Response to Reviewer #1

The review is pasted below; our responses are interspersed in blue font.

Summary

This manuscript discusses several indicators of the position of the Gulf Stream and finds that those based on following a specific isotherm at a specific depth (i.e., 15ºC at 200 m or 12ºC at 400) become biased northward relative to the maximum velocity core in models under warming scenarios. This result is perhaps a bit obvious, but it is worthwhile to make this point explicitly as well as quantify the size of the bias. The manuscript is clearly written and thoroughly documented. I am not convinced, however, that the location of the maximum velocity core is the best indicator for the "true" latitude of the Gulf Stream (see comment 1) and the statistical tests are not necessarily appropriate to the hypotheses being tested (see comment 2). The second comment should be straightforward to address and it is possible the first can be addressed by providing additional motivation in the introduction.

Response: We appreciate the reviewer's comments that our results are worthwhile and that the manuscript is clearly written and thoroughly documented. In our revision, we have made efforts to further clarify the motivation and intended focus of our study.

For detailed responses to comments 1 and 2, including how they were addressed in the revised manuscript, see below.

Specific Comments

1. The authors motivate interest in knowing the latitude of the Gulf Stream by invoking the Gulf Stream's connection to the AMOC and the Gulf Stream's impact on ecosystems. They then adopt the location of the maximum surface velocity (i.e., the SSH front at the surface) as the "true" location of the Gulf Stream, but it is not clear that the location of the SSH front is relevant to the AMOC or ecology. Biology, in particular, is much more sensitive to temperature than SSH or current speed, so the location of the Gulf Stream temperature front (at various depths) is likely more relevant to biology and regional climate than the location of the SSH front. Chi et al. (2019) showed that the two fronts have distinct spatiotemporal variability—especially downstream of the New England Seamounts—such that motion of one is not necessarily correlated with motion of the other. It is likely that the 12 & 15ºC isotherms also become biased north of the location of the temperature front under warming scenarios, but the size of the bias is likely to be different than the bias between these isotherms and the location of the SSH front. In light of these facts, I suggest that the authors provide stronger motivation for their interest in the location of the maximum velocity at the surface.

Response: The motivation for our study is that the rapid warming of the northwest North Atlantic shelf and adjacent slope regions, which are among the fastest warming ocean regions in the world, is commonly attributed to a northward shift in the Gulf Stream (see references on line 21).

This northward shift is commonly diagnosed using a temperature-based criterion which, as we show in the manuscript, is problematic. We believe we provide strong evidence that the warming of the shelf and slope region can occur without a northward shift of the Gulf Stream (see FOCI model projections) and that isotherm-based criteria are misleading in that they suggest a northward shift even when no such shift is occurring (see FOCI) or the shift is much smaller (see CM2.6). We have revised the Abstract and Introduction to articulate this motivation more clearly. We have also revised the Results, Discussion, and Conclusions to streamline and focus the manuscript on the intended research question. We introduced a new Figure (Fig. 1) that shows pronounced warming of the northwest North Atlantic shelves in both simulations.

In the previous version of our manuscript, we mentioned the Gulf Stream's connection to the AMOC because it felt like an omission not to do so but have removed this in the revised manuscript because it was commented on as misleading. We modified the first sentence in the abstract to remove mention of the AMOC, removed its mention and one reference from the Introduction, and removed one mention from the Discussion. We left the following two sentences unchanged because we believe they are relevant for grasping the implications of our study, but would be open to removing them as well if reviewer or editor prefer.

> "Reconstruction of surface or subsurface temperature patterns in the Atlantic Ocean is a common approach to constraining the strength of the AMOC (Caesar et al., 2021; Rahmstorf et al., 2015; Thornalley et al., 2018). However, our results indicate that temperature patterns alone may provide a limited or biased representation of ocean currents dynamics."

We don't state that the Gulf Stream location has an impact on ecosystems in the northwest North Atlantic shelf and slope regions, but that others commonly link these two causally. It is well documented that rapid warming in the shelf and slope region has severe impacts on ecosystems, but this is not the focus of our study (we agree with the reviewer that, for ecosystems, temperate matters not SSH or current speed). Our point is that the warming is often causally linked to a northward shift of the Gulf Stream – an association that we believe is made in error because the temperature-based criterion can falsely indicate a northward shift even when none is occurring. We have clarified this in the revised Abstract and Introduction.

We agree with the findings of the Chi et al. (2019) study which used data from 1993 to 2016 and documented a divergence between the isotherm criterion and the actual path of the Gulf Stream east of 71°W, in other words, a divergence in space. They attributed the divergence to the activity of mesoscale eddies, which is plausible. Our study addresses a different question, namely whether the two criteria diverge over time in a warming world. We have clarified this in our revised manuscript and now state explicitly that our research question has not been addressed previously in the published literature (to the best of our knowledge).

We fully agree with the reviewer's points about the isotherm criteria.

In summary, we believe there is a strong motivation for our study and hope we have articulated it much more clearly in the revised version.

2. While both the changes and slopes of the various indicators are all different from each other, it is not clear that these differences are statistically significant. Tables S1 and S2 give *p*-values for the slopes, but these only indicate that the slopes are significantly different from *zero*. Since the trends are being compared to each other, rather than zero, it would be more appropriate to give confidence intervals for the slopes so that the reader can assess whether or not the confidence intervals overlap.

Response: We agree and have added confidence intervals in Table S1.

Response to Reviewer #2
The review is pasted below; our responses are interspersed in blue font.

General comments

In the manuscript by Garcia-Suarez and collaborators, the authors test the hypothesis of whether isotherm base criteria for the location of the Gulf Stream are appropriate in the context of a warming ocean. To do this they use two simulations, using two different model architectures, forced by two different scenarios. Their results suggest that widespread warming in the slope associated with the climate change signal can be erroneously interpreted as a northward shift in the current axis. The manuscript is well written and the language is clear. The overall framing of what current and future changes in the world's western boundary current systems tell us about the overturning circulation and continental shelf environment is a very relevant one and it would be of interest to a large part of the community. However, I'm afraid that the study lacks both the novelty and rigour that are expected for a peer reviewed scientific publication. The question that is posed by this study, presented as their hypothesis in lines 50-51, is already addressed in Todd and Ren 2023, which the authors already cite in the manuscript. Todd and Ren clearly articulate how to decompose temperature signals to isolate warming from axis shift based in the observations. A similar approach could have been taken here to isolate warming from shift. With regards to experimental design, while I value the authors' effort to demonstrate that their results hold across multiple models, the different model architecture and different forcings lead to very different GS behaviours, and the manuscript presents no evidence that the either model is able to reproduce variability. Unfortunately, I believe the work required to bring this manuscript into acceptable form is beyond the scope of the review process, and as such I have to recommend against publication.

Response: We appreciate the reviewer's comments that our manuscript is well written and clear and that the topic is a "very relevant one and it would be of interest to a large part of the community."

We are surprised by the comment that the study lacks the novelty and rigour of a peer-reviewed study. Regarding novelty, the reviewer states that our hypothesis is already exhaustively addressed by Todd and Ren (2023). We respectfully disagree. As stated by the reviewer, our hypothesis was given on lines 50-51 (lines 40-41 in the revised manuscript) as the "15 ºC isotherm [..] criterion overestimates GS northward shifts." Todd and Ren (2023) did not address this hypothesis at all, in fact, they don't use the 15 ºC isotherm criterion. Todd and Ren (2023) used in situ observations and provided an excellent and detailed analysis of Gulf Stream temperature, salinity, and density gradients, the warming and changes in stratification occurring in the Gulf Stream, and an estimate of the rate at which it moved laterally from 2001 (they find a northward movement of 6 +/- 3 km per decade). Todd and Ren (2023) do not explicitly address the question of whether the isotherm criterion is appropriate to track lateral shifts in Gulf Stream location in a warming ocean and they don't have at their disposal multi-decadal model projections, which we analyze here. Their finding of a modest northward shift in GS path is not, in any way, contradictory to our results. We added the following text about the Todd and Ren (2023) study in our revised Introduction:

> "Bisagni et al. (2017) found that between 1993 and 2013, the GS actually moved southward while Todd and Ren (2023) documented a modest northward shift using observations collected predominantly between 2015 and 2023."

And

> "Todd and Ren (2023) used a dense observational data set consisting of Argo float measurements (since 2001) and underwater glider measurements (since 2015) to reconstruct the three-dimensional water properties of the Gulf Stream. They decomposed temperature and salinity signals as they contribute to changes in density, isolated the warming trend from a shift in the GS axis, and documented a modest northward shift of 6 ± 3 km per decade during their analysis period. Their method will become increasing useful as the record of autonomous observations grows."

Furthermore, we clarified that, to the best of our knowledge, our hypothesis has not been addressed in the published literature:

> "To the best of our knowledge, our hypothesis that background warming renders the temperature-based criteria unreliable has not be posed or systematically analyzed by others."

Regarding the reviewer's criticism that we do present no evidence that either model is able to reproduce variability, we would like to note that both model runs have been extensively discussed and used in the published literature, including in studies that make inferences about lateral shifts in the location of the Gulf Stream (Saba et al. 2016, Ceasar et al. 2021) and discuss mesoscale variability in the region (Martin et al. 2022, Martin and Biastoch 2023, Huo et al. 2024). More specifically, the same CM2.6 simulation we are using in our study is discussed and used by Saba et al. (2016 [cited 600 times]), Ceasar et al. (2021 [cited 460 times]), Claret et al. (2018 [cited 117]). The FOCI model we are using is more recent and not yet as frequently cited as CM2.6, but it has been validated (Matthes et al. 2020 [cited 26 times]) and used in a number of follow-up studies including Martin and Biastoch (2022), Martin et al. (2023), and Huo et al. (2024). All the studies referring to CM2.6 were already cited in our manuscript. Regarding FOCI, we had only cited Matthes et al. (2020) and have added the other studies in our revised manuscript.

Caesar, L., McCarthy, G. D., Thornalley, D. J. R., Cahill, N., and Rahmstorf, S.: Current Atlantic Meridional Overturning Circulation weakest in last millennium, Nat. Geosci., 14, 118–120, https://doi.org/10.1038/s41561-021-00699-z, 2021.

Claret, M., Galbraith, E. D., Palter, J. B., Bianchi, D., Fennel, K., Gilbert, D., and Dunne, J. P.: Rapid coastal deoxygenation due to ocean circulation shift in the northwest Atlantic, Nature Clim Change, 8, 868–872, https://doi.org/10.1038/s41558-018-0263-1, 2018.

Huo, W., Drews, A., Martin, T., & Wahl, S. (2024). Impacts of North Atlantic model biases on natural decadal climate variability. Journal of Geophysical Research: Atmospheres, 129, e2023JD039778. https://doi.org/10.1029/2023JD039778

Martin, T. and Biastoch, A.: On the ocean's response to enhanced Greenland runoff in model experiments: relevance of mesoscale dynamics and atmospheric coupling, Ocean Sci., 19, 141–167, https://doi.org/10.5194/os-19-141-2023, 2023.

Martin, T., Biastoch, A., Lohmann, G., Mikolajewicz, U., & Wang, X. (2022). On timescales and reversibility of the ocean's response to enhanced Greenland Ice Sheet melting in comprehensive climate models. Geophysical Research Letters, 49, e2021GL097114. https://doi.org/10.1029/2021GL097114

Matthes, K., Biastoch, A., Wahl, S., Harlaß, J., Martin, T., Brücher, T., Drews, A., Ehlert, D., Getzlaff, K., Krüger, F., Rath, W., Scheinert, M., Schwarzkopf, F. U., Bayr, T., Schmidt, H., and Park, W.: The Flexible Ocean and Climate Infrastructure version 1 (FOCI1): mean state and variability, Geosci. Model Dev., 13, 2533–2568, https://doi.org/10.5194/gmd-13-2533-2020, 2020.

Saba, V. S., Griffies, S. M., Anderson, W. G., Winton, M., Alexander, M. A., Delworth, T. L., Hare, J. A., Harrison, M. J., Rosati, A., Vecchi, G. A., and Zhang, R.: Enhanced warming of the Northwest Atlantic Ocean under climate change, J. Geophys. Res. Oceans, 121, 118–132, https://doi.org/10.1002/2015JC011346, 2016.

Specific comments

(1) The paper goes back and forth between the concept of the GS as a driver of the changes in the Slope (both introduction and discussion) and changes in the slope explaining the warming in the northern flank of the GS (the explanation for the mismatch between velocity and thermal proxies of the GS). I think there's a real opportunity for the authors to address this question, but a more systematic approach to decomposing changes in temperature and circulation in the western North Atlantic would be needed.

Response: In our revised manuscript we took care to be clear about what concept we are addressing and to remove sidenotes that could be distracting to the logic flow. We believe the presentation of the manuscript is streamlined and much improved as a result. We believe our approach is systematic and are curious to know why the reviewers suggests it is not.

As a starting point considering the actual spatial patterns in warming may already reveal some interesting information. At the moment because the Slope temperature is calculated as an average over the region relative to the mean GS position in 2015, and figure 4 shows that at least in CM2.6 there is a non negligible trend in the actual GS path (both blue and black lines in panels c and d confirm, and the magnitude of the temperature trends over the entire period in FOCI are much less clear due to the nature of the forcing post 2040 I suspect), some of the warming attributed to the Slope is likely a real shift in the GS axis.

Response: We agree that considering spatial patterns of warming is interesting. In fact, the spatial patterns of warming in CM2.6 are discussed in detail in Garcia-Suarez and Fennel (2024). However, the focus of this manuscript is the connection (or, more precisely, the lack thereof) between Gulf Stream latitude and warming in the northwest Nort Atlantic shelf and slope regions that has been made based on application of the T15 criterion. We agree with the reviewer that there are real shifts in the GS path in the models. In fact, we calculate these directly using the velocity fields to diagnose the core of the GS path (shown in Fig. 2) and use the core path throughout the analysis to calculate deviations of the T12, T15, and SSH proxies from the true core of the GS (see Fig.s 3 and 5). We clearly state in the manuscript that the path is shifting in both models. These movements do not contradict, in any way, our conclusion that the temperature-based criteria T15 and T12 are flawed in a warming ocean and that the GS path, when diagnosed using T15, appears to be moving at a much different rate than the actual GS core.

Garcia-Suarez, L. and Fennel, K.: Physical Drivers and Biogeochemical Effects of the Projected Decline of the Shelfbreak Jet in the Northwest North Atlantic Ocean, Journal of Advances in Modeling Earth Systems, 16, e2024MS004580, https://doi.org/10.1029/2024MS004580, 2024.

In the discussion of the warming in the slope as an explanation for the apparent shift in the GS inferred by T15, the authors show evidence that the slope is warming at an accelerated rate compared to the subtropical ocean (panels c/d versus e/f). If this is the case, the thermal gradient across the GS must also be increasing. For the historical period, because a big portion of the observed temperature changes are salinity compensated, as discussed in Todd and Ren (2023), they have no impact in the current structure. Using the heave/spice framework as presented could help distinguish which part of the warming is a driver and which is an impact. This framework along with the vertical structure of the temperature anomalies shown in Figure 5 offers some likely candidates for what are the actual drivers of the warming in the continental slope and shelf (lines 156-161) in these models.

Response: The reviewer seems to suggest our paper should address the drivers of the warming in the northwest North Atlantic. While we agree that it would be a worthwhile and interesting study, this is not the intention with the present manuscript. As stated in our response to reviewer 1, the motivation for our study is that the rapid warming of the northwest North Atlantic shelf and adjacent slope regions, which are among the fastest warming ocean regions in the world, is commonly attributed to a northward shift in the Gulf Stream (see references on line 21). This northward shift is often diagnosed using a temperature-based criterion which (see references to 5 studies on lines 37 in the revised manuscript]), as we show in the manuscript, is problematic. We believe we provide strong evidence that the warming of the shelf and slope region can occur without a northward shift of the Gulf Stream (see FOCI model projections) and that isotherm-based criteria are misleading in that they suggest a northward shift even when no such shift is occurring. We believe we have articulated this objective much more clearly in the revised version of our manuscript.

(2) Amongst the key differences between models that make it hard to assess whether these models capture the real behaviour of the Western North Atlantic are the following.

Firstly, in the assessment of the mean GS path (Figure 1) in CM2-6 the SSH and maximum velocity criteria show important differences (with the sign of the difference varying across longitudes), this is compared to both observations and FOCI for which the two criteria tend to agree to the west of the Grand Banks. Secondly in the discussion about the Slope supported by figure 5, in CM2.6 the slope continuously cools down for most of the control run, while it oscillates around 0 in FOCI. The magnitude of the warming for different layers is also quite different between models, with both the 200 and 450m levels warming faster than the surface in FOCI, but only the 450m layer warming faster than the surface in CM2.6 according to Figure 5.

Response: We agree with the reviewer that the models have shortcomings, but would like to point out again that they are widely used in the literature, including in the widely cited study by Ceasar et al. (2021) that suggests a causal link between the warming in shelf and slope region and a northward movement of the Gulf Stream (which we believe Ceasar et al misdiagnosed by using the isotherm criterion).

(3) Lastly, as I final note, the North Wall is not expected to match the maximum velocity core (as stated in lines 145-146), but instead represents the northern flank of the current. As such it is expected to be located to the north of the GS core even in equilibrium. The question is whether the two metrics vary in a coherent way. Figure 4 suggests that within the eastern part of the domain, where the Gulf Stream is more coherent (A1 domain according to the labelling), a deeper isotherm could be used to track the evolution of the current core quite accurately in both models. A shallower isotherm, such as T15 does overestimate the evolution based on a simple linear fit. However, in close inspection, for FOCI (A1), most of the difference between T15 and T12 occurs at the beginning of the record, prior to 2024, while they approximately track each other from that point onwards. As I said, I think this work presents some very interesting preliminary results that the authors could build on in a future study.

Response: We agree with the reviewer that the North Wall is expected to be north of the Gulf Stream core. Our point is that the North Wall index suggests a strong northward movement, which has been proposed as a driver of the rapid warming in the northwest North Atlantic, when the core may not actually move northward at all (as is the case in the FOCI simulation) or only modestly (as in the CM2.6 simulation.) In other words, the two metrics **do not** move in a coherent way which we clearly show. We have made efforts to clarify this in the revised manuscript. More specifically in response to the comment that the North Wall is expected to be north of the core, we contrast the difference between the temperature-based criteria for the North Wall and the core in the pre-industrial and the warming simulations in the Results section (Figure 2b, c). Indeed, in both models T12 and T15 are located to the north of the GS core (east of 55° W) in the control simulations (preindustrial without warming; shown as the dotted lines) while in the warming simulations, already in 2015 the northward deviation is more than doubled (see solid lines). This cannot be explained by the fact that the northern edge of the GS is north of its core. We believe we have described this more convincingly in the revised manuscript.

The reviewer states that our study “presents some very interesting pre-liminary results that the authors [we] could build on in a future study” without providing any clear arguments as to why they perceive our reasoning as flawed or incomplete.

We believe we have clarified the motivation of our study, have explained that spatial patterns of warming are analyzed elsewhere and are not within the intended scope of this study, and that the models have been widely used already in the published literature.