

Answer to Reviewer 2 (answer in italic)

General comments:

This paper describes changes in aerodynamic characteristics of an eddy covariance site following a windthrow. It highlights the importance of aerodynamic characteristics for flux measurements and determining the flux footprint.

The results are presented clearly and concisely, but the paper is narrowly focused on describing the specific characteristics of this site and doesn't include much discussion of the literature or the implications of this work. Overall, the paper would benefit from adding more background and discussion to connect this work to the broader context. In addition, more details should be included about the estimation of the zero-plane displacement and the sensitivity of the results to this estimation.

We thank the reviewer for the helpful comments. The comments are largely identical to the comments made by the first reviewer, in response to which we have added a paragraph to the introduction (see response to Reviewer 1). Regarding the sensitivity of the individual methods, we would like to refer to the LES study by Maurer et al (2015) but, at the same time, have extended somewhat the discussion section .

Specific comments:

Lines 21-26: The introduction would benefit from more clearly explaining the background and motivation for this work. This opening paragraph is focused on the effects of heterogeneity, although this doesn't seem to be the focus of this study. I suggest expanding the introduction to include more context from previous work and explain the motivation for this study.

See our note to the introductory text.

Lines 31-39: Since this study simply uses an estimate of zero-plane displacement before and after windthrow, this section should be rephrased to make clear that the objectives were to study the effects of the change in zero-plane displacement and roughness on the turbulence characteristics and flux footprint. Line 31 implies that this study is examining the zero-plane displacement itself.

*In line with this comment, we have rephrased the sentence of lines 31/32 as follows: **“This prompted us to examine the changes in the roughness parameter before and after the windthrow, under the assumption of a linear dependence of zero-plane displacement on stand height.”***

Lines 49-50: The previous paragraph states that an objective of the study was to examine changes in the zero-plane displacement height, so this statement about the

zero-plane displacement height is confusing. I suggest clarifying here the time periods of data that are used in this study (before and after the windthrow), and that $d=3.8$ just refers to the before-windthrow calculations.

To avoid this confusion about the zero-plane displacement height, we propose to add "before windthrow" on line 50, after Oliveira et al. (2021), in addition to the changes proposed to lines 31/32 under the previous comment.

Lines 74-79: Given the focus of this paper on changes in the zero-plane displacement, this section should include some justification of how these values of the zero-plane displacement were chosen.

We believe that the change proposed to lines 31/32 also address this comment.

The sensitivity of the results regarding the stability parameter, turbulence characteristics, and footprint to the chosen value for zero-plane displacement (and roughness) should be examined further. How do the results change if different values of d and z_0 are used?

Since we did not use a synthetic but a real data set (that from 2020), we see little sense in manipulating the stability and turbulence characteristics. Furthermore, as show in Figure 4, the influence of zero-plane displacement and roughness parameters on turbulent fluxes and stratification (frequency correction) was found to be irrelevant, both before and after the windthrow. Stability influences on the calculation of fluxes are described extensively in the literature (e.g. Foken et al., 2012), so that we see little added value in extending the present work in this respect.

For example, Line 239 states that the difference in footprint area “would probably not have been as large if a slightly better value of $z_0=0.5$ m had been assumed before the windthrow.” Calculations could be repeated with this different value to show the sensitivity of the calculated footprint area to d and z_0 .

The application of the parameter set (zero-plane displacement, roughness parameter, measurement height) to the calculation of the footprint (fraction of the target area in the footprint) was performed using the 2020 dataset for the conditions before as well as after the windthrow and with reduced measurement height. As shown in Fig. 5, the obtained result seems to be as expected. Therefore, we believe that further combinations of d and z_0 would not provide relevant further insights. However, we agree that it is worthwhile, we will repeat the calculation with the changed roughness length for the conditions before the wind break. If these new results are relevant, we will integrate them in Fig. 5.

The discussion sections should include more connections to the literature and discuss the broader implications of these findings.

The additional paragraph in the introduction provides a more extensive citing of the existing literature. We used the classical approach, which is comprehensively presented and discussed in existing handbooks (e.g. Foken, (2017, Sect. 3.1.1 and 3.1.2). The newer approaches which involve a parameterization of stand structure could not be applied in our case, either after the forest fire and after the windthrow. This because the required stand parameters were too heterogeneous at our study site or, as for example in the case of the leaf area index, could not be determined from the data collected at the site or the information available from existing sources. At the same time, we have added the following sentences to the discussion:

Before the windthrow, a very low roughness height of $z_0 = 0.4 \text{ m} = 0.05 z_c$ was assumed because of the very wind-permeable nature of the maritime stands which, in turn, reflected the fact that the fire had consumed the complete crowns of the bulk of the pine trees. This very low roughness height could not be confirmed by either the calculations using Eqs. 2 and 4 or Fig. 6. The obtained value of $z_0 = 0.7 \text{ m}$ agreed with the simple relationship of $z_0 = 0.1 z_c$ (Foken, 2017; Monteith and Unsworth, 2013) but not that of $z_0 = 0.2 z_c$ (Kaimal and Finnigan, 1994). The same applied, mutatis mutandis, after the windthrow. By contrast, the assumed values for the ratio d/z_c of 0.5 before the windthrow and 0.666 after the windthrow were confirmed by the observations. These findings further confirmed a linear relationship between d and z_c , in line what was found using approaches that explicitly consider parameters describing stand structure (Maurer et al., 2015).

Line 246-250: This should be moved to the discussion section.

This statement is intended to inform users of the planar-fit method and, in our opinion, fits better in the Conclusions than in the Discussion, including because it does not deserve discussion. We will again emphasize at an appropriate place in the text that double rotation was used in all calculations, as this has only been described in detail so far in Oliveira et al. (2021).

Some of the data is not publicly available and is listed as “available on request.” From my understanding, Biogeosciences’ data policy requires data to be in a public repository.

Changes of z_0 and d in the flux and footprint calculations must be made in the programs that use the 20 Hz data as input data. Such high-resolution data sets are very large and are usually not published. Furthermore, we believe that the analysis of this detailed data sets would require our active involvement, because of our in-depth knowledge of the data set as well as of the ancillary data set and the study site in general. Therefore, we provide the dataset only to interested colleagues "on request". Worth stressing is perhaps that the dataset used in Oliveira et al. (2021, Results) has already been published (Oliveira et al., 2020). At the moment, we are analysing the EC data over the first three post-fire years and, in due time, will also make this data set available in the same format as Oliveira (2020).

Technical corrections:

Line 170: “For the 2020 period”

Line 175: “windthrow were”

Line 243: Remove second period after “reasonable as well”

Thank you very much for your careful review and for having spotted these mistakes. We will correct all of them in the next version.

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