The Alongshore Tilt of Mean Dynamic Topography and its Implications for Model Validation and Ocean Monitoring

Christoph Renkl, Eric C.J. Oliver and Keith R. Thompson

Response to Comments by Reviewers

We thank both reviewers for their insightful comments. We have responded to all the comments as detailed below. The labels to the comments of Reviewer 1 are of the form [R1.x.y], where y is the number of the comment in Section x of the review. Similarly, labels to the comments of Reviewer 2 are of the form [R2.x].The reviewers' comments are repeated below in bold font, our responses are given regular font with, and modified parts of the text including the respective line numbers is indicated by italicized font. We believe the changes have resulted in a significant improvement to the manuscript and hope the reviewers and editor will find the revised version suitable for publication.

Reviewer 1

This paper covers quite a lot of ground. It presents a model calculation of the mean dynamic topography along the Scotian Shelf - Gulf of Maine region, including comparison with tide gauge observations and interpretation in terms of terms in the equation of motion along the coastline (which in the model is at a mean depth of 23.4 m). These sections of the paper are a nice piece of work in themselves, similar in spirit to the work of Lin et al. (2015) as cited by the authors.

However, the scope is much broader, including a theoretical section relating the alongshore sea level slopes to offshore processes, sections on the Stommel model and on Csanady's Arrested Topographic Wave, and interpretation in terms of a regional average of upwelling, followed by a test of this interpretation using diagnostics of the time-varying component of the ocean model. I find this broader aspect of the paper unconvincing, and the presentation rather disorganised. The maths appears to be correct (except that the wind stress terms should all be divided by ρ_0 and the quickly-abandoned nonlinear terms are incorrect), but the interpretation and link with other models is not clear and, in particular, the description of the relevant diagnostic as an area-averaged upwelling does not seem appropriate. Accordingly, my recommendation for the paper is that it needs a major revision.

Thank you very much for your constructive comments and feedback on our paper! Following your suggestions, we reorganized the manuscript, simplified the maths, and clarified the interpretations and links between the considered models as detailed below.

R1.1 Interpretation and link to idealised models

[R1.1.1] The crucial diagnostic derived in section 3 is $-(u - u_g + u_{gb})\bullet\nabla H$, which is described as an upwelling, presumably on the basis that the terms other than $u_g\bullet\nabla H$ represent upslope flows. However, $(u_g - u_{gb})\bullet\nabla H$ represents the offshore geostrophic flow relative to the bottom (i.e. a thermal wind referenced to the bottom), which on an f-

plane has no associated dw/dz, and u is the total, depth-averaged flow, which includes the wind-driven Ekman flow - another component which need not involve any vertical motion. More insight comes if we write $u = u_g + u_E$, separating the depth-averaged flow into geostrophic and frictional (Ekman) components (the latter includes the effect of wind stress, bottom stress, and lateral friction). The important quantity can now be rewritten as $-(u_E + u_{gb})\cdot\nabla H$, representing the combination of Ekman and bottom geostrophic onshore flows. In shallow water for example, where we would expect the onshore wind-driven Ekman term to be increasingly balanced by offshore Ekman flow due to bottom (and lateral) friction, this term tends to zero as that balance is established, although the exchange of water between upper and lower Ekman layers represents a downwelling. Equally, a deep water balance of onshore wind-driven Ekman flow and offshore barotropic flow would clearly be a downwelling flow, but again this term would be zero. In short, it cannot meaningfully be described as an upwelling.

Instead of describing the diagnostic as upwelling, we are now referring to it as net Ekman pumping velocity that results from the flow across isobars (l. 333). Following this modification, we have adjusted the wording throughout the text. Please let us know if we can bring this out more clearly.

[R1.1.2] The discussion of the Stommel model seems irrelevant. All models are consistent in that the sea level slope at the boundary is related to the difference between wind stress and frictional stress at the boundary (this is simply the boundary condition), but beyond that the Stommel model depends essentially on beta - the boundary current represents a balance between bottom stress curl and the beta term - so the f-plane derivation of section 3 is not relevant.

We have removed the discussion of the Stommel model from the manuscript.

[R1.1.3] The Arrested Topographic Wave mode, while consistent with section 3, is barotropic and, in the light of the section 7 results which show the term related to stratification to be dominant, it seems to add little of relevance.

The discussion of the idealized models serves primarily to illustrate the coastal and regional views, and, in conjunction with the realistic GoMSS model, show the versatility of these interpretations. In that regard, Section 5 (previously Section 4.2) is intended to provide more context to the theory in Section 4 rather than "explain" the results in Section 7. We have clarified the purpose of the section in the text:

"In Section 4, two views of the dynamical role of the alongshore tilt of MDT at the coast are introduced and subsequently tested in both idealized models of shelf circulation (Section 5) and the realistic GoMSS model (Section 6)." (l. 64-66)

"In the following sections, we further discuss the coastal and regional views of $\Delta\eta_c$ in the context of idealized models for shelf circulation and finally demonstrate that these interpretations also hold in the realistic, high-resolution model GoMSS. Overall, these sections serve to illustrate the versatility of the dynamical interpretations that further highlights the usefulness of the tilt of MDT for ocean model validation and monitoring." (l. 337-340)

“In Section 4 it was shown that the alongshore tilt of MDT at the coast can be interpreted in terms of the coastal and the regional circulation. Using idealized models, it was demonstrated that $\Delta\eta_c$ is a measure of the mean alongshore current at the coast (coastal view), but can also be related to the net Ekman pumping due to vortex stretching offshore (regional view). Here, we will test whether these views also hold in the realistic GoMSS model with a focus on the nearshore region between the reference points s_1 and s_2 outlined by the red polygon in Figure 1.“ (l. 395-399)

R1.2 Maths

The derivation of (22) is correct, but very roundabout, with a number of approximations introduced gradually through the derivation. It is in fact a slight rewrite of quite a standard equation (the use of the boundary condition being the main innovation). If we remove the nonlinear terms from (3) (these are incorrect because the depth average of, for example, u squared is not the square of depth-averaged u), and note that the term in brackets on the left is pb/gρ0, replace h with H (an approximation used later in the paper), and introduce a streamfunction such that ρ0Hu = k x ∇ψ, (3)xH becomes

(A) $f\nabla\psi = \rho_0\nabla\chi + H\nabla p_b - \tau$

(τ represents all the friction terms).

Dividing (A)S by H then taking the curl gives the barotropic potential vorticity equation, which integrates to a form of (22) when the boundary condition is used to replace the wind stress integral (it is helpful to work in terms of depth-integrated pressure P instead of sea level at the boundary, noting that P = ρ0χ + Hpb). This provides a much more straightforward derivation, without the "upwelling" interpretation.

We have simplified the derivations in Section 4 (previous Section 3) by stating all assumptions at the beginning, neglecting the inverse barometer effect, and starting from the linearized moment equation for depth-averaged flow using a slightly rewritten form of the one presented by Csanady (1979) with differences described in the text. The full definition of η_s and χ are now given in (4) and (5). We also adopted your suggestion to suggestion go straight to H and the stress terms are now divided by ρ_0 (see comment R1.4.2).

R1.3 Organization

It seems odd to have the derivation of (22) at the beginning of a paper which then focuses on the time-mean flow and the boundary interpretation. It would be much more helpful to have a self-contained "steady state, boundary interpretation" part of the paper, then move to the "regional, time dependent" ideas and diagnostics. I'm not sure how useful (22) actually is (it seems to be a way of assessing which part of the dynamics that needs ultimately to be balanced by a bottom pressure term, has not been balanced by it until the sidewall is reached, thus needing a pressure gradient (sea level slope) along the sidewall), but it is certainly interesting, and particularly interesting that the χ term plays such a big role. In many ways this is almost 2 separate

papers, but I do see the sense in keeping them together, as long as the logical progression is made clearer.

We have restructured the manuscript by moving the model validation in Section 3 (previous Section 5) before the dynamical interpretation sections. This new structure divides the paper into two self-contained parts: the first part (Sections 2 and 3) addresses the first research question raised in the Introduction “can new observations of geodetically referenced coastal sea level help validate high-resolution models?” and the second part addresses the dynamical interpretation of the alongshore tilt of MDT using theory (Section 4), idealized models (Section 5), and the realistic numerical high-resolution regional model GoMSS (Section 6). Since the two interpretations complement each other, we prefer to present them side by side for each model.

R1.4 Minor Issues

[R1.4.1] The description of how the permanent tide is accounted for is confusing (I sympathise! It is hard to explain this issue clearly). I suggest using (1) as the basis throughout, and explaining how he and N are calculated by correcting GPS heights and geopotential heights from tide-free to mean-tide system, and then applying (1), rather than saying (1) is applied then corrected.

Following your suggestion, we first describe how he and N were calculated and then apply (1) to estimated coastal MDT. The modified text reads:

“GPS coordinates are generally expressed in a tide-free coordinate system (Woodworth et al., 2012) as is the geoid model CGG2013a. In order to make geodetically referenced MSL observations comparable to ocean circulation models, mean tidal effects on the coordinate systems have to be considered. Therefore, he and N were calculated by converting GPS heights and geoid heights from tide-free to mean tide coordinates using the corrections provided by Ekman (1989). Note that the minus sign error reported by Woodworth et al. (2012) was taken into account.” (l. 117-121)

[R1.4.2] Line 138 - note that MDT and model MSL differ by an unknown constant (dynamically irrelevant) offset.

We have modified the text:

“Therefore, MSL predicted by the model is equal to the MDT (plus an unknown, dynamically irrelevant constant) and can be directly compared to the geodetic estimates.” (Line 137-138)

[R1.4.2] Equation 3 and line 168 - a full definition of χ is needed (I think it is the Mertz and Wright one, which is actually depth-integrated PE anomaly divided by ρ_0), the nonlinear terms should be removed (and I would recommend going straight to H instead of h), the wind stress term should be divided by ρ_0, and a reference should be given for the source of the equation (as noted above, there are other simplifications of presentation that could be made too).

We have implemented your suggestions in the text. Please see response to comment R1.2 for details.

[R1.4.2] Line 179 - this seems to be a definition of bottom pressure torque rather than the JEBAR term, which is better defined in the quotation used later.

We have modified the text:

"As shown by Mertz and Wright (1992), this can be interpreted as the baroclinic contribution to the torque related to the depth-averaged pressure." (l. 262-263)

[R1.4.2] Line 202 - "wind setup" suggests the effect of a wind blowing towards, not along the coast.

We changed "wind setup" to variations of "sea level difference along the coast due to wind stress" and "wind-driven tilt along the coast" throughout the text.

[R1.4.2] Line 254 - "corrected for" seems wrong here - u* is the total flow minus the geostrophic flow relative to that at the bottom.

We have corrected the wording to

"total flow minus the geostrophic flow relative to that at the bottom" (l.332)

[R1.4.2] Line 607-8 (regarding the scale factor) - but what would be an appropriate value to use for "water depth at the coast", when it is not in a model with a fixed, finite sidewall?

We now provide one approach how the water depth at the coast could be determined:

"For practical applications, when the water depth cannot be inferred from a numerical model with a sidewall, it could be determined from bathymetric soundings as the mean water depth along the outside the surf zone." (l. 557-559)

In conclusion, I see that the authors have done a lot of work to interpret the coastal sea level signals they are investigating. The data analysis is good, the topic and results are interesting if not completely conclusive, and the cited literature is appropriate - I would like to see this paper published. But it does need some streamlining and reorganising to make it clearer what has actually been shown, and to improve the logical flow of the ideas.

Thank you again for your constructive feedback! We hope the restructured manuscript and clarifications outline above have helped improve the logical flow of the ideas and make the paper easier to follow.

Reviewer 2

This is an interesting extension of earlier studies comparing the geodetic and ocean model approaches to the estimation of the alongshore tilt of MDT. The paper is broken into two main sections – the first looking at the geodetic estimates to validate a regional model, and the second considering how the alongshore slope can provide information on the shelf circulation.

Overall the first section, using geodetic estimates of MDT to validate the GoMSS regional model, is a worthwhile addition to the literature. However I'm left with the - sense that spatially-sparse geodetic measurements with relatively high uncertainties are of limited utility in validating a high-resolution model such as GoMSS. I think that the authors slightly overstate the agreement between the model and observations. Qualitatively there is broad agreement, but only when some measurements are excluded and other discrepancies explained.

The second section, deriving and describing the relationship between the alongshore tilt of MDT and the coastal and regional circulation, is less compelling. The coastal view, as it is described by the authors, is similar to the momentum balance presented by Lin et al., but improved through the use of a higher-resolution model. This is of value in terms of understanding the tilt of MDT and, perhaps, in terms of monitoring the alongshore flow. The value of the regional view is less clear.

The layout of the manuscript is challenging. Whilst I think I understand why the derivations are presented up front (sections 3 and 4), these sections are lengthy and heavy going. The results and interpretation sections consequently appear somewhat lost.

Many thanks for your valuable comments and suggestions on our paper! We addressed your concerns as described below and hope that the revised manuscript is now easier to follow.

I suggest the following to improve the manuscript:

[R2.1] Consider reducing the derivations and/or moving them to an appendix. Whilst I won't try to fault the math, the derivations are convoluted and rely on many assumptions along the way. Much of this is published elsewhere and I can't help but think that it could be simplified.

We have reduced and simplified the derivations in Section 4 (previous Section 3) by stating all assumptions at the beginning, neglecting the inverse barometer effect, and starting from the linearized moment equation for depth-averaged flow using a slightly rewritten form of the one presented by Csanady (1979) with differences described in the text (see response to comment R1.2 for details). We have also removed the discussion of the Stommel model (previous Section 4.1).

[R2.2] I'm not convinced that the regional view is adding to the story here. It seems more of a theoretical study that doesn't sit well with the rest of the paper. If it is to remain, the utility of an integrated measure of upwelling needs to be demonstrated.

The usefulness of $\Delta\eta_c$ for model validation and implications ocean monitoring are discussed in the final paragraph of Section 7. We modified the text to highlight the value for model validation, as shown in our study. The implications for ocean monitoring are intentionally speculative and provide directions for future studies:

"This close relationship between coastal MDT and integrated nearshore circulation highlights utility and value of tilt estimates based on geodically referenced sea level observations for model validation. Furthermore, it also has obvious implications for ocean monitoring: For example it may be possible to use long coastal sea level records to estimate time series of upwelling rates. Such information may be of interest to biological oceanographers interested in understanding changes in nutrient cycling on the shelf over recent decades. This speculation applies not only to the Scotian Shelf. For example, in the future, it would be interesting to test the idea on the west coast of North America given the large number of long, geodetically referenced sea level records (e.g., Lin et al., 2015) and the large amount of hydrographic data, e.g., the California Cooperative Oceanic Fisheries (CalCOFi) Database. Finally, our findings have implications for future deployments of tide gauges if they are to be used to monitor the shelf-scale ocean circulation. Coastal MDT can be affected by local processes, e.g., strong tidal flow around headlands and therefore, the location of the tide gauges should be exposed to the open ocean and at distance to areas where local processes dominate." (l. 575-585)

[R2.3] The final paragraph of section 5 (lines 446 to 451) would benefit from some edits. The "good agreement" is qualitative and subject to a number of caveats (for example, the removal of some stations, and the uncertainty in the geodetic measurements). Also I think that the statement that "The agreement gives confidence that GoMSS captures the mean circulation, including the effect of tidal rectification…" is a little misleading. I believe that the set down at Yarmouth is the evidence of tidal rectification, but I struggle to see anything in the geodetic measurements to really support this.

We have reworded that part of the text that now reads:

"The overall agreement of $\Delta\eta_c$ estimated independently by the hydrodynamic and geodetic approaches provides validation of the ocean model. This gives confidence that GoMSS captures the mean shelf-scale ocean circulation on the Scotian Shelf and in the Gulf of Maine. While there are some limitations to this method (see Section 7), the dynamical interpretation of $\Delta\eta_c$ below illustrates that the alongshore tilt of MDT is an integrated measure of nearshore ocean dynamics highlighting its usefulness for model validation. Specifically, the next two sections, the address the following questions: What can the alongshore tilt of MDT at the coast tell us about shelf circulation? What are the implications for coastal monitoring?" (Lines 235-241)

Additionally, we have added the following text to the discussion in Section 7:

"It is important to note that some tide gauges are deployed in geographic settings (e.g., inside harbors or behind sandbanks) that are not resolved by ocean models and where sea level variations are likely dominated by local processes. Therefore, a comparison between observed and predicted MDT is only meaningful in locations that are relatively exposed to the open ocean. At the tide gauges considered in this study, the uncertainties in the geodetic MDT estimates are less than 1.6 cm and are primarily due to estimated error of the geoid

model. With the ongoing efforts to improve these models, the uncertainties are expected to become smaller in the future. Nevertheless, the use of $\Delta\eta_c$ *for model validation is limited to regions with long, geodetically referenced sea level records." (l. 535-541)*

In conclusion I think that the paper is interesting and worthy of publication. However I think that it needs some significant reformatting to improve the readability, and perhaps a reduction in scope to focus on the key messages.

Thanks again for your feedback! Your comments are much appreciated.

References

Csanady, G T. 1979. "The Pressure Field along the Western Margin of the North Atlantic." *Journal of Geophysical Research* 84 (C8): 4905. https://doi.org/10.1029/JC084iC08p04905.

Mertz, Gordon, and Daniel G. Wright. 1992. "Interpretations of the JEBAR Term." *Journal of Physical Oceanography* 22 (3): 301–5. https://doi.org/10.1175/1520-0485(1992)022<0301:IOTJT>2.0.CO;2.