The Cryosphere Discuss., 4, C1125–C1128, 2010
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The Cryosphere Discussions

Interactive comment on "The sea level fingerprint of 21st century ice mass fluxes" *by* J. Bamber and R. Riva

Anonymous Referee #2

Received and published: 16 November 2010

Review of: The sea level fingerprint of 21st Century ice mass fluxes Author(s): J. L. Bamber and R.E.M. Riva MS No.: tc-2010-46

This paper gives a relatively compact and comprehensive update to the computation of RSL changes caused by trends observed in the cryosphere during approximately 2000-2009. The manuscript provides an excellent reference for what can be known with some confidence during the very earliest 21st century and the very tail end of the 20th.

While the manuscript receives generally high marks for its significant scientific content, it is still in need of some further explanations. First and foremost the title is misleading, and perhaps could be just amended to end: " ... fluxes during 2000-2009", or " ... of the first decade of the 21st Century". The time frame is somewhat murky since the

comprehensive work of Dyurgerov and Meier (2005), for example strongly sample the evolution of mass balance through the 1980's and 1990's. While my remarks below are numerous, I think this is a valuable contribution to understanding the interaction of the cryosphere and sea level.

I recommend the following changes before the paper should be accepted. These are not major things and it is up to the authors as to whether they wish to recomputed some of the cases, or simply to give some improved dialogue with respect to the Figure content.

1. Line 6, Introduction. While there has been a lot of press regarding the non-uniformity of sea level rise due to the self-gravitation effects and motion of the earth's pole through the crust since the Mitrovica et al (2001) paper, there has been little progress in actually observing the phenomenon. The authors might want to say something about this, or to update the reference for background material to include new work by Chambers et al. (2010) JGR- in press), who discuss GRACE, altimetry and the corrected GIA correction, and new work by Tamisiea et al. (2010) wherein the annual signal is discussed. The annual signal has very large amplitudes. (No lengthy discussion is asked for here, but this is up to the authors to decide how to handle this.)

2. Lines 25 to end of paragraph, Introduction. Other sources that have secular-like times scales also affect time-evolution of relative sea-level heights: water empoundment by reservoirs (e.g., Chao et al, 2008, Nature Geosci. and Fiedler and Conrad 2010, GRL).

3. Lines 25 to end of paragraph, Introduction. While the ocean dynamics may have spectral power concentrated at periods below about 4 years, (Carl Wunsch treats this very well in some review articles) they will also produce some finite variability that approach decadal time-scale. Likewise, the ice sheet losses seen in Greenland where the losses have evolved from east to west coast also imply some time-variability of sub-decadal time scales. The authors should acknowledge this, perhaps even quanti-

TCD
4, C1125–C1128, 2010

tatively.

4. In the Methods section (which is by and large well-written), there is not much discussion of errors or how some discrepancies in results that have been discovered recently. I especially encourage the authors to consider the results of Yu et al (2010) for the ice balance of the Lambert-Amery system (encompassing a large territory of East Antarctica) which are at about + 23 Gt/yr. These contrast with the results of both Rignot and Thomas (2002) and Rignot, Bamber et al. (2008) (which report near balance). Perhaps this is the level of uncertainty? Different reporting periods? In any event, the discrepancies cannot (should not) be pushed 'under the rug'.

5. In a related issue, there should be errors reported with the mass balances of Table 1. Also, it would give greater clarity if there were footnotes for the periods actually involved in the observations of "Primary source", since they are actually not all from precisely 2000-2008. (However, it is still fair to call these "Mean mass loss 2000-2008").

6. The authors might want to improve the colours of Figure 1. In printed versions the ice mass changes DO NOT show very well. This should be easy to fix with a better colour table using Global Mapping Tool.

7. The authors should explain that the four frames in Figure 2 can only approximately add to get Figure 3, since the governing differential (or integral) equations for the sea level are non-linear, so that solutions don't really add in any exact sense. This is not a major criticism, but the authors need to clarify this.

8. The supplementary material has a map showing GIA corrected sea level for ICE-5G, VM2. Was this map recomputed by the authors? It would appear not. The authors must give the details (web site, ftp site, or other details about where they obtained this. Alternatively, I very strongly recommend that the authors contact Don Chambers, John Wahr or Mark Tamisiea for an update to this controversy over the appropriate GIA correction. That up date would be a very important improvement for this supplementary material.

TCD
4, C1125–C1128, 2010

9. Finally, in the conclusions, since this paper is really about the gravitational effects of the cryosphere, there is considerable literature that predate Mitrovica et al. (2001) that is primarily oriented to measuring ice-related gravity change from both space and with terrestrial instrumentation. An example of recent results on the terrestrial components is given in Amalvict et al. (2009). A compelling case for the strength of these effects was reported by Ivins, Rignot, et al. (2005) for Antarctica. Having comprehensive local measurements of the potential field changes (in gravity) as driven by ice changes. Note these are those that simultaneously drive changes in the sea level observed by ocean-based and/or space data. This would provide a robust link: in fact a way of verifying that the underlying causality assumption motivating this paper.

A few references:

Yu, J., H. Liu, K. C. Jezek, R. C. Warner, and J. Wen (2010), Analysis of velocity field, mass balance, and basal melt of the Lambert Glacier–Amery Ice Shelf system by incorporating Radarsat SAR interferometry and ICESat laser altimetry measurements, J. Geophys. Res., 115, B11102, doi:10.1029/2010JB007456.

Amalvict, M., P. Willis, G. Wöppelmann, E.R. Ivins, M-N. Bouin, L. Testut and J. Hinderer (2009), Isostatic stability of the East Antarctic station Dumont d'Urville from long-term geodetic observations and geophysical models, Polar Res., 28, 193-202, doi: 10.1111/j.1751-8369.2008.00091.x. 2009.

Ivins, E.R. E. Rignot, X. Wu, T.S. James and G. Casassa, Ice mass balance and Antarctic gravity change: Satellite and terrestrial perspectives, In, Earth Observation with CHAMP: Results from 3 Years in Orbit, Ch. Reigber, H. Luhr, P. Schwintzer and J. Wickert, (eds.) Springer-Verlag, Berlin, 3-11, 2005, ISBN: 3-540-22804-7.

Interactive comment on The Cryosphere Discuss., 4, 1593, 2010.