Response to reviewer 1

We thank reviewer 1 very much for her/his helpful comments. We took her/his remarks into account and improved the manuscript accordingly.

Major comments:

1. *The differences between the five ensemble members in Figure 1 are striking. There seems to be very little consistency among them. Is there more consistency for global or hemispheric temperature? My understanding is that all five have the same forcing and different initial conditions. More information should be given about the forcing, including references for the datasets used for solar irradiance, volcanoes, etc. Also, "The atmospheric CO2 concentration is resolved interactively in the model." Does this mean that CO2 forcing does or does not include anthropogenic effects? I realize that some of this information might be available in the Jungclaus reference, but this information is important enough to the interpretation that it should be provided.*

Indeed, a large spread can be found among the five ensemble members for the AIMR in summer, which can be attributed to strong internal model variability. All members have the same external forcing but different initial conditions of the ocean, which can develop its own long-term internal variability with a response to atmospheric processes like precipitation. The 2m-air temperature changes averaged over the AIMR region show a slightly better consistency among the different members, in which the prominent temperature epochs of MWP and LIA have a larger spread than the transition periods (see figure f01s in supplementary). Jungclaus et al., 2010 analyses the simulated NH temperature evolution for all five ensemble members during the past 1000 years and compared it with reconstructed temperatures (see Figure 3a, b; Jungclaus et al., 2010). All members indicate also a significant fluctuation on different time scales due to internal variability. Further the E1 ensemble (which we used in our analysis) includes a weaker solar forcing leading to less dominant temperature anomalies. This has to be considered in our interpretation of the physical mechanisms driving the rainfall anomalies in summer – especially with respect to the strong impact of TSI. We additionally refer to these results in the description of the model forcing.

For a comprehensive understanding of the five ensemble members from the long-term Millennium simulation, we extended the paragraph and some references about the applied set of external forcing based on Jungclaus et al., 2010 (see below). The new information is highlighted as red font in our manuscript (page 5, lines 28-31 and page 6, lines 1-22). In this regard, the atmospheric CO_2 forcing also includes anthropogenic effects: "Fossil fuel emissions for the historical period are prescribed after Marland et al. (2003)".

"A set of standard full forcing has been considered in running the model for all ensemble members: a) Solar forcing: Variations in the Total Solar Irradiance (TSI) have been used with a standard increase of 0.1% from the Maunder Minimum (1647-1715 AD) to today sampled by Krivova et al., 2007. Further the 11-yr activity cycle based on reconstructed sunspot numbers is included. The TSI time series from 800 AD to the Maunder Minimum are reconstructed on the basis of cosmogenic isotope ^{14}C concentrations in tree rings (Solanki et al., 2004). In this study we used the mil0014 member of the E1 ensemble simulations with a weaker solar variation compared to ensemble E2, which has to be considered in the analysis of TSI fluctuations. A solar constant value of 1367 Wm^{-2} is applied (Jungclaus et al., 2010); b) Volcanic forcing: The volcanic effect on the radiation is estimated interactive in the model by using aerosol optical depth (AOD) time series at 0.55 µm and of the effective radius (Crowley et al., 2008); c) Land cover changes: A reconstruction of global agricultural regions and land cover is used to describe the anthropogenic land cover changes (Pongratz et al., 2008) by merging published agriculture maps from 1700 to 1992 AD and a population based approach from 800 to 1700 AD. The maps have a spatial resolution of 0.5° and include 14 vegetation types; d) Orbital forcing: Periodic variations of the Earth's orbit around the sun including short-term fluctuations are represented in ECHAM5 by applying the Variation Seculaires des Orbites Planetaires (VSOP) analytical solution (Bretagnon and Francou (1988) with a determination of the orbit from – 4000 to 8000 years with respect to the year 2000; e) Greenhouse gas forcing: The CO_2 concentration is computed interactively within the model. Fossil fuel emissions for the historical period are prescribed after Marland et al. (2003) and the present-day ozone climatology is used from Fortuin and Kelder (1998); f) Aerosol forcing: The climatological background aerosol distribution is based on time independent spatial distributions of tropospheric and stratospheric background aerosols (Tanre et al., 1984) with a maximum AOD of 0.55µm. The data have been interpolated linearly between 1750 and 1850."

2. *p. 710 line 24, What is the reference period used for the proxies? For consistency with model anomaly plots, it should be 1800-2000 AD. These proxies are said to reflect "moisture." Does this mean some might contain a precipitation-evaporation signal rather than just precipitation? If so, it could be more fruitful to compare them with modeled P-E.*

The reference period of the proxies corresponds to the model period from 1800-2000 AD.
The proxies reflect a moisture signal. Therefore, we followed your suggestion to plot the simulated normalized annual P-E anomalies between the three different time slices instead of just precipitation to better compare it with the reconstructed moisture signal. The result is shown in the new Figure 6 with the statistical significance at 95% confidence level based on a two-tailed T-test (see also your comment 3.). In addition we revised the paragraph about the model-proxy-intercomparison.

3. *Statistical significance for differences: Please show for temperature and precipitation (possibly by adding stippling) in Figures 5, 6, 7. p. 717 lines 10-17 this could be presented in a table with statistical significance also shown.*

Done. We calculated the statistical significances at the 95% confidence level for Figure 5, 6 and 7 (gray dots) using a two-tailed T-test assuming a nearly Gaussian distribution for all variables. Since the SLP and vertical velocity anomalies at 500 hPa in these figures haven't been that significant for the further analysis of the mechanisms leading to spatial rainfall changes between the three time slices, we don't show them in the revised version – also because of clearness in the figures.
In addition we recalculated the temporal correlation coefficients and their statistical significances at the 99% confidence level between the AIMR in summer and different annual based climate indices for the 200-yr long climatology, and here we summarize and present them in the new Table 2. Some climate indices, which had been used for the discussion paper, are neglected in the revised manuscript since they show no

importance for the following interpretation. Especially the PDO-ENSO-AIMR relationship will be discussed and highlighted in another manuscript later, which is already in preparation. Therefore, we also deleted some paragraphs about the introduction of the climate indices.

4. *I am confused regarding which results come from COSMO-CLM. The 30-yr time slices are described in the text and figure captions as coming from ECHAM, but this doesn't agree with Figure 1.*

All results, which are shown in this manuscript, focus only on the three time slice simulations of ECHAM5 model in T63L31. Figure 1 describes the general three-step methodology of climate model simulations used in this project. Since the results of ECHAM5 time slice experiments (B) are further used to simulate them with the higher resolved regional climate model COSMO-CLM (C), we additionally want to present this in the main methodology to get a complete overview. COSMO-CLM results are not shown in this manuscript, but will be highlighted in a separate manuscript soon. The selection of the 30-yr time slices of strong and weak monsoon activity are based on the results of ECHAM5 T63L31. We analyzed 200-yr long time slices of ECHAM5 and identified the 30-yr long periods of extreme monsoonal rainfall to further a) analyze them according the dry monsoon case study as shown in this manuscript and b) to downscale ECHAM5 model with COSMO-CLM to emphasize on a regionalization study, which is beyond the scope of this study. We clarified the misunderstanding within the description of the climate model simulations and pointed out, that the COSMO-CLM simulation results will not be shown in this manuscript (2.1) (page 5, lines 4-5).

5. *p. 720 and Figure 8: My understanding is that these correlations were calculated using the 30-year composites for wet and dry years. These composites are very short, though, and for the question you are trying to address – how does TSI (or internal mode) correlate with AIMR – it would probably be better to do a temporal correlation using all 200 years (or even all 1000 years).*

Thanks a lot for your helpful comment. Since the 30-yr periods of the composites seem too short for a statistical significant correlation analysis between the AIMR and the different climate indices in wet and dry monsoon years, we followed your suggestion and calculated the correlation only for the monsoon climatology on centennial scale. It is also obvious, that the differences of the correlation coefficients between centennial and multidecadal time scale based indices are not that large. Therefore, we delete the previous Figure 8 and add the Table 2, which summarizes the 200-yr averaged correlation coefficients and its statistical significant values at the 99% confidence level.

6. *p. 720: As someone more used to orbital time scales, the conclusion that increased solar irradiance causes a weaker monsoon doesn't completely make sense to me. Why does increased solar irradiance warm the ocean more than the land? Generally, you'd expect the opposite given land's lower thermal mass (this seems to be the case in Figure 7c for 2-m air temperature). And, according to figure 6, proxies show that the MWP (higher solar irradiance) is wetter than the LIA (lower solar irradiance).*

1. Climatological 200-yr time slice analysis:

The TSI value has been calculated for summer (JJAS) averaged over the AIMR domain and the 200-yr time slices of MWP, LIA and REC to better compare it with the summer rainfall signals during these periods in that region. The centennial-scale temporal anticorrelation between both is very high (e.g., - 0.95 for MWP). Due to various interacting physical mechanisms and feedbacks on different spatio-temporal dimensions, we assume a complex and overlaying non-linear process that impacts the negative relationship between solar irradiance and Indian rainfall in summer months not locally but more under a large-scale aspect, which has been already discussed in other studies (e.g., Meehl et al., 2009). An overall warming (cooling) period like the prominent MWP (LIA) is not solely leading to a homogeneous warming (cooling) over all regions, but more to a regionalization and inhomogeneity due to overlaying internal factors. Secondly, local effects of solar forcing (higher TSI leads to more rainfall and vice versa) are mostly suppressed and dominated by large-scale processes between the atmosphere and the ocean (e.g., evaporation and horizontal advection of moisture based on changed atmospheric circulation). Further it has to be considered that the total solar irradiance in the model is influenced by an orbital-scale change and short-term 11-yr solar fluctuations, which are more important for the interpretation of these mechanisms since the long-term changes don't show any significant fluctuation and trend within the 200-yr time slices between MWP and REC and thus can be neglected in the analysis of centennial-scale rainfall composites and its external forcing. For a better understanding of these mechanisms, we additionally calculated the spatial distribution of TSI anomalies. The climatological 200-yr averaged spatial TSI differences "MWP minus REC" (not shown) indicate a mostly zonally orientated spatial pattern with an increase of solar irradiance in a belt from the central Arabian Sea, central and southern India, the Bay of Bengal and southeastern Asia corresponding to drier conditions. Lower TSI values (not shown) are simulated from eastern Himalayas, over southwestern Tibetan Plateau to eastern China in correspondence with wetter conditions. In LIA epoch the respective spatial TSI anomalies compared to REC show again an increase from the Arabian Sea over southern India, the Bay of Bengal and southeastern Asia, but a more pronounced decrease from northern India over the Ganga Plains towards the eastern regions as well as a slightly increase over the Tibetan Plateau. Especially the southward shift of the positive TSI anomaly pattern over India and thus the stronger gradient between southern and northern India including the surrounding ocean basins can trigger the enhancement of the moisture penetration towards the Ganga Plains resulting in higher rainfall in LIA than in MWP. We conclude, that related to the large-scale aspect, a higher (lower) solar shortwave irradiance at the top of the atmosphere over Indian land surface (better now referred as solar activity of the 11-yr cycle) leads to a mid- and lower tropospheric warming (cooling) of the corresponding areas, but as the insolation changes are spatially inhomogeneous over India, a spatial inhomogeneity in temperature, atmospheric circulation and rainfall patterns are simulated. The different thermal evolutions result in a weakening (enhancement) of the monsoonal-driving large-scale meridional temperature gradient between the ocean and the land surface with a reduction (intensification) of the cross-equatorial monsoonal winds at the lower troposphere. This is followed by a decrease (increase) and a shift in the position of the monsoonal heat low with the corresponding deep convection area over northwestern India embedded in the ITCZ, which is shifting south - eastward (north - westward). Finally a weaker (stronger) ISM due to the rainfall intensity can be identified. The 2m-air temperature anomalies are mostly influenced by the rainfall strength via negative

moisture-temperature feedbacks. Higher (lower) rainfall amounts are associated to evaporation cooling (warming). In addition the rainfall intensity impacts the cloud-albedo-relation, which is beyond the scope of our study.
The external forcing is overlain by internal variability modes of the ocean (e.g., ENSO), which modifies the intensity of solar variations on the monsoon strength. Therefore, only a combined multifactorial interdependence influences the Indian rainfall on longer time scales. Our correlation analysis of external and internal drivers points this out. We clarified the explanation of the different mechanisms with respect to the 200-yr rainfall composites (page 12, lines 24-32, page 13, lines 1-33 and page 14, lines 1-10) and modified the paragraph according to your comments.

2. 30-yr dry monsoon composites (see chapter 4.2.1):

Since the calculation results of the temporal correlations for the dry (wet) summer monsoon composites are quite similar to the 200-yr climatology, we delete the figure 8 completely also in accordance to your comment about the unrealistic time length for a correlation analysis of 30-yr composites especially for TSI, even if the included 11-yr solar activity cycle can influence the rainfall patterns in shorter extreme monsoon years. We focus more on the changes in SST and atmospheric circulation changes due to ENSO and DMI, which are some of the important internal drivers for extreme dry monsoon conditions on decadal time scale (see other studies like Krishnan and Sugi, 2003; Mujumdar et al., 2012). For example, the mechanisms leading to the inhomogeneous distribution of simulated rainfall anomalies during dry years of MWP and LIA are mostly related to a) large-scale air-sea feedbacks of periodic El Niño/La Niña events with a shifting Walker circulation leading to a weaker (stronger) ISM and less (more) rainfall, but these drier (wetter) conditions are b) modified regionally via different non-linear feedbacks (e.g., internal SST variability in the Indian Ocean and regional changes in the atmospheric circulation via shifting of the ITCZ position) further expressed by significant wetter (drier) conditions in the Indo-Pakistan region in MWP (LIA). We have rewritten the paragraph for the dry summer monsoon composite study according to the higher relevance of the ENSO phenomenon (page 16, lines 21-33 and page 17, lines 1-12). The temperature signals shown for the dry composites indicate a more pronounced response of precipitation than vice versa. Warm (cold) anomalies in the 2m-air temperature patterns are associated to evaporation warming (cooling) due to less (more) rainfall and weaker (stronger) cloudiness in the region as already discussed for the climatological 200-yr anomalies. Therefore, we clarified the rainfall-temperature-response in both paragraphs (for the 200-yr and 30-yr anomaly composites).

7. *I'm skeptical about the use of the All India Monsoon Rainfall (e.g., Figure 1, Figure 8) because AIMR seems to combine regions that respond differently through time (e.g.,Figures 5,6,7). Also, the recent paper by Conroy and Overpeck 2011 Journal of Climate 24: 4073 shows that precip does not change coherently across India on the interannual time scale.*

Thanks a lot also for that critical comment. Firstly, we want to agree with your legitimate concern, since the AIMR domain shows a spatial inhomogeneity in rainfall patterns especially on interannual time scale as demonstrated also in our study, however to get a consistency in our analysis we want to use the state-of-the-art entire AIMR domain, which has been successfully applied in numerous other ISM studies before (e.g., Krishnan and Sugi, 2003). For a more critical discussion of the AIMR domain we additionally added some remarks about the inhomogeneity rainfall distribution within that region and we highlighted some comments that the region has to be analyzed more carefully and critically with respect to the discussed problems (page 9, lines 11-13). Further we added your suggested reference from Conroy and Overpeck, 2011 to underline the statement. We will consider your arguments within our following studies and think about a better usage / regionalization of the AIMR domain.

8. *Why isn't PDSI correlated with prescribed SSTs rather than SST observations? That would seem to be the most direct comparison since the modeled PDSI is responding to SSTs in the model. It would also allow for the analysis to be done for the other periods (MWP and LIA). As it stands, the PDSI section of the paper seems out of place because it moves away from the focus on the differences between MWP-LIA-REC. Also, it seems odd to focus on EOF#4. It explains only 5% of the variance, which is probably below the threshold for which PCs should be retained. What does the scree plot/log-eigenvalue diagram look like? Why aren't EOFs #2 and #3 discussed?*

The aim of this correlation has been on the direct model-proxy-intercomparison in order to follow the algorithm of Cook et al., 2010. Therefore, we correlated the Principal Components of modeled PDSI with the observational SST. Compared to Cook et al, 2010 we calculated higher correlation coefficient values between SSTA and PCs especially for PC4. Since there are no observation data for LIA, we focused only on the REC time slice. The comparison with prescribed SSTs are currently done in another study (will be submitted soon) and thus is beyond the scope of this paper. In this context, the PDSI section seems a little bit out of place regarding the previous results, but it should emphasize on a model evaluation with respect to available proxy data, which is the first important step for further analysis. As presented in the scree plot (see figure 05s) the first leading EOF shows the highest variance of 15.75% and the following 3 EOFs indicate almost constant variances. PC3 doesn't have a significant correlation with SSTA (see figure f04s in the supplementary). The EOF4 shows a better agreement in the spatial patterns compared to Cook et al., 2010 (DEOF4) although the correlation patterns are quite similar for PC2 and PC4 in comparison to the corresponding SSTA. Therefore, we discussed EOF1 and EOF4 in our analysis.

Minor comments:

1. *Some formatting with regards to paragraph breaks might have been lost. P. 706 line 7, new paragraph should begin at "Several. . ." P. 706 line 23, new paragraph should begin at "The following. . ." p. 720 line 2, new paragraph should begin at "Figure 8 …"*

Done. We formatted the corresponding paragraphs with new breaks.

2. *Figure 3: The generalized wind vectors don't seem realistic, especially the ISM vector extending far north into China. It would be preferable to plot actual wind vectors from NCEP or another source. Also, for clarity, it should be specified that the dashed line shows the *maximum northward position* of the ITCZ/monsoon trough. What is the* *source of the line labeled "ITCZ?" The position you have drawn over the Tibetan Plateau is much further south than in other analyses, see for example Conroy and Overpeck, 2011 Journal of Climate 24: 4073.*

Thanks a lot for your remark. We agree with you, that the wind vectors show a very generalized and unrealistic position, and thus differ from other studies. Further the ITCZ is located too much southward over the Tibetan Plateau. We checked the study from Conroy and Overpeck, 2011 and decided in agreement with your suggestion to add the summer monsoon wind fields at 850 hPa from the ERA-Interim reanalysis data (1989-2011) showing the main lower-tropospheric wind directions. The ITCZ band is not explicitly added in that plot for a better clearness of the figure (Figure 3).

3. *p. 710 line 19, Similar to comment above regarding Figure 3 – Are these really affected by westerlies? Others would argue that the climate here is monsoonal.*

The paleoclimatic sites in the Himalayan region are mostly affected by ISM in summer, when the ITCZ shifts northward towards the Tibetan Plateau crossing the Himalayas leading to a lower tropospheric cross-equatorial southwesterly wind component with embedded moisture advection towards the Indian peninsular, and in winter the sites are mostly affected by Westerlies due to the southward retreat of the ITCZ and the penetration of moisture from the Mediterranean origin crossing the Indo-Pakistan region further eastward. We clarified the seasonality of the moisture source origin in the description of the reconstructions (page 7, lines 18-24).

4. *p. 711 section 2.3.1 these two sentences seem contradictory (TSI = distinctive range, TSI=all wavelengths). Perhaps these sentences could be combined. What TSI reconstruction was used? Please provide reference.*

Done. We combined the two sentences to clarify the explanation. "The ECHAM5 calculated annual TSI (Wm^{-2}) value, used in this study, is taken from the "net shortwave incoming solar radiation" model parameter at the top of the atmosphere. The reconstruction of the TSI variations in the driving MPI-ESM has been done by Krivova et al., 2007 and Solanki et al., 2004." (page 8, lines 10-19).

5. *Figure 4 caption: Explain that these are for temporal correlations with APHRODITE (I presume?) for the region 0-50N, 60-120E.*

Done. We clarified the description of Figure 4 and added your suggested explanation (page 34).

6. *p. 714 line 20: Should this read 120 E rather than 12 E? This is much bigger than "a box over India."*

Thanks a lot for that detailed remark. We changed the longitude to 120 E (page 10, line 23).

7. *p. 714 line 20-21: . . .interpolated to a coarser grid." I assume that only the high-res model was interpolated and that the coarser grid used was that of the lower-res model?*

We agree with your remark and added your suggested explanation in the text. The high resolution ECHAM5 model (T63L31) and the observations have been interpolated to the coarser resolved model (page 10, lines 23-25).

8. *P. 714 line 23-25: Really the correlation shown in Figure 4 is between D/C and A, correct? From the figure, it is not clear whether D or C is better correlated to A or to B.*

   For a better visualization we additionally highlighted APHRODITE (A) as reference data set in the figure. MPI-ESM (D) is slightly better correlated than ECHAM5 (C).

9. *P. 714 line 25: "closer" than to what? APHRODITE?*

   „The standard deviation of the coarser resolved model is also closer to GPCC6". We clarified the sentence (page 10, lines 28-29).

10. *p. 714 line 27: "which would also limit the potential agreement between model simulations due to interpolation errors." I don't fully understand. How does interpolation introduce error?*

   Thanks for your comment. We discussed the problem again and have to modify our statement, that the interpolation can introduce errors. There might be other possible influences on model biases like physical parameterization of cumulus convection (see also comment 12), which has to be studied more. According to that, we changed our sentence to "which might be one possibility to limit the potential agreement between model simulations" (page 10, lines 30-31).

11. *Figure 4: Would it be possible to also show non-interpolated results?*

   Done. We additionally attach the non-interpolated results in the supplementary (figure f02s). The calculated statistical parameters indicate a slightly weaker agreement of the different data sets compared to the APHRODITE reference especially for GPCC6. Therefore, we used the interpolated results in our analysis.

12. *Figure 4: It seems odd that low resolution does better. Explanations for this?*

   We agree with your comment that a lower resolution implies a better temporal agreement compared to the observations. For a better understanding, we additionally divided the total rainfall of MPI-ESM (T31L19) and ECHAM5 (T63L19) into convective and large-scale precipitation. The figure f03s shows the 2-yr low-pass filtered time series of both precipitation components and both model resolutions for 1800-2000 AD averaged over the box (0-50 N and 60-120 E). ECHAM5 illustrates a much higher interannual variability throughout the period compared to MPI-ESM for both precipitation components. The bias in the large-scale precipitation is less than in the convective precipitation, where ECHAM5 is overestimated. Therefore, the higher RMSE of ECHAM5 is due to the cumulus convection parameterization scheme. However, the interannual variability is much better resolved by ECHAM5. We added some comments in the end of the paragraph (page 11, lines 2-7).

13. *p. 718 line 6, It is not clear to me that either more sites or higher resolution will fix this problem. It is likely related to the forcing or to model biases.*

We agree with your argument that the disagreement between model and proxy data are more related to the external forcing or to model biases due to physical parameterization. According to that we changed the explanation (page 15, lines 1-3). Further we shifted and changed the sentence “Therefore, more sites and higher resolved climate model simulations have to be considered in the analysis to better compare the model results with respect to the inhomogeneities in the spatial rainfall distribution” to the conclusion part as outlook for upcoming studies: “… more sites and higher resolved climate model simulations have to be considered in the analysis for an improved model-proxy comparison with respect to the inhomogeneities in the spatial rainfall distribution and the reconstruction-inherent uncertainties of proxy data” (page 19, lines 25-28).

14. *p. 718 line 23, What is a “dry rainfall event.” This sounds like an oxymoron.*

Done. We changed the word to “weak rainfall event” (page 15, line 20).

15. *p. 719 line 25: “This relationship is approved for MWP and LIA respectively.” Maybe instead: “This relationship is shown for both MWP and LIA.” This sentence is one example of the proofreading for English grammar that should be done for the entire paper.*

Done. We revised the sentence to: “This relationship is shown for both MWP and LIA” (page 16, lines 15-16). Additionally we proofread the entire manuscript for the English grammar.

16. *Figure 8: Arrows from ONI and DMI to TSI could be interpreted as these modes having an effect on TSI.*

We completely agree with your comment, that the arrows from ONI and DMI to TSI show the wrong direction. The TSI has an influence on ONI and DMI (SST anomalies via thermal heating/cooling) but not vice versa. Due to less applicability of 30-yr composites for the correlation analysis, we deleted the figure.