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# *Interactive comment on* "Net accumulation rates derived from ice core stable isotope records of Pío XI glacier, Southern Patagonia Icefield" *by* M. Schwikowski et al.

**Anonymous Referee #2**

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Schwikowski and coauthors present a valuable study of past accumulation rates based on water isotopic profiles from a firn core drilled on Pio XI glacier. Considering the quality of the data, very much due to the succesful site selection, as well as the inherent difficulties of a drilling operation at this weatherwise extreme site, this study should certainly be considered for publication in TC. The manuscript with title "Net accumulation rates derived from ice core stable isotope records of Pio XI glacier, Southern Patagonia Icefield" is generally well written and the presentation of the data is clear and thorough. There are however some sections of the manuscript that could benefit from a slightly more carefull explanation of the methods used.

**General comments**



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 $\delta$D 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 $1^{st}$ 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 $1^{st}$ it is rather confusing that the authors claim that the $\delta$D 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).

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 $\delta$D and $\delta^{18}$O 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?

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.

**Specific comments**

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

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.

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

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.

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 $\delta$D and $\delta^{18}$O signal is used in section 3.2. A short comment on this would be very appreciated.

p5298l20 Is the firn to ice transition defined as the depth at which $\rho = 917\text{kgm}^{-3}$. 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 $\rho = 917\text{kgm}^{-3}$?

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.

p5300l25 Replace "1 February" and "31 January" with "February $1^{st}$" and "January $31^{st}$".

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.

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.



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)?

Fig. 4 Depth scale is given in m weq. Have you used the measured or the H-L modelled 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.

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Interactive comment on The Cryosphere Discuss., 6, 5291, 2012.