This submission presents the first asynchronously coupled ice sheet and LoveClim transient modelling results for the MIS 7 to 6d (240 to 170 ka) interval. It is therefore plenty novel and relevant for CP. Two of its main conclusions reflect those of another submission currently under CPD review examining last glacial inception (Bahadory et al, cp-2020-1, of which I'm a co-author): that 1) LoveClim appropriately coupled with an ice sheet model can within uncertainties capture major interglacial-glacial-interstadial transitions and 2) replicating such transitions can impose strong constraints on the coupled model. The third conclusion, that orbital forcing has more impact than GHG changes during glacial inception, is not surprising based on associated radiative forcings and chosen fixed forcing states but does not necessarily hold when one considers say the whole last glacial cycle (Tarasov and Peltier, JGR 1997, albeit with a much simpler 2D energy balance climate model and isothermal ice sheet model).

As to the quality and limitations of the scientific methods, aka model configuration and experimental design, the component PSU ice sheet model and LoveClim EMIC are well documented and well used models. They arguably remain near state-of-the-art for transient glacial contexts (though LoveClim is sorely in need of a replacement and ongoing work with transient GCM/ISM models are defining a new state of the art for very small ensembles). However, I do find some limitations that need to be made explicit in the manuscript. As shown in Bahadory et al, GMD 2018 (again from my group and curious why this paper is not cited), accounting for orographic forcing of precipitation in downscaling to the ice sheet grid and accounting for topographic changes in meltwater routing can each have significant impact on modelled ice sheet evolution. To be concrete, inclusion of the latter, for instance can, result in more than 15 m eustatic sealevel equivalent discrepancies within 4 kyr (IBID). The current submission is not even explicitly clear if the modelled surface freshwater routing changes during the glacial cycle (though it appears not to be the case). There are also a host of other sources of model uncertainty that are not mentioned, including: length of model spinup, topographic upscaling from ISM to LOVECLIM, the requirement of much higher ice sheet resolution or subgrid mass-balance accounting to adequately represent restricted glaciation over Alaska, and the dozens of poorly constrained parameters in both models that the modellers have chosen to not vary.

The other hole in the paper for me is pervasive in paleo ice sheet modelling: a very limited exploration of the impact of model uncertainties, given the small number of ensemble parameters and limited ensemble size. This is an exploratory work, and so arguably gets a pass with this limited ensemble, but I encourage the authors to expand their set of ensemble parameters and ensemble size in future work. And the paper needs a bit more attention to discussion of uncertainties that arise from the very limited ensemble size and the potential impact thereof.

The paper structure is logical. The abstract is concise and appropriate. The language is fluent, though there are instances where precision is lacking (eg "reasonably well", cf detailed commments below) as are some important (to me at least..) details about model setup.

Once the issues above and below are addressed, I would see this submission appropriate for TC publication.

Specific comments.

For a range of model parameters, the simulations capture the
reconstructed evolution of global ice volume reasonably well
What does "reasonably well" mean. Be precise

It is demonstrated that glacial inceptions 20 are more sensitive to
orbital variations, whereas terminations from deep glacial conditions
need both orbital and greenhouse gas forcings to work in unison

this likely depends on your choice of fixed orbital configuration
cf Tarasov and Peltier, JGR 1997.

This poses a general challenge for transient coupled climate-ice sheet
modeling.
on the flip side, it poses a strong constraint opportunity, cf
Bahadory et al, cp-2020-1.pdf in TCD

which correspond to about 1.3 mm/year global sea level equivalent
during the build-up phase.
that number is more than a factor too small for last glacial
inception if one goes by the cited LR04 stack

fig 3 captions
again mixing up ensemble with ensemble run. An ensemble is a collection
of model runs.

fig 3
does this show all the model runs in the non-fixed forcing ensemble you
carried out? If so, please make this clear.

including multi-ensemble simulations
do you mean mult-run or did you actually carry out multiple ensembles?
If so, how large was each ensemble?

The effect of CO2 variations with respect to the reference CO2
concentration (365ppm) on the longwave 120 radiation flux is scaled up
by a factor α, to account for the low default sensitivity of ECBilt to
changes in CO2 concentrations (Friedrich and Timmermann,
2020;Timmermann and Friedrich, 2016). α is determined based on
transient past and future simulations.
Please provide the pCO2 ECR for alpha=2 with your setup. This would
let reader better judge how consistent this resultant sensitivity is
compared to that of IPCC grade GCMs. Also, it would be worthwhile
comparing your \alpha to that found based on 1D radiative-convective
modelling (Ramanathan et al, 1979 JGR).

2.2 PSUIM surface mass balance description, eq 1 and 2
on what timestep is this carried out? If longer than 1 hour
(presumably), what accounting is there for diurnal variations?

after eq 4 : with $r = max$J0, $min[1, (T^* + 3)/3]$
Based on my on examination of ice sheet model horizontal basal
temperature between along flow adjacent grid cells (which provides
logical upper bound for the transition range), 3 C is a wide

transition range for warm based sliding. How is this justified?

For the NH, a binary sliding coefficient map ... low sliding over present-day land ($C(x, y)$ = ... representing non-deformable rock).
Much of Southern Canada and Northern USA (regions of glacial ice
cover) is covered by tills, not hard beds and this can significantly
influence ice sheet evolution (eg Tarasov and Peltier, 2004
QSR). How do you justify making all this hard bedded?

Preliminary experiments (not shown) with different acceleration factors suggest that model results do not change significantly when N <= 5.
Please be more precise by what "significantly" means.

Furthermore, for surface temperature T, a lapse-rate correction of 8˚C km^{-1} is applied to account for differences between LOVECLIM orography and PSUIM topography and precipitation is multiplied by a Clausius–Clapeyron factor of 2^.. with ΔT being the temperature lapse-rate correction, to account for the elevation desertification effect (DeConto and Pollard, 2016).
How do you justify using a lapse rate that is inconsistent with
the lapse rate LOVECLIM uses internally? For future work, I would
strongly advise inclusion of orographic forcing given the impact
thereof missed in a coarse grid EMIC (cf eg Bahadory
and Tarasov, GMD 2018)

Basal melting and liquid runoff from PSUIM is discharged via LOVECLIM's runoff masks in both hemispheres;
do these masks account for changing topography? And if so, what
accounting is there for critical subgrid gateways for southern
drainage from the NA North American) ice complex (cf eg Tarasov and
Peltier, QSR 2006).

Increasing the value of m (Eq. (1))
as a reader, it is a pain to flip back 5 pages to find out what m
is, please add a few descriptive words (surface energy offset term
or some such) ditto for \alpha

3.2 Ice sheet evolution
this section would be strengthened with more contact with the
(albeit limited) glacial geological litterature. The key relevant
data are Late Pleistocene glacial limits. Does your model respect
them everywhere? If not, what are the main discrepancies? The only
regions I see that could be at issue are your Alaskan incursion and
Northern Siberia.

the glaciation 235 into MIS 6 is delayed by ~3ky (191ka instead of 194ka).
Do you really believe that temporaly uncertainty in inferred
sealevel is < 3 kyr that far back?

After a relatively stable interglacial state till MIS 7a, the system moves into the next glacial and reaches a glacial equilibrium state.
This description does not accurately reflect your figure 3, I see no
sign of a "glacial equilibrium"

...Batchelor et al. (2019), have suggested a larger Eurasian ice sheet
over the MIS 6 period (160-140ka),
"suggested" does not accurately nor precisely reflect the
inferences. Be more accurate: eg glacial geological record indicates
that the asynchronous maximal MIS 6 ice margins are outside of MIS 2
ice margins.

leading to temperatures low enough (Fig. 6d) to avoid ablation even if
the Laurentide extends equatorward
There is always seasonal ablation on an northern ice sheet. Be more
precise.

Figure 7:
makes it a lot easier for the reader if subplots have descriptive
headings on the plot. Having to visually jump between each subplot and a large
caption disrupts reader assimilation of the plots.

Fig 7 caption two ensembles of
do you mean two ensemble members?

Fig 7f-i
I find the colour scheme has insufficient and distorting colour range. Eg
for 7h the 0.3:0.5 colour is just a shade darker than the -0.3:-0.1 range
colour. Furthermore, it makes no sense that the plot has regions where
these colour border each other without any intermediate ranges showing.

Fig 7:
I am a bit confused why there is such limited glaciation east of the
Canadian Cordillera, given the northwesterly (and therefore relatively colder)
absolute winds and rainfall anomalies that match (within the colour
scheme) other sectors with significant ice cover. Is this due to
the temperature bias correction or limited rainful or ? On that note,
a short discussion on the impact of the bias correction would aid
interpretation of its role in your results.

this behavior is reminiscent of a saddle node bifurcation

We find that small changes in the Laurentide's ice distribution for
similar total ice volumes can lead to a saddle node 400 bifurcation of
the system
which is correct? Have you shown this to be a saddle node bifurcation
or is this reminiscent of a saddle node bifurcation?

Also, the stationary wave feedback reported here 410 could be a model
dependent feature of LOVECLIM, given it has only three atmospheric
levels
and LOVECLIM is run at a relatively coarse T21, while the
litterature indicates that at least T42 is needed to avoid major
resolution sensitivity of the eddy driven jet (eg Lofverstrom and
Liakka, 2018).

Results also suggest that our coupled simulations are realistic over a
narrow range of parameters
what does "realistic" mean? Again, be precise

is more difficult than conducting timeslice experiments
I would say much more difficult and therefore offers much more
self-constraint

Fig S1
summer (JJA for NH and DJF for SH) temperature is much more critical
for ice sheet growth than mean annual temperature, given surface mass-balance
dependencies, so please add these plots.