Reviewer 1

In this work, the authors carry out a heat budget analysis in the Weddell Gyre using a suite of observations, including a gridded climatological product that covers 2002-2016. They find that the budget is very noisy on the grid scale, with local errors that are sometimes as large as the quantity being estimated. However, the authors claim that integrating over sufficiently large spatial scales allows them to make quantitative statements about the relative importance of advection and diffusion in both the horizontal and vertical directions. Specifically, they find that in the interior of the Weddell Gyre, net warming from horizontal turbulent heat diffusion is largely balanced by net cooling from mean horizontal advection. In contrast, in the southern limb of the Weddell Gyre, the relative contribution of these two terms switch signs, i.e., net cooling from horizontal turbulent heat diffusion is largely balanced by net warming from mean horizontal advection. As far as I am aware, this key result is novel, and it is certainly relevant to our understanding of the Weddell Gyre heat budget.

Observational analysis in the Weddell Gyre is extremely challenging due to the sparsity of measurements from that remote, harsh part of the world. The authors have managed to extract some novel estimates from these sparse observations and noisy heat budget by integration. The paper is very thorough and detailed, and it contributes to a growing body of work that the lead author has been assembling in recent years.

I do have some concerns that mostly have to do with how some of the results are presented. Although the authors have been up front about the errors in their estimates, I believe the errors should be emphasized even more in certain plots (e.g., the spatial maps). For example, when comparing Fig. 2a with Fig. S8, we see total propagated errors that are at least as large as the estimates themselves, although the saturated color bars make it difficult to compare in some regions. It is not clear to me whether some of the features in the estimate maps that are discussed in the text (e.g., the bands of alternating convergence and divergence) are robust, given the size of the propagated errors. Overall, after some edits for clarification, this manuscript will make a valuable addition to the literature; I recommend that this article be returned for minor revisions. I have included some specific suggestions below; I hope that they are helpful.

We would like to thank the reviewer for the constructive and very positive feedback, and for taking the time and effort to go through this rather long paper. We have followed your suggestions and we feel they have resulted in a much-improved manuscript. In addition, we have made some key changes to the manuscript, in consideration of the comments here and also of the other reviewer. We summarise these here briefly before responding to your individual comments. We provide the tracked changes manuscript to highlight these changes, as well as pdf file of all figures, captions and tables, for easy viewing for the reviewer. Line numbers refer to the line numbers of the changes in the manuscript with tracked changes.

The three major changes that we have made are as follows. Instead of arbitrarily defining the diffusivities within the Weddell Gyre (i.e., where data from Sevellec et al. are unavailable), we directly use diffusivities provided by Donnelly et al. (2017), given these are derived from observations throughout the Weddell Gyre. Thus, κ_H is now 247±63 $m^2 s^{-1}$ (the error range also coming from the paper) and κ_V is now (2.39 ± 2.83) × 10^{-5} m^2 s^{-1} (lines 185-188). We think this simplifies things and is also more representative of the gyre. We also include the approximate locations of the ship stations from Fig 3 in Donnelly et al (2017) in Fig. S4, showing the maps of κ_H.

Secondly, we changed the zonal analysis to focus on the regions west of Maud Rise, at 3°E (line 308). This is due to the high uncertainties in the eastern Weddell Sea, largely due to unresolved mesoscale eddy processes. This means that all the figures are new in section 4.2. The mean and net heat fluxes for the different terms are thus different, but the overall interpretation of the results remain unchanged. The text has been altered, where descriptions east of Maud Rise were removed. We also provide a zonal and meridional mean and integration of the whole western Weddell Gyre region (i.e., IC + SL) in the supplements. These show the same results as separate regional analyses in section 4.2, which is why they are kept in the supplements (section S8).

Lastly, we altered our definition of the SL region (line 298). Initially, we focused on the region where the streamlines extend zonally across the whole extent of the gyre. However, by defining it in this manner, there was a slight overlap with the IC region, which resulted in ambiguous interpretations of the results. Thus, we now define SL as the open inflow region, where the streamlines still span the full zonal extent of the Weddell Gyre, where the water masses entering the open inflow zone are entering from outside the gyre from the east. The IC, in contrast, is still defined by a streamline forming a fully enclosed circuit, and thus focuses on recirculation water masses within a true gyre structure (Fig. 4). Again, this results in a slight change to the mean and net heat fluxes (interestingly resulting in a closure of the SL heat budget, but not of the IC), but the overall interpretation remains the same.

Text

Line 55: Change to “en route” (it’s a phrase borrowed from French)

Done (line 69)

Line 71: Is the seasonal cycle “unresolved”, or has it been excluded in your analysis in this paper? Here it’s not clear whether you are saying that Argo does not resolve the seasonal cycle, or if you have excluded it by discarding everything above your dynamic upper boundary. Please clarify in the text.

The seasonal cycle has indeed been excluded, due to inadequate data density. When considering smaller regions where data is present (e.g., individual floats or small groups of floats), one could observe the seasonal cycle. We are, however, attempting to understand the broader gyre-scale perspective and therefore lack the data to incorporate a seasonal analysis into our optimal interpolation. Therefore, we exclude the upper 50 m. Adjusted the sentence as follows:

Line 85: *“Given Argo float data lacks the spatial-temporal coverage to resolve the seasonal cycle while also objectively mapping the entire region, a full basin analysis of the upper 50 m is unfeasible.”*

Line 130: Shouldn’t this be “the rate of change of heat storage of a certain ocean volume”? It’s the rate of change that is affected by the net flux.

Good point, changed accordingly, thanks (line 145)

Lines 73-74: Is this elongation of the “shape of the area of influence” a consequence of how the objective mapping was calculated? Please clarify in the text: why does the f/H fractional scale alter the shape of the area of influence?

The “shape of the area of influence” is a result of the e-folding decay scale of the covariance function (details in Reeve et al., 2016), which is determined by both the horizontal length scale and the difference in f/H between the grid point one is interpolating to, and the data points themselves. E.g., say you have 2 datapoints, equidistant to the grid point, but one has a similar f/H to the grid point, whereas the other has a much shallower (or deeper) water column depth, H (assuming close proximity of data point to grid point limits the impact of changing f, which suggests that bottom bathymetry variation plays a larger role). Then the data point with a more similar f/H will carry a larger weight. Whereas horizontal length-scales alter the size of the "area of influence", scaling the covariance with f/H impacts its shape. A nice demonstration of this can be found in Fig. 9 in Reeve et al., 2016. This is to reflect the tendency of water masses to conserve potential vorticity.

Altered sentence as follows:

Line 209: *"The fractional scale on the effect of f/H allows us to take into account the tendency of water parcels to follow paths of constant f/H in order to conserve vorticity (for a visual demonstration of the impact of including f/H in the scaling, see Fig. 9 in Reeve et al. 2016)."*

Lines 202-206: I suggest moving this detailed description to the start of section 4.2, just before you start discussing it.

Done. Last part of section now reads:

Line 240: "*whereas in part 2, we consider the larger-scale zonal variation of the heat budget*"

And the beginning of Section 4.2 now reads:

Line 298: "*In this section, we consider the zonal variation of the heat budget, for two regions: (1) the open southern limb (SL) and (2) the interior circulation cell (IC). The open SL region (i.e., the magenta stippled area in Fig. 4) is defined by the stream function as $16 \leq \Psi \leq 26$ Sv, which describes the open inflow zone where water masses enter the gyre, and spans the entire zonal extent of the double gyre system, from just west of Gunnerus Ridge (~33°E) to ~50° W, where the streamlines veer northwards to follow the coastline of the Antarctic Peninsula. The southern boundary of the SL is the southernmost streamline that does not intersect with the coastline (16 Sv). This definition of the SL enables us to focus on the water that enters the gyre from the east, and circulates the entire zonal extent of the gyre, thus reaching into the south-western interior. The IC region (i.e., the blue stippled area in Fig. 4) is defined as $\Psi \geq 26$ Sv, which is the largest streamline that spans the entire zonal extent of the double gyre system, this time forming a fully enclosed circuit. This definition of the IC allows us to focus on the recirculating waters of the gyre, from just west of Gunnerus Ridge to near the continental shelf edge of the northern tip of the Antarctic Peninsula (~50° W). For both regions the area east of Maud Rise (3° E) is omitted, due to large uncertainties east of Maud Rise (discussed in Section 5)."*

Suggestion for new figure: I would suggest adding a new, separate figure to show your two analysis regions of integration in a more visually intuitive. At present, the depictions of the regions are incorporated into an already-busy figure, and the blue contour risks causing some confusion about the extent of the regions, even with the stippling (e.g. the portion of a blue contour in the bottom right of Fig. 2d is distracting). The new figure could have isobaths, streamlines, and stippling to show exactly where the two regions exist, with no extra information competing for attention.

We have now added the figure as you suggested (Fig. 4), thank you for the suggestion. We also altered our definition of the SL region slightly to focus only on the open inflow (before there was a slight overlap with the IC region), where the streamlines imply an inflow of water from the east, outside the gyre. We've done this in order to avoid ambiguity in our interpretation of the relationship between the SL and IC regions. This has resulted in a change in the numbers, but not the overall findings (and the heat budget even comes to a close in the SL region now (0.3 ± 3 TW, or 0.002 ± 0.02 °C/yr).

Lines 211-212: Why is this “small patch of divergence” being singled out? Its significance is not obvious, and given the large size of the propagated errors (Fig. S8), it’s not clear if these features are robust.

We agree with the reviewer on this and have removed the statement and the subsequent sentence, and replaced them with the following:

Line 249: “*While small areas of heat flux divergence are found throughout the southern limb, these small areas appear to be less prominent than the areas of positive heat flux and are unlikely to be significant given the high associated errors (Fig. 3a, right panel).*”

Section 4.1 overall: As discussed above, the large propagated errors calls the robustness of these features into question. It’s not clear if detailed interpretation of these features, especially ones with small magnitudes, tells us very much. By comparison, the results that come out of the later large-scale integrations seem more robust. I suggest de-emphasizing some of the discussions of specific, detailed features.

Thank you for the suggestion. We deleted the feature-specific descriptions as shown in the previous response. We limited the description to generalized broad areas such as east vs west. We also deleted a sentence describing slightly enhanced vertical turbulent diffusion on line 277. We also include the error maps alongside the maps of the terms, in Fig. 3. As per your suggestion under figures and captions.

Line 224-225: It does imply that the net effect of the vertical advection is warming. The second part of the phrase could be somewhat confusing. I suggest rephrasing this as “…through the top, implying that the net effect of vertical advection is to warm the layer.”

Done (line 266)

Line 242, 245: Replace “chapter” with “section”.

Done

Line 342-344: Change to a separate sentence, i.e. “There is an overall decrease in temperature along the southern limb of the Weddell Gyre, and an overall increase in temperature along the northern limb (Fig. 6).” That way the order of the text matches the order of someone reading the figure left-to-right.

Done (line 416)

Line 458: Is the vertical grid uniform? A changing vertical grid could also introduce a spatial scale bias in the estimates.

This is a good point. The vertical grid is not uniform, with highest resolution in the shallower part of the water column. We take the upper boundary as the mean across neighboring grid points, and assume that the associated bias would be minor in comparison to the vertical boundary conditions themselves. We added the following in section 5.1.1 on vertical boundary limits:

Line 453: “However, this, along with the non-uniform resolution of the vertical grid, may introduce some noise into the analysis from grid cell to grid cell, owing to the different depths of the water column the heat budget is integrated over.”

Line 478: Perhaps change “negative air-to-sea heat flux” to “radiative heat loss” for simplicity?

Done (line 557)

Section 5.2.2: This section is very thorough, but it is very long and cumbersome. If allowed by EGU formatting, it could benefit from paragraph headers (sometimes called inline headings) to help orient the reader. Alternatively, it could be section 5.3, broken up into smaller sub-subsections.

We have now added inline headings to this section, thank you for the suggestion.

Lines 483-484: This statement doesn't work as a standalone phrase. Perhaps rephrase as "

The reviewer's suggestion for a rephrase was missing above. While we are still open to suggestion, in the meantime we have rephrased the sentence as follows:

Line 562: "*The heat budget shown in section 4 does not close, especially when integrated over smaller areas*"

Figures and captions

Figure 1: The red stars are very difficult to see. Perhaps make the markers much bigger and change the color so they stand out better? Figure 1 caption: Isn't "conservative temperature at the depth of the temperature maximum" just "maximum temperature?"

Done, and altered the caption to "sub-surface conservative temperature maximum"

Figure 1 caption: Change to "(1) Gunnerus Ridge, (2) Astrid Ridge, and (3) Maud Rise" instead of using the "respectively" construct.

Done

Figure 2: This figure risks being unintentionally misleading. If I have understood correctly, on a local scale, the propagated error is often just as large as the estimate itself, which throws the robustness of these spatial patterns into question, patchy as they may be. There is a risk of over-interpreting features, as they may reflect biases in the estimates. The authors have been very up-front and thorough about these biases in the manuscript overall, but casual readers may get the wrong idea glancing at these figures. I'd suggest moving the spatial patterns to the supplemental information and focusing on the integrals, and/or putting the propagated errors in their own column next to the mean terms for easy comparison.

The authors agree with the reviewer here, and decided to move the propagated errors to a column alongside the main terms, in Fig. 3. In reference to your earlier comment (i.e., 3rd paragraph, stating concerns about associated errors to the alternating bands of heat flux convergence and divergence in Fig. 3a), the large errors sit directly north to these alternating bands. We added the following to the text on line 256:

"*Note associated errors become increasingly large directly to the north of the northern limb, related to the highly dynamic boundary.*"

Figure 3 caption: Explain the meaning of the shaded areas here in the caption, since this is where they are first introduced in the figures.

Added the following to Fig. 5 caption: "The shaded errors provide the associated propagated error (detailed in section 3.2 and the supplement)."

Figure 6: I would suggest making this figure even more visually obvious – consider that people may only glance at it briefly. You could add a dashed vertical line separating the northern limb from the southern limb. On their side of that line, you could add a text box with either "northern limb" or "southern limb", just to make it explicit and clear. I'd also suggest using a different symbol for 30°W (north) and 30°W (south).

Thank you for the useful suggestions. We hope the reviewer finds now Fig. 8 much improved.

Order of Fig. 5, 6, and 7: Perhaps figure 6 should come before figure 5? You could introduce the temperature tendency first and then show the integrated budget terms afterwards, as a way of explaining these temperature trends.

We understand the logic of the suggestion and gave this a go but it didn't really work with the restructuring of the text, since this is specifically relating to the inverse relation between horizontal advection and turbulent diffusion in Fig. 7a. Thus, we have decided to leave it as is.

Figures 5 and 7: Please add y-axis labels for every panel, as the unit by itself is not sufficient. What quantity are we looking at?

Done

Figure 8: The inset figure appears to be distorted, and the y-axis labels have been cut off. As for the main figure, I suggest using a legend to help quickly orient the reader as to the meaning of the symbols and numbers. At present, trying to understand the figure from the caption is visually overwhelming. You may also want a simple figure with a legend for talks. If you get this figure right, lots of people will show it in their own talks, summarizing your important results.)

Thank you for the positive feedback and useful suggestion. We have now provided a legend which summarises the content of the original caption, and subsequently shortened the caption by half (now Fig. 10).

Additional references

I would suggest adding two references to the paper, if the authors agree that they are suitable. (Note: I was not involved with either of these papers.) First, there is a recent numerical modeling study that explored the seasonal and interannual variability of the Weddell Gyre in a high-resolution model. I suggest that the authors mention this in the discussion section; it is a concrete illustration of seasonal and interannual biases, which are relevant to this climatology.

Neme, J., England, M. H., & Hogg, A. M. (2021). Seasonal and interannual variability of the Weddell Gyre from a high-resolution global ocean-sea ice simulation during 1958–2018. Journal of Geophysical Research: Oceans, 126, e2021JC017662. https://doi.org/10.1029/2021JC017662

Second, the authors end the paper by mentioning the vulnerability of the Filchner-Ronne ice shelf. There is a more recent reference that explores the tipping point beyond which the cavity beneath the Filchner-Ronne Ice Shelf will flood with warm water:

Naughten, K.A., De Rydt, J., Rosier, S.H.R. et al. Two-timescale response of a large Antarctic ice shelf to climate change. Nat. Commun. 12, 1991 (2021). https://doi.org/10.1038/s41467-021-22259-0

Thank you for the recommendations and for pointing out these interesting papers. We have added the following:

Line 590 (section 5.2.2): "*The summertime bias and coarse resolution likely results in an underestimate of the mean gyre strength, and thus the velocity field derived from the stream function (Neme et al., 2021).*"

And:

Line 805: "*While Naughten et al. (2021) argues that global temperatures would need to reach 7°C for warm water to intrude to the ice-shelf cavity, which exceeds the pledges in the Paris Agreement, nonetheless, the authors argue that unless global temperatures level out, melting of the ice shelf will at some point prevail.*"