

Response to referee 2.

We are grateful to the reviewer for their thorough and thoughtful comments. Although we do not agree with every suggestion, we are certain that this input will improve the clarity of the paper.

We will address each comment in turn. The first (unnumbered) comment is about the time frame and, in particular, Dyurgerov and Meir's estimates for mountain glaciers and ice caps. We have two comments concerning this. First, Fig 1 of (Meier et al., 2007) relates specifically to the period 1995-2005 and includes comparison with a collation by Kaser in the same figure. Second, we only rely on this study for NW USA (see Table 1), which makes a rather insignificant contribution to the combined sea level signature shown in Fig 3 of our paper. We purposefully do not include error bars in Table 1, because these numbers are not meant to be used as new or definitive estimates of ice mass loss (see response to R Way's comment). Hence the extrapolation of, for example, the Alaskan value for 2002-08 to cover 2000-2008 is felt to be valid in the context in which the values are being used: to assess the influence of regional land ice mass loss on RSL, not to assess or derive a best estimate for regional mass loss.

1. Observing the sea level signature. This is similar to the comment made by referee 1 on the same topic. However, referee 2 suggests including some references to this, which we have done. Tamisiea et al. (2010) is about the annual cycle, so not strictly related to secular signals. It is true that the annual cycle is large but that is not really too relevant here. The paper by Chambers et al. (2010) (which only appeared in Nov 2010 in JGR) is more related to the point 8 by this referee. We, therefore, decided on some unashamed self-promotion by citing (Riva et al., 2010)! This paper specifically deals with the secular fingerprint seen by GRACE but was published after we submitted this paper. Figure S1 of this paper is essentially the same as Figure 1 of (Wu et al., 2010) and we discuss in our paper that some of the regional signals are due, not to a secular trend, but to the very large annual variations over, for example, the Amazon Basin.

2. Agreed. This point (other sources of secular variation) was also brought up by referee 1 and Hans Oerlemans and we have changed the text accordingly to indicate that i) there are other secular-like signals and ii) that the *relative* spatial distribution of the dominant land ice sources may not be necessarily constant over multi-decadal time scales. In particular, the relative contributions from West Antarctica and Greenland may change enough over the next ~few decades to have an impact on the RSL signature shown in Fig 4. We make this clearer in the text.

3. Sub-decadal variability in the ice sheets. It is certainly correct to state that there is inter-annual variability in mass loss from the ice sheets. This is, in fact, dominated not by ice dynamics but by the surface mass balance (e.g. Fig 2A (van den Broeke et al., 2009)). It is also a valid point to make that the loss from an ice mass is not a constant through time and we have emphasised these points in the introduction and discussion sections of the paper. This is linked to point 2 above.

4. This comment suggests we consider the discrepancy between estimates of different authors and discuss, as an example, a paper on the Lambert drainage basin. As we stated in our reply to R Way (who brought up a similar comment in a different context) we are not attempting to provide "best estimates" for the values we use. That is far, far beyond the scope of this study and would require an immense review and critique of the many papers that have

been published in the last ~5 years on Greenland and Antarctic mass balance. There are even conflicting estimates for the contribution of Alaska over the last ~decade (Berthier et al., 2010; Luthcke et al., 2008). We do not see any merit in doing this partially. A complete assessment is a major effort that will, likely, be undertaken within the framework of the IPCC AR5. To illustrate the issue, we include a figure from an AMAP report on Greenland that shows the range of estimates for Greenland from a variety of sources. It does not include all estimates and, in particular, Wu et al 2010. The situation for Antarctica is similar.

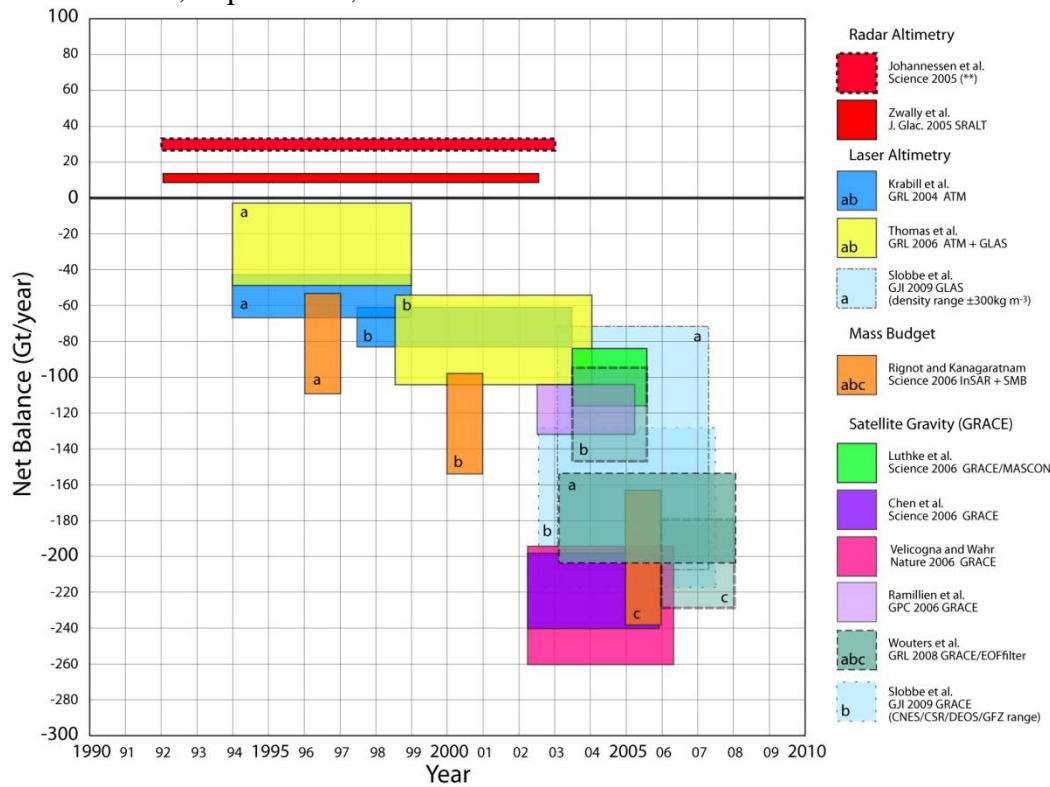


Fig 1. Mass balance estimates for the Greenland ice sheet from altimetry, mass budget and gravimetry (Dahl-Jensen et al., 2009), circa mid 2009.

We accept, however, that the concern (that the estimates we use are uncertain) is a valid one and we have added a paragraph to make this clear.

5. Add error bars to Table 1. We address this point in the first paragraph of our response above. We made a conscious decision **not** to include error bars as we do not want readers to use these numbers as any kind of definitive statement on the mass trends for each region. However, we accept that some estimate of the uncertainty in the numbers we use is useful in assessing the RSL fingerprints we show. We have, therefore, included the errors quoted in the relevant references for each source, scaling them cover the period and values we use. We stress, however, that these are not our estimates of the uncertainties in mass trends for each source. We have made this clear in the description to Table 1 and also added a column, as suggested, which shows the observation epoch for each region.

6. Colours/clarity of Fig 1. Agreed. We have entirely redrawn Fig 1 using a polar stereographic projection, which makes the pattern of mass exchange much clearer.

7. We have added a statement to make it clear that the signals can only approximately be added linearly. Fig 3 is **not** the linear sum of the individual signals but was estimated from the total mass exchange and we have made this clear in the text.

8. There is no such thing as an exact or “appropriate” GIA correction. Peltier has now produced ICE-6G which apparently produces ~twice the mass contribution to the oceans from GRACE compared to ICE-5G (Cazenave et al., 2009) but as (Bevis et al., 2009) point out, four of the leading GIA models for Antarctica all appear to be deficient in their own way. However, we have updated the text on the GIA calculation to make it clear what we did and its limitations including a reference to (Bevis et al., 2009).

9. We’re not entirely sure what point the referee is trying to make here except, perhaps, to make the link with other satellite gravity observations and ground based observations of GIA? We could certainly discuss papers on J2 variations but we feel that this goes beyond the scope and remit of the paper. We have added some text to place our analysis in the wider context of satellite and ground based geodesy.

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