Thank you for your comments, these have greatly improved the paper.

Hydrodynamics: The paper describes a range of simulations with varying tidal levels and wave conditions, primarily representative of relatively quiescent conditions (Hs < 1.4 m). However, only results for varying wave power and tide are presented here. I have two concerns regarding this: First, the time series shows several larger wave events and more oblique waves than represented in the selection of simulations. Why did the authors select the set of hydrodynamics to simulate? Are they representative of times when swimmers are at the beach? Considering there are many users in HS3 conditions, should larger wave events be considered? Second, wave power is the only wave property presented here (Hs * Tp). Wave direction is only mentioned as 'unimportant' during mid-tide but is not otherwise described in the paper. Presumably, wave direction is important if waves are adequately oblique to reduce the energy entering the embayment. Wave direction is also shown to be important for channel rip hazards (e.g., Dusek and Seim, 2013). Is there little dependence on wave direction here due to the embayment geometry? Additionally, previous work on channel rip currents (e.g., Moulton et al., 2017) suggests that wave height is important for rip speeds but does not incorporate wave period. Assuming wave breaking is triggered at a wave height to depth ratio, wave height defines surfzone width. By presenting these values solely as wave power (which combines both period and height), this paper potentially glosses over some of the dynamics relevant for 'blocking exits', etc

The conditions we simulated cover a wider range of conditions than you mention in your comment - in Section 3.5 we had summarised the boundary forcing conditions as 'wave heights of 0.5–3 m, wave periods of 6–12 s, and wave approaches from 269°–304°'. The conditions therefore cover the full range of summer wave conditions experienced at the site and do not represent quiescent conditions as mentioned. However, to clarify this I've now removed these words and instead added a summary table of the forcing conditions, and amended the text in Section 3.5 as follows:

"Once the model was calibrated (Section 3.7), seventy-two combinations of wave and tide conditions were run in the model covering the full range of summer wave conditions (Table 1), with each set of wave conditions run over a mean neap tidal cycle and a mean spring tidal cycle (with 30 minutes spin up time). The most energetic conditions are approximately 3.5 times higher than the summer (June, July, August) average wave power, equivalent to approximately the 1-in-1 year return period and would be conditions under which the lifeguards would close the beach to bathers. Each 12-hour simulation was then divided into 1-hour tidal segments at 30-minute increments, providing 1,728 unique combinations of wave and tide forcing from which to evaluate circulation patterns and bathing hazard from the simulated flow fields."

Thanks for your suggestion to delve into the influence of wave direction in more detail. Wave direction was varied in the simulations, but only the mean wave direction was presented in the bubble plot in Figure 10. This was to simplify the results and to make the bubbles legible (i.e. not overcrowding the figure with too many data points). I have now added an additional few sentences (see below) to describe the effects of wave direction, in comparison to wave power and tidal stage. Following your suggestion of using wave height instead of wave power to present the results, I replotted figure 10 to see what the hazard pattern looks like when plotted as wave height (x axis) vs tide level (y axis). This doesn't change the overall conclusions one would draw from figure 10. However, conditions with the same wave height but different periods plot on top of one another, making the figure harder to interpret than the original. While I take your point about wave height being a key parameter controlling exits, and acknowledge that Moulton observes this, the Moulton study uses a limited range of wave periods (5-10 s observed, 7 s modelled). Furthermore, from the new analysis mentioned below, surfzone exits at Crantock are slightly more sensitive to wave power than wave height. Therefore, following your suggestion, I've kept the summary plot in Figure 10 the same, but have added the following text to comment on the influence of wave height, period, and direction:

"Figure 10 is presented in terms of relative wave power as Scott et al. (2014) and Castelle et al. (2019) both found this to be an important parameter in controlling the occurrence of rip incidents in southwest England and southwest France. Although they studied only a limited range of wave periods, Moulton et al. (2017) did not observe a dependence of rip current velocity on wave period and concluded that only wave height and direction (as well as water depth) were important for offshore directed flow velocity, due to their control on breaker-induced setup and alongshore current speed. Here we find that surfzone exits are slightly more sensitive to relative wave power (incorporating wave period) than wave height alone. Below mid tide when the estuary is inactive, Uoff and E varied up to 0.16 m/s and 51%, respectively, when averaged at each simulated wave height, while changing the level of wave power varied Uoff and E by up to 0.17 m/s and 63%, on average. The simulations therefore indicate that seaward flow velocity is influenced to a similar degree by either wave height or power, but that wave power exerts a greater influence on surfzone exits than wave height alone. Wave direction also appears to play some role in controlling the exit potential at Crantock. Below mid tide, wave direction varied Uoff by only 0.008 m/s on average, but E increased by 12% when wave direction was varied from the most oblique wave approach simulated (45°) to a shore-normal wave approach (0°). Wave directional influence is likely to be limited, however, by the highly embayed nature of the beach. Overall, the simulations indicate that tidal stage plays the most important role in controlling both surfzone exits and the velocity of surfzone currents at an embayed, estuarine beach like Crantock, as it varied Uoff and E by up to 0.44 m/s and 70%, respectively, when averaged at each tidal level."

Hazard quantities: The paper primarily relies on two metrics to define hazard: maximum offshore flows and percent surfzone exits. Both are helpful metrics to assess bather hazards. Percent surfzone exits represent a free-floating bather's likelihood of being ejected offshore (but does not represent speed). Maximum offshore flows target how fast a bather may be advected offshore and, therefore, the feasibility for a bather to react (swim) from an offshore flow (but does not represent the offshore flow distribution). These metrics do not represent the number of locations with sufficiently strong offshore flows to eject a swimmer offshore. Providing an additional metric could help. For example, this could be represented as the percent of the locations alongshore with Uoff exceeding a threshold value or, possibly, the Uavg and the rms alongshore of Uoff. This similarly ties into the section on how these flows change with morphology. While Uoff,max does not vary strongly, the position and possibly this distribution of these flows change. This could be explored with an additional hazard metric.

While I appreciate the point you are making about using another hazard metric that describes the spatial distribution of offshore flows, at this beach feeder currents are ubiquitous and direct bathers towards offshore flows that are either situated in the river channel, or in the headland rip channels at either side. We did test uoff over different sections of the beach (south and north) and in fact use these in the live forecast system, but including this in the paper over complicated the results, and didn't alter the key conclusions. Furthermore, the spatial distribution of the offshore flows is described (at least qualitatively) by the spatial plots in figures 9, 11, and 13, and in the text. Many previous studies use only the offshore directed flow velocity (for example Moulton et al. (2017)), and therefore it seems unnecessary for this study to use more than two hazard metrics, although I can appreciate the motivation for doing so that you suggest. The two hazard metrics used adequately capture the occurrence of past incidents (Section 5), as was also found in a previous study (Austen et al., 2013). However, we acknowledge that spatial distribution is not well captured by the metrics we used in the paper, so I have added the following to the text at the end of Section 3.6:

"To forecast bathing hazard (Section 5), Uoff and E were quantified at each time step across three different sections of the beach (northern half, southern half, and estuary mouth) to acknowledge the fact that offshore flow velocity varies in different places along the shore and to differentiate the hazard a bather might experience in one part of the beach from another. However, given the large range of forcing and bathymetric combinations described in Section 4, the results presented in Section 4 summarise the variables as a single value across the beach for brevity."

Hazard forecasts: While most methods and results are thorough, the forecasting bathing hazard section needs to be better described. I found the definitions of different terms and how the hazards were predicted and allocated difficult to follow, especially since some definitions are different in the figure caption. For example, the use of seemingly redundant terms (risk, hazard) and the overly brief explanation of the hazard scoring.

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.

Limitations: The paper should describe the limitations of these hazard predictions. For example, the modeled velocities are often underestimated, resulting in non-conservative hazard estimates. The surf-beat depth-averaged model cannot represent all rip current types (e.g., flash rips). The findings are highly tuned to this specific estuary and may not represent other combined surfzone and estuary flows.

We have now added a limitations section following your suggestion. This outlines the limitations you suggest, and also those around other relevant bathing hazards not considered by the forecast system.

Figure quality: Figures 9 and similar layouts are very challenging to read. The quivers are barely visible, and the figures are grainy. Consider using plots similar to Figure 8c,f.

Thank you for this suggestion. On reflection, I fully agree that those figures are too busy and that coloured quivers are not the best mechanism to show the flow behaviour. I have now replaced Figures 9, 11, 13 with simplified figures showing only the wave dissipation, depth contours, and bin averaged lagrangian velocity as colours (not quivers).

Line-by-line:

L126: I expect the SfM DEMS to perform well at GCP locations because those locations were input into the algorithm to resolve camera geometry. Thus, this may not represent the accuracy of the DEM well. Can the DEM accuracy be checked by comparing the regions overlapping with both surveys (intertidal zone)?

The SfM DEMs were not developed using the GCPs to tie in the DEM. The drone used has RTK capability, so positioning was provided by the on-board RTK corrections. The GCPs were only used to check the accuracy of the derived DEM and are therefore considered a suitable ground-truth of the DEM data. I have now added the following text to clarify this point:

"The DEM achieves a vertical RMSE of 0.03 m compared to independent spot checks against ground control points not used to geolocate the DEM during processing."

L133: How was this RMSE computed? By comparing with?

Thanks for pointing out this omission. I've now added some text to clarify:

"The bathymetric survey achieves a vertical RMSE of 0.1 m in the intertidal region, when compared to the previously mentioned ground control points"

L143: List drifter dimensions. Is it truly a surface drifter or representative of a depth-avg current?

The drifters are submerged approximately 0.5 m deep, so they represent a surface current in all but the shallowest areas. I have now added text to clarify this:

"The drifters were submerged approximately 0.5 m beneath the surface and therefore mapped the surface flow patterns."

Figure 5: Define acronyms in Figure.

We have now added definitions for all acronyms in Figure 5.

L264: How do these tuned values compare with previous studies?

I have now added comparison of tuning parameters with a previous (comparable) rip modelling study.

L336: Report bias since the sign is important here (i.e., if the flows are over or underpredicted).

Thanks for this suggestion, which I agree is useful. I have now added bias as a metric.

Figure 8: Why are there large drifter passes at single points in the inner surf zone? Do drifters stagnate there?

This is an interesting observation. These are quiescent areas in the inner surfzone where low velocity circulation between onshore and offshore flows is occurring. Therefore, I would agree that drifters are stagnating There. I have added the following text to Section 4.2 to reflect this observation:

"Interestingly, large numbers of virtual drifters are predicted to pass through certain points in the inner surfzone (**Error! Reference source not found.**, panel e), which are interpreted as stagnation points where quiescent circulation between onshore and offshore flow is occurring. These features are not present in the real GNSS drifter tracks because of the statistical limitations of deploying a small fleet of drifters, but the velocity patterns in these areas are reproduced."

Section 4.4: Is this supposed to be a new section?

Thanks for pointing this out. This has now been changed to a new sub-section (4.3.1… etc).

Figure 10: Error in the x-label -> HsTp/overbar{HsTp}.

Thankyou, this has now been corrected.

Section 4.6: The introduction claimed that the increase in swimmer rescues in recent years may be due to changes in the river channel. Here, the authors could add a simulation with synthetic bathymetry representing the previous, potentially less hazardous river channel to see how it compares with these morphologies.

Thankyou for this suggestion. While I agree this would be insightful in clarifying the contribution of the change in river channel to the increased hazard, it would require a significant modelling effort to generate a reliable synthetic bathymetry (in the absence of a measured bathymetry from that time) and re-run a comparable set of scenarios. Furthermore, we don't even know the depth of the river channel at that time, and this would elicit considerable uncertainty in the predicted velocities. Therefore, I have instead added a point to the new limitations section of the discussion that this would be a desirable future area of study:

"The high variability in the river channel morphology appears to not fundamentally vary the bathing hazard in terms of U_{off} and E, based on the four bathymetric data sets that were collected (Section 4.3.3). However, a more systematic change in the river channel morphology could feasibly occur, and this has not been simulated in the present study. For example, if the river channel were to be naturally or artificially relocated against the north headland (Section 2), then U_{off} and E near the estuary could be significantly increased as the channel would likely be straighter, narrower, and deeper. Conversely, U_{off} and E on the remainder of the beach could be drastically reduced, as the control of the river channels and ebb-shoal delta on the flows would be lost, as was the case prior to 2015. Lifeguards believe that it is the increased spatial variability in the flows, and resulting increase in bather exposure, that has increased the lifesaving burden on the beach. Future iterations of this research may seek to verify the additional hazard posed by the new river position compared to its former position against the north headland, by performing simulations with a synthetic river channel morphology that mimics the former river position."

L555: The spatial variability described here should be incorporated more often throughout the paper.

Thanks for this suggestion. I have now added text in the results section to mention the spatial variability.

L600: NOAA's rip current hazard forecasts consider hydrodynamic conditions (https://oceanservice.noaa.gov/news/apr21/rip-current-forecast.html). Perhaps specify that this is the only forecast model considering estuarine flow.

We appreciate that NOAA's rip current forecast does consider hydrodynamic conditions in terms of forcing. What was meant by this sentence is that this is the first forecast that provides detailed hydrodynamic predictions (in terms of temporal and spatial variability in the flow field), which NOAA's forecast and others does not (i.e. they simply provide rip warning levels). I have now clarified this sentence to make this point clearer to the reader, as we are not trying to detract from the excellent information that those systems provide at all:

"The forecast system developed for Crantock (Section 5) has been implemented operationally at the beach since 2022 and provides real-time warnings about where and when peak bathing hazards will occur, in addition to simplified flow visualisations, via novel digital display screens located at the two main beach access points (**Error! Reference source not found.**). To the best of our knowledge, this represents the first process-based forecast system used to provide bathing warnings directly to the public."

Reference:

Dusek, G., and H. Seim. "Rip current intensity estimates from lifeguard observations." Journal of Coastal Research 29.3 (2013): 505-518.

Thank you, I have now added this citation.

Moulton, Melissa, et al. "Comparison of rip current hazard likelihood forecasts with observed rip current speeds." Weather and Forecasting 32.4 (2017): 1659-1666.

Thank you, also now incorporated.