Response to reviewer

We would like to deeply thank the reviewer for constructive comments which greatly improve the manuscript. The followings are detailed response to each comment. Corresponding modifications are highlighted in the new submission.

To Reviewer 2,

1. It should be emphasized a little more that the switch to the KE stable state in 2017 is not wind-forced but is a response to an occurrence of the Kuroshio large meander, whereas the stable KE path during 2012−2015 results from wind stress forcing (Qiu et al., 2020). In other words, although the KE is in the stable state in both periods of 2012−2015 and 2018−2021, backgrounds are not necessarily the same.

Reference:

1. Qiu et al. (2020): On the reset of the wind-forced decadal Kuroshio Extension variability in late 2017. Journal of Climate, 3, 10813-10828. doi:10.1175/JCLI-D-20-0237.1

Response: Thanks. We would expect more detailed explanation here for readers' understanding (L77-82). We have modified the expression in introduction as "In August 2017, KE switched from an unstable state to a stable state in association with the occurrence of the Kuroshio large-meander (LM) path south of Japan (Figure 1), although negative SSH and MTD anomalies associated with the positive PDO phase were arriving from the central North Pacific (Qiu et al. 2020). Since then, Kuroshio LM and the stable state of the KE have lasted for more than six years (Qiu and Chen, 2021; Qiu et al., 2023; Usui, 2019), while the NPSTMW volume has declined (Oka et al., 2021). It is worth noting that the current stable KE state seems to have begun with the initiation of LM (Qiu et al., 2020), it has also been supported by basin-wide wind forcing (Qiu et al., 2023)."

2. An impact of wind-induced stratification changes on the STMW formation should be carefully considered. Figure 11a shows Pacific decadal oscillation (PDO) is positive during 2014−2017. This means that upper ocean stratification in the central North Pacific is intensified (i.e., shoaling of the main thermocline is caused). Such signals propagate westward as oceanic Rossby waves and affect the STMW formation region during 2018−2021, considering a 4-year lag suggested by a previous study (Sugimoto and Kako, 2016 cited in the present study). According to Sugimoto and Kako (2016), this may also influence the STMW formation decline during 2018−2021 by hindering deep convection and then development of winter Mixed layer.

Response: Thanks. The impact of wind-induced subsurface stratification changes (i.e., the changes of MTD) on the STMW formation is carefully considered in the section 3.5 of the new submission following the reviewer's the suggestion (L393-404): The NPSTMW volume variations in 2006-2009 (2012-2015) (Figure 4a) are closely controlled by the MTD because the PDO-related stronger (weaker) subsurface stratification from the seasonal thermocline to the main thermocline propagated from the central North Pacific (Figure 13). Meanwhile, Figure 16 also shows the distant effects of MTD (i.e., subsurface stratification) change on NPSTMW formation in the western North Pacific with some time lag (about 3-4 years). Although PDO is positive during 2014−2017, such signals of negative MTD, which propagates westward as oceanic Rossby waves, vanishingly affect the NPSTMW formation region due to the persisting strong sea surface warming (with positive SSH anomaly as its proxy) in NPSTMW formation region during 2018−2021 (Figure 13). However, since 2018, the PDO has transitioned from a positive to negative phase (Figure 13a), with the concomitant positive SSH anomaly and deepened MTD (Figures 13b and 13c). The positive MTD anomaly which weakens the background stratification should have facilitated the development of wintertime MLD and NPSTMW formation, as suggested as that in 2012-2015. On the contrary, the MLD and the subduction of NPSTMW do not have such sufficient developments in the late winter of 2018-2021 (Figures 8 and 9).

3. The authors focused mainly on formation/volume (and density) of STMW. I suggest that STMW temperature should also be intensively examined as well. According to previous studies (and Figure 13a in the present study), temperature of subducted STMW (i.e., summertime STMW) tends to increase (decrease) during the unstable (stable) KE when the winter mixed layer becomes relatively shallow (deep) (e.g., Oka et al., 2019 cited in the text; see their Figure S5). However, Figure 13a of the present study indicated different feature of STMW temperature between 2012−2015 and 2018−2021. This is also remarkable and is related to the interest of this study. Comprehensive analyses not limited to STMW formation/volume would give us much deeper understandings of STMW.

Response: We thank the reviewer's valuable comment. We examined the core layer potential temperature (CLT) of the NPSTMW using the ML heat budget analysis in section 3.4 of the new submission as well following reviewer's suggestion (L341-388).

4. Descriptions are too focused on periods of 2012−2015 and 2018−2021. Although I understand that the authors have an interest in STMW in 2018−2021 and compare it with that in 2012−2015, discussions can be started from overall features. Explanation with a wider view would be better.

Response: Thanks. Discussions with a wider view has also been added into sections of results following reviewer's suggestion below in the new submission (e.g., L229-236, L359-375).

5. I concern that STMW in 2018−2021 is compared with only one period of the KE stable state (2012−2015). Besides 2012−2015, the KE is in the stable state during 2003−2005. If the authors use another gridded dataset using Argo float data (MOAA-GPV; Hosoda et al., 2008) in addition to data by Roemmich and Gilson (2009), it is possible to further compare STMW formation/volume in 2018−2021 with another KE stable period. Although the number of Argo float profiles is not large in early-2000s, a *previous study discussed STMW using Argo float profiles since 2003 (Sugimoto et al., 2013).*

Reference:

1. Hosoda et al. (2008): A monthly mean dataset of global oceanic temperature and salinity derived from Argo Float observations. JAMSTEC Report Research and Development, 8, 47-59. doi:10.5918/jamstecr.8.47

2. Sugimoto et al. (2013): Marked freshening of North Pacific subtropical mode water in 2009 and 2010: influence of freshwater supply in the 2009 warm season. Geophysical Research Letters, 40, 3102-3105. doi:10.1002/grl.50600.

Response: Thanks. Following reviewer' suggestion, we have compared NPSTMW volume in 2018−2021 with that in 2003−2005 using MOAA-GPV dataset in section 3.1 of the new submission (L229-236, L253-258).

6. The authors pointed out importance of Ekman transport in the text. But Ekman heat advection is not discussed. It can be calculated from wind stress and temperature data, and further its effect may be considered in eq (5).

Response: Thanks. We have discussed Ekman heat advection in the new submission (L424-425): The horizontal heat advection rate by the Ekman velocity ($-\left(\mathbf{u}_e \cdot \nabla T_m\right)$) has correspondingly declined by 16% during 2018-2021 relative to 2012-2015. Through the ML heat budget analysis in section 3.4 (L359-369), we find: The temperature tendency in climatological average is counterbalanced by the air-sea heat exchange (41.2%), the vertical entrainment through the base of the ML (26.7%), the Ekman advection (8.2%), and the geostrophic advection (23.9%). Thus, the temporal variability of MLT in the cooling season is dominated by the anomalies of the air-sea heat exchange, the vertical entrainment and the geostrophic advection in climatological average. The contribution of the Ekman advection to the annual temporal variability of

MLT in the cooling season is so small. Finally, the effect of Ekman heat advection is not needed to considered in Eq (5).

7. Timeseries of winter NHF and heat flux due to Ekman transport in the STMW formation region is worth discussing and being compared with late-winter MLD and subduction volume.

Response: We thank the reviewer's valuable comment. We have added the ML heat budget analysis in section 3.4 and section 3.5 of the new submission (L370-388, L418-427, L434-440). Timeseries of winter Q_{net} and heat flux due to Ekman transport in the STMW formation region have been discussed and being compared with late-winter MLD and subduction volume.

8. How about to add horizontal bars or something to panels of timeseries in Figures 4, 9, 10, 11, and 13 in order to show periods of stable/unstable states of the KE? Further, in those panels, average values of 2012−2015 and 2018−2021 is better to be plotted to be compared easily because their difference is focused throughout the present study.

Response: Thanks. Modified following reviewer's suggestion (Figures 4, 9, 10, 13, and 16 in the new submission).

9. L12 and many places

"–" instead of "-".

Response: Thanks. Modified (e.g. L12 and so on).

10. L31.

To explain a role of STMW as the memory of atmospheric conditions, it would be better to mention the reemergence of sea surface temperature anomalies (e.g., Hanawa and

Sugimoto, 2004; Sugimoto and Hanawa, 2005).

Reference:

1. Hanawa and Sugimoto (2004): 'Reemergence' areas of winter sea surface temperature anomalies in the world's oceans. Geophysical Research Letters, 31, L10303. doi:10.1029/2004GL01990

2. Sugimoto and Hanawa (2005): Remote reemergence areas of winter sea surface temperature anomalies in the North Pacific, Geophysical Research Letters, 32, L01606. doi:10.1029/2004GL021410.

Response: Thanks. Modified following reviewer's suggestion (L34-38).

11. L59

"Cerovečki and Giglio (2016)" Instead of "(Cerovečki and Giglio, 2016)".

Response: Thanks. Modified (L65-66).

12. L70-73

Information written in the text is not up-to-date. The recent Kuroshio large meander persists for more than six years and is the longest record in the past 70 years (Qiu et al., 2023). Even if the present study would examine STMW before 2022, this should be written correctly.

Reference:

1. Qiu et al. (2023): Why did the 2017 Kuroshio large meander event become the longest in the past 70 years? Geophysical Research Letters, 50, 32023GL103548.

doi:10.1029/2023GL103548.

Response: Thanks. Modified (L79-80).

13. L83 (Figure 1 caption)

"Copernicus Marine Environment Monitoring Service (CMEMS)" instead of "CMEMS". Figure 1 is referred to at L70, but full name description of the CMEMS in the text is at L99.

Response: Thanks. Modified (L93).

14. L95, L158, L213 (Figure 6 caption), ...

Unify the abbreviation of net surface heat flux (HF or NHF).

Response: Thanks. Modified as Q_{net} (L108, L171, L435).

15. L103

"RG dataset" and "RG region" are so confusing. I suggest to use other words. Further, it is not appropriate that the abbreviation of RG region is used in the caption of Figure 2 (referred at L94) before it is defined in the main text (at L113).

Response: Thanks. We have checked abbreviations and used the abbreviation of RGA dataset and RG region in the new submission (L99). We have modified the abbreviation of RG region is used in the caption of Figure 2 (L117).

16. L141

Declare what is Q in the text explicitly.

Response: Thanks. We have declared as "the PV of subducted water is related to the subduction rate” in the text explicitly (L152).

17. L169-170

I cannot understand this sentence.

Response: We are sorry for the poor expression of this sentence in the original manuscript. We mean, “In a short time period of 2006-2009 when the KE jet is unstable, the NPSTMW volume has a dramatic decrease” (L211-212).

18. L199

Does the “the averaged area of the NPSTMW outcrop window” mean the “the averaged formation area of the NPSTMW”? If so, write so somewhere.

Response: Yes. Modified (L263-264).

19. L199

I felt that descriptions at L199-200 and at L207-208 contradict a little. The authors may want to improve the explanation.

Response: Thanks. We have removed the expression of sentence in L207-208 of the original manuscript.

20. L213, L240, ...

The word of “winter” and ”wintertime” should be defined uniquely in the text at the first reference. In the current manuscript, two definitions (January−March and February−March) exist.

Response: Thanks. The word of “winter” and ”wintertime” is defined as January−March in the text at the first reference (L120).

21. L221-L223

Show the value of transformation rates and/or their difference in the lighter NPSTMW range to support the result.

Response: Thanks. Modified (L281-283).

22. L230 (Figure 7 caption)

"(a), (c) Annually averaged surface transformation rate and ..." instead of "Annually averaged (a) and (b) surface transformation rate and".

Response: Thanks. Modified (L286-287).

23. L223-

Does it mean that the STMW formation is concentrated on its lighter density range? If so, write so somewhere explicitly. Further, explain why the STMW formation is concentrated on the lighter range.

Response: Thanks. The Figure 7 and discussion of this figure have been modified (L274-287).

24. L227-228

Need more explanation.

Response: Thanks. The Figure 7 and discussion of this figure have been modified (L274-287).

25. L242-245

The KE has shifted northward during 1993−2021 (e.g., Wu et al., 2021 cited in the manuscript; Kawakami et al., 2023). I suggest to discuss a relationship to the KE shift.

Reference:

1. Kawakami et al. (2023): Northward shift of the Kuroshio Extension during 1993−2021. Scientific Reports, 13, 16223. doi:10.1038/s41598-023-43009-w.

Response: Thanks. Modified (L300-303).

26. L254

Suggest to discuss the mean MLD distributions during winter of 2012−2015 and 2018−2021 rather than showing only their difference.

Response: Thanks. Modified (L312).

27. L264

Subduction volume depends on MLD probably not only 2018−2021 but also throughout the analysis period. Although I recognize that the main target of the present study is STMW formation in 2018−2021, how about start a discussion from an overall relationship between MLD and STMW formation by performing a correlation analysis or something else?

Response: Thanks. Modified (L320-330).

28. L276

"Figure 4b" instead of "Figure 2b".

Response: Thanks. Modified (L393).

29. L283

"Figures" instead of "Figure".

Response: Thanks. Modified (L404).

30. L285 (Figure 11)

It would be better to locate the panel (a) at the left-hand (or right-hand) side of panels (b) and (c) with rotation of 90º as done in Qiu et al. (2023) (mentioned above) so that readers can compare the PDO index and sea surface height (SSH) and main thermocline depth (MTD) anomalies easily.

Response: Thanks. Modified (L407).

31. L285

In panel (c), was the global-mean sea level rise due to freshwater flux and thermal expansion removed?

Response: Not removed in the original manuscript, but we have removed the steric height changes in the new submission (L197-207, L407).

32. L294

Specify where the central North Pacific is in Figure 12a.

Response: Thanks. Specified in Figure 14c (L428).

33. L301 (Figure 12)

The Order of panels are unnatural. Consider to rearrange as (a) 2012−2015, (b) 2018−2021, and (c) their differences. Further, sea level pressure (SLP) can be *superimposed by contours. SLP helps the readers to find the Aleutian Low discussed in the text.*

Response: Thanks. Modified (L428).

34. L313

Specify what is stratification anomaly. How was it calculated?

Response: Thanks. The values of stratification anomaly are relative to the 2004−2021 climatology (L448). It is calculated from $N^2 = -\frac{g}{\rho}\frac{\partial \sigma_\theta}{\partial z}$.