Climate of the Past Discussions

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Comment on cp-2022-75

Anonymous Referee #2

Referee comment on "Earliest meteorological readings in San Fernando (Cádiz, Spain, 1799–1813)" by Nieves Bravo-Paredes et al., Clim. Past Discuss., https://doi.org/10.5194/cp-2022-75-RC2, 2022

Reviewer comment on _Earlyiest meteorological readings in San Fernando (Cádiz, Spain)_ by Nieves Bravo-Paredes et al.

I have read the paper by Bravo-Paredes et al and I think that there are parts of the paper which are valuable and, therefore, I am inclined to support its publication. On the other hand, I find that some parts of the paper are extremely speculative and, therefore, I suggest some major revisions before the paper may be accepted in Climates of the Past.

Major points

1. In general, the paper needs a serious revision of English. I have seen that there is a comment suggesting many changes before. Even though English is not my mother tongue, I have found many examples of sentences hard to read, with lack of correct position of words, lack of proper conjunction of sentences and similar errors. For instance, the junction between sentences in lines 15-17 of the abstract is not correct. There are many such instances. This must be corrected before a new version is submitted.

2. In page 11, the authors discuss a result which is a little bit surprising for me, which is the fact that **summer temperature** (page 11) during 1799-1813 was apparently **lower** than current ones (1997-2021). This is a little bit surprising for me. The authors mention later that (sic) **this higher than usual winter temperatures**. I, therefore, think the authors must clarify whether they mention winter or summer. On the other hand, they select some locations over Europe (Madrid, Mallorca San Fernando or Switzerland) that show a *similar* behaviour, with some of them (Switzerland) really far away. I think the authors must make a better effort in this part either by a deeper analysis of literature or additional reconstructions (instrumental or multi-proxy) covering that period, so that they can give a satisfactory explanation here. Otherwise, for me, it is hard to accept their line 286-287 without further (and better) analysis.

3. Page 14, line 320. Authors write (sic) *the highest precipitation values were recorded in these months* and they previously mention Nov 1812, Dec 1812, Oct 1813, Nov 1813 and Dec 1813 (line 319 and Figure 9). However, when looking to Figure 8, if the label of the horizontal tick marks corresponds to January, I don't see that November 1812 or November 1813 are particularly rainy at all. This is consistent with the maps in Figure 9. Thus, I wonder whether the sentence is true, or whether they refer to anomalies or whether there is an alternative explanation for their sentence. If there is not a better explanation, I find the sentence misleading and not consistent with the information provided by the paper.

4. My major objections, however, deal with the application of the dataset for its application to two case-studies, the Battle of Trafalgar and the volcanic eruption in 1809. I find this part is in general very speculative and that the authors do not provide sound support for their interpretations.

- The Battle of Trafalgar. In this case the interpretation seems to me relatively straightforward, but I find that this interpretation is actually sound because of the use of the EKF400v2 dataset rather than because of the analysis of a single time series in San Fernando. I can accept that this part seems reasonable, and that the dataset prepared by the authors points in the same direction as the synoptic data reconstructed by EKF400v2.
- Volcanic eruption of 1809. I find this part really speculative.
  - On the one hand, the correct date that the eruption happened can not be accurately fixed. The authors find the lowest anomalies during July (at San Fernando). Being the volcanic influence on temperature probably global, I find urgent that the authors cross-check their findings with global or hemispheric reconstructions. If this is true in San Fernando and not an artefact, it shuld be true for a wide area of Europe as well. Is it?
  - I find strange the phrase *anomalies calculated with the period 1805...2021)* in lines 373-174. I would say that the anomalies computed with the series x by using its own average $\bar{x}$ ($\Delta x^* =x-\bar{x}$) will always be higher (not lower) than the ones calculating after adding a constant positive bias (greenhouse warming) to the series ($\Delta x^\dag =x-(\bar{x}+\bar{X}_{GH})$). Thus, I can not understand this sentence by the authors. On the other hand, this must be reconciled with point 2 above, which deals with the fact that the authors mention that current temperatures are lower than the ones in early XIXth century (which I still find hard to believe, but which is opposite to their assertion here).
  - I find sentences 381-392 totally speculative and I think they are not supported by a detailed analysis.

Thus, the references to the volcanic eruption are, in my opinion, a problem of this paper. I think that the production of the dataset and making it available for other scientists has some value that might merit a publication. However, the analysis of the volcanic eruption is a very weak point of the paper. The authors should make it robust or just remove it.

Minor points

- I guess words *tiempo, planetas, longitudes terrestres and fisica celeste* in line 69 should be translated to English for better multicultural understanding
- Line 76. The authors mention subdaily observations but then, later, they mention that data collection was carried out at 2 pm (which is just daily from my POV).
- Line 116. "The prevailing wind direction...1813". I think the authors must expand this explanations. Was any recording instrument involved? Was that recording just evaluated subjectively by observers?
- There are some missing references in page 8.
- Is there any good reason that a relatively truncated approach to local gravity (Eq 7) is used instead of current values of gravity at the area (which probably are better and more precise)?
- Page 9. Together with the value of R^2, the sample length and, even better, the confidence interval of the null hypothesis (do thermometers read linearly related temperatures?, I guess).
- In Figure 4 (low panel) and Figure 5, I find problematic the selection of the solid data for the modern period and the dashed lines for the data recovered by the authors. The reason is that the modern period is very far in time and, as such, it is expected to be less related to the actual measurements than the means computed from the daily data. However, the use of solid color for the modern dataset makes it more visible. Thus, I really encourage the authors to use solid line for the monthly (daily) data retrieved by the authors in this study and dashed lines for the data from the modern 2006-2021 period. The text mentioning these two figures (mentions to solid or dashed lines) should be changed accordingly.
- Line 296. Authors mention that westerlies are more common in the area, which is basically to be expected, since it is located in extratropical latitudes of the Northern Hemisphere. May be just add a sentence explaining this. For me, it is not a surprise.