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Interactive Comment

Interactive comment on "Non-climatic signal in ice core records: lessons from Antarctic mega-dunes" *by* A. Ekaykin et al.

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The study presents a field and snow chemistry study of an area of megadunes just east of Sub-glacial Lake Vostok. The study is a clear and well-conducted study and is appropriate for TC. There is some ambiguity in the analysis and the conclusions regarding the relationship of these megadunes to other true megadune areas. There is also some confusion or 'blurring' of what constitute true megadunes and other wind-related snow features. The paper will be a solid contribution, but I think there will need to be some significant revision to the Introduction, Results, and Discussion section. It is a bit unclear exactly what the paper is concluding at this point. One strong recommendation is to look more closely at the Vostok –area isotopic data and compare it with the megadune region they have studied. It may also be valuable to do a closer comparison with the structural characteristics reported in Frezzotti et al. studies (slopes, spacing of dunes, accumulation variations).

Introduction – to many 'the' - and, in most studies the features are referred to as 'megadunes', without a hyphen. It is not clear to me that all the older studies are referring to true megadunes; Megadunes are (properly) repeated snow accumulation features (or accumulation / ablation pairs) that are not tied to a bedrock-driven high: something created by an oscillation in the atmosphere. It is true that at the upstream end of megadunes, there is usually a bedrock-driven break in slope, but the train of dunes following the slope break are atmospherically driven. Many studies have confused the strong accumulation variations around bedrock-driven ice sheet undulations, or areas where glaze is often seen on the lower ice sheet, as 'megadunes', and they are not the same. I think this term should be reserved for the features similar to those observed by Frezzotti, Fahnestock and Scambos, Arcone (some of his profiles, not all of them); and your area near Vostok. There is much to sort out regarding wind redistribution of snow in Antarctica and Greenland. Megadunes are part of it, but the term is sometimes applied too broadly. It would be good to distinguish these from the menagerie of wind-related forms. I'm a bit unclear as to excactly what you mean (later on in the paper) by 'meso-dunes' – do you mean longitudinal dunes? Or complex multi-formational sastrugi features? It would be good to spend a paragraph sorting these things out. I agree that true 'megadunes' occupy about 500,000 km2 of the East Antarctic interior, but not that they encompass all these studies.

P6912-L12 – suggest you remove citation of Zwally et al., 2015. You are citing works that agree with you, and one that did not properly account for accumulation (Zwally). Citing incorrect studies (Zwally) is confusing to the reader.

Data and methods – keep track of superscripts, in some cases the 17O is not superscripted properly. (Perhaps there is a new convention about '17O-excess' that I am not aware of? To me, it should be superscripted.)

P6916, l10: . . .Figure 2, we show the GPR registration recorded. . . (replace 'showed')

Results – on line 20, p6916, yes, both smaller wavelength horizontally, and smaller in amplitude – this may be a factor in the degree to which the glaze / ablation effect is present, perhaps it explains the minor differences seen in mean accumulation and surface structures here. What is the amplitude of the waves relative to the regional slope? And the slope variations from the windward and leeward faces?

P6917, L20 ". . .during the field work seasons. . .' (need to change 'field-works', not commonly used). P6919, L1-3, restate the slopes as 0.000X, etc., per mil is not used for slope. P6920, L7-8 ". . ..a dune drifts by one full wavelength in about 410 years." P6921, L21 "..the core has a ∂D isotopic composition of -420%

The other major issue I have is that there is a strong isotopic variation but the authors do not infer much ablation . . . or even any net mass movement by vapor ? At one point the idea of summer versus winter accumulation is introduced, but disproved — and so, how do the large variations form if not by ablation? Yet I agree that it is perplexing that the total accumulation rate is so similar to the mean Vostok stake array value of 23 mm / year w.e.

One thing to add to the paper (to Table 1 and then to the discussion) is a few values for the mean isotopic composition of the Vostok snow itself – enough well-analyzed samples for the reader to see the variability near the Base but away from the megadunes. How do these average values for snow on the flat lake surface compare with the megadune range of values? Does this provide some insight into formation processes or post-depositional modification of snow?

[Some discussion, not formally related to my review here: I think there may be a range of megadune characteristics. Your study area appears to me to be incipient megadune formation, with measurable accumulation through the entire wave (according to the radar layering). Frezzotti's work showed a somewhat more developed pattern, much closer to zero accumulation in the leeward faces, but still traceable. In our study of large-wavelenth -3 to 5km, ∼8 m amplitude dunes downhill from Vostok by ∼300 km, we saw complete erasure of layering in the areas where glaze surfaces formed on the leeward sides, strong layering and high accumulation on the windward faces, and strong isotopic cycles in a vertical core.

From my(our) field work and the existing literature, my best assessment of how megadunes and associated glaze areas form is firmly linked to local variations in wind speed and the effective humidity of the near-surface air layer. In decending, drying conditions, it is not easy for snow grains to stick together; in ascending or flat airflow, the near surface layer quickly saturates with water by sublimation of entrained snow. Thus snow is not 'trapped' by the surface easily on the leeward slopes, and is either blown to the windward slope or consumed by evaporation. See Scambos, Frezzotti, et al., 2012; also Das et al, 2013 and 2015.

Interactive comment on The Cryosphere Discuss., 9, 6909, 2015.