Response to Reviewer #1

The paper investigates an observed asymmetry in the response of coastal currents to fluctuating wind stress on a bank offshore of Korea. Observations are supplemented by a realistic model in which the asymmetry persists. Using momentum balance analysis and an idealized model, the authors identify spatial differences in mass divergence driven by Ekman transport as the primary mechanism underlying the observed asymmetry. The findings are then generalized to account for differences in offshore circulation and wind patterns, suggesting that such asymmetries may not be limited to the study area. The paper is well-conceived, and the text and figures are generally clear.

I recommend the manuscript be accepted with only minor revisions

Response: We thank the reviewer for the positive assessment of our manuscript. The reviewer's careful reading and constructive comments have been helpful in polishing the manuscript. Our detailed point-by-point responses are provided below.

1. The additional sensitivity experiments in response to reviewer suggestions are not discussed in the manuscript. In discussions where these additional experiments would strengthen or nuance arguments, it may be beneficial to mention them (as "not shown" or perhaps even in the appendix/supplementary material). For example, the authors state that they find congruence between modifying bottom friction and the ATW framework; this may be appropriate to present in some way.

Response: We thank the reviewer for this constructive suggestion. We agree that incorporating the findings from our additional sensitivity experiments strengthens the interpretation of our results. Following the reviewer's suggestion, we have incorporated the key results from the sensitivity experiments (modifying bottom friction, uniform density, and steady wind stress forcing) into Sections 3 and 4. To maintain the concise flow of the manuscript, we have decided to present these findings in the text and reference them as "not shown".

We have added the following sentences to the revised manuscript:

Lines 278-281: Experiments with steady westward and eastward wind stress indicated that the alongshore surface currents reached nearly steady values comparable to the peak currents in the oscillatory wind experiments with 8-day and 12-day forcing periods, with the eastward wind case showing less stable *variability in surface currents (not shown).*

Lines 309-312: Furthermore, experiments with varying bottom friction suggest that current variability and cross-shore sea level gradients in the western region become stronger as bottom friction decreases (not shown), which is consistent with the ATW framework, where friction controls the spatial adjustment of the response.

Lines 341-345: An experiment with uniform density suggested that the asymmetric response over the bank remained present, indicating that stratification is not essential for the asymmetry itself (not shown). However, if stratification is sufficiently strong to keep the Ekman layer shallower than the depth, oscillatory alongshore wind stress may weaken or suppress asymmetric responses over the bank because alongshore convergence and divergence would be weak (Hsueh and Kenney III, 1972; Jung and Cho, 2023).

2. The sensitivity experiments presented in the results section (and those in response to reviewer suggestions) are not mentioned in the methods section; a brief note on the changes to the wind stress period and magnitude may be appropriate there.

Response: We thank the reviewer for pointing this out. We agree that all sensitivity experiments, including those added during the revision process, should be introduced in the "Data and methods" section. Following the reviewer's suggestion, we have added a new subsection at the end of Section 2. This subsection now outlines the variations in the wind stress period and magnitude compared to our control run. In addition, we included a brief description of the experimental setups conducted in response to previous reviewer comments.

The following subsection has been added to Section 2:

2.4 Sensitivity experiments

In addition to the control simulation, which is forced by an oscillatory alongshore wind stress with a period of 8 days and an amplitude of 0.05 N/m², a series of sensitivity experiments were conducted to evaluate the robustness of the modeled asymmetric response and to assess its dependence on wind stress characteristics. To test the sensitivity to wind periods, periods of 4 and 12 days were used. Additionally, amplitudes of 0.025 and 0.075 N/m² were applied to *test the sensitivity to wind amplitudes. Furthermore, additional idealized experiments were performed to examine specific dynamic contributions. To test the role of bottom friction, weaker ($5x10^{-5}$ m s^{-1}) and stronger ($5x10^{-3}$ m s^{-1}) bottom drag coefficients were applied. A uniform density experiment was also conducted to assess the influence of stratification on the occurrence of the asymmetry. Finally, steady westward and eastward wind stresses were applied to compare with the oscillatory forcing responses. Although only the results of the sensitivity experiments on wind amplitude and period are presented in detail, the key findings from the other experiments are briefly mentioned to support the physical interpretation of our results.*

3. Although the points presented in Figures 13 and 14 are valuable for the generalization of the findings, the figures themselves may perhaps be combined, moved to an appendix or supplementary material, or otherwise be reduced. Since the sensitivity experiments are shown to produce a linear response, the number of subplots may be reduced, in my opinion. For example, perhaps the first two panels in Figures 13 and 14 are sufficient for the body of the manuscript and could be combined into one figure, while Figures 13c, d and 14c, d could be removed or moved to an appendix/supplementary material.

Response: We agree with the reviewer that condensing these figures improves the overall flow and readability of the manuscript. Figures 13a,b and 14a,b are now combined into a new Figure 13 in the revised manuscript, and the following sentences have been added to the revised manuscript:

Lines 286-287: Power spectral density analyses of these sensitivity experiments consistently exhibit a single dominant peak that exactly matches the period of wind forcing in all cases (not shown).

4. The discussion of the ATW framework, coastal-trapped lee waves, topographic Rossby waves, and stratification is now quite convincing; however, its presentation could be improved by a clearer introduction to which concepts will be discussed, how they differ, and how they would affect the asymmetry. This would primarily apply to the latter three, as the first is consistent with the explanations presented in the results section.

Response: We thank the reviewer for this constructive feedback. We agree that providing a clear roadmap at the beginning of the Discussion section improves the readability and logical flow of the manuscript. Following the reviewer's suggestion, we have added a new introductory paragraph at the very beginning of Section 4. This paragraph previews the ATW framework, coastal-trapped lee waves, topographic Rossby waves, and stratification, briefly defines their dynamical differences, and outlines how each mechanism is evaluated regarding its role in the current asymmetry.

The added introductory paragraph reads as follows:

Lines 294-301: To clarify the physical mechanisms driving the observed asymmetric current response over the coastal bank, it is necessary to evaluate several candidate dynamical frameworks. These include the arrested topographic wave (ATW), coastal-trapped lee waves, topographic Rossby waves (TRWs), and the role of background stratification. The ATW framework describes a frictionally arrested and quasi-steady spatial adjustment to wind forcing. In contrast, coastal-trapped lee waves are transient, propagating features generated downstream of topographic variations. Similarly, TRWs, generated by flow-topography interactions, could redistribute momentum and energy by propagating at intrinsic wave timescales. Finally, background stratification introduces baroclinic effects that can influence the observed asymmetry. In the following discussion, we evaluate each of these concepts to clarify their specific roles and limitations in generating the observed asymmetry.

5. The question of how representative the observational period is, and whether the asymmetric response to wind forcing is seasonal, is not addressed in the text. Perhaps a short note on the representativeness would be appropriate.

Response: We thank the reviewer for raising this important and practical point. We agree that addressing the temporal representativeness of our observations adds valuable context to the manuscript. As the reviewer pointed out, our observational analysis focuses on a specific period (July to September 2019), and the identified 8-day wind period represents a characteristic synoptic timescale during this window rather than a year-round constant.

While a comprehensive investigation into the seasonal variations of wind forcing is beyond the scope of this study, we addressed this question through our numerical modeling framework. Our sensitivity experiments confirm that the asymmetric current response over the bank robustly emerges across a wide range of forcing periods, as well as under steady wind conditions. This demonstrates that the underlying physical mechanism is structurally robust and representative of the bank's fundamental dynamics, regardless of seasonal variations in wind periodicity. Following the reviewer's suggestion, we have added a note addressing this representativeness in Section 4:

Lines 354-361: It should be noted that the observational data analyzed in this study span a specific period from July to September 2019. While the dominant 8-day wind period identified during this observational window may not reflect the full year-round wind climate, it effectively captures the characteristic timescales of synoptic weather events in this region. Although a detailed investigation into the seasonal variations of wind periodicity remains a subject for future study, our numerical sensitivity experiments demonstrate that the asymmetric current response over the bank robustly persists across a broad spectrum of forcing periods (from 4 to 12 days) and even under steady wind forcing (not shown). This consistent response indicates that the asymmetry proposed in this study is a fundamental feature of the coastal bank's general dynamics, independent of specific seasonal wind periods.

6. On line 118, the Coriolis parameter "f" is shown as "f"

Response: Corrected.

Response to Reviewer #2

I have read the revised version of the manuscript by Jung et al. entitled "Asymmetric response of coastal currents to oscillating wind stress over a coastal bank", together with the authors' response to my earlier review. While I was encouraged by the response, I am feeling disappointed with the revised manuscript. The authors do now talk about the arrested topography wave of Csanady and its likely importance for understanding their model results. However, little has changed in the manuscript to reflect this conclusion, as I document below in my Major Concern 1. Likewise, I am concerned about what the authors mean when they talk about the Ekman transport (what definition are they using – see Major Concern 2 below) and there is still the issue of the rectified flow that I continue to find strange. A common problem throughout is that while the authors answered the questions in my first review, it seems they did not adjust the manuscript to take account of the consequences of their responses. I feel I must therefore recommend another major revision.

Response: We appreciate the reviewer's constructive feedback and fully acknowledge the reviewer's concern. We have now revised the manuscript by incorporating our previous responses and explicitly clarifying key theoretical concepts throughout the text. Detailed point-by-point responses are provided below.

Major Concerns:

1. I find Section 4: Discussion to be particularly in need of revision. Starting on line 281, the authors acknowledge the likely importance of the arrested topographic wave (ATW) framework of Csanady (1978). But starting on line 300 and continuing through to line 308, they rule out any role for coastal trapped waves and even topography Rossby waves, claiming that the model response of governed by local dynamics. While I agree that propagating or transient waves are unlikely to be playing a role here, the ATW framework is highly non-local and involves a frictionally arrested response in the direction of topography Rossby wave propagation. This is analogous to the situation in Stommel's famous 1948 paper (I use this only as an illustrative example) in which a steady response to wind stress curl forcing exhibits a nonlocal, western-intensified response when the variation of the Coriolis parameter is included, thereby introducing the asymmetry associated with the group velocities of long and short Rossby waves. So, yes, the authors are correct to rule out transient waves playing a role but they are not correct in ruling out frictionally arrested waves. Particularly concerning is the text starting on line 304, and again on line 314, where they rule out a role for topography Rossby waves, including such

waves that "respond in a quasi-steady manner". The ATW framework is rooted in precisely such waves that respond in a quasi-steady manner! Likewise, on line 308, why do the authors concern themselves with stratified conditions when they state in their reply to Major Concern 5 of my previous review that "stratification is not essential for the asymmetry itself"?

Response: We appreciate the reviewer's rigorous physical insight and fully understand the reviewer's concern. We realize that the confusion arose entirely from our imprecise use of the word "local". When we stated that the response is governed by "local dynamics", our intention was strictly geographical, meaning the response is "bank-confined", as opposed to being driven by remotely generated waves propagating into the domain from outside. We did not mean a point-wise local dynamical balance, and we completely agree with the reviewer's assessment regarding the non-local nature of the ATW spatial adjustment. Furthermore, by ruling out TRWs, we only intended to rule out the role of propagating, transient TRWs, rather than the frictionally arrested, quasi-steady TRW dynamics.

Regarding the discussion on stratification, we included the stratified TRW period estimations because our control experiment includes background stratification. However, to eliminate any confusion and align with the reviewer's suggestion, we have clarified lines 341-345 to emphasize the role of background stratification.

To address these issues, we have revised Section 4 by removing the misleading term "local" and explicitly framing the process as a "bank-scale spatial adjustment".

The discussion section has been modified as follows:

Line 327-340: Given the shallow bank depth (< 30 m over the bank and < 60 m on the flanks), bottom friction is expected to substantially limit the propagation of coastal-trapped waves. Thus, although transient or some trapped wave responses cannot be ruled out, the wind-driven response over the shallow bank is more likely governed by topographic adjustment over the bank rather than by propagating wave processes. Another potential source of the asymmetry is propagating TRWs, which can be generated through the interactions between the flow and bottom topography. Under the conditions of the present idealized model along the steep bathymetric gradients around the bank flank (buoyancy frequency of 0.02 s^{-1} and bottom slope of 0.0026), the estimated intrinsic TRW periods under stratified conditions (e.g., Ku et al., 2020; Rhines, 1970) are *approximately 1 day and 0.6 days for wavelengths of 50 km and 100 km, respectively. However, the power spectral densities of surface currents and depth-averaged currents consistently exhibit a single dominant peak at 8 days (not shown), with no significant energy at shorter periods, including these intrinsic TRW timescales. In addition, given the bottom drag coefficient ($5x10^{-4}$ m s^{-1}) used in the idealized model, the associated spin-down timescale along the bank edge is approximately 0.7 days, indicating that propagating TRWs would be strongly damped in this region. This confirms that the observed asymmetry is governed by frictionally arrested, quasi-steady dynamics over the bank rather than by propagating wave processes.*

2. At the start of Section 4, the authors discuss the Ekman transport. But what do the authors mean by Ekman transport? Is this the vertically integrated model response to the wind stress minus the specified bottom stress that includes the bottom friction arising from the geostrophic current? The authors need to be clear about this. From the text, the bottom stress is clearly a contributing factor. Note also that in a quasi-steady state, as in the authors' model, the Ekman transport acts to the right of the stress in the northern hemisphere and I wonder if this is consistent with what the authors write in the text? Indeed, it should be easy to compute the Ekman transport by computing the vertically integrated flow that is perpendicular and to the right of the surface wind stress minus the bottom stress, the latter two divided by a representative density. Since the bottom stress is clearly strongest on the western side of the bank, where the currents are strongest (see Specific Comment 7 below) this could prove an interesting calculation.

Response: Thank you for this constructive comment. The term "Ekman transport" in our manuscript indeed referred to the vertically integrated transport driven by the net stress, which accounts for both the surface wind stress and the bottom stress. Although the surface wind stress causes the water to flow to the right of the wind direction, bottom friction-induced bottom Ekman transport causes the current to align more closely with the wind direction over shallow depths, driving the spatially asymmetric mass convergence and divergence. To avoid any confusion with pure theoretical wind-driven Ekman transport and to clarify our terminology, we have replaced "Ekman transport" with "vertically integrated transport" throughout the text.

3. Lines 320-325: The authors talk about stratification. It would help if they noted that in their model, stratification is not important, as shown by a sensitivity experiment which uses uniform density throughout.

Response: We thank the reviewer for this helpful suggestion. We agree that explicitly mentioning the uniform density sensitivity experiment in the text clarifies the primary driver of the asymmetry. As suggested, we have added the following sentences to Section 4 of the revised manuscript:

Lines 341-345: An experiment with uniform density suggested that the asymmetric response over the bank remained present, indicating that stratification is not essential for the asymmetry itself (not shown). However, if stratification is sufficiently strong to keep the Ekman layer shallower than the depth, oscillatory alongshore wind stress may weaken or suppress asymmetric responses over the bank because alongshore convergence and divergence would be weak (Hsueh and Kenney III, 1972; Jung and Cho, 2023).

4. I remain concerned about the presence of rectified flow apparent from Figure 6. I raised this issue as Major Concern 4 of my previous review but I am afraid I do not understand the response of the authors. To have rectified flow, one would think that the nonlinear terms have to be important but this is not what the authors claim and also seems inconsistent with the weakest wind stress amplitude case they consider: the weaker the forcing, the weaker should be the nonlinear terms. This has me wondering if the presence of this flow is something to do with the offshore boundary condition used by the authors and I suggest running an experiment with uniform density and a simple zero normal gradient condition on the normal velocity at this boundary and free slip on the tangential velocity at the boundary.

Response: We are grateful for reviewer's persistent and insightful questioning regarding the rectified flow in Figure 6. We understand the reviewer's concerns and agree that nonlinear terms might typically cause a rectified flow. However, as you correctly suspected, this rectified flow is not driven by nonlinear terms. In our model, it is a baroclinic feature caused by variations in bathymetry and the presence of background stratification. During westward winds (downwelling), warm water over the shallow region is rapidly advected to the deeper region, efficiently elevating the local dynamic height. In contrast, during eastward winds (upwelling), cold water takes longer to replace this deep warm water and restore the initial sea level. This temporal lag in the baroclinic adjustment between the two phases results in a rectified appearance over wind cycles. The reviewer's previous suggestion to run a uniform density experiment revealed that this rectified flow completely disappeared, definitively proving that it is a baroclinic response rather than a numerical boundary artifact or nonlinear momentum effect.

We would like to note that this rectified flow is not the main cause of, nor does it significantly affect, the primary asymmetric response discussed in this study.

Specific Comments:

1. Lines 16-17: Here is mention of the Ekman transport. I guess Ekman transport has to include that associated with bottom stress? Also, mention could be made of the ATW framework? Similar comments apply to the Conclusion section.

Response: Thank you for these helpful suggestions. As suggested, we have replaced "Ekman transport" with "vertically integrated transport" and have explicitly incorporated the arrested topographic wave framework in both the Abstract and Conclusion sections.

In Abstract:

Lines 16-17: The asymmetry arises from spatial differences in mass convergence and divergence driven by spatially varying vertically integrated transport over the bank, which is subsequently governed by arrested topographic wave dynamics.

In Conclusions:

Lines 374-378: This contrast arises because vertically integrated transport over the shallower region is more closely aligned with the wind direction, resulting in stronger convergence during westward winds and stronger divergence during eastward winds in the western region. The resulting asymmetric mass redistribution leads to enhanced energy flux convergence in the western region, consistent with the ATW framework, associated with enhanced current variability and stronger sea level gradients in the western region.

2. Line 43: Maybe “can respond” is better than “responds”?

Response: Changed as suggested.

3. Line 85: The orange curve has a number of peaks. I guess the 5.7 day peak is the one that is marginally the highest? It would help to say so.

Response: We have clarified in the text that the 5.7-day peak is the marginally highest one among the multiple peaks in the orange curve, as you suggested.

The text has been revised as follows:

Lines 85-87: Although the PSD at station F shows an additional peak at a 5.7-day period, which is marginally the highest among the multiple peaks, it also exhibits a prominent peak at the wind-driven 7.5-day period.

4. Section 2.1 and Line 142: The authors gave lots of correlations in response to Specific Comment 1 in my previous review but chose not include these in the manuscript?

Response: Thank you for pointing this out. We have now incorporated the specific correlation values provided in our previous response into Section 2.1.

Lines 157-159: Alongshore currents are well correlated with the alongshore wind (Fig. 2). The alongshore currents and the alongshore wind show statistically significant correlations (p-value < 0.05) at all stations except station E. The correlation coefficients at stations A, B, C, D, and F are 0.44, 0.57, 0.59, 0.46, and 0.42, respectively.

5. Equation (2): The authors told me what "z" means in their response but they still do not define "z" in the manuscript. I think they should.

Response: We have added the explicit definition of z following Equation (2).

Line 127: where z is the vertical coordinate, defined as positive upward with $z = 0$ at the surface.

6. Line 134: Over what region is the body force applied?

Response: We have clarified in the text that the body force is applied over the entire model domain.

Lines 137-138: was applied as a body force (Zhang et al., 2011) over the entire model domain to generate the eastward offshore current similar to that along the southern coast of the Korean Peninsula.

7. Line 204: The wind stress is only offset by the bottom stress in the shallowest water…and interestingly, the bottom stress is strongest on the western side of the bank!

Response: We agree with the reviewer's observation. As suggested, we have updated the text around Line 204 to explicitly highlight that the bottom stress is strongest in the western region.

Lines 221-222: while the wind stress is offset by bottom stress over the shallower region. Notably, the bottom stress is strongest in the western region under both wind directions.