Replies to Reviewer #1

Thank you very much for your comments. The reviewer has three main concerns as follows.

The authors have provided a very in-depth response to the reviewers; however, the changes to the manuscript itself are not very extensive, and do not fully reflect the reviewers concerns, in particular relating to model biases and model dependence, but also regarding statistical significance, which is still not discussed in the manuscript. The manuscript could still benefit from a clearer motivation and structure. It is still heavy on description, and light on physical explanation for a lot of the changes seen, which makes it hard to understand potential model/bias sensitivity of the results. I would recommend these concerns are addressed in the manuscript before publication.

Responses: Thank you very much for these comments. Your suggestions help us improve the manuscript greatly.

In this revision, we added an appendix, to provide careful discussions on the model biases and the significance of the TP-induced changes.

The introduction was re-written, in which we added a paragraph to state the motivation of this work.

We are very sorry to see that you may still feel this revision is heavy on descriptions. It may be true in some way, because this work is the first paper of our series on the global impacts of the TP. The goal of this very first paper is to draw a panorama picture of TP's global impacts. We have paid less attention to the mechanisms of the remote changes occurring in the Pacific and Atlantic. For example, why does the Northern Hemisphere experience a warming in the beginning and then a remarkable cooling in the end, in response to the TP removal? And how are these changes related to the ocean circulation, in particular, to the Atlantic meridional overturning circulation? These questions are answered thoroughly in our follow-up papers, namely, "*Investigating the Role of the Tibetan Plateau in the Formation of Atlantic Meridional Overturning Circulation (AMOC),*" and "*Investigating the Role of the Tibetan Plateau in the Formation of Pacific Meridional Overturning Circulation (PMOC)*", which have been submitted to *J. Climate* and are in the 2nd round of review.)

We have tried our best to provide detailed physical explanations for the changes seen in this work. To provide in-depth explanations would have to involve global meridional overturning circulations, that is, the AMOC and PMOC, which would make this manuscript too long. The total words would then far exceed the length limit requirement of *J. Climate*.

Major comments:

1. *Model bias:*

Thank you for the in depth comparison between the model and observations - some of this should be available to other readers, not just reviewers. Please, in the manuscript, directly cite a paper that discusses model biases for the model version and resolution you use, or include at the very least some discussion of them in the manuscript.

Responses: Thank you very much for this suggestion. We added an appendix to discuss the model biases and resolution. Also in the discussion section, we mentioned the important work of Kerkhoff et al. (2014) and Reifen and Toumi (2009).

The model does a reasonable job at simulating current climate (except, perhaps, sea-ice cover, and, I would suspect, cloud distribution); however, I do not agree that the potential for model bias on the results can be neglected. This assumes stationarity of biases under different climates (see e.g. Kerkhoff et al. 2014, Reifen and Toumi 2009 for evidence this may not be a valid assumption). In addition, biases can impact the mechanisms behind the response to the Tibetan plateau. For example, if ocean stability is incorrect, then the ocean may be more sensitive to changes in surface freshwater forcing than the real world; if clouds mechanisms are poorly represented, then cloud changes in response to the Tibetan plateau may not be correct. I am not suggesting you shouldn't use this model, but I do think you should acknowledge the biases in the model and discuss how they may impact your results, rather than simply discounting the biases - currently the word 'bias' does not appear in the manuscript, which does not equate to a reasonable discussion of the impacts of biases.

Responses: Thank you very much for this suggestion. We totally agree with you that the model bias could significantly affect the mechanism at work. In the discussion section (lines 584-593 of this revision), we cited your comment above. The model bias is also discussed in the introduction and appendix.

Kerkhoff, C., H.R. Künsch, and C. Schär, 2014: Assessment of Bias Assumptions for Climate Models. J. Climate, 27, 6799-6818, https://doi.org/10.1175/JCLI-D-13-00716.1

Reifen, C., and Toumi, R. 2009: Climate projections: Past performance no guarantee of future skill? Geophys. Res. Lett., 36, L13704, doi:10.1029/2009GL038082.

2. Context of the study

I feel the introduction in particular is lacking a summary of how this study adds to a relatively large body of research on how the Tibetan plateau impacts climate. Your first paragraph cites a lot of studies that show how important the Tibetan plateau is for the climate, but tells the reader little about what this research tells us, and what questions are left - in other words, why is another study necessary? Boos and Kuang (2016), Fallah et al (2016), Shi et al. (2015), Sha et al. (2015), Su et al. (2018), should all be cited in the introduction. Essentially, this is not the first paper to look at the impact of the Tibetan plateau, including in a coupled atmosphere-ocean model (Su et al. 2018), so you need to explain more clearly what the current state of knowledge is, and what this study adds, to give the reader a good sense of why they should read this paper. Your manuscript reads a little like *a review of all of the impacts that the Tibetan plateau has globally, but without reviewing all the previous literature.*

Responses: Thank you very much for this comment. The introduction was re-written carefully.

The motivation of this work is pretty straightforward. Despite numerous observational and modelling studies on the TP, there is still a lack of a panoramagram of TP's global impacts. We try to quantify the effects of the TP on global climate in a unified coupled climate model. This is much needed because on the one hand the ecosystem of TP region is fragile and very sensitive to current global warming, and on the other hand the changes over the TP region would have profound feedback to the global climate. Observations have shown that during the past half century, the warming rate around the TP region was about 0.35°C per decade (Xu et al., 2017), three times of that of the global-mean surface temperature. Therefore, we need to answer a very fundamental question first: how different would the global climate be without the TP?

We cited Boos and Kuang (2016), Fallah et al (2016), Su et al. (2018) in the introduction, and added Shi et al. (2015) and Sha et al. (2015) in sections 2 and 5, respectively. We would like to say that, using different models, different researchers show a similar concept that continental mountains can affect the state of thermohaline circulation to a great extent; however, the mechanisms proposed in different studies are very different. For example, Schmittner et al. (2011) showed that it is the Rocky Mountains that block water-vapor transport from the North Pacific to the Atlantic Ocean, contributing to increased salinity and deep-water formation in the Atlantic. For the mechanism of the weakening of Atlantic thermohaline circulation in response to the TP removal, Fallah et al. (2016) emphasized the role of reduced northward ocean heat transport from the tropical Atlantic, while Su et al. (2018) stressed the critical role of northward atmospheric moisture transport over the North Atlantic. The discrepancies in mechanisms suggest the complexity of the TP in affecting global ocean circulations. More sensitivity experiments using different models are needed. Of course, the significance of our model results presented here also needs to be validated in other coupled models.

3. Significance

Tell readers which changes are significant, not just the reviewers - masking on the figures would be a good way to do this, particularly given your focus on remote changes, which tend to be smaller. If you've done this, then tell the reader.

Responses: Thank you very much for this suggestion. In the revision, we added an appendix showing the readers that the changes are significant (Fig. 13 in the text). Please also refer to Fig. R1 below.

The changes of variables in this study are significant in both transient stage and quasi-equilibrium stage. We carried out the Mann-Kendall trend test for all figures related to changes. Fig. R1 shows the SST, SSS and SSD changes during in the transient stage (Stage-I: data averaged over years 10-50) with significance levels. Stippling indicates changes of all these variables exceeding the 95% significance level in most regions according to the Mann-Kendall trend test.

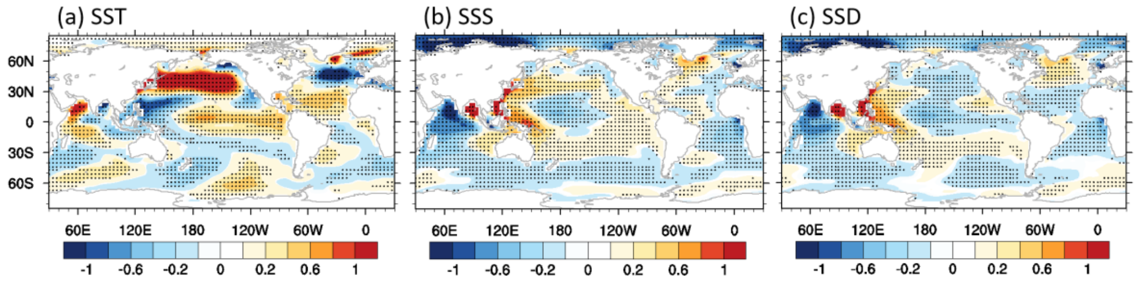

Fig. R1 Changes in (a) SST (°C), (b) SSS (psu) and (c) SSD (kg/m^3) in Stage-I of NoTibet. Stippling indicates changes exceeding the 95% significance level according to the Mann-Kendall trend test.

Other comments:

1. *Introduction: Lines 49-67. There is a lot about decadal and interdecadal oscillations, yet you don't look at the effect of the TP on these. I don't understand why this paragraph is here.*

Response: Thank you very much for this comment. We deleted these lines.

2. *Section 2: Did you also change the subgrid-scale orographic height variability parameters (SGH and SGH30)? If so, what to? If not, discuss this choice and how it may impact results.*

Response: Yes. SGH and SGH30 are set to zero in the TP region where the topography is flat.

3. *Section 3: I remain confused by this section and what it adds beyond showing the surface height that you have in Figure 1. By looking at the difference in surface air temperature you are not looking directly at a thermal forcing of the system - a large part of this signal is just the different height in the atmosphere, so you are mostly just showing us the change in surface height. This is relevant in terms of the atmosphere radiating to space, and thus outgoing longwave radiation, but that doesn't seem to be your focus here. If you really want to show temperature differences, consider potential temperature instead? Similarly for surface pressure - try to separate out the 'forcing' from just the simple change in height (which we see in figure 1)*

Responses: Thank you very much for this suggestion. To avoid confusion, we simply deleted this section.

This section added more info (beyond Fig. 1), namely, we can *see the exact changes* in local surface temperature and pressure. Of course they are all caused by the height change. This is obvious, and there is nothing new or special. In the old version of the manuscript, we kept this section, because Fig. 1 did not tell us what the exact forcing was.

Deleting this section makes the manuscript more succinct. In later sections (Figs. 4 and 7), we mention that the local changes in the TP region are mostly caused by the local height change.

4. *Section 4.1: Can you give a physical explanation for the cloud changes and therefore discuss how model dependent you think your overall radiation results will be? Can you use results from previous studies to get a sense of this?*

Responses: Thank you very much for this suggestion. Both the low and high clouds over the TP region are decreased by about 30%, which are simply due to the strong anomalous high over the TP region because of the lowered topography. The anomalous high is more than 400 hPa, which was shown in the old Fig. 2. This is the reason we kept Fig. 2 in the previous version. Anomalous high restrains both air mass convection and moisture convergence, which reduce the formation of clouds. The physical explanation for the cloud change has been added in section 3.1 (see lines 179-183 of the revision).

The dynamics between the higher pressure and less clouds is straightforward. The dynamics is so fundamental that it should be less model-dependent. Otherwise the model is untrustworthy. However, the magnitude of cloud change in response to a 400-hPa anomalous high can be model-dependent.

We think the signs and patterns of the radiation changes shown in Fig. 3 are reliable, since they are consistent with each other, and are also consistent with SST and SAT changes shown in Figs. 6 and 9. However, the magnitude of radiation changes can be largely model-dependent. This is actually a very complex problem, since in different models the cloud physics and model sensitivity to it, and the radiation scheme and model sensitivity to it can be very different. We are sorry we cannot answer this question satisfactory now.

There are so many studies on clouds using so many different models. Recent studies suggested that the magnitude of changes in clouds and radiation, and thus the climate feedbacks, depends on surface warming pattern (e.g., Zhou et al., 2016). The magnitude of decadal cloud feedback deviates from that of the long-term cloud feedback due to decadal variation in the spatial pattern of SST anomalies. On the one hand, cloud feedbacks depend on the spatial pattern of SST warming, which varies on decadal timescale. On the other hand, feedbacks of clouds on climate change strongly influence the magnitude of SST anomalies (Zhou et al., 2016). Climate model simulations show that the global-mean cloud feedback in response to decadal temperature fluctuation varies dramatically due to time variation in the spatial pattern of SST anomalies. Cloud anomalies associated with these patterns significantly modify the Earth's energy budget. Zhou et al. (2016) showed that the decadal cloud feedback between the 1980s and 2000s is substantially more negative than the long-term cloud feedback. This is a result of cooling in the tropical regions where air descends, relative to warming in the tropical ascent regions, which strengthens low-level atmospheric stability. Under these conditions, low-level cloud cover and its reflection of solar radiation increase, despite an increase in global-mean surface temperature.

The SST-pattern-induced low-cloud anomalies could have contributed to the period of reduced warming between 1998 and 2013 (i.e., the global warming hiatus), and they offer a physical explanation for why climate sensitivities estimated from recently observed trends are probably biasedly low.

Zhou, C., M. D. Zelinka, and S. A. Klein, 2016: Impact of decadal cloud variations on the Earth's energy budget. *Nature Geoscience*, **9**, 871-875, doi: 10.1038/NGEO2828.

5. Section 4.1: Line 174. Section x.x??

Response: Sorry for being careless. It is section 3.2 in this revision.

6. Section 4.1: Line 198-200. '1' in the figure references for figure 4?

Response: Sorry, I am not sure what this is about.

7. Section 4.2: Figure 5: are you including the region over which you've changed the surface height? This should probably be excluded, and if it is, state this explicitly.

Response: Thank you very much for this suggestion. Figure 5 is replotted (now is Fig. 4), and the TP region is excluded. In both the text (section 3.2, lines 189-192) and figure caption, we stated this explicitly. Please also refer to Fig. R2 below.

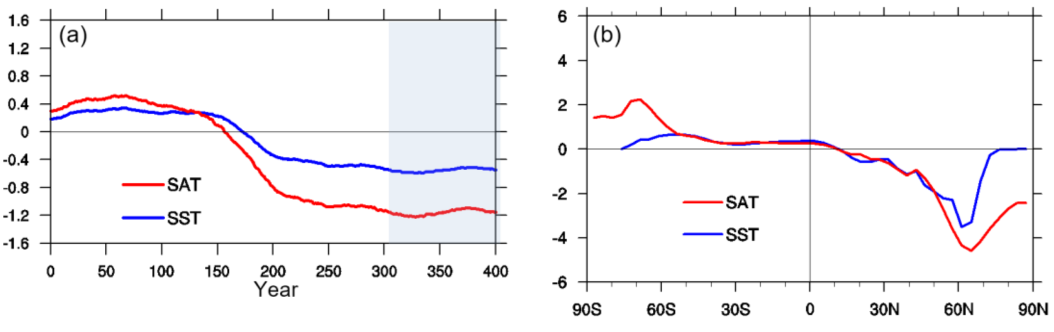

Figure R2 Changes in SST and SAT due to the TP removal. (a) Temporal evolutions of SST (blue; °C) and SAT (red; °C) changes averaged over the Northern Hemisphere (NH), and (b) time and zonal mean changes in SAT (red) and SST (blue). The TP region is excluded when calculating the mean NH SAT and zonal-mean SAT.

8. *Section 4.2: Line 210-218: more details of WHY the NH warms (if you are excluding the region of higher surface temperature; if you are not, that's not really a warming, just an artefact of how you are analyzing the data)*

Response: Thank you very much for this suggestion. We have excluded the local warming over the TP region. The NH warming in the beginning is meaningful, which is simply caused by the strong local heating over the TP region. The local SAT is increased by as much as 20°C due to the lowered topography (Fig. 6a), which acts as a heating source to the global atmosphere. We added more details in lines 209-212 of the revision.

9. *Section 4.2: Lines 220-224. What are these numbers from? Zonal mean?*

Responses: These number are obtained from zonal mean. The local SAT change over the TP region is also excluded.

10. *Section 4.2: Lines 236-240 Also seen in White et al. 2018 (GRL)*

Response: Thank you very much for this suggestion. We added White et al. (2018) here.

11. *Section 5: This section still focuses a lot on the AMOC changes, which are said to be press in other papers, and are also studied in Su et al. 2018 (which should be cited and discussed).*

Responses: Thank you very much for this suggestion. We added Su et al. (2018) in the revision

Because the cooling in the NH, particularly the strong cooling in the North Atlantic, is caused by the weakening of the AMOC, we have to talk about the AMOC. We cannot avoid this point. However, why the AMOC is changed is not the focus of this section. In our follow-up paper ("*Investigating the Role of the Tibetan Plateau in the Formation of Atlantic Meridional Overturning Circulation*"), detailed dynamic analyses are presented.

Although the phenomena of NH cooling and the AMOC shutdown are also found in Su et al. (2018), the physical explanation for these changes is different in our work from that in Su et al. (2018). We must mention that Su et al. (2018) used the same CESM model (but different version) as ours. Su et al. (2018) did not show the initial strengthening of the AMOC, and they thought that the northward moisture transport over the North Atlantic is critical for shutting down the AMOC. Our study shows an initial strengthening, followed by a decline of the AMOC in response to the TP removal. Moreover, we emphasize that the atmospheric moisture relocation from the tropical Pacific to the North Atlantic is the key that triggers the weakening of the AMOC, and the positive feedback between the southward expansion of sea ice and AMOC leads to the AMOC shutdown. These mechanisms are remarkably different from those in previous studies. The discrepancies in mechanisms suggest the complexity of TP's role in affecting global ocean circulations. More sensitivity experiments using different models are needed.

In this section, we cannot discuss too much about the mechanism of AMOC change, to avoid too much focus on the AMOC.

12. Section 5: Lines 375-378, consistent with Su et al. 2018. Does your explanation differ from that of Su et al.?

Responses: Thank you very much for this suggestion. Here, we mean the phenomena of NH cooling and the AMOC shutdown are consistent with those found in Su et al. (2018). However, our explanation differs remarkable from that of Su et al. (2018). Please also refer to our reply to the previous question.

Replies to Reviewer #3:

Thank you very much for your comments. We have revised the manuscript carefully again, based on your suggestions. Our point-to-point replies are given below.

The authors have considered my concerns to some extent. But some questions still need to answered.

1. *Some figures and more statements related to the proxies may be reasonably added in the text.*

Responses: Thank you very much for this suggestion. We are very sorry that we did not add figures related to the proxies in this revision, since we have not analyzed any proxy data ourselves. Our results from model experiments, however, can be used to explain the findings deduced from the proxies.

In section 1 (see lines 60-64 of the revision), we added more statements related to the proxies: “The accumulated dust over the Pacific could have resulted in more precipitation and low clouds, by providing more nucleus of condensation to the atmosphere, leading to freshening and cooling of the North Pacific (Rea et al., 1998). Paleoclimatic evidence shows a possible strong North Pacific deep-water (NPDW) formation between 70 – 30 Ma and a diminishing NPDW since 30 Ma. The NPDW nearly disappeared since 10 Ma (Ferreira et al., 2018, and references therein), whose timing was consistent with that of the rapid TP uplift.”

2. *"Different from previous studies focusing on the TP effect on East Asia (Zhao et al., 2009, 2011, 2012)" and "We have read these papers and cited their works in this version. Zhao et al. (2011, 2012) focused more on the TP effect on East Asia, and on atmosphere circulation." are incorrect. These references (including other references, such as Zhao et al., 2018, 2019) are mainly for the effects of the Asian land or the Tibetan Plateau on the Pacific and Atlantic atmospheric circulation and SST (also including North American climate). Please you check them. These references have revealed some mechanisms for the effect of the Asian land or the Tibetan Plateau on the atmospheric circulation and SST over the Pacific and Atlantic and North American climate. The related statements in the text should be revised.*

Responses: Thank you very much for pointing out this mistake. In section 3, lines 317-323, we refer to their work as follows: “The teleconnection patterns shown in Fig. 7 agree well with those in previous studies (Zhao et al., 2007, 2009, 2011, 2012). The mechanism of teleconnection between the TP and atmosphere circulations over the North Pacific and North Atlantic has been studied in atmospheric general circulation models (e.g., Zhao et al., 2007, 2009, 2012). The similar teleconnection pattern in our coupled climate model to those in atmosphere models (Zhao et al., 2007, 2009, 2012) suggests that this teleconnection is established mainly via atmospheric processes (Chiang and Bitz, 2005; Broccoli et al., 2006; Zhao et al., 2012; Liu et al., 2013).”

In section 5, lines 605-614, we refer to their work as follows: “How much more will the TP be warmed? How will the future change around the TP feed back to the global warming? These imperative questions have drawn considerable attention from Chinese scientists, and the Third Tibetan Plateau Atmospheric Scientific Experiment (TIPEX-III) has begun formally in 2014 and will continue for 8-10 years (Zhao et al., 2018). An in-depth investigation on these questions will greatly enhance our ability to cope with the climate change in Asia and the world (Zhao et al., 2018, 2019). This work is only our first attempt to answer these questions, based on our model experiments. More investigations on the impact of future change around the TP on the global climate, using different coupled models and through various sensitivity experiments, and combining with observations from TIPEX-III, are urgently needed."

3. *About the TP-MP，the authors should add some statements in the text, such as the TP in this text includes the MP.*

Response: Thank you very much for the suggestion. In section 2, line 147-153 of the revision, we added "In addition, we would like to mention that the TP region specified in this work also includes the Mongolian Plateau (MP) (Figs. 1b, d). Some studies found that the MP, despite its smaller size, exert a great influence on the wintertime subtropical westerly jet (Shi et al., 2015; White et al., 2017), and play a significant role in strengthening the East Asian winter monsoon (Sha et al., 2015). For the summer monsoon circulation over the Euro-Asian region, they all stated that the TP plays a much more important role. In this work we did not separate the roles of the TP and MP."

In section 5, line 554-572, we also have detailed discussions on the effects of the TP and MP.