

Interactive comment on “Imprint of chaotic ocean variability on transports in the Southwest Pacific at interannual timescales” by Sophie Cravatte et al.

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General comments

This study investigates the transport variability in the Southwest Pacific on interannual time scales with the help of the $1/4^\circ$, 50-member ensemble simulation OCCIPUT. The focus is put on the influence of intrinsic (chaotic) oceanic variability on the observed transports in contrast to the deterministic variability forced by the atmosphere. The study also points out a general need for ensemble simulations to better quantify oceanic variability on interannual time scales, and includes a valuable discussion of the study's implications for the interpretation of e.g. observations.

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One important scientific value of this study is the validation or falsification of some results that have already been published previously, but were based on single-member simulations or point-observations. If these results can (cannot) be reproduced with the OCCIPUT ensemble, they are much more (less) significant, due to the smaller errors using an ensemble and the advantage of estimating the chaotic variability that emerges internally in the ocean.

The study is well-structured and comprehensive, and mostly needs some clarifications and minor technical revisions as listed below in the “Specific comments” and “Technical Comments”. However, there are three issues, that require some attention:

1. In the introduction, the latitude band 15-30°S is identified as a region where many processes are not well understood and the OCCIPUT ensemble can help clarify. However, the current systems outside this latitude band (where previous studies reached more consistent conclusions) also receive substantial attention in this manuscript. I therefore recommend, to more clearly point out that the validation of previous results with this novel modeling approach is an important aspect of this study, and not only the investigation of processes that could not be explained satisfactorily so far.

2. Another issue that needs some more work is the discussion about the results of this study in contrast to Travis and Qiu (2017) and Rieck et al. (2018). While I have no doubt that the general conclusion you draw is valid and the interannual variability in the STCC region is mostly intrinsic, there are two points that should be clarified:

- The different regions used in this study compared to Travis and Qiu (2017) and Rieck et al. (2018). Given that the simulations are global, you could easily extend the region you use to define the STCC and thus make it better comparable to the regions used in the other two studies. (Also see specific comment II. 463-465 below)

- While Travis and Qiu (2017) and Rieck et al. (2018) investigate decadal variability, this study focusses on interannual variability. This, and the implications, should be made more clear.

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3. Additionally, the manuscript would benefit from an overview map. I suggest adding a new, large Fig. 1 to the manuscript, showing the bathymetry of the Southwest Pacific and the locations of the different seas (e.g. Coral Sea, Solomon Sea, etc ...), currents, and islands.

Specific comments

1. Introduction

I. 30 “equatorial band”: Do you mean the current bands?

I. 38 “These currents”: Please specify which currents are referred to here; is the statement valid for all currents mentioned in the paragraph above?

I. 48 “wind anomalies in the southern hemisphere”: Are these wind anomalies related to the SAM as well?

I. 57 “[...] south of 20°S [...]”: In the paragraph above, ENSO is said to affect wind stress curl to 30°S. Here however, ENSO’s influence is said to be restricted to North of 20°S. This is inconsistent.

I. 64 “Between these two latitudes [...]”: Does this refer to 15-30°S? If yes, there is no need for this first part of the sentence.

2. Data, model description, methods

I. 130: I suggest to include the discussion about the impact of using a coupled system here (ll. 521-527). It is an important discussion but it does not fit at the end of the manuscript in my opinion.

I. 131: Is there any specific reason to restrict the analyses to the period 1980-2015? Does the ensemble need the 20 years for the solutions to sufficiently diverge?

I. 131: How is the PDO index defined?

I. 144-145: Do you use the 180-day low-pass filtered velocities as the mean in the EKE

calculations? Or as the deviations from that mean? This is not clear.

3. Deterministic versus chaotic oceanic transport variability

I. 219: I suggest to better specify what “realistically” means in this case. Are the simulated current strengths within a certain range of the observed ones?

II. 235-237: It is not clear to me how the different percentages relate. 15% on line 235, 10-20% on line 236 and 20% on line 237.

I. 241: As in line 219 (and following), I suggest to add a bit more information on what “reasonably similar” means. Some numbers would be beneficial to allow reproducibility and comparison with other studies.

I. 265: “south of 20°S” is rather unspecific. The EAC and EAUC systems and the STCC are also south of 20°S. Additionally, there are also regions south of 20°S, where the intrinsic interannual variability is lower.

II. 295-297: There might be some answers (or hints) to these questions in Oliver and Holbrook (2014) and Bull et al. (2017). I agree though, that a thorough testing of this hypothesis should not be undertaken in this study.

I. 319: How is “veering eastward” defined?.

6. Discussion and conclusion

II. 463-465: You should note that the two studies (Travis and Qiu, 2017 and Rieck et al., 2018) investigated different regions. Travis and Qiu (2017) investigated a region from 165°E - 130°W which is much larger than the region used to investigate the STCC in this study. Averaging over such a large region should automatically lead to a smaller impact of intrinsic variability, as noted on lines 482-483. Rieck et al. (2018) investigated a region from 175°W - 153°W, which is almost entirely outside the region investigated here. You should better justify, why a comparison of this study with Travis

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and Qiu (2017) and Rieck et al. (2018) is nonetheless valid. Penduff et al. (2011), Sérazin et al. (2015) and Rieck et al. (2018) all show that the ratio of intrinsic to total variability is not zonally uniform.

II. 472-473: Given your filtering strategy to confine the analyzed variability to interannual time scales of 1 - 9 years (II.142-143), it is surprising that you state to have found a link on decadal time scales.

Technical corrections

1. Introduction

I. 25: Instead of “[...], differently for different oceanic depths.” I suggest to write something like “[...] with different impacts at different oceanic depths.”

II. 29-30: It should either be “Low-Latitude Western Boundary Currents” or “LLWBC”.

I. 36: currents’

I. 37: masses’

I. 54: For better readability, I suggest to move “accordingly” to the end of the sentence.

I. 74 “imprint”: Should be either “impact” or “imprint on”.

I. 111 “hampers”: Should be “hamper”.

2. Data, model description, methods

I. 162 “low-ass”: Should be “low-pass”.

II. 186-197: The NCJ, SCJ and Tasman Front are all three said to be labelled 3 on Figure 1a. The labels mentioned here do not agree with the labels in Table 1. The label 8 on Figure 1a is not described here. Given that the discussion quite prominently features the STCC, I suggest to add a section describing the STCC here, which should also be presented in Fig. 3.

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3. Deterministic versus chaotic oceanic transport variability

- I. 232:* No comma after “(not shown)”.
- I. 235:* I do not see dots in Fig. 1. Maybe there is a problem with the figure?.
- I. 238:* “EAC’s and Tasman Front’s”.
- I. 251:* See comment to line 235.
- I. 256:* I suggest writing “of the ensemble mean 0-1000m zonal transport”.
- I. 257:* no comma after “atmospherically-forced”.
- I. 317:* I suggest using either “isoline” or “contour”, not both.
- I. 321:* “eddie’s ”.

4. Drivers of deterministic variability

- I. 373:* Tchilibou et al. (2020) is not in the references.

5. Spatio-temporal structure of the chaotic oceanic variability

- II. 400-401:* “imprint the transport” should be “impact the transports’ ”.
- I. 402:* I suggest deleting “hints of”.
- I. 407:* I suggest writing “computed first” instead of “first computed”.
- I. 407-410:* Aren’t these two sentences describing the same thing?.
- I. 423:* “consists in” should be “consists of”.
- I. 424:* I suggest writing “first two EOFs” instead of “two first EOFs”.
- I. 432:* “behaves” should be “behave”.
- I. 433:* “shows” should be “show”.

6. Discussion and conclusion

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I. 446: “than the deterministic atmospheric variability” should be at the end of the sentence.

I. 457: “varies” should be “vary”.

I. 467-468: “density anomalies remotely forced” should be “remotely forced density anomalies”.

I. 468: “EKE” should be “EKE’s”.

I. 469: “authors” should be “authors’ ”.

Author contributions: “run the experiments” should be “ran the experiments”.

Figures

Fig. 3: The figure would benefit from a title (just for the whole figure, not for each panel), so the reader can see what this figure is about at the first glance. Additionally, at least the y-axes should get a unit.

Fig. 4: Panel d) lacks units for the colorbar.

Fig. 5: Panel a) lacks a colorbar.

Fig. 11: Units are missing.

References

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Oceans, 119, 2788–2805, doi:10.1002/2013JC009591, 2014.

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