This is a worthwhile study comparing downslope drainage of GIA−based reconstructions (and one chronology from a glaciological model) against geological inferences. It is interesting to see how consistent or not GIA reconstructions are with inferred deglacial meltwater drainage.

The inclusion of only one glaciological model that lacks any data documented calibration and that is not self−consistent with the GIA model used to infer deglacial drainage in this study is a bit unfortunate.

Technically, the drainage computation is sound. However, as detailed below, more consideration needs to be given to uncertainties in the inferred "data−driven" reconstruction. This is especially the case wrt temporal uncertainties in the inferred high/low drainage. "~15.6 ka" has no clear interpretation. I've also noted some errors in the inferred chronologies stemming from out of date references with old age models. I am not on top of the current litterature and as such there may be more such errors.

There are also some inaccurate statements and some poor referencing as detailed below.

I am curious why proglacial lakes were ignored. The GRASS solver could easily have computed these and would have enabled some quantitative data comparisons as opposed to the purely qualitative comparison currently.

Anway, once comments below are addressed, I would support publication in ESurf. The topic is appropriate for Esurf, figures and abstract are appropriate,...

** detailed comments

generating up to âM−^H¼4 km of high−albedo ice−surface topography (Kutzbach and Wright, 1985; Ull− man et al., 2014)
Neither of those references are appropriate for a claim of "~4 km" elevation
(and high−albedo is obvious). Cite an appropriate primary source.

but are evaluated using limited (Tarasov and Peltier, 2006) to no (all others) geologic evidence for past drainage patterns.
Incorrect as worded, Tarasov et al, 2012 used the same geological strandline
data from 2006 as constraints.

Reconstructions of past ice−sheet thickness have proliferated (e.g., Tushingham and Peltier, 1991; Lambeck et al., 2002; Peltier, 2004; Tarasov and Peltier, 2006; Tarasov et al., 2012; Gregoire et al., 2012; Argus et al., 2014; Peltier et al., 2015), ... Instead, they are tested against terminal moraine positions and glacial isostatic adjustment,
Incorrect. The models are either tuned or calibrated to these
constraints. This is different than "testing".

"glacial isostatic adjustment" what? That is a physical
process. Do you mean "records of"?

Also, Gregoire et al, 2012 was not tested nor tuned against
terminal moraine positions nor glacial isostatic adjustment records
contrary to what I'm inferring you are currently stating.

```
with the latter being a response that is spread across hundreds of to
~1000 kilometers due to the high flexural rigidity of the lithosphere
# Incorrect scales. Yup, there is some smoothing to order 4*
# Lithospheric thickness (so order 400 km for a start), but I can also
# get significantly different RSL response between 2 sites that are
# less than 50 km apart or you can also look at RSL records to also
# note such resolution sensitivity


many studies do not include any well defined
picture of drainage basin evolution. This....
# With putting out this partial straw-man, why is there
# no mention of the Tarasov and Peltier, 2006 that does
# actively compute the self-consistent drainage chronology,
# and that documents the change in drainage basins?

In general, there exists a lack of recognition of the importance of
Pleistocene drainage rearrange- ment on river systems.
# You are creating another unsupported strawman, especially since you
# subsequently cite more references that do recognize the changes
# compared to those that don't. Yes there has been a problem to date
# with most (but not all) GCMs using fixed present-day routing, but
# there is a lot more to paleo science than GCM modelling.

This ice-sheet surface is chosen to drive the flow-routing
calculations because this is what drives ice flow (cf. Cuffey and
Paterson, 2010),
# "drive" makes no sense. The ice sheet surface is used for flow
# routing because you are extracting surface drainage. This is
# independent of the physics of ice flow.

Greenland Ice Sheet, where the subglacial topography is interpolated
from the etopo1 1-arcminute global topographic data set (Amante and
Eakins, 2009).
# I suggest you obtain a more current DEM for Greenland for any future
# work where accuracy is critical

H_i, ice-sheet thickness,...therefore are interpolated using an
iterative nearest-neighbor approach to remove stepwise discontinuities
that would otherwise introduce artifacts in the flow-routing
calculations.
# Please make the description clear and precise. How do you decide
# where the ice margin is when downscaling from order 1 degree
# resolution to 30 arc-second resolution?  This would be a critical
# issue for getting drainage correct, especially when considering
# switching between Mississippi, St. Lawrence and Arctic drainage for
# Lake Agassiz region.

Ice physics, on the other hand, are sensitive to the thermal evolution
of the ice-sheet, related thermomechanical effects on ice
rheology, and the ice basal conditions
# Thermal evolution is a derivative uncertainty as the physics of heat
# transfer is well represented in current ice sheet models. The
# primary uncertainty for paleo glaciological ice sheet modelling is
# the climate forcing (which drives the thermal evolution).  For
# regions with present-day ice cover, another primary uncertainty is
# the basal conditions (topography, roughness, sediment cover,
# geothermal heat flux).
```

```
Therefore, I investigate both types of models.

# Hardly. You only examine one glaciological model and one that was
# subject to few constraints.

and matches geological evidence for the locations of the ice domes and
ice−sheet outlines
# Misleading: Geological evidence (ie the Dyke 2004 ice margin
# chronology) for ice−sheet outlines is IMPOSED on ICE6G.


GMSL was ~135 m lower than present
# Given different interpretations of what GMSL means (relative to center of
# mass of earth, relative to present−day shorelines,..), you need to define
# exactly what you mean (Note, Lambeck 2014 eustatic sealevel is neither of
# the two definitions I provided above in the brackets)


This gravitationally self−consistent sea level theory and its
numerical implementation are de− scribed by Mitrovica and Milne (2003)
and Kendall et al. (2005)
# Keen to stay out of the politics of the GIA community, but it is
# scientific convention to also cite the originators of a theory and
# not just a recent description of it.

truncated at spherical harmonic degree and order 256. This satisfies
the Nyquist flexural wavenumber for the solid Earth models employed,
thereby allowing the solutions to be interpolated to an arbitrarily
high resolution
# Provide a reference for "Nyquist flexural wavenumber" (couldn't find
# one on google, web of science,..).  Most readers will not understand
# what this means and even more will not know it's value is
# determined...

VM5a is largely a simplification of VM2,
# Not really. Better to describe it as a modification of VM2 (yes it
# has a simplified structure but it also has 10^21 Pa s viscosity
# layer between the Lithosphere and mantle that is not present in VM2.

I initiate the GIA modeling of G12 in equilibrium at the LGM
# Should mention that is a source of error, as there is no evidence to
# support isostatic equilibrium for North America at LGM

Our flow−routing calculations neglect geomorphic change for three
major reasons. First, signif− icant changes Lake Agassiz outflow
directions occurred as a precursor to spillway incision
# This approximation is reasonable since you are not modelling lake
# levels and are therefore not making direct comparisons to
# strandlines.

Precipitation and evaporation inputs changed through time
(Fig. 2). These changes were calculated by Liu et al. (2009) and He
(2011) using a continuous LGM (22 ka) to present run of the National
Center for Atmospheric Research (NCAR) Community Climate System Model
version 3 (CCSM3) (Collins et al., 2006).
# Should mention that the TraCE−21K simulation used ICE−5G as a
# boundary condition, and so will not be self−consistent with other
# deglacial ice thickness reconstructions you are using.
```

```
Flow routing and drainage basins can be calculated only on time steps
when the ice−sheet and GIA models provide surface topography (Section
2.3, below), but the ice−sheet contribution to runoff re− quires
ice−sheet thickness to be differenced, and thus should be most valid
for the times halfway between the ice thickness time−steps.
# There is no basis for the last claim when you have 500 year
# timesteps (eg ICE5G). On that note, you need to explicitly provide
# what timesteps were used


This means that these models implicitly include the lakewater volume
within the ice mass.... as ice must be used in the models to represent
the surface loads of these large lakes in the geologic past....
# Incorrect and no easy choices here. Since most of the proglacial
# lake sites lack proximal RSL constraints, there is little basis to
# assume that the GIA models are implicitly taking pro−glacial lake
# loads into account (except wrt global ice volume required to match
# far−field constraints). Most proglacial lake regions are more
# constrained in current GIA models by present−day vertical
# velocities, but this has much poorer time resolution. Furthermore,
# models such ICE−5G have their ice load spatial extent set to (though
# with no clear documentation whether any discrepancies are allowed)
# the Dyke et al 2004 ice margin chronology. This means they have no
# load where there are proglacial lakes.

# It would be best to repeat at least one model calculation with
# inclusion of pro−glacial lake loads to quantify the uncertainty.


models to match the ob−served ice−sheet geometry,
# "observed"? Sure wish we had observations of paleo ice−sheet geometry...


While it is essential to ignore local depressions in the DEM
# Why? "Local depressions" = lakes = more data for comparision.

This is an improvement over âM−^@M−^\hosingâM−^@M−^] experiments (e.g., Condron
and Winsor, 2012)
# This citation does not make sense for this context. Better to cite
# PMIP III. Unlike PMIP III hosing that distributed meltwater fluxes
# across a band of of the North Atlantic, Condron and Winsor for the
# first time resolved what would happen (all be it for only a few
# years) to discharge from major river outlets. If you are citing
# Condron and Winsor as an example of why PMIP style hosing has no
# geophysical basis, then you need to rewrite the sentence to make
# this clear


The fully−distributed GCM meltwater inputs create a more realistic
ice−age ocean
# A bit simplistic. It is unclear whether fully−distributed GCM
# meltwater inputs can create a more realistic ice−age ocean in
# current paleoclimate modelling AOGCMs since these models lack the
# resolution to accurately resolve meltwater flux transports and
# turbulent mixing
```

```
I developed a set of data−driven drainage basin boundaries for each of
the study basins ... The second is the ice−sheet margin chronology of
Dyke et al....
# There are many significant uncertainties in this approach, that need
# some tabulation For instance, the Dyke et al chronology has
# significant temporal uncertainty (cf Tarasov et al, 2012), that
# could significantly affect deglacial drainage chronologies.

The second is the ice−sheet margin chronology of Dyke et al. (2003);
Dyke (2004), which, when lacking independent information, I use as an
approximate set of contours of ice−sheet thickness
# Does not make sense. How do you get from an ice margin chronology to
# ice sheet thickness contours?


As such, disagreements between these data−derived basins and those
derived from models, especially where the data−driven drainage basins
are tightly−constrained
# For such interpretation, you then need to provide uncertainty
  estimates for the data driven approach

but by 19.9 ka,
# I’m assuming calendar before present? Needs to be clarified

# Figures 6−9: "from data" panel: what is the age uncertainty?

# Figures 10−15 the high/low/none inferred discharge shading needs
# a visually representation of temporal uncertainties in the transitions
# eg, with hatching or someother visual texturising

18.2âM−^@M−^S17.7 ka (Rashid et al., 2003)
# You need update your citations, this is not a consensus estimate

Heinrich Event 0, corresponding to the Younger Dryas,
was 12.8âM−^@M−^S12.3 ka (Clark et al., 2001)
# Again, this is an out of date termination estimate.
# Current chronologies end it around 11.6 ka

# Your age extraction from old references also ignores recent
# refinements in ice core age chronologies and C14 calibration

 started ~15.6 ka
# what does "~" mean? +/− 0.1 ka, +/− 1 ka or ?

At 13.1 ka, ice retreat rerouted Glacial Lake Peace
# What dating scheme is going to give 100 year accuracy at 13.1 ka???

I then generated histograms for the high−flow and low−flow segments of
the model results, and performed KolmogorovâM−^@M−^SSmirnov tests between the
pairs of distributions for each ice model and river system to test how
different they are from one another
# again how was temporal uncertainty taken into account?

The ice−physics−based G12 performed the best in generat− ing
discharges during high−flow periods that were much higher than those
during low−flow periods.
# Again need to make clear what timesteps were used. Was the above due
# to G12 being provided at higher temporal resolution or not?
```

```
(1) evaporative loss that is generally >1.5 times todayâM−^@M−^Ys
observations, in spite of cooler temperatures and a longer lake−ice
season
# you ignore the likelihood of much higher mean wind speeds near an
# ice margin and their resultant impact on evaporative loss. This
# needs to be mentioned as a potentially offsetting factor to your
# one−sided critique.
```