Interactive comment on "Current temporal asymmetry and the role of tides: Nan-Wan Bay vs. the Gulf of Elat" *by* Yosef Ashkenazy et al.

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We thank very much Reviewer 2 for the important comments. Reviewer 2 stated that *"This paper makes some interesting and important contributions, in particular the introduction of the somewhat unusal distribution the authors promote; at least to me the distribution was not exactly a household name. The paper is unquestionably competently done, and well presented.".* At the end of review, reviewer 2 wrote: *"But - once they made the choice to compare those two fundamentally different bays, and once they chose the physical variable they did, the authors did an excellent job deonstrating a very useful and practical application for these quite unique data sets."* We thank the reviewer for this evaluation and address below all the other comments of the reviewer.

There are two main issues I thought the authors can better address in a future review.

The first is the rationale for the comparison of the two bays. As I read the ms, my strong impression was that the comparison was made for one reason only: the presence of the radar system the authors use in both locales. This is not a complleing reason for comparison. On the contrary are more compelling arguments against the comparison, in particular that the bays are SO dramatically bathymetrically and geometrically different, and that they are forced by fundamentally different processes. These two observations taken together suggest that neither bay is a particularly interesting or apt conparison for the other. To be sure, each is perfectly interesting in its own right. It's just the comparison that feels strained.

These two bays are very different, as described in manuscript, while at the same time, high-quality radar data exist for both. This is exactly why we choose these two locations. It wouldn't be that interesting to compare two similar bays. Moreover, had the results show that both bays share the same statistics (in all aspects), it would have been very surprising. That some of the characteristics are the same despite the large differences between the bays, suggest that our choice was actually not so bad, and that the reported conclusion regarding the natural variability of the statistical properties of surface currents is relevant in other marine environments.

We stress that we purposely choose such different environments in page 8, lines 12-14. We also state in the Summary section:

> We fitted the PDFs of the surface currents to the Weibull distribution and found large spatial and seasonal variability of the Weibull distribution parameters (the shape k and scale λ parameters) in both basins, in spite of the many differences between the two regions.

Second, I can think of many physical variables to characterize the flow by. Time of acceleration of surface current seems secondary at best, and the authors do not provide compelling rationale for this peculiar choice.

We agree that are many other measures that one could use. Yet, we find the parameters of the probability density function as well as the asymmetry of the time series to be fundamental and interesting enough to be considered. Moreover, as now suggested in the Summary, these statistical characteristics can be easily used as a benchmark for model performance. We hope to explore more features of the time series in the future.

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