General comments:

This observational and modeling study explored swimmer hazards in an understudied setting, where estuary mouth flows encounter surfzone currents including bathymetric rip currents and boundary rip currents with large tidal variations. The authors found that river channel morphology can facilitate not only strong estuary flows, but also strong rip current flows when the river channel modifies wave breaking patterns, similar to what occurs in a surfzone bar-channel system. In addition, prior studies of surfzone hazards have typically found hazard to depend on the water level, which modifies wave breaking, with no dependence on the tidal phase (ebbing vs flooding); in contrast, this study found bathing hazard was different during rising versus falling tides when ebbing or flooding estuarine flows were interacting with the surf zone. I found these conclusions to be very interesting, novel, and supported by the analysis. I do have several concerns about (1) the framing of the paper, (2) the forecasting hazards analysis, and (3) the clarity of the text and figures.

Thank you for your comments, these have greatly improved the paper. I am pleased you found the conclusions interesting, novel, and supported by the analysis.

General comment (1): The paper emphasizes estuary mouth flows and bathymetric rip currents. Some discussion of headland/boundary rip currents is included but should be expanded given the clear importance of the boundary flows in this system. In addition, the paper lacks discussion of other rip-current types like flash rip currents, which I would expect to be present, as well as embayment rip currents, which would form in the center of an embayment rather than at the boundaries. Given the importance of the flows resulting from the embayment geometry, I wonder if the title and framing of the paper should be adjusted to "Combined surfzone, embayment, and estuarine bathing hazards."

While we recognise that boundary flows are important at this site and discuss them in the results and to some extent in the discussion (including comparing velocities and surfzone exit rates to those found in other boundary rip current studies), the paper's focus and novelty is on what happens when estuary flows enter a surfzone. To maintain a reasonable word count, boundary rips and flash rips are not dealt with in detail in this paper. However, as boundary rips are mentioned in the results, I've added the following text to the introduction:

"The body of previous research has demonstrated various forcing mechanisms for rip currents (Castelle *et al.*, 2016), including hydrodynamic instabilities in the surfzone ('shear instability rips' and 'flash rips'), bathymetric control of wave breaking and return flows ('channel rips' and 'focus rips'), and control of wave driven flows by headlands or other boundaries ('deflection rips' and 'shadow rips')."

And have added the following text to the discussion about cellular rip currents:

"It is also noteworthy that the main river channel tends to exit seaward in approximately the middle of the embayment, albeit with some variation in its position (Section 4.3.3). Narrow embayments with curvature at the shoreline such as Crantock have previously been demonstrated to promote cellular rip circulation (Castelle and Coco, 2012), where seaward flows form in the centre of the bay, especially during energetic conditions (Castelle *et al.*, 2016). This wave-driven process may, therefore, influence the position of the river channel at Crantock, by promoting channelisation in the middle of the beach and enhancing seaward flows."

I’ve also mentioned flash rips in the new limitations section of the paper:

“The surfbeat mode of XBeach was employed in this study, which captures the wave variations and associated wave-driven flows at the wave group (infragravity) timescale (Roelvink *et al.*, 2010). Therefore, transient flows driven at the incident wave timescale such as flash rips (Castelle *et al.*, 2016) are not captured by the model. However, given the topographic control over wave breaking and circulation on this coastline (Austin *et al.*, 2010; Austin *et al.*, 2014; Scott *et al.*, 2014), bathymetric and topographically controlled rips driven by wave group scale forcing are far more common.”

General comment (2): The forecasting bathing hazard section isn’t clearly described and is lacking important details. In particular, the analysis relies on a look-up table of hazard statistics from a prior study, but the authors don’t provide a summary of this study or it’s applicability to their study. The authors emphasize how this study site and combination of processes is understudied, so it warrants some explanation why a hazard model developed for a different setting would be the right choice here. The relationship between risk, hazard, and exposure isn’t explicitly stated, and there is some redundancy in presenting all of these separately. Proportions of hazard scores are presented, but a forecast skill assessment should be performed.

Thanks for your feedback on this section; on reflection I agree that this section was far too brief and not clear. I have now re-written many of the paragraphs in this section in order to remove the redundant terms (e.g. risk is no longer referred to) and given a more detailed explanation of the hazard scoring. Importantly, I’ve clarified how we optimised the hazard scoring based on our data, as the thresholds were not from the previous paper, just the overall approach of combining seperate hazard scores. Traditional skill scores cannot be applied as the predicted variables (Uoff and E) don’t directly match any of the measured variables (incidents, hazard, exposure), so we can’t compute R2, RMSE, AIC, etc. Instead, I’ve clarified how we used the Probability Of Detection metric (also known as Recall or Sensitivity), which is simply the rate of true positives and false negatives achieved, as this is the only useful skill score that can be determined from this sort of data. I’ve now added significant text to Section 5 to explain this.

General comment (3): Prior to publication I think some improvements to the clarity of the text and figures are needed, particularly to emphasize limitations of the study and make sure that key results are discernable in the figures (see Specific comments).

Thank you for this valid suggestion. I have now included a limitations section within the discussion and have improved the clarity of Figures 9, 11, and 13, by using a similar style to Figure 8c/f.

Specific comments:

L38-43: This text suggests that rip-current patterns/behaviors, rather than speeds, classify the hazard level. This is then inconsistent with the next statement that places importance on speeds. I think this section would be clearer if the authors started with a statement that it is expected that a combination of factors, including pattern and speed, influence the hazard.

I have now included the following text there:

"On beaches with rip currents, a combination of factors, including circulation pattern, speed, and surfzone retention influence the bathing hazard."

L46: Here it is concluded that flows in estuary channels may pose "an equal or potential even higher bathing hazard than rip currents" because the speeds are equal or greater than the speed of rip currents. I don't think this is known.

I'm not quite sure what you mean here – do you not think that the velocities are greater in an estuary, or do you not think that the hazard could be equal or higher than that of a rip current? Hopefully we have provided sufficient evidence from the literature to pose this as a hypothesis at least. We are not claiming to know this with certainty, just pointing out that the literature suggests that (a) estuary flows are strong relative to rips and (b) this represents an understudied hazard (as flow speed is routinely related to hazard level in rip studies).

L75: "embaymentisation ratio (length/depth)" Maybe "headland amplitude" and "embayment width" would be clearer terms than depth/length? Labeling these scales in Figure 2 could be helpful too.

I have added "(alongshore length/headland length)" to the text to clarify this.

L75-77: When do boundary rip currents occur, in which fast flows are along the headlands, versus a headland circulation, in which the fast flows are in the middle of the embayment? If a headland circulation is occurring some of the time, could this enhance the flows out of the estuary channel?

This is a good point, and rather than mentioning it briefly in Section 2, I've added it to the discussion section, as per General Comment (1) above.

L82: It may be helpful to label features such as "ebb tide delta" on the figure.

Thanks for this suggestion, I've added that to the figure.

L98: Did you introduce available data on water users?

This is now mentioned in Section 5 where we talk about the lifeguard head counts

L133: Give some information on how the echosounder and UAV datasets are merged. Is this the same as the process described later for the model bathymetry?

I've now signposted section 3.4, which explains the merging of the survey data sets

L136: Flow measurements were at 0.1 m above the seafloor. It would be worth describing why this is expected to be a good representation of swimmer hazard, and if there are times when it might not be.

The Eulerian measurements are not used to describe swimmer hazard. We are primarily using them to characterise surfzone velocities where estuary flows occur, and further using them for model calibration/validation.

L144: Are there concerns about when drifters may be poorly tracking the currents, e.g., if they are scraping the seafloor or surfing waves?

I've added the following text to clarify this:

"Lagrangian measurements were collected using GNSS-tracked surfzone drifters (**Error! Reference source not found.**), which are designed to mimic a static bather being carried by the surface flows (submerged approximately 0.5 m beneath the surface) and avoid surfing landward on waves. These were telemetered in real-time allowing shore based logging using QPS Qinsy software package (following Mouragues *et al.*, 2020). Six drifters were deployed at numerous locations multiple times across the survey area throughout the tidal cycle and were retrieved from the shallows before they ran aground."

L175: can delete "respectively"

Done.

L200: "tidal variation was imposed uniformly on all four corners of the model domain" – what does this mean?

This means that the water level was increased in a spatially uniform manner, i.e. there is no tidal gradient across the model domain. I've altered the text to clarify this:

"tidal variation was imposed uniformly across the modal domain."

L206: Wave directions are mentioned here, but not wave directional spread, which seems to vary tidally in the observational record. How does this influence the results of this study? I don't think spread was varied in the model runs? In addition, it would be helpful to know if the observed wave spectra are well described by a single peak period and direction, or if a wave systems approach would be more accurate. How does this affect the results? I suggest referencing the observations here to say how the model spans the observational conditions (shown for this year, is this similar for other years?).

Thanks for this suggestion. We haven't explored the influence of wave directional spreading in this paper, although it was varied during the calibration and validation runs. For the 72 model runs used to populate the hazard look up table we used the average spreading value for the site of 30 degrees. I have now mentioned spreading in the text and added a table of forcing conditions in Section 3.5 to clarify this. We also do not explore bi-modality in the spectra in this study, because it is not deemed important at this site due to the narrow range of wave approach angles on this coast and lack of energetic wind waves during typical bathing conditions. I have added some text to the new limitations section to reflect these omissions:

"The influences of wave directional spreading and bi-modality in the wave spectra have not been explored in this paper."

L213-223: Does this method of forcing the estuary flows miss any river-estuary interactions, or does it include them because it's based on a measurement near the estuary mouth? A small discharge is added to "conservatively account for fluvial flow" – can you elaborate and say if the results are sensitive to these choices?

The river input is small relative to the estuary input (typically <2% of the spring tide discharge) at this beach and is expected to also be small at other similar sites (as per reference in first para of introduction). The only river-estuary interaction I can think that would be relevant to bathing hazard is an enhancement of flows, which has been accounted for conservatively by adding the 5% exceedance river discharge (which rarely occurs during the summer bathing season). The surfzone flows are not sensitive to this level of discharge, as we found in initial tests. I have added the following text to clarify this:

"For the scenario simulations, the discharge applied at the boundary was computed from the estimated spring and neap tidal discharge rate at a given point in time, plus an additional 2 m^3/s to conservatively account for fluvial flow (5% exceedance river discharge). However, initial tests with only fluvial discharge applied showed that this fluvial discharge rate has a negligible effect on surfzone flows.**"**

L232: "Each virtual drifter was advected for 20 minutes, or until they had returned to a safe water depth (<0.7 m)." Could you elaborate on these choices and how they affect the results? Why 20 minutes? Why is a safe water depth 0.7 m? Why not keep running the drifters to see what happens next even if they enter safe water?

0.7 m is a minimum 'safe' depth taken from the cited studies by McCarroll (2014 and 2015). I have added text to clarify this:

"Depths shallower than 0.7 m are deemed 'safe' as bathers can stand up without being swept off their feet by typical surfzone currents."

I have also added the following text to elaborate on the 20 minute timeframe:

"The 20 minute timeframe was chosen to represent a typical timescale of a bathing incident – it is likely that a person in a strong current would either be rescued or in a critical state within 20 minutes. Furthermore, as we simulate with non-stationary tides, leaving drifters to circulate for longer blurs the effects of different tidal stages."

L246: should this be lowercase u_off?

Thank you, now corrected.

L328: Mention what may be the cause of these fluctuations (e.g., related to infragravity pulsations, instabilities, or flash rip currents?).

We did not study the time-variation in the measured drifter velocities in this paper. The mentioned average velocity is a spatial average. I have added the following text to clarify this:

"The spatially-averaged lagrangian velocity during this phase of the tide was 0.3 m/s, with peak velocities exceeding 0.6 m/s…"

L355: Given the spatial complexity of the flows, a point to point agreement may not be expected. You could plot the modeled maximum flows and flow range within a spatial region around the observational point for a more fair comparison?

We completely agree, the stochastic nature of the wave-driven flows means we would never expect the real/virtual lagrangian driftrers to agree exactly. The purpose of the lagrangian comparison is to check qualitatively that the spatial circulation pattern is approximated by the model, while the Eulerian data are used to provide quantitative comparison. This is explained in the text.

L395-404: There is little or no discussion of rip currents here. How does the variation in rip current strength and characteristics with wave power or other factors influence the hazard metrics?

Rip currents are not the focus of this paper, and we are not trying to characterise changes in rip flow under different forcing. The focus of the paper is to characterise how estuary flows influence bathing hazard when they enter a surfzone. Rip currents occur at this beach during mid and low tides, but we are mainly using the low tide E and Uoff values to provide context to the values driven by estuary flows at high tide. E.g. this beach has typical rip current characteristics at mid-low tide, but enhanced flows at high tide due to the estuary. This is discussed in detail in the discussion section

L427-429: The ebb shoal delta acting as a bar rip system is interesting. Is this mentioned elsewhere in the literature, e.g., papers on flows near a small river mouth encountering a surf zone (Kastner et al., Rodriguez et al., 2018).

Thanks for these references, I had previously struggled to find existing examples of this in the literature. From Kastner et al I also found a good citation (Olabarrieta et al) for wave-driven flows over an ebb-shoal delta. I have now included these in the introduction and discussion

L510: It seems the authors are following a prior method (Austin et al., 2013), but more explanation here of how these thresholds and scores were developed is needed to understand this section.

This has now been expanded and explained better. We are not using the thresholds of the previous paper

L512: Assuming I'm correct that Risk = Hazard x Exposure (which isn't spelled out here), and Exposure is a measured quantity, it seems redundant to me to present accuracy results for both Risk and Hazard.

This section has been re-written to better explain these variables. To lifeguards who will ultimately implement the science developed here, understanding how many incidents occur at each predicted hazard level is of equal importance to understanding how much hazard occurs. Also, while assessing how the observed level of hazard increases at each skill score tells us qualitatively how the system performs as a hazard forecast, the number of incidents (risk) can be used to determine the predictive Recall (rate of true positives/false negatives), which is the most useful skill metric that can be generated from this data.

Figure comments:

Figure 1: x and y axes are not labeled in the top panel.

This has now been clarified in the caption

Figure 5: The panel b transect where the Gannel estuary enters the beach shows the topography over a long distance, which does not seem necessary, and makes it harder to see the channel. It may be more useful to see a transect across the shoals to show the scales of the estuary channel and other channels connected to the swimmer hazards.

This has now been zoomed in to show the channel morphology more closely

Figure 6: Were other days similar? This could be interesting to look at as a composite, though maybe that would be difficult given the variation in the behavior with tidal range.

Other days were very similar in the overall signature. I've added the following text to Section 4.1 to mention this:

"Each of the measured tidal cycles showed a similar hydrodynamic signature"

Figure 7: Do other time periods look similar? It could be interesting to show a scatter plot comparing measured and modeled flows.

Again, other tidal cycles were basically the same comparison, so we chose to show just two tidal cycles to demonstrate the pattern and ability to replicate the pattern. These and the skill scores are deemed sufficient to demonstrate the model fit to the data.

Figure 8: This figure compares observed drift tracks with gridded results of model drift tracks. It may be helpful to also show example model drift tracks (not expected to reproduce the observed tracks, but presumably similar patterns), and gridded observational data, so that there's a more one to one comparison. Does this figure only show two of the three regimes described in the text (L374-379)? I suggest using colormaps that are colorblind friendly and perceptually uniform (e.g., Thyng et al., 2016).

Thanks for this suggestion. I've now included some example virtual drifter tracks (showing all would cluter the figure). I've also changed the colormap to exclude the use of red and tested this on a colorblind simulator. The figure does shows the 'main' regimes of interest, i.e. the high-ebbing tide when the estuary is active and the low tide period when the estuary is inactive.

Figures 9&11&13: Vectors are small and very difficult to see. Colormap for the vectors includes dark blue, which is the same color as the background water color. This figure shows the variation in the wave breaking, water levels, and inundated bathymetry, but is not readable for information about flow patterns.

On reflection, I completely agree that this figure was not well conceieved. I've now simplified this significantly to show the seaward velocities and depth contours only.

Figure 10: This is an interesting figure, though a bit difficult to interpret. It's clear there is a difference between rising tide and falling tide. Showing line plots of Uoff vs the wave factor for three example tidal elevations for rising and falling tides may be helpful as a summary of this figure.

Thanks for this suggestion. I've now included an extra panel below the bubble plot to display this summary, as suggested.

Figure 12: I'm not sure I find the change plots very helpful. It may be more useful to show how the flows differ for the different cases, to show different spatial patterns, but similar magnitudes?

Figure 13 shows the mentioned flow comparison with different morphologies. On reflection, I agree that Figure 12 doesn't add much to the paper and in the interest of brevity I've decided to remove it.

Figure 14: Is proportion the most useful comparison? This doesn't show anything about the timing of events. Why isn't a forecast skill assessment shown? I'm not quite sure what to take from this figure.

Traditional skill scores cannot be applied as the predicted variables (Uoff and E) don't directly match any of the measured variables (incidents, hazard, exposure), so we can't compute R2, RMSE, AIC, etc. Instead, I've clarified how we used the Probability Of Detection metric (also known as Recall or Sensitivity), which is simply the rate of true positives and false negatives achieved, as this is the only useful skill score that can be determined from this sort of data. I've now added significant text to Section 5 to explain this.

References:

Kastner, S. E., A. R. Horner-Devine, and J. M. Thomson (2019), A Conceptual Model of a River Plume in the Surf Zone, J. Geophys. Res. Ocean., 124(11), 8060–8078, doi:10.1029/2019JC015510.

Thankyou, this is a highly relevant reference. I have now cited this in the intro and discussion

Rodriguez, A. R., S. N. Giddings, and N. Kumar (2018), Impacts of Nearshore Wave-Current Interaction on Transport and Mixing of Small-Scale Buoyant Plumes, Geophys. Res. Lett., 45(16), 8379–8389, doi:10.1029/2018GL078328.

Thankyou, this is a highly relevant reference. I have now cited this in the intro

Thyng, K. M., Greene, C. A., Hetland, R. D., Zimmerle, H. M., & DiMarco, S. F. (2016). True colors of oceanography. Oceanography, 29(3), 10. link: http://tos.org/oceanography/assets/docs/29-3_thyng.pdf, https://matplotlib.org/cmocean/

Thanks for this, we have now changed to a Matlab colormap (Parula) similar to their Haline colormap deemed suitable to colorblind readers