

Reply to Remarks on Referee Report

As posted on egusphere on 23 Oct 2025-10-23 by Roberto Minguez

In response to

"Evaluation of Extreme Sea-Levels and Flood Return Period using Tidal Day Maxima at Coastal Locations in the United Kingdom"

Stephen Taylor

Date: 3 November 2025

The referee states "the scientific framing and validation are not yet sufficient for design-grade use: key assumptions (tail type, independence/declustering, stationarity) are not stress-tested; uncertainty is not fully quantified at the site level; and reproducibility is limited by brief methodological descriptions (peak selection, QC, and parameter tuning). " and "Substantial impact would require stronger validation and generalization beyond the UK testbed".

Unfortunately the referee has missed the point of the paper. The study does not intend or pretend to be a world-wide exhaustive study of the proof of the described TMAX method in all situations; neither is it a request for international approval of the TMAX method. It is a case study comparing the results of extreme sea level calculations based upon either a method using the 75 year old Gumble's original method using Type 1 statistics (with a slight difference in selection method) to a single UK government study, using the SSJPM method.. The title indicates what it is, an evaluation of a single study.

It would be very difficult indeed to extend the study to other countries as the reviewer suggests because such detailed studies using the SSJPM method have not been carried out in most countries. Neither is the accompanying source data available over sufficiently long periods for the analysis by either TMAX or SSJPM for comparison. Therefore the broader type of study the reviewer suggests cannot be carried out in practice, without a major international effort.

It may make difficult, uncomfortable reading for some to accept that there has been little or no improvement in accuracy despite some 75 years of statistical tidal research, but that is the general conclusion of the study, and in this paper I back up this claim with sufficient data to warrant its publication.

The study was fully automated and required no manual intervention and this is presented, quite correctly I believe, as an advantage. However, the referee tries to turn this into a disadvantage stating "Automation is valuable for broad regional screening, but for site-specific design one would typically prefer the best-supported method (SSJPM or POT/GEV with explicit diagnostics), even if harder to automate." This broadside side-steps the main issue to be drawn from the paper.

The automated method described gave comparable or better results than the UK study, whose initial aim was to apply a general method, and yet which subsequently required manual intervention in 25% of the sites. This surely shows an advantage of the TMAX method described. It should not be falsely claimed as a disadvantage as the referee tries to make out. The advantage of the method is clear in this particular case.

It is unfortunate that it is "a particular case" and not a more general case, but that is because of the practical reasons explained above

I accept that, in order to prove the general applicability of the TMAX method, it could benefit from more testing and evaluation work, and that its readiness for general use is perhaps somewhat overstated in my paper. I have indicated in my response to a previous referee, and repeat it again here, that I am willing a willing to dilute these broader claims.

However, this should not detract from the main feature of the study.

The paper indicates that a 75 year old method (slightly modified) still holds up astonishingly well in comparison with one of the latest statistical flood risk methods, the SSJPM.

This surely is a valid and is a most important point of which readers of EGU Ocean readers should be made aware, and this point should not be hidden by obfuscation and statistical jargon.

Dr. S E Taylor.