Besnard et al. present a new global forest stand age product which uses a random forest trained on forest inventory data to predict forest stand age based on a gridded global biomass product and climatological variables. Forest stand age distributions have emerged as an important determinant of estimates of forest carbon uptake, with the legacy of past disturbances, particularly anthropogenic, often contributing to a large carbon sink (Caspersen et al., 2000; Kondo et al., 2018; Pugh et al., 2019; Shevliakova et al., 2009). Whilst they only capture aspects of structure related to stand-replacing disturbances and not smaller-scale changes in structure, they are a major step forward compared to neglecting the legacy effects of past disturbance completely. To my knowledge, there has, until now, only been one stand age product available with global coverage (Poulter et al., 2019). Alternative products using complimentary methods, which can help identify and address uncertainties in the existing knowledge of global forest age structure are therefore very much needed. The manuscript is clearly written and structured. The generated maps seem to show reasonable skill, at least for middle-aged (70-130 year old) forests. However, I have several questions around the methods and concerns around the interpretation which are detailed below.

Major comments

The method has similarities to the biomass age curve approach (Chazdon et al., 2016; Zhang et al., 2017), but instead of using observed age curves, the random forest regression effectively uses climatological data to implicitly predict stand biomass growth curves, combining these with the biomass information to produce age. I.e. it assumes that growth and (non-stand-replacing) mortality rates can be estimated well by climatological variables alone. Given the importance of edaphic variables and management for determining these rates, I was a bit surprised not to see them included in the set of predictors. Whilst management is a tricky thing to assess globally, there are at least roughly-related variables, e.g. human development index, which could have been tested. Possibly there is substantial autocorrelation of management with climate, which may help out here in terms of the accuracy of the classification, but then this would also potentially confound the current interpretation of climatic variables relating to Fig. 5. At the very least it would be good to see some discussion of the potential influence of management, if not testing the importance of some management and soil-related variables.

As things stand, I think that the interpretation of the influence of climate variables on the age estimates (Fig. 5) has to be made in the context of these variables effectively driving an implicit stand growth model which converts the biomass data into age. The biomass product is the only predictor which contains information about the current state of the forest including the cumulative effect of its full land-use, management and disturbance history. I.e. the climatic variables are not explaining age, they are (or at least likely are) explaining how biomass relates to age. A direct interpretation of climate effects on age would in any case be flawed because of management not being considered. I think that assessing the drivers of age distributions themselves (as opposed to trying to make the best map) would be clearer using a model which only included drivers and not also the state (i.e. biomass).

The masking out of low tree cover is a nice technique to reduce the negative bias on pixel biomass, and thus stand age, caused by a mix of forest and non-forest landcovers within a 1 km^2 pixel. However, although the size of a stand is a loosely defined concept, it is generally much smaller than 1 km^2. This leaves me wondering why the authors aggregate the biomass dataset from 100 m to 1 km? Is this to reduce noise? 100 x 100 m is already much bigger than most of the inventory plots which will underlie this product, but it would make the scale mismatch between plot-level training data and the biomass product used for extrapolation much less acute. Is a reduction in noise worth the loss of this important small-scale variation? An assumption of homogeneity within a 1 km^2 cell (even after masking for tree cover) would tend to reduce the extremes of low and high biomass which would have been seen in the plot-level training data. This might go some way to explaining the relative dearth of young stands in the global age product (Figs. 9 and 10).

Do all training datasets resolve up to 300 years of age (Line 100)? How did you deal with this if not? Are all the methods used to determine age likely to be accurate going back this far? Given the biases in prediction at greater than ca. 130 years (Fig., 3), and that the age of old growth forest is both perspective and biome dependent, perhaps the classification between old-growth and non-old-growth would have been more accurate if a younger age threshold was taken? Whether or not you want to test this is of course up to you (perhaps you did already?), but at least a bit more clarity and discussion would be good.

I'm struggling with the interpretation around Fig. 7, where my reading of the graphs doesn't find the same features.

- L312. My reading of this graph is that in warm regions the whole range of forest ages is found, whereas in cooler regions, only a relatively narrow range of ages exists. This itself is pretty weird, as the plot seems to suggest that cold (presumably boreal) forests only have ages of around 100 years, with no younger stands. This doesn't seem very plausible.
- L320. It seems very strange that the youngest stands should only be found in the tropics (i.e. regions of high temperature, Fig. 7c). How can you explain this result?
- L323. I don't see any association between age and water availability in Fig. 7d. The points basically seem to form a square, apart from a small indent in the upper left, which seems challenging to interpret causally.

The comparison against other age products is very nice, however, not all comparator datasets are created equally. For instance, Pan et al. (2011) is based on a spatially systematic inventory system (at least in the US). Similarly, much of the temperate and boreal data in GFAD is based on summaries of statistics from national forest inventories. Whilst these come with substantial uncertainties, I would argue that they provide a sterner evaluation of the MPI-BGC product than the comparison with products based on biomass age curves. I suggest that this is reflected in the discussion of these results and also that the comparison with GFAD provides separate histograms for regions where GFAD is based on inventories and those where it is based on the biomass-age approach.

Other comments

Line 59. "Yet, an intrinsic..." It's unclear (at least to me) what this sentence is saying. Please can you rephrase it?

L125. Typo, "was covered in"

L230. I don't think this result necessarily implies that vapr is a strong determinant of forest age. It can also imply that vapr is a strong control of how AGB relates to age. Given that high vapr is likely to limit both growth rates and maximum biomass, this seems to me the most likely interpretation. An influence of vapr on fire frequency, as suggested in the text, would surely primarily act through the effect of fire on AGB.

Fig. 3d. Are the under and overestimations distributed evenly geographically, or concentrated in specific regions? Providing some maps which break down the bias would be very useful for interpreting the results of the upscaled map.

L288. Ceccherini et al. mainly show increases in Sweden and Finland. These increases are disputed (Palahi et al., 2021; Wernick et al., 2021) but also only occur after 2016 and therefore are not relevant to this map dated 2010.

L290. See also (Vilén et al., 2012) for data on European age class distributions.

L292. I agree that different fire regimes seem plausible (Shorohova et al., 2011), but there is also substantial harvest identified in southern Siberia (Curtis et al., 2018), which should also play into this discussion.

Fig. 4 caption. Clarification. "... in predicting forest age estimates in the regression model."

L367-368. This sentence is really hard to follow, please can you rephrase it? I think what is being presented is the fraction of the random forest ensemble which predicts an old-growth forest for pixels which the mean of the ensemble attributes to old growth, but after reading it several times I'm not 100% sure.

L392. Are you able to speculate a bit more on the methodological differences that drive the differences of the new forest age product with Pan et al.?

Figure 9. I don't follow why the age map without using a tree cover threshold needs to be applied for consistency. Surely the tree cover threshold deemed to be most appropriate should be used for comparison. Or, perhaps better, all tree cover thresholds should be compared and the one which provides the best agreement with the independent, inventory-based, datasets (at least for regions where the inventory is systematic) might be recommended? It's really helpful that you explore this uncertainty due to tree cover and make all the maps available (thanks!), but many users will need to make a choice on a single map to use and it would be helpful to have a best recommendation to support this.

Figures 6 and 9. Is this mean or median age per pixel? If mean, how did you deal with the old-growth age class having an infinite upper age bound?

Figure 10. The integrated forest area does not appear to be the same for GFAD and MPI-BGC products. Differences also appear to be shown in regions that have no forest (high northern latitudes, interior Australia). Please could you check? Also, which tree-cover correction was used in this comparison?

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