

Imminent rapid decline of the Indonesian Throughflow after reaching a turning point of CO₂ concentration

Corresponding Author: Professor Shijian Hu

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Attachments originally included by the reviewers as part of their assessment can be found at the end of this file.

Version 0:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors used two CIMP6 simulations, the historical and 1% CO₂ increase, to identify a nonlinear response of the ITF transport to the CO₂ levels. They found a turning point in the rapid decline of the ITF at the current CO₂ level and one at a high CO₂ level when the decline in ITF becomes stable. The findings are new to the climate community and the topic is worthwhile for a quick publication.

Here are some comments for the authors to address before the manuscript can be finalized.

The authors propose that the current CO₂ level is a turning point for the ITF. Is it also a turning point for other climate indices? could they be related to the ITF variation and change?

A few studies have examined the projected ITF changes under the RCP scenarios. For example, the CO₂ levels in Fig. 2 may be related to what is expected by the end of this century.

Previous studies have related the ITF reduction to deep water mass and deep upwelling. It is useful to understand the connection between the surface circulation and the thermohaline circulation contribution.

Line 86-88: the CMIP models, with only greenhouse gas forcing, wouldn't be able to capture the interannual variations of the ITF in observation, which can only be captured with realistic wind forcing. By examining Fig. S1, the correlation is mostly due to the annual cycle of the ITF.

Fig. 3: there are studies suggesting the equatorial Indian Ocean winds are important for the ITF variability. So it's not clear the usefulness of using the 22-8S band to represent the Indian Ocean wind response.

In any case, the wind forcing may be secondary to the thermohaline forcing?

The lower panel of Fig. 4 shows the mean state, which doesn't illustrate the potential changes.

It would be nice to pinpoint the causes of a second turning point.

Reviewer #2

(Remarks to the Author)

Using results from idealized CMIP6 annually 1% CO₂ rise experiments, the authors have assessed the thermosteric and halosteric components of the ITF transport and concluded that the increasing CO₂ concentration in the atmosphere shall eventually lead to a decline in ITF transport. The authors also proposed two turning points based on the changing rate of the ITF transport. The manuscript is generally well written except that the citation numbers are not correctly indexed and the figure captions need improvement. The findings are important but should be presented with the right context. I have some significant concerns about the findings and the interpretations which are listed below. By addressing these concerns, the manuscript could be more rigorous, appropriately contextualized, and valuable to the scientific community.

Major Concerns:

- 1) The idealized 1% CO₂ rise experiment used in the study neglects other anthropogenic sources of radiative forcing, such as aerosols. Since aerosols, which are on a declining trend, counteract some of the warming effects of increasing CO₂, this could bias the results, and the proposed vanishing ITF transport might be expedited under more realistic conditions. The authors could address this concern by acknowledging that the 1% CO₂ experiment is a simplification and that, in reality, aerosols and other factors (such as land use changes and different greenhouse gases) might influence the rate and magnitude of ITF transport changes. The authors should clarify that their results represent an idealized scenario and not a direct prediction of future conditions, emphasizing the need for more comprehensive modeling that incorporates multiple forcings. They should also discuss the potential uncertainties in their conclusions, given the absence of realistic time-evolving aerosol and other greenhouse gas forcing in the experiment.
- 2) The study assumes a fixed level of no motion at 1100 m, but the bathymetry at 114°E reaches much deeper (~6000 m) between 11°S – 22°S. Recent studies have shown significant barotropic variabilities in the Indo-Pacific basin (<https://doi.org/10.1016/j.dsr.2023.104104>), suggesting that the level of no motion should extend to the ocean bottom for more accurate transport estimates. This may become all the more relevant with the impending climate change wherein frequent extreme events are predicted. An atmospheric disturbance can generate barotropic responses if the wavenumber and the frequency meet certain criteria (see M.J. Lighthill, Time-varying currents, Philos. Trans. Roy. Soc. Lond. Math. Phys. Sci., 270 (1971), pp. 371-390). The authors should consider adjusting their ITF transport calculation to account for deeper bathymetry, potentially extending the level of no motion to the ocean bottom. They could discuss how this adjustment would likely improve the accuracy of the total ITF transport estimates. I do understand that any additional experiment with CMIP6 is time and resource consuming. The authors should at the least present a sensitivity analysis based on the revised assumption of the level of no motion and show that extending the bathymetry to the ocean bottom does not significantly alter the findings of this study.
- 3) The causality of the declining ITF transport lies in the changing wind stress and precipitation. Figure 3b and 3d represent a turning point (5 × 10¹⁵ kg CO₂) where the wind stress and precipitation anomaly changes sign. Why the turning point related to wind stress and precipitation anomalies (as shown in Figures 3b and 3d) is not considered more relevant than the two turning points proposed by the authors. The concern is that the authors may have missed a potentially important relationship between the changes in wind stress and precipitation and the ITF transport. The authors could revise their interpretation of the turning points and consider whether the changes in wind stress and precipitation are more directly tied to the decline in ITF transport than the two turning points they originally proposed.
- 4) I have concerns that the abstract may sensationalize the findings, suggesting an imminent and rapid decline in ITF transport. The authors should carefully revise the abstract to better reflect the idealized nature of the study and clarify that the findings pertain to a specific experimental scenario rather than an immediate real-world prediction.

Minor Comments:

- 1) Line 39-42 : The authors may wish to apprise the readers about the MJO driven ITF transports.
- 2) Line 52-53: If the winds are strong enough, it can affect the entire water depth.
- 3) Line 62: It will be good to inform the readers about the time needed for the global temperature to reach 1.5 °C given the current trend.
- 4) Captions in Fig S1(a) and Fig S1(b) appear to be swapped. Caption mentions cm/s whereas the colorbar appears in m/s.
- 5) The authors use the abbreviations in tandem without describing it. For example, MITF is used in the Results section but it is described much later in the Methods.
- 6) Line 77: Even though CMIP6 is pretty well known across the earth science community, it is a good practice to expand the initials given the varied reader base of this journal.
- 7) Line 88-90: What is the correlation between the ITF transport from CMIP6 and MITF and the level of significance ? The authors should mention it in the caption of Figure S1b if not in the text.
- 8) Line 95-97 : The magnitude differs by ~ 3 Sv. The trends appear similar though. The authors should mention it apart from highlighting that the two transports are in good agreement.
- 9) Line 108-112: Why does not ITF_T and ITF_S add up to the total ITF given that the residual term is negligible as claimed in Hu and Sprintall 2016.
- 10) ITF' used in Fig S2b and S2d is not defined anywhere.
- 11) Fig 2: The shades in Fig 2b should be properly described in the caption.

- 12) Fig 2(a) : 1pctCO₂ -EN ??? What does it refer to ?
- 13) Line 184-187: I suppose that the temperature and salinity is averaged over depth. If yes, it is averaged up to what depth ? These details should be mentioned in the text.
- 14) Figure S4 : I don't see black quadrilaterals in panels (a) and (c).
- 15) Fig 3: Captions (b) and (c) should state that the points are from all CMIP6 models. In fact, the authors should be more attentive about the captions across all figures.
- 16) Line 241 : Typo.... Change existed to exist.
- 17) Figure 4 should be described in some detail in the text rather than leaving it to the interpretation of the readers.
- 18) Line 266-268 : "We find that the response of zonal wind stress in the Indo-Pacific Ocean is featured by nonlinear weakening trend with two turning points like ITF's response". This statement is based on Figure 3. It's not clear from the plot why those two points should be called turning points.
- 19) Line 329 : The indexed reference is actually number 15 instead of 14.
- 20) Line 287-289 : It is important to elaborate how CO₂ increment is implemented in the models. Are the 1% increments spread over months or sudden at the end of a year ? What about the other greenhouse gases? Not all readers may be aware of the DECK experiments within the CMIP6 framework. The manuscript will be more complete if these relevant details are mentioned instead of directing the readers to ref no 47.
- 21) Since this journal admits 10 display figures, I don't see a need to include Supplementary Figures in this manuscript.

Reviewer #3

(Remarks to the Author)

Review of "Imminent rapid decline of the Indonesian Throughflow after reaching a turning point of CO₂ concentration", by Shijian Hu et al.

Submitted to Nature Communications

This manuscript discusses the response of the Indonesian Throughflow (ITF) to increasing CO₂, in 25 CMIP6 climate models. The model runs studied are the historical (HIST) and 1% per annum CO₂ increase (1pctCO₂) runs. The manuscript notes that when plotted against CO₂ concentration the ITF response is nonlinear, with an initially slow rate of weakening, which accelerates and then slows again as a function of CO₂ concentration. The second part of the paper begins to unpick some of the mechanisms of the ITF decline, finding that a balance/competition between wind-driven differences in warming, and precipitation-driven changes in salinity, appear to be the main drivers of the ITF weakening. The authors conclude by speculating, based on the 1pctCO₂ results, that current CO₂ concentrations may be such that a rapid weakening of the ITF may be imminent.

The topic of the paper, changes in the ITF in response to increasing greenhouse gas forcing, is interesting and important, and to the best of my knowledge has not been extensively studied in the literature to date. So the study is welcome. Unfortunately there are some methodological weaknesses in the analysis which to my mind invalidate the discussion of the nonlinear response and the conclusion that a rapid ITF decline may be imminent. I have noted these under 'Methodological issues' below. I did find the discussion of mechanisms of ITF weakening interesting and more convincing.

Overall, my feeling is that the authors have the basis of a valuable and interesting study, particularly on the mechanisms driving the ITF weakening, but that a large part of the analysis presented, and the conclusions drawn from it, need to be revisited, to the extent that any revised version would essentially be a different paper. Hence I cannot recommend acceptance of this paper, but I hope that the authors will continue with this work and in particular publish and develop further their analysis of mechanisms. Noting the dependence on supplementary figures, it may be that this analysis would work best in a longer format journal.

I'm sorry I can't be more positive about the present manuscript, but I hope that these comments are useful and constructive in taking this interesting work forward.

Methodological issues

Apart from Fig. 1, the authors present their analysis of the 1pctCO₂ run in terms of CO₂ concentration rather than time (Figs 2-4). They note a levelling off of the ITF weakening at higher concentrations. This is entirely to be expected as the radiative forcing from CO₂ increases logarithmically with the concentration. Indeed this is why the standard protocol follows a percentage increase of concentration per year rather than a linear increase. The same levelling off would be seen in any climate variable (e.g. global mean temperature), if plotted against CO₂ concentration. Since there is no direct mechanistic link between CO₂ concentration and ITF (only the indirect link through the response of the climate system), it really makes no sense to plot the response in this way.

The above does not explain the initial slow response of the ITF. This can equally well be seen when plotting the response

against time (Fig. 1a). This is most likely an issue of the initial response time to perturbations. Other variables such as global mean temperature show a 2-timescale response to CO₂ perturbations, with a fast timescale of a decade or two followed by a longer adjustment time. It is seen most clearly in the instantaneous 4xCO₂ experiment but is also there in the 1pctCO₂ experiments (see e.g. <https://journals.ametsoc.org/view/journals/clim/26/6/jcli-d-12-00195.1.xml>). This initial adjustment time may be different for the ITF than for global mean temperature, and in fact understanding this response time would be a valuable addition to the mechanistic analysis in this study. In any case, it seems a priori far more likely that the initial slow response is related to an inherent timescale of the model dynamics rather than a specific threshold CO₂ concentration. The apparent 'threshold' CO₂ concentration would therefore be entirely dependent on the specific timeseries of concentration used to force the model. I think if the claim that there is a specific threshold CO₂ concentration that results in acceleration is to be made, it would at least have to be shown across several different CO₂ forcing scenarios.

Following on from the above, the claim in the Discussion section that the real ITF may be on the verge of a rapid decline, based on current CO₂ concentrations, is not justified. Even if CO₂ concentration were somehow the direct control variable (which hasn't been demonstrated), what about all the other radiatively active forcing species in the present and future atmosphere? In any case, to assess the claim, why not simply look at the projection runs (ScenarioMIP) to see if the models actually project a rapid ITF decline?

Version 1:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors have improved the manuscript based on the previous review comments. I believe the manuscript is valuable for the climate research community and is worth a quick publication. Still, there are some minor revisions necessary before the manuscript can be considered for publication.

1. Regarding the previous comment on Fig. S1:

I noted that the comparison was with the historical simulations. Even in the "historical simulations", unlike the pacemaker model experiment, the models will not be able to generate ENSO events in the same years as in the observations. I checked the Santoso paper, and they only compared the ITF annual cycle with observations. I suggest that the authors separate the annual and interannual signals in the ITF and then compare them with the observed ones. What you would find is that for the annual cycle, it is very similar to Santoso et al. (2022), but for the interannual part, the correlations would be insignificant.

I note that this is not a critical part of the text. However, it is a fundamental concept of the CMIP models, so it needs to be correctly stated.

2. First "turning point":

The turning point would be of interest to most readers. There is a dilemma. The climate models project a weakening ITF; however, in observations, we haven't captured a weakening trend so far. The ITF transport has strengthened over the recent decades. We somehow know that this ITF trend is due to climate variations, such as the phase of the PDO. It is a fair question to ask when the weakening trend of the ITF will emerge from its natural variability.

As this study is mostly statistical, it would be useful to compare the greenhouse effect-driven ITF trend with that of the natural variability. I don't know how the authors derive trends in Fig. 2b. For natural variability, they probably can calculate 30-yr trends in the ITF transport in the CMIP historic runs.

Anyway, I would be happy to see this work being further improved.

Reviewer #2

(Remarks to the Author)

The authors have responded to all the queries satisfactorily. The manuscript can now be considered for publication.

Reviewer #3

(Remarks to the Author)

Please see review in attached pdf

Version 2:

Reviewer comments:

Reviewer #1

(Remarks to the Author)

The authors have addressed my previous comments, and the manuscript can be considered for publication in the journal.

Reviewer #3

(Remarks to the Author)

Review of "Imminent rapid decline of the Indonesian Throughflow after reaching a turning point of CO₂ concentration", by Shijian Hu et al.

Submitted to Nature Communications

This is my third review of this manuscript. I thank the authors for their careful responses to my last set of comments, including performing substantial additional analyses. I think these analyses, shown in new supplementary figures, shed further light on what is going on. Unfortunately I still find the framing of the results in the main text of the manuscript unconvincing, so I can't recommend publication in its current form. I do think there is a way forward to produce a highly impactful paper, but it would be more of a rewrite than a revision.

First I have some comments on the new supplementary figures:

Fig. S4(a) (plot of ITF against GMST). This apparently shows an initial slow response of the ITF to increasing GMST. If real, to me this is more unexpected and more striking than the low-CO₂ response in the plot of ITF against CO₂ concentration (Fig. 2a), which as discussed in my earlier reviews might be expected due to the initial slow warming in the 1% runs. Presumably a slow ITF response at lower warming levels is due to lags in the system such as the multi-decadal adjustment time of the ITF to instantaneous radiative forcing (new Fig. S7). However as it stands I don't find Fig. S4 convincing. It shows no data for GMST <3.5 deg C. I would expect it to go right down to GMST=0, as there is plenty of data at these GMST values available in the 1pctCO₂ runs. The figure also needs an indication of the ensemble spread or confidence interval for the ensemble mean values shown. Because there is a large range in the Transient Climate Response (TCR) between the models (see IPCC AR6 WGI Chapter 7), a given GMST value is likely to be reached at very different CO₂ concentrations between models, so a fairly large spread is to be expected. These additions are needed to convince the sceptical reader that the apparent slow initial response is real.

Fig. S7 (Response of ITF in the instantaneous 4xCO₂ runs). The response is very interesting here, showing that the (ensemble mean) ITF stabilises at ~6Sv after a few decades, even though the GMST is still warming at this point in most/all of the runs. This does provide evidence (more convincing evidence than Fig. 2a) that a levelling off of the ITF at a non-zero value can be expected at some future point. I think this behavior potentially sheds valuable light on the response dynamics of the ITF. A more in-depth process analysis to determine what sets the timescale and the stabilisation level would be very valuable, building on the insights to the wind and buoyancy forcings in the manuscript. To facilitate this it would be important to show how much the stabilisation time and level vary among the ensemble members/models.

Fig. S8 (ITF as a function of CO₂ in HIST and SSP585 runs). By eye I can't see a clear discontinuity in the slope between the HIST and SSP585 segments. Given the large ensemble spread (evident from the individual plots in the figure), and noise even in the ensemble mean, I doubt that there is any statistically or practically significant difference. The final drop in the timeseries (and in the rate of change in panel b) looks like an edge effect (noting that similar drops are seen at earlier points in the figure, e.g. around CO₂ = 650 and 850 ppm). To me, this figure does not as it stands support the assertion in the title of "imminent rapid decline of the ITF".

The new figures have started to unpack some potentially very interesting behavior, and have the potential to really move forward understanding of the future of the ITF. The evidence presented suggests that the response of the ITF to increasing CO₂ is dependent on the history of CO₂ forcing as well as the current concentration, due to the significant delay timescales of both surface temperature and the ITF itself. This means that conclusions drawn purely from analysis of the 1% CO₂ runs are strongly contingent on that specific forcing scenario. It might be more convincing if the SSP585 and 1% timeseries could be plotted together, using consistent processing methods (e.g. it looks as if Figs 2a and S8 use different smoothing), and with a realistic estimate of uncertainty/ensemble spread.

In short, I think the figures (including the new supplementary figures) contain a lot of valuable information about the response of the ITF to a warming climate, although some need a little more work to be convincing. The initial response seems to be a combination of more than one dynamical delay timescales, and so is particularly dependent on forcing scenario, while the later levelling off of the ITF response looks like a more robust feature. By combining the information on the response to different CO₂ forcing scenarios with the process analysis of the response (a strength of the manuscript) I think there is the potential to really move this topic forward. Unfortunately I still feel that the current framing of the paper in terms of instantaneous CO₂ concentration as a primary control on ITF strength, and the claim in the title of imminent rapid weakening, are unconvincing. I'm sorry I can't recommend publication in its present form. I think the authors have produced most of the analysis needed for a really valuable paper, with a reframing to focus on the processes and timescales that determine the transient ITF response. But it would be a rewrite rather than a revision of the current manuscript.

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Point-by-point response to Reviewer#1

We thank the reviewers for their helpful suggestions and insightful comments. In the following, black indicates the comments by the reviewer; blue is our response to the reviewer; and changes in the manuscript are highlighted by red.

Reviewer #1 (Remarks to the Author):

The authors used two CIMP6 simulations, the historical and 1% CO₂ increase, to identify a nonlinear response of the ITF transport to the CO₂ levels. They found a turning point in the rapid decline of the ITF at the current CO₂ level and one at a high CO₂ level when the decline in ITF becomes stable. The findings are new to the climate community and the topic is worthwhile for a quick publication. Here are some comments for the authors to address before the manuscript can be finalized.

Response: Thank you for your encouragement and constructive suggestions. We have made careful revisions according to your comments. Please see our point-by-point responses below.

The authors propose that the current CO₂ level is a turning point for the ITF. Is it also a turning point for other climate indices? could they be related to the ITF variation and change?

Response: Thank you for these important questions. Yes, it is possible that the current CO₂ level is also a turning point for other climate indices and especially for those related to the ITF variation and change. After the current CO₂ level, the ITF will be rapidly weakened and the Indo-Pacific Ocean is going to be in a state with weak inter-basin exchange, likely leading to rapid heat changes in the Indian Ocean and Pacific Oceans. The turning point for other climate indices definitely needs further in depth study, however in this study, we mainly focus on the response of ITF to the CO₂ level.

In the revised manuscript, we have added some sentences to describe the possibility of turning point for other climate indices related to ITF.

Please see Lines 321-325: “Given that the rapid changes in the ITF in response to changes in CO₂ concentration would lead to changes in the inter-basin exchange of heat and freshwater between the Indian Ocean and Pacific Ocean, turning points might also occur for other Indo-Pacific climate indices, and further studies are needed.”

A few studies have examined the projected ITF changes under the RCP scenarios. For example, the CO₂ levels in Fig. 2 may be related to what is expected by the end of this century. Previous studies have related the ITF reduction to deep water mass and deep upwelling. It is useful to understand the connection between the surface circulation and the thermohaline circulation contribution.

Response: Yes, some previous studies have examined the projected ITF changes by the end of this century under the RCP scenarios. As you note, several previous studies have examined the relationship between the ITF reduction and deep ocean circulation (i.e., related to deep water mass and deep upwelling) as well (Sen Gupta et al. 2016; Feng et al. 2017). For example, Feng et al. (2017) examined the annual mean ITF volume transport in the historical and RCP8.5 runs using a near-global 1/10° ocean general circulation model, and suggested that deep ocean circulation, through changes in net deep ocean upwelling in the tropical and South Pacific, is as important as wind-driven processes to the ITF transport and variability. Sen Gupta et al. (2012) suggested that the ITF reduction in the CMIP3 models might result from a weaker Mindanao Current transport given that the changes in the two transports have similar magnitudes. As pointed by Sprintall et al (2024), “One alternative hypothesis suggested for the ITF transport weakening is a projected reduction in the upwelling and circulation of the deep waters entering the Pacific from the Southern Ocean that ultimately upwell to exit via the ITF (Sen Gupta et al. 2016; Feng et al. 2017).” So, changes in the deep ocean circulation might indeed play an important role in the low-frequency variability and long-term trend of ITF. In any case, whether ITF changes are generated by the wind or by the deep-upwelling related to thermohaline circulation, would be contained in the changes of Indo-Pacific pressure gradient.

In the revised manuscript, we have added a few sentences to discuss the connection between the surface circulation and the thermohaline circulation contribution and the potential role of deep-water mass and deep upwelling in the ITF reduction.

Please see Lines 305-309: “For example, previous studies suggested that changes in the deep ocean upwelling also plays an important role in the long-term trend of the ITF in addition to the changes in the Indo-Pacific wind forcing^{24,32}, implying that the response of the halosteric and thermosteric ITF to CO2 levels might occur through both wind forcing and deep ocean upwelling.”

Line 86-88: the CMIP models, with only greenhouse gas forcing, wouldn't be able to capture the interannual variations of the ITF in observation, which can only be captured with realistic wind forcing. By examining Fig. S1, the correlation is mostly due to the annual cycle of the ITF.

Response: We are sorry for the ambiguity in describing the models. The CMIP simulations mentioned in Lines 86-88 and Fig. S1 are CMIP6 historical simulations. As noted by Eyring et al. (2016), “These simulations are forced, based on observations, by evolving, externally imposed forcings such as solar variability, volcanic aerosols, and changes in atmospheric composition (GHGs and aerosols) caused by human activities. The CMIP historical simulations provide rich opportunities to assess model ability to simulate climate, including variability and century timescale trends (e.g. Flato et al., 2013). These simulations can also be analysed to determine whether climate model forcing and sensitivity are consistent with the observational record, which provides opportunities to better bound the magnitude of aerosol forcing (e.g. Stevens,

2015).” Hence, the CMIP historical simulations are usually used to examine natural variability and compare with historical observations. For example, Santoso et al. (2022) examined “20 models from phase 5 of the Coupled Model Intercomparison Project (CMIP5) and the SODA-2.2.4 ocean reanalysis” and “found that the CMIP5 models overall simulate aspects of ITF variability, such as spectral and vertical structure, that are consistent with the reanalysis, although inter-model differences are substantial”.

In the revised manuscript, we have added a few sentences to specify the experiment of CMIP6 (i.e., historical run) we used, and discussed why it is proper to examine the variability using CMIP6 historical simulations.

Please see Lines 351-354: “It should be noted that the external forcing for the Hist runs are based on observations over 1850-2014^{51,52}, and the Hist runs are usually used to examine natural variability and compare with historical observations¹⁷. For example, Santoso et al.¹⁷ found that the CMIP5 models overall simulate aspects of ITF variability.”

Fig. 3: there are studies suggesting the equatorial Indian Ocean winds are important for the ITF variability. So it's not clear the usefulness of using the 22-8S band to represent the Indian Ocean wind response. In any case, the wind forcing may be secondary to the thermohaline forcing?

Response: Thanks for your question. Yes, some studies suggest that the equatorial Indian Ocean winds are important for the ITF variability, but this is dependent on the specific time scales. Please note that the ITF flows into the Indian Ocean in the southern hemisphere within the latitudes of 22-8°S, and the major mechanism that the equatorial Indian Ocean winds influence the ITF is through generating equatorial Kelvin waves which propagate to the equatorial eastern Indian Ocean and then southward to the ITF region along the coasts. This means that the influence of the equatorial Indian Ocean winds on the ITF variability is relatively confined in the time scales of Kelvin waves (typically range from intra-seasonal, seasonal to interannual time scales).

On longer time scales, things are quite different. For example, Sprintall et al. (2009) found that “To first order, the seasonal cycle of transport in the thermocline (100–150 m) in all three exit straits is dominated by regional monsoon forcing, with maximum ITF during the southeast monsoon.” On decadal time scales, the ITF variability is likely controlled by mostly the Pacific winds and then the southeastern Indian Ocean. For example, Lee et al. (2015) carried out sensitivity experiments to understand the relative importance of wind forcing from the different ocean basins in driving the ITF variability, and they conclude that “Using real-time surface winds inside the Pacific Ocean and climatological winds outside of the Pacific Ocean (EXP_PAC), the abrupt increase of the Indian OHC700 during the 2000s was successfully simulated.”

As to the role of the wind forcing and the thermohaline forcing, we did not separate the relative contribution of the wind forcing and the thermohaline forcing to the ITF change. The contribution due to temperature change may include contributions from both the

wind forcing and the thermohaline forcing. But this is really an interesting question we need to study in the future.

In the revised manuscript, we have added some sentences to describe why we mainly examined the wind forcing in the western Pacific Ocean and eastern Indian Ocean. And please note that we have presented the wind forcing change in the equatorial Indian Ocean as well, in addition to the time series of wind stress in the eastern Indian Ocean.

Please see Lines 219-221: “Note that, for the wind forcing, the ITF variability on decadal and longer time scales is likely controlled by mostly the Pacific winds and then the southeastern Indian Ocean⁶.”

The lower panel of Fig. 4 shows the mean state, which doesn't illustrate the potential changes. It would be nice to pinpoint the causes of a second turning point.

Response: Thanks for your suggestions. Yes, the lower panel of Fig. 4 shows the mean state. In the revised manuscript, we have updated the Fig. 4 and illustrate the potential changes of surface winds and precipitation as well. Please see the updated Fig.4. As to the causes of a second turning point, we are sorry that we are not sure about the causes of a second turning point by now. But in the revised manuscript, we have discussed the potential causes of a second turning point.

Please see Lines 246-249: “After exceeding the second turning point, the responses of wind stress and precipitation differences in the western Pacific and eastern Indian Ocean to CO₂ are both weakened, leading to a reduced ITF response, which may be associated with changes in the background state and the weakened Indo-Pacific exchange under this regime.”.

The Fig.4 has been updated.

Thank you again for your insightful comments and helpful suggestions.

Point-by-point response to Reviewer#2

We thank the reviewers for their helpful suggestions and insightful comments. In the following, black indicates the comments by the reviewer; blue is our response to the reviewer; and changes in the manuscript are highlighted by red.

Reviewer #2 (Remarks to the Author):

Using results from idealized CMIP6 annually 1% CO₂ rise experiments, the authors have assessed the thermosteric and halosteric components of the ITF transport and concluded that the increasing CO₂ concentration in the atmosphere shall eventually lead to a decline in ITF transport. The authors also proposed two turning points based on the changing rate of the ITF transport. The manuscript is generally well written except that the citation numbers are not correctly indexed and the figure captions need improvement. The findings are important but should be presented with the right context. I have some significant concerns about the findings and the interpretations which are listed below. By addressing these concerns, the manuscript could be more rigorous, appropriately contextualized, and valuable to the scientific community.

Response: Thank you for your encouragement and constructive suggestions. We have made careful revisions according to your comments, and the citation numbers have been corrected, the figure captions have been improved. Please see our point-by-point responses below.

Major Concerns:

1) The idealized 1% CO₂ rise experiment used in the study neglects other anthropogenic sources of radiative forcing, such as aerosols. Since aerosols, which are on a declining trend, counteract some of the warming effects of increasing CO₂, this could bias the results, and the proposed vanishing ITF transport might be expedited under more realistic conditions. The authors could address this concern by acknowledging that the 1% CO₂ experiment is a simplification and that, in reality, aerosols and other factors (such as land use changes and different greenhouse gases) might influence the rate and magnitude of ITF transport changes. The authors should clarify that their results represent an idealized scenario and not a direct prediction of future conditions, emphasizing the need for more comprehensive modeling that incorporates multiple forcings. They should also discuss the potential uncertainties in their conclusions, given the absence of realistic time-evolving aerosol and other greenhouse gas forcing in the experiment.

Response: Thank you for your insightful comments. We totally agree with you that the idealized 1% CO₂ rise experiment neglects other anthropogenic sources of radiative forcing, such as aerosols; and neglecting these anthropogenic sources may counteract some of the warming effects of increasing CO₂ and might bias the results. Following your suggestions, we have:

a) acknowledged that the 1% CO₂ experiment is a simplification and that, in reality, aerosols and other factors (such as land use changes and different greenhouse gases) might influence the rate and magnitude of ITF transport changes in the abstract and main text;

b) clarified clearly that these results represent an idealized scenario rather than a direct prediction of future conditions, and emphasized the need for more comprehensive modeling that incorporates multiple forcings;

c) in the Discussion section, we have added sentences to discuss the potential uncertainties in their conclusions, given the absence of realistic time-evolving aerosol and other greenhouse gas forcing in the experiment.

Please see:

Line 21-23: “Here using idealized CMIP6 simulations, we find that the response of the ITF to increasing atmospheric carbon dioxide (CO₂) concentration is nonlinear and exhibits an abrupt rapid decline after surpassing a turning point.”.

Lines 25-27: “The current level of CO₂ concentration has reached a turning point, indicating that a rapid decline in the ITF and a state of transition in the Indian and Pacific Oceans is likely imminent.”.

Lines 325-328: “However, as noted, large uncertainty still exists among the climate models, particularly the idealized 1pctCO₂ experiment, in which aerosols and other factors such as land use changes and other greenhouse gases are not considered. These factors may also result in changes to the rate and magnitude of the ITF transport.”.

Lines 315-318: “In addition, the results represent an idealized scenario rather than a direct prediction of future conditions, given that other anthropogenic sources of radiative forcing such as aerosols are neglected, and hence more comprehensive modeling that incorporates multiple forcings is needed.”

2) The study assumes a fixed level of no motion at 1100 m, but the bathymetry at 114°E reaches much deeper (~6000 m) between 11°S – 22°S. Recent studies have shown significant barotropic variabilities in the Indo-Pacific basin (<https://doi.org/10.1016/j.dsr.2023.104104>), suggesting that the level of no motion should extend to the ocean bottom for more accurate transport estimates. This may become all the more relevant with the impending climate change wherein frequent extreme events are predicted. An atmospheric disturbance can generate barotropic responses if the wavenumber and the frequency meet certain criteria (see M.J. Lighthill, Time-varying currents, Philos. Trans. Roy. Soc. Lond. Math. Phys. Sci., 270 (1971), pp. 371-390). The authors should consider adjusting their ITF transport calculation to

account for deeper bathymetry, potentially extending the level of no motion to the ocean bottom. They could discuss how this adjustment would likely improve the accuracy of the total ITF transport estimates. I do understand that any additional experiment with CMIP6 is time and resource consuming. The authors should at the least present a sensitivity analysis based on the revised assumption of the level of no motion and show that extending the bathymetry to the ocean bottom does not significantly alter the findings of this study.

Response: Thank you for your comment. Yes, we agree that the level of no motion should probably extend to the ocean bottom for more accurate transport estimates.

We considered two reasons assuming the 1100 m as the level of no motion: (1) the sill depth of the main core of the ITF within the Indonesian Seas is about 1100 m (e.g., Andersson and Stigebrandt, 2005; Hu and Sprintall, 2016); (2) variability of the ITF and Temperature/Salinity is confined within the upper layer ocean as shown in in situ observations (roughly in the upper 700 m layer; Lu et al., 2024; Sprintall et al., 2009); (3) in situ observations show that the velocity in the 1000-1200 m layer is close to zero (Sprintall et al., 2009); and (4) in situ observations are mainly available in the upper layer (~700 m for XBTs and 2000 m for Argo floats). We really appreciate your understanding that additional experiment with CMIP6 is time and resource consuming. Following your suggestion, we have undertaken sensitivity analysis to test if the findings are sensitive to the choice of the level of no motion or not.

Figure R1 presents a comparison of the ITF transport anomalies (i.e., minus the mean ITF transport) using 7 assumed levels of no motion, from 1100 m to 2500 m. Here the geostrophic current velocities are calculated using decadal mean temperature and salinity data from WOA18, which is based on in situ observations and has a high resolution (0.1°). (Note that there are extremely few data available below 2500 m so we are reluctant to assume level of no motion at the bottom). As we can see, the ITF transports with different assumed levels of no motion show quite similar variability and long-term trend, indicating that the findings are not sensitive to the choice of the level of no motion. This is reasonable, because the variability of both geostrophic currents and temperature/salinity, is much larger in the upper layer than in the deep layer (Figure R2 for example).

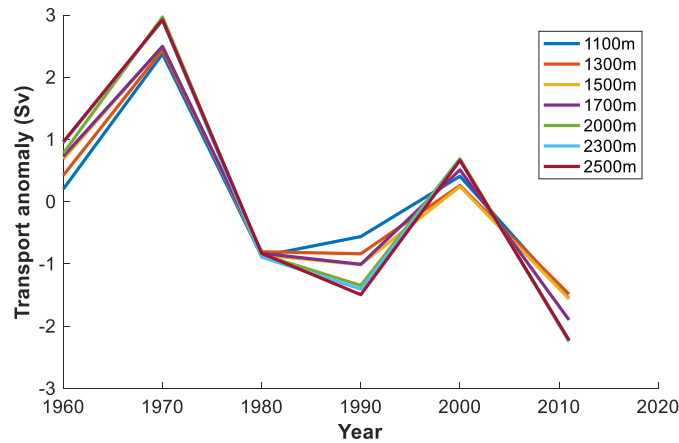


Figure R1. Comparison of ITF transport anomalies between various assumed level of no motion.

[FIGURE REDACTED]

Figure R2. Salinity and temperature trends across the 114°E section over the period 1958–2017 using WOA18 (a, b) and the ensemble mean of five datasets (c, d). This is a figure from Lu et al. (2024).

In the revised manuscript, we have added some sentences to discuss the influences of the assumed level of no motion on the results, described the result from the sensitivity analysis, and discussed the uncertainty of the results due to using an assumed level of no motion.

Please see Lines 373-375: “Here we used the 1100 m as the level of no motion considering the sill depth of ITF within the Indonesian Seas¹⁶. Using deeper depth as the level of no motion leads to no significant difference of the fluctuation of ITF transport.”

References:

Andersson, H. C., and A. Stigebrandt (2005), Regulation of the Indonesian throughflow by baroclinic draining of the North Australian Basin, *Deep Sea Research Part I: Oceanographic Research Papers*, 52(12), 2214-2233.

- Afroosa, M., B. Rohith, A. Paul, F. Durand, R. Bourdallé-Badie, and S. S. C. Shenoi (2023), The MJO-driven Indo-Pacific barotropic see-saw, *Deep Sea Research Part I: Oceanographic Research Papers*, 199, 104104.
- Hu, S., and J. Sprintall (2016), Interannual Variability of the Indonesian Throughflow: the Salinity Effect, *Journal of Geophysical Research: Oceans*, 121, 2596-2615.
- Lighthill, M. J. (1971), Time-Varying Currents, *Philosophical Transactions of the Royal Society of London. Series A, Mathematical and Physical Sciences*, 270(1206), 371-390.
- Lu, X., S. Hu*, and J. Sprintall (2024), The role of precipitation and salinity effect in multi-decadal changes and long-term trends of the Indonesian Throughflow, *Journal of Climate*, 37(4), 1317–1331.
- Sprintall, J., S. E. Wijffels, R. Molcard, and I. Jaya (2009), Direct estimates of the Indonesian Throughflow entering the Indian Ocean: 2004–2006, *Journal of Geophysical Research: Oceans* (1978–2012), 114(C7), C07001.

3) The causality of the declining ITF transport lies in the changing wind stress and precipitation. Figure 3b and 3d represent a turning point (5×10^{15} kg CO₂) where the wind stress and precipitation anomaly changes sign. Why the turning point related to wind stress and precipitation anomalies (as shown in Figures 3b and 3d) is not considered more relevant than the two turning points proposed by the authors. The concern is that the authors may have missed a potentially important relationship between the changes in wind stress and precipitation and the ITF transport. The authors could revise their interpretation of the turning points and consider whether the changes in wind stress and precipitation are more directly tied to the decline in ITF transport than the two turning points they originally proposed.

Response: Thank you for your comments. The point you mentioned is indeed a noteworthy one, as it represents an inevitable intermediate stage in the long-term variations of wind stress and precipitation—specifically, the point (5×10^{15} kg CO₂, i.e., the middle of 2 to 10×10^{15} kg CO₂) where the anomaly value is zero and a sign change occurs before and after it. We have carefully considered this issue, but we believe that this transition point has little impact on understanding the rate of change in the ITF trend we are discussing due to two main reasons:

- (1) Figure 3b and 3d display anomalies in wind stress and precipitation relative to a mean value, so there must inherently be an intermediate point where the sign transitions, i.e., the zero point, so the existence of the point 5×10^{15} kg CO₂ is expected.
- (2) Before (after) this intermediate point (5×10^{15} kg CO₂) corresponds to a positive (negative) anomaly of ITF transport (i.e., sign of Q , Q is ITF transport), rather than long-term trends of ITF (sign of $\partial Q / \partial t$). By contrast, the response of the ITF to the two turning points we presented in this study indicate a dramatic jump of the ITF trend (sign of $\partial Q / \partial t$), rather than simply positive or negative anomalies.

In the revised manuscript, we have added a sentence to discuss this point in Lines 227-230: “An intermediate point exists at 5×10^{15} kg of CO₂ where the wind stress and precipitation anomaly changes sign, but this is mainly because we display anomalies relative to a mean value over a CO₂ range of 2 to 10×10^{15} kg.”

4) I have concerns that the abstract may sensationalize the findings, suggesting an imminent and rapid decline in ITF transport. The authors should carefully revise the abstract to better reflect the idealized nature of the study and clarify that the findings pertain to a specific experimental scenario rather than an immediate real-world prediction.

Response: Thank you for your comments. We would like to clarify that the conclusions contain both the idealized aspect and a practical aspect. The idealized aspect, as you mentioned, is because we used simulations with specific experimental scenario (i.e., CMIP6 experiments). The practical aspect is embedded in the fact that the observed CO₂ level is indeed passing the first turning point and that the observed wind and precipitation trends agree with the simulations. Anyway, the key findings of this study are based on some idealized numerical experiments with large uncertainties. In the revised manuscript, we have adjusted the wording in the abstract and main texts to emphasize the idealization and uncertainty of the results while ensuring respect for the objectivity of the data. Please see:

Line 21-23: “Here using idealized CMIP6 simulations, we find that the response of the ITF to increasing atmospheric carbon dioxide (CO₂) concentration is nonlinear and exhibits an abrupt rapid decline after surpassing a turning point.”.

Lines 25-27: “The current level of CO₂ concentration has reached a turning point, indicating that a rapid decline in the ITF and a state of transition in the Indian and Pacific Oceans is likely imminent.”.

Lines 325-328: “However, as noted, large uncertainty still exists among the climate models, particularly the idealized 1pctCO₂ experiment, in which aerosols and other factors such as land use changes and other greenhouse gases are not considered. These factors may also result in changes to the rate and magnitude of the ITF transport.”.

Lines 315-318: “In addition, the results represent an idealized scenario rather than a direct prediction of future conditions, given that other anthropogenic sources of radiative forcing such as aerosols are neglected, and hence more comprehensive modeling that incorporates multiple forcings is needed.”

Minor Comments:

1) Line 39-42 : The authors may wish to apprise the readers about the MJO driven ITF transports.

Response: Done. Thanks.

2) Line 52-53: If the winds are strong enough, it can affect the entire water depth.

Response: Indeed. We've revised this sentence.

3) Line 62: It will be good to inform the readers about the time needed for the global temperature to reach 1.5 C given the current trend.

Response: We have added a sentence here following your suggestion. Thanks.

Lines 64-65: "Recent studies suggest that the increase of global temperature will exceed 1.5°C in less than 10 years according to the current emissions trajectory³⁸."

4) Captions in Fig S1(a) and Fig S1(b) appear to be swapped. Caption mentions cm/s whereas the colorbar appears in m/s.

Response: Thank you, we have corrected the captions.

5) The authors use the abbreviations in tandem without describing it. For example, MITF is used in the Results section but it is described much later in the Methods.

Response: Thanks for pointing out this. We have described these abbreviations when they appear for the first time in the manuscript.

6) Line 77: Even though CMIP6 is pretty well known across the earth science community, it is a good practice to expand the initials given the varied reader base of this journal.

Response: We agree. We have expanded the initials of CMIP6 in Line 73 where it is first used.

7) Line 88-90: What is the correlation between the ITF transport from CMIP6 and MITF and the level of significance? The authors should mention it in the caption of Figure S1b if not in the text.

Response: Please note that the MITF is not a continuous time series, which was interrupted during about 2011-2013. Hence we calculated the correlation between the monthly ITF transport from CMIP6 and MITF during the longest portion of the MITF time series for Jan 2004-Jul 2011. As a result, the correlation between the ITF transport from CMIP6 and MITF is 0.25 which is significant at 90% confidence level. We have included this information in the caption of Figure S1b.

8) Line 95-97: The magnitude differs by ~ 3 Sv. The trends appear similar though. The authors should mention it apart from highlighting that the two transports are in good agreement.

Response: Done. Thanks.

9) Line 108-112: Why does not ITF_T and ITF_S add up to the total ITF given that the residual term is negligible as claimed in Hu and Sprintall 2016.

Response: As noted, the ITF_T (0.60 Sv per decade) and ITF_S (-0.32 Sv per decade) exactly add up to the total ITF (0.28 Sv per decade). So the results are in good agreement with that claimed by Hu and Sprintall (2016).

10) ITF' used in Fig S2b and S2d is not defined anywhere.

Response: Done. Thanks.

11) Fig 2: The shades in Fig 2b should be properly described in the caption.

Response: Done. Thanks.

12) Fig 2(a): 1pctCO₂ -EN ??? What does it refer to?

Response: “1pctCO₂ -EN” refers to the ensemble mean of 1pctCO₂ experiments. We have added some words to specify what it means. Thanks.

13) Line 184-187: I suppose that the temperature and salinity is averaged over depth. If yes, it is averaged up to what depth ? These details should be mentioned in the text.

Response: Yes, they are averaged over depth. This information was mentioned in the caption of Supplementary Fig. S4. Now, we have added it to the main text as well (Line 192).

14) Figure S4 : I don't see black quadrilaterals in panels (a) and (c).

Response: The words “black quadrilaterals” have been deleted. Thanks.

15) Fig 3: Captions (b) and (c) should state that the points are from all CMIP6 models. In fact, the authors should be more attentive about the captions across all figures.

Response: Sorry for the unclear description. We originally intended to express that it is from all models by using the words “Ensemble mean”. Now we have added a sentence to be clearer: “Note the dots in panels b and d are ensemble mean of all models.”

16) Line 241: Typo.... Change existed to exist.

Response: Done. Thanks!

17) Figure 4 should be described in some detail in the text rather than leaving it to the interpretation of the readers.

Response: Good idea. We have added some sentences to described in more detail about Figure 4. Please see Lines 260-262: “Figure 4a presents a schematic diagram illustrating the response of the ITF to CO₂ forcing, concisely demonstrating the nonlinear response process of the ITF across different CO₂ phases as observed in CMIP6 models.”

Lines 289-291: “Figure 4b schematically illustrates the effect of the Indo-Pacific oceanic dynamics on the ITF during the “Exchange state”, highlighting the substantial influences of wind forcing and freshwater forcing on ITF variations.”

18) Line 266-268: “We find that the response of zonal wind stress in the Indo-Pacific Ocean is featured by nonlinear weakening trend with two turning points like ITF’s response”. This statement is based on Figure 3. It’s not clear from the plot why those two points should be called turning points.

Response: Yes, this statement is based on Figure 3. The reason we call them as “turning points” is: before and after the two points, the zonal wind stress is not quite sensitive to CO₂ changes (i.e., its variation is minimal with changing CO₂); however, when CO₂ is in between the two points, the zonal wind stress is rapidly decreased with increasing CO₂.

19) Line 329 : The indexed reference is actually number 15 instead of 14.

Response: Corrected. Thanks.

20) Line 287-289: It is important to elaborate how CO₂ increment is implemented in the models. Are the 1% increments spread over months or sudden at the end of a year? What about the other greenhouse gases? Not all readers may be aware of the DECK experiments within the CMIP6 framework. The manuscript will be more complete if these relevant details are mentioned instead of directing the readers to ref no 47.

Response: Thank you for your suggestions. Yes, it is better to mention these important details.

We have communicated with some of the model development teams, and it appears that there are slight differences between the various models. In some models, CO₂ increases month by month, while in others, it suddenly rises by 1% at the end of the year. Other greenhouse gases in the 1pctCO₂ experiment is supposed to be the same with the control run, which means the experiment conditions (e.g. greenhouse gas concentrations, ozone concentration, surface land conditions) should be representative of Earth around the year 1850.

The 1pctCO₂, i.e., a simulation forced by a 1%yr⁻¹ CO₂ increase, is one of the four baseline experiments of the Diagnostic, Evaluation and Characterization of Klima (DECK) experiments (klima is Greek for “climate”), which is one of the three major components of CMIP.

In the revised manuscript, we have added more descriptions about the relevant details of the experiments. Please see:

Lines 341-343: “The 1pctCO₂ is one of the four baseline experiments of the Diagnostic, Evaluation and Characterization of Klima (DECK) experiments, which is one of the three major components of CMIP.”

Lines 354-357: “The 1pctCO₂ runs branches the piControl as well but are driven by idealized forcing that is the same as that in piControl except for the prescribed CO₂

concentration increasing at $1\% \text{ yr}^{-1}$ (month by month or suddenly rises by 1% at the end of the year) since the 1850 without considering any observations.” Thanks!

21) Since this journal admits 10 display figures, I don't see a need to include Supplementary Figures in this manuscript.

Response: Yes, this journal admits 10 display figures, but we feel like the selected figures in the main manuscript tell the salient points of our story, whereas the supplementary figures are more secondary supporting evidence.

Again, we would like to express our sincere gratitude for your valuable comments and suggestions on our manuscript. Your insightful feedback has significantly improved the quality of our paper.

Point-by-point response to Reviewer#3

We thank the reviewers for their helpful suggestions and insightful comments. In the following, black indicates the comments by the reviewer; blue is our response to the reviewer; and changes in the manuscript are highlighted by red.

Reviewer #3 (Remarks to the Author):

This manuscript discusses the response of the Indonesian Throughflow (ITF) to increasing CO₂, in 25 CMIP6 climate models. The model runs studied are the historical (HIST) and 1% per annum CO₂ increase (1pctCO₂) runs. The manuscript notes that when plotted against CO₂ concentration the ITF response is nonlinear, with an initially slow rate of weakening, which accelerates and then slows again as a function of CO₂ concentration. The second part of the paper begins to unpick some of the mechanisms of the ITF decline, finding that a balance/competition between wind-driven differences in warming, and precipitation-driven changes in salinity, appear to be the main drivers of the ITF weakening. The authors conclude by speculating, based on the 1pctCO₂ results, that current CO₂ concentrations may be such that a rapid weakening of the ITF may be imminent.

The topic of the paper, changes in the ITF in response to increasing greenhouse gas forcing, is interesting and important, and to the best of my knowledge has not been extensively studied in the literature to date. So the study is welcome. Unfortunately there are some methodological weaknesses in the analysis which to my mind invalidate the discussion of the nonlinear response and the conclusion that a rapid ITF decline may be imminent. I have noted these under ‘Methodological issues’ below. I did find the discussion of mechanisms of ITF weakening interesting and more convincing.

Overall, my feeling is that the authors have the basis of a valuable and interesting study, particularly on the mechanisms driving the ITF weakening, but that a large part of the analysis presented, and the conclusions drawn from it, need to be revisited, to the extent that any revised version would essentially be a different paper. Hence I cannot recommend acceptance of this paper, but I hope that the authors will continue with this work and in particular publish and develop further their analysis of mechanisms. Noting the dependence on supplementary figures, it may be that this analysis would work best in a longer format journal. I’m sorry I can’t be more positive about the present manuscript, but I hope that these comments are useful and constructive in taking this interesting work forward.

Response: Thank you for your comments, suggestions, and encouragement. As per yours and other reviewer recommendations, we have revised the manuscript, with particular emphasis on refining the methodology and analysis sections you

highlighted. We hope that our revisions will help address your concerns and lead to a more favorable evaluation of our manuscript. Please see our point-by-point responses below.

Apart from Fig. 1, the authors present their analysis of the 1pctCO₂ run in terms of CO₂ concentration rather than time (Figs 2-4). They note a levelling off of the ITF weakening at higher concentrations. This is entirely to be expected as the radiative forcing from CO₂ increases logarithmically with the concentration. Indeed this is why the standard protocol follows a percentage increase of concentration per year rather than a linear increase. The same levelling off would be seen in any climate variable (e.g. global mean temperature), if plotted against CO₂ concentration. Since there is no direct mechanistic link between CO₂ concentration and ITF (only the indirect link through the response of the climate system), it really makes no sense to plot the response in this way.

Response: Thank you for your critical and valuable comments.

First of all, please note that the ITF's response to CO₂ increases is not simply "a levelling off of the ITF weakening at higher concentrations", but rather, a nonlinear process with a rapid weakening between two turning points and slow changes at concentrations lower than the first turning point or higher than the second turning point.

[FIGURE REDACTED]

Fig. R3. Response of global mean surface temperature to CO₂ (From Nicholls et al., 2020).

Second, such a nonlinear response is not really expected and previous studies suggest that same levelling off would be NOT necessarily be seen in other climate variable such as global mean temperature. For example, Nicholls et al. (2020) reported the response of global mean surface temperature to CO₂ forcing using 1pctCO₂ simulations. As we can see in Fig.R3, the global mean temperature shows a linear increasing as the CO₂ increases and no turning points can be found in the response of global mean temperature to CO₂ increasing. So, the response of the ITF is very different from that of other

climate variable such as global mean temperature. It is really surprising to see the ITF shows such interesting and unique response to CO₂ increasing.

Nonetheless, we agree with you that “there is no direct mechanistic link between CO₂ concentration and the ITF”. This does not mean that the CO₂ concentration is not essential to the ITF, but is simply because the Earth’s climate system is super complex. As the primary external forcing driving long-term changes in the Earth system, rising greenhouse gas emissions affect all key processes and physical parameters within the system - and the ITF is no exception. CO₂ is one of the most representative greenhouse gases and an effective indicator of external forcing. Studying the ITF's response to the projected CO₂ increase helps predict the ITF future behavior and the impact that those changes might have on both the Indian and Pacific Oceans.

Nicholls, Z. R. J., et al. (2020), Reduced Complexity Model Intercomparison Project Phase 1: introduction and evaluation of global-mean temperature response, *Geosci. Model Dev.*, 13(11), 5175-5190, doi:10.5194/gmd-13-5175-2020.

The above does not explain the initial slow response of the ITF. This can equally well be seen when plotting the response against time (Fig. 1a). This is most likely an issue of the initial response time to perturbations. Other variables such as global mean temperature show a 2-timescale response to CO₂ perturbations, with a fast timescale of a decade or two followed by a longer adjustment time. It is seen most clearly in the instantaneous 4xCO₂ experiment but is also there in the 1pctCO₂ experiments (see e.g. <https://journals.ametsoc.org/view/journals/clim/26/6/jcli-d-12-00195.1.xml>). This initial adjustment time may be different for the ITF than for global mean temperature, and in fact understanding this response time would be a valuable addition to the mechanistic analysis in this study. In any case, it seems a priori far more likely that the initial slow response is related to an inherent timescale of the model dynamics rather than a specific threshold CO₂ concentration. The apparent ‘threshold’ CO₂ concentration would therefore be entirely dependent on the specific timeseries of concentration used to force the model. I think if the claim that there is a specific threshold CO₂ concentration that results in acceleration is to be made, it would at least have to be shown across several different CO₂ forcing scenarios.

Response: Thank you for raising this crucial point regarding the existence of both fast and slow response processes in the ocean system - this idea provides extremely valuable insights for understanding the ITF's response to CO₂ increasing!

We agree with you that the nonlinear response of the ITF to CO₂ forcing might be related to the inherent timescale of the model dynamics. However, the relationship between the ITF and CO₂ level, i.e., ITF=ITF(CO₂), is actually different from that between ITF and time, i.e., ITF=ITF(t), when the CO₂ increases independently with time. In the time-domain relationship ITF=ITF(t), the ITF’s response, i.e., $\frac{\partial ITF}{\partial t}$, might

be similar to that discussed by Geoffroy et al. (2013), when the changing rate of ITF is determined by the fast and slow response processes, and hence the initial slow response is attributed to the slow response process.

By contrast, in the CO₂-domain relationship $ITF=ITF(CO_2)$, the changing rate of ITF, i.e., $\frac{\partial ITF}{\partial CO_2}$, is not related directly to time, and hence is not necessary directly associated with the fast and slow response processes. The CO₂-domain relation $ITF=ITF(CO_2)$ simply reflects the sensitivity of ITF to CO₂ changes. We suppose that at any specific CO₂ level, the adjustment of the ITF might have been completed, i.e., the fast and slow response processes has been completed. Actually, in the 1pctCO₂ experiment, the CO₂ increases 1% per year, indicating that the time from the first value for the CO₂ level to reach the first turning point is about 40 years, much longer than the known adjustment time scales of ITF (e.g., wind, Rossby waves, precipitation, deep ocean currents) according to previous studies (e.g., Sprintall et al., 2019).

In the revised manuscript, we have added new sentences to discuss the potential role of the fast and slow response processes, previous studies including Geoffroy et al. (2013) are cited, and in particular, the role of the inherent timescale of the model dynamics in the initial slow response is discussed.

Lines 309-315: “In addition, the response of global mean temperature to CO₂ perturbations shows a fast timescale of a decade or two followed by a slow timescale (Geoffroy et al., 2013). Even though the response of the ITF to CO₂ is expected to be different from that of global mean temperature, the fast and slow response processes might be important in determining the turning points, especially the initial slow response before the first turning point. Further in-depth research is still needed to understand the physical processes and mechanisms underlying these two turning points.”

Following on from the above, the claim in the Discussion section that the real ITF may be on the verge of a rapid decline, based on current CO₂ concentrations, is not justified. Even if CO₂ concentration were somehow the direct control variable (which hasn't been demonstrated), what about all the other radiatively active forcing species in the present and future atmosphere? In any case, to assess the claim, why not simply look at the projection runs (ScenarioMIP) to see if the models actually project a rapid ITF decline?

Response: Thanks for your comments.

Following your suggestion, we examined the changes of the ITF in different projection runs. Fig.R4 presents a comparison of projected changes of ITF velocity between ssp585 and 1pctCO₂ runs. As you can see, the ITF changes in the ssp585 is quite similar to that in the 1pctCO₂ runs, and the weakening of ITF in the ssp585 is slightly stronger than that in the 1pctCO₂ runs. Most importantly, the ssp585 experiment shows a turning point at about 1026 ppm as well, after which the decline of ITF is much weaker than

between 400-1026 ppm. Please note that the starting CO₂ concentration is about 400 ppm, which exceeds the first turning point already, so we are not able to examine the first turning point in the ssp585 experiments. Nonetheless, the ITF response to the CO₂ in the ssp585 experiments is similar to that in the 1pctCO₂ experiments.

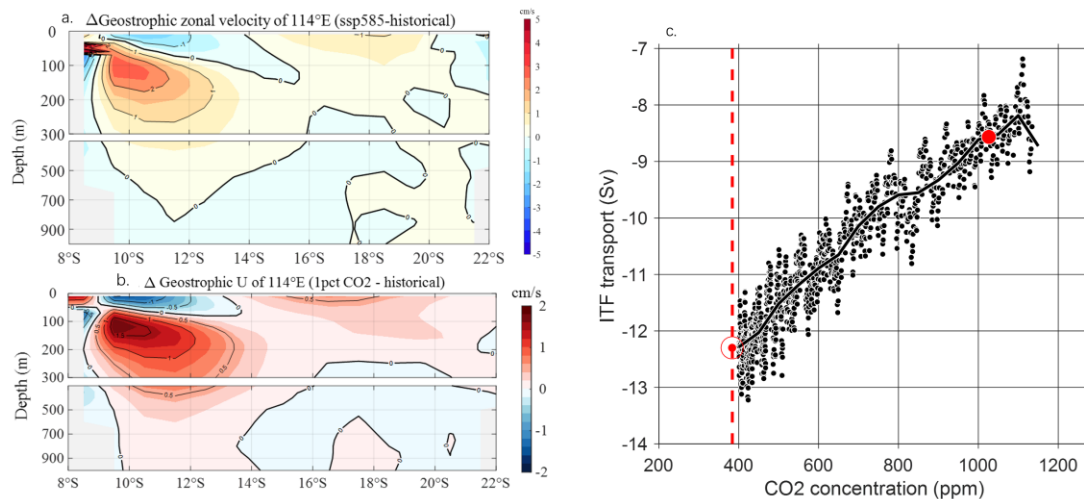


Fig. R4. Left panels: Comparison of changes of the ITF velocity in ssp585 (a) and 1pctCO₂ (b) runs. Right panel: response of ITF to CO₂ concentration in the ssp585 experiments from 16 models (black dots for each model and black line for ensemble mean of all models). The solid red dot denotes the second turning point, while the dashed red line and red cycle denote the first turning point.

As to the decline of ITF, some previous studies have shown that the CMIP models project a significant ITF decline under the business-as-usual emission scenario RCP8.5 (see Fig. R5 from Hu et al., 2015, Nature). As shown in Fig. R5, the ITF averaged over the 2050–2100 period under RCP8.5 is about 3 Sv weaker than the historical mean, indicating a future decline of ITF under RCP8.5. Under representative concentration pathways (RCP) 4.5 scenario, a robust ITF transport slowdown is found as well at depths below 100 to 1200 m (Santoso et al., 2022).

[FIGURE REDACTED]

Fig. R5. Modelled transports of annual-mean Pacific WBCs and their projected changes. a, Historical mean transport averaged over the twentieth century (positive values indicate northward, and negative, southward transports), and b, projected change from the historical mean, averaged over the 2050–2100 period under RCP8.5, for selected Pacific currents. The ITF is highlighted by the blue box. (From Hu et al., 2015, Nature).

Hence, the decline of the ITF presented in our study is in agreement with the projection runs and previous studies that show a weakening trend of the ITF in the context of global warming. However the turning points and the rapid decline after the first turning point have not been reported previously.

As to other radiatively active forcings, we agree that the CO₂ concentration is definitely not the only external forcing. But as you mentioned, the CO₂ concentration was the direct control variable in the 1pctCO₂ experiments, and this is the reason why 1pctCO₂ experiments are suitable for studying the response of ITF to greenhouse gas emissions. Our paper aims to investigate the response of ITF to the CO₂ concentration: the response to “all the other radiatively active forcing species”, which is obviously more complex, is beyond the scope of this study.

In the revised manuscript, we have added sentences in the Discussion section to discuss the potential uncertainties, given the absence of other radiatively active forcing such as aerosol and other greenhouse gas forcing in the experiment.

Please see Lines 273-276: “The decline of the ITF presented in our study is in agreement with the projection runs and previous studies that show a weakening trend of the ITF in the context of global warming^{4,17}, but the turning points and the rapid decline after the first turning point have not been reported previously.”

Lines 315-318: “It should be noted that the results represent an idealized scenario rather than a direct prediction of future conditions, given that other anthropogenic sources of radiative forcing such as aerosols are neglected, and hence more comprehensive modeling that incorporates multiple forcings is needed.”.

Again, we thank you so much for your insightful comments, which are helpful and enlightening for not only this study, but also our future studies.

Point-by-point response to Reviewer#1

We thank the reviewers for their helpful suggestions and insightful comments. In the following, black indicates the comments by the reviewer; blue is our response to the reviewer; and changes in the manuscript are highlighted by red.

Reviewer #1 (Remarks to the Author):

The authors have improved the manuscript based on the previous review comments. I believe the manuscript is valuable for the climate research community and is worth a quick publication. Still, there are some minor revisions necessary before the manuscript can be considered for publication.

Response: Thank you for your encouragement and constructive suggestions. We have further revised the manuscript. Please see our point-by-point responses below

1. Regarding the previous comment on Fig. S1:

I noted that the comparison was with the historical simulations. Even in the "historical simulations", unlike the pacemaker model experiment, the models will not be able to generate ENSO events in the same years as in the observations. I checked the Santoso paper, and they only compared the ITF annual cycle with observations. I suggest that the authors separate the annual and interannual signals in the ITF and then compare them with the observed ones. What you would find is that for the annual cycle, it is very similar to Santoso et al. (2022), but for the interannual part, the correlations would be insignificant. I note that this is not a critical part of the text. However, it is a fundamental concept of the CMIP models, so it needs to be correctly stated.

Response: Thank you for your valuable feedback. We better understand now your previous comments about the errors with respect to the HIST RUNs. Indeed, as you pointed out, in the multi-model ensemble mean ITF time series, its variations primarily reflect the response to external forcing, while signals related to internal variability (such as ENSO-related interannual variability) are essentially negligible.

As you have stated, for this reason, previous studies (e.g., Santoso et al., 2022) primarily focused on evaluating CMIP models' performance in simulating the climatology (including seasonal cycles) rather than validating interannual signals—especially the positive/negative phases of interannual variability. This was the case, even though the focus of their manuscript was largely on ENSO and IOD variability in the CMIP models.

In the revised manuscript, we have now removed the interannual signal comparison in Fig. S1 and replaced it with climatological and seasonal cycle analyses (reproduced

below as Fig. RR1). Additionally, all related discussions on interannual variability comparisons have been deleted. We sincerely appreciate your feedback!

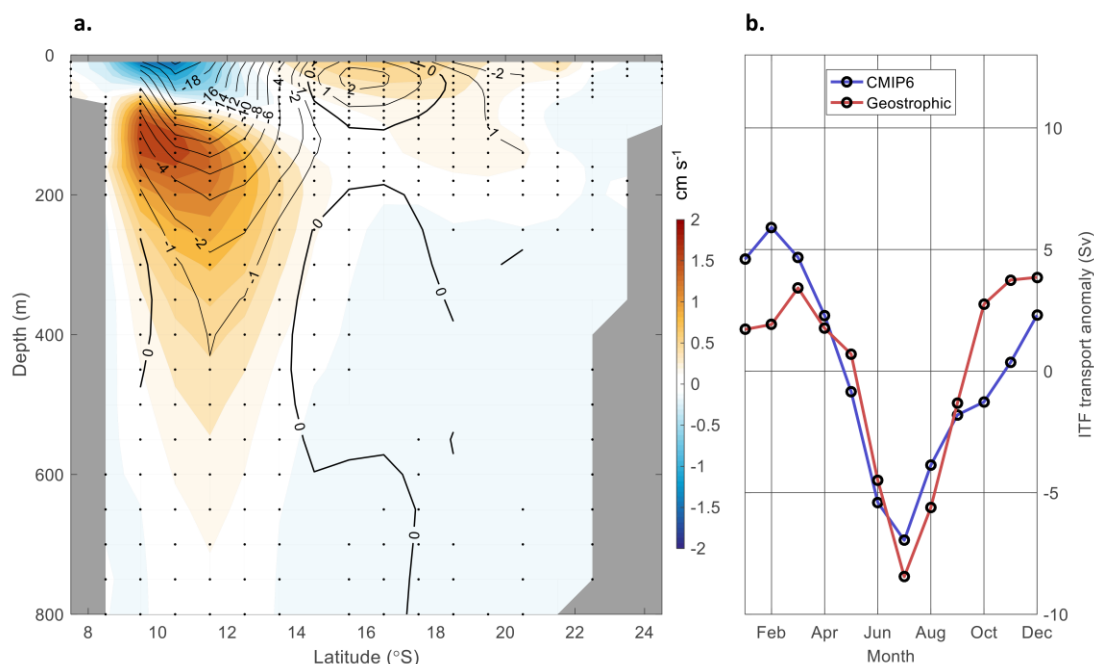


Fig. RR1. Updated Fig. S1 with a new panel b to present the comparison of the seasonal cycle of the ITF in CMIP6 and observations. Left panel (a): Climatological mean zonal velocity in the Hist runs at the 114°E section (contour lines, cm s⁻¹, positive for eastward) and its change (mean in 1pctCO₂ runs minus mean in historical runs; shading color). (b) The seasonal cycle of ITF transports (Sv) from the historical runs of CMIP6 models (multi-model ensemble mean; blue line) and geostrophic calculations with observational datasets from Lu et al. (2024) (red line). Black dots in the left panel indicate positions where change is greater than standard deviation.

Lines 80-94: “We mainly use simulations from the historical runs (Hist, 1850-2014) and idealized 1% annually increasing CO₂ concentrations experiments (1pctCO₂, 1850-1999) from 25 CMIP6 coupled climate models to ITF response to increasing CO₂ concentration. First, we validated the ITF strength in the CMIP6 models and found that the multi-model mean reasonably reproduces the observed ITF³⁰ and captures its mean structure and seasonal cycle. The vertical structure of the climatological mean zonal currents and their change in 1pctCO₂ relative to the Hist runs across the 114°E section is shown in Figure S1a. The mean ITF in the CMIP6 models shows a core between 11°S and 13°S with maximum velocity greater than 20 cm/s. The ITF transport averaged over the 150 years in the Hist runs of CMIP6 is about -13 Sv (hereafter negative for westward into the Indian Ocean), quite close to the -15 Sv determined from direct observations⁴². Figure S1b presents a comparison of the seasonal cycle of ITF transports from the historical runs of CMIP6 models and the geostrophic calculations

using observations-based datasets³⁰. The observed seasonal cycle of ITF transports is well reproduced in the CMIP6 Hist runs. The correlation coefficient is 0.82 (significant at a 99% confidence level) between the ensemble mean of CMIP6 models and the observed geostrophic transport.”

Lines 20-25 in the Supplementary Information: “**Supplementary Fig. S1.** (a) Climatological mean zonal velocity in the Hist runs at the 114°E section (contour lines, cm s⁻¹, positive for eastward) and its change (mean in 1pctCO2 runs minus mean in historical runs; shading color). (b) The seasonal cycle of ITF transports (Sv) from the historical runs of CMIP6 models (multi-model ensemble mean; blue line) and geostrophic calculations with observational datasets from Lu et al. (2024) (red line). Black dots in the left panel indicate positions where change is greater than standard deviation.”

2. First "turning point": The turning point would be of interest to most readers. There is a dilemma. The climate models project a weakening ITF; however, in observations, we haven't captured a weakening trend so far. The ITF transport has strengthened over the recent decades. We somehow know that this ITF trend is due to climate variations, such as the phase of the PDO. It is a fair question to ask when the weakening trend of the ITF will emerge from its natural variability. As this study is mostly statistical, it would be useful to compare the greenhouse effect-driven ITF trend with that of the natural variability. I don't know how the authors derive trends in Fig. 2b. For natural variability, they probably can calculate 30-yr trends in the ITF transport in the CMIP historic runs. Anyway, I would be happy to see this work being further improved.

Response: Thank you. Indeed, it is an essential question when the weakening trend of the ITF will emerge from its natural variability. The trends in Fig.2b are $\partial ITF / \partial CO_2$ rather than $\partial ITF / \partial t$, in order to examine the response to ITF to increasing CO2 and also considering that the CO2 in the experiments is not increasing linearly with time.

Following your suggestion, we have calculated the running trends of the ITF transport in a 30-yr window in both the CMIP historic runs and 1pctCO2 experiments, in order to compare the greenhouse effect-driven ITF trend with that of the natural variability. As shown in the Fig.RR2, the running trend fluctuates within the range of roughly -2 to 2 Sv/century, which is a result of natural variability. By contrast, in the 1pctCO2 experiments, the 30-yr running trend reaches 4 Sv/century rapidly, and the fluctuations of the 30-yr running trend are masked by the significant decreasing trend at the model month of about 370, when the CO2 reaches the first turning point. The trends of ITF in

both the time domain (Fig. RR2) and the CO₂ domain (Fig.2b) agree that the first turning point is the time of emerging of weakening trend from its natural variability.

In the revised manuscript, we have added a new figure, Supplementary Fig. S5, to present the running trend of ITF in a 30-yr window in the 1pctCO₂ runs and historical runs. The comparison between natural variability and the long-term trend and the emerge time of the weakening trend of the ITF is discussed in more detail in the main text and Supplementary Figures, as indicated below.

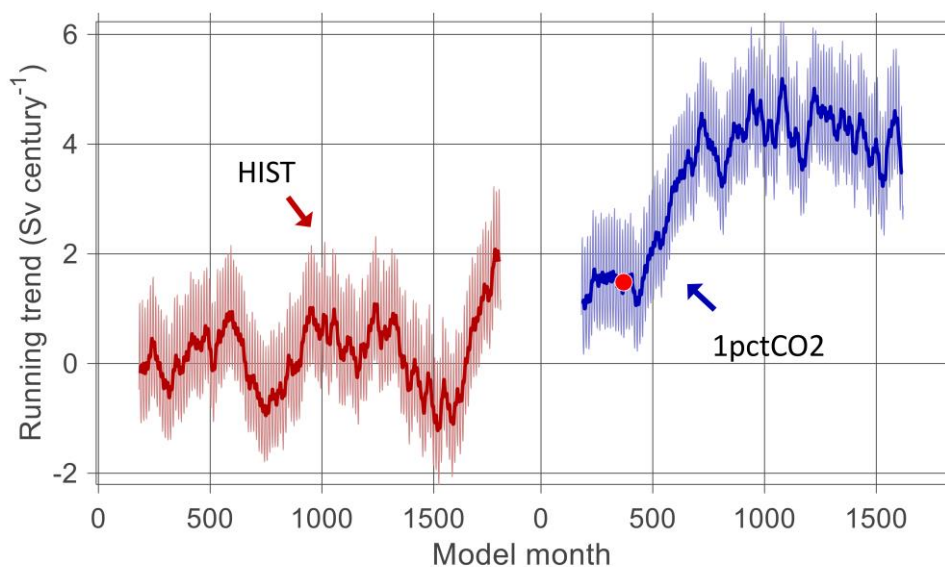


Fig. RR2. Running trend of the ITF in 30-yr windows in the 1pctCO₂ runs (blue curves) and historical runs (red curves). The red dot denotes the first turning point as detected in the change in ITF transport over change in CO₂, as shown in Figure 2b. (This is the newly added Supplementary Fig. S5).

Lines 175-182: “To further examine the emerging time of the long-term trend of the ITF from its natural variability, we have calculated the running trends of the ITF transport over 30-year windows in both the CMIP historic runs and 1pctCO₂ experiments. As shown in Figure S5, the running trend in the Hist runs fluctuates within a range of roughly -2 to 2 Sv/century, which is a result of natural variability. By contrast, in the 1pctCO₂ experiments the 30-year running trend reaches 4 Sv/century rapidly, and the fluctuations are masked by the significant decreasing trend when the CO₂ reaches the first turning point, indicating that the first turning point is indeed the emerging time of a weakening ITF trend that is distinct from its natural variability.”.

Point-by-point response to Reviewer#2

We thank the reviewers for their helpful suggestions and insightful comments. In the following, black indicates the comments by the reviewer; blue is our response to the reviewer; and changes in the manuscript are highlighted by red.

Reviewer #2 (Remarks to the Author):

The authors have responded to all the queries satisfactorily. The manuscript can now be considered for publication.

Response: Thank you for your feedback.

Point-by-point response to Reviewer#3

We thank the Reviewer#3 for his/her helpful suggestions and insightful comments. In the following, black indicates the comments by the reviewer; blue is our response to the reviewer; grey is our last response to the reviewer's first round of review; and changes in the manuscript are highlighted by red. To facilitate reading, I have added numbering to your review comments.

Reviewer #3 (Remarks to the Author):

Thanks to the authors for their responses to my earlier review. As indicated in my earlier review, I think the work in this paper is interesting and discusses an under-researched component of the climate system's response to increasing greenhouse gases. My main concern, as in my earlier review, is with the framing of the analysis in terms of CO₂ concentration, and the conclusions that the authors draw from that analysis, which I believe are misleading. Unfortunately the authors' responses and text revisions haven't resolved these concerns, so my overall assessment is as before, that I don't feel able to recommend the paper for publication in its present form. I do think the investigation of the mechanisms of change is valuable, and I hope the authors will be able to reframe their work in response to the points in my earlier review (and the further comments below). I believe this will be in their interests.

As it stands I think the issues raised around interpretation and framing will be immediately obvious to many climate scientists reading the paper, leading them to dismiss the whole paper and so miss the very worthwhile analysis of mechanisms. I'm sorry I can't be more positive about the paper as it stands, but I emphasise that I do see some very valuable analysis here, and I hope these comments are constructive and ultimately helpful to the authors.

Response: Thank you for your valuable critical comments and encouragement. We have carefully reviewed and discussed your feedback. New calculations have been conducted (every additional calculation you proposed has been performed), and revisions have been made to the descriptions of certain conclusions. While we highly respect all of your comments, for a few points, after repeated analysis of extensive data, we have decided to maintain our original views. Below, we provide a point-by-point response to each of your comments.

Comment 1:

“Response: First of all, please note that the ITF's response to CO₂ increases is not simply “a levelling off of the ITF weakening at higher concentrations”, but rather, a nonlinear process with a rapid weakening between two turning points and slow changes

at concentrations lower than the first turning point or higher than the second turning point. Fig. R3. Response of global mean surface temperature to CO₂ (From Nicholls et al., 2020). Second, such a nonlinear response is not really expected and previous studies suggest that same levelling off would be NOT necessarily be seen in other climate variable such as global mean temperature. For example, Nicholls et al. (2020) reported the response of global mean surface temperature to CO₂ forcing using 1pctCO₂ simulations. As we can see in Fig.R3, the global mean temperature shows a linear increasing as the CO₂ increases and no turning points can be found in the response of global mean temperature to CO₂ increasing. So, the response of the ITF is very different from that of other 17 / 21 climate variable such as global mean temperature. It is really surprising to see the ITF shows such interesting and unique response to CO₂ increasing. Nonetheless, we agree with you that “there is no direct mechanistic link between CO₂ concentration and the ITF”. This does not mean that the CO₂ concentration is not essential to the ITF, but is simply because the Earth’s climate system is super complex. As the primary external forcing driving long-term changes in the Earth system, rising greenhouse gas emissions affect all key processes and physical parameters within the system - and the ITF is no exception. CO₂ is one of the most representative greenhouse gases and an effective indicator of external forcing. Studying the ITF’s response to the projected CO₂ increase helps predict the ITF future behavior and the impact that those changes might have on both the Indian and Pacific Oceans. Nicholls, Z. R. J., et al. (2020), Reduced Complexity Model Intercomparison Project Phase 1: introduction and evaluation of global-mean temperature response, *Geosci. Model Dev.*, 13(11), 5175-5190, doi:10.5194/gmd-13-5175-2020.”

In the figure R3 provided (from Nicholls et al 2020), I see exactly the same qualitative response as a function of CO₂ concentration (or cumulative emissions which is used as the x-axis in that figure). There is an initial slow response (quite hard to see but clearly present in the MCE curves over the first 100 years), followed by a faster growth that gradually levels off over time (when plotted against cumulative emissions). This is just the same as Fig. 2a of the manuscript. When plotted against time (approximately the same as plotting against radiative forcing, for the 1pctCO₂ runs) the trend looks initially slow, and thereafter linear, for both the ITF (Fig. 1a of this manuscript) and for GMST (Fig. 2b of Nicholls et al., or also see Geoffroy et al).

The timescales of this behaviour would not necessarily be the same for ITF and GMST, and the different behaviours of the thermal and haline components for the ITF (Figs. 1b and 1c of this paper) contain valuable information about these timescales and the mechanisms of the response. But the null hypothesis has to be that the response is broadly linear to radiative forcing (which is the physical driver of the changes). A simple way to demonstrate that some different inherent timescales are operating for the ITF compared to GMST, would be to plot ITF against GMST. It wouldn’t surprise me

if that was the case, but it needs to be demonstrated. I agree that studying the idealised CO₂-only scenarios is a valuable contribution to understanding the response to more complex forcings. I have no problem with the choice of runs to analyse, just the interpretation of the results.

Response: Thank you for your additional comments which are helpful for us to better understand this issue. Following your suggestion, we have plotted ITF against GMST (Figure RR3a, added to the revised manuscript as Supplementary Figure S4). Just as you pointed out, the ITF shows a similar qualitative response as a function of GMST to that with CO₂, with an initial slow response followed by a faster growth (Figure RR3a). We have also plotted the GMST as a function of CO₂ mass to examine the relationship between the two (Figure RR3b). Triangles in panel (a) roughly denote the turning points in GMST, which is determined by the GMST-CO₂ relationship shown in Figure RR3b.

As we can see, the GMST increases linearly with the increasing of CO₂ mass and no turning points can be found in the GMST-CO₂ (Figure RR3b). The GMST-CO₂ relationship is not at all surprising because CO₂ concentration—through the greenhouse effect—is a physical factor that directly influences temperature changes. Therefore, the consistency of the ITF-CO₂ and ITF-GMST relationships is also not surprising, considering the linear relationship between GMST and CO₂ mass. The nonlinear relationship between ITF and GMST reflects the same ITF response to long-term external forcing, and to some extent serves as additional evidence supporting the nonlinear response of ITF.

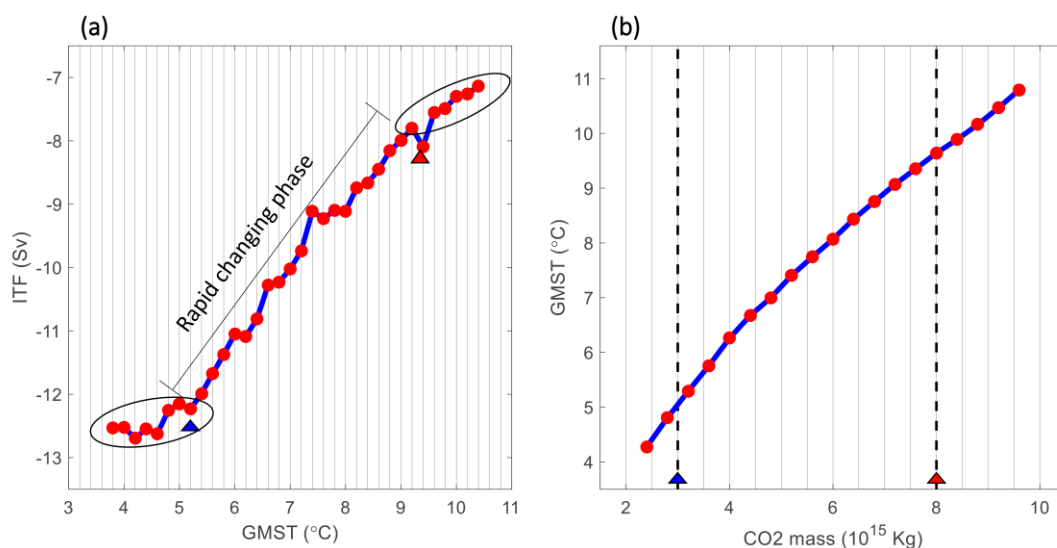


Figure RR3. Relationship of ITF-GMST and GMST-CO₂. Panel (a) presents the ITF transport averaged over bins of GMST with a bin width of 0.2°C. Panel (b) presents the GMST averaged over bins of CO₂ mass with a bin width of 0.5 (10¹⁵ Kg). The ITF, GMST and CO₂ mass are ensemble means from the 1pctCO₂ experiments of CMIP6.

The triangles represent the rough position of the two turning points and the circled areas represent the phases outside the two points. (This is the newly added Supplementary Fig. S4).

We agree with you that the ITF's response is broadly linear to radiative forcing, particularly between the two points (Figure RR3a). However, as you mentioned, there are indeed turning points, before and after which the ITF response is different (Figure RR3a).

In the revised manuscript, we have added a new paragraph to discuss the relationships of ITF-GMST and GMST-CO2.

Lines 169-174: “In addition, the ITF transport versus global mean surface temperature (GMST), and GMST as a function of CO2 mass, is shown in Figure S4. We find that the GMST increases linearly with the increasing of CO2 mass with no turning points (Figure S4b), but the GMST shows a similar qualitative response as a function of CO2 concentration, with an initial slow response followed by a faster growth and two turning points (Figure S4a). The consistency of ITF-CO2 and ITF-GMST is not surprising, considering the linear relationship between GMST and CO2 mass.”

Comment 2:

“Response: Thank you for raising this crucial point regarding the existence of both fast and slow response processes in the ocean system - this idea provides extremely valuable insights for understanding the ITF's response to CO2 increasing! We agree with you that the nonlinear response of the ITF to CO2 forcing might be related to the inherent timescale of the model dynamics. However, the relationship between the ITF and CO2 level, i.e., $ITF=ITF(CO_2)$, is actually different from that between ITF and time, i.e., $ITF=ITF(t)$, when the CO2 increases independently with time. In the time-domain relationship $ITF=ITF(t)$, the ITF's response, i.e., $\partial ITF / \partial t$, might be similar to that discussed by Geoffroy et al. (2013), when the changing rate of ITF is determined by the fast and slow response processes, and hence the initial slow response is attributed to the slow response process. By contrast, in the CO2-domain relationship $ITF=ITF(CO_2)$, the changing rate of ITF, i.e., $\partial ITF / \partial CO_2$, is not related directly to time, and hence is not necessary directly associated with the fast and slow response processes. The CO2-domain relation $ITF=ITF(CO_2)$ simply reflects the sensitivity of ITF to CO2 changes.”

It is simply not possible to make such a deduction from analysing a group of runs, all of which have the same CO2 as a function of time.

Response: We're sorry our earlier reply didn't address this concern to your satisfaction. With respect to this comment in the first review, you suggested "I think if the claim that

there is a specific threshold CO₂ concentration that results in acceleration is to be made, it would at least have to be shown across several different CO₂ forcing scenarios. ". So, we have looked at the TPs from different CO₂ forcing scenarios. As you can see in the following from our response to your comments as well as in our response to Reviewer 1 (see new Figure S5), we have examined this issue using multiple experiments—including HIST, SSP585, abrupt 4×CO₂, and 1pctCO₂ (each contains dozens of members) in both the domain of CO₂ (ITF as a function of CO₂) and the time domain (ITF as a function of time). It seems that, across multiple experiments, the ITF consistently demonstrates a nonlinear response to CO₂, the existence of two turning points, and a significant weakening trend between them." Please see our response to your other comments below for further support.

Comment 3:

We suppose that at any specific CO₂ level, the adjustment of the ITF might have been completed, i.e., the fast and slow response processes has been completed.

There's no evidence presented to support this, and as far as I can see no a priori reason to believe it. The initial adjustment timescale would be the slower of the inherent adjustment timescale of the ITF, and the response timescale of the climate variables that force the ITF, to the forcing. The latter is hard to estimate for the 1pctCO₂ scenario. It would help to sort out the different timescales, to look at the instantaneous 4xCO₂ runs.

Response: Thank you for your comment. We agree with you that it is hard to estimate the response timescale of the climate variables that force the ITF to the forcing for the 1pctCO₂ scenario. So, following your suggestion, we have looked at the ITF change in the abrupt 4xCO₂ runs combining the Hist runs, to examine the response timescales (Fig. RR4).

As we can see in Fig. RR4, the ITF shows a rapid decrease after the CO₂ abruptly shifts to 8.88×10^{15} kg, corresponding to a significant adjustment phase. This adjustment lasted for more than 40 years and then the ITF reaches a dynamically stable state, indicating that the time scale of the ITF in response to abrupt CO₂ change is roughly 40 years. In the Hist runs, the CO₂ increases from 2.22×10^{15} kg to 3.11×10^{15} kg, but that is by and large below the first turning point; while in the 4xCO₂ runs, the CO₂ is a constant and greater than the second turning point of $\sim 8 \times 10^{15}$ kg. The non-significant trends in the Hist runs and the significant decreasing trend in the 4xCO₂ run agree with what we find in the 1pctCO₂ runs.

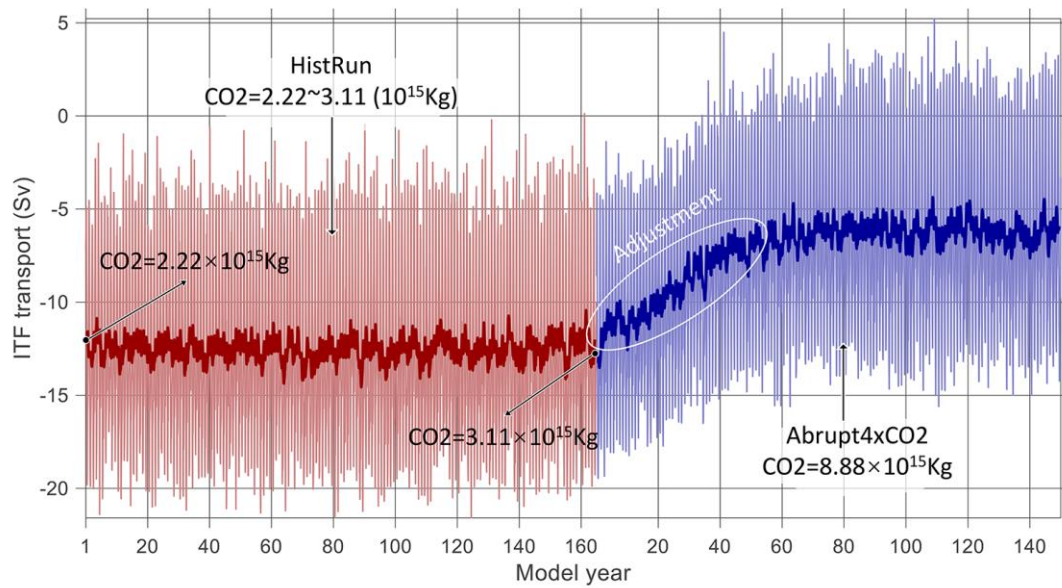


Figure RR4. ITF in the Hist Runs (red lines, bold line for 13-month running mean) and Abrupt4xCO2 experiments (blue lines, bold line for 13-month running mean). Note that the CO2 in the 4xCO2 experiments is constant. (This is the newly added Supplementary Fig. S7).

In the revised manuscript, we discuss this issue on the basis of the above new calculations.

Lines 326-329: “The time scales of the ITF’s response to abrupt increasing of CO2 is examined using the abrupt 4xCO2 runs combining the Hist runs. We found that the adjustment of the ITF lasted for about 40 years and then the ITF reaches a dynamic stable state (Figure S7).”

Comment 4:

“Actually, in the 1pctCO2 experiment, the CO2 increases 1% per year, indicating that the time from the first value for the CO2 level to reach the first turning point is about 40 years, much longer than the known adjustment time scales of ITF (e.g., wind, Rossby waves, precipitation, deep ocean currents) according to previous studies (e.g., Sprintall et al., 2019). In the revised manuscript, we have added new sentences to discuss the potential role of the fast and slow response processes, previous studies including Geoffroy et al. (2013) are cited, and in particular, the role of the inherent timescale of the model dynamics in the initial slow response is discussed. Lines 309-315: “In addition, the response of global mean temperature to CO2 perturbations shows a fast timescale of a decade or two followed by a slow timescale (Geoffroy et al., 2013). Even though the response of the ITF to CO2 is expected to be different from that of global

mean temperature, the fast and slow response processes might be important in determining the turning points, especially the initial slow response before the first turning point. Further in-depth research is still needed to understand the physical processes and mechanisms underlying these two turning points.”

I’m not saying this new text is wrong (indeed I suspect it is right to some extent), but there is no evidence presented to support it. Again, plotting ITF against GMST would shed some light on this, as would plotting the ITF response in the instantaneous 4xCO₂ runs.

Response: Thank you for your comment. The new text in the first revision was added for discussion about what we should do in the future based on the Geoffroy et al. (2013) reference you mentioned. Nonetheless, in response to the reviewer’s remarks above, the ITF against GMST is now shown in Figure RR3a and the ITF response to the instantaneous 4xCO₂ runs is presented in Figure RR4. As we can see in Fig.RR4, this adjustment of the ITF to the abrupt 4xCO₂ lasted for more than 40 years and then the ITF reaches a dynamically stable state, indicating that the time scale of the ITF in response to an abrupt CO₂ change is roughly 40 years. By contrast, the ITF in the Hist run when CO₂ increase gradually shows no significant trend. Figure RR4 seems to indicate that the ITF’s response to the CO₂ increase is nonlinear and very different from that of GMST, and also different from what Geoffroy et al. (2013) concluded.

In the revised manuscript, we have rephrased these sentences.

Lines 322-330: *“Previous studies suggest that the response of global mean surface air temperature to CO₂ perturbations shows a fast timescale of a decade or two followed by a slow timescale⁴⁷. Even though the response of the ITF to CO₂ is expected to be different from that of global mean temperature, the fast and slow response processes might be important in determining the turning points. The time scales of the ITF’s response to the abrupt increasing of CO₂ is examined using the abrupt 4×CO₂ runs combining the Hist runs. We found that the adjustment of the ITF lasted for about 40 years and then the ITF reaches a dynamic stable state (Figures S7). Further in-depth research is still needed to understand the physical processes and mechanisms underlying these two turning points.”*

Comment 5:

“Response: Following your suggestion, we examined the changes of the ITF in different projection runs. Fig.R4 presents a comparison of projected changes of ITF velocity between ssp585 and 1pctCO₂ runs. As you can see, the ITF changes in the ssp585 is quite similar to that in the 1pctCO₂ runs, and the weakening of ITF in the ssp585 is slightly stronger than that in the 1pctCO₂ runs. Most importantly, the ssp585

experiment shows a turning point at about 1026 ppm as well, after which the decline of ITF is much weaker than 19 / 21 between 400-1026 ppm.”

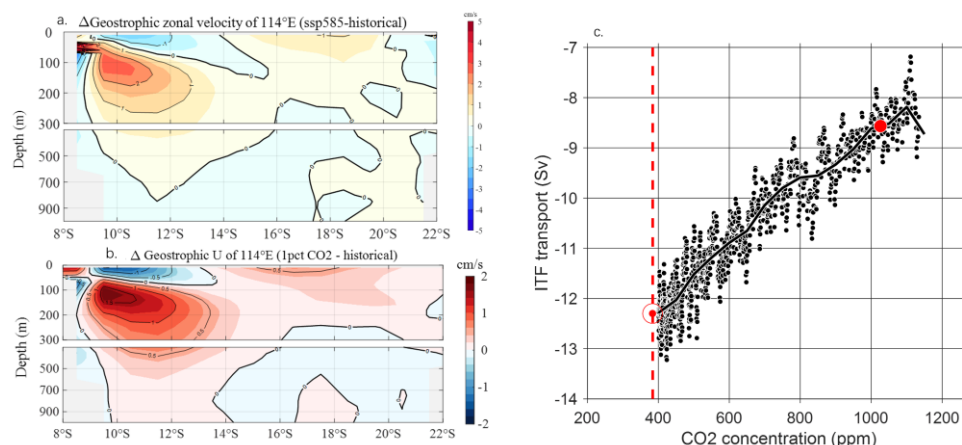


Fig. R4. Left panels: Comparison of changes of the ITF velocity in ssp585 (a) and 1pctCO2 (b) runs. Right panel: response of ITF to CO2 concentration in the ssp585 experiments from 16 models (black dots for each model and black line for ensemble mean of all models). The solid red dot denotes the second turning point, while the dashed red line and red cycle denote the first turning point.

Please note that the starting CO2 concentration is about 400 ppm, which exceeds the first turning point already, so we are not able to examine the first turning point in the ssp585 experiments.

Sorry, I simply don't see this in the figure (Fig.R4) provided. It just looks like a minor blip at just above 1000ppm, similar to others that appear in the curve.

In that case, and there is a special point below 400ppm, why is there no evidence of any acceleration of the ITF decline in the HIST runs (Fig. 1c-e)? If you stitch the HIST runs to their corresponding SSP585 members do they show an imminent rapid ITF decline?

Response: In order to present the ITF response to CO2 in SSP585 more clearly, we have plotted the ITF in the Hist runs and SSP585 runs in two forms: as a function of time and as a function of CO2 concentration. The change rate of ITF ($\partial \text{ITF} / \partial \text{CO}_2$, units in Sv per 100 ppm) in the Hist runs (CO2 < 400 ppm) and the SSP585 (CO2 > 400 ppm) run was also calculated (Figure RR5). As you can see, the ITF decline shows no acceleration in the HIST runs, which is reasonable because the CO2 in the HIST runs is less than the first turning point. We have stitched the HIST runs to their corresponding SSP585 members, and we can see that the ITF in the SSP585 after the first turning point shows an imminent rapid ITF decline (Fig. RR4).

We may more intuitively observe the response of the ITF to varying CO2 levels through the rate of change in ITF transport. The rate of change in ITF transport to CO2 ($\partial \text{ITF} / \partial \text{CO}_2$) in the SSP585 run indicates that the ITF decline between the two turning

points is much more rapid than before the first turning point or after the second turning point (Fig. RR4c). The ITF response to CO₂ in the SSP585 run agrees with the conclusion from the 1pctCO₂ runs.

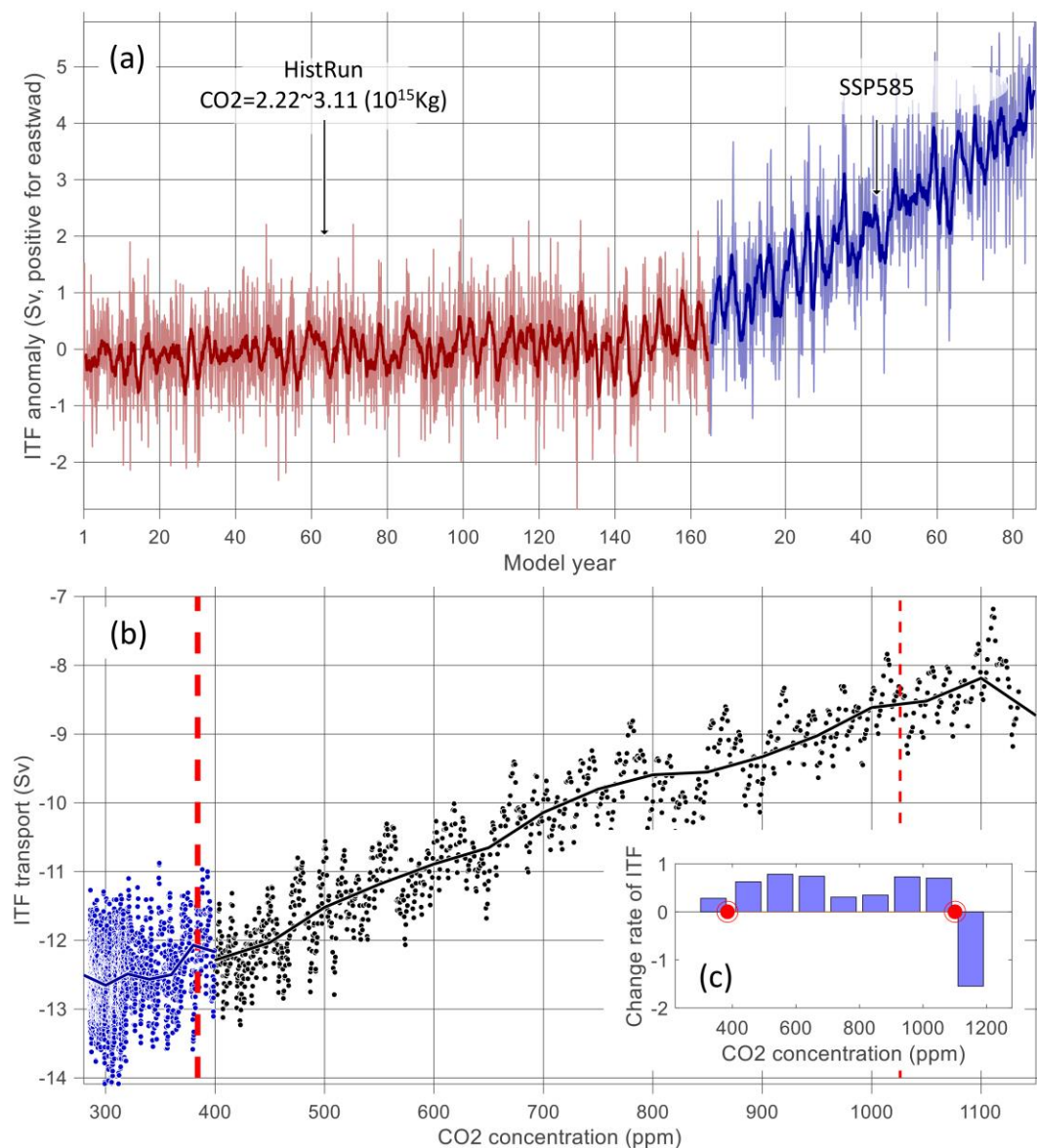


Figure RR5. ITF transport (Sv) in the Hist runs and SSP585 runs. Panel a presents the ITF transport anomaly in the Hist runs (red) and SSP585 runs (blue). Panel b presents the ITF transport as a function of CO₂ concentration (ppm) in the Hist runs and SSP585 runs. Panel c shows the rate of change of ITF transport ($\partial \text{ITF} / \partial \text{CO}_2$, units in Sv per 100 ppm) in the Hist runs (CO₂<400 ppm) and SSP585 (CO₂>400 ppm). The red dashed lines in panel (b) and the two red circles in panel (c) denote the two turning points, between which the ITF change rate is significantly positive (i.e., the ITF is decreasing). A new figure Supplementary Fig. S8 has been added to display the above results in the revised manuscript.

Lines 346-353: “What’s more, we have examined the ITF response to CO₂ in the Shared Socioeconomic Pathway (SSP) 585 as well through stitching the Hist runs to their corresponding SSP585 members. The ITF in the SSP585 scenarios shows an imminent rapid decline after the first turning point, the rate of change in ITF transport as a function of CO₂ concentration between the two turning points is much more rapid than before the first turning point or after the second turning point, indicating that the ITF response in the SSP585 is the same as that in the 1pctCO₂ experiments (Figure S8). However, regardless, it must be noted that these results are numerical simulations under ideal conditions and carry significant uncertainties.”.

Comment 6:

Nonetheless, the ITF response to the CO₂ in the ssp585 experiments is similar to that in the 1pctCO₂ experiments. Fig. R4. Left panels: Comparison of changes of the ITF velocity in ssp585 (a) and 1pctCO₂ (b) runs. Right panel: response of ITF to CO₂ concentration in the ssp585 experiments from 16 models (black dots for each model and black line for ensemble mean of all models). The solid red dot denotes the second turning point, while the dashed red line and red cycle denote the first turning point. As to the decline of ITF, some previous studies have shown that the CMIP models project a significant ITF decline under the business-as-usual emission scenario RCP8.5 (see Fig. R5 from Hu et al., 2015, Nature). As shown in Fig. R5, the ITF averaged over the 2050–2100 period under RCP8.5 is about 3 Sv weaker than the historical mean, indicating a future decline of ITF under RCP8.5. Under representative concentration pathways (RCP) 4.5 scenario, a robust ITF transport slowdown is found as well at depths below 100 to 1200 m (Santoso et al., 2022).” Hence, the decline of the ITF presented in our study is in agreement with the projection runs and previous studies that show a weakening trend of the ITF in the context of global warming.

I think this decline is uncontroversial.

Response: Thank you for your reply. We agree.

Comment 7 (summary):

“However the turning points and the rapid decline after the first turning point have not been reported previously. As to other radiatively active forcings, we agree that the CO₂ concentration is definitely not the only external forcing. But as you mentioned, the CO₂ concentration was the direct control variable in the 1pctCO₂ experiments, and this is the reason why 1pctCO₂ experiments are suitable for studying the response of ITF to greenhouse gas emissions. Our paper aims to investigate the response of ITF to the

CO2 concentration: the response to “all the other radiatively active forcing species”, which is obviously more complex, is beyond the scope of this study.”

As indicated above I agree that there is value in considering the idealised CO2 forcing. But I can't think of any reason to assume that the ITF will respond to CO2 in any way other than through radiative forcing. So for the real world, with multiple forcings, it is not possible to deduce that the ITF may be about to decline rapidly, based purely on these model results and the observed CO2 level (and an implicit assumption that the ITF is always in equilibrium with the forcing – see discussion of timescales above). This unjustified deduction is presented as a key result of the paper in both the discussion section (lines 265-268 of the revised manuscript)) and the abstract (lines 25-27) and is likely to detract from the interesting mechanistic analysis that the authors present. I strongly recommend that the authors carefully consider what conclusions can validly be inferred from their analysis, and remove other claims, or add some analysis to support them. It's an interesting study and doesn't need to be spiced up by unsupported and potentially misleading claims.

Response: Thank you for your positive feedback and valuable comments. As the primary greenhouse gas emitted by human activities, changes in CO₂ concentration primarily affect the Earth's climate system through the radiative balance and energy budget, as the reviewer points out. The ITF, as one component of the climate system, therefore responds to CO₂ indirectly—through adjustments such as the modified regional differences in warming and also through salinity changes. Therefore, when we state that CO₂ is the fundamental driver of long-term ITF changes, we do not exclude the possibility that direct causes may involve other dynamic or thermodynamic factors. However, at least in the CMIP6 model simulations, increasing CO₂ remains the root cause of the ITF's long-term evolution (please see e.g. revised Lines 343-347).

Based on the above analysis, we find that across multiple experiments—including HIST, SSP585, abrupt 4×CO₂, and 1pctCO₂ (each contains dozens of members)—the ITF consistently demonstrates a nonlinear response to CO₂, the existence of two turning points, and a significant weakening trend between them. While climate models inevitably have their limitations, the ones used here represent the current state-of-the-art in simulating the Earth's climate system. We strive to understand future climate changes and pursue perfectly accurate results, but this is the most advanced level we can achieve at present.

If we accept the numerical simulation results—particularly the existence of the two tipping points—while observational data simultaneously indicate that CO₂ concentrations have already reached or are approaching the first turning point, then we inevitably arrive at the conclusion that the ITF will soon undergo rapid decline. As we replied to the Reviewer#1 above, we have added a new figure, Supplementary Fig. S5,

to present the running trend of ITF in a 30-yr window in the 1pctCO₂ runs and historical runs to investigate the emerge time of the weakening trend of the ITF and also support that the first turning point is the time of emerging of weakening trend from its natural variability.

In response to your valuable suggestions, we have thoroughly revised the manuscript to explicitly address these uncertainties. We have made clear to readers that: (1) we are presenting a bold and potentially contentious inference; (2) our conclusions carry inherent limitations; and (3) this situation mirrors the intellectual risks confronted by many pioneering studies in their early stages of publication—where theoretical advances often precede definitive observational confirmation.

We have made the following changes in our manuscript to better explain our conclusions.

Lines 25-27: “The current level of CO₂ concentration has reached the turning point, indicating that a rapid decline of ITF and a transition Indo-Pacific state is likely imminent despite the remaining uncertainties.”

Lines 175-182: “To further examine the emerging time of the long-term trend of the ITF from its natural variability, we have calculated the running trends of the ITF transport over 30-year windows in both the CMIP historic runs and 1pctCO₂ experiments. As shown in Figure S5, the running trend in the Hist runs fluctuates within a range of roughly -2 to 2 Sv/century, which is a result of natural variability. By contrast, in the 1pctCO₂ experiments the 30-year running trend reaches 4 Sv/century rapidly, and the fluctuations are masked by the significant decreasing trend when the CO₂ reaches the first turning point, indicating that the first turning point is indeed the emerging time of a weakening ITF trend that is distinct from its natural variability.”.

Lines 342-353: “Therefore, when we state that CO₂ is the fundamental driver of long-term ITF changes, we do not exclude the possibility that direct causes may involve other dynamic or thermodynamic factors. However, at least in the CMIP6 model simulations, increasing CO₂ remains the root cause of the ITF's long-term evolution.

What’s more, we examined the ITF response to CO₂ in the Shared Socioeconomic Pathway (SSP) 585 as well through stitching the Hist runs to their corresponding SSP585 members. The ITF in the SSP585 scenarios shows an imminent rapid decline after the first turning point, the rate of change in ITF transport as a function of CO₂ concentration between the two turning points is much more rapid than before the first turning point or after the second turning point, indicating that the ITF response in the SSP585 is the same as that in the 1pctCO₂ experiments (Figure S8). However, regardless, it must be noted that these results are numerical simulations under ideal conditions and carry significant uncertainties.”

Point-by-point response to Reviewer#1

We thank the reviewers for their helpful suggestions and insightful comments. In the following, black indicates the comments by the reviewer; blue is our response to the reviewer; and changes in the manuscript are highlighted by red.

Reviewer #1 (Remarks to the Author):

The authors have addressed my previous comments, and the manuscript can be considered for publication in the journal.

Response: Thank you for your feedback.

Point-by-point response to Reviewer#3

We thank the reviewers for their helpful suggestions and insightful comments. In the following, black indicates the comments by the reviewer; blue is our response to the reviewer; and changes in the manuscript are highlighted by red.

Reviewer #3 (Remarks to the Author):

Review of “Imminent rapid decline of the Indonesian Throughflow after reaching a turning point of CO₂ concentration”, by Shijian Hu et al. Submitted to Nature Communications

This is my third review of this manuscript. I thank the authors for their careful responses to my last set of comments, including performing substantial additional analyses. I think these analyses, shown in new supplementary figures, shed further light on what is going on. Unfortunately I still find the framing of the results in the main text of the manuscript unconvincing, so I can't recommend publication in its current form. I do think there is a way forward to produce a highly impactful paper, but it would be more of a rewrite than a revision.

Response: Thank you for your comments and suggestions. We have adopted most of the suggestions you raised this time, such as enhancing the analysis of ensemble spread, and have further revised the manuscript accordingly. Specifically, we have made substantial modifications to Figures S4, S7, and S8. While we highly respect all of your comments, for a few points, after repeated analysis of extensive data, we have decided to maintain our original views. Below, we provide a point-by-point response to each of your comments.

First I have some comments on the new supplementary figures:

Fig. S4(a) (plot of ITF against GMST), This apparently shows an initial slow response of the ITF to increasing GMST. If real, to me this is more unexpected and more striking than the low-CO₂ response in the plot of ITF against CO₂ concentration (Fig. 2a), which as discussed in my earlier reviews might be expected due to the initial slow warming in the 1% runs. Presumably a slow ITF response at lower warming levels is due to lags in the system such as the multi-decadal adjustment time of the ITF to instantaneous radiative forcing (new Fig. S7). However as it stands I don't find Fig. S4 convincing. It shows no data for GMST <3.5 deg C. I would expect it to go right down to GMST=0, as there is plenty of data at these GMST values available in the 1pctCO₂ runs. The figure also needs an indication of the ensemble spread or confidence interval for the ensemble mean values shown. Because there is a large range in the Transient

Climate Response (TCR) between the models (see IPCC AR6 WGI Chapter 7), a given GMST value is likely to be reached at very different CO₂ concentrations between models, so a fairly large spread is to be expected. These additions are needed to convince the sceptical reader that the apparent slow initial response is real.

Response: Thank you very much for your comment. In particular, your suggestion regarding adding the ensemble spread or confidence interval for the ensemble mean values has been very helpful to us.

Yes, Fig. S4 (a) shows an initial slow response of the ITF to increasing GMST. However, as we discussed in the previous round of review, the response of ITF to GMST is inherently expected to be the same as its response to CO₂ because the GMST and CO₂ exhibit an almost linear relationship. Precisely for this reason, there is nothing unexpected in Fig. 4S—it is a reasonable result.

However, we do not believe that the slow ITF response at lower warming levels is due to lags in the system, for the following reason: the initial slow response of the ITF at lower warming levels is not only shown in the 1pctCO₂ experiment (Fig. S4a), but also shown in the Hist runs (Figs. S7 and S8a). Although this phase is confined to a relatively short segment of CO₂ levels, it is important to note that the corresponding duration of this brief CO₂ increase is quite long—particularly in the Hist runs, where this phase lasts for more than 150 years, far exceeding the time required for natural decadal adjustments.

Yes, there is no data shown for GMST <3.5 deg C. Please note that this is GMST rather than GMST anomaly relative to a certain value, so we would NOT expect it to go right down to GMST=0. Out of caution and with full respect for your opinion, we have calculated the minimum GMST (by and large equivalent to the initial value) for each model. As shown in Table R1, the minimum GMST for these models ranges from 12.6 to 14.1°C, with an average of 13.6°C. (We apologize that in the previous Fig4S, the calculation of the global mean GMST did not incorporate area-weighting to account for grid cell variations. This error has been corrected in the current version, but the correction does not affect the conclusions.)

Table R1. Minimum GMST (°C) in the 1pctCO₂ runs of various models

Model name	Minimum GMST	Model name	Minimum GMST	Model name	Minimum GMST
CESM2	13.6	CanESM5	13.3	MPI-ESM-1-2-HAM	13.5
CESM2-WACCM	13.5	CESM2-WACCM-FV2	14.0	MPI-ESM1-2-HR	13.8
IPSL-CM6A-LR	12.6	SAM0-UNICON	12.7	CMCC-ESM2	13.5
CMCC-CM2-SR5	13.7	ACCESS-ESM1-5	14.1	ACCESS-CM2	13.7
CAMS-CSM1-0	13.8	EC-Earth3-AerChem	13.8	EC-Earth3-CC	13.9

Following your suggestion regarding adding the ensemble spread, we have calculated the model spread (the standard deviations among the models), added this information to Fig.S4, and revised the Fig.S4. As you can see in the updated Fig.S4, the model spread (shading) is indeed large and should be noted. However, the overall trend is much greater than the model spread, no matter the ITF-GMST relationship or the GMST-CO2 relationship.

In addition, we have plotted the ITF anomaly (minus their corresponding mean values) in the three CO2 scenarios (1pctCO2, SSP585 and Hist) together (Fig.S4c). As we can see in Fig.S4c, these scenarios indicate similar ITF response to CO2 forcing.

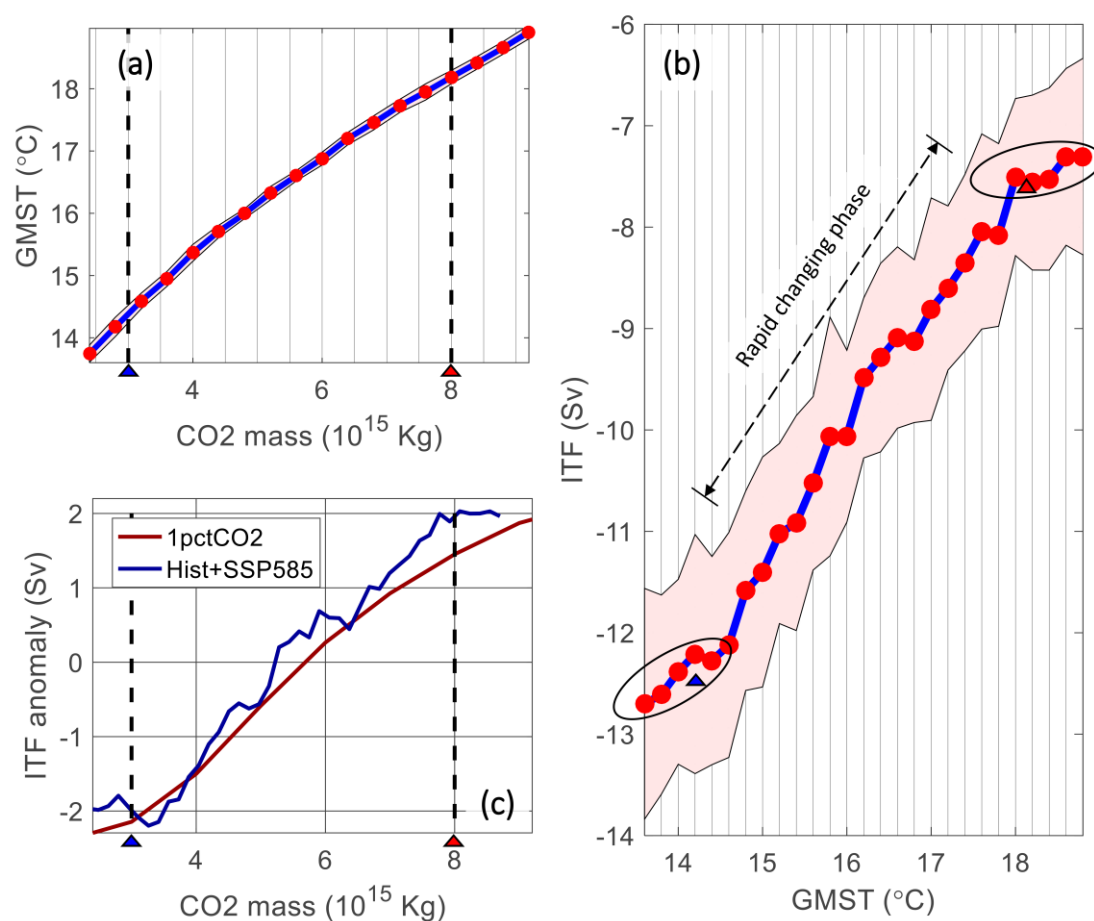


Figure R1 (revised Fig.S4). Relationships of: (a) global mean surface temperature (GMST) and CO2 mass, and (b) GMST and ITF. Panel (a) presents the GMST averaged over bins of CO2 mass with a bin width of 0.5 (10^{15} Kg). Panel (b) presents the ITF transport averaged over bins of GMST with a bin width of 0.2 $^{\circ}$ C. Panel (c) plots the ITF anomaly (minus their corresponding mean values) in the three CO2 scenarios (1pctCO2, SSP585 and Hist) together. The ITF, GMST and CO2 mass are ensemble mean of the 1pctCO2 and/or SSP585, Hist experiments of CMIP6. The triangles represent the rough position of the two turning points and the circled areas represent

the phases outside the two points. Seasonality is removed. The shaded color with thin black lines denotes the standard deviations among the models.

In the revised manuscript, we have calculated the model spread (the standard deviations among the models), revised the Fig.S4, and added discussion on the model spread in the ITF-GMST and GMST-CO2 relationship.

Please see updated Fig.S4, Lines 174–176: “The model spread in the simulated ITF-GMST relationship is notable, but the overall trend of ITF in response to GMST is much greater than the model spread (Figure S4b).”;

and Lines 177-180: “The ITF anomalies (minus their corresponding mean values) in the three CO2 scenarios (1pctCO2, SSP585 and Hist) are plotted together for comparison, and these scenarios indicate similar ITF response to CO2 forcing (Figure S4c).”.

Fig. S7 (Response of ITF in the instantaneous 4xCO2 runs). The response is very interesting here, showing that the (ensemble mean) ITF stabilises at ~6Sv after a few decades, even though the GMST is still warming at this point in most/all of the runs. This does provide evidence (more convincing evidence than Fig. 2a) that a levelling off of the ITF at a non-zero value can be expected at some future point. I think this behavior potentially sheds valuable light on the response dynamics of the ITF. A more in-depth process analysis to determine what sets the timescale and the stabilisation level would be very valuable, building on the insights to the wind and buoyancy forcings in the manuscript. To facilitate this it would be important to show how much the stabilisation time and level vary among the ensemble members/models.

Response: Thank you for your comments and suggestions.

Indeed, we also found the results of the 4xCO2 runs presented in Fig. S7 to be very interesting. This experiment clearly demonstrates that after an abrupt increase in CO2 from historical levels to 4xCO2, the ITF rapidly weakens. This weakening reaches a relatively stable equilibrium of a weaker ITF after approximately 40 years.

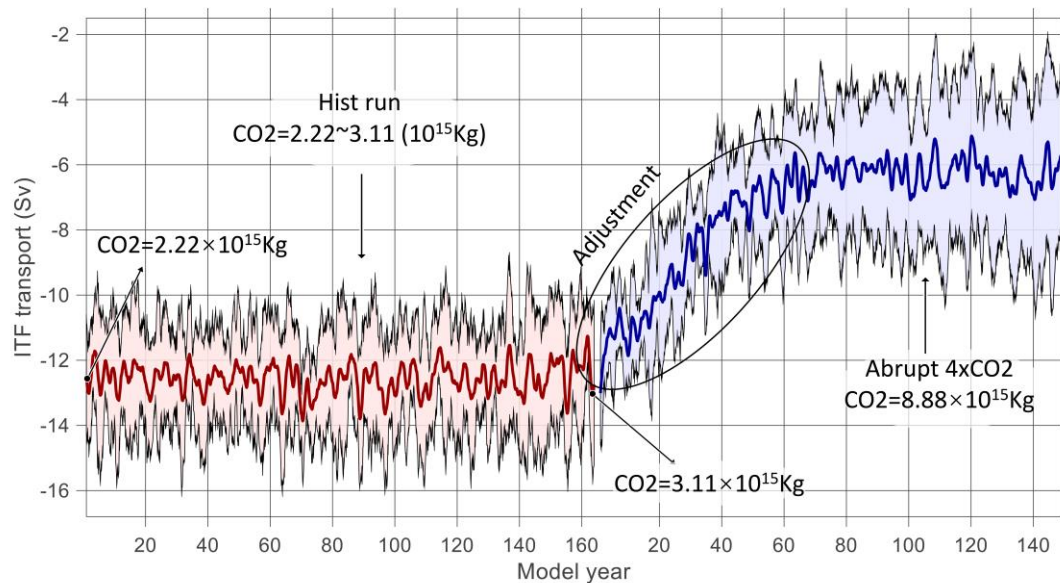


Figure R2 (revised Fig.S7). ITF in the Hist Runs (red lines, bold line for 13-month running mean) and abrupt 4xCO₂ experiments (blue lines, bold line for 13-month running mean). Note that the CO₂ in the 4xCO₂ experiments is constant. The shaded color with thin black lines denotes the standard deviations among the models.

Following your suggestion, we further analyzed the differences among various numerical models and updated Fig. S7 accordingly (i.e., Figure R2 here). As shown in the revised Fig. S7, during the rapid weakening of the ITF after the abrupt CO₂ increase, the differences among the models are minimal relative to other phases, indicating that different models undergo roughly the same adjustment period.

In the revised manuscript, we have updated Figure S7, integrated Fig. S7 with other results, such as Fig. 2a, to provide further discussion and analysis on this issue. As for what exactly determines the timescale of ITF adjustment, more systematic analysis and research will be needed in the future to reach a definitive conclusion.

The relevant revisions can be found in the revised Fig. S7 and Line 332-339: “We found that the adjustment of the ITF lasted for about 40 years and then the ITF reaches a dynamic stable state (Figure S7), indicating that the projected rate of ITF weakening in these models is also influenced by multidecadal variability. It should be noted that, during the rapid weakening of the ITF after the abrupt CO₂ increase in the 4×CO₂ runs, the differences among the models are minimal relative to other phases, indicating that different models undergo roughly the same adjustment period (Figure S7). The results from the 4×CO₂ experiment are consistent with and corroborate the findings of the second turning point observed in the 1pctCO₂ experiment (Figures 2 and S7).”.

Fig. S8 (ITF as a function of CO₂ in HIST and SSP585 runs). By eye I can’t see a clear discontinuity in the slope between the HIST and SSP585 segments. Given the large

ensemble spread (evident from the individual plots in the figure), and noise even in the ensemble mean, I doubt that there is any statistically or practically significant difference. The final drop in the timeseries (and in the rate of change in panel b) looks like an edge effect (noting that similar drops are seen at earlier points in the figure, e.g. around CO₂ = 650 and 850 ppm). To me, this figure does not as it stands support the assertion in the title of “imminent rapid decline of the ITF”.

Response: Thank you for your further comments.

Figure S8 combines the experimental results from the HIST and SSP585 scenarios. In the previous version of Figure S8, the discontinuity at the two turning points was indeed not particularly pronounced. As you noted in your comments, the lack of clear characterization at these turning points may be due to the absence of smoothing to filter out high-frequency variations, as was done in Fig. 2. Therefore, following your suggestion, we further revised Figure S8 by applying a 3-point smoothing process to the time series and replotted the figure (Fig. R3). As shown in Fig. R3a, after smoothing out the high-frequency variations, the ITF shows very significant differences near the two turning points: the ITF weakens rapidly between the two turning points but remains relatively stable and insensitive to CO₂ changes outside these points.

Regarding the model ensemble spread, we have previously presented and discussed the calculation results. While the ensemble spread is indeed not small, it is considerably smaller than the rapid weakening of the ITF between the two turning points. For example, the ensemble spread of ITF transport in the SSP585 experiment is approximately 3 Sv, whereas the change in ITF between the two turning points exceeds 4.5 Sv, significantly larger than the ensemble spread. Additionally, it is important to note that the ensemble spread primarily reflects differences in the phases of natural oscillations due to factors such as initial conditions, rather than differences in the response to external forcing. Nearly all models consistently show rapid weakening between the two turning points. Therefore, the observed ITF response is highly significant.

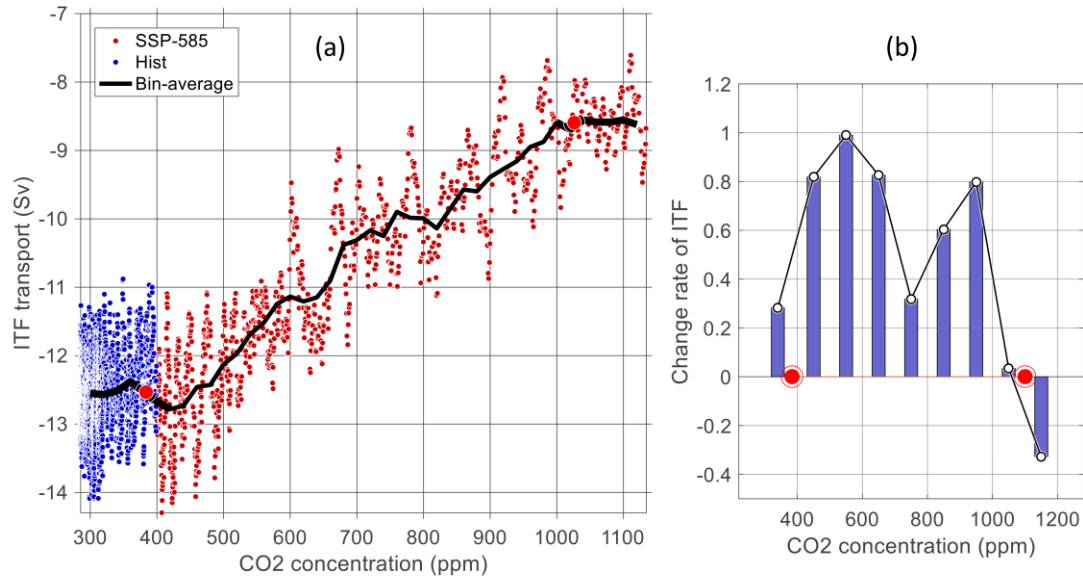


Figure R3 (revised Fig.S8). ITF transports in the Hist runs and SSP585 runs (a). The right panel presents the change rate of ITF transport as a function of CO2 concentration ($\partial\text{ITF}/\partial\text{CO}_2$, unit in Sv per 100 ppm). The red circles denote the two turning points, between which the ITF change rate is significantly positive (i.e., the ITF is decreasing). Note that the time series here has been subjected to a 3-point moving average to focus on the low-frequency response.

As for the final drop in the time series, as mentioned earlier, natural oscillations and fluctuations may indeed exist in the evolution of the ITF. Thus, weakening at other points in the time series, e.g., around $\text{CO}_2 = 650$ and 850 ppm, is not unexpected. However, this does not imply that the final drop is a "boundary effect." The reasons are as follows:

- (1) The duration of the final drop is significantly longer than other brief drops, especially after smoothing;
- (2) The stabilization after the second turning point is observed not only in SSP585 runs but also in the 1pctCO2 experiment, and is even more pronounced in the previously shown 4XCO2 experiment;
- (3) The calculated change rate of the ITF indicates that the final drop does not only occur in the last bin but begins to decrease to near zero even before the last bin, further demonstrating that it is not a boundary effect.

The consistency across multiple experimental datasets fully confirms the robustness of the final drop. We highly value your critical feedback. In the revised manuscript, we have followed your suggestions by smoothing the time series, evaluating inter-model differences, updating Figure S8, and discussing the robustness of the results and related findings.

In the revised manuscript, we have updated Fig. S8 and added corresponding texts. Please see the revised Fig. S8 and Lines 68 – 72 of the Supplementary Information: **“Supplementary Fig. S8. ITF transports in the Hist runs and SSP585 runs (a). The right panel presents the change rate of ITF transport as a function of CO2 concentration ($\partial\text{ITF}/\partial\text{CO}_2$, unit in Sv per 100 ppm). The red circles denote the two turning points, between which the ITF change rate is significantly positive (i.e., the ITF is decreasing). Note that the time series here has been subjected to a 3-point moving average to focus on the low-frequency response.”**

The new figures have started to unpack some potentially very interesting behavior, and have the potential to really move forward understanding of the future of the ITF. The evidence presented suggests that the response of the ITF to increasing CO2 is dependent on the history of CO2 forcing as well as the current concentration, due to the significant delay timescales of both surface temperature and the ITF itself. This means that conclusions drawn purely from analysis of the 1% CO2 runs are strongly contingent on that specific forcing scenario. It might be more convincing if the SSP585 and 1% timeseries could be plotted together, using consistent processing methods (e.g. it looks as if Figs 2a and S8 use different smoothing), and with a realistic estimate of uncertainty/ensemble spread.

Response: Thank you for your positive feedback and suggestions.

Indeed, after revising our work based on your comments, we have obtained more in-depth and interesting findings. Particularly in the 4×CO2 experiment, we observed how the ITF responds to rapid changes in CO2 and reaches a dynamic equilibrium after several decades of adjustment. This is of great significance for improving our understanding of the ITF’s response to external forcing. We also agree with your point that using both the 1pctCO2 experiment and other experiments for cross-validation helps enhance the credibility of our conclusions. At the same time, estimating the uncertainty and model ensemble spread is crucial for readers to evaluate the results.

Following your suggestion, as we mentioned in our previous response, we have integrated the 1pctCO2 experiment, historical runs, SSP585 experiment, and 4×CO2 experiment. We also applied similar smoothing to the SSP585 experiment and found that the ITF response in the SSP585 experiment (Fig. S8) aligns very well with that in the 1pctCO2 experiment (Fig. 2a), both in terms of the time series and the ITF change rate. Additionally, we assessed the uncertainty by calculating the model ensemble spread, which indicates that the rapid response of the ITF is significantly greater than the uncertainty and is thus statistically significant. For more details, please refer to our previous response.

Please see the revised Fig. S8 and Lines 68 – 72 of the Supplementary Information: **“Supplementary Fig. S8. ITF transports in the Hist runs and SSP585 runs (a). The right panel presents the change rate of ITF transport as a function of CO₂ concentration ($\partial\text{ITF}/\partial\text{CO}_2$, unit in Sv per 100 ppm). The red circles denote the two turning points, between which the ITF change rate is significantly positive (i.e., the ITF is decreasing). Note that the time series here has been subjected to a 3-point moving average to focus on the low-frequency response.”**

In short, I think the figures (including the new supplementary figures) contain a lot of valuable information about the response of the ITF to a warming climate, although some need a little more work to be convincing. The initial response seems to be a combination of more than one dynamical delay timescales, and so is particularly dependent on forcing scenario, while the later levelling off of the ITF response looks like a more robust feature. By combining the information on the response to different CO₂ forcing scenarios with the process analysis of the response (a strength of the manuscript) I think there is the potential to really move this topic forward. Unfortunately I still feel that the current framing of the paper in terms of instantaneous CO₂ concentration as a primary control on ITF strength, and the claim in the title of imminent rapid weakening, are unconvincing. I'm sorry I can't recommend publication in its present form. I think the authors have produced most of the analysis needed for a really valuable paper, with a reframing to focus on the processes and timescales that determine the transient ITF response. But it would be a rewrite rather than a revision of the current manuscript.

Response: Thank you for your comments.

Regarding the nonlinear response of the ITF to external CO₂ forcing, we generally agree with your perspective that the ITF exhibits relative stability in both the initial and later response phases. By integrating different CO₂ forcing scenarios, we can better characterize the response features and mechanisms of the ITF to CO₂. We also appreciate your remark that our work has the potential to "really move this topic forward."

However, as previously discussed in our earlier responses, the initial response of the ITF is *independent* of the specific forcing scenarios. We clearly observed that the adjustment period of the ITF to CO₂ changes is approximately 40 years, and the so-called "initial response" phase is prolonged—lasting over 150 years in historical runs, hence any dynamical delay timescales involved have already completed their adjustment. The response characteristics observed in the "initial response" phase in the ITF–CO₂ space correspond to fully adjusted outcomes in the ITF–time space. These represent the ITF's response to external forcing and show no dependence on the specific

CO2 increase pathways, as further supported by the consistency between the SSP585 and 1pctCO2 experiments.

Regarding the issue of treating CO2 concentration as the primary controlling factor of ITF strength, we have discussed this point in previous rounds of review as well. We would like to emphasize once again, in the numerical experiments we conducted, CO2 is indeed the dominant external forcing and the principal driver of climate change, therefore, attributing the ITF response to changes in external CO2 forcing is well justified.

As for the phrasing "imminent rapid weakening" in the title, we have also elaborated on this in earlier replies. **On one hand**, the two turning points in the ITF's response to CO2 have been consistently identified through multiple datasets and approaches. Current atmospheric CO2 levels are supported by observational evidence. Unless the theoretical or model-predicted turning points are invalidated, we cannot ignore the fact that current CO2 concentrations are approaching or are about to exceed the first turning point. Given the pronounced rapid weakening of the ITF beyond this point, it is reasonable to conclude that such a decline may be imminent. **On the other hand**, we have thoroughly addressed the associated uncertainties. In the title, we explicitly state that the "imminent rapid decline" is conditional—"after reaching a turning point of CO2 concentration". Moreover, throughout the article (in both the abstract and main text), we have repeatedly highlighted remaining uncertainties, referring to the phenomenon as "likely imminent despite the remaining uncertainties."

Once again, we thank you very much for your insightful comments and constructive suggestions.

Second review of “Imminent rapid decline of the Indonesian Throughflow after reaching a turning point of CO₂ concentration”, by Shijian Hu et al.
Submitted to *Nature Communications*

Thanks to the authors for their responses to my earlier review.

As indicated in my earlier review, I think the work in this paper is interesting and discusses an under-researched component of the climate system's response to increasing greenhouse gases. My main concern, as in my earlier review, is with the framing of the analysis in terms of CO₂ concentration, and the conclusions that the authors draw from that analysis, which I believe are misleading.

Unfortunately the authors' responses and text revisions haven't resolved these concerns, so my overall assessment is as before, that I don't feel able to recommend the paper for publication in its present form. I do think the investigation of the mechanisms of change is valuable, and I hope the authors will be able to reframe their work in response to the points in my earlier review (and the further comments below). I believe this will be in their interests. As it stands I think the issues raised around interpretation and framing will be immediately obvious to many climate scientists reading the paper, leading them to dismiss the whole paper and so miss the very worthwhile analysis of mechanisms.

I'm sorry I can't be more positive about the paper as it stands, but I emphasise that I do see some very valuable analysis here, and I hope these comments are constructive and ultimately helpful to the authors.

Detailed response to points raised in the authors' response to my first review:

“Response: First of all, please note that the ITF's response to CO₂ increases is not simply “a levelling off of the ITF weakening at higher concentrations”, but rather, a nonlinear process with a rapid weakening between two turning points and slow changes at concentrations lower than the first turning point or higher than the second turning point. Fig. R3. Response of global mean surface temperature to CO₂ (From Nicholls et al., 2020). Second, such a nonlinear response is not really expected and previous studies suggest that same levelling off would be NOT necessarily be seen in other climate variable such as global mean temperature. For example, Nicholls et al. (2020) reported the response of global mean surface temperature to CO₂ forcing using 1pctCO₂ simulations. As we can see in Fig.R3, the global mean temperature shows a linear increasing as the CO₂ increases and no turning points can be found in the response of global mean temperature to CO₂ increasing. So, the response of the ITF is very different from that of other 17 / 21 climate variable such as global mean temperature. It is really surprising to see the ITF shows such interesting and unique response to CO₂ increasing. Nonetheless, we agree with you that “there is no direct mechanistic link between CO₂ concentration and the ITF”. This does not mean that the CO₂ concentration is not essential to the ITF, but is simply because the Earth's climate system is super complex. As the primary external forcing driving long-term changes in the Earth system, rising greenhouse gas emissions affect all key processes and physical parameters within the system - and the ITF is no exception. CO₂ is one of the most representative greenhouse gases and an effective indicator of external forcing. Studying the ITF's response to the projected CO₂ increase helps predict the ITF future behavior and the impact that those changes might have on both the Indian and Pacific Oceans. Nicholls, Z. R. J., et al. (2020),

Reduced Complexity Model Intercomparison Project Phase 1: introduction and evaluation of global-mean temperature response, *Geosci. Model Dev.*, 13(11), 5175-5190, doi:10.5194/gmd-13-5175-2020.”

In the figure R3 provided (from Nicholls et al 2020), I see exactly the same qualitative response as a function of CO₂ concentration (or cumulative emissions which is used as the x-axis in that figure). There is an initial slow response (quite hard to see but clearly present in the MCE curves over the first 100 years), followed by a faster growth that gradually levels off over time (when plotted against cumulative emissions). This is just the same as Fig. 2a of the manuscript.

When plotted against time (approximately the same as plotting against radiative forcing, for the 1pctCO₂ runs) the trend looks initially slow, and thereafter linear, for both the ITF (Fig. 1a of this manuscript) and for GMST (Fig. 2b of Nicholls et al., or also see Geoffroy et al).

The timescales of this behaviour would not necessarily be the same for ITF and GMST, and the different behaviours of the thermal and haline components for the ITF (Figs. 1b and 1c of this paper) contain valuable information about these timescales and the mechanisms of the response. But the null hypothesis has to be that the response is broadly linear to radiative forcing (which is the physical driver of the changes). A simple way to demonstrate that some different inherent timescales are operating for the ITF compared to GMST, would be to plot ITF against GMST. It wouldn't surprise me if that was the case, but it needs to be demonstrated.

I agree that studying the idealised CO₂-only scenarios is a valuable contribution to understanding the response to more complex forcings. I have no problem with the choice of runs to analyse, just the interpretation of the results.

“Response: Thank you for raising this crucial point regarding the existence of both fast and slow response processes in the ocean system - this idea provides extremely valuable insights for understanding the ITF's response to CO₂ increasing! We agree with you that the nonlinear response of the ITF to CO₂ forcing might be related to the inherent timescale of the model dynamics. However, the relationship between the ITF and CO₂ level, i.e., $ITF = ITF(CO_2)$, is actually different from that between ITF and time, i.e., $ITF = ITF(t)$, when the CO₂ increases independently with time. In the time-domain relationship $ITF = ITF(t)$, the ITF's response, i.e., $\partial ITF / \partial t$, might be similar to that discussed by Geoffroy et al. (2013), when the changing rate of ITF is determined by the fast and slow response processes, and hence the initial slow response is attributed to the slow response process. By contrast, in the CO₂-domain relationship $ITF = ITF(CO_2)$, the changing rate of ITF, i.e., $\partial ITF / \partial CO_2$, is not related directly to time, and hence is not necessary directly associated with the fast and slow response processes. The CO₂-domain relation $ITF = ITF(CO_2)$ simply reflects the sensitivity of ITF to CO₂ changes.

It is simply not possible to make such a deduction from analysing a group of runs, all of which have the same CO₂ as a function of time.

We suppose that at any specific CO₂ level, the adjustment of the ITF might have been completed, i.e., the fast and slow response processes has been completed.

There's no evidence presented to support this, and as far as I can see no a priori reason to believe it. The initial adjustment timescale would be the slower of the inherent adjustment

timescale of the ITF, and the response timescale of the climate variables that force the ITF, to the forcing. The latter is hard to estimate for the 1pctCO₂ scenario. It would help to sort out the different timescales, to look at the instantaneous 4xCO₂ runs.

Actually, in the 1pctCO₂ experiment, the CO₂ increases 1% per year, indicating that the time from the first value for the CO₂ level to reach the first turning point is about 40 years, much longer than the known adjustment time scales of ITF (e.g., wind, Rossby waves, precipitation, deep ocean currents) according to previous studies (e.g., Sprintall et al., 2019). In the revised manuscript, we have added new sentences to discuss the potential role of the fast and slow response processes, previous studies including Geoffroy et al. (2013) are cited, and in particular, the role of the inherent timescale of the model dynamics in the initial slow response is discussed. Lines 309-315: “In addition, the response of global mean temperature to CO₂ perturbations shows a fast timescale of a decade or two followed by a slow timescale (Geoffroy et al., 2013). Even though the response of the ITF to CO₂ is expected to be different from that of global mean temperature, the fast and slow response processes might be important in determining the turning points, especially the initial slow response before the first turning point. Further in-depth research is still needed to understand the physical processes and mechanisms underlying these two turning points.” “

I'm not saying this new text is wrong (indeed I suspect it is right to some extent), but there is no evidence presented to support it. Again, plotting ITF against GMST would shed some light on this, as would plotting the ITF response in the instantaneous 4xCO₂ runs.

“Response: Following your suggestion, we examined the changes of the ITF in different projection runs. Fig.R4 presents a comparison of projected changes of ITF velocity between ssp585 and 1pctCO₂ runs. As you can see, the ITF changes in the ssp585 is quite similar to that in the 1pctCO₂ runs, and the weakening of ITF in the ssp585 is slightly stronger than that in the 1pctCO₂ runs. Most importantly, the ssp585 experiment shows a turning point at about 1026 ppm as well, after which the decline of ITF is much weaker than 19 / 21 between 400-1026 ppm.

Sorry, I simply don't see this in the figure provided. It just looks like a minor blip at just above 1000ppm, similar to others that appear in the curve.

Please note that the starting CO₂ concentration is about 400 ppm, which exceeds the first turning point already, so we are not able to examine the first turning point in the ssp585 experiments.

In that case, and there is a special point below 400ppm, why is there no evidence of any acceleration of the ITF decline in the HIST runs (Fig. 1c-e)? If you stitch the HIST runs to their corresponding SSP585 members do they show an imminent rapid ITF decline?

Nonetheless, the ITF response to the CO₂ in the ssp585 experiments is similar to that in the 1pctCO₂ experiments. Fig. R4. Left panels: Comparison of changes of the ITF velocity in ssp585 (a) and 1pctCO₂ (b) runs. Right panel: response of ITF to CO₂ concentration in the ssp585 experiments from 16 models (black dots for each model and black line for ensemble mean of all models). The solid red dot denotes the second turning point, while the dashed red line and red cycle denote the first turning point.

As to the decline of ITF, some previous studies have shown that the CMIP models project a significant ITF decline under the business-as-usual emission scenario RCP8.5 (see Fig. R5 from Hu et al., 2015, Nature). As shown in Fig. R5, the ITF averaged over the 2050–2100 period under RCP8.5 is about 3 Sv weaker than the historical mean, indicating a future decline of ITF under RCP8.5. Under representative concentration pathways (RCP) 4.5 scenario, a robust ITF transport slowdown is found as well at depths below 100 to 1200 m (Santoso et al., 2022)."

Hence, the decline of the ITF presented in our study is in agreement with the projection runs and previous studies that show a weakening trend of the ITF in the context of global warming.

I think this decline is uncontroversial.

However the turning points and the rapid decline after the first turning point have not been reported previously. As to other radiatively active forcings, we agree that the CO₂ concentration is definitely not the only external forcing. But as you mentioned, the CO₂ concentration was the direct control variable in the 1pctCO₂ experiments, and this is the reason why 1pctCO₂ experiments are suitable for studying the response of ITF to greenhouse gas emissions. Our paper aims to investigate the response of ITF to the CO₂ concentration: the response to "all the other radiatively active forcing species", which is obviously more complex, is beyond the scope of this study."

As indicated above I agree that there is value in considering the idealised CO₂ forcing. But I can't think of any reason to assume that the ITF will respond to CO₂ in any way other than through radiative forcing. So for the real world, with multiple forcings, it is not possible to deduce that the ITF may be about to decline rapidly, based purely on these model results and the observed CO₂ level (and an implicit assumption that the ITF is always in equilibrium with the forcing – see discussion of timescales above). This unjustified deduction is presented as a key result of the paper in both the discussion section (lines 265-268 of the revised manuscript)) and the abstract (lines 25-27) and is likely to detract from the interesting mechanistic analysis that the authors present. I strongly recommend that the authors carefully consider what conclusions can validly be inferred from their analysis, and remove other claims, or add some analysis to support them. It's an interesting study and doesn't need to be spiced up by unsupported and potentially misleading claims.