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Interactive comment

Interactive comment on "Estimating Annual Water Storage Variations Using Microwave-based Soil Moisture Retrievals" *by* Wade T. Crow et al.

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We thank the reviewer for their useful comments. These comments highlighted some areas of weakness within the discussion paper.

Reviewer quote #1:

This work approaches a very challenging issue: how far we are from a satellite-based estimation of every term of the terrestrial water balance? Probably this is the underlying idea of the authors, but for some reason the focus of the paper, as reflected by the title, is switched to the possibility of estimating water storage from microwave-based surface water content. May be the intention is still there, and this paper represents just a first piece of results. However, it would be useful to clarify the original idea behind the concepts presented here

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Discussion paper

Response:

The motivation (or in the words of the reviewer "the original idea") for estimating surface water storage from microwave remote sensing is laid out in the first paragraph of the original discussion paper. To quote:

"Within the past decade, the analysis of data products from the Gravity Recovery and Climate Experiment (GRACE) satellite mission (Tarpley et al., 2004a; 2004b) has led to an enhanced appreciation of the role played by inter-annual variations of total terrestrial water storage (S) within the terrestrial water budget (Chen et al., 2009; Rodell et al., 2007; Syed et al. 2008). However, the application of GRACE S retrievals is potentially limited by their extremely coarse spatial resolution (∼200,000 km2). In contrast, microwave-based surface soil moisture (θ) retrievals can be obtained at relatively finer resolutions (typically ∼1,000 km2). However, such retrievals are hampered by both shallow vertical support (reflecting soil moisture conditions only in the top several centimeters of the soil column) and substantially-reduced accuracy for dense vegetative cover. As a result, they are generally assumed to be of limited value for examination of S variations and commonly neglected in water budget studies. However, recent empirical work demonstrates that microwave-based θ retrievals are well correlated with GRACE-based S estimates in certain regions (Abelen et al., 2013; 2015). This suggests that θ retrievals retain some value for water-balance studies - particularly at spatial scales finer than the resolution of GRACE products."

Or stated more concisely: 1) gravity remote sensing has revealed that inter-annual variations in terrestrial water storage are important; 2) gravity remote sensing suffers from severe resolution limitations; and 3) microwave remote sensing of soil moisture offers a potential approach for providing higher-resolution assessments. This is the rationale behind looking at microwave remote sensing.

We feel this rationale is laid out clearly early in the manuscript. However, it could perhaps be (re-)emphasized more throughout the entire manuscript to address the confusion noted by the reviewer.

Reviewer quote #2:

. . .alternatively, it should be emphasized the relevance of thermal data in context of this study.

Response:

This point is directly addressed by the second paragraph in the original discussion paper. To quote:

“Confirming such potential will require the availability of accurate terrestrial water flux variables. Recent progress in the remote sensing of S and θ has been mirrored by the increased consideration of satellite-derived evapotranspiration (ET) retrievals in a water balance context (Senay et al., 2011; Hain et al., 2015; Hendrickx et al., 2016; Wang-Erlandsson et al., 2016). In particular, when combined with precipitation (P) and basin-outlet steam flow (Q) measurements, satellite-derived ET estimates can be used to verify estimates of S variations (dS/dt) obtained from various independent sources (Han et al., 2015). This opens up the possibility for the objective “top-down” evaluation of dS/dt estimates obtained from various remote sensing sources and the opportunity to empirically confront “bottom-up” expectations for these products based solely on theoretical considerations.”

Or stated more concisely, thermal-based remote sensing observations are needed to provide evapotranspiration estimates which - when combined with rainfall and stream flow measurements - can be used to independently verify estimates of terrestrial water storage variations obtained from various remote sensing sources.

Again, we feel that these first two paragraphs of the discussion paper directly address the overarching motivation issues raised by the reviewer. However, if given a chance to further revise the paper, we will ensure that these points are better carried throughout the entire manuscript.

Reviewer quote #3:

In a similar way, the calendar year aggregation deemed as questionable by the authors themselves (pag.16, Discussion) appears as an “exit-strategy” following a monthly-scale analysis that provided unsatisfactory results.

Response:

The reviewer is misunderstanding our point on page 16 (of the original discussion paper) regarding the use of calendar year averaging. Our point here is not to undercut the motivation for an analysis of inter-annual water storage variations, rather to acknowledge that there is some sensitivity to the particular set of “book-end” months used to define a year (i.e. January 1 to December 31 versus June 1 to July 30). This was simply done to acknowledge a potential source of sensitivity in inter-annual results and not to underline the value of inter-annual results in general. This will be clarified during revision.

In fact, the impact of inter-annual terrestrial water storage variability on the terrestrial water cycle is an area of significant scientific interest. See, for example, recent work aimed on the detection of decadal-scale variability in terrestrial storage due to long-term meteorological drought and patterns of anthropogenic ground water extraction or work on the role of groundwater in modulating the impact of climate trends on the hydrologic cycle. These are important scientific issues which can be largely addressed via the measurement of inter-annual water storage variations. This can (and should) be emphasized more in the discussion paper. Obviously, improved temporal resolution (down to e.g. monthly) would be useful in some cases. However, it is unfair to characterize the resolution of inter-annual variations as a fall-back “exit-strategy” meant to mask a failure to achieve a more important goal. The characterization of inter-annual variability is a key goal in and of itself. The discussion paper can easily be edited to make these points more clearly.

Reviewer quote #4:

It would have been reasonable to support the period of temporal aggregation with some considerations about the hydrological yearly cycle in each basin.

Response:

This is a fair point. Ideally, the period of temporal aggregation would have been based on hydrological considerations. However, there is an important practical issue to consider. Preliminary analysis suggests that adequately capturing monthly variations requires seasonally and spatially-varying parameters (to capture the relationship between surface soil moisture and terrestrial water storage). Given the (quite-limited) temporal sample at our disposal (i.e. 8 years), it quickly becomes impossible to adequately calibrate and validate such a high-parameter approach. So while we suspect that a finer (e.g. seasonal) scale approach is possible, we simply lack the data to adequately validate it. This point was already made in Section 5 of the original discussion paper but will be clarified further in future drafts.

Reviewer quote #5:

At the end, the overall impression is that the authors tried in every possible way to extract a similarity between Grace and AMSR-E datasets, and they finally got it.

Response:

This is not a fair impression (although we acknowledge that weaknesses in our write-up may have contributed to it).

As described in discussion paper, we “tried” only two operations (i.e. linear smoothing and temporally lagging) to resolve both monthly and inter-annual water storage variations (dS/dt). Both operations were applied via only two parameter degrees of freedom (i.e. the application of 3 monthly weighting parameters constrained to sum to one).

Based on our attempts, we did not feel like we could adequately validate the monthly approach and stated that conclusion clearly in the original discussion paper (see above and Section 5 of the write-up).

In contrast, inter-annual dS/dt estimates derived from both water balance consideration and GRACE are actually extremely robust. This point was made in Section 4.2 of the original discussion paper:

"Our primary goal is determining the potential for explaining observed annual P-Q-ET variations in Figure 4 using the microwave-based dSPM/dt proxy introduced above. Our first priority is empirically evaluating the assumptions - expressed in (4-6) - which underlie the proxy. The first issue is the degree to which the appropriate temporal averaging of microwave-based soil moisture via (4) can be used to obtain a robust linear proxy for P-Q-ET. Figure 5a addresses this by plotting the average linear correlation for all the medium-scale basins between annual P-Q-ET and dθPM/dt obtained using all potential combinations of WDec, WNov and WOct (where WDec + WNov + WOct = 1.0). Plotted correlations in Figure 5a are generally greater than 0.50 [-]. In fact, even after realistically accounting for the impact of over-sampling due to spatial and temporal auto-correlation in the P-Q-ET fields (Section 2.3), sampled correlations are statistically-significant (one-tailed, 95% confidence) for all possible combinations of WDec, WNov and WOct." To summarize, AMSR-E is transformed into a proxy representation of inter-annual dS/dt (dθPM/dt) via the application of only three monthly weighting parameters (constrained to sum to one). All possible combinations of these parameters lead to an expression of dθPM/dt which has a statistically-significant relationship with (independent) basin-scale measurements of rainfall minus evaptotranspiration minus stream flow.

Therefore, this is not a result that needs to be aggressively "extracted." It is a robust relationship which emerges from any parameterization of a simple weighted average. Also, given the important role of inter-annual water storage variations in a number of research and water resource application issues, it is not a conclusion which can be fairly characterized as a "fall-back" consolation prize.

In the original write-up, we devoted considerable space to a discussion of weakness and limitations in our approach. This (attempt at) objectivity may have contributed to the reviewer's perception that our presented results were obtained only after multiple failed attempts and arduous manipulation of the data. We believe this is a misperception that can be corrected via modest revision of the discussion paper.

Reviewer quote #6:

To this extent, the paper is valuable, and it is able to bring new knowledge, even if the fee paid to the empiricism is probably too high.

Response:

We are unclear what empirical "price" is actually being paid here. As described above the proposed empirical relationship (between surface soil moisture and annual dS/dt) is simple, robust, and statistically-significant (when applied appropriately at an inter-annual time) scale. It is a robust empirical "top down" result which will potentially shape our "bottom-up" understanding of large-scale processes linking surface soil moisture with deeper hydrologic units As such it provides a "dividend" rather than paying a "price."

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