We thank Reviewer #2 for the detailed and thoughtful review, which have prompted a number of clarifications and additions that we believe strengthen the clarity and rigor of our manuscript substantially. Following is our point-to-point response to the reviewer's comments.

 # Review 2:

"The manuscript "Plant hydraulic traits regulating productivity and drought resistance in boreal crops" written by Tang et al., explored the responses gross primary products and evapotranspiration for some crops and grasses via perturbating some aboveground plant hydraulic traits. Those traits are defined and linked to the specific modeling parameters and recently development soil-plant model. The shown data was complex with mix of measured (EC and Sentinel2 which have been published elsewhere) and simulated data for two sites with contrast soil and evaporative demand and different modeling configuration (some selected hydraulic traits).

The MS could be considered further for the publication however some critical points need to be considered: (i) clear objectives and hypothesis are required and followed by the structured results and discussion."

Reply #2.1: We thank the reviewer for this suggestion. We agree that stating the objectives explicitly, and then carrying them consistently through the Results and Discussion, will substantially improve the logical structure of the paper. We will therefore add the following three objectives at the end of the Introduction (see also Reply #1.2). Objective (i) validates the modelling framework on which the analysis rests, while objectives (ii) and (iii) directly test the hypotheses introduced earlier:

1. To evaluate whether the SPY-C framework can reproduce the observed GPP and ET fluxes at the two contrasting crop sites, thereby validating the model as a tool for inferring apparent plant hydraulic traits
2. To quantify and compare the apparent hydraulic traits of the perennial (forage grass) and annual (oat) crops -and to assess how they regulate productivity;
3. To determine whether the two crop types differ in their resistance to drought, and how any such difference is linked to their hydraulic traits.

We will then revise the Results and Discussion to follow this same order, so that the logical thread from hypotheses and objectives, through results, to interpretation becomes easy to follow.

"(ii) it is important to explain how to simulate different crop/grass traits where the sites contain many species. The name of two sites with two dominant species: GRASS and OAT are confusing and misleading since there are more species there"

Reply #2.2: We thank the review for this critical comment. This is an important point that we share, and it also recurs in several detailed comments (see also our reply #2.5, #2.10, #2.25, #2.29, #2.30). Here, we would like to clarify the conceptual basis of our approach and then describe the changes we will make.

In this study, SPY-C does not simulate each species separately. Instead, following the model-inversion literature (Liu et al., 2020), it retrieves **apparent** (effective, ecosystem-level) hydraulic traits – i.e., an average over the collective of individuals within the eddy-covariance (EC) footprint, weighted by their contribution to the measured GPP and ET fluxes. This is precisely why we use the word "**apparent**" throughout (definition at lines 67–69 of the

original). In the revised manuscript, we will make this much more prominent by defining "apparent hydraulic traits" at first use in the Abstract and Introduction. We will also add sentences in the Methods (Sect. 2.2.3) explicitly stating that the inferred traits represent effective traits at field-level rather than any single species. We note that at both sites, the mix of different crop species were quite homogeneous in space. They were not planted in patches. This supports that the footprints of EC tower can be treated as a homogeneous effective community.

We take the reviewers' point that "GRASS" and "OAT" can imply monocultures. We will rename the sites neutrally as "Qvidja (Perennial grass-mixture)" and "Hauho (Oats and cover crop)" throughout the text and figures. We will ensure that the species composition is restated in the captions of Tables 1–2 and Figs. 2, 3, 5, 6 and 10 so the naming is never read in isolation.

"(iii) The limitation of model and work have been discussed with regards of lack measurement in plant hydraulic conductance. The MS only focused on the aboveground traits and root roles did not consider or mention which are even more important in responses and adaptation."

*"*Line 25: How important of the root in the hydraulic regulation and overcome the drought?*""*

Reply #2.3: We agree that roots are central to drought response and that this deserves explicit treatment. We would like to clarify how roots currently enter SPY-C, and we will expanded the limitations section accordingly:
(1) The inferred Kp is the maximum whole-plant conductance per unit leaf area (Eq. 1 in the manuscript), i.e., it lumps the entire soil-to-leaf hydraulic pathway, including the root and root–soil interface, into a single effective conductance. So roots are not absent — they are embedded in Kp — but the model does not resolve a separate root, stem, and leaf conductance, nor does it represent root-system dynamics (rooting depth, root distribution, hydraulic redistribution etc.).
(2) Water uptake is taken from a single soil layer of fixed effective root-zone depth (0.6 m in this study), and root carbon is tracked as a pool (Eqs. 17, 20) but does not feed back on hydraulic conductance or rooting depth.

We will add a dedicated paragraph to Sect. 4.3 ("Model uncertainties and future development") stating explicitly that (a) the present analysis concerns aboveground/whole-plant apparent traits, (b) root traits are only implicitly represented within Kp and the fixed root zone, and (c) resolving root hydraulics and dynamic rooting depth — which can dominate drought adaptation in cereals and grasses (e.g., Canales et al., 2021, who reported root hydraulic regulation in oat) — is an important next step for future studies. We will also note this caveat where we interpret the soil-drought "resistance" result, since avoidance via the root zone is part of that mechanism. Please see Reply #2.13 and #2.22 which addresses similar issues.

"*(iv) may terms and variables are mentioned. Overall, they are quite well explained however, some are not, in terms of unit and name (see below).*"

Reply #2.4: We thank the reviewer for pointing this out. We have had Table S2 in Supplement summarizing all the parameters including their units and default values. But we did not mention the table explicitly in the manuscript. We will add a clear reference to Table S2 in the revised version of the manuscript. In addition, we will also carry out a systematic pass over all model variables for naming and units, and extend Table S2 to also include model input and output variables with their symbols, definitions, and units. The specific unit issues raised (Figs. 8, 9, 10; lines 294–295) are addressed individually below.

Abstract

"*Line 2 Crop and grass definition*"

Reply #2.5: *We will clarify the terminology at first use.* In this study "crop" refers to the cultivated field vegetation at each site. The two contrasting crop types studied are a perennial forage grass (a multi-species sown grass harvested for fodder) and an annual cereal (oats). We have reworded the opening sentences of the Abstract and Introduction so that they are defined the first time they are used.

"*Line 6: what is hydraulic conductance, plant or root or stem?*"

Reply #2.6: It is the maximum whole-plant hydraulic conductance per unit leaf area (Kp), integrating the soil-to-leaf pathway. We will make this explicit in the Abstract ("maximum whole-plant hydraulic conductance") and at every subsequent first mention, and we contrast it with leaf-level conductance where relevant. See also our Reply#2.3 on the root component of hydraulic conductance.

Introduction

"Determination and definition of hydraulic traits are still vague. What are exact hydraulic traits are you looking at?"

Reply #2.7: We will add an explicit, concrete definition early in the Introduction in the revised manuscript. The hydraulic traits we infer are the two parameters of the Phydro hydraulic formulation: (1) Kp – maximum whole-plant hydraulic conductance per unit leaf area (a measure of hydraulic efficiency); (2) ψ50 – the water potential at which 50 % of maximum hydraulic conductance is lost (a measure of hydraulic safety). We will name these traits explicitly in the Introduction and again in Sect. 2.2.3, so the reader knows exactly which traits we are looking at.

"See also the apparent hydraulic traits which also mentioned somewhere? What they are? i.e. in line 68-69 and line 71. Which exact scale of hydraulic traits considered in your modeling approach?"

Reply #2.8: See also our reply #2.2. "Apparent" hydraulic traits are effective, field-/ecosystem-scale traits – averaged over all individuals within the EC footprint – as opposed to the organ- or individual-level traits measured on single plants in the laboratory. The scale is therefore the field or ecosystem scale, consistent with the EC and Sentinel-2 data used to constrain the model. We will expand this definition so it appears clearly at first use.

"Line 23-24: this is not clear sentence, what is inherent structural constraints. How stomatal regulation could be a trade-off?"

Reply #2.9: We will rewrite this passage to remove the ambiguity. The intended meaning is: the efficiency–safety trade-off arises because xylem anatomical features that increase conductance (e.g., wider, more interconnected conduits) also tend to increase vulnerability to embolism — these are the "inherent structural constraints". We have replaced that phrase with the explicit anatomical mechanism. Regarding stomata: we did not mean that stomatal regulation is itself a trade-off, but that an analogous efficiency–safety trade-off has been described for stomatal behaviour (a more "acquisitive"/anisohydric strategy maximises gas exchange but increases the risk of reaching damaging water potentials).

"Line 25: I still do not see the diverse hydraulic strategies, especially how it is relevant for grass and oat in boreal regions. Why did you choose these two types of plants? These two plants are "grass" with stem-hollow plants, how the xylem metabolism is important in this case?

Reply #2.10:

- Diverse hydraulic strategies: We will soften/clarify this sentence: the point from the literature is that the strength of the efficiency–safety trade-off is debated, indicating that different species adopt different positions along the efficiency–safety spectrum. Whether boreal crops are acquisitive or conservative is precisely the open question we address.

- Why these two plant types: They were not selected as botanical contrasts in xylem anatomy but as the two agronomically dominant and contrasting cropping systems (perennial grass mixture vs. annual cereal) in boreal agriculture for which are expected to show contrasting hydraulic traits and water use strategies at the field level. We will make this rationale more explicit in the Introduction by listing our objectives clearly following the comments by both reviewers (see also our reply #1.2 and #2.1).

- Hollow-stemmed grasses / xylem: We thank the reviewer for this comment. The reviewer is right that our studied crops are stem-hollow graminoids. However, we note that water transport is not associated with the hollow pith but occurs through numerous vascular bundles embedded in the stem wall (e.g., Heckwolf et al. 2015). Each vascular bundle contains functional xylem vessels that form a continuous hydraulic pathway connecting the roots, stems, and leaves. Therefore, xylem hydraulics remains essential for long-distance water transport in grasses, despite the absence of secondary woody tissues. We will clarify this point in the revised manuscript.

Line 64: the reference was a bit too less, there are more work ongoing (to include plant hydraulic into crop models) in this topic.

Reply #2.11: Thank you for the suggestion. We will expand the citations here to better reflect the growing body of work coupling plant hydraulics with crop models, such as Wang et al. (2020), Gleason et al. (2022) and Kubota et al. (2026).

"Line 75: you said "our hypotheses" what are they? Or do you mean the summarize of three hypotheses that you draw out from the existing literatures i.e. safety, efficiency, and plasticity? One needs a better and clear hypothesis, objectives and how they are related to the clearly defined hydraulic traits. Look at the results section, the results are not so well structured and clearly link to the hypotheses and objectives."

Reply #2.12: We agree with the reviewer that our hypothesis and objectives are not clearly linked to the results sections. We will revise the Introduction to present clearer hypotheses and objectives that are reflected in the results sections consistently. Please see also our reply #2.1.

Materials and methods
Line 86: how about the other component of plant conductance? Root, stem?
Reply #2.13: Please see our Reply #2.3. Kp is the maximum whole-plant conductance lumping root, stem, and leaf resistances together. The model does not partition among them. We will state this explicitly at the definition of Kp and cross-referenced the limitation in Sect. 4.3.

Line 88: how estimate the gs and which one is leaf and/or plant water potential? It is not clear to me the gradient of water potential in the plant or canopy.

Reply #2.14: gs is not prescribed; it is solved as part of the optimality calculation. Phydro finds the gs (equivalently the stomatal-regulated Δψ) and Jmax that maximise the net carbon profit F (Eq. 10), subject to the steady-state leaf water balance (Eq. 1) and CO2 balance (Eq. 9). The relevant water potentials are: ψs = soil (root-zone) water potential, derived from simulated soil moisture via the van Genuchten retention curve (Eq. 13); and ψleaf = ψs −

Δψ, the leaf water potential, where Δψ is the soil-to-leaf potential drop. We will add a few sentences making the derivation of gs and the definitions of ψs, ψleaf, and Δψ explicit at Eqs. 1 and 9.

Line 100: Vcmax, how do you estimate the carboxylation capacity of leaves? Is it from Rubisco?

Reply #2.15: Vcmax is also emergent, not prescribed. It follows from the photosynthetic-coordination hypothesis (Eq. 4): the carboxylation-limited rate Ac is set equal to the light-limited rate Aj at the prevailing light, temperature, CO2, and the optimised Δψ and Jmax. This determines the acclimated Vcmax without assuming a fixed leaf Rubisco content. We will clarify this in the text.

Line 105: how the soil water status is updated (after one day, one hour, one week) and how did it link to photosynthesis? This is more important.

Reply #2.16: The SpaFHy soil-water balance is updated at hourly resolution, the same time step as Phydro, so that photosynthesis and transpiration are tightly coupled to soil moisture within the day. At each hourly step the updated volumetric soil moisture is converted to ψs (Eq. 13) and passed to Phydro, which then solves gs, GPP, and transpiration through Eq. 1. The transpiration in turn withdraws water from the soil store in the next time step. We will make this hourly two-way coupling explicit in Sect. 2.1.2 and in the new coupling/workflow description.

Line 125: How do you measure the alpha and gamma parameters and at which scales? We need of table with variable name of parameters with unit and used values for the calibration and validation work for different plant types.

Reply #2.17: α (unit cost of maintaining photosynthetic capacity) and γ (unit cost of maintaining the hydraulic pathway) are not measured. They are theoretical cost coefficients in the optimality framework of Joshi et al. (2022). Their values are inferred at the field/ecosystem scale by selecting the parameter-perturbation ensemble (PPE) members that best reproduce the observed GPP and ET of the EC footprint. We will clarify this in Sect. 2.2.3 that these are inferred, not measured.

All the model parameters including their units and default values for the two study sites have been listed in Table S2 in Supplement. In response to the reviewer's comment, We will add a clear reference to Table S2 in the revised version of the manuscript.

Line 135: It seems that the model treats LAI as single canopy that do not separate the photosynthesis, transpiration over different leaf types and depths?

Reply #2.18: This is correct. SPY-C uses a big-leaf representation: a prescribed total -sided green LAI (from Sentinel-2) with within-canopy light attenuation via Beer's law (Eq. 8) and per-leaf-area absorbed PAR scaled by LAI (Eq. 7). It does not resolve sunlit/shaded fractions or multiple canopy layers. We will state this explicitly and add it to the limitations in the revised version of the manuscript.

Line 149, soil to root water potential was estimated? And how root play a role here? What is belowground boundary of model and simulation?

Reply #2.19: Soil water potential ψs is computed from its volumetric moisture via van Genuchten (Eq. 13). There is no explicit estimation of root water potential in our model. Root water uptake from soil is merely the transpiration demand met from this layer, and root water

conductance has been lumped into whole-plant hydraulic conductance (Kp). The belowground boundary of the model (i.e., the boundary below soil layer) is free drainage according to Ksoil when soil water potential is above field capacity. We will clarified the belowground boundary of soil and how roots play a role in water balance in the revised version of the manuscript (see also our Reply#2.3).

Line 158: how ET is calculated? How to link the evaporative demand with soil and plant root water uptake. What is the aboveground boundary?

Reply #2.20: Total ET = canopy interception evaporation + soil/ground evaporation + plant transpiration. Transpiration is computed by Phydro (Eq. 12, Tr = 1.6 gs D · LAI), i.e., it is driven by atmospheric demand (VPD) but constrained by the supply through the left-side of Eq. 1 and hence by ψs and Kp — this is the supply–demand coupling. Canopy interception evaporation and soil evaporation follow the SpaFHy formulations, and calculated using Penman-Monteith equation (interception evaporation) and soil-water availability - restricted Priestley-Taylor approach (soil evaporation). The aboveground boundary condition is the meteorological forcing (radiation, T, VPD, CO2, precipitation, wind, air pressure) at the canopy top. We will spell out the equation for calculating ET from the three components and the upper boundary in Sect. 2.1.2 in the revised version of the manuscript.

Line 160: how the phenology was simulated as the authors mentioned here

Reply #2.21: Phenology is prescribed, not dynamically simulated: the seasonal canopy development is imposed through the daily LAI interpolated from Sentinel-2 (Fig. 2c,d), and management events (harvest, grazing, fertilisation) are imposed on the prescribed dates shown in Fig. 2. The PA module then allocates daily GPP to leaf/root/grain pools (Eqs. 16–18). We will reword Sect. 2.1.3 to state clearly that canopy phenology is observation-driven rather than prognostic.

Line 165: how root growth and root distribution are simulated and used in the other modeling routine and how it relates to the hydraulic traits

Reply #2.22: Please see also our reply #2.3. Root carbon is tracked as a prognostic pool (Eqs. 17, 20) subject to allocation, respiration, and turnover, and it supplies litter to soil decomposition model YASSO. However, this root carbon does NOT dynamically change rooting depth or hydraulic traits: the root-zone depth (0.6 m) and Kp are fixed within a simulation and root profile implicitly assumed constant with depth in accordance with the soil water bucket approach. Hence root growth currently feeds the carbon/soil-carbon cycle but is decoupled from the water/hydraulic routine. We will make this explicit in the revised version of the manuscript and flag the coupling of dynamic root growth to water uptake and Kp as an important aspect for future improvement.

Line 192: there are different modeling routines in the whole model which are different from temporal resolution (hourly, daily) how do the sub-model link at that point and how the soil-plant-atmospheric are coupled. I did not see the role of root and set-up of below and above boundary of the model. How dynamics of root growth, leaf area growth contribute to the simulation of water and carbon fluxes.

Reply #2.23: We thank the reviewer for this question, which prompted us to describe the model coupling, boundaries, and treatment of roots/leaf area more explicitly. We will add a dedicated "Model coupling" section at the end of Sect. 2.1 to address the issues raised by the review. Following are our response to the reviewer's questions:

– Time steps and coupling: SPY-C runs on two nested time steps. Phydro (photosynthesis/hydraulics) and SpaFHy (soil water balance) are integrated hourly and coupled two-way: each hour, SpaFHy's soil moisture is converted to soil water potential ψs (Eq. 13) and passed to Phydro, while Phydro solves gs, ψ_leaf (= ψs − Δψ), GPP (Eq. 11) and transpiration (Eq. 12). The transpiration together with soil evaporation, is then withdrawn from the soil water store, updating moisture for the next hour. The carbon routines PA (phenology and allocation module) and YASSO (soil decomposition module) run daily: hourly GPP from Phydro is accumulated to a daily total, allocated by PA to leaf/root/grain pools (Eqs. 16–18), and the resulting litter drives YASSO, yielding RECO and NEE on daily scale (Eqs. 22–23). The hourly to daily link is GPP accumulation, while the daily to hourly link is the prescribed canopy state (i.e., LAI). We will state each module's time step explicitly in Fig. 1 in the revised manuscript.

– Roots and boundaries: Please see also our reply#2.3, #2.19, and #2.20. The upper boundary is the ERA5-Land atmospheric forcing at the canopy top (i.e., hourly solar radiation, air temperature, VPD, CO2, precipitation, wind, pressure). The lower boundary of the model is free drainage according to Ksoil when soil water potential is above field capacity. Roots mainly act as a carbon pool that generates CO2 fluxes through growth and maintenance respiration and supplies litter to soil decomposition model YASSO. The impacts of root on plant hydrology, however, are implicitly lumped into the whole-plant conductance Kp and Δψ (Eq. 1). So root uptake is the transpiration demand supplied from the 0.6 m soil store, constrained by ψs, Δψ and Kp.

– Effect of leaf-area and root growth: Leaf area growth (i.e., LAI changes) is currently prescribed from daily-interpolated Sentinel-2 retrievals (Fig. 2c,d), not simulated prognostically. It enters the hourly Phydro calculation directly via absorbed PAR (Eqs. 7–8) and scales GPP and transpiration to the canopy (Eqs. 11–12), so leaf-area growth modulates both carbon and water fluxes every hour as an observation-driven boundary condition. Root growth is carried only in the carbon balance and affects its growth and maintenance respiration as well as litter input to soil.

Line 196: What are the mineral soil crop fields? It is not clear writing.
Reply #2.24: "Mineral soil" is used in contrast to organic-rich peat soils, which are common and agronomically important in Finland and behave very differently in both its hydrology and carbon cycle. We chose two mineral-soil fields to keep the soil carbon and water dynamics within the regime for which SPY-C/YASSO is parameterised. We will reword the text to "Two agricultural fields on mineral (i.e., non-peat) soils in Southern Finland" in the revised version of the manuscript.

Line 197 and 202: there are mix crops and do not know how big shares of crop are? The name of sites in 205 did not sound reasonable, see 225.
Line 225: The names are confusing and not correct because the footprints contain mix of species. It is not sure how the model treated this difference in terms of species within footprint.

Reply #2.25: Please see also our Reply #2.2. We would like to clarify that the mix crops here mean mixed species sown in the field. They did not grow in patches but well mixed, thus can be treated as an effectively homogeneous canopy. We will clarify that the model treats each footprint as one effective community and infers apparent traits at field level. It does not assign separate traits to individual species.

Line 224: is it similar for both sites? The data showed differently for two sites
Reply #2.26: The simulations were set to start on 01.01.2018 for Qvidja and 01.01.2022 for Hauho, respectively. Both simulations ended on 31.12.2023. The meteorological forcing data

for the two sites were retrieved from ERA5-Land reanalysis data using their geographical locations. They are indeed different. We will reword the sentence to make the message clearer.

Line 225: this did not make sense. There were the measurements of climate data at the EC but why the authors use the ERA5 data which could result many uncertainties for modeling results. Any check or compared for the location data?

Reply #2.27: This is a fair concern. We used ERA5-Land forcing for three reasons: (a) it provides a gap-free, internally consistent set of all required drivers (radiation, T, VPD, precipitation, wind, pressure) over the full simulation period, whereas the on-site meteorological records contain gaps and do not cover every required variable; (b) it makes the workflow transferable to sites and periods without complete in-situ meteorology, which is our intended application (MRV); and (c) for the variables that matter most to the hydraulic response (T, VPD, radiation) ERA5-Land agrees well with the on-site measurements. We indeed tested the model using the local observation data, and the results are quite similar. We will add a short comparison of ERA5-Land vs. on-site T/VPD/radiation/precipitation in the Supplement and a sentence justifying the choice in the revised version of the manuscript.

Line 226: Since the footprint has mix of crop and species, how the authors differentiate the LAI based on the Sentinal-2. This is very critical. How the crop map was used in the foot-print? Did the authors have the detailed crop map?

Reply #2.28: Sentinel-2 gives a total single-sided green LAI per pixel (20x20 m). We do not attempt to split it by species, consistent with our apparent-trait, big-leaf approach (see also our Reply #2.2). The LAI used is the footprint-aggregated total LAI, which is the correct quantity to drive a big-leaf canopy and to compare against the footprint-integrated EC fluxes.

There may have been some unclear description and misinterpretation, as the entire footprint area consisted of similar vegetation dominated by the species in question, and thus did not, for example, consist of subareas dominated by different species. As different species are well-mixed and are not grown in patches, we did not use a per-species crop map. We will state explicitly that the total (not species-resolved) single-sided LAI is used and why this is consistent with the apparent-trait framework in the revised manuscript.

Table 1 and Table 2 are confusing, while the author emphasized two locations with GRASS and OAT, the table mentioned again different crops and grass which is very inconsistent to the text. It is really not clear whether the study simulate single plant or mix of plant per site or all sites.

Reply #2.29: We will make the tables internally consistent with the two-site framing and with the apparent-trait concept (See our Reply #2.2). Specifically, Table 1 now groups the best-fit parameter sets under the two sites, and we modified "perennial grass" to “Perennial grass mixture (Qvidja)” representing the EC footprint community of the Qvidja site; "Oat" to “Oats (Hauho)” emphasizing the EC footprint of Hauho during periods when oats grow; "Cover crop mixture" to “Cover crop (Hauho)” to denote the specific period at the Hauho site dominated by the undersown annual Italian ryegrass. We also added a caption sentence: "Each parameter set is an apparent, field-level trait set for the dominant community of the corresponding footprint/period, not a single-species value." in Table 1. Table 2 likewise now uses the same names for crop types. We hope this removes the apparent inconsistency.

Line 199, did the authors simulate different grass, legume, oat, clover, rye grass? It is not clear, for which analysis and objectives?

Reply #2.30: See also our reply #2.2 and #2.29. No–we did not run separate species simulations. The species listed (timothy, meadow fescue, white clover at Qvidja; oat and undersown Italian ryegrass at Hauho) describe the observed footprint composition. The model infers one apparent trait set per footprint/period. We will reword the text so that the species lists are clearly descriptions of cover, not separate model runs.

Figure 2b, it is not clear the range of SWC for two locations, look like Qnidja with purple color has very high SWC, around 0.5 cm3 cm-3, or the legend did not show clearly. For the LAI in 2c, it should be clear how to create the interpolated LAI, especially the off-seasons

Reply #2.31: We will improved Fig. 2: (a) we relabelled and re-coloured the Qvidja/Hauho measured-SWC envelopes and clarified the legend; the high-saturation end for Qvidja reflects the near-saturated clay-loam soil in winter/spring (which is physically reasonable and motivates the full-saturation initialisation); (b) We added to the caption (and Methods) that the daily LAI was obtained by Gaussian-process-regression interpolation of the Sentinel-2 retrievals (Vira et al., 2025). In the presence of data gaps the interpolation tends towards the mean of the Gaussian process, which is set to zero. This approach was found to perform well under Finnish conditions, since extended data gaps were almost exclusively caused by wintertime snow cover. The gaps are reflected by the error estimates of the interpolated time series. We will state this explicitly so the winter values are not misread. We also note that although the off-season LAI has larger uncertainties, its impact on GPP is small.

Line 263, the Ψ50 is the leaf water potential or stem water potential? For the herbaceous crops or grass, -3 MPa is already too low that plant might be almost wilt thus the lost of hydraulic conductance at this threshold might be too low for 50% reduction.

Reply #2.32: ψ50 here is the water potential of the whole-plant vulnerability curve (Eq. 2), expressed on the same water-potential basis as ψleaf, not a stem-specific value. We agree −3 MPa is at the conservative (safe) extreme for herbaceous crops; it is the lower bound of the prior range [−3, −1] MPa, deliberately set wide to avoid prematurely excluding values. Consistent with reviewer's physiological intuition, our results do NOT favour the low end, and indicates that higher ψ50 (≈ −1 MPa, i.e., the less-negative/wilting-prone end) can well reproduce the observed stomatal behaviour (Fig. 10).

Line 294 and 295, units?

Reply #2.33: We have added the missing units. pWUE (Eq. 26) = GPP/Tr is in μmol CO2 (mmol H2O)$^{-1}$ (or, equivalently, the unit shown in Fig. 5); iWUE (Eq. 27) = (Aj+Rd)/(1.6 gs) is in μmol CO2 (mol H2O)$^{-1}$. These now also appear in the consolidated symbol table (see Reply #2.4) and are consistent with the axis labels in Figs. 5 and 8.

Section 2.2.3, it is not clear the table 2 should belong to the results. Also, it is not clear how the modeling sensitivity analysis and perturbation contribute to the objective and hypothesis (see repetition in section 3.2)

Reply #2.34: Table 2 defines the experimental design (the seven PPE experiments and their settings), so we will keep it in the Methods. The modeling sensitivity analysis and perturbation will serve the newly added objective well (Please see our reply #2.1). More specifically, the PPE + best-fit selection serves the **inversion** (which traits the crops have), while the Morris sensitivity analysis on the same ensemble serves the **attribution** (how each trait affects GPP/ET/WUE and the soil- vs atmospheric-drought response).

It would be better to provide a diagram to show the work flow. It is really confusing there with two sites with different soils and SWCT, different growing seasons, initialization, different PPE with soil moisture and VPD then different bet-fist of parameters. It is complex to follow up the information.

Reply #2.35: We agree, and thank the reviewer for the suggestion. We will add a workflow/experimental-design schematic figure showing how PPE experiments per site (ctrl/swrc3/fullSM/noVPD) were designed and how best-fit selection (inversion) and Morris sensitivity (attribution) were done, respectively for what objectives. This will complement the structural diagram (Fig. 1) and should make the design much easier to follow.

Results and discussion
Line 305: The figure must be right away after the first mentioned text.
Reply #2.36: We will adjust the figure placement so that each figure appears as close as possible after its first citation.

Line 312: now the crop mixture comes, how the simulation was done with crop mixture and how hydraulic traits were considered?
Reply #2.37: This refers to the cover crop (Italian ryegrass) period at the Hauho site. As with the other footprints, we did not simulate species individually. We inferred a separate apparent trait set for the cover crop dominated period (the blue "plus" symbols in Fig. 3a,b). We will clarify this in the newly added workflow figure (See Reply #2.35) and the revised text (Please see also our Reply #2.2).

Line 320: "diurnal"
Reply #2.38: Thanks for noticing the typo. It will be corrected in the revised version of the manuscript.

Figure 3: what does it mean "grass (all), "Oat (all). Why there is no red and blue crosses in c) and d)
Reply #2.39: "GRASS (all)" / "OAT (all)" denote the best-fit sets selected when all available benchmarking periods are used, as opposed to the period-specific selections. We will replace these labels with clearer wording in the legend and caption (e.g., "all periods" vs. the specific benchmarking window). The red and blue crosses mark the position of the five best-fit parameter sets as shown in Table 1. They all have lower α values. So they do not show up in (c) and (d) which are fir higher α values. We will add a more detailed explanation on the panel layout in the caption.

Figure 4: from which best fit parameters? There are different ones? Or mean of different best fit parameter sets.
Reply #2.40: Fig. 4 shows the average of the five best-fit PPE members for each site using QV_ctrl and HA_ctrl — the violins show the spread across days, not a single set and not a single mean. We will state this explicitly in the caption.

Figure 5. The author mentioned the measured soil moisture content at 5-10 cm soil depth, I did not see the measured soil water content here. And which simulated SWC are shown here in 5i and 5j. See also the figure 6., which soil depth or over soil profile? How is to determine the wilting point? It is confusing to use the inconsistent term: best fit parameter and five best fit parameters for the results.
Reply #2.41: Figs. 5i,j show the modelled root-zone (0–0.6 m, single-layer) soil moisture across the PPE, with the best-fit members as red circles. The model's single bulk layer is not directly comparable to a point soil moisture sensor at 5–10 cm. We therefore did not overlay the point measurement in the violin panels. To address the comment we will state explicitly in the caption that 5i,j show modelled bulk root-zone soil moisture (θ), and the modelled-vs-

measured soil moisture has been provided in Fig. S1 in the Supplement. In Fig. 6, sensitivities are likewise for the modelled bulk root-zone soil moisture.

Wilting point shown in Figs. 5i,j is defined as the soil moisture at a reference matric potential (−1.5 MPa). We now state this definition explicitly where the wilting point is first used and in the captions of Fig. 5. To remove the "best-fit parameter" vs "five best-fit parameters" inconsistency, we will use a single consistent phrase — "the five best-fit parameter sets" — throughout, and define it once in Methods.

Figure 8: check the units of variables, micromole C?
Reply #2.42: Thanks for noting this. We have and corrected the units in Fig. 8. The assimilation rate, Vcmax, Jmax, etc. are all in μmol CO2 m^{-2} s^{-1} to be consistent with the units of pWUE/iWUE which use μmol CO2 (not μmol C). Stomatal conductance should also be in umol CO2 m^{-2} s^{-1} (not mol C m^{-2} s^{-1}).

Figure 9: Figure 9a needs unit of plant water conductance to avoid the ratio unit
Reply #2.43: Thanks for noting this. Fig. 9a now shows plant water conductance with its physical unit (10^{-16} m).

Figure 9c uses the same term ET or transpirations like in other graphs
Reply #2.44: The quantity in Fig. 9c is Transpiration ratio (between simulations with higher Kp and that with lowest Kp), not the absolute transpiration values. We will double check that "ET" and "transpiration" are used consistently and correctly distinguished throughout (ET = transpiration + canopy + soil evaporation).

Why the change of GPP is much less compare to change of ET when increase in plant water conductance. The GPP in 9b even still maintains or increase around 2 %? There will be no reduction of GPP due to the soil water reduction as sequence of high plant hydraulic conductance
Reply #2.45: This is exactly the central mechanism the figure is meant to illustrate, and the behaviour is physically expected. Increasing Kp relaxes the hydraulic supply limit, so transpiration responds strongly with changing hydraulic conductance (large change in panel c), but the carbon gain saturates because photosynthesis becomes light/biochemistry-limited rather than water-limited — hence GPP changes little (panel b). A faster decline of GPP (ratio from 1 to 0.88) indeed occurs when soil moisture is below the wilting point (i.e., <-1.5 MPa) and soil water limitation becomes significant. But in this regime, the decline of transpiration is even faster than that of GPP, therefore pWUE exhibits a significant increase. We will add a sentence to the Fig. 9 discussion making explicit why there is the absence of a GPP reduction with the decline of soil moisture.

Figure 10 now shows for different crops and grass which should be somewhere in very beginning. Why now measured data for oat in 10e. Why plant conductance has unit in m (see also somewhere in the materials and methods).
Reply #2.46: Fig. 10 compares our inferred whole-plant conductance and stomatal response against empirical data for similar crop species from temperate region — it is deliberately in the Discussion (Sect. 4.3) because it is a model-vs-literature evaluation and a key part of the uncertainty discussion.

Panel (e) does not show measured data for oat because in the original study, i.e., Canales et al. (2021), oat (Avena sativa) leaf hydraulic conductance was not measured. This study only measured root hydraulic conductance (no root water potential measured), and it is done separately from leaf water potential measurements, making it impossible to link the leaf water potential (the X-axis) with the root hydraulic conductance in the plot. We will explain this clearly in the revised version of the manuscript.

We use “m” as the unit for plant hydraulic conductance to be consistent with that used in Phydro formulation (Eq. 1) (Please see also our reply #2.4). Indeed, in most of the literature, hydraulic conductance is in a different unit, such as mmol m-2 -s Mpa-1. Therefore, we have listed the values using different units in Table S3 in the Supplement. We would like to refer to that table, if the readers are interested in checking the values of hydraulic conductance in different units.

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