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Interactive comment on “Glaciological and geodetic mass balance of ten long-term glaciers in Norway” by L. M. Andreassen et al.

Anonymous Referee #2

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Glaciological and geodetic observations offer mostly independent means to constrain changes in glacier mass balance, as has now been demonstrated multiple times (e.g. Cox and March, 2004; Cogley et al., 2009; Van Beusekom et al., 2010; Zemp et al., 2013). This extensive re-analysis complements the existing literature, providing a similar analysis for 10 glaciers in Norway. The results demonstrate a more cohesive regional signal and suggest an important role for internal and basal mass balances. The writing is well structured, but in agreement with the review by Johannesson, I found many typos or flaws in English grammar that should be corrected prior to publication. Overall the paper reads fairly well.

However, this paper straddles the fence between a data release more appropriate for a journal like ESSD and a research paper that should be published in a journal like TC or

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J. Glac. It is complete for neither venue, and in my opinion should be significantly modified before acceptance into either journal. To be accepted into TC, the authors should go beyond the accepted benefits of a thorough re-analysis and interpret the newly produced records in terms of regional glacier change. For example, why is this work, and the more cohesive suite of time series important scientifically? What does it tell us about climate forcing and or the role of glacier geometry as a control on mass balance for the region? Does the re-analysis hint at what field methods worked best, or worst? For either venue (data release or TC paper), the authors should provide this amazing analysis effort as a companion data set with the paper. I would expect the starting and re-analyzed data sets would be published with the text. As is, there is no opportunity for other groups to process the data to get a data-driven sense of uncertainty in the approach. Choosing between a data release and a more rigorous interpretive paper (with much of the methods, and all of the data placed in a supplement) will greatly enhance the value of this contribution to the broader glaciological community.

The primary scientific contribution (beyond a beautifully-done re-analysis) is the suggestion of the importance of internal and basal terms, which largely falls out as a residual between glaciological and geodetic approaches. The lack of sensitivity testing alongside propagation of errors results weakens this suggestion. Sensitivity testing may refine the uncertainty analyses, but as presented these small terms are not presented convincingly and could simply be the result of errors that are larger than propagation methods suggest. Error bars that do not cross the 1:1 line in figure 7 emphasize the need for a data-driven uncertainty analysis.

MINOR COMMENTS: 1. After so much rigor in homogenizing the records, the authors should refrain from hand-drawn fits to mass balance profiles.

2. Provide AADs in the Figure 3 example.

3. Figures 2 & 4 are hard to read.

4. A third panel in Figure 8 showing the difference between original and homogenized

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time series would be helpful in assessing the magnitude of the changes driven by the re-analysis.

5. In section 3.1.1. how is density estimated for stake measurements in the accumulation area?

6. Ending the paper with more research is needed is weak. Please end with something positive that this effort has contributed towards a better understanding of Norwegian glacier change.

Interactive comment on The Cryosphere Discuss., 9, 6581, 2015.

TCD

9, C2748–C2750, 2016

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