Nat. Hazards Earth Syst. Sci. Discuss., 2, C2307–C2315, 2014
www.nat-hazards-earth-syst-sci-discuss.net/2/C2307/2014/

Natural Hazards and Earth System Sciences
Discussions

Interactive comment on "Use of historical information in extreme surge frequency estimation: case of the marine flooding on the La Rochelle site in France" *by* Y. Hamdi et al.

Anonymous Referee #1

Received and published: 20 October 2014

Overview

I think this paper addresses a very important issue for statistical modelling of extremes values, which is to partial overcome the inevitable scarcity of data on extreme events by incorporating historical information (HI). This has been achieved by supplementing the typical likelihood for extreme value distributions—based on measurement data, for example—with terms corresponding the HI. The HI are not restricted to specific values, but can also be ranges, or known minimum and maximum values, ie. censored data. While a potentially very useful method that could reduce uncertainties and in-

crease the reliability of extremal estimates, there are, however, many changes that I think should be implemented, primarily to ensure that the usefulness of the authors' proposed method is conveyed at a standard that is consistent with that of the underlying concept. I can see that this work could be used in many applications, and bring improvement on the standard methods already available for such applications.

General comments

Clarity and notation

There is great scope for this paper to be made clearer and easier to read. Streamlining notation is one example. Some suggestions have been made below for Section 3.1.1, which I think can be adopted throughout the paper. Otherwise the reader comes across n, e, k and g (plus others), all of which are counts, and would be obvious if n was used throughout, with appropriate subscripts. (When reading the paper, I often found that I only needed to know that a number was a count, for what I was reading to make sense.) I have also made suggestions for how the clarity of the likelihoods in Section 3.1.2 and Section 3.2 might be improved. The term 'frequency analysis' is used throughout; however I think 'extreme value analysis' (and suitable variants) is more appropriate.

Methodology

One the whole I felt able to follow the methodology. I did, however, reach the end of Section 3 and realise that I was uncertain about how $f_X(\,)$ and $F_X(\,)$ are estimated. It is briefly mentioned in Section 2 (p.5652 l.23) that the generalised extreme value and generalised Pareto distributions will be used, and as the authors consider annual maxima and threshold exceedances they are sound, theoretically justified choices. How-

ever, they are not mentioned at all in Section 3, whereas I think they should be central to the development of the methodology.

Application to marine flooding

The application follows on nicely from the methodology. However, I was surprised to see that for both the BMH and POTH models, only one piece of HI had been used for each. That this additional data has made the Xynthia datum seem more consistent with the estimated distributions could therefore just be a coincidence arising as a consequence of the extreme value distributions' shape parameter being difficult to estimate with precision (Coles, 2001, pp.106). This is speculative, but it raises two questions: would the fits change much if one piece of *different* HI was used, or would the fits change more if *more* HI were used? I think the last question is particularly interesting because extreme value data are inevitably scarce, resulting—almost invariably—in large uncertainties in estimates, especially of extrapolated return levels. If the addition of HI can reduce these uncertainties *and* maintain the robustness of the extreme value statistical methods, then I think the authors are offering a very useful tool for practitioners. However, I do not think that improved results seen from the addition of one piece of HI are conclusive, especially in terms of transferability to other applications.

Specific comments

Section 1: The authors state that 'statistical characterization of extreme storm surges, using HI, has not been handled in the literature'. While not explicitly using HI, Coles and Tawn (2005) adopt a Bayesian approach to storm surge estimation that could easily accommodate HI through prior distributions. More generally, perhaps the authors might like to consider how incorporating HI compares to informative priors in a Bayesian

analysis.

Section 3: If I understand correctly, type iii data comprise a time range above which no exceedances of some threshold occurred and the BMH model has be ruled out for such data. If this time range exceeds and encompasses one year, then it should be possible to treat type iii data as left-censored within the BMH model. See also general comments on censoring later.

Section 3: The generalised extreme value and generalised Pareto distributions are not mentioned at all in this section, but are a crucial part of the methodology, and so should be central to the presentation of the methodology.

Section 3.1: The 'second difficulty' highlighted by the authors is a key point, and is fundamental to the validity of the proposed methods. I can see that other users could benefit from the proposed methodology, and that it could easily be applied to other phenomena. Perhaps the authors should bring to the fore that it is *vital* that the mechanism behind the HI is fully understood *and* compatible with the systematic record, in particular that the HI should not bias the estimated distributions. Based on this, perhaps the comment on p.5651 l.29 should be checked, as presumably we want the outliers to be represented fairly.

Section 3.1.1: Quite a few entities are defined here and there seem to be quite a few combinations for each. I think there is scope to improve clarity. For example, u seems to have been reserved in the literature for threshold, as in Coles (2001). Furthermore, if I understand correctly, these are non-random, so lower case notation might be better. Similarly perhaps p_t might work better for the probabilities. I think if the authors can restrict themselves to u for thresholds, t for times and time periods, n for counts and p for probabilities, with suitable subscripts chosen, then this section will be much clearer, and much more consistent with typical statistical notation.

Section 3.1.2: Use of a partition, as in eq. (13), is very informative. I wonder whether it would be simpler to separate the systematic part of the model completely (as in

eq. (25) for the POTH model), so that the likelihood has a term $\prod_{i=1}^{s} f_X(x_i \mid \theta)$. Then the historic part can be given separately, and partitioned according to exact, lower bound and range data. A related and important point is that the lower bound data are essentially being treated as left-censored and the range data as interval-censored. These are well recognised statistical terms, and I think if referred to would significantly improve the clarity of this section. I also wonder whether it would then be simple to treat non-exceedances of the threshold of perception as right-censored. A suggestion for further improvement to clarity is to precede the likelihood functions for the BMH and POTH models with background containing likelihoods for the standard BM and POT models, which can then be drawn upon when deriving the BMH and POTH likelihoods.

Section 3.1.2 and Section 3.2: The likelihood derivations are exhaustive, but perhaps better suited to being in an Appendix. This comment also extends to some of the calculations presented in Section 3.1.1.

Technical corrections (or clarifications)

1. p.5648 l.11: Is there any reason for using 'systematic record period' as opposed to 'data record period', or perhaps 'conventional data' is more appropriate than 'systematic data'?
2. p.5648 l.12: Do 'frequency models' refer to the statistical models used for the surge data? If so, I think the word 'frequency' is misleading as it's magnitude that's of greater interest. See above also.
3. p.5648 l.14: 'sea levels' → 'sea level'
4. p.5648 l.17: Are the authors stating that 'classical Historical Maxima' are traditionally used in the POT method? If so this contradicts the preceding statement.

5. p.5649 l.7: 'when the storm' → 'when storm'
6. p.5649 l.15: 'the Great Storm of 1987' may be a more familiar name to most than just '1987'.
7. p.5649 l.17: 'Surges frequency' → 'Surge frequency'
8. p.5649 l.27: 'lead to a bad adequacy and selection' → 'lead to poor choice'
9. p.5649 l.28: 'as well as on the' → 'and'
10. p.5650 l.11: 'increases the representativity' — Can the authors elaborate on this? Also, 'representativity' is not in the Chambers English Dictionary: perhaps 'representativeness', although it's a bit cumbersome? Maybe 'influence'?
11. p.5650 l.12: 'The regional estimation' → 'Regional estimation'
12. p.5650 l.18: 'HI' needs defining
13. p.5651 l.16: I think one or two lines regarding use of ML is sufficient.
14. p.5653 l.10: 'iid' not defined until l.22
15. p.5653 l.15: 'another' → 'the other'
16. p.5653 l.29: Can the authors clarify why Weibull plotting positions are being used since Weibull distributions aren't being fitted?
17. p.5656 l.1: 'an illustration' → 'a schematic'
18. p.5656 l.23: '$(t = 1, 2, \ldots, m)$'

19. p.5656 l.25: Notation for n_{tp} here does not seem consistent with eq. (5). Upper case usually adopted for matrices, perhaps M. See also additional comments for Section 3.1.1. In eq. (5) should the integral be a summation? Also n_{tt} and $n_{(p,t)}$ are not consistent.

20. p.5657 eq (6): Repeated use of $\hat{P}_\bullet$. Perhaps $\hat{p}(i)$ instead.

21. p.5658 l.1: Clarify 'Hazen ($a = 0.5$)'.

22. p.5658 l.8: I think the ML estimation is sufficiently well-known to not need description. Does '$\leftrightarrow$' under θ serve a purpose?

23. p.5658 l.15: 'iid' already defined.

24. p.5659 l.3: the 3-level notation is somewhat unconventional; perhaps $\{y_{low,i}\}_{i=1,\ldots,n_{low}}$ would be better, especially since it avoids using lb as both an index and number.

25. p.5659 l.8: y_i does not appear in eq. (11).

26. p.5659 eq. (12): p has been used previously (p.5653 l.29).

27. p.5661 l.15: 'from' $\rightarrow$ 'form'

28. p.5664 l.6: 'in a one' $\rightarrow$ 'to form one'

29. p.5664 l.26: 'we basically searched' $\rightarrow$ 'we searched'

30. p.5665 l.15: 'both a HI' $\rightarrow$ 'both HI'

31. Section 4: Scope for a more informative title.

32. p.5667 l.22: 'By fitting' $\rightarrow$ 'fitting'

33. Figures 7 & 8: Just the return level plot is probably sufficient, as the Q-Q plot is equivalent apart of a different x-axis scale.

34. p.5669 l.11: 'It is ... HI' $\rightarrow$ 'Including HI'

35. p.5669 l.14: 'data is' $\rightarrow$ 'data are'

36. p.5670 l.6: 'parameters and quantiles' $\rightarrow$ 'parameter and quantile'

37. p.5670 l.26: 'annoying' $\rightarrow$ 'unsuitable' or 'inappropriate'

38. p.5671 l.11: 'models settings' $\rightarrow$ 'model settings'

39. p.5671 l.12: 'why in' $\rightarrow$ 'why for'

40. p.5672 l.2: 'methods how' $\rightarrow$ 'methods for how'

41. p.5672 l.3: 'applied on' $\rightarrow$ 'applied to'

42. p.5672 l.5: 'is rather based' $\rightarrow$ 'is based'

43. p.5672 l.17: 'parameters' $\rightarrow$ 'parameter'

44. p.5672 l.20: 'Despite of the fact that' $\rightarrow$ 'While'

45. p.5673 l.10: The authors state that the POTH models include 'more systematic data', which reduces uncertainty estimates. However, a statement regarding the presence of any temporal dependence in the threshold exceedances is needed to qualify this. Essentially, can the threshold exceedances be treated as independent? If not, the uncertainty bounds might be over-confident.

References

Coles, S. (2001). *An Introduction to Statistical Modeling of Extreme Values*. U.S. Government Printing Office.

Coles, S. and J. Tawn (2005). Bayesian modelling of extreme surges on the uk east coast. *Philosophical Transactions of the Royal Society A: Mathematical, Physical and Engineering Sciences 363*(1831), 1387–1406.

Interactive comment on Nat. Hazards Earth Syst. Sci. Discuss., 2, 5647, 2014.