Response to Referee #2

We thank Referee #2 for their additional feedback and for maintaining a constructive tone in the second-round comments. The remaining concerns are well articulated and have helped enhance the manuscript's precision and transparency. We address each point in detail below. Reviewer comments appear in *italics*, followed by our responses. Line numbers correspond to the revised manuscript.

General Comments

Comment 1: *The main objective ("we present the first continental assessment of structural and compositional selectivity in recent forest disturbances across Europe.") is not still enough accurate to what the authors did.*

Response: We agree. The objective has been revised to explicitly reference the two disturbance agents under investigation. The revised sentence is: "Here, we present the first continental assessment of structural and compositional selectivity in bark beetle and windthrow disturbances across European temperate and boreal forests." This revision ensures that the stated objective is fully aligned with the title, abstract, and scope of the analysis.

Comment 2: *The paragraph added to the methods section would benefit from the inclusion of additional references, particularly to support the statement regarding "the documented shift from wind-dominated to bark beetle-dominated disturbance regimes".*

Response: We agree. We have added the following references to support this statement: Patacca et al. (2023), who document a significant increase in bark beetle disturbance relative to wind across Europe since the 1950s, and Senf and Seidl (2021), who demonstrate the persistent dominance of bark beetle impacts following the 2018 drought. The revised sentence in Section 2.1 [L. 94-96] now states: "Fire and mixed disturbances were excluded from this study because our research question specifically concerns the structural consequences of the documented shift from wind-dominated to bark beetle-dominated disturbance regimes (Patacca et al., 2023; Senf and Seidl, 2021)."

Comment 3: *We suggest providing supporting references for the statement "Spruce was retained as a distinct group given its documented preferential susceptibility to bark beetle outbreaks and its dominant role in Central European disturbance dynamics." According to Berthelot et al. 2021, it would be worthy to focus on Picea and Larix.*

Response: We thank the reviewer for this suggestion. References to Hlásny et al. (2021) and Berthelot et al. (2021) have been added to substantiate the statement regarding spruce susceptibility. The revised sentence in Section 2.2 [L. 113-115] is: "Spruce was retained as a distinct group given its documented preferential susceptibility to bark beetle outbreaks (Hlásny et al., 2021; Berthelot et al., 2021) and its dominant role in Central European disturbance dynamics (Patacca et al., 2023)." Although Berthelot et al. (2021) emphasise the vulnerability of both Picea and Larix, the present data do not support a statistically robust separation of Larix at the continental

scale due to its comparatively limited spatial extent. Consequently, Larix remains within the other needleleaf group, as described in the Methods.

Comment 4: *We believe that "functional group" is not the most appropriate term in this context. A more precise wording would be "disturbance-response groups" or simply "groups". In addition, we recommend homogenizing the terminology to refer to these groups, as several terms appear to be used interchangeably (e.g. functional group (L111); functional group class (L. 139); genus class (L197); genus (L204); forest genus (L217).*

Response: We agree that inconsistent terminology reduces clarity. We have replaced all previous terminology with the consistent term "genus group," which more accurately reflects the taxonomic basis of the classification (genus-level EO data) and clarifies that the grouping is analytical rather than strictly taxonomic. The terms "functional group," "functional group class," "genus class," and "forest genus" have been removed from the manuscript. When referring to the classification map input, we use "genus classification"; when referring to the three analytical categories, we consistently use "genus group."

Comment 5: *The main text only reports the percentage of pixels affected in both periods by natural disturbances (~3%), while the proportion of pixels disturbed twice due to harvesting, which is higher (~8%), is not mentioned. We believe that this value should also be reported in the main text and discussing its potential influence on the results and their interpretation.*

Response: We agree that this omission should be corrected. The following sentence has been added to Section 2.4 [L. 162-166]: "The proportion of pixels experiencing two or more harvest events within the study period was higher (~8%), reflecting shorter rotation cycles compared to natural disturbance return intervals. Although this proportion is modest, repeated harvest events in the same pixel do not alter the fixed 2010 biomass baseline used in the analysis and are therefore unlikely to systematically bias the harvest-related structural comparisons between periods. However, results for harvest should be interpreted with this caveat, especially in regions characterised by short rotation forestry."

Comment 6: *We remain concerned that this selection criterion may also introduce a bias toward regions with relatively high disturbance activity. As a result, the analysis may not be fully representative of the broader study area, and differences in disturbance intensity across regions could influence the interpretation and comparability of the reported results.*

Response: We appreciate this concern and have explicitly acknowledged it in the limitations paragraph in Section 4 [L. 618-622]: "The 50-pixel hexagon retention threshold may also introduce a geographic bias toward regions with elevated disturbance activity, as landscape units with sparse disturbance records are excluded from the analysis. This means that the continental estimates reported here may overrepresent structurally vulnerable, high-disturbance landscapes and may not fully reflect conditions in low-disturbance regions. Readers should therefore interpret the pan-European summaries as characterising the most dynamically active portions of the European forest domain rather than the full breadth of European forest conditions."

Comment 7: *It is important to specify that age refers to 2010 baseline age anytime and discuss the results accordingly. For example, in figure 1, the age class refer to the age class in 2010 that could be different from the age class at the time the disturbance occurred.*

Response: We agree that this distinction requires consistent application. Accordingly, we have implemented the following revisions: (1) The caption for Figure 1 now explicitly states, "Age classes refer to the 2010 baseline age of each pixel, not the age at the time of disturbance." (2) The caption for Figure 3 now includes, "Biomass values reflect 2010 ESA CCI v6.0 estimates fixed prior to any disturbance within the study period." (3) In the Discussion, we have revised sentences interpreting age-class or biomass-class results to include the qualifier "2010 baseline" where this distinction is not already evident from the context.

Specific Comments

Comment 8: *In line 107, the manuscript states that "we downscaled the 10 m European genus classification map." The term downscaled is not appropriate in this context, as the spatial resolution is being coarsened from 10 m to 100 m. We therefore recommend replacing downscaled with aggregated or upscaled.*

Response: We appreciate the reviewer's attention to this detail. The term "downscaled" has been replaced with "aggregated" throughout Section 2.2. The revised sentence is: "To incorporate tree species composition into the analysis, we aggregated the 10 m European genus classification map (De Keersmaecker et al., 2024) (EPSG:3035) to a 100 m grid in EPSG:4326."

Comment 9: *The revised explanation now makes it clear why this threshold was chosen although a reference to support this threshold would further strengthen the rationale for its selection.*

Response: We recognise that a direct reference for the specific 30% threshold is difficult to identify, as this value is not standardised in the literature. We have therefore revised the justification to make the empirical rationale more explicit rather than citing a precedent that does not directly apply. The revised sentence now reads: 'A minimum forest fraction of 30% was applied to exclude pixels dominated by non-forest land cover while retaining partially forested cells representative of Europe's fragmented forest landscapes. This threshold was chosen to balance the exclusion of predominantly non-forested pixels against retention of the mixed land cover conditions typical of European agricultural-forest mosaics.'

Comment 10: *We believe that the ecological meaning of "structural heterogeneity" should be explained in greater detail. Structural heterogeneity was calculated within hexagons of 100 km in diameter, an extent that is likely to encompass multiple forest types, stands, and even non-forest land covers. We therefore encourage the authors to clarify the ecological interpretation of this variable and discuss how its spatial scale relates to the mechanisms proposed in the Discussion.*

Response: Thank you for highlighting this important issue. To address this concern, we have implemented two specific changes. First, we added a clarifying sentence to Section 2.6 [L. 218-221]: "It is important to note that CV computed at the 100 km hexagon scale reflects landscape-level variability in biomass across multiple stands and forest types, rather than within-stand

structural complexity. It should therefore be interpreted as a proxy for the degree of structural uniformity across the disturbance-prone landscape mosaic, rather than a direct measure of stand-level heterogeneity." Second, we revised the relevant paragraph in Section 4 to explicitly acknowledge the scale mismatch: "While the mechanisms proposed here (i.e., host connectivity, microclimatic buffering, and predator refugia) operate at the forest-stand scale, our CV metric captures structural variability at the landscape scale (100 km hexagons). The declining CV we observe is therefore best interpreted as a landscape-level signal of increasing structural uniformity across the disturbance-active forest mosaic, which is consistent with, but not a direct measure of, the stand-level homogeneity that facilitates bark beetle spread."

Additional update not requested by the referee. During the revision period, Kowalski et al. (2026, Nature Geoscience) published a continental assessment of aboveground biomass losses from forest disturbances across Europe, reporting increased sensitivity of biomass loss to disturbed area in temperate forests after 2018. Given the direct relevance to our findings, we have cited this work in the Introduction to help frame the knowledge gap addressed by our study, and in the Discussion to provide independent corroboration of the decoupling between disturbance area and carbon loss that our structural analysis explains mechanistically. We have also refined our novelty statement in the Abstract to clarify that our contribution lies in resolving the structural basis of this susceptibility, rather than merely identifying the susceptibility itself.