We are very grateful to the reviewers for their very constructive and helpful comments. We will revise the manuscript following the reviewers instructions and remarks. The revision will certainly lead to a stronger manuscript.

REVIEWER #1

From the abstract and conclusion, it seems that the focus of this paper is on the response of African and Indian monsoon to precession and obliquity, but this is only briefly mentioned in several places and the analysis of processes was not done by this paper but was refereed to other studies which have similar findings. The authors should stress what is new in the paper and its original contribution to interglacial and monsoon study.
We will revise the abstract and the conclusions to better describe the content and goal of the present study.

The conclusion of the authors that the global monsoon concept is challenged is actually based on the differences between 416kyr and 394kyr and between 495kyr and 516kyr where the precession is very similar between two time slices. It means that the role of precession is minimized in these comparisons. However, Fig10 tells that both Africa and Indian monsoon are mainly controlled by precession and both are highly and negatively correlated with precession. It means that at the astronomical time scale, both monsoon systems would co-vary with precession, and therefore the global monsoon concept could still be valid.
Probably the manuscript was not clear enough regarding this point. The global monsoon concept is challenged by the time slices of 394 ka and 615 ka, where the North African rainfall anomaly has opposite sign compared to the Indian anomaly (see Table 2). This clearly points to the fact that the two regional monsoon systems do not always vary in concert. We will revise the manuscript to make this point clearer.

In sections 3.1-3.6, it would be more interesting and add more value to the paper if the CCSM3 results are compared to proxy data and to other model results, even qualitatively. Moreover, in most of the discussions, only insolation has been used to explain the changes, and the role of CO_2 seems to have been forgotten.
An in-depth model-data analysis is beyond the scope of this study. Several time slices from our set of experiments have already contributed to comprehensive model-data comparison (Lunt et al., 2013; Milker et al., 2013; Kleinen et al., 2014) and we refer to those studies. Comparison to other model studies is part of the discussion. Following the reviewer's suggestion we will extent this comparison.

Specific comments:
1. Title: Not all the interglacials from MIS15 to MIS1 have been analyzed in this paper, so please be precise.
We will change the title to “Intra-interglacial climate variability: Model simulations of Marine Isotope Stages 1, 5, 11, 13, and 15”

2. Please change everywhere “orbital” to “astronomical” because obliquity is not orbitally related.
Will be done.

3. Page 3037:
L1: for the periodicity of the astronomical parameters, Berger (1978, J.Atmos.Sci) deserves to be cited
Will be done.

L1-4: about the influence of evolution of astronomical parameters on the internal structure of interglacials, I recommend the paper Yin and Berger (2015, QSR) to the authors.
The reference will be included.

L8-29: many interglacial simulations (both snapshot and transient) have been done in earlier time with both EMICs and GCMs, eg. Kubatzki et al (2000, Clim Dyn), Crucifix and Loutre (2002, Clim Dyn), Loutre and Berger (2003, global planetary change), Yin and Berger (2012, Clim Dyn) and Yin and Berger (2015, QSR). These deserve to be included in the introduction.
These references will be included.

L22-26: please specify what is the advantage of using realistics interglacial astronomical configurations as compared to the idealized astronomical forcing.
We consider realistic and idealized forcing experiments equally important. Idealized experiments provide important insight into the climate system's response to astronomical forcing. However, since this response may be non-linear, using extreme values of astronomical parameters in idealized experiments tells us only a part of the story. Therefore, idealized and realistic forcing experiments should be considered complementary. Obviously, realistically forced experiments have a stronger potential for model-data comparison. We will add one or two sentences to the manuscript for clarification.

4. In Section 3.1, 3.2 and 3.3, only insolation is used to explain the difference between the interglacials, but what is the role of CO2?
Without individual forcing experiments as e.g. in Yin and Berger (2012) final conclusions about the role of CO2 can not be drawn (given the computational expense of additional individual forcing experiments with the comprehensive CCSM3, such experiments are however beyond the scope of this study). We partly circumvent this problem by introducing the correlation maps in Section 3.6, which provide a hint on the importance of GHG forcing. However, for the individual seasons, we surmise that insolation forcing is dominant in most regions of the globe. CO2 may play a larger role for the annual means (cf. Yin and Berger, 2012). Nevertheless, we will add some remarks on the potential role of CO2 forcing (especially with respect to the southern high latitudes, see below) to sections 3.1, 3.2 and 3.3 with reference to Yin and Berger (2012).

5. In section 3.1, please explain the southern ocean cooling in Group I. This is quite similar to the results of Yin and Berger (2012) where this cooling is attributed to summer remnant effect of local insolation.
We will add an appropriate explanation.

6. Page3078, L15-17: is the cooling over southern hemisphere continents statistically significant? By the way, are the features given in fig3, 4, 5,6 significant?
Yes, all colored responses are significant (t-test, $p<0.05$). Note that the maps were calculated from 100-yr means.

7. Page3079:
L8: : : :.southern hemisphere (except Antarctica)
ok.

Page3079, L19-23: are these observed in your model or in other study? The same processes have been demonstrated in Yin and Berger (2012) where the definition of “summer remnant effect” was given.
Both. We will appropriately cite Yin and Berger (2012).

L24: I would add “probably” before “masked”.
Agreed.

8. Page3080, L1-3: is it possible to give explanation about the temperature change?
Yes, we will add that low GHG forcing (in particular due to CH4) in the 394 ka experiment certainly plays a role here.

9. Page3081, L29-28: why does the JJAS warming over southern ocean and Antarctica not appear in 495-516? For 416-394, the summer remnant effect happens over the polar oceans, how to explain the warming over Antarctica continent and a cooling over western Antarctica? CO2 effect needs to be discussed here.
We fully agree and will discuss the role of GHG.

10. Page3082:
L11: the effect of obliquity on annual insolation at high and low latitudes does not need to be implied, it is explicitly demonstrated in Berger et al (2010, QSR).
We will remove this statement.

L18-22: what is the role of lower CO2 at 495 than 516 to explain the weaker Sahel rainfall increase during MIS13 than during MIS-11?
The GHG forcing is too small to exert a dominant effect on the Sahel rainfall change (cf. Figure 10). A more convincing argumentation will take the change in meridional insolation gradients into account, which is much larger in MIS11 than in MIS13. We will add a paragraph for clarification.

11. Section 3.6: how were the correlations made? Are these correlations statistically significant?
An explanation is given in the first paragraph of Section 3.6. For clarification we will add some more information, e.g. that climate variables (temperature, precipitation) were averaged over the last 100 years of each experiment. Linear correlation coefficients were calculated at each grid point. Significance of correlations was tested by a two-sided Student's t test with 95% confidence level. Only significant correlations are shown, non-significant regions are white.

12. Page3083:Figre9a: why is the correlation between GHG and high latitude temperature very weak? This seems not consistent with the knowledge that high latitudes response to GHG change is much larger than the other part of the world.
The correlation maps have to be interpreted carefully. Just because the correlation coefficients are small, this does not mean that GHG have no effect. The correlation is weak, because other forcings (obliquity, precession) have a much larger influence in our set of experiments, where GHG variations are relatively small. We will add a paragraph for clarification.

13. Page 3083: For the relative impact of obliquity and precession on surface temperature and precipitation, I recommend the paper Yin and Berger (2015, QSR) where results were obtained from transient simulations covering a large range of precession and obliquity.
We will add Yin and Berger (2015)

14. Page 3083, L15: how about the monsoon change in other Southern Hemisphere regions?

The effect on surface temperature is indeed much smaller in South America and South Africa. We will add a remark.

15. Page 3083, L17: in some doubling CO2 experiments, it is shown that monsoon precipitation is sensitive to CO2 change (eg. IPCC report), but in your figure 10a, there is no correlation between the two. Please explain
Please note that the CO2 variations are relatively small, i.e. far from being doubled. The effects of astronomical forcing on the monsoons are way larger than the relatively small GHG variations during the interglacials. Hence, the absence of a significant correlation in Figure 10a is reasonable.

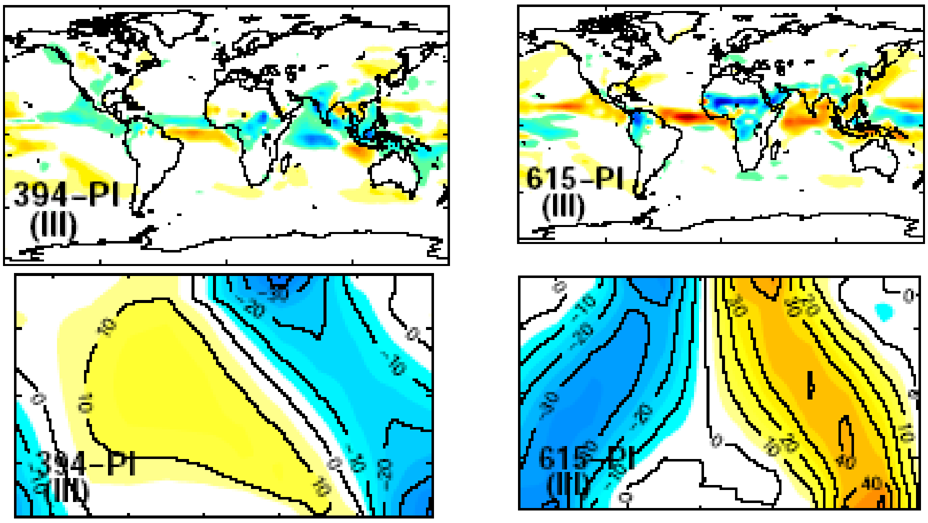
16. Page 3083, L23-24: how about the precession influence on the East Asian monsoon in your model?
East Asian rainfall shows a somewhat heterogeneous pattern and is, in general, only weakly coupled with the Indian and African monsoons. This finding is consistent with a recent model intercomparison study by Dallmeyer et al. (Clim. Past, 11, 305-326, 2015) who found a stronger response of the North African and Indian monsoon systems to insolation forcing than of the East Asian monsoon.

17. Page 3085:
L15-16: pay attention that the GHG and precession are not exactly the same between the time slices.
We agree and will include a cautionary note. Moreover, the potential role of GHG forcing in modifying Antarctic surface temperature changes during MIS11 will be discussed.

L26-29: the dating uncertainty and the tuning procedure of LR04 stack should not be ignored here in such discussion. Moreover, lag between climate forcing and ice sheet response should also been taken into account
We will add a cautionary note.

18. Page 3086, L1-5: although 495kyr is the warmest in Group II, it is still much cooler than Pre-industrial in NH summer (fig3). How can you conclude that this cooling is not enough for ice sheet growth? In the simulation of Ganopolski and Calov (2011), there is a small ice sheet developed around 495 kyr
We cannot provide a final conclusion without coupling the model to an ice sheet model. Our suggestion has to be considered a hypothesis. We will rephrase the sentence to be more careful.

19. Page 3087, L4-6: please demonstrate this statement
This conclusion simply derives from the consideration of the two corresponding insolation maps of the 394 and 615 kyr experiments (see below), suggesting that the sensitivity of the tropical monsoons is not uniform: the North African monsoon is more sensitive to summer insolation while the Indian monsoon to spring-early summer insolation. Similar results have been found by Braconnot et al. (Climate of the Past 4, 281-294, doi:10.5194/cp-4-281-2008, 2008). It has been argued that the reason is a resonant response of the Indian monsoon to the insolation forcing when maximum insolation anomalies occur near the summer solstice and a resonant response of the African monsoon – which has its rainfall maximum one month later in the annual cycle than the Indian monsoon – when the maximum insolation change is delayed after the summer solstice. We will include an explanation and the reference (Braconnot et al., 2008) into the revised manuscript.



20. Page 3087:
L16-17: please specify which is more important in controlling the Africa monsoon, precession or obliquity.
Both are important, but the response to precession is still stronger. We will add this information.

L21-23 and Page 3072, L15-17: These lines are not convincing, see my comment 18.
Agreed. We will rephrase (see above).

21. Page 3088, L8-9: I remind that transient simulations for earlier interglacials have been given in Yin and Berger (2015, QSR).
We agree, but our statement only refers to CGCMs (i.e. general circulation models) and does not include EMICs.

REVIEWER #2

Main comments
A- Introduction. The introduction should be extended to provide more information. In particular, it would be useful to the reader to briefly discuss some main characteristics of the interglacials considered here, such as the length, the maximum temperature anomaly reconstructed compared to preindustrial and the relative sea level, if available. It should also be explained why the early Brunhes interglacials (MIS 13 and before) are different from the later interglacials, as later in the paper this is referred to. In addition, the main findings of previous modelling studies that have focused on several of the considered interglacials, should be briefly discussed. This is especially relevant for the Herold et al. (2012) study that was conducted with the same CCSM3 model. It should also be explained more clearly what the novelty of the present study is compared to these previous studies. This should include a rationale for selecting these specific interglacials and these 14 time slices, as this is not clear from the introduction.
We will include more information into the introduction as the reviewer suggests. The selection of the time slices is described in Section 2.3 in detail. We will include a reference to Section 2.3 in the introduction.

B- Setup of experiments. Orbital forcing: The authors should discuss the season definition that they have used for the insolation in the different experiments (see Joussaume and

Braconnot, 1997). I suspect that the date of vernal equinox has been kept fixed at today's value. The choice of calendar should be made clear, as it has potentially a huge impact on the results.
We used a fixed calendar based on a 365 day year with vernal equinox fixed to March 21 (the Day/Month values refer to the present calendar). We agree that a fixed calender may affect the results, however, the strongest effects are known to occur in boreal fall, whereas the effect in boreal summer and winter (the seasons discussed in our manuscript) are small (e.g. Timm et al., Paleoceanography, 23, PA2221, 2008). Still, we will add a cautionary note.

C- Results. The results section could be improved, as the explanation of the results is in a few instances not very convincing.

On page 3079, line 15, the warm conditions in winter in the Arctic in the Group I experiments is discussed. "However, anomalously warm conditions in the Arctic stand in contrast to the global DJF cooling at 6, 9, 125, 405, and 416 kyr BP. The Arctic warming is due to the remnant effect of the polar summer insolation through ocean–sea ice feedbacks ". I wonder if this is the full explanation. Why is this Arctic winter warming not present in the other Group I simulations for 504 ka and 579 ka? For instance, looking at the insolation anomalies in Figure 2, the forcing looks very similar for 125k and 504 ka, but the Arctic warming in winter is absent in the simulation for 504 ka. Please elaborate
We thank the reviewer for this important point. The role of GHG forcing has to be taken into account, since in the early Brunhes time slices (504 and 579 ka) the low-GHG cooling masks the summer remnant effect in the Arctic. We will add this point to the manuscript.

In Section 3.5 (page 3081), the effects of obliquity is discussed by comparing the anomalies of 416 minus 394 ka and 495 minus 516 ka. It is concluded on line 22 that in the 416 ka and 495 ka cases with maximum obliquity forcing, the boreal summer temperature in monsoon regions is lower than in the minimum obliquity cases because of higher rainfall. However, as can be seen in Figure 8f, the precipitation anomalies are very small (less than 0.2 mm/day) in monsoonal areas in the 495 ka case, making this conclusion highly unlikely, at least for 495 ka. I think it is more plausible that the negative insolation anomaly at low latitudes depicted in Figure 7 is the direct cause of the negative temperature anomaly. For the 495 ka case, the June-July insolation at 10_N is more than 10 Wm-2 less than in 516ka.
The reviewer is absolutely right. Direct insolation forcing is the major cause for the low latitude cooling in the 495 ka experiment. We will re-formulate the paragraph.

At line 20, the small rainfall anomaly in the Sahel in the 495-516 ka plot (8f) is explained by the high precession at 495 ka which counteracted the obliquity-induced increase in monsoonal rainfall expected by the authors. This is an implausible explanation, as precession has similar values at 495 and 516 ka (Figure 1). However, even if precession values would have been different, the modelled climate does not "see" the high precession (or high obliquity), as it is only forced by the insolation anomalies that result from the changes in astronomical parameters. These insolation anomalies are shown in Figure 7. I think it is deceptive to consider variations in astronomical parameters as direct forcings of climate change in particular areas. Instead one should consider the net effect of these astronomical parameters on the insolation, which as a result varies per latitude and per month as is clear from Figure 7.
We agree. We will rephrase the explanation in terms of insolation forcing. Indeed, negative local insolation anomalies (see Fig. 7b) do affect the strength of the West African monsoon.

D- Discussion and conclusions. The discussion should be extended to include several limitations of the study. As mentioned in the conclusions, the model experiments did not include appropriate ice sheet configurations, while it is known that changes in ice sheets also affected interglacial climates. The potential effect of prescribing preindustrial ice sheets should be properly discussed, and not just be mentioned in the conclusions. In fact, it is not a conclusion from this study.
We agree and will change the discussion accordingly.

In addition, also the impact of the choice of calendar on the results should be discussed in Section 4. The conclusions should also stress more clearly what the added value of this study is compared to the various other recent modelling studies that have focused on interglacial climates. Do the experiments provide improved understanding of certain features seen in proxy-based reconstructions? If so, where?
Will be included into the manuscript.

Minor comments
Page 3074, line 24: "have usually set to extreme values" should be "have usually been set to extreme values".
ok.

Page 3074, line 26: "our analyzes are based on realistic orbital configurations and hence climate states". I disagree with this statement. The fact that realistic orbital configurations are prescribed does not necessarily mean that the simulated climate states are also realistic. For instance, preindustrial ice sheets have been prescribed in all experiments, while it is well known that there have been substantial changes in ice sheet configuration during the considered interglacials, which will have impacted the climate as well.
We will remove 'and hence climate states'.

Page 3075, line 6. Starting from the preindustrial spin-up, each experiment was run for 400 years, of which the last 100 years were used in the analysis. After 400 years, the deep ocean is still adjusting to the change in forcings (e.g. Renssen et al. 2006). For this reason, other similar studies have used a longer run time, for instance, 1000 years in Yin & Berger (2012) and Herold et al. (2012). Although in the present study the focus is on the surface climate, for which 400 years is probably sufficient, I would still suggest that to discuss this issue in Section 2.2.
We agree. Especially in the Southern Ocean (deep-water upwelling) effects of the integration time cannot be completely ruled out (e.g. Varma et al., Geosci. Model Dev. Discuss., 8, 5619-5641, 2015), although this would probably more affect the magnitude rather than the sign of changes. We will add a short discussion.

Page 3080, line 6: I propose to rephrase this sentence (2x precipitation). "precipitation shown in Fig. 5 exhibits intensified precipitation: : :"
ok.

Page 3080, line 11: " The most interesting results regarding the tropical rainfall response to astronomical forcing appear in Group III, where the monsoonal precipitation anomalies show opposite signs in North Africa and India." This is the case for the 615k simulation, but it is not clear for the 394k experiment, as Figure 5 clearly shows for 394k enhanced precipitation in N Africa and India. Please revise.
Figure 5 clearly shows reduced (yellow) precipitation in the West African Sahel region in the 394 ka experiment (enhanced precipitation is only visible in Central Africa). For clarity, we will replace "North Africa" by "West Africa".

Page 3080, line 25: "In high Arctic latitudes, vegetation advances (NPP increases) in the Group I simulations: : :" If NPP increases, does it necessarily reflect an advance of vegetation? It could also reflect a change of the vegetation at the site itself, couldn't it? I would say it is not so straightforward to interpret simulated NPP changes in terms of shifts in vegetation. But maybe the authors have also checked other output from their DGVM to come to their interpretation. If this is the case, I suggest explaining this in the manuscript. The same is true for the NPP decline in the Arctic in the Group II simulations.
We agree and will rephrase the paragraph.

Page 3082, Section 3.6. I would propose to explain in more detail how the correlation maps are constructed and what they mean. The values of the GHG forcing are not necessarily independent from the values of precession and obliquity. For instance, CO_2 and CH_4 levels in the atmosphere depend on exchange between carbon pools, which in turn is affected by climate due to changes in astronomical parameters. So if there is a positive correlation of temperature with GHG forcing, we are not purely looking at correlation to the radiative forcing, but potentially also at the correlation to orbital forcing in the background. What does the correlation to GHG radiative forcing mean, and how should it be compared to the correlation with precession and obliquity?
An explanation is given in the first paragraph of Section 3.6. For clarification we will add some more information, e.g. that climate variables (temperature, precipitation) were averaged over the last 100 years of each experiment. Linear correlation coefficients were calculated at each grid point, etc.
We further note that GHG forcing and astronomical parameters are not significantly correlated in our set of experiments, hence the reviewer's point is not an issue.

Page 3088, line 12. I do not consider CCSM3 a "state-of-the-art" model, as it was released more than 10 years ago. We have already the next generation: CCSM4 (and CESM).
Ok, we remove 'state-of-the-art'.

Additional references ➔ **The two references will be added**
Joussaume, S. and Braconnot, P. 1997: Sensitivity of paleoclimate simulation results to season definitions. Journal of Geophysical Research 102, 1943-1956.

Renssen, H., Driesschaert, E., Loutre, M.F., Fichefet, T. 2006: On the importance of initial conditions for simulations of the Mid-Holocene climate. Climate of the Past 2, 91-97.