SUPPLEMENTAL MATERIAL (SM)

Response to Comment by T. SCAMBOS and C. SHUMAN (2016) on 'Mass gains of the Antarctic ice sheet exceed losses' by H. J. Zwally and others (2015)

Saturation Corrections

Scambos and Shuman (2016) offer no valid evidence to support their assertions "*.... that there are two main contributions to their strong positive EAIS mass trend from ICESat: 1) ICESat's Geoscience Laser Altimeter System's (GLAS) detector saturation and its residual effects after applying standard corrections to the data; and 2) the use of an unconventional method to estimate documented biases between the ICESat data collection campaigns.*

For the laser 3 campaigns, we applied refined saturation range corrections developed with laboratory tests and Unyni data (Sun and others, 2005; Sun and others, in review) to the NSIDC Release 34 data used in our paper. For L1a and L1b data with saturated returns, we applied similar corrections developed with the laboratory tests. The saturation range corrections are a monotonic function of the received-laser energy. For the campaigns with low laser energy early in the mission (i.e. L2b and L2c) and the later campaigns (e.g. L3j, L3k, L2d, L2e, and L2f), the applied corrections are negligible. The saturation-corrected GLAS elevations are in good agreement with the GPS elevation measurements on the Uyuni salt flats, without any apparent trend. These saturation range corrections, which have been on the Release 33 data products at NSIDC since 2012 (as well as the later Release 34), need to be applied by the users. Time series using earlier or incomplete versions of the saturation corrections could include false trends. However, Scambos and Shuman' rely on their out-dated analysis in non-peer reviewed conference abstracts, and apparently have not updated their analyses of the saturation effects using the latest accurate corrections.

ICESat Inter-campaign Biases

Our methods for determining the ICESat inter-campaign biases are robust and well-documented in the peer-reviewed literature. As we stated: "We use methods for determining the ICESat inter-campaign biases that have been used in the satellite-altimeter mapping of the level of open water and thin ice in leads and polynyas in sea ice by ICESat in the Antarctic (Zwally and others, 2008) and the Arctic (Farrell and others, 2009), in the joint mapping by ICESat and Envisat of the mean dynamic topography in the Arctic Ocean (Farrell and others, 2012), and in the analysis of temporal changes in the ocean dynamic topography observed by Envisat in the western Arctic Ocean (Giles and others, 2012). Advantages of our method compared to other studies of campaign biases (Urban and Schutz, 2005; Hofton and others, 2013; Borsa and others, 2014) include: (1) smooth surfaces in leads and polynyas that do not require a sea-state bias (significant wave-height) correction, (2) measured laser reflectivity of 0.42 that is closer to the 0.53 reflectivity of the adjacent sea ice and of ice sheets compared to the measured low reflectivity of 0.12 over open ocean, (3) availability of independent Envisat measurements of the vertical motion of the sea surface reference level, and (4) coverage over the reference surface by most of the laser tracks during each campaign."

Very importantly, our methodology accounts for the effects of both seasonal and interannual variations in ocean dynamic topography by using independent simultaneous Envisat measurements of the same ocean reference level. It is precisely such an independent measurement of reference level that is lacking from most other bias determinations. Scambos and Shumans' comment about changes in the gravitational attraction from changes in coastal ice mass on sea level is not relevant, because it equally affects the sea level measurements of both ICESat and Envisat, as well as being spatially limited.

Their comments regarding the radar altimeter footprint show a profound misunderstanding of the methodology for selecting the stronger radar returns from leads and polynyas as pioneered by Seymour Laxon and colleagues and described in our referenced literature. The impact of adjacent sea ice floes has minimal impact on the measured height of the ocean reference level by either laser or radar altimeters, and thinning of the Arctic sea ice is also not relevant for the same reason.

Furthermore, they incorrectly state; *"While this method is adequate for estimating sea ice freeboard, polar oceans are not a good reference surface analogue for ice sheet areas, since they are comparatively dark and rarely have saturated returns."* As we stated: "....(2) measured laser reflectivity of 0.42 (i.e. in leads and polynyas) that is closer to the 0.53 reflectivity of the adjacent sea ice and of ice sheets compared to the measured low reflectivity of 0.12 over open ocean, ...". Low ocean reflectivity does, however, have an effect on attempts to determine the biases over the ocean, because the combination of low reflectivity and lowering of the laser output energy during the mission severely modifies the GLAS coverage over oceans, which increases the influence of undetermined regional variations in dynamic topography especially in the lower latitudes of the Pacific.

Scambos and Shumans' suggestion to replace our biases with any of the other inter-campaign bias determinations given in their Fig 2 or Table 1 would not be advisable. The other *"bias assessments"* are not *"relatively self-consistent".* When comparing bias estimates for consistency and validity, important issues to evaluate include: A) whether the G-C correction (not small) was applied and how (by applying the G-C difference for each laser pulse to the ranges before deriving the biases or by applying averages of G-C corrections to biases derived with uncorrected data, which is less accurate); B) whether the GLAS saturation range corrections on NSIDC data products were applied by the user; C) whether an independent and sufficiently-accurate determination of reference-level motion was applied; D) for oceans, whether a sea-state bias correction was made, whether a global sea-level-rise correction of 3 mm a^{-1} was applied (and why), and whether the effects of large and variable undetermined regional changes (e.g. 30 mm a^{-1}) in ocean topography were accounted for; and E) whether the signs (+ or -) used to define the biases are consistent, or inconsistent, as for example in Richter and others (2014) *"The biases have to be subtracted from the elevation data:* $H_{corr} = H_{orig} - \Delta H$*"* versus by Ewert and others

(2012) *"All biases are corrected elevation minus original ICESat elevation",* which is $\Delta H = H_{corr} - H_{orig}$ (i.e. $H_{corr} = H_{orig} + \Delta H$).

G-C Correction

Scambos and Shuman's state: *"...most of which have incorporated a small correction factor for how the range is measured, which was recognized in late 2012 ('G-C correction'; Borsa and others, 2014). Note that the G-C correction, if not applied, would be included as part of IC-bias estimates, and would have little net impact for statistically large ICESat data sets, as Helm and others (2014) and Zwally 2015 note."*

Their comment is not correct and is very misleading. First, instead of being a *"small correction"*, it is as we stated: "Therefore, although the net effect of using ice-sheet data without the G-C correction applied is very small if commensurate bias corrections are applied, the error is significant –1.29 cm a^{-1}) if the G-C correction is only applied to the data and not to the bias determinations (i.e. incorrectly causing a less positive or more negative d*H*/d*t*). The error is similar if the G-C correction is applied, but no inter-campaign bias adjustments are applied as in Helm and others (2014) in which the volume change obtained from ICESat for 2003–09 for the AIS is consequently negative at -60 ± 44 km^3 a^{-1}". Their comment is misleading because it might further encourage application of the G-C correction without the use of commensurate inter-campaign bias adjustments determined using data with the G-C correction applied (Zwally, 2013), or without any bias adjustments at all.

Radar-Backscatter Correction

A critical unresolved issue for satellite radar altimetry is the reliability of corrections made for the highly-variable penetration of the radar-signal into the firn, particularly for Envisat and Cryosat. The variation of the penetration depth of linearly-polarized radar signals into firn was initially shown by Legresy and others (1999) from analysis of ERS-1 data to be dependent on the orientation of the polarization (and therefore the direction of spacecraft travel) with respect to the anisotropy of the surface properties, which causes differences in elevations measured at orbital crossovers and variations in the strength of the radar-backscatter signal. The circularly-polarized altimeters on SeaSat, Geosat, and Topex were not affected by orientation. Successful empirical corrections for the seasonal and interannual variations in the penetration depth as functions of the backscatter signal were developed and applied for ERS radar altimetry (Wingham and others, 1998; Davis and Ferguson, 2004; Zwally and others, 2005; Yi and others, 2011; Khvorostovsky, 2012), but the methods for deriving the corrections vary including their dependence on the range-retracking algorithm used and other factors. As noted by Arthern and others (2001) a procedure "*.... suggested by Zwally et al., [1989] as a means to reduce the impact of ascending versus descending biases in the satellite orbit solution.... serves equally well to remove ascending versus descending biases in radar penetration, so this procedure is to be encouraged, ...",* but its use may vary among investigators. The variable-penetration issue is further described in Remy and others (2012) along with an improved correction and analysis of residual errors. Although the orientation of the linear polarization differs from along-track on ERS-1 and ERS-2, to 120° off-track on Envisat, to perpendicular on CryoSat, it is not clear how those differences in orientation affect the biases in crossover analysis and the validity of various correction methods. In general, we believe the corrections developed for the biases caused by variable penetration depth of radar altimetry require further research, validation, and documentation.

Miscellaneous

Scambos and Shuman's state: *"There is little evidence for recent changes in annual snowfall in EAIS (e.g., Anschütz and others, 2011; Lenaerts and others, 2013). Therefore, Zwally 2015 infer that the thickening they detect across the EAIS interior is a consequence of accumulation increases that began at the end of the last ice age (~14 ky bp) that have not yet equilibrated with the EAIS ice flow system."*

As we stated: "We also find that both datasets give $\mathrm{d}M_a/\mathrm{d}t$ losses in EA for both measurement periods (–6 and –33 Gt a^{-1} for RACMO and –11 and –11 Gt a^{-1} for ERA-Interim)." However, the lack of recent increases in snowfall is not why we conclude that the dynamic thickening of 147 ± 55 Gt a^{-1} and 147 ± 34 Gt a^{-1} in the two periods is a result of accumulation increases that began at the end of the last ice age. First, the same EA dynamic thickening in both measurement periods simply means there is no significant recent dynamic change, in contrast to WA2 and AP. Our specific conclusion was: "A dominant characteristic of the dynamic thickening in EA is its persistence, with no significant change between periods. This thickening is likely a result of the marked increase in accumulation that began in the early Holocene, ~10 ka ago, with a 67–266% increase from the Last Glacial Maximum as derived from six ice cores (Siegert, 2003)."

Scambos and Shuman's state: "*If a product of post-glacial responses, the long-term thickening invoked by Zwally 2015 should continue the present and beyond.*"

The long-term thickening has continued, as shown by GRACE data for EA (e.g. King and others, 2012; Luthcke and others, 2013; Harig and Simons, 2015), even though the GRACE rate of increase is smaller at about 45% of ours. In fact the GRACE data show an increase in the Queen Maud Land region of EA after 2009 (Shepherd and others, 2012) , which is likely to be caused by an increase in accumulation superimposed on the long-term dynamic thickening.

Scambos and Shuman's state: *"....Zwally 2015 arbitrarily suggests that a better specific density for new snow might be 0.30, rather than 0.33 as used by Richter and others (2014), despite numerous snow pit density measurements (Ekaykin and others, 2004). Zwally 2015 similarly invokes a variety of other estimates of accumulation rate rather than accept the result s of Ekaykin and others (2004) who acquired data in the immediate vicinity of the stake grid, using a combination of shallow ice cores and snow pits....".*

The relevant density needed to estimate the new snow growth above a GPS marker is the density near the snow surface (ρ_{ns}) and not the density in ice cores or pits. The value of 0.30 is not "arbitrary", but is from density measurements at Dome Fuji in EA (Takahashi and Kameda, 2007). Nevertheless, the exact value used (e.g. 0.30 versus 0.33) does not substantially change our conclusions regarding the large uncertainty caused by the choice of accumulation rate (A) used to calculate the rate of new snow growth (dS_{ns}/dt), because as we noted: "However, dS_{ns}/dt is not only sensitive to the choice of ρ_{ns} for the near-surface snow/firn, but is even more sensitive to the choice of A by a factor of $1/\rho_{ns} > 3$". The examples of A we used as possible alternates to their "choice of a 200 year mean A= 2.06 cm w.e. a^{-1} from ice cores and pit measurements" included "their 1970–95 instrumental mean (Richter and others, 2008) of A= 2.29 cm a^{-1}", which gives a dS_{ns}/dt of 7.63 cm a^{-1} and dh_{gps}/dt (GPS-based surface-elevation change) of +1.42 cm a^{-1} (or +0.73 cm a^{-1} using $\rho_{ns} = 0.30$).

Scambos and Shuman's state *"Further, ...Subglacial Lake Vostok is in hydrostatic equilibrium, at least in its central ice surface area. This means that any valid ice-driven elevation change seen in the surrounding EAIS region should be reduced to ~1/12 the grounded-ice rate over the lake surface. Zwally 2015....show no discernable difference in elevation change rate from the grounded ice to the lake surface. The uniformity.....strongly implies that their results are affected by some kind of measurement bias unrelated to real ice sheet thickening."*

In our Fig 5, the along-track profiles of derived dh/dt, $\sigma_{dh/dt}$ and h at 172 m spacing over Lake Vostok show that the dh/dt varies from <0 cm a^{-1} near the northern end of track 0071 to >3.0 cm a^{-1} on the southern end of track 0077. There clearly is no discernable systematic reduction in dh/dt at the transitions from the grounded ice to the ice over the Lake or in the central area, certainly not in a ratio of 1/12. Scambos and Shuman' offer no possible cause for their speculation of *"some kind of measurement bias"*. Considering the uniformity of the surface properties and the characteristics of the laser measurements, such a bias is extremely unlikely. The conclusions of Ewert and others (2012) regarding"hydrostatic equilibrium", which are based on topographic considerations other than changes of dh/dt, are addressed further in our response to Richter and others (2016).

Scambos and Shuman's state *"One positive outcome of this analysis is that the Subglacial Lake Vostok surface, in fact, offers what may be the best possible reference surface for future altimetry assessments of ice sheet elevation change."*

It certainly would not be a positive outcome if assessments made to a surface for which the vertical motion is not unambiguously determined to an accuracy of a few mm a^{-1} were to be accepted. Considering the relatively large spatial and temporal variability of dh/dt observed by ICESat, not only over Lake Vostok, but over the ice sheets in general, such determinations of surface elevation change by in-situ measurements at a few points are unlikely to be of sufficient accuracy for the required validation. Aircraft and surface traverses can provide the required sampling taking into account the spatial and temporal variability of the surface elevation changes.

Scambos and Shuman' also raised the importance to the general public and included selected web pages *".... offering initial responses to the Zwally 2015 study"*. Most of the extensive media coverage of our paper beginning with the NASA news story (and the samples below) presented very balanced views of our findings and their relation to ongoing climate change, including positive comments from numerous scientists. In contrast, the last two links below reveal several interesting preconceived negative views expressed by scientists. In speaking with the media, we emphasized that our results are not in contradiction to the IPCC consensus report nor an indication that serious impacts of climate warming are not occurring globally, including the marked thinning and areal reduction of Arctic sea ice and the increased ice melting in Greenland.

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