Response to Anonymous referee #1

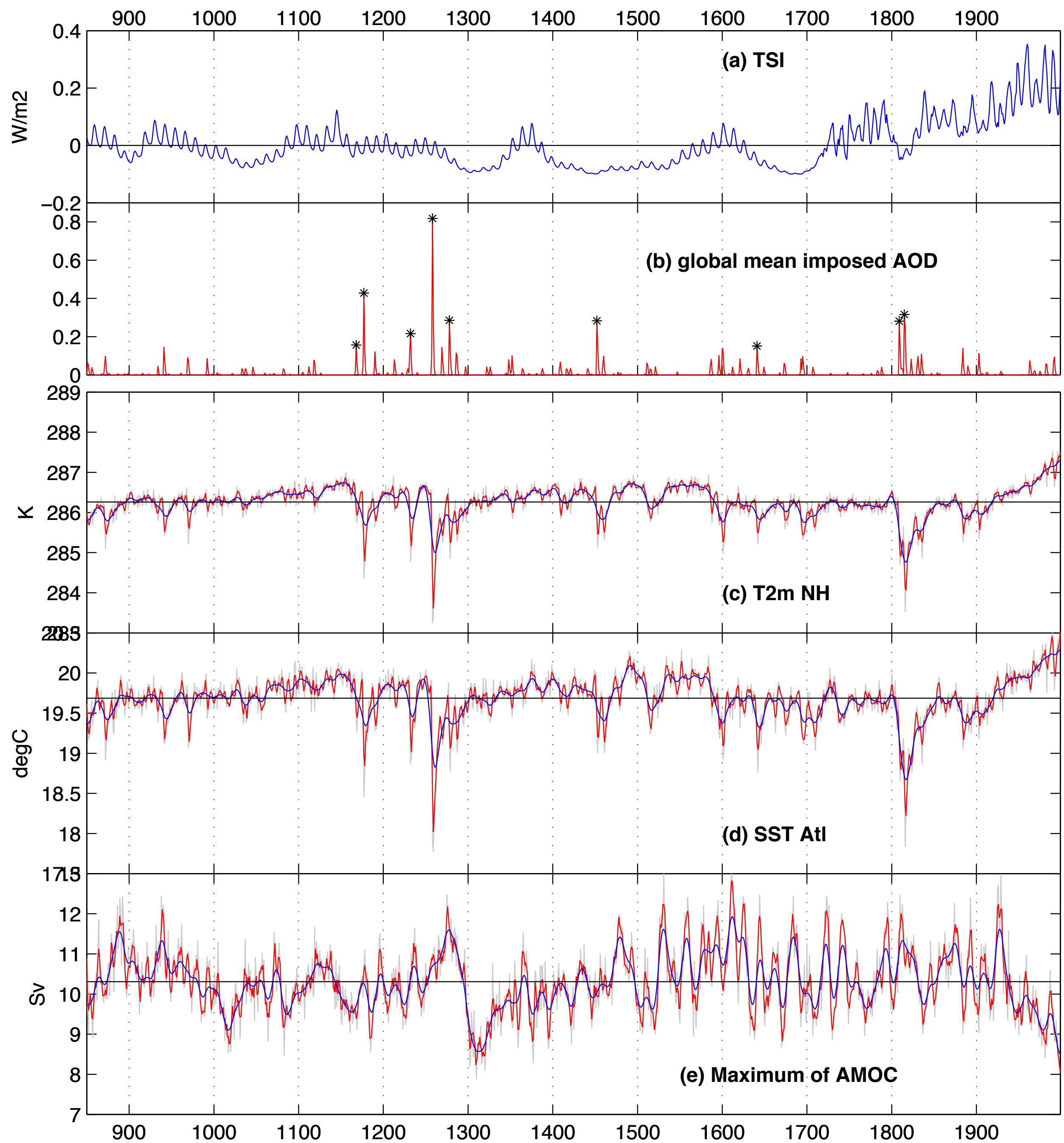
The manuscript presents an analysis of a long paleoclimate simulation over the past millennium with a general circulation climate model. The analysis is focused on the response of the ocean to imposed volcanic eruptions, with especial emphasis on the North Atlantic and the Meridional Overturning circulation (MOC). The response of the ocean is characterized by general cooling after volcanic eruptions that penetrates into the deeper ocean in the Tropics and at high-latitudes in the regions of convection. This general pattern can be modulated by changes in the MOC, which also induce temperature anomalies. One of the most important conclusions of the study is that the response of volcanic eruptions before and after about 1300 display different behavior. The atmospheric response (winds and fresh water fluxes) seems to be different and this has consequences on sea-ice cover and the MOC itself. It turns out that the MOC response to later volcanic eruptions is weaker but more persistence in time. This could explain why previous modeling studies had found contradicting MOC responses.

In general terms I found the manuscript clearly written. I also think that it contains interesting information about a topic that has not been investigated in a very detailed manner so far. The particular details of the study seem to me to be correct, but I have some general comments on the manuscript.

My first comment is related to the paleoclimate simulation in general. The authors state that they will focus on the ocean response and do not comment on general aspects of the simulation, such the general evolution of the simulated global temperatures. However, the simulated Northern Hemisphere temperature, displayed in Fig 1 c does not really resemble any of the paleo reconstructions published so far. The temperatures at the end of the 20th century are not remarkably high in the context of the millennium. One can see many centennial periods in which temperatures were higher than in the 20th century, even than in the last decades of the 20th century. This is at odds with all available information from proxy data.

Fig. 1 does not show temperatures of the 20th century. As indicated in the manuscript (end of section 2.2), the period of investigation is deliberately limited to the pre-industrial era, namely 850 to 1849 A.D., in this study. Indeed we want to focus on the role of external natural forcing and avoid the debate on the industrial period. Furthermore, in this simulation, tropospheric aerosol concentrations are kept constant throughout the whole simulation, including the industrial period. Thus this simulation should not be used for this period.

Nevertheless, we reproduce here the same figure as Fig. 1 in the submitted draft, with time series extending to 1999 A.D. This figure confirms that the model does reproduce the strong (and probably over-estimated because of the lack of tropospheric aerosol concentration variations) warming at the end of the 20th century, consistent with reconstructions, observations and proxy data.



Also, there is no discernible Little Ice Age, and in general the centennial variability seems to be very small. The authors indicate that they have performed the simulation using the solar forcing provided by Krivova and Solanki, the one that will also be used as base line for the CMPI5 simulations. This solar forcing displays smaller amplitude than previous reconstructions, e.g. Lean, and I wonder whether this can be the reason for the small simulated variability. Temperature reconstructions are still burdened with uncertainty but I think that the authors should at least add some comment on this discrepancy between the simulation and most, if not all, reconstructions. It seems to me, and the authors may confirm or rebut this, that the combination of the model used together with the Krivova and Solanki solar forcing is not compatible with the known proxy-based reconstructions.

We agree with the reviewer that centennial variability is relatively weak in this simulation, and that some centennial features such temperature shifts between the medieval climate anomaly and the little ice age are not simulated in the model, unlike several reconstructions. Whether this is due to a model deficiency or the weak TSI variations is difficult to assess within the present study. We admit nevertheless that under TSI reconstructions with larger variations (Crowley et al. 2000), the same model led to much larger SST low frequency variations, in relative agreement with paleoreconstructions, except for the onset of the MCA (Servonnat et al., CP, 2010). We agree to lengthen this discussion in the revised manuscript.

Note however that as stated below, this aspect does not invalidate our study on the oceanic response to the volcanic forcing, and it was in fact one of our motivations to focus on the volcanic response.

.I would like to remark that a new reconstruction of solar forcing during the Holocene (Shapiro et al, 2011 doi: 10.1051/0004-6361/201016173) indicates much larger variations.

We will add this reference in the presentation of the solar forcing (section 2.2).

This does not necessarily invalidate the study, because it is admittedly more narrowly focused on the response to volcanic forcing, but it rather places the manuscript in the frame of a sensitivity study. As far as the response to sudden volcanic eruptions does not depend too strongly on the level of mean temperature, the conclusions may be considered valid, but there is a caveat.

We agree on this point, and we propose to make it more clear at the beginning of the paper (end of section 3) and in the conclusion.

Another general concern is the conclusion that the volcanic eruptions pre and post 1400 may have a different character. The authors indicate that maybe the seasonality of the eruptions may be a relevant fact here, but there is often no information about the exact season when the eruption occurred. The study but Gao et al, which the authors follow here, states that in those cases where the season cannot be derived from historical sources, it is assumed to have occurred in April. This is also the case for quite recent eruptions, e.g 1809. Since the number of eruptions analogized here is not that large, I also wonder to what extent the different seasonality of the model eruptions is just an artifact of the uncertainty in the seasonality. I guess that in the simulation pre 1400 eruptions would be more often prescribed to occur in April. Again, this would not invalidate the study in as much the authors would be analyzing the difference in the response of 'model eruptions' with a prescribed seasonality, but the conclusions would not necessarily apply to the differences between pre- and post 1400 eruptions in the real world.

The role of the seasonality of the eruptions was proposed only as a suggestion, and we do not have strong arguments to prefer it to the other hypothesis, namely the magnitude effect or the cumulative effect. Information given by the reviewer indeed puts this hypothesis into question.

Related to this is how statistically significant are the difference in the responses be- tween pre and post 1400 eruptions. Maybe I did not understand properly, but I interpret that the authors tested the significance of the volcanic response with respect t to the years previous to the eruptions and not between these two sets of eruptions. As I wrote, the number of eruptions is limited, and I wonder to what extent the simulated differences may be due to the small sample size.

Indeed, we do not test the significance of the volcanic response between the two sets of eruptions. But we do not either test it only relative to the years previous to the eruptions. Given the potentially long influence of the volcanoes on the ocean, such procedure would probably not be sufficient. Our protocol is based on a Monte Carlo procedure: consider for example the response of the SST 1 year after the volcanic eruption. If the SST time series is reshuffled prior to computing the composite, in other words the numbering of years is randomly permuted, there is no reason, except by chance, to find again an anomalous cooling. If this re-shuffling I done a larger amount of times (here 500 times), we can compute statistically the confidence that we have that the "original" signal was obtained by chance, or really differs from those 500 "random signals". This will be clarified in the revised version of the manuscript. Such test is rather classical in climate studies, and the results presented here are robust even when the number of permutations is increased to 1000.

The question of the small sample size of eruptions is relatively separated from the question of the significance test, but it is nevertheless a real question. As indicated in the text, major results are robust when the eruption threshold is taken down to 0.1. Nevertheless, the amplitude of the response in these cases is more marginal as compared to the background variability. For this reason, we preferred focusing on "large eruptions", as indicated in the title. We agree again that this is certainly a limitation of our study, and as indicated in the conclusion section, more sensitivity experiments are required to be more conclusive.

By looking at Figure 1e displaying the evolution of the MOC in the past millennium, it seems that the variability has a different character pre and post 1300, with more high- frequency variability in the later period, especially after 1500 What could be the reason for this ? Could this also have an influence in the perceived different response to pre and post 1400 eruptions ?

The change of AMOC variability pre and post 1400 was also one motivation for the study. Given this curve, one hypothesis could have been that the succession of eruptions between 1150 and 1300 modifies strongly the AMOC on the short term (see the strong increase and decrease following these events) and on the longer term (change of variability). However, nothing in our results allowed confirming such hypothesis…

On the other side, could this different variability be the reason for the different response? The different response of the SLP and AMOC to eruptions pre and post 1400 occurs as soon as the year of the eruption. For this reason, we do not believe that interannual variability could be the cause.

Some minor comments:

1 'culated using smoothing in the time and space domain (Grinsted et al., 2004), with the 5 % signiïn˘A ̧cance level determined from a Monte-Carlo simulation of 1000 sets of surro- gate time series. The two temperature time series show episodic coherency with the'

Could the author provide a little more detail about how the Monte Carlo testing has been conducted ?

This has been addressed above, in response to one of the reviewer's major comment.

2 'However, these processes require a much higher resolution in the stratosphere to be properly represented. In fact, episodic coherency between SST and TSI variations at 11 years timescale is also signiïňˇA ̨cant from the control data, suggesting that the signal'

I think this result indicates that there is no significant TSI signal in the simulated SST and that the test for coherence in the forced run is thus too liberal.

We agree with this conclusion, this was the point of adding the remark on the control run, for cross-validation. This conclusion will be clarified in the revised version.

3 The reconstruction of solar forcing displays in Fig 1a contains a 11-year cycle that is artificial. It is just an extrapolation back in time of the 11-cycle observed in sunspots after year 1600, but as far as I know there are no observations, direct or proxy-based, of this cycle pre 1600. To my knowledge, the proxies for TSI, either Be10 in ice cores or C14 in tree rings do not have enough resolution to resolve this cycle. The wavelet spectrum in this frequency band is therefore also an artifact.

We are grateful to the reviewer for this point that is also linked to the reviewer's general comment on the realism of the solar forcing. We will include this point in the revised version. Thank you.