We thank Shepherd for his Short Comment on our paper. We note that the comments, in general, relate to phrases that the he believes could be better worded and we will address them, where relevant, in an updated version of the paper.

We also note that, in submitting the SC, Shepherd has confused an SC, which is unsolicited, and a review which is solicited. Shepherd has provided the former not, as erroneously stated in the title, the latter.

In the summary paragraph at the beginning, there are some errors of substance that need addressing. First, 2003-2009 is a 7 year interval, not 6 (in fact, our study is from 02/2003-10/2009, which is 6.8 years). Second, Shepherd states "mass losses from West Antarctica based on two of the three available satellite geodetic techniques – satellite altimetry and satellite gravimetry, but not satellite mass budget – and also global positioning system observations of rock uplift". There are only two satellite geodetic approaches here. The third approach Shepherd refers to (the Input Output Method, IOM) is not a "geodetic" approach and relies on satellite observations for just one component of the three key variables required in the IOM (velocity, ice thickness and SMB). Third, and most critically, our mass trend assessment is not "based on two of the three available satellite geodetic techniques". Our inversion, is based on a suite of observations, which includes altimetry and gravimetry, but also other input data and priors that play an important role in constraining the solution, all of which are detailed in section 2. This is an important point, and one that distinguishes our approach from the more "conventional" approaches that Shepherd alludes to in his comment. It is unfortunate that Shepherd appears to have misunderstood this, as it is fundamental to the approach we use and it seems to have created some confusion in later comments.

Shepherd queries the sentence While there is a general consensus that West Antarctica has experienced ice loss over the past two decades, the range of mass-balance estimates still differ significantly (compare, e.g, Shepherd et al., 2012, with Gunter et al., 2014) and suggests a rewording, which we will consider in light of the most recent published estimates. We note, however, that even in Shepherd et al, 2012, mass balance estimates still differ significantly (i.e. > than their combined errors). See for example Table S6.4 in Shephered et al. 2012.

P2997 Line 22 – I don't think that most readers will consider van den berg 2006 to be a recent estimate? Also, it's not entirely clear, but it seems that King et al 2012 is introduced as another estimates of Antarctic ice sheet mass imbalance, but this seems a little odd following the earlier discussion.
We're not sure why citing King 2012 et al is "odd". We agree that van den berg 2006 is not the most recent and will replace this citation.

P2997 Line 24 – The authors again use the phrase "recent studies". I am not sure what it means – within 2, 5, 10, 20 years? The phrase is used often in the paper to mean ostensibly different things. I suggest reviewing the context throughout to make sure you are consistent.
We will review the use of the word recent and remove any ambiguity.

P2998. The authors state that "we eliminate the dependency of the solution on solid-Earth and climate Models", but I don't think that this is the case. The altimetry solutions will, for example, be dependent on surface mass balance fluctuations in a way that the authors' model does not admit, as the effects of electromagnetic interaction with the snowpack are not considered. So it is too strong to say that the effects are eliminated - an attempt is made to eliminate them
Unfortunately, Shephard appears to be very confused about the approach used here (see earlier reply above) and the statement above is incorrect and muddled. First, the satellite radar altimeter (SRA) data have been corrected for EM interaction with the snowpack (see Flament and Remy, 2012). Second, the point about EM interaction and the use of climate models or solid earth models are unrelated and Shepherd has muddled two different things here. However, for an entirely different reason (the use of priors for correlations and length scales that are derived from climate models) the word "eliminate" will be replaced by "mimimised"

P3011 line 5 – I don't think that the numbers -97 +/- 20, -105 +/- 22, and -76 +/- 15 can be considered different, when they agree to within 0.5 sigma. So I suggest a rewording here when the new results are presented. Also, where does the figure of -97 +/- 20 Gt/yr attributed to Shepherd et al come from? My reading of the Shepherd et al. puts their estimate of West Antarctic Ice Sheet mass imbalance at -67 +/- 21 Gt/yr over the IceSat period (Table S2)
The numbers and the wording of this paragraph will be reviewed.

P3005. The authors appear to be suggesting that a "simple average .. of corrected data sources" is not statistically sound. I am not sure what to think of this statement, because I don't really know what the term "sound" means (at least in a scientific context). Nevertheless, I suspect that most readers will consider an average to be a statistically sound calculation, so perhaps a change of wording is required here
Agreed. Replaced "simple averaging" with "unweighted average" and clarified what we meant. The point we were trying to make here is that taking the unweighted average of estimates with large differences in errors (as was done in Shepherd et al 2012) is statistically flawed. It results, for example, in an estimate with errors of 1 unit having the same weight (influence) as one with errors of 10, 100 or 10^5 units. This is clearly not statistically sound and an unweighted mean can deviate dramatically from a weighted mean and similarly with the errors. We will make this clearer in the text.

P3011, line 7. Again, Shepherd et al is not an altimetry-based estimate- it includes observations from 4 satellite techniques – more, in fact, than the present study!
Unfortunately more confusion is present in this comment. In his opening sentence, Shepherd states "...*ice mass losses from West Antarctica based on two of the three available satellite geodetic techniques*", while here he mentions four. Perhaps more serious is that we are incorporating observations from five satellite based data sets, so the statement is both confused and incorrect. We should emphasise, however, that our objective was not to discuss, at length, the full range of other estimates of mass trends and this section is, therefore, illustrative rather than exhaustive. To do so would require too much space and is not the focus of this study.