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Interactive comment on "Strong changes in englacial temperatures despite insignificantchanges in ice thickness at Dôme du Goûter glacier (Mont-Blanc area)" *by* Christian Vincent et al.

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This manuscript is of wide interest since it reports on the physical change due to warming of a high altitude Alpine snow field using a multi-methods approach. The reported change is similar to what is happening in other high altitude snow fields as given by the references in the manuscript, but also actual in polar settings with mass and energy are relocated from the surface into the interior of the snow field by latent heat release from percolating meltwater. As such it may bring an example of the future development of these high altitude ice fields, and how this can be implemented by models used on

polar ice fields. With that, the question risen in the title may not be too surprising for a student of polar ice fields, since vertical relocation of mass, rather than horizontal runoff or sublimation, is the primary vector.

The manuscript is well organized and well written, and the conclusions are well supported by the data presented, although some questions appear and below are suggestions for further improvements.

Suggestions:

1. It is not really clear on how the ice velocity data was calculated, and what exact uncertainties were involved for each measured velocity vector. From section 2.2 it is understood they were drilled in nine times, but the velocity intervals are only three. How was the ice motion cumulated from the nine observations into the three intervals? It is further stated in the text that the stakes was replaced at the same place each year of replacement. Does this mean that the stake was replaced on more or less the same Cartesian coordinate at the start of each of the nine replacements, or only in the start of each of the three periods? This is an important part of information since the motion downhill integrates the velocity field in a Lagrangian fashion. This is not a problem for small displacements, but if speeds are large enough, or the time period is long enough the ice speed marker may be advected into an area where the dynamic situation may be different. If the stakes was allowed to flow downhill for 10 years this may be more than 100 m of displacement, and in such complex topography this may be a factor to consider. This is especially sensitive for the emergence velocities, since these velocities are calculated using surface slopes. My suggestion would be to present a table (perhaps as a supplement, or at some depository) in which all the measured ice velocities are presented and another where they are cumulated.

2. Another issue with the ice velocity measurements is the more generic uncertainties used in the figures. It is quite understandable to use this wider error bars from the older theodolite measurements, but the DGPS measurements give better resolution as

reported in li 98-99. There vertical and horizontal uncertainties vary for each measurement and can be reported specifically for each measurement in the table suggested above (or in some depository). It may be so that the tilt of the stakes is much larger than the DGPS uncertainty, so this reporting seem meaningless. But if the tilt was reported for each measurement, this can be accounted for.

3. A third issue with the velocities is the question of false vertical motion of the stakes that sometimes is a problem of measurements on snow fields. This “self-drilling” may happen if the stake gets warm enough to pressure melt itself vertically. Usually this is less of a problem if using a low conducting plug at the base of the stake, or having a long enough stake that penetrate well into the firn. Was this a potential problem at this site? I guess there may possibilities for warm / high radiation days on this site.

4. The data presented in the Discussions section (Figures 8 and 9) should be introduced in the Methods section, and such be dimensioned properly. As I understand the data in these two figures are not a product of already published work, so they deserve a short section in the Methods section. As presented they appear in a somewhat ad hoc fashion. Perhaps these two figures were a late addition to the manuscript, and such not yet fully baked into the structure? As written now it is not clear what data was used to generate Figure 9. Was it snow/firn column data, or SAT data?

5. It was interesting to see that the density profile did not obviously change between 1994 and 2012. At least this is not possible to find out by an ocular inspection. Did it change using a statistical analysis? The temperature development in the firn column is most likely due to latent heat release from percolating melt water, but this is not shown as a densification of the column. On the other hand the water volumes percolating proportional to 1.2 mwe may be distributed and contained in the full profile without a large change of the average value. Perhaps this can be further discussed, and perhaps calculated? This may be another part in the question of the relocation of mass.

Minor comments: 1. Abstract. Please add the time period for which the study encom-

pass in the abstract. 2. Li 269. How was the number flux change 9.7% calculated? Should it be 7.7%? 3. Li 277-278. How was the argument made? What is the link between SMB and ice thickness and velocity? Please provide the equation of how this was calculated. 4. Li 312-313. I do not understand the negative feedback mechanism. Please rephrase and/or expand. 5. Figures 4 and 6. Perhaps better use Observed rather than Measured for velocities.

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