

## Response to Reviewer #1

We thank the reviewer for their very constructive comments and suggestions as well as for the time spent reviewing the paper. In the following, the reviewer's comments are in black, and our responses are in red.

Thank you for your document "Simulated and Observed Transport Estimates across the Overturning in the Subpolar North Atlantic (OSNAP) Section". It is a very useful contribution to our understanding and obviously contains a lot of analysis. My recommendations are mostly about exploring some relationships a bit more, putting results into context with some other studies and improving the clarity of the results. My suggestion is for major revisions – I do not think the revisions themselves are particularly serious, but am aware that they might take some time.

Thank you for your kind words, appreciating the usefulness and value of the manuscript for the community. We have revised the manuscript, following most of your suggestions. Please see below for details of how we addressed each of your comments.

Major

1. There are a couple of places where the authors make the point that we don't know that OW dominates OE for decadal and longer variability (L243, final sentence). This is true, but an obvious question is what do the models show? Do any/much of the models suggest that stronger variability can be seen at OW? Is there a relationship between decadal variability at OW and shorter timescale variability/mean state which might let us infer what the observations might show?

First, to clarify, we make statements such as "we cannot yet address the relative roles of the LS vs. the eastern subpolar gyre in AMOC variability on decadal and longer time scales" within the context of available observational datasets. Since the observational record is short, i.e., only 8 years for OSNAP, we cannot assess whether OW (or the Labrador Sea side) dominates low-frequency variability. A discussion and summary of what the models show had been already provided in paragraphs 3, 5, and 6 of the Introduction. Specifically, "in many simulations, the associated low-frequency variability mechanism involves surface buoyancy flux anomalies primarily over the LS region, but also with contributions over the Irminger Sea, that arise from long episodes of the positive phase of the North Atlantic Oscillation (NAO)." As stated in the 8<sup>th</sup> paragraph (second to the last) of the Introduction, our study focuses on providing a basic, yet much-needed evaluation of simulated transports against OSNAP observations for the 2014–2022

period using a large set of forced ocean – sea-ice simulations. Our effort thus aims to produce an assessment of model fidelity as well as a benchmark for simulated transports and related properties at the OSNAP sections in comparison to these observations for only this specific period when OSNAP is available. As such, a detailed assessment of low-frequency variability in these simulations and its relationship with shorter timescale variability is beyond the scope of the present study. However, we are pursuing exactly these topics as part of a separate ongoing study.

2. You split your models into high and low resolution, but it seems to me that there are 3 groups of about 6 models each – low resolution ( $>0.25$  degrees), medium resolution ( $\sim 0.25$  degrees), and high resolution ( $<0.25$  degrees). Does this split change anything about your conclusions of resolution?

Thank you. Following your suggestion, we have now split the simulations into 3 groups: Low Resolution (LR; near  $1^\circ$ ), Medium Resolution (MR;  $\sim 0.25^\circ$ ), and High Resolution (HR;  $0.1^\circ$  or finer). We have redone all the relevant figures accordingly. This new split does not change any of our conclusions.

3. Your timeseries plots are difficult to read because it's difficult to tell some of the colors apart. Some possible options: use different line styles as well as colors; split the models into low, medium and high resolution, with legends for each separately.

In response to item 2, we have followed your suggestion of splitting the simulations into LR, MR, and HR groups. Accordingly, all the relevant timeseries plots are redone with fewer lines in each panel, making them easier to see. We now also use a single color for the simulations from the same group, making it easier to compare them across resolutions. Similarly, the scatter plots use different symbols for LR, MR, and HR, but with the same color for the same group. We appreciate these suggestions; they certainly improve clarity and readability of the figures.

4. The paper is quite long, however there is quite a lot of space taken up by description of plots, such as values for particular models etc. This might be important to highlight in some cases, but some parts of the text feel like a long description of the figure. Ideally this information could be read off the plot for someone interested, and more space could be used to discuss the more interesting findings such as: common model biases; differences with resolution; relationships between variables; possible causes and implications of specific differences (eg the authors do discuss the implications of having a better overflow

representation in some models); how results fit with other findings. This is done sometimes, but not always. I've made some suggestions in the points below.

Based on our experience with writing similar multi-model intercomparison manuscripts, we find that it is important to provide values of fields for some simulations wherever appropriate, helping the reader. We also think that it is good practice to introduce a figure before its discussion to set the stage, except for some details which are mentioned in the figure captions. To the extent feasible and practical, as the reviewer also indicates, we include discussions on the listed topics. We have now included additional discussions following your suggestions as detailed below. In particular, we now split the models into three groups: low-, medium-, and high-resolution following your suggestion. Accordingly, all relevant figures have been replotted to distinguish these groups. We have also added a new figure (new Fig. 7) to show west-to-east cumulative transports across the OSNAP section in response to another reviewer's suggestion. We have removed Appendix B.

Minor

L52 Remove 'general' – I'm not sure what it means here.

Removed.

L75 These reviews are not recent anymore!

Deleted "recent". Also, added a reference to Jackson et al. (2022) review article.

L88 comma after weakening

We think that this sentence should not use a comma (or commas), because its intent is to emphasize all mentioned aspects (weakening, stability, and collapse) equally, noting also that "weakening" refers to "AMOC" which is at the end of this sentence.

L123 I think I'm correct in saying that fluxes may also be important over the Irminger sea as well as the Lab sea?

The sentence has been modified to include the Irminger Sea as well.

L125-131 The text implies that all models show a link between low frequency AMOC variability and the Labrador sea which is not true, certainly in coupled models - eg see <https://doi.org/10.1007/s00382-023-07069-y>

It certainly was not our intention to imply that all models show a link between the Labrador Sea variability and that of AMOC. We have clarified this point by saying “In many simulations,...” and by mentioning a role for the Irminger Sea as well, following your suggestion above. In addition, we have added a sentence that explicitly mentions connections with the GIN Seas and the Arctic Ocean, citing Zhao et al. (2024), at the end of this paragraph.

L200 I’m surprised that the authors do not also include a discussion of Yeager et al <https://doi.org/10.1126/sciadv.abh3592> , which has a different mechanism for the interaction between the regions

Actually, Yeager et al. (2021) was introduced and discussed earlier in the introduction on lines 136-140 of the original manuscript in the paragraph concerning low-frequency AMOC variability. The paragraph on lines 178-207 of the original manuscript covers the OSNAP period. We also note that Yeager et al. (2021) is cited in Section 4 and Section 9.

L221 conclude that coupled models

Modified to read “ .... conclude that coupled models analyzed in their study are .... ”.

L276 It’s not clear to me why MOM5 and 6 are listed as different models, but the NEMO versions are not.

MOM6 is a completely re-written version of the Modular Ocean Model that combines various elements of MOM5 and the Generalized Ocean Layer Dynamics (GOLD) ocean model. As such, there are major architectural and structural differences that include elimination of the B-grid in favor of the C-grid in the horizontal and a generalized Lagrangian-coordinate in the vertical. In contrast, the NEMO versions used here (3.6 and 4.0) differ primarily in their scalability and optimization, physics options, and coupling capabilities among several others. In our view, these differences do not rise to the level of being much different in their architectural and structural aspects.

L286 Define  $z^*$  or reference something to explain what it is

Added the following sentence: “Both  $z$  and  $z^*$  formulations are utilized in the simulations with depth coordinates where the  $z^*$  approach enables inclusion of large amplitude variations of the sea surface by distributing such changes over the full water column depth (Adcroft & Campin, 2004).”

L378 I think the  $dz/d\sigma$  should be inside the  $dx$  since the thickness is normally integrated over  $x$  as well.

Corrected.

L405 You quote densities as eg 24 kg/m<sup>3</sup> rather than 1024 kg/m<sup>3</sup> – you need to make this point somewhere.

Since these are sigma densities, by definition they are in values / magnitudes given in the text. Nevertheless, this is now explained in the first paragraph of Section 2.5 where the sigma density is defined.

L465 It looks to me that the black line is plotting the density of the maximum transport rather than a timeseries of the maximum transport itself.

The black lines are indeed the density timeseries of the maximum positive transport. This has been clarified both in the text and the Fig. 3 caption.

L482 Do you mean annual mean of the maximum over depth of the monthly mean transports? It's a bit long, but at the moment it isn't clear what the maximum and average are taken over

These are the annual averages of the maximum transports based on the monthly mean transports in density space. This has been clarified.

L497 Is the weakening trend at OE also seen in most models? How does the overall trend compare to other studies eg <https://doi.org/10.1038/s43017-022-00263-2> and references therein

Yes, this weakening trend is consistent with what is seen in other model simulations as well as with those from other estimates. In response, we have added a new paragraph which discusses the low-frequency variations in these transports. We now provide a quantitative trend comparison for the total transports with those trends reported in Jackson et al. (2022).

L558 The reference to 'high densities' seems to suggest relatively high densities for what we are interested in, however it looks like you're referring to densities where the transport is maximum here. Suggest rewording

The sentence is correct as it is written as it is about comparison of profiles for densities greater than 27.7 kg m<sup>-3</sup> which includes the ranges of densities at which maximum transports occur.

L565 This suggest that in the observations the northwards transports have a narrower range of densities that the models. Is there anything which can be said about why that might be?

We do not think that this discrepancy necessarily suggests a narrower density range in the observations than in the simulations. Indeed, T-S diagrams for the upper 500 m (not shown) reveal largely comparable density ranges among the simulations and with OSNAP. As such, we think that this discrepancy is likely related to differences in current pathways and their T and S properties. We also think that possible under-sampling of the Greenland shelf waters in the observations can contribute. A sentence has been added to list these factors.

L632 What are the deeper/denser levels? Can you say anything about why this might be and the implications?

Quantitative values for “deeper” and “denser” were provided in the previous sentence. Specifically, deeper than 1500 m and denser than  $36.75 \text{ kg m}^{-3}$ . We do not think that much can be said about sources of these differences and their implications without more extensive analysis of the models which is beyond the scope of this study. We do not think that these differences are simply due to the representation of overflows. Since the main discrepancies arise from northward dense flows, deep gyre circulations need to be also considered.

L650 This isn't obvious to see from the plots – it looks like the scale is saturated for some plots. Could you do a quantitative comparison, eg comparing the mean of the north/south flows?

The requested comparisons of the mean (or summed) northward and southward flows / transports had already been provided as the thin red and blue lines, respectively, in the vertical profile panels both in depth (right panel) and density (top panel) space for each simulation and OSNAP.

L654 maximum aggregated transports

Added.

L705 Is there also a relationship of mean strength or monthly variability with annual/decadal variability?

We do not think that such an analysis will produce meaningful relationships on annual variability with only 8 years of data, which certainly exclude decadal variability.

L709 simulated ensemble mean SDs

Added.

L710 and elsewhere. The SD should have units of Sv I believe

Thank you for catching this error. The units have been added.

L732 clarity?

Deleted this part of the sentence.

L738 This sounds like you're saying that the densification is happening on the OSNAP section rather than that the upper/lighter waters are being densified to the north and then returning south. I think this needs a bit of clarification

No. It was not our intention to imply any driving mechanisms associated with these changes as these T-S diagrams cannot really identify mechanisms. We have added a sentence to clarify this. It reads "We note that these T-S diagrams do not necessarily identify mechanisms driving these T and S properties which can result from various processes both locally and remotely, including mixing and advection."

L743 Why might the models be also freshening as well as cooling unlike the obs? Could this be related to too much mixing in overflows? Or something else?

In general, models tend to be *spicy* in their T and S contributions to density. So, it is not surprising to see such behavior, which may arise due to various processes and their combinations. We think that imprints of overflows are more visible in the T-S diagrams at OE. Indeed, the last paragraph of this section discusses impacts of the Denmark Strait and Faroe Bank Channel overflows. We have decided not to include any additional discussions here as our follow-on paper will be looking into some of these in more detail, including a water mass transformation analysis.

L769 Note that UKMO25 captures the density though not T and S.

This information was already included just three lines above on line 766 of the original manuscript.

Fig 16 and others. It is hard to see the deep values – it looks like the deep values are plotted first and then the shallower ones later, and in some cases it looks like they are plotting over the deep values and obscuring them. It might make the figures clearer to plot the shallower data first and deeper data last.

We have checked the plotting package used for creating these figures. Indeed, we have confirmed that the order of plotting is from the shallowest first to the deepest last. We also note that there are fewer deeper points due to coarser vertical resolution with depth and presence of bottom topography.

Section 8. There are a lot of points here which are not explored. There are other studies showing a stronger AMOC in more saline models eg

<https://doi.org/10.1175/JCLI-D-22-0464.1> <https://doi.org/10.5194/os-17-59-2021>  
<https://doi.org/10.1016/j.ocemod.2013.10.005> So discussion about how this seems a robust feature would be welcomed. Also some more discussion about density compensation would be useful. Are there any thoughts about why there is density compensation in some places but not others? What about density compensation in the observations? This paper suggests that there is density compensation for OW <https://doi.org/10.1038/s41561-019-0517-1> How does that compare with what you see? Could biases in the models compared with the observations affect the density compensation? Finally you could do with some motivation about why you're looking at Fig 18 as well as 17 and whether anything can be concluded from considering results together.

Following the reviewer's suggestion, we have expanded this discussion by adding two new paragraphs: one at the end of this section and another in Summary and Conclusions right after the related summary paragraph. Robust aspects are now clearly identified. We have also clarified the justifications for this section and the figures.

Fig 18 The caption is a little unclear. Is the y axis the total AMOC across both sections rather than OW and OE separately?

Yes, your understanding is correct. Since the "total" is already defined earlier and used elsewhere, we have not modified the caption.

L886 total velocities -> net transports

Done.

L900 Are there differences in biases with resolution?

No. The answer had been already provided in the last sentences of both paragraphs of Section 8. We have now added a sentence to the end of this paragraph as well.

L929-935 I found this rather confusing – are you always using mean as time-mean and max as max over density? Is the point that you're making here that the annual time mean of the maximum over density is not the same as the maximum over density of the annual time mean?

The answer to both questions is yes. Just to clarify the second question a bit: the annual time-mean of the maximum transports based on monthly mean transport profiles in density space are not the same as the maximum transports based on the annual time-mean of the transport profiles in density space. Our analysis uses both approaches following what is done in the literature. For example, while Fig. 4 uses the

former, Fig. 6 uses the latter approach. What we do is indicated wherever needed in the text. Regarding these particular line numbers referred to by the reviewer, this paragraph (lines 929–942) should be considered in its entirety as further clarifying information is included in the second half of this paragraph. Nevertheless, we made several edits in the first half of this paragraph towards clarification.

L1109 ‘the same numerical schemes and parameterizations’ – but do they not have different ice models? This sentence implies that the overall models have similar set up other than resolution, but this isn’t true. Maybe just needs a comment here.

Agreed. The sentence has been deleted.

## Appendix B

It really isn’t clear why the focus here is on two models only, particularly since the authors note that the differences are likely to be model dependent. What is the aim of this section? If it is to document differences then surely it would make sense to plot them all. Otherwise please explain the motivation/aim of the section.

We agree with the Reviewer’s assessment. Accordingly, we have removed Appendix B. This also shortens the manuscript.

## Response to Reviewer #2

We thank the reviewer for his very constructive comments and suggestions as well as for the time spent reviewing the paper. In the following, the reviewer's comments are in black, and our responses are in red.

This work provides a valuable benchmark for the modelling community for evaluating simulations at the OSNAP section. It draws on six different ocean GCMs and a wide range of model simulations. All models are consistent with the earlier findings of Lozier et al. (2019), showing that OSNAP East largely determines the strength and variability of the AMOC. This further supports the conclusion that convection in the Labrador Sea may not, as previously thought, be the primary driver of AMOC variability.

Although all models reinforce this central message, the model spread at OSNAP is, unsurprisingly, substantial. The analysis is comprehensive and highly valuable, and the paper is clearly structured and professionally written. The manuscript would be suitable for publication in GMD following the authors' responses to the minor comments listed below.

Thank you for your kind words, appreciating the relevance and value of the manuscript for the community.

line 55. worth mentioning here that they cancel each other at constant depth and density.

Done.

line 116: Do you have any citations for this? I am unsure whether this statement is correct. One can certainly say many things about coupled versus forced AMOC simulations. Xu et al. (2019), if I remember correctly, reported different observations in their paper 'On the variability of the Atlantic meridional overturning circulation transports in coupled CMIP5 simulations' (<https://doi.org/10.1007/s00382-018-4529-0>). Gent (2018) also discussed this issue in 'A commentary on the Atlantic meridional overturning circulation stability in climate models' (<https://doi.org/10.1016/j.ocemod.2017.12.006>).

The statement is indeed correct. It comes from comparisons of AMOC transport timeseries from forced simulations (e.g., Danabasoglu et al. 2014; 2016) to those participating in CMIP5 (e.g., Cheng et al. 2013) and CMIP6 (e.g., Weijer et. 2020 – supplemental Information). Figure 3 of Xu et al. (2019) also supports this statement for simulations participating in CMIP5. Gent (2018) is not directly relevant here. As

requested, we have added several citations to this sentence, including Xu et al. (2019).

line 126: the findings by Ortega et al. 2012, 2021 were further confirmed using AWI-CM by Sidorenko et al. in AMOC variability and watermass transformations in the AWI climate model. Journal of Advances in Modeling Earth Systems, 13, e2021MS002582. <https://doi.org/10.1029/2021MS002582>

Added a citation to Sidorenko et al. (2021).

line 186: also in FESOM forced ocean by Sidorenko et al. (2020). AMOC, water mass transformations, and their responses to changing resolution in the Finite-volume Sea ice-Ocean model. Journal of Advances in Modeling Earth Systems, 12, e2020MS002317. <https://doi.org/10.1029/2020MS002317>

Added a citation to Sidorenko et al. (2020).

line 188: I am a strong supporter of the work by Megann et al. (2021), which you cite above. Their interpretation is that Subpolar Mode Water formed in the northeastern Atlantic, which initially retains relatively high buoyancy, is advected into the Irminger Sea, where it loses buoyancy, and is subsequently transformed in the Labrador Sea—following further buoyancy loss—into Upper North Atlantic Deep Water before being exported southward. This view is consistent with McCartney and Talley (1982). Megann et al. (2021) also introduced a buoyancy-loss-ramped accumulation index that successfully reproduces the decadal variability of the AMOC, which could be a valuable approach to consider in the proposed follow-up study.

We concur with you that it would be valuable to consider the approach used in Megann et al. (2021) in a follow-up study.

line 353: How would you expect the results to change if instantaneous sampling were used to compute the AMOC in density space?

We have added the following text in the manuscript:

“Finally, we employ monthly mean fields for both OSNAP and model simulations as this temporal frequency is what is available from all model simulations. Compared to use of higher frequency sampling, e.g., instantaneous, in transport calculations if they were to be available, we do not expect our results to change in any significant way for the spatial scales considered here, supported by the findings of Ballarotta et al. (2013) for transport calculations in an eddy-permitting regime in the Southern Ocean.”

line 590: If the transports within each bin are provided in Sv, I would suggest plotting the thin blue, red, and black lines in Figures 7–10 in a stepwise manner, since the transports are already aggregated within the respective bins (depth ranges).

We prefer to keep these figures the way they are already plotted. This is because any discretized variable, e.g., potential temperature, salinity, velocity components, can be interpreted as representing the spatial mean for a given grid cell in a model. Nevertheless, we conventionally plot all model fields not in a stepwise or block manner.

line 708: Just a question: to what extent would a more realistic simulation of the Arctic Ocean improve the representation of the OSNAP transports?

This is a difficult question to answer. While a more realistic simulation of the Arctic Ocean would produce transports and water masses that agree better with limited observations, properties of these waters will likely be impacted by surface fluxes and lateral and vertical mixing processes by the time they reach the OSNAP latitudes. A forced ocean experiment in which a model's potential temperature and salinity fields are restored to their (limited) observed counterparts may be helpful to shed light on this question.

line 785: Here you use the depth range above 700 m, whereas in the section above you considered depths deeper than 500 m for the T–S diagrams, in order to exclude the larger surface biases. How would Figures 17 and 18 change if deeper biases were considered instead?

Section 7 (original Figs. 13–16) and Section 8 (original Figs. 17 and 18) serve different, but complimentary purposes. Specifically, Section 7 is about deep and abyssal ocean water mass properties. In contrast, Section 8 concerns the upper-ocean biases as they directly impact deep convection in model simulations. So, deeper biases should not be included in Figs. 17 and 18. In response to both this comment and another one by Reviewer #1, we have modified a couple of sentences in this section to clarify its purpose, including the very first sentence.

line 834: More generally, in the context of model bias analysis, this suggests that the AMOC may transport water masses with similar (and less biased) densities that nonetheless occupy different regions of T–S space. This behaviour appears to be common across many regions of the global conveyor belt.

Original Fig. 18 supports this summary for our analysis region.

lines 874–876: Here,  $\sigma_0$  should also be explicitly mentioned and a reference to Fig. 6 added; otherwise, the reader may become confused.

Done.

line 886: The same correction as in the abstract applies here: both flows cancel each other at constant depth and density.

Done.

I wonder whether a figure similar to Fig. 2 in Lozier et al. (2019) would show comparable behavior across the models. Such a comparison could potentially serve as a useful benchmark. The same applies to their Fig. 3. Some brief discussion of this point would be nice to have.

Thank you for this suggestion. We have added a new figure (Fig. 7) along with its discussion in Section 4. The figure reproduces the top panel of Fig. 2 from Lozier et al. (2019) for all simulations. Indeed, it provides a useful benchmark, showing that the HR simulations show better agreement with the OSNAP observations than those of MR and LR. We decided not to include an additional figure akin to that of Fig. 3 of Lozier et al. (2019), because similar information is already provided in Fig. 4. Furthermore, since these simulations use the same wind product, the Ekman components would be rather similar among the simulations.

### Response to Reviewer #3

We thank the reviewer for his very constructive comments and suggestions as well as for the time spent reviewing the paper. In the following, the reviewer's comments are in black, and our responses are in red.

This paper presents a coordinated comparison of modelled and observed overturning transports across OSNAP–West and –East sections, using a large number of forced OMIP–type simulations spanning a wide range of ocean/sea-ice models and resolutions. The paper is explicitly framed as descriptive and benchmarking, rather than mechanistic explorations, and is largely successful in achieving that goal. The topic is highly relevant for the ocean and climate modelling community, and it will provide valuable insights into how well ocean models capture the OSNAP overturning circulation in both depth and density space. The paper is overall well-written, and I only have some minor/technical comments.

Thank you for your kind words, appreciating the relevance and value of the manuscript for the community.

- Near the end of the Introduction, the authors clearly state that the paper is “largely descriptive”, which is welcome; however, before that, there is heavy text with extensive discussions of, e.g., the control of Labrador vs. Irminger Seas, which might make it read like a hypothesis–driven AMOC mechanism paper. I am not sure if/how this could be improved, though – how about clarifying this somewhere earlier in the text? I am not sure if the authors share a similar view, and I will leave it to them to decide.

We think that it is important to give a relatively comprehensive overview of the AMOC and its impacts, variability, and drivers to provide a solid context to discuss the implications of the new findings from the OSNAP observations. This sets the stage for the importance of our benchmarking as well as our assessment of model fidelity. We now make the goals of the manuscript clearer at the beginning of this paragraph, including *assessment of model fidelity* as part of our effort.

- The differences in results between  $\sigma_0$  and  $\sigma_2$  space (such as the relative contributions from OW) are important findings. For a broader audience, it would be beneficial if the authors, where appropriate, could expand and articulate a bit why using  $\sigma_2$  alters the results.

Such an explanation had been provided in the 4<sup>th</sup> paragraph of Section 2.5. In response, we added a reference to this section in the conclusions section (Section 9).

- L47, "...ocean - sea ice simulations..."

Corrected.

- L206, "compensation"

Corrected.

- L344-349, the authors claim that they "analyze the same forcing cycles for a given LR and HR set of simulations from the same group"; however, this does not seem to be the case in the following descriptions. For example, ANU10 (HR) is not in the same cycle as its LR counterparts of ANU1 and ANU25.

This has been noted as an exception.

- L489, "FSU72"

Corrected.

- Figure 7 and Figure 8 (and other pairs of figures), here the figures are not separated as HR vs. LR (as in previous text and figures), but rather separated alphabetically - is there any rationale behind the change? It would be helpful to explain a bit, or at least mention this change in the text.

In these more extensive figures, we adopted an alphabetical ordering to expedite comparisons within a set of simulations from the same group. This is now mentioned at the beginning of Section 5.

- Fig. 18, if I understand correctly, the MOC here is the total, i.e., OW+OE; is there a reason not to separate it, as in Fig. 17? Also, would it be useful to add the OSNAP to the scatter plots, as in Fig. 17?

Your understanding is correct: Original Fig. 18 uses the total overturning transports. In contrast to original Fig. 17 which shows the biases at the actual OSNAP sections, Fig. 18 is for the broader regions depicted in Fig. 1. For this reason, we think that it makes more sense to use the total transports in this figure. For the same reason, we do not plot the OSNAP values in Fig. 18.