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

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General comments

This paper presents a method for upscaling low-frequency observations (in this case those by GRACE, with a wavelength of around 300km) to resolutions of around 26km. As in most publications of a similar nature, the key problem is signal leakage, i.e. determining whether the cause of the observed signal is due to processes within the region of interest or otherwise. In general the paper is well-written and clearly presented. I am not an expert in GRACE and signal processing techniques used to treat these particular data and so I will only comment on the mathematical concepts utilized which I

have considerable interest in.

My chief concern with this work is the method employed. Inverse methods are notorious for seeming simple from the outset, but also being subject to several issues of posedness by which I mean solution-uniqueness and sensitivity to initial conditions. The added complication in this case is the use of uniformly distributed variables in (2), the reasons for which might be various but not clearly explained (why not normally distributed for example?). To the best of my knowledge this algorithm is non-standard. My concerns are made explicit in a simple thought experiment which can mimic the overall behaviour with few parameters.

Consider a simple two-location problem. We have observed $\dot{M}_1^G = \dot{M}_2^G = 1$ very accurately (we can assume that our $\delta\dot{M}^G$ is negligible) and we know that this is in fact a low-frequency representation (in fact just the *average*) of two, high resolution quantities, $\dot{m}_1^G = 0$ and $\dot{m}_2^G = 2$. The task of Equation (2) is then to recover $\dot{m}_1^G$ and $\dot{m}_2^G$ from the smooth observations. Obviously there are an infinite number of solutions to this problem, and the problem addressed in the paper is not as ill-posed, but certain interesting features emerge from studying this toy problem.

1. *The ensemble mean is dependent on initial conditions*: With the initialization $\dot{m}_1^G = \dot{m}_2^G$, then the ensemble means of $\dot{m}_1^G$ and $\dot{m}_2^G$ will be identical. Otherwise the difference is proportional to the difference in the initial condition. With hard boundaries and thresholding this might be a non-issue. However I still would like to see a sensitivity analysis with respect to the initial conditions, where the initial conditions are allowed to vary at the same order of magnitude as the anticipated fields.

2. *The ensemble uncertainty is dependent on initial conditions and the observation value*: Re-running the experiment with $\dot{M}_1^G = \dot{M}_2^G \in \{1, 2, 3, 4, 8\}$ and zero initial conditions reveals an empirical standard deviation which scales linearly with the observed value. Changing the initial conditions also changes the ensemble un-

certainty. I therefore suggest a sensitivity analysis of the ensemble uncertainty with the initial conditions. Incidentally this observed relationship also suggests a reason why, on the whole, the uncertainty in Fig. 8 resembles the fields of Fig. 9.

3. *The ensemble mean depends on the fractional coverage* F_{ij}: With identical initial conditions, the one with the higher F will dominate in accordance to how large it is with respect to surrounding nodes. This does not seem like a desirable property with this application where maximum mass loss should be permitted at peripheral nodes. This also explains why in Fig. 9A all peripheral nodes within ice-containing regions (when zoomed in) are reported to experience near zero mass loss and why on Pg. 11 Line 23 the authors state 'The Canadian Arctic mass loss is concentrated in numerous discrete ice masses where $F_{ij} \rightarrow 1$'. Another observation is that lower Fs also result in less uncertainty than what would be obtained with $F \rightarrow 1$.

The *R-software* code for the above is given in the appendix below. Other papers which authors cite do not use this form of stochastic inversion. In general I think the method is not sufficiently well-known to be assumed as given. A toy study should be carried out to validate the method (maybe a larger, 1-D extension of my simple thought experiment above, using a Gaussian filter, but where true values are known and uncertainties can be quantified as accurate or otherwise).

Other Comments

- Pg. 4 Line 8: 'We do not purport to resolve spatial heterogeneity in mass loss at the scale of individual glaciers'. Do you mean a sub-26km scale? If not, surely resolution at this scale is required if one wishes to determine whether ice loss

took place in either the ice-sheet proper or the GrPGIC?

- Pg. 4 Line 15: The reason why a spherical harmonic representation of the mascon solution is used as opposed to the original solution is a persistent question-mark throughout the paper. In this line you state that the mascon solution is not appropriate because of the hard boundaries which is understandable. However a more straightforward approach would be to work at getting finer-resolution mascons straight-off rather than reverse-engineering a spherical harmonic solution and then upscaling the resolution. The reason why this should be better than working with a modified mascon approach should be discussed.
- Pg. 4 Line 18: Barletta et al. (2012) also use a Monte Carlo approach by perturbing the spherical harmonic coefficients to assess uncertainty, this should be clarified.
- Pg. 5 Line 7: Is seasonal detrending applied? This should be made explicit.
- Pg. 5 Equation (1): The perturbation is *not* a sample from the spatial field under uncertainty. What you are sampling is a constant bias which is very different (and can change the results considerably). What I mean is depicted in Fig. 1-3 in this manuscript. Assume you are in 1D space (*s*), and your 1σ and 2σ intervals are given by the dark and light shading (Fig. 1). Then random samples from this error field are given by Fig. 2. Equation (1) produces samples as in Fig. 3, i.e. at each location the error is constrainted to be proportional to the error at every other location with known proportionality constant. This, to me, seems unrealistic.
- Pg. 6 Line 6: 'spherical harmonic *representation* numerous times'.
- Pg. 7 Equation (3): This equation is not correct. If you are staying in the spatial domain, the Gaussian filter is a convolution operation. Straight-off multiplication is only possible in the Fourier domain.

- Pg. 9 Line 10: There is something unclear with the use of the threshold in non-ice areas. According to (2), if you start with $\dot{m}^0_{ij} = 0$, then for $F_{ij} = 0$, $\dot{m}^k_{ij} = 0$ for all k. Therefore I'm not sure in what situations thresholding is required?
- Pg. 10 Equation (5): Please define F^{GrIS}_{ij}. Also, if this is the fraction of ice-covered areas attributed to the ice-sheet proper then shouldn't (5) simply read as $\dot{m}^{GrIS} = \sum_{ij} \dot{m}_{ij} F^{GrIS}_{ij}$? If not then something is unclear.
- Pg. 11 Line 23: Why this is observed for $F_{ij} \to 1$ can be explained from the above thought experiment.

Appendix: Toy study R-code

```
N <- 1000 # number of perturbed observations

# Assume I have observed a global mean of 5 pm 2
std_obs = 0.001     # observation std
mu_true = c(0,4)    # true mdot
M_obs = mean(mu_true) # true Mdot

n_sub <- length(mu_true)
f = c(1,1)   # fractions

# Initialize simulation vectors
m_sim <- matrix(0,2,N)
M_sim <- rep(0,N)
```



```
# We know that these are reproduced by high-res processes.
# Now run the algorithm
for (j in 1:N) {
 #Initial estimates
 M = 0          # Initial Mdot
 m = c(0,0)     # Initial mdot
 M_pert <- M_obs + std_obs * rnorm(1) # Perturb observation
 for (i in 1:100) { # For each perturbed observation
   # Find the difference between the
   # perturbed observation and Mdot from (1)
   delta <- M_pert - M
   m <- m + delta*runif(n_sub)*f  # Run equation (2)
   M <- mean(m)    # Smooth observations (psuedo-3)
 }
 m_sim[,j] <- m
 M_sim[j] <- M

}
```

Interactive comment on The Cryosphere Discuss., 7, 3417, 2013.

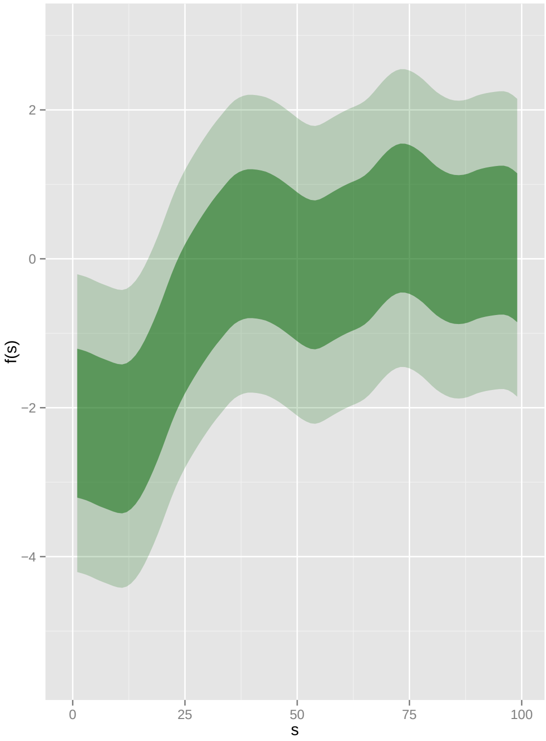

Fig. 1.

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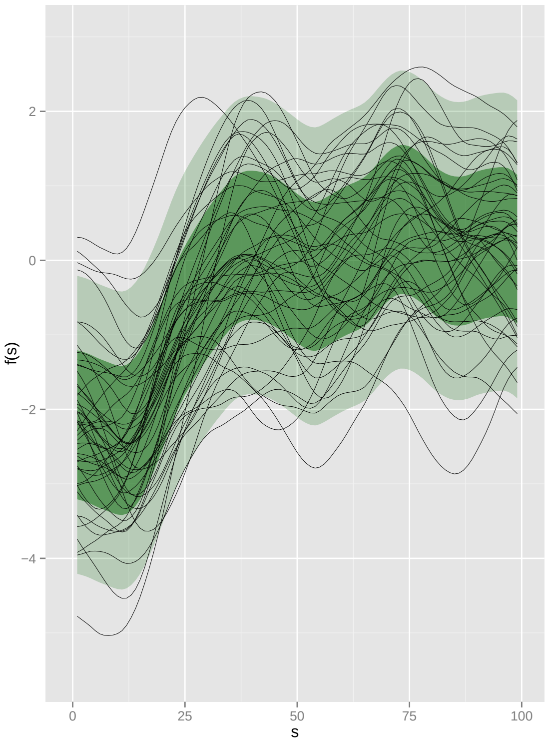

Fig. 2.

C1121

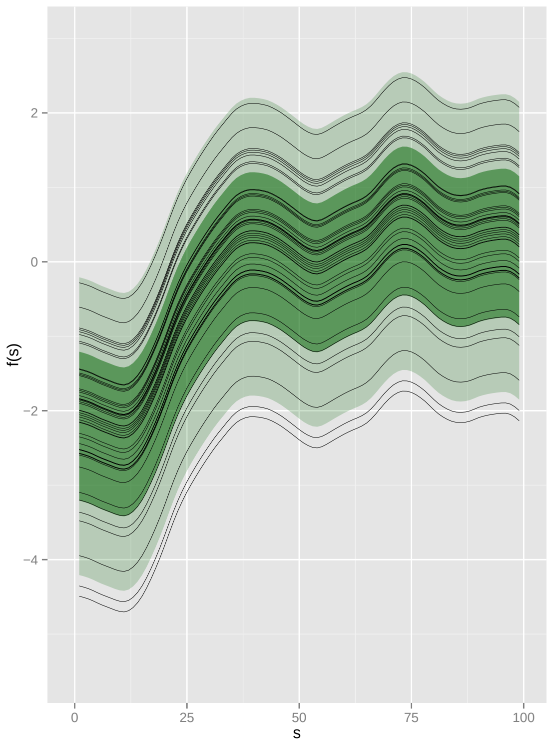

Fig. 3.