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Interactive Comment

Interactive comment on "Amplification of obliquity forcing through mean-annual and seasonal atmospheric feedbacks" *by* S.-Y. Lee and C. J. Poulsen

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We thank the reviewers for their comments. We found the reviews to be very constructive and have acted upon most of them. Our revisions and detailed responses to the reviewers' comments are provided below. In sum, our major revisions include: 1) Clarifying the experimental methodology regarding our higrad and lograd experiments. 2) Adding additional analyses and clarification of the wintertime and summertime snowfall response to obliquity. 3) Discussing snow ablation and PDD. 4) Adding additional results pertaining to the snowfall response to precession.

Main comments. [R1-1] Comment about reality of experiments. The coupled ocean-atmosphere runs in this study are intended to be sensitivity experiments. Our goal here

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is to analyze the influence of seasonal and annual forcing on climate, not to simulate any particular time slice in earth history. Thus, we agree completely with R1's statement that "...the 'AM' insolation (Fig 1b) cannot possibly be obtained on a spherical earth." To clarify this point, we have added the following sentence to the Introduction: "To be clear, the insolation conditions in (2) are idealized and provide a useful sensitivity experiment, but, would not have occurred anytime during Earth history."

[R1-2] Comment about experimental setup and insolation during polar night. Our second set of experiments (i.e. the MA case) was designed to estimate the climate response to the mean-annual forcing caused by a decrease in obliquity. This was accomplished by adding mean-annual insolation anomalies from the high and low obliquity to a present-day experiment. In the case of high obliquity, this anomaly is negative. Adding this negative insolation anomaly during polar night causes the incoming insolation to become negative (less than zero), which is unreasonable. To avoid this problem, the minimum insolation was set to 0 Wm-2 during polar night. We reiterate: this only (slightly) affects the winter season (polar night) and winter seasonal meridional insolation gradient, and does not affect the summer insolation or the summer meridional insolation gradient. We have now added this description to Section 2 (paragraph 3), and regret its omission in our original draft.

We discuss the effect of changes in insolation on snowfall in Section 3, and summer snowfall specifically in Section 3.3. About 1/2 of the change in summer snowfall is due to the local insolation change. The other 1/2 is related to changes in the seasonal insolation gradient. R1 states that "...it is not clear that the effect of insolation on snowfall is not purely local, at least in the summer." There are at least four lines of evidence that are consistent with our argument that the summer snowfall response due to seasonal insolation forcing is not purely local: (1) there is an increase in water vapor transport to the region (Fig. 5c), (2) the local decrease in rainfall is $<$ 1/2 of the increase in snowfall (Fig. 4), (3) the increase in relative humidity in TOTAL coincides with the region of enhanced baroclinity (Fig. 5a), and (4) local evaporation decreases (and thus can't be the source of the snowfall).

R1 also indicates that both experiment set-ups lead to latitudinal gradients; this is true, both experimental sets have mean-annual insolation gradients (as we noted in Section 2 and as shown by the zonal-averages in Fig. 1). However, in the MA case, the meridional insolation gradient is nearly constant throughout the seasonal cycle (except as noted above during polar night from 70-90 N). In the TOTAL case, the meridional insolation gradients vary substantially from month to month (Fig. 1). It is worth noting that the snowfall difference between the TOTAL and MA cases are mainly centered at 60-65 N, and extend from 30-90 N (Fig. 2). Thus, the polar night issue can not be the reason for the snowfall differences. This statement is now explicitly stated in Section 2, paragraph 3.

[R1-3] Question about summer snowfall response. As described in Section 3.3, we mainly attribute the increase in summer snowfall to changes in vapor transport. We have added Fig. 4 showing that the decrease in rainfall at ˜60 N cannot completely account for the increase in snowfall. We have also added the following text to Section 3.3: "However, two lines of evidence indicate that the local temperature decrease only partially explains the summer snowfall increase delta-TOTAL. First, the increase in (water-equivalent) snowfall is twice the simulated decrease in rainfall (Fig. 4b-c), indicating the existence of another moisture source. Second, changes in lower troposphere relative humidity do not directly track changes in temperature. In delta-TOTAL, for example, the greatest increases in relative humidity occur in the mid-latitudes near 45°N (Fig. 5a), while insolation and temperature reductions are greatest at high latitudes (Fig. 1a and Fig. 4a)."

[R1-4] Question about winter snowfall response and mechanism. In response to R1's comments, we re-examined the mechanisms responsible for the increase in winter snowfall. During this exercise, we found an error in the units of our former Fig. 3c, upon which we were largely basing our interpretation that low-clouds were responsible for the air-temperature decrease. We have now rewritten Section 3.2 and added several new

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figures (Fig. 3a-f) in support of our analysis.

[R1-5] Request for clarification of text. We have clarified these sections by specifying specific/relative humidity to remove any ambiguity.

[R1-6] Comments about snow ablation. We have tried to be very clear about the goals and limitations of this study. For example, in Section 1, "The goal of this study is to systematically quantify the influence of both mean annual and seasonal insolation changes resulting from Earth's obliquity on continental snowfall, and to determine the climate mechanisms that respond to these insolation variations." In Section 5, we also describe in detail the limitations of our methodology.

We considered adding estimates of the change in positive degree days (PDD). But, there are several reasons why this calculation is fairly meaningless in the context of this study. First, the conclusion will be very obvious. On the basis of the insolation changes, it is clear that the change in PDD will be much larger in the case (delta-TOTAL) with seasonal (summer) insolation variations. Second, the absolute changes in PDD values will be very large because we have used modern trace gas (i.e. pCO2) values. In our current configuration (modern pCO2), the mass balance is negative because ablation during the summer is very high. Using a lower pCO2 would reduce the PDD and allow accumulation, but even then the absolute changes in PDD would be very uncertain. The PDD is very sensitive to the melt factor that is chosen. The melt factor has a very high natural range, so that our absolute values would have a high range of uncertainty. The best solution to estimating mass balance is to use a coupled GCM-ice sheet model, a methodology that we are currently working toward.

To address these points, we have added the following to Section 5: "Ablation is often quantified using the positive-degree-day index (PDD), an estimate of melt based on the number of days with near-surface air temperature above the melting point, and a local melt factor. The change in PDD will be much larger in delta-TOTAL than delta-MA simply because the change in summer insolation (Fig. 1) and continental surface

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temperature (Fig. 4a) is much larger. As a result, the inferred ice-volume changes between our delta-TOTAL and delta-MA cases are probably too small. We have not calculated the absolute change in PDD here because the values are sensitive to the mean high-latitude climate, which is strongly influenced by our choice of (modern) trace gas values, and to local melt factors that are not well constrained."

[R1-7] Suggestion to delete penultimate paragraph and other minor comments. We agree that the penultimate paragraph isn't necessary, and have removed it. We have also addressed the "mis-quotations and imprecisions" listed here. In Section 2, we now specify that the insolation gradients are "equator-to-pole insolation gradients".

[R3-2.1] Comments about snow ablation and PDD. R1 had similar concerns. We address these concerns above [in R1-6].

[R3-2.2] Additional details about precession experiments. We have added an additional figure (Fig. 6) and text (Section 4) describing results from our precessional experiments.

[R3-3.1] Comments about terminology. We have changed our terminology, deleting "non-linear". We do not completely agree with R3's dislike of "feedbacks" and believe that it still has a place in our lexicon.

[R3-3.2] Comments about figures. The lines are now consistent, Red-Total, Black-Mean Annual.

[R3-3.3] Confusion over terminology. This paragraph has been deleted based on suggestions from R1.

[R3-3.4] Suggestion to replace delta-MA with delta-SEA in figures. We added a panel to Fig. 2 illustrating the snowfall response in delta-SEA. We considered R3's suggestion to show all delta-MA results as delta-SEA results. However, because the text compares the delta-MA and delta-TOTAL cases, we found this add a layer of confusion.

[R3-3.5] Concern about surface equilibrium. We calculated global and high-latitude

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sea-surface trends. After model year 120, these are 0.02 and 0.05 C/decade. We have added text to Section 2 (paragraph 5) with this result.

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