The submission offers novel application of the unscented transform/Kalman smoothing inverse approach to a moderately small scale glacial context and this aspect deserves publication.

However on the science side, I find this paper adds little novel support for the key claims made in the abstract as currently stated: "Our results indicate that Holocene warming coincided with elevated precipitation. ... The importance of precipitation in controlling ice sheet extent during the Holocene underscores the importance of Arctic sea ice loss and 10 changing precipitation patterns on the future stability of the GrIS." Anyone taking an undergraduate course in weather and climate should be able to infer that expansion of upwind open and warmer water conditions is likely to result in proximal increased precipitation.

The latter statement (ie in the quotes above) also adds little new science (though yup, our collective research system incentives such grand and somewhat vacuous statements). Surface mass balance is accumulation minus melt, so apriori changes in accumulation are important. What is at issue is the magnitude of the precipitation changes. And for a future projection, I would put more trust in a direct high resolution current generation regional climate model projection for such inference than the amount in this study which is based on the much lower resolution TRACE CCMS3 model (decade old) result used by Buizert et al, 2018 combined with the questionable eq 4 precipitation dependence on temperature. Furthermore the largest precipitation discrepancies (\Delta P) occur prior to 8.2 ka, when substantial ice over Baffin Island and Labrador may have affected regional atmospheric circulation, calling into question the extent to which this could be a useful analogue for future regional changes.

These science issues can in part be addressed with more precise and careful statements. Eg, perhaps drop the future reference/justification and put more emphasis on the actual precip changes instead of the anomalies from a questions precip parametrization). Your figure 7 (snowfall relative to present-day) is the most interesting to me, and yet gets relatively little attention. It would also be easier to interpret if a corresponding plot for total precip was also provided. This figure raises interesting questions. Eg, what is behind the snow fall high at 11 ka (where it would help to see if this was also a precip high or just an increase in relative fraction of precip that fell as snow)?

This along with some reframing to more emphasis on the testing and description of the inversion methodology would make this a stronger and worthwhile submission in my view.

I would also urge more clarity on the exact audience for which this paper is intended. I suspect there are few within the paleo community with adequate technical background to follow section 2.5, even though it’s a reasonably good exposition for those with such background. So please add a more conceptual overview accessible to a broader community.

Finally, as detailed below, there are a number of model parameter/parametrization choices that are given with no justification.

```
# specific comments:

Looking at figure 6, it appears that the strongest precipitation
correction (\Delta P, prior to 10 ka) could be largely required as an
offset to the default temperature dependence (though a definite
assessment would need provision of actual regional precip and not just
\Delta P.

I'm also troubled by the simplistic precipitation parametrization for
temperature dependence (eq 4).  I can see it being defended as it
matches what continental scale models have tended to use to date. But
the context is different. This is clearly a maritime environment and
precipitation will have strong dependence on upwind surface marine
conditions. Based on my own modelling, I also find that the 0.03:0.11
2 sigma range is too small (at least on the lower end).


"Enhanced precipitation may have stabilized the ice sheet during the
HTM by increasing overall surface mass balance due to elevated winter
snowfall (Thomas et al., 2016), or accelerated retreat due to the
presence of more surface water in summer"
# And yet here you do not take into account the latter effect.  You
# should at least show separate solid/liquid fraction precip time
# series.

"1D, isothermal"
# why isothermal? 1D models are cheap, and given the rheological
# dependence on ice temperature, along with basal sliding dependendence
# on proximity to the pressure melting point, some justification is required
# for going with an isothermal model.

"Basal water pressure is assumed to be a fixed fraction Pfrac = 0.85
of the ice overburden pressure"
# justification?

"5 C kmâM-^HM-^R1 is the lapse rate."
# justification for this value?

"We take p = 0.07, which corresponds to a doubling of precipitation
for every 1 C increase"
# -> 10C increase
# And why this choice? (ie can refer to later sensitivity calibration).

# SMB doesn't take into account impact of rain.

"from 11.6 to 10.3 ka BP, followed by rapid retreat (100 km on both
flowlines) from 10.3 to 8.1"
# what dating methodology has \pm 0.1 kyr uncertainty that far back in
# time? Explicitly indicate uncertainties.

"Evidence suggests the terminus was terrestrial during this period in
spite of changes in sea level and bedrock elevation due to isostatic
uplift. As such, isostatic effects are neglected in the model."
# Please briefly provide a summary of the evidence. And even if
# terrestrial, there could still have been significant changes in bed
# elevation and therefore ice surface slope, which would strongly
# affect ice flow. So I find this decision to ignore isostatic uplift
# problematic.
```

```
pg 7 l 23: " [\Delta p l|^T as a multivariate Gaussian distribution"
# I have no idea what this means. Transpose of some kind of product
# of two vectors?
# farther down I see you mean the concatenated vectors. State so.

"Moreover, unlike in Kalman smoothing, we approximate the full
posterior distribution rather rather than the probability
distributions"
# I understand smoothing to be the joint determination of the full
# history (eg Muller and Storch, 2004) so am confused by this
# statement.


"We randomly sample curves from a multivariate Gaussian with the same
covariance structure as the GMRF prior outlined in Section 2.5.3"
# justification as to why the GMRF provides an appropriate distribution
# for this context?

 eq 26
# what unit in \Delta P in?


"is not a full-fledged substitute for MCMC methods, which can compute
30 expectation integrals to higher levels of accuracy"
# And which do not need to assume a Gaussian distribution


"In an effort to obtain conservative uncertainty estimates, we compare
the unscented transform to a higher order cubature method that is
second order accurate for estimating the covariance (Appendix A)."
# good to see this

"In practice, however, the number of function evaluations
needed for MCMC methods makes them intractable for some problems"
# Not if appropriate emulators are available.

"According to Bintanja and Selten (2014), declining sea ice may cause
an increase in net accumulation over areas of Arctic land ice."
# Again this statement could be made by climate system reasoning,
# without need for recourse to citation. "net accumulation" is also
# unclear. I would take "net accumulation" to mean net surface mass
# balance, in which case this statement has no support.  If you just
# mean total accumulation, then just state so.

Table 1
# please also show the parameter values you obtain from the
# Sensitivity testing inversion (section 3.5), both mode and 2 sigma
# bounds
```