Biogeosciences Discuss.,
https://doi.org/10.5194/bg-2020-427-RC1, 2021
© Author(s) 2021. This work is distributed under
the Creative Commons Attribution 4.0 License.

Biogeosciences Discussions
Open Access

Interactive comment on "The motion of trees in the wind: a data synthesis" *by* Toby D. Jackson et al.

Anonymous Referee #1

Received and published: 29 January 2021

The paper evaluates the factors influencing the tree responses to wind loading. To that purpose, the Authors resembled an heterogeneous database including tree motion and wind velocity time series over different trees: broadleaf and coniferous in forests and in open environments. Two factors influencing the tree responses emerged: the tree fundamental frequency (f_0) and the high-frequency slope of the tree power spectrum (S_{freq}). The Authors further found that (1) the fundamental frequency of forest conifers was better predicted according to the cantilever beam model while for broadleaves it was better approximated using a simple pendulum model, and (2) the slope of the tree energy spectrum remained constant from medium to high wind speeds. The paper concludes by some future research directions.

While I find that the Authors did a remarkable job resemble and analyzing this heterogeneous dataset, I find the results on the tree fundamental frequency not so new as compared to the first Author recent paper (Jackson et al. 2019), and I am quite skepti-

Printer-friendly version

Discussion paper

cal about the meaning and importance given in the paper to the slope of the tree power spectrum and about the robustness of its evaluation.

Major concerns:

a) In the Introduction section, the Authors give the impression that most of our understanding on the tree fundamental frequency comes from conifers and much less from broadleaves: "Previous data syntheses have focused on the fundamental sway frequency (f_0) of conifers [...]. This finding demonstrates that conifers can be approximated by a cantilever beam (i.e. a beam with distributed mass), but it is unclear whether this model extends to other types of trees." However, in a recent study (Jackson et al. 2019) the first author did apparently "a comprehensive view of natural sway frequencies in trees by compiling a dataset of field measurement spanning conifers and broadleaves, tropical and temperate forests" (their abstract). They concluded that "The field data show that a cantilever beam approximation adequately predicts the fundamental frequency of conifers, but not that of broadleaf trees" (still their abstract). I am, therefore, wondering what is new in this paper compared to Jackson et al. (2019) on the tree fundamental frequency? Does it require a new publication? What are the differences between the dataset used in both papers? The Authors should better position their paper compared to the previous one.

b) I find the meaning of the slope of the tree energy spectrum not clear in the paper. In lines 94-95, it is written that "the slope of the power spectrum (S_{freq}) can be used as an overall measure of energy transfer between wind and tree at different frequency ranges (van Emmerik et al., 2018; Van Emmerik et al., 2017)". I am not sure to agree with this statement that S_{freq} represents the energy transfer between wind and tree. In my opinion, it is more representative of the tree energy transfer (cascading) or damping from f_0 to high frequencies. Indeed, f_0 is usually located at the level of the inertial subrange of the wind velocity spectrum (see Figures S6 and S7), i.e. at frequencies larger than the frequencies of the main eddy motions at canopy top. I would think that the energy transfer between wind and tree occurs mainly at lower frequencies than f_0,

Printer-friendly version

Discussion paper

where the tree power spectra exhibit the same distribution with frequency as the wind spectra. I think S_{freq} reflects how the tree damps/transfers its energy independently of the wind. Maybe a way to verify which flow motions are involved in tree motions is to look at the momentum flux cospectrum, assuming that the momentum flux at canopy top is totally absorbed by the trees. For example, if you look at Figs 4 and 6 of Dupont et al. (2018, Agric Forest Meteorol., 262, 42-58), you can see that most of the canopy-top momentum flux occurs at frequencies lower than f_0. Smaller eddies than the dominant canopy-top eddies may transfer as well energy to the tree but I would think it mainly concerns branches and less the trunks where the measures presented in this paper have be done. Branch motions are not necessarily in phase with the trunk motions. The lower S_{freq} for broadleaves than for conifers may just reflect their difference in architecture. I would think that S_{freq} is representative of the tree properties, but not representative of the wind. Is it really new/surprising to observe differences between tree species in energy cascading/damping knowing that this mechanism depends on the tree properties (architecture, stiffness. . .)?

c) I am skeptical about the estimation of S_{freq}. First, tree energy spectra do not show scaling law between the fundamental frequency and high frequencies because of the presence of secondary maxima. It is therefore quite questionable to define a slope there. Second, this slope has been defined for a specific frequency range in Hz (lines 167-168), while this frequency range should start from a frequency depending on f_0. The estimated slope is certainly sensitive to the height and width of the tree spectrum fundamental and secondary maxima. There are many cases where it seems impossible to define S_{freq} (see the tree spectra in Figures S6 and S7). I am, therefore, not surprise to see some erratic behaviors in S_{freq} in Figures 4d. At least, these erratic behaviors should have been removed. Third, the Authors seem surprised and present as a result the fact that below a threshold wind speed value, S_{freq} decreases with wind speed (Figures 4c-d). In my opinion, this decrease of S_{freq} reflects the increasing noise of the tree data at high frequencies as the wind diminishes. With decreasing wind, the frequency of the main canopy motions gets lower. Consequently, f_0 is shifted to the

Printer-friendly version

Discussion paper

bottom (high frequencies) of the inertial subrange of the wind velocity spectrum, where there is much less energy. The high-frequency trunk motions become negligible. I am, therefore, not surprise to see that S_{freq} decreases with wind speed, its evaluation becomes irrelevant and should not be presented.

d) The location of the wind speed measurement should be clarified. The Authors compared the tree inclination angle against the wind speed between summer and winter but they do no say clearly where the wind has been measured (at canopy top, outside the plot…). It is just written that "We note that wind speeds were measured outside the forest or at canopy height in a single location" (Lines 304-305). This difference in wind speed measurements between experiments makes it difficult any comparison. For measurements outside the forest, are winter and summer measurements representative of the same footprint? It is difficult to conclude on Fig 4a because we do not know where the wind speed has been measured. The wind speed should be normalized by a reference wind speed.

Specific comments:

1) Line 87: Which balance are you talking about? Can you be more specific?

2) Lines 97 and 46: "This study brings together all available data on tree motion". This is quite a strong statement. I see at least two datasets that were not considered or mentioned in this study: Sellier et al. (2008, Forestry 81, 279–297) and more recently Dupont et al. (2018, Agric Forest Meteorol., 262, 42-58).

3) Line 102: Correct the parentheses for the Lubba et al. reference.

4) Line 124: "We therefore focus on analyses which do not require these data (although we explore this data in supplementary S2)." This is confusing, why talking about these data if you did not use them?

5) Lines 138-139: "although open-grown trees exposed to strong winds may experience slowly varying displacements due to the mean wind speed on this timescale

Printer-friendly version

Discussion paper

(Angelou et al., 2019; James et al., 2006).” I do not understand. Could you clarify?

6) Line 144: How did you define the windy conditions?

7) Lines 227-229: “The frequency range in which energy transfers from the wind to the tree will therefore shift, and this will be reflected in the slope of the power spectrum.” I do not understand why it will be reflected in the slope of the power spectrum. Shift in which direction?

8) Line 276: “It shows a clear separation between forest conifers and open-grown broadleaves, driven by S_{freq}, which is related not only to the damping efficiency of the tree, but also to the energy spectrum of the local wind loading”. I do not understand the justification for the last part of this sentence.

9) Lines 328-329: “This regularity could be related to the wind environment (i.e. a turbulent wind environment leading to lower regularity)”. I do not understand the notion of “wind environment”, nor what is written between parentheses.

10) Line 342: “separate models”, to which models are you referring to?

11) Line 347: “which suggests a difference in the frequency range of the peak wind-tree energy transfer.” Could you clarify? I do not understand.

12) Line 378: “This could be because the size of the turbulence structures containing most energy are smaller than the tree crown at high wind speeds”. I do not think so. The main turbulent motions at canopy top should not change much size with wind speed. In my opinion, the plateau of S_{freq} just shows that S_{freq} does not inform on the wind-tree energy transfer but only or mainly on the energy cascading/dissipation of the tree motions from f_0 to high frequencies, which only depends on the tree properties and much less on the wind intensity.

13) Line 422: “the oscillatory component of tree sway diminished with wind speed for four forest Scots Pine trees”. I am not sure to understand this sentence and how it demonstrates the presence of a resonance mechanism.

CC BY

14) Lines 423: “the all the trees”, please rephrase.

15) Line 447-448: “All trees in this study exhibited a remarkably constant slope of the power spectrum from medium to high wind speeds in both summer and winter. This suggests that the relationship between wind loading and tree deflection is simply related to wind speed in the high wind speed range.” So, it does not depend on the tree properties? I would say that it shows that S_{freq} depends on tree trunk and branches properties and less on the presence or not of leaves.

16) Figures S6-S7: Is it the same graduations for the vertical axis of the tree and wind energy spectra? Could you show the graduations? It would be nice to show the -2/3 slope. Some velocity energy spectra look very flat but it may just be a question of graduation.

Interactive comment on Biogeosciences Discuss., https://doi.org/10.5194/bg-2020-427, 2020.

CC BY