Review of "Holocene land cover change in North America: continental trends, regional drivers, and implications for vegetation-atmosphere feedbacks"

The manuscript by Dawson et al. presents new gridded reconstructions of land cover changes in North America, combining pollen-based vegetation cover reconstructions and a Bayesian spatial interpolation model. The new reconstructions are a valuable community effort and will be of great use for future studies of large-scale vegetation changes, land-atmosphere feedbacks, and anthropogenic land use during the Holocene. The maps can serve as boundary conditions for climate simulations and for evaluating Earth system model simulations with dynamic vegetation. Especially the high number of collected records covering the early Holocene is an impressive feature. The paper is well-written and my comments are mostly minor.

General comments

- The Bayesian interpolation methodology is sound and has been established over several studies. Nevertheless, it was originally developed for individual time slices (spatial reconstructions) while the new LandCover6k efforts and hopefully further data compilations in the future aim at spatio-temporal reconstructions. While I don't think that any adjustments of the interpolation strategy are needed for this study which does not aim at progressing the statistical interpolation methodology, I would appreciate discussing not just limitations of REVEALS but also of the interpolation methodology in Sect. 4.3. Moving from time slice to spatio-temporal reconstructions offers new statistical and data science challenges and opportunities which would be worthwhile discussing. In particular, can you comment on the potential for handling age uncertainties in the reconstruction algorithm, how uncertainties from REVEALS are propagated to the interpolation algorithm, using an actual spatio-temporal interpolation algorithm instead of reconstructing a set of time slices (see my comment below), and testing the impact of the non-uniform distribution of site locations through, e.g., bootstrapping. Do you think that cross-validation experiments in which some portion of the REVEALS reconstructions is left out from the interpolation could be a way to evaluate the spatial (or spatio-temporal) reconstructions?

- The GMRF method provides reconstruction uncertainties for all grid boxes. However, so far, the uncertainties are only visualized for the continental and regional mean curves. To understand the statistical significance of the spatial land cover variations, it would be very helpful to also plot maps of the reconstruction uncertainties, for example together with Fig. 2. For the change maps (Fig. 4a, 5a, 6a, 7a), hatching areas with statistically significant changes would be valuable to assess the importance of the temporal changes.

- There are three other aspects related to the applications and improvement of the datasets that I kindly ask the authors to discuss or enhance the respective discussion.
  The authors mention the under-representation of arid regions due to a lack of pollen records. Do you see prospects for including other proxies, either for vegetation or for (hydro-)climate to improve the reconstruction for arid regions (e.g., biomarkers, isotopes from speleothems, or lake levels)?
  Currently, only three land cover types are separated. Is there the potential in terms of data availability to also separate boreal, temperate, and subtropical forest / grassland types in addition to evergreen, summergreen, and open land?
  Finally, the current separation into time slices of 1kyr (and potentially further smoothing from

age uncertainties) precludes the analysis of sub-millennial trends. Do you see the possibility to also identify multi-decadal to multi-centennial variations on the regional and continental scale with the existing data coverage, potentially using an improved spatio-temporal interpolation methodology?

- The authors discuss implications for biophysical atmosphere-vegetation feedbacks very well (e.g., l.49-53, Sect. 4). In this context, it would be suitable in my opinion to also mention biogeochemical feedback mechanisms as the new land cover reconstructions should also be a useful tool for studying these ones, in particular since more and more Earth system models having capabilities for prognostic carbon and nutrient cycles.

- Data availability: The posterior mean reconstructions have been made available as csv files through a github repository. In the interest of maximing reusability and making it easy to cite the dataset, it would be very valuable to (i) publish the data sets also in a FAIR repository with a permanent identifier, and (ii) publish the reconstructions as netCDF files which are more suitable for gridded data and allow for a better interoperability with climate and vegetation simulations. Additionally, I would recommend to make not just the posterior means but also the uncertainties available in a suitable data format, either as marginal (point-wise) uncertainties or, better, by publishing MCMC samples which allow quantification of spatially correlated uncertainties.

Specific comments:

- Regarding all maps in the manuscript, please consider using a different projection that displays the size of regions better since in the current projection the high latitudes are heavily overrepresented compared to, e.g., Mexico.

- l. 33: I kindly ask you to use the term "Holocene temperature conundrum" instead of just "Holocene conundrum" given the number of other conundrums that have appeared in the literature over the last decade(s).

- l. 57: Would it be suitable to mention not just land cover dynamics in the last sentence of the abstract but also Holocene climate dynamics?

- l. 63: Should it be "net-negative" instead of "negative-net"?

- l. 114-117: Is there a specific reason why there was no prior continental-scale reconstruction for North America?

- l. 163: The authors use Bchron for the age modeling which is an appropriate choice in my opinion. I'm just curious if there is a specific reason to not use BACON which seems to be used more often in recent studies? While both model would be justifiable choices from my outsider view, it could be of interest to the community if the authors see specific advantages of Bchron for the vegetation reconstructions at hand.

- l. 168-169: Given that the taxa selection seems very important for the vegetation cover reconstructions, can you provide some more information on the selection criteria and the representativity of these taxa for the vegetation at the different locations? In addition, I would strongly suggest to move Supplementary Table 2 to the main manuscript (potentially with some additional information on the importance of the taxa like the average pollen percentage of those taxa).

- l. 192-207: Can you provide the number of lakes used in the different workflow steps?

- l. 218: Can you provide some support, e.g., in the supplement, for the very strong statement that "these vegetation reconstructions indisputably overrepresented larch"?

- l. 226: Please remove the gray shading in Trondman et al. (2015).

- l. 260-261: I don't understand the rationale behind the "mean relative cover". Excluding ice covered areas and areas without reliable reconstructions is reasonable, but why would you not use the area weighted mean of all grid boxes with reconstructed vegetation instead of just the average over those grid boxes?

- Fig. 1: To understand the spatio-temporal coverage properties better, can you provide maps similar to Fig. 1a for the individual time slices in the supplement?

- l. 332: I struggle to connect the mentioned increase from 56% to 91% with the results presented in Fig. 2 where the increase looks much smaller. Can you please clarify where these numbers come from?

- Fig. 4c: Spruce and pine are prominently mentioned in the text (l. 374-378) but are not included in the figure with the coverages of important taxa. Is there a reason for this exclusion?

- Sect. 4.1 and 4.2.1 are fairly long considering that they are mostly a literature review while being relative unconnected to results from the new reconstructions. Therefore, I'd ask you to either state more explicitly how the new results are in agreement with / contradicting previous studies or consider shortening this part given that the paper is already rather long. If the goal is mainly to state potential applications of the new reconstructions, I don't think this long discussion of the previous literature is needed.

- Sect. 4.1.1 - 4.1.3 should be 4.2.1 - 4.2.3.

- l. 784: It is stated that the GMRF creates a spatio-temporally complete vegetation reconstruction. Is this an appropriate characterization? From my understanding of the methods section and previous studies using the GMRF method, it creates spatially complete reconstructions for a set of predefined time slices but without considering temporal dependences between the time slices. If this is an misunderstanding on my site, it would be helpful to state more explicitly in the methods section how the time dimension is handled in the GMRF since the referenced studies only apply it for spatial reconstructions.

- l. 843-845: Maybe consider simplifying or splitting this sentence.

- l. 861: Is data assimilation the appropriate word here? From my understanding, the reconstructions would either be used as boundary conditions in simulations or for comparison with dynamically simulated vegetation, whereas data assimilation would refer to a dynamic simulation in which the simulated values would be relaxed towards the reconstructions. The latter is something that hasn't be done so far with vegetation reconstructions as far as I know.

- In many instances (e.g., l. 129, 196, 214, 216, 252), the parentheses in citations are set inconsistently. I kindly ask you to check the citations throughout the paper for typesetting errors.

- I kindly ask you to check the reference list again and harmonize the used reference style, in particular by providing DOIs consistently wherever available. Additionally, Dawson et al. 2019a and Dawson et al. 2019b seem to be the same reference, and Githumbi et al. 2022b and Githumbi et al. 2022c also seem to be the same reference.