**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.

N. Diodato et al.

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To the Editor of Climate of the Past

Subject: reply to Reviewer #3, manuscript cp-2010-91

Dear Editor,

The Reviewer #3 has sent his comments at two stages, once on the originally submitted version and then on the revised version available online since 14 February 2011. This revised version addresses some issues raised by the Reviewer. The authors have made an effort to answer to all questions and have submitted an improved version

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of the manuscript. Hope further comments in the future will refer to the most recent submission.

Sincerely, The authors

Anonymous Referee #3 Received and published: 03 March 2011

Reviewer. I must recognize that it has been really hard to me to read it, so I recommend to reconsider it after a major revision.

Authors. The new version of the manuscript features an enhanced focus on research as the foundation for methodology, along with improved readability and more background.

R. 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)

A. Indeed the text does not reflect a conventional structuring (Materials and Methods, Results, etc.). However, the logical flow of the text is placed at the end of Introduction (lines 107-111). The Editor may tell us if a radically different structure of the text is needed.

"The next section describes the geographical environment, the datasets and the developed methods. Section 3 illustrates the novel mixed-model approach in detail. Its results on temperature series estimation were evaluated over the MSA. Conclusions (Section 4) point out the main results and look at horizons for future research".

R. Other concerns are regarding the choice of the methodology nd 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.

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A. The series by Camuffo et al. (2010) is a homogenised one from different sources, with no modelling effort. It was basic for the model development in the Mediterranean Sub-regional Area, as illustrated in this paper. The other source (Diodato and Bellocchi, 2010) is a working document, preliminary to this study.

R. I recommend the authors to rewrite the paper in order to clarify and focus in the main goal of the paper.

A. Evidence of improvements in the text is documented in the resubmitted manuscript, since the one supplied on 14 February 2011, revised after the review process described in the answers to Reviewers.

R. 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.

A. "Model parameterization and evaluation" is now a distinct section (section 2.3). Section 2.1 has become "Study area and datasets". We have not intended this paper as an "in depth comparison". Main issue here is the downscaling of continuous data (available from regional-scale simulation) using documentary proxies (available at the sub-regional scale). We claim our approach innovative and prone to extended applications (in section Conclusions). Interest in it stems from the critical review of the approaches published so far (lines 75-94).

R. 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.

A. The incident was taken out (line 32). The unfinished sentence (lines 88-90) was completed ("variance" was missing). Hope the revised manuscript is clearer in quality.

R. 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??).

A. This is a comment on the manuscript first submitted. The new abstract reads as follows (lines 15-21):

"This paper has exploited, for Southern and Central Italy (Mediterranean Sub-Regional Area), an unprecedented historical dataset as an attempt to model seasonal (winter and summer) air temperatures in pre-instrumental time (back to 1500). Combining information derived from proxy documentary data and large-scale simulation, a statistical downscaling approach in the form of multiscale–temperature regression (MTR)–model was developed to adapt larger-scale estimations (regional component) to the sub-regional temperature pattern (local component)".

R. 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.

A. In the revised manuscript, the relative paragraphs were rearranged according to a logical outline (lines 75-94): 1) importance of pre-modern instrumental weather indices for climate reconstructions (with no reference to spatial scales), 2) examples from the international literature, 3) limitations in the approaches used in the cited studies, 4) the strength (and gaps) in current evidence of using documentary sources. The latter opens up to the mode in which this study is introduced (lines 95-97).

"Several authors such as Luterbacher and Xoplaki, (2003), Pauling et al. (2003), and Ge et al. (2005) suggested that pre-modern instrumental weather indices may be promising to enrich climate reconstructions. Different sets of proxy-variables have indeed been used to find out relationships between predictors and predictands in high-resolution climate time reconstructions (e.g. Wang et al., 1991; Briffa et al., 2002; Larocque and Smith, 2005; Moberg et al., 2005; Diodato, 2007; Davi et al., 2008). Many of these reconstructions depend on empirical relationships between proxy records and climate data. Comparing linear algorithms and neural networks, Helama et al. (2009) proved that both the approaches are reliable for temperature reconstruction. Although regression-based techniques have been used with considerable success for climate reconstructions, they can engender bias in the estimates if not employed with care (Robertson et al., 1999; Moberg et al., 2005; von Storch et al., 2005). Moreover, these relationships are seldom based on a training process capable to capture all the possible data combinations that occur when extrapolation is performed (i.e. reconstruction period). With reference to dendroclimatological studies, correlation between tree-ring proxies and temperature data was found to only explain about 50% of the variance (Liang et al., 2008; Helama et al., 2009; Tan et al., 2009). Documentary data series are expected to better correlate with temperature, the overall explained variance being of about 70% (Leijonhufvud et al., 2008; Dobrovolná et al., 2010). However, there are few estimates of uncertainty in documentary based climate reconstructions (Moberg et al., 2009). In this study, we have considered an alternative approach to address the statistical modelling of temperature variability, based on documentary records and previous large-scale reconstructions".

R. 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.

A. The text gives the reasoning behind the calibration steps. Particular attention was paid to climate periods and number of years used for calibration. These choices are

justified in the text (lines 204-206). Few more details are given as it regards the workflow based on a set of software tools (lines 217-218). The recursive procedure is described at section 3.1 (lines 269-298). Results of calibration procedures are illustrated in section 3.2, where estimated parameter values are reported (line 302). Figure 4 and Table 2 (and the relative text in section 3.2) provide graphical and statistical evidence of calibration tests. Some limitations in the calibration procedures are now discussed in section 3.3 (lines 372-385).

"Historical climate in the MSA and the modelling background of the authors represented the basis of this study. Indeed, there may be other ways to assess temperature series at sub-regional scale. This implies that the proposed multiscale model is an applied model with a sufficient degree of reliability, but more rigorous and robust analytic techniques could be applied to better precisely define and evaluate the same problem. For instance, structural equation modelling accounts for correlations among predictors and can estimate indirect effects of predictors on other predictor variables that taken together affect the outcome (Hair et al., 1998). To determine an appropriate balance between computation, complexity and uncertainty, we have relied on ad-hoc model development and trial-and-error assignment of model parameter values via spreadsheet utility. The use of MS Office Excel 2003 solver to minimize the square error of estimation is indeed a common solution, as in previous papers (e.g. Diodato and Bellocchi, 2007, 2010), although other products are available which offer algorithmic improvements (e.g. Menascé et al., 2008)".

R. 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.

A. In the original form, the phrase may have indeed introduced some ambiguities. The reconstructed series by Camuffo et al. (2010) – homogenization of multi-observational

datasets - is more reliable for the two time slices chosen for validation, where measured data are the only available for Central-Southern Italy in historical times (lines 210-213).

"In particular, the periods 1742-1754 and 1792-1818 were selected for model validation. Measured data are available for these two intervals in Central-Southern Italy, which are therefore considered reliable records in the historical time for this area".

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

A. The focus here is not on the temperature series analysis itself that would need to accommodate too many aims and objectives in only one paper. A paragraph was added to "Conclusions" in relation to future work on this issue (lines 432-434).

"The temperature series analysis itself is also an issue, which could be the subject of a future paper".

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

A. The Reviewer is probably referring to Figure 1b. In the revised manuscript, the legend gives full explanation.

"Winter temperature correlation patterns (values rendered in white are not significant, $p>0.05$) between one grid-point of Northern Italy ($46°$ North, $12°$ East) and grid-points over central Mediterranean Europe (the MSA is squared), as processing by Climate Explorer with E-OBS version 3.0 gridded dataset (http://eca.knmi.nl/download/ensembles/ensembles.php) for the period 1950-2010".

R. 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.

A. The revised manuscript shows evidence of improvement in the accuracy of the modelling approach (section 3.1).

R. 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.

A. The use of categorical data to obtain a continuous-scale time series is not indeed the main issue. The use of categorical (local-scale) data to downscale continuous series from regional to sub-regional scale constitutes the original contribution of this paper. Section 3.3 is an account of some limitations in the methodology, which addresses the issue raised by the Reviewer.

R. 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)?

A. Excel solver is of common use to handle with optimisation options. The procedure chosen for calibration corresponds to published approaches, also used by the authors in previous papers (e.g. Diodato and Bellocchi, 2007, J. Hydrol. 345, 224-236; 2010, 387, 119-127). Possible inaccuracies introduced by the optimisation procedure are now discussed (with reference) in section 3.3 (lines 380-385).

R. Calibration and validation are just mentioned, but it's described as a critical procedure in page 2630 (line 20). They need to be properly described (method and results).

A. Section 2.3 explains the choice made for selecting the validation set. The choice

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made for validation of two time slices (1742-1754 and 1792-1818) is now better justified (lines 210-213). Figures 5 and 6, and Table 2 (and the relative text in section 3.2) provide evidence of the validation of the model.

R. 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, ...

A. The model has a multiscale spatial structure because it combines local indices (documentary proxy-based weather anomalies) and regional temperature data. This is now specified in Introduction (lines 102-104).

"(MTR)–model has a multiscale spatial structure because it combines documentary proxy-based local-scale weather anomalies with large-scale temperature data to adapt regional temperature data to specific sites and seasons".

R. 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.

A. In this paper, we do not intend to compare alternative regional reconstructions. Here, we are trying to show the usefulness of combining local documentary proxies with regional simulations in order to obtain reliable estimations at sub-regional scale.

Anonymous Referee #3 Received and published: 23 March 2011

R. I must apologize as I place a comment for the original manuscript as you can see by the page references that I mention but I still hold my opinion as the key points are still in the new version. A substantial rewriting should be done in order to help reading and understanding of the manuscript.

A. Hope the new submitted version is clearer in quality.

R. In addition, it can be helpful if the authors can provide information about the nature of their datasets. Luterbacher's data are a gridded reconstruction (we know space and

time resolution) but I don't know if the new data analyzed by the authors correspond to a set of series from several localities (implying a certain continuity in space and time of the data) or sparse news (implying discontinuities in time and space). In the paper they show a single reconstruction of a "sub-regional series" but I don't know the number of localities used for the reconstruction. Also, I don't know if the data are used directly in the model or the authors have made a previous manipulation of data. This is specially relevant in order to know what are we speaking of.

A. For their reconstruction, Camuffo et al. (2010) provided this kind of information (continuity/discontinuity in space/time). Few lines have been added to precise this issue (lines 132-136). We have developed a modelling approach also based on these published data, thus more details in our paper about the original series would make the text unnecessarily heavier.

"It is in Moio and Susanna Manuscript (Ferrari, 1977) that a continuous temperature series is supplied for Catanzaro (38° 54âĂš North, 16° 36âĂš East) from 1461 to 1768. More recent weather information is available from different sources at sparse sites, which are suitable for reconstruction of primitive series (Camuffo et al., 2010)".

Please also note the supplement to this comment:
http://www.clim-past-discuss.net/6/C1579/2011/cpd-6-C1579-2011-supplement.pdf

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