Rebuttal

We would like to thank reviewer #2 for his review. We agreed with most of the comment and implemented the changes as requested.

The updated manuscript will be added with marked changes.

General comments

This paper evaluates the sea level budget in regions of the North Atlantic basin using altimetry, GRACE, and steric Argo. Because of the treatment of each dataset is novel and the analysis of budget closure is on sub-basin scales, the subject matter is worthy of publication. However, there are several errors in the description of data processing and in citing previous work that will need to be corrected. (1) More important, the treatment of GIA for both the altimetry and GRACE data is too vague for me to evaluate the claims of sea level budget closure. Another reviewer has already identified several spelling and grammar errors, which I will attempt not to repeat. (2) The paper needs a better justification for the selection of the North Atlantic as an area of study. The North Atlantic has been well surveyed before Argo, and other hydrographic profiles or gridded products could have been evaluated. For example, the NOAA Ocean Climate Laboratory's Global total steric sea level anomaly fields include both Argo, XBT, CTD, and other hydrographic data:
https://www.nodc.noaa.gov/OC5/3M_HEAT_CONTENT/fsl_global.html
(1) The description of the GIA correction is extended and clarified. This is further addressed below at 'page 12' and 'page 26'.

(2) Reviewer #1 also brought this to our attention. We decided to update the title to: "Sub-basin scale sea level budgets from satellite altimetry, Argo floats and satellite gravimetry: a case study in the North Atlantic". Secondly, we added a motivation why apply the method to the North Atlantic in the introduction. We needed a location where enough Argo observations were available during the period 2004-2014. Additionally, the North Atlantic is a quite dynamical region, with the Gulf Stream, a neighbouring ice sheet and large gradients in GIA, so it was possible to investigate the method over regions with varying conditions.

Page 12: I am confused by this statement: "Ultimately, the mean GIA OBP over a polygon (degree 2 and higher, as measured by GRACE) are subtracted from the results, to make them compatible with altimetry." To be compatible with the treatment of the altimetry, a correction based on the same GIA model should have been used. It's not clear from the paper what GIA model was removed from the GRACE data, which is crucial to understanding how the sea level budget was closed for trends.
For altimetry and GRACE we consistently use the solution of Peltier et al. (2015). For altimetry it is corrected for the geoid response and for GRACE the EWL response.

We adjusted the text on page 9 (under altimetry processing), to clarify that for altimetry and GRACE the same model is used. On page 12 we clarified and extended the text related to the correction for GRACE.

Page 26: The paper is also unclear about the meaning of the "GIA correction of 10-20%" referred to on page 26. 10 to 20% of what? Since the choice of GIA model used is critical for closure, this issue needs to be more

clearly explained.
The sentence is updated to “an error of 10-20 % on the GIA correction is assumed”.

Minor comments

Introduction, 2nd paragraph. The time periods for a couple of the cited papers is not correct Willis et al., 2008 looked the budget over 2003.5 and 2007.5, not 2003 to 2007. The budget was closed within error by Leuliette and Miller (2009) for 2004-2008, but in Leuliette and Willis (2011) over the period was 2005-2010.5.
The periods have been corrected.

Page 2, line 4: I’d recommend that throughout the paper the term “GRACE ocean bottom pressure” not be used. Strictly speaking, OBP is the sum of atmospheric and oceanic mass variations, which is what would be recorded by a pressure gauge in the ocean. Here, of course, in equation 1, the authors intend for H_OBP to reflect the sea level component of ocean mass changes. The IPCC and other authors have opted to call this component “barystatic sea level”. I would recommend using this term or “ocean mass component” instead of OBP to avoid confusion. Also, I’d recommend describing how the inverted barometer correction applied to the altimetry data and the GRACE data.
The IB correction is applied as a linear function of regional sea level pressure variations with respect to the time-varying mean atmospheric sea level pressure over the oceans. Fluctuations in atmospheric pressure are caused by mass fluctuations in the atmosphere.
Altimetry without IB correction measures steric changes + ocean mass changes. However, by applying the IB correction regional variations of atmospheric mass (with respect to the global mean over the oceans) are also taken into account.
Since the mean atmospheric mass over the oceans is not part of our measurements we agree with the reviewer that the term 'ocean bottom pressure' is not appropriate. However, since regional variations of atmospheric mass/pressure (by the IB correction) are taken into account, we think the term 'ocean mass' is also not appropriate.
According to Gregory et al. (2012): “The barystatic effect on sea level change is the mass of freshwater added or removed, converted to a volume using a reference density of 1000 kg m^{-3}.” As it does not take into account the IB correction, 'barystatic sea level' is also not appropriate.
As a solution, we will now refer to 'Mass Component (MC)' or 'mass' as the IB corrected ocean mass. Throughout the whole manuscript including Eq. (1), we now use the term MC. We make clear that the MSL and MC in Eq. (1) are inverse barometer corrected.

Furthermore, a short description is included for all the corrections applied to the altimetry, including the inverted barometer (IB) correction, which is part of the dynamic atmosphere correction. We slightly adjusted the description of the GRACE data, to clarify that time-varying global mean atmospheric mass over the ocean is excluded, as it is also for altimetry.

Page 2, line 20: Purkey (2014) should be Purkey et al. (2014).
Changed to Purkey et al. (2014).

Page 7, line 1: The “inclination weighting” scheme that is a function of the latitude of the measurement and the orbit inclination angle of the satellite and used to generate the University of Colorado time series of GMSL was

first suggested by Wang and Rapp [1994] (see the report "Estimation of sea surface dynamic topography, ocean tides, and secular changes from Topex altimeter data" at http://sealevel.colorado.edu/files/pubs/wang_rapp_report_430.pdf). Tai and Wagner (2011) simplified the approach using a spherical Earth approximation. I would recommend citing one or both of these papers. Rather than calling this weighing the "Nerem method," it would more properly be termed the "inclination/latitude weighting" or the Wang and Rapp weighting.
We agree with the reviewer that the derivation of the inclination weighting technique was described by Wang and Rapp (1994). Therefore we changed the name of the weighting method to Wang and Rapp weighting. Additionally, both Tai and Wagner (2011) and Wang and Rapp (1994) references are included.

Table 2: EWL is not defined.
We changed EWL into EWH to be consistent with the rest of the manuscript. We define EWH again in table 4.

Additional changes by author

Page 9, line 6: Changed 'the EWHs' into 'MC from GRACE'.
Page 13, line 12: Added a reference to Tamisiea (2011).

We noted that sea level anomalies in Eq. (1) were not exactly what is computed in methodology. First of all, for all time series the mean is removed, because they have different reference periods. So, at the end of Sect. 3.1, 3.2 and 3.3 we mention that the mean of the msl, steric and mass time series is removed. Secondly, we clearly state in Eq. (1) that the mean sea level and the mass component are GIA-corrected.

References

Gregory et al. (2013). Twentieth-century global-mean sea level rise: Is the whole greater than the sum of the parts?. *Journal of Climate*, *26*(13), 4476-4499.
Tamisiea, M. E. (2011). Ongoing glacial isostatic contributions to observations of sea level change. Geophysical Journal International, 186(3), 1036-1044.