

**Dear Editor,**

Thank you for the opportunity to revise our manuscript and for managing the review process. We would like to express our sincere gratitude to you and the Reviewers for your time and constructive feedback provided.

We have carefully addressed the concerns raised and have incorporated substantial revisions to improve the quality and clarity of the work. Where we have diverged from the Reviewers' recommendations, we have provided a detailed justification in our point-by-point responses below.

To address the comments from both Referees, we have thoroughly revised our discussion. We have restructured Section 4.2 and divided it into subsections. We have removed detailed information that is not crucial to the core message of our study and added broader context for the observed ecosystem shift.

In addition, we have carefully checked the manuscript for cited references, ensured the completeness of the reference list, and corrected any typographical or linguistic errors. We have revised the document's formatting where necessary.

Once again, we thank the Reviewers for their thoughtful recommendations, which have been instrumental in refining our work. We believe the revised manuscript is significantly improved and addresses all previous concerns in full.

We appreciate your editorial consideration and look forward to your response.

Sincerely,  
Ewa Zin

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**Dear Reviewer 1, prof. Normunds Stivrins,**

Thank you very much for your positive and encouraging feedback. We truly appreciate your thorough review and your commendation of our interdisciplinary approach and the overall quality of the study. We are grateful for your constructive criticism, which has been very helpful and has certainly assisted us in improving our manuscript. Please find our responses to your comments below.

**Specific comments**

**P1, line 28-52: The abstract is informative, but some repetition could be reduced (e.g., multi-proxy reconstruction, biotic and abiotic factors etc). A brief final sentence highlighting implications for restoration or conservation would strengthen the abstract's applied relevance.**

Thank you for this relevant comment. We fully agree that underscoring the implications of our study for the conservation and restoration of peatland ecosystems is essential. To address the feedback from both Referees, we have thoroughly revised the abstract to eliminate repetition, increase the precision of the reported results and conclusions, and better emphasise our key findings. Finally, we have added a concluding sentence to the abstract to summarise the results and highlight the applied relevance of our findings.

## Introduction

**p.3, line 120-134: The link between human impacts and the interdisciplinary approach could be made more explicit “to address these long-term human-environment interactions, we combine palaeoecological, dendrochronological, and historical archives ...”**

Thank you for this valuable suggestion. To incorporate it, we have rephrased the final part of our introduction as follows:

“A long-term perspective is particularly vital for peatland ecosystems, as understanding their past and present dynamics is essential for predicting future development, including responses to climate change, vegetation shifts, carbon sequestration potential, and restoration and conservation needs (Lindbladh et al., 2013; Marcisz et al., 2022). To address the aforementioned long-term human–environment interactions (e.g., Joosten, 2016; Bąk et al., 2024), we applied an interdisciplinary approach, combining palaeoecological, dendrochronological, meteorological, and historical data to explore the linkages between tree growth, hydrology, climate, and possible human impact in a peatland located in one of the less studied regions of Central Europe: southeastern Poland (Edvardsson et al., 2022). Notably, a unique feature of Poland is that, following the geopolitical changes of the late 18th century, different parts of its territory were incorporated into the distinct economic and administrative systems of the neighbouring Enlightenment-era monarchies: Prussia, Russia, and Austria (Davies, 2005; Lukowski and Zawadzki, 2006). This significantly influenced local land management and, consequently, the landscape of the Polish lands, including forests and peatlands (Broda, 2000; Jaszczak, 2008a, b, c; Bąk et al., 2024; Przybylski et al., 2025).

Given the complexities of peatland ecology and functioning (e.g., Waddington et al., 2015; Edvardsson et al., 2019; Marcisz et al., 2022) and their global and regional vulnerability (e.g., Joosten, 2016; Jassey et al., 2018; Manton et al., 2025), we hypothesized that the peatland ecosystem under study has experienced substantial structural and functional changes, likely including disturbances, throughout its development history, driven not only by natural abiotic factors but also by pronounced human impact. Therefore, in this study we aimed to (1) reconstruct long-term ecosystem dynamics by integrating diverse proxy records from both natural and human archives, (2) assess peatland ecosystem stability, and (3) evaluate the underlying drivers of ecosystem transformation, including anthropogenic influence.” (L. 150–168)

## Materials and Methods

**p.10-12:**

**(1) the terminology describing peat decomposition should follow common usage. My suggestion is to use “highly decomposed” instead of “heavily decomposed”, and “poorly decomposed” instead of “weakly decomposed”. Please consider this throughout the manuscript.**

We have made the necessary modifications throughout the manuscript.

**(2) Clarify the reason for selecting a 50 cm peat core (e.g., focus on recent centuries or sampling limitations).**

Our peat sample represented the peat depth at the sampling point, which was selected to be relatively close (<500 m) to our sample trees, representing an old-growth Scots pine population

providing a long tree ring chronology. We have added this information to the description of our sampling points in the Material and methods section by rephrasing the text as follows:

“Our sampling site was selected based on the following specific criteria: (1) the presence of old-growth tree populations (>180 years), providing a long tree ring chronology, and (2) location within a peatland area, allowing peat sampling in close proximity (<500 m) to the sampled trees.” (L. 197–199)

**p.11: radiocarbon dating including Table 1: The table indicates “pollen (extracted)” samples for 14C. Please specify how pollen was extracted for dating (chemical isolation, manual picking etc) and how much material was used. These details are important for assessing the reliability of pollen-based radiocarbon ages.**

The pollen extract was prepared according to the procedure specified in the Oxford Long-Term Ecology Laboratory’s “Pollen preparation procedure for radiocarbon dating” protocol, based on Brown et al. (1989). Pollen was extracted from 2 cm<sup>3</sup> of sediment. We have added these details in the Materials and methods section (L. 352–354), including Table 1, along with the corresponding reference (Brown et al., 1989).

**p.11, section 2.4.2.: The methods mention using Lycopodium tablets to estimate palynomorph concentrations, yet no pollen concentration results are reported later. If these were not used, please clarify and rephrase the methodology accordingly. If data exist, consider presenting absolute pollen concentrations for pine and alder, as these could potentially provide additional insights into vegetation productivity changes.**

Thank you for this important comment. Concentration was necessary to calculate the annual influx, which was presented in the case of microcharcoal. For clarity, we have rephrased the description of our methodology accordingly (L. 375–376).

**Possible term improvement: in figure captions and text, replace “charcoal grains” with “charcoal particles”. Also, use “charcoal” in singular form instead of “charcoals” (current version in some pages and figures).**

We have made the necessary modifications throughout the manuscript.

## **Results**

**p.18, line 511-518: The authors interpret a hiatus at ~37 cm based on a lithological change and supposed charcoal enrichment. However, the macroscopic charcoal record (Fig. 8) shows low charcoal concentration at this depth, with the main peak around 18 cm. This does not support the statement about high charcoal content at 37 cm.**

**Considering both the lithological change and the age-depth model, this interval likely reflects a shift in sedimentation and decomposition processes rather than a true hiatus. The most probable explanation is a period of reduced accumulation under drier conditions, consistent with the climate context described in Section 4.1.2. Such transitions are typical in peatlands when moisture regimes change from drier to wetter phases. My suggestion is to rewrite this part to describe it as a “transition in sedimentation regime” rather than a depositional hiatus.**

Thank you for this insightful comment. We have carefully considered this point during revision. We agree that the charcoal distribution alone (with higher values above the boundary) is not sufficient to

argue for a fire-driven depositional break. However, we would like to retain the interpretation of a hiatus in peat accumulation (i.e., a period of strongly reduced or absent net accumulation, potentially including oxidation/loss of previously formed peat), because this is best supported by our chronological evidence.

Specifically, the two radiocarbon ages that bracket the transition indicate an interval of ~400 years with no preserved accumulation between the underlying, highly decomposed massive peat and the onset of the overlying *Sphagnum* peat. In our age–depth framework this appears as an abrupt step rather than a gradual change in accumulation rate, which is more consistent with a stratigraphic gap (non-deposition and/or loss of material) than with simply slower peat growth. We therefore interpret the boundary as a real interruption in net peat accumulation, followed by a marked qualitative shift to *Sphagnum*-dominated peat formation beginning around ca. 1830.

At the same time, we acknowledge that an alternative (and not mutually exclusive) explanation is that peat originally deposited between the two dated horizons was subsequently degraded and lost (e.g., through oxidation/decomposition during lowered water tables), producing an apparent gap in the preserved stratigraphy.

In this context, the charcoal peak above the boundary can be understood as reflecting burning during or after the re-initiation of peat accumulation, rather than at the exact boundary itself. This is also consistent with the independent tree ring evidence of a disturbance in the early 19th century. First, we have clear evidence of fire in 1830 within our study site (in situ), recorded by our sample trees (Fig. 4; L. 831–834), which were located within 500 m of the peat sediment sampling point; one tree sampled near the peat coring location shows a fire scar dated to 1830. Second, the presence of young pine regeneration may also indicate a disturbance that opened the landscape at that time. Together, this suggests a disturbance close to the time when *Sphagnum* peat accumulation started.

We have revised the text in the Results section (3.3.1 Age–depth model and sediment composition; L. 549–550) to make clear that the hiatus interpretation is primarily based on radiocarbon bracketing (the missing time interval) and the abrupt shift in peat type. In our interpretation, charcoal is treated as supporting contextual evidence for disturbance timing rather (L. 838–840) than the sole basis for identifying the hiatus.

For chronological control of the transition, we intentionally selected two radiocarbon samples: (1) from the highly decomposed, massive peat, and (2) from the overlying *Sphagnum* peat above. This bracketing strategy was designed to constrain the timing of the end of the former state and the onset of the new *Sphagnum*-dominated phase. In contrast, dating charcoal from the transition horizon would primarily provide the age of the burned wood (potentially including inbuilt age and reworking), and would not reliably constrain the timing of the fire event nor the ecosystem shift, often with a large uncertainty. We have clarified this rationale in the revised manuscript (L. 347–352).

Regarding the climate context, changes in sedimentation regimes and peatland transitions, we have broadened our discussion by adding the following text (L. 1257–1282):

“Moreover, a high level of hydroclimatic variability throughout the entire period of our reconstruction, spanning over 2,300 years (ca. 330 BCE–2022 CE), is clearly evidenced by both our data and regional records. These document not only exceptional dry and wet years such as 1811–1812, 1813, and 1815 (Figs. 3 and S3; AS4–14; see also Section 4.1.2), but also several pluvials and droughts in Europe, including megadroughts around 1000–1200 CE, 1400–1480 CE, and 1770–1840 CE (Cook et al., 2015, 2022; Büntgen et al., 2011, 2021a). Notably, the latter two overlap with the period during which the ecosystem transition occurred (ca. 1400–1830 CE) (Fig. 10). Additionally, existing

data do not indicate that this period was drier than the modern era of anthropogenic climate change (Cook et al., 2015, 2022; Büntgen et al., 2011, 2021a). If climate had been the primary driver of the documented peatland transformation, we would expect this to have happened substantially earlier during the Late Holocene, namely around 4000–2000 years ago, as elsewhere in the region (Lamentowicz et al., 2015; Väiliranta et al., 2017; Stivrins et al., 2025). Consequently, we suggest that factors beyond climate must have played a significant role in the ecosystem shift we documented.

In addition to external forcing, autogenic processes play a crucial role in peatland development (Belyea and Baird, 2006; Waddington et al., 2015), with various successional pathways possible (e.g., Marek, 1965; Väiliranta et al., 2017; Blaus et al., 2020). However, the transition from a base-rich, minerotrophic fen to an acidic, ombrotrophic bog (cf. Wheeler and Proctor, 2000) is a common and widely reported trajectory. Despite the high decomposition rate and generally slow peat accumulation during the fen stage, with values of 0.05–0.4 cm yr<sup>-1</sup> reported from Poland, Lithuania, Latvia, and Ukraine (Lamentowicz et al., 2007; Mažeika et al., 2009; Dobrowolski et al., 2010; Stivrins et al., 2025), the gradual thickening of the peat layer reduces the influence of mineral-rich groundwater, thereby decreasing available nutrients and surface pH. This lowers the decomposition rate, further accelerates peat accumulation, and eventually eliminates the impact of groundwater, resulting in full ombrotrophy (Lamentowicz et al., 2007; Väiliranta et al., 2017). Notably, alder carr (i.e., wooded fen) is identified as a possible successional stage within the fen–bog transition (Marek, 1965). In temperate–boreal peatlands across the Baltic region and Finland, low fen peat accumulation rates and transitions in sedimentation regimes and peatland types have been broadly documented, often highlighting the complex interplay of autogenic and allogenic processes, including climatic variability, as key drivers (Lamentowicz et al., 2007; Hiiemaa et al., 2014; Väiliranta et al., 2017; Stivrins et al., 2025). Therefore, neither climate nor autogenic succession can be omitted from the factors involved in the observed ecosystem change.”

## Discussion

**Throughout the manuscript, the transition from *Alnus*-dominated den to *Pinus*-dominated bog is strongly attributed to human activity. While these drivers may have played an important role, the evidence presented also suggests that natural hydroclimatic factors and autogenic peatland processes could have contributed significantly to this transformation. For instance, the transition coincides with a period of dry climatic conditions (Section 4.1.2) and with signs of increased peat decomposition and reduced peat accumulation rate, which could result from lower effective moisture and gradual peat surface elevation. Such processes are common in fen-bog succession even without direct anthropogenic disturbance. I would recommend acknowledging also that observed vegetation shift likely reflects a combination of natural climatic variability and human influence, rather than being entirely human-driven phenomena. This would make the discussion more balanced and ecologically realistic. In addition, then I would suggest modifying the title: “From *Alnus* to *Pinus*: natural and human drivers of temperate peatland transformation”.**

Thank you for this thoughtful and constructive suggestion. We agree that attributing the *Alnus*-to-*Pinus* transition too strongly to human activity may give an unbalanced perspective. While our data and historical context indicate that human influence likely contributed to the transformation, we acknowledge that natural hydroclimatic variability and autogenic peatland processes could also have played an important role. In particular, the timing overlaps with the drier climatic conditions discussed in Section 4.1.2 and with evidence for increased decomposition and reduced apparent peat accumulation, which can be consistent with reduced effective moisture and internal peatland

dynamics typical of fen–bog succession. Therefore, we have thoroughly revised the Discussion section (particularly L. 1246–1293) to frame the vegetation shift more explicitly as the outcome of interacting natural (climatic and autogenic) and anthropogenic drivers.

We also sincerely appreciate your suggestion to modify the title to better reflect this multi-driver perspective. After careful consideration, however, we have decided to retain our original title, as we believe it best captures the main focus and narrative of the manuscript and remains consistent with the overall structure and aims of the study. To address your important point, we have instead strengthened the wording throughout the discussion (and where relevant in the abstract) to ensure that the balanced interpretation is clearly communicated in the manuscript itself.

**Section 4.1.2. (Transition period): The authors themselves note that this interval corresponds to dry climatic conditions, which supports the interpretation of enhanced decomposition and slower accumulation rather than a hiatus. Please make this link more explicit in the text.**

As we explained in our earlier response (above), our data confirm drier conditions in the early 19th century, although high hydroclimatic variability persisted (Figs. 3 and S3). Due to the substantial hydroclimatic variability recorded in Europe throughout the period of our reconstruction, it is unlikely that climate alone explain the gap in our peat archive. Therefore, we argue that non-climatic factors must have played a significant role alongside these documented climate shifts. For clarity, we have revised the Discussion to more explicitly characterise the ecosystem shift as a response to the complex interplay between anthropogenic influences and natural climatic or autogenic processes (particularly Section 4.2).

**In addition, the observation that peat accumulation was low during the *Alnus* carr stage is fully consistent with fen hydrology, where fluctuating water tables lead to variable aeration and high decomposition of biomass. Similar low fen peat accumulation rates and shifts in sedimentation rates and peatland types have been recorded also in Lithuania, Latvia, Estonia and Finland. Consider explicitly mentioning this as a natural fen characteristic rather than a sign of disturbance.**

Thank you for this comment. We agree with the main point: low peat accumulation during the *Alnus* carr (fen) stage can be fully consistent with natural fen hydrology. In systems with fluctuating water tables, periodic aeration commonly enhances organic matter decomposition and can reduce net peat accumulation even in the absence of direct disturbance. We have revised the text to make this clearer (L. 714–717; L. 1271–1273) and to avoid implying that low accumulation rates during the *Alnus* stage necessarily indicate anthropogenic impact.

At the same time, our interpretation is that the observed transformation reflects a synergy of drivers rather than a single cause. We consider that hydroclimatic variability (including the drier conditions described in Section 4.1.2) and autogenic peatland development provided a natural trajectory and vulnerability, while human activity likely contributed additional pressure. In our view, these interacting factors culminated in a threshold response (“tipping point”), leading to a rapid reorganisation of ecosystem state from *Alnus*-dominated carr to a *Pinus*-dominated bog. We have adjusted the Discussion section to add broader context for the observed ecosystem shift and to distinguish background fen characteristics from the factors associated with the ecosystem turning point (L. 1246–1293).

Following your suggestion, to support broader, ecologically realistic framing, we have added brief context (and respective references) noting that similarly low fen peat accumulation rates and

transitions in sedimentation regimes and peatland types have been documented in temperate–boreal peatlands across the Baltic region and Finland (L. 1278–1281).

**Line 778-785: Above other factors, authors list also peat mining. Please clarify whether peat extraction actually occurred at the study site. Considering thin layer of peat at sampling site, it seems that peat mining probably was not economically feasible. If peat mining (and other mentioned factors within these lines) was not done at the study site, please revise text and include only relevant factors such as drainage, forest management, fire, and natural hydroclimatic variability.**

We fully agree that our original sentence may be misleading, as there is no evidence of peat mining at our study site. Our intention was to list various possible factors known from the literature that could have triggered cohort tree regeneration in a peatland. We agree with the comment. We have revised the text by removing “peat mining (Freléchoux et al., 2000)” (L. 796).

### **Figures and table**

**Overall, figures are nice and well contribute the main text. One possible suggestion, if possible, please enlarge slightly text in Figs. 5-6 for readability.**

We have corrected this accordingly. Similarly, we have modified Figure S5 in the Supplement.

**Ensure figure captions reflect correct terminology (e.g., charcoal particles).**

We have revised figure captions throughout the manuscript to ensure they use correct terminology.

### References:

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**Dear Reviewer 2, prof. Katarzyna Marcisz,**

Thank you very much for your thoughtful and constructive feedback on the manuscript. We greatly appreciate your recognition of our multi-proxy approach and our contribution to understanding long-term peatland dynamics in a relatively unexplored area in this regard. We value your comments and have addressed them carefully. Please find our responses below.

**Specific comments:**

**Title “From *Alnus* to *Pinus*: [...]” – in its current form the title suggests that *Alnus* and *Pinus* were the sole species in this ecosystem. Maybe it would be better to change this to something like: “Transition from alder- to pine-dominated forest: [...]”?**

Thank you for your comment. Our aim was to keep the first part of the title concise to attract potential readers. Therefore, more detailed information is provided in the second part of the title and later in the manuscript. We fully agree that alder and pine were likely not the only tree species present in the communities we discussed, especially in transition zones towards locally different microsites where a more diverse dendroflora should be expected (Faliński, 1986; Leuschner and Ellenberg, 2017). However, black alder (*Alnus glutinosa*) and Scots pine (*Pinus sylvestris*) are both major, defining components of both the alder carr (black alder bog forest, *Carici elongatae-Alnetum*) and the Scots pine bog forest (*Vaccinio uliginosi-Pinetum*) communities, with overriding dominance over

other tree taxa, which are largely outcompeted in these very specific habitats (Matuszkiewicz, 2007; Leuschner and Ellenberg, 2017). This is also reflected in our data, with distinct differences in both pollen percentages and the proportion of plant macrofossil remains of *Alnus* and *Pinus* between the lower and upper parts of the peat profile (Figs. 8 and 9). Therefore, we consider referring to *Alnus* and *Pinus* in the title of our study to be entirely justified and have kept the title unchanged.

**Lines 47-52: these conclusions are very vague; a bit more specific information would be useful here. I would also suggest to add a summary sentence at the end of the abstract to recap the results.**

We appreciate this important comment. To address the feedback from both Referees, we have thoroughly revised the abstract to eliminate repetition, increase the precision of the reported results and conclusions, and better emphasise our key findings. Finally, we have added a concluding sentence to the abstract to summarise the results and highlight the applied relevance of our findings.

**Line 153 – is Solska Forest marked properly on the map in Fig. 1a? In the figure it looks like it is located further than 30 km from the border with Ukraine.**

Thank you for this valuable remark. In the description of our study area, we stated that the Wielkie Bagno peatland is located approx. 30 km from the border with Ukraine and that the Solska Forest is a large forest area stretching from the Vistula River in the west to the border with Ukraine in the east (50°48'N, 21°56'E–50°13'N, 23°26'E), which is approx. 150 km. The point shown in Figure 1a indicates the approximate centre of the Solska Forest (near Biłgoraj, about 50–60 km from the state border), not the Wielkie Bagno itself. To avoid confusion, we have removed the information about the 30 km distance from the border with Ukraine and rephrased the text (L. 173–175) as follows: “Wielkie Bagno (Eng. Great Swamp) peatland is located near the town of Biłgoraj in the Solska Forest (50°31'N, 22°50'E) in south-eastern Poland, which is a large forest area covering over 1,400 km<sup>2</sup> in the Biłgoraj Plain, stretching from the Vistula River in the west to the border with Ukraine in the east (50°48'N, 21°56'E–50°13'N, 23°26'E).”

**Line 156 – I do not think this region is included in the globally important biodiversity hotspots list <https://www.nature.com/articles/35002501.pdf> ; please rephrase.**

We acknowledge this comment and agree that our original wording was not justified. To improve clarity, we have rephrased the text, replacing “globally” with “regionally” (L. 176).

**Figure 1 – on the map we can see two peatlands highlighted – Wielkie Bagno and Rakowe, while the manuscript is focused only on Wielkie Bagno. In the text it is stated that Rakowe is another wetland located in the vicinity of Wielkie Bagno (Lines 892 and 898), whereas later on (lines 1067-1068) authors write that this is another name for Wielkie Bagno. Please clarify.**

Thank you for this important comment. Historically, the name Rakowe Bagno (or Rakówka Bagno) referred to a larger area, which included not only our study site, now called Wielkie Bagno, but also extensive surrounding areas, mainly to the south, including meadows and the heavily drained Rakowe peatland (Fig. 1). The historical name Rakowe likely originates from a small river that begins in the area of the present Wielkie Bagno peatland. We agree that the information about the historical name of our study site from the late 18th century may cause confusion. As this information is not essential to the discussion, and to improve clarity and conciseness, we have revised the text by removing: “, the Wielkie Bagno peatland, known in the late 18th century as Rakowe Bagno or Rakówka Bagno (Eng. *Rakowe Swamp* / *Rakówka Swamp*),” (L. 1078).

**I also think Fig. 1 could be enlarged – the details on maps 1i-e are not well visible.**

Thank you for this remark. We agree that the details on maps 1i–e were not clearly visible at the initial figure size when the first part of the caption appeared on the same manuscript page. We have addressed this by enlarging Figure 1 to full page size. However, we must acknowledge that the final size of Figure 1 will ultimately be determined during the technical editing process, only after the manuscript is accepted.

**Line 330 – please explain what methodology was used for pollen radiocarbon dating.**

The pollen extract was prepared according to the procedure specified in the Oxford Long-Term Ecology Laboratory's "Pollen preparation procedure for radiocarbon dating" protocol, based on Brown et al. (1989). Pollen was extracted from 2 cm<sup>3</sup> of sediment. We have added these details in the Materials and methods section (L. 352–354), including Table 1, along with the corresponding reference (Brown et al., 1989).

**Lines 381-384 – test construction and mixotrophs sum – it is interesting to look at functional traits of testate amoebae, however, this information is included in the Figure 11 but it is not discussed later on in the manuscript. Can you include the interpretative value of traits in the description of results and discussion?**

Thank you for raising this important point. We have expanded the details in both the Results (Section 3.3.6 Hydrology reconstruction – testate amoebae data) and the Discussion section by adding the following text:

"The proportion of mixotrophic testate amoebae ranged from 1 to 15% and showed a decreasing trend." (L. 661–662)

"A pronounced decline in mixotrophic testate amoebae was recorded, with their abundance becoming nearly absent." (L. 669–670)

"Towards the end of the zone, an increase in mixotrophic testate amoebae was observed to 4%." (L. 676–677)

"Furthermore, the decline and near disappearance of mixotrophic taxa in the second half of the 20th century (Fig. 11) can be linked to drying conditions and environmental disturbances (Marcisz et al., 2016; Zhang et al., 2020)." (L. 984–986)

"An increase in mixotrophic testate amoebae in the upper part of the record (Fig. 11) may indicate a return to more stable and wetter conditions (Marcisz et al., 2020; Łuców et al., 2022)." (L. 1022–1024)

**Lines 393-395 – what is the potential distance of fires from the studied site? Do you have any estimates? Please add this info to the text as it is important for interpretation.**

Thank you for this important remark. For clarity, we have revised the text as follows:

"Subdividing macrocharcoal size classes can provide information on the potential distance of fires from the studied site (e.g. Clark, 1988; Vanni re et al., 2008; Conedera et al., 2009). While macroscopic charcoal particles are generally interpreted as a proxy for local fires, occurring within a few hundred metres (or as close as 1 m) of the sampling point (e.g. Ohlson and Tryterud, 2000; Higuera et al., 2005; Tinner et al., 2006; Conedera et al., 2009), they can also reflect a more distant,

regional fire signal. This is due to the complex patterns of charcoal deposition and transport, which are influenced by fuel type, fire intensity, burn area, and meteorological conditions (e.g. Tinner et al., 2006; Peters and Higuera, 2007; Oris et al., 2014; Vachula et al., 2023).” (L. 421–427)

**Figure 2 – I would suggest changing the figure a bit – put first data and later data resolution, than the graph.**

Thank you for your comment. As the reading direction is from left to right, placing “Data” in the leftmost panel is indeed a very good idea, following the logic from general to detail. When designing the figure, we placed the graph in the centre to highlight the importance of the information it presents, namely the different time periods covered and the continuity versus discontinuity of the data. In addition, this design maintains graphical balance between the central graph and the two text panels symmetrically positioned on either side. Therefore, we prefer to keep our original concept. However, we have modified the figure by switching the text panels – “Data” in the left panel and “Data resolution” in the right panel.

**Chapter 3.1.1 – I do not understand why the comparison of meteorological data from two stations is important here? Why not use the data from a nearer located station rather than from two stations?**

As explained in the Materials and methods section (2.2 Climate data), we used meteorological data from two stations for the following reasons: (i) to assess long-term climate fluctuations in the region and long-term climate-tree growth relationships at the study site over the longest possible period covered by instrumental data (>200 years, meteorological station in Kraków); and (ii) to verify the representativeness of this long-term data set (Kraków) for our study area by comparing it with a substantially shorter data set from the nearest weather station to our study site (>60 years, meteorological station in Tomaszów Lubelski). We believe the information provided in the Materials and methods section (2.2 Climate data) explains this clearly. Therefore, we prefer not to expand this section substantially. However, as a compromise, we have revised the first sentence of the section as follows: “To assess long-term climate fluctuations in the region and the climate–tree growth relationships at the study site over the longest period covered by available instrumental data, average monthly, seasonal, and annual values of air temperature (1792–2020) and atmospheric precipitation (1811–2020) from the meteorological station of the Department of Climatology at Jagiellonian University in Kraków were used.” (L. 260–263).

**Figures 3, 5 and 6 – are all these needed in the main body of the text? Maybe some can be moved to supplement as there is a lot of figures in the manuscript already.**

Thank you for this comment. We agree that the number of figures in the manuscript is high. However, Figures 3, 5, and 6 present a substantial share of our key results, achieved through analyses of the meteorological and tree ring data. As this is an interdisciplinary study aiming to combine different archives and data types, maintaining a balance in presenting the results was our primary goal. Removing the figures listed above would distort this balance by overrepresenting the illustration of the palaeoecological results. Therefore, we have decided to keep Figures 3, 5, and 6 in the main body of the text.

**Lines 458-459 – can you mark these years on Fig. 4? It will be helpful to see it presented together with chronology.**

Thank you for your comment. We aimed to correct Figure 4 accordingly; however, this appeared untenable for several reasons, explained below.

During the analysis of our tree ring data, we determined pointer years using various methods such as normalisation in a moving window (Cropper, Neuwirth), Relative Growth Change, Interval Trend, z-transformation, bias-adjusted Standardised Growth Change (for two periods: 1777–2020 and 1831–2020) (van der Maaten-Theunissen et al., 2015, 2021; Buras et al., 2020, 2022), and the list method (Yamaguchi, 1991). As is commonly the case (Jetschke et al., 2019, 2023), different methods yielded inconsistent results in the pointer year analysis (Fig. SF1, below). Because this analysis was not crucial to the main questions addressed in our study, already broad and rich in the results presented and discussed, we decided not to include it in the manuscript.

Instead, we listed in the text only those pointer years also identified in other studies from the region (Cedro and Lamentowicz, 2008; Dauškane et al., 2011; Edvardsson et al., 2015, 2019), or in our unpublished data from the Białowieża Forest peatland sites, as we considered this relevant for some potential readers as an indication of regional growth synchrony of Scots pine in peatlands.

We followed your suggestion and prepared a revised version of Figure 4, showing the pointer years listed in the text (Fig. SF2, below). However, we are not fully satisfied with the result, as it does not depict all pointer years of our site chronology, which we determined using different methods (Fig. SF1, below). Therefore, for clarity and conciseness, we decided to keep both Figure 4 and our original text referring to pointer years (L. 488–491) unchanged.

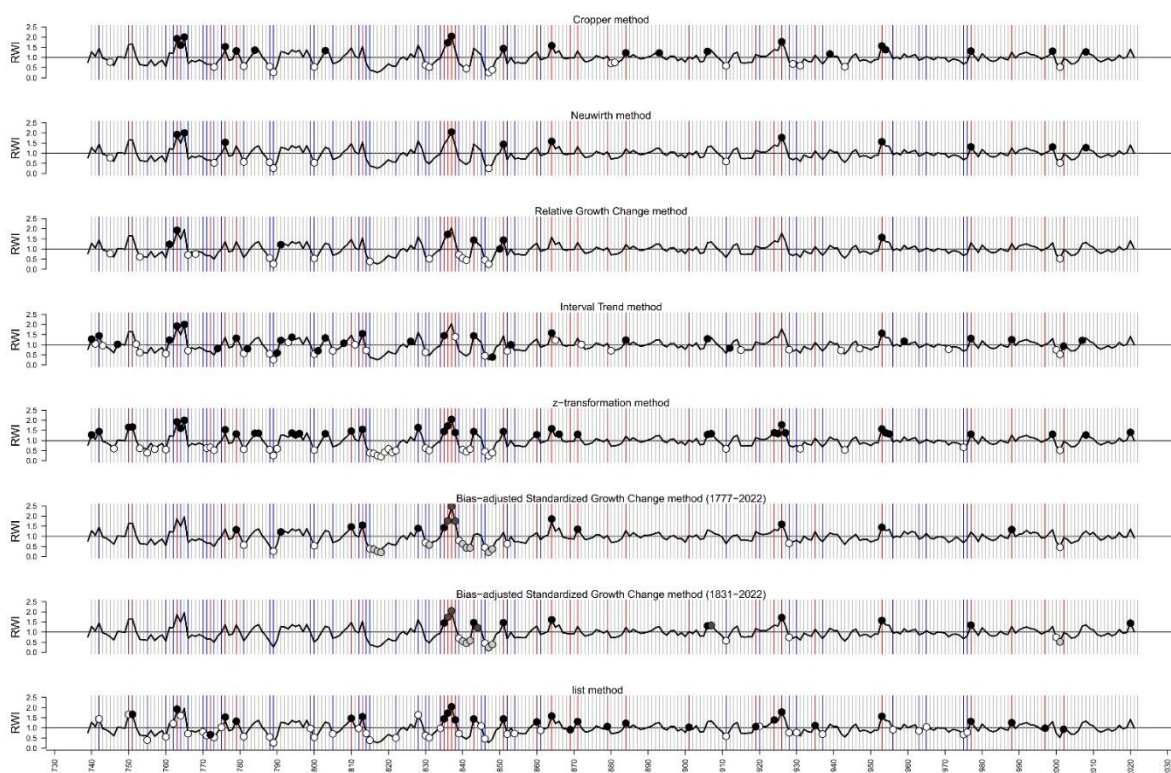


Fig. SF1. Pointer years of Scots pine determined by different methods: Normalisation in a moving window (Cropper, Neuwirth), Relative Growth Change, Interval Trend, Z-transformation, Bias-adjusted Standardised Growth Change (for two periods: 1777–2020 and 1831–2020) (van der Maaten-Theunissen et al., 2015, 2021; Buras et al., 2020, 2022), and the list method (Yamaguchi, 1991). The following parameters were applied: for Cropper and Neuwirth, a 13-year moving window was used to calculate local growth deviations, with a series threshold requiring 75% of trees to exhibit an event year; Neuwirth further applied thresholds of 1, 1.28, and 1.645 to identify weak, strong, and extreme events. Relative Growth Change (RGC) was determined by comparing each year to the 4 preceding years, defining a pointer year when at least 75% of trees showed a growth change exceeding +60% or -40%. The Interval Trend method identified years where a minimum of 95% of trees displayed a consistent upward or downward growth

trend, while Z-transformation defined pointer years based on a standard deviation (z-score) threshold of 1. Black (white) circles denote positive (negative) pointer years determined by the respective method. Red (blue) lines denote all positive (negative) pointer years determined by different methods.

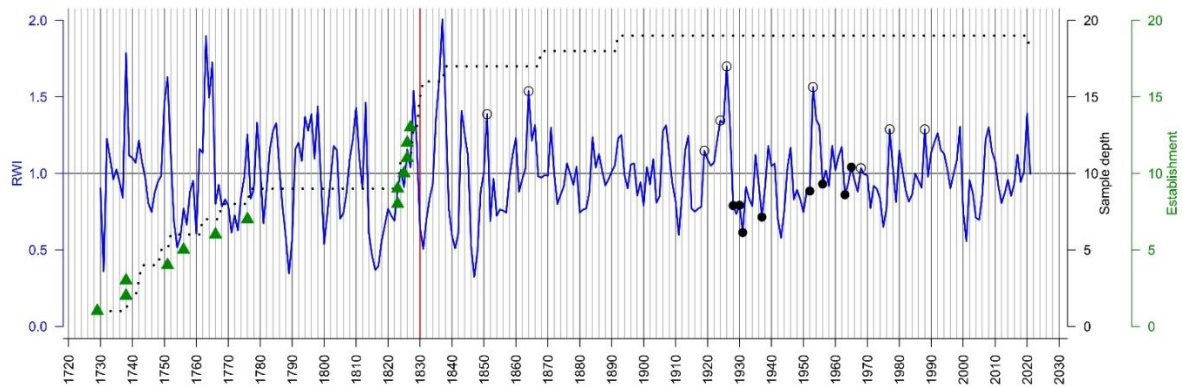


Fig. SF2. Scots pine (*Pinus sylvestris* L.) chronology (blue line), selected pointer years aligning with regional data (Cedro and Lamentowicz, 2008; Dauškanė et al., 2011; Edvardsson et al., 2015, 2019) (circles), and tree recruitment dates (green triangles) of living peatland trees in Wielkie Bagno, Solska Forest. RWI – ring width index. Sample depth is denoted by a dotted line, fire date recorded in the tree ring material by red vertical line, and positive (negative) pointer years by open black (filled black) circles.

**Figures 8, 9 and 11 – the text is too small, please enlarge the font.**

We have made every effort to improve the readability of Figures 8, 9, and 11. However, for Figure 11, due to the high taxonomic density and the small proportions represented, increasing the text size caused extensive overlap of names. To maintain visual clarity and ensure readers can accurately associate names with their respective data points, we have retained the original font size, which we found to offer the most legible balance.

**The hiatus – is it really a hiatus? Charcoal is present above the boundary, not at the boundary level or below. Maybe it is an effect of decrease in peat accumulation rates or high decomposition rates at that time? Why was charcoal not used for dating? As the main line of the interpretation is that the hiatus is an effect of fire, dating charcoal would be a better approach. As charcoal counts are not very high in the hiatus horizon, the amount of charcoal is increasing several layers above – maybe it would be worth considering other line of interpretation, e.g., not presence of the hiatus, but a decrease in peat accumulation due to dry conditions in the peatland? This interpretation could be supported by testate amoeba data and the lack of tests below the transition zone.**

Thank you for this comment. We agree that the charcoal distribution alone (with higher values above the boundary) is not sufficient to argue for a fire-driven depositional break. However, we would like to retain the interpretation of a hiatus in peat accumulation (i.e., a period of strongly reduced or absent net accumulation, potentially including oxidation/loss of previously formed peat), because this is best supported by our chronological evidence.

Specifically, the two radiocarbon ages that bracket the transition indicate an interval of ~400 years with no preserved accumulation between the underlying, highly decomposed massive peat and the onset of the overlying *Sphagnum* peat. In our age–depth framework this appears as an abrupt step rather than a gradual change in accumulation rate, which is more consistent with a stratigraphic gap (non-deposition and/or loss of material) than with simply slower peat growth. We therefore interpret

the boundary as a real interruption in net peat accumulation, followed by a marked qualitative shift to *Sphagnum*-dominated peat formation beginning around ca. 1830.

At the same time, we acknowledge that an alternative (and not mutually exclusive) explanation is that peat originally deposited between the two dated horizons was subsequently degraded and lost (e.g., through oxidation/decomposition during lowered water tables), producing an apparent gap in the preserved stratigraphy.

In this context, the charcoal peak occurring above the boundary can be understood as reflecting burning during or after the re-initiation of peat accumulation, rather than at the exact boundary itself. This is also consistent with the independent tree ring evidence of a fire scar (and post-fire growth reaction) dated to 1830 and the local pine regeneration signal, which together suggest a disturbance close to the time when *Sphagnum* peat accumulation started (and landscape openness increased). We have revised the text to make clear that the hiatus interpretation is primarily based on the radiocarbon bracketing (the missing time interval) and the abrupt shift in peat type (L. 549–550). In our interpretation, charcoal is treated as supporting contextual evidence for disturbance timing rather (L. 838–840) than the sole basis for identifying the hiatus.

Regarding why charcoal was not used for dating: we intentionally focused on dating the peat immediately below and above the boundary to constrain the end of the former depositional state and the onset of the new *Sphagnum* accumulation regime. Dating charcoal from around the transition would mainly provide the age of the burned wood (with potential inbuilt age and/or reworking), and would not necessarily constrain either the duration of the accumulation break or the timing of the ecosystem shift with the same reliability. For clarity, we have explained this rationale in the revised manuscript (L. 347–352).

Finally, we agree that dry conditions and enhanced decomposition may have contributed to the accumulation interruption, as discussed in our study (L. 843–845). For clarity of our interpretation, we have strengthened the linkage to the proxy evidence, including the testate amoeba pattern and preservation below the transition (L. 716–718). Moreover, we have broadened our discussion by developing the part discussing the possible climate impact and changes in sedimentation regimes and peatland types (L. 1246–1282).

**Line 674 – was there now peat below the sampled core? Did authors reach the bottom of the entire peat deposit in this sampling location?**

Thank you for this comment. As shown in Figure 7, we reached the bottom of the entire peat deposit at our sampling location, confirming a relatively thin (50 cm) peat layer. For clarity, we have added this information to the Materials and methods section (2.4.1 Core collection, lithology, chronology and numerical analysis) as follows: “In June of 2022, a peat core with a diameter of 5 cm and a length of 50 cm was collected with an Instorf corer, reaching the bottom of the peat deposit at the sampling point (Fig. 7).” (L. 338–339).

**My main suggestion for changes is to substantially shorten the Discussion, especially sections 4.1 and 4.2. Now the Discussion covers 15 full pages, which is too long. I checked word count for this section and it is over 12 000 words – this sum makes a word limit for the entire manuscript in most of the palaeoecological journal (in fact, most journals have word limit for submissions between 8 000 and 12 000 words). Also, there are long sections stretching several pages with no sub-sections or highlights of the main message. Because of that it is hard to stay focused while reading this text. I suggest moving some of the text to supplement (less vital information that is not essential for data interpretation), and shorten**

**the remaining text. Especially the amount of historical data included in the interpretation should be shortened: 1st of all, it is too overwhelming and it is hard to focus on the text with so much data in it; 2nd of all – Biogeosciences journal is not a historical journal so in its current form the discussion is not fully in line with journal’s scope. E.g.:**

- **lines 710-855 – these are 3.5 pages of results description with lots of historical detail. In my opinion most of this text with detailed historical context should be moved to the supplement and this chapter should be max 1.5 page long. The information is interesting, but it does not really improve the interpretation. If some readers will be interested in historical detail, then they can look it up in the supplement. Otherwise, the text is suited more to historical journal and not palaeoecological one.**
- **Chapter 4.1.3 is also very long – 4.5 pages. It is hard to focus on reading as there is a lot of information that is not vital for the interpretation. Detailed descriptions can be moved to a supplement (e.g., meticulous information about the estate ownership and which administrative regions it belonged to – this is not crucial to interpret palaeodata) and only the most important events should be presented in the main manuscript.**
- **Chapter 4.2 – again, too much and too detailed historical data description.**

Thank you for raising this point. We are fully aware that the Discussion section is very long. The main reason for this is that we did not follow the commonly used approach in paleoecological studies of presenting the paleoecological results together with their interpretation (as a “Results and interpretation” section or subsection; e.g., Marcisz et al., 2015; Bąk et al., 2024). Instead, we presented these results without interpretation in the Results section (3.3 Paleoeecology, L. 541–683) and included their interpretation in the Discussion section.

Furthermore, our primary goal in designing the discussion was to address all our proxies and data collectively by combining different disciplines and data types, which were presented separately in earlier sections of the manuscript (Materials and methods, Results), and to use them together for an interdisciplinary interpretation following our multi-proxy approach, integrating human and natural archives. Combining qualitative and quantitative data is challenging, especially when dealing with different spatial and temporal data resolutions, potential biases, and data formats. We addressed this in our discussion (4.4 Challenges and opportunities in palaeoecological research: an interdisciplinary approach). Historical data, often qualitative and descriptive, are particularly challenging to integrate with natural environmental proxies. However, in interdisciplinary studies, they offer valuable context, which cannot always be condensed without losing understanding for a broad audience, not just historians.

Although “Biogeosciences” can certainly be classified as a paleoecological journal, it has a history of publishing interdisciplinary studies (e.g., Bąk et al. 2024). In addition, it invites research on “all aspects of the interactions between the biological, chemical, and physical processes in terrestrial or extraterrestrial life with the geosphere, hydrosphere, and atmosphere”, and its objective is to “cut across the boundaries of established sciences and achieve an interdisciplinary view of these interactions”, as stated in the the journal’s aim and scope. Anthropogenic impact is an essential part of “ecosystem functioning” and “the interactions between the biological, chemical, and physical processes in terrestrial life with the geosphere, hydrosphere, and atmosphere” (which are aspects and fields covered by the journal), especially considering landscape-scale drainage activities, deforestation and afforestation, and global climate change (e.g., Allen and Chapman, 2001; Bąk et al., 2024). Therefore, we find the assessment of our discussion in its current form as “not fully in line with journal’s scope” unjustified. Notably, “Biogeosciences” imposes no length restrictions on research articles that would require substantial text reduction or selection of another journal.

We consider the suggestion to move some of the text from our discussion to the Supplement suboptimal, as it would be impractical for readers, forcing them to jump between the main article and the supplement to read and reflect on the discussion in full.

While we acknowledge the manuscript's length, the preprint has already attracted significant interest from the scientific community, with over 2,300 views and more than 270 downloads since September 2025. We believe this high level of engagement indicates that the audience values the depth and interdisciplinary nature of the discussion. We consider it essential to maintain this level of detail to address the complexities of the study adequately.

However, we have critically reviewed the entire Discussion section and made every effort to ensure the writing remains as concise as possible. Specifically, we focused on eliminating redundancy and removing details not crucial to the core message of our study. Following your suggestion, we have shortened Section 4.1.2, for example by reducing the historical details by over 60% (L. 773–785). Nevertheless, in response to suggestion from Reviewer 1, we have included broader context regarding the observed ecosystem shift (L. 1257–1282). Furthermore, to improve clarity and highlight our main findings, we have restructured Section 4.2. We have also divided it into subsections (L. 1167–1422), adopting your constructive recommendation.

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