General comments

* This paper presents a novel way to predict rip current and shore break wave hazards, providing a new means to forecast these coastal hazards ahead of time. It uses a relatively novel physics-based approach to predicting these processes. The methods have been calibrated and validated over a single summer season at a beach in France using lifeguard perceived hazard estimates, indicating that the system performs very well against those test data. This contribution has the potential for wide reaching impacts in coastal hazard prevention through forecasting rip and shore break hazards (which is alluded to in the introduction), as long as the authors can address some of the issues I raise below regarding transferability of the approach.

> *Reply: We thank Reviewer #1 for their support for publication and constructive criticism. In our detailed response below you will see that all the comments have been carefully considered and required changes have been made. We warmly thank Reviewer #1 for their comments which really helped strengthening our paper.*

* The paper addresses a relevant scientific question that is within the scope of the journal. The approach is novel for the purposes of hazard prediction and the methods are clearly outlined. The results are sufficient to support the conclusions reached. In the introduction you argue that previous approaches have been 'validated on a limited number of beaches' and that 'more generic surf-zone hazard models' are required 'to be applied to a wide range of sandy beaches'. Given this justification for developing a new method of rip forecasting, I think more discussion is warranted on how feasible the methods would be for a large number of beaches (e.g. a national-scale rip forecast) and how transferable the methods are for non-lifeguarded beaches. I.e., is this a method that's useful and accurate but is only really feasible for a handful of high-risk sites where it's worth undertaking a lengthy calibration effort to gather lifeguard perceptions, or is this a method that can realistically be generalised and tested on a regional or national-scale? If so, briefly indicate how you would propose for this to be done?

> *Reply : Reviewer #1 raises an important point, and we agree that how our approach can be applied and transfered at larger scale deserves more discussion. This is now addressed in the Discussion section of the revised manuscript (see also reply to last specific comment) : "However, while parameters such as bar crest depth and channel depth are relatively simple, obtaining them remains challenging due to the difficulty of surveying the surf zone, which is not routinely monitored at most locations. This raises important considerations for the large-scale transferability of the models. Future applications will need to determine how these parameters can be feasibly obtained, whether through direct surveying, remote sensing, or empirical estimations based on regional morphology. Additionally, while the calibrated values used in this study may serve as a reference, their applicability to other sites remains uncertain, and further research is needed to assess whether re-calibration against lifeguard observations or other validation datasets is necessary at each new location."*

Specific comments

* Line 53: consider citing Stokes et al. (2024) 'New insights into combined surfzone, embayment, and estuarine bathing hazards' here for a current reference. They predominantly forecast estuary hazards, but they also forecast rips with a process-based model

Reply : Thank you for this suggestion. Stokes et al. (2024), which was not published at the time we submitted our manuscript is now cited where listing the different rip current forecast approaches

* Line 57: You should also cite Scott et al. (2014), 'Controls on macrotidal rip current circulation and hazard' here, which was their earlier paper describing the data and analysis, while this reference describes the application to lifeguarding. I would also mention that this final approach relied on lifeguard incident data for calibration/validation of thresholds

Rely : Thank you for pointing this reference which is now included with mention of the lifeguard incident data

* Lines 70-72: This could do with rewording - it's not been mentioned yet what the free parameters are, which in itself is not a problem, but it makes it confusing when you mention setting up the parameters with either beach morphology (presumably an input parameter) or lifeguard-perceived hazard data (presumably the target variable). Consider briefly summarising what the free parameters are, or explain how they relate to beach morphology and lifeguard perceived hazard.

Reply : This is now specified "... only require a limited number of time-invariant free parameters related to beach morphology and wave breaking onset. These parameters can either be given based to some knowledge of the beach morphology, or through calibration using lifeguard-perceived hazard data."

* Lines 72-73: This may be dealt with later in the discussion, but you mention earlier that a shortcoming of previous systems is that they were calibrated on only a few beaches, and that more generic models are needed to apply to a wide range of sandy beaches. However, in this study you only use a single beach. I would suggest in this final paragraph of the introduction you should manage the readers expectations - is this new system demonstrated on a single beach, or is it shown to be applicable to a wide range of sandy beaches?

Reply : We fully agree – We now specify that it is a single-beach application : "The proposed framework, here applied to a single beach in southwest France, offers new opportunities for ..."

* Lines 106-107: While the specifics of this Wavewatch model are not fundamental to your conclusions, it does influence your results to some extent, and I therefore think slightly more detail about the wave model is warranted. It would be good to know the extents of this model - is it a local area model or is it simulating waves along the entire French coast? How far offshore does it extend? Was it developed for this study? If this information is in another paper, then you can cite that instead.

Reply : Thank you for this comment. There is little meterial published on this model, which is the operational model of Météo-France running for operational sea state forecasting. More detail is now given in the revised manuscript : "The MFWAM (Météo-France Wave Model) based on the spectral wave model WAM (WamdiGroup, 1988) is the French version of the European Centre for Medium-Range Weather Forecasts (ECMWF) WAM model used by Météo-France for operational sea state forecasting, with a 0.1◦ grid resolution in the northeast Atlantic. It forces a high-resolution WaveWatch 3 wave model (Tolman et al., 2002), forced by winds from the ARPEGE model of Météo-France. The model uses an unstructured grid (Roland and Ardhuin, 2014), allowing the French Atlantic coast to be described with a *resolution of approximately 200 m, with mesh size increasing to approximately 10 km at the boundary of the model a few hundred of kilometres offshore. Different coastal processes are represented in this model, such as unified parameterization of wave breaking from offshore to coast, wave reflection at the coast, refraction due to currents and bathymetry, and bottom friction. Modelled wave conditions were extracted in approximately 10-m depth in front of La Lette Blanche beach, i.e. to estimate the wave conditions outside of the surf zone. "*

* Line 106: I suggest re-wording the sentence slightly - Wavewatch can use an unstructured grid, but it doesn't have to use one. I would briefly mention the min and max grid resolutions used and explain that the unstructured grid allows computational efficiency. You could after all resolve the French coast at 200 m with a regular square grid (although you wouldn't necessarily want to!).

Reply : see reply ro previous comment

* Line 110: As you've mentioned that the model can simulate coastal processes such as wave breaking, I think it's worth mentioning here explicitly that the model is not being used to simulate surfzone conditions - it is being used to estimate wave conditions just outside the surfzone.

Reply : see reply to previous comment

* Line 112: Please state over what time period the results were gathered.

Reply : this is now specified : "Over the period from July 1 to August 31 of 2022, results show ..."

* Line 134: I'm not sure I agree with how this is worded – rather than saying 'alongshore variability in depth of the sandbar' isn't it more accurate to say something like 'alongshore variability in depth between the sandbars and intervening drainage channels'

Reply : We agree with this rewording which can now be found in the reviesed manuscript.

* Line 147: The Larson et al. (2010) equation is absolutely fine for the purpose it's being used, but you should give some mention to what the equation includes and excludes. As a minimum, you should mention that this is a simple linear shoaling equation with a refraction law included, and assumes a simple linear seabed slope. It doesn't consider complex (barred) surfzone slopes or bed friction. To be more complete, you could include the full formulation (which is arguably appropriate, given that it is a key equation in your predictive system).

Reply : Thank you for raising this point. The full formulation includes several equations that, in our view, would not enhance the readability of the paper. Instead, we provide a brief summary of the primary assumptions : "This formula allows to compute the incipient breaking wave properties based on a simplified solution of the wave energy flux conservation equation combined with Snell's law, assuming shore-parallel depth iso-contours."

* Equation 2: it is not completely clear from this formula which Hs is being used here - i.e. is this Hs at breaking, or does this Hs need to be locally defined? Please clarify this in the preceeding or proceeding text.

Reply: Indeed this is Hs at breaking i.e. computed through the Larson formula, this is now clarified

* Equation 4: Intuitively, I would assume the velocity in the rip channel will depend on the gradient in wave setup between the bar and the channel, not the absolute difference between the two.

However, I see from your diagram below how this can be neglected in an idealised case. As these gradients are present in equation 3, please explain in conceptual terms (and referencing the figure below) why the x and y dimensions can be safely neglected and are no longer important in order to estimate the flow velocity.

> *Reply : Thank you for this comment. We agree that such simplicification is not straightforward. This is now briefly explained in the revised manuscript, also referring to Moulton et al. (2017b) who developed this in more detail : "Following Moulton et al. (2017b), we assume that the ratio of bottom stress to the advection term is small, and that the balance of pressure gradients and advection along a streamline can be approximated using the Bernoulli equation. By further neglecting the effects of inertia in a longshore current driven by obliquely incident breaking waves, the rip flow velocity V can be approximated as:"*

* Line 171: Wave power at breaking can be derived from the wave breaker energy (Eb = 1/8 rho g Hb^2) and shallow water group velocity (Cg = sqrt(g hb)) as Pb = EbCg. This would seem a more obvious way to compute breaker wave power. I assume your motivation to derive a different formula here is because the local beach slope is not considered in the Larson (2010) calculation of Hb, and that this therefore provides an inferior estimation of wave power if you happen to know (or can estimate) the local beach slope. Please explain and justify in the text why you choose this approach over a more common airy wave theory approach.

> *Reply : We chose to use a proxy of wave energy instead of the energy flux for the sake of simplicity. We, however, tested different parametrisation like, using the wave period (used in Cg or in the wave factor Wf) however, it did not improve the results.*

* Line 175: This equation is commonly presented as h = Ay^(2/3). Please cite where this version comes from and define how you use this equation. For example, a is usually a sediment dependent scale parameter and b is usually taken as 2/3; How have you defined them in this application?

> *Reply : This is a modified Bruun/Dean profile, typically expressed as $h=Ax^m$, where A and m vary from site to site. Although m=2/3 is often assumed to be a good approximation, in this case, we account for large tidal variations and must also consider the subaerial beach. For this reason, we added 5 m so that z(x=0)=5. By not fixing A and m, we aimed to give the model as much flexibility as possible to determine the best-fit parameters, especially since we are dealing with an intertidal, potentially bermed profile. This differs significantly from the idealized Dean profile, which typically extends deeper. This clarification has been incorporated into the revised manuscript and we now do not refer to Dean profil to avoid confusion: "Note that here we did not consider a Dean profile (b=2/3) because be are interested in the intertidal, potentially bermed, part of the beach profile"*

* Line 184: 'in between, offshore wave breaking occurs' - I suggest using a different term here as 'offshore' sounds seaward of the surfzone/ sandbars. However, I assume you are referring here to wave breaking on the sandbar?

> *Reply : The Reviewer is right, we now clarify : "... there is no wave breaking across the terrace, if ..."*

* Line 191: The quantile-quantile approach you used to transform your values into a 5-level scale warrants explanation in the text. How exactly was this done?

Reply : this is explained a couple of sentences later in the paragraph : "Second, the values of V and Esb concurrent to lifeguard observations were sorted and thresholds were computed in order to obtain the same number of modelled hazard levels (Table 1). Based on these ranges of V and Esb , the complete time series of V and Esb were transformed into modelled rip-current (RHm) and shore-break wave (SHm) hazard on the same 5-level scale as for lifeguard observations."

* Line 195: Please explain in more detail how thresholds were determined to distinguish the five levels.

Reply : We are not sure to fully understand your concern as this is further explained later in the paragraph "the values of V and Esb corresponding to lifeguard observations were sorted, and thresholds were computed to ensure the same number of modeled hazard levels." This means, for example, that for the first threshold distinguishing between levels 0 and 1, if there were 25 lifeguard observations of a level 0 rip current hazard, the threshold for V was set at the 25th smallest value of V. This process was repeated for all hazard levels.

* Line 247: to be completely transparent, the top 4 lifeguard perceived hazard values are understimated by the model. However, the correlation and performance is generally very impressive.

Reply : We agree, this sentence now reads "Figure 10 also shows that, although the largest lifeguard-perceived hazard days are underestimated by the model, the model fairly well predicts daily-mean shore-break wave hazards. "

* Discussion: Another point that may be interesting to investigate (although entirely optional) now that you have well calibrated models, is what proportion of time this beach exists at each hazard level. This would simply require running the models over a longer time frame (a few years of wave and tide data, for example) and plotting the distribution of different hazard levels for rips and shore break waves. As I say, this is an entirely optional suggestion.

Reply : This is an extremely relevant comment! This analysis is planned as part of a broader study examining the impact of climate change on summer wave conditions and, in turn, surf-zone hazards. Specifically, we aim to explore how the proportion of each hazard level may evolve over the long term during summer. Although morphological changes will necessarily be excluded, this will provide a preliminary assessment of potential changes in surf-zone hazards.

* Line 266: Another approach that is probably worthy of discussion and that follows from previous papers (scott et al (2014), for example) would be to test the developed models against lifeguard recorded incident data. This would have benefits and limitations (e.g. mixing risk and hazard), but it would be interesting and valuable to see how well the models pick out periods of incident occurrence. This may be one of the only feasible ways to test the model's applicability on a large number of beaches, where gathering lifeguard perceptions may not be so feasible.

Reply : We fully agree. This is now included in this paragraph of the discussion section : " Since collecting consistent hourly lifeguard-perceived hazard data over a few weeks and under varying tide and wave conditions may not be feasible at many locations, an alternative approach is to use lifeguard-reported incidents (see, for instance, Scott et al., 2014). While such data also incorporate the exposure component of risk (Stokes et al., 2017), they are more *widely available and can be highly valuable, particularly for assessing whether the model can identify mass-rescue days"*

* Lines 266-267: 'The validation approach proposed here can be applied anywhere pending lifeguard hazard assessment can be performed' - This is not a trivial undertaking! Can you comment on how many lifeguard observations would be required at each site to robustly tune the models?

Reply : We fully agree that this is very challenging. At this stage it is hard to say, but it would require quite a few weeks of hourly lifaguard estimations for representative wave and tide conditions, this is why we precise (see also reply to previous comment) : " Since collecting consistent hourly lifeguard-perceived hazard data over a few weeks and under varying tide and wave conditions may not be feasible at many locations..."

* Lines 288-289: For completeness, can you comment on how Wf performs when applied hourly? Presumably, the model you present here performs better at hourly resolution as it captures tidal variation?

*Reply: We warmly thank Reviewer #1 for this insightful suggestion. The reviewer is correct that our model performs better than Wf at an hourly resolution as it captures tidal variations. However, the correlation between hourly Wf and lifeguard-perceived rip current hazard remains quite high (R = 0.65), indicating that despite tide modulation, Wf = Hs*T accounts for more than 40% of the observed lifeguard-perceived rip current hazard variability. This has now been included in the discussion section of the revised manuscript "It is also important to note that the correlation between the hourly lifeguard-perceived rip current hazard (RHl) and the hourly wave factor (Wf) remains relatively high (R = 0.65). This indicates that, although Wf alone does not account for tidal modulation, it still explains more than 40% of the observed variability in lifeguard-perceived rip current hazard."*

* Line 291: daily mean hazard would also help lifeguard managers roster lifeguards, some days ahead, to the beaches where they will be most needed

Reply : We, once again, fully agree, the text has been modified into : "TThe daily-mean rip-current hazard forecast is important for providing a straightforward message to the general public, and can also assist lifeguard managers in scheduling lifeguards in advance, ensuring they are deployed to the beaches where they will be most needed. In this context, the daily-mean wave factor Wf) appears to be a simple yet powerful tool for predicting and communicating high rip-current hazard days. "

* Line 304: This is an important point, as it suggests that the predictive method is not sensitive to the sort of changes in the bar and channel that might be expected within a single season. Can you comment on what a typical range of bar elevations and channel depths are expected to be (at least at this beach)?

Reply: Thank you for this comment, this is now clarified in the revised manuscript : "For instance, the correlation between V and RHl decreased slightly from 0.77 to 0.75 (≈ −3%) when assuming a higher bar crest (zbar = −2 m instead of -3 m) or a much shallower channel (d = 2 m instead of 6.5 m), which are closer to average values in southwest France. This suggests that a decent model skill can be achieved with a rough estimate of the bar/rip morphology, further implying that temporal variability in beach morphology can be neglected in the model."

* Line 318: Please comment on the calibrated gamma value you found in this study - it is significantly lower than would be typically expected. How sensitive is the model to this value? What correlation would be obtained if you used a more typical value for gamma?

> *Reply: Thank you for this comment. Please note that the breaker index used here is for random waves, which differs from the breaker index for regular waves, which indeed typically ranges from 0.6 to 0.8. This has now been clarified in the revised manuscript : "TThe optimal Hs/h breaker indices ($\gamma = 0.23$, $\gamma s = 0.4$) for random waves, sometimes referred to as the incipient breaker index, are different from the typical empirical breaker index (equivalent to H/h, with H the individual wave height) used, for instance, in the parametric random wave models, which typically range from 0.6 to 0.8. In line with previous field work (e.g. Raubenheimer et al., 1996; Power et al., 2010), our Hs/h breaker indices for random waves are significantly smaller than 0.6-0.8."*

* Line 348: This sounds like it's a limitation of your study, but that's only true if predicting overall risk is of interest (for lifeguard resourcing, for example). I suggest re-iterating here that even without predicting exposure the present system provides useful prediction of the underlying level of hazard, which is the primary factor of interest to both the public and lifeguard services.

> *Reply : Thank you for providing this insightful comment and indeed it read too much like a limitation of our work. Based on your comment this now reads : "While further research is needed to improve predictions of exposure, the present work already provides valuable forecasts of the underlying hazard level. Since hazard itself is the primary concern for both the public and lifeguard services, these predictions can be highly useful even without explicitly accounting for exposure."*

* Conclusions: As per my general comment regarding the discussion of transferability to other sites, I think the conclusions need to at least briefly address how feasible it is to apply the developed models at a large number of sites. i.e. how can these models feasibly be calibrated/validated at other locations? You need to be more realistic about how feasible it is to collect the required parameters at other sites. The rip model morphological parameters (bar crest depth and channel depth) are 'simple', but they are not trivial to measure. As the authors know, the surfzone is notoriously difficult to survey and is not routinely surveyed by monitoring programmes, and only at select locations globally is measured occasionally for research purposes. Therefore, the conclusions need to briefly address how these parameters are expected to be gathered for future application of these models, especially if they are to be useful for a large number of sites. Are the calibrated values used here expected to be applied elsewhere (along with some form of validation)? Are the parameters expected to be re-calibrated at each new location against lifeguard observations? or perhaps through direct surveying of the surfzone morphology?

> *Reply : Thank you for raising this important point. In line with this limitation we decided to add some text to the 4th paragraph of the discussion section, otherwize the conclusions section would have been too negative. This new text reads : "However, while parameters such as bar crest depth and channel depth are relatively simple, obtaining them remains challenging due to the difficulty of surveying the surf zone, which is not routinely monitored at most locations. This raises important considerations for the large-scale transferability of the models. Future applications will need to determine how these parameters can be feasibly obtained, whether through direct surveying, remote sensing, or empirical estimations based*

on regional morphology. Additionally, while the calibrated values used in this study may serve as a reference, their applicability to other sites remains uncertain, and further research is needed to assess whether re-calibration against lifeguard observations or other validation datasets is necessary at each new location."

Technical corrections

Reply : All the technical corrections suggested below have been made, except where a specific reply is provided, and with a more specific reply for the comment on figures 3d and 4b.

* Line 2: Change 'expose' to 'be exposed'

* Line 2: I suggest changing 'The most severe and widespread natural hazards' to ' The most severe and widespread natural bathing hazards'

* Line 8: Change 'from July 1 to August, 2022' to 'during July and August of 2022'

* Line 9: Change 'into' to 'into a'

* Line 12: Change 'where wave forecast is available' to 'where a wave and tide forecast are available'

* Line 39-40: Rather than 'due to alongshore-variable sandbar depths' I think it would be more accurate to say something like 'alongshore variability in depth between the sandbars and intervening channels'

* Line 52: Change 'increased understanding in rip current dynamics' to 'increased understanding of rip current dynamics'

* Line 69: Change 'quantitative estimate' to 'quantitative estimates'

* Line 81: Change 'rip current are ubiquitous' to 'rip currents are ubiquitous'

* Figure 1: The text within panel (b) doesn't show up well unless you zoom in on it. I would suggest changing the text to another colour. In the caption below the figure the abbreviation 'SMGBL' should be spelt in full on it's first use. This would also make it more consistent with the previous photo credit

* Line 103: Change 'operate surf-zone hazard forecast' to 'operate a surf-zone hazard forecast'

* Lines 103-104: Change 'we used numerical wave hindcast' to 'we used a numerical wave hindcast'

* Line 133: Change 'the deeper channel' to 'the deeper channels'

* Line 148: Change 'consider simple' to consider a simple'

* Lines 155-156: U, V, and w should all be defined here (currently only V is defined). Also, please specify where h should be defined as you have a depth over the bar and a depth in the channel. Which is this h supposed to represent?

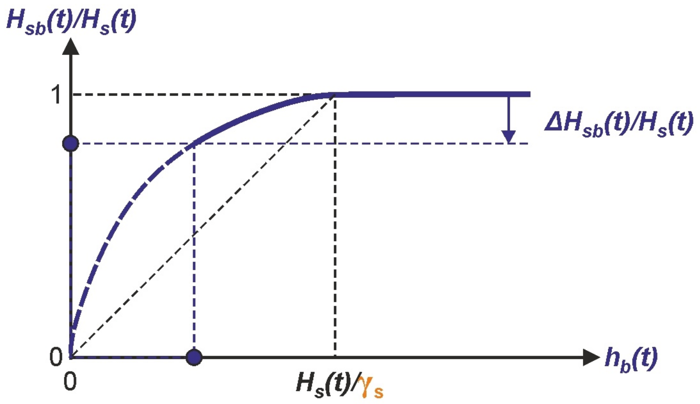
Reply : U is not used anymore, and hc and hb are now defined in both the text and Figure 3

* Line 159: change 'proceeds as follow :' to 'proceeds as follows:'

* Line 160: you should define h_b and h_c here

* Figures 3 and 4: the x and y axes of (d) are not clearly defined. Also, gamma appears here as Y which on first reading seems like a new parameter.

> *Reply : Thank for for this comment, which is in line with a comment of the other reviwer. Figures 3d and 4b have been revised to provide clear insight into the wave height decay model, which arte now of the form (\gamma police was also changed for clarity) :*



* Line 167: Change ‘for break type’ to ‘for breaker type’

* Line 171 and equation 6: Parameter names change from Essb and Hsb here to Esb and Hssb below. Please check all parameter names are consistent.

* Line 180: it is not clear where this wave height is being defined. I assume this is breaker height at the sandbar? If so, it would be clearer and more consistent to refer to this as Hsb (as per earlier definition for rips)

* Line 181: Change ‘model proceeds as follow :’ to ‘model proceeds as follows:’

* Line 190: Change ‘Hsl’ to ‘SHl’

* Figure 6 caption: Change ‘pdeth’ to ‘depth’

* Line 228: Change ‘RHl’ to ‘SHl’

* Line 264: Change ‘perception can influenced’ to ‘perception can be influenced’

* Figure 11 caption: the terminology of ‘shore-break wave intensity I’ is inconsistent with the parameter naming used up to this point (Esb)

* Line 305: Change ‘are modified based a the quantile-quantile’ to ‘are modified based on the quantile-quantile’

* Figure 12 caption: Change ‘with the blue (dotted) blue lines’ to ‘with the solid (dashed) blue lines’

* Line 323: Change ‘rip current tends’ to ‘rip currents tend’

* Lines 330-331: Change ‘it provides a direct Information on’ to ‘it provides direct Information on’

* Line 351: ‘allow to compute the time evolution’ - I think it would be fairer to say ' allow to estimate the time evolution'