"50 years return period wet-snow load estimation based on weather station data for overhead line design purpose"

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Referee Specific comments: Referee #2 - Authors answers

General comments

We would like to thank referee #2 for the time spent reviewing our paper.

Unfortunately, that review does not help us to improve our initial text for the five following reasons:

1. Rule n°7 of the "General obligations for referees" (*Referees should explain and support their judgments adequately so that editors and authors may understand the basis of their comments*) has not been respected.
2. Negative comments on another paper (Nygaard et al. 2013), already reviewed and published in another world-renowned journal, have been made.
3. Comments on basic probabilistic or statistical issues are erroneous.
4. Other comments show a lack of comprehension of the text.
5. The final comment (conclusion) is very difficult to understand.

Moreover, there is not a single comment on one of the most important aspects of the paper: the description of the accretion model and its parameterization.

Supplement

1 - Rule n°7 of the "General obligations for referees" (Referees should explain and support their judgments adequately so that editors and authors may understand the basis of their comments) has not been respected with statements as:

- ✓ The paper is loaded with unjustified assumptions and these affect the outcome of the results. (detailed list of such assumptions could be useful)
- ✓ This is only one of many ad-hoc assumptions in the paper. (idem)
- ✓ The analysis is not clearly outlined and therefore the paper is not possible to understand in its present form. (As the editor, who has reviewed the paper before its publication in NHESSD, and the referee #1, who has made a positive recommendation, seem to have understood the paper without difficulty, that statement seems to be an abusive comment.)
- ✓ The authors say that their simulations follow a normal distribution but that is not believable (reasons explaining why such a conclusion is not believable could be useful)
- ✓ The only real analysis results in the paper seem to be in Table 2 based on Figure 3. (that statement seems to be excessive)

2 - Negative comments on another paper (Nygaard et al. 2013), already reviewed and published in another world-renowned journal, have been made:

- ✓ The Icelandic data appear to be the only data set available and they do not follow a GPD. I fit the Icelandic data (2013 paper) to 60 distributions using 5 goodness-of-fit tests and found best fit is a Gumbel distribution with GPD among the worst, it failed the Kolmogorov Smirnov, Anderson Darling and Chi-squared tests. Next best to Gumbel was Gen. Extreme Value but it and the GPD have the disadvantage of 3 parameters for such a small data set.
  It is important to notice that the data set presented in Nygaard et al. (2013) corresponds to highest observed loads and not annual maxima. Therefore, it is strange to try to fit those data with a Gumbel distribution.
- ✓ The results should be stated in diameter to compare with the Icelandic data. Calculation with the Icelandic data assuming random Poisson arrivals with random Gumbel loads gives a 50 year diameter of about 13 cm which suggests that the Icelandic data (50 year values) are more than a factor of 10 higher than given in this paper.
  Considering the difference between Icelandic and French climates, it is not surprising that characteristic wet-snow loads in Iceland are greater than characteristic wet-snow loads in France.

3 - Comments on basic probabilistic or statistical issues are erroneous:

- ✓ The authors say that their simulations follow a normal distribution but that is not believable and it follows from the Central Limit Theorem on basis of their simulation assumptions. For example a gamma distribution applied many times will converge to a normal distribution.
  The Central Limit Theorem deals with the arithmetic mean of a sufficiently large number of random trials of independent random variables from the same parent distribution. That arithmetic mean only is normally distributed.
- ✓ Are the values just exceedance probability 0.02 for the GPD?? If that was the case then for the 241simulated events in Fig 3., results in Table 2 would represent return periods of about 5 years not 50.
  In table 2, the calculated 50 years return period load is 2.6 kg m^{-1}. Looking at Fig. 3, it is possible to see that the five biggest values in 25 years are: 0.9, 0.9, 1.2, 1.4 and 3.2 kg m^{-1}. Thus, it is obvious even without any calculation that it is not possible to say that 2.6 kg m^{-1} is a 5 years return period load.
- ✓ For the GPD, it is possible to get the same return period values by varying the scale or location parameters or both or the shape parameter.
  It is a POT method: the threshold can be adjusted. The shape and scale parameter estimation depends on that threshold.
- ✓ Why should the shape parameter be different at the same location?
  At the same location, the data set can vary in function of the threshold. If the data set is different, the parameters can be different.
- ✓ For Table 2, the scale and location parameters need to be listed for each case.
  The location parameter, which is the threshold in the case of a POT method, is already given in Table 2.
- ✓ There is no justification for assuming a Generalized Pareto Distribution (GPD) based on data.

There are mainly two classical methods to deal with extreme values: methods based on the analysis of annual maxima (GEV) and methods based on the analysis of excesses over a threshold (POT). All information can be found in Coles (2001) (*An Introduction to Statistical Modeling of Extreme Values*). The use of a GPD ensues from the choice of the POT method.

4 - Other comments show a lack of comprehension of the text:

- ✓ The authors need to explain how time enters into their model.
  How does time enter?
  See for instance Eq. (5)
- ✓ The fact that some of the lines in France are designed for 5 – 10 kg/m ..2-4 times the values here could be of some importance.
  See chapter 4.1.
  10 kg m^{-1} is used in mountains areas only. Those specific areas whose overhead lines are mainly concerned by rime loads are not the object of our paper.
  Our paper deals mainly with wet-snow events in plains at less than 500 m a.g.l.
  The experience feedback (*It is important to underline the fact that all the lines designed according to 5 kg m^{-1} (4 cm of accreted snow of density 600 kg m^{-3}) have never been damaged by any wet-snow events in plains since their construction.*) is quite consistent with worst simulated events in plains: characteristic wet-snow loads corresponding to ISO ice class R5 (5 kg m^{-1}).

5 - The final comment is very difficult to understand:

- ✓ There are no data on ice accretions in this paper and the results and assumptions appear not to agree with the only data set available.
  If there are "no data on ice accretions", it is very difficult to understand what is «the only data set available" supposed to be inconsistent with assumptions and results of the paper.
  If "the only data set available" is the Icelandic data set, it is also very difficult to understand the point of trying to compare it to French wet-snow events.