

Interactive comment on “A sixty year ice-core record of regional climate from Adélie Land, coastal Antarctica” by S. Goursaud et al.

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The paper by Goursaud and co-authors is presenting a new isotopic and snow accumulation rate records obtained from an ice core drilled in the Adélie Land coastal area (in the proximity of the French Dumont d'Urville station) in a site which is characterized by a relatively high snow accumulation rate (about 220 mm w.eq. yr⁻¹) and covering the 1947-2006 period. The paper is much focused on the dating issues and on the comparison with the data simulated from the high resolution atmospheric general circulation ECHAM5-wiso model ($\delta^{18}\text{O}$ as well as precipitation) and with the regional atmospheric model MAR. The authors also suggest a method to improve the dating considering particular isotopic signature in the inter annual variability of ECHAM5-wiso isotopic data. The obtained results suggest that also a single ice core from a coastal area can capture the main climatic signals although a multi-core approach should be

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desirable in order to reduce the stratigraphic noise which is unavoidable. The paper is interesting, the data are well presented and I have found the reading quite smooth although some parts could be reduced a little (e.g. paragraph 3.1.3). I recommend its publication after the authors have been considered to the following comments.

Page 2, line 3: ice core chronology: the period covered by the core should be 1947-2006 rather than 2007, in fact the drilling has been carried out in January 2007, so the topmost snow layers should at maximum be referred to the year 2006. Please, check this in the whole manuscript, as well as in the tables, figures and related captions. I did not understand if this is just a refuse or a real mistake. Page 3, line 3: the reference Ahmed is not correct! Please change into "PAGES 2k Consortium, 2013". Page 3, line 5: the reference Jones et al., in press. is published. Page 3, line 26: please add here Schlosser et al., 2008 (Neumayer station data) as well as Stenni et al., 2016 (TC). Page 4, line 6: delete the "." Page 4, lines 25-27: the sentence "Data obtained context." is not clear at all. Please explain what do you mean. Page 4, line 31: please change the Laboratory of Glaciology into LGGE, already defined before. Page 5, lines 13-14: the citations Delmas and Pouchet (1977) and Magand et al (??) (2009) are lacking in the Reference. Page 5, line 21: why not shown? May you consider to have a figure on this in the Supplement? Page 5, line 27: delete "e" and add "the". The method is the "CO₂/H₂O equilibration method". Please add. Page 5, line 28: add \pm before 0.05 and after % add (1 sigma). Page 5, line 29: I would not refer to the figure 2 here but rather in the result section. Page 6, line 10: delete "in" after Antarctic sites and add "the". Page 6, line 29: add "from" before the Nimbus Page 7, line 25: the citation Bintanja (2000) is lacking in the Reference. Page 7, lines 27-29, line 31: the citations Kessler (1969), Lin et al. (1983), Meyers et al. (1992), Levkov et al. (1992), Morcrette (2002) and Galle et al. (2013) are lacking in the Reference. Page 8, line 4: the authors refer here to one year DDU record of precipitation. Are these data available? Are these the same data cited later on in the text (paragraph 3.3.1)? Please, explain better. Perhaps the data could be added in the Supplement? Page 9, line 1: paragraph 3.1.3: I suggest to reduce a little this paragraph by using a table or a figure. Page 10,

line 20: add a space between “calculated” and “ from”. Line 10, line 28: less depleted values Different seasonal pattern: the mean $\delta^{18}\text{O}$ at S1C1 is -18.9‰ . not very far from -18 at DDU considering the difference in elevation. I mean the results seems consistent, obviously less depleted than DC. Then regarding the difference with Dome C it’s not surprising but I cannot comment on the different seasonal pattern if all the data (DDU) are not provided as a figure Moreover, considering the large inter-annual variability of Antarctic climate and the only less than 1-year record at DDU I do not think that it is appropriate to discuss about different seasonal patterns. Page 10, line 29: Stenni et al., 2016, now published (to be changed also in the Reference). Page 11, line 9: when comparing S1C1 site with Law Dome you have to consider the extremely different snow accumulation rate between the two sites. Please add something about this. Also add the Johnsen (1977) citation about post-depositional effects. Page 11, 13-22: please refer to the appropriate figures, 4 and 5, in the text otherwise it is not easy to follow. Page 11, line 23: delete “accumulation” after $\delta^{18}\text{O}$ or an “and” is lacking? Please, correct. Page 11, line 33: at Dome C we used daily values or monthly values and obtained a slope $0.49\text{‰}\text{°C}$. Not sure what you have used here, please explain. Page 12, line 1: please check the reported statistical values. Also there seem to be a typo error Page 12, line 2-3: at Dome C we considered daily values, so we did not exclude the seasonal cycle. We also obtained a higher slope (1.4) if considering the only 3 annual values, but its significance is low since it is calculated on 3 years only. Here, on the other hand, you are considering inter-annual variability. Page 12, line 14: rather than metamorphism I would say exchanges between surface snow and water vapour. Page 12, line 26: the first reported values ($r=-0.48$ and $p=8.0\text{E}-3$) seem significant. Please, check. Page 13, line 21-22: in Antarctica this problem was nicely shown by Frezzotti et al, 2007 JGR. Also for snow accumulation rate values as those found by the authors for this Adelie Land site S1C1 and also considering the wind effects (see comment by E. Isaksson)! Page 13, line 27: add “scale” after “horizontal”. Page 14, line 15: the year should be 1986 and not 1985. Page 15, line 7: add a space between “the” and “period”. Page 15, line 10: what do you mean by

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“climatic deposition signal” . . . not clear. Page 16, line 3: why “his”? Page 16, line 6-7: comparison with the ECHAM5-wiso output BUT with which chronology? Page 16, line 9: contradictory: may you explain better? Page 16, line 20: Stenni et al., 2016. Page 16, line 21: see also the conclusions by Schlosser et al., 2016 (ACP) about the inter-annual difference. Page 16, line 28: if I am not wrong, this statement “but a significant and weaker correlation between $\delta^{18}\text{O}$ from the S1C1 core and simulated by ECHAM5-wiso” depends on the age scale considered, isn't it? Page 17, line 10: this adjective “coherent” seems in contradiction with what you have just said few lines before. Page 18, line 4: change sake into stake Page 18, line 7: please consider that the difference in the snow accumulation rate (and wind action) at the different sites and their effects on the diffusion effects and so on the smaller amplitude of the seasonal isotopic cycle. See comment before. Page 18, line 10-12: the sentence “ We stress . . . relationship.” seems not valid if I consider δ core and Techam. May you check? Page 18, line 17: I would also add that precipitation sampling at DDU would be desirable. Page 18, line 34: the reference Ahmed is not correct! Please change into “PAGES 2k Consortium, 2013”. And also Jones et al. is published. Page 19, line 5: please specify what is PSA2. References: some are not completed (Jones et al, Lemeur submitted, Masson-Delmotte et al., 2008 the authors list is not completed) and some must be changed from discussion to final accepted papers (Ritter, Schlosser). Please, make a careful check. Table 1. The precipitation column is not only precipitation (ok for ECHAM) but in the case of the core data is accumulation. What is u? I suppose average. . . . Table 2: be careful about the year 2007 (the period covered by the core). . . . see my comment above (page 2, line 3). Table 3: something is lacking in the first column: You should have 1956-2006 (annual and decadal) and then the same for 1979-2006. Why you did not discuss in the text the relationship between Techam and $\delta^{18}\text{O}$ S1C11 and S1C12, if I am not wrong? (see my comment above Page 18, line 10-12). Figure 1: add “hatched area” in the caption after “Passive Microwave Data”. Figure 2: in the caption: you do not have dimethylsulfure data but MSA data!! I would also make the labels larger than they are. Figure 4: the different lines are not distinguishable. Also in

this case I would use larger labels. In the caption: resampled data: specify with which step. The annual mean is calculated by the annual layer dating? Specify. Figure 5: Also in this case I would use larger labels. In the caption, some typos. . . . Figure 6: I had some difficulty in comparing the values referring to the red (cityscape) and the blue lines. Moreover, check also here the correct period covered by the core. The first year is 2006, the accumulation record seems OK but for the $\delta^{18}\text{O}$ why you have a value for 2007? Here the dimensions of the different labels are OK. Figure 7: Also in this case I would use larger labels. Figure 8: Also in this case I would use larger labels. Also here the first $\delta^{18}\text{O}$ seems to be 2007. . . . To be checked.

[Interactive comment on The Cryosphere Discuss.](#), doi:10.5194/tc-2016-179, 2016.

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