Many thanks for these additional comments, we appreciate the time and effort you have put in to improve this manuscript.

Reviewer 1

Minor comments:

- line 10: "found to vary from 0.25 to 4 times"?

  > *Response:*
  > Done.

- line 23: being picky here, there is no eddy dynamics as such in a non-eddy resolving model, and "eddy feedback" onto the mean state or similar is really what is being meant

  > *Response:*
  > Modified.

- line 25: capital W in "McWilliams". In bibtex, can do this by author = {P. R. Gent and J. C. {McWilliams}}

  > *Response:*
  > Fixed.

- line 30, 31, eq(1): inconsistency with kappa or kappa_gm, choose one (probably latter)

  > *Response:*
  > Sorted.

- line 50 (and line 72-73): either acronym used before being defined (ACC + AMOC), or be consistent and use AMOC in lin 72-73 (probably the former)

  > *Response:*
  > Sorted.

- line 50: could have a reference to Marshall et al (2017) for the mechanism that increasing eddy dissipation increases transport

  > *Response:*
  > Added prior to Mak et al. (2022b).

- line 55: "numerical optimisation" could mean optimising a numerical model, which is definitely not what was done in that paper. "Inverse method" or "PDE-constrained optimisation" would be more unambiguous

> *Response:*
> Added.

- eq 3: no comma

> *Response:*
> Done.

- line 153: Wunsch (1997) to be more specific and consistent with prose style.

> *Response:*
> Done.

- line 250: would think that "close to zero" is sufficient. I guess "close to, but not, zero" would be ok, but that seems unnecessarily jumpy

> *Response:*
> Done.

- line 252: consider "background wind; see Wilder et al (2022)" since the two clauses are closely related

> *Response:*
> Done.

- line 291: "We first focus on the first 150 days"

> *Response:*
> Done.

- line 394: full stop at end of sentence

> *Response:*
> Thanks.

- line 404: unnecessary dash after 0 degrees

> *Response:*
> Fixed.

- bib: the Gent and McWilliams reference typesetting as above

Response:
Fixed.

Many thanks for these additional comments, we appreciate the time and effort you have put in to improve this manuscript.

Reviewer 2

Minor comments

1. Line 25: it is not correct to say that the GM scheme "advects tracers downgradient". In fact, for most tracers there is no connection between the GM advective velocity and the tracer concentration. I noticed this issue when I reviewed the first version of the manuscript, but somehow, it failed to make my list of comments...

   Response:
   Thanks. Eddy fluxes are downgradient! I have rephrased that line.

2. Lines 70 and 392: "killing" is a strong word. Can something less emotive be used? How about "dissipation"?

   Response:
   I think 'killing' is okay in both of these contexts. The paper by Rai is entitled 'Scale of oceanic eddy killing by wind from global satellite observations'.

3. Line 76: I suggest replacing "its use in this" by "the" – much simpler!

   Response:
   Done.

4. Equation (6): The second term in the brackets on the right hand side is missing a minus sign.

   Response:
   Thanks. Corrected.

5. Line 141: I do not understand how there can be gains due to negative work due to R that will be cancelled out by positive work. This is because, from Figure 2b, the wind work difference is always negative? I would delete the text in this sentence that begins with "whereby".

   Response:
   Done.

6. Are the figures shown in Figure 2 computed using the approximation given by (8), or are they exact? One could clarify this in the caption.

> *Response:*
> We use the full expression for this. This is now clarified in the figure caption.

7. Line 144: Surely there are more fundamental (earlier) references that can be given here, notably Duhaut and Straub (2006) and Zhai and Greatbatch (2007)?

> *Response:*
> Have inserted Zhai and Greatbatch (2007).

8. Line 157: I am not comfortable with the word "singular". How about "In this work, we proceed, for simplicity, by representing an eddy using only the first baroclinic mode".

> *Response:*
> Changed.

9. Line 184: I do not like the wording immediately after equation (15). After all, it is not that solutions to (15) can only be found in the limit g'/g -> 0 but rather that the solutions you give are only valid in that limit. I suggest to delete the text that completes the sentence immediately after (15) and, instead, change the sentence that starts on line 185 to. "In the limit g'/g -> 0, the BT is described by etc...".

> *Response:*
> Thanks. Changed.

10. Equation (19): Somewhere you need to say that z is measured positive downward.

> *Response:*
> Done.

11. Line 232: It is not true that gamma governs the stratification of the water column. In fact, it only influences the stratification within the eddy, and it is only a modifiying influence because the basic stratification is set by T_ref.

> *Response:*
> Yep, you are right. We will say 'influencing' instead.

12. Lines 232-233: In general, H_1 is not the point of zero crossing for horizontal velocities. This is only going to be the case in special situations, as is acknowledged on line 244. I do not think any mention of the zero crossing should be given on lines 232-233. Rather, the issue should be discussed in the paragraph beginning on line 243.

> *Response:*
> That makes sense. Text has been appropriately modified.

13. Lines 236-237: I do not see how delta rho can be the density difference between the top and the bottom of the ocean, not least because there is also a contribution from the first term on the right hand side of equation (20).

> *Response:*
> Perhaps this is a bit confusing. Have removed the brackets from N^2(z/g). The first term is a linear profile, and the second term gives the shape of an upper and lower layer profile with a thermocline.

14. Lines 253-254: One could give a reference for the drag coefficient that is used. Is it one of the standard formulae, such as given by Large and Pond?

> *Response:*
> Done.

15. Lines 257-259: I can understand running the model for 10 days to allow the geostrophic adjustment to complete. But what about when the wind is switched on? This will also generate inertial oscillations?

> *Response:*
> Yes, you are right. The wind speed of 7 m/s is moderate so the extent of the inertial oscillations is minimal. The model has been tuned appropriately to deal with this.

16. Equation (26): P_rel, as given by equation (11) is negative. So, I guess P_rel here is actually the modulus of P_rel?

> *Response:*
> Yep. Thanks for pointing that out.

17. Line 339: It would help to refer to equations (11) and (27).

> *Response:*
> Done.

18. Equation (29): It is worth pointing out that in the limit H_1/H_2 -> 0, mu -> g'/g.

> *Response:*
> Have modified text.

19. Equation (31): Strictly speaking the boundary condition is (1/N^2)d phi/dz = 0. This can become important because N^2 typically tends to zero in the surface mixed layer and, potentially also, in the bottom boundary layer.

> *Response:*
> That's a good point. Have added this.

20. Lines 370-371: I assume H here is the local depth and so varies in space?

> *Response:*
> Added 'and so varies in space'.

21. Lines 373-374: Better to write "and locations where the ocean depth is shallower than 300m are not considered".

> *Response:*
> Changed.

22. Line 374: "like" -> "by".

> *Response:*
> Done.

23. Line 384: "slower" -> "lower".

> *Response:*
> Done.

24. Line 393: Better to write "The region between 5 S and 5 N has etc..."

> *Response:*
> Done.

25. Line 408: Is "reductions" correct? Both R_d and |u_a| increase offshore, with opposing effect on the dissipation rate?

> *Response:*
> Thanks. Have said 'most likely due to an increase in R_d and reduction in |u_a|'

26. Line 410: To write "display weaker eddy energy" begs the question "weaker than what"? I guess weaker than in a high resolution ocean-only simulation? However, this needs to be clearly stated. I suggest the text on this topic, including the following line, is revised.

> *Response:*
> Have reworded this bit because I think it may have been confusing.

27. Line 421: "does not vary" -> "are similar" and earlier on this line "pattern" -> "patterns"
    .

> *Response:*
> Changed to 'We also see that the pattern is similar to that seen in Lambda_{rel}(R_d) (Fig. 8a,b), particularly across the North West Atlantic and Southern Ocean. This is clearly because the spatial pattern of the chosen eddy length scale (R_d or (L_e/hat{Rd})R_d) are similar, since (L_e/hat{Rd})R_d depends on R_d.'

28. Line 423: How is hat{lon} defined?

> *Response:*
> Defined.

29. Line 445: Should "winds" be "relative winds"?

> *Response:*
> Changed.

30. Line 486: Better to say that surface modes can be computed over variable topography and that the horizontal velocities at the bottom tend to be smaller than if a flat bottom is assumed (they are not always zero). My own view on this is that using the standard modes, as done by the authors, is more straightforward and is to be preferred.

> *Response:*
> Thanks. Updated text.