Round #2

Author's Reply:

by Hervé Cochard, 2021-01-13 16:47

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Revision requested for your preprint

Editor (Hervé Cochard)

xx.

Thank you for your encouragements. This paper is based on an old dataset on reaction woods, that had been insufficiently valued, and our judgement was clouded by the need to make the most of it. In this new version we have chosen what seemed to us the most urgent point to focus on, and left other aspects for future work. We hope that it is clearer now. Most figures have been redrawn.

Reviewer 1 (B. Gardiner)

The paper is much better now and there is a clear idea and purpose. I would have liked to see some estimation of uncertainty in the "model" equations provided but this would mean a much more "modelling" approach and I think the work presented is too valuable to not publish now. Maybe the authors can consider in the future a more thorough approach using linear mixed models and looking for the best models to describe each parameter of interest.

Thank you, we are glad that based on your remarks and those of the other reviewer, we were able to produce a readable document. We are aware of the insufficient number of species studied, and repetitions within each species, to produce a fully reliable model to convincingly reconstruct the biomechanical history of any tree based on observations of its wood. What we intended to show, based on what we have, is the likely attainability of such an objective provided more work is done. In addition to the usual uncertainty of measurements, we had here the problem of positioning: angular variations are rapid, so that a small difference of position between corresponding specimens can lead to large errors. We also miss chemical data on matched specimens, that would probably have improved greatly the predictive capacity of our models: for instance, to estimate the maturation strain of normal wood. It would be rather easy to extract wood for chemical analysis from the zone where the maturation has been measured, either by the single-hole method or any other more conventional method.

Corrections in the pdf file

Thank you very much for the language corrections. Except in one case, included in the list below, we have simply accepted them.

In the following we list all suggestions or questions included as comment in the pdf, with our reply for each.

P.2, L.12: Could you please put abbreviations and notations alphabetically with normal alphabet first followed by Greek alphabet.

Done.

P3, L.3: I am not sure "communication" is the best word here. What do you mean?

Replaced by: “transfer of material and information”.

P.4, L.12: F and F_m seem to be used interchangeably in the paper. Should just be one or the other.

We replaced by *F* everywhere.

P.5, Fig.1: It would be good to have the equivalent diagram for conifers. In Fournier et al (1994) Fig. 14 there are only 2 trees.
We do not have equivalent datasets on softwood. Other scientists have measured the growth strains of softwoods, but these data are not available to us. In particular the Japanese group of Nagoya University did measure them extensively, although they usually show growth stress values in their publications, rather than growth strains. We will try again and if we manage to obtain such information will make it available in a future publication.

P.8, L.13 and L. 14: How do you define young? < 30 years? - Sufficient for what purpose?
Actually what mattered in the present case was not the small age (that was unknown in the case of tropical trees), but the small diameter. And the purpose was to have enough reaction wood for preparing specimens. Changed to "Rather small diameter trees … where chosen (Fig. 2). Small diameter trees are more efficient in restoring verticality; they have been selected to have a sufficient thickness (more than 25 mm) of reaction wood in the upper or lower part of the trunk, just above the curvature, in order to cut material for lab tests."

P.8, bottom: I think a link to CIRAD's web page would be better here.
Done as suggested.

P.11, L.6-10: Normally add City and Country
We added the information for each equipment

P.12, L.17: Previously you discussed a chamber with T = 20C. Are these different? Did you use both a chamber and a climate controlled room?
Sorry for the mistake, it was 20°C indeed.

P.14, L-5 from bottom: I wonder if "are the parameters resulting from the development of the living wood until cell death" would read better?
OK, we adopted the suggestion.

P.14, L-1 from bottom: Why underlined? Does a subscript g mean "green"? It is not explained anywhere. Or I missed it.
Underlining removed; notation added to the list of abbreviations

P.16, Fig.9: Although the patterns for Fig. 9 and Fig. 10 are very clear what do we learn from these figures that we do not get from a regression plot or a linear fit? I am not convinced by these plots when the paper already has so many figures.
As explained in the preceding text ["there is often an angular shift between α_m and *LS* which are not exactly measured at the same position. This explains why, besides the high similarity between profiles of the two parameters, regression coefficients (R^2) are not so high (Fig. 8)."] and recalled in the conclusive discussion ["the two properties were not measured at the exact same place in the tree and the angular variations of properties can be very sharp"] direct regressions for each tree would yield poor correlation, although visual examination of each pair of curves shown in the Excel file is rather convincing. It is true that with mean values of *LS* and α_m for all softwoods or all hardwoods, the regression would have been better. We prefer, however, to show the result as fig. 9 and 10 because they introduce the graphs at individual tree level made available in the Excel file. We hope that the readers will not resent to much the large number of figures.

P.16, Fig.9: Why /3 (or why /10)? Why is there no y-axis for LS?
LS and α_m being both given in μstrain, so was LS/3 or LS/10 so that there was no need to use a second axis. A comment was added in the text: "In these graphs, the same scale has been used for α_m and *LS* divided by an ad-hoc value, both being given in μstrain."

P.16, Fig.9, "The angular position was shifted (+225°) in order to have the suspected reaction wood position in the middle of the profile": For all trees? Did all measurements start at the same point? Point of perceived maximum RW formation?
Yes, perceived from the general trend of tree inclination, which usually works very well in the field: even for slightly inclined trees, the direction of RW occurrence can be guessed. As explained in the Material and methods [Eight *GSI* measurements are performed on each tree or tree level, equally spaced around the circumference, beginning on the top of the inclined trunk for hardwoods, where TW is expected, or on the bottom for softwoods, where CW is expected.] the data - as given in the Excel file - were ordered starting from the suspected RW position. For these graphs, we systematically shifted the data in order to observe RW peaks in the middle of the curves. In the legend, "suspected reaction wood" changed to "expected reaction wood".

P. 17, bottom: How old?
Info added: "The 3 conifer trees were rather young (between 10 and 15 years old)".

P.18, L.1-2: I think all Latin species names and genus names should be in italics.
Done

P.20, Fig. 14, "µdef": Is this standard? Normally, I would write µstrain
Ok, systematically replaced in the text and graphs

P.20, L.8, "be it annual or not": What do you mean? Are you thinking of tropical species with no clear annual rings?
Yes. We changed to: "(be it annual or not, as in tropical species)"

P.20, L.21-23: This is a repeat of the 1st paragraph above.
Right! § removed.

P.24, L.15, "For α_m estimation, the same rules using LS can be used.": I don't understand this sentence.
Sentence removed

P.24, L.16-17: I think some assessment of uncertainty on predictions from these set of "model" equations would have been useful. I think these model equations are incredibly useful but the paper would have been enhanced by this assessment of uncertainty in predictions.
This very good comment is in line with your initial remarks, to which we have partially answered. As explained in the following sections of the document, in addition to the usual uncertainty of measurements, we had here the problem of positioning: angular variations are rapid, so that a small difference of position between corresponding specimens can lead to large errors. We also missed chemical data on matched specimens, that would probably have improved greatly the predictive capacity of our models: for instance, to estimate the maturation strain of normal wood. It would be rather easy to extract wood for chemical analysis from the zone where the maturation has been measured, either by the single-hole method or any other more conventional method. Anyway, we added to this §: "In the discussion that follows, some suggestions for improvement will be made".

P.25, L.3 "Most of the time RW is only considered as "muscle" able to produce a motor action": change to "Most of the time RW is only considered as…"
We disagree on this one: what we meant is that RW only, not NW, is usually considered as the muscle of trees - not that RW is considered as muscle and not, for instance, as skeleton. We changed as follow, hoping that is removes the ambiguity: "Most of the time RW alone is considered as…".