Ocean Sci. Discuss.,
doi:10.5194/os-2016-3-RC2, 2016

Ocean Science
Discussions
Open Access
EGU

Interactive comment on "Dissipation of the energy imparted by mid-latitude storms in the Southern Ocean" *by* J. Jouanno et al.

Anonymous Referee #2

Received and published: 18 February 2016

The authors set out to examine the dissipation of near inertial energy imparted by mid-latitude storms in the Southern Ocean. Idealized experiments are designed and utilized that seem well suited for this examination. The analysis is well designed and leads to conclusions on the generation and fate of near inertial energy that are not physically surprising. The influence of Southern Ocean storms on the MOC in models is certainly new. Further, the influence of model parameters is novel and important for other models of the Southern Ocean to consider. The work in this manuscript is well thought out and will make an excellent addition to the Southern Ocean literature. My comments, for the most part are minor, but I offer a few thoughts and possible additions for the authors.

The influence of Southern Ocean storms on MOC are certainly interesting. They seem to be in partial agreement with the observational results of Hogg et al. (JGR, 2015) who

Printer-friendly version

Discussion paper

examined the response of EKE and MOC to increases in southern ocean winds. Given that Figure 7 represents a 10 year average, the average storm influence is behaving a bit like a somewhat uniform increase in winds. In fact, I wonder if you may find similar MOC results for an experiment where the wind stress was uniformly increased everywhere since the storm tracks may average out over the 10 year cycle to a *roughly* uniform increase in the domain. Either way, it seems that your results are consistent and you might consider referencing Hogg et al. On the other side, a recent paper by Gent (2016, Ann. Reviews) suggests that southern ocean winds cannot be a primary driver of MOC due to eddy compensation. I think placing your results in the context of these two types of results would be useful.

Expanding on a comment from the other referee, I think the details of your momentum transfer parameterization are important. In addition to inclusion of the moving ocean, I think the role of wave generation is important. Some of the momentum input due to storms will go into wave generation and not directly into the local currents. The waves may break far from the cyclone thus changing the local dissipation profiles (see for example Suzuki et al 2014, JPO or Curcic 2015, UMiami Dissertation). Further, wave mean flow interactions (e.g. langmuir cells) may dramatically modify local dissipation. This result may be difficult to tease out as the wave drift relative to currents will rotate 180 degrees as the storm passes over (e.g. Sullivan et al 2012, JPO). No further experiments are necessary, but a short discussion on possible effects would be interesting.

In a few places in the manuscript the submesoscales are referenced. For example, section 3 seems to suggest that the submesoscales are for $\lambda < 60km$. This seems incredibly large. The first rossby radius of deformation computed from a model configuration not unlike that presented here (computed via the method in Chelton et al 1998, JPO) is 10km near the ACC implying that the submesoscales are even smaller. Using scalings from Fox-Kemper et al (2008, JPO) submesoscales in the ACC are roughly 2 km, suggesting your 1km results are borderline submesoscale permitting. This suggests that the shallowing of the mixed layer noted in section 5 is most likely not due to the submesoscales.

In your paper you note the importance of resolving mixed layer dynamics (pg 15), yet you never discuss or test the sensitivity of your results to different mixed layer parameterizations. I would expect that accurate representation of mixed layer processes will have an impact on your results. For example, the GLS scheme could be used with different parameters to produce $\kappa - \omega$ or $\kappa - l$ schemes. Further, to the best of my knowledge the GLS scheme does not consider non-local processes. While I agree with your assessment that this will not matter significantly during the storms passage, I would point out that the non-locality (most active from the prior winter) may change the depth of the mixed layer prior to storm passage, which in turn may influence your results of NIE fate. While KPP certainly has its own issues, the inclusion of non-locality may be important. Why did you choose not to consider other vertical mixing parameterizations except the GLS $\kappa - \epsilon$ form?

Specific Comments

Page 1

–Line 28 – efforts should be effort

–Line 29 – with should be to, and semi-colon needed after on

Page 4

–Line 13 – NEMO is hydrostatic, so it really doesn't solve the three-dimensional primitive equations, correct? I would assume w is diagnosed from integrated divergence. I think you could just leave out that phrase and say it is discretized on...

– Line 18 – you reference the resolution of layers in the vertical for the 320 m test, but never for the 50 m baseline. It would be nice for a comparison

Page 5

Printer-friendly version

Discussion paper

–Line 19 – is there any sensitivity to the chosen 70-days (during the final year of the simulation) over which you average?

Page 6

–Line 20 – again, how are you determining that $\lambda < 60km$ is submesoscale?

Page 7

– Paragraph 1 – What is the vertical coordinate treatment in NEMO relative to the bottom? Do you use terrain following / z with partial bottom cells / z with shaved cells / something else? This feels like an important point as it will have an impact on how the flow interacts with the bottom topography in the model.

– line 8 / 9 – starting at the absence of topographic, I think it should be something like "the absence of a topographic ridge and narrow passages does not allow us to obtain"

Page 10

– line 2 – what is your near bottom resolution in the 50 layer, baseline case? Could this explain the absence of enhanced bottom dissipation as you probably are not resolving the bottom boundary layer?

Page 11

– line 8 – no comma is needed after fig 6e

– line 14 – should be – the submesoscale

Page 12

– line 2 – the sentence beginning with – At the difference of this general balance – is very confusing to me, in particular the first two clauses. It also feels like this could be broken into multiple sentences, perhaps after the second comma.

– line 32 – it feels like coherent should be consistent

Page 13

– paragraph 1 of section 5.3 – does your analysis here have any dependence on the assumed diameter and or the threshold deviation from a circular shape? For example, in your vorticity snapshot, many of the eddies are very elongated.

Page 14

– line 29 - 30 (and other places) (resp. ....) I am unfamiliar with this notation. I think this is 'respectively', but am unsure. If it is, I don't think that is necessary. Most often the respectively is omitted.

Page 15

–Vertical resolution section, did you consider sensitivity to resolution at the mixed layer interface? Appropriate simulation of entrainment/detrainment should have non-negligible impacts on your results. Your 50 vs 320 layer case may have sufficient enough differences in the respective resolutions at the boundary layer depth to speak to this already.

–Advection schemes, what resolution was the biharmonic viscosity coefficient used with and was it scaled with resolution? Was it actually used in other simulations? If it was used and not varied with resolution, it seems like your high-resolution simulations would be overly diffuse.

–Advection schemes, Note that when you compute numerical diffusion relative to a even order scheme, it is a slight over-estimate as these schemes, 2^{nd} order especially are anti-diffusive. It would be nice to make a quick mention of this in the text.

Page 18

– line 18 – parenthesis needed around 2011

Page 21

– line 11 – The sentence beginning Although the settings have... is very confusing to me. I think it might be fixed by adding a comma after "differences" and change "consists in" to is

Page 22

– line 24 – Life time should be The lifetime

– line 26 – affect should be affects

– line 26 – each should be every

Figure 2

–Can you explain why the MLD bias is so large in what I believe is the restoring region $(y > 2600km)$? It seems that if you restore to something resembling observations you shouldn't have such a large bias. Or is this related to the comparison to de Boyer Montegut versus some other climatology?

–I would move the clause mentioning how the MLD is calculated up in the description to where the MLD is introduced (first line)

Figure 6

Is the MLD defined as in Figure 2? it should be stated.

Figure 7

– The subscripts on the plot labels Ψ in (c) - (f) are very difficult to see, but I think they say "iso"? Could these be enlarged?

– do the dotted and dashed lines in (e) - (f) and (c) - (d) respectively represent the same thing (likelihood of a parcel being at the surface)? The caption only mentions dotted lines.

– I think you should remove the 'and' following Abernathy et al 2011 and add a comma

Printer-friendly version

Discussion paper

– Do the dotted lines in (e)/(f) correspond at all to modeled mixed layer depths? It would be interesting to see if all buoyancy classes that don't exist at least part of the time always reside in the mixed layer or not.

Figure 8

– be consistent between the caption and panel for (d) and (e) you have Ev Eh versus ϵ_v and ϵ_h

– the 10 after log10 should be subscript

– There is an odd symbol after your rms definition, I'm guessing it is <>

Figure 10

– Why did you choose to separate your analysis at 200 m depth? Is this a rough estimate of the boundary layer depth? Or some change in the shape profile perhaps?

Figure 11

– Should "UBS_C2" be "UBS - C2" instead? Same for "UBS_C4"?

Figure 13

– If the horizontal dissipation is due to advective scheme biases, it seems that this dissipation should reduce (assuming it is the implicit diffusion due to upstream bias) and not increase in panel h. I can't seem to find this panel discussed in text, but may have missed it.

Figure 15

– the abbreviation VFORM should be included after "vector form" in the caption.

Interactive comment on Ocean Sci. Discuss., doi:10.5194/os-2016-3, 2016.