12: 222 More precisely "…captures only ~ 15-20% of equivalent water depth variability in some regions of the Pacific Ocean…"

Response 12: We have modified the sentence in the revised manuscript (line 193-194) as suggested.

Comment 13: 228-231 Statement seems to imply that Fig. S5 shows information on matter and motion terms, but that is not the case.

Response 13: **We apologize for this inadvertent mistake. The relevant figure was included in the Response to Reviewer letter. However, we missed including it in the Supplementary. We have now included the relevant figure in the revised Supplementary (Supplementary Figure 6).**

Comment 14: 232 "…demonstrates that the wind stress over the small region…"

Response 14: We have modified the sentence in the revised manuscript (line 204) as suggested.

Comment 15: 248 Delete "unlike previously known oceanic manifestations of the MJO". It is not an accurate statement.

Response 15: We have removed the sentence in the revised manuscript as suggested.

Comment 16: 255-256 As phrased, this is another inaccurate statement. The role of the ocean on intraseasonal excitation has been known for a while.

Response 16: We understand that there are studies that have pointed out the role of the ocean in exciting polar motion. However, to the best of our knowledge, no studies exist that conclusively link the variability of polar motion to a mode of variability. We have therefore rephrased the sentence to bring forth more clarity (line 215-219). The modified sentence in the revised manuscript reads as:

"The large-scale oceanic mass redistribution in the Indo-Pacific basin, accompanied by large-scale to-and-fro transports in the two basins associated with this see-saw, benefits from a favourable geometry to excite polar motions. The strong 2013 MJO allowed us to detect for the first time the signature of a mode of variability on the polar motion through the ocean."

Comment 17: 264 fig 5 If the authors insist in having this schematic, then there are at least two main issues. The winds are depicted as "cyclonic" based on what? I am not an expert on the MJO but I always thought the main surface signature in the tropics is in the zonal winds.
The other issue is the depiction of equivalent water depth, which shows much lower anomalies all around the coasts, without a clear basis for it?

Response 17: We thank the Reviewer for pointing this out and agree that zonal wind is one of the prominent surface signatures of MJO. Rohith et al, 2019 demonstrated that the basin wide intraseasonal barotropic sea level rise (fall) in the Indian ocean is associated with positive (negative) wind stress curl over the maritime continent and vice versa. We understand that the depiction of winds in the schematic diagram may be confusing. We have therefore revised the schematic diagram.
There is no basis to depict lower equivalent water depth anomalies other than to improve the visual experience of the reader. However, we realise that it may convey a wrong message and have therefore modified the schematic diagram (Fig 5 in the revised version).

Comment 18: 288 This reference needs to be corrected to include all authors.

Response 18: We have corrected the reference in the revised manuscript as (line 373-374):

"Gross, R. S., Fukumori, I., Menemenlis, D Atmospheric and oceanic excitation of the Earth's wobbles during 1980–2000. J. Geophys. Res. 108, 2370 (2003)."