Answer to reviewers

First of all thanks to the two anonymous reviewers and comments from C. Charalampidis who provided constructive feedback and suggestions for improvement of the manuscript.

We have updated all the calculations, figures and numbers in the text so that they are based on a catchment delineation derived from the subglacial topography as presented in Lindbäck et al. (2015). Given the difference on catchment shape and size the numbers change. However, the general pattern with the remarkable difference in the discharge response to the energy input between the two years 2010 and 2012 remain the same. In addition to this, the following changes have been made to the manuscript:

- The abstract have been shortened.
- The former supplementary figure is now included in the manuscript as Figure 2.
- Method section 2.5, about the surface energy balance model have been expanded to provide more details on the calculations.
- A method description about the firn saturation model have been added as section 2.6.
- The method description of the catchment delineation (section 2.8) have been rewritten.
- Minor changes have been made to the result section and primarily consist of adjusting the text to the new numbers derived from the updated calculations.
- The discussion have been edited in order to make a better flow in the manuscript.

Specific answers to Referee #1

As explained above, we have expanded our method explanation about the energy balance model. All data and graphs regarding the ΣPDD has been replaced with calculations from the energy balance model, with the updated catchment. In the previous version, the text was not always clear enough about whether we used the ΣPDD's or the Energy balance model. This have now been corrected and it should now be more clear what we have done. The energy balance model description have been expanded, although still kept relatively short due to the overall length of the manuscript. The model is well tested and used in numerous studies and described in detail in the papers cited in section 2.5. We do not consider the focus of this paper to be on the energy balance as such and have therefore chosen not to make a figure showing the components of the energy balance for KAN_L and KAN_U in 2010 and 2012 respectively. This was an optional suggestion made me Referee #1.

Specific answers to Referee #2

The concerns raised about the catchment by Referee #2 is fair and we believe to have addressed that in the best possible way. We now use a catchment derived from basal topography in Lindbäck et al. (2015) and therefore address the specific comment (3).

In the revised method section, we address the problem with potential for subglacial water piracy (Specific comment (1)) and refer to Lindbäck et al. (2015) that find that Watson River catchment to shift during the season due to differences in subglacial water-pressure. However, the subglacial catchment, along with its hypsometry, remains effectively constant over the melting season. Therefore the calculated melt rates based on results from the energy balance model is considered robust.

We do not present a multi watersheed approach in the manuscript as such (speciffic comment (2)). However, the results presented in the previous version and the current revised version is based on two different catchment areas and arrive on the same conclusions. This support that the results presented here are robust. In Lindbäck et al. (2015), previous cahcment deleniations for the Watson River Catchment is compared and at lower elevations (where the melt rates are highest), they difer only little.

Specific answers to C. Charalampidis

We have no reason to doubt our calculations and the results presented in Table 2 and we are convinced that the differences lie in the point vs. catchment wide study. It is also worth noting that the absolute differences in melt at elevations above the mean ELA is small and the relative difference therefore is very sensitive to these differences.

Answer to the second and third point by C. Charalampedis: We do not include melt from the elevation interval between 1850 and 2050 m a.s.l. in this revised Figure 3F (previously Figure 2F). We do show the calculated melt from that interwal on Figure 3D&E and include it as a part of Table 2. The reason for not including it in the calculated totals we use in the conclusion and Figure 3F is that satelite imagery indicates that free surface water ocourred below 2050 m elevation. However, we don't know exactly at what elevation the melt potentially runoff rather than percolating and refreezing. Therefore we still show the calculated melt from the 1850 to 2050 m elevation interval in Table 2 and Figure 3D&E. Again we don't see any reason to doubt the calculations. Even very small meltrates at this elevation summes up to relatively large amounts due to the hypsometric effect on the catchment that result in a large area for this elevation interval.

As explained above we have rewritten parts of the manuscript and believe it is sufficient to meets the comments on this point. We see no problem in having a youtube link in the text; it's an easy and illustrative way to provide valuable information about the river for the readers not familiar with the location. We are afterall living in the 21'st century.