General Comments

This is an outstanding first attempt to model North American late glacial freshwater discharge. I appreciate the side by side comparison that results from different ice sheet models, and a synthesis of the extensive literature concerned with estimating meltwater routing and discharge from North America. Licciardi et al (1999) is the oft-cited study to which this effort closely compares – but Licciardi et al is based on data interpretations versus the models used here. It could be useful to compare the Licciardi data to the output here and discuss the differences and similarities directly (perhaps in Fig. 15)?

The synthesis of the freshwater routing provided by this study is excellent, but there is a lot of uncertainty in the chronology of meltwater routing– especially with respect to dating. Agassiz routing is particularly contentious – but it's also the most intensively studied. Part of this challenge is attributed to the size of the basin, but I also think the more lines of evidence that are uncovered, the greater the potential for uncertainty (at least at first).

Another challenge I see when comparing model output and data is that the geologic record is often highlighted by short term (<100 yrs) flood events (e.g. Kankakee Torrent, Champlain freshening event, Murton flood, 9.3 event, etc) – and this modeled output has to average discharge over longer periods of time. Some of the flood events must have resulted from short term release of meltwater stored in lakes, which this modeling attempt cannot resolve (perhaps in the future?) But I also wonder if melt rates may have changed suddenly at times due to climate (cf. pg. 11, 315-323). The varve records from Lake Agassiz, Hitchcock, Superior, and Ojibway clearly illustrate that varve thickness (which must correlate with discharge) could rapidly fluctuate on decadal scales – and the Hitchcock records shows this was at times the result of climate change. I would think that combined, these two factors (storage of meltwater in lakes, and climate change on 10-100 yr scales), may have had dramatic effects overall freshwater flux.

It'd love to see a future attempt to model $\delta^{18}O$ output for the modeled discharge. Can these ice sheet models be combined with a dynamic oxygen isotope model for the LIS - as well as a model for d18O precipitation? I wonder how the $\delta^{18}O$ composition of meltwater changed through time – prior attempts to estimate glacial meltwater discharge assume a static $\delta^{18}O$ composition for meltwater (or range of values). We see decreasing $\delta^{18}O$water values for Lake Agassiz and Minong, and these have been interpreted as evidence for increasing meltwater discharge – but is this valid? Perhaps meltwater was becoming progressively enriched in O-16 as the ice sheet decayed? Long term, the best records to reconstruct freshwater discharge continue to be $\delta^{18}O$ records (both from the interior, and offshore), and this seems to be an important unknown to interpreting these records. Circulation dynamics and the influence of climate on the overflow of water from the large pro-glacial lakes could be another important complicating factor.

Specific and Technical comments

Abstract/Conclusions – for me examining the differences between the ice sheet models is an important contribution - but there's no mention in the abstract or conclusions regarding how well the models compare with the synthesis provided. Your synthesis suggests the 6G model performs better than predecessors – and I wonder if this is worth drawing attention to?

Pg. 2(43) – $\delta^{18}O$ records can provide estimates of meltwater volume, e.g. Moore et al., 2000 (YD interval & outflow from the LIS, Paleoceanography 15(1), 4-18.) I think the treatment of the $\delta^{18}O$ composition of Lake Superior overflow and LIS is treated too simply in this model – but it's an excellent attempt to get at meltwater volume from geologic data.

Pg 2, line 48. "connective tissue" is an odd phrase and ensure is repeated. The sentence could be simplified.

Pg 3, lines 67-74, fig. 4b – it's odd to see native names given for rivers. Later on in the text, all the rivers are simply referred to by their English name. Perhaps just use the official US, Mexican, and Canadian names? - otherwise you are making errors of omission. For example, the Inuit name for the Mackenzie is omitted, as are native names for the Colorado and Columbia. My suspicions is that there were other native names for the Hudson and St. Lawrence.

Pg. 4 (102) – missing parenthesis

Pg. 5 (144-145) I'm confused by the idea that the St. Louis River is progressively capturing the Upper Mississippi R - I don't see this is Dean and Philips (a challenging field guide to decipher), and haven't looked at van Hise and Leith 1911. Presumably this is at the divide near Floodwood? It seems like that in general southern basins would capture northern basins due to rebound?

Pg. 6 (182) – perhaps which recent models include a more massive LIS

(pg. 7, sec. 2.1.3) I wonder about the accuracy of the GIA models – the ice sheet models are visually easy to compare to the Dyke ice sheet reconstructions – but this isn't true for GIA, but there are clear flaws in GIA models for the mid-continent after these data are compared to strandline reconstructions (e.g. Lewis et al, GPQ, 2005). I don't have a suggestion, I just wonder how we can go about assessing and describing the spatio-temporal accuracy of the various GIA models.

Pg. 8 (255), add "in" after changes

Pg. 9 (263-265) I can image the 30 arcsecond grid may create problems for areas near drainage divides, where the potential sills have poorly approximated elevations? It seems like it would be a good idea to compare elevations in the DEM used to higher resolution topographic data – and modify the 30arcsecond DEM if necessary. I'm thinking about headwater areas – such as west of Lake Nipigon, or across potential NW outlet divides of Lake Agassiz.

Pg. 12 (351) consider using Caribou "sub-basin" rather than basin, as this is a sub-basin of the great Lake Superior basin.

Pg. 12 (359), "Our" is used? – consider "the"

Pg 15 (407) missing parenthesis after Figure 5

Pg. 15 (426) missing period before For

Pg. 18 (448) seems like the eskers and ice streams should only provide good evidence for ice divides for time periods when each were active.

Pg. 19 (481) perhaps cite the other discharge studies driven by direct interpretations of data

Pg. 30 (521) perhaps add Levac et al., 2015 (GPC 130: 47-65)

Pg. 30 (522) I'd remove the reference to Breckenridge, 2015. It looks like the NW outlet didn't open until water dropped from the Tintah, and the model I used for rebound would place after the onset of the YD. I'd argue the main Moorhead low did not occur until long after the onset of the YD.

Pg. 30 (528) Roy et al (2011) QSR 30: 682-692 would be a key publication for the timing of Ojibway drainage. As you note in line 539, our work shows there were two drainages – an initial partial drainage, and a second complete drainage sometime later. The first drainage is not accounted for in current models/hypotheses. I would guess that Lake Agassiz drained first in Manitoba, then either completely re-filled from closer/blockage of that drainage pathway, or the divide between Ojibway and Manitoba was covered by ice advance, re-filling Lake Ojibway. I'm curious if your models suggest other drainage possibilities (water routed from Agassiz to the Mackenzie via Wollaston-Athabasca?)

Pg. 31 (557) I'd change or add the reference, or modify the text. Fisher and Lowell (2012) specifically state that Agassiz could not have gone NW "until 10.6-10.1 ka cal BP)". IMO they are ignoring contrary evidence in Fisher et al. 2002 and Fisher and Souch 1998, and place too much confidence on the utility of basal radiocarbon ages to establish ice margins – but they don't suggest a 10.8 drainage in Fisher and Lowell (2012).

Pg. 34 (624) most favored hypothesis for NW routing is too generous. As noted earlier, until the Tintah can be directly dated, the strandlines suggest Agassiz did not drain NW until after the YD onset. I have unpublished sediment core data that suggests Agassiz drained east prior to routing NW – as others have argued, there was likely an early Moorhead low – caused by the opening of eastern routes – which was followed by a major Moorhead low when a NW path opened.

Pg. 34 (626) – correct the phrase "as the model simulate it"

Pg 34. (630) missing parenthesis around Anderson et al., 2014

Pg 35 (653) where is this gap to the southwest exactly? This could be an explanation for early (pre-8.2k) partial drainage of Lake Ojibway?

Figures

I might keep the scale of the axes consistent between the different model runs.

On the paleogeographic maps, only the "from data" figures include political boundaries – I find these useful when looking at the details between each map.

It might be useful to see total discharge to the oceans through time compared for all the models.