Review of Revised Phillippe et al., 2016 manuscript:

The revised manuscript by Phillippe et al. has notable improvements, particularly in regards to the ice-flow modeling and reconstruction of the accumulation rate history. The collaboration with other researchers doing ice-dynamics work has led to a robust and convincing correction of strain thinning.

Unfortunately, the development of the timescale remains flawed. The attempt to identify Tambora is unconvincing and undermines the entire development of the timescale. A critique of the timescale development is below.

Overall, the authors need to admit that they:
- cannot reliably identify any volcanic events and thus have no age control beyond annual layer counting
- did not sample the core at high enough resolution and may thus be overcounting, predominantly in the deep core
- and thus the interpretation of an increase in recent accumulation is tentative and more work is needed to answer this interesting question definitively.

This work can become published, but only after giving up on identifying volcanic peaks and giving an honest and detailed assessment of the annual layer interpretation and the likelihood of overcounting in the deeper part of the core. The type of questions that needs to be answer is: What would the record look like if you subtracted 1 year in every 20 below 40 m depth? Or 1 year in every 10? And then discuss whether your annual layer count is reliable at that level. One approach could be to use automated techniques (such as Mai Winstrup's straticounter: https://github.com/maiwinstrup/StratiCounter) on the different data sets to get a sense of the uncertainty. My guess is that the real uncertainty is closer to 6% than 0.6%.
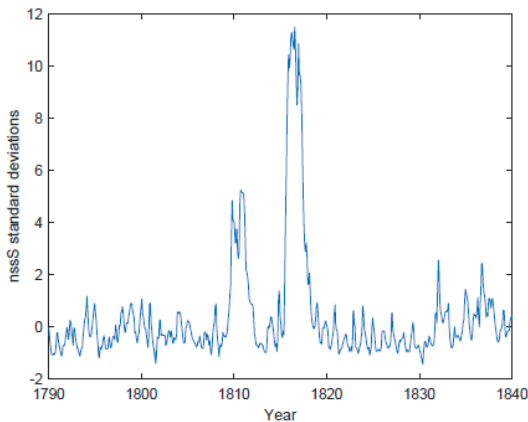
The Timescale:
The paper hinges upon accurate interpretation of the depth-age relationship. The main conclusion is that the accumulation rate has increased in recent decades compared to the previous couple of centuries. This interpretation relies both the timescale and the corrections for density and ice-flow induced thinning. The authors have greatly improved the corrections, which can now be both understood and trusted. However, the timescale remains both poorly described and untrustworthy.

The magnitude of the inferred accumulation rate depends directly upon the thickness of the identified annual layers. Thus, supporting the interpretation of increased recent accumulation requires showing that the annual cycles are properly identified. The authors attempt to do this in two ways: 1) the presentation of data with distinct seasonal cycles to convince the reader that the seasonal cycles at all depths of the core are unambiguous (or at least nearly unambiguous) and 2) identify horizons (such as volcanic events) that can be tied to other cores (or other paleoclimate records) that confirm the annual cycle interpretation.

-Tambora
I recommended focusing on Tambora in my previous review but am deeply frustrated by the revised manuscript. It is worth discussing here why Tambora is so distinctive in Antarctic ice core records. Tambora is indeed the largest event in the past few hundred years. But part of the reason it is so distinctive is that is in preceded by the second largest event of the past few hundred years, yielding

distinctive double peak. I've attached a figure of Tambora and the unknown events as shown by Sigl et al. (2013) from the WAIS Divide ice core. It is worth noting that both events have durations of 3 years.

The purported Tambora in IC12 lacks all resemblance to this event found elsewhere.

1) the authors make no attempt to identify the preceding event (commonly know as the unknown 1809 event).
2) The sulfate peak associated with Tambora is not even the largest in the figure. The authors do not explain why the ECM is so anomalously high while the source of acidity, H2SO4, does not result in high SO4 levels.
3) The figure starts at 101 m, conveniently hiding the fact there is no data from 100 to 101 m – something that I do not believe is discussed in the text.
4) The lack of chemistry measurements of the full core means there is no ability to reliably identify and compare SO4 peaks along the core
5) The ECM data is heavily filtered, indicating it has major quality-control issues and reducing any confidence that it can reliably detect volcanic events. Further, the filtering methods are unclear with the techniques used reference (Karlof et al. 2000) being more complicated than what is described in this text (just the Savitsky-Golay filter and normalization) – I'm still not sure what was done to the ECM data presented here. Further again, the ECM data is corrected for density based on borehole optical televiewing that is likely not that well depth-referenced (the methods are not described except for an unprovided *in review* manuscript) and regardless, is not appropriate for ~10 cm scale variations anyway, as explicitly stated in the given reference, Hubbard et al., 2013. This technique likely just introduced a bunch of noise at annual-to-volcanic frequencies into an already noisy record.
6) The authors claim that the ECM peak occurs in wintertime, despite being in an Na trough with a clear So4/Na peak (and hence SO4). Though the water isotopes are indeed unusual, there is a shoulder which may indicate a lack resolution to identify peaks. This seems more likely to be a thin year than volcano, let alone Tambora. Further, the logic of a wintertime peak is faulty. Tambora is a multi-year event such that the peak should be highest in summertime when there is both volcanic deposition and ocean-derived deposition. I guess the authors could argue that the coastal characteristics of deposition could truncate the duration of Tambora - but they would need to do that and be convincing with climate model output and observation al data.

My biggest issue with the "identification" of Tambora is that it is so far from convincing I simply have no trust in the authors development of any part of the timescale. This is particularly important because the

timescale was clearly developed iteratively; the chemistry measurements were only made in areas of uncertain annual layering indicating that the annual layers (of d18O and ECM) were interpreted prior to the chemistry measurements clarifying the annual layer iterpretation. It is likely that the volcanic matching was also done before, and thus the annual layer interpretation may have (consciously or subconsciously) been interpreted to get the right age at Tambora. While this sort of issue is not uncommon producing annual timescales, the lack of awareness of this issue in the manuscript is troubling especially given the propensity to pick too many years in undersampled data.

-Annual layer interpretation
Evaluating annual interpretation in publications has to be based largely on trust since it is difficult to present that data and interpretation in a manageable way. The authors do a good job of presenting the data with the addition the supplementary figures. However, the authors fail to convince me of their uncertainty, and hence the underlying timescale. This is in part because of the unconvincingly attempt to identify Tambora, but also because the measurements are just not of sufficient quality and continuity to get 0.6% accuracy. Some of the issues:
- The stable isotopes, the primary parameter used to identify annual layers, are of insufficient resolution much of the time. The histogram in Figure R1 shows that the mode of the number of data points per annual layer is 4, with a significant number of layers identified with 3, 2, and even 1 data point. 4 data points is not enough for annual layer interpretation, less alone fewer. The authors may say that the other data sets define these layers, so I address that below:
- The ECM data is heavily filtered with a 301-point window and unclear other techniques (see above). The authors do not describe how this impacts the annual interpretation, which is a major shortcoming. But what I see of the ECM data suggests to me that it is of questionable reliability for interpreting annual layers, with an uncertainty of 10% not less than 1%.
- The chemistry data is sporadic with samples sizes that appear larger than for the stable isotopes (it's hard to see in Figure S2), such that the same criticisms of the stable isotope record apply to the chemistry records.
For instance, between 103 and 104 meters, there is a sequence of 3 thin years which is interpreted exactly the same in the oldest and youngest scenarios (1837-1835 or 1813-11). Yet the Na/So4 looks like there are only two years. In both cases, the sampling frequency is too low to be sure peaks and troughs are being resolved. This type of interpretation may be finding an extra year, biasing the annual layer thickness low, and underestimating the accumulation rate. While individual interpretations can always be nit-picked, the real concern here is that the authors do not even acknowledge the uncertainty.