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Climate of the Past Discussions

Interactive comment on "Distinct responses of East Asian summer and winter monsoons to orbital forcing" *by* Z. Shi et al.

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We thank Dr. A. L. Berger and the anonymous referee for their constructive comments. A detailed response to the issues raised by Dr. A. L. Berger is given below.

Zhengguo Shi

On behalf of all authors

By comparing both geological data and simulated results with obliquity and precession, the authors show that the East Asian summer monsoon is mainly dominated by precession signal as many previous studies have shown, but the winter monsoon responds more to obliquity, a result claimed to be "not mentioned before". The finding that the East Asian summer monsoon and winter monsoon responding differently to precession and obliquity is very interesting.

Re: Thanks for the referee's comments.

General Remarks: 1. I advise to replace everywhere "orbital" by "astronomical" because obliquity is related to the rotation of the Earth about its axis which is more than orbital. I know it is a common mistake that I also do sometimes.

Re: Thanks. All "orbital" is now changed to "astronomical".

2. section 3.1 gives a good description of the geological records and the climate model responses for the EA summer and winter monsoons. Section 3.2 attempts to give a description of the difference between situations where NH summer occurs at perihelion (mimimum precession or maximum NH summer insolation) and similar situations where NH summer occurs at aphelion (maximum precession or minimum NH summer insolation). You also give a description of the difference between the situations related to maximum obliquity (or maximum insolation in high latitudes of the summer hemispheres) and situations where obliquity is minimum. In both cases, the selection of these particular astronomical situations does not prevent to have both precession and obliquity acting on the climate model system through daily insolation. This makes the interpretation of figures4-7 difficult because they all contain responses to both precession and obliquity. How is the average made over the 12 precession cycles or the 7 obliquity cycles eliminates/dampens respectively the obliquity or precession influence? How to explain that a high obliquity vs a low one generates cool summer in northern Asian between 80E-160E (Fig4b) and also cool winter (Fig4d) of the same magnitude? Fig 4a and 4c related to precession situation are easier to explain as a direct effect of precession itself. I do not know any insolation parameter having a behavior like figures 4b and 4d along the latitudes. This let me conclude that obliquity might play a minor role or that strong feedback mechanisms are acting. Please clarify.

Re: Thanks for the comments. First, we have to apologize for our mistake in the Figures 4-7(b), which confused the referee. The original figures show the TH-TL differences in

JJA climatology but actually they are calculated by TL-TH. Thus, a reverse pattern is indicated although it does not affect our final conclusions on different monsoon cycles. In the revised manuscript, we have redrawn the figures and revised the statements. In the revised figures, we now get the right response of surface temperature in obliquity composites (Fig. 1bd, summer warming and winter cooling in NH). But we have to admit that the composites can not purely represent the behaviors on the obliquity band because we can not totally exclude the precession impact. However, the composite analyses is still one of the most effective ways to appeal the general behaviors on specific period and it has been widely used in previous studies (e.g., Kutzbach et al., 2007; Chen et al., 2010). Further, the insolation changes in both boreal summer and winter due to obliquity are always showing more significant responses in high-latitudes than in low-latitudes (Fig. 1ac), which is similar with our temperatures (Fig. 1bd). This different feature from precession might indicate that the composites in our study can show the "direct" response of surface temperature to obliquity and the precession impact might be smaller. Thus, we still preserve our composite analyses in the manuscript but modified some relevant discussions (e.g., using the obliquity composites instead of the response to obliquity; not detailed shown here) in order to avoid the possible overstating. Further, we add some statement (P950, L2) to emphasize this point.

"Here we should note that the composites can not purely represent the behaviors of temperature responding to one astronomical forcing since we can actually not totally exclude the impact from the other."

3. It is generally accepted and recalculated many times that: (1) daily insolation is primarily a function of precession with the obliquity signal being more important in high than in low latitudes. It is forgotten that obliquity remains however less important than the precession signal in high latitudes. (2) the total irradiation over a period of time during the year (seasons) is only a function of obliquity but this is only true for noncalendar period of time (Berger et al 2010, QSR). However, such a primary and different role played by precession vs obliquity was first calculated and discussed in length in Berger

(1978, Long term variations of caloric insolation resulting from the Earth orbital elements, Quaternary Research, 9, 139-167. in particular pages 142-160). It was as well summarized in Berger (1988, Milankovitch theory and climates, Review Geophysics, 26(4), 624-657) where it is also clearly stated that “the latitudinal gradient of extraterrestrial insolation is characterized by a periodicity of 40 ka whereas for the absorbed insolation it exhibits a higher frequency which corresponds to a period in the range of 21 ka”. For a full study about the latitudinal gradient, see Tricot and Berger (1988, Sensitivity of present day climate to astronomical forcing, in ‘Wanner H and Siegenthaler U (eds), Lecture notes in Earth science, vol 16, Springer Verlag, 132-152). How do you take into account this difference between extraterrestrial insolation and insolation available at the surface in your conclusions? Please refer to these publications where you speak about the influence of precession and obliquity in daily insolation and in latitudinal gradient.

Re: Thanks. First, we have referred to these papers when we introduced the effect of astronomical parameters on the insolation (P945, L18).

“Owing to its dominant role in the daily insolation variation (Berger, 1978; Berger, 1988), precession has been widely emphasized as the key factor in the monsoon evolution (Clemens et al. 2003; Wang et al., 2008) and "less-important" obliquity is often neglected. However, obliquity can control the total irradiation over a period of time during the year (Berger et al. 2010) and large-scale meridional gradient of insolation in the summer hemisphere (Tricot and Berger, 1988). Thus, how the astronomical parameters affect the EAM, especially the relative importance on these two bands, still needs to be clarified.”

Second, we have reexamined our results on the question of the difference between extraterrestrial and surface insolation. Since the insolation/radiation budget is not available in our model outputs, we can not directly focus on it. However, we have calculated the meridional surface temperature gradient (Fig. 2), and found that the response of temperature gradient (the origin of meridional curculation) is generally showing a remarkable cycle of 40Ka in both summer and winter hemispheres. This is similar with the extraterrestrial insolation (Fig. 3a) but quite different from the response of insolation at the surface (20Ka cycle). Thus, we propose that the temperature gradient is more likely controlled by the extraterrestrial insolation (not only via the absorbed shortwave radiation but blackbody longwaves) not merely the surface insolation, although the mechanism how they actually affect the temperature is not very clear (and also not our focus).

4. Please comment about the FOAM model in particular its resolution and the possible bias induced by the accelerated technique in an atmosphere-ocean coupled system.

Re: Thanks. The model and the accelerated technique are detailed introduced and discussed in the Kutzbach et al 2008 paper. Thus, we only add some necessary description in the revised text (P947, L6, 13).

"FOAM is a fully coupled global ocean-atmosphere model, with a horizontal resolution of about 4 latitude by 7.5 longitude for atmosphere and 1.4 latitude by 2.8 longitude for ocean."

"We restrict our focus on the atmosphere-upper ocean system. Since the response time of this system is much faster (about 1000 times) than the precession cycle, it allows us to extract the astronomical-scale changes from interannual variability in the model."

5. The Asian summer low is used as a summer monsoon index. Why not use the land/ocean pressure contrast as claimed to be at the origin of the summer monsoon in the first page.

Re: The pressure contrast is also all right. We used the Asian summer low as summer monsoon index since it is simple and on continent. These choices do not affect our proposals.

6. Very few comments are made about the feedbacks. For example, as the winter

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monsoon is driven by the Siberian high, the remnant effect of summer insolation might be very important for explaining the response of the high latitudes in winter during which the energy is quite low (Yin and Berger, 2011, Climate Dynamics).

Re: Thanks for the comment. As the reviewer said, the remnant effect of summer insolation might be important for high-latitude climate, including the Siberian high (and Asian winter monsoon). However, how it affects on the distinct monsoon cycles and whether the effect is significant are still unknown. It is also beyond the purpose of our paper and we do not have the results for potential feedbacks (e.g., sea ice, vegetation). Therefore, we have just simply mentioned the potential importance of such feedbacks in the revised text (P952, L11).

"Various climatic processes, e.g., vegetation (Tuenter et al., 2005) and sea ice (Yin and Berger, 2011), might also impose their potential feedbacks, not discussed in this study, on the direct impacts of astronomical forcings on the monsoon evolution."

Other remarks: Page 945, lines 7-8: The partition of insolation and ice sheet influence on the East Asian monsoon is discussed in Yin et al 2009 (Climate of the Past).

Re: We add this reference in the revised manuscript.

Page 945, line 18: please specify "owing to its dominant role in the DAILY insolation (Berger, 1978, Quaternary Research), : : :"

Re: Thanks. We add "Daily" and this reference.

Page 947, line 2: "for our purpose, : : :", please remind your purpose which I assume is only looking in the precession and obliquity bands because in your model the ice sheets are prescribed and therefore it is not expected to see the 100 ka cycle.

Re: Yes. We are just to evaluate the relative importance of precession and obliquity in the Asian monsoon evolution. And in the revised text we have emphasized this point (P947, L2).

“To better explore the relative importance of precession and obliquity in the monsoon evolution, the 100-ka eccentricity component (greater than 67-ka) has already been filtered out.”

Page 947, line 18: “: : :.The calendar effect: : :.”, please explain this effect because what you give is the origin of this effect (the length of the seasons).

Re: Thanks. We mean that we used the modern calendar (that is, summer equals JJA) to analyze the simulation results which does not take into account the changes in length of months and seasons. The calendar effect is certainly related to the definition of season and has been discussed in the Joussaume and Braconnot (1997). We have explained this in the revised text (P947, L16).

“Modelling results are analyzed using the modern calendar, thus, the “calendar effect”, which means changes in length of months and seasons that occur with the changing season of perihelion (Joussaume and Braconnot, 1997), is not taken into account in the current study.”

Page 948, bottom: please comment on the phase relationship between the geological data and the simulated pressure which might not reflect the reality because only time-dependent insolation was used to drive the model (no greenhouse gas, no ice sheet).

Re: Thanks. The phase lead of pressure system to the proxy, as we have mentioned in the text, is just to show that among various monsoon index/proxies there are also some phase difference (but maybe not remarkable). For example, our pressure responds earlier to the solar radiation than the precipitation, which the stalagmite delta O18 represents, although all of them can be considered as supportive to Kutzbach’s hypothesis. As the reviewer said, the simulated pressure can not reflect the reality and it can not be perfectly compared with the proxy data. From this view, the phase difference might be lack of significance. In addition, the phase relationship is indeed not our focus of this paper. Thus, we delete the sentences in order to avoid the misunderstanding.

Page 951-952: in section 4, mechanisms are looking for to explain the dominant obliquity signal in winter monsoon. I have nothing against using the latitudinal gradient (meridional insolation difference) but what about the total irradiation received during a given season. In winter, there is very low energy in high latitudes, therefore the daily insolation might play a less important role as compared to the accumulated energy during a given season which is function of obliquity only (Berger et al 2010). Can you comment on the advantage of using the latitudinal gradient instead of the total energy over a given season? Is your model providing results in favor of one or the other? In section 4, the physical processes suggested by the authors need to be justified using their model results.

Re: Yes. Obliquity controls not only the meridional gradient but also the total irradiation during a given period/season. The reasons we used "meridional gradient" to explain the 40ka signal in the winter monsoon are in the following: (1) In Berger et al 2010 paper (Fig 5 and 6), the given period is chosen as λ=60° to λ=120° or annual (λ=0° to λ=360°). The period is long enough to counteract the precession signals, thus presenting an obliquity cycle. However, if the period is chosen as monthly (e.g., July), the mean insolation/irradiation is still controlled by precession. In our transient simulation, the monsoon index (Siberian High) in DJF (used in our paper) is consistent with that in a single month (January), characterized by a more remarkable 40 ka cycle. Thus, it may indicate that the 40ka signal in monsoon index is more likely controlled by meridional gradient rather than accumulated energy. (2) Furthermore, our simulation results can also support the proposed mechanism. A significant intensification of the global meridional circulation in boreal winter is observed in the TH composite than in the TL composite (Fig.4). Hence, we prefer to use the meridional gradient to explain the obliquity-dominated winter monsoon. And we add the figure and relevant discussions in the revised text (P952, L6).

"In the modeling results, we can also observe a significant response of meridional circulation in the obliquity composites (Fig. 9). In the TH composite, the cross-equatorial

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circulation with a downward airstream over Siberian region is intensified than the TL composite, supporting that our explanations should be reasonable."

In Fig 8: Although I find back the insolation values of figure 8 for 30N June and 20S December using Berger (1978, JAS), I can not find your values for 50N December insolation. Please give the reference of the paper used for calculating the insolation values (same remark holds for obliquity and precession used in figures 2 and 3). More importantly, why do you use the ratio between 20S and 50N instead of the latitudinal gradient (20S minus 50N)? In the first case, you get an obliquity signal as in fig8 blue curve, but in the second case you would get a precessional signal. Please comment.

Re: We have got the astronomical parameters (precession and obliquity) from the website http://aom.giss.nasa.gov/srorbpar.html, which is generally based on Berger et al. (1978). The specific insolation is calculated from our simulation results (In the model, the insolation is calculated by your method too). However, these calculations might be not accurate and not very appropriate, as the reviewer said. It is mainly due to the relative low resolution of the model. Following your comment, we have redrawn the figure directly using the insolation from Berger et al. 1978 and cited the reference. We note that these changes do not affect our discussion. Additionally, the reason why we used a ratio is that it can well show the role of obliquity on insolation (to control the meridional thermal contrast). Please see the response to General Remark 3 for information. From Figure 2 and 3, we can see that the meridional temperature gradients (the origin of meridional circulation) are showing a remarkable obliquity cycle in both summer and winter hemispheres, more consistent with the ratio. Furthermore, we believe that the thermal contrast in the summer hemisphere might be more important in controlling the intensity of meridional circulation. We have nothing against the minus insolation, but from our views the ratio is certainly preferred.

Interactive comment on Clim. Past Discuss., 7, 943, 2011.

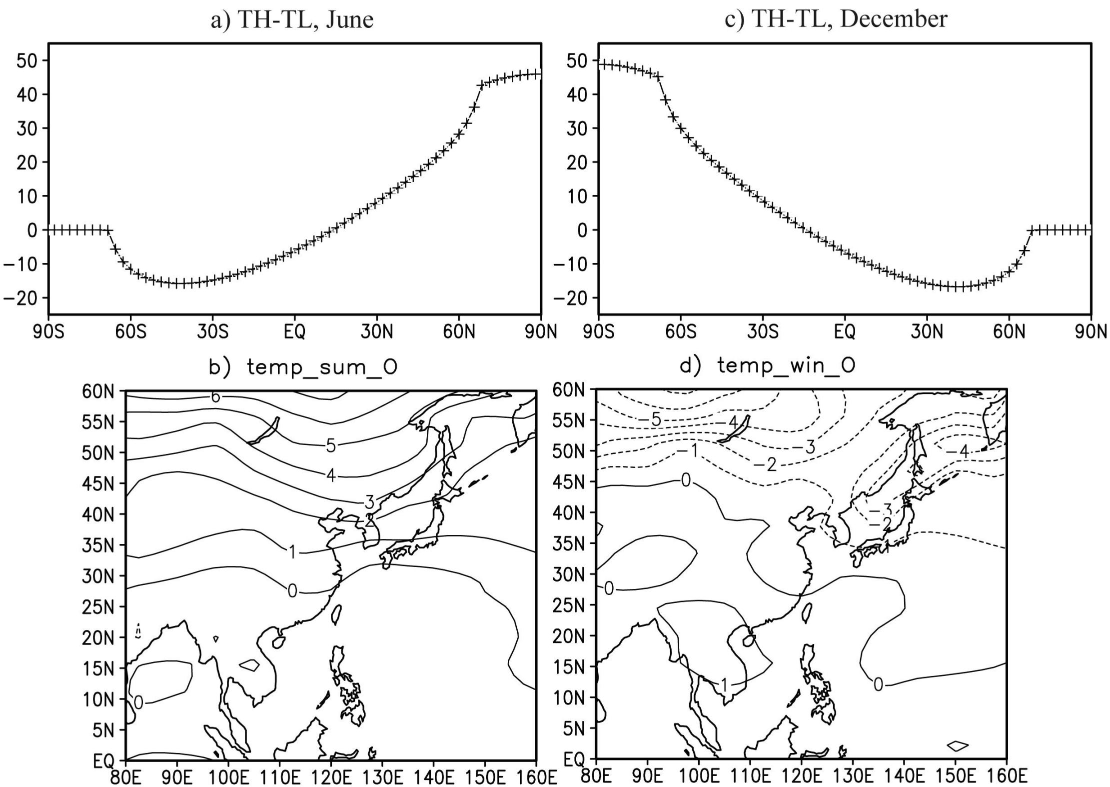

Fig. 1. Insolation changes in June (a) and December (c) in TH-TL scenario (TH: 24.4°, TL:22.2°; in both cases, precession is kept as present) and simulated temperature differences (b,d) in obliquity composite

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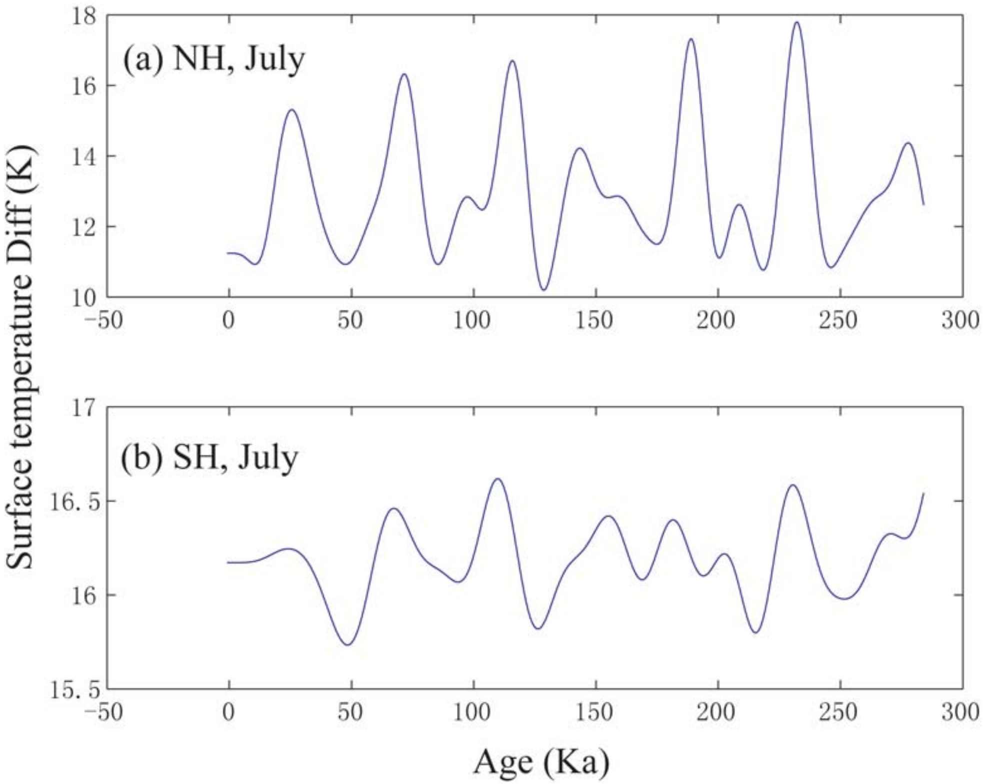

Fig. 2. Simulated July meridional temperature gradients in two hemispheres (between 30 and 60 degrees) during the last 280ka

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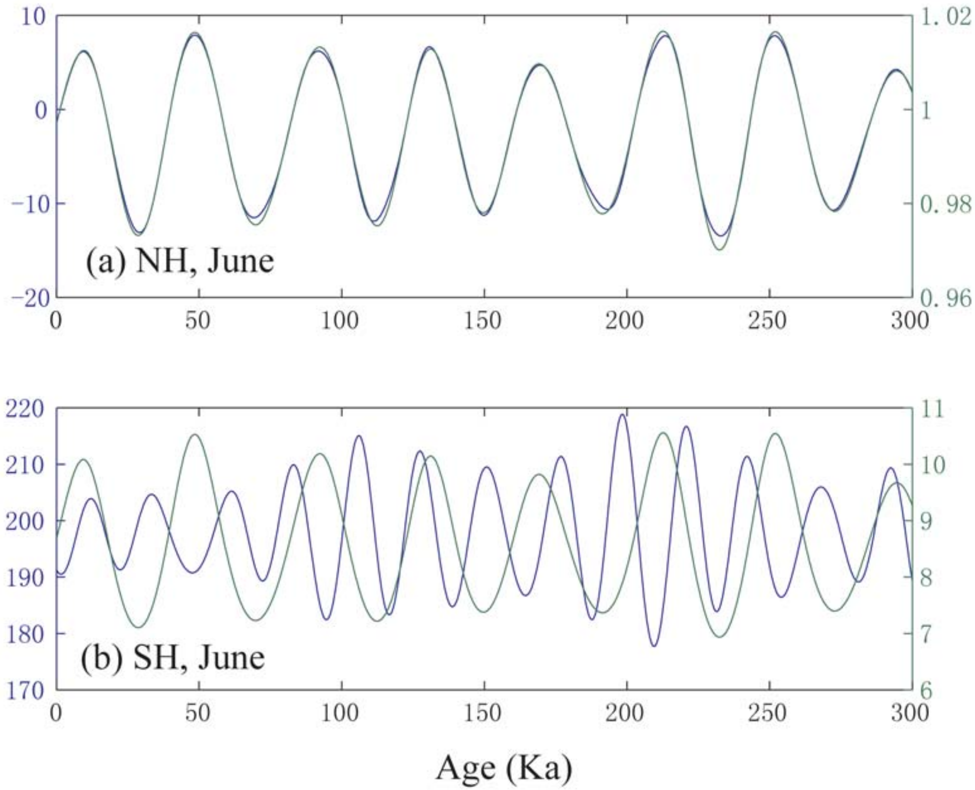

Fig. 3. Referenced insolation gradients (minus: blue; ratio: green) in two hemispheres (between 30 and 60 degrees)

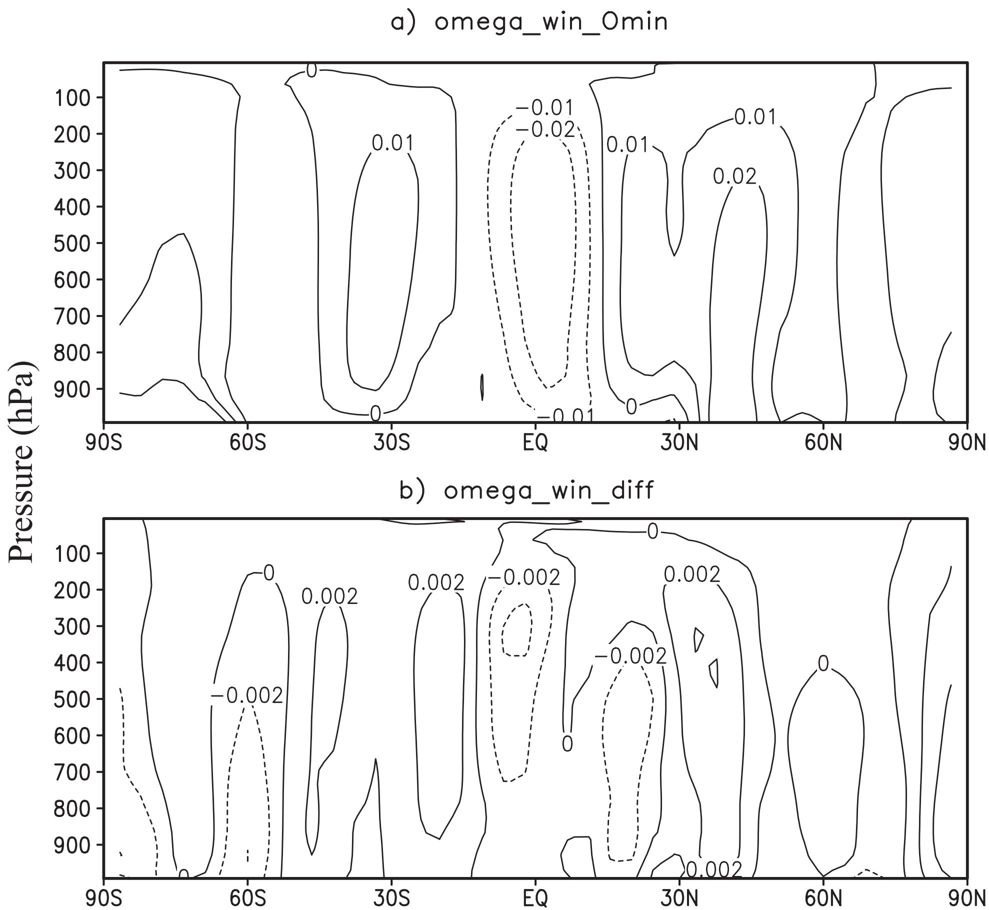

Fig. 4. The TL composite of boreal winter vertical velocity (Pa/s) averaged for 80-150°E (a) and the difference between TH and TL (b).

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