Response to Reviewer 1
River effects on sea-level rise in the Río de la Plata during the past century

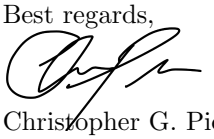
by Christopher G. Piecuch

*****Reviewer's comments in black*****
*****Author's responses in red*****

Dear Reviewer 1,

Thank you for your comments on my manuscript. They made the paper stronger, clearer, and more precise. I revised the manuscript based on your reviews, and point-by-point responses follow below. Thank you for your time and consideration.

Best regards,



Christopher G. Piecuch

Reviewer 1—

Review of "River effects on sea-level rise in the Río de la Plata during the past century" by C. Piecuch (OS 2022)

Summary: This manuscript investigates the dynamic link between the variability of Rio de la Plata discharge and coastal sea level variability observed in the immediate vicinity of its outlet, primarily focusing on the long timescales. It starts with an in-depth analysis of the few long tide-gauge and streamwflow datasets available there, which reveal increasing trend of both the discharge and the coastal sea level over the past century. Then it proceeds with two idealized modeling frameworks that are derived in order to explain the observed co-existence of discharge and sea level trends, in a causal fashion. The first framework is essentially a one-dimensional barotropic frictional model, where the water-air interfacial slope is balanced by the bottom friction in the along-estuary direction. This framework is applied to the inner estuary, around Buenos Aires. The second framework is a more complex two-dimensional baroclinic framework, where the plume would induce a coastal jet of brackish water, in thermal wind balance with a sea level higher at the coast than further offshore, off the plume offshore edge. It is applied further downstream, around Montevideo. It is concluded that both these modelings results, although based on highly idealized assumptions, stand in very good agreement with the observed relationship between discharge trends and sea level trends, for both regions. Hence it is concluded that the link between Rio de la Plata discharge trend and sea level trend in these two regions is causal in nature.

General comments: This study nicely tackles the long-lasting issue of long-term trends of sea level in estuarine ambients. It does so in a very relevant region, home of virtually the sole long-term observational records of the east coast of South America, itself in a very poorly observed basin as far as sea level is concerned: the southern Atlantic. The author makes clever use of the few observational records available, be it of streamflow or of coastal sea level. The approach is sound, and the results are convincing, in that the manuscript consistently backs its findings with statistical analyses. The merit of the study is to end up with a very simple conceptual framework that manages to explain the potentially complex and non-linear dynamics underlying the observed relationship between discharge and coastal sea level. This said, I am somewhat doubtful about the practical strategy of the author when it comes to test the validity of his two idealized frameworks against his observational findings, through the choice of numerical parameters of the required quantities (see my specific comment hereafter). I strongly encourage him to assess the relevance of his idealized models against one (or ideally several) OGCM outputs, typically considering the latest class of eddy-admitting CMIP6 historical runs. This would add considerable strength to the present manuscript. There are very concrete and practical implications of the findings reported here, as regards to the general understanding of the sea level budget and its closure over the southern Atlantic basin, as the observational databases available therein are deeply influenced by the handful of stations analyzed in the present manuscript.

I thank the reviewer for their positive review. Below they will find responses to all their comments.

Specific comments:

R1.CA l. 90: "To (...) reduce dimensionality": isn't there a more fundamental reason than just reducing dimensionality? Reducing the impact of observational errors, typically?

The revision identifies error reduction as a motivation for averaging the data.

R1.CB l. 195: "As a mental model": an actual schematic would help the reader here.

The revision includes a schematic illustration to aid interpretation (Figure 9).

R1.CC Table 4: shouldn't U be simply dictated by the geometry of the estuary at $x = 0$, given the value of q observed? It is unclear what relevance the tidal current has in this steady-state model. Is the value chosen for H_0 some sort of optimum resulting from a tuning, so as to achieve best consistency of the prediction wrt to the result of the regression analysis based on observations?

The reviewer identifies good questions here and in **R1.CD** and **R1.CE** below. To address these issues, I've now included a new Appendix B, which describes in more detail how and why the numerical values of the various model parameters were selected:

- *To evaluate Equations* (13) *and* (29)*, numerical values need to be assigned to the various model parameters. Here I detail the rationale behind the*

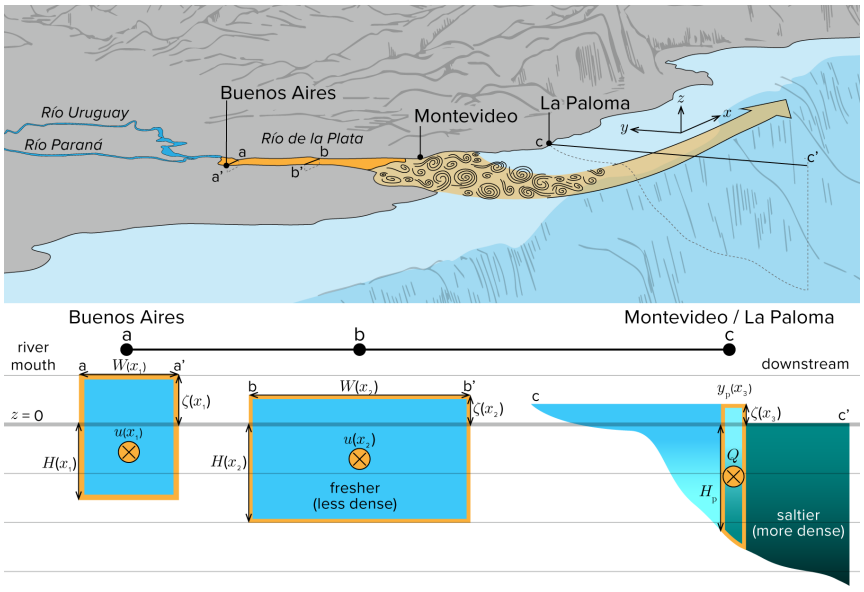


Figure 9: *Schematic of study region with key model quantities identified. Top shows plan view of region. Bottom shows cross sections at various locations in and around the Río de la Plata. Locations a and b are upstream near Buenos Aires, where the barotropic theory developed in section 4.1 applies. Location c is downstream of Montevideo near La Paloma, where the baroclinic theory developed in section 4.2 applies. In the bottom, yellow ⊗ identifies flow into the page, blue indicates fresher, less dense water, and green denotes saltier, more dense water. Other symbols and quantities are as defined in the text of sections 4.1 and 4.2. Illustration by Natalie Renier, WHOI.*

values tabulated in Table 4.

I chose standard reference values for freshwater density ρ_f, seawater density ρ_0, gravitational acceleration g, and the Coriolis parameter at the latitude of the Río de la Plata.

Ranges on the initial estuary width W_0 and depth H_0, and the width and depth scales L_W and L_H from Table 4 correspond to best estimates plus and minus two standard errors on these parameter values as determined by fitting exponentials in the form of Equations (10) *and* (11) *to the bathymetry data in Figure 2 using nonlinear least squares as described in the Figure 2 caption.*

The q value is the time-mean of the Río de la Plata streamflow time series in Figure 3 plus and minus twice its standard error.

Typical values for C_d range from 0.001 to 0.003 (Adcroft et al., 2018, 2019). I selected a middle-of-the-road value of 0.002.

The velocity scale U parameterizes the influence of unresolved processes, and is typically selected to represent tidal motions (Adcroft et al., 2019). Regional tidal-current amplitudes are 0.5–0.8 m s^{-1} (O'Connor, 1991; Piedra-Cueva and Fossati, 2007). Multiplying by a factor $2/\pi$, the average amplitude of a sine wave, gives the range of 0.3–0.5 m s^{-1} used here. This is larger than the background mean flow from river discharge, $\langle\overline{u}\rangle = q/H(x)W(x)$, which is 0.12 m s^{-1} at Buenos Aires ($x = 65$ km), using the q, H_0, W_0, L_H, and L_W values in Table 4 discussed earlier.

R1.CD l. 254-256: "These theoretical estimates agree with the coefficient of $(7.3\pm1.8)\times10^{-6}$ m m^{-3} s and the streamflow-driven sea-level trend of 0.71 ± 0.35 mm yr^{-1} found earlier from regression analysis of observed streamflow and sea level at Buenos Aires": indeed, the values do agree very, very well. Hence it is needed here to get a feel of the extent of ad-hoc tuning, implicit in the choice of parameters listed in table 4, so as to ensure this quasi-perfect match.

Please see my response to **R1.CC**.

R1.CE l. 257-260: I have the same concern for the results of the Montevideo idealized model.

Please see my response to **R1.CC**.

R1.CF l. 322: "Theories developed here may be helpful in this regard": indeed, nowadays there is a whole batch of centennial model outputs that became open for public use, several of which resolve—at least partly—the baroclinic Rossby radius of deformation at the latitude of Rio de la Plata. As part of CMIP6 for instance, multi-centennial historical simulations of the present climate as well as century-long projections have recently become commonly available (see e.g. Held et al 2019 https://doi.org/10.1029/2019MS001829, among many others). If indeed the two idealized dynamical balances proposed in the present study successfully explain the observed relationship between discharge variability and coastal sea level variability in the estuary at long timescales, this relationship should be captured by these long model simulations that have the full physics required to capture these, and much more (in particular that have realistic mixing schemes, and do not impose the idealized frontal structure of the plume density nor its linearly stratified density profile). These OGCMs have, with their 1/4° typical resolution in the ocean, the capability to resolve to a fairly large extent the thermal-wind balance invoked in the present idealized framework. I strongly encourage the author to consider at least one of this class of state-of-the-art model historical simulations, and to assess the relationship between Rio de la Plata discharge trend, along-shore equatorward coastal current trend slightly downstream of the outlet, and cross-shore sea level slope trend, at the long timescales of interest here. If the idealized framework presented in the present

manuscript holds there as well, this would add considerable strength to the results reported here (as the conclusion would not depend in any fashion on the potentially subjective choices of parameters listed in Table 4). The encouraging results observed from altimetry call for such an independent assessment of the idealized framework.

I forgo analysis of high-resolution climate-model output because, while it would substantially expand the paper's scope, it would not add value to the manuscript:

1. As the reviewer acknowledges, one of the paper's merits is that it presents a simple mechanistic framework for interpreting the observed relationship between discharge and coastal sea level. Including the kind of multi-model analysis advocated by the reviewer would require substantially expanding the scope and content of the manuscript. Doing so would detract from the clear and focused message, and result in a fundamentally different paper.

2. It's unclear whether current climate models resolve the relevant physics. For example, GFDL CM4.0, referenced by the reviewer, has a horizontal resolution of ~ 25 km. This is larger than the first baroclinic Rossby radius of deformation related to Río de la Plata streamflow, which is ~ 10 km (cf. Figure S3 in Piecuch et al., 2018). Indeed, Holt et al. (2017) determine that $1/12^\circ$ models only resolve the first baroclinic Rossby radius in $\sim 8\%$ of ocean regions shallower than 500 m, meaning that much higher resolution models are needed to faithfully represent coastal processes and shelf seas. Likewise, the geometry of the estuary also poses a challenge for models like CM4.0. For instance, that model's 2-m vertical spacing near the surface (section 2.1.4 in Adcroft et al. 2019) is coarse compared to the shallower depths at the head of the estuary (Figures 1 and 2 here).

3. Relatedly, global climate models parameterize boundary friction effects. For example, GFDL CM4.0 represents bottom boundary layer stress in terms of a quadratic bottom drag, which requires the selection of a constant bottom drag coefficient and a velocity scale (section 2.2.4 in Adcroft et al. 2019), just as in the analytical model here. Thus, high-resolution climate models like CM4.0 don't offer an escape from having to place subjective numerical values on model parameters for representing important physical processes.

4. Even if they resolved the basic physics, currently available high-resolution global climate model solutions wouldn't allow for more unambiguous causal attribution. I would still be restricted to a statistical analysis of relations between sea level and streamflow, which would carry the same caveats as the observation-based correlations and regressions presented in the paper. In other words, to rigorously quantify sea-level changes due to streamflow variation would require forcing perturbation experiments where discharge is alternately turned on or off in a given run (Chandanpurkar et al., 2022). Including such a modeling analysis would require coordinating numerical simulations with modeling centers, which is beyond the scope of this study.

While I haven't undertaken the modeling exercise suggested by the reviewer, I acknowledge their points in the revised discussion section by including the following sentence advocating for future studies to perform such experiments:

- *In the future, as global climate models with improved representation of the coastal ocean and shelf seas become more widely available, numerical experiments could be performed to broadly test analytical predictions made here regarding relationships between sea level and streamflow.*

Technical corrections:

R1.CG l. 48: centred

The sentence is written in the present tense and so the word "center" is preferred to "centred".

R1.CH fig3 caption: "thick black" line is not seen

Thanks for catching the typo. It's been corrected in the revision.

R1.CI l. 149-150: it should be Sections 4.1, 4.2 and 4.3

Thanks for catching the typos. They've been corrected in the revision.

R1.CJ l. 249: should be sections 4.1 and 4.2

Thanks for catching the typos. They've been corrected in the revision.

References

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