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Comment

Interactive comment on “Can we retrieve a clear paleoclimatic signal from the deeper part of the EPICA Dome C ice core?” by J.-L. Tison et al.

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The manuscript attempts to determine whether the lowermost 60 m of the EPICA Dome C ice core provides a climatic signal by evaluating trends in numerous ice properties in terms of competing physical and chemical processes. The task is not straightforward and no simple solution arises. However, given the number of ice properties involved and their contrasting variations with depth, I believe the paper does a commendable job of rationalizing the data into a suite of explanations that is well argued and has important implications. The interpretation is far from unambiguous and necessarily involves some ‘corner cutting’, but it is probably the best that can be done given the complexity involved.

I have two general comments.

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First, the paper names the lowermost 60 m (excluding ~6 m located immediately above the ice-bed interface that remained uncored) as ‘bottom ice’ and divides this in turn into debris-free uppermost 48 m as ‘deep ice’ and the debris-rich lowermost 12 m as ‘basal ice’. Following the analysis, it is argued that all 60 m is influenced by processes dictated by proximity to the bed. I believe the current names work satisfactorily (not perfect though) for the palaeoclimatic arguments presented in this paper, but it does not work well for students of basal ice who may subsequently wish to compare the ice types investigated here with those from elsewhere. Alternatively, a descriptive scheme would serve both purposes. Although it is not explained in the paper (and I think it should be) the 60 m analysed is I believe different from the overlying ice (Fig. 1b). In which case I would call the 60 m the ‘basal ice’ or ‘basal zone ice’ and divide the two ice types involved into basal ice facies: probably ‘clean’ underlain by ‘dispersed’ or ‘dispersed banded’. Indeed, the latter could be of use in distinguishing between the lowermost 2 m of the currently basal ice, into which the paper claims basal debris may have been introduced. If there are insufficient data from the core to ascertain whether the debris is banded or not then I would probably still refer to the two sections as ‘clean’ and ‘dispersed’ rather than ‘deep ice’ and ‘basal ice’.

Second, I am not convinced by the vertical thickening as it is currently presented, especially considering it has such important implications for the age-depth scale. I have no issue with invoking lateral compression, but without modelling I see no independent evidence for absolute thickening as shown in Figure 7. Actually, I wonder whether such thickening is needed to explain the data anyway. Does the argument not still hold as long as the rate of vertical thinning is slowed relative to comparator age-depth scales – i.e., a ‘relative thickening’ rather than an ‘absolute thickening’? Also, no argument is presented in the paper for how much relative thickening is needed to explain the uniformity of the data – but I would be far happier with an argument along the lines of relative thickening than actual thickening. If the authors agree, then it could be explained in a few sentences.

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I also have some specific comments, listed below:

Location/Line Comment

Title I would remove 'clear' as it is undefined. I would probably also reword the title to 'EPICA Dome C basal ice reveals palaeoclimatic signal' or 'A palaeoclimatic signal from the ...'. I believe that this is what the paper concludes. P3 L24 Replace allochtone with allochthonous. How about simplifying further: '... incorporation of debris from the ice sheet's substrate'. P4 L9 'this palaeoclimatic information' P5 L11 'less well documented' P6 L13 and elsewhere Need to be consistent in terms of tense (I would delete 'have' here and elsewhere and always keep in past tense [the present is also used elsewhere]). L21 Replace 'bottom' with 'lowermost' as the paper uses 'bottom' in a specific sense (see general point). P6 L28 I would define basal ice more broadly as ice that has acquired a distinctive character as a result of processes driven by the presence of the ice-bed interface. Thus, it does not have to be debris rich (and indeed, one can have debris-rich ice that is not basal ice). P7 L20 Delete ', if'. L22-3 I would delete from 'using...' to '...ice' to leave '... be processed in continuity...'. P9 L2 No need for 'respectively'. P10 L2 (& P11 L16) I would replace 'clues for' with 'indicators of' or 'evidence compatible with' L19 Replace 'convincing' with 'indicative'. L21 I would replace 'happens to be' with 'is'. P10 L28 'comparison of the mean deep and bottom ice values with those ...' P11 L2 I would replace 'good' with 'close'. P12 L6 '... considered as two groups ...'. L18 It may be simpler and just as accurate to shorten the sub-heading to 'Distribution and relocation of ...' P13 L28 'suggested' (and I would move the 'however' to the beginning of the sentence. P14 L3 I would replace a 'fair share' with 'much' or similar. P15 L7 This reference to meteoric ice 'above' could be placed into context by addressing whether this 60 m section was analysed and reported on here because it is different from the ice above (as I suspect and hope) or because this is how the core was distributed. This may well be in the paper and I may have missed it, but if it is not then it should be stated. P19 L28 'means' L29 I am not familiar with 'exsudation'; can 'expulsion' be used? P20 L1 These processes would form ice that may be similar in

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appearance to the 'clear' facies reported at temperate glaciers and to the cryo-warmed ice reported towards the margins of the Greenland Ice Sheet by Thomas Phillips and colleagues. If this is right then both associations should be mentioned here or in the Discussion. L1-2 I am not sure of the argument linking basal melting to '... propagate the two zones of deep and basal ice upwards...'. P20 L25 '... scale, thereby providing some...' P21 L8 I would delete 'elevated' from 'elevated depth'. L28 Giga-Joules = GJ (?) P22 L12-15 It is not easy to combine all the competing data into coherent theories and a good deal of latitude must be given. However, I am not completely convinced by the treatment of lithic particles and the difference between the uppermost ~10 m of the basal ice (no mechanism of incorporation from the bed) and the lowermost ~2 m into which basal debris particles may have been incorporated. Is it possible from the available data to make a clearer statement as to whether all of the debris above ~2 m is precipitated and then whether the basal 2 m shows any other evidence of having been influenced by a different process. At present this 10 m / 2 m division seems a little too informal relative to the precision of some of the other arguments. Is the lowermost ~2 m a different basal ice facies (see general comment)? P23 L10 The comment immediately above has a bearing here too. P 40 Fig. 7 If this is kept then I would stress its conceptual nature. Maybe just begin the caption with 'Schematic illustration of the nature of ...'.

Interactive comment on The Cryosphere Discuss., 9, 567, 2015.

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