Response to reviewer 1:

We thank reviewer 1 for his constructive and useful comments, which have helped to strongly improve the discussion concerning the robustness of the results and of the clarity of the proposed mechanisms.

We have used the few reconstructions proposed by the reviewer to evaluate the robustness of our conclusion.

First of all, we have analysed the Luterbacher et al. (2004) reconstruction. Over the whole European region, this reconstruction showed a better correlation of 0.45 with solar variations as compared to Guiot et al., but still lower than the model (cf. new Fig. 1)

The Luterbacher et al. (2004) reconstruction is shorter than the Guiot et al. (2010). As a consequence, the regression over solar forcing was only possible for 350 years in the Luterbacher et al. reconstruction. Moreover, the spatial extent of this reconstruction is smaller as compared to Guiot et al. Consequently, it is difficult to really draw robust conclusions from this dataset.

Nevertheless, we compute the same regression as for Guiot et al. and the model, but only over 350 years. The results, shown in Fig. R1, reveals a very strong warming over Northern Europe and almost no signal over Southern Europe. This pattern is similar to what we found in Guiot et al. and the model concerning Scandinavia. It exhibits clear differences over Central Europe, where no significant signal was found in the model and the Guiot et al. (2010) reconstruction. Nevertheless, when we compute the same regression over only 350 years in Guiot et al. and the model (Fig. R1), we notice that very few points are significant in the Guiot et al. (2010) reconstruction. Thus, we believe that evaluating solar forcing on a longer time frame clearly improve the significance of our regression by increasing the ratio signal-noise. Indeed, over the period 1001-1500, the regressions show similar pattern than over the whole period used in the paper (Fig. R2).

Thus, the latitudinal response to solar forcing found in the Luterbacher et al. (2004) reconstruction is different from what we found in the Guiot et al. (2010) reconstruction. Although it shows a latitudinal decrease from the north to the south, the minimum is more to the south as compared to Guiot et al. (2010) and the model, which exhibit a minimum around 45°N to 55°N (Fig. R3). Thus the mechanism we depict in the model in Central Europe is not detectable in the Luterbacher et al. (2004) reconstruction. Nevertheless, the spatial (the reconstruction stops at 40°E, contrary to Guiot et al. (2010) reconstruction that goes east to 60°E) and temporal (350 years as compared to 850 years) limitations of this reconstruction prevent to clearly reject our hypothesis. Nevertheless, this brings us to be more cautious concerning our results and to better insist on the limit of our hypothesis.

Moreover, Mc Caroll told us (personnal communication) that his North Fennoscandia reconstruction show little similitude with solar variability. His reconstruction is nevertheless not available yet. We find similar results when comparing Lindholm et al. (2010) reconstruction over Scandinavia and solar forcing. This caveat has also been included in the corrected version of the paper.

Concerning the Büntgen et al. (2011) reconstructions, we compare them with solar forcing and find that the correlation coefficient (0.15) is not significant at the 90% level over the period 1000-1850. Nevertheless, our analysis reveals that the signal over Central Europe is very weak due to the negative feedback that we depict here, so that this can mask the solar signal. The same result of non significant correlation with solar forcing is found for the reconstructions in the Pyrenees (Büntgen et al. 2008), which is a region of better signature of the solar forcing in our Fig. 2. We therefore discuss this in the conclusion, to insist on the fact that the results presented here are not coherent with other reconstructions and should therefore be taken with caution, and need further work to be confirmed. We also discuss the hyptohesis that the differences between the tree ring reconstructions (Lindholm et al. 2010, Büntgen et al. 2008, 2011) and the Guiot et al. (2010) reconstruction may be related with the inclusion of pollen data in the latter that may improve the low frequency representation.

All the main changes done in the manuscript are highlighted in yellow in the revised version of the manuscript.

Specific comments

2.1 2 Experimental design

- Please add a brief (1-2 sentences) model description (model components, spatial resolution) to the experiment description.
  We add this brief description.
- Please mention the amplitude of the solar forcing used, e.g. by comparing the value in the Maunder Minimum to present days value.
  We now mention that solar forcing variations for the TSI reconstruction that we use represents a 0.24% decrease of the mean TSI between present day and the Maunder Minimum
- related to the assumption, that regression captures mainly the solar signal: The authors could exclude the years that are affected by volcanic eruptions (e.g. the 10-20 years following an eruption) and calculate the regression again. If the results are comparable this would strengthen the assumption.
  We have done the proposed analyze and the results are shown in Fig. R4, where we exclude the 11 years following the eruptions larger than Pinatubo. There are 713 years remaining for the regression.
  The main regression pattern remains similar when excluding 11 years after the eruption, with no significant signal in Central and Eastern Europe and a significant warming in the North and South Europe both in the data and the model. This gives a few more confidence in our assumption. We added a word on this test in the manuscript.

2.2 3 Result

- Related to the model-reconstruction comparison: Between 1500 and 1700 the reconstructions and the model results differ strongly. Moreover, the reconstruction show more or less the opposite pattern than the solar forcing: the highest values in the reconstruction can be found in periods with the lowest solar activity, the Spörer and the Maunder Minimum. Could you please comment on this and maybe explain, why we find this pattern in the reconstruction but not in the model. Please also comment on how this period affects the solar-temperature regression. In which way does the regression change, when the period 1500-1700 is excluded from the regression?
  We take advantage of the additional reconstruction from Luterbacher et al. (2004) that we include in the new Fig.1. This reconstruction shows a better agreement and correlation with solar forcing over the period 1500-1700. Therefore the disagreement between the model and the Guiot et al. (2010) reconstruction over the period 1500-

1700 is not confirmed using the Luterbacher et al. data. The cause for this disagreement can therefore come from uncertainty in the reconstruction for this time period. We can also propose that during this time period, internal variability of the climate was very large and the model does not capture this effect. We add a discussion on this point the manuscript.

- Why do you refer to the region with increased cloud cover etc. as 'Central Europe'. For me it looks more like 'Central and Eastern Europe', at least the maximum cloud cover reductions are found over western Russia.
  We replaced "Central Europe" by "Central and Eastern Europe" as advised.
- related to Fig. 4: It would be easier to compare the degree of similarity between model and reconstruction if confidence intervals for the zonal mean regression coefficient were included in the figure. Moreover this would help to interpret the difference between the latitudes. Since less land grid cells are found in the north compared to the south, the regression slopes are not directly comparable without an estimate of the uncertainty.
  We have added an error bar on the Fig. 4 that represents two standard deviations of the residual of the regression. This helps to account for the difference in the number of points for the zonal mean and is indeed very useful to have a better idea on the uncertainty in the regression.
- '... we diagnose that 65 % of this increase is related to convective precipitation'. Where does this number come from? Is it possible to distinguish convective precipitation from large-scale precipitation in the model and is the estimate based on this information? Then please mention it.
  Indeed, in the model, the convective precipitation can be distinguished from the large-scale precipitation. We have used the convective precipitation to evaluate the amount of precipitation that it represents over a region (10°E-50°W, 45-65°N, Cf. Fig. R5) and obtain the value 65% cited in the text. We have clarified this in the manuscript.
    - if this is not possible, how robust is the existence of the Schär et al. (1999) then? Could it also be, that the increased precipitation is only a local process with a 'recycling of the additional evapotranspiration'?

    The former diagnostic and Fig. R5 show that this not only a local recycling process, but affect the large scale precipitation as in Schär et al. (1999).
- the explanation why the mechanism is not working in the North is to short. Please explain this more in detail. The warming is explained by sea ice retreat. I would prefer a more detailed analysis of this sea ice reduction and the feedbacks involved. How much sea ice is lost? When (summer or winter)? Where? Does this change the pressure patterns and circulation systems or is this only a local process?
  We have added a new figure (Fig. 8) to discuss and depict more precisely the mechanisms related to sea-ice cover changes. We find in summer, in the Nordic Seas a decrease of $1.52x10^{11}$ $m^2/W.m^{-2}$ of sea-ice cover when the solar forcing increases. This decrease reaches $2.74x10^{11}$ $m^2/W.m^{-2}$ in winter. In the Nordic Seas, the changes are mainly found at the sea ice edge and in the Barents Sea. The impact on the local sea-level pressure is not significant as shown in Fig. 7b. We therefore argue that the process is mainly local and related to the albedo feedback from the sea ice. We tried to improve the description of these mechanisms in the manuscript following this explanation and the additional figure.
    - The local sea-ice feedback after major volcanic eruptions (that, according to their results, leads to the transition into the little ice age) is analyzed in Miller et al. (2012, GRL) and Zhang et al. (2011, Clim Dyn). Following their finding,

your assumption, that the regression captures mainly solar signals, could become crucial for the Northern Europe response. Please comment on the possibility, that the Northern Europe signal could be influenced by volcanic eruptions, which then lead to the LIA transition.
We do not find a similar process as in Miller et al. (2012). Indeed, no rapid transition in sea ice cover or temperature in the North Atlantic is occurring in the decades following notably the large 1258 eruption. We rather find slight changes that mainly follow linearly the solar variations. Volcanic eruptions certainly have a signature, but no transition towards another stable states (this is how we interpret the Miller et al. (2012) results). We now further discuss the effect of volcanoes in the manuscript as asked also by reviewer 2.

- Why is Seneviratne et al. (2006) cited here? I do not find any reference to the claim in this paper. Northern Europe is not really discussed in this paper, as far as I can see. Why is the radiative flux availability the limiting factor here?
  Indeed, there was a mistake in the manuscript, since the correct reference should be Seneviratne and Stockli (2008). They argue at the end of page 181 that in "high latitude regions for instance, evapotranspiration is limited by the net radiation and the length of the growing season." Radiative flux availability is the limiting here because lots of water is available in the soil, while the radiative flux is mainly available a few months in the summer time. We correct the manuscript accordingly.

2.3 Conclusions

- Please discuss the amplitude of your solar reconstructions and compare it with other state of the art solar reconstructions. Is the reconstruction used in this study characterized by a comparable large amplitude? A good review on this is Gray et al, 2010 (Reviews of Geophysics).
  We have added such a discussion in the conclusion.

3 technical corrections

- page 1303, line 13: "Evapotranspiration is a actually..." – please delete 'a'
  Revised as advised.
- page 1304, line 20: reference to Fig 1a should be Fig. 1, I guess.
  Revised as advised.
- page 1305, line 14: Please replace 'figure' by 'value'
  Revised as advised.
- page 1306, line 5: '...twice larger than...' - check phrase
  We replaced "twice larger than" by "two times larger than".
- page 1306, line 12: 'largest changes', instead of 'mains changes'
  Revised as advised.
- page 1306, line 17: 'the changes in evap. is the' better '... are the...'
  Revised as advised.
- page 1307, line 22f: do you mean Swingedouw et al 2011 (not 2010)? And most likely Fig 3a should be referred, not 2a.
  Revised as advised.
- page 1308, line 2: Fig 4 should be referred here, not Fig. 4.
  We assume that the reviewer want to say Fig. 5 in plave of Fig. 4. We revised in that sense.
- page 1308, line 13: 'Such a long time scale as the...' - check phrase

We replaced “Such a long time scale as the last millennium” by “A time scale long enough such as the last millennium”

- page 1309, line 8: ’The extension ... also improves the ...’ – with trailing s
  Revised as advised.
- Fig. 1.: ’... with its own axis on the right’. There are no labels on the right axis.
  We were thinking of “the other right” i.e the left… We modified the legend accordingly.
- Fig. 2: last line: brackets around 2010 (Guiot citation)
  Revised as advised.
- Fig. 3.: ’...and negative...’ (instead of ’negatibe’). The labels on the color bar are a bit to small.
  Revised as advised.
- Fig. 4: the unit on the x-axis should be ’deg C / Wm-2’; please mention that the zonal mean temperature was regressed on the solar forcing.
  Revised as advised.
- Fig. 6: ’each line corresponds to a computation each century...’ I do not understand this sentence.
  We have replaced it by: “The different lines in panel b correspond to a computation for half a century starting from 1001-1050 (until 1801-1850).” This has been done in order to evaluate the stationarity of the relationship in the model on a 50-yr time scale as used in the reconstruction from panel b.
- better: ’solar forcing’ instead of radiative forcing. X-Axis: better Jan-Dec, than 1.0-12.0.
  We have left “radiative forcing” instead of “solar forcing” because the diagnostic use the net radiative forcing at the surface, which is the important quantity for the evapotranspiration fluxes. We added that it is the “net” radiative forcing. We change the X-Axis as suggested.
- Fig. 7: mention which part of the scheme is based on the mechanism proposed by Schär et al.
  We have highlighted in the new Fig. 7 the part of the scheme that was related to the Schär et al. (1999) positive feedback loop.
- Why is there a plus between ’surface SW’ and ’surface temperature’?? Less surface SW results in higher temperatures?
  The sign plus or minus in this figure is devoted to explain the relationship between the two variables at each side of the arrow, and only these two quantities. Thus we put a sign plus between SW and surface temperature because more surface SW induces higher temperature in the absence of any feedbacks.
  It is therefore the sign minus between convective clouds and surface SW that leads to a negative response of surface temperature to TSI after the whole chain has been covered.
  We clarify that the sign are only characterizing the two variables at each side of the arrow in the legend of the corrected manuscript.

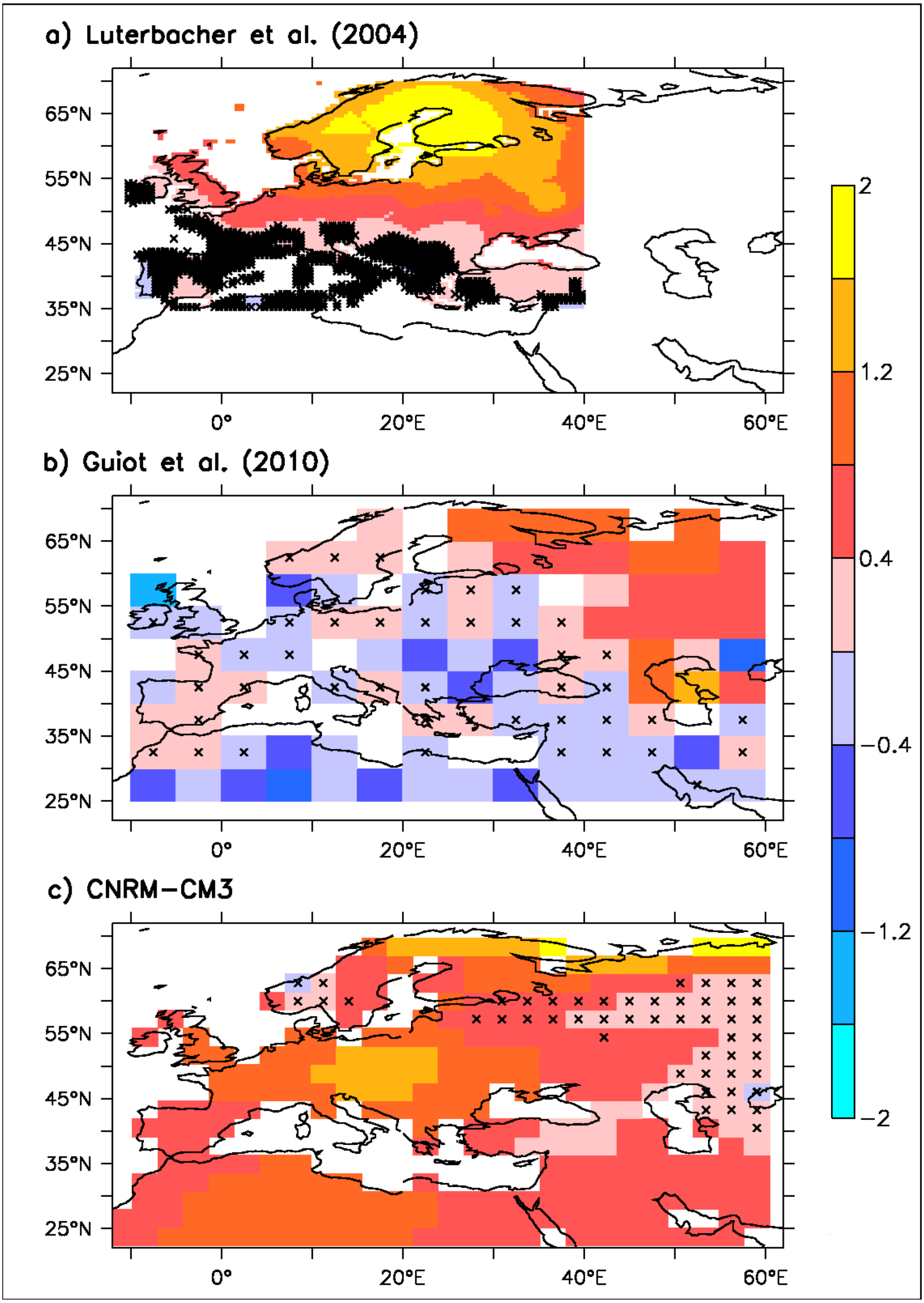

Fig. R1: Same as Fig. 2 but for a regression over the the period 1500-1850. The Luterbacher et al. reconstruction has been added in panel a.

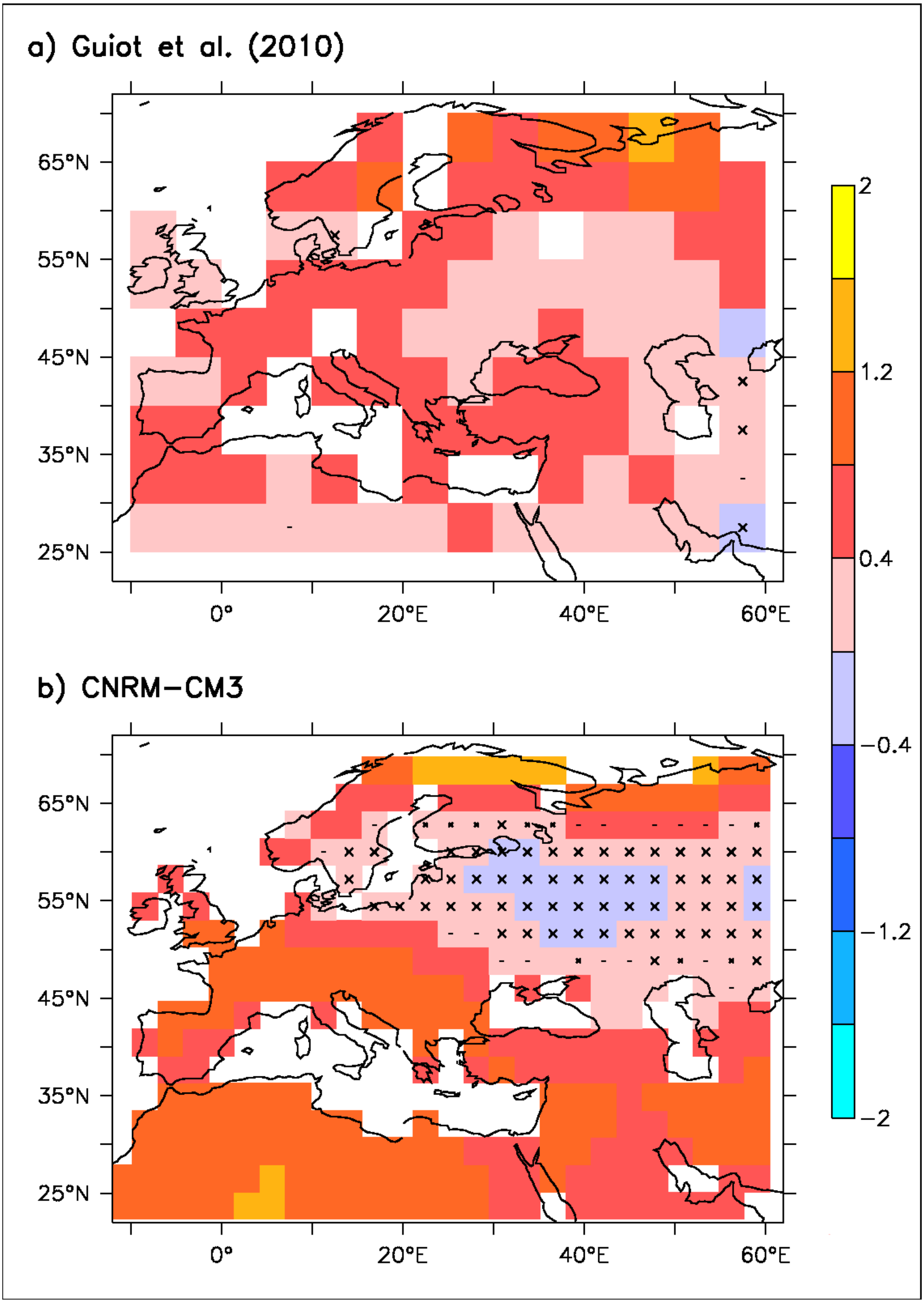

Fig R2: Same as Fig. 2 but for a regression over the period 1001-1500.

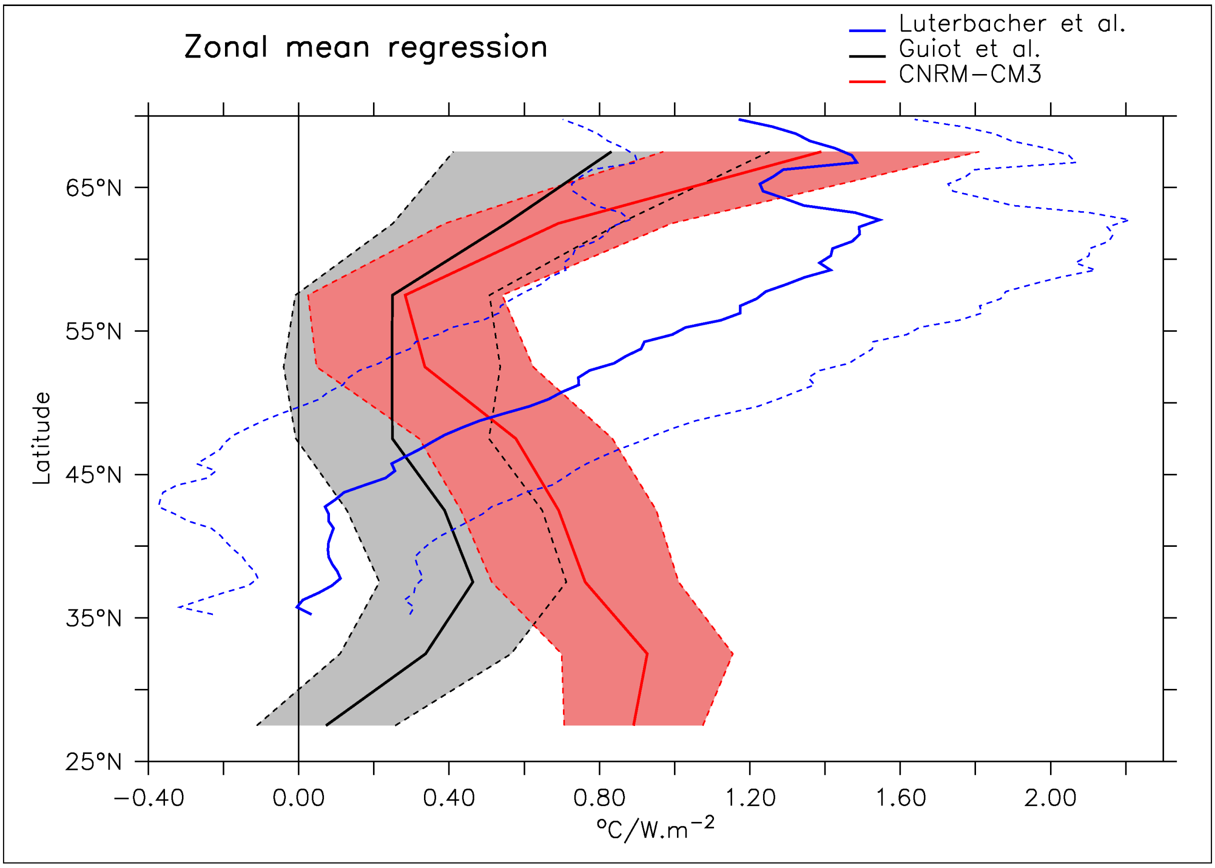

Fig. R3: Same as Fig. 4 but with the inclusion of the regression of Luterbacher et al. (2004) reconstruction over the period 1500-1850.

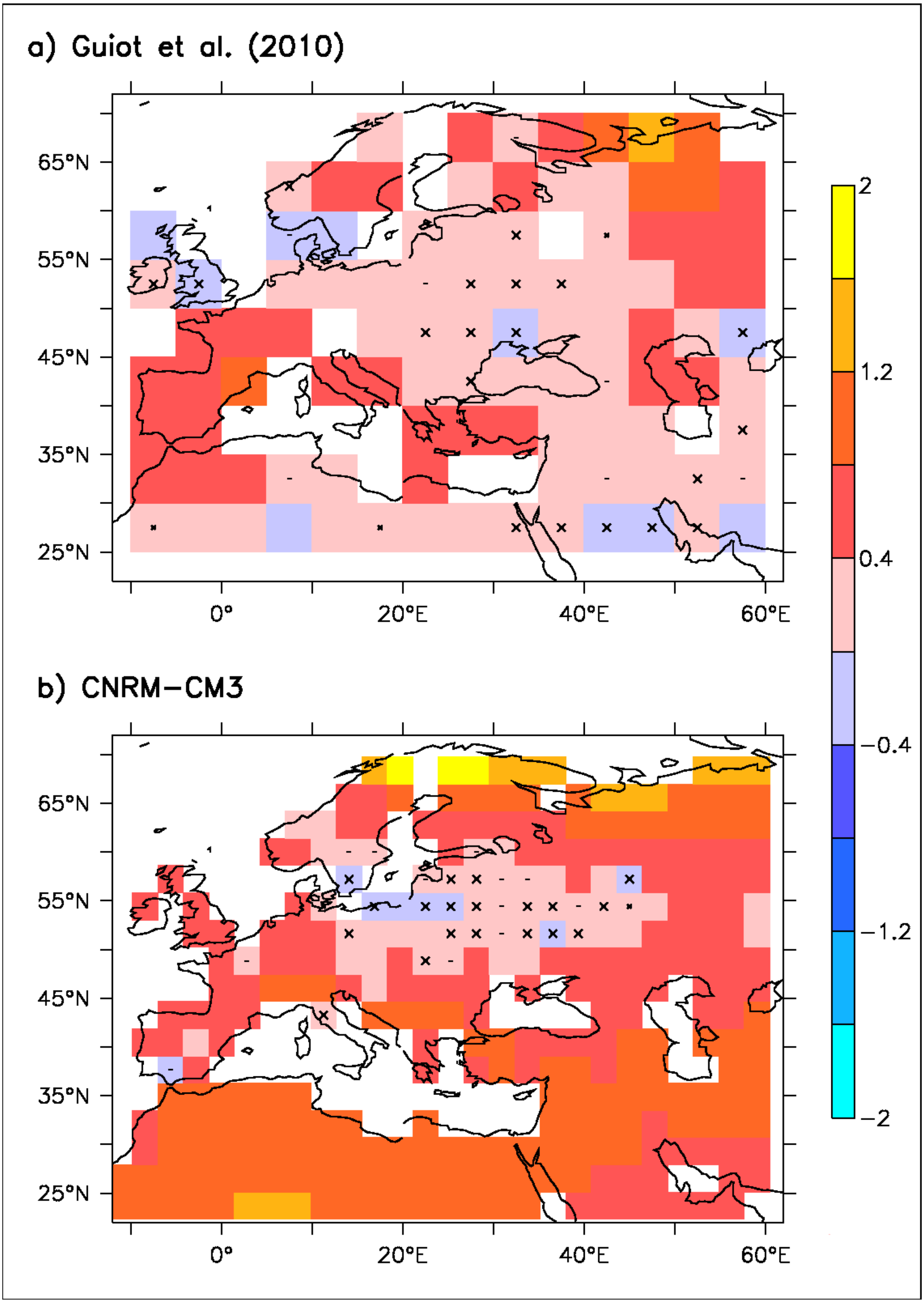

Fig. R4: Same as Fig. 2 but for a regresson that do not include the 11 years following eruptions larger than Pinatubo.

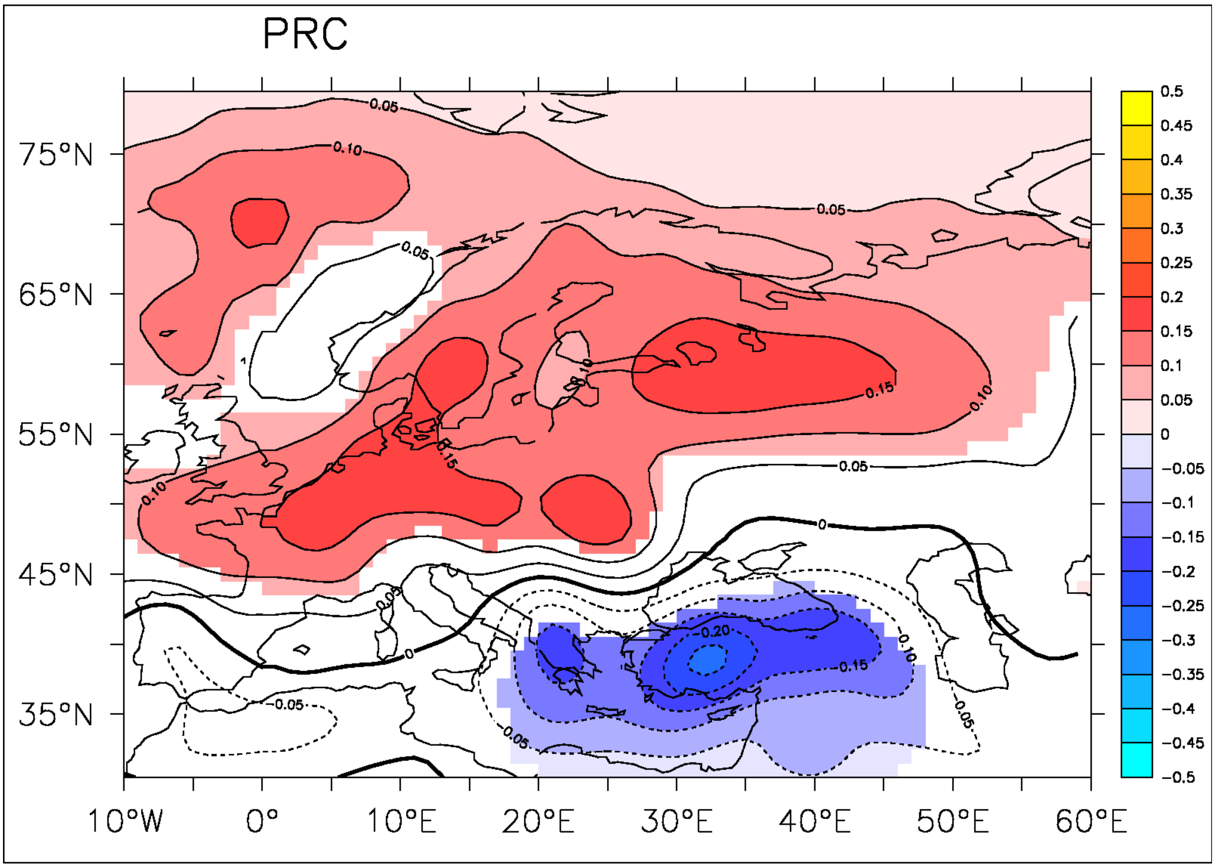

Fig. R5: Same as Fig. 3c but for the convective precipitation, which is computed in the model.

Response to reviewer 2:

Response to the general comments:

We thank the reviewer for his interesting comments. In the following our response to his comments are written in blue. We begin with a response to his general comments that he and we divided into two points:

First: When analysing the effect of solar forcing over Europe, we have looked at first to any dynamical changes in the model. We found that the circulation changes were not significant in summer over most of Europe. Thus we decide not to show any figures showing such a little effect of solar forcing on the main circulation and only discuss in the text the absence of effect. This leads to turn our analysis towards other influences, and the effect of evapotranspiration and soil-atmosphere coupling appears as crucial in a large part of Europe. Nevertheless, we believe that it is important to better insist on this fact and we now show the regression of the summer sea-level pressure over solar forcing (New Fig. 8b).

We clarify and insist more on the absence of change in the large-scale circulation over Europe in the corrected version of the paper. Moreover we discuss in more details the impact of solar forcing on climate and make a reference to Gray et al. (2010). Indeed we add other references to strenghten our arguments and discussions all along the mansucript.

Second: We agree with the reviewers that the caveat and limitations of the present study were not discussed enough. In particular, the robustness of the presented results in regards to other reconstructions is now discussed in more details. It appears that our conclusion, mainly clear the model, are far from being so clear in the real world and in particular with other reconstructions than Guiot et al. The results we obtained for Central Europe are not found in the Luterbacher et al. reconstruction for instance (cf. response to reviewer 1) or in Buntgen et al. (2011). Nevertheless, the Guiot et al. reconstruction is on a larger scale and for a longer period than Luterbacher et al. (2004). The Guiot et al reconstruction also includes pollen data, which is not the case for the Luterbacher reconstruction. We argue that those two points may explain the large differences. We included a discussion of this in the mansucript.

We did not claim that in Central Europe Warm period were wet, but we rather show that in this region, it should be very difficult to find any solar signal in Summer, because it is largely damped and cancelled by a strong negative feedback, but the signal remains weak.

Concerning what is happening in the Mediterranean region, we did not find any substantial increase in subsidence (contrary to what is happening in scenario with larger radiative forcing) and therefore attributes most of the signal found in the model to the soil-atmosphere interaction that could be seen as an amplifier when the soil mositure reservoire are smaller (cf. Seneviratne et al. 2006). The response of the large scale circulation is now shown through SLP regression in a new figure (Fig. 7b)

Lastly we clarify the link with sea-ice cover for Northern Europe thanks to a new Figure (Fig. 7a).

More details:

- page 1302, line 21-23: This last sentence of the abstract is well intended, but somewhat loosely connected. Correlations are low, the magnitude of the signals are different between model and reconstruction, and the reasons for the difference are not

well explored. There are different issues at play.
We have deleted this sentence, which was maybe too optimistic.

- p. 1302, l 25, opening of introduction: IPCC is not a good source here because IPCC is a summary of the literature. Therefore cite the real literature, or maybe even more importantly state what is physical reality: a forcing doesn't generate the same signal everywhere because of regionally and locally different feedback processes. And then make the reference to summaries within IPCC, for example.
  We have rewritten the first sentence of the introduction in order to better describe the physical processes at play in the projections over Southern Europe. We now cite the real literature, but we keep our first citation of IPCC, since we believe it is important to go from the general, from what is clearly known (summarized by the IPCC) and then to depict a few examples from the literature.
- p. 1303, line 2ff: is the Mediterranean warming only due to feedbacks or is increased subsidence (with a northward shift of the storm track) the actual signal? The reader is led to believe its all local feedbacks. I don't think this is true. Check and better reference this claim that no fundamental circulation change is a work here. I think the authors will quickly realize that since a couple IPCC reports this region has been one of the few significant change areas identified and it has to do with circulation, not only local feedbacks.
  We now discuss the effect of large-scale changes for the Mediterranean warming in the introduction.
- p. 1303, l. 12: parameterization (spelling)
  Revised as advised.
- p. 1303, l. 26: the reason for better detection in winter is that dynamics are involved rather than pure local radiative forcing
  We added this precision.
- p. 1304, l. 9-10: I would like to hear more than model reproduces Northern Hemisphere temperature. I'd like to hear, without going to the paper, that the multidecadal variations that are often associated with forcings are reproduced, how they fall within the range of reconstructions (at the hemispheric level), etc.
  We have added a few more details on the previous results from this simulation in the manuscript.
- p. 1304, l. 25: in a comparison where particular episodes are important, I'd like to hear that solar minima are independent of co-existing volcanic activity. Similarly, high solar activity should be compared with volcanic activity. An overall correlation is maybe quite misleading when we are mostly interested in specific periods timing (see various papers in literature: Hegerl et al, Ammann et al.,).
  We are now more cautious concerning the co-variance between solar and volcanic forcing. We add references to the literature (Hegerl et al. 2007, Amman et al. 2007) to better depict the issues related to this point. Moreover as suggested by reviewer 1, we test the effect of deleting 11 years following the large volcanic eruptions (larger than Pinatubo) for the regression computation (Fig. R4). We find a small effect and the main pattern remains similar, so that we believe that most of the signal of our regression is coming from solar forcing.
- p. 1305, l. 8ff: Looking at Figure 1, I'm not sure that the fit of the red-line and the black line (not solar) is actually that good. The flat period in black up to about 1420 is not well represented, the coolest episode around 1600 is not captured, the sustained rel. warm interval 1680-1890 is not picked but sees more of a trend. The model has certainly a much better correlation with the solar forcing than the real world. Its worth pointing this out.

We point this fact more clearly. Moreover, we added on this figure the reconstruction from Luterbacher et al. (2004) over a large part of Europe. This gives an idea concerning the large uncertainty that exists in the reconstruction. For instance the warm interval around 1680-1890 in the Guiot et al. reconstruction begins later (around 1720), more in agreement with our simulation, although this one missed the amplitude of the warming occurring after 1720. We depict this new curve in the manuscript as well as the uncertainties and misrepresentation in the simulation.

- p. 1305, l. 13: "respected" is not a good term.
  We replaced it by "simulated by the model"
  p. 1305, l. 14: see above: I'm not very impressed by the correlations. Why is it good that the early part has a higher correlation? Aren't these simply artifacts from low frequency variations with very few degrees of freedom?
  We tried to better insist on the weakness of the correlation in the corrected paper. We were insisting on the 1100-1500 period because it was not considered in the Hegerl et al. (2011) paper, which finds very few effects of solar forcing in the Luterbacher et al. (2004) reconstruction. We add a word on that in the manuscript.
- Figure 3a and p. 1305, l. 20ff: the color scheme is not well chosen. Use just the range that is present in the figure. Right now I would jump to the probably wrong conclusion that the model is extremely low resolution and thus the signal described might simply be due to the coarseness of the grid, which I'm sure its not. Figure 3a can be done much better to show what is actually happening in terms of radiative forcing.
  We have modified the palette as well as the range for the radiative forcing. The former choice was done to show that the variations over Europe are very small, which was indeed already stated in the text.
- p. 1306, l. 7-8: given that radiative forcing is even, there is still no a priori expectation that the response is as well! Local radiative response and dynamics are different things and they express themselves in different ways spatially.
  We agree that for a climate scientist knowing the IPCC literature is not surprising. Nevertheless, we believe it is important to insist on the fact that this response is not spatially linear and involves feedbacks. We have tried to improve this sentence, not to appear too much naïve…
- p. 1306, l. 11: is the cloud increase related to the local increase in solar radiation, or could it be a dynamical response that is large scale? I'd like to see something about the large scale circulation indices: see next comment:
  Change in circulation was one of the first things that we test when we began the comparison of the Guiot et al. reconstruction with our simulation. Contrary to what we find for winter delayed response (Swingedouw et al. 2011); we did not find any significant responses here. An illustration of this matter of fact can be found in the new Fig. 8, showing that the regression of SLP changes is hardly significant in most of the Northern hemisphere (and Southern Hemisphere but we only show the North for convenience since the focus is put over Europe). We therefore assume that the changes in summer are too small to lead to any significant changes in the large-scale circulation. We also test the weather regimes for summer (as shown for winter in response to the next questions) and not find any significant cross correlation between the indices of the four regimes over the North Atlantic and the solar forcing. Concerning the winter changes in NAO, we discuss it in Swingedouw et al. (2011) and od not find any significant signal in phase with solar forcing (but rather delayed).
- p. 1307, l. 18: how was the "weather regime" issue tested?
  These regimes over the North Atlantic are shown in Fig. R6 for winter and their

occurrences are shown in Fig. R7. We have tested cross correlation of these different indices with the solar forcing but do not find any significant responses.

I'm not talking about individual events, but circulation indices. And here is the issue raised above that links the discussion to the historical record. Was it really wetter during warm periods in central Europe? Because only then large scale warming from the sun could be buffered by increased moisture. So are we just looking at Clausius Clapeyron? Is the signal in the model (and reality) much stronger of a negative feedback early in the season than late season, which one would expect given late winter soil moisture?

We plot the temperature response in early (AMJ) and late (JAS) summer (Fig. R8) and find a larger cooling effect in late than in early summer. This behavior shows that if the better winter loading of soil moisture induces an effect in early summer, the positive feedback loop described in Fig. 7, which leads to an increase in summer precipitation not only from local recycling, but also from large scale precipitation as proposed in Schär et al. (1999), is playing a role. Since this is a positive feedback loop, the effect increases with the season so that we find a larger impact in late than early season.

- p. 1308, l. 1: how far away is the sea ice from Scandinavia and the other areas of largest warming? What indications exist that this played a role in real world?

  We add a figure to show the changes in sea ice cover. The sea ice changes are indeed very close to Scandinavia with large changes in the Barents Sea for instance. The sea ice cover is larger in this sea during the last millennium as compared to the last 30 years in the model, because of the anthropogenic forcing of the last century has already slightly changed the mean state in the model.

  On the data side, a reconstruction of sea ice using the IP25 proxy north of Iceland exists over the last millennium (Massé et al. 2008). It shows a large increase in sea ice during the little ice age, and notably a strong maximum around 1690, which is very close to the Maunder solar minimum (without very large eruptions at that moment).
- p. 1308, l. 12: reconstructions can be useful to evaluate low frequency variability. Well, given Figure 1, I'm not so sure that this has been established. I certainly agree with the statement, but the authors could do a better job to show this.

  We hope the improvements we have added, following reviewers' suggestion put more weigh to the proposed sentence. We therefore let it as it was in the corrected manuscript.
- p. 1308, l. 16: what real world data exists for summer at 75 N?

  Indeed, there is no real data there, but the reconstruction covers this place using extrapolation (cf. Guiot et al. (2010). Nevertheless, given the Fig. 4, it is more correct to say around 65°N, where there are more data. We corrected the manuscript accordingly.
- p. 1309, l. 6: could it be that the signal over Europe is very mixed in the real world because the actual causality is weak and we are more looking at a combined volcanic solar effect?

  Given the results we have shown for other regions where the signal is slightly larger, and given the results from our model, we believe that our interpretation for Central Europe is an interesting hypothesis to explain the data. Nevertheless, we agree that more work is required before to clearly prove that the proposed mechanism found in the simulation was at work in the real world. This mechanism has been observed in the recent years for inter-annual variability, we believe its impact on decadal scale could be hidden in the last millennium dataset.
- p. 1309, l. 13-16: yes, I agree with this statement.

Thank's.

- p 1310, l. 12: Mennendez typo
  Revised as advised, thank's.
- p. 1311, l. 1: check last author of Nature Geosci reference. It's not Lingling, it should probably be: "Suo, L."
  Revised as advised, thank's.
- p. 1311, l. 12, remove "vol"
  Revised as advised.

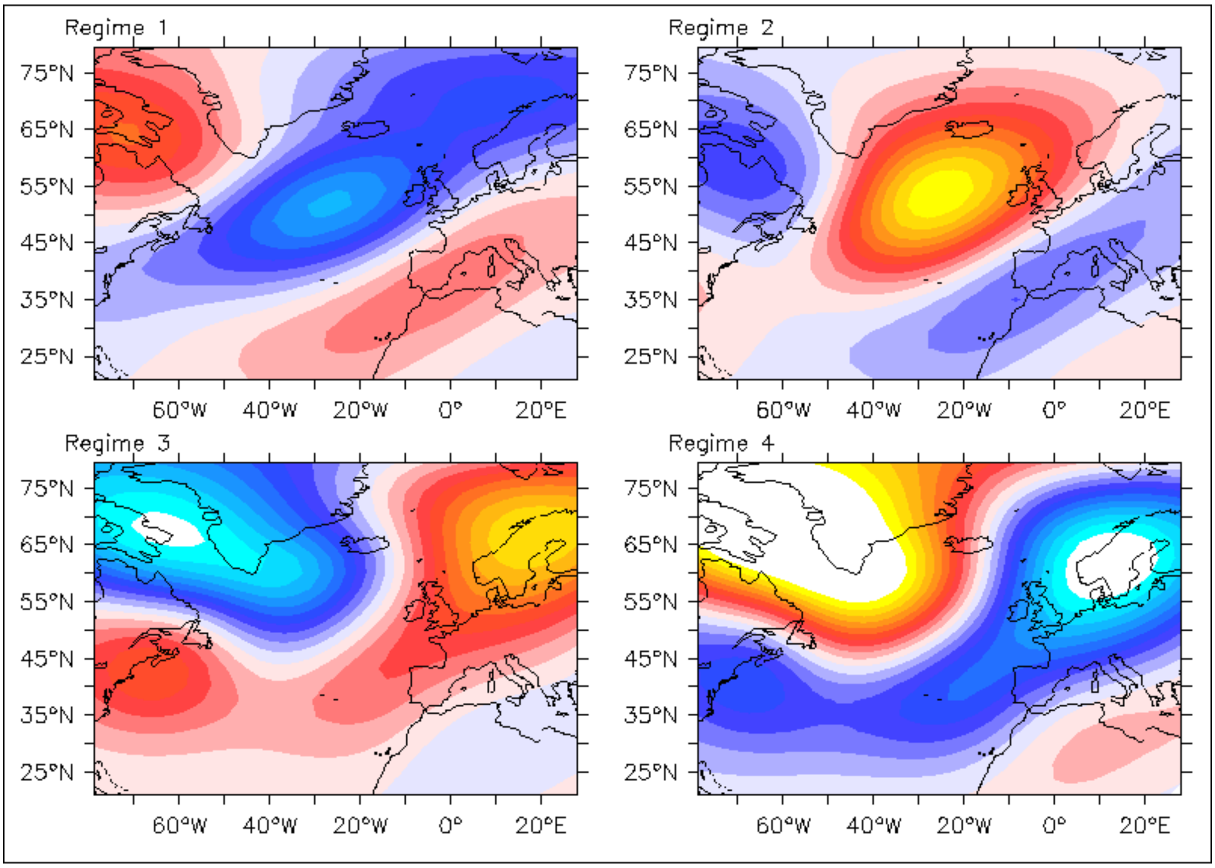

Fig R6: Map of the different regimes found after clustering using daily 500 hPa data over the winter (ONDJFM) period going from 1001 to 1850 following Cassou (2008) methodology.

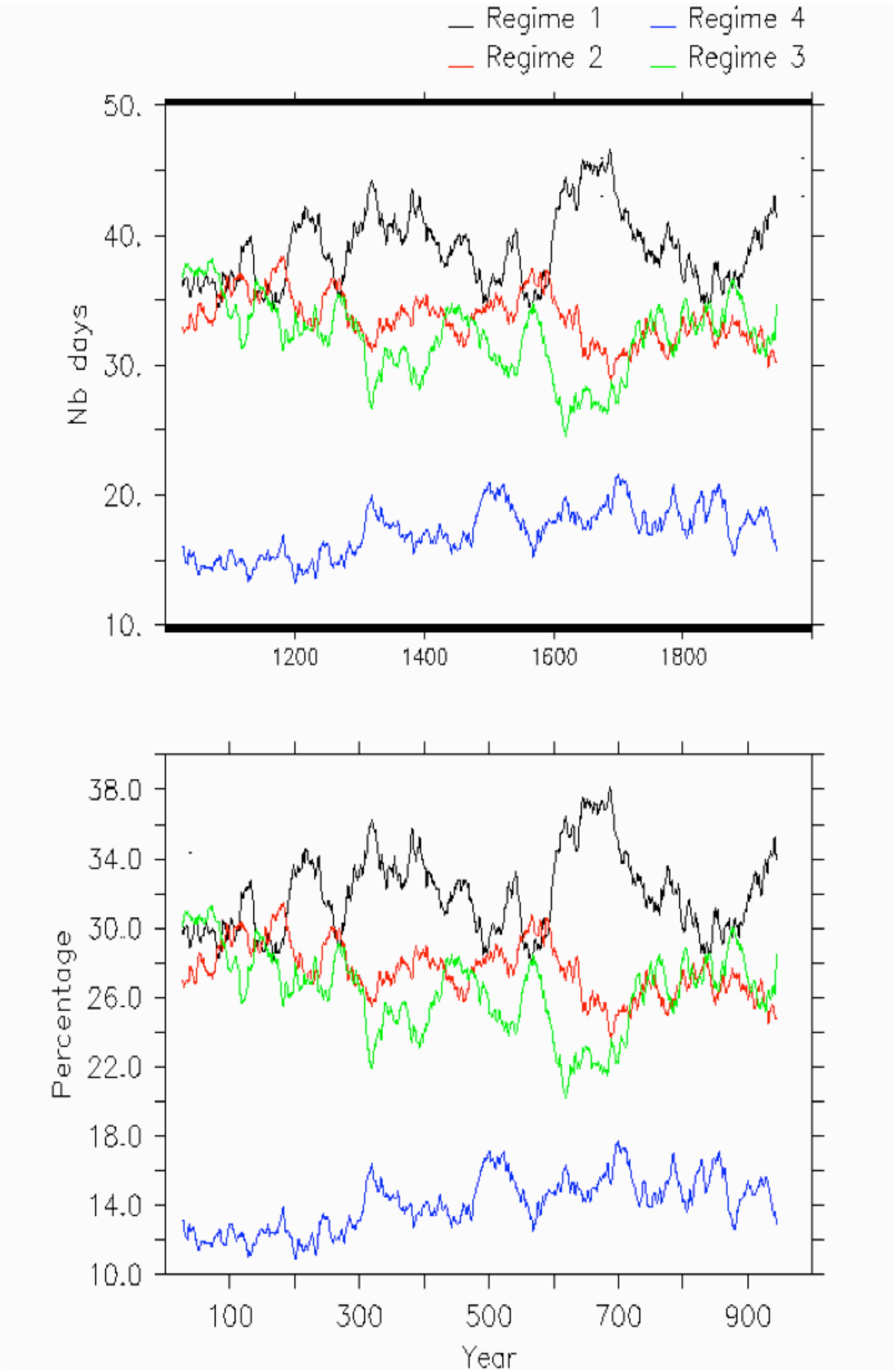

Fig. R7: Yearly occurrence of the different regimes over the winter time in number of days for each winter in the top and as a percentage of the total day in the bottom. A 13-yr running mean has been applied to all the time series.

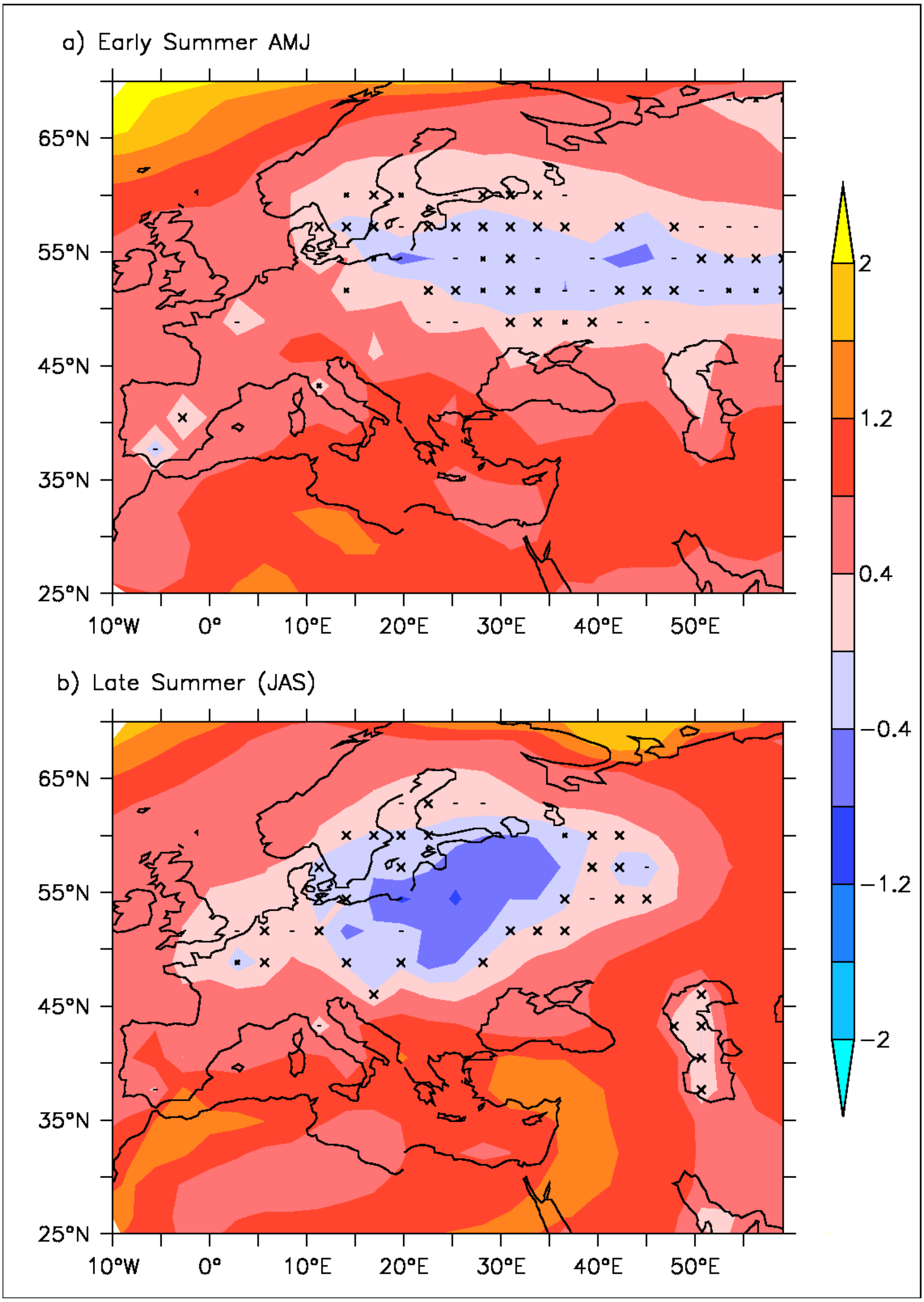

Fig. R8 : Regression of temperature over the solar forcing for the period 1001-1850 of the simulation **a** for early summer (April-May-June) and **b** for the late summer (July-August-September). The large black crosses indicate the point not significant at the 90%, the smaller one, the region not significant at the 95% and the horizontal line at the 99% level.

Supplementary bibliography:

Cassou, C., 2008: Intraseasonal interaction between the Madden-Julian Oscillation and the North Atlantic Oscillation. *Nature*, **455**, doi:10.1038/nature07286, 523-527.