1 General Comments

The paper describes a new update / improvement for the thermomechanical ice sheet model RIMBAY and then several experiments highlighting the insights gained from this new development:

1. Sliding laws are coupled in part to the properties of the subglacial water system
2. Water is routed by a mass conserving algorithm that appears to be similar to the upwinding practiced by time-dependent version of the *LeBrocq et al.,* (2009) water model for the Siple Coast.
3. When water is routed into an enclosed basin within the hydropotential, it is allowed to pool until they overfill at which point the lake basin overflows at the lowest point along the hydropotential rim.

This is an important development as water budgeting work by *Carter et al.,* (2011) showed that many active subglacial lakes are found at collection points in the hydropotential and that when a subglacial lake is filling it prevents the meltwater generated upstream from travelling any further. Additionally it is significant that the authors are considering the co-evolution the ice sheet geometry and subglacial hydrology as did *Johnson and Fastook* (2002) did for the Pleistocene glaciations for North America. Furthermore, uncertainties about the basal environment continue to pose challenges for predicting future sea level rise in particular from Antarctica (*Lemke et al.,* 2007). The largest concern is that the work in its current form does not place itself particularly well in the context of existing literature and consequently makes several claims that are not very reinforced. And while the model in its current form is a new way of illustrating the point the authors are trying to make, there is no consideration of how their approximations may affect reality. A prime example of this comes from the description of the topography in which the authors describe the Gamburtsev Subglacial mountains as "West Antarctic" and then use a terrain generation algorithm that results in an inordinately high number of completely enclosed bedrock basins, completely inconsistent with published descriptions of the Gamburtsev Mountains topography (*Bo et al.,* 2009; *Bell et al.,* 2011).

2 Specific comments

Page 5230:
Line 17: any thoughts as to why one might be able to calibrate the reference flux to various regions but not the whole ice sheet?

Line 20: at a minimum this should be changed to (e.g. *Wingham et al.,* 2006), as work by *Stearns et al.,* 2008, and *Carter and Fricker,* (2012) have also inferred flux rates by the same technique). Secondly the *Wingham et al.,* (2006) number represented the water flux during a lake drainage event occurring over a time frame of ~2 years, and then went on to estimate a refilling time of 30 years.
Other efforts to quantify flux have stemmed from integrating estimates for basal melt over a whole catchment (e.g. *Joughin et al.,* 2004; *Carter et al.,* 2009a). There have also been efforts to infer water flux from seismic (*Winberry et al.,* 2009).

In general, there are quite a number of ways people have inferred this value in the past and range reasonable values for this parameter.

Page 5231:

Line 12: I am a bit confused by the wording here. Again a quick read though of *Joughin et al.,* (2004) would show that this term can be quite significant, especially in ice stream tributaries. Also you should reference the term for frictional basal heating. Again I recognize this model a simplification that works for the point you're trying to illustrate, but in order to get a more realistic representation of ice sheet flow in the future, this is HUGE.

Line 20: You probably want to cite *Shreve* (1972) here.

Page 5232:
Line 4: This is an important issue you highlight. It would be useful to describe how you illustrate the water layer thickness term more specifically.
Line 7: the wording here is a bit awkward
Line 9: You're mentioning that the model is based on finite differences only NOW?
Line 20: Tautology. You may be able to eliminate this sentence.

Page 5233:
It may be useful to explain briefly in 2-3 sentences how this is different from up-winding which is commonly practiced in other models (e.g. *Rutt et al.,* 2009)

Page 5236:
Line 10: I am pretty sure the Gamburtsev's are in EAST Antarctica not WEST Antarctica (*Bell et al.,* 2011).
Also it seems wise to enquire about the nature of terrain generation algorithm used? The presence of small-scale deep enclosed basins does not seem particularly realistic (Anderson and Anderson, 2010).
Line 20 - 25: This is an example of a perfectly acceptable and well-justified "fudge factor"

Page 5237:
Line 11: This is a really high melt rate for East Antarctic conditions (*Carter et al.,* 2009a; *Pattyn et al.,* 2010; *Bell et al.,* 2011).

Line 25: If you compare this against the lake density reported in some of the various lake inventories (*Wright and Siegert* (2011) being the most recent) you will find your number somewhat high. For the prposes fo the model this is fine, but if the intention is to bring the model into a more realistic doman some mention of this discrpenacy is in order.

Page 5237:

Line 1: You might want to call this the "uncoupled run"
Line 1: *Tabacco et al.,* (2006) has some commentary on terrains in which subglacial lakes are located.

Line 5: In fact once again there are several papers including some by your respective co-authors describing this (e.g. *Thoma et al.,* 2012)

Line 7: Relative to the control model. This whole sentence needs to be rethought.

Line 10: This is a VERY high number for basal melt rate outside of those locations where shear heating is active (*Joughin et al.,* 2004; *Carter et al.,* 2009). For the purposes of what you are trying to illustrate it is perfectly acceptable, but one should be aware that your number are probably and order of magnitude too high.

Line 22: Some of the Siegert papers estimated the % age of the bed covered with sublacial lakes. It is substantially lower than then number you present. I suspect this has to do with the topography used. Again, for what you're attempting to illustrate this is fine, but awareness of this decrepancy is necessary. In fact one of those

Page 5239:
Line 6: Here is a time when you may want to mention that other authors do this as well, sine you are showing your work as an improvement upon the straw people. In fact BwB looks remarkably similar to the time varying water model LeBrocq et al., 2009, except it's coupled to an ice sheet.

Page 5241:
Line 10: In fact *Anandakrishan and Alley,* (1997) report this as having happened, and it appears to have be validated by *Carter and Fricker* 2012)

Line 13: You may want to look through *Folwer* 1999, before making this statement, or *Evatt et al.,* 2006, applies more directly to ice sheets, before making too many claims about subglaical lake drainage. *Scambos et al.,* (2011) actually describes lake drainage in response to changing ice geometry.

Figure 6: This is great.
It would be also nice to see how ice volume evolves over time with each of the different model runs.

Figure 7: This needs a different color scale.

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