

Review of the manuscript “Improved GRACE regional mass balance estimates of the Greenland Ice Sheet cross-validated with the input-output method” by Z. Xu, E. Schrama, W. van der Wal, M. van den Broeke, and E. M. Enderlin.

The manuscript investigates, and attempts to improve, aspects of the GRACE analysis and the input output method (IOM) for the estimation of basin-wise mass balances of the Greenland Ice Sheet.

As for the GRACE analysis, the study builds on a mascon inversion method investigated by Wouters et al. 2008, Schrama and Wouters 2011, Bonin & Chambers 2013 (see reference list in the manuscript). For a set of prescribed patterns (each representing a homogeneous mass change over a part of the ice sheet) scaling factors are estimated in a least-squares adjustment to the GRACE gravity field variations. The incompleteness (or over-simplification) of the set of prescribed patterns causes errors in the inferred basin mass changes, which may be called leakage errors. The manuscript seeks improvement in correcting these leakage errors by employing a subsequent leakage error correction (similar to King et al. 2012, Nature), which is based on simulation results.

As for the IOM, the theory is simple: take the difference between surface mass balance (SMB) and ice discharge (D). In practice, SMB and D data are incomplete and subject to biases. It is therefore common to consider *anomalies*, rather than total values, of SMB and D. These anomalies need to be defined with respect to a certain reference state in which the ice sheet is assumed to be in balance. Different approaches to determine the reference SMB and D exist. The manuscript addresses their differences and the choice of the most appropriate approach. It cross-validates the IOM results with the GRACE results with the aim of supporting the evaluation of methods.

At least that is what I understand from the manuscript. Unfortunately, at many places it is unclear, inconsistent, wrong, or incomplete. I cannot believe that the five authors have carefully read and checked the manuscript prior to submission. In return, the voluntary reviewers have to invest more time. This is not fair.

I address two major deficiencies and append an incomplete list of minor comments

The **first major deficiency** is the corrupt mathematical development in Section 2.2. Let's start with Equ. 5. I understand that SMB_t and D_t are mass rates [in units of mass change per time] while δTMB_t is a cumulative mass change [in units of mass]. It is really uncommon to denote an integration by the symbol δ . Very confusingly, this symbol is used to denote a difference some lines later (p. 4669, line7). Also, t is used in two different senses in the same equation (as running variable and as upper limit of the integral). In equation 6, the upper integration limit is t_n , instead. In Equ. 6, I understand that SMB_0 and D_0 are cumulative mass changes [unit of mass] again. Then, the equation in the line after Equ. 6 cannot be correct because it contains a mass rate [mass change per time] at the left side and a cumulative mass change [mass] on the right side. All the later discussion in the manuscript on D_0 and SMB_0 suffers from the confusion in the formalism by which these quantities are introduced. Equation 7 is wrong because the total mass balance in the interior must depend on ice flow across the 2000m contour.

The **second major deficiency** is that the “IOM-based” simulation used to derive the leakage correction in the GRACE results is not well described. Therefore the reader cannot assess the validity of the leakage correction based on this simulation.

Page 4673, line 8: How can one interpolate D on a grid? Of course you can express the ice flow-related mass balance component locally, in theory. But I don't think you have the data to

evaluate it practically. The discharge D data is just the integral of the flow-related mass balance component over the entire basin.

Note that the ice-dynamics part of the story is the complicated part because it is so spatially concentrated, different from the SMB part. To avoid, or “correct” leakage effects that are compatible with both kind of mass balance components will be challenging unless one uses over-simplified assumptions.

Given the incomplete description of the procedure, it is not clear whether the perceived improvement of the GRACE method follows a circular reasoning: Adapt the GRACE results so that they better fit the IOM results, and subsequently “validate” the success by the same IOM results.

From Table A1 it appears that in the simulation, the leakage errors of the different basins do not sum up to zero but to about -36 Gt/yr. This needs to be commented. Does it mean that previous applications of the mascon method were subject to an error of this magnitude?

Page 4676, line 7: Does the vector y represent values of a grid?. If so, are the errors added in line 7 assumed to be spatially uncorrelated? Does the simulation use a full time series or just a linear trend?

Some more comments:

Section 2.3: first 3 lines are unclear to me. I don't see why averaging should result in an error. Also, no averaging is done, but cumulation.

Page 46671, line 27: Unclear why discharge needs an SMB correction.

Page 4672, Line 14: how can the discharge be negative?

Line 23: not clear to what numbers the word “they” refers.

References to figures and tables are wrong on p. 4672, line 12, page 4670, line 11

Abstract, line 13: “runoff-based discharge estimates” is not clear without further explanation

Equation 9 is flawed: inconsistent fonts, inversion missing.

p. 4680, line 18: No Ellesmere Island results are shown, actually, in Fig. 5

Same line: “This suggests ...”: I don't understand the line of argument

There is so much repetition from Xu et al. 2015 (Geophys. J. Int.). Refer to this article more stringently and save some of the reader's time.

Appendix A2: The annual frequency is defined as $2\pi * 13/12$, that is, with a period different from one year. Can this be correct?

Fig. 4: “modified simulations” were not mentioned before, so that it is unclear what they are. End of the caption is missing.

Table A1: k_0 and k_1 are α_0 and α_2 in the main text. k_0 must have a unit.

Reference Noel et al. is missing in the list. Reference Colgan et al. could rather use the final version in Remote Sensing of Environment.