Review of "Constraining GRACE-derived cryosphere-attributed signal to irregularly shaped ice-covered areas "

I feel that there is not enough logical information or explanation in the manuscript for readers to fully understand the rationale and method. So, some of my comments below could be mistaken. But I am going to try the best guess possible any way. The scientific objective of the manuscript is very important and clear. By trying to attribute the signals to specific regions using some ground truth such as ice coverage, the manuscript attempts to enhance the spatial resolution of GRACE data and partition the mass loss signals of Greenland ice sheet proper and the peripheral ice fields. While the fact that incorporating ground knowledge can enhance GRACE results in resolution and accuracy is appreciated and agreed, I have some doubts about the level of success outlined in the manuscript. Part of the reason for my reservation may be caused by my failure to fully understand the method and the logic as described. The topic is very important and timely. The method has potential to enhance GRACE results by reducing and quantifying leakage. But I feel that more explanation and/or work need to be done to make it a solid piece of work with significant impact on the community. Therefore, a major revision is recommended:

Major points:

1. My primary reservation about the method is with the spherical harmonic truncation at 60 and the Gaussian filter with a scale of 200 km. This very much limits the spatial resolution of the method. It is not clear to me whether the R_{ij} perturbations in each iteration can successfully pass the 26-km ground information into the inversion if spherical harmonic representation of only up to degree 60 is involved in the inversion. I do not know if the inverted parameters are grid values or spherical harmonic coefficients. The way the manuscript is written got me confused. In page 3420, I thought dot M_{ij}^G are spherical harmonic coefficients. Then equation (3) implies spatial domain with nodes rather than spherical harmonic domain. Otherwise, d_{ij} would not make any sense. In equation (2), it sounds like in grid domain again. But they all use the same notation dot m or M_{ij}. And there are so many such notations that make it very confusing. As another example, the σ in page 3420 is referred as a standard deviation when it should be described as a characteristic length scale. If only I knew whether dot M_{ij}^k is spherical harmonic coefficients or some kind of node values, I would understand the method a lot better. So, I can only assume that the data to the inversion is spherical harmonic coefficients, but the parameters to be inverted are in node domain. Ok, now I think the iterations are done in spherical harmonic domain also. There is just not enough information there to know how the method actually works. Equation (2) also seems to be inconsistent. Is dot M_{ij}^G Gaussian smoothed? But part of your Δ_{ij}^k is smoothed (dot M_{ij}^k), and then used to update the un-smoothed m_{ij}.
2. For a new method to be validated, it would be nice to have some kind of simulations for the inversion. For example, a ground truth can be assumed. Then spherical harmonic data are generated with or without noises. The said method can be applied to the spherical harmonic data. Then, the inverted values can be compared with the truth that went into the simulated data. This would be a clear indication whether the method works or not.

3. I do not understand why they use the mascon-derived spherical harmonic representation as data. Can they start with a normal L2 product? They claim in the summary that it is possible.
4. Figure 1A is for cryosphere-attributed mass changes as mascons. Why are there so big changes in the oceans? Did Luthcke et al., 2013 apply any a priori constraints to the ocean grid at all? These do not qualify to me as cryosphere-attributed mascons. The manuscript repeatedly compares with this cryosphere-attributed mascon in spherical harmonic representation and claim to have similar results. Do they imply that this somehow validates their results? This kind of language leaves the reader very confused.
5. I assume that the purpose of R in equation (1) is to try to simulate GRACE errors. It is not clear that a constant R for all coefficients for each simulation is statistically enough to represent the full effect of data noises. The meaning of R_{ij} also is not very clear. If they keep changing, how do we know that the method converges to something that is desirable and unbiased.
6. A journal paper should be logical and straightforward for a reasonably informed reader to understand. For example, the method should be described one step at a time, what are the data? What are the parameters? What is the purpose of R, what is the purpose of R_{ij}? Why do they need to start with mascons and then compare with mascons. Why reproduce?

Minor points:

1. There are quite some imprecise languages or technical terms in the manuscript that I suggest the authors to proof–read it again after addressing the major points. Some examples are listed below.
2. "Unlike other techniques, a Monte Carlo inversion approach does not require an assumption that rates of mass change are constant within or across pre-defined regions, such as drainage systems. Perhaps, this should be some other techniques. Not all other techniques assume this.
3. σ=200 should be σ=200 km, and this should not be called standard deviation, better with physical meaning to avoid confusion. Also, I am not sure if equation (3) is even correct. It implies that dot M and dot m have different unit because σ has the unit of a length. If (3) is in spatial domain, what is the meaning of Mij? Covariance? If in spherical harmonic domain, i is for degree, j is for order, then what is the meaning of dij?