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The Cryosphere
Discussions



# *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.

**Christian Vincent et al.**

christian.vincent@univ-grenoble-alpes.fr

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Response to Reviewers:

We thank the Reviewers for their comments and suggestions to improve this manuscript. We address their comments below. Reviewers comments are in italics, and our responses are in normal font below. Changes to the text have been highlighted in the revised manuscript.

Reviewer 2 : Veijo Pohjola

This manuscript is of wide interest since it reports on the physical change due to warming of a high altitude Alpine snow ïňĄeld using a multi-methods approach. The reported change is similar to what is happening in other high altitude snow ïňĄelds 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 ïňĄeld by latent heat release from percolating meltwater. As such it may bring an example of the future development of these high altitude ice ïňĄelds, and how this can be implemented by models used on polar ice ïňĄelds. With that, the question risen in the title may not be too surprising for a student of polar ice ïňĄelds, 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?

The stakes used for velocity measurements were set up in 1997, 1998, 1999, 2002, 2003, 2004, 2008, 2009 and 2016. We compared the velocity measurements over three periods : 1997-2004, 2009-2011 and 2016-2017. For each period, all measurements were used without any average. In other words, we assume that the ice flow velocities are similar within each period. It is reasonable for the last two periods which cover 2 years and one year respectively. It could be questioned for the first period which cover 7 years. The network of this first period is dense but the locations of stakes are not similar each year. Consequently, the comparison of the ice flow velocities is not easy within this period because one cannot make the comparison stake by stake at each location. However, the locations of stakes were similar in 1997 and 2004 making possible a stake-by-stake comparison. The velocity measurements in 1997 and 2004 are not significantly different and we assume that the 1997-2004 ice flow velocities did not change significantly within this period. For the comparison between the periods, the ice flow velocities of the period 1997-2004 have been interpolated over the whole area (Figure 4). We added explanations in Section 3.2 of the manuscript. “Thanks to the numerous measurements performed between 1997 and 2004, the ice flow velocities have been interpolated over the whole colored area shown in Figure 4. By comparing the velocities measured at similar location during this period, we conclude that the ice flow velocities did not change significantly between 1997 and 2004. “

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 ïňĄeld 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 ïňĆow 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.

Completely agree, it is crucial to compare the ice flow velocities values at the same locations as mentioned in Reply to the previous comment. For the first period 1997-2004, the stakes locations are not always the same. For this reason, the ice flow velocities have been interpolated over the whole area in order to obtain a spatial field of ice flow velocities which can be compared with other periods. After 2009, the stakes were always set up at the same locations. In this way, we can accurately compare the ice flow velocities over three periods, 1997-2004, 2009-2011 and 2016-2017 at the same locations, i.e. the locations of stakes set up in 2009 and 2016. For the sake of clarity and as suggested by Reviewer, a Table has been added in Supplement.

2. Another issue with the ice velocity measurements is the more generic uncertainties used in the ïňĄgures. 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 line 98-99. There vertical and horizontal uncertainties vary for each measurement and can be reported speciïňĄcally 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.

Agree. The main uncertainty of position measurements is related to the tilt of the stakes. Unfortunately, the tilt has not been reported for each measurement. Here, we estimated the maximum uncertainty of 1 m and 0.1 m in horizontal and vertical displacement respectively. The uncertainties mentioned in our manuscript are probably pessimistic for most of the cases.

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 ïňĄelds. 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 ïňĄrn. Was this a potential problem at this site? I guess there may possibilities for warm / high radiation days on this site.

Our wooden stakes are 5 m long and 10 cm in diameter. Their thermal conductivity is low and no enhanced melting was observed at their base. In addition, the temperature of the firn ranges between -5 and -15°C. Again, the main uncertainty of position measurements is related to the tilt of the stakes. We added a sentence in Section 2.2 about this point: “Another uncertainty could come from a “false” vertical motion due to the warming of the stake from solar radiation. Given that the stakes are wooden stakes 5 m long and 10 cm in diameter with a low thermal conductivity and that the temperature of the firn ranges between -5 and -15°C, we assume this effect is negligible.”

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 ïňĄgures 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 ïňĄgures 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/ïňĄrn column data, or SAT data?

We are not sure to fully understand this comment. Figures 8 and 9 are not related to Discussions. In addition Figure 8 shows the englacial temperatures which have been described in details in Data Section. Figure 9 shows the reconstructed temperatures which have been compared to Lyon Bron and Chamonix temperature records. We assume that the comment of Reviewer is related to Figure 10 (and partly to Figure 9 about air temperature records) which shows accumulation/precipitation changes with time in the Mont Blanc area. We agree with this comment: the meteorological data and mass balance data are not mentioned in Data Section. Consequently, we added a new Section 2.4 in Data Section : “2.4 Mass balance and meteorological data In addition to the observations performed at Col du Dome, glaciological and meteorological observations carried out in the Mont Blanc area have been used in this study. As part of the GLACIOCLIM observation facility (https://glacioclim.osug.fr/), winter and summer surface mass balances are measured each year on two glaciers in the vicinity of Col du Dome since 1995 (Argentière and Mer de Glace), using the glaciological method (Cuffey and Patterson, 2010). Winter surface mass balances used in this study are located in the accumulation zone of the glaciers (>3000m) and are measured at the end of April using snow cores and density measurements. We also used annual and winter precipitation data from the SAFRAN reanalysis (Système d'Analyse Fournissant des Renseignements Adaptés à la Nivologie, System of analysis for the provision of information for the science of snow). This data set is available back to 1958 (Durand and others, 2009). SAFRAN disaggregates large-scale meteorological analyses and observations in the French Alps. The analyses provide hourly meteorological data for the Mont-Blanc range, as a function of slope exposures and altitude (at 300 m intervals). In this study, we used SAFRAN precipitation determined at 4300 m a.s.l."

5. It was interesting to see that the density proïňĄle did not obviously change between 1994 and 2012. At least this is not possible to ïňĄnd out by an ocular inspection. Did it change using a statistical analysis? The temperature development in the ïňĄrn column is most likely due to latent heat release from percolating melt water, but this is not shown as a densiïňĄcation of the column. On the other hand the water volumes percolating proportional to 1.2 m we may be distributed and contained in the full proïňĄle 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.

Agree, from Figure S1, it is not obvious that the density did not change between 1994 and 2012. For the sake of clarity, we calculated the density difference between 2012 and 1994 for each interval of 10 meters. We added a new panel in Figure S1in the Supplementary material. If we assume that the density uncertainty is about 5% of density (Thibert et al., 2008), one can conclude that the density differences are hardly significant. The snow density did not change significantly over the first 30 meters deep and did not affect the calculated submergence velocities. However, one can notice that the layer of 1994 is about 65 m deep and that the density difference is 0.03 between 30 and 65 m deep, which is 4.6% of density. Between the surface and 65 m deep, the mean density difference is 0.016. In addition, the modelled average surface melting is 0.10 m w.e. a-1 between 1994 and 2012. The modelled average surface melting shows an increase of 0.02 m w.e. a-1 between 1994 and 2012 compared to the period 1976-





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1994. For a net annual accumulation of 2.7 m w.e. a-1, it corresponds to an increase of 0.004 in density which is lower than the observed change. It is therefore unlikely that melting increase is responsible for the observed slight density increase that is may be rather linked to reduced snow accumulation. Some explanations have been added in the manuscript (Section 4, Discussion) : "From these measurements, it can be seen that the snow density did not change significantly over the first 30 meters deep. It means that the calculated submergence velocities are not affected. However, we can detect a mean density difference of 0.016 between the surface and 65 m deep (Fig. S1) that correspond to the firn accumulated between 1994 and 2012. The modelled average surface melting shows an increase of 0.02 m w.e. a-1 between 1994 and 2012 compared to the period 1976-1994. For a net annual accumulation of 2.7 m w.e. a-1, it would correspond to an increase of 0.004 in density, which is lower than the observed change and not detectable from density measurements. It is therefore unlikely that melting increase is responsible for the observed slight density increase that is may be rather linked to reduced snow accumulation." A panel in Figure S1 has been added in the Supplementary material to show the mean density difference. We thank Reviewer 2 for this relevant comment.

Thibert E., R. Blanc, C. Vincent, N. Eckert. Glaciological and volumetric mass balance measurements: error analysis over 51 years for Glacier de Sarennes, French Alps. (2008). Journal of Glaciology, 54 (186), 522-532

Minor comments:

1. Abstract. Please add the time period for which the study encompass in the abstract.

It has been done.

2. Li 269. How was the number ïňĆux change 9.7% calculated? Should it be 7.7%?

The flux change is affected by both velocity and thickness changes. The mean horizontal velocity of the cross section has decreased by 7.7%. The thickness has decreased by 2 %. Consequently, the ice flux has decreased by about 9.7 % (product of thickness and velocity decreases).

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.

According to Glen's flow law and the laminar flow assumption (Cuffey and Paterson, 2010, Equation 8.36 p.310), the depth averaged horizontal ice velocity is proportional to ïĄą3 and H4 and therefore the ice flux to ïĄą3 and H5 ,where ïĄą is the surface slope and H is the glacier thickness. This means that, to a first order approximation and in the absence of large slope changes with time, the relative change in ice thickness (in %) is a power 1/5 of the relative flux change or relative change in surface mass balance (in %). Therefore, the ice thickness is not highly sensitive to surface mass balance. For instance, a thickness change greater than 10% on the Dome du Goûter ice cap would require a surface mass balance change of more than 60%. We did not add any Equation in our manuscript but we added some explanations and added the reference of Equation 8.36 in (Cuffey and Paterson, 2010, p.310): "According to Glen's law and the laminar flow assumption (Cuffey and Paterson, 2010, Equation 8.36, p.310), the depth averaged horizontal ice velocity is proportional to ïĄą3 and H4 and therefore the ice flux to ïĄą3 and H5 ,where ïĄą is the surface slope and H is the glacier thickness. This means that, to a first order approximation, the relative change in ice thickness (in %) is a power 1/5 of the relative flux change or relative change in SMB (in %) in the absence of large slope changes."

4. Li 312-313. I do not understand the negative feedback mechanism. Please rephrase and/or expand.

We expended the explanation to make clearer the mechanism we wanted to describe: "Indeed, the increasing refreezing rate could start to create impermeable ice layers that prevents meltwater to percolate deeply in the firn. As the refreezing would occur closer to the surface in this case, the energy added by latent heat would be lost in winter in comparison with the case where latent heat is released deeper, below an insulating snow layer. This effect would significantly limit the effect of meltwater refreezing on firn temperature."

4. Figures 4 and 6. Perhaps better use Observed rather than Measured for velocities.

It has been done.

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