

Review of “Winds with destructive potential across a topographic and seasonal gradient in a Central Amazon forest ”

November 30, 2024

Outline of manuscript

This manuscript presents a statistical analysis of wind gusts at three towers erected in a transect in an undisturbed (by human activity) Amazonian forest. The underlying motivation is the potential that high winds have to bring trees down. The experimental setup is well designed, and the 3 towers cover a topographical transect with a ~ 78 -m altitude difference. (per Fig. 1c). An impressive two-year measurement campaign of meteorological data (mainly horizontal wind and precipitation) was undertaken. The statistical analyses performed by the authors are extensive and, to the best of my understanding, correct.

The statistical treatment involves near-simultaneous analysis of rainfall intensity. The overall results are also associated with different meteorological patterns and phenomena known to occur in the Amazon region, as well as to the wet and dry seasons.

The authors try to establish connections, one the one hand, between causal or at least explicative phenomena (season, meteorological phenomena such as mesoscale convective systems, heavy rainfall, . . .) and wind gusts with destructive potential, and on the other hand, between the gusts themselves and windthrows. In this second part of the manuscript, the authors are less successful, and their related discussion is far from convincing.

This is not the authors’s fault, but I think is inherent to the dataset that was collected. First, the statistical connection between possible explicative variables and the wind gust characteristics (number of gusts, representative wind speed and representative duration) is weak. In several parts of the manuscript, the authors try to weigh earlier (contradictory) results from the literature with their own results from the present dataset. The attempt fails, and one is left with the impression that the contradictions are being justified by the complexity of the phenomena involved, or by further, non-explored factors such as soil moisture, wood density and canopy architecture, etc..

Second, during two years there were no windthrows (from what I can understand) in the forest around the 3 towers. This goes against the expectation given the recorded wind speeds and durations, but is a fact duly reported in the manuscript.

It is my view that this second part of the manuscript is trying to deal with the unexpected weak signal between rainfall and wind gusts, and season and windgusts (for example), and the unexpected lack of windthrows, by unduly expanding the discussion around meteorological phenomena present in the Amazon region. The discussion shows that

the authors have a firm and deep knowledge of the meteorology of the Amazon, but the dataset (and statistical approach adopted) simply does not allow to establish definitive connections. It is better then just to state the facts, and refrain from discussing every possible cause for what was observed (or not) in the present data.

Finally, I have some reservations on the use of “innovative analytical approach” to describe what was done. In my area of research, the word “analytical” is reserved for deriving or employing equations that describe a certain phenomenon. For example, the Central Limit Theorem is an analytical result, and so are the mathematical forms of the Extreme Value Distributions. Here, the term refers (as best as I could understand) to some statistical criteria to define what a Wind with Destructive Potential Event (*i.e.* a Gust) is. The criteria are perfectly fine, and all data analyses of this type need some criteria to be adopted. But this is not an analytical approach; rather it is a set of criteria adopted by the authors to *analyze data statistically*.

I make specific comments in the next section.

Specific comments

1. 35 “Meteorological tower” is a strange keyword
1. 140 “The anemometer data is stored at the rough frequency”
The anemometer data **are** stored at the **raw** frequency. (Data is the plural of datum).
1. 179 “Attributes of number”. In this manuscript, “number” is often used with the meaning of “number of events”, “number of WDP events”, or simply “number of WDP”. In the interest of accuracy and clarity, please always use one the latter forms.
1. 189 “We summarized the WDP attributes of number”. Number **of events**.
1. 207 Rainfall rate in mm min^{-1} . **Suggestion:** in the manuscript you mix rainfall rates in mm min^{-1} and mm hr^{-1} . Consider using only one of them, because it makes it easier for the reader to compare and comprehend them. I personally prefer the latter, because (say) 30 mm is a particularly heavy rainfall for a duration of one hour, and easier to grasp than the rainfall during 1 minute.
1. 219 $\log(y + 1)$: I suppose you are summing 1 to avoid the occurrence of $\log(0)$ in your data processing. But this is problematic, and you are not giving any explanation. It is acceptable if all the y s are $\gg 1$; if on the other hand all, or a substantial number, of them are $\ll 1$ then your regression may suffer or become meaningless. You may want to adopt a more refined statistical analysis.
1. 257 “Our results suggest that the topographic effect of the WDP is attributable to fewer and shorter events, with only those reaching the highest recorded speeds propagating down to the lower portions of the relief (*i.e.*, lower portions of slope and valley).” But then they have to be near-simultaneous; otherwise, you are conjecturing. You should check the near-simultaneity of WDPs at

the 3 towers and then, for the simultaneous occurrences, verify if those in the valley are shorter, show lower wind speeds, etc..

Fig 7 has a huge caption. Shorten the caption, and transfer the necessary explanations/discussions to the text.

1. 328 “Wind speed generally increases with altitude”. I think you mean it increases with **height**.

Section 4.1 Consider re-writing and shortening this section substantially. There are many general statements that do not contribute to understand better WDPs. A few samples are:

- “Thus, the wind speeds we recorded were likely affected (reduced) by interaction with the canopy and lost some energy”. A wind speed cannot lose energy.
- “Theoretically, wind gusts lose speed when entering valley formations because the wind encounters resistance from the trees, which reduces its strength (Quine et al., 2021)”. But there are trees everywhere in the forest. What am I missing here?
- “The shelter effect creates more recirculation eddies after the wind has passed the obstacle (Cassiani et al., 2008; Detto et al., 2008). In high-slope valleys, as typically found in our study region (Rennó et al., 2008), the rapid change in altitude overcome the sheltering effect produced by trees” Not clear what the authors mean here. A “shelter effect” is usually related to a wind barrier, right? But I do not see any such barrier in the present study. What feature of the terrain are you talking about? Can you draw a figure explaining what you mean **for this site**?
- “This process has already been described in adjacent forests with similar structure (Tóta et al., 2012) and corroborates that turbulent flows from tree and wind drag may have influenced more the critical duration of gusts than their speed” I cannot understand this: it is too qualitative. What do you mean that turbulent flows affect more the duration than the speed of gusts? How can you prove it?.

1. 378–405 This is a very long discussion of meteorological phenomena in the Amazon region. Enlightening, but what is the point? It would be nice to have a relatively simple physical explanation of WDPs, but it looks more like the authors are trying to connect *all* of these phenomena to WDPs, and in the end I was left without a clear picture. Are all of these things possible causes of WDPs? Should you state that at the outset? Or are you conjecturing about possible causes? Again, I suggest shortening and stating clearly what is known and what is not known regarding the links between general meteorology and WDPs.

1. 429 “We recorded 122 WDP (29% of total) not accompanied by rain in a time lag ≥ 20 min, *being one of them with speed of 17.94 m s^{-1}* ”. This is not proper English to the best of my knowledge.

1. 441 “Topographic scale” What is this?

Section 4.3 I am confused again after reading this section several times. You did not find windthrows related to the gusts in your data, and this is clearly causing difficulties. In fact, you have no observations that can justify the title of this section. From what I can understand, then, this section is now only a general discussion of possible causes of tree mortality. The only concrete conclusion that I can draw is that the *a priori* criteria for WDP, based on previous studies, did not correlate to actual windthrows, and that perhaps the relation between WDPs and tree mortality is more site-specific than previously imagined. Again, I suggest to shorten the discussion, be clear about the absence of data on tree mortality to compare with the met data, and stop there.

Section 4.4 Is long and again veers on general discussion of literature results. Maybe it could be shortened and joined with the “Conclusions” section.