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Interactive Comment

Interactive comment on "Constraining GRACE-derived cryosphere-attributed signal to irregularly shaped ice-covered areas" *by* W. Colgan et al.

W. Colgan et al.

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Dr. Bamber, We appreciate the feedback we have received in TC's interactive discussion forum. The "fresh eyes" of both referees have yielded some exceptionally constructive comments. With this feedback we are confident that we can further improve our methodological description. As you know, we answered the major/key comments of both reviewers as quickly as possible during the open discussion period. We now address the remaining minor comments below.

Anonymous Referee #1

Re: Pg. 4 Line 8 – We will ensure that this caveat more clearly states that the partition

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of mass loss between the ice sheet and peripheral glaciers is purely statistical at sub-grid cell resolution (i.e. derived from the fractional areas of ice sheet and peripheral glacier relative to total ice-covered area as per Eq. 5).

Re: Pg. 4 Line 15 – In our revised manuscript, we will clarify that conversions between spherical harmonics and mascons are established practice, and describe that this is done by representing mascons as a set of differential potential coefficients ("delta" coefficients) added to the mean GRACE L2 field (e.g. Eq. 1 and 2 in Luthcke et al., 2013; Chao et al., 1987). While we acknowledge that the absolute accuracy of mascons is (arguably) better than that of spherical harmonics, and thus mascons make a more desirable inversion target, the implementation of a mascon-based inversion requires an additional algorithm module to aggregate inferred signal to the mascon level. While this is by no means impossible, we regard it as a non-trivial undertaking. Unlike a spherical harmonic inversion, in a mascon inversion the same Gaussian filter cannot be applied to all nodes. Instead, a mascon inversion must employ some sort of asymmetrical geometric filter, the precise shape and symmetry of which varies by node. We are currently working towards a framework to develop and implement such a node-varying inversion filter.

Re: Pg. 4 Line 18 – We will clarify that Barletta et al. (2012) also use a Monte Carlo approach to perturb spherical harmonic coefficients to assess uncertainty.

Re: Pg. 5 Line 7 – We will explicitly state that seasonal detrending has be applied to the gravimetry solution we employ.

Re: Pg. 5 Eq. 1 – We are aware that we are perturbing the entire domain of the input GRACE-derived mass trend by a constant (random) proportion of the trend error in each simulation, and we will state this more clearly in a revised version of the manuscript. We note that relative to an absolute mass anomaly field (in units of mass (i.e. Gt) at a given node), a derived mass trend field (in units of mass per time (i.e. Gt/a) at each node) is indeed expected to exhibit increased spatial autocorrelation.

The inherent ability of trend analysis to overcome the influence of spatial and temporal noise contributes to the cryospheric community's general preference for deriving mass changes through trend analysis rather than anomaly differencing (e.g. Schrama and Wouters, 2011; Jacob et al., 2012; Tedesco et al., 2013). For our purposes, over an ensemble of simulations, any given region (or group of nodes) is inverted under relatively high and low initialized rates of mass change.

Re: Pg. 6 Line 6 – We will reduce our usage of the phrase "spherical harmonic representation" throughout this passage.

Re: Pg. 9 Line 10 – We acknowledge that the treatment of non-ice containing nodes is inconsistent with Eq. 1 as presented. In our revised manuscript, we rectify this inconsistency by explicitly stating that rate of mass change values at non-ice containing nodes are allowed to vary within the prescribed absolute threshold, without constraint by fractional ice coverage. Conversely, rate of mass change values at ice containing nodes are allowed to freely vary without a prescribed absolute threshold, but constrained by fractional ice coverage.

Re: Pg. 10 Eq. 5 – In our revised manuscript we will define all terms in this equation in an appendix of notation. We will also restate that the inferred rate of mass change at a given node (m_ji) is the sum of both the mass changes due to the ice sheet proper as well as peripheral glaciers, and that we derive the proportional rates of mass change of each ice type by using the proportional areas of each ice type.

Pg. 11 Line 23 – We agree with this interpretation of the passage. We will improve the statement that Canadian Arctic mass changes are concentrated where F_ji goes to one, to ensure that F_ji is presented as the cause of the spatial distribution of m_ji (rather than vice versa).

Anonymous Referee #2

Re: 1. – We will remove imprecise language and non-technical terms in the revised manuscript.

Re: 2. – We will clarify that the "assumption that rates of mass change are constant within or across pre-defined regions" is unique to mascons.

Re: 3. – We acknowledge that there is presently an inconsistency in the units of this equation. We thank Anonymous Referee #2's for their attention to detail. We will ensure that units of km follow all instances of sigma = 200 km. We assert, however, that sigma is indeed the standard deviation of the Gaussian filter. We note that the sigma we implement is functionally equivalent to the standard deviation in the conventional formulation of a Gaussian probability density function (e.g. http://en.wikipedia.org/wiki/Normal_distribution). We will clarify that the units of "1" in the numerator are implicitly km (i.e. consistent with the units of all variables in the equation).

References

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