Climate of the Past Discussions

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Comment on cp-2021-179

Anonymous Referee #1

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., https://doi.org/10.5194/cp-2021-179-RC1, 2022

The paper describes a meteorological record covering the late 17th and early 18th century for Paris. Given the length of the record for a period in which instrumental records are very rare, the data and results presented are relevant to the scientific community. However, in my view the manuscript presents major flaws in terms of how the data are analysed and the results interpreted.

General comments:

- The comparison with modern data is misleading, as the historical data are affected by unknown biases related to the measurement location, thermometer scale, and other factors. These biases can easily surpass climate variability in magnitude. I understand the appeal of comparing historical extreme climate events with modern climate, but this is not possible in a scientifically sound way without applying the necessary bias adjustments - which is very hard if not impossible in this case given the lack of metadata. This is particularly true for temperature and cloud cover.

- In addition to the previous point, the Meteoblue dataset used by the authors is a commercial product that, as far as I know, has never been evaluated in peer-review literature. If that is true, it should not be used for a scientific article. My suggestion is to drop the use of a modern reference period for temperature and cloud cover - except perhaps to assess data quality such as for the NDR calculation - and concentrate on the decadal variability of the studied period.

- The temperature record is clearly affected by inhomogeneities. The authors actually do a very good job in pointing them out, by mentioning relocations, changes in the temperature scale, and changes in the ventilation of the instrument (Tab. 1; also Fig. 1a

and 1b point to at least two important inhomogeneities). However, this fact is completely ignored when analysing the data. There are some confusing sentences about this at the end of Sect. 3.3 that actually raise even more doubts about the quality of the data. I believe that some kind of statistical homogenization is necessary, even though reference series for this period are scarce. Beside the Central England Temperature series, there exist many temperature reconstruction that could be used.

- How does this record relate to the widely available long monthly temperature series for Paris ? Is that series also based on Morin's observations? Are there any differences from your data?

- Many equations and definitions appear in the results. They should be moved to the methods section.

- I am not a native English speaker but the quality of the language seems rather poor to me, to the point that I had difficulties understanding some sentences.

Specific comments:

- The procedure to convert the temperature readings to Celsius need to be explained more in details, since the given references are in French. Besides, the conversion formulas (Eqs. 1-3) are not completely clear to me: I would expect that the TM in the three equations refer to different observation times, but this is not indicated. Moreover, it is often mentioned in the manuscript that the the thermometer was filled with spirit: is this an assumption or a known fact? How do you explain that a linear conversion does not introduce a bias at high temperatures?

- Equation on page 10 (number missing): I believe the indices i,j,k here are in the wrong positions.

- P11, L207: Dai (2006) shows that the effect of pressure on snowmelt is negligible in the lower troposphere. Besides, increasing humidity cause the melting point temperature to decrease (i.e. a lower temperature is required for snow), not increase. More importantly, precipitation phase at the surface depends on the temperature profile above the station, of which surface temperature is merely a proxy (e.g. it can be significantly warmer 1 km above the surface than at the surface, hence it can rain with negative temperature). Another important factor is precipitation intensity (higher intensity implies higher melting point temperature).

- P11, L215: "So, if..." - something wrong with this sentence, snowfall frequency is not

measured in °C.

- Equation on page 12 (number missing): What is the factor 2 for? The notation for the sums is confusing.

- P12, L231: What is a typical value for NDR for data measured indoor? How relevant is the change from 0.8 to 0.95 in 1688?

- P13, L263: The Maunder and Spörer Minima are defined by solar activity, not by climate, and the influence of solar activity on climate is still uncertain. This is mentioned briefly later in the manuscript, but I believe it should be clarified already in the introduction. The choice of LMM to describe the period covered by the data is perhaps not the best as it gives the impression that the climate anomalies were mainly driven by solar activity.

- P22, L349: "This means that..." - Circulation is an essential requirement for cold winters rather than an additional driving factor. Even a possible solar influence would mainly act through changes in circulation (e.g. Barriopedro et al., 2008).

- P22, L367: How is exactly the DI calculated from modern data? Are clear days excluded? If not, there would be an obvious bias with respect to Morin's observations. How dependent are the results from the choice of the levels? This comparison should be done using an open, peer-reviewed dataset (e.g. ECMWF ERA5 reanalysis), or dropped.