

## ***Interactive comment on “Synthesis and evaluation of historical meridional heat transport from midlatitudes towards the Arctic” by Yang Liu et al.***

**Anonymous Referee #1**

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Review of Synthesis and evaluation of historical meridional heat transport from midlatitudes towards the Arctic by Liu et al

The present study is concerned with the inter-annual to decadal variability of poleward atmospheric and oceanic heat transports. For this purpose, the authors computed these quantities from three atmospheric and three oceanic reanalysis products, respectively. They find marked discrepancies in the results from the different products, which leads them to the conclusion that the current generation of reanalyses are of limited use for investigation of low-frequency variability in the derived fluxes. The layout of the study is good and its results are of interest to the community. However, it appears there exist severe technical shortcomings in the computation of the atmospheric transports (which I will describe in the more specific comments), which puts into question all

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the subsequent results and conclusions. I therefore encourage the authors to carefully check their chain of computations and revise their results. I consider this a necessary precondition for publication of the present study. More specific comments are in the following.

Major concerns: 1) Figure 3a indicates that the annual cycle of AMET from ERA-Interim and JRA55 is very different, with yearly minima in JRA55 going down to  $\sim 0.8\text{PW}$ , while annual minima in ERA-Interim remain closer to  $1.5\text{PW}$ , which implies a huge relative discrepancy ( $>50\%$ ) in the amplitude of the annual cycle of AMET from the two products. From our own computations using basically identical scripts for both products I can tell that their annual cycle of AMET actually agrees very well (within  $<10\%$ ). Also, low-pass-filtered variability of AMET looks quite different from our results compared to the authors' figure 3b. Thus, I presume there is something wrong in the authors' computation of AMET (at least for ERA-Interim and JRA55, I cannot judge the results from MERRA2), possibly in the mass adjustment that they apply. I recommend to thoroughly check the chain of computations. NCAR provides a quite detailed step-by-step instruction how to perform these computations: <http://www.cgd.ucar.edu/cas/catalog/newbudgets/index.html>

2) The authors note that there exist improved diagnostic equations for energy budget diagnostics (Mayer et al. 2017; Trenberth and Fasullo 2018), but do not use those. I strongly recommend to make use of these updated equations. There is no reason not to do so.

3) Ocean energy budget (section 2.3.2) and discussion about reference temperature: The authors discuss the need for a reference temperature as long as the mass budget is not closed. This is indeed important and this issue has been extensively discussed in the oceanographic literature, most notably Schauer and Beszczynska-Möller (2009). However, the present study only considers oceanic transports in a zonally integrated sense. Full zonal cross-sections should have a net mass flux close to zero, making the use of a reference temperature unnecessary. The same applies for the statement



about recirculation (p8 l13). In fact, for zonal integrals, there only is a small imbalance coming from P-E, leaving a small ambiguity, which in the same manner applies to the atmosphere. For that reason, the same reference temperature should be used for both atmosphere and ocean to obtain consistent results (Mayer et al. 2017). The discussion on these issues must be clarified.

Minor comments:

Generally: the plural of “reanalysis” is “reanalyses”, while the plural of “reanalysis data set” is “reanalysis data sets”. Please correct throughout the manuscript.

There are many inaccurate formulations throughout the manuscript. I picked only a selection in the following, but I generally recommend to carefully revise the manuscript in order to make it more concise.

P2L16: Is this result based on models? Please clarify.

P2L24: In this context it might be worth mentioning that ocean reanalyses do not show a clear sign of Arctic amplification in Arctic OHC increases (Mayer et al. 2016; von Schuckmann et al. 2018)

P2L34: Please rewrite the sentence to something like: “These are representations of the historical state of the atmosphere and ocean optimally combining available observations and numerical simulations using data assimilation techniques.”

P3L3: Please spell out this acronym (and all others).

P3L7: Please be more specific about the “model”. Is this a forced ocean model run?

P3L23: “higher” than what?

P3L24: change “preferably” to “preferable”

P3L25: “For an inter-comparison purpose, they better not resemble each other”. What is meant here exactly? Please reword.

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P4L7-8: Is there a reference for the statement about divergent winds? It might be worth checking Graversen et al. (2007)

ESDD

P4L14: add “scheme” after “assimilation”.

P4L22: add “upper air” before “observations”

P4L26: oceans → ocean’s

P4L30: data with 3D-Var assimilation → analyses with a 3D-Var FGAT assimilation scheme

P5L9: Not quite right. The forcing is a combination of ERA-Interim fluxes (e.g. short-wave radiation) and bulk formulae using ERA-Interim near-surface parameters. Please correct.

P5L16-17: “To be consistent with the other two reanalyses datasets assessed in this study, the SODA 3.4.1 is chosen since it applies surface forcing from ERA-Interim”. This statement seems to be opposite of what you say above in P3L25

P5L18:  $r \rightarrow R$

P6L5: What is the “Drakkar forcing data” based on?

P7L21: explain  $u_c$  and  $v_c$

P8L10: the equation gives OHC across a certain circle of latitude. Is this meaningful? How would that relate to the transports across that latitude? Did the authors mean OHC integrated across the area north of a given latitude?

P8L11-12: I can reassure you that ocean reanalyses do have sources and sinks from temperature increments, but they do not suffer from mass inconsistencies as atmospheric reanalyses do. The divergence of ocean currents exactly balances the surface freshwater flux and local sea level variations, so there is no mass adjustment needed. Please see the NEMO documentation for details (Madec 2008).

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P9L3: Do you mean “decorrelation”?

P9L6: Do you mean “by a factor of 3”?

P9L8: Why not do this the other way round? Apply filter first, and then estimate effective degrees of freedom.

P9L10: I do not understand the statement about statistical significance

P9L18: not only solar radiation, but also OLR. In that sense, transports balance NET radiation.

P9L26: How do you know this?

P9L29: “ERA-Interim res” is not shown in the figure.

P9L21: 1.21PW is probably too high (see major comment #1)

P10L10: Here it is important to know whether you are using monthly or sub-monthly data, which should be stated in the methods section. If you use monthly means, you will miss eddy transports and consequently underestimate ocean heat transports.

P10L12: What is plotted in Fig4? The poleward component of OMET? How is this obtained, if OMET is computed on the tripolar NEMO grids?

P10L19: Is “hindcast” the appropriate term? Isn’t it a forced model run?

P11L1: correlation or regression?

P11L5:  $r=0.07$  seems very small (again, see major comment #1)

P11L20: With these diagnostics, you are running into problems with reference temperature, as the point-wise transports obviously do not have a zero mass flux. Figure 7d (showing  $T_{\text{mean}} \cdot \Delta_v$ ) will have much larger values when you use K instead of C, leading to a different conclusion. Which one would be correct? I suggest to remove 7c and 7d and discuss a and b in more detail. Instead of 7c and d, it would be interesting to show the difference sections also for JRA55.

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P12L5: Please use a different name than “NEMO”, as all your ocean products are based on NEMO.

P12L15ff: Similar to my above comment: I do not understand why you look at bands of OHC and not at OHC north of 60N.

P13L9: Better use an independent SIC product. SIC from ERA-Interim is of questionable quality, which can be seen e.g. from the “disc” around the North Pole. Also, ORAS4 does not have an active sea ice model. Would you expect a correlation between OMET and SIC then?

P13L18: no -> not

P13L32: Fig 13: I am not sure what is shown here. Are these instantaneous regressions? The legend says 1-month lag - does this make sense when using 12-monthly smoothed data? At which lag do you get highest correlation?

P14L1: I think one has to be careful with the timescales here. What timescales are the cited studies looking at?

P14L25: How can you see this from the time series?

P14L27: Be cautious: A lot of heat transported across 60N is stored in the North Atlantic or released from there through air-sea fluxes and will never reach sea-ice covered regions.

P14L28: “patterns” of what?

P14L31: remove “the” before “tropical”

P15L8: “less consistent” than what?

P15L24-26: Hard to understand. Please rewrite the sentence.

References:

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