Response to **Anonymous Referee #2**

We thank the reviewer for the valuable comments which are addressed point-by-point below.

Comment: Inference of past accumulation rates from an ice core in principle requires two elements (i) a profile of annual layer thickness and (ii) a thinning function.
For the first, the authors use the time series of the δD and $\delta^{18}O$ signals. There is a plausible explanation in section 3.2 on why one can use the water isotopic signal as a proxy for seasonal temperature variation. However, the assignment of the February 1st depth appears to be rather arbitrary or not thoroughly explained. One could certainly expect that using the maxima would be an acceptable way of defining annual layer thicknesses in the core. Additionally considering that the dashed lines in Fig. 4 represent February 1st it is rather confusing that the authors claim that the δD and $\delta^{18}O$ maxima are attributed to February instead of December/January as it looks in Fig. 4 (maxima always precede the dashed lines).

Response: *The dashed lines in Fig. 4 are indeed misplaced (except the one at 15 m weq). This is a mistake which will be corrected in the revised manuscript. We thank the reviewer for spotting this! The assignment to February 1st is based on instrumental data (700 hPa data from Punta Arenas and 700 hPa ERA-Interim data), showing that this is the month with highest temperatures (see Fig. 8). Accordingly we attribute the δD and $\delta^{18}O$ maxima to February (now visible in the revised Fig. 4). We think the fact that accumulation is calculated from February 1st to January 31st does not induce a major uncertainty since accumulation is evenly distributed throughout the year (Fig. 6).*

Comment: Determining the thinning function for this study is arguably a challenging task considering the melt features apparent in the core. The coauthors interpolate linearly using the maxima of the δD and $\delta^{18}O$ maxima signal in order to produce an age scale. This is approximately correct. However, this approach disregards in a way the possibly valuable information contained in the density and melt feature datasets nicely presented in Fig. 3. Assuming one optimizes the correlation of the $\delta^{18}O$ signal and the Punta Arenas temperature in Fig. 8 by tuning the age scale generated by the Herron-Langway (H-L) model, how do the tuned H-L model parameters compare with those used in Fig. 3?

Response: *The H-L model is a firn densification model and does not yield any thinning information. We applied a Nye type model (Nye, 1963) which shows discernable thinning from a depth of 60 m weq, whereas the upper part discussed here is not affected by thinning. We acknowledge that the Nye model was not developed for small scale glaciers, but we do not have the input data for more sophisticated modeling. The finding is consistent with data from other alpine glaciers, where the thinning is insignificant above 50 m (see e.g. Schwerzmann et al., JGR 2006, Knüsel et al., JGR 2003). We will add a corresponding sentence in the revised manuscript.*

Comment: Measurements of melt features are challenging, usually present a low signal to noise ratio and can pottentially be subjective. The method of measuring melt features is not outlined very clearly and the way the 4 different bands of melt features (0, 1.2, 2.4, 3.5 and 4.7) are utilized is rather ambiguous. It would be beneficial for the manuscript to elaborate on the method used in a little bit more detail.

Response: *Melt features were determined on the samples cut for stable isotope, which had an average length of 4.7 cm. Classes of melt of 0, 25, 50, 75, and 100% were attributed when 0, 25, 50, 75 or 100% of this 4.7 cm showed melt features. This corresponds to melt layer thicknesses of 0, 1.2, 2.4, 3.5, and 4.7 cm, respectively (we will add the 75% in the revised manuscript). Since this procedure is subjective and not so precise we averaged the melt percent over 1 m core length (Fig. 3). We will include this more detailed description in the revised manuscript.*

Specific comments
p5296l12 Did the radar survey indicate the presence of water at the depth of 50 m already?

Response: *The radar data collected on site were not clear enough to see a continuous internal layer associated to a possible water table. We will include this information in the revised manuscript.*

p5297l1 Considering the ice temperature was close to melting it would be interesting to comment on the quality of the ice core upon extraction from the drill.

Response: *The quality of the ice cores was good (see example of core #27 below). Few of the cores broke into two sections during transport with the freezer truck on the way from Villa O'Higgins to Santiago (ca. 2000 km).*

Photo of core #27 to show the ice core quality.

p5297l7 Could you elaborate on the choice of the variable sampling resolution?

Response: *The sampling resolution was a bit larger at the top to obtain sufficient material for chemical analysis and was reduced towards the bottom with increased density. When selecting the sampling resolution we did not know that the accumulation is that high. Otherwise we would have chosen lower resolution. We will add a corresponding sentence.*

p5297l12 Since the analysis of ions are mentioned in the manuscript it would be appropriate to show a plot or at least explain why the dataset is not presented.

Response: *We added the Na+ concentration record to Fig. 4 in the revised version as example.*

p5297l19 I assume that isotopic measurements are reported in permil with respect to VSMOW. No reference to this is made through the text or the plots. While the authors comment on the precision of the measurement, there is no information given on the calibration procedure followed as well as the accuracy or combined uncertainthy of the system. This is especially important when the combined δD and $\delta^{18}O$ signal is used in section 3.2. A short comment on this would be very appreciated.

Response: *We agree that we did not present enough information. δD and $\delta^{18}O$ values are reported in permil deviation of the isotope ratio to an international accepted standard (Vienna Standard Mean Ocean Water, VSMOW). For calibration and correction for instrument drifts, two in-house standards were used ($\delta^{18}O$= -9.82‰ and -20‰, δD= -70.3‰ and -162‰, respectively) which were calibrated against the IAEA reference standard. We will include these details in the revised manuscript.*

p5298l20 Is the firn to ice transition defined as the depth at which $\rho = 917 kgm^{-3}$. If yes, is it accurate to claim that at 50.6m the ice transition was reached? What is the depth at which the two implementations of the H-L model reach $\rho = 917\ kgm^{-3}$?

Response: *We stated that the firn-to-ice transition was probably reached, since the density was $\rho = 899\ kgm^{-3}$ (instead of 890 kgm^{-3}, we will correct this value in the revised manuscript). For this statement we indeed assumed a firn to ice transition density of $\rho = 917 kgm^{-3}$. Considering the uncertainty of the density determination we think that this statement is valid. The H-L model was developed for densities between 0.55 and 0.8 kgm^{-3} (close off depth of dry firn). We do not think it is reasonable to calculate the depth at $\rho = 917 kgm^{-3}$ with this model which would be a purely hypothetical value not including any ice lenses.*

p5300l15 With only 5 values for the inferrred accumulation a mean value may not be a representative measure of the distribution. Those values can possibly be presented in a table or Fig.4.

Response: *We will include a corresponding table in the revised manuscript.*

Year	2005	2004	2003	2002	2001
Accumulation (m weq)	6.0	6.5	5.8	7.1	3.4

p5300l25 Replace “1 February” and “31 January” with “February 1st” and “January 31st”.

Response: *Will be replaced.*

p5300l16-21 This is not a “hard” correction. However the phrasing in the sentences referring to the regularity of the flowering technically makes them look like they contradict each other.

Response: *We will rephrase it: Pollen peaks occur generally earlier than the $\delta^{18}O$ maxima attributed to February, which is reasonable considering that flowering starts during the austral spring (Hechenleitner et al., 2005). Pollen was not detected in every spring season identified in the stable isotope records. The absence of pollen during a particular spring-summer season can be explained by the fact that trees do not flower regularly and in some years widespread non-flowering occurs. Thus, the pollen records alone do not allow dating, but they confirm the identification of annual layers based on stable isotopes.*

p5301l17 Why not use the time scale inferred by the density profile or the H-L model? The z-t relationship is ought to be slightly non linear. The result is likely not very different but might possibly improve the correlation with the Punta Arenas temperature time series.

Response: *See above comment on the H-L model and thinning.*

Fig. 3 The H-L model is evaluated with a firn temperature of -1 C while the borehole measurements indicate a slightly warmer borehole. In the same time the accumulation rate used to drive the H-L model is at the lowest range of what is inferred in section 3.2 (3.4 m weq.). Would you argue that the melt free–dry firn density should be represented by the line fitting the minima of the the measured density profile? If yes why does the H-L model fit the profile poorly when a mean accumulation value of 5.8m weq. is used (that would lie between the 3.4 and 7.1 m weq H-L implementations)?

Response: *We showed the two calculated H-L density profiles to underline that the density at Pío XI glacier is slightly higher than the pure firn densification suggested by H-L. However, there is still a big difference to the density profile of glaciers with superimposed ice. The melt-free firn somehow follows the envelope of the two H-L simulation, but we think the data is not precise enough to conclude which is the better representation. We will include more detail in the revised version.*

Fig. 4 Depth scale is given in m weq. Have you used the measured or the H-L modeled density profile for this conversion? Assuming your assigned “beginning of the year” depths correspond to Feb. 1st, then the water isotopic maxima look like they are located in December/January instead of February as mentioned in the text.

Response: *We used the measured density for calculating the m weq depth scale. The attribution of the isotope maxima is corrected in the revised version of Fig. 4 (see above).*