Response to M. Pelto's short comment

Medley et al. (2014) provide a comprehensive and valuable assessment of mass balance on some of the most important outlet glaciers of WAIS. The combination of methods and data are an advance over previous efforts and represent what are current best practices. Below are several comments for further clarification in this well written and important paper.

We thank M. Pelto for the positive comments on our work and respond to the suggestion below in bold.

957-20: The hypsometry is quite different for TG and PIG in Figure 2. This is worth a brief mention here or in the paragraph below, but also in the discussion section in terms of impact on temporal mass balance response observed to date if any.

We have added a sentence in the results section to briefly discuss the differences in catchment hypsometry. "The impact of the different catchment hypsometries is apparent in Figs. 8 and 9: the Thwaites catchment has substantially more area at higher elevations (Fig. 2) where the modeled values are less than our accumulation grid, resulting in a larger spread in basin-wide accumulation than for the Pine Island catchment."

961-3: Is the isochronal dating reported here from the density-depth time model or from the horizon counting using hydrogen peroxide summer maximum and other annual markers? Given the reported values below I would assume the latter. It is critical to contrast dates derived for at least Horizons H1 and H3 from the firn cores analysis using the direct observations of hydrogen-peroxide concentration, water-isotope ratios, and non-sea-salt sulfur to sodium ratio versus dates and from the density- time model in a table this is the validation we need.

The radar horizons were dated using the PIG2010 depth-age scale derived, as we state in the text 960-23: "here we used the summer maxima in hydrogen-peroxide concentration, water-isotope ratios, and non-sea-salt sulfur to sodium ratio to identify annual layers. Known volcanic horizons identified by marked increases in wintertime sulfur concentration provided verification of the annual layer counting, indicating a dating uncertainty of less than 1 year." We evaluated the isochronal accuracy by independently dating the same horizon at the THW2010 and DIV2010 sites. We did not evaluate the accuracy of H2 and H3 because they were not mapped to these cores since we were able to map H1 to them (i.e., H2 and H3 were only mapped where we lose H1). The depth-density model used only influenced the horizon dates through derivation of the two-way travel time to depth conversion. We did not derive horizon ages using a simple flow model.

961-10: The comparison referred to here without the suggested table above is not sufficient evidence for the statement made.

We have rephrased the sentence to be more specific about the comparison. "The comparison of horizon ages derived from each of the three cores confirms that H1 is isochronous over large distances, consistent with others studies from this region (Arcone et al., 2004; Spikes et al., 2004)."

962-22: Why not refer to H2 and H3 dates and depths found in the cores here? The values if reported in the table comparing model versus chemical dating would naturally be discussed here.

H2 and H3 were not dated at the other cores because we were able to map H1 along the flight paths connecting the cores (See above note).

966-10: I agree with the assessment of almost all surface velocity resulting from basal sliding; however, is there a reference or data you can provide to make this assertion. Nick et al (2007) provide a good means of assessing depth average velocity, which is not required here if previous referencing is available.

The comment is fairly taken, and we have included a sentence supporting our assertion. "Our velocity measurements are made at the ice sheet surface but we assume they are equivalent to column average velocity due to the high-degree of sliding at near the grounding line. Based on analysis of estimated deformation velocities that are internal variables of a temperature model (Joughin et al., 2009), any biases introduced by this assumption are less than 1% and are not included in our error analysis."

975-18-25: Does the numeric data reported here need to be restated, since it is in the tables, figures and text? It takes away from the larger points that follow.

The reviewer make's a good observation. We have shortened the first two sentences so as not to take away from the more interesting point below.

Table 4: Provides critical flux gate discharge data that should be depicted in a figure to better illustrate the temporal variations and the difference between the glaciers in this progression. This is more important than the sea level rise contribution axis in Figure 10, and could replace that or as a separate figure. The SLR contribution could easily be placed in Table 5 or text.

Figure 10 is actually depicting the variations in ice discharge in terms of mass balance (the left axis). Because we assume a constant accumulation rate, all the variations shown in Figure 10 are due to discharge, and thus we take no action.

Nick, F. M., van der Veen, C. J., and Oerlemans, J.: Controls on advance of tidewater glaciers: Results from numerical modeling applied to Columbia Glacier, J. Geophys. Res., 112, F03S24, doi:10.1029/2006JF000551, 2007.

We thank M. Pelto once more for his thoughtful comments on our work.