How to simulate forest management?

Mail: connect CMIP7 Julia Pontgratz, Anja Ramming, Ben Smith, Thomas Pugh, Dave Lawrence, Rosie Fischer, … (Sebastiaan)
Connect ESM2025: Meeting in August (Nicolas Vuichard).

IPCC recognizes the need to account for forest management. The standard approach used by all/most model groups including IPSL-CM is to prescribe the biomass removals through maps (based on the model of Hurtt). ORCHIDEE has, since the work of Valentin, a different approach based on different management strategies. The question that emerged is which approach do we want/need for the CMIP7 in 7 to 10 years from now?

A. **For CMIP7, IPSL-CM will follow what the rest of the models are doing. Therefore, the default of the ORCHIDEE model will be based on the Hurtt maps. We drop our own approach based on management strategies.**

Not really a viable option because we have three projects funded that rely on the strategy-based approach.

B. **For CMIP7, IPSL-CM will follow what the rest of the models are doing. Therefore, the default of the ORCHIDEE model will be based on the Hurtt maps but we maintain both approaches.**

This is a viable compromise but it means maintaining two approaches. We expect that the strategy-based approach will be better developed and evaluated than the default (which is a bit in conflict with the definition of a default).

C. **For CMIP7, IPSL-CM will use its own approach. The default will be strategy-based but we will add the Hurtt approach to the trunk as a way to compare both approaches.**

This is a viable compromise but it means maintaining two approaches. We probably need this setup if we ever want to test whether the strategy-based approach is not simply more realistic but also generates different results (which I'm not sure of).

D. **For CMIP7, IPSL-CM will use its own approach. The default will be strategy-based.**

This would save a lot of work but requires good faith in the strategy-based approach because we cannot easily compare it to any other approach.

Rethinking the spinup procedure

As the structure of and functionality of ORC4.0 has changed compared to previous versions, the spinup procedure requires some attention. This far, two issues stand out: (a) stand structure now evolves over time, this might result in very long time periods needed to reach equilibrium and in synchronicity between neighbouring pixels, and (b) ORC4.3 will contain disturbances that depend on the climate drivers, cycling over just 10 years of forcing might result in unwanted transient behaviour once the configuration no longer cycles through the forcing.

A. Age classes

1. What do we have in the code ?
The current documentation gives a truthful description of the mortality process:
https://forge.ipsl.jussieu.fr/orchidee/wiki/Documentation/TrunkFunctionality4#Mortality

- The residence time, which is responsible for biomass saturation with age in ORCH2.2, now only complements self-thinning. If self-thinning becomes too low in old-aged forests, then background mortality determined by the residence time will take over. But apparently, its current setting in ORC4.0 fails to effectively bring the biomass to a stable state in old-aged forests. The name of the parameter is “residence_time” and it is still used in stomate_mark_kill.f90 but we have set it to 500 years. That might be the reason why it looks like it is not working.
- Note that even though background mortality successfully makes forest stands reach a stable biomass at the old-growth stage, we still lack mechanisms to represent stand-replacing disturbance events in nature which drive stand age dynamics. Stand-replacing events on average reduce forest stand age and biomass. Lacking such a mechanism will cause overestimation of biomass, which explains why we need the next point.
- Instead, ORCHIDEE trunk uses a threshold of low density as a criterion to initiate a stand replacing disturbance for unmanaged forests. The trunk uses minimum density thresholds and the residence time to determine mortality (note that residence time results in constant mortality, not in abrupt mortality). For most PFT, the density threshold should be used well before the residence time criterion (as the residence time in ORC4.0 was only kept as a fail-safe option).

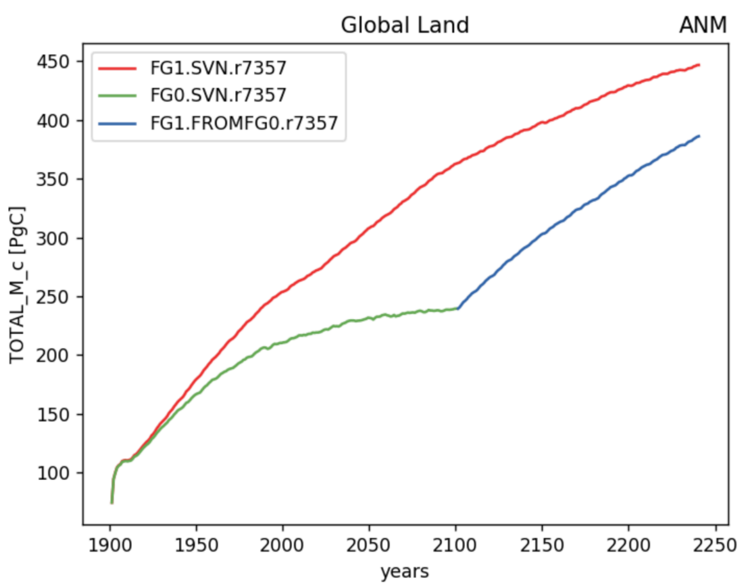
2. What is the problem?
There are two (related) problems: (1) biomass saturates too late with age, requiring very long spinup times (>340 years but possible ~700 years) leading to too high biomass for a forest stand, and (2) very different stand longevity between FG0 and FG1 resulting in a huge increase in biomass when switching from FG0 to FG1.

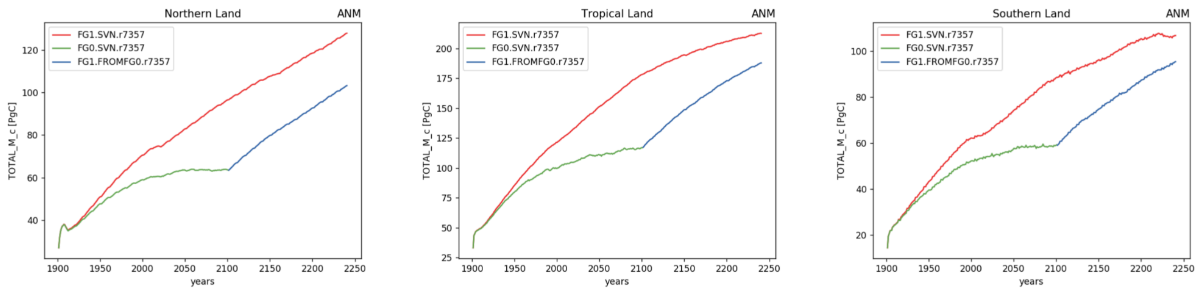
3. A more detailed assessment of the problem

For a single forest stand that it's never hit by a stand-replacing event, the biomass will saturate with age because gap-scale mortality will happen followed by recruitment of small trees. This is the default for ORC4.0 to simulate unmanaged tropical forest PFTs (i.e., mimicking gap mortality). The current scheme kills big trees and the subsequent recruitment dilutes the size (not the biomass) of remaining big trees a bit. We would like to make such saturation happen for a time span (forest age) within a reasonable length of the spinup.

In temperate and boreal regions, the saturation time needed to reach stable biomass is expected to be longer than in tropical forests in the real world if they are lucky to escape stand-replacing disturbances. In ORC4.0 all unmanaged temperate and boreal forests are subjected to stand-replacing disturbances which prevent them from reaching the age needed to stabilize their biomass (also note that ORC4 does not apply recruitment in temperate and boreal forest, biomass would follow self-thinning and would never saturate anyway). We are mimicking the stand-replacing disturbance in nature by prescribing a clearcut when the stand density drops below the threshold.

Preindustrial terrestrial biomass carbon stock should be close to today's carbon stock because fossil fuel emissions for 1850-2014 (395 Pg C according to GCP 2020) are almost completely absorbed by the ocean and the atmosphere (145+235 = 380 PgC), with further assumptions that no change occurred in soil C. This means preindustrial global biomass C should be around 450-650 PgC. From the figure below, given long enough time, both the red or blue curve will surpass the 450 Pg C and possibly but not certainly the 650 Pg C.





This supports the following observations:
(1) Biomass of tropical tree PFTs is not saturating within 340 years by the mechanism of mortality and recruitment (Tropical land - red line).
(2) It will probably take over 340 years to reach a stable global biomass around 450 Pg C (Global land - red line) because FG1 likely overestimates the biomass in the Northern and Southern land. Even then the global biomass would just reach 450 Pg C.
(3) In temperate and boreal regions, stand longevity (Rstand) is too long (Northern land -red line).
(4) When using a max stand longevity of 200 years in a 200 year long spinup (FG0), biomass saturates below half (200 Pg C, Global land - green curve) its target value (450 Pg C).
(5) Synchronicity is not at the core of the problem. Suppose all forests have a perfectly randomized age. As long as the Rstand is long, given a long enough time, total biomass will surely reach a very high level. In fact, we used FG0 to create a landscape with certain heterogeneity in forest age, and the high growth in the blue line (following FG1) showing the same track as the red line shows the validity of this idea. We might have a reasonable total biomass if we cut the line somewhere and stop the spinup, but the then unrealized growth will persist in our transient simulation and inflate the transient NBP (which violates the whole idea behind the spinup).

4. Possible solutions and none solutions

Based on the evidence and reasoning above, the solution should contain the following elements:

(1) Shorten Rstand of temperate and boreal forest for the time being. Once we have a mechanistic representation of stand-replacing disturbances in ORCHIDEE, this should replace this prescribed threshold.
(2) Lengthen the spinup time to have tropical forests reach equilibrium biomass or make tropical forests grow faster such that the maximum biomass is reached in a shorter time span.
(3) Stand mortality should be identical (meaning governed by the same processes but not necessarily fixed - changing mortality could be part of the transient run) and persist through all simulations, irrespective of spinup, or transient run.
(4) FG0 to break synchronicity to avoid oscillations when biomass starts to saturate and to mimic the randomness in spatio-temporal distribution of disturbances.

4.1 Reparameterizing residence time (no solution)

Note that reducing the residence time will not solve our problem. The fact that our stand-level return interval for disturbance is too long will not be compensated. If we reduce the residence time to the extent that background mortality replaces self-thinning the model would reach the minimum density (see below) faster but we would go back to a constant mortality and give up on density dependent-mortality (=self-thinning) entirely. The forest would still be killed at the minimum density and not at the residence_time. Only if by coincidence the minimum density would occur at the residence_time, the forest would get killed at the residence_time. If not, killing would be sooner or later than the residence time depending on when the minimum density is reached.

4.2 Reparameterizing minimum density (solution)

Minimum density is already PFT-dependent but can only hold one value per PFT irrespective of the forest management type. The issue is that in reality the minimum density of managed (even aged) forest is low compared to the density of unmanaged forest. Either the value works well for managed forest or it works well for unmanaged forests. As the current value does not work very well for unmanaged forests, we can only hope it works better for managed forests. One solution could be to separate the minimum densities for managed and unmanaged forests. That way mortality would remain density-dependent, would remain a function of growth (rather than age), and could differ for different management strategies.

This approach would imply that we first need to run an FG1 to estimate the longevity of each PFT and that we can then use this longevity in FG0 to break the synchronicity the next time we do a spinup. The density-dependent mortality depends on growth which in turn depends on many other parameters. Every time we reparametrize the model we would have to reassess the PFT-dependent longevity used in FG0. Feasible but far from optimal.

4.3 Speeding up biomass saturation in the tropics

Biomass saturation as the net result of mortality and recruitment should happen within a time span (forest age) that is less than the length of the spinup. This cannot simply be controlled by a single parameter.

4.3.1 Make the trees grow faster (solution)

Growth of tropical trees is expected to increase, once the parameter optimization has found a new parameter set that enables faster growth (and less N-limitation) of tropical forests. This should help to reduce the time needed to reach biomass saturation but it is not clear how much faster it would be reached. Once GPP and growth are satisfying (compared to, for example, biomass chronologies), there are no parameters left that could be tuned.

4.3.2 Make recruitment more abundant (no solution)

Although the following reasoning needs to be confirmed, it is expected that more recruitment would speed up the time needed to reach saturation but would also reduce the biomass level at saturation. The reasoning is as follows. In the absence of recruitment, self-thinning continues until very high biomasses are reached for very low densities. By adding recruits, a stable stand density will be reached at a higher density and thus a lower biomass, adding more recruits would help to reach saturated biomass faster but because the stand density will be higher, the actual biomass will be lower. This is probably not what we want right now as we will need high tropical biomass to reach a global biomass of 450 Pg C.

4.4 Adding a fixed stand age (solution)
This is by no means the easiest solution. Unmanaged stand would then be hit by a stand-replacing disturbance if the minimum density or the maximum age is reached. This solution would work for the current set-up of the temperate and boreal forests but would be from a conceptual point of view in conflict with the approach for the tropical forest. Having a fixed age would make the transition from FG0 to FG1 straightforward.

4.5 Breaking synchronicity
Given our thinking on tropical PFTs, we don't need FG0 for the tropics. The use of FG0 would thus be limited to the boreal and temperate zone. If we limit FG0 to 100 years we will already break a lot of the synchronicity but we would still have some transient behaviour between FG0 and FG1. This looks ugly but it is not a problem. In the end we don't care what happens during the spinup as long as equilibrium is reached towards its end. Combining FG0 and FG1 will probably require a longer spinup compared to only using FG1 (see figures above). Is the extra time worth the relatively small improvements FG0 brings to the synchronicity (apparent in albedo but not too many other variables).

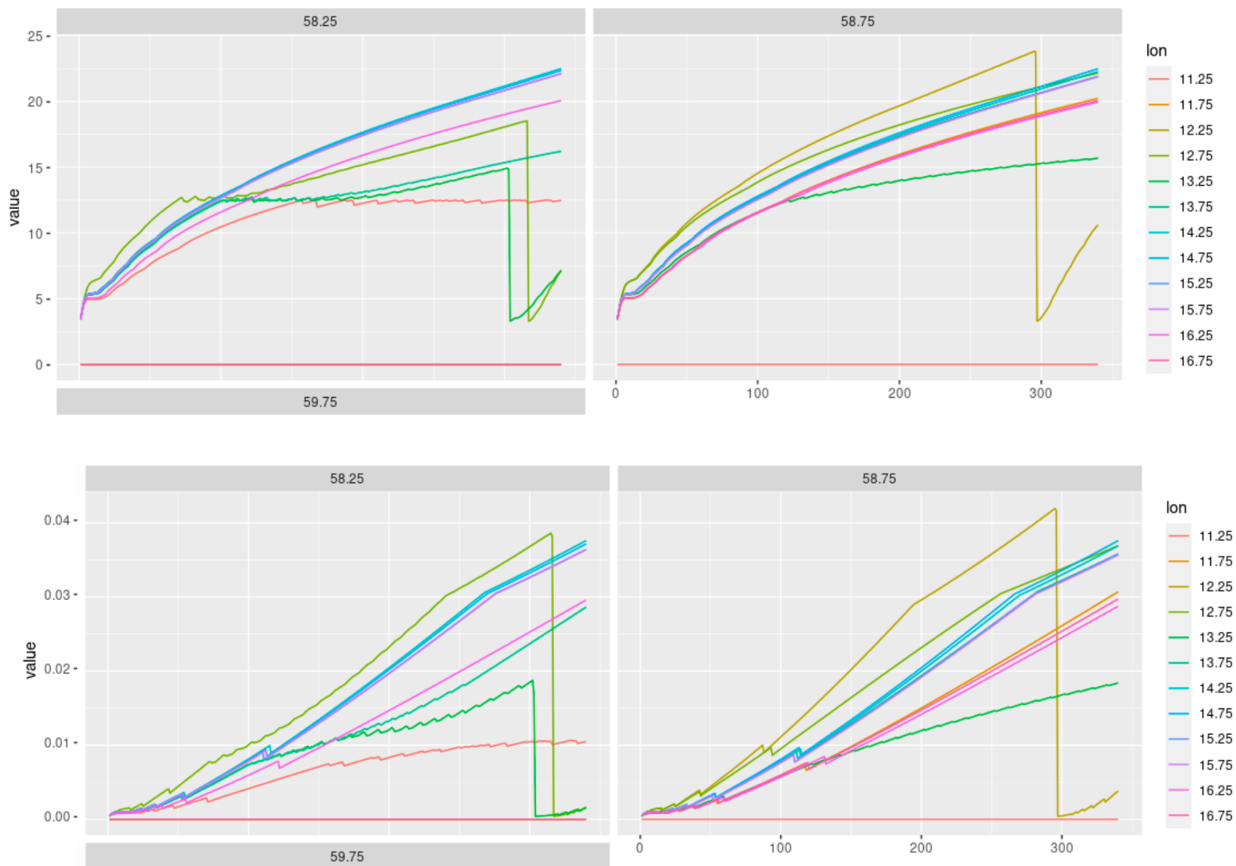
5. Further refinements of FG0
Check after we decide whether we will continue with FG0.

B. Disturbances and cyclic climate forcing

1. What do we have in the code ?
As for ORC4.0 and 4.1 we only have forest management. Abrupt mortality from forest management is driven by a density threshold which does not depend on the climate. The issue is to be expected from ORC4.3 onwards. This version will contain: (a) mortality from wind storms, which depends on the wind speed data in the climate forcing; (b) mortality from droughts, which depends on temperature and precipitation data in the climate forcing; (c) mortality from bark beetles, which depends on temperature in the climate forcing; and (d) mortality from fires, which depends on temperature and precipitation data in the climate forcing.

2. What is the problem?
The current spinup procedure cycles over 10 years of climate forcing (1901-1910), for pixels where these 10 years contain an extreme event, the extreme event will reoccur every 10 years. In regions where the disturbance frequency is lower, the spinup procedure will result in a too low biomass. Following the spinup and the transient simulations (which also cycle over the same 10 years of climate forcing), the disturbance frequency will suddenly change, enabling regrowth. In other words, following the year 1910 part of the NBP in FG2 will be due to the shift from the too frequent disturbances in the spinup procedure to the realistic disturbance frequencies in FG2.

3. A more detailed assessment of the problem



Dominant height (above) and Wood volume (below) from FG2 spinup for 340 years (PFT7). Dominant height (PFT7) from FG2 spinup for 340 years. Titles show latitudes and the longitudes are marked in the legend.

In the pixel lat: 58.25, lon: 11.25, the stand could not grow more than 10m-height due to continuous mortality from the wind.

4. Possible solutions and none solutions

4.1 Creating climate forcing for spinup (solution)

The climate forcing from 1901 to 2015 could be detrended and their anomalies could be used to create a 115 year long time series spinup the model. Given that many disturbances are thought to have a frequency of 100 years or less, this is probably the best that can be done for now. This approach would require double the storage for the climate forcing files. If it works, storing the 6-hourly mean of a single year and its std would be enough to let ORCHIDEE generate its own climate forcing on the fly (this could even speed up the spinup as no forcing files will have to be read.

4.2 Decrease damage during simulations using a cyclic climate forcing (no solution)

We could add a flag in the code that reduces the damage from disturbances during simulations using a cyclic climate forcing. Although this is easy to code, the reduction of the damage would be arbitrary. Going from FG1trans to FG2 would still involve a shift in the disturbance regime which would be reflected in the NBP. Avoiding a change in the disturbance regime would require running FG2, calculating the disturbance regime and then using it as a driver in FG1 and FG1trans. This would also imply that this procedure needs to be repeated every time the model is reparameterized.

4.3 Spinup without disturbances (no solution)
As done in Chen et al 2018, FG1 and FG1trans could be configured without the abrupt disturbances (although if we could use the abrupt disturbances we would no longer need FG0 - see above). At the start of FG2 we would have too high biomasses which would be slowly disturbed as FG2 progresses. This would also result in an unwanted transient NBP.

4.4 Use a climatology
A mean single year climatology could be calculated for the period 1901 to 1910. This year could then be used in the model spinup. This would simplify the spinup and would be very cheap in terms of storage but there is a concern that the lack of inter annual variability would also result in substantial changes in the biomass (and other pools) when continuing with FG1trans and FG2. One possible solution to the problem of interannual variability would be to combine the climatology along with the statistical approach. But I'm not completely sure how this would be different from 4.1, in that case. I was imagining taking the climatology files and the distributions, and then creating ten years of forcing data by adding anomalies drawn from the distributions at each timestep. This would result in a set of 10-year files which likely have one ten-year anomaly for every pixel, but where only one pixel in ten has a 100 year anomaly. But that still means that 10% of pixels may have a problem. Probably better to constrain the distributions to only sample from 10-year anomalies, which may be what Chao was suggesting. It would result in needing to create and store 10 years of forcing data for every climatological dataset we are interested in running.

C. Parameters and forest management
Some of the observed problems (slow growth) are partly mitigated by using forest management. With forest management density decreases faster (than without forest management) which may help to resolve some of the issues mentioned under A. It is to be expected that individual trees in managed forest grow faster than in unmanaged forests (that is the whole purpose of FM, i.e., to accumulate the biomass in fewer trees). Because of the above it is difficult to predict whether the total biomass should be higher or lower in managed forests compared to unmanaged forests. Irrespective of the expected differences between managed and unmanaged forests, the dominant tree height (and thus also diameter) in unmanaged forests seems too low with the current parameters. Also note that we are basing this judgement on unmanaged forest prior to 1750 and present day observations of managed systems. Not the most solid comparison.

D. Within pixel synchronicity

For large pixels (e.g., 2 x 2 degrees) dominated by a single forest PFT, the whole pixel will be replanted at a single step following a mortality event. When running the model with age classes, this issue could be mitigated (if we have more than 1 age class present in that pixel). For smaller pixels, the issue would still exist but is expected to be less impactful on the climate.

It needs to be further discussed whether this would result in (new) difficulties for evaluations against observations. One way of looking at this is that the mortality events in ORCHIDEE 4.0 are too large and will poorly compare to observations. ORCHIDEE 2.2 has no mortality events and should (if not overtuned) poorly compared to observations which have many mortality events (i.e., all managed forests). There is little doubt that large-scale simulations will not match small-scale observation but the target should probably be to compare large-scale simulations with large-scale observations. In such a comparison spatial compensation could occur. For example, in ORCHIDEE we might have 1 pixel that dies entirely but 15 pixels that grow. On average these 16 pixels are an OK representation of reality. In ORCHIDEE 2.2 all pixels would die 1/16th. The abrupt mortality in ORCHIDEE 4.0 would give us a change in albedo, roughness, etc. The continuous mortality in ORCHIDEE 2.2 would hardly (or not at all) propagate into the surface variable.

Summary of solutions after discussion between Sebastiaan and Chao (20/12/2021)

For temperate and boreal:

- Define properly Rstand.
    - Use minimum density. But still better to align some age because age is more widely used and observed.
    - Then We need the relationship between age~density. We can run FG1 to get this for different grid cells. We can average over grid cells (with vegetmax higher than a certain value) for the same PFT, derive an average density~age curve, select the density threshold for each PFT.
    - Such FG1 should be driven by preindustrial climate and CO_2. Because the density~age relationship will change under transient conditions, we will have the transient effects on Rstand.
    - We will use different parameters for minimum density for unmanaged and managed forests.
    - We might want to further refine for different regions for the same PFT (e.g., PFT7, northern europe, central Siberia, North America). This could be done by externalization of PFTs in the future.
- Break synchronicity: to break synchronicity is definitely needed. However, whether we need a FG0 for this purpose will depend on how long it takes for tropical forest to reach stable biomass in spinup. For instance, if tropical forest need 600 years for spinup, during this time length we expect temperate and boreal forests will already undergo stand-replacing events (for a few times maybe), then FG0 is no longer needed. If it takes 300 years for tropical forest to reach stable, then we might need FG0 for temperate and boreal forests.

For tropical forest:

- Hopes are for first tuning productivity given the low LAI in high-rainfall regions. We should have a reasonable relationship between LAI~age, so that the productivity side is constrained.
- Tune recruitment to reach reasonable relationships between Biomass~age, so that turnover is constrained. Valid Biomass~age should reflect sensible self-thinning (backuped by background mortality) during early succession, and sensible recruitment at old-growth stage forest.
- Stand-replacing disturbances seem not justified but it will be used as a last resort if the above things fail.

We note here parameter tuning and validation start to mix. Further refinements on validation will follow, including their presence in the trunk4.1 GMD manuscript.

Given the considerations above, priorities are:

- Get good parameters for growth (tropical forest and PFT6), linked to current ongoing optimization led by Vlad.
- Make minimum density FM-dependent.
- Check biomass accumulation (tropical forest)

- Check synchronicity.

7. Known problems

The concept of a minimum density to trigger a stand-replacing event is not justified in the case of, for example, fires.

8. Minutes of a related meeting on PFT2 and PFT3 in March 2021

Time: 2021/03/15 09:00 CET
Participants: Stephen, Sebastiaan, Guillaume, Chao
PFT2 = tropical evergreen broadleaf forest
PFT3 = tropical raingreen broadleaf forest

1. Summary of the results:

• Total biomass per tree is the highest for Class3 but total biomass per area basis is the highest for Class2 because of the higher density in Class2. Guillaume suggests this is because merging of Class1 with other classes increases density in Class2.

• In general no suspicious behaviour is found. Maybe pixels of P1/P3/P5 just need more time to reach stability. This is considered as an ecological sound. But investigating why P0/P2/P4 reaches stable state (they are quite close to P1/P3/P5) could provide some hints. This leads the needs to:

◦ Check whether differences in soil properties (e.g., N pool) lead to differences in “paired” pixels. This could be intriguing but the results might not be useful for our desire to have an equilibrium state with a reasonable time of simulation.

◦ Check the veget_max (i.e., ground fractions of the PFT in question) for these pixels. There is no consistent linkage between veget_max and whether the pixel can reach stable or not, which I think is good news.

◦ To test these hypotheses, we could run with impose_cn=y, and (impose_veg=y plus setting a 100% forest coverage) and re-run the simulation.

◦ To check the differences in recruitment and mortality for Y300 to Y500 for pixel P0/P2/P4, to understand why they reach stability.

2. Practical approaches:

• To introduce some “persistence disturbance agent”, e.g., clearing 1/400 forest area per year, to have a heterogeneous forest age distribution. But the problem of this approach is that this agent has to be maintained in any transient simulation, which brings extra complexity to the simulation.

• To tune the parameter of `st_dist`, which prescribes relative fractions of the number of trees in each diameter class when the self-thinning rule predicts a certain number of trees dying. But there are a few problems:

◦ The parameter of this value is now universal. But we don’t have any problem in temperate and boreal forest PFTs. => The need to externalize this parameter.

◦ For tropical forest PFTs, we can increase the fraction of dying trees in older diameter class. Like this we hope to increase the mortality rate of large-diameter trees, therefore bring the biomass stable sooner. But the problem is that we still have pixels like P0/P2/P4 which are OK. The other issue is that this approach might reduce the diameter of big trees too much. Sebastiaan therefore suggests changing this parameter value only when the diameter of Class3 (or largest diameter class) exceeds a certain threshold like 80cm.

######## FORMAL TEXT END#######
Scrap text - exchanges between Chao and Sebastiaan

In summary, for temperate and boreal regions, we need both a short enough Rstand and asynchronicity. Both should persist in all simulations for unmanaged forests. With this reasoning, we have no longer the distinction between FG0 and FG1. But how can we realize both ? In principle I agree but I do not agree with the wording. We can run FG1 for a very long time and sooner or later we will break synchronicity. As an alternative we could add a bit of code that allows us to start the model with mature trees (we need some estimates for the heartwood to do so - and we have no idea whether the trees would ever reach allometric equilibrium - they probably would). That way we could start FG1 without synchronicity. This is probably the most elegant solution. Third option, probably the most pragmatic, is to use FG0 to break synchronicity. This implies that Rstand in FG0 and FG1 should be coupled (Chao: I see your point here Sebastiaan. We need FG0 to break the synchronicity. We need to apply Rstand through all simulations to keep a reasonable return interval of stand-replacing events).

Ideally the FG0 maps have a different mortality frequency for each PFT. This should be the same frequency as the maximum age found in the model simulations. If we want to avoid problems we should integrate the FG0 functionality in the ORCHIDEE code. What we need is a function to select a random year within the length of Rstand for eachPixel+PFT to initiate a stand-replacing event and then this Pixel+PFT will have stand-replacing event with a constant return interval of Rstand). Before doing so, it is probably better to experiment a bit more with the maps. I think we can make it two steps: (1) reading a Rstand map from outside so that we can account for some spatial heterogeneity. For example, you different Rstand for PFT7 for North America (black spruce), Siberia (scots pine) and Northern europe (Norway spruce). We can search for papers to get a rough idea. (2) write FORTRAN code for FG0 (= randomly select a time slice within the length of Rstand to initiate the first stand-replacing event).

After solving the problem of Rstand, the remaining question is whether ORCHIDEE can simulate the relationship between biomass~age up Rstand. This should be part of validation in GMD paper Trunk4.1

Possible improvements to include:
i) for fires, there are some data showing the fire return interval for some grid cells. => agreed but may be too much as a start? We can try a PFT-dependent fixed Rstand.

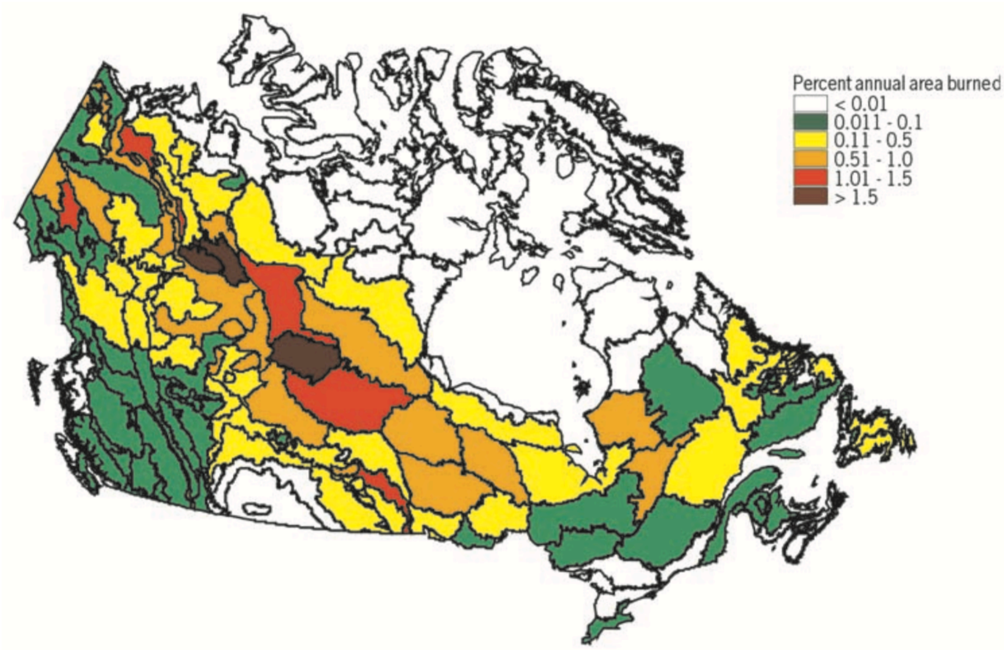


Figure 3. LFDB percent annual area burned (PAAB) distribution across Canada for 1959–1997 period by ecoregions.

ii) otherwise, we can use existing biomass divided by NPP to have Rstand for only unmanaged forest and then extrapolate. => Not sure how reliable this is. Remote sensing NPP and remote sensing biomass for unmanaged stand? Three very uncertain variables. Agree, not a good approach.

2020 GCP bugdet

Table 8. Cumulative CO_2 for different time periods in gigatonnes of carbon (GtC). All uncertainties are reported as $\pm 1\sigma$. The budget imbalance provides a measure of the discrepancies among the nearly independent estimates. Its uncertainty exceeds ±60 GtC. The method used here does not capture the loss of additional sink capacity from reduced forest cover, which is about 20 GtC and would exacerbate the budget imbalance (see Sect. 2.7.4). All values are rounded to the nearest 5 GtC and therefore columns do not necessarily add to zero. Cement carbonation sink is included in E_{FOS}.

	1750–2019	1850–2014	1959–2019	1850–2019	1850–2020[a]
Emissions					
Fossil CO_2 emissions (E_{FOS})	445 ± 20	395 ± 20	365 ± 20	445 ± 20	455 ± 20
Land-use change CO_2 emissions (E_{LUC})	255 ± 70[b]	200 ± 60[c]	85 ± 45[d]	210 ± 60[c]	210 ± 60
Total emissions	700 ± 75	595 ± 65	450 ± 50	650 ± 65	665 ± 65
Partitioning					
Growth rate in atmospheric CO_2 concentration (G_{ATM})	285 ± 5	235 ± 5	205 ± 5	265 ± 5	270 ± 5
Ocean sink (S_{OCEAN})[e]	170 ± 20	145 ± 20	105 ± 20	160 ± 20	165 ± 20
Terrestrial sink (S_{LAND})	230 ± 60	195 ± 50	145 ± 35	210 ± 55	215 ± 55
Budget imbalance					
$B_{IM} = E_{FOS} + E_{LUC} - (G_{ATM} + S_{OCEAN} + S_{LAND})$	20	20	0	20	20

[a] Using projections for the year 2020 (Sect. 3.4). Uncertainties are the same as the 1850–2019 period. [b] Cumulative E_{LUC} 1750–1849 of 30 GtC based on multi-model mean of Pongratz et al. (2009), Shevliakova et al. (2009), Zaehle et al. (2011), and Van Minnen et al. (2009). 1850–2019 from mean of HandN2017 (Houghton and Nassikas, 2017) and BLUE (Hansis et al., 2015). 1750–2019 uncertainty is estimated from standard deviation of DGVMs over 1870–2019 scaled by 1750–2019 emissions. [c] Cumulative E_{LUC} based on HandN, BLUE, and OSCAR. Uncertainty is estimated from the standard deviation of DGVM estimates. [d] Cumulative E_{LUC} based on HandN, BLUE, and OSCAR. Uncertainty is formed from the uncertainty in annual E_{LUC} over 1959–2019, which is 0.7 GtC yr^{-1} multiplied by length of the time series. [e] Ocean sink uncertainty from IPCC (Denman et al., 2007).

Table

Table Global forest carbon stock (Pan et al. 2011 Sicence Supplement; Table S3; data for the year 2007)

	Total living biomass	dead wood	litter	soil	total carbon stock
Boreal	53.9	16.1	27	174.5	271.5
Temperate	46.6	3.3	12.1	56.7	118.6
Tropical	262.1	53.5	4	151.3	471
Global	362.6	72.9	43.1	382.5	861.1