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Interactive comment on "Extracting the time variable gravity field from satellite gravity data using a sawtooth filter" *by* E. Gurria and C. López

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General comments

This manuscript addresses the problem of the 'stripiness' of the GRACE-derived gravity models. These striping patterns occur due to the specific orbital configuration of the GRACE mission (leader-follower twin in coplanar near-polar orbits), in combination with background mismodelling and instrumental errors, and the required downward continuation of gravity that tends to amplify any error pattern with increasing harmonic degree. A number of authors have suggested post-processing methods (filters) to suppress this phenomenon while retaining the geophysical signals, with a recent emphasis on developing 'anisotropic' or 'decorrelation' methods. As these filters directly impact the spatial resolution of the geophysically interpretable GRACE results, any improved method would be of great benefit to the GRACE user community. In this sense the topic is interesting and the manuscript is timely.

At the same time, the manuscript falls seriously short in addressing the state of the art in GRACE post-processing. The authors develop a new method (the ‘sawtooth filter’) that is contrasted only against the Gaussian isotropic filter. Much more advanced filter methods have been developed in the mean time and are in common use in the GRACE community. Users of GRACE without specific background in geodesy can download gridded (anisotropically) postprocessed maps from several institutions. Probably most common anisotropic methods are the Swenson/Wahr (2006) destriping filters and the DDK filters by Kusche et al. 2007, 2009, similar ’optimal filters’ by Klees et al 2008; these are not even mentioned in the current manuscript. In the light of these newer (i.e. post-2006) quasi-standard methods, it is not clear whether the manuscript adds any improvement.

Another major issue is that no real attempt at geophysical interpretation of the GRACE data filtered through the new method is made. GRACE-derived potential change and, in particular, change in surface water column, has been shown to correlate quite well to geophysical models of mass variability (terrestrial water storage in response to rainfall, runoff and evapotranspiration, eustatic sea level, melting ice sheets, etc.). It is expected that GRACE results, when improved through better postprocessing, allow these comparisons at smaller spatial scales. No such attempt is made here.

Specific comments

The proposed filter is claimed to be simpler to implement and use than the spherical Gauss filter – this statement is simply wrong. In case of the Gauss filter, the factor f (in Eq. 4) depends only on n (it depends on n, m and phi), and these spectral coefficients can be easily precomputed and stored.

I find that the authors’ explanation of the striping cause may be based on a misconception. It is true that the orbital configuration in connection with the measurement geometry is not helpful, but this all acts more as an amplifier. It has been shown through many simulations that pure instrumental noise, when amplified in this way, cannot explain the observed stripes. The most likely reason of the stripes is unmodelled systematic effects (errors introduced by imperfect reduction of short-time mass motions in ocean and atmosphere, and correlated instrumental effects).

The paper of Swenson and Wahr (2006) is referred to, but only in connection with the Gaussian filter. The authors apparently are not aware that Swenson and Wahr develop an advanced anisotropic filter method in this paper that goes far beyond the Gaussian filter.

In section 2, page 1876, the authors claim that "as a consequence of the previous observations... 1. the dW/dx component contains very little if any vertical striping noise". In fact the dW/dx is not shown in the figure, and I find this statement totally unsupported by evidence in the present manuscript.

In fact, if it would be true, we could reconstruct W from the noise-free derivative dW/dx, and the other two derivatives would not be required at all.

The fact that the dW/dy and dW/dR components are observed as similar, but out of phase (i.e. similar after phase alignement) is trivial and can be read off (1) and (2) directly.

In section 3, it appears the authors build their metric for characterizing the noise on the variability of the coefficients with respect to the yearly mean (AvCnm). (As an aside, it should read anomaly and not "dispersion" – dispersion is always positive). However, this disregards that a part of the coefficients is real time-variable signal (storage variability, ocean mass change). One way to take this into account is to compute coefficients residual to an estimated annual/semiannual model for the coefficients, another way would be to compute residuals with respect to a multi-year climatology of the coefficients. But the total variability, as used here, grossly over-estimates the noise.

Like I said in the above, I totally miss a physical interpretation of the results. Many people have shown that GRACE data contains plenty of geophysical signals (even with simple postprocessing), and better postprocessing methods should enhance these signals. It might be helpful to discuss results not in terms of radial or horizontal derivatives of the potential (the radial derivative is close to the gravity anomaly), but in terms of potential or in terms of equivalent water storage.

Technical corrections

When the authors speak of ‘vertical’ striping, they mean North-South oriented striping. This should be clarified.

‘Dynamic’ gravity potential is usually called anomalous potential.

Interactive comment on Solid Earth Discuss., 5, 1871, 2013.