Response to referee #2, 2nd round:

Overall the paper has much improved. I think it is fine to publish this content.

Thanks very much to the reviewer for their re-review and comments. Our responses and corresponding revisions to the text are given below in red text. Line numbers refer to the "tracked changes" version of the manuscript, and differ from the version without tracked changes.

Some comments:

L20 – "Along frontal gradients" is jargon that I would not use to open a paper with. Maybe rewrite this sentence and be clear what this actually is. Specially the combination along and frontal.

The first sentence of the introduction has been rewritten to use more general wording:

"In regions of the ocean where waters of different temperatures and densities converge, instabilities form that transport heat across latitude lines." (lines 20-21)

L198 – You mean the surface middle of the ocean, not the "interior" or Abyss, right? Please clarify.

Yes, this is correct. The sentence has been rewritten to be more specific:

"We note that the significant low EKE bias in the more quiescent middle regions of the ocean (Figure 1) implies that MTF may be underestimated in these regions, especially at low latitudes." (lines 201-202)

L255 – Maybe tone down this statement. Mesoscale is locally important perhaps, but it seems overrated here.

We see no reason to tone down the statement given the results that are shown in Figure 7. The mesoscale contribution to meridional HT is of comparable magnitude to the overturning and large-scale contributions near 40° latitude in both hemispheres. The statement has been slightly revised to specify the latitude range that is being discussed:

"This suggests that the mesoscale plays an important part in conveying poleward meridional HT from the subtropical to subpolar gyres in the Northern Hemisphere near 40°N, and across the equatorward edges of the Southern Ocean near 40°S." (lines 257-259)

L345 The diffusivity is positive! It can operate on downgradient fluxes, hence the minus sign.

Since it is more commonly used, we will adopt the convention where the diffusivity is positive in most cases (i.e., when there is a downgradient flux). Equations (12)-(13) and the text have been changed accordingly.

L345 – This is very rough, as you are ignoring the east-west velocity. How does this affect the conclusions in this paragraph? These are important in many regions.

There is an assumption implicit in equation (13) that the standard deviations of the zonal and meridional velocity are comparable; if so then $\sqrt{v'^2} \approx \sqrt{\frac{1}{2}\left(u'^2 + v'^2\right)} = \sqrt{EKE}$. This assumption has been stated explicitly in the revised text:

"(The approximation in eq. 13 assumes that $u'^2 \approx v'^2$, which is valid where dynamics are generally isotropic, but not where flows are strongly asymmetric and nearly aligned with the zonal or meridional axis.)" (lines 351-352)

L365 – If you want to address the effect of eddies this way, perhaps it is better to use the maps of diffusivities that include suppression effects? Examples are Klocker and Abernathey 2014 or Groeskamp et al (2020). Here MLT is used, together with suppression and this could be a better proxy.

The reviewer makes a good point, that maps of the suppression factor might also provide insight as to where suppression effects would interfere with the EKE-MTF relationship. A mention of this has been added to the text:

"Moreover, strong velocity jets tend to suppress diffusivity across fronts by reducing the mixing length (Ferrari and Nikurashin, 2010); the suppression effect of the flow is pronounced in many of the same energetic regions where EKE is a poor proxy for MTF (Klocker and Abernathey, 2014; Groeskamp et al., 2020)." (lines 369-372)

L389 – "The effects of these recirculations are not included in a temporally-defined eddy flux, but these recirculations still need to be parameterized as they will not be accurately simulated in a coarse-resolution" I'm not sure if you can make this statement with such certainty based on what we have seen. Maybe you have presented the arguments, if so, I suggest to provide them here with exact references to figures and section in this paper, and preferably other literature. I'm sure this is not the first time this is mentioned. This, because it is a big statement to make. If correct, potentially very important. Which would be a nice outcome of the paper.

This is in fact a major conclusion of the paper, and we think that the evidence from Figures 5 and 6 supports the conclusion that it is essential to simulate mesoscale structures to accurately represent basin-scale (and global) meridional heat transports. We have stated this more specifically in the conclusion section of the paper.

“Moreover, stationary mesoscale recirculations associated with western boundary currents contribute substantially to mesoscale meridional HT (e.g., the Kuroshio and Agulhas as shown in Figure 5). The effects of stationary recirculations are not included in a temporally-defined eddy flux, as these recirculations are part of the time-mean circulation. However, a coarse-resolution model that does not accurately simulate temperature and velocity gradients at western boundaries (Figure 6) may neglect contributions to the meridional HT that are >0.1 PW, even if the model accurately represents the large-scale flow and mass transport. Therefore, the effects of these stationary recirculations still need to be parameterized just as the effects of transient eddies are parameterized.” (lines 393-400)

Response to referee #3, 2nd round:

Thanks very much to the reviewer for their re-review and comments. Our responses and corresponding revisions to the text are given below in red text. Line numbers refer to the "tracked changes" version of the manuscript, and differ from the version without tracked changes.

The authors did a great job streamlining the paper and addressing concerns raised by the reviewers. I find
it much easier to follow now and I think the paper can be published as it is, if the following very minor
concern about the mixing length equations is addressed:
- Equation (12) $MTF \approx -\kappa(\partial T/\partial y)$

And remove the "which is generally negative or downgradient" (line 347). Diffusivity is per definition positive, a negative value of κ would indicate something going from a diffuse state to a
concentrated state, which physically makes no sense. The negative sign "belongs" to the temperature
gradient, indicating that the flux is downgradient.

Equations (12)-(13) have been changed so that the diffusivity is positive when there is a downgradient flux.

- Equation (13) $\kappa \propto \sqrt{v'2}\ Lmix \propto \sqrt{EKE}\ Lmix$
Since $\sqrt{v'2} \neq \sqrt{EKE}$, but they are proportional with a factor of $\sqrt{2}$.

The factor of $\sqrt{2}$ is actually not necessary; if the assumption is made that the variances of u'^2 and v'^2 are comparable (as they would be in generally isotropic turbulence), then $\sqrt{v'^2} \approx \sqrt{\frac{1}{2}\left(u'^2 + v'^2\right)} = \sqrt{\mathrm{EKE}}$. This assumption has been explicitly stated in the revised text.

"(The approximation in eq. 13 assumes that $u'^2 \approx v'^2$, which is valid where dynamics are generally isotropic, but not where flows are strongly asymmetric and nearly aligned with the zonal or meridional axis.)" (lines 351-352)

One major concern raised by reviewer#1 and me was about the underestimation of EKE in the model. I think the Appendix B addresses this problem adequately regarding the analysis of this paper and shows that the results are not overly sensitive to the low EKE. However, for the future it might be reasonable to analyze why the POP2 model (contrary to other ocean models with comparable horizontal resolution) underestimates EKE so substantially and to what extend this influences results from analyzing output of this specific ocean model.

We agree that this is an important question, though beyond the scope of our paper, and there is no clear answer known to us. Other high-resolution "eddy-permitting" models also have a low EKE bias, though the bias in this POP2 simulation is particularly pronounced. The bias is fairly pervasive in the middle of ocean basins, but generally limited to areas where mesoscale energy is low or moderate (quiescent regions). Some of the bias can be attributed to the misplacement of currents (e.g., the Azores Current, the absence of the Northwest Corner in the North Atlantic Current, the Azores Current). Another possible reason is that grid-scale (biharmonic) viscosity and diffusivity values are set for optimum behavior in energetic current regions, but that these values are not ideal for more quiescent regions and tend to suppress instability development too much. As stated in Section 2.1:

"One possible explanation is that the biharmonic viscosity ν_0 = -2.7 x 10^{10} m^4 s^{-1} and diffusivity κ_0 = -3 x 10^9 m^4 s^{-1} values used in this model simulation are well tuned for energetic regions such as western boundary currents, but may suppress too much mesoscale activity in the less energetic ocean interiors. However, the low bias in time-mean EKE is not unique to this eddy-permitting model (e.g., Volkov et al., 2008, 2010; Tréguier et al., 2012)." (lines 95-99)