Response to review comment #1:

this paper is interesting and deals with an important subject

it is well written and well analyzed

my main concerns unfortunately pertain to the bases of the methodology

Thanks very much to the reviewer for their comments. We have described our revisions to the manuscript to address these comments below. Note: The responses to each of the comments are given in red text. Line numbers in the responses refer to the "tracked changes" version of the revised manuscript, so the line numbers are different in the version without tracked changes.

1) it is extremely rate that a high resolution numerical model hasa lower EKE level than AVISO (here 0.1 deg model for 0.25 deg altimetry)

this casts a serious doubt on the results of the paper if the level of mesoscale turbulence is not well represented (then the heat fluxes may have a major bias)

It is not uncommon for surface EKE to be underrepresented in models compared to satellite observations, even in "eddy-permitting models" such as 1/10° POP. As noted in the revised text (line numbers 106-107):

"However, the low bias in time-mean EKE is not unique to this eddy-permitting model (e.g., Volkov et al. 2008, 2010)."

This happens because even though the numerical model has a higher-resolution grid spacing than the observational data product, the model may not actually resolve more dynamics. We would only expect to see the model simulate phenomena which have wavelengths encompassing many grid points.

To address (in part) the concern about the low EKE bias in the model, we have constructed alternate estimates of time-mean temperature fluxes and heat transport (Appendix B) by averaging fluxes only during times when the zonally-smoothed EKE distribution is higher, such that the mean EKE during "composite" time periods closely resembles observed EKE (Figure B1). It is found that the change in meridional heat transport from this adjustment (Figure B3) is not that substantial, probably because most of the low bias in EKE occurs in quiescent parts of the ocean that are contributing relatively small mesoscale temperature fluxes.

We also note that many of our key conclusions are derived from regions where the model EKE is not seriously impacted by this bias (the Southern Ocean, most western boundary currents). Most of the fluxes associated with the stationary mesoscale circulation (Figure 3) occur in these areas where the model EKE is realistic; these fluxes are substantial, even though temporal definitions of the "eddy" flux exclude them. This is mentioned in the revised conclusions (Section 6):

"While POP simulates EKE well in the Southern Ocean and western boundary currents where MTF is substantial, the model has a low EKE bias in some subtropical and tropical regions (Figure 1). Hence the spatial decomposition method should be applied in models that simulate mesoscale activity and variability more accurately in these regions." (line numbers 569-571)

2) using lambda_0 = 10 deg to separate large scale from mesoscale is in my opinion, really large (e.g. compared to the first internal radius of deformation or to the Rhines scale at say 15N) - at 15N-30N this leads to lambda0=800-1000km with lambda0/2=D the eddy diameters = 400-500 km which is really large

First of all, the 10° scale threshold we chose is meant to represent wavelength threshold, which corresponds to a 2.5° radius threshold. We agree that this is still a factor of two larger than the typical (dominant) eddy scales (e.g., Chelton et al. 2011). We chose this threshold intentionally to minimize leakage of large-eddy signals into large scales. In early uses of this methodology we tried a threshold that was closer to typical eddy scales, but some of the variability associated with larger eddies was leaked into large scale v and T profiles. It can be seen in the spectra of meridional velocity (Appendix A, Figure A1) that the mesoscale peak is fairly broad. A typical eddy radius of 120 km at 20° latitude (Chelton et al. 2011 figure 12) corresponds to a wavelength of 480 km or 4°-5° longitude; however the velocity spectra show that variance remains elevated from the mesoscale peak (at ~4° wavelength) to almost double this scale (panel e in the spectra plot). A separation scale of 10° longitude (and larger near the equator) therefore assures that larger eddies will still be classified as mesoscale, and be treated distinctly from gyre-scale motions and long planetary waves.

See also this quote added to Appendix A:

"The wavelength (not radius) lambda_0 was deliberately chosen to be larger than the size of nearly all eddies, in order to ensure that their signals are retained in the mesoscale. For instance, an eddy radius of 1° longitude and diameter of 2° is typical at mid-latitudes (Chelton et al. 2011), corresponding to a wavelength of 4° longitude. If lambda_0 at mid-latitudes is chosen to be 4° or 5° longitude, much of the signal of larger eddies will be included in the large-scale profiles along with basin gyres and long planetary waves. Hence a choice of lambda_0 = 10° results in a cleaner separation of these phenomena." (line numbers 601-605)

therefore I advise to reconsider these two aspects