

Interactive comment on “On the similarity and apparent cycles of isotopic variations in East Antarctic snow-pits” by Thomas Laepple et al.

Anonymous Referee #3

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The manuscript is devoted to the study of the nature of the high-frequency (with periods < 1 m) cycles observed in the vertical profiles of stable isotopes in snow-firn thickness of central Antarctica. The authors convincingly show that these cycles with a typical wavelength of the order of 20 cm could be formed solely by the diffusion of the initially random (i.e. consisted of white noise) values of snow isotopic content, thus confirming a very low signal-to-noise ratio in the isotopic series.

To my opinion, this manuscript is an important new step towards understanding the process of formation of the vertical isotopic profiles in Antarctic snow, which, in turn, crucial for correct interpretation of the deep ice core isotopic data.

I do not have major remarks for this manuscript, only a few minor comments and corrections, as listed below.

C1

A general note: in your model of isotopic vertical profiles you consider the diffusive smoothing but neglect another part of this process, namely, isotopic and mass exchange with the atmosphere that may lead to the alteration of the mean isotopic composition of snow (compared to that of precipitation). I may guess that this process does not play role here, but it would be better to state it explicitly.

The title: probably it worth thinking of a title that better describes the main result of the manuscript, e.g.: “Non-climatic origin of the apparent cycles in (high-frequency?) stable water isotope variability in central Antarctic snow”. . . It would look stronger and more attractive.

Page 2 lines 3-5 – may be better to write “The ice thickness and accumulation rate affects the temporal scale and resolution of the climate reconstructions that can be obtained from a given ice core”

Page 2 line 16 – “including those at Vostok. . .”

Section 2.5 title – better write “vertical profiles” instead of “time series”

Page 9 line 6 – better cite Stenni et al (2016) where a good review of different slopes is given:

<https://www.the-cryosphere.net/10/2415/2016/tc-10-2415-2016.pdf>

Table 2 – if 10 m temperature is preferable, then it is better to take -57°C for Vostok instead of -55°C (Lefebvre E., L. Arnaud, A. Ekaykin, V.Y. Lipenkov, G. Picard and J.R. Petit. - Snow temperature measurements at Vostok station from an autonomous recording system (TAUTO): preliminary results from the first year operation. - Ice and snow, 2012, v. 4, p. 138-145.)

Figure 8 caption – should be “simulation for 90% noise”?

Page 20 lines 10-11 – just a comment: I see your point that the observed cycles can be solely explained by the suggested mechanism (white noise + diffusion), so you do not

C2

need to invoke other processes. But this does not necessarily exclude the influence of dunes that may be hidden somewhere in this noise, as the large error bars on your spectra allow it. Moreover, at least on the scale of mega-dunes it is proven that the dunes do influence the spatial and temporal variability of the snow stable water isotope content (<https://www.the-cryosphere.net/10/1217/2016/tc-10-1217-2016.pdf>), so the dunes do matter. However, the influence of the dunes on the relevant time scales (years or decades) is still an open question.

Page 20 line 26 – an odd “the”

Page 21 line 20 – an odd “and”

Title of Appendix B – I suggest to rename to “Observed and simulated cycle length for the sites with one (single) available profile”

Page 24 line 1 – missed dot.

Page 25 line 2 – please replace “Ekyakin” by “Ekaykin”

Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2017-199>, 2017.