

Interactive comment on “A Lagrangian analysis of the present-day sources of moisture for major ice-core sites” by A. Drumond et al.

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We would like to thank you for your review. Your commentaries have been re-arranged focusing on the different questions arisen. Totally agreeing that the methodology was not explained in the original version in details, this section has been re-written in this revised version focusing on answering your questions.

*The method was not clear to me at all. * The abstract speaks about backward trajectories, but the more I read I assumed these were forward trajectories analysed backward, but I am not sure. *Or are trajectories calculated forward but then the analysis treats them as backward trajectories? * P. 4, L. 10: "backward methodology": forward trajectories analysed backward?

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-The method consists in analysing the 10-d backward trajectories of the particles identified over the 14 ice core sites studied. These particles are advected backward in time using three-dimensional wind taken from the meteorological data (e.g. reanalysis project) every time step.

* Furthermore, it is not clear to me whether the frequency distribution of particles is accounted for or not. The 2 million particles may be evenly distributed initially, but the sampling procedure must introduce large differences in the density of trajectories. * P. 3. L. 2: "By summing (e-p)..." Do you mean integrating? (I anyway struggle with the units here). Is there any control (e.g., weighting) of the vertical distribution or is it sampled well enough that this is not necessary? Do you need the vertical distribution at all? * Since these are backward trajectories (as mentioned in the abstract) you do not need 2 million but only those that arrive at the ice core locations, right? * Conversely, is the number of particles above a given ice core site the same for all time steps? If not, do you weigh the results somehow?

- The FLEXPART data set used in this study was provided by a global experiment in which the entire global atmosphere was divided into approximately 2.0 million 'particles', and the number of particles per time step was kept constant along the analysis. The approximate number (there is a small variation) of particles found per time step over each of the fourteen ice core sites was included in the table 1 of the manuscript. Stohl and James (2004) state that the estimative of the moisture budget is valid when the number of particles per grid column of the input meteorological data exceeds the number of the layers. The analysis was based on the particles that arrive at the ice core locations.

* For instance, for the sampled particles, "summing up" (e-p) in the vertical will not give (E-P) at the surface (because there may be layers in between whose air travels in a

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different direction and will not reach the ice core site). * The paragraph on P. 2 to 3 on dq/dt implies that you are tracking moisture changes along a trajectory, but the further I read I think you do not. Rather, it seems that you overlay the position of the particles onto a field of E-P which you could have obtained as well from a Eulerian approach (precipitable water tendency plus vertically integrated moisture flux divergence). I admit I have not read Gimeno et al. (2012), but I think the reader should be given more information here. * P. 4, L. 11: "(E-P > 0)" Now I am confused. Why not "(e-p > 0)"? If the assumption is that each particle (i.e., e-p) behaves in the same way as the integrated column (i.e., E-P), then there would be no need to track moisture at all. Simply use (Eulerian) E-P from ERA-Interim and the position of the particle from FLEXPART. Is that what you do? So why do you initially integrate (e-p) rather than taking it directly from ERA-Interim? * P. 5, L. 1: The difference between vertically integrated moisture flux divergence and E-P is the tendency in precipitable water (i.e., storage), which can be neglected over long time periods. So the E-P figure would look identical, right? * P. 4, L. 13: "E-P averaged over the whole tracking period (10 days)" Again, I understand this such that you basically use a (Eulerian) map of E-P and then sample it at the locations and time where air parcels pass it (at any altitude) and then reach the ice-core site within at most 10 days. But you only do that if E-P is positive. It seems that there is no weighting that accounts for unequal distribution for particles. But this should be stated (and justified). I am still confused, though, why you need e-p at all.

- We have detailed in more depth the methodology in the manuscript. By summing (e-p) for all the particles residing in the atmospheric column over a given area A, we obtained the surface freshwater flux (E-P). If we consider all the particles present in the atmospheric column, the results would be similar to the freshwater flux calculated via the Eulerian reference (Stohl and James, 2004). Nevertheless, the Lagrangian methodology allows us to identify the particles affecting a particular region and to calculate the surface freshwater flux (E-P) using information on the trajectories of these selected particles. Figures 3 and S2 of the manuscript are based on (E-P) of the tracked particles averaged over the 10-d period and redistributed on a regular 1° grid.

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These points were clarified in the new version of the manuscript.

* Furthermore, it seems that the vertical dimension is neglected. Let's assume a (climatological) circulation such that, when your trajectories pass over a location where evaporation usually takes place, the trajectories pass mostly at high altitudes and the particles do not actually pick up a lot of moisture whereas the moist lower layers rain out upon reaching the first mountain chain and never reach the ice core site. Wouldn't this matter?

- The vertical dimension is taken into account in this approach. In the model the atmosphere is divided homogeneously into three-dimensional finite elements (hereafter 'particles'), each representing a fraction of the total atmospheric mass (Stohl and James, 2004). These particles may be advected backward or forward in time using three-dimensional wind taken from the Era-Interim. This issue has also been detailed in the revised manuscript.

*Furthermore, since you track the particles for 10 days, how often is the model re-initialised? If it is only initialized once and then everything else is done by sampling, I think it needs to be stated that the trajectories provide a good sample. For instance, after 30 years of simulation all particles might have ended up in the subtropical jet and stay there.

- The particles are advected backward (or forward) in time using data taken from Era-Interim every time step.

* P. 3. L. 11: Are retro-trajectories backward trajectories?

- Yes. However, the term was replaced by backward trajectories in order to avoid any

misunderstanding.

* P. 4, L. 6: What are "target areas": Are these areas where particles end after a 10-day period or an area over which they pass at any time during a 10-day period?

- In this study the ice core sites are the target areas. Our study is based on backward in time trajectories of the particles identified over the ice core sites, the area where the particles end is the ice core site in day 0, and the previous days are day-1, day-2,... day-10. This explanation was added in the new version of the manuscript.

* P. 4, L. 13: "95th percentile" Do I understand this correctly: For each ice core you would show the 5% grid points with the highest E-P (if you showed the annual mean)?

- Yes, the 95th percentile criteria would show the 5% grid points with the highest positive (E-P) values in the annual mean map obtained for each ice core site. This explanation was added in the manuscript..

* P. 4, L. 32: The VIMF is calculated directly from ERA-Interim, right? Or is it from the Lagrangian approach?

- The VIMF is calculated directly from ERA-Interim. It was clarified in the text and in the respective figure captions.

* The first paragraph of the introduction is rather vague as to the processes causing changes in isotope abundances in ice cores: they depend on "local conditions," changes in "relative moisture of the source" (what is that?), "changes in these source conditions," etc. It would be good to be more specific

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- The first paragraph was re-written in order to clarify the points arisen by the Reviewer and the new version is below. The reference Merlivat and Jouzel (1979) has been replaced by a more recent one (Jouzel et al., 2013).

“The most successful reconstruction of past climate has been due to the fact that stable water isotopes are conserved in ice cores (e.g. Jouzel et al., 1982; Dansgaard et al., 1993). The isotopic composition of precipitation, in deuterium, oxygen-18 and oxygen-17, depends on the climatic conditions prevailing in the oceanic regions where it originates (i.e. the sources), mainly the sea surface temperature and the relative humidity of air (Jouzel et al., 2013). The deuterium excess, for example, may be seen as a control parameter of air moisture trajectory history, because it will change when the trajectory moves over regions presenting different moisture conditions (e.g. sea/land, dry/wet land). Deuterium excess variations have been traditionally associated to changes in the temperature of the oceanic sources, but nowadays it is thought to be also related with changes in the relative humidity of the air in the source region (Pfahl and Sodemann, 2014). In any case, deuterium excess variations in ice cores may reflect past changes in the climate conditions of the oceanic sources (e.g. Masson-Delmotte et al., 2005; Steffensen et al., 2008). This information can be very useful to understand changes linked to modifications in the atmospheric circulation because the position and conditions of the moisture sources for precipitation could be altered (e.g. Masson-Delmotte et al., 2005). That is why the knowledge on the transport of moisture is crucial for the interpretation of stable isotopes in precipitation and in paleo-archives through the understanding of the physical climatic processes involved (Sodemann and Zubler, 2009).”

Jouzel, J., Delaygue, G., Landais, A., Masson-Delmotte, V., Risi, C., and Vimeux, F.: Water isotopes as tools to document oceanic sources of precipitation. *Water Resour. Res.*, 49, 7469–7486, doi:10.1002/2013WR013508, 2013.

* P. 2. L. 29: Give a reference for MERRA.

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- The reference was included: Rienecker, M.M., Suarez, M.J, Gelaro, R., Todling, R., Bacmeister, J., et al: MERRA: NASA's Modern-Era Retrospective Analysis for Research and Applications. J. Climate, 24, 3624–3648. doi: <http://dx.doi.org/10.1175/JCLI-D-11-00015.1>, 2011

* P. 2, L. 19: "unrealistic fluctuations in humidity can be taken as moisture fluxes": I do not understand that.

- When applying a time derivative of the humidity, the numerical errors associated with the temporal variations in the moisture present in a particle can be taken as moisture fluxes. In consequence, if the reanalysis data used to drive the method do not properly close the water budget, then the method may suffer from considerable inaccuracies(Gimeno et al., 2012).

* P. 3. L. 23: "low data density": Do you mean the number of particles or the quality of the reanalysis there.

- We refer to regions with low observational data coverage. It was explained in the text.

* P. 10, l. 16: Anchmann -> Auchmann Done

* P. 11, last reference: Chack names. Corrected. Thanks.

* P. 13, Table 1: couverture -> coverage Done. Thanks.

* Fig. 2: The arrows do not help very much; perhaps make them larger Done.

Interactive comment on Earth Syst. Dynam. Discuss., doi:10.5194/esd-2015-97, 2016.

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