We would like to thank the two anonymous reviewers for their reviews of our paper. In response to their suggestions, we did the following main changes to the manuscript:

1) the title was modified replacing 'glacier disintegration' with 'glacier fragmentation'
2) the three simpler and empirical methods for the calculation of the air temperature over glaciers (Khodakov, 1975; Davidovich and Ananicheva, 1996; Braithwaite et al.,2002) have been removed, as suggested by the Reviewer 1
3) as Reviewer 1 notes, we incorrectly choose weather stations with different flow path lengths for supporting the hypothesis of reduced cooling effect over smaller glaciers. The first part of the Discussion has been modified, comparing weather stations with similar flow path lengths.
4) a figure with the flow path length of the studied glaciers has been added to the manuscript

Reply to the Interactive comment by Referee 1 (anonymous)

R1-1) The authors present a new dataset from several high-elevation weather stations installed during summer 2010 and summer 2011 to study the air temperature distribution over three glaciers in the Italian Alps. This work aims to provide a comparison of different methods for calculating on-glacier temperature from off glacier data. The methods are commonly applied by mass balance models forced with off-glacier data and the paper demonstrates how the accuracy of air temperature estimations impacts the outputs of such a model. Overall the paper is well written. The motivation and methodology is clear and well described. However, on the basis of the data presented, I do not see any new insights regarding the effect of glacier disintegration on air temperature variability. Title, abstract and conclusion 1 suggest that the paper provides new experimental evidence about the changes in the air temperature field during glacier decay. This finding is supposedly supported by stronger cooling effects observed in the ablation area of the larger La Mare Glacier than on the very small Careser Occidentale Glacier. However, the authors ignore in their discussion that this difference in temperature depression can be very well explained by differences in flow path lengths (FPL), which is the most important variable of the tested methods to extrapolate air temperature over a glacier. The FPL explains perfectly the differences in the cooling effect observed at Car-gl_3144 (FPL 354 m, cooling effect -0.18°C) and Mar-gl_3140 (FPL 805 m, cooling effect -0.47°C) or Mar-gl_2973 (FPL 2132 m, cooling effect -0.9°C) in summer 2011. There is indeed no sign of a "reinforcement mechanism during glacier decay" (page 6148, line 13), and glacier disintegration seems to have no effect at all on air temperature variability in function of FPL. This is my major concern and I therefore doubt that the title of the paper reflects the content of the paper and major findings of the study.

As noted by the Reviewer, the FPL explains the differences in the cooling effect observed at the mentioned weather stations, and their choice was not appropriate for supporting our findings. Actually, they are supported by the stronger cooling effect observed on the larger Careser Orientale Glacier (-1.01°C at Car-gl_3082) than on the smaller Careser Occidentale Glacier (-0.18°C at Car-gl_3144), because the two sites have very similar FPL (Table 1). We have modified the first part of the Discussion accordingly.

R1-2) For a revised paper, the authors should consider calculating glacier-wide summer glacier mass balances and not only point mass balances at stake locations like in the current paper. The main interest of the different methods to distribute air temperature over a glacier is at the distributed scale, and not at the point scale. I also think the meteorological data that are available have the potential to provide insights for distributed modeling beyond the discussion of existing methods for air temperature distribution. In the current manuscript the authors stick to the methods available from literature although it is known that the methods are not valid for the specific conditions. For instance the G&B model is not valid close to the glacier borders (the G&B model only considers two processes of an air parcel traveling down an infinite glacier slope: adiabatic heating and turbulent cooling), and the G&B model is also not applicable to Car-gl *since this glacier does not have a down-glacier wind, which is the main assumption of the model. In a revised paper the authors could therefore address several research questions regarding this issue: What would be the best modeling strategy for sites like Mar-gl_2709 or the Careser glaciers? How relevant is it for distributed glacier mass balance modeling to take into account 'border effects' on air temperature? The authors are aware of these open questions as they make clear by conclusion 2 ("these methods... still need refinements, in particular for areas close to the margins and for the smaller units..."). The paper would be much more interesting if some of these questions could be answered or at least addressed and if the relevance of these open questions for distributed mass balance modeling could be clarified*.

We intended this paper as a first step of assessment of existing methods available in the literature. Based on these results, in a following step we aim to develop a generalized modeling strategy, to be implemented in a distributed mass balance model. However, probably more data are required to do that, and to assess the relevance and generalizability of our peculiar observations (i.e. the border effect at Mar-gl_2709, up-glacier wind at Car-gl_3082, reduced cooling at Car-gl_3144). For example, further investigations are needed to check if the up-glacier wind and relatively strong cooling effect at Car-gl_3082 are specific for this site, or if they are common features over the Careser Orientale Glacier and over glaciers with similar characteristics. For this reason, we preferred to test existing methods and to avoid developing a generally valid temperature transfer function. Moreover, the experimental setup was not intended to investigate in detail any peculiar aspect of the glacier boundary layer, which would probably require a higher density of measuring sites and different instrumentation. We added these considerations in the Discussion.

R1-3) Based on the text it seems that the method by Khodakov (1975) is valid only for the location of the firn line. It is not clear to me how this method can be used to calculate the cooling effect at all the stations. Is L in equation (1) equal to the Flow Path Length (FPL) or is it a constant?

R1-4) *A comment on the first three methods: All of them are empirical and the coefficients were calculated in very different environments. In my opinion, they oversimplify the problem and are clearly inferior to S&M and G&B methods, which reflect a better understanding of the physical processes involved in the air temperature distribution over melting glaciers. They are also not commonly applied by mass balance models. I wonder if it is necessary to include them in the paper*.

We agree with these considerations and removed these methodologies from the paper. Now they are only mentioned in the Introduction. The reasons why they have not been used (suggested by the Reviewer) were added in the Methods (par. 3.4)

R1-5) Title: not sure if 'glacier disintegration' is the correct term here. Maybe 'glacier fragmentation' or 'glacier retreat' would be more appropriate.

The title was modified replacing 'glacier disintegration' with 'glacier fragmentation'

R1-6) 6148 25: What is snowfall limit?

Ok, specified (snowfall '*elevation'* limit)

R1-7) 6149 20-30: Maybe more recent references than Charbonneau (1981) and WMO (1986) would be more appropriate.

Here we have cited four works that, in different epochs during the last decades, analyzed and assessed the issue of input data spatialization in model applications. Indeed in the following we cite the papers by Machguth et al., (2008) and Carturan et al., (2012).

R1-8) 6152 10: Which was the explanation gave by Petersen et al (2013) for this result?

They attribute this result to the fact that the thickness of the boundary layer is variable in space. Added in the text.

R1-9) 6152 17: Indicate by numbers what you consider small, medium and large glaciers

Ok, added

R1-10) 6153 22: What do you mean by "active" retreat?

Added ‘towards higher altitudes’. The concept of active retreat is well explained in the paper by Small, (1995), which is already cited.

R1-11) 6156, Section 3.3: It seems most of this is repeated later in section 4.2. Consider removing this section.

In section 4.2 we explain in more detail the tested methods for the calculation of ambient temperature. In particular, in 4.2 we detail the phrase ‘Different combinations of lapse rates (i.e. fixed standard or hourly-variable obtained by linear regression of temperature vs. elevation) and subsets of weather stations were tested’ which is reported in 3.3. We considered the opportunity to move the explanation (in 4.2) just after this phrase (in 3.3), but we think that the explanation fits better in 4.2, because it enables improved comprehension of the results reported in the following. On the other hand, Section 3.3 is required to explain how the data have been analyzed.

R1-12) 6160: The mass balance model only considers clear sky radiation and not the daily cloudiness. Daily variations in cloudiness therefore represent a source of error for mass balance calculations. Since incoming shortwave radiation is measured at the AWSs, why not considering daily cloudiness for mass balance calculations?

We did not use the radiation measurements because we wanted to test a ‘general purpose’ model, applicable to glaciers with limited data availability. Often, the only available variables are the air temperature and the precipitation, whereas the incoming shortwave radiation is less commonly available. We tested a parameterization of the cloud cover in function of the air temperature, as suggested e.g. by Pellicciotti et al., (2005), but we didn’t find significant correlation between the two variables. Explanations added in the text.

R1-13) 6161 15 and elsewhere: I think is better to say that the lapse rates are ’steeper’ and not ’lower’.

Ok, replaced

R1-14) 6161 19-22: Here it is not clear when you are talking in general and when you refer to your data. Please re-phrase.

Ok, clarified

R1-15) 6161 20: Write standard deviation instead of SD.

Ok, replaced

R1-16) 6162 15: You do not know which is the best method to extrapolate above your highest off-glacier station (Bel_3328) since you have no data from there. Please re-phrase.

Based on our data, method (ii) provides the best results, but we agree that uncertainty persists in extrapolations at altitudes above the available weather stations. Rephrased for clarity.

R1-17) 6166 9-11: Could you explain better why x0 is larger (x0=1440) when the freezing level is above the top of the flow line than when the freezing level is below this point (x0=0)? This is not clear to me.

These settings were suggested in the paper by G&B. When the freezing level is above the top of the flowline, in order to take a climate sensitivity <1 at the top of the flowline into account, a distance x0 = 1440 m is added to the distance along the flowline. Added in the text.

R1-18) 6168 13: I do not agree that these results provide a quantification of mechanisms during glacier disintegration. Differences in the glacier cooling effect between Margl_2973 and Car-gl_3144 can be explained by differences in the FPL (see general comments above).

As explained in the reply to the first comment, the comparison between these two weather stations was not appropriate to support our findings, because their different cooling effect can be explained by differences in FPL. The comparison between Car-gl_3144 and Car-gl_3082 is more appropriate because they have almost equal FPL. We have modified this part of the Discussion accordingly.

R1-19) 6169 5-6: The loss of sensible heat does not have the opposite sign in the case of up-glacier wind.

We meant that, in katabatic flows, the loss of sensible heat is to some extent compensated by the adiabatic heating of descending air. Rephrased for clarity

R1-20) 6170 10-12: It needs to be mentioned that those methods also fail because there are other processes, apart from glacier cooling, influencing temperature at those sites.

These methods have been removed in the revised version of the paper (see the reply to comments R1-3 and R1-4).

R1-21) I think that the paper needs a Figure with the FPL of the studied glaciers. This is a key variable for all the methods (or at least S&M and G&B).

Ok, figure added.

R1-22) Figure 7 caption text: 'summer 2010 and 2010', please correct.

Ok, corrected.

Reply to the Interactive comment by Referee 2 (anonymous)

R2-1) The paper addresses the amplification of glacier melt while glaciers start to disintegrate and tries to explore a potential way to quantify this effect. This is definitely of interest for e.g. estimating rates and duration of melt water production in catchments where glaciers are about to disintegrate. At the same time, if going beyond simple parametrizations of gross amounts of melting, this is a non-trivial endeavor. The authors try to make use of an unusual data set, collected from 8 different weather stations both off and on the glaciers of the rapidly disintegrating ice bodies of the Careser-La Mare basin. The data availability and the interesting question are definitely motivational and, hence, the authors try to (i) improve mass balance modeling by (ii) developing a generally valid temperature transfer function from a measurement site to a glacier surface, based on (iii) analyzing the effects of glacier disintegration on near surface temperature distributions from the available records. Unfortunately, the authors fail to reach any of these targets due to incorrect assumptions and consequent misconceptions.

The implicit goal of this paper is obviously to provide a contribution to the improvement of distributed glacier mass balance models. However, as reported at the end of the Introduction, the specific aims are the following: i) analyze the temporal and spatial behavior of air temperature and glacier cooling effect in the study area, ii) test different methods for calculating on-glacier temperatures from off-site data, and iii) evaluate their impact in mass balance simulations using an enhanced temperature-index model.

As remarked by the Reviewer 1, in this paper we do not develop any general transfer function. At the contrary, we test *already-existing formulations* proposed in the literature (Khodakov, 1975; Davidovic and Ananicheva, 1996; Greuell and Böhm, 1998; Braithwaite et al., 2002; Shea and Moore, 2010) for calculating distributed fields of air temperatures over glaciers, and assess their impact on a existing enhanced-temperature index model, whose general approach is commonly used for applications on glaciers given the low requirement of input data (e.g. Cazorzi and Fontana, 1996; Hock, 1999; Hock, 2005; Pellicciotti et al., 2005; Huss et al., 2008a). We have clarified the objectives of the paper writing explicitly that the tested formulations and model already exist.

R2-2) Both the "glacier cooling" and the "glacier damping effect" over melting glacier surfaces, as compared to the environmental temperature, are the result of the melt rates. This is the principle of why positive degree day (PDD) type models function so successfully and the reason why they should only be used with temperatures measured outside the influence of the glacier. The sum of positive (above 0 C) Celsius temperatures measured outside the glacier stands for the environmental energetic potential for melting ice. Using on-glacier temperatures (being permanently close to the 0 C of the melting ice surface) can only weaken the potential of a PDD approach. There is, consequently, no reason for knowing the near ice surface temperature beyond the interest in the dynamics of katabatic winds and the respective role in a fully process resolving energy balance study.

We agree that, in principle, PDD model applications should use input temperatures measured outside the thermal influence of the glaciers, which can represent daily variations in the global radiation flux better than the damped boundary layer temperatures above the melting glaciers (Gudmundsson and others, 2009). However, given the very high spatial and temporal variability of degree-day factors, (in particular in steep mountain terrain, Hock, 2003, and references cited therein), and the interest in spatially distributed melt estimates, there is increasing need for *spatially distributed* temperature-index models (Hock, 2005). Enhanced Temperature-Index models (ETI models) are distributed models which vary degree-day factors in a fully distributed manner, using computed solar radiation. Such distributed models rely on accurate estimations of the air temperature, which is usually extrapolated from off-glacier weather stations using environmental lapse rates, assumed to be constant in space and time (e.g. Klok and Oerlemans, 2002; Hock and Holmgren, 2005; Machguth et al., 2006; Huss et al., 2008b; Farinotti et al., 2012). Measurements performed over melting glaciers, however, demonstrate that this assumption is not valid (e.g. Greuell and Böhm, 1998; Strasser et al., 2004; Shea and Moore, 2010; Petersen and Pellicciotti, 2011; Petersen et al., 2013; this work). Compared to environmental conditions, the peculiar glacier boundary layer leads to lower temperatures, different lapse rates and, most importantly, lower 'climatic sensitivity' (i.e. the ratio of changes in the 2 m temperature above a glacier to changes in temperature outside the thermal regime of that glacier), with significant inter/intra glacier variability. Neglecting these differences has strong impacts on model calibration/application (Marshall et al., 2007; Minder et al., 2010). In addition, as the cooling and damping effects are mostly related to the size of the glaciers, and as glaciers adjust their size in response to climatic changes, these processes are important feedbacks which modulate the response of glaciers to climatic changes (Khodakov, 1975; Greuell and Böhm, 1998; Paul, 2010; Shea and Moore, 2010). This is the reason why several authors have attempted to improve the on-glacier temperature calculation from off-site data, before applying PDD and ETI models (e.g. Khodakov, 1975; Davidovic and Ananicheva, 1996; Greuell and Böhm, 1998; Braithwaite et al., 2002; Shea and Moore, 2010; Petersen et al., 2013). Our results (Table 6, Figure 9) and previous model applications (Carturan et al., 2012) clearly demonstrate that, neglecting the dominant processes involved in the spatial distribution of the air temperature over glaciers, lead to recursive spatial clustering of simulation errors and to distortions in parameter calibration.

R2-3) Extrapolating from environmental temperatures to near glacier surface temperatures in order to feed an empirical statistical model, such as PDD day models of any degree of complexity, for estimating melt rates means weakening the potential of such a model. The centrally cited authors Greuell and Böhm (1998), who have explored the temperature distribution on a glacier surface with great care, state this very clearly when they say: "....if a constant lapse rate is used to compute 2 m temperatures above the glacier from temperatures recorded at climate stations or predicted by atmospheric models, the sensitivity of ablation to variations in atmospheric temperature will be overestimated'.

We think that the phrase from Greuell and Böhm (1998) does not support the statements of the Reviewer. Air temperature extrapolation from climate (also said 'environmental', or 'off-glacier') weather stations is required in distributed mass balance models, and can be performed in different ways. Greuell and Böhm (1998) are speaking about the use of constant (environmental) lapse rates to compute 2 m temperatures above the glaciers from temperatures recorded at climate stations or predicted by atmospheric models. According to them (and to other authors as for example Strasser et al., 2004; Brock et al., 2010; Minder et al., 2010, Petersen et al., 2013), this method has severe limitations if applied over glaciers, and given the peculiar distribution of the air temperature above them, it leads to overestimations of the sensitivity of ablation to variations in atmospheric temperature. The key concept that Greuell and Böhm (1998) want to stress, is that a *'suitable' calculation procedure* has to be implemented, and applied instead of the commonly-used constant (environmental) lapse rate, in order to achieve a better estimate of the surface mass balance sensitivity to changes in the temperature outside the thermal regime of glaciers.

R2-4) "The ideal solution to this problem is to use the temperature outside the thermal influence of the glacier as forcing and to compute melt by coupling a melt model to a mesoscale atmospheric model. The latter should extend beyond the thermal influence of the glacier and resolve details of the structure of the boundary layer above the glacier."(Greuell and Böhm, 1998).

As Greuell and Böhm, (1998) say, this is the best and more desirable solution, but they continue writing that "such an approach is computationally expensive, and appropriate models still have to be developed. This paper provides an alternative approach in the form of a simple thermodynamic model of the glacier wind".

We note that valid alternatives to the Greuell and Böhm, (1998) and to the Shea and Moore, (2010) methods have not been developed so far. For this reason, we tested these procedures in our paper. Several works in the literature clearly demonstrate that dominant processes, even with high complexity and spatial variability, can be accounted for in PDD-like models using statistical/empirical approaches, which enable improved modeling skill. This is the case, for example, of the snow redistribution by wind and avalanches, which can be effectively captured using topographic indexes extracted from a Digital Elevation Model (e.g. Gruber, 2007; Farinotti et al., 2010; Carturan et al., 2012), or of the snow albedo, which can be parameterized in function of surrogate variables (e.g. air temperature or cumulated melt), without accounting for all the variables that physically control its variability (e.g. Brock et al., 2000). It should be noted that capturing the dominant processes using statistical/empirical approaches is often the unique possibility in model applications with reduced input data availability.

R2-5) In other words: only if the structure of the boundary layer above the glacier is resolved in appropriate detail it makes sense to compute and use the 2m above the glacier surface temperature and Greuell and Böhm (1998) also make clear that any computation of the near ice temperature from off-glacier temperatures is different from case to case. A generalizing solution can only be found in a model that accounts for and resolves all potential influences on air temperature changing from an off-glacier site to a glacier site. Such models could be found in the limited area atmospheric simulation approaches and by *including the full dynamics of boundary layer meteorology. Yet, related degrees of freedom are usually far beyond the availability of data for the respective variables*.

The low requirement of meteorological input data is the main reason why PDD-like models are so frequently used. However, in our opinion, the simplicity of the modeling approach does not exclude a priori the possibility of capturing the dominant processes and feedbacks which control the climatic sensitivity of glaciers. The available experimental datasets provide increasing evidence that simple parameterizations in function of topographic attributes, as flow path length and slope, can account for much of the spatial variability of the air temperature over glaciers with very different geometric characteristics and geographic settings (Figures 5 and 7), with few exceptions. In our opinion these results are promising and potentially useful for future works aimed at developing generalized solutions, of any degree of complexity.

R2-6) This said, none of the targets envisaged in the presented draft can be reached: 1. The "effects of glacier disintegration on (near glacier surface) temperature variability" can only be studied by a full resolution of all processes acting between the free atmosphere and the complex surfaces of a glacierized mountain basin including high spatiotemporal resolution of boundary layer dynamics. This requires both a powerful model and respective data input considerably beyond what is available from the 8 weather stations.

Even if this aspect was not among the main objectives of our work, we think that it's worth discussing it, because it provides evidence of the reduced effectiveness of small glaciers (deriving from the fragmentation of larger glaciers) in cooling the air above, compared to wider glaciers or wider portions of the same parent glacier. This is suggested by the two weather stations on the Careser Occidentale and Orientale glaciers which, despite being at almost the same flowline distance from the upper glacier margin (Table 1), have very different cooling effects (Table 4, Figure 3), which largely explain errors in modeled ablation rates (Figure 9; Figure 8 from Carturan et al., 2012). In consideration of the high number and contribution to the world's total ice volume of smaller glaciers (Haeberli et al., 1989; Paul et al., 2004; Zemp et al., 2008; Bahr and Radić, 2012), and given the absence of previous experimental data from such small ice bodies, these results deserve discussion even if they are not conclusive.

R2-7) 2. For the same reasons, the search of a generally valid equation for linking off-glacier temperatures with on-glacier temperatures and, finally for calculation lapse rates above the glacier surface must fail. An "inter-comparison of calculation methods" is, thus, only of value per se but cannot lead to a generally valid solution. In particular, the parameter settings for the presented transfer models must be calibrated for each site and for each case individually. Again, a full process resolution approach would be the only promising one but cannot be reached with the proposed methods and the available data. 3. As a consequence, mass balance modelling cannot be improved from the available data and with the proposed approach.

As already mentioned before, the good match of the experimental data from our study area with the already-existing parameterizations proposed by Shea and Moore (2010) and by Greuell and Böhm (1998) (Figures 5 and 7) point to a good generalizability of these methods, without solving the full processes involved in boundary layer dynamics. This is even more remarkable, considering the heterogeneous physical characteristics and geographic setting of the investigated glaciers. In our opinion, and in agreement with the remarks of the Reviewer who says that "*related degrees of freedom are usually far beyond the availability of data for the respective variables*", it would be almost impossible to implement and use a modeling tool based on a full process resolution approach, given the large amount of input data required, with suitable spatio-temporal resolution.

I suggest the authors to find another use of the interesting data but I have, unfortunately, to recommend rejecting the present paper.

This point is difficult to understand, because the Reviewer does not provide any useful suggestion on how to modify or improve the manuscript, nor he suggests anything less generic than “another use” of our interesting data. Moreover, in his review he doesn’t report any reference in support of his statements. From the comments above it is clear that, according to him, mass balance models cannot be improved from the available data and with the proposed approach (even if, as noted by Reviewer 1, this approach is commonly applied on mass balance models forced with off-glacier data). On the other hand, he clearly states that the only way to account for the spatial variability of 2 m temperature over glaciers is a full process resolution approach, which however ‘has high degrees of freedom, far beyond the availability of data for the respective variables’. In our opinion, stating that distributed PDD and ETI models cannot be improved using the proposed approach, and that empirical/statistical procedures cannot account for the spatial distribution of the air temperature over glaciers, contradicts previous findings and published works from other Authors (see the references cited in our paper and in the comments above), whose proposed methods and results are corroborated by the results of our study.

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