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

**Jackson Toby**

tobydjackson@gmail.com

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Thank you for your review. I am discussing your points with co-authors and we will make a joint reply in due course. For the time being, I am replying personally as first author.

I think that some of the issues you raise, while appropriate for a simple single site study, are necessary complications when conducting a multi-site study such as this. For instance, some studies did not record wind data at all, others used a MET station near the forest to give hourly mean wind speeds and a few studies collected wind data at canopy height at 10 Hz using a 3D sonic anemometer. These differences preclude some types of analysis that would otherwise have been valuable. However, I think it is still useful to bring together these diverse data sets and compare them. Many of our conclusions (see 'Future work directions') address these differences between sites



and we suggest best practice for future studies.

The aims of this paper were (1) to bring together numerous tree motion data sets; (2) to test whether previous results hold across multiple sites and (3) to describe the key similarities and differences that emerge when studying tree motion at this scale. I will respond to your points in order below.

Major concerns:

a) As you point out, there is certainly some overlap between this paper and Jackson et al (2019). I agree with you that this analysis should be better positioned with respect to Jackson et al (2019) and the key differences explicitly stated. However, Jackson et al (2019) was primarily a simulation study, using finite element analysis to explore the relationship between tree architecture and natural sway frequencies, and it used the field data only to demonstrate the range of natural variation. Furthermore, there are three key differences between the analysis in Jackson et al (2019) and the current study:

(1) The data set we presented in Jackson et al (2019) was comprised of only summary data on the fundamental sway frequency, calculated using different methodologies in each individual study. In the current study, we have collated the raw tree motion data and calculated the fundamental sway frequency using the same method (wavelet analysis) for each time-series. This makes the current analysis more robust.

(2) There is more data in the current study. Comparing table S1 from Jackson et al 2019 with Table 1 of the current study shows 39 additional trees from conifer forests, 8 additional open-grown conifers, 61 additional open-grown broadleaves and 17 additional trees from broadleaf forests. The 61 additional open-grown broadleaves, which were under-represented in Jackson et al (2019) make a substantial contribution to the current study.

(3) We did not test the simple pendulum model in Jackson et al (2019), which turns out

to be quite important in the current study.

I believe that, given the aims of this paper, it is valuable to include the fundamental frequency analysis (Figure 1) despite the overlap with Jackson et al (2019). This analysis gives the reader a good understanding of the variation in tree motion across our data set (i.e. the 'slowest' tree in our study takes approximately 10 seconds to complete one oscillation, while the 'fastest' takes approximately 0.7 seconds). It is also an important feature of tree motion that has been widely used in the literature and would therefore be a strange omission from a paper which aims to synthesis tree motion data. I will adapt the text to reflect these updates.

b) I agree with you that the momentum flux cospectrum, or the transfer function would be preferable, but this was not possible for the majority of trees in this study due to the lack of high-resolution wind data. Most studies of tree motion do not include high-resolution wind data (L121-125) because of the prohibitive costs, we discuss this in the 'Future research directions' section of the discussion. We included the slope of the power spectrum in this study because that two previous studies (van Emmerik et al 2017 and 2018) found it to be important and useful in the absence of high-resolution wind data. Given our results, that it was an important factor in distinguishing types of trees, I think its inclusion was justified.

The interpretation of the slope of the power spectrum is something we discussed at length while writing the manuscript. I will therefore respond to your comment at greater length after discussing it with my co-authors. However, in response to your query, I would point out that this result is certainly new (to the best of our knowledge) and shouldn't have to be 'surprising'.

c) I explored different methods to estimate the slope of the power spectrum in this study and found that using a large fixed interval was the most robust. This is similar to the approach taken in previous publications (van Emmerik et al 2017 and 2018). As you suggest, defining a different frequency range for each tree based on its fundamental



frequency is an attractive idea, but in practice this method was too noisy to be applied across such a diverse range of data. A number of trees in our study exhibited no consistent fundamental frequency peak, so the automatically extracted slope of the power spectrum would be undefined in these cases. However, I am happy to remove the most erratic lines from figure 4d.

The definition we use gives a measure of the decline in energy in the tree spectra from 0.05 to 2 Hz. We find a rather consistent pattern across all trees and our results show this to be an important feature.

I will address your third point (whether the decreasing trend is driven by noise) after discussion with the co-authors. We considered this possibility while writing the manuscript and decided otherwise.

d) The location of the wind speed measurement is different for each study (L121-125), this is one of the challenges in working with such diverse data sets. Importantly, Figure 4d shows the changes over time for each tree individually, we are not comparing the magnitude of wind speeds across sites. It is these changes with increasing wind speed that we compare and find to be remarkably similar.

Part of the value of this study is that, in bringing together these data sets, we can compare the advantages and disadvantages of different experimental setups. I can include more information on the location of the wind sensor for each site individually in table 1. Furthermore, the online data repository will include descriptions of the individual sites.

RE figure 4a – in this case a single tree is presented as an example and the wind speed measurement was taken outside the forest at a nearby MET station. I will include this information in the figure caption. Thank you for pointing this out.

I don't understand what you mean by 'the wind speed should be normalized by a reference wind speed'. In each site wind speed was measured in a different way, therefore a 5 m/s wind speed measured outside the forest in site A is not the same as a 5 m/s measured at canopy top in site B. It would be nice to work out how these two measurements compare, perhaps with reference to some standard measure of wind speed 100 m above the surface. However, I do not think methods exist to make this conversion across sites from cities to tropical forests. If you have suggestions / corrections on this I would be happy to learn more.

Specific comments:

In this section I will again leave comments which mainly concern the slope of the power spectrum until I have discussed them with co-authors.

1) Thank you for pointing this out. This is the balance between energy transfer from the wind to the tree, and energy dissipation by damping. I will update the text to explain this explicitly

2) Thanks, the previous comment from Damien Sellier also mentioned this. I will change this statement in the text to: 'a large data set of available tree motion data' or similar. Additionally, I will reach out to these authors and attempt to include their data in the study. As I understand it, these studies represent 2 and 3 medium sized conifers, respectively. This is the group of trees best represented in our sample so their inclusion is highly unlikely to change the results. I could also include these data in the online repository for future use. Also, we are aware of three other small data sets, but they were not available to this study.

3) I will correct this.

4) This refers to the fact that different data sets have different wind data associated with them (L121-125). This is a key point in understanding the challenges of this type of multi-site study, as discussed in detail above. This confusion may have contributed to some of the 'Major Concerns' discussed above. I mention these high-resolution wind data because they are the 'gold standard' in experimental design and many of the more recent papers on this subject rely on these data. I suggest that, instead of omitting this



important point, I expand upon it and lay out what the lack of these data means for the subsequent analysis.

5) When processing raw tree motion data, it is generally necessary to define a zero or mid-point at which the tree is vertical. This allows the data to be interpreted in terms of displacement from this position in different directions. This is complicated by the fact that some motion sensors 'drift' over time, i.e. an offset builds up due to a number of factors (L133-134). Most previous studies have done this by assuming that the tree will pass through its mid-point often. Therefore, we subtract a running mode or apply a high-pass filter to each interval of data (1 hour or 10 minutes are commonly used) to correct for this offset. This has been shown to be effective in a number of studies and is standard practice for trees in forests. However, open-grown trees may behave differently. In particular, they may be displaced from the vertical for a long period of time due to the effect of the mean wind speed. This is discussed in detail in the cited paper (Angelou et al 2019) and we mention it here as a caveat to our results. I will explain this in greater detail in the text, thanks for pointing it out.

6) I chose the windiest available period for each data set. I will update the text to reflect this.

7) I will address this after discussing with co-authors.

8) I will address this after discussing with co-authors.

9) Perhaps this is a poor choice of phrasing on my part. By 'wind environment' I am referring to the wind conditions affecting a tree or group of trees. For example, tree A, situated in a dense forest will experience turbulent wind conditions with most of the loading on the upper canopy, while the lower parts of the tree are sheltered. Tree B, growing on a flat coastline with no other trees or obstacles nearby will experience consistent wind speeds and the wind loading will be distributed across the height of the tree. In the parentheses I am suggesting that the motion of tree A may be more regular than that of tree B.

10) Sorry for the confusion, I will elaborate and make this point clear in the text. There have been a number of attempts to model tree response to wind loading (e.g. Forest-Gales) but these are mostly based on uniform stands of conifer plantations. It would be valuable if these models could be transferred to more 'natural' forest environments and to open-grown trees. If we had found a clear separation between different types of trees in Figure 3, i.e. they move in distinct ways, transferring these models between these types of trees would have been unlikely to work. We therefore suggest it is good news, from a modelling perspective, that the trees overlap in Figure 3.

11) I will address this after discussing with co-authors.

12) I will address this after discussing with co-authors.

13) Thanks for pointing this out – I will expand upon it in the text. This sentence is describing the work in the cited paper (Schindler and Mohr, 2018) and suggests that no resonant effects were found. They used singular spectrum analysis to separate the oscillatory components of tree sway and found that their importance diminished wind increasing wind speed. We are suggesting that this analysis should be carried out across our newly collated data set, in order to test whether this result holds more generally.

14) Sorry about that, I will correct it.

15) I will address this after discussing with co-authors.

16) I am happy to adjust the figures in the supplementary materials – thanks for your suggestion. Comments outstanding

In summary, I have addressed a number of your comments above but have left those related to the slope of the power spectrum for discussion with co-authors. To help keep track of this, I have pasted your comments which are still outstanding below. Thank you again for your review of our paper, and I will respond to the following comments in the near future.



Major concerns:

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 (Sfreq) 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 Sfreq represents the energy transfer between wind and tree. In my opinion, it is more representative of the tree energy transfer (cascading) or damping from f0 to high frequencies. Indeed, f0 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 f0, where the tree power spectra exhibit the same distribution with frequency as the wind spectra. I think Sfreq 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 f0. 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 Sfreq for broadleaves than for conifers may just reflect their difference in architecture. I would think that Sfreq 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: : :)?

Part of c) Third, the Authors seem surprised and present as a result the fact that below a threshold wind speed value, Sfreq decreases with wind speed (Figures 4c-d). In my opinion, this decrease of Sfreq 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, f0 is shifted to the 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 Sfreq decreases with wind speed, its evaluation becomes irrelevant and should not be presented.

Specific comments 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 Sfreq, 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.

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 Sfreq just shows that Sfreq does not inform on the wind-tree energy transfer but only or mainly on the energy cascading/dissipation of the tree motions from f0 to high frequencies, which only depends on the tree properties and much less on the wind intensity.

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 Sfreq depends on tree trunk and branches properties and less on the presence or not of leaves.

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