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Climate of the Past Discussions

Interactive comment on "Using beryllium-10 to test the validity of past accumulation rate reconstruction from water isotope records in East Antarctic ice cores" *by* A. Cauquoin et al.

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The paper by Cauquoin et al. discusses and compares different accumulation rate reconstructions in Antarctica. The main novelty of the paper and probably its greatest merit is to show a high resolution record of Be-10 on a whole glacial-interglacial cycle (MIS 8 to 10), which is a "first time" to my knowledge and which has been long awaited in the ice core community. This Be-10 record is then discussed in term of accumulation reconstruction assuming a Be-10 flux as reconstructed from marine cores. The paper shows that the classical reconstruction of accumulation rates based on deuterium isotopes in ice are mainly correct, albeit with differences of up to 16% for the optimum of

MIS 9.3. The paper also discusses some AGCM modeling results of the deuterium-temperature and deuterium-accumulation relationships in Antarctica, but I find this part less convincing as I will explain below although it probably has some merit too.

Generally, the paper is well focused and has a good potential but some clarifications and improvements in the discussion need to be made. I am hesitating between major and minor revisions. Anyway, I hope the authors will take these remarks as constructive remarks to improve the manuscript and will consider the proposed modifications.

- The main hypothesis that Be-10 deposition in only dry is central to the manuscript but it is only briefly mentioned in the introduction, without any reference. I think it deserves more explanations and at least one reference. - It is not clear how \beta was tuned to obtain the value of 0.0160-0.0171 given in p. 3433, l. 5 (note that the line numbers are incorrect on CPD print version). Again, this needs more explanations and maybe a figure. - The fact that deuterium underestimates accumulation for the optimum of MIS 9.3 is perfectly in agreement with the study by Parrenin et al. (2007, "1D...") which suggested that deuterium-based reconstructions underestimate accumulation for the optimum of the Holocene. This certainly should be mentioned in the manuscript because it gives more strength to one of the main discovery. - In the manuscript, it is not made mention to the availability of the Be-10 dataset. As I said in introduction, this dataset has been long awaited in the ice core community and many datasets are now freely available on internet databases. I am not sure what the policy of CP is with respect to data availability but for sure it would be a plus if this dataset would be made available. - Regarding GCM experiments, I disagree with the statements made in the abstract and in the conclusion that the temp-accu relationship is comparable when using ice core data and AGCM simulation. People will take as bring-home message that AGCM work, but this is far from being true. The spread of AGCM results and the difference with the deuterium-based and Be-10 reconstructions are so large that my personal conclusion is that "AGCM do not work well for simulating the temperature-accu relationship (although they work in average and some work) and they need to be

improved in the future". - Note also that the title does not reflect the part related to the AGCM simulations. A title more representative of the content of the manuscript should be found. - the introduction is strangely structured. Generally, one introduces all the concept before the sentence "In this study...". Here, Be-10 is introduced after.

Minor comments: - abstract, l. 16: The 16% underestimation only holds for the optimum of MIS9.3. As it is written here, the reader will think this underestimation is true for all the period covered. - choose ka of kyr - p. 3426, l. 10-11: this description does not reflect the equation, since Psat is divided by T in the equation. - p. 3427, l. 24: "... it appears that the accumulation rate is ALMOST exponentially linked..." The exact relationship is not an exponential but it can be very well approximated by an exponential (at least for the relationship given in Ritz (1991), I have not checked with the one given in Wagner and Pruss). - p. 3432, l. 8-9: you use the term "centennial" and then the term "secular". This is confusing. - p. 3433, l. 3: "The variance is minimized..." Which variance? I suppose it is the variance of the difference between the deut-based and Be-10-based accu reconstructions, but this should be made more explicit. - table 2: is this table really necessary? This is just extracted from Bazin et al. and Veres et al. (2013) if I understand correctly. - Figure 3b, legend: "AICC2012", noy "AICC20102". - Figure 3: It is not clear to me what the y axis is. If this is a Delta-logarithm, as the label seems to suggest, there should be no unit.

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