Ocean Sci. Discuss.,
https://doi.org/10.5194/os-2020-102-AC1, 2021

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

Sophie Cravatte et al.

sophie.cravatte@ird.fr

Received and published: 2 January 2021

General comments This study investigates the transport variability in the Southwest Pacific on interannual time scales with the help of the 1/4âŮę, 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. One important scientific value of this study is the validation or falsification of some results

Printer-friendly version

Discussion paper

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”.

We thank Jan Klaus Rieck for his encouragements, his very careful reading and his useful suggestions that helped to improve the manuscript. We agree with almost all his suggestions, and took into account all his comments.

However, there are three issues, that require some attention: 1. In the introduction, the latitude band 15-30âŮęS 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 recieve substantial attention in this mansucript. 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 satisfacatorily so far.

We agree with this point. Indeed, we also investigate the deterministic versus chaotic variability equatorward of 15°S, and poleward of 30°S. We added this point in the introduction: “The results obtained with this novel modeling approach will help to understand the processes governing the interannual variability in the not well documented 15°S-30°S area, and will allow revisiting previous studies conclusions on transport and EKE deterministic forcing.”

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 ll. 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. This is true, indeed. The simulation is global, but extracting all the fields for extending the analyses is far from straightforward. This, and performing again all the computations and analyses would take a significant amount of time. Instead, we acknowledge these differences in the revised manuscript, and clearly state that our results are not immediately comparable.

We reformulated the discussion as follows: "Our results on the origin of the EKE variability (dominantly intrinsic) are in contrast with the findings of Rieck et al. (2018), who concluded that the EKE decadal variability in the STCC region was, uniquely among the subtropical gyres, mostly deterministic, driven by wind-forced decadal changes. In agreement with Travis and Qiu (2017) analyses of observations, they explained these decadal EKE modulation by decadal changes in vertical shear, driven by wind stress curl changes in the South Pacific, while still recognizing that other processes such as changes in stratification or remotely forced density anomalies also contribute to modulate the EKE's low-frequency changes. It is worth mentioning that our analyses are not directly comparable: firstly, the regions considered in these two studies differ from our region of focus here, and are much larger, extending eastward to the central part of the basin. Yet, Rieck et al. (2018) found a mostly deterministic EKE variance also in the 22°S-28°S to 160°E-180° region considered here, as can be seen in their Figure 2. Secondly, Rieck et al. (2018) studied the EKE decadal variability, whereas we focus on the interannual timescales. As we go toward larger spatial and temporal scales, it is probable that the intrinsic contribution to the total variance would diminish. It is also worth noting that we adopted here a different modeling strategy. Rieck et al. (2018)

used a long climatological run to study the sole imprint of intrinsic interannual variability. Here, we use 50 members with fully varying atmospheric forcing, which gives access to both intrinsic and atmospherically driven interannual oceanic fluctuations. Fully understanding the reasons behind our different conclusions on the EKE variability nature would require more analyses, which are beyond the scope of this paper." We hope this is now more rigorous and will satisfy the reviewer.

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. We agree, that's a good suggestion that will help the readers not familiar with the region. We added this Figure in the paper, as Figure 1.

Specific comments 1. Introduction l. 30 "equatorial band": Do you mean the current bands? We meant the equatorial current system. This is now corrected.

l. 38 "These currents": Please specify which currents are referred to here; is the statement valid for all currents mentioned in the paragraph above? Done: it now reads "the currents in the whole Southwest Pacific"

l. 48 "wind anomalies in the southern hemisphere": Are these wind anomalies related to the SAM as well? Wind anomalies in the area (wind stress curl changes) have been suggested to be related to the Southern Annular Mode [Cai, 2006; Roemmich et al., 2007], or to decadal ENSO variations in the subtropics [Holbrook et al., 2005a, 2005b; Sasaki et al., 2008; Holbrook et al., 2011]. So it is not well known, and these different suggestions indicate that the changes in the region still need investigation. l. 57 "[...] south of 20âŮęS [...]: In the paragraph above, ENSO is said to affect wind stress curl to 30âŮęS. Here however, ENSO's influence is said to be restricted to North of 20âŮęS. This is inconsistent. Thanks for pointing this apparent inconsistency. To be clearer, we modified the latitude in both sentences as "25°S", limit for the ENSO wind stress curl anomalies and ENSO's influence. In fact, there is no clear latitudinal limit, as the wind curl anomalies decrease regularly poleward, and the Rossby waves phase propagation increase poleward, regularly decreasing the impact of ENSO basin-scale anomalies on the western transports. We hope this is now clearer. l. 64 “Between these two latitudes [...]: Does this refer to 15-30âŮęS? If yes, there is no need for this first part of the sentence. Agree; this is removed.

2. Data, model description, methods l. 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.

Thanks for this suggestion. However, we consider that the important perspective of intrinsic variability in a coupled system fits well at the end of the manuscript. This is a suggestion for further studies, and we preferred to keep it where it was.

l. 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?

Thanks for this question. We now give explanation for this choice as follows: “ We focus our analyses on the 1980-2015 period; before 1980 indeed, the buoyancy fluxes derived from the DFS5.2 forcing are devoid of interannual variability. Starting our analyses in 1980 thus yields an effective spinup time of 41 years within each member.”

l. 131: How is the PDO index defined? (in fact line 215). We added the description of the PDO index: the leading principal component of North Pacific monthly sea surface temperature variability (poleward of 20°N for the 1900-93 period)

l. 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.

We use the 5-days velocities, filtered at high frequency by removing signals at frequencies lower than 180 days. Thus, we use the 180-day low-pass filtered velocities as the mean in the EKE calculations. We tried to make that clearer in the text.

3. Deterministic versus chaotic oceanic transport variability l. 219: I suggest to better

specifiy what “realistically” means in this case. Are the simulated current strengths within a certain range of the observed ones?

We added that in terms of latitudinal and longitudinal extensions, and in terms of mean transports, most currents are realistically simulated. We also added more details in the text referring to the description of the various currents, and compared the simulated mean transports to those observed from gliders, sections, or cruises.

ll. 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.

Only areas where the spread is greater than 15% of the mean are shaded with dots (line 235). The other values are given to give more details, but cannot be inferred from the Figure. This is now stated by adding “not shown”.

l. 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.

Absolutely. We agree and added some EKE values, and provided more details about the model deficiencies.

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

Yes. “South of 20°S” is now removed.

ll. 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.

We thank the reviewer for these suggestions. Although we knew about the Oliver and Holbrook paper, the interesting Bull et al. study did escape our research. We added these papers in the references, and added sentences reflecting their conclusions.

l. 319: How is “veering eastward” defined?. We added “(ie, when the SSH isoline longitude starts to increase)”, hoping this is now clearer.

6. Discussion and conclusion ll. 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 re- gion from 165âŮęE - 130âŮęW 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 175âŮęW - 153âŮęW, which is almost entirely outside the region investigated here. You should better justify, why a comparison of this study with Travis and Qiu (2017) and Rieck et al. (2018) is nonetheless valid. Penduff et al. (2011), SeÌĄrazin et al. (2015) and Rieck et al. (2018) all show that the ratio of intrinsic to total variability is not zonally uniform.

Yes, this is true. Travis and Qiu (2017) and Rieck et al. (2018) focused on different regions, larger and further east. Yet, a careful look at their Figures (Figure 2b of Rieck et al., 2018) reveal that the ratio of EKE intrinsic variance on total EKE variance they found significantly differ from ours (see our Figure 4c). See also above how we reformulated the discussion.

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

Thanks. We changed to “interannual”.

Technical corrections 1. Introduction l. 25: Instead of “[...], differently for different oceanic depths.” I suggest to write some- thing like “[...] with different impacts at different oceanic depths.”

The term Âń impact” did not seem appropriate here; we changed to “with different

Printer-friendly version

Discussion paper

connectivity for different oceanic depths”

ll. 29-30: It should either be “Low-Latitude Western Boundary Currents” or “LLWBC”. l. 36: currents’ Thanks. Corrected.

l. 37: masses’ Thanks. Corrected.

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

l. 74 “imprint”: Should be either “impact” or “imprint on”. Corrected, thanks. l. 111 “hampers”: Should be “hamper”. Corrected, thanks. 2. Data, model description, methods l. 162 “low-ass”: Should be “low-pass”. Thanks!

ll. 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.

Many thanks for pointing these errors. These are corrected. The STCC is a broad flow, composed of various branches. Defining its transport is thus complex, and we preferred not to isolate it as a specific key transport.

3. Deterministic versus chaotic oceanic transport variability l. 232: No comma after “(not shown)”. Corrected l. 235: I do not see dots in Fig. 1. Maybe there is a problem with the figure?. We corrected the mistake: it should refer to Figure 1a and not 1b. We changed also “small black circles”, hoping it is clearer. On our version at least these circles appear very clearly.

l. 238: “EAC’s and Tasman Front’s”. Corrected l. 251: See comment to line 235. Quite surprising! They are even more clear in this Figure…

l. 256: I suggest writing “of the ensemble mean 0-1000m zonal transport”. l. 257:

Printer-friendly version

Discussion paper

no comma after “atmospherically-forced”. l. 317: I suggest using either “isoline” or “contour”, not both. l. 321: “eddies’ ”. All done

4. Drivers of deterministic variability l. 373: Tchilibou et al. (2020) is not in the references. Thanks! Added.

5. Spatio-temporal structure of the chaotic oceanic variability ll. 400-401: “imprint the transport” should be “impact the transports’ ”. l. 402: I suggest deleting “hints of”. Done, thank you. l. 407: I suggest writing “computed first” instead of “first computed”. We removed “first”. l. 407-410: Aren’t these two sentences describing the same thing?. No. This is now better explained: first, EOF are applied on individual members. Then, on a combined 50 members together. l. 423: “consists in” should be “consists of”. l. 424: I suggest writing “first two EOFs” instead of “two first EOFs”. l. 432: “behaves” should be “behave”. l. 433: “shows” should be “show”.

All done, thank you.

6. Discussion and conclusion l. 446: “than the deterministic atmospheric variability” should be at the end of the sentence. l. 457: “varies” should be “vary”. l. 467-468: “density anomalies remotely forced” should be “remotely forced density anomalies”. l. 468: “EKE” should be “EKE’s”. l. 469: “authors” should be “authors’ ”. All corrected, thank you.

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. Thanks for pointing this. Units were initially cm2/s2 per the total period (36 years). Ti ease comparisons with other studies, with changed to cm2/s2 per year. Fig. 5: Panel a) lacks a colorbar. Fig. 11: Units are missing.

All the figures have been corrected accordingly.

Interactive comment on Ocean Sci. Discuss., https://doi.org/10.5194/os-2020-102, 2020.