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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.

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Authors express their gratitude to "Anonymous Referee #2" for the review and the proposed edition of the manuscript. The joint answers on the comments are provided below:

"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." Yes, indeed, not all the data obtained is presented in the submitted manuscript, because we would need much more time to complete interpretation of such amount of results. However, we feel that need to start publishing the results for referring to in upcoming more specific-focused publication and hope you would support such an attempt.

"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." The introduction chapters took 8 pages (from 48 in TCD format) and we did discuss several times on what should be in and what should be not initially and after getting Your review. Both Russian and French team members still think that the presented information and list of references had never been published in English before, and that is why it might be useful for further work and citations. We did make some rather small "cleaning" of the information but could not agree on what else should be in and what could be deleted. Sorry for that. We hope in the present form it still can be accepted.

"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 prevalent wind direction was mentioned for Elbrus in general, and this was the same for the cliff. Right now we made it more clear by writing in the corresponding paragraph. More detailed meteorological analysis of the role of wind in distribution of snow accumulation over the glacier is not yet possible, but definitely is something to look into. "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." We agree and in the revised version we add Figure 10 showing Tritium measurements and the chemistry of snow layers attributed to the Katmai eruption. We also add a few words on the 1840-1833 AD layers. Following that we have updated section 3.3.6 as follows: "Dating based on annual layer counting of the chemical stratigraphy is in a fairly good agreement with the tritium 1963 time horizon that is located at the core depth of 50.7 m w.e. (dated at 1965 using the ammonium stratigraphy, Fig. 10a). In addition it fits very well with the dating achieved so far (i.e. core down to 106.7 m) on the base of the seasonal stratigraphy of the stable isotope profile. Whereas stable isotopes predict the year 1924 at a core depth of 106.7 m, the chemical stratigraphy leads to estimate the year 1926 in this depth. To anchor the depth age relation with further absolute time horizons, a first inspection of the sulfate profile was made in view to identify volcanic horizons as found in other northern hemisphere ice cores between 1912 (Katmai) and 1783 (Laki eruption) in Greenland (Legrand et al., 1997; Clausen et al., 1997) and at Colle Gnifetti (Bohleber 2008). However since the Elbrus is an in active volcanic crater, it is sometimes difficult to attribute a peak either to a well-known global eruption or to a local event. Furthermore, numerous sulphate peaks in the Elbrus ice core originate from terrestrial inputs as suggested by the presence of concomitant calcium peaks. So far, the Katmai eruption in 1912 could be clearly identified at 87.7 m w.e. (dated at 1911 using the ammonium

stratigraphy) with several neighboured samples showing relatively high sulfate levels (up to 1200 ppb, i.e. 25 μEq L-1) compared to those seen in sulphate peaks generally present in summer layers of the early 20th century. Furthermore, as seen in Fig. 10b, in contrast to neighboured summer sulphate peaks located at 87.2, 87.4, 88.0, and 89.3 m w.e., that are alkaline (see Fig. 10b), the acidity of samples of the 87.7 m w.e. sulphate peak reaches 8 μEq L-1 at the bottom part of the sulphate peak. Furthermore, samples located of the top part of the 87.7 m w.e. sulfate peak remains neutral in spite of a large presence of calcium (similar to those seen in neighboured summer sulphate peaks). As seen in Figure 11 it appears that within one-year uncertainty this horizon is in excellent agreement with our annual counting." Below 88 m w.e., we were still able to easily proceed annual counting down to 113 (1860), whereas further down the dating become more uncertain (see the blue line in Fig. 9). Below 88 m w.e., 7 significant potential volcano horizons can be suspected on the basis of the ionic balance and sulfate levels (not shown), from which however at least 1 are of local origin (as suggested by small stones with size of up to 1- 2 mm were found in the corresponding layer). Nevertheless, a series of 3 narrow spikes was located at 118-120 m w.e. (dated at around 1840-1833) among which two that are characterized by an increase of sulphate and acidity (up to 7.8 μEq L-1, not shown) may be related to the well-known eruptions observed in Greenland in a time distance of 2 years around 1840 (one of them being possibly due to the Coseguina eruption in 1835) (Legrand et al., 1997)."

Since Figure 10 reports on the calculated acidity of snow layers the text in section 3.3.1 we consistently updated as follows: For cations (Na+, K+, Mg2+, Ca2+, and NH4+), a Dionex ICS 1000 chromatograph equipped with a CS12 separator column was deployed. For anions, a Dionex 600 equipped with an AS11 separator column was used with an eluent mixture made on the base of H2O, NaOH at 2.5 and 100 mM and CH3OH. A gradient pump system allows determining inorganic species (F-, Cl−, NO3-, and SO42-) as well as short-chain monocarboxylates (denoted MonoAc-) and dicarboxylates (denoted DiAc2-). For all investigated species, ion chromatography and ice core decontamination blanks were found to be insignificant with respect to respective levels found in the ice core samples. As discussed in Sect. 3.3.5 the search of volcanic horizons in the Elbrus ice cores needs examination of the acidity (or alkalinity) of samples that can be evaluated by checking the ionic balance between anions and cations (concentrations being expressed in micro-equivalents per liter, μEq L-1): [H+] = ([F-] + [Cl-] + [NO3-] + [SO42-] + [MonoAc-] + [DiAc2-]) - ([Na+] + [K+] + [Mg2+] + [Ca2+] + [NH4+]) (4)

"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." As is written in the manuscript (p.3684 lines 27-29 and p.3685 lines 1-3) the "simple model" is the "best fit" number of a constant accumulation (which, we know, was not constant). Differently to that, the Salamatin's model has several parameters to be quantified in the model run, with the "constant accumulation" value (now taken as the "best fit" from Nye) as just one of them. The others were taken as default values for the Salamatin's model (Salamatin et al., 2000) and the result is shown in the Figure. Evidently, playing with all (around 5 relevant parameters) from the Salamatin's model we could produce much better fit into the actual data. But for now we are not able to provide a physical basis for choosing one or another "temperature factor", "Firn compressibility factor", "Creep index" and "Slip parameter" (previously shown to work well for a Kamchatka glacier). That is why we left it as it is in the submitted manuscript. We even discussed the issue with Salamatin. He is considering possibility of further tuning up his model with our data, though the results and the discussion can only be a topic for some future publications.

"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 gen-

eral 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." We can only thank the reviewer for the suggestion. Yes, we are considering developing of a new 3D ice flow model based on the Elbrus data. But this is also task for the future. The accumulation spatial data variability is indeed unavailable for now. "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." We did make the suggested changes in the Figures captions and made our best with improving English

"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." We think that showing Figure 7 with a robust smoothing would need a discussion on the low frequency climate variability, which is out of the scope of the present paper focusing on the dating. But clearly these past isotopic changes as well as long-term trends revealed by chemical records will be discussed in details in foreseen manuscripts. We had no success in considerable shortening section 2 (and consider the other review as a support for keeping it). The requested additions would hopefully make our manuscript acceptable.

“Specific comments” The “blue-line” issue is hopefully solved in the new version of the figure. The new figure caption is as the following: “Figure 8. Seasonal course of NH_4^+ (a, b), succinic acid (c, d), and $\delta^{18}O$ (e, f) signals at to different sections of the Elbrus ice core. Red marked sections assigned samples selected with the winter criterion: Succinic acid smaller than 5 ppb, NH_4^+ smaller than 100 ppb for recent years and smaller than 50 ppb prior to 1950; green marked sections correspond to the winter-background criterion: Succinic acid smaller than 3 ppb, NH_4^+ smaller than 50 ppb for recent years and smaller than 20 ppb prior to 1950. Black bars in ionic plots refer to the winter criteria. The black bars in the $\delta^{18}O$ plots refer to the respective mean value.” The colors at the Figure 3 are changed The caption of Figure 4 is edited The error bars at the Figure 5 would be less than the thickness of the “bars” showing the density at the graph. The green line is just some running mean to connect the actual data bars. The chapter is corrected.

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