Dear reviewer,

we thank you for the careful read and helpful comments to improve the paper. We feel we can address the main points of the comment and think that the paper has gained in quality because of the modifications. For readability, we have numbered and italicized the individual comments. Our response immediately follows the individual comments.

The authors have carefully examined a part of the measurements contained in Louis Morin's observation notes kept from 1665 to 1713. These notes are of great importance, given the scarcity of measurements at that time, for understanding the climate during "The Little Ice Age". Temperature and pressure measurements have been the subject of previous studies, well mentioned by the authors. We believe that the interest of the article relates to:

- the confirmation of the high quality and reliability of Morin's temperature measurements.

- the confirmation of the characterization, using these, of the climate of this period

- the study of observations of the direction of the wind deduced from the movement of clouds, which had not yet been the subject of previous studies.

This last point allows the authors to establish a link between the temperature differences (in particular the remarkably very cold winters-springs, but also the summers-autumns close to modern values) with the characteristics of the atmospheric circulations.

The abstract gives a precise idea of the content and of the conclusions of this study.

C2.1-Morin's Manuscript, which can be consulted at the Institut de France and has already been used by several of the authors cited in reference, includes observations up to 1713. We are surprised that the authors of the article could not use a complete copy of Morin's manuscript or even failing that, did not use in their analysis the data from 1710 to 1713 published in volume 2 of the 1992 note by Legrand and Legoff, cited in the article.

We had access to the Legrand and Le Goff book, of course, but we wanted to base the study on the original data. I.e. in this book, only processed ones are published. In the case of cloud cover and direction of the movements of the clouds they published data only up to 31 December 1709 in tabular form.

However, after your comment I consulted the Institut de France and added the data from 1710 to 1713. Thank you for the hint. After consultation with the director of the Institut de France, we are allowed to use the data for the analyses, but in order to publish them day per day in tabular form, we have to wait for a meeting on May 17, 2022. Until then, we can only make the daily data public in tabular form up to and including 1709.

C2.2-To be more in conformity with the title of the article, it would be desirable to include these data de 1710 à 1713 in the study or otherwise to modify the title of the article in : "Subdaily meteorological measurements of temperature, direction of the movement of the clouds, and cloud by Louis Morin in Paris from 1665 to 1709".

Because we have extended the data to the period addressed, we would like to keep the title.

Some other points, indicated below, seem to us to be able to be improved:

C2.3-*Line 87: it would be preferable to indicate here only the Fontenelle reference. The same passage quoted by Legrand and Le Goff is taken from the original in French*

Thank you for this comment. We will change that.

C2.4-*Line 142 to 164. The authors seem to have completely adopted the method developed by Legrand and Le Goff, 1992 to convert Morin's measurements into °C, a method explained in 10 pages. In this article, the summary which is given in 21 lines does not make it possible to understand the principle, in particular that the 2 periods indicated in line 155 are the years of observation of Morin from 1676 to 1712 and those of the Observatory of Paris from 1816 to 1852 considered to have identical average maximum and minimum temperatures. Are the values â€‹ â€‹ in °C recalculated by the authors identical to those published in volume 2 of the book by Legrand and Le Goff?*

We will provide a more detailed explanation. And yes, the values are identical. (See C1.7 of the first reviewer)

C2.5-*Line 164 and 165: "Rousseau, 2009" and not "Rousseau, 2013"*

Thank you for this comment. We will change that.

C2.6-*Line 166: "grape harvest dates" and not "harvest date"*

Thank you for this comment. We will change that.

C2.7-*Line 172: "monthly" instead of "daily".*

Thank you for this comment. We will change that.

C2.8-*Line 171 and Figure 4: .*

C2.9-*The assessment of a bias in Morin's measurements from 1776 to 1780* [sic, it should be "1676 t0 1680"], *which may possibly be taken into account for the calculation of monthly temperatures, is different according to Figure 4 of this article or Figure 1 of the article Rousseau, 2009. It seems that value for 1677 of harvest dates in Beaune used in the article (series of Labbé et al) is not in agreement with the chronology of the temperatures of Morin,* *which is in phase with the chronology of the Dijon series (series taken from Angot) used in Rousseau, 2009.*

Analogous to Fig. 1 (Rousseau, 2009), we created the same graph (See below). Note that we have excluded September for GST, but Rousseau has included it. This leads to discrepancies in the temperature mean of GST. The red dots show results for the 1681-1713 time period (Linear Fit: Long blue line) and the blue dots show results for the 1676-1680 time period (Linear Fit: Short blue line). The black line shows the linear fit over all data points. We do see higher values for the period of 1676-1680, but far not that pronounced as in Rousseau 2009. Furthermore, the slope of the black fit is well below 1 °C / 10 days. I.e. a distinction between measurement error and climate variability seems to be difficult. As already mentioned in the article: We see the unusually high anomaly but are not sure if it can be homogenized to this extent as in Rousseau 2009. Therefore, we did not homogenize these years, but did point out the high anomalies in temperature. Probably more studies in continental Europe will be needed to differentiate in a more confident way in between inhomogeneity of the data and climate variability. So, we want to keep the raw-data for our analysis, but will discuss this possible anomaly.

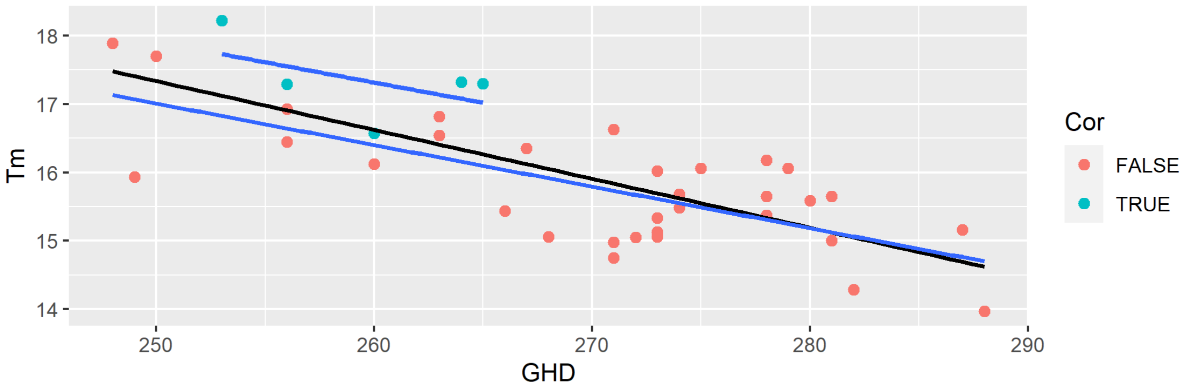


Figure 1: Comparison of grape harvest dates (GHD) with the growing season temperature (GST - Tm). The blue dots show the yearly values from 1676 to 1680. The black line represents the linear regression for all years.

C2.10-Line 177 to 179: Writing too technical, difficult to understand. In addition it seems that this smoothing of the data does not allow better readability, the curves somewhat masking the raw data. Wouldn't a data visualization for Figure 5, 8 and 9 similar to that used in Figure 4 be better?

We checked the visualization as suggested and agree to this comment. Furthermore, the yearly variability can be seen more clearly. This is especially true for the temperature in the DJF season. This means that we can delete the part which is meant to be too technical.

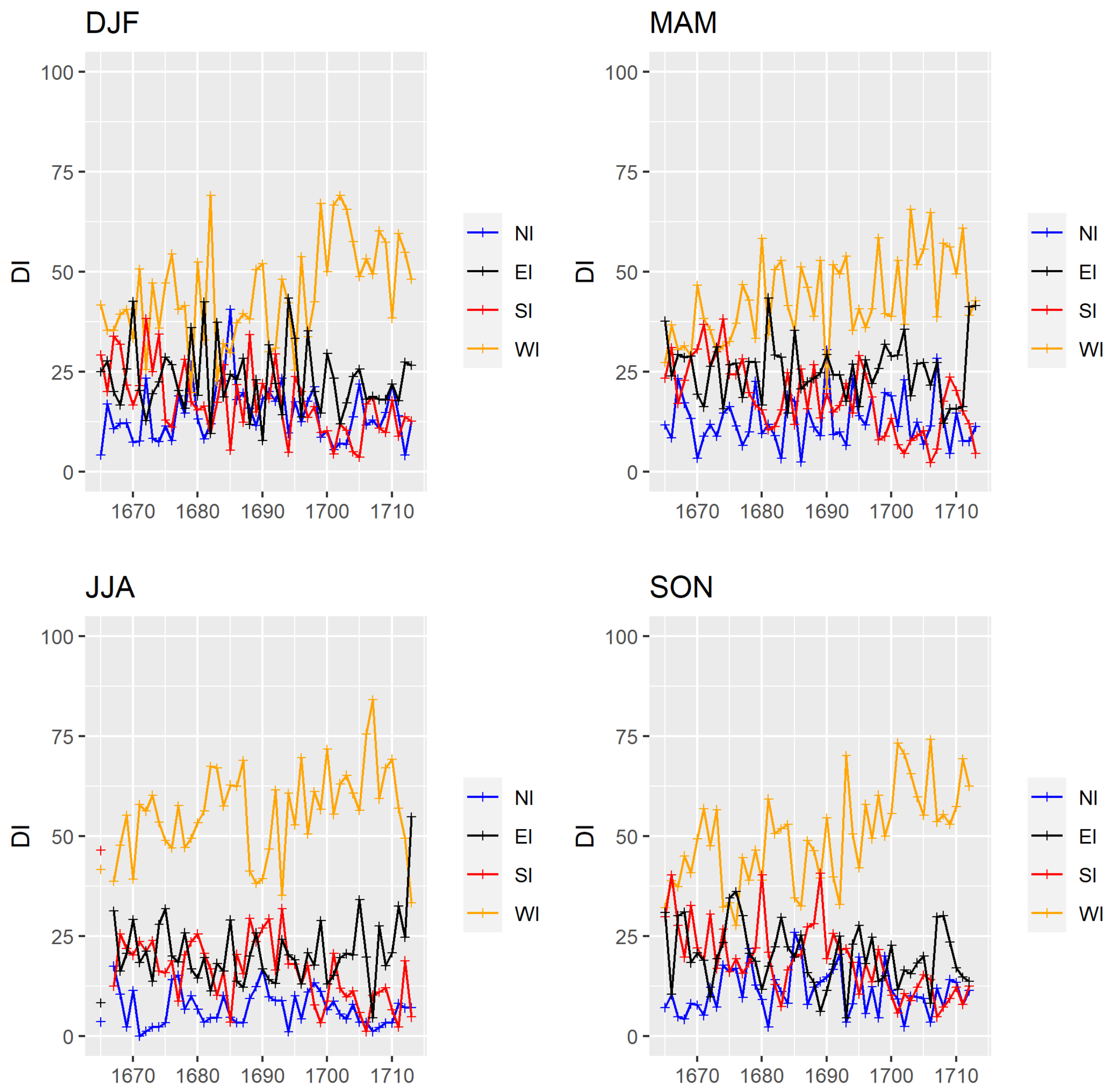


Figure 2: Directional index (DI) for all seasons

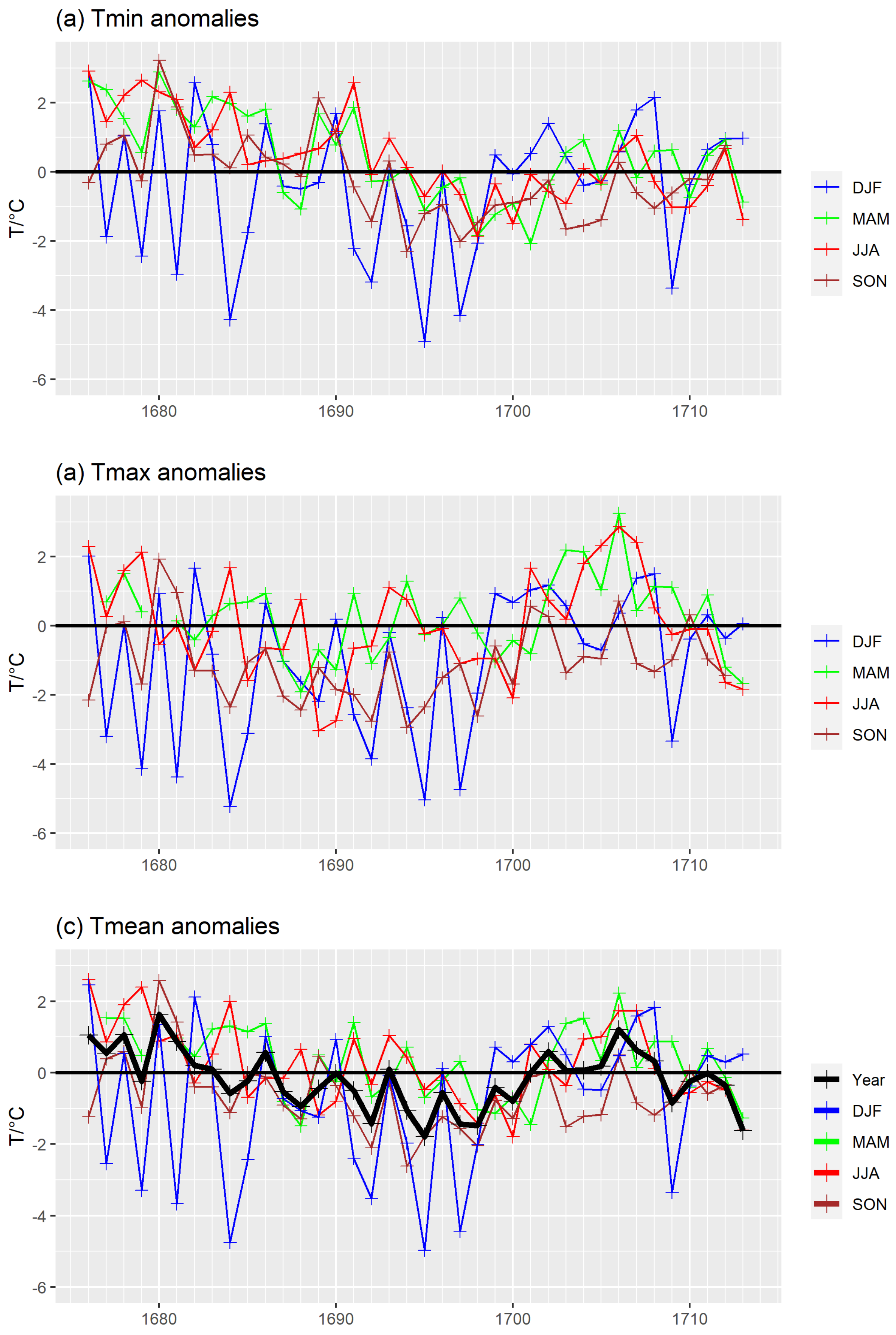


Figure 3: Tmax, Tmin and Tmean from 1676 to 1713

C2.11-Line 204 and figure A1: For comparison it would be interesting to provide for the Meteoblue data from 1986 to 2015 the 2 Figures similar to those of appendix A1 concerning the Morin data.

One must differentiate here what is comparable and what is not. Since the ERA5 data are given in full hours, we are limited in this sense and have chosen 15 o'clock for the midday temperature and 19 o'clock for the evening temperature. I.e. with this choice, one will not get precisely the maximum temperature. Thus, for methodological reasons, the absolute values are only comparable to a limited extent. However, two important analogies to Morin's temperature measurement become apparent: (1) There are also years with exceptionally high values (Although the majority is detected in the preliminary version from 1960 to 1978) and (2) the monthly distribution is quite similar to that of Morin. It shows high values in winter months and low values in autumn. A difference can be seen comparing the summer months (June and July), where the calculation of ERA5 data reveals higher values.

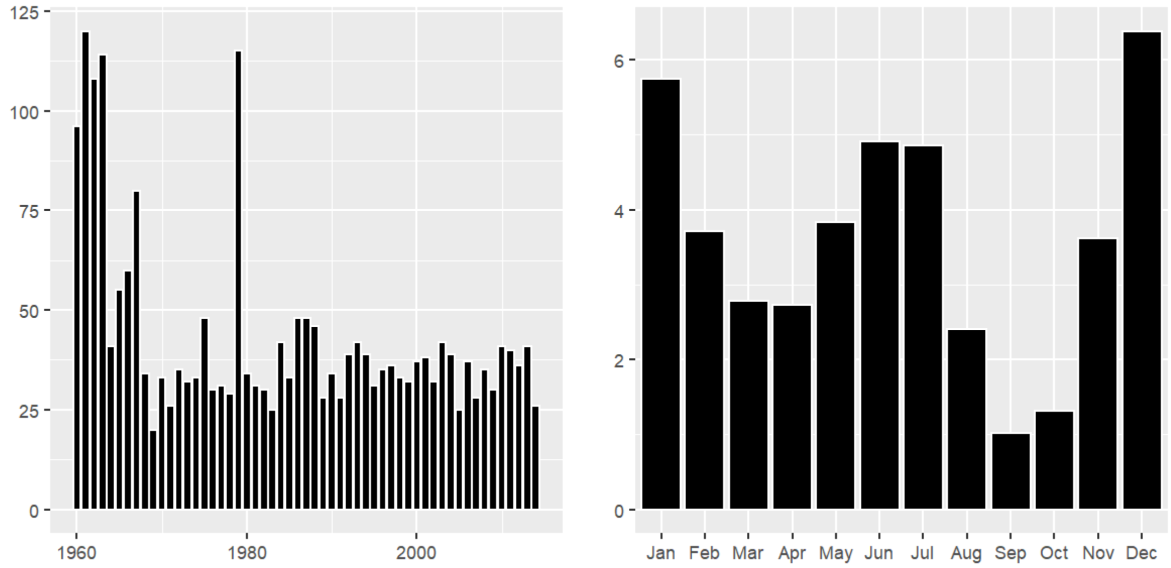


Figure 4: Number of days per year when Tev > Tmi and the monthly distribution of the mean number of days when Tev > Tmi

Furthermore, the monthly distribution of the remarkable years in terms of high values does not show clear anomalies or similar results when comparing the monthly distribution of those years. See below.

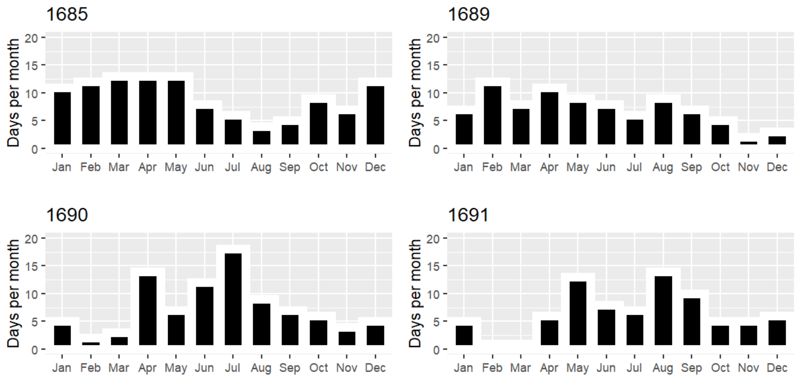


Figure 5: Monthly distribution of years in Morin's measurements which show a high value in terms of Tev > Tmi

We will address the differences shortly in our Paper.

C2.12-*Line 221 and figure 3: Is the same difference observed between morning, noon and evening on the Meteoblue data? Morning, noon and evening Meteoblue curves could be shown in Figure 3.*

This comment makes us reconsider this graphic. We were too focused on the mean of the Morin data giving a satisfactory result. However, looking at the results for the midday and evening measurements, one might suspect an inhomogeneity in the measurement series. Looking at the ERA5 results, one sees only small deviations in the result between the different times. Two problems in the methodology arise: (1) Rain or snow was measured by Morin by eye. I.e. if Morin noted snow, then it may have fallen in a specific time period, whereas temperature was read at fixed times. (2) The table of Morin's measurements illustrates another problem: Morin has six possible entry points for precipitation (column 12). At the respective measuring points for the temperature and in between.

Considering Morin's strict daily routine, the entry of the value which is on the line separating day for day should be noted at 2 o'clock in the morning. The next value was noted in line with the first temperature measurement. Thus, (2) can be satisfactorily explained for the morning temperature (1) is not so significant, since the temperature difference between 2 o'clock and 7 o'clock is a smaller one in the winter months. That is, point (2) for the morning has a tendency to minimally decrease the temperature of the 50% snow-rain-threshold. This fact is also reflected in the comparison with the ERA5 data.

However, for the midday and evening temperature these points are more problematic. In case of the midday temperature, it may be that it snowed at lower temperatures (e.g., at 10 a.m.) and then the temperature was measured at the daytime maximum. This also becomes visible in a comparison. For the evening temperature, an attribution is equally more problematic. Here, it seems to us that points (1) and (2) do not matter as much as that we applied Legrand and Le Goff's calibration for the evening temperature. This calibration has been elicited for the maximum temperature and thus, when applied to the evening temperature, will result in higher temperatures at the 50% snow-rain-threshold.

We will address these issues in the paper and elaborate further, as in this response.

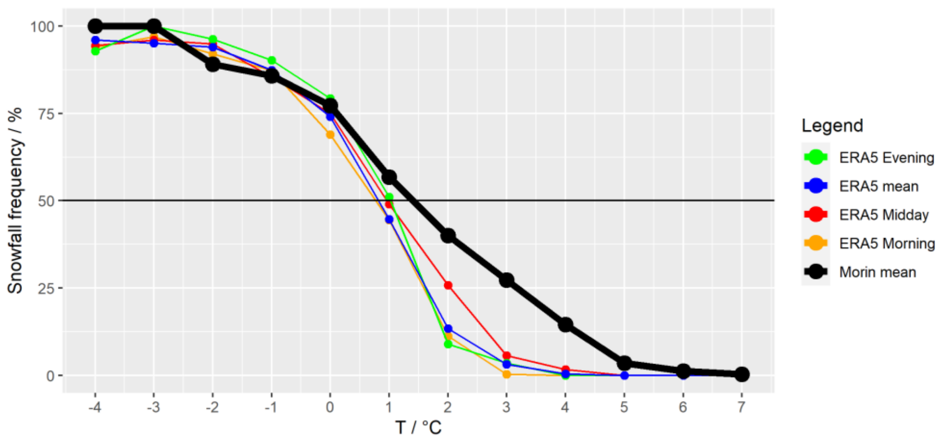
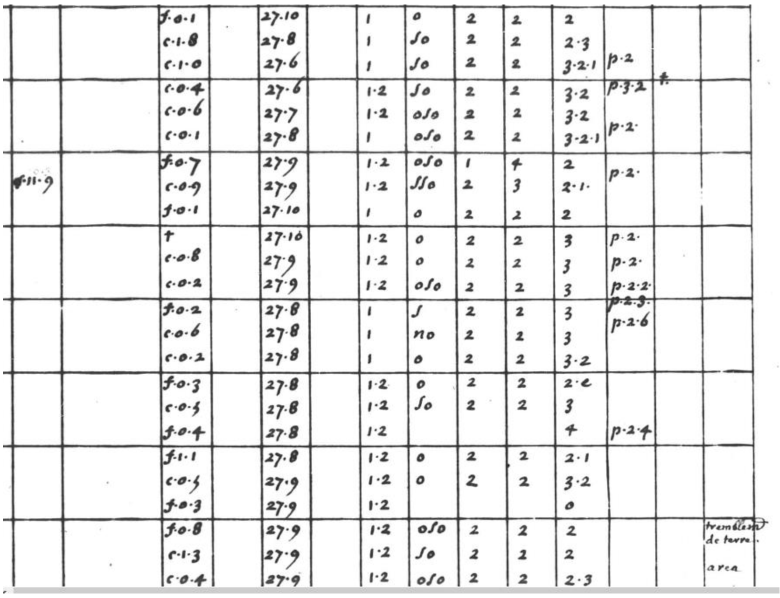


Figure 6: Snowfall frequency of Morin and ERA5 reanalysis data (1960 to 1990)



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	c·1·0	27·6	1	So	2	2	3·2·1	p·2			
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	c·0·1	27·8	1	oSo	2	2	3·2·1	p·2·			
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	c·0·9	27·9	1·2	SSo	2	3	2·1·				
	f·0·1	27·10	1	o	2	2	2				
	+	27·10	1·2	o	2	2	3	p·2·			
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	c·0·2	27·9	1·2	oSo	2	2	3	p·2·2·			
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Figure 7: Example of Morin's notes

C2.13-Line 235 and figure A2B: the differences noted in the thermal amplitude illustrated in the A2b would deserve comments and undoubtedly a more in-depth study (on a finer scale than the year), which could possibly make it possible to detect more precisely ruptures of the homogeneity of the series, the extreme values â€ ‹ â€ ‹ being more sensitive to the local environment of the observation. Figure A2b seems to indicate breaks around 1680, 1690, 1700, 1705 which do not coincide with Morin's changes of domicile. Are deviations of such great amplitude observed in Meteoblue data? Are these differences related to inter-annual variability or to changes in location or others modifications of measure conditions?

We think that the most remarkable change in terms of DTR appeared in September 1688. Also, the following 2 years are characterized by low values of DTR with especially low values during warm months. We will consider the points raised up in this comment and will look into more detail. E-OBS-data show no year where DTR does not exceed 10 °C from 1950-2019, whereas in the years 1689, 1690 and 1691 DTR stays mostly below 10 °C. So, this may be an inhomogeneity. However, it has to be pointed out that the drop of DTR of about 5°C can be explained because it seems that a cold front appeared. We will indicate this inhomogeneity in the paper and make a proposal for homogenization in the mentioned years.

This means that we want to keep the raw data for the analysis, but in a provided data file we will include a time series of the temperature measurement with the raw data as well as a time series of the temperature measurement with the included homogenizations.

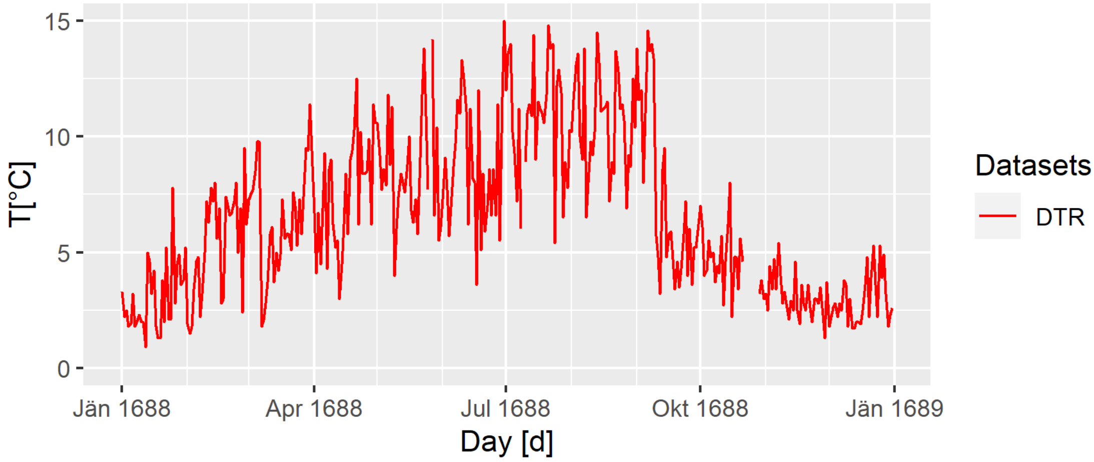

Figure 8: Diurnal temperature range (DTR) of 1688

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C2.14-Figure 4 – Legend: "Beaune" and not "Dijon"

Thank you for this comment. We will change that.

C2.15-Line 276 and table 2: "the extraordinary positive anomaly" observed between 1676 and 1680, which is not found in the CET temperatures, confirms the hypothesis of a break in the homogeneity of the Morin series in 1680 which could be discussed here

We discussed the positive anomaly and point out that Rousseau (2009) did a calibration on monthly basis by using the CET temperatures. We are not sure if the calibration can or should be done over the whole year. (See comment 2.9)

C2.16-Line 285: The same analysis of cold days for the period 1665 to 1675 seems feasible despite the lesser precision of Morin's small thermometer (distribution of measurements noted f4?) even if it means homogenizing with respect to the following period, due to a different threshold.

Because of the following points we want to exclude this time period: (1) We have less knowledge of the measurement instrument and (2) some methods we performed to validate the temperature measurements cannot be performed for this time period (Snow-rain-threshold,…). However, we can show cumulative number of values <= f4. Note that Morin used a different scale for 1665-1669 compared to 1670-1675. So, f4 will have different °C values in both time periods (See Rousseau, 2013)

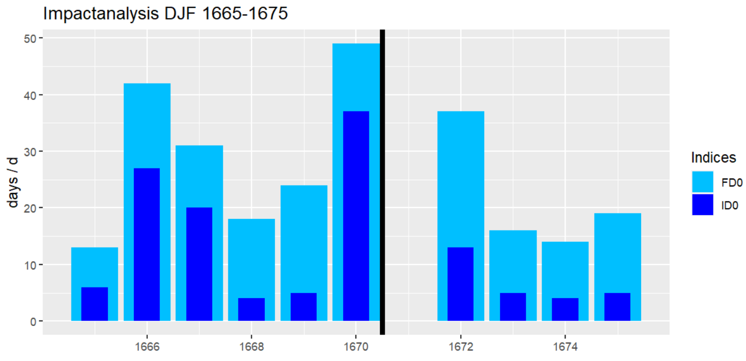

Figure 9: Impact analysis for DJF

C2.17-Line 293: Same comment concerning the hot days (distribution of measurements c4?) of the period 1665-1675

Same as in C2.16. Note here that for 7 June 1666 to 6 Sept 1666 Morin did not make measurements.

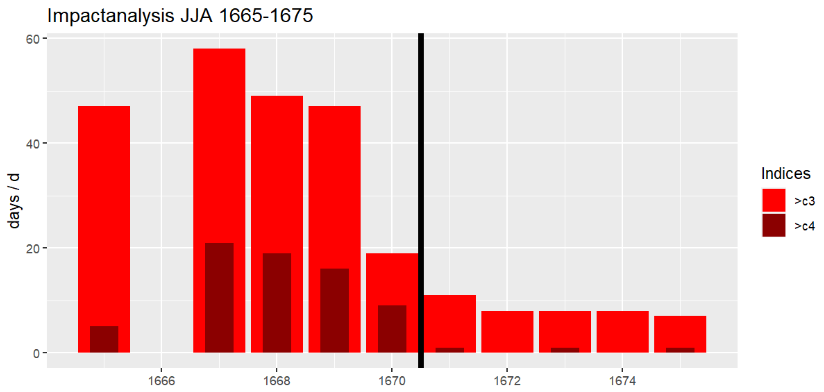

Figure 10: Impact analysis for JJA

C2.18-Line 318: The growth of TCC does not take place over the entire period 1676 to 1709 and therefore the use of a growth rate of TCC from a linear regression established over the entire period is not actually justified. We note very clearly in figure 8 that the 5 curves would be rather decreasing or stationary after 1693

Thank you for pointing this point out. We will change the graphic. Furthermore, we want to address in the continuous text that the increase may be due to an increase of his notes, because the highest value (4) increases from zero appearances in 1670 to 37 in 1690.

C2.19-Line 393: Is the only strong deviation of westerly winds observed in the decade 1700-1710 not due to the fact that it is the only decade including a complete phase of positive temperature deviations? The other decades present both rather warm or rather cold temperature phases of multi-decadal fluctuations. It would be interesting to examine whether a division into 4 periods, corresponding to alternately cold and warm phases of multi-decadal *fluctuations 1672-1675, 1676-1686, 1687-1701, 1702-1708 (cf Le Roy Ladurie et al., Fluctuations du climat, 2011) would not give clearer differences.*

For comparison, we looked at the indicated time periods in the polar plots. The strength of this illustration is that one can clearly see different distributions (Strong tendency of 1700s to WI, higher frequency of EI in DJF, etc...). However, smaller differences of individual indices are difficult to see. Thus, we will present differences of main wind directions (NI, EI, SI, WI) either in continuous text or in tabular form. However, we would like to keep the presentation of decades because we have focused on decade means in this paper. In particular, the difference in the DJF season between the 1690s and 1700s is salient, where the former has a WI=42% and the latter a WI=58%.

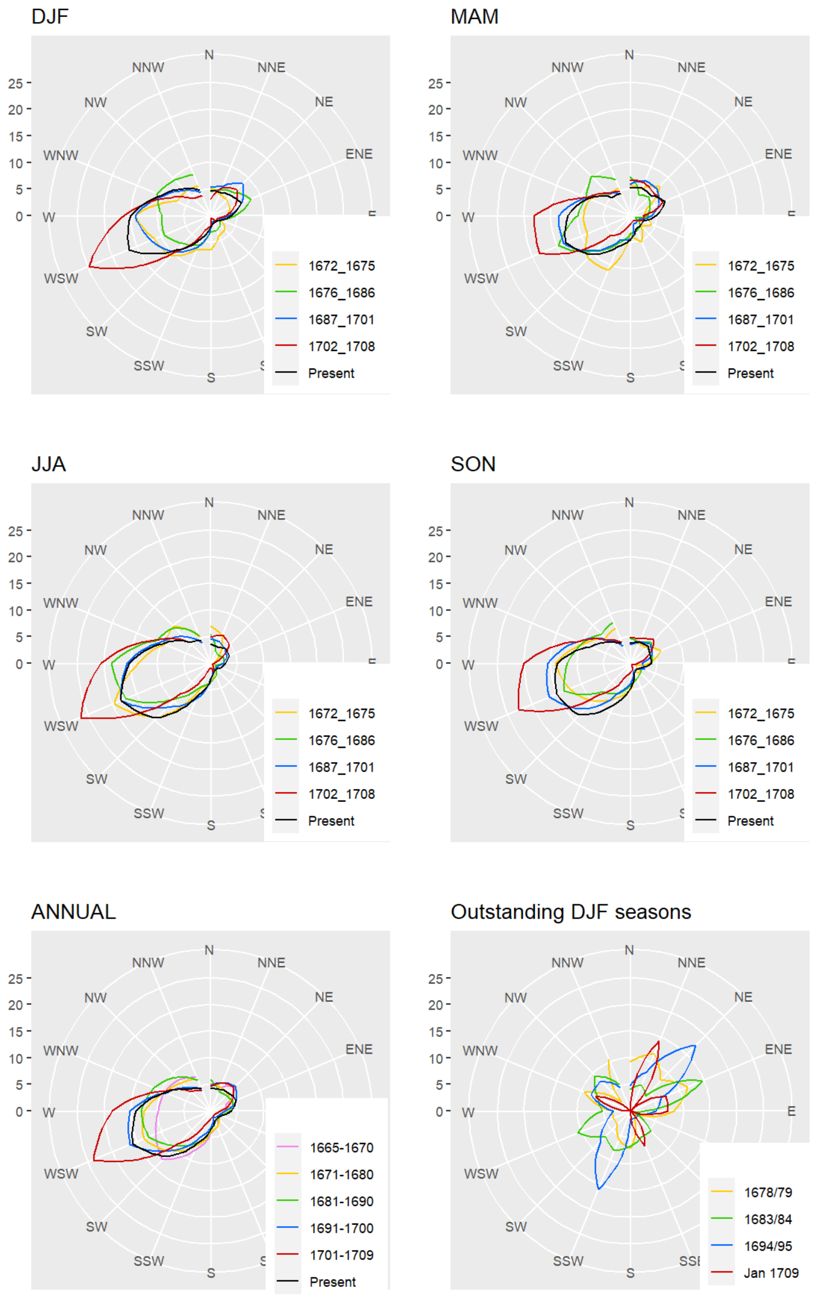

Figure 11: DI of the seasons and annual with different time periods

C2.20-Line 454-455: The complete consideration of temperature data from 1665 to 1675 (winter 1672, summer 1675 remarkable in particular) and from 1710 to 1713 as well as the question of the break in the homogeneity of the measurements suggested by the figure A2b, seems to us to deepen later if the article does not deal with it.

Thank you for this comment. We will address the last two points as already mentioned in the answers above but will not consider the temperature data from 1665 to 1675 in this paper (See C2.16).