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Interactive comment on "Investigation of a deep ice core from the Elbrus Western Plateau, the Caucasus, Russia" *by* V. Mikhalenko et al.

Anonymous Referee #2

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General comments

The summit glaciers of the Caucasus range may provide valuable paleoclimate information in a region that offers other proxy reconstructions but so far lacks a complementary ice core record. The manuscript by V. Mikhalenko et al. presents a glaciological and glacio-chemical investigation at the Western Mt. Elbrus Plateau regarding its suitability for ice core studies. This includes a broad overview of the glaciological settings of the greater region and a detailed characterization of the drilling site at the Western Plateau, comprising information from ground-penetrating radar and ice flow modeling. In 2009, a 182 m ice core was drilled down to bedrock and subsequently found essentially undisturbed by melting, which is backed by profiles of density and englacial temperature. The stable isotope, ammonium and succinic acid records are deployed

for annual layer identification in order to obtain an age-depth relationship, which is extended in depth based on flow model considerations. Other chemistry data, including further interpretation of the respective time series is not yet presented in this work.

The manuscript includes a separate section with a thorough report of past glaciological activities in the Elbrus region, which can be understood as a summary of previous studies that have mainly been reported on in Russian only. However, from my point of view it takes up a disproportionally large part of the manuscript and should be made more concise with respect to setting the stage for the actual ice core drilling site. This especially concerns shortening sections 2.2 and 2.3. To give an example of where additional details regarding the glaciological settings of the drilling site would be helpful: It may be noteworthy to mention the prevalent wind direction with respect to the large ice cliff visible in Figure 1 d in proximity of the drilling site – if located downwind such a cliff can act as a strong sink for drifting snow and lead to substantial reduction in net accumulation. However, it appears for this particular drilling site the wind direction does not favor snow loss via the ice cliff.

The identification of annual layers and their counting is convincing and in agreement with multiple reference horizons. What would be helpful is providing an estimate of the counting uncertainty. The authors only mention that there is a small difference between counting in the stable isotope profile vs. counting in the ammonium profile (page 3683 line 25). An illustration of the chemical signature of at least one of the volcanoes would be also helpful. In Figure 9 I am wondering why the simple flow model consideration following Nye is in much better agreement with the annual layer counting as compared to the thermo-mechanical coupled model. Notably, the age scale predicted by the latter (Salamatin) does not show an asymptotic behavior at bedrock. More discussion on this is needed. This also concerns the trajectories derived from the thermo-mechanical coupled model shown in Figure 10: At around 200 – 300 m, trajectories seem to run into bedrock, although the authors state that basal melting should be negligible. On a general note, it would be interesting to use the trajectories

from the flow model to estimate the upstream source region vs. ice core depth. This is of special interest with respect to spatial variations in net accumulation and therefore could help investigating the fluctuations in the ice parts of the density profile as well as a potential incomplete snow preservation upstream (as discussed on page 3683 line 16 with respect to annual layer identification). Unfortunately, information on spatial variability in accumulation seems to be lacking.

The manuscript is well written and has good figures, although some captions are rather short (see specific comments below). I recommend a native speaker read the manuscript in order to help improving some minor difficulties with the English language.

Finally, it would have been interesting to see the actual time series of stable isotope and chemistry, but I am guessing that this is left to a future paper. For now, it would be interesting to show at least a robust smoothing of the stable isotope profile in Figure 7 to illustrate some of the low frequency variability. On the whole this is an interesting paper demonstrating the potential of an exciting ice core record from a new geographic region. I believe the manuscript would benefit from shortening section 2 thus allowing to add more regarding the ice core results where needed. With these changes the paper would be worth publishing in TC.

Specific comments

p. 3684 line 12: It is unclear what is meant by referring to the blue line in the context of dating uncertainty increasing with depth. A dedicated counting uncertainty estimate would be better. Also, there are two blue lines in this Figure which some readers may find confusion in this reference in the text.

The comparison between the text on page 3682 and Figure 8 would profit from making it more clear what is the “winter criterion” and the “winter-background criterion”, e.g. by summarizing the respective definitions in the caption.

Figure 3: Use different colors, dark blue and black are sometimes difficult to distinguish.

Figure 4: Add in the caption that the green triangle marks the drilling site.

Figure 5: What is the meaning of the green dashed line, a robust spline? And, more importantly, what is the typical uncertainty of a single bulk density measurement? A single representative error bar would be illustrative.

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