

**“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: B. Wichura - Authors answers**

Structure of comments: Page, Line: “Citation (if necessary)”, Comment

1) 5139, Title: To my opinion the title would reflect the contents of the paper more clearly if it would refer to the data sets used. On one hand the goal of the paper is to show the applicability of the modified parameterization in countries where wet snow events occur under less severe and windy conditions than in Iceland. This would be an argument for the title as it is (a more general one). On the other hand, authors adjust, implement and test the model (parameters) in order to estimate the wet snow loads at the 87 weather stations in France and they compare it with observations in France.

We could add “in France” at the end of the title to take that comment into account.

It is also consistent with some of the modifications proposed in the following.

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

2) 5140, 16: The abstract should include information of main results of chapters 4.3 to 4.5.

At the end of the abstract, we could add the main result, which is:

A practical method is given to determine the 90% confidence intervals of those characteristic values. As there is only one value of the ice class masses proposed by ISO 12494 in each interval, characteristic loads can be expressed in terms of ice classes. Applied to France, that method shows that ice classes ranging from R1 (0,5 kg m<sup>-1</sup>) to R5 (5 kg m<sup>-1</sup>) could be used for overhead line design.

3) 5141, 19: equation (1), M is defined as “linear mass density”. Shouldn’t be A defined as cross-sectional area per unit length instead of “cross-sectional area of the ISO reference collector” due to dimensional reason? [kgm<sup>-1</sup>s<sup>-1</sup>] ≠ [kgm<sup>-3</sup>m<sup>-2</sup>ms<sup>-1</sup>] if M [kgm<sup>-1</sup>],  $\eta_1$ ,  $\eta_2$ ,  $\eta_3$  [-], w [kgm<sup>-3</sup>], A [m<sup>2</sup>], V [ms<sup>-1</sup>]

We fully agree.

We will change the definition of A and adopt the above proposal:

A is the cross-sectional area per unit length of the ISO reference collector with respect to the direction of the particle velocity vector V

4) 5143, 1: equation (6), sqrt of outer bracket is wrong, cp. equation (2)

We can do nothing but fully agree and we will correct that careless mistake.

5) 5143, 5: “ ... compares very well with the ISO reference collector as: – the height a.g.l. of a snow covered conductor is close to 10m ...” Maybe I am wrong, but to my understanding this might be true for distribution networks (i.e. “medium” voltage networks) but it does not for transmission networks (i.e. “high” voltage networks), what EN 50341-1 (2012) is valid for, too. For those networks conductors are located in higher levels above ground, aren’t they?

There are about 450 000 km of overhead lines in France that are concerned by EN 50341-1 and distributed according to the following table.

Voltage	km	Average height a.g.l. of snow covered conductors	bundles	% of snow incidents
HTA (20 kV)	350 000	About 10 m	no	No information
HTB1 (63/ 90 kV)	50 000	from 10 m to 15 m	no	80%
HTB2 (225 kV)	25 000	from 15 m to 20 m	no	14%
HTB3 (400 kV)	25 000	from 20 m to 30 m	yes	6%

What is called “a snow covered conductor” is a conductor loaded by the minimum administrative loads given by the French law:  $1 \text{ kg m}^{-1}$  for HTA and 2 cm of snow with density  $600 \text{ kg m}^{-3}$  for HTB

The percentage of snow incidents is calculated according to all HTB wet-snow events recorded between 1980 and nowadays.

The authors have stated that a real conductor compares very well with the ISO reference because:

- In France, the height of almost 90% of the conductors in the scope of EN 50341 is close to 10 m a.g.l.
- 80% of the events of the French wet-snow database concern the “low” transmission overhead lines (close to 10 m a.g.l.)

Nevertheless, the authors proposed to modify the sentence:

It is important to notice that **one meter of** real conductor compares very well with **one meter of** the ISO reference collector as:

- the height a.g.l. of the snow covered conductors **of the distribution or low transmission overhead lines (90% of French overhead lines)** is close to 10m

6) 5143, 7: “ ... the torsion resistance of a conductor is such that it can slowly rotate around its axis everywhere but near its fixations ...” To my understanding, that’s true for single conductors (most often used in distribution networks), but not for conductor bundles (often used in distribution networks), cp. comment 3).

In France, it is only true for the 400 kV overhead lines. That explains why the percentage of snow incidents is very low for those lines in the above table.

Nevertheless, the sentence could be modified as:

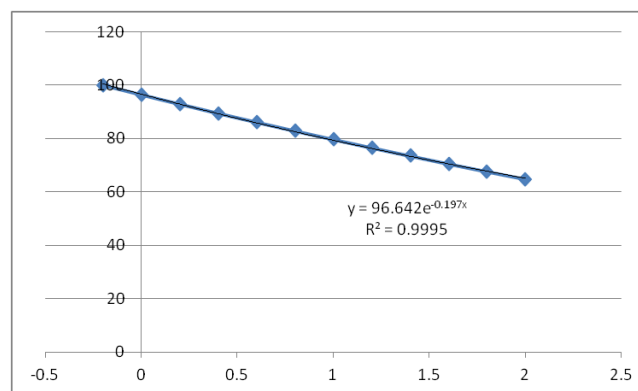
- the torsion resistance of a conductor is such that it can slowly rotate around its axis everywhere but near its fixations (or near spacers in case of bundle conductors)

7) 5144, 24: equation (10), Any reason for using this equation (simplicity in comparison to the original equation of Makkonen (1989))? Figure 1 (below) shows, that the criterion of eq. (10) is more conservative than the one used by Makkonen (1989) for pure phase, applied for  $T_{wb} = -0.2^\circ\text{C}$  (green dashed line in figure 1). Furthermore, it is slightly more conservative than the calculation of relative humidity (RH) for moist air conditions, based on the calculation of RH by psychrometer formula (green dotted line in figure 1).

The reason is indeed simplicity.

Equation (10) comes from an exponential interpolation of the couples ( $T_a$ , RH) of the following table that have been calculated according to the “Weather Calculator” proposed by the “National Oceanic and Atmospheric Administration” (NOAA) ([http://www.srh.noaa.gov/epz/?n=wxcalc\\_wetbulb](http://www.srh.noaa.gov/epz/?n=wxcalc_wetbulb)) with a constant wet bulb temperature of  $-0.2^\circ\text{C}$  and a constant pressure of 1000 hPa.

$T_a$	RH
-0.2	100
0	96.4
0.2	92.88
0.4	89.45
0.6	86.1
0.8	82.83
1	79.63
1.2	76.52
1.4	73.47
1.6	70.5
1.8	67.61
2	64.78



The main idea of that article is to give practical methods and equations to readers to build their own models.

We think that those practical methods must be slightly conservative in order to build models that may lead to safe (but realistic) characteristic values.

8) 5145, 7: equation (11), Any reason for using this equation instead of  $T_{wb} = 1.2^\circ\text{C}$ ? Figure 2 of the paper shows, that the criterion of eq. (11) is in the air temperature range from  $0.7^\circ\text{C}$  to  $1.7^\circ\text{C}$  less conservative than the criterion  $T_{wb} = 1.2^\circ\text{C}$  (see shaded area in Fig. 1). Equation (11) was proposed by Matsuo et al. (1981) on the basis of meteorological data of one station in Japan (Masumoto). “... Matsumoto is [located] about 90 km inland from the sea shore, Nikko being about 120 km inland. The heights of the Stations [above sea level] are [...] 610 m at Matsumoto, and 1,292 m at Nikko ...” (Matsuo et al., 1981). Are these locations comparable to (mainland) sites in France?

Equation (11) is already very practical and the region of Matsumoto (inland at a “reasonable” altitude close to 500 m) is quite comparable to French plain area, which is the main topic of the article.

It is true that Eq. (11) is a bit less conservative than “ $T_{wb} = 1.2^\circ\text{C}$ ”.

Nevertheless, we've decided:

- ✓ To refer to the possibility to use  $T_{wb} = 1.2^{\circ}\text{C}$  instead of (11)
- ✓ To choose Eq. (11) for our study as the snow may be too wet in the shaded area of figure 1 to stick effectively on the conductor surface.

9) 5145, 22: "... For instance, 1000m can mean equivalent water precipitation intensity of  $0.5\text{mmh}^{-1}$  as well as  $5\text{mmh}^{-1}$  according to ..." The sentence is misleading as it is, to my opinion due to two reason: First, to my understanding, the intention is to illustrate the statement in line 18 "... Moreover, the same visibility may lead to very different snowfall rates, ...". Second, its position after eq. 13 may lead to misinterpretation. Rasmussen et al. (1999) state "... Under rapidly changing conditions, visibility is an unreliable indicator of snowfall intensity unless crystal types and degree of riming and aggregation are known ..." Knowing the reason of different visibilities (crystal types and degree of riming and aggregation) the relation to eq. 13 would be obvious. So, maybe just rearranging and/or short explanation would make it clearer.

We fully agree.

We will remove the full sentence: "... For instance, 1000m can mean equivalent water precipitation intensity of  $0.5\text{mmh}^{-1}$  as well as  $5\text{mmh}^{-1}$  according to ...".

We will follow your advice and modify the previous sentences:

One approach is to estimate the mass concentration  $w$  from observed visibility  $V_m$  by a formula presented in Makkonen (1989). That approach has not been chosen by the authors as visibility has not been widely recorded in the past 25 years at sufficiently many French weather stations. Furthermore, according to Rasmussen et al. (1998) in chapter "Comparison of observations with theory", the same visibility may lead to different snowfall rates:

Under rapidly changing conditions, visibility is an unreliable indicator of snowfall intensity unless crystal types and degree of riming and aggregation are known in real time.

As the equivalent water precipitation intensity  $P$  is automatically recorded in many weather stations, Eq. (13), where  $V_t$  is the snowflake terminal velocity, has been chosen by the authors to determine  $w$ .

10) 5146, 17: Equation 14 is valid for Hellmann rain gauge. I did not find information about rain gauges used in France. Are they comparable to Hellmann?

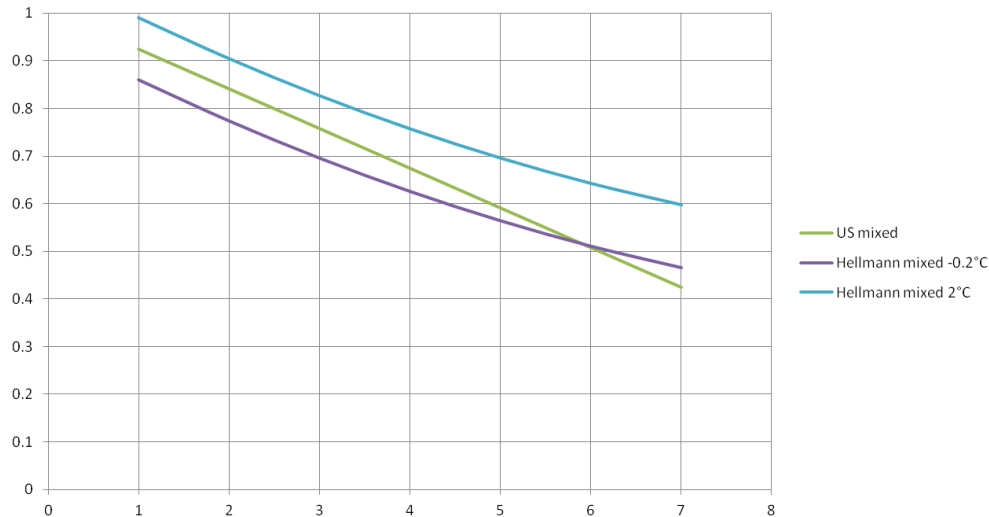
We fully agree.

We should have specified that Eq. (14) is valid for the Hellmann gauge, which is a bit narrower than gauges used in France.

Even if there is no such equation in France, it has been measured that the catch ratio for pure snow can be as low as 20% when the wind speed is  $10\text{ m s}^{-1}$  (Leroy (2002)). Therefore, it was not possible to use recorded precipitation values without applying correction factors.

As the mean wind speed during wet-snow events in France is about  $4\text{ m s}^{-1}$  according to Strauss and Magnan (1985), it can be seen in the following figure that the mean catch ration is about 70%. Thus, the mean correction factor is about 1,4, which can really not be considered as negligible.

It can also be seen that the effect of the temperature is sensitive. For  $4 \text{ m s}^{-1}$ , in the “wet-snow window” (from  $-0.2^\circ\text{C}$  to  $2^\circ\text{C}$ ), the catch ratio ranges roughly from 65% to 75%. Thus, the correction factor ranges roughly from 1.3 to 1.5. That is the reason we’ve chosen the equation for the Hellman gauge, which takes into account the temperature.



In order to take into account your comment, we propose to add at the end of that chapter the following sentence:

It must be underlined that Eq. (14) is valid for the Hellmann gauge, which is not used in France. Unfortunately, no such equation has been developed for typical unshielded French gauges. As the effect of the wind cannot be neglected, the authors have decided to use Eq. (14) assuming it is the best default option.

11) 5147, 6: “... wet-snow loads in France, the reasonable constant value of  $400 \text{ kg m}^{-3}$  proposed in ISO annex for wet-snow density  $\rho_s$  is adopted. ...” Admirat (2008) used the equation  $\rho_s = 200 + 20U$  for  $1 < U < 10 \text{ ms}^{-1}$ , i.e.  $\rho_s = 400$  for  $U = 10 \text{ ms}^{-1}$ . Maybe a reference to Admirat (2008) supports the statement before.

It is written in Admirat (2008) that the calibration  $\rho_s = 200 + 20U$  is an empirical calibration that comes from 22 documented events in France (included one event in Alsace that happened in 1962) and several events at the Luchon station.

It is possible to find the reference to the same 22 documented events in Admirat (IWAIS – Paris – 1988) “Calibration of a snow accumulation model based on actual cases in Japan and France”, where it is written for the 1962 event:

“...However, Japanese calibration method produced values not exceeding  $1.4 \text{ kg m}^{-3}$ , and which were too low to explain the damage caused to the power lines at the time indicated. To obtain higher overload figures, we decided to reduce the value of  $\rho$  substantially, dropping from  $300 + 20U$  to  $100 + 20U$ ...”

As far as Luchon station is concerned, figure 4.21 in Admirat (2008) leads to another value. At 4.30, it can be seen that diameter is about 17 cm and linear mass is about  $1.7 \text{ kg m}^{-1}$ . It means that the density is around  $75 \text{ kg m}^{-3}$ , which is not consistent with the formula  $\rho_s = 200 + 20U$ .

As we have trouble to understand the origin of that formula, we have decided not to use it.

12) 5147, 23: “ ... and CR (which leads to k) ...” Please explain, how CR leads to k. As far as I understand the equations before, CR is used to correct P. P itself is a recorded value (as mentioned by the authors before) leads to w. As far as I understand the discussion in chapter 2.2 authors set  $k=1$  ?!

13) 5148, 4, 10:  $n=0,5$  or  $n=-0,5$  as well as n-range from -0,6 to -0,4, cp. to eq. 7 and 9?

Those are both careless mistakes that come from an obsolete draft that we will correct:

CR (which leads to c) and  $n = -0.5$  and values can range from -0.4 to -0.6.

14) 5148, 4, 11: How  $T_a$  was handled in CR for uncertainty estimation?  $T_a$  is not mentioned to be a recorded value or a parameter. Tests show, that uncertainty estimation as described by the authors is quite sensitive to variations in  $T_a$ .

$T_a$  is a recorded value (as well as RH) that is necessary to set  $\eta_3$  to 0 or 1 (chapter 2.2) and that is also used in Eq. (14).

15) 5148, 4, 16: “ ... The two classical parameters of that gamma law can easily be determined from its mean, which is 1.1 greater than its median value, and its coefficient of variation, which is 35 %. Those two estimations come from simulations of 20 000 cases, which are built with different couples (P ,U) and different durations. ...” To my opinion this is not clearly to understand.

As far as I understand the paragraph before (lines 12-15), numerical simulations with fixed couple(s) of (P, U, duration) with randomly varying ( $V_t$ ,  $p_s$ ,  $n$ , what about CR, see comment 12) result in “ ... a gamma law whose median value is the value calculated according to the mean values of the four parameters ...” That’s clear and reproducible.

On the other hand, the formulation “ ... Those two estimations come from simulations of 20 000 cases, which are built with different couples (P ,U) and different durations ...” doesn’t say if in those simulations the parameters ( $V_t$ ,  $p_s$ ,  $n$ , CR) are fixed and (P, U, duration) are varied (randomly)? (I guess that’s what you did.)

Or do all values (P, U, duration) as well as ( $V_t$ ,  $p_s$ ,  $n$ , CR) vary randomly? If so, what distributions did you assume for (P, U, duration)? Or did u use measured values for (P, U, duration)?

Furthermore, does “ ... which is 1.1 greater than its median ...” denote:  $\text{median}=1.1*\text{mean}$ ? If so, the formulation “ ... and its coefficient of variation, which is 35 %. ...” says exactly the same: The coefficient of variation (cv) of the gamma law depends on the shape parameter of the gamma law only. If that cv is (about?) 35%, the shape parameter of the gamma law is approx. 8,16 and the relation  $\text{median}=1.1*\text{mean}$  follows from cv “by definition”, i.e. it does not depend on the scale parameter. The factor 1,1 and/or the cv seem to be approximate values: If  $\text{cv}=35\%$  then the factor should be 1,04, i.e.  $\text{median}=1.04* \text{mean}$ ?

We agree that we should have been clearer.

The simulations consist in processing 10 different fixed couples ( $P_{xt}, U$ ) with 20 000 random sets of parameters as ( $V_t$ ,  $p_s$ ,  $n$ , CR), which leads to 200 000 random trials.

The 10 couples (Pxt,U) have been chosen because they lead to reasonable values of wet-snow linear masses in France. In those simulations,  $T_a$  is a fixed value set to 0.5°C (CR determination).

The following table (called Table xx hereafter) gives the main results:

Pxt	V	M cal	M ave	M ave / M cal	CV	M 5%	M 5% / M cal	M 50%	M 50% / M cal	M 95%	M 95% / M cal
20	2	1.05	1.11	1.06	27%	0.73	0.70	1.06	1.01	1.67	1.60
40	2	2.86	3.09	1.08	31%	1.91	0.67	2.91	1.02	4.87	1.70
60	2	5.45	5.91	1.09	33%	3.55	0.65	5.51	1.01	9.57	1.76
80	2	8.80	9.65	1.10	35%	5.63	0.64	8.99	1.02	15.89	1.80
20	5	2.34	2.55	1.09	34%	1.51	0.65	2.38	1.02	4.14	1.77
30	5	4.39	4.84	1.10	36%	2.75	0.63	4.49	1.02	8.09	1.84
40	5	7.03	7.76	1.10	38%	4.29	0.61	7.19	1.02	13.07	1.86
10	8	1.56	1.70	1.09	34%	1.00	0.64	1.59	1.02	2.79	1.78
20	8	4.50	4.95	1.10	38%	2.70	0.60	4.58	1.02	8.41	1.87
30	8	8.80	9.85	1.12	41%	5.19	0.59	9.00	1.02	17.35	1.97
				1.09	35%		0.64				1.80
			rounded	1.10	35%		0.60				1.80

Practical rounded values are underlined in orange.

We need for the next step a “simple” distribution that fit reasonably those results and we’ve found that a Gamma distribution could be of practical use. The next table gives the comparison between the results of the simulations and the outcomes of the Gamma distribution:

Gamma distribution - $\alpha=1/0.35^2$ and $\beta=(1.1 \times M \text{ cal}) \times 0.35^2$					
Gamma 5%	Delta %	Gamma 50%	Delta %	Gamma 95 %	Delta %
0.58	-21%	1.10	4%	1.88	13%
1.58	-17%	3.02	4%	5.15	6%
3.01	-15%	5.75	4%	9.81	2%
4.86	-14%	9.29	3%	15.85	0%
1.29	-15%	2.47	4%	4.21	2%
2.42	-12%	4.63	3%	7.90	-2%
3.88	-10%	7.42	3%	12.66	-3%
0.86	-14%	1.65	4%	2.82	1%
2.48	-8%	4.75	4%	8.10	-4%
4.86	-6%	9.29	3%	15.84	-9%
	-13%		4%		1%

We must modify our text to explain that process in a clearer way. We propose:

200 000 random trials composed of 10 different fixed couples (Pxt,U) associated with 20 000 random sets of parameters ( $V_t$ ,  $\rho_s$ ,  $n$ , CR) have been carried out and the main results of those simulations are presented in Table xx (see above).

From a practical point of view, it can be noticed that:

- ✓ The linear mass value directly calculated from the mean values of the four parameters corresponds to the median value of the distribution of each set of 20 000 random trials
- ✓ The mean linear mass is 10% greater than the calculated linear mass
- ✓ The 90% confidence interval is easily determined using the simple rule: [60% of the calculated linear mass – 180% of the calculated linear mass]

A simple distribution that fits reasonably well with the outcomes presented in Table XX is required in the next chapter.

The authors suggest using a Gamma distribution whose classical parameters are:

- ✓  $\text{Alpha} = 1 / 0.35^2$
- ✓  $\text{Beta} = 1.1 \cdot 0.35^2 \cdot \text{calculated linear mass value}$

16) 5148, 21: The specified confidence interval [60% of median-180% of median] seems to be a result of approximation, too. Exact values are [58% of median-188% of median] for  $\text{cv}=35\%$ ?

That is true, 60% and 180% are practical rounded values.

It has already been taken into account in the above proposal for a new text.

17) 5149, 9: Any reason for using POT instead of other methods (for instance “classical” Gumbel-approach)? See comment 22), too.

There are three reasons to use a POT method instead of a classical Gumbel method:

- ✓ In some areas of the southern part of the country, there are some years of the studied period without a single event or with only one event. In that case it is not mathematically correct to use a Gumbel method, which is based on the use of annual maxima. The POT method is a solution often used in that case. For instance, in the case of Carcassonne, there are 3 winters without event and 3 winters with only one event.
- ✓ In some other areas, there are a lot of events. Nevertheless, using only annual maxima leads to information losses. In that case, the POT method gives characteristic values that are more conservative than the values obtained according to a Gumbel method. For instance, in the case of Grenoble, a classical Gumbel method leads to a characteristic value of  $5.3 \text{ kg m}^{-1}$  whereas the POT method leads to a  $6 \text{ kg m}^{-1}$  (+13%).
- ✓ The authors consider that a POT method, whose parameters are estimated according to L-moments method, is far easier to program in that case

As those reasons are very classical, we have thought that it was not necessary to modify the text.

18) 5149, 18: Simulations for 4 weather stations. Why 4 stations? Results similar? Stations in different regions?

Those stations are:

- ✓ Lille (187 327 meteorological records, 241 calculated linear masses), in the north of France (with one exceptional high value in 2012)

Threshold	Events	Shape param.	Scale param.	M (50 years)
0.1	74	-0.46	0.13	2.54
0.2	40	-0.49	0.1	2.54
0.3	22	-0.49	0.21	2.55
0.4	13	-0.29	0.39	2.53
0.5	10	-0.31	0.41	2.54

- ✓ Chartres (159 543 meteorological records, 230 calculated linear masses), in the middle of France

Threshold	Events	Shape param.	Scale param.	M (50 years)
0.1	36	-0.47	0.15	2.2
0.2	21	-0.4	0.23	2.2
0.3	13	-0.35	0.31	2.2
0.4	10	-0.34	0.33	2.1



- ✓ Carcassonne (163 325 meteorological records, 80 calculated linear masses), in the south of France

Threshold	Events	Shape param.	Scale param.	M (50 years)
0.05	32	-0.63	0.16	3.3
0.1	24	-0.61	0.2	3.3
0.15	17	-0.5	0.33	3.4
0.2	16	-0.56	0.29	3.3
0.25	11	-0.36	0.58	3.5
0.3	10	-0.35	0.61	3.6
0.35	10	-0.44	0.5	3.5
0.4	8	-0.28	0.77	3.6
0.5	6	0.07	1.4	3.7

- ✓ Grenoble (189 705 meteorological records, 330 calculated linear masses), at less than 500 m a.g.l. but surrounded by mountains

Threshold	Events	Shape param.	Scale param.	M (50 years)
0.2	66	-0.58	0.22	6.4
0.3	40	-0.45	0.41	6.0
0.4	31	-0.45	0.47	6.0
0.5	25	-0.42	0.54	6.0
0.6	21	-0.43	0.57	6.0
0.7	17	-0.36	0.73	5.9
0.8	12	0.01	1.51	5.5
0.9	11	0.08	1.65	5.4
1	10	0.19	1.88	5.3

19) 5149, 21: Cv is “ ... about 15% ... ” means, that 15% is an approximation. Why do you use this approx. here. The exact value seems to be 17% in order to be in agreement with the simple rule specified thereafter: “ ... [79% of the “calculated value”–141% of the “calculated value”] ... ”. As far as I can see it, you do not use cv=15% anymore. Or is cv=15% and the limits of the “simple rule” are adjusted in order to “fulfill” the “conditions” regarding ISO ice classes (see line 25 and thereafter)?

We agree, there is no need to use practical values for the coefficient of variation or the mean value.

There are only used to build the 90% confidence interval.

The “real” values are 17% and 1.11 and they lead to [83% of the calculate value – 139% of the calculated value]:

- ✓  $1.11 - 1.65 \cdot 0.17 = 0.83$  or 83% (consistent with the simulations: 82%)
- ✓  $1.11 + 1.65 \cdot 0.17 = 1.39$  or 139% (consistent with the simulations: 143%)

	Calc. M	Mean	CV	Mean / Calc. M	M 5%	M 5% / Calc. M	M 95%	M 95% / calc. M
Lille	2.6	2.86	17%	1.10	2.07	80%	3.71	143%
Chartres	2.2	2.45	17%	1.11	1.81	82%	3.15	143%
Carcassonne	3.6	3.91	17%	1.09	2.86	79%	5.11	142%
Grenoble	6	6.77	16%	1.13	5.11	85%	8.56	143%
			17%	1.11		82%		143%
			15%	1.1		79%	Practical	141%

We can modify the text as follows:

10 000 numerical simulations for 4 representative weather stations of different areas in France (Lille, Chartres, Carcassonne and Grenoble) have showed that the 50 years return period wet-snow load is well represented by a normal law whose mean value is 1.11 greater than the “calculated value” and

coefficient of variation is 17 %. Thus, the 90% confidence interval is [83% of the “calculated value” – 139% of the “calculated value”].

To determine the ISO IC classes, the authors suggest using the following practical interval: [79% of the “calculated value”–141% of the “calculated value”].

Those precise values have been chosen...

20) 5154, 26:  $2,6 \text{ kgm}^{-1} \cdot 1,1 = 2,86 \text{ kgm}^{-1} \approx 2,8 \text{ kgm}^{-1}$  ?

In fact, the calculated value is 2.55 (rounded to 2.6), which leads to 2.8 instead of 2.86.

With the “exact” value of 1.11, it will lead to  $1.11 \times 2.55 = 2.83$  (rounded to 2.8) or  $1.11 \times 2.6 = 2.88$  (rounded to 2.9).

It is always tricky to use rounded values.

21) 5155, 1:  $2,6 \text{ kgm}^{-1} \cdot [0,79 \text{ to } 1,41] = [2,05 \text{ to } 3,67 \text{ kgm}^{-1}] \approx [2,0 \text{ to } 3,6 \text{ kgm}^{-1}]$ ?

Again,  $0.79 \times 2.55 = 2$  and  $1.41 \times 2.55 = 3.6$ .

We should write 2.55 instead of 2.6 for the Lille calculated value.

22) 5155, 8: What’s the advantage of using POT instead of other methods (for instance “classical” Gumbel approach)? You get the same “amount” of data as for Gumbel, i.e. no additional values (that’s the most common explanation/advantage of using POT instead of other methods). Maybe it is just the simple fact, that in some years there are no wet snow events, i.e. the application of Gumbel for instance would bear a conceptual problem? Does POT “solve” this problem?

Please, see 17)

23) 5155, 13: “ ... such a value, which has been associated with a real noticeable event (one 225 kV collapsed tower and a dozen of 400 kV damaged 15 towers) is more than extremely rare. ...” How do you decide, that “ ... such a value ... is extremely rare ... ” out from a (limited) data set of observations and measurements?

To ask the other way around: What is the basis for the assumption, that the data (observations, measurements, damages) just reflect a real rare event?

In this homogenous area of France (north and west plains, which represent more than 50% of the country), only two noticeable events (with damaged towers) have happened for 50 years. Lille event in 2012 is only the second one.

This is the reason why we think we can talk about an extremely rare event.

The concept has been used in the background document by Sanpaolesi (1998). It is still under discussion by the community, see Sadošský et al. (2012) and Kasperski (2014), for instance. Kasperski (2014), for instance, notes: “ ... Without considering a consistent strategy in regard to uncertainties in confined sets of observations, the background document to the Eurocode recommends the general approach to neglect exceptional observations for the specification of the characteristic value. [...] Strictly speaking, the exceedance probability of such a limiting value depends on the type of

probability distribution and the variation coefficient ..." Sanpaolesi (1998) used the Gumbel distribution for their analysis.

To make it more clear: The concept may be used because it is mentioned and has been used in/for the Eurocode. Maybe it is a good idea to point out more clearly, that it is under discussion and may imply additional uncertainties.

I must admit (H. Ducloux) that I didn't know those two references.

We can modify the text to introduce them:

The above criterion for identifying "exceptional load" is still under discussion by the community (Sadovský et al. (2012) and Kasperski (2014)). Therefore, the authors suggest using it only when the exceptional aspect of the value can be checked, i.e. according to a real event database. The case of the Lille event of March 2012 can be considered as exceptional as it has been the second serious event of that kind in a homogenous area as big as half the country for 50 years.

24) 5156, 3: "... It is suggested to use the above criterion only when the exceptional aspect of the value can be checked, i.e. according to a real event database, as in the case of the exceptional Lille event of March 2012. ..." cp. Comment 23)

Please, see 23)

25) 5156, 6: Any chance to compare the results to Admirat (2008)? Are they consistent to each other? (At least the maps of wet-snow events are similar. The values are valid for different return periods. Any chance to compare them?)

There are too many differences in the two methodologies (correction factor for the wind effect, density, wet bulb temperature, efficiency factors, etc.) to try to compare the results.

See also 11)

#### Technical corrections:

Structure of comments: Page, Line: "Citation (if necessary)", proposed correction

1) 5146, 6: "... velocity is bigger than  $1\text{ms}^{-1}$  ..." , greater than ?

2) 5146, 3: "... of snowflakes (Böhm, 1999; ...", of snowflakes (Böhm, 1989;

3) 5146, 7: "... Based on the work of Yuter et al. (2006) and Nygaard et al. (2013a) set  $V_t$  to  $1.7\text{ms}^{-1}$  ...", ... Based on the work of Yuter et al. (2006), Nygaard et al. (2013a) set  $V_t$  to  $1.7\text{ms}^{-1}$  ...

We fully agree. It will be corrected.

4) 5146, 13 (and thereafter): "... given in WMO report no.67 (1998) ...", either add WMO (1998) to references or cite as Goodison et al. (1998)

We will write Goodison et al. (1998)

5) 5148, 14: "... a gamma law whose median value ...". a gamma distribution whose median value

6) 5148, 12: “which is 1.1 greater than its median value ...”, ... which is 1.1 times greater than its median value ...

7) 5149, 19: “ ... represented by a normal law whose ... “, ... represented by a normal distribution whose ...

8) 5151, 18: “ ... wet-snow events in plains since ...”, ... wet-snow events in specific plains since ...

9) 5152, 18: “ ... in areas slightly above 500 m. ...”, ... in areas above 500 m. ...” (this depends on the point of view ...)

10) 5153, 19: “ ... to check if the criterion proposed in ... “, to check if the criteria proposed in

We fully agree. It will be corrected.

11) 5161, Table1: maybe it is a good idea to add a “totals” line, chapter 4.3 would be easier to read

It will be added.

12) 5168, Figure 3: Please enlarge numbers and text.

We will try but it is not easy.

13) 5170, Figure 5: Please improve figure quality.

Unfortunately, it is impossible. We have only a copy of the original document and that quality level is the best we can do.