**Climate
of the Past
Discussions**

Interactive comment on "Multiscale regression model to infer historical temperatures in a central Mediterranean sub-regional area" *by* N. Diodato et al.

Anonymous Referee #3

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The manuscript "Multiscale regression model to infer historical temperatures in a central Mediterranean sub-regional area" by Diodato et al., can be a relevant contribution as supplies methodological issues concerning the reconstruction of temperature series for a poor-covered geographical region.

However, I must recognize that it has been really hard to me to read it, so I recommend to reconsider it after a major revision. The first issue is that the organization of the paper is very confusing as the authors split the ideas among sections and, therefore, it is really difficult to follow the methodological reasoning (which I understand it's the main issue of the paper). Other concerns are regarding the choice of the methodology

and the differences among this paper and that one of Camuffo et al. (2010) "500-year temperature reconstruction in the Mediterranean Basin by means of documentary data and instrumental observations", Climatic Change, 101: 169-199 or Diodato and Bellocchi (2010) "Accomodated high-resolution historical temperatures in the Mediterranean area" Met European Research Observatory Report, 1. I recommend the authors to rewrite the paper in order to clarify and focus in the main goal of the paper. It's necessary to clearly define the method (being readable) and present clear arguments (mode details about the calibration and validation processes, a more in depth comparison with other reconstructions and methodologies) that support the goodness of their method. Probably, a change in the structure and redaction will clarify many points and will led to a better valuation of the contributions of the authors.

From a "literary" point of view, the English usage is confusing, there are some incidental comments (as the one starting the Introduction), there are unfinished sentences (i.e., in page 2628, line 24), the mathematical/statistical terms are sometimes confused, and the organization is chaotic extending the methodology along several sections.

In the Abstract the authors introduce "a statistical methodology in the form of multiscale-temperature regression (MTR)-model" that seems to be the focus of the paper after led us to think that they are going to present an "unprecedented historical dataset" (Diodato and Bellochi, 2010; Camuffo et al., 2010??).

Following the manuscript, the Introduction presents the need to obtain more representative temperature series for a better spatial coverage and makes a short review of some regional and global reconstructions (each one resulting from different methodologies). The importance of documentary data (categorical data) is then mixed with other sources of information (i.e., continuous-scale data as tree-rings) without leading to any specific idea.

In section 2, there are confusing paragraphs as "Particular attention was paid to the calibration procedure" (page 2630, line 20) without a later reference to this crucial point.

Also unreferenced conclusions as "1742-1754 and 1792-1818. These two intervals are considered the only reliable records in the historical time for this area" contribute to lose the reader. I think that the model lines of section 2.1 should be joined with the more methodological sections 2.2 and 3 and reordered to achieve a more organized explanation of the methodology.

I miss a section analyzing the resulting series (probably taking part of the end of section 3.2) prior to the conclusions.

Just finishing with the "design" aspects, figure 2b lacks a better explanation (I supose that the numbers are the correlation coefficients, among what?)

From a scientific point of view, I think that the idea can be promising if it's well explained. But I think that some points must be clarified.

First of all it's an old subject regarding the use of discrete data (categorical) to obtain a continuous-scale time series. From an statistical point of view there is a huge amount of literature presenting different methods to treat with both kind of data. Therefore my question is, why this method and not other (like structural equation modeling)? or have you compared the performance of the method to other ones (like those mentioned in section 1) This is not to judge the validity of the model but the reason of why this better than other approaches.

Another one is a "technical" question; there is no information about the method to solve the system of equations (derived from the different equations and that mix linear and non-linear terms) but in the section 2.1 you mention that MS Excel 2003 was used to model development (page 2631, lines 1-2). Excel is well-known for its poor ability to deal with rounding and error propagation is a critical point in the resolution of such complex systems. How did you estimated/considered this error (due to method selection and software choice)?

Calibration and validation are just mentioned, but it's described as a critical procedure

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in page 2630 (line 20). They need to be properly described (method and results).

I'm also confusing about the use of the term "multiscale". I'm not sure if it means the downscaling in the spatial sense, the change in time resolution, ...

I also miss a more in depth comparison among the different reconstructions for the regional scale, not just that one of Luterbacher, in order to present a demonstration of the efficiency of the model presented.

Interactive comment on Clim. Past Discuss., 6, 2625, 2010.