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Climate of the Past Discussions

Interactive comment on "Advection (non-climate) impact on the South Pole Ice Core" *by* Tyler J. Fudge et al.

Anonymous Referee #2

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General comments

This paper discusses measurements of present-day accumulation rate, water isotopes, surface velocity, and 10 m temperature in an area upstream of the SPICEcore drilled at South Pole to estimate the effect of advection on climate histories inferred from the ice core itself. The analysis is interesting and discusses many of the relevant effects one needs to take into account to separate the climate information in an ice core from effects introduced by elevation change when the ice core is drilled away from an ice divide. The analysis is however not very sophisticated as it is almost solely based on qualitative reasoning and no use is made of any ice flow modelling, which would be the more appropriate tool to quantify the advection effect. This results in a lot of hand-waving and questionable assumptions underlying the analysis.

More specifically I feel the paper has 3 major problems which makes the paper not publishable in its present form.

Most crucially the authors assume that accumulation rates are kept constant to their Holocene values for ice older than 10 ka, and consequently, ice velocities keep their present-day values along the presumed flowline for the last 55 ka. Even though the authors acknowledge there are good reasons to believe this is not the case (p. 14, lines 278-292), they ignore ice velocity variations citing Pollard and DeConto (2009). However, Pollard and DeConto (2009) do not have a figure in their paper showing the glacial ice flow pattern. Their supplemental material video V3 only shows details of the ice velocity during the last 8800 years. Even simple models will show that accumulation rates, and consequently, ice velocities, should be roughly halved during the glacial period. This bears directly on the determination of the location of ice deposition over time, the crucial underpinning of the paper. At the very least, the authors should have presented an alternative distance traveled vs time assuming lower velocities during the glacial period and the glacial-interglacial transition. Equally the cascade of assumptions made for ice deposited beyond 70 km (constant present-day flow direction, the unconstrained straight flow line for ice older than 21 ka, the linear decrease of ice velocity for the oldest part) are so rough that the conclusion that the oldest SPICEcore ice originated $\sim$35 km downstream from the assumed divide position (p. 15, lines 306-307) is hard to believe.

Secondly the paper only discusses the impact of advection and ignores elevation changes from ice dynamics. Therefore absolute statements on the temperature correction required to interprete the climatic information in SPICEcore can not be made.

Thirdly, there is much overlap with Lilien et al. (2008) concerning the discussion of the measurements on which the analysis is based. Even though the focus of the current paper is different, and the time period considered is longer, this introduces unnecessary duplication of material.

Specific comments

Abstract, p. 2, lines 23-24: 'Assuming a lapse rate. . .'. The statement is ambiguous: the LGM-to-modern temperature change is a fixed climatological quantity, and cannot depend on the place of deposition. What is meant here is that the apparent LGM-to-modern temperature change from the core is 1.5°C lower than would have been the case if the ice was deposited locally. Also this number ignores the contribution to elevation changes from ice dynamics and should not be misinterpreted as the total temperature change.

p. 4, Figure 1: One would expect the flow directions obtained from the GPS measurements to be perpendicular to the elevation contours, but that is apparently not the case for quite a few of the arrows shown. Why is that? Errors in the drawn orientation of some of the arrows, errors in the plotted surface contours, or another genuine reason? If so, which one?

p. 4, Figure 1: why does the BedMap2 surface elevation for ITASE 07-04 deviates from the GPS measured 3090 m? Can it be a mix-up of ellipsoidal versus geoidal heights?

p. 5, line 112: the assumption is made that the there is no shearing in the upper 1750 m of the ice column. The validity of this assumption needs more discussion as it depends on the thickness of the ice along the flowline, i.e. to what fraction of the total thickness the ice was located along the flowline for a certain depth at the drill site. I would like to see the thickness along the presumed flowline together with an estimate of the depth of the deepest trajectory for the oldest SPICEcore ice at 1750 m. The authors should then at least discuss the no shearing assumption based on the vertical distribution of horizontal velocity. For isothermal ice, there is an analytical expression for the depth dependence of horizontal velocity that can be found in any textbook on ice dynamics, and this would give an estimate of the maximum possible deviation of the horizontal velocity at depth from its surface value.

p. 12, Figure 5: the red and blue lines for d18O and dD respectively, and the symbols

for individual measurements, almost overplot one another because of the respective axis scaling. For better readability, the authors could opt to show both variables separately in two adjacent plots.

p. 13, line 254: 'consistent with a dry adiabatic lapse rate'. As the authors acknowledge, the inferred surface temperature gradient is imprecise because of the duration the thermistors were left in the boreholes. Nevertheless, one would expect a higher lapse rate than dry adiabatic on the Antarctic plateau because of the strength of the surface inversion layer that increases with lower temperatures. This should be discussed. What are the implications of this rough estimate of the lapse rate for the analysis?

p. 15, line 305: 'assuming a balance velocity in an ice sheet with uniform thickness'. Please discuss how good this assumption is based on available ice thickness reconstructions/ measurements for this area. What does BedMap2 show for ice thickness along the presumed flow line? See the comment on the no shearing assumption above.

pp. 16-17: Advection impact on water isotopes. This analysis evidently ignores the effect of elevation changes over the presumed flowline for the period since 55 ka due to ice dynamics. Pollard and DeConto (2009) show the evolution of surface elevation over the glacial cycles from which a rough estimate of this effect could be made?

p. 18, lines 377-379: 'advection has enhanced the glacial-interglacial . . . by 1‰' This is only true if you assume that the present-day spatially derived elevation gradient of d18O also holds back into time. This should be discussed.

p. 18, line 379: what is the status of Steig et al. (in prep.)? Otherwise use a published reference here.

p. 18, line 381: what is 'WDC'?

p. 19, line 419: 'originated at elevations up to ∼250 m higher': this is only true for the advection part and so this inference in absolute terms cannot be made here. Elevation

changes due to ice dynamics not considered in the paper must have contributed as well to the total elevation change.

Technical

A space should be left between value and unit, e.g. 9 m instead of 9m (except for °C).

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