Response Referee #1

We are not sure where the confusion arose about specifying full boundary conditions appropriate to each time slice. This naturally includes precession, obliquity, and eccentricity, as is clearly delineated in [*Berger and Loutre*, 1991]. Nowhere is it mentioned or implied that only precessional changes were used. We chose not to include a 'figure' to describe the seasonal changes in insolation as this may be easily calculated from the published literature.

The reviewer may not be aware that the authors, and this model, GISS ModelE-R, are long time participants in the PMIP program. This version of the model (0k and 6k experiments) is indeed a part of the PMIP2, and there is a multitude of literature that has arisen as part of PMIP related study and research that puts this model into the PMIP context. This program is an excellent addition to study of past climates. However, this paper is not attempting to be a single replication of this program. This paper is attempting to address the use of water isotopologues as measures of past climate conditions. Water isotopes are not part of the PMIP program. For further information on this model (including 6k and 0k time slices) and the PMIP program please refer to http://pmip2.lsce.ipsl.fr/ In addition to data-proxy comparisons in these papers, several other papers have already made much use of isotope- GISS model comparisons for the Holocene : [*Carlson et al.*, 2008; *Oppo et al.*, 2007; *Schmidt et al.*, 2007]. The paper already discusses ITCZ shifts in section 3.1. If the reviewer is interested specifically in how PMIP2 models (including this one) mid-Holocene simulations perform with ITCZ shifts in Africa compared to data, [*Wohlfahrt et al.*, 2008] (in Climate Dynamics) and [*Zhao et al.*, 2007] (CD) include thorough discussions. Also, the ITCZ tracks the warmest SSTs, so the northward incursion is naturally in JJA – it is not clear what the reviewers point is here.

The reason that much of the paper compares 6k to 0k or 9k to 0k is that the patterns observed in the former are generally applicable to all the other (1k,2k,3k,4k,5k) experiments, except that their magnitudes are larger (i.e., page 1138, lines 15-24). The 9k expt. has somewhat different patterns since it also includes orographic and sea level changes. The utility of so many experiments is included in the discussion section. Briefly, though, the changes are largely 'linear'. In order to regress climate versus water isotope changes, we include 8 time slices since this yields 8 'points', making the regressions more meaningful. Figures 5, 6, and 7 (and text discussion) highlight how these other time slices respond to climate.

Prescribing SST changes is unsatisfactory because it forces the entire model to come into line with those conditions. For instance, you could have a '0k' experiment where you impose SSTs reconstructed from 21k – this would yield global cooling much more in line with the 21k world than today (with the exception of the orographic changes of course). If the reviewer is interested in the difference between fixed surface d18Osw and real d18Osw variability in the modern on d18Opr, please see [*LeGrande and Schmidt*, 2006] – differences are up to +/- 1.5 permil.

The authors will have another paper (Schmidt and LeGrande, In Prep) that discusses d18Op changes across Asia in much greater detail (including the seasonality). The three speleothem records chosen here are from two widely situated places in China and Oman. The point here is that these three sites, separated by 1000s of km agree on d18Oc changes on the 1000 year timescale – and that the model agrees with them as well. It is clear from Figure 2 that the correlation between d18Opr and PR across this region is poor.

The reason that the discussion of the Bering Strait flux is limited is that the sensitivity experiment (closing the Strait) is clearly just a 'sensitivity' experiment, with the actual opening of the Bering Strait occurring several thousand years prior to 9 kya. The authors do plan to expand on this idea in future experiments with orographic, GHG, and orbital forcings appropriate to the timing of the termination of the Younger Dryas in future experiments.

The deuterium excess changes in these simulations are not particularly dramatic or compelling. Even in the observations there is a lack of strong correlation between GRIP and North GRIP deuterium excess changes [*Masson-Delmotte et al.*, 2005], making it unlikely that these records primarily recording source water changes. Also, in this study, for instance, despite clear changes in 9kyr precipitation source for Greenland (through the LIS induced southward shift in westerly jets), the changes in deuterium excess were of both signs and highly variable over Greenland,. Further, previous studies also note the importance of other effects besides source on d-xs [*Kavanaugh and Cuffey*, 2002; *Schmidt et al.*, 2007]. Further, deuterium excess is strongly influenced kinetic effects – those parts of the isotope code that are most highly parameterized and uncertain [*Cappa et al.*, 2003*; Boaz Luz, pers. comm.*]. We intentionally left out any discussion of deuterium excess in the original draft for these reasons; however we have now added the following paragraph:

> Deuterium excess changes are more complicated. The 1k through 6k time slices have small to no change over Greenland in the 1K through 6K cases, and small changes of both sign in the 9K case. In Antarctica, deuterium excess is about 1‰ less over much of the continent by 9K, consistent with ice core evidence [*Vimeux et al.*, 2001]. As in figures 2 and 3, EH sea surface temperatures are largely (annual mean) cooler around Greenland, particularly in winter, due to lower NADW production. This region, particularly at 9K represents the bulk of the original source of precipitation for Greenland (due to 'blocking' from the remnant Laurentide of the Pacific source). Neither cooling, nor general changes in source region, translate into a consistent change in deuterium excess over Greenland. Previous studies have also noted a lack of correlation between source region changes and deuterium excess changes [Kavanaugh and Cuffey, 2002]. However, it should be noted that very little is known about mean changes to ocean water deuterium (even less than oxygen-18) at various time slices through the deglacial. Further, deuterium excess is the most sensitive of all the forward modeled water isotopes to parameterization and kinetic effects, and thus the 9K mean results (not pattern) should be used cautiously.

In other work, explicit model tracers are being used to track water vapor and precipitation source globally and we intend to look more closely at the interesting comparison between these tracers and deuterium excess, but this is outside the scope of the present manuscript.

The ice core community does realize that there is no single 'translation' of d18O to SAT. However, the practical (quantitative) implementation of this information is still lacking. As a consequence, the 0.3 permil/degC slope is still applied in papers, and there are figures that routinely plot 'ice core data' as SAT versus time.

We have added suggested references and clarified short (=direct observational) from long time periods.

We have added this text to the proxy comparison section:

> It should be noted here, however, that though the model can make direct comparisons to precipitation and seawater records of $\delta^{18}O$, some proxy measurements actually measure a slightly more complicated product. For instance, calcite $\delta^{18}O$ also has a temperature dependent fractionation [*Epstein et al.*, 1953]. This dependence is usually accounted for in marine records, but not in speleothem records (temperature variability in caves is thought to be minor compared to the impact of cave water $\delta^{18}O$). Terrestrial records of $\delta^{18}O$ are often complicated since re-evaporation or variable residence times in the ground can alter the $\delta^{18}O$ from that originally in the precipitation. Forward models of these processes are under development for the GISS model.

The words 'gradient' and 'slope' are equivalent.

We have added explanation on the iceberg calving – net accumulation scheme used in the model.

We do not discuss d18O changes in the stratosphere because it is mostly directly calculable from methane changes. (And since there are no proxy records for stratospheric water vapor d18O changes, what would be the point?)

Annual averages in ModelE are always calculated based on total accumulations over the year, and are not influenced by the length of the month or year.

Response Referee #2

The goal of this paper was not to separate the effects of GHG forcing, orbital changes, and orographic changes. The purpose here was to capitalize on the water isotopes and water isotope comparisons. Other groups (Hadley comes to mind) have done an excellent job partitioning out the various contributions of GHG changes, orbital changes, orographic changes, etc., but in order to do so, they ran a suite of 'sensitivity' simulations that took out some of the forcings. Thus, in order for us to complete (fully quantify sensitivity) this type of analysis, each 'time slice'

(1k-6k) would need to be repeated twice, without GHG or orbital in each case, and the 9k simulation would need to be repeated six more times with the orbital, GHG, and orographic changes turned alternately on and off (2^(n params)-2). Thus, this is well beyond the scope of our current study (i.e., we cannot address it without completing an additional 16 experiments X 2+ months of simulation time a piece). There is a 'general' discussion of features partitioning in the first paragraph of the discussion.

I don't think that it is necessary to rewrite the intro sentence into the conclusions. However, to stress these points, I have made sure that the paragraph breaks in the conclusions address each point. (Note that points 1 and 3 are intrinsically linked.) To highlight the point of forward modeling, I add:

> "Regional to hemispheric wide scale changes in water vapor transport are the component most often ignored (or unable to be addressed) in interpretations of individual time series of $\delta^{18}O$ from proxy records. This component is the aspect for which interpretations can be most greatly improved through the forward modeling of $\delta^{18}O$ tracers."

A section has been added to the methods to detail the (very) simple ice module. In order to prevent long-term drift in salinity (and thus make the results highly dependent on integration times), we 'lock' salinity by balancing accumulation over land ice with calving of land ice. This allows the integration to converge on the appropriate 9K 'base' where salinity/ice volume/etc. may be inferred (and here are specified) from sea level reconstructions.

The melting of the Laurentide Ice Sheet does not directly lead to the 8.2 kya event. This event is specifically linked to the abrupt drainage of glacial Lake Agassiz. (Though, both are *ultimately* related to the changes in insolation and general decay of the Laurentide Ice Sheet.) The link here is that the LIS melting suppresses deepwater formation in the Labrador Sea, which turns out is important in mitigating the magnitude and length of the 8.2 kya event [*LeGrande and Schmidt*, 2008]. A paragraph has been added to 3.1 to reflect this.

The 6K and 9K time slices have been constrained against much additional data in [*Oppo et al.*, 2007] and [*Carlson et al.*, 2008], in addition to the comparisons shown in this paper. There are many more potential comparisons, and collaborations with expert paleodata colleagues are underway.

I tried to pull out more ocean sites to make more definitive $\delta^{18}O$ comparisons, but I found that most lacked the temporal resolution required. However, I point out that both the 6K and 9K time slices have already been compared in detail to the proxy record by 'proxy data' researchers in [*Oppo et al.*, 2007] and [*Carlson et al.*, 2008]. For atmospheric records, I intentionally focus on two spots where there is very high resolution chronology: Greenland ice cores and Asian speleothems. A future paper (collaboration) may look at making comparisons to more of the speleothem records.

Most of the specific points have been addressed.

I don't really have control over the size the editors made the figures. Hopefully, the zoom button works for pdf's.

Response Referee #3

This Nature Geoscience paper came out, several months after this paper was up on the Climate of the Past Discussions site and after the other referee comments had been published for over a month. And it post-dates [*Carlson et al.*, 2008] that already discusses the 9K simulations. However, it is pertinent so we now cite it.

We have included data comparisons for the tropics and ice core regions in addition to the Asian monsoon regions. Some previous collaborations, in particular, have thoroughly measured our 9 kyr [*Carlson et al.*, 2008] and 6 kyr [*Oppo et al.*, 2007] slices – see above responses to reviewer as well.

There is not really any paleo water vapor (explicit) data to make comparisons to. In the modern, however, the GISS model matches reasonably well with water vapor and water isotope measurements from the TES instrument on Aura (Jeonghoon Lee, John Worden, others, pers. comm).

As in the response to the above reviewer, deuterium excess is an ambiguous measure of changes in precipitation source according to proxy data, and is not the most robustly modeled element in water isotope enabled simulations. We will explore this issue thoroughly, however, in a future paper that highlights the use of additional model water vapor source tracers.

[*Aleinov and Schmidt*, 2006] describe the water isotopes in the land surface scheme. Yes, it is most certainly a deficiency of the GISS model that we do not have dynamic vegetation. Changes in vegetation, whether from observed syntheses or from calculated offline models such as BIOME4 or LPJ are clearly an important next step.

The remnant Laurentide covered up the Hudson Bay – yes, there are some land-sea mask changes at 9 kyr.

[*Broecker et al.*, 1990] (And others afterwards) hypothesize about changes in the Atlantic-Pacific salt oscillator. But, I am not really arguing for these ITCZ/Atlantic-Pacific water vapor changes, and I am not aware of a specific study that addresses trans-Panama water vapor transport.

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