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Interactive comment on "A 2700-year annual timescale and accumulation history for an ice core from Roosevelt Island, West Antarctica" *by* Mai Winstrup et al.

Anonymous Referee #2

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This manuscript provides a necessary timescale for the last 2700 years of the RICE ice core. It gathered many information obtained by Continuous Flow Analysis, especially chemical concentrations and acidity to identify annual cycles as well as volcanic peaks. The timescale is extensively compared to the WD2014 timescale which is one aspect not so clear from this manuscript: is this timescale tuned or not to WD2014 ? Finally, there is a discussion on the accumulation rate reconstruction and its evolution over the last 2700 years. Because of the strong uncertainties associated with the reconstruction, only the recent decrease can faithfully be discussed. While I believe that this manuscript will make an important contribution for following papers dealing with the RICE ice core, major comments should be addressed before its publication.

- The dating strategy is not clear. There is a mixing of layer counting constraints as well as use of volcanic peaks (+ nuclear bomb tests) but the uncertainty is only defined from layer counting at least on the upper part. It thus seems that the uncertainty is a bit overestimated ? Below 42.5 m, WD2014 seems to be taken as reference for the dating of the volcanic peaks as well as for adjusting the StratiCounter algorithm. Then WD2014 is used for "validation" of the timescale. By reading the methodology, it thus seems that there is something circular in the approach if WD2014 is used both for construction and validation of the timescale – could you please explain this better? - The methane constraint for the RICE timescale is not clear. First, it refers heavily to a paper that is in preparation (Lee et al., 2017). Second, the uncertainties associated with such tie-points are large and it is thus complicated to use them faithfully for timescale validation. Finally, the procedure mixing Monte-Carlo technique and manual adjustment is rather unclear. I imagine that everything will be in the Lee paper but details are missing to really make use of this part which is not very robust as written here. - Some parts are very long and not useful (most of section 3.2.3.2, part of section 3.3 on l. 10 or p. 15) – I suggest to reduce these sections and better concentrates on the method and associated uncertainties. - Accumulation rate is certainty an input of the firn model described in p. 13 while only forcing using a site temperature history is mentioned. It is very surprising that accumulation forcing is not mentioned here since one of the aim of this paper is to provide an accumulation scenario. We are thus expecting the use (or at least validation that everything is coherent with Dage or d15N measurements) of the accumulation rate scenario in the firn model. - What is the "model" mentioned in l. 45, p. 13 ? - The discussion on the ASL influence on the accumulation rate in the region is both in the "Results" section and in the "Discussion". This is also the case for other ideas that are repeated several times and a reorganization and simplification of the manuscript is needed. - The accumulation reconstruction should ideally have been compared to accumulation rate scenario used for the firn model as well as with water isotope profiles. It could strengthen the discussion and conclusion parts on the accumulation aspect that are rather short.

Interactive comment on Clim. Past Discuss., https://doi.org/10.5194/cp-2017-101, 2017.