

Rebuttal 19 October

The authors have done an excellent job with the revisions of the previous version of the manuscript, as well as the responses to review comments. I have a few additional comments, mostly related to clarifying things.

General comment: There are a few places where I think some clarification and caveating of the results would be appropriate. The simulations you use are for global-scale models, yet it is well known that in many cases finer-scale (often regional) models are necessary to capture transport through narrow straits. As such, your results are likely off (as is evidenced in lines 260ff), although the overall transport is not bad (lines 237ff). I think some more clarity would be helpful, in particular descriptions as to when we can likely trust the results. This is especially important because we don't have observations for geoengineering, so we need to know when the metrics are giving trustworthy answers.

There are several factors here that are playing a role. The Island rule was specifically formulated to take into account the difficulties in measuring flow in complex topography. Instead, as we explain the Sverdrup theory of wind forcing was developed, and this much larger scale methodology should also be suitable for the global models we have analysed here.

Some of the large differences between the observations and the method are probably because the buoyancy hypothesis for the ITF is incorrect, or possibly the relatively small regions of the DBP (Fig. 1) are not well captured in the global models. We point this out more explicitly in the Introduction: "In contrast with the reasonable agreement for the Amended Island Rule estimates of ITF, the alternative buoyancy method behaves much worse, indicating that the hypothetical forcing is not as good an explanation for ITF as the Amended Island Rule, or that the models used do not capture the specific details of the DBP. But although the Amended Island Rule matches the short duration of observed fluxes and variability better than buoyancy, it is possible that changes in buoyancy forcing may affect volume transport of the ITF on decadal scales under a changing climate and so we examine its changes under the geoengineering scenarios."

In the Summary we also add this text: "The Island rule was specifically formulated considering the difficulties in measuring flow in complex topography. Instead, the Sverdrup theory of wind forcing was utilized, allowing much larger scale observations to provide useful estimates of ITF. This methodology should also be suitable for the global models we have analysed here. This contrasts with the relatively small regions of the DBP (Fig. 1), that may not be consistently captured in the global models we analysed."

Figure 4: I like this figure, but it's hard to see what's going on. I'd like to see (in addition) a version of this figure that focuses on the ITF inlet. I'll let you decide which of the figures goes in the main article, supplement, etc.

We made a new plot and decided to have this in the supplemental material (Figure S3):

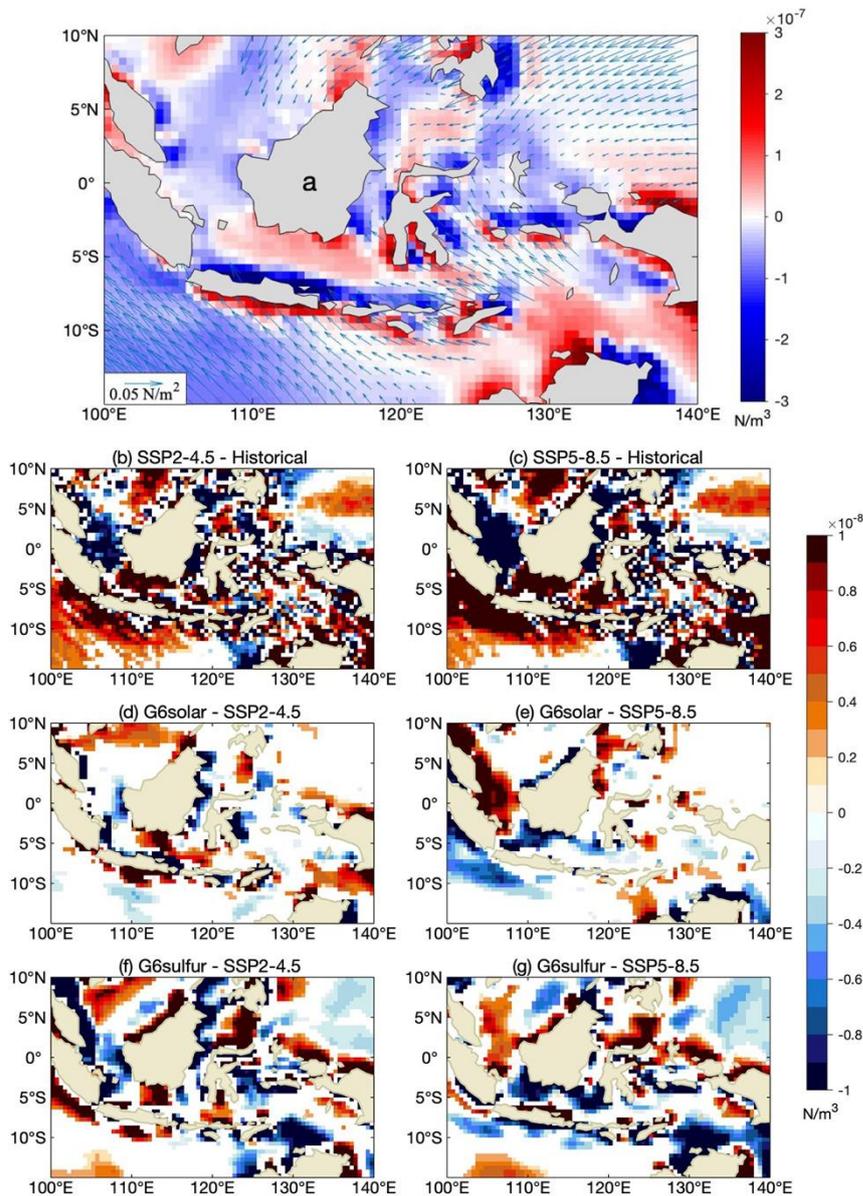


Figure S3. The ITF inlet region around the Indonesian archipelago in more detail than shown in Fig. 4. The multi-model mean differences in wind stress curl (a) the historical mean and the arrows show the wind stress, (b) SSP2-4.5 and historical, (c) SSP5-8.5 and historical, (d) G6solar and SSP2-4.5, (e) G6solar and SSP5-8.5, (f) G6sulfur and SSP2-4.5, (g) G6sulfur and SSP5-8.5. The historical period is 1980-2014, and the future scenarios period is 2080-2100. Regions where differences are not significant at the 95% level by the Wilcoxon signed-rank test are masked in white.

Section 4.2.2: I found this section to be written confusingly. The section jumps back and forth between topics and isn't clear about when you're looking at climate change vs geoengineering. I'd recommend some organization.

Agreed. We have broken the text into 3 paragraphs and generally clarified the structure.

Section 5: The first two paragraphs and Figure 8 aren't part of the summary. They're a new thing. I'd move these into their own subsection in Section 4.

Agreed, and the text is expanded for clarity.

Figure 8: I don't doubt the correctness of the figure, but I found it impossible to read. It has 24 panels, all with a lot of information. It took me 5 minutes of staring to even see that there were arrows, which made the last line of the caption make a lot more sense.

We show larger plots of the CESM2-WACCM results as an example and move the old 24 panel figure in the supplementary

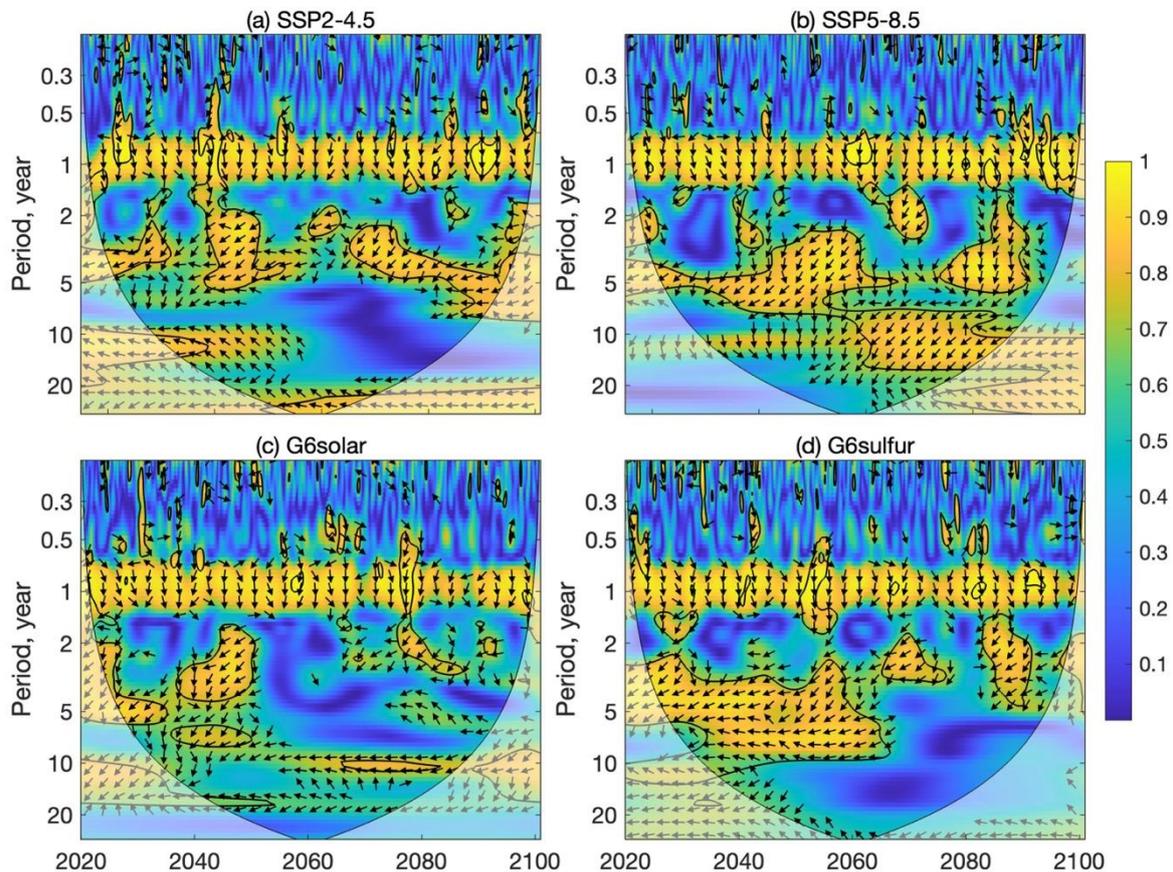


Figure 8. The squared wavelet coherence between the Nino3.4 (representing ENSO) and the wind-driven ITF transport monthly anomalies under the two SSPs (2015-2100) and two G6 (2020-2100) scenarios in CESM2-WACCM model. The 95% significance level above the background of 1000 Monte-Carlo ensemble of series of identical mean and standard deviation with identical power spectra but phase-randomized Fourier noise (chosen instead of the usual first order autoregressive null hypothesis here because of the strong annual signal; Xia et al. (2023)), is represented by a thick contour line. The arrows indicate the relative phase relationship, that is, in-phase points to the right, anti-phase points to the left, the arrow up indicates that the ITF anomaly leads ENSO by 90° , and a down arrow indicates that the ITF anomaly lags ENSO by 90° . The other models are shown in Fig. S4.

Line 27: Typo in "dimming"

Done.

Lines 101-102: How much smaller?

Changed to "although the average transport was only half the transport observed during INSTANT."

Lines 105-107: This is a bit opaque.

Modified to: While the Amended Island Rule matches the short duration of observed fluxes and variability better than buoyancy, it is possible that changes in buoyancy forcing may affect volume transport of the ITF on decadal scales under a changing climate.

Line 175: How was the interpolation done?

All fields were bi-linearly interpolated (except for sea water vertical velocity, for which we use conservative interpolation) onto a common $0.5^\circ \times 0.5^\circ$ grid.

Line 179: I think you're listing the number of grid boxes, not the resolution.

Yes, thanks.

Line 188: Can you be more specific about which models?

Feng et al. (2011) used an eddy-permitting numerical model, ORCA025, to verify that the Island Rule can capture the decadal variability of the ITF transport.

Line 252: Which test did you use?

Wilcoxon signed rank test.

Line 265: Missing word.

Added "to".

Lines 269-271: I'm not sure I understand this.

Rewritten as : "the equations are the regression trend lines (2015-2100 under the two SSP scenarios and 2020-2100 under the two G6 scenarios) and the significance of the slope".

Line 278: The differences don't look significant (although I'm sure they are). Can you talk more about your significance testing?

Rewritten as "SAI and SD geoengineering methods clearly have different impacts on wind driven contributions to ITF transport for all models (Table S1) and the ensemble mean (Table 2) according to the Wilcoxon signed- rank test, and smaller although still significant differences in upwelling for the 6 model ensemble mean, although significant differences individually only for CESM2-WACCM (Figure 2a, b, Table 2; Table S1)."

Line 282: Here as well - what significance test?

Yes. Table 2 should be below this paragraph which explains the test between scenarios. "all these differences between scenarios are significant ($p < 0.05$, Wilcoxon signed-rank test; Table 2)".

Line 293: Can you be more specific about the zonal integration?

We added this explanation to section 3.2: "The contribution of deep ocean upwelling is integrated over the whole Pacific north of 44°S (considering volume conservation and the sill depths of the Indonesian seas is less than 1500 m)."

And this in section 4.1 “zonally integrated, starting at of 44°S and proceeding northward until 60°N, upwelling contributions “.

Line 343: Typo?

Corrected.

Line 351: Is this using the Wilcoxon test?

Yes, the details have been added to Table S1 as in Table 2, which we cite at this point as well in the text.

Line 356: Are these components or different methods? By calling them components, the implication is that you can add them all up and get the "true" ITF.

The wind and upwelling can be summed, the buoyancy is an independent method. The caption is expanded to clarify this: “The differences in monthly ITF Transport (2020-2100)^a and its components according to the different methods; Wind is the ITF transport derived from Island Rule and used in the Amended Island Rule; Upwelling is the area integral of Pacific upwelling rate at 1500 m used in the Amended Island Rule; Wind and Upwelling is the ITF transport calculated by Amended Island Rule; Buoyancy is the ITF transport by buoyancy forcing and used independently of the other two components.”

Line 360: Can you call the "Total" column something different, like "Wind+Upwelling"? Total implies you should be adding in buoyancy.

Yes, replaced as suggested.

Line 398: DJF?

Yes, thanks.

Line 476: Typo.

Corrected.