Climate of the Past Discussions

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Comment on cp-2021-179

Anonymous Referee #2

Referee comment on "Subdaily meteorological measurements of temperature, direction of the movement of the clouds, and cloud cover in the Late Maunder Minimum by Louis Morin in Paris" by Thomas Pliemon et al., Clim. Past Discuss.,
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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.

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.

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".

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

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

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?

Line 164 and 165: "Rousseau, 2009" and not "Rousseau, 2013"

Line 166: "grape harvest dates" and not "harvest date"

Line 172: "monthly" instead of "daily".

Line 171 and Figure 4: .

The assessment of a bias in Morin's measurements from 1776 to 1780, 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.

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?

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.

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.

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?

igure 4 – Legend: “Beaune” and not “Dijon”

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 dicussed here

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.

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

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

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.

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.